Economic Crises, Civilian Mobilization, and Repression in Developing States

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Abstract

Research on the causes of repression had limited success in connecting economic crises to state-led violence. We develop an explanation for violent government repression in urban areas, which links the importance of urban infrastructure in enabling civilians to wage an effective opposition campaign with the stress caused by economic crisis, empirically validating the underlying mechanisms using disaggregated geospatial data. We then confirm the empirical expectation that governments will violently repress during times of economic crisis where the civilians' capacity to wage a collective action campaign is high using a disaggregated global sample of urban areas within developing states.

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Governments frequently use violence to repress civilian dissent (Ritter and Conrad, 2016; Davenport, 2007; Gurr and Lichbach, 1986; Poe and Tate, 1994). Although scholars posited that economic crises can engender violent repression (Davenport, 2007), the linkages between economic decline and state-led violence remain unsupported. Focusing on urbanization and development, this study identifies a causal arrow flowing from economic crises to violent repression, discusses its various aspects in great detail, and validates it empirically using disaggregated global data.

Because repression is often employed against political opposition, studies typically find that "an increase in dissent yields an increase in repression unconditionally" (cited in DeMeritt, 2016); if the opposition mobilizes, the government often chooses to repress. Scholars identified several key causes of repression including political conflict, democratization, economic uncertainty, and international treaties (Poe and Tate, 1994; DeMeritt, 2016; Gurr and Lichbach, 1986). Yet, a key deficiency in extant research lies in its limited ability to explore "the disaggregation of repressive behavior across time, space, and types of activity," especially because "[m]ost researchers employ data aggregated to the nation-year in an effort to understand causal relationships (Davenport, 2007, 18). The same limitations are echoed by Pierskalla (2010, 135-136), who notes that "[w]e still do not have a clear understanding of when governments can successfully deter protest, when repression of protest can be effective, and under which conditions escalating violence breaks out," a difficulty that, "stems in part from a lack of well-specified theory" and in part due to empirical analysis limitations.

Analytically, we expand on past research into the role of urbanization and economic shocks in generating crises. Wallace (2013), for instance, stresses the importance of large urban areas in generating dissent against the regime. Bellemare (2015) and Hendrix and Haggard (2015) find linkages between food shortages and urban demonstrations, while Weinberg and Bakker (2015) find a relationship between food shortages and civil war. In contrast, in taking a broader perspective on economic shocks, Bazzi and Blattman (2014) find no robust effect of export commodities and civil war. More recently, scholars have linked urban development levels to state-led mass killing during food shortages (Mukherjee and Koren, 2018; Koren and Mukherjee, 2019).

We expand on this growing body of research in three main ways. First, we analyze the link between urban development and repression broadly defined, rather than constraining our focus to its most extreme variance, i.e., civil war and mass killing. Second, whereas past research focuses specifically on food shortages (Mukherjee and Koren, 2018; Koren and Mukherjee, 2019), we examine how repression patterns are affected across all types of economic shocks, again, broadly defined. Finally, whereas analyses of the food-conflict linkages looked at variation across rural and urban areas, we constrain our analysis to look at variations solely within urban areas, which allows us to more effectively identify the relevant mechanisms implied by our theory.

Our theoretical contributions are fourfold. First, we identify not only when, but also *where* strategic incentives are likely to generate large-scale mobilization and government repression. Our focus on "where" is more than just geographic; it emphasizes the *substantive* features of some urban locations – namely, developed infrastructure – that can increase the probability of mobilization (Wallace, 2013; Habermas, 1970) and hence, under some conditions, repression. In doing so, we subject canonical wisdom to a new empirical test – if repression occurs in response to dissent, as we claim, then the locations of repression events and their intensity should logically "map" onto the locations where civilians have great capacity for mobilization and collective action. Second, we theorize (and provide evidence showing) how urban features produce strategic complementarities, increasing the possibility of sufficiently large-scale opposition as to produce a credible threat to the government.

Third, we explain why governments use *observable* rather than latent repression in such constructed areas. We argue that observable violent repression follows a *logic of information*, where the purpose of repression is to illustrate the civilians that the government has the capacity and will to limit the civilians' ability to exchange information and muster enough resources to sustain their mobilization effort, thus affecting the citizens' strategic calculations. Finally, we validate the important role of economic shocks in engendering mobilization and, correspondingly, state repression. In doing so, we substantiate research on the linkage between economic shocks and repression (Davenport, 2007). We broadly assess the intermediary mechanisms implied by our theory using models with disaggregated geospatial data in the Supplemental Appendix.

To empirically test the linkages between economic shocks, urban infrastructure, and violent repression, we estimate a set of empirical models on a subnational, global sample operationalizned as the 0.5 decimal degree grid of urban cells (as defined by Bontemps et al., 2009) within 106 developing countries for 1994-2007.[1] Building on past research (Henderson, Storeygard and Weil, 2012; Mukherjee and Koren, 2018; Koren and Mukherjee, 2019), we use nighttime light emissions in these urban locations normalized by (log) population size to operationalize a time-varying annual measure of concentrated infrastructure and development levels available to the population within each urban cell. To operationalize economic shocks, we rely on the effective exogenous measure of commodity price shocks created by Bazzi and Blattman (2014).

By interacting these variables, we find that severe economic shocks increase (to at least the $p < .05$ level) the (log) number of civilians killed by government forces annually in a given urban cell, based on information coded in the UCDP Georeferenced Event Dataset (GED) (Sundberg and Melander, 2013) (with robust tests that rely on alternative datasets), only where urban infrastructure is relatively or highly developed. This finding departs from past research, which hypothesized a linear relationship between economic shocks and social conflict (e.g., Bazzi and Blattman, 2014; Weinberg and Bakker, 2015). Indeed, we find that in the absence of effective infrastructure, economic shocks actually have a statistically-significant *pacifying* effect on government killings. Moreover, the findings that economic crises, amplified by developed urban infrastructure, can lead to a higher incidence of political violence within urban areas also diverges from a large body of research that emphasizes state-led categorical attacks on civilians is primarily a rural phenomenon (Kalyvas, 2006; Weinstein, 2005). Although our conclusions do not contradict these studies, they do highlight the importance of cities as a neglected area in research on political violence. Accordingly, in the conclusion, we outline relevant implications for policymakers concerned with mitigating the effects of economic crises on political violence.

Theoretical Argument

In this section, we develop a theoretical argument that links export commodity price shocks and urban capacity with state-led repression. Our study makes several novel contributions to existing research. First, whereas past studies that analyze economic shocks' impact on mobilization often look the negative effects of variations in imports on consumption (e.g., Hendrix and Haggard, 2015), import prices are also likely to have an endogenous relationship with conflict (Bazzi and Blattman, 2014). Export price shocks, however, can be – under some conditions – exogenous to political conflict, as discussed by Bazzi and Blattman (2014) and in more detail in the empirical section. Accordingly, we study *primary commodity export prices* and illustrate how – like *increases* in import prices – *decreases* in export prices can have a sharp impact on domestic consumption and welfare. Doing so also allows us to employ an exogenous measure of primary commodity export price shocks developed by Bazzi and Blattman (2014) in our empirical models.

Second, we theorize a *moderated effect* of economic crises on political violence. Emphasis is therefore placed on the importance of urban infrastructure in providing civilians with the *capacity* to mobilize effectively, and hence the regime's willingness to use repression. Focusing on this moderated effect within a particularly relevant context adds nuance to past research, which often presumes a linear relationship between economic hardship (e.g., Bazzi and Blattman, 2014; Hendrix and Haggard, 2015) or urbanization (e.g., Wallace, 2013) and civilian mobilization. Third, whereas past research looks at the impact of shocks on consumption, or urbanization on civilian mobilization, (Hendrix and Haggard, 2015; Wallace, 2013), we extend this interactive logic to *state-led violence*. In doing so, we identify new linkages between research on civilian mobilization (e.g., Chenoweth and Stephan, 2011; Hendrix and Haggard, 2015; Wallace, 2013) on the one hand, and research on political violence and repression (Davenport, 2007; Valentino, Huth and Balch-Lindsay, 2004) on the other.

Export Price Shocks and Social Welfare

Over the past several decades, most developing and emerging states have become heavily integrated into the global economy. This has led to a sharp increase in cross-national movements of a large variety of goods and services, as well as short- and long-term capital flows into the developing world. While the costs and benefits of economic globalization are still debated (e.g., Obstfeld, Taylor et al., 2004; UNCTAD, 2012), the move toward a global economy has increased developing states' exposure to (high) economic volatility (Dehn, 2000; Collier, 2003; UNCTAD, 2012). As a result, the susceptibility of developing countries that rely heavily on *primary* commodities (e.g., minerals, ores, cocoa, coffee, rubber) to severe negative price shocks[2] increased, as well as their probability of experiencing adverse political consequences (Jensen, 2000; Collier, 2003; World Bank, 2016).

Specialization in primary (or export) commodity production, e.g., agriculture, mineral extraction (Dehn, 2000; Bazzi and Blattman, 2014; IMF 2015), not only leads to a lack of risk diversification across different sectors, but also makes developing states highly vulnerable to severe commodity price fluctuations due to demand and supply shocks (Dehn, 2000; UNCTAD, 2012; Hattendorff, 2014). Economic research finds that since 1990, more than two-thirds of all developing and emerging economies have been exposed to acute price shocks in primary commodities, and many suffered deleterious impacts as a result (Jensen, 2000; Obstfeld, Taylor et al., 2004; Koren and Tenreyro, 2007). Research has also consistently shown that workers employed in primary commodity producing and exporting industries constitute between two-thirds to nearly the entire labor force in developing economies (Dehn, 2000; UNCTAD, 2012; Hattendorff, 2014). Importantly, approximately 33%-50% of these workers live in cities (Sagebien and Lindsay, 2011, 43-47; UNCTAD, 2017). These urban-based workers' income is therefore also highly susceptible to sharp contractions in primary commodity prices.

Focusing on political instability and repression, the relative frequency of export price shock occurrence and their economic impact on developing economies raises two key questions: What are the *political* consequences of these shocks? And, more specifically, can these adverse effects be strong enough to generate a violent government response?

Theoretically, we must answer the first question before answering the second, which is arguably the more important and less studied question. When decreased demand for primary commodities exports in developing economies leads to declining revenues, the costs of imports or production nevertheless remain constant. Thus, the *relative* costs to producers increase, in addition to the costs of holding stocks, which further erodes profits. These deleterious effects then spill over to industries that produce intermediate goods, or flow downstream to industries whose supply chains, output, and profitability are directly tied to the economic health of primary commodity producers. The devastating financial impact of these price shocks is therefore not limited to a set of primary commodity producers, but is borne by the entire supply chain, including these intermediate industries, which employ a substantial share of city-based workers (Satterthwaite, 2007; UNCTAD, 2017).

Urban workers will respond to these adverse shocks by curtailing investment and capital expenditures, leading to further contractions in overall economic output (Williamson, 2011; UNCTAD, 2012). As Collier (2003, 3) notes, in developing (African) states, "[i]n the case of the typical large negative export shock...the shock then triggers a cumulative contraction in the economy. . . leading to an additional loss of output of around 14% of initial GDP." Output contraction leads to additional job losses and depresses incomes for a multiple sections of society, especially urban denizens (e.g., Jensen, 2000; UNCTAD, 2017). Such commodity price shocks can also trigger balance of payments crises, especially in primary commodity producing developing economies (Page and Hewitt, 2001; UNCTAD, 2017), raising the prospects of deep recessions, high unemployment, and additional losses to income. Urban households can additionally find smoothing their expenditures in the face of severe shocks particularly difficult because they are often excluded from credit and insurance markets, which exacerbates the pressure on their already depressed incomes (Williamson, 2011; Hattendorff, 2014).

Given that the primary commodity supply chain spans numerous (urban-based) industries, the adverse material effects in cities can persist for long periods of time. As investment, operation, and management of financial assets are all conducted by *urban* residents, investors are far less likely to successfully smooth net returns on their assets during an export price shock. As a result, investors are compelled to curb their investment, or else diversify their portfolio using low-risk alternatives that yield lower average returns for their shrinking capital. The resulting diminished income can aggravate the recessionary environment, leading to significant cuts in public transfers, which are necessary for maintaining basic consumption for broad swathes of society (Jensen, 2000; Koren and Tenreyro, 2007; World Bank, 2016).

When faced with severe decreases in their income and consumption levels, citizens will often turn to the government for financial assistance, substantially increasing societal pressure on incumbents to solve the crisis (Hendrix and Haggard, 2015). If they have the capacity or receive international assistance, governments will generally try to take some measures to address the economic shock's adverse impacts. Nevertheless, they are often unable to fully compensate for these adverse effects, even when they make earnest efforts to do so (Obstfeld, Taylor et al., 2004; Williamson, 2011; Arezki and Brückner, 2012; Dehn, 2000; World Bank, 2016), for at least three reasons

First, most developing countries lack the necessary domestic policy instruments to manage the impact of commodity price shocks, as well as the technical expertise to develop such instruments (Collier, 2003; Dehn, 2000; Koren and Tenreyro, 2007). If the price shock generates a balance of payments crisis, governments are compelled to reduce public expenditure, further weakening their ability to develop effective consumption-smoothing mechanisms to help civilians stabilize their consumption levels. This can be especially harmful to developing states where large sections of the domestic population persist on relatively low incomes (World Bank, 2015).

Second, primary commodities producers typically cannot protect themselves in advance against sudden drops in export prices (Jensen, 2000; Arezki and Brückner, 2012; IMF 2015). The inability to hedge against such shocks raises unemployment levels in the main extraction areas. Laid off workers often move to cities in search of work (Becker et al., 2005), exacerbating social pressures in these locations. The income levels of employees in "downstream," city-based industries associated with primary commodities (e.g., processing plants) are also reduced (Venables, 1996). The number of people under the poverty line in *cities* therefore increases even more than their share in the national population, especially given the government's weak capacity to provide funds and facilitate consumption smoothing.

Third, governments may lose revenues directly as a result of forgone taxation and customs duties, a critical source of income for most developing state governments (Obstfeld, Taylor et al., 2004; Williamson, 2011). These lost revenues make it extremely challenging for the state to borrow at cheap rates, both locally and internationally, which further limits their ability to smooth public investment. Indeed, although the government could deal with the crisis if it has savings or is able to secure a loan, many developing economies already have massive levels of debt, making smoothing infeasible in any case. The adverse welfare effect experienced by the citizens in these states due to the commodity price shock is therefore accentuated by their governments' dependence on the trade in primary commodities.

These three factors have critical implications for political instability. The government's inability to address the economic crisis sends the citizens a strong signal about its bureaucratic incompetence. Because the government lacks the capacity to develop and implement policy instruments or fund consumption smoothing mechanisms, it also cannot credibly commit *ex ante* to improve the citizens' plight during these adverse periods. This will drive many citizens to place the blame for their predicament on the government's ineffective response. At least in some of these cases, as resentment against the government builds up among citizens, a challenge to the status quo may arise, when civilians or the opposition demand a policy change, a more effective response, or even, in extreme cases, the government's removal from office (Chenoweth and Stephan, 2011; Hendrix and Haggard, 2015).

The Role of Urban Infrastructure

Faced with a commodity price shock, each citizen can more easily believe that the other citizens' preferences also lean strongly against the government, and hence that anti-government sentiment is widespread. Nevertheless, each citizen's ability to mobilize with her fellow citizens is susceptible to collective action problems, especially if she is uncertain *ex ante* about whether others will actively oppose the government if she mobilizes. Moreover, the costs of participating in protests can be high, requiring enough citizens to invest a sufficient amount of resources toward mobilizing. An economic crisis is hence insufficient, in-and-of-itself, in explaining *when and where* citizens will act on such sentiment.

Enter the role of urban infrastructure. We argue that a key explanation for whether citizens will overcome their collective action problems and invest more resources (both individually and collectively) in mobilization during an economic crisis is determined by whether or not they live in cities where infrastructure is sufficiently developed. Developed urban infrastructure facilitate coordination among the citizens, providing them with more resources (e.g., money, time) to invest in a common pool and encouraging them to collectively mobilize against an ineffective government (Wallace, 2013). As we argue below, the citizens' willingness (due to the economic crisis) and ability (due to developed urban infrastructure) to pose a threat to political stability can exert strong pressures on the government to employ violent repression. Indeed, past research linked urban development levels to state-led mass killing during food shortages (Mukherjee and Koren, 2018; Koren and Mukherjee, 2019). As discussed in the introduction, we expand on this research by looking at the impact of urban infrastructure (per capita) (i) on repression more broadly, not just its most extreme variant, (ii) during all types of economic shocks, not only food shortages, and (iii) how they vary within urban areas, specifically.

There are, of course, alternative explanations. For instance, one might argue that these effects are simply more likely in cities where population densities are greater, as there are more potential protesters to partake in the campaigns (Wallace, 2013). However, greater concentrations of urban denizens, on its own, does not guarantee civilians will mobilize in masses, or that they have enough resources to contribute toward such mobilization. Accordingly, the focus on infrastructure normalized by the size of the population within these areas better explains how and which cities will experience more mobilization.

Conceptually, higher levels of urban infrastructure can have two important impacts on the citizens' ability to mobilize. The first is in improving *efficiency* by reducing the costs of participating in opposition activities. As past research illustrated, higher levels of urban infrastructure imply better communication technology and greater mobilization capacity for each individual citizen (Henderson, Storeygard and Weil, 2012; Wallace, 2013; Mukherjee and Koren, 2018; Pierskalla and Hollenbach, 2013). Each citizen residing in a relatively well-developed urban location, in other words, enjoys not only greater access to communication technology, but also more disposable income and other resources that can be directed toward opposition activities (Habermas, 1970; Wallace, 2013). As anti-government sentiment spreads in the wake of an economic shock, access to more resources and better infrastructure enhances the material capacity of the citizens. This, in turn, lowers the costs they incur from mobilizing against the government. This effect is amplified by better communication technology, which additionally mitigates mobilization costs by promoting information sharing and coordination among the citizens. The resulting efficiency effect thus creates added incentives for the civilians to openly oppose the government during an economic crisis (Wallace, 2013).

A second impact of infrastructure happens through *alignment*. Developed infrastructure promotes widely shared beliefs among the citizens that the government's capacity to maintain political control and social stability is low, which further hinders the latter's ability to address the crisis created by the economic shock. Eventually, a sufficiently high number of citizens in these more developed cities will recognize that their fellow citizens are not only willing to mobilize against the government, but are also capable of mobilizing *en masse* due to the decreased mobilization costs engendered by the efficiency effects. Although this belief may be misguided (as we discuss below), each citizen in these locations will share the same belief that the challenge is large enough to overwhelm the government, forcing the latter to make some form of policy concessions.

The efficiency and alignment effects, in turn, generate *strategic complementarities*. As more citizens come to expect the mass of protesters will be large enough to extract policy concessions from the government, these expectations are reinforced, creating a stronger incentive to challenge the government, thus engaging more and more urban citizens in overt opposition.

Case-specific evidence broadly supports the amplifying effect of urban infrastructure on anti-government mobilization during economic crises. In Indonesia, for instance, the 1997 economic crisis triggered a recession and subsequent diverging coalition preferences, impacting the welfare of citizens in the country's largest cities, thus prompting massive protests and the eventual removal of the regime (Pepinsky, 2009). Similar dynamics in major cities hastened regime changes in Tunisia and Egypt during the 2011 protests (Wallace, 2013), which were similarly heavily motivated by economic instability. These dynamics are not unique to nondemocratic states: in India in September 2015, "a sharp drop in commodity prices dented [prime minister's] Modi's popularity, and led thousands of farmers and laborers to paralyze the Indian capital Delhi in a protest against what they called the anti-people policies of Prime Minister Modi's government...Traffic in many parts of Central Delhi came to a halt as the protesters marched towards Parliament street in the heart of the capital" (Mahurkar, 2017, 59). Likewise, a sharp decline in primary commodity prices in Brazil during the summer of 2013 illustrated the government's "inability to meet the citizens demands in response to this collapse of key commodity prices [and] led to enormous street protests in São Paulo" (Montero, 2014, 87). As we discuss at the end of this section and in great detail in the Supplemental Appendix (specifically Table A3), we also conduct empirical exercises using cross-national data to validate the theoretical mechanisms linking urban infrastructure and exogenous price shocks to repression via affecting civilian mobilization.

Civilian Mobilization and State Repression

Once mobilized, the protesting citizens have committed themselves to challenging the government, which means they cannot credibly commit not to force its eventual removal as the campaign progresses. Recognizing the threat to its political control, the government also knows that if the opposition grows further, it may overwhelm its repressive capacity, possibly forcing the government to step down, a possibility of which all protesting citizens are also aware.

We posit that when the government realizes the citizens pose a political threat, it becomes *less* likely to provide peaceful concessions, e.g., institutional reforms, to solve the economic and – now political – crisis. One reason is that the government anticipates the citizens will perceive such concessions as a sign of weakness in the face of mounting opposition to its rule. This, in turn, strengthens the strategic dynamics highlighted above, meaning that concessions can potentially induce the protesting citizens to *intensify* rather than step back from their campaign, possibly encouraging other non-protesters to join. Again, this is especially true in cities with developed infrastructure, where the citizens are able to draw on more resources available to them and leverage their campaign.

Correspondingly, as urban infrastructure development levels rise, the fear of *ex post* removal from office will drive the besieged government to *strategically* employ violent repression against its citizens *ex ante* during periods of economic crises. The purpose of the violence in this case is to provide *information* to the citizens about the state's repressive capacity – i.e., to clearly illustrate that the belief that it can be overwhelmed is misguided – and curtail the citizens' ability to exchange necessary information about their campaign.[3] This "logic of information" suggests the government will repress for at least three reasons.

First, if the government wants to maintain power, it must send not only send a signal, but specifically a strong signal, to convince the opposition that the costs of mounting a political challenge are prohibitive. This means the government will carry at least part of its violent repression out in the open, so that the violence can be clearly attributed to the regime (Davenport, 2007; DeMeritt, 2016). From such overt repression the demonstrators unambiguously learn about the government's ability to impose retaliatory costs if the civilians persist with their anti-regime mobilization.

Second, as the state escalates its degree of repression, the citizens believe the government's capacity to eliminate the demonstrations and maintain political control is not only substantial, but also much higher than previously anticipated. This increases the citizens' uncertainty about their ability to maintain collective action, which is enough to dissuade some demonstrators from sustaining their mobilization efforts, thus further weakening the opposition. From this perspective, visibly and violently repressing in cities with more developed infrastructure mitigates the aforementioned alignment effect.

Third, violent repression reduces the citizens' mobilization capacity by restricting their access to communication technology or by limiting the degree of resources that can be employed against the government. Recognizing that violence puts pressure on the urban citizens' resource availability, the challenged government will have added incentives to employ repression in cities with better infrastructure to reduce the material capacity available to civilians, thereby mitigating the efficiency effect. Credible retaliatory repression hence diminishes both the size and the prospects of success of the opposition in developed urban areas, generating greater returns for violent behavior by the government.

Case-based examples lend support to these claims. For instance, during the 2013 anti-government demonstrations in São Paulo (Brazil), which were engendered by a sharp decline in primary commodity prices, the government's "brutal military police turned a mostly peaceful demonstration into a terrifying rout. Dozens of videos...show officers with their name tags removed firing stun grenades and rubber bullets indiscriminately at fleeing protesters and bystanders and hunting stragglers through the streets" (The Economist, 2013). This violent repression was motivated, at least partly, by the fact that the government was "afraid that the protests are challenging the power and privileges of an economic elite that has been created in Brazil" (Montero, 2014, 92). Similarly, during anti-government protests in Delhi in 2015 – which occurred, again, in the context of an economic crisis – the police opened fire on protesters: "[t]he crackdown resulted in the death of the protesters, and the injury of many others" (Mahurkar, 2017, 63).

Again, there are possible alternative explanations. For instance, cities with higher population densities might simply be more likely to experience a higher rate of causalities due to repression because more "potential targets" live there rather than improved infrastructure in these cities pose a greater risk to the regime. Therefore, before deriving a testable research hypothesis, we empirically validate the linkages between urban infrastructure, exogenous price shocks, civilian mobilization and repression, as theorized in this section. Systematically testing the theoretical expectation that citizens in urban areas with developed infrastructure will believe *ex ante* that other citizens will mobilize against the government during an economic crisis is challenging, as data to operationalize such perceptions is not (to our knowledge) publicly available. However, the Armed Conflict Location and Events Dataset (ACLED)

(Raleigh et al., 2010) provides an exceptional coverage of a large number of multiple types of political violence events, including riots and protests. Accordingly, in the Supplemental Appendix, we our conduct validation exercises using ACLED data for all African urban areas (measured at the same annual 0.5 degree grid cell resolution discussed in the next section) for the 1998–2007 period, using the same independent variables discussed in detail in the next section.[4]

We begin this exercise with a set of probit models corresponding to the most robust specifications discussed in the next section to show that the interaction of urban infrastructure levels and price shock (discussed in detail below) have a robust, positive, and statistically significant (to at least the $p < .05$ level) effect on the decision of civilians to mobilize or not prior to repression (that is, in year $t-1$). We then use a Heckman selection model to show the interaction of our variables of interest has a positive and statistically significant effect (to the $p < .05$ level) on both (i) the probability of civilians will mobilize in a given African urban grid cell i during a given year $t-1$; and (ii) the risk of repression at year t, *conditional on civilian mobilization occurring at* $t-1$. These validation exercises thus enable us to develop a broad, global empirical expectation, which we test on a geospatially-disaggregated data on all developing states in the next section:

> **Hypothesis:** *Within developing states, the levels of violent repression in urban areas during a severe export commodity price shock will be zero where urban infrastructure levels are low, but will increase substantially where development and infrastructure levels rise*

Empirical Analysis

In this section we describe our empirical strategy and sample, statistical analysis results, substantive implications, and sensitivity analyses illustrating our findings' robustness to alternative confounders, modeling, and operationalization choices.

Sample, Variables and Statistical Methodology

We test our hypothesis on a sample encompassing 14 years (1994-2007)[5] and 106 states.[6] These data are first structured into a cell-year level dataset wherein cells – our cross-sectional unit of interest – are measured at the 0.5 x 0.5 decimal degree resolution (Tollefsen et al., 2012).[7] We then retain only grid cells denoted as "urban" (i.e., cells with artificial surface coverage of 50% or more) by Bontemps et al. (2009), thereby ensuring that our developing-country cell sample most closely corresponds to the urbanized context assumed by our theory; and verifying that any reporting biases, if exist, will likely be accounted for due to improved coverage across urban locations (Pierskalla and Hollenbach, 2013). Indeed, our theoretical focus is on overt repression as a clear signal to the public, it is empirically justifiable to focus on media- and NGO-reported events in this case. There are approximately 5,503 urban cells observed for any given year within our 1994–2007 period, with the average developing country in our sample containing roughly 52 urban cells.[8]

To test our hypothesis, we operationalize our dependent variable, $\textit{Deaths by Government}_{it}$ as the yearly (t) count of the number of civilians killed by government forces in a given urban cell i (based on best estimates available), as done in past research on state-led political violence (e.g., Valentino, Huth and Balch-Lindsay, 2004). This indicator was coded from the UCDP GED (Sundberg and Melander, 2013), which is considered one of the most comprehensive geolocated political violence data in existence and covers the entire terrestrial globe between 1989 and 2017 (with the exception of Syria). The GED defines one-sided attacks as attacks perpetrated by an organized actor against civilians resulting with at least one civilian casualty, which allows us to operationalize our dependent variable as death counts (compared with datasets that code only incidents with, say, five casualties or more). To ensure theoretical comparability, only attacks denoted by GED as perpetrated by (i) government forces (ii) against civilians were retained. To ensure geospatial comparability with the PRIO-Grid 0.5degree cell framework, only events coded by GED as occurring at the second administrative level (district) or below were included.[9] A total of 138,387 civilians were killed within our urban-year sample in a total of 1,312 different attacks by the government.[10]

We interact two explanatory variables to test the moderated effect posited by our hypothesis: (i) a continuous measure of the extent of infrastructure and its concentration within our urban-year sample, and (ii) a binary measure denoting severe negative economic shocks affecting a given country during year t, and lag each to year $t-1$, considering that our the effects we posit likely unfold over a relatively long period of time. We construct our *Concentrated Urban Infrastructure*$_{it-1}$ as a thinly disaggregated, time-varying measure of urban development, infrastructure, and material capacity in two stages. Our approach closely follows past research that similarly utilized nighttime light emissions normalized by (log) population to measure development level within urban areas (Henderson, Storeygard and Weil, 2012; Mukherjee and Koren, 2018; Koren and Mukherjee, 2019).

First, we use mean annual levels of nighttime light in a given cell from the PRIO-Grid dataset (Tollefsen et al., 2012) to measure local infrastructure and development, as past research illustrates nighttime light a highly-effective measure of the expansion of local infrastructure including roads, telecommunication and electricity (Henderson, Storeygard and Weil, 2012; Koren and Sarbahi, 2018). To ensure that our indicator captures the annual *concentration* of infrastructure within each urban grid cell accounting for population densities, in the second step we normalize nighttime light emissions by (the natural log of) population size residing in a given urban cell during year t. This indicator is hence similar a variable used by Mukherjee and Koren (2018) and Koren and Mukherjee (2019), although it differs from the latter in that it focuses solely on urban areas, whereas the other two studies also include rural areas in their operationalization.[11]

We operationalize our second explanatory variable, *Price Shock*$_{jt-1}$, as the occurrence of a severe decrease in primary commodity export prices in country j in two stages. In the first stage, we rely on information from Bazzi and Blattman (2014), who developed a new country-specific measure of the change in annual commodity export prices (in USD). This measure was calculated as "the annual difference in each country's log commodity export price index," where "[e]ach country's price index is a geometric average of all commodity export prices weighted by lagged export shares" (Bazzi and Blattman, 2014, 7-8), and provides a major improvement over past data in several ways. First, it incorporates nearly 50% more commodity-price data than used in previous studies. Second, by focusing on export price shocks, specifically, rather then commodity prices more broadly, this indicator is preferred to other, import-based measures in that it is far less susceptible to endogeneity concerns (Bazzi and Blattman, 2014, 6-12). In the second stage, we exploit within-state variations in exogenous export prices by operationalizing *Price Shock*$_{jt-1}$ as a binary variable whereby all cells within country j during a year $t-1$ that experienced a decrease in commodity export prices that was one standard deviation or more *below the country's mean* were given a value of one, otherwise zero. In Tables A8-A10, Supplemental Appendix, we illustrate our findings are robust to employing several alternative thresholds of our economic shock indicator, thus providing additional evidence to suggest our specific choice of threshold is not driving the results.

Considering that our hypothesis assumes an interactive effect of concentrated urban infrastructure and economic shocks on increasing repression levels, we introduce the interaction term *Concentrated Urban Infrastructure*$_{it-1}$ $\times$ *Price Shock*$_{jt-1}$ into our models, and control for its individual components. Considering that any effects of economic shocks and urban concentration likely unfold over a relatively long period of time, these explanatory variables were all lagged by one year, and are hence denoted as $t-1$.

Our models also include different variables, similarly measured at the 0.5 degree cell level, accounting for alternative explanations. All cell-level independent variables (with the exception of the dependent variable lags) were included in the PRIO-Grid dataset (Tollefsen et al., 2012). The first variable is designed to account for the role of civil conflict[12] in generating violence against civilians during year t, an important potential confounder considering that the GED measures political violence, broadly defined. We also ensure that any observed effects of our explanatory variables are the result of concentrated urban infrastructure, specifically, and not simply the number of citizens residing in a given cell more broadly, by accounting for (logged) population densities. Following research that emphasizes the role of climatic factors in impacting political violence and repression (e.g., Mukherjee and Koren, 2018; Koren and Mukherjee, 2019; Ritter and Conrad, 2016; Weinberg and Bakker, 2015; Bellemare, 2015), we include variables accounting for the impact of variations in drought frequency,[13] temperature, and (logged) rainfall (in millimeters) in a given cell i during year t.

Next, we include geospatial indicators to account for the state's repressive capacity and ensure that any observed effects of *Concentrated Urban Infrastructure*$_{it-1}$ $\times$ *Price Shock*$_{jt-1}$ are not the result of the government's higher effectiveness in repressing in these locations. These controls are: (logged) distance to the capital, (logged) distance to the nearest borders (in kilometers) and the degree of a given cell covered with mountains. Additionally, to account for the potential role of ethnic enmities in generating mobilization – as coordination might be easier among ethnic kin – and repression – considering some ethnicities might be a specific target of state violence – we include an indicator denoting whether there were any groups that were excluded from political power (discriminated or powerless) in a given cell i during year t. Finally, we include a one-year lag of the dependent variable to ensure that any serial correlations in repression from one year to the next are taken into account.

Because one of our main explanatory variables, *Price Shock*$_{jt-1}$, is measured at the state-year level, we also include country-level confounders in our model. These include (i) a country's degree of political openness based on the ordinal *Polity2*$_{jt}$ indicator (Marshall, Jaggers and Gurr, 2013); (ii) its quadratic term to account for the possibility that including "middle-of-the-way" democracies, or anocracies, in our sample is driving the results (Ulfelder, 2012); and (iii) country size, which might affect the citizens' ability to mobilize and the government's ability to respond.[14] Summary statistics for all variables, including those used in the sensitivity analyses, are reported in Tables A1, Supplemental Appendix.

Considering the relatively wide range on our *Deaths by Government*$_t$ variable (0 ⇔ 89,836), we log this variable prior to analysis. Accordingly, we rely on ordinary least squares (OLS) estimators for statistically assessing our hypothesis in a rigorous manner (Angrist and Pischke, 2009). To ensure that the observed relationships are not the particular result of across- or within-country j and grid cell i variations, we estimate each model three times: (i) with only year fixed effects, (ii) with year and country fixed effects, and (iii) with year and grid cell fixed effects. To account for heterogeneities within countries, which can override heterogeneities within smaller geospatial and administrative units, standard errors in all models are clustered by country. The moderated relationship between export commodity price shocks, urban infrastructure, and repression is hence identified using the following equations:

$$\ln \mathbf{y_{it}} = \beta_0 + \beta_1 \mathbf{s_{jt-1}} + \beta_2 \mathbf{d_{it-1}} + \beta_3 \mathbf{s_{jt-1}} \times \mathbf{d_{it-1}} + \beta_{4-k} \mathbf{X_{it}} + \phi_\mathbf{t} + \varepsilon_{jt} \tag{1}$$

$$\ln \mathbf{y_{it}} = \beta_0 + \beta_1 \mathbf{s_{jt-1}} + \beta_2 \mathbf{d_{it-1}} + \beta_3 \mathbf{s_{jt-1}} \times \mathbf{d_{it-1}} + \beta_{4-k} \mathbf{X_{it}} + \psi_{\mathbf{j}} + \phi_{\mathbf{t}} + \varepsilon_{jt} \quad (2)$$

$$\ln \mathbf{y_{it}} = \beta_0 + \beta_1 \mathbf{s_{jt-1}} + \beta_2 \mathbf{d_{it-1}} + \beta_3 \mathbf{s_{jt-1}} \times \mathbf{d_{it-1}} + \beta_{4-k} \mathbf{X_{it}} + \omega_{\mathbf{i}} + \phi_{\mathbf{t}} + \varepsilon_{jt} \quad (3)$$

In these equations, $\ln \mathbf{y_{it}}$ is a vector of (logged) *Deaths by Government*$_t$ by grid cell *i* for each year; $\mathbf{X_{it}}$ is a matrix of variables accounting for potential confounders; $\psi_{\mathbf{j}}$, $\omega_{\mathbf{i}}$ and $\phi_{\mathbf{t}}$ denote fixed effects by country, grid cell, and year, respectively; and ε_{jt} denotes standard errors are clustered by country. In these models, $\mathbf{s_{jt-1}}$ refers to *Price Shock*$_{jt-1}$, $\mathbf{d_{it-1}}$ to *Concentrated Urban Infrastructure*$_{it-1}$, and $\mathbf{s_{jt-1}} \times \mathbf{d_{it-1}}$ to their interaction.

Results

Table 1 first reports a baseline model of *Deaths by Government*$_{it}$, which includes only our interaction term *Concentrated Urban Infrastructure*$_{it-1}$ $\times$ *Price Shock*$_{jt-1}$ and its constitutive terms, in addition to year fixed effects. This baseline model is followed by a full specification, which adds all the controls discussed above, and then by corresponding baseline and full specifications that add fixed effects by country and grid cell to ensure the observed relationships are driven only by within-country and withing grid cell factors. Note that in the grid fixed effects models, all cell and country specific variables that do not vary over time are omitted.[15]

All results strongly support the hypothesized effect of *Concentrated Urban Infrastructure*$_{it-1}$ $\times$ *Price Shock*$_{jt-1}$ on repression. The individual coefficient estimates on *Price Shock*$_{jt-1}$ are negative, and although they are statistically significant only in the standard and country fixed effects models, they suggest that – in line with our theory – *Concentrated Urban Infrastructure*$_{it-1}$ is nil, lagged economic shocks do not noticeably increase the probability of state-led repression within a given urban grid cell *i*, and might even have a pacifying effect. However, the coefficient on our *Concentrated Urban Infrastructure*$_{it-1}$ $\times$ *Price Shock*$_{jt-1}$ interaction is positive and significant, implying that, in support of our hypothesis, increases in concentrated infrastructure have an added repression-intensifying effect following an economic shock, though we must examine our marginal effects below to fully ascertain the significance of this result. Finally, the coefficient estimate for *Concentrated Urban Infrastructure*$_{it-1}$ itself – which corresponds to the citizens' mobilization capacity within urban cells when no economic shock occurs – is positive in all models, and statistically significant in the standard and country fixed effects specifications, suggesting that such urban grid cell years may be naturally likely to experience some repression, albeit to a much lesser extent compared with when an economic shock occurs.[16]

Table 1 about here

To evaluate whether our interaction supports our hypothesis, we use our full model estimates for each dependent variable to calculate the change in *Price Shock*$_{jt-1}$'s coefficient on the expected (log) number of civilians killed by regime violence across the range of *Concentrated Urban Infrastructure*$_{it-1}$ (0.08 $\Leftrightarrow$ 4.65), holding all other control variables to their means or modes. We plot these estimated marginal effects for our three full model specifications, along with their 95% confidence intervals, in Figure 1. *Price Shock*$_{jt-1}$'s expected effects in urban areas where *Concentrated Urban Infrastructure*$_{it-1}$ is at its minimal value (i.e., = 0.08), is negative, meaning price shocks are having a *pacifying* effect in these low infrastructure cities. However, as *Concentrated Urban Infrastructure*$_{it-1}$ levels increase to its maximum (i.e., =4.65), so does the expected effect of *Price Shock*$_{jt-1}$ on the frequency of violent repression. *Price Shock*$_{jt-1}$'s coefficient increases from zero to $\sim$0.12 across all (logged) *Deaths by Government*$_{it}$ models (that is, $\sim$ 13% increase in the average number of civilians killed) compared with the baseline. These results support our hypothesis and argument more broadly, and suggest that while economic shocks are unlikely to generate government repression, presumably due to the citizens' lack of substantial mobilization capacity in these areas, violent regime response becomes much more likely as concentrated infrastructure levels increase.

Figure 1 about here

Next, our argument and the logic of information advocated therein suggests that an event with a large number of deaths will gain more public attention and hence work as a stronger signal of the government's repressive capacity while minor repression is less likely to do so, suggesting our interaction's coefficient should be closer to zero (Weidmann, 2016). Accordingly, to evaluate of how the impact of our interaction varies across different severity levels of repression, we plot the change in the coefficient *Concentrated Urban Infrastructure*$_{it-1}$ × *Price Shock*$_{jt-1}$ across four logit models with dichotomous repression indicators, with and without country fixed effects, in Figure 2.[17] The binary dependent variables were operationalized as whether values on *Government Killings*$_{it-1}$ were at least (i) one civilian death, (ii) five civilian deaths, (iii) 15 civilians deaths, and (iv) 30 civilian deaths (each given a score of one, zero otherwise). Even at the 30 cases threshold, rarity of events (i.e., a ratio of 65 urban grid years that experiences such severe repression to 76,977 that did not over our 1994–2007 period, or 0.08%) – a welcome occurrence in the real world, of course – becomes an overriding empirical concern (as illustrated by the wide 95% confidence intervals in Figure 2). Nevertheless, the trend line in Figure 2 illustrates that the size of *Concentrated Urban Infrastructure*$_{it-1}$ × *Price Shock*$_{jt-1}$'s coefficient indeed increases with the severity of repression. This evidence suggests that exogenous price shocks actually produce *greater* incentives for more violent repression as urban development levels increase.

Figure 2 about here

To evaluate the sensitivity of our findings to alternative confounders, sampling, and modeling choices, we estimate a large number of robustness models corresponding to the full specification in Equations 1–3. We report all corresponding robustness tables in our Supplementary appendix (Tables A4–A11), and summarize each model here. Briefly, these robustness models account for: (i) operationalization of repression (our dependent variable and its lag) as the number of incidents of attacks against civilians in urban areas by government forces; (ii) using the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al., 2010) instead of the UCDP GED to operationalize our dependent variable *Government Killings*$_{it}$ and its lag, and (iii) using the Social Conflict Analysis Database (SCAD) Version 3.3 dataset (Salehyan et al., 2012) for operationalizing our dependent variable (and its one-year lag) as the number of civilians killed by state-led repression of riots against the regime; [18] (iv) a control-inclusive model accounting for a large number of additional confounders; (v) using a higher threshold (1.5 standard deviation below the mean) to dichotomize our price shock variable; defining economic shock as all cell years within a country that was (vi) in the lowest 15th and (vii) 25th percentiles, respectively, of the entire sample in terms of exogenous commodity price shocks; and (viii) including cell fixed effects and their interactions with cubic splines of time trend, which hence allow us to identify unit-specific time trends (see, e.g., Carey and Horiuchi, 2017; Xu, 2017). Crucially, our findings hold in every model and nearly every specification, suggesting a causal effect of our interaction on government repression.

Conclusion

Our findings outline several important future directions of research. First, by focusing on the origins of anti-regime collective action based on the dynamics discussed above, we are able to explain how, why, and where economic crises generate credible threats to the government. Past research argued that urban areas can serve as a hotbed of anti-regime resistance once population densities become sufficiently high (Wallace, 2013; Habermas, 1970). We improved on this perspective by showing that, within these areas, repression is more frequent where the civilians have the highest capacity to overcome collective action problems and generate a credible threat to the government, a function not (only) of high population densities, but also of material and social capabilities that ensure a large number of citizens take the streets. Indeed, our main analyses and robustness models suggest this effect is causal.

Future research on repression and political violence more broadly would benefit from accounting more effectively for the role of urban areas as hotbeds of resistance rather than focus almost exclusively on violence occurring at the countryside. A second direction would be in exploring new sources of data to understand better to causes of mobilization and repression. In this study, we rely on a disaggregated proxy to estimate how local variations in urban infrastructure correlate with the prevalence of state repression. Moving forward, using systemic surveys can help validating the specific mechanisms underlying the citizens' choice for collective action.

For policymakers, our theory and findings elicit important mechanisms governing the variation in systematic killings, especially with respect to development, urbanization, and economic crises. Increases in urban development and infrastructure can generate intense political pressures under the right conditions. States and international organizations should devise political and economic plans to "smooth out" some of the economic shock's effect. International actors should additionally acknowledge the need to preempt and mitigate social and political pressures related to urbanization in developing states more broadly. Considering that, if present trends continue, urbanization levels in the developing world are only going to increase, preemptively addressing some of these challenges can help reduce domestic conflict and save lives.

Notes

[1]The period for which information on all variables was available.

[2]Defined below as strong negative deviations from average export commodity prices for a given country.

[3]Indeed, future research can build on Acemoglu and Robinson's (**?**) canonical model to further explore whether the government's choice of resorting to violence rather than providing concessions is influenced by income inequality. Considering that this possibility is a potential confounder, we empirically account it in our empirical models below.

[4]ACLED also covers some Middle Eastern and Asian states, but only starting 2010 (and in some cases much later), whereas Bazzi and Blattman's (2014) commodity price shock indicator covers only the period up to and including 2007.

[5]The period for which information was available on our (lagged) dependent and key independent variables.

[6]As defined by Bazzi and Blattman (2014). See Table A2, Supplemental Appendix for a list of these states.

[7]I.e., cells of approximately 55 x 55 kilometers at the equator (3025 square kilometers area).

[8]A map of urban grid cells analyzed is reported in Figure A1, Supplemental Appendix.

[9]For details about the types of sources used to compile the GED, see Croicu and Sundberg, 2017.

[10]We illustrate our findings robustness to the decision to rely on death counts for operationalizing our dependent variable by estimating models where the dependent variable is operationalized as the count of incident of such attacks, also obtained from the UCDP GED, in Table A4 Supplemental Appendix.

[11]Importantly, although using an interaction with an exogenous shock helps to alleviate such concerns, this variable might be susceptible to endogeneity with repression. Nevertheless, if such an endogenous relationship exists in the data, the expectation is that it will be *negative* (Koren and Sarbahi, 2018). As Table 1 illustrates, the coefficient on both the constitutive $\textit{Concentrated Urban Infrastructure}_{it-1}$ variable and its interaction is positive, suggesting that endogeneity, even if it exists, is not driving the results.

[12]Defined as a conflict with at least 25 combatant casualties (Tollefsen et al., 2012).

[13]This variable is operationalized as the proportion of months out of 12 months that are part of the longest streak of consecutive months ending in the given year with Standardized Precipitation and Evapotranspiration Index (SPEI) values below -1.5 (Tollefsen et al., 2012).

[14]We account for an extensive number of additional controls in the sensitivity analyses reported in the Supplemental Appendix.

[15]Also note that $\textit{Country Area}_j$, which does have some variation over time at the country level (and is hence included in the country fixed effects models) is automatically dropped from this model due to its practical lack of temporal variation at the grid cell level.

[16]Note that we do not discuss the effects of other variables in our models considering the interpretation of such "controls" outside of purely experimental setting is problematic (Keele, Stevenson and Elwert, 2020).

[17]The estimates of these four models, corresponding to our full specification with year fixed effects and with country and year fixed effects, are reported in Tables A12–A13, Supplemental Appendix.

[18]Note that when using ACLED, we lose both four years of data, ACLED data is not available prior to 1997, and all countries outside of Africa, which ACLED does not code for our temporal period of analysis; and in using SCAD we lose all countries excluding Africa and the Caribbean, the only world regions covered by SCAD. Also note that in the SCAD models, while our results hold in the year only and country+year fixed effects models, the interaction term's coefficient is not significant in the grid cell + year fixed effects model, although it still maintains the (expected) positive sign.

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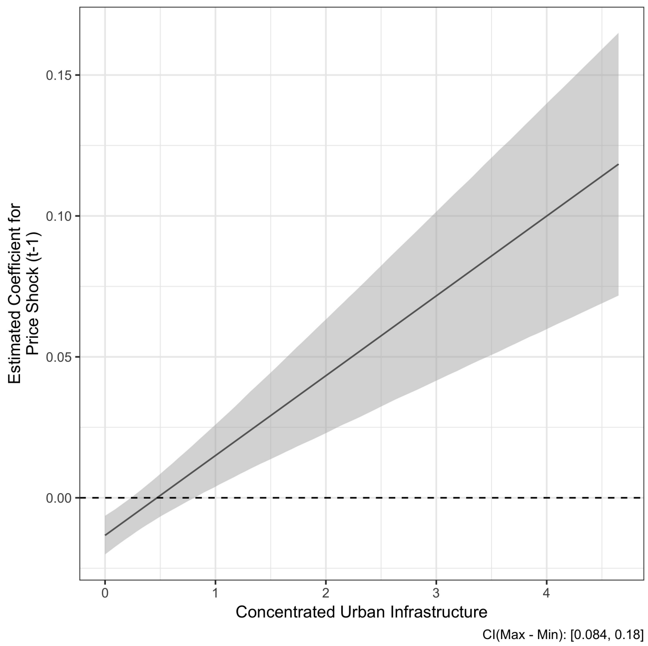
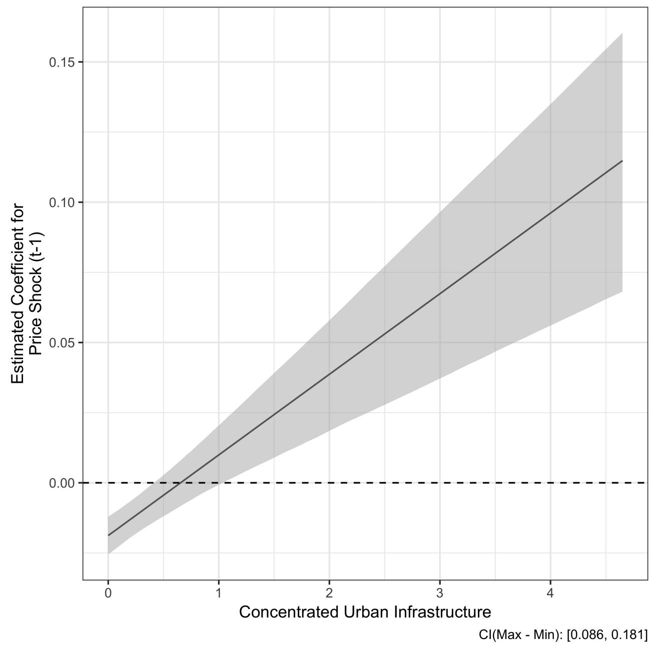
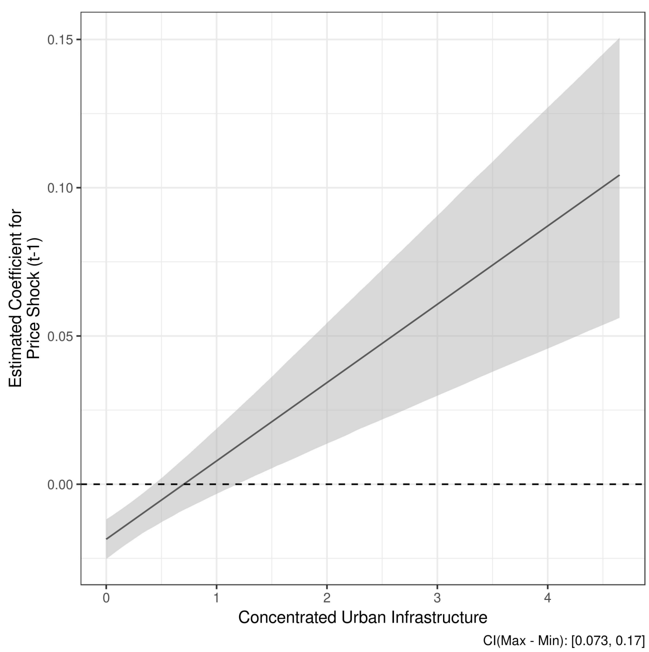
Figure 1: Estimated Effect of Price Shocks on Urban Development PC's Propensity of *Government Killings*$_{it}$



Year FEs



Country and year FEs



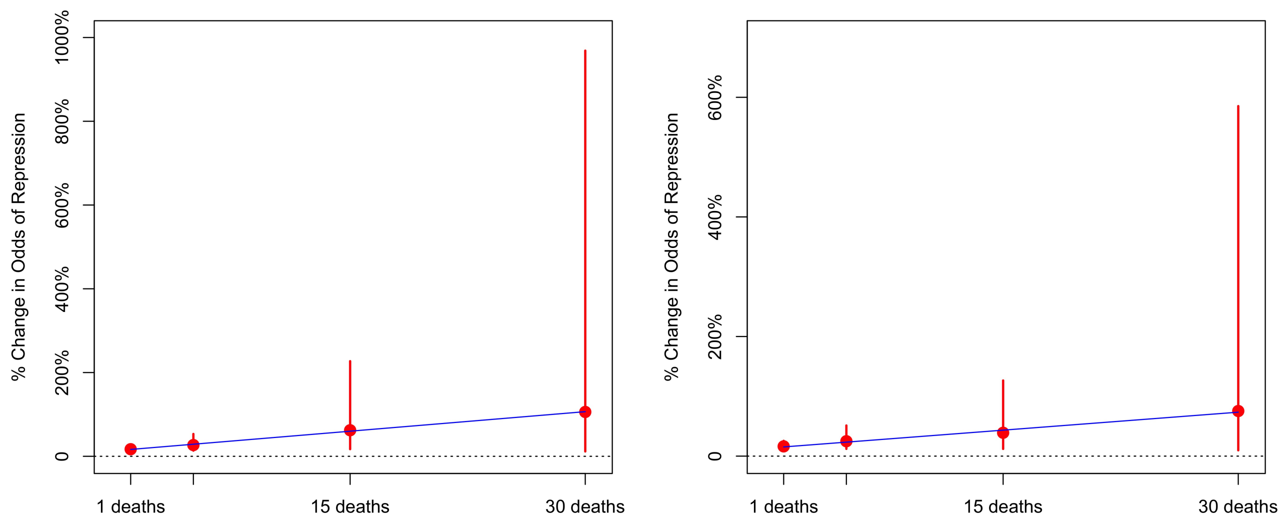
Grid-cell and year FEs

Table 1: Determinants of Killings of Civilians by Government Forces, 1994-2007

	Baseline			Full		
	Model1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Concentrated Urban Infrastructure$_{it-1}$	0.009*** (0.002)	0.023*** (0.003)	0.004 (0.015)	0.006** (0.003)	0.016*** (0.003)	0.024 (0.019)
Price Shock$_{jt-1}$	−0.013*** (0.003)	−0.014*** (0.003)	−0.013 (0.011)	−0.013*** (0.003)	−0.019*** (0.003)	−0.019 (0.013)
Concentrated Urban Infrastructure$_{it-1}$ × *Price Shock*$_{jt-1}$	0.030*** (0.005)	0.027*** (0.005)	0.020** (0.010)	0.028*** (0.005)	0.029*** (0.005)	0.026** (0.013)
Government Killings$_{it-1}$[1]	–	–	–	0.199*** (0.004)	0.146*** (0.004)	0.023 (0.030)
Population$_{it}$[1]	–	–	–	0.002*** (0.001)	0.003*** (0.001)	0.012 (0.022)
Civil War$_{it}$	–	–	–	0.049*** (0.003)	0.059*** (0.003)	0.078** (0.030)
Ethnic exclusion$_{it}$	–	–	–	0.013*** (0.002)	0.007*** (0.002)	0.014 (0.018)
Drought$_{it}$	–	–	–	0.050*** (0.011)	0.036*** (0.011)	0.028 (0.043)
Temperature$_{it}$	–	–	–	0.0001 (0.0001)	−0.0002 (0.0002)	0.004 (0.003)
Precipitation$_{it}$[1]	–	–	–	0.006*** (0.001)	−0.002 (0.001)	0.004 (0.004)
Capital Distance$_{i}$[1]	–	–	–	−0.008*** (0.001)	−0.001 (0.001)	–
Border Distance$_{i}$[1]	–	–	–	−0.006*** (0.001)	−0.002** (0.001)	–
Mountains$_{i}$	–	–	–	0.008*** (0.002)	0.008*** (0.002)	–
Polity2$_{jt}$	–	–	–	−0.001*** (0.0002)	−0.0003 (0.0004)	0.001 (0.001)
Polity2$^2_{jt}$	–	–	–	−0.0002*** (0.00004)	−0.0001 (0.0001)	−0.0001 (0.0003)
Country Area$_{j}$[1]	–	–	–	−0.009*** (0.002)	−0.003 (0.002)	–
Constant	0.042*** (0.004)	0.007 (0.034)	–	0.129*** (0.017)	0.031 (0.020)	–
FEs	Year	Country+year	Grid cell+year	Year	Country+year	Grid cell+year
Observations	77,042	77,042	77,042	68,074	68,074	69,935
R^2	0.003	0.070	0.190	0.063	0.105	0.198
Adjusted R^2	0.003	0.068	0.128	0.063	0.104	0.136

*p<0.1; **p<0.05; ***p<0.01.
Variable coefficients are reported with standard errors clustered by country in parentheses. Fixed effects by year, country, and geospatial level are not reported.
[1] Natural log

Figure 2: Change in *Concentrated Urban Infrastructure*$_{it-1}$ $\times$ *Price Shock*$_{jt-1}$'s Coefficient across Different Repression Severity Thresholds



Year FEs Country and year FEs