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Identifying the Dynamic Effects of Income Inequality on Crime

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Abstract

What happens to crime after an increase in income inequality? The microeconomics literature that attempts to answer this question often employs identification strategies that exploit external sources of variation that provide quasi-experiments to identify causal effects. In contrast, this paper tackles this question by using structural vector autoregressions (SVAR), a methodology typically employed in modern empirical macroeconomics to identify and estimate dynamic causal effects of exogenous shocks. Unlike the macroeconomic SVAR models that are often applied to time-series data, we exploit the time series and cross-sectional dimensions of our data, leading to the estimation of panel SVAR models. Using U.S. state-level data for the period 1960–2015, our results indicate that structural shocks to inequality increase both violent and property crime. Variance decomposition analyses show that inequality has little explanatory power for movements in crime.

I. Introduction

Although economic and sociological theories predict that income inequality increases crime, empirical support for this link remains as elusive as ever. Fajnzylber *et al.* (2002a,b), for example, report positive and significant relationships between income inequality and violent crime in a panel of countries. Based on municipal-level data for Brazil, Scorzafave and Soares (2009) estimate an elasticity of pecuniary crimes relative to inequality of 1.46. Demombynes and Özler (2005), who analyze data on a cross-section of South African neighbourhoods, also find that inequality is associated with property and violent crimes. Recently, Enamorado *et al.* (2016) examine the impact of inequality on crime rates during Mexico's drug war, reporting that a one-point increase in the Gini coefficient raises drug-related homicides per 100,000 inhabitants by more than 36%. Kelly (2000), and Choe (2008), using U.S. state-level data, find that income inequality has a strong and robust impact on several measures of violent crime but no effect on property crime. In contrast, Chintrakarn and Herzer (2012) provide evidence of a negative correlation between inequality and crime; Brush (2007) provides mixed evidence, with inequality having a positive link with crime in a cross-section of U.S. counties, but a negative association in his

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time-series analysis; while Doyle *et al.* (1999), and Neumayer (2005) find no significant link between inequality and violent crime.

Underlying this mixed empirical evidence is a serious endogeneity problem that arises from measurement error, unobserved heterogeneity, and the simultaneous determination of crime and income inequality. Income inequality is affected by some of the same factors that drive individuals' decisions to engage in criminal activity, making it necessary to control for reverse causality and simultaneity. As pointed out by Enamorado *et al.* (2016), while inequality may increase crime, an increase in crime may also affect inequality by, for instance, prompting higher income individuals to move out of areas ridden by a high incidence of criminal activity. As well, suppose state and local governments attempt to reduce crime by encouraging education.[1] The resulting increase in education, in turn decreases inequality, causing the estimate of the coefficient on inequality in an OLS regression of crime on inequality to be positively biased.[2] Evidently, cause and effect are not well defined in regressions of crime on income inequality, to the extent that these studies fail to address this simultaneity bias. Another important caveat that cannot be overlooked, especially in cross-country studies, is the bias that results from omitted variables and measurement error. Cross-country crime and inequality statistics are often susceptible to measurement error, given the difficulty of obtaining reliable and comparable data across countries. In addition, even after controlling for a number of observed factors that drive both inequality and crime, unobserved heterogeneity remains a problem that plagues much of the empirical literature.

Recent empirical papers have resorted to differences-in-differences or instrumental variables (IV) estimation methods to as a way around these endogeneity problems and to estimate the causal effect of inequality on crime. Enamorado *et al.* (2016) use the predicted income distribution of Mexico's municipalities based on the initial income distribution of the municipality and the national patterns of income growth to construct an instrumental variable for the observed Gini coefficient. Intuitively, their strategy measures change in municipal income inequality not affected by local factors, such as crime, but by national trends. Hence, the instrument captures movements in a municipality's inequality that are exogenous with respect to crime in that municipality. To address the issue of unobserved heterogeneity, Enamorado *et al.* (2016) include state-year fixed effects in all regressions. Additionally, they limit their analysis to Mexico's municipalities to circumvent international data quality and comparability problems. Similarly, Choe (2008) attempts to sidestep problems associated with unobserved heterogeneity and measurement error by using U.S. state-level panel data from 1994 to 2004, and deal with simultaneity by using the Arellano and Bond (1991) Generalized Method of Moments (GMM) estimator. Scorzafave and Soares (2009) use data for Sao Paulo's municipalities, and address concerns with endogeneity by using lagged regressors.

This article takes a fresh look at the question of the empirical link between income inequality and crime. We address the aforementioned endogeneity problems by relating U.S. state-level crime rates to measures of exogenous shocks to income inequality using structural vector autoregressive (SVAR) models, an approach typically employed in modern

[1] See e.g. Bell *et al.* (2016), Lochner (2004), Lochner and Moretti (2004), Machin *et al.*, (2011, 2012) for literature on the crime-reducing effects of education.

[2] See e.g. Sylwester (2002), and Abdullah *et al.* (2015) for papers that document a negative impact of education on inequality.

empirical macroeconomics to identify and estimate dynamic causal effects of exogenous shocks. Our proposed VAR methodology offers several advantages. First, like VAR models in the macroeconomics literature, our panel SVAR approach helps us to identify the linearly unpredictable movements in income inequality, and allows for reverse causality. Specifically, identification restrictions are imposed on the VAR model so that movements in state income inequality that are exogenous with respect to state crime rates can be recovered trivially. Consistent with the broader macroeconomics literature, we refer to these linearly unpredictable movements in inequality as 'shocks' or 'innovations' to income inequality. Because these shocks or innovations are exogenous with respect to crime, their impacts on crime represent *causal* effects. Second, the panel VAR methodology allows us to trace out the dynamic effects of these shocks to inequality on crime through the use of impulse response functions. Tracing out the dynamic responses of crime to shocks to inequality is quite important, as it potentially explains the wide range of estimates documented in the literature. Third, our empirical framework enables us to quantify how much of the variability in crime rates, on average, is accounted for by shocks to income inequality, as opposed to other shocks. We do this through the use of forecast error variance decompositions. Fourth, unlike VAR models applied to time series data, our *panel* VAR model also exploits the cross-sectional dimension of our data, thereby allowing us to control for state-specific unobserved heterogeneity. Fifth, with annual data for the period 1960–2015 for the fifty U.S. states and D.C. (56 observations per state), a traditional time-series VAR model is likely to be inadequate to uncover the effects of inequality on crime for each state.[3] Combining the cross-sectional and time-series aspects of the data yields a sample that exceeds 2,800 observations, which is reasonably large to obtain reliable estimates of the effects of inequality on crime. Sixth, while we make no claims that our data are free of measurement error, our use of U.S. state-level data mitigates some of the measurement error problems associated with cross-country studies, namely data quality and comparability. Given the difficulty of finding valid instruments, this panel VAR approach provides an ideal alternative to estimate the causal impact of inequality on crime.[4]

Our baseline empirical results imply that structural innovations to income inequality increase aggregate violent and property crime rates. While the contemporaneous responses of both aggregate crime measures are statistically indistinguishable from zero, subsequent responses display positive and statistically significant hump-shaped patterns, with respective peak effects of 0.4% and 0.3% following a 1% point increase in the Gini index. Decomposition of variance analyses reveal that shocks to inequality account for negligible proportions of the variability of crime rates. In no instance do shocks to income inequality explain more than 3% of variance of crime. Our main findings are largely robust to alternative identification restrictions, and different measures of violent crime and property crime.

The remainder of the paper is organized as follows. The next section presents the data and discusses some stylized facts concerning the relationship between inequality and crime.

[3] Washington, D.C. is drastically different than any state in the sample. Also, local governance in D.C. began in 1975. Results (not shown to save space) do not change when D.C. is dropped from the analysis.

[4] Similar panel VAR methods have been employed by e.g. Love and Zicchino (2006), Fort *et al.* (2013), Atems and Jones (2015), Atems (2018, 2019a,b), Matvos, Seru and Silva (2018), and Caballero, Fernández and Park (2019).

Section III provides details of the identification strategy and the estimation method. Section IV documents the paper's empirical results. Concluding remarks are contained in Section V.

II. Data and stylized facts

Data

Data on U.S. state-level crime rates

Our data on U.S. state-level crime rates are annual covering the period 1960–2015. The data come from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR). The UCR data do not measure actual crimes committed, but the number of offences *reported* by law enforcement agencies to the FBI UCR program each time an individual is arrested, cited, or summoned for an offense. The agencies reporting to the FBI UCR program include city, university and college, county, state, tribal, and federal law enforcement agencies. Three aspects of the FBI UCR crime data are worth mentioning. First, not all crimes are reported to law enforcement agencies, and not all law enforcement agencies report data to the FBI. Second, for offenses that lead to arrests, only the most serious arrests are recorded. For example, if an individual is murdered during the course of a burglary, only murder is recorded. Third, since individuals may be arrested several times over the course of the year, the FBI UCR data do not reflect the number of persons arrested, but a number of times in which individuals are arrested. For these reasons, the FBI UCR data suffer from underreporting bias. This bias, however, is not too concerning for our purposes because it is likely to be correlated with other variables used in the paper (Kelly, 2000). Furthermore, despite the fact that not all agencies report to the FBI UCR program, Evans and Owens (2007), find that agencies that report to the FBI UCR program cover between 88% and 96% of the U.S. population. As well, while not all offences are reported to law enforcement, and not all reported offences lead to arrests, Lochner and Moretti (2004) find considerably high correlations between arrests and actual crimes, with correlations of 0.97 for burglary, 0.96 for rape and robbery, 0.94 for murder, assault, and burglary, and 0.93 for auto theft. For these reasons, the FBI UCR crime data are routinely used in the literature.[5]

The crime data consist of data on aggregate violent crime and aggregate property crime. Aggregate violent crime is the sum of aggravated assault, murder and non-negligent homicide, rape, and robbery. Aggregate property crime consists of auto theft, burglary, and larceny. The aggregate and separate crime measures are all expressed in per 100,000 state residents.

Data on U.S. state-level income inequality

The inequality data are annual for period 1960–2015. Four state-level income inequality measures, namely the Gini coefficient, the Theil entropy index, the top decile income share, and the share of income held by the top 5% of the state's population are used. The data are constructed by Frank (2009), with updated data provided on his website `http://www.shsu.edu/eco_mwf/inequality.html`. Frank (2009) constructs the inequality measures using individual tax filing income data from the U.S. Internal

[5] See e.g. Doyle *et al.* (1999), Kelly (2000), Lochner and Moretti (2004), Choe (2008), Evans and Owens (2007), and Chintrakarn and Herzer (2012).

Revenue Service's (IRS) *Statistics of Income*. The IRS income data used by Frank (2009) include labor income (wages and salaries), capital income (dividends, interest, rents, and royalties), and entrepreneurial income (self-employment, small businesses, and partnerships), but exclude interest on state and local bonds, and state and local government transfers. We refer the reader to Frank (2009) for further details on the construction of the state-level income inequality measures.

A limitation of the inequality measures, pointed out by Frank (2009), is that the IRS data on gross income that is used to compute the inequality indices, omits individuals with earnings below some threshold. The threshold depends on, among other factors, marital status, age, and the tax filing year. While other sources of U.S. state-level income inequality data exist that do not omit low-income individuals, these data are not available annually for a long enough time period. For these reasons, we rely on the Frank (2009) income inequality data. We use the Gini coefficient as the main income inequality measure in the paper, and verify the robustness of our results using three other measures mentioned above.

Other state-level data

We also use per capita state and local police expenditures to control for the role of police in apprehending criminals and deterring crime. Data on state and local police expenditures come from the Annual Survey of State and Local Government Finances (U.S. Census Bureau). We express them in per capita terms by dividing them by annual state population from the Census. We also have data on state employment, collected from the Federal Reserve Economic Database (FRED) of the Federal Reserve Bank of St. Louis. Data on state per capita income are collected from the Regional Economic Accounts (Bureau of Economic Analysis). In the empirical models shown hereinafter, we control for the role of urbanization on crime by using state population density from the Census. Other state-level variables used in the paper (sources in parentheses) include the minority and white population shares (Census Bureau), labour force participation rate (Bureau of Labor Statistics), poverty rate (Census Bureau), income tax rate (National Bureau of Economic Research, `http://www.nber.org/taxsim/state-rate`), and state-level per capita wage (Regional Economic Accounts). The start dates for the variables vary, but all variables end in 2015.

Summary statistics

Table 1 summarizes the basic characteristics of all the data used in the paper. The crime measures each contain five less observations because the FBI UCR data do not contain data for the state of New York for 1960–64. Similarly, per capita police expenditures have fewer observations because the Census provides state but not local expenditures for the years 2001 and 2003, so for this variable, we omit those years.[6] As expected, property crimes are more preponderant than violent crimes, with respective means of 3,626 and 397 per 100,000 state residents. Equally worth noting is the larger within-state variation of most property crimes and the higher between-state variation of violent crimes. Further evident in the table is the relatively high average income inequality across states over the 1960–2015 period.

[6]The number of observations for the other variables depend on their availability, but since they do not enter the empirical models directly, we do not provide much discussion on them.

TABLE 1

Summary statistics for crime rates, income inequality, and other variables

Variable	*Obs.*	*Mean*	*Standard deviation Overall*	*Standard deviation Between state*	*Standard deviation Within-state*
A. Crime measures					
Aggregate violent crime	2,851	397.4	295.7	244.0	170.7
Assault	2,851	235.6	162.6	120.6	110.3
Murder	2,851	6.6	6.1	5.1	3.3
Rape	2,851	29.0	16.0	9.4	13.0
Robbery	2,851	126.2	148.0	129.6	74.2
Aggregate property crime	2,851	3,626.3	1,424.1	883.2	1,123.2
Auto theft	2,851	350.1	226.6	169.6	152.1
Burglary	2,851	908.4	443.3	262.7	358.7
Larceny	2,851	2,367.7	908.2	542.4	731.9
B. Income inequality measures					
Gini coefficient	2,856	53.1	6.4	1.8	6.1
Theil index	2,856	58.7	23.2	9.6	21.2
Top decile share	2,856	38.0	6.2	2.9	5.4
Top 5 income share	2,856	26.6	5.9	2.8	5.2
C. Other variables					
Police spending per capita	2,754	120.0	118.3	49.5	107.6
Employment (thousands)	2,856	1,989.3	2,273.1	2,130.5	845.5
Per capita income	2,856	19,020.6	14,717.3	2,974.8	14,419.4
Population density	2,856	325.9	1,340.4	1,343.5	162.5
Minority population share	2,346	16.4	13.9	13.7	3.2
White population share	2,346	83.7	13.8	13.6	3.2
Labor force participation rate	2,040	6.1	2.1	1.1	1.7
Poverty rate	1,785	13.2	3.9	3.4	2.0
Income tax rate	1,938	5.3	3.3	3.1	1.3
Productivity growth	2,040	4.9	3.7	0.3	3.7
Per capita wage	2,856	8,337.3	6,962.7	2,656.9	6,446.4

Notes: Authors calculations using data from the sources mentioned in text.

Inequality and crime in U.S. states: some stylized facts

This section documents the salient facts of the data that motivate our analysis. Statistics on income inequality and crime come from the sources mentioned above. We start by characterizing the trends in U.S. state-level crime rates, inequality, and other key variables. We then review the theoretical channels through which inequality potentially impacts crime.

Trends in U.S. state-level crime rates

Figure 1 depicts the annual trends in aggregate violent and property crime rates, and the seven disaggregate crime measures averaged over the fifty states and DC for our entire sample period 1960–2015. The figure shows a significant rise in all types of crimes between 1960 and 1980, a temporary decline in the early 1980s which was followed

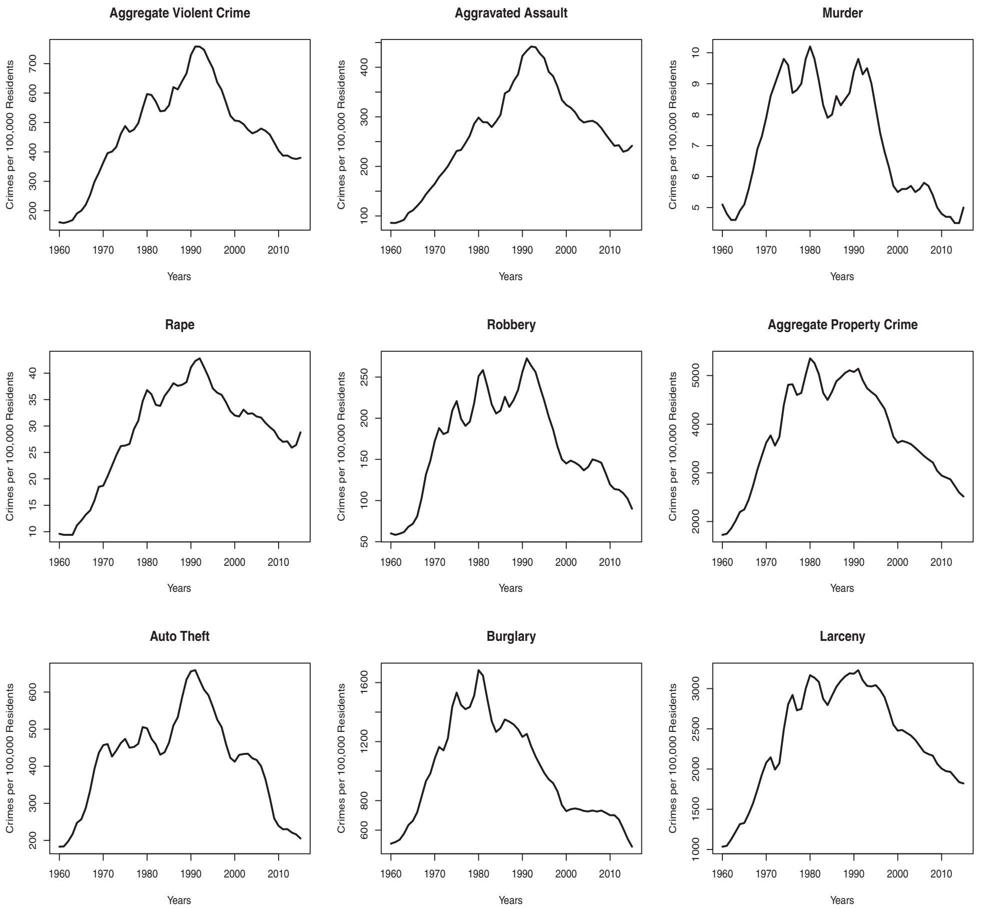

Figure 1. State-averaged measures of violent crime and property crime rates: 1960–2015

by another episode of rising crime through the early 1990s, after which crime rates dropped continuously. The most striking features of the figure are the precipitous rise in all types of crimes between 1960 and 1980, and the persistent fall after the early 1990s. From 1960 to 1991, the average number of violent crimes more than quadrupled, from a low of 161 per 100,000 people to a peak of 758 per 100,000. Looking at specific violent crimes, murder rates more than doubled during the same time period, while robbery, rape, and assault, on average, increased by 34%, 35%, and 42%, respectively. The figure displays a similar pattern with respect to property crime, with average aggregate property crime, auto theft, larceny, and burglary, respectively, rising by more than 20%, 26%, 21%, and 15%. By 2015, while some crimes, notably murder, auto theft, and burglary had fallen to or below their 1960 levels, Figure 1 shows that other crime categories, even though lower than their peaks of the 1990s, were still considerably higher than their 1990 levels. Averaging over states as in Figure 1 may conceal the heterogeneity in crime dynamics across states. This is not the case, as we find very similar dynamics in each of the fifty states and DC. To save space, we do not plot the

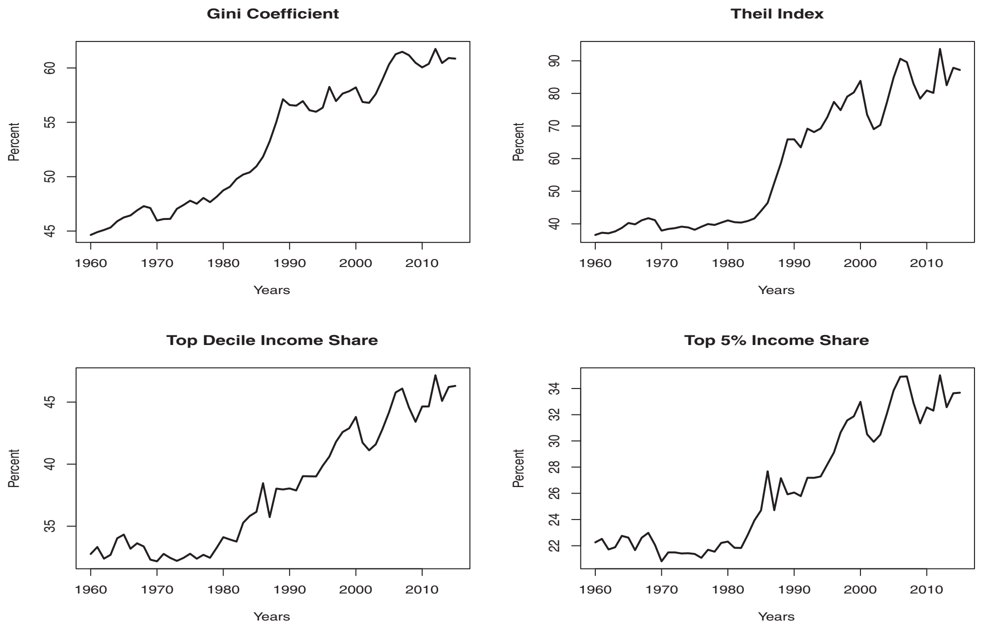

Figure 2. State-averaged measures of income inequality: 1960–2015
Notes: Author's calculations based on data described in text

measures of crime for each state. These plots, nonetheless, can be found in the online appendix.

Trends in U.S. state-level income inequality

Figure 2 graphs the Gini coefficient, the Theil index, the top decile share, and the top 5% income share averaged over the fifty states and DC for the 1960–2015 period. All four measures tell a relatively well-known story. Income inequality in most U.S. states was relatively low in the 1960s and 1970s, rose sharply from the mid-1980s through 2000, and then fell temporarily in 2001 and 2002, only to rise again thereafter. As measured by the Gini index, average income inequality was about 16 points higher in 2015 than in 1960. The pattern is considerably similar for the other measures of inequality, with the Theil index, the top decile share, and the top 5% share, respectively, rising by about 50, 13, and 11 points over the same time period. While several factors possibly explain this trend in inequality, the dramatic increases in inequality depicted in Figure 2 occurred after the Tax Reform Act of 1986. Similar patterns of responses have been documented by Frank (2009) for U.S. states, and Piketty and Saez (2003), Piketty (2014) and Saez and Zucman (2016), using aggregate U.S. data.

Trends in other key U.S. state-level variables

In Figure 1, all measures of violent and property crimes show an increase and then a subsequent decrease, whereas Figure 2 shows a persistent increase in income inequality over the 1960–2015 period. Thus, a quick examination of the figures reveals no clear pattern of the link between income inequality and crime. To help the reader get a good grasp of the data, we provide time series graphs of other state-averaged variables included in the SVAR model discussed below. In particular, Figure 3a plots the natural logarithm of state-averaged per capita income, employment, population density, and per capita police expenditures for the period 1960–2015. All four variables exhibit a strong upward trend over this time period. Because this overall upward trend is very strong, it is difficult to distinguish the cyclical movements. To get a sense of the annual fluctuations in these variables, Figure 3b displays the percentage change in each of the four variables. The figure shows substantial volatility in all four variables. As expected, per capita income, employment, and police expenditures decreased in periods of recessions, while the growth rate of population density significantly rose in the 1960s, followed by a general decrease since. Given the substantial literature that documents that these variables impact both crime and income inequality, it is not surprising that a mere examination of Figures 1 and 2 does not provide a clear picture of the rather intricate relationship between income inequality and crime.

Mechanisms through which income inequality can impact crime

The socio-economic literature proposes at least three theoretical channels whereby inequality may impact crime: the economic theory of crime (Becker, 1968); the relative deprivation theory (Merton, 1938); and the social disorganization theory (Shaw and McKay, 1942).

Becker's (1968) economic theory of crime argues that criminals are rational agents whose decisions to commit crime depend on the difference between criminal returns and returns from legitimate work, the likelihood of apprehension, and severity of penalties if apprehended. As discussed by Kelly (2000), in communities with high income inequality, individuals with low returns from legitimate work are in close proximity to high-income individuals who have assets worth taking. In this case, since the marginal returns to crime for individuals at the lower end of the income distribution more likely exceeds their market returns, areas with high-income inequality are likely to experience an upward spiral in crime. As well, since the opportunity costs of crime – the forgone earnings when engaging in criminal activity and/or income lost if apprehended and incarcerated – is low for low-income individuals, the theory contends that areas with high-income inequality are likely to witness high crime rates. Support for this theory has been documented by, among others, Chiu and Madden (1998), Machin and Meghir (2004), Bourguignon (2001), and Demombynes and Özler (2005).

Relative deprivation posits that an atmosphere of deprivation relative to others is a precursor to crime. Hence, according to this view, income inequality triggers feelings of envy, inadequacy, frustration, and mistrust by the poor against the rich. These feelings generate pent-up aggression and hostility (Xiong, 2015), such that extreme circumstances, individuals at the lower end of the income distribution who feel deprived relative to others may resort to burglary, larceny, and other forms of crime to 'redistribute economic resources' and 'satisfy their sense of injustice' (Roberts and Willits, 2015). Thus, the theory postulates that crime rates should increase as the income gap between the wealthy and the poor widens. Empirical tests of this theory can be found in Stiles, Liu and Kaplan (2000), and Burraston *et al.* (2018).

Shaw and McKay's (1942) social disorganization theory goes beyond the independent effects of relative deprivation on crime, and rather focuses on how low economic status,

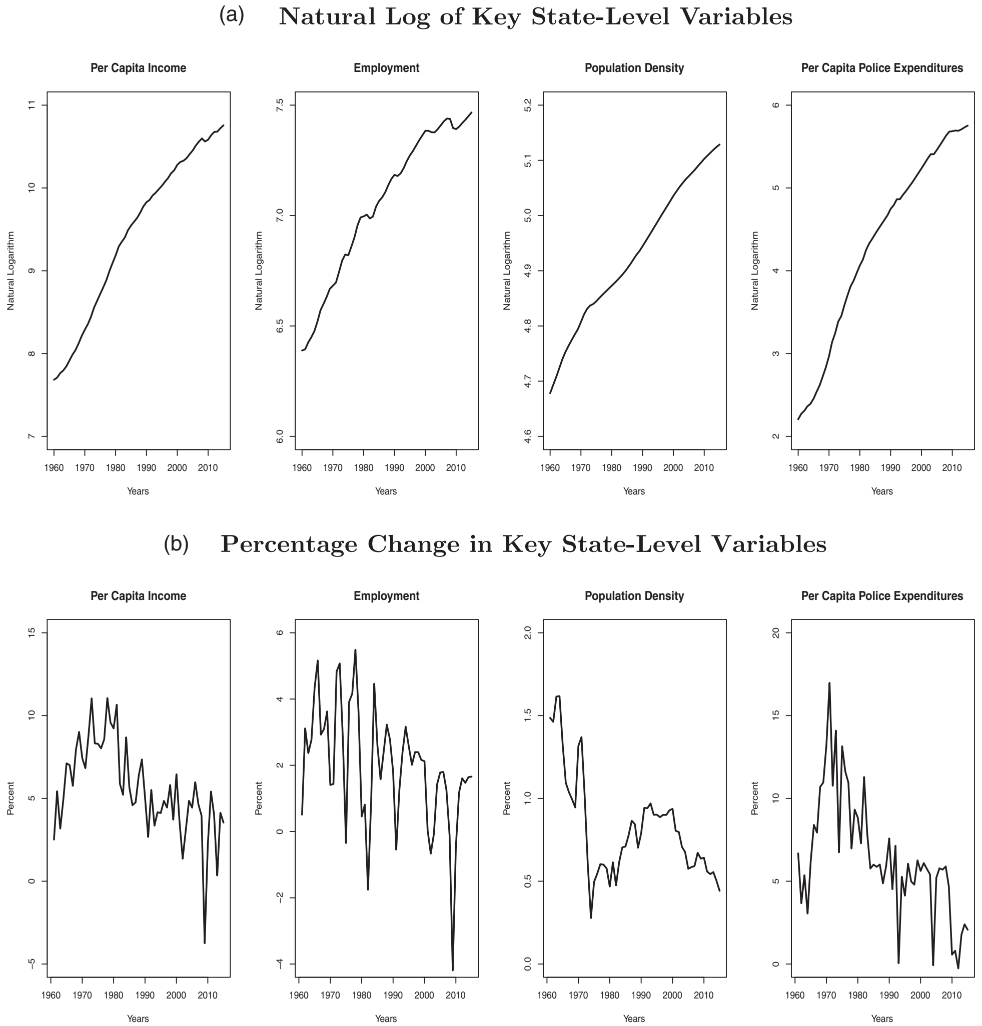

Figure 3. Other key state-level variables: 1960–2015
Notes: Author's calculations based on data described in text

ethnic heterogeneity, residential mobility, and family disruption affect the ability of a community to realize the common values of its residents and maintain effective social controls (Kornhauser, 1978; Bursik, 1988). Blau and Blau (1982) point out that:

> ascriptive socio-economic inequalities undermines the social integration of a community by creating multiple parallel social differences which widen the separations between ethnic groups and between social classes, and it creates a situation characterized by much social disorganization and prevalent latent animosities.

Hence, according to this view, income inequality disorganizes the community social structure, which, in turn, triggers an increase in crime and delinquency. Sampson and Groves (1989), and Blau and Blau (1982) provide support for this view, documenting much higher rates of criminal victimization and criminal offending in areas of high inequalities.

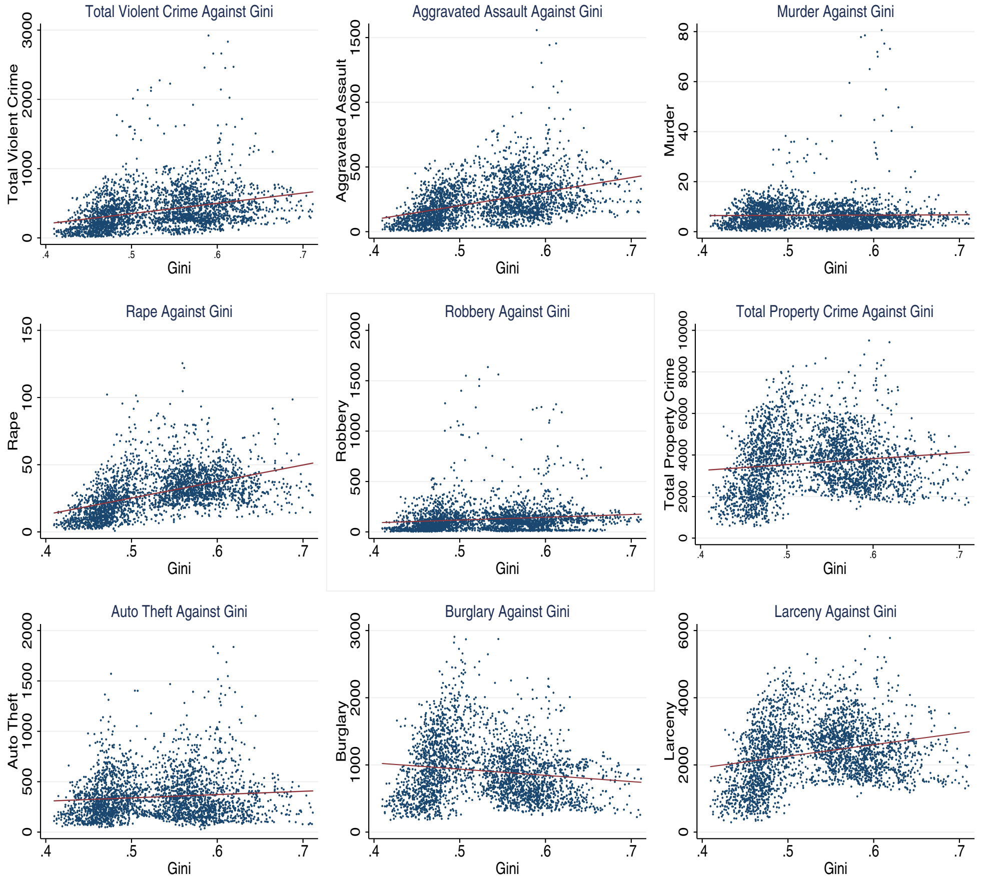

Figure 4. Scatter plots of crime rates against Gini coefficient
Notes: Author's calculations based on data described in text. Data are for the period 1960–2015 for all fifty U.S. states and DC

While these channels are expected to induce a positive impact of inequality for most crimes, the question of the effect of inequality on crime is ultimately an empirical question. As a prelude to the empirical analysis, Figure 4 displays simple relationships between the Gini index and each of the nine measures of crime used in this paper. In five cases, the correlation is noticeably positive. For murder, robbery, and auto theft, the relationship appears to be essentially zero, whereas the Gini index and rape exhibit a negative relationship. Due to endogeneity arising unobserved heterogeneity and simultaneity, is not clear, however, that these bivariate correlations reflect causal links. Section III presents details of our identification approach to go beyond these simple relationships and address issues of causality.

Time-series properties

We now examine the time-series properties of the data as related to stationary, before estimating the VAR models. For this purpose, we carry out, perhaps, two of the most widely used unit tests for panel data, namely the Im, Pesaran, and Shin (2003) and the

TABLE 2

Panel unit root tests

	A. Variables in log levels		*B. Log first differences*	
Variable	*IPS*	*ADF*	*IPS*	*ADF*
Crime measures				
Aggregate violent crime	−12.7(0.0)	−13.6(0.0)		
Assault	−13.0(0.0)	−13.8(0.0)		
Murder	−4.7(0.0)	−4.6(0.0)		
Rape	−7.3(0.0)	−8.8(0.0)		
Robbery	−8.7(0.0)	−9.7(0.0)		
Aggregate property crime	−6.0(0.0)	−6.8(0.0)		
Auto theft	−2.8(0.0)	−3.6(0.0)		
Burglary	0.3(0.6)	0.2(0.6)		
Larceny	−7.2(0.0)	−8.7(0.0)		
Income inequality measures				
Gini coefficient	4.8(1.0)	5.4(1.0)	−49.4(0.0)	−31.0(0.0)
Theil index	6.4(1.0)	6.8(1.0)	−46.6(0.0)	−27.8(0.0)
Top 10 income share	7.4(1.0)	8.3(1.0)	−57.3(0.0)	−39.9(0.0)
Top 5 income share	6.8(1.0)	7.8(1.0)	−62.9(0.0)	−35.9(0.0)
Other variables				
Police spending per capita	−3.4(0.0)	−8.0(0.0)		
Per capita income	11.6(1.0)	9.3(1.0)	−19.2(0.0)	−13.5(1.0)
Employment	1.5(0.9)	10.4(1.0)	−21.6(0.0)	−22.8(0.0)
Population density	3.0(1.0)	3.1(1.0)	−13.6(0.0)	−12.5(0.0)

Notes: P-values in parentheses, and test statistics next the P-values. For the Augmented Dickey-Fuller (ADF), the test statistics are the Z statistics. For the IPS test, the test statistics are the $\bar{W}_t$ statistics.

Fisher-type Augmented Dickey–Fuller (ADF) tests. The null hypothesis of both tests is that all panels contain unit roots. Their alternative hypotheses, however, differ slightly. Under the IPS test, the alternative hypothesis is that some panels are stationary, while the alternative underlying the ADF test is that at least one of the panels is stationary.

The results of the unit root tests are reported in Table 2. We present the estimated test statistics and report their corresponding P-values in parentheses. For the ADF, the test statistics are the Z statistics. For the IPS test, the test statistics are the $\bar{W}_t$ statistics. The tests are performed on variables in logarithms. For the variables found to be non-stationary, we perform the tests on the percentage change in each of the variables. Panel A shows that for all the crime categories, except burglary, both tests reject the unit root null hypotheses. The tests also reveal that per capita police spending is stationary in (log) levels. In contrast, the four measures of inequality, together with per capita income, employment, and population density, are all shown to contain unit roots. Panel B, however, provides some evidence of stationarity of these variables after first differencing. Therefore, police expenditures, and the various crime categories enter the panel VAR models in logs, while per capita income, employment, population density, and the measures of inequality enter the VAR models in log-first differences. It is worth noting that the evidence of stationarity of the (logarithm of) various crime categories and other variables rules out the need to test for co-integration.

III. Identification and estimation methodology

We apply a *panel* structural VAR method to identify the effects of inequality on crime. Our analysis has several distinguishing and attractive features. First, like VAR methods in the time-series literature, the identification restrictions imposed on the panel VAR model allows us to credibly recover the linearly unpredictable component of movements in income inequality that are exogenous with respect to crime. Second, by exploiting the time-series and cross-sectional aspects of our data, the panel VAR allows us to model additional complexities than if we had either only time-series or cross-sectional data. Like many static and dynamic panel data models, we are able to control for unobserved characteristics across states that simultaneously determine income inequality and crime using state fixed effects, while the time-series aspect allows us to include sufficiently long lags of the endogenous variables, thereby practically eliminating concerns about endogeneity (Koop and Korobilis, 2016). Third, the time period for estimation, before taking lags, differencing, and other standard data transformations into account, is 1960–2015 ($T=56$) for the fifty states and DC. This sample period is not long enough to estimate VAR models for each state. By pooling the data, the number of observations now exceeds 2850, resulting in significant efficiency gains. Fourth, we minimize problems of omitted variable bias by estimating a reasonably large, yet parsimonious structural VAR model that permits us to identify shocks to income inequality and their effects on crime without the need to make strong contestable identification restrictions. Fifth, the panel VAR helps us to uncover the dynamic effects of shocks to inequality on crime through impulse response functions. Finally, we are able to quantify the proportion of the overall variability of crime rates that is explained by shocks to income inequality through variance decomposition analyses. These advantages make our empirical approach an ideal alternative for studying the causal impact of inequality on crime.

Because panel VAR methods are not likely to be in the standard toolkit of researchers who study inequality and crime, we present, as clearly as possible, details of the empirical specification in section Empirical specification below, and discuss the identification strategy in section Identification. The identification restrictions underlying the VAR models may be subject to some disagreement. Therefore, in section Discussion of the identification assumptions, we provide further discussion of the identification restrictions to ensure that the results of the paper are robust to alternative identification schemes, and not driven by an unreasonable identification strategy.

Empirical specification

Consider the following six dimensional reduced-form panel VAR model:

$$x_{it}=A(L)x_{i,t-1}+\delta_i+\varepsilon_{it} \qquad \varepsilon_{it}\sim N(0,\Sigma_i) \tag{1}$$

where, $x_{it}=[\Delta y_{it}\,\Delta e_{it}\,\Delta d_{it}\,p_{it}\,\Delta z_{it}\,c_{it}]'$ is a six-dimensional vector that includes the percentage changes in state per capita income (Δy_{it}), in state employment (Δe_{it}), and in state population density (Δd_{it}); the log of per capita state and local real police expenditures (p_{it}); the percentage change in a measure of state income inequality (Δz_{it}); and the log of a measure of crime (c_{it}). $\varepsilon_{it}=[\varepsilon_{it}^{\Delta y}\,\varepsilon_{it}^{\Delta e}\,\varepsilon_{it}^{\Delta d}\,\varepsilon_{it}^{p}\,\varepsilon_{it}^{\Delta z}\,\varepsilon_{it}^{c}]'$ is the corresponding six-dimensional vector of reduced-form residuals. $t=1,\ldots,T$ indexes time; $i=1,\ldots,N$ denotes states; L refers to the lag operator, $A(\cdot)$ is a polynomial in L, and δ_i represents unobservable state effects. The variables Δy_{it}, Δd_{it}, Δe_{it}, and Δz_{it} are in percent changes, while p_{it} and c_{it} are in log levels following the results of the unit root analysis in section Time-series properties.

The combination of the lagged-dependent variables, $x_{i,t-1}$, and the state fixed effects, δ_i, renders standard fixed effects estimation biased and inconsistent. This is the well-known Nickell bias (see Nickell, 1981). The dynamic panel data literature has proposed a number of ways to alleviate this bias. One common approach involves first-differencing equation (1) to remove δ_i. However, $x_{i,t-1}$ remains endogenous due to the correlation between $\Delta\varepsilon_{it}=(\varepsilon_{it}-\varepsilon_{i,t-1})$ and $\Delta x_{i,t-1}=(x_{i,t-1}-x_{i,t-2})$. Nevertheless, if ε_{it} are independent and identically distributed, instruments for $x_{i,t-1}$ can be constructed by using second and higher order lags x_{it}, as these lags will be highly correlated with $x_{i,t-1}$ but uncorrelated with ε_{it}.

One undesirable aspect of the first difference transformation, however, is that it widens gaps between observations in unbalanced panels. To overcome this weakness, Arellano and Bover (1995) propose the 'forward orthogonal deviations' transformation, sometimes known as the 'Helmert' transformation. Unlike the first difference transformation which takes the difference between the current observation and the previous one, the forward orthogonal deviation takes the difference between the contemporaneous observations and the average of all available future observations. This addresses the weakness of the first difference transformation because regardless of the number of gaps in the data, the transformation can be calculated for all observations, except the last observation. This has the potential of considerably minimizing data loss in unbalanced panels.

More formally, the forward orthogonal transformations of x_{it} and ε_{it} are as follows:

$$\tilde{x}_{it}=\left(\sqrt{\frac{(T_i-t)}{(T_i-t+1)}}\right)_{it}\left(x_{it}-\bar{x}_{it}\right) \tag{2}$$

$$\tilde{\varepsilon}_{it}=\left(\sqrt{\frac{(T_i-t)}{(T_i-t+1)}}\right)_{it}\left(\varepsilon_{it}-\bar{\varepsilon}_{it}\right) \tag{3}$$

where tildes indicate that variables have been transformed by orthogonal deviations, and $\bar{x}_{it}$ and $\bar{\varepsilon}_{it}$ refer to the forward means of x_{it} and ε_{it}, given by the following:

$$\bar{x}_{it}=\sum_{s=t+1}^{T_i}\frac{x_{is}}{(T_i-t)} \tag{4}$$

$$\bar{\varepsilon}_{it}=\sum_{s=t+1}^{T_i}\frac{\varepsilon_{is}}{(T_i-t)} \tag{5}$$

This transformation allows us to specify the reduced-form VAR model (1) in terms of the transformed variables, as:

$$\widetilde{x}_{it}=A(L)\widetilde{x}_{i,t-1}+\tilde{\varepsilon}_{it} \tag{6}$$

Notice that the formulas for $\widetilde{x}_{it}$ and $\widetilde{\varepsilon}_{it}$ do not contain lagged observations. Therefore, lagged values of x_{it} are now orthogonal to $\widetilde{x}_{it}$, so they are valid as instruments. As a result of this orthogonality, estimation of the panel VAR model (6) can be implemented using the GMM estimator suggested by Arellano and Bover (1995).

Identification

Let $\widetilde{u}_{it}$ be the vector of structural innovations, which are related to the reduced-form residuals according to $\widetilde{\varepsilon}_{it} = A_0^{-1}\widetilde{u}_{it}$. The reduced-form VAR model can then be written in structural form by premultiplying (6) by A_0:

$$A_0\widetilde{x}_{it} = B(L)\widetilde{x}_{i,t-1} + \widetilde{u}_{it} \tag{7}$$

where $B = A_0A$. Because of the simultaneity between income inequality and crime, it is not possible to talk about the effect of 'shock' to inequality without further restrictions. It is necessary to impose identifying assumptions on equation (7) to consistently estimate the structural shocks from the reduced-form residuals. We do so by orthogonalizing the reduced-form residuals by the Cholesky decomposition. Specifically, we identify the structural disturbances by assuming that current realizations to income inequality and the other variables affect crime, but that inequality and the other variables do not react to contemporaneous unexpected movements in crime. Therefore, crime depends on lagged values of crime, and on contemporaneous realizations to shocks to the other variables. Hence, we impose a recursive structure on A_0^{-1}, which enables us to decompose the reduced-form innovations according to:

$$\widetilde{\varepsilon}_{it} \equiv \begin{pmatrix} \widetilde{\varepsilon_{it}^{\Delta y}} \\ \widetilde{\varepsilon_{it}^{\Delta e}} \\ \widetilde{\varepsilon_{it}^{\Delta d}} \\ \widetilde{\varepsilon_{it}^{p}} \\ \widetilde{\varepsilon_{it}^{\Delta z}} \\ \widetilde{\varepsilon_{it}^{c}} \end{pmatrix} = \begin{bmatrix} a_{11} & 0 & 0 & 0 & 0 & 0 \\ a_{21} & a_{22} & 0 & 0 & 0 & 0 \\ a_{31} & a_{32} & a_{33} & 0 & 0 & 0 \\ a_{41} & a_{42} & a_{43} & a_{44} & 0 & 0 \\ a_{51} & a_{52} & a_{53} & a_{54} & a_{55} & 0 \\ a_{61} & a_{62} & a_{63} & a_{64} & a_{65} & a_{66} \end{bmatrix} \begin{pmatrix} \widetilde{u_{it}^{\Delta y}} \\ \widetilde{u_{it}^{\Delta e}} \\ \widetilde{u_{it}^{\Delta d}} \\ \widetilde{u_{it}^{p}} \\ \widetilde{u_{it}^{\Delta z}} \\ \widetilde{u_{it}^{c}} \end{pmatrix}, \quad \begin{array}{l} \leftarrow \textit{shock to per capita inc.} \\ \leftarrow \textit{shock to employment} \\ \leftarrow \textit{shock to pop. density} \\ \leftarrow \text{shock to } \textit{police spending} \\ \leftarrow \textit{shock to inequality} \\ \leftarrow \textit{shock to crime} \end{array} \tag{8}$$

The ordering of the other variables in the VAR reflects the practice in the literature of ordering the more exogenous variables ahead of the less exogenous ones. Hence, the identifying assumptions underlying the ordering of the other variables in equation (8) may be motivated as follows: (*i*) Changes in economic activity, measured by per capita income, affect the other variables within the year, however, changes in per capita income do not react instantaneously to shocks to the other variables, given the sluggishness of aggregate economic activity; (*ii*) Given the substantial literature on the impact of labour markets on inequality and crime, current changes in employment are likely to impact police expenditures, density, crime, and inequality contemporaneously, but realizations in these variables feed through to employment with a lag, since employment is generally a lagging indicator of the state of the economy; (*ii*) Changes in population density affect police expenditures, income inequality, and crime on impact, but population density will not respond immediately to shocks to these variables, given substantial costs of relocation. We assume that families in a state will wait to see whether the shocks to the other variables are permanent or transitory, before incurring the sometimes exorbitant costs of moving; (*iv*) The model imposes the restriction that an increase in resources available to law enforcement (police) to apprehend criminals and deter potential criminals will decrease crime immediately, but that crime will not impact current levels of income inequality and police spending, as budgetary appropriations for police departments are determined at the start of the fiscal year, so that police departments are unlikely to respond to crime changes contemporaneously, but with a lag of at least a year; (*v*) it is reasonable to assume that movements in inequality affect crime immediately in light of the various theoretical mechanisms discussed above (*vi*) innovations to crime that are not explained by shocks to the other variables are crime-specific shocks.

While it is possible to include more variables in the VAR, we limit the number of variables to six for several reasons. First, extensive literature has shown these variables to be among the most consistent determinants of crime (see e.g. Fajnzylber *et al.*, 2002a). Second, large VAR models generally suffer from technical and computational complications, as the number of VAR parameters increases as the square of the number of variables (Stock and Watson, 2001). Third, and perhaps more importantly, estimating a large VAR model implies that more - and often controversial – identification restrictions will be imposed. In fact, while we believe that our identification restrictions discussed above are reasonable, some might find them questionable. For this reason, we present evidence in a robustness section that the results are generally not sensitive to the ordering of the variables in the VAR. In estimating the VAR models, lag lengths are selected using the the Akaike Information Criterion (AIC).

IV. Empirical results

After reporting impulse response functions, we assess the quantitative importance of shocks to inequality and other variables for the volatilities of the various crime measures through decomposition of variance analyses. The impulse response functions are computed for forecast horizons of up to 20 years. The solid lines in the figures below are the impulse response coefficient estimates, and broken lines represent the 95% confidence bands computed by Monte Carlo simulation methods with 1,000 replications.

Results for the benchmark panel VAR model

Impulse responses and variance decompositions of aggregate violent and property crimes to shocks to income inequality

Figures 5 and 6, respectively, illustrate the impulse response functions of aggregate violent and property crimes to a shock to each of the six variables in the VAR system. Figures 5a and 6a display the results of most interest, namely the impulse responses of violent and property crimes to an innovation of one percentage point to state income inequality. The responses are qualitatively quite similar. In both cases, a shock to inequality leads to hump-shaped increases in aggregate violent and property crime. The contemporaneous rise in crime is not significantly different from zero. Subsequent responses, however, are

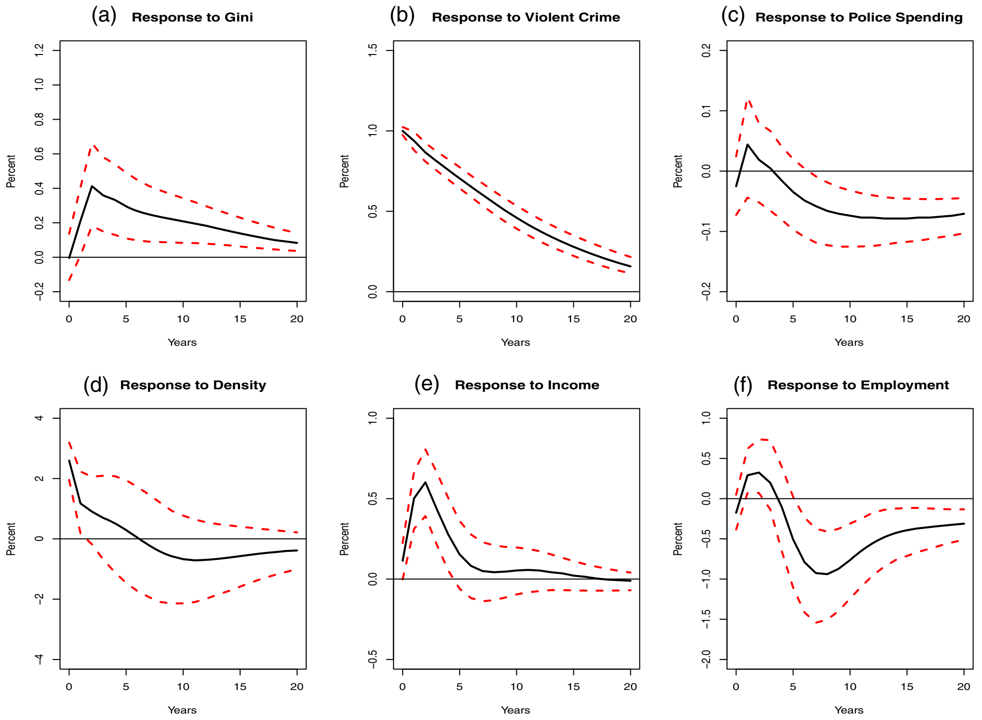

Figure 5. Responses of aggregate violent crime to shocks to Gini and other variables
Notes: Solid lines represent the impulse response coefficient estimates, while the dashed lines depict the 95% confidence bands computed by Monte Carlo simulation methods with 1,000 replications

significant throughout, with peak responses occurring two years after the shock, at which horizon aggregate violent crime rises by 0.4% and aggregate property crime by 0.3%. This finding is particularly novel, and potentially reconciles some of the conflicting findings in the empirical literature. Tracing out the crime responses using impulse response functions captures several important dynamics. First, it is possible that previous studies that find insignificant impacts of inequality on crime may be capturing the insignificant contemporaneous responses shown in Figures 5 and 6a. Second, the literature is saturated with a host of different estimates of the impact of income inequality on crime. Our results here indicate that the impact of inequality on crime can vary significantly. In Figure 5a, the response of aggregate violent crime is 0.01% on impact, peaking at 0.4%, and then dropping again to about 0.2% at horizon 20. Similarly, aggregate property crime rises by 0.01% contemporaneously, and then reaches a maximum of 0.3%, and after which it falls to 0.17% by the 20-year horizon. Clearly, considering dynamics is important as it potentially puts into perspective the range of varying estimates in the empirical literature.

The finding that shocks to inequality have insignificant contemporaneous effects on violent and property crimes, but propagate positive responses over short and long horizons is consistent with the theories discussed in section Mechanisms through which income inequality can impact crime. Consider, for example, the theory of relative deprivation, which argues that inequality triggers feelings of envy, inadequacy, frustration, mistrust, aggression, and hostility by the poor against the rich, causing the poor to resort to burglary,

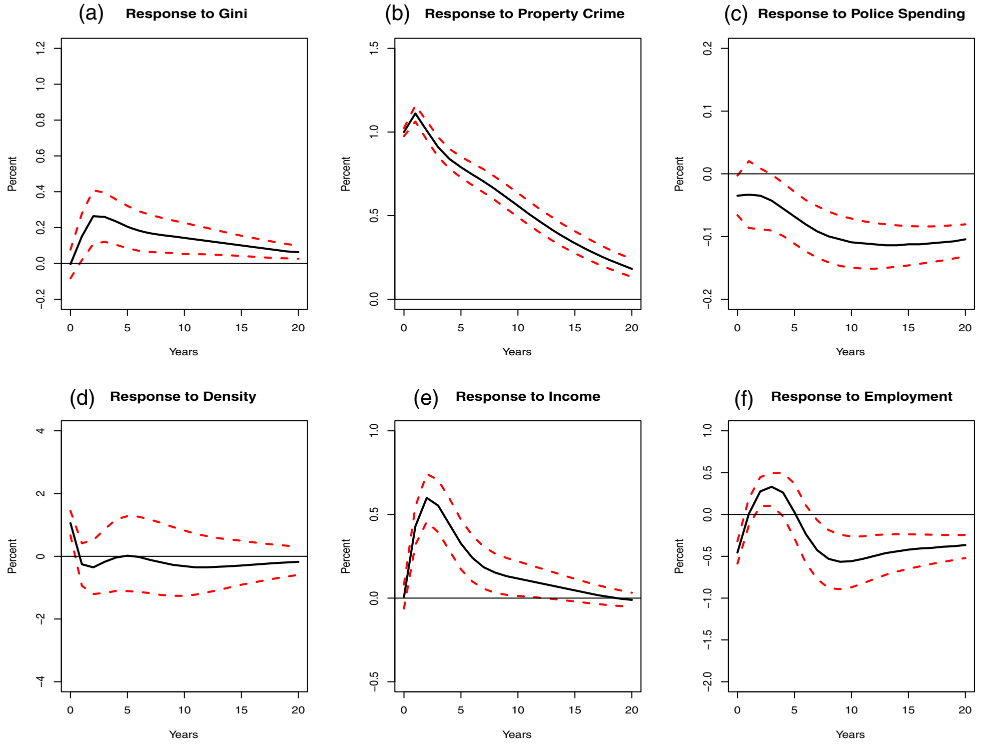

Figure 6. Responses of aggregate property crime to shocks to Gini and other variables
Notes: Solid lines represent the impulse response coefficient estimates, while the dashed lines depict the 95% confidence bands computed by Monte Carlo simulation methods with 1,000 replications

larceny, and other forms of crime to 'redistribute economic resources' and 'satisfy their sense of injustice' (Roberts and Willits, 2015). Clearly, in this theory, the response of crime to changes in inequality is not expected to be immediate as it takes time for the pent-up aggression and hostility generated by these feelings to result in crime (hence, the insignificant contemporaneous responses in Figures 5a and 6a). Nonetheless, one would expect that over time, as these feelings grow, so too will the propensity to commit crimes by individuals at the low end of the income distribution (consistent with the positive and significant responses in Figures 5a and 6a). Depending on the severity of inequality, these feelings can propagate crimes over long horizons, as depicted in the figures. Similar arguments can be made with respect to Becker's (1968) economic theory of crime, and Shaw and McKay (1942) social disorganization theory. While methodologically different, evidence of a short- and/or long-run effect of inequality on crime has been documented by, among others, Saridakis (2004), Chintrakarn and Herzer (2012), and Buonanno and Vargas (2019).

Tables 3a and b present variance decompositions to help assess the overall contribution of the shocks to the variability of crime rates. To this end, the percentage of the k-step ahead forecast error variance of crime that is attributable to various shocks is estimated. For now, we focus on the first columns of Tables 3a and b, which, respectively, show the percentages of variance of aggregate violent crime and aggregate property crime that are accounted for by innovations to income inequality as the forecast horizon, k, increases. The estimated

TABLE 3

Percent contribution of shocks to income inequality and other variables to overall variability of violent and property crime rates

Horizon	*Income inequality*	*Police spending*	*State-level employment*	*Per capita income*	*Population density*	*Violent crime*
A. Percent contribution of shocks to income inequality and other variables to overall variability of violent crime rates						
5	0.74	0.04	0.47	1.99	1.59	96.02
10	0.85	0.15	1.71	1.39	1.13	95.36
15	0.91	0.33	2.13	1.24	0.95	94.82
20	0.92	0.50	2.27	1.19	0.89	94.51
25	0.93	0.64	2.36	1.17	0.87	94.28
30	0.94	0.73	2.41	1.17	0.88	94.13
35	0.94	0.78	2.43	1.17	0.89	94.05
40	0.94	0.81	2.44	1.18	0.89	94.01
B. Percent contribution of shocks to income inequality and other variables to overall variability of property crime rates						
5	0.68	0.19	0.98	4.60	0.20	93.37
10	0.76	0.71	1.29	3.72	0.14	93.38
15	0.80	1.39	1.93	3.30	0.17	92.42
20	0.82	2.04	2.32	3.14	0.18	91.50
25	0.82	2.57	2.63	3.09	0.19	90.71
30	0.82	2.94	2.83	3.09	0.19	90.13
35	0.82	3.17	2.95	3.11	0.19	89.77
40	0.82	3.29	3.01	3.13	0.19	89.56

Notes: The percentage of k-step-ahead forecast error variance of aggregate violent (panel A) and property (panel B) crimes due to police spending shocks are based on structural panel vector autoregressive model described in text.

percentages range from 0.74% to 0.94% in the case of aggregate violent crime, and from 0.68% to 0.82% for aggregate property crime. Evidently, shocks to income inequality explain very little of the unpredictable movements in aggregate violent and property crime rates. In fact, the table shows that much of the variability in violent and property crimes are explained by their own shocks (the last columns of Tables 3a and b).

Impulse responses and variance decompositions of aggregate violent and property crimes to shocks to other variables

One way to check the reasonableness of our identification scheme is to verify that the crime responses to shocks to each of the other variables are consistent with theory and/or previous empirical findings. Figures 5b and 6b, respectively, show that aggregate violent and property crimes rise in response to their own shock, as expected. An important result is shown in panel C of both figures, which display the responses of violent and property crimes to an innovation of 1% to state and local police expenditures. Aggregate violent and property crime rates respond with a delay, decreasing by as much as 0.1% in the long run. While we are unaware of many, if any, studies that employ a VAR methodology to examine the impact of police expenditures on crime, our finding here, that shocks to police expenditures lead to significant and persistent declines in aggregate violent and property crime are in line with much of the empirical and theoretical literature on the link between crime and deterrence (see Levitt, 1997, 2002; McCrary, 2002; Evans and Owens, 2007; Draca, Machin and Witt, 2011; and Chalfin and McCrary, 2018).

The dynamics of aggregate violent and property crimes to an increase in population density, shown in panel D of Figures 5 and 6 exhibit large and statistically significant contemporaneous responses, with violent crime rising by 3% and property crime by 1%. As pointed out by Glaeser and Sacerdote (1999), a positive impact of urbanization on crime is expected because highly dense areas provide greater pecuniary returns to crime as criminals have greater access to the wealthy and face a greater density of victims. As well, committing crimes in urban areas is less likely to lead to an arrest due to the larger number of suspects. Furthermore, they argue that densely populated areas tend to attract crime-prone individuals, making crime an urban phenomenon. The positive responses of crime to urbanization support these hypotheses, and are in line with the literature on the link between urbanization and crime (see e.g. Kelly, 2000; Fajnzylber *et al.*, 2002a,b; Soares, 2004).

Aggregate violent and property crimes display marked hump-shaped responses to shocks to a shock to per capita income (Figures 5e and 6e). The impact responses, while positive, are not significantly different from zero. For both aggregate crime measures, their responses turn significantly positive after one year and peak two years after the initial shock to per capita income, at which horizon both aggregate crime measures rise by 0.6%. The effect of the shock to income becomes statistically indistinguishable from zero in year five for aggregate violent crime, and year twelve for aggregate property crime. While these responses may appear puzzling, recall that this variable was included to control for the pro-cyclical consumption of criminogenic commodities (Raphael and Winter-Ember, 2001). The positive crime responses in Figures 5e and 6e, therefore, support the view that higher income is associated with higher consumption of criminogenic commodities, which based on past research has been shown to increase crime. In addition, if the increase in income is correlated with an increase in inequality, then our findings here, that shocks to per capita income raise both violent and property crimes, have some credence.

In Figures 5f and 6f are the responses of aggregate violent and property crimes to shocks to employment. In both cases, the contemporaneous effect of the shock is a decrease in crime. The responses, however, turn positive and significant between years two and four, after which crime decreases significantly and persistently. These dynamics are exactly what one would expect. Recall that while this variable is included to control for legal economic opportunities, it also captures some of the income effects discussed above. Thus, the temporary increase in crime following a shock to employment depicted in Figures 5f and 6f can be explained by the fact that an increase in employment increases wages and incomes, leading to an increase in the consumption of criminogenic commodities, and hence overall crime. On the other hand, the crime-reducing impacts of employment in the figures suggests that higher employment levels represent legal economic opportunities in the form of decent wages, especially if these opportunities disproportionately benefit low-skilled individuals or those at the bottom of the income distribution. As well, the negative responses of aggregate violent and property crimes to a shock to employment can be attributed to the 'self-incapacitation' effect of employment. That is, the time that individuals spend working limits the time available to them to participate in criminal activity.

We conclude this subsection by discussing the relative importance of the various shocks to the overall variability of aggregate violent and property crime rates. Tables 3a and b show that shocks to employment, per capita income, population density, and police spending have negligible explanatory power for movements in crime. This is especially true for violent crime, where at no forecast horizon does the combined explanatory power of the four shocks account for up to 6% of the overall variability of aggregate violent crime. While the four shocks together explain almost 10% of the variability of aggregate property crime at most forecast horizons, the general conclusion from the table, however, is that movements in crime are mostly accounted for by their own shocks.

Discussion of the identification assumptions

The results thus far have been based on a six-variable panel VAR model in which identification was achieved by the Cholesky decomposition of the variance–covariance matrix of the residuals of the reduced-form panel VAR models. It is widely known that the results of VAR analyses can be sensitive to the identification restrictions underlying the Cholesky decomposition. This section discusses the identification strategy underlying this paper to ensure that the results are not driven by an unreasonable identification.

Ordering of the variables in the VAR model

The main identification restriction underlying the impulse response functions in Figures 5 and 6 is that whereas structural shocks to inequality and the other variables affect crime on impact, shocks to crime affect the other variables, including inequality, with a delay of at least one year. The remaining identification restrictions underlying the ordering of the variables in $x_{it} = [\Delta y_{it}\ \Delta e_{it}\ \Delta d_{it}\, p_{it}\ \Delta z_{it}\ c_{it}]'$ were discussed in section Identification. We acknowledge that the restrictions underlying the ordering of the variables in the VAR model may be subject to some disagreement. Thus, it is incumbent upon us to present evidence that the empirical results of this paper are robust to the ordering of the variables.

One way to check to validity of our identification assumptions is to examine the correlation between the identified shocks from the VAR and other shocks. As pointed out by Ramey (2016), if the identification assumptions are valid, then our identified shocks ‘must be uncorrelated with other exogenous shocks; otherwise, we cannot identify the unique causal effects of one exogenous shock relative to another’ (Ramey, 2016, page 3). Table 4 presents the contemporaneous correlation between the estimated shocks to crime ($\widetilde{u_{it}^{c}}$) and several variables that have been theoretically shown to affect both inequality and crime. The table also displays similar bivariate correlations between the estimated shocks to income inequality ($\widetilde{u_{it}^{\Delta z}}$) and the same variables. In column (1) is the correlation between the variables and the shocks to violent crime recovered from the six-variable violent crime VAR model, and in column (2) are the correlations between the variables and the inequality shocks from the violent crime VAR model. Columns (3) and (4) are the corresponding correlations between the variables and the same shocks recovered from the property crime VAR model. Consistent with the recursiveness assumption underlying the Cholesky decomposition, the correlations are all zero or very close to zero, suggesting that the impulse response functions are generally robust to the ordering of the variables in our VAR model.

TABLE 4

Contemporaneous correlations of crime and inequality shocks with other variables

Variable	*(1)* *Violent crime VAR model* $\widetilde{u^{c}_{it}}$	*(2)* *Violent crime VAR model* $\widetilde{u^{c}_{it}}$	*(3)* *Property crime VAR model* $\widetilde{u^{\Delta z}_{it}}$	*(4)* *Property crime VAR model* $\widetilde{u^{\Delta z}_{it}}$
Productivity growth	−0.01	0.02	0.01	0.02
Income tax rate	−0.02	−0.02	0.01	−0.01
Labor force participation	0.02	0.01	0.02	−0.01
Per capita wage	0.02	0.01	0.00	0.00
Minority population share	0.01	−0.03	−0.03	0.00
White population share	−0.01	0.03	0.03	0.00
Poverty rate	0.02	0.01	0.03	0.03

Notes: The variables $\widetilde{u^{\Delta z}_{it}}$ and $\widetilde{u^{c}_{it}}$ respectively denote the identified shocks to crime and income inequality from the six-variable panel structural vector autoregressive (SVAR) models. Other variables are defined in Table 1.

As an alternative way to verify that our general findings are not sensitive to the ordering of the variables in the VAR models, Figure 7 plots the impulse responses of aggregate violent crime (panel A) and aggregate property crime (panel B) to shocks to the variables from a VAR model with reverse ordering. Of course, with a six variable model, it is not practical to show the impulse response functions from all the possible ordering schemes, hence, we only present the results for the ordering $x_{it} = [c_{it}\ \Delta z_{it}\ p_{it}\ \Delta d_{it}\ \Delta e_{it}\ \Delta y_{it}]'$. By construction, the impact responses of violent and property crime to shocks to income inequality are zero, while subsequent responses are qualitatively and quantitatively similar to those displayed in Figures 5a and 6a. That is, the responses of violent and property crimes are positive, significant, and persistent. The violent and property crime responses to shocks to the other variables are generally similar to those in panels B to F of Figures 5 and 6. Variance decompositions from this specification (not shown, but available to the reader upon request) are also similar to those in Table 3. These results, together with those in Table 4 provide some evidence that our findings regarding the impact of income inequality on crime are generally robust to changes in the identification restrictions underlying our VAR.

Omitted variable bias

Another potential concern is that the impulse responses and variance decompositions are based on a six-variable VAR model. While we rely on the empirical literature for guidance as to what variables to include (especially Fajnzylber *et al.*, 2002a), it could be argued that other potentially important variables have been omitted. For example, much research exists on the role of education (see e.g. Bell *et al.*, 2016; Lochner, 2004; Lochner and Moretti, 2004; Machin *et al.*, 2011, 2012), gender, age, family background, neighbourhood effects, and the consumption of criminogenic substances, such as drugs, alcohol, and guns, as determinants of crime (see e.g. Levitt and Lochner, 2001; and Eriksson *et al.*, 2016). It is not possible to control for all these factors due to data limitations, and technical and computational complications as the number of VAR parameters increases as the square of the number of variables (Stock and Watson, 2001). Furthermore, estimating large VAR

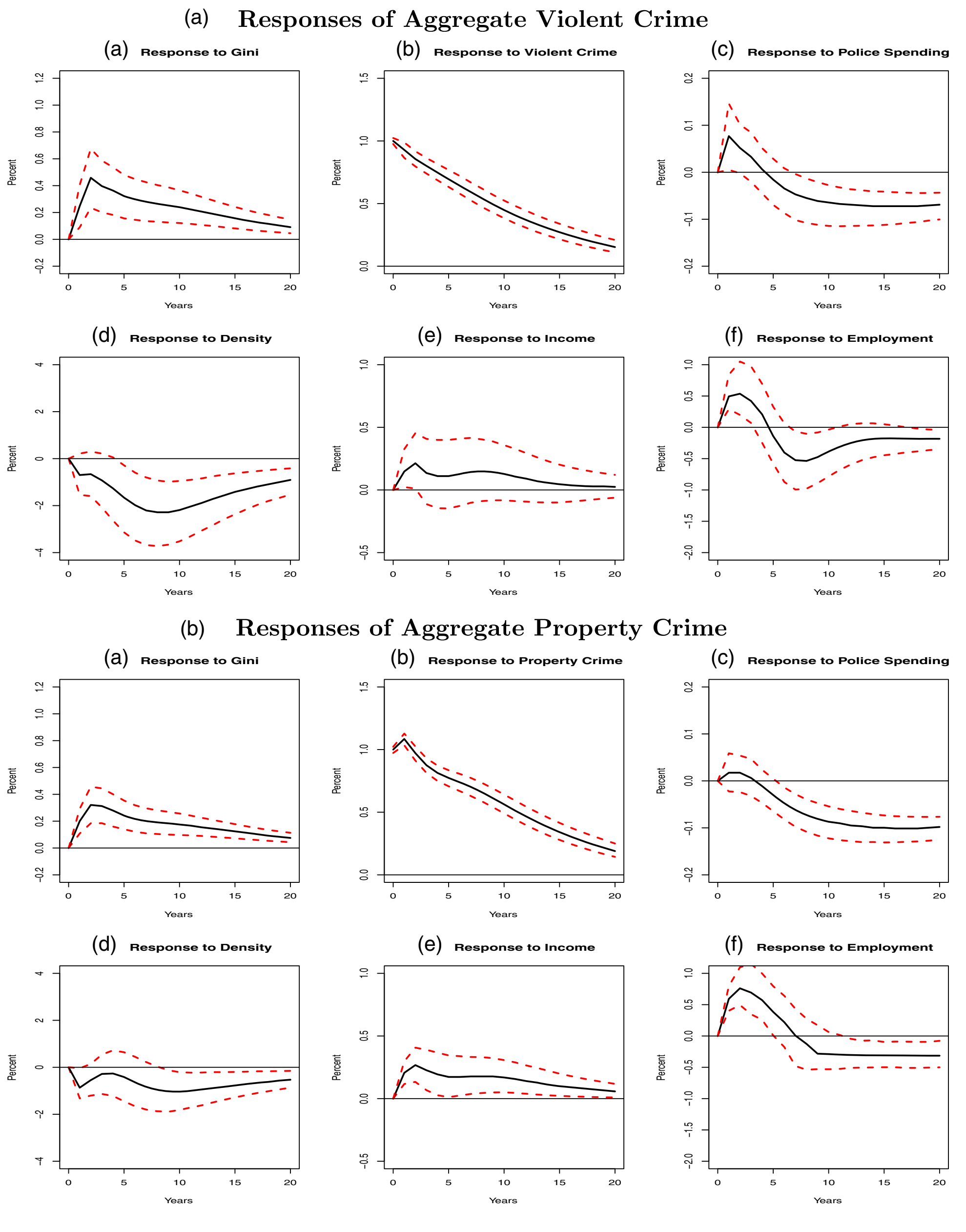

Figure 7. Responses of aggregate violent crime to shocks to Gini and other variables: reverse order
Notes: Solid lines represent the impulse response coefficient estimates, while the dashed lines depict the 95% confidence bands computed by Monte Carlo simulation methods with 1,000 replications

models entails imposing a lot more identification restrictions, which are often difficult to justify. Yet it is important to provide some evidence that the shocks recovered from our VAR models are not contaminated by shocks to other variables due to omitted variable bias.

TABLE 5

Regressions of identified shocks to crime and inequality on other variables

Variable	$\widetilde{u_{it}^{c}}$: violent crime VAR R^2	F-stat	P-val	$\widetilde{u_{it}^{\Delta z}}$: violent crime VAR R^2	F-stat	P-val
A. Regressions of crime and inequality shocks from violent crime VAR						
Productivity growth	0.00	0.57	(0.56)	0.00	1.00	(0.37)
Income tax rate	0.00	0.40	(0.69)	0.00	0.38	(0.69)
Labor force participation rate	0.00	0.25	(0.78)	0.00	0.80	(0.45)
Per capita wage	0.00	2.65	(0.07)	0.00	0.17	(0.85)
Minority population share	0.00	2.13	(0.12)	0.00	1.68	(0.19)
White population share	0.00	0.88	(0.41)	0.00	1.49	(0.23)
Poverty rate	0.00	0.77	(0.46)	0.00	0.50	(0.61)

Variable	$\widetilde{u_{it}^{c}}$:property crime VAR R^2	F-stat	P-val	$\widetilde{u_{it}^{\Delta z}}$:property crime VAR R^2	F-stat	P-val
B. Regressions of crime and inequality shocks from property crime VAR						
Productivity growth	0.00	0.24	(0.79)	0.00	1.08	(0.34)
Income tax rate	0.00	1.84	(0.16)	0.00	0.22	(0.80)
Labor force participation rate	0.00	1.23	(0.29)	0.00	0.94	(0.39)
Per capita wage	0.00	2.40	(0.13)	0.00	0.29	(0.75)
Minority population share	0.00	1.58	(0.21)	0.00	0.33	(0.72)
White population share	0.00	1.48	(0.23)	0.00	0.19	(0.83)
Poverty rate	0.00	1.28	(0.28)	0.00	0.50	(0.61)

Notes: Models estimated by pooled OLS. VAR, vector autoregressive.

Table 5 presents estimates of regressions of the identified shocks to crime and income inequality on contemporaneous and lagged values of the other variables not included in the VAR models:

$$\widetilde{u_{it}^{c}} = \alpha + \sum_{j=0}^{1} \beta_j y_{i,t-j} + \eta_{it} \tag{9}$$

$$\widetilde{u_{it}^{\Delta z}} = \alpha + \sum_{j=0}^{1} \beta_j y_{i,t-j} + \eta_{it} \tag{10}$$

where $\widetilde{u_{it}^{c}}$ and $\widetilde{u_{it}^{\Delta z}}$, respectively, denote the identified shocks to crime and income inequality, y denotes one of the other variables, and η_{it} denotes the error term. We estimate equations (9) and (10) by pooled OLS methods, and present the R^2 from each regression, together with the F-statistic of overall significance of the models and their corresponding P-values.

The idea is that if the identified shocks to violent crime, property crime, and income inequality are contaminated by shocks to other variables, then contemporaneous and lagged values of those variables should have some explanatory power for the shocks to violent crime, property crime, and income inequality. In Table 5, the R^2 from all the regressions are zero, suggesting that movements in the other variables have no explanatory power for the shocks to violent and property crimes, and income inequality. In addition, F-tests fail to reject the null hypothesis that the coefficients on contemporaneous and lagged values of the other variables are all zero, with P-values all greater than 0.05.

Taken together, the findings in section Ordering of the variables in the VAR model and this subsection provide some validity of the identification strategy of this paper, and hence, the empirical results discussed herein.

Alternative measures of inequality

The results so far have been based on the Gini coefficient as the measure of inequality. Despite its appeal, the Gini index, like any measure of inequality, has shortcomings (see Deininger and Squire, 1996 for discussions of the advantages and shortcomings of various measures of income inequality). We now ask whether our findings can be generalized to other measures of income inequality. Three alternative measures of income inequality, namely the Theil Index, the Top Decile Income Share, and the Top 5% Income Share are now considered.[7] As expected, these measures are highly correlated, with their correlations with the Gini coefficient as high as 0.88 for the Theil index, 0.81 for the top decile income share, and 0.80 for the top 5% income share.

Figure 8, panel A displays the impulse response functions of violent crime to a shock to each of the three inequality measures. Panel B shows the corresponding responses of property crime. The results are qualitatively analogous to those reported in Figures 5 and 6. That is, for each inequality measure, aggregate violent and property crime rates rise with a delay of at least one year, and then remain significant throughout. The exceptions to this pattern are the responses of violent crime to a shock to the top decile share and top 5% income share measures, where crime displays a significant contemporaneous rise, which remains significant at all forecast horizons. Quantitatively, there is significant heterogeneity in the crime responses to different inequality measures. In all cases, the magnitude of the crime responses to the alternative measures shown in Figure 8 are smaller than the crime responses to the Gini index in Figures 5 and 6, where the maximum violent and property crime responses to a 1% point shock to the Gini index are 0.4 and 0.3, respectively. Looking at Figure 8a and Figure 8b, in no instance is the rise in either crime greater than 0.25%. Some level of heterogeneity in the crime responses to alternative measures of inequality is expected, as these measures are all calculated differently, and capture different forms of inequality (gross versus net income inequality), yet the general finding that crime rises (albeit to different magnitudes) following shocks to different measures of inequality lends credence to the central themes of this paper. While their empirical approaches differ from ours, Fajnzylber *et al.* (2002a,b), and Scorzafave and Soares (2009), also document robust positive relationships between crime and various measures of income inequality.

Table 6 summarizes the explanatory power of innovations to the other measures of income inequality for fluctuations in the violent and property crimes. Consistent with the variance decompositions in Table 3, shocks to the three measures of inequality have little to no explanatory power for fluctuations in aggregate violent and property crime rates.

[7] We refer the reader to Frank (2009) for details of the construction of these measures.

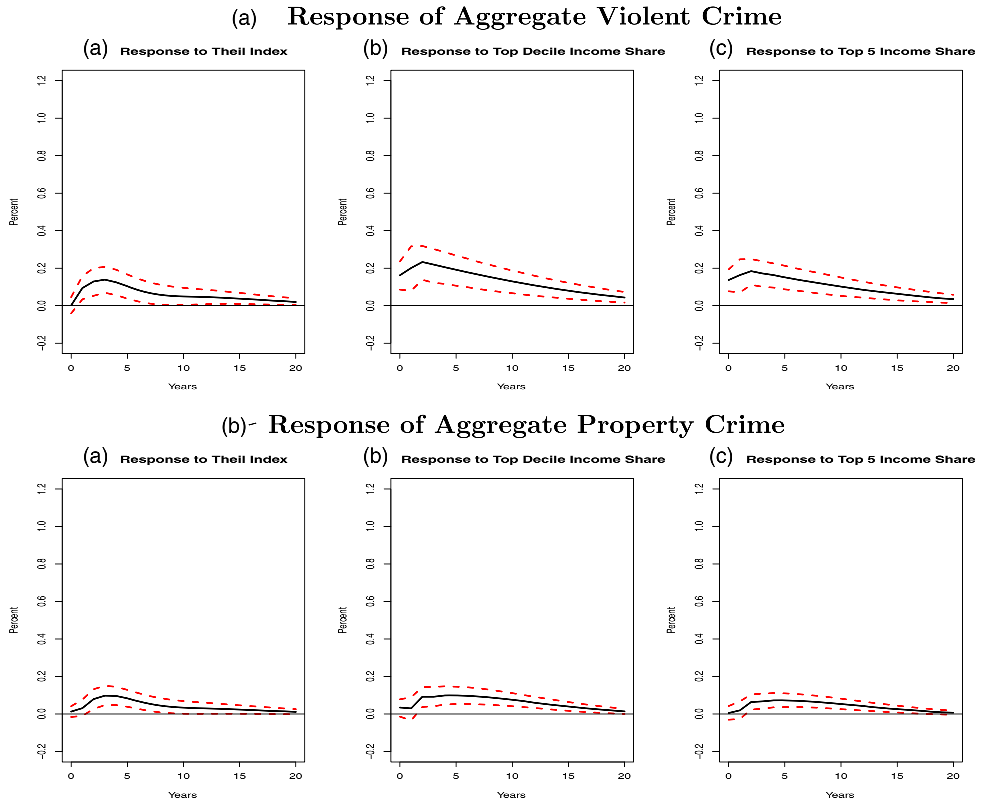

Figure 8. Response of aggregate violent and property crime to other measures of inequality
Notes: Solid lines represent the impulse response coefficient estimates, while the dashed lines depict the 95% confidence bands computed by Monte Carlo simulation methods with 1,000 replications

Effect of income inequality on disaggregate measures of violent and property crime rates

The effect of inequality on crime is not necessarily uniform across different categories of violent and property crimes. In fact, it is possible, and perhaps reasonable to expect that different types of crime behave differently in response to an increase in inequality. To uncover this possible heterogeneity in the effect of inequality across separate crime types, we disaggregate total violent crime into four distinct measures, namely aggravated assault, murder and non-negligent manslaughter, rape, and robbery. Aggregate property crime is disaggregated into auto theft, burglary, and larceny. We estimate separate VAR models for each of the separate crime measures, replacing the aggregate crime measures with the disaggregate measures. In estimating the VAR models, we use the Gini index as the measure of inequality.

The estimated impacts of inequality on the separate measures of violent and property crimes are reported in Figure 9. The top half displays the responses of disaggregate violent crimes and the bottom half reports the effects on the more detailed types of property crimes. The figure shows that the effects of inequality exhibit considerable heterogeneity across the different categories. Looking first at the impulse response functions of the separate violent crime types (panel A), it is evident that over the entire forecast horizon, murder exhibits the

TABLE 6

Percent contribution of alternative measures of income inequality to variability of crime rates

	A. Aggregate violent crime			*B. Aggregate property crime*		
Horizon	*Theil index*	*Top decile income share*	*Top 5 percent income share*	*Theil index*	*Top decile income share*	*Top 5 percent income share*
5	0.87	1.01	1.10	0.72	0.34	0.28
10	0.86	1.11	1.19	0.80	0.54	0.47
15	0.84	1.12	1.20	0.76	0.59	0.51
20	0.85	1.12	1.20	0.75	0.59	0.51
25	0.85	1.12	1.20	0.74	0.59	0.50
30	0.84	1.11	1.19	0.73	0.58	0.50
35	0.84	1.11	1.19	0.73	0.58	0.50
40	0.84	1.11	1.19	0.72	0.59	0.51

Notes: The percentage of k-step-ahead forecast error variance of aggregate violent (panel A) and property (panel B) crimes due to police spending and other shocks are based on an eight-variable structural panel vector autoregressive model described in text.

largest and most significant response following a positive innovation to inequality. The peak response occurs two years after the shock to inequality, with murder rising by 0.76% in response to a 1% point increase in inequality. The contemporaneous responses of assault and robbery, like the aggregate violent crime response, are insignificant. Subsequent responses, however, are significant. The estimated peak response of assault and robbery are 0.4% and 0.39%, respectively, both occurring two years after the inequality shock. Surprisingly, at no horizon are the estimated effects on rape different from zero in a statistical sense. The bottom half of Figure 9 reveals that a one point increase in the Gini index raises auto theft on impact by about 0.22%, with a maximum response of 0.78% in year two. Larceny rises with a delay of one year, peaks two years following the shock, then gradually declines but never loses statistical significance. On the contrary, the response of burglary is small, and statistically distinguishable from zero only at longer forecast horizons.

The decomposition of variance analyses in Table 7 reveal, as has been the case throughout the paper that shocks to inequality account for negligible proportions of the variability of crime rates. Overall, the results of this section suggest that inequality is positively correlated with many different crime categories, even though its quantitative importance for explaining fluctuations in these crime categories is minimal at best.

V. Conclusion

A number of theoretical reasons suggest that income inequality increases crime. A fundamental problem with this prediction is that empirical support for this link remains weak. Income inequality, however, is affected by some of the same factors that drive individuals' decisions to engage in criminal activity, making it necessary to control for reverse causality. This means that cause and effect are not well defined in regressions of crime on income inequality, to the extent that these studies failed to address this simultaneity bias. Recent empirical studies have resorted to IV estimation to address this endogeneity bias.

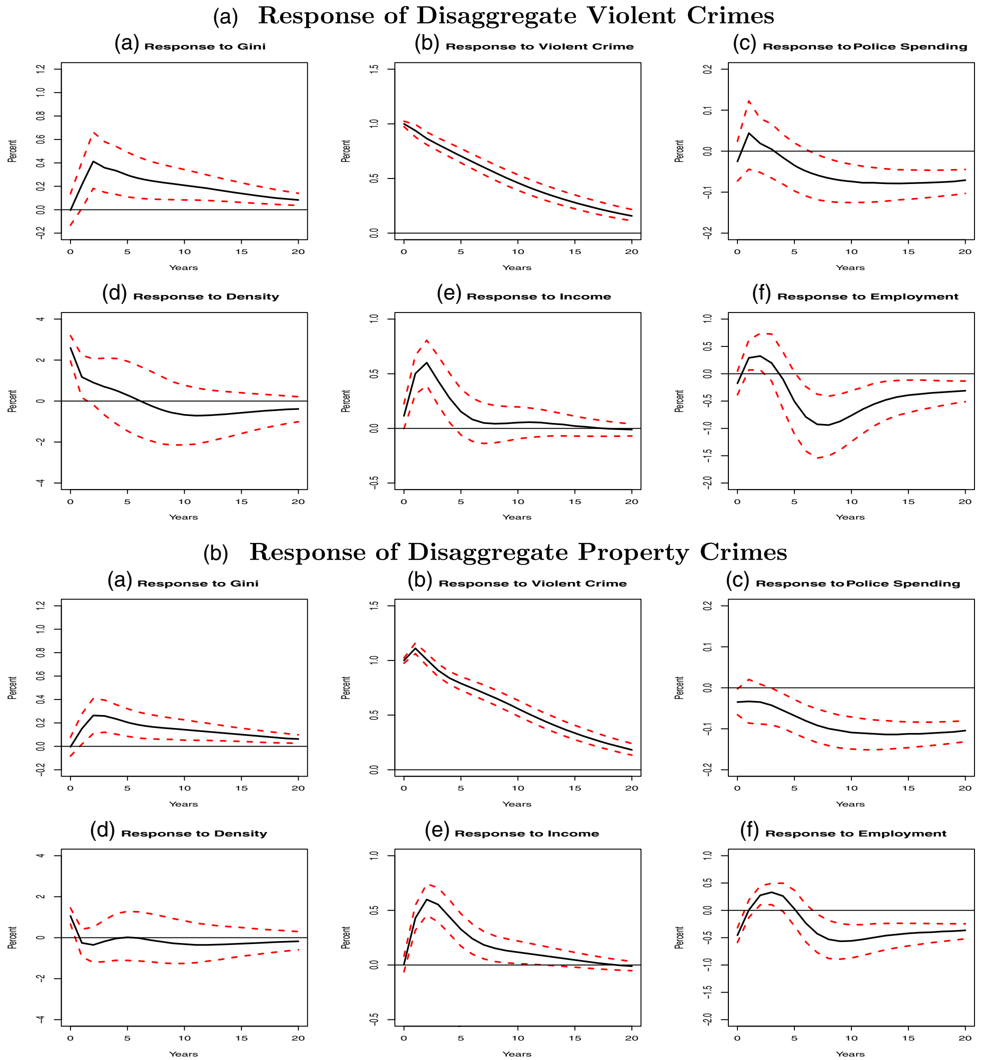

Figure 9. Responses of disaggregate violent and property crimes to Gini shock
Notes: Solid lines represent the impulse response coefficient estimates, while the dashed lines depict the 95% confidence bands computed by Monte Carlo simulation methods with 1,000 replications

This paper proposes an alternative approach for estimating the causal effect of income inequality on crime. We identify linearly unpredictable movements in inequality that are exogenous with respect to crime using panel structural VAR methods, and then estimate their dynamic impacts on crime using impulse response functions. Our identification strategy relies on the assumption that whereas income inequality affects crime rates instantaneously, structural shocks to crime impact inequality with a lag of at least one year. This assumption corresponds to a standard Cholesky decomposition where inequality is ordered above crime in the VAR model. The paper's empirical framework also allows us to quantify the importance of shocks to income inequality for unpredictable movements in crime through variance decompositions. Using annual data from the fifty U.S. states and D.C.

TABLE 7

Percent contribution of police spending shocks to variability of disaggregate violent and property crime rates

	Disaggregate violent crimes				*Disaggregate property crimes*		
Horizon	*Assault*	*Murder*	*Rape*	*Robbery*	*Auto Theft*	*Burglary*	*Larceny*
5	0.36	0.79	0.05	0.42	1.94	0.16	0.59
10	0.42	0.86	0.04	0.44	2.06	0.17	0.71
15	0.45	0.87	0.04	0.46	2.14	0.18	0.76
20	0.46	0.87	0.05	0.47	2.16	0.19	0.78
25	0.47	0.87	0.05	0.47	2.17	0.19	0.79
30	0.47	0.87	0.05	0.47	2.16	0.19	0.80
35	0.47	0.87	0.05	0.47	2.16	0.19	0.80
40	0.47	0.87	0.05	0.47	2.16	0.19	0.80

Notes: The percentage of k-step-ahead forecast error variance of disaggregate violent and property crime rates due to police spending shocks are based on structural panel vector autoregressive model described in text.

over the period 1960–2015, our analyses consistently show that shocks to inequality raise violent and property crimes significantly and persistently. Variance decomposition analyses, however, show that the quantitative importance of structural shocks to inequality for the variability of crime are minimal at best. The paper's main findings are largely robust to alternative identification restrictions and different measures of violent crime and property crime.

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Supporting Information

Additional Supporting Information may be found in the online version of this article:

Appendix S1. U.S. state-level violent and property crime rates.