The Geography of Repression in Africa[*]

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Abstract

I ask how the location of a protest affects how forcefully governments crack down. This geography of repression provides insight into a larger strategic problem: under what conditions do leaders meet protests with violence? I argue that protests in rural areas pose a smaller threat and, thus, prompt less frequent intervention. However, when governments decide to repress rural protests, they are less concerned that lethal repression might incite a backlash, as there are fewer bystanders in more rural areas that can join the fray. I uncover two patterns consistent with this theory: (1) repression is 30% more frequent in response to social conflicts in urban areas; but (2), if the state does employ repression, it is 75% more likely to kill dissidents in rural areas. The empirical relationships I report cannot be explained by reporting bias, international sanctioning, proximity to past armed conflicts, or the presence of natural resources.

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In January 2004, university students in Nairobi, Kenya "went on a rampage," destroying property and demanding that the University of Agriculture Technology fire its vice chancellor and reopen (AFP 2004*b*). Nine months later 600 University of Nairobi students violently demonstrated in response to proposed fee hikes, blocking streets and stoning passing motorists (AFP 2004*a*). In both instances, the Kenyan police used tear gas to quell the riots. In June 2005, students and parents in a village outside of Garissa, Kenya (350 kilometers northeast of Nairobi) protested the seizing of school land for private development. In this case, the police response was more heavy-handed: police opened fire on these students and parents, killing at least one and wounding dozens of others (AFP 2005). All three conflicts occurred under the same regime and involved students speaking out against their schools' administration. If anything, the available reports suggest that the students in the capital were more numerous and violent. What then explains why police opened fire in the rural exurbs of Garissa but employed non-lethal means in Nairobi?

Most existing studies focus on cross-national differences in government repression and, thus, provide little insight into why the Kenyan authorities responded differently to these protests. This paper focuses instead on subnational variation and asks how the location of a protest affects whether and how forcefully governments crack down on the demonstration. In particular, I explain how and why governments respond differently to protests that occur in large cities like Nairobi versus more rural areas. This geography of repression provides insight into a broader strategic problem: when do leaders meet protests with violence? The theoretical and empirical analysis in this paper reveal conditions under which leaders' concerns about backlash temper their use of violence.

Building on research into urban bias in Africa (e.g., Bates 1981), I argue that leaders feel especially threatened by large protests in capitals and other major cities. Governments, thus, take a more active role in managing these threats, intervening in a larger proportion of urban demonstrations. Yet, when governments repress urban protests, they have to be wary that state violence does not touch off an angry response from bystanders. A large literature in sociology and political science observes that repression is a double-edged sword that can both suppress and inflame dissent (e.g., Goldstone and Tilly 2009). Governments' concerns about such backlash are likely to be heightened in denser urban settings, where there are more citizens that could join the fray if angered. As a result, when governments intervene in urban protests, they tend to use a "lighter" touch, opting for non-lethal forms of repression (e.g., tear gas, mass arrests) rather than opening fire on demonstrators. I formalize this dynamic in a simple game, in which the government weighs the demobilizing effect of severe forms of repression against the threat of backlash.

I find empirical support for this argument using recently compiled data on social conflicts (e.g., protests, riots, or strikes) in African countries between 1990 and 2012 (Hendrix and Salehyan 2012). I uncover two widespread patterns: (1) repression is thirty percent more frequent in response to social conflicts in urban areas; but (2), if the state does employ repression, it is seventy five percent more likely to kill demonstrators in more rural areas (defined as localities with fewer than 100,000 residents).[1] The first pattern is apparent in over seventy percent of countries in the sample; the second, in over ninety percent of countries.

Guided by the model, I look at two mechanisms that can help explain these patterns. First, protests in urban areas tend to involve more participants; I find evidence that these larger and, thus, more threatening demonstrations are more likely to be repressed. This positive relationship between protest size and the probability of repression exploits within-country variation and is robust to the inclusion of controls related to the head of state and grievance and tactics of demonstrators. Second, there are more bystanders in densely populated urban areas. I show that, when the state intervenes, the probability that it opts for lethal repression is significantly lower when the event occurs in more densely populated localities — a correlation that is robust to a battery of controls.

In the final section of the paper, I enumerate and rule out five plausible alternative explanations for why governments employ repression differently across their territory. First, one might be concerned about reporting bias — namely, that protests in rural areas are only covered if the government deploys lethal force. Yet, I show that the relationship between population density and the deployment of lethal repression holds after restricting the sample to cities; that lethally repressed protests in rural areas do not garner greater press attention; that including the distance between reporters' hubs and protest events does not attenuate the effect of population density; and, finally, that reporting bias would have to be much larger than existing studies suggest is plausible to explain away the patterns. Second, governments may avoid killing demonstrators in capital cities, where diplomats and donors' offices reside. However, I find no evidence that administrations heavily reliant on foreign aid, and thus vulnerable to international sanctions, curtail their use of lethal repression in urban areas. Third, rural protests in the periphery may occur near the sites of past armed conflicts.

This history of violent confrontations may predispose security forces to deploy lethal force in response to social conflicts in rural areas. I do not find that the number of armed conflicts or battle deaths occurring near a protest (within fifty kilometers and five years) affect the likelihood that the state uses lethal repression when it intervenes. Fourth, regimes may crack down more severely on protests that threaten the continued extraction of natural resources. Including additional covariates that capture the number of diamond mines or onshore oil fields near a protest site does not affect my results. Finally, the reader might be concerned that different types of protesters or security forces mobilize in cities versus smaller towns. The results, however, are robust to dropping the relatively small proportion of events involving either the military or armed groups. I also find that my results hold when I restrict attention to events involving the same types of protesters, e.g., students and teachers or "citizens" and "civilians". These results suggest that the identities of those doing the protesting or repressing across urban and rural areas do not explain the strong correlation between population density and the use of lethal repression.

This paper describes and rationalizes two widespread patterns in the use of repression across Africa. For scholars of African political economy, the theory and empirical evidence complement existing research on urban bias, which has focused on the allocation of public spending and not the deployment of state violence. Mobilization by city dwellers threatens leaders in these weakly institutionalized states — that we learned from Bates. I show that this heightened threat prompts a more pro-active response to protests in capitals and major cities. For scholars interested in understanding the motives for repression, this paper illustrates how greater potential for backlash restrains governments' use of lethal violence in cities. The literature on protest has struggled to find empirical evidence that repression backfires. This paper suggests one reason why: leaders more often deploy heavy-handed tactics where the risk of escalation is minimal.

1. Managing Public Dissent in Africa

Much of the prior literature on repression does not consider how the setting or scale of a protest affect leaders' calculus about whether to intervene. But all else equal, how should we expect a leader to respond to a protest in a major city versus a more rural locality? Unfortunately, most existing studies of repression focus on cross-national variation (see Davenport 2007; Cingranelli and Richards 2013). This work yields three notable findings. First, repression is costly and, thus, unlikely to be employed against a docile population (Lichbach

1984; Earl and Soule 2006). Second, democracies repress less, because they can peacefully incorporate opposition groups and oust abusive leaders (Davenport 1995).[2] Finally, more recent work by Hendrix and Salehyan (2016) shows that regimes with a history of internal divisions (e.g., coups) are less likely to employ repression, as these rulers may not want to empower their security forces. While important, these three findings do not consider sub-national variation in the use of repression and, thus, provide little guidance for predicting the actions of our hypothetical leader.

Recognizing this gap, Davenport (2007, 18) implores scholars to move beyond country-year analyses: "Such an approach is not only essential for gauging the robustness of the propositions developed in this [cross-national] literature but also allows us to explore other arguments that have previously been ignored." This call has been answered by a small if growing number of studies (e.g., Bohara, Mitchell and Nepal 2006; Murtagh, Spagat and Restrepo 2009; Vadlamannati 2008), none of which consider the role of geography.

1.1 Urban Bias in Africa

The work on urban bias offers more guidance. In his seminal work, Bates (1981) argues that African leaders feel especially threatened by protests in major cities. As such, these rulers manipulate policy to pacify city dwellers, generating an "urban bias" in the allocation of public spending — a differential noted in earlier work by Lipton (1977). Bates claims that governments depress food costs in cities to appease workers and, thus, avoid destabilizing social conflict in urban areas:

> "The issue that most frequently drives African city dwellers to militant action is the erosion of their purchasing power…Sadat, Nimeiri, Kaunda, Moi, Gowan, and Tolbert are among the other African leaders whose governments have felt the political pressures generated by the erosion of the purchasing power of urban dwellers; in the face of these pressures, several have fallen" (pp. 30-2).

This argument suggests that our hypothetical head of state may be especially concerned about limiting mobilization in cities (Hendrix and Salehyan 2016, 6). Complementary research by Roessler (2011) also contends that African leaders focus their attention on threats originating in their capitals and, thus, invest heavily in coup-proofing strategies rather than policing the periphery.[3]

Bates's work on urban bias not only speaks to how executives assess the threat of protests in urban versus rural areas, but also how they manage dissent in these places. To depress urban food prices, rural farmers must be paid below-market prices. Where farmers resist this expropriation, leaders respond with brutal force: "Through the use of violence, the governments of Africa have forestalled the mobilization of the rural majority against policies that harm their interests" (p. 112). Yet, when protests or strikes erupt in cities, Bates observes that these same leaders must proceed more cautiously: "Direct attacks on labor movements are open to reprisals; in moments of economic stress, labor movements can join with their urban constituents, paralyze cities, and create the conditions under which ambitious rivals can displace those in power" (p. 33).

In sum, social conflicts in cities pose greater threats to the leader's survival. It is this latent power that provides city dwellers with greater influence over policy. Yet, while leaders may want to take a more pro-active role in policing these urban protests, they can not handle city dwellers with the iron fist that they wield in rural areas. The risk that urban protests might escalate restrains rulers' use of violence.

1.2 Repression: A Demobilization Strategy with Downside Risks

The dilemma that Bates identifies — that leaders need to demobilize (urban) protests without inflaming them — is not unique to Africa. Past research recognizes the repression can be an effective tool for managing dissent, as it imposes a cost on demonstrators (Balbus 1973; Lichbach 1984; Opp and Roehl 1990; Pierskalla 2010; Magaloni et al. 2016). Moreover, repression can signal the regime's willingness to fend off present and future challengers (Pierskalla 2010; Walter 2006).

If, as this research suggests, repression can pacify or discourage protests, why would our hypothetical leader not employ it uniformly? First, it may be costly: repression requires that police or military be deployed, and governments face budget constraints. Scholars have also argued that governments pay indirect costs for violating international norms against human rights abuses (Hafner-Burton 2005*b*; Hendrix and Wong 2012). A leader may not want to bear these costs to repress a protest in a remote area that poses no real threat.

A more common explanation for restraint contends that repression can backfire and actually inflame dissent.[4] Goldstone and Tilly (2009, 181) summarize a number of case studies, which suggest that repression was counter-productive (from the government's perspective):

"Khawaja's (1993) study of Palestinian protest in the West Bank, Rasler's (1996) study of Iranian protests in 1977-79, Francisco's (1996) study of protest in Germany, and Olivier's (1990) study of Black protest in South Africa all find, as the latter clearly states, that 'the effect of repression on the rate [of collective action] is not negative! Repression led to a significant increase in the rate of collective action.'"[5]

Rasler (1996, 142-7), in her analysis of protests prior to the Iranian Revolution, suggests that murders of demonstrators (i.e., lethal repression) "represent important turning points in collective action…these events propel large numbers of people into collective action." The deaths of earlier protesters were acknowledged in "mourning ceremonies," and "these observances produced violent clashes between security forces and the public and generated new deaths and a new cycle of mourning throughout the country." In Rasler's account, lethal repression incited other citizens to publicly oppose the Shah, and this escalation contributed to his eventual ousting.

Opp and Roehl (1990, 524) summarize several reasons why repression might provoke this adversarial response among bystanders: First, "repression may thus be regarded as immoral, and individuals who are exposed to repression or who know about it may feel a moral obligation to support a movement's cause and even to regard violence as justified." Consistent with this mechanism, research has found that repression that harms the innocent or is aimed at groups that the public considers legitimate is more likely to spark backlash (see Goldstone 2001, for a review). Second, "repression may cause system alienation, i.e., discontent with a society's political institutions, which will in turn lead to more protest if persons believe they can change these conditions by means of protest."

Returning to our hypothetical head of state, how might the location of a protest affect their concerns about backlash? Siegel (2011, 105) argues that if the targets of repression are isolated, then any outrage or alienation that follows repression remains confined to their locality: "anger has little aggregate effect when network structure doesn't allow it to spread. However, once there is a sufficient number of weak ties, anger-driven participation can spread throughout the network rapidly enough to overwhelm repression and trigger a backlash" (p. 105). His model suggests that governments should worry less about backlash in sparsely populated areas where there are fewer bystanders (i.e., individuals in Siegel's model with weak-ties to demonstrators), who might be incited to act upon witnessing repression. Christensen and Garfias (2016) make a similar argument about the role that cell phones and social media play in creating ties that enable escalation.

2. A Theory of Repression and Escalation

To synthesize this past work, repression offers the government a tool for suppressing public dissent, but also carries the risk of revealing that a government cares little about its citizens — a reputation that can inflame dissent. Given these benefits and costs, I argue that leaders' responses to social conflicts depend (in part) on where a protest takes place. In particular, larger protests pose a greater threat to the executive, prompting more frequent repression. However, in densely populated areas the government has greater concerns about the reactions of bystanders to brutal forms of repression, as police brutality could incite more people to take to the streets. Hence, we are more likely to observe a police response in urban areas, where larger demonstrations or riots tend to take place. Yet, when leaders do repress urban protesters, they more often opt for non-lethal repression, fearing that lethal force might provoke a backlash.

I formalize this argument in the appendix (section A) but focus here on the intuition behind the relevant comparative statics. First, as the initial size of the protest declines, executives (particularly more benevolent types) see less value in deploying repression, given its downside risk. In the limit, as the initial size of the protest becomes negligible, all leaders prefer simply to ignore these small protests and avoid any risk of angering bystanders. This motivates the first hypothesis:

H1: *Increasing the initial size of the demonstration increases the probability of repression.*

Second, as we decrease the number of bystanders, this reduces the risk of backlash and, thus, makes more brutal forms of repression appealing. As the audience for repression shrinks to zero, all executives opt for more severe repression, as this demobilizes the vanguard without risking escalation. This suggests a second hypothesis:

H2: *Increasing the population of bystanders reduces the severity of repression should it be employed.*

Given my interest in how citizens respond to repression, I define bystanders as those individuals who observe, but do not participate in, the initial protest (akin to Braun and Koopmans 2014, p. 3).[6] Without knowing what each individual observes, we can not precisely measure the size of this audience for each event. Looking ahead to the empirics, I use the population density in each protest location as a proxy. This assumes that the number of bystanders is larger when protests occur in densely populated cities — an assumption consistent with a sizable literature on the importance of proximity and face-to-face interaction in spreading information and mobilizing individuals to participate in collective action (see Weidmann 2007, for a review).[7]

2.1 Illustrative Case: Food Riots in Cameroon

A good case for evaluating the realism of the model involves an executive facing protests that are motivated by similar grievances, but differ in their initial size or take place in localities of varying density. From 25-29 February 2008, riots broke out in 31 localities across Cameroon. These riots were all motivated marked increases in the costs of food and fuel, as well as an attempt by President Paul Biya to extend his rule through a constitutional amendment (AFP 2008). The riots erupted spontaneously, and the composition of rioters does not appear to have differed across localities:

> "Many youths spontaneously descended to the streets to express their disillusionment and loss of hope for a better future. The strike then became a widespread movement. No political organization or trade union instigated the protesters. It was all spontaneous. The cities were totally paralyzed. Peaceful demonstrations could be seen everywhere" (National Human Rights Observer N.d., 10).

Rioting was concentrated in the five northwestern regions of Cameroon, with an additional riot occurring the capital, Yaounde.[8] The concentration of riots in the northwestern regions reduces concerns about ethnic heterogeneity driving variation in the use of repression: while these regions contain four major ethnic groups (the Baileke, Duala, Tikar, and Tiv), none of these groups share ties with the Bulu, who comprise much of the country's political and military elite, including the President (Cederman, Min and Wimmer 2010; Group 2007).

Critically, the 31 localities varied in terms of their population density, allowing us to explore how Biya's use of repression varied across urban and rural areas. In Yaounde and Douala, the country's largest cities and political and commercial capitals respectively, Biya rapidly deployed police and other security forces to repress protesters. Initially, these forces deployed non-lethal means – arrests, batons, tear gas, and water canons. Remarkably, only two deaths were reported in Yaounde, a police officer and another individual whose cause of death is unknown (National Human Rights Observer N.d.). In Douala, police did resort to using lethal force, but reports suggest that this change in tactics occurred only after officers on the front lines lost control of the situation. By contrast, in more rural areas police did not bother to use these same non-lethal means: in Bafou looting was punished with "summary executions in the plantations"; in Loum, "many rioters were shot and killed, including six young people" (p. 12-13). Biya's responses to these riots are largely consistent with the predictions of the model: riots in urban centers provoked an immediate, initially non-lethal response; in the countryside, reports suggest that repression was more erratic, but also more likely to involve the use of live ammunition than the tear gas or water canons deployed in the larger cities.[9]

As the model suggests, Biya's attempts to avoid killing demonstrators in major cities may have been motivated by a concern that lethal repression could signal disregard for rioters' legitimate concerns about rising food and fuel prices and, thus, expand protests in densely populated areas. However, Biya's actions are also consistent with an alternative explanation — namely that Biya ordered that non-lethal means be used in Douala and Yaounde to avoid raising the ire of international observers, who are concentrated in these cities. The International Crisis Group notes that Biya's regime increasingly relies on mass arrests or "judicial repression, which attracts less criticism from international human rights organizations and the international community" (International Crisis Group 2010, 14). This short case study suggests that the models' predictions are consistent with the actions taken by an executive that faced multiple protests prompted by the same grievances but staged in different locations. The analysis below offers further confirmatory evidence and helps to rule out alternative explanations, including monitoring by international observers.

3. Empirical Strategy

This is a descriptive paper without causal claims; I offer a set of conditional correlations consistent with my hypotheses. Nevertheless, I endeavor to make the tightest possible comparisons by incorporating all available information about protest events and the context in which they occur. By comparing the use of repression in response to social conflicts that happen in the same country, under the same ruler, motivated by the same issue, organized by participants using similar tactics, the goal is to isolate how the initial size of a protest or the population density in a protest location (my proxy for the number of bystanders) affect the government's use of repression.

To make such comparisons, I estimate the following linear-probability model:

$$y_{ict} = \alpha_c + \delta_t + \boldsymbol{\beta} \boldsymbol{Z}_{ict} + \boldsymbol{\zeta} \boldsymbol{W}_{ct} + \varepsilon_{ict} \tag{1}$$

where i indexes the event; c, either the country or ethnic homeland; and t, year. $\boldsymbol{Z}_{ict}$, a matrix of event-specific characteristics (e.g., the issue motivating an event); $\boldsymbol{W}_{ct}$, a matrix of country-year characteristics, such as the contemporaneous head of state. Depending on the model, the dependent variable (y_{ict}) is an indicator for whether the event was repressed or repressed lethally in cases where some repression occurred.

Putting aside functional form assumptions, any strategy that relies on matching — or "automated matching," to borrow the synonym for regression used by (Angrist and Pischke 2014) — is especially vulnerable to concerns about omitted variables. Section 6 on alternative explanations enumerates and includes a set of additional observables (e.g., distance to AP reporting hubs, international aid flows) that might be related both to an event's location and the state's use of repression.

Inferences are based on robust standard errors clustered on country. This accounts for spatial and temporal dependence in the errors for events that occur within the same country, which is arguably the largest relevant political unit.

4. Data

4.1 The Social Conflict in Africa Database

The data on repression comes from the Social Conflict in Africa Database (SCAD) compiled by Hendrix and Salehyan (2012), which covers all African countries (with a population exceeding 1 million) between 1990 and 2012. The data is hand-coded from *Associated Press* (AP) and *Agence France Presse* (AFP) news wires. If a news wire contains information on multiple events, each distinct event enters separately; if the same event is covered by multiple wires, it only enters the data once (though information is recorded about whether both the AP and AFP covered the event).

The SCAD focuses on low-level social conflicts (e.g., protests, riots, and strikes) and, thus, excludes events that occur in the context of armed conflicts (defined by the Uppsala Conflict Data Program's threshold of 25 battle deaths per year). When using events' location, I also exclude conflicts that cannot be geo-located to a specific place name (e.g., nationwide or regional events) or where the geo-coding is not considered precise.[10] Finally, if related protests occur in multiple locations, each location enters the raw data as a separate row. However, coders do not record unique information about the number of participants or repression for each location. Per the authors' advice, I retain a single entry for each event, not each location.

The analysis below relies on additional information provided about each event. Critically, for every event, coders note whether the government used violent repression against participants and, if so, whether this repression was lethal (i.e., resulted in at least one participant's death.) The SCAD is unique in this regard: most other event datasets (e.g., ACLED, GDELT, and ICEWS) do not contain information about the government's response to social conflicts. Second, all retained events include information about whether the event occurred in an urban area (i.e., the capital or a city of over 100,000 people). Third, for just under half of all events, coders record the number of participants using the following categories: $[0, 10)$, $[10, 100]$, $(100, 10^3]$, $(10^3, 10^4]$, $(10^4, 10^5]$, $(10^5, 10^6]$, $(10^6, \infty)$. Third, in the analysis below, I frequently include a set of "event controls." These controls are a set of indicator variables that capture the issue under dispute (i.e., economic, political, ethnic/religious, or other), whether the central or regional government was targeted, whether the participants employed violent tactics, and whether the event was organized or spontaneous.

To provide a sense for the social conflicts contained in the dataset, I provide short descriptions of three events. As readers are likely familiar with urban protests, these examples are drawn from rural Kenya and vary in the use of repression:

(1) Roughly 100 protesters blocked the drilling of four geothermal wells in the Rift Valley in May 2010, complaining that existing wells contaminated local water sources and produced a hissing noise that made it difficult to sleep. There are no reports of police intervention, and officials working on the geothermal project indicated that they were working on an agreement to resettle the affected communities. This

protest exemplifies small scale conflicts between larger companies involved in agribusiness or the extractives sector and communities affected by these operations (Christensen 2016).

(2) Several thousand agricultural workers at tea plantations in Western Kenya went on strike in September 1998 demanding higher wages. Newspaper reports suggest that most of the striking workers were employed by major tea exporting companies and were not union members. Four days into the strike, a plantation official was attacked and seriously wounded by striking workers. This prompted an armed intervention by police, which did not claim any lives but left twenty people injured. The strike ended the day after this clash between police and striking workers. While strikes in rural areas are not a regular occurrence it is more common to see actions initiated by farmers, protesting low prices or disadvantageous trade policy.

(3) An estimated three thousand farmers from the Kirinyaga district in Central Kenya rioted in January 1999, accusing the government of buying rice at below production costs. The farmers refused to deliver their harvest, leaving large quantities in the fields. One person was killed and two others hospitalized with bullet wounds when police fired on the farmers, who, by some reports, were armed with stones, petrol bombs, and bows and arrows. Repression followed by the arrest of the MP, Chairman of the local cooperative society, and several farmers appears to have quelled the riot.

While these events are taken from a single country in the sample, they are indicative of protest events that occur outside of major urban areas.

4.2 Other Covariates

Knowing where and when each event occurred, I can merge in additional information about the context in which the conflict took place. First, I extract information on the population density in the event location using the 2011 LandScan Global Population Database, which provides global raster data at the 1 km resolution (Oak Ridge National Laboratory 2012).[11] Second, I use the geo-referenced Ethnic Power Relations data to determine the ethnic homeland that each event occurred in (Cederman, Min and Wimmer 2010).[12] Third, using the (ending) date and country of each event, I code the associated head of state by extending the Archigos dataset to the present for all countries in the sample (Gleditsch and Chiozza 2009).

In evaluating alternative explanations, I use the country and year of the event to integrate information about official development assistance (i.e., foreign aid) from the World Development Indicators (The World Bank 2012). Second, I determine the number of armed conflicts and battle deaths that occurred within 50 kilometers of, and less than five years prior to, each social conflict using the geo-referenced UCDP data (Melander and Sundberg 2012*a*). Third, I determine the number of known diamond mines and onshore oil wells that fall within a fifty kilometers of an event using data from Lujala, Rod and Thieme (2007) and Gilmore et al. (2005), respectively.

5. Results

5.1 Descriptive Statistics: Repression of Urban and Rural Protests

Looking simply at the use of repression across urban and rural areas reveals two patterns. First, repression is more frequent in response to social conflicts in urban areas. But, second, if the state does employ repression, it more frequently kills dissidents in rural areas. (Note that protests in "rural" areas more often take place in large towns or small cities, than in villages.) The proportions reported in table 1 imply that protests in urban areas are seven percentage points (or over thirty percent) more likely to be met with repression.[13] Moreover, this heightened responsiveness to urban protests is true of most countries in the sample. Figure 1 shows that, among countries with more than ten events in both urban and rural areas, over seventy percent repress urban protests at a higher rate.

[Table 1 about here.]

[Figure 1 about here.]

Even more striking, when states do employ repression, they are seventy five percent more likely to employ lethal repression in rural areas. That is, when the state intervenes in social conflicts in rural areas it is nearly twice as likely to kill a demonstrator. Again, looking across all countries in the sample with more than ten protests in both urban and rural areas, the probability of lethal repression is higher in rural areas in nearly 90% of cases.

Nearly 65% of countries conform to both of these patterns (the gray rectangle in figure 1), suggesting that the Cameroonian case is not an outlier. Moreover, these proportions align with the hypotheses presented above. First, protests in urban areas tend to be larger: 41 percent of social conflicts in urban areas exceed 1,000 participants compared to 26 percent of rural areas; 10 percent of urban events exceed 10,000 participants compared to just 2 percent of rural conflicts. Per the first hypothesis, I expect these larger events to be repressed at a higher rate. Second, urban areas, by definition, contain more bystanders. Thus, when the state intervenes in protests located urban settings it opts to use non-lethal tactics, consistent with heightened concerns about backlash in more densely populated cities (H2).

5.2 Regression Analysis

To better assess the hypotheses' all-else-equal claims, I employ multiple regression to partial out ("control for") predictors that do not relate to the size of the vanguard or the number of bystanders.

Turning to the first hypothesis — that the probability of repression increases with the initial size of a social conflict — I employ two measures. First, I use data from the SCAD on the number of participants involved in a social conflict. What figure 2 (based on the estimates in table 2, model 2) reveals is that the likelihood of repression increases sharply with the number of participants involved in a social conflict. Relative to an event involving fewer than ten demonstrators (the omitted category), a demonstration with between 10 and 100 participants is roughly 20 percentage points more likely to be repressed. The probability of repression increases almost monotonically with the number of participants. Model 2 includes country and year fixed effects, as well as a full set of dummies for each head of state. I also enter event controls that include the issue that motivated the event, whether the participants used violence, whether the event targeted the government, and whether the event was organized or spontaneous. This set of covariates addresses concerns about time-invariant characteristics of countries or leaders, global time trends, and some event or participant characteristics that could influence the use of repression. As noted above, the standard errors are clustered on country.

[Figure 2 about here.]

This measure is imperfect: it may capture the final (rather than the initial) size of protests, and it is missing for nearly half of the events. A second approach uses logged population density as a proxy for the size of the vanguard. Given the lower costs of collective action and a larger population of potential vanguard members in cities, I expect protests in densely populated areas to be larger than those in more sparsely populated localities — a prediction that's borne out in the SCAD data. As we would expect if population density proxies for the initial size of protests, the coefficient on this variable suggests a positive relationship, robust to a similar set of controls.[14]

[Table 2 about here.]

[Table 3 about here.]

In these regressions (models 3 and 4), population density not only proxies for the initial size of protests, but also contains information about the number of bystanders where a social conflict occurs. To evaluate the model's comparative statics, we need to simultaneously include variables related to an event's size and the the number of bystanders. Column 5 includes both the number of participants and population density. The results conform to the theory: holding the number of bystanders constant, small protests are less likely to be repressed; holding the size of the protest constant, increasing the number of bystanders reduces the likelihood of repression (though this last result does not achieve conventional levels of significance).[15]

[Table 4 about here.]

[Table 5 about here.]

To evaluate the second hypothesis about the severity of repression in areas with fewer bystanders, I restrict attention to events involving some form of repression. I regress an indicator for whether the state employed lethal repression on the (logged) population density where an event took place, my measure for the number of bystanders. Consistent with my second hypothesis, I find that states are significantly less likely to employ lethal repression in densely populated areas. This relationship is very robust: model 4, for example, include year, leader, and ethnic group fixed effects, as well as the event controls mentioned above (i.e., issue, target, tactic, spontaneity). In model 5, I restrict attention the sample to just urban areas and still find that social conflicts in more sparsely populated cities are more likely to be met with lethal repression (when repression is employed). Including categories for the number of participants only amplifies the coefficient on population density.

To interpret the magnitude of the effects, moving from the 10th to 90th percentile of (logged) population density (i.e., from 5.9 to 10.9) reduces the probability of lethal repression by 14 percentage points or over half of the mean of the dependent variable (or roughly a third of a standard deviation).[16] As another point of comparison, the probability that the South African state employed lethal repression (when it intervened) declined by roughly ten percentage points with its transition to democracy (from 1990-4, Pr(Lethal | Repression) = 0.47; from 1995-2000, Pr(Lethal | Repression) = 0.38). The magnitudes of the coefficients reported in table 4 are meaningful relative to institutional changes that affect the mix of repressive tactics employed by governments.

6. Alternative Explanations

6.1 Reporting Bias

Suppose that news wires fail to report on social conflicts in rural areas where the state does not employ lethal repression. International reporters are often based in major cities, and they may be unaware of or unwilling to cover social conflicts that occur in remote areas if these events do not involve deadly repression. If true (and pronounced), this type of reporting bias could lead me to overstate governments' inclinations to employ lethal repression outside of cities. (Note that if reporters focus on more violent events in both urban and rural areas it would not confound the results.) I adopt a number of strategies to assess the plausibility of this alternative explanation.

First, the final model in table 4 restricts the sample to urban areas (i.e., capitals and cities with more than 100,000 residents). We might expect that, across these locations, reporting bias is limited. Nonetheless, I still find that governments, when they intervene, are more inclined to use lethal repression in response to social conflicts in more sparsely populated cities.

Second, recall that the SCAD is based on two news sources, the AP and AFP. For each event, the dataset contains information about whether it was covered by one or both of these sources. The reporting bias story presumes that reporters are more inclined to cover rural events involving lethal repression. If that is true, then we might expect (1) that events involving lethal repression are more likely to be covered by both news sources (as this state violence makes events more "news-worthy"), *and* (2) that this is especially true of rural areas. However, looking at table 6, we see that lethally repressed protests in rural areas are only three percentage points more likely to be covered by both news sources than rural events involving non-lethal repression. Compare that to urban areas: the probability that both sources cover an urban social conflict jumps 19 percentage points if the government uses lethal (as opposed to non-lethal) repression. This table flips the reporting bias story on its head, indicating that urban events attract much more news attention if they involve deadly clashes between demonstrators and police. In table 7, I regress an indicator for whether the event was covered by both news sources on indicators for whether the social conflict occurred in an urban area, was lethally repressed, and the interaction. (The sample here is restricted to events involving some repression as in table 4.) Again, the coefficients suggest that the use of lethal repression has no effect on whether a rural event is covered by both sources; however, events in urban areas are much more likely to garner the attention of multiple sources if lethal repression occurs. These findings are robust to the inclusion of country, year, and leader fixed effects, as well as the set of event controls employed above.

Third, the reporting bias story suggests that reporters are unwilling to trek out to remote, rural areas unless lethal repression occurs. Hence, if we knew where reporters were stationed, we would find that more distant events are more likely to involve lethal repression. Moreover, controlling for this distance, the local population density would be irrelevant. Unfortunately, the Associated Press has not maintained a history of where its reporters were stationed in Africa over the past two decades. However, I pull down every story filed by the AP wire in Africa between 1990 and 2011. Extracting the datelines of these stories, I determine how many wires AP reporters filed in every city in every year.[17] I then geo-code those place names. This dataset allows me to calculate the distance between each SCAD event's location and (1) the closest place where an AP reporter filed a story in the previous year, and (2) the closest reporting hub (defined as a location where AP reporters filed an average of at least five stories in each of the previous three years). While I find that events further from filing locations and reporting hubs are slightly more likely to involve lethal repression,[18] including these variables does not significantly change the coefficient estimates reported in table 4 (see table 8).

Finally, we can determine how large any reporting bias would have to be to account for the observed difference in the use of lethal repression reported in the final column of table 1.[19] This bounding exercise reveals that, to explain away the difference, news wires would have to never miss a social conflict in urban areas and miss over 55 percent of all events in rural areas where the state uses non-lethal repression. This level of underreporting dramatically exceeds the reporting bias that Weidmann (2015) detects in conflict data from Afghanistan, an active war zone.[20] It also greatly exceeds recent estimates of reporting bias for the SCAD data computed by Hendrix and Salehyan (2015, 7), who find that a death during an event only increases the likelihood of reporting by 17 percent. While it is impossible to definitively rule out reporting bias, I find nothing to suggest that it is occurring, especially on the order necessary to explain the large observed difference in governments' propensities to employ lethal repression when they intervene in rural social conflicts.

6.2 International Sanctioning

INGO workers and foreign diplomats spend much of their time in major cities and, thus, may receive less information about what is happening in the countryside. If true and governments believe that these actors might sanction lethal repression, then this uneven monitoring across urban and rural areas could contribute to a lower probability of repression, especially lethal repression in urban areas. Some reports on Cameroon suggested that Biya's use of mass arrests in Yaounde and Douala might have reflected concerns about how international observers react to more brutal repression.

Unfortunately, I am not able to measure variation in international monitoring across countries' territories. I rely instead on a indicator of the potential costs of international sanctioning, foreign assistance as a percentage of gross national income or government revenue from the World Development Indicators. The potential costs of international sanctions in response to (lethal) repression should be higher in those countries where foreign aid flows comprise a larger share of the economy or government budget.

First, I find no evidence that increased foreign aid reduces the likelihood that repression is employed in response to social conflicts. Models 1 and 2 from table 10 simply regress an indicator for repression on aid flows (as a percentage of GDP or government revenue), including country and year fixed effects. Neither model returns the negative relationship we would have expected based on this alternative explanation. Second, dependence on foreign aid does not appear to condition governments' decisions about whether to employ lethal repression in urban areas. If increased aid reduced the use of lethal repression in dense cities, then we would expect the interaction of population density and aid flows to be negative in models 3 and 4; these coefficients are both positive but effectively zero.

These results suggest that aid dependence does not affect the use of lethal repression across protests in cities and more sparsely populated localities. However, this does not imply that aid is an ineffective "carrot." Suppose that foreign aid reduces the likelihood of lethal repression, but does so equally across countries' territories. In this hypothetical, aid deters lethal repression; yet, it does not affect the relationship (i.e., slope) between population density and deadly crackdowns on protest. What my findings do show that lethal repression more often occurs outside of major cities. If domestic or international NGOs want to document and deter repression, then they should expand their reach beyond capital cities. Furthermore, efforts to train and equip police to deescalate protests or riots should not be confined city patrols.

6.3 History of Armed Conflict

The SCAD excludes events associated with armed conflicts. However, it could be that rural protests are more likely to occur in remote localities with histories of peripheral rebellion. The government's propensity to deploy lethal force in response to these rural events could then be explained by a history of armed confrontations between the state and insurgent groups.

To address this possibility, I determine the number of prior armed conflict events and battle deaths that occur within fifty kilometers of every protest location using the UCDP's georeferenced event dataset (Melander and Sundberg 2012*a*).[21] I only count events and battle deaths that occur in the five years before each protest event. While fifty kilometers and five years are arbitrary cutoffs, the results are robust to alternative choices (e.g., 100 or 200 kilometer radii).

The results in table 12 do not support this alternative explanation. The incidence of conflict, number of conflict events, and number of battle deaths near a protest location do not affect the likelihood that governments employ lethal repression when it intervenes. Moreover, the inclusion of these variables does not affect the relationship between population density and the use of lethal repression reported above.

6.4 Proximity to Natural Resources

Perhaps it is not a history of armed conflict, but rather the presence of natural resources that affects whether and how forcefully a government represses social conflicts. Regimes may respond particularly swiftly or harshly to protests or riots that threaten the continued extraction of valuable natural resources. We might then expect social conflicts occurring near these resources to be repressed more frequently or lethally.

Walter (2006), however, provides good theoretical reasons to be skeptical of this claim: even if regimes are especially concerned about conflict in resource-rich regions, they do not accommodate challengers in resource-poor regions for fear of developing a reputation for weakness and emboldening potential future challengers. Nonetheless, I determine the number of diamond occurrences (i.e., sites of production or confirmed discovery) and onshore oil and gas fields within fifty kilometers of each protest location. The data on diamond mines and oil wells comes from Gilmore et al. (2005) and Lujala, Rod and Thieme (2007), respectively.[22]

There is some indication that social conflicts within fifty kilometers of onshore oil fields are more likely to be repressed; yet, this relationship attenuates and loses significance with the inclusion of leader fixed effects and event controls (see table 14). Including these additional covariates does not affect my earlier findings regarding governments' propensity to employ lethal repression in more sparsely populated areas when it does repress.

6.5 Identities of Protesters and Repressers

In rural areas, the government may rely more on the military to repress social conflicts. By virtue of their training or orders, these security forces may be more inclined to deploy lethal repression than police. Reviewing the 919 actor and target codes in my sample, I identify 75 events involving the military.[23] Dropping these social conflicts does not affect the results reported in table 4, model 1.

Security forces — be they police or military — may more often confront insurgent groups in rural areas. If true, and confrontations with these groups are more likely to turn deadly, then this could explain why lethal repression is a more likely outcome when the state intervenes in rural areas. Again, I use the actor and target codes to identify 109 events involving insurgent or armed groups. Dropping these events is inconsequential. (The results are robust to dropping events involving the military and/or rebel groups.)

In cities, police forces may be concerned about firing live rounds into a crowd that might include bureaucrats or students, occupational groups that may be underrepresented in rural social conflicts. First, table 4, model 5 shows that the result is robust to limiting the sample to urban areas, alleviating some of this concern. Second, I can drop all events that take place in capital cities, where bureaucrats (and many economic elites) are concentrated. Third, I use the actor and target codes to identify subsets of social conflicts that involve similar participants. I find similar and significant results when I restrict my sample to only those 433 events involving students and teachers or, alternatively, to the 384 events involving "citizens," "civilians," and "women." Taken together, these results suggest that variation in the identities of those protesting or repressing across urban and rural areas do not explain the strong correlation between density and the use of lethal repression. (See table 16 for the results referenced in this sub-section.)

7. Conclusion

Past research focuses on features of regimes that make them more or less inclined to violently suppress dissent (e.g., autocracy or autarky). Yet, scholars of African politics have long observed that public spending and state power are not uniformly administered within a country's borders under the same leader (e.g., Bates 1981; Herbst 2000). Just as states might spend or tax disproportionately in some regions (e.g., generating urban bias), they may also deploy repression differently in response to public challenges.

Exploiting event-level data, I explore this subnational variation and discover two patterns: first, protests in urban areas are more likely to be repressed; but, second, when the state intervenes, it is more likely to employ lethal repression in rural areas. These patterns are consistent with governments feeling more threatened by larger urban protests — and, hence, intervening more often — and, yet, also being more concerned that severe repression could spark a sizable backlash in densely populated areas with more bystanders, leading them to opt for non-lethal force when they crack down in cities. This model of government decision-making implies that protests with more initial participants should be repressed at a higher rate, and that lethal force should be more common when the state intervenes in more sparsely populated areas. I find empirical support for both predictions.

While this paper is descriptive, I isolate the relationships between protest size or population density and the use of repression by including covariates related to characteristics of the event, as well as the regime in power. I also compile data to rule out alternative explanations related to reporting bias, sanctioning by international actors, proximity to past armed conflicts, the local presence of natural resources, and the identities of both those protesting and responding to protests.

This paper extends existing research on urban bias in Africa by revealing how (and why) regimes manage protests differently in cities and more rural areas. Moreover, it contributes to a larger literature on the conditions under which repression inflames the opposition. Past work has struggled to find convincing evidence that repression sparks a backlash. If, as I argue in this paper, governments more often employ severe repression where the potential for backlash is limited (e.g., in rural areas), then we would not observe a positive correlation between repression and escalation, even if the true causal relationship is strongly positive.

Notes

[1]Social conflicts include events, such as riots, strikes, and protests that do not occur during a civil conflict, which is defined by the Uppsala Conflict Database as a conflict over territory or government with more than 25 battle deaths per year.

[2]A number of works associate economic variables, such as development, inequality, and openness with levels of repression. However, empirical work has frequently yielded conflicting results: Hafner-Burton (2005*a*), for example, demonstrates that the correlation between openness and repression is highly measure and model dependent.

[3]Arriola (2012) finds that, *even within the national capital*, Ethiopian leaders appear particularly threatened by protests near the executive office. This suggests that geography affects the use of repression at even smaller scales.

[4]Quantitative analysis using either cross-sectional or time-series data on repression and protest has not provided convincing evidence in support of backlash. However, these mixed findings could, in part, be attributable to a selection problem: governments are strategic actors whose decisions to employ repression incorporate the probability of backlash. We may then observe repression primarily in contexts where backlash is less likely and, thus, underestimate the potential for repression to inflame dissent.

[5]In forthcoming work, Lawrence (N.d.) provides more recent evidence from Morocco that information about police brutality increased support for the movement's vanguard.

[6]Other studies of protest and escalation define this group more broadly, sometimes including all citizens not involved in the initial protest (e.g., Shadmehr and Boleslavsky 2015).

[7]Proximity is not the only determinant of whether an individual observes a protest. Christensen and Garfias (2016) demonstrate the role that communication technology plays in expanding the audience for protests and repression.

[8]The NRHO report contains a reference to Muea, a town much further east than the other sites. Given the ambiguous place name, I do not feel confident about this riot's location and exclude the event.

[9]These different police responses may also reflect disparities in the resources or oversight provided to forces operating in cities versus more rural areas. However, differential funding and oversight are not alternative explanations, so much as additional outcomes that are consistent with strategic logic outlined above: if leaders are less concerned about brutal tactics prompting backlash in rural areas, then they should not deploy water canons to these localities or concern themselves with closely monitoring police conduct.

[10]Version 3.1 of the SCAD includes several events in Johannesburg, where the latitude and longitude were inadvertently reversed.

[11]A bilinear interpolation uses the average of the four closest raster cells to calculate the local population density in each event location. The decision to interpolate is not consequential for any results.

[12]Readers familiar with this dataset know that the polygons of ethnic homelands frequently overlap. First, I only use homelands from the study period, excluding historical polygons. Second, if an event falls in the intersection of multiple contemporary homelands, I either assign the largest non-aggregate group or, if no non-aggregate group exists, I assign a hybrid group name (e.g., Kikuyu-Meru-Embu for some locations in Central Kenya).

[13]Hendrix and Salehyan (2016) also report that urban protests are more likely to be repressed.

[14]Substituting ethnic homeland for the country fixed effects does not change any results.

[15]Many of the protests with over 10,000 participants occur across an entire region or "nationwide" and, thus, can not be assigned the population density of a single locality. Hence, model 5 drops many of the observations that fall in the upper bins of the number of participants variable.

[16]To offer concrete examples of localities at roughly these quantiles: Donga Town in Eastern Nigeria (still a smallish city) falls at roughly the 10th percentile; Kano City, with over two million residents, falls at the 90th percentile.

[17]Unfortunately, AFP wires do not consistently report filing locations.

[18]This positive effect may have nothing to do with reporting bias. Regimes may be less concerned about backlash from lethal repression when conflicts take place far from major population centers and capital cities, where reporters most often file their stories.

[19]The assumptions used to construct the bounds are described in appendix B.1.

[20]Moreover, if events involving no repression were also underreported in rural areas at the same rate, this would imply that roughly the same number of social conflicts occurred in urban and rural Africa and, thus, that population and protest incidence are unrelated.

[21]The UCDP defines an event as, "The incidence of the use of armed force by an organised (sic) actor against another organized actor, or against civilians, resulting in at least 1 direct death in either the best, low or high estimate categories at a specific location and for a specific temporal duration." To be included, the event must be part of a conflict between two actors that crosses the 25 death threshold in any year of the UCDP data (Melander and Sundberg 2012*b*, 8). Given the limited temporal scope of the geocoded data, it is not possible to evaluate the effects of armed conflicts pre-1989.

[22]While the diamonds dataset is comprise of geolocated points, the oil and gas fields are mapped as polygons. I use the centroids of the oil and gas polygons to compute distances.

[23]Note that due to missingness on other variables, some of these events may not have been in the sample.

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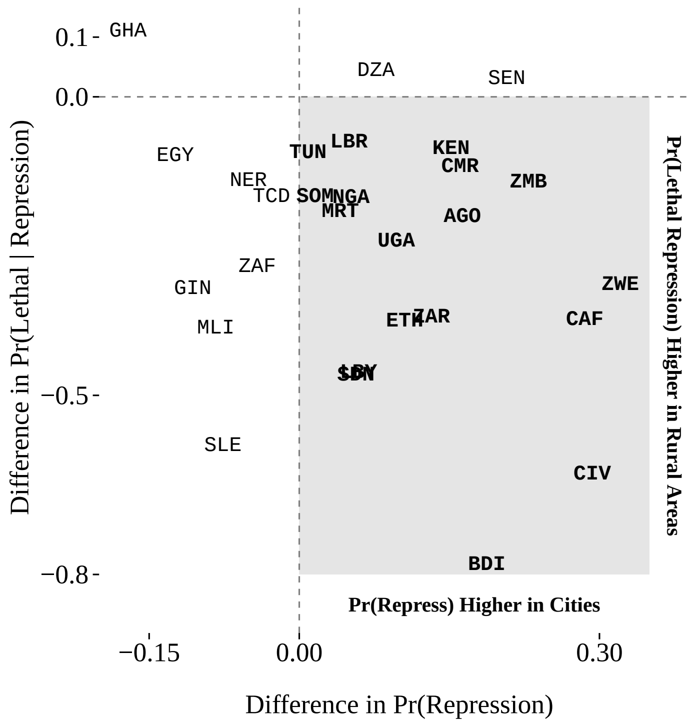
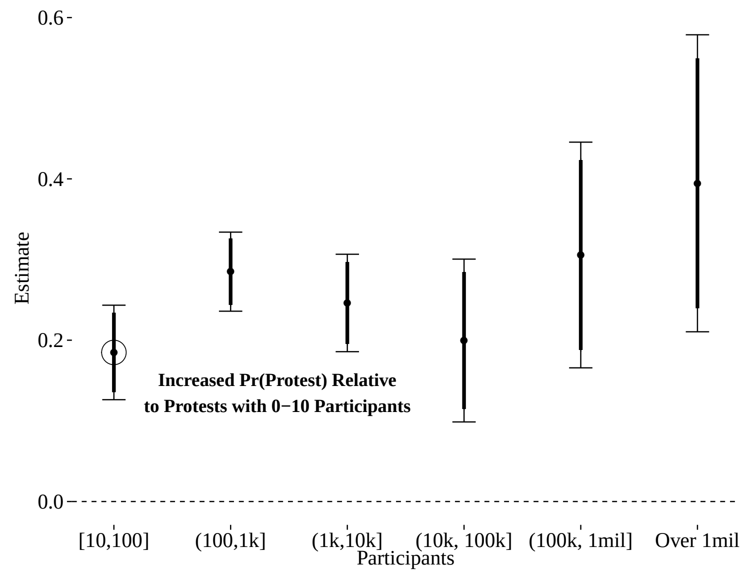
List of Figures

Figure 1: Patterns of Repression Across Countries
Patterns noted in table 1 hold across most African countries.



For all countries in the sample with ten or more protests in both urban and rural areas, I plot, on the x-axis, the difference in the probability of repression in urban versus rural areas and, on the y-axis, the difference in the probability of lethal repression in urban versus rural areas (for events involving repression). Countries that fall in the gray rectangle conform to both of the patterns described in the text: in these cases, repression is more likely in urban areas, but, when it is employed, repression is more likely to be lethal in rural areas.

Figure 2: Pr(Repression) by Number of Participants



This figure plots the coefficient estimates and 95% confidence intervals for different categories of protest size from table 2, model 2. The figure suggests that the likelihood of repression increases almost monotonically with the number of demonstrators. The omitted category is protests with fewer than ten participants.

List of Tables

Table 1: Patterns of Repression across Urban and Rural Protests
Urban events repressed more often; when rural events repressed, it more often involves lethal force.

Location	Pr(Repression)	Pr(Lethal |Repression)
Rural	0.22 [0.2, 0.24]	0.42 [0.36, 0.47]
Urban	0.29 [0.28, 0.3]	0.24 [0.22, 0.26]

The first column lists the probability of repression for social conflicts occurring in urban or rural areas; the second column, the probability of lethal repression in the sample of events involving some repression. The bootstrapped 95% confidence intervals for these proportions are included in brackets.

Table 2: Pr(Repression) by Protest Size and Population Density
The probability of repression is lowest for small protests.

	Dependent variable:				
	$\mathbb{1}$(Repression)				
	(1)	(2)	(3)	(4)	(5)
[10,100]	0.136*	0.185*			0.138*
	(0.040)	(0.030)			(0.039)
(100,1k]	0.218*	0.285*			0.219*
	(0.047)	(0.025)			(0.047)
(1k,10k]	0.154*	0.246*			0.160*
	(0.037)	(0.031)			(0.035)
(10k, 100k]	0.053	0.200*			0.054
	(0.034)	(0.051)			(0.033)
(100k, 1mil]	0.146*	0.306*			0.038
	(0.058)	(0.071)			(0.063)
Over 1mil	0.228*	0.395*			0.137
	(0.073)	(0.094)			(0.229)
Log(Pop. Density)			0.010*	0.007*	−0.007
			(0.004)	(0.002)	(0.005)
Year FEs	23	23	23	23	23
Country FEs	48	48	48	48	48
Leader FEs		164		173	
Event Controls		✓		✓	
Observations	3,664	3,579	6,737	6,429	3,215

Note: Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$

Columns 1-5: linear-probability models (see equation 1). All models include country and year fixed effects; models 2 and 4 also include fixed effects for each unique head of state. Event controls include indicator variables that capture the issue under dispute, whether the government was targeted, whether the participants employed violent tactics, and whether the event was organized or spontaneous. The unit of analysis is the social conflict. Data sources are outlined in section 4.

Table 3: Summary Statistics: Pr(Repression) by Protest Size and Population Density

Statistic	N	Mean	St. Dev.	Min	Max
$\mathbb{1}$(Repression)	7,996	0.262	0.440	0	1
Participants	3,664	3.298	1.158	1	7
Log(Pop. Density)	6,737	8.643	2.405	0.000	11.194
Year	7,996	2,003.491	6.709	1,990	2,012

Table 4: Pr(Lethal Repression) by Population Density
When used, repression is more likely to be lethal in sparsely populated areas.

	Dependent variable:				
	$\mathbb{1}$(Lethal Repression)				
	1	2	3	4	5 (Urban Sample)
Log(Pop. Density)	-0.033^{*}	-0.028^{*}	-0.023^{*}	-0.024^{*}	$-0.021^{\dagger}$
	(0.005)	(0.004)	(0.007)	(0.006)	(0.012)
Year FEs	23	23	23	23	23
Country FEs	48	48			47
Leader FEs		146		128	
Ethnic Group FEs			106	106	
Event Controls		✓		✓	
Observations	1,842	1,834	1,442	1,434	1,502

Note: Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$

Columns 1-5: linear-probability models (see equation 1), where the sample has been restricted to only those events involving some form of repression. All models include country and year fixed effects; models 2 and 4 also include fixed effects for each unique head of state. Event controls include indicator variables that capture the issue under dispute, whether the government was targeted, whether the participants employed violent tactics, and whether the event was organized or spontaneous. The sample in model 5 has been further restricted to events involving repression that occur in urban areas. The unit of analysis is the social conflict. Data sources are outlined in section 4.

Table 5: Summary Statistics: Pr(Lethal) by Population Density

Statistic	N	Mean	St. Dev.	Min	Max
$\mathbb{1}$(Lethal)	2,094	0.287	0.452	0	1
Log(Pop. Density)	1,842	8.918	2.141	0.000	11.194
Year	2,094	2,003.643	6.581	1,990	2,012

Supporting Information

The Geography of Repression in Africa

Following text to be published online.

Contents

A. Formal Model

A.1 Setting up the Model

Consider a protest by a vanguard of dissidents. With publicly known probability, $\alpha \in [0,1]$, these protesters face the brutal type, who receives greater utility from repression than the good type, $\theta_j \in \mathbb{R}^1_+$ for $j \in \{G, B\}$ such that $\theta_B > \theta_G$. There is a continuum of bystanders in the locality where this protest occurs, each indexed by i and of total measure n.

The sequence of the game is as follows. There has been a protest by a vanguard of dissidents of size p. The game starts with the executive choosing a level of repression, $r \in \{0\} \cup [\underline{r}, \ \overline{r}]$, where $r = 0$ corresponds to no repression, $\underline{r}$ to non-lethal repression, and any $r > \underline{r}$ to increasingly brutal forms of repression. Each bystander observes r and decides whether or not to join the vanguard in protesting.

The bystander receives $v_i \in \mathbb{R}^1$ if they join the protest against the bad type, 0 if they join the protest against a good type, and $q \in \mathbb{R}^1_+$ for not joining regardless of the executive's type. v_i captures bystander i's costs of joining and the positive utility they derive from protesting against a brutal executive. Note that v_i can be less than zero for individuals that face very high costs of joining a protest or receive no positive utility from dissent, even against a bad government. v_i is distributed according to a commonly known distribution function $F_V(\cdot)$.

Leaders care about maintaining power and the associated stream of benefits. I focus here on the component of their utility that varies with protests and leaders' decisions about how to deploy repression. The executive's payoff is $u_j = p\theta_j r - cn[1 - F_V]$ for $j \in \{G, B\}$. This function captures the intuition that repression is double-edged sword, which can both quell and inflame dissent. Looking at the first term, the returns to repression are increasing in the size of the initial protest, $p \in \mathbb{R}^1_+$. Large protests are more likely to shut down major roads and disrupt commerce and government activities. Restoring order in these cases through the use of repression, thus, brings greater returns to the executive.[24] However, the executive also pays a cost, $c \in \mathbb{R}^1_+$, for every additional bystander that joins the protest. The cost of any backlash is captured in the second term, which multiplies this marginal cost by the measure of bystanders that join after observing the executive's decision regarding whether and how forcefully to intervene in the protest.

A.2 Equilibria and Comparative Statics

Proposition 1. *Assuming conditions (1) and (2) (defined in the proof) hold, there exists a Perfect Bayesian Equilibrium in which the following conditions hold:*

(A) Both types employ non-lethal repression ($r^* = \underline{r}$).

(B) Given this pooling, bystanders can not update their prior beliefs and choose to join the protest only if their payoff to participating exceeds their reservation utility ($v_i\alpha > q$). The measure of bystanders that join is then $n[1 - F(q/\alpha)]$.

(C) Off-the-path, bystanders infer that they face a brutal type if they observe $r > r^*$, while observing $r < r^*$ leads them to concludes that they face the good type with probability 1 (where r^* is the equilibrium level of protest).

Proof. See appendix A.3. □

The two conditions that sustain this pooling equilibrium relate back to the earlier intuitions. First, if the vanguard of protesters is large enough (relative to the population of bystanders), even benevolent rulers can justify repression at the expense of remaining indistinguishable from the more brutal type. The second constraint implies that even executives with little regard for their citizens' welfare will not want to lethally repress protesters in settings where revealing their type can touch off a sizable backlash — settings where there are a large number of bystanders relative to the size of the vanguard (or where bystanders' prior strongly suggests that the executive is benevolent).

How does the equilibrium shift as we change the size of the vanguard (p), holding the measure of bystanders and off-the-path beliefs fixed? As the initial size of the protest declines, executives (particularly more benevolent types) see less value in deploying repression, given its downside risk. In the limit, as the initial size of the protest becomes negligible, all types prefer simply to ignore these small protests and avoid any risk of angering bystanders. This generates a first hypothesis:

H1: *Increasing the initial size of the demonstration increases the probability of repression.*

Suppose now that we vary the measure of bystanders, holding the initial size of the protest and off-the-path beliefs fixed. As we decrease n, this reduces the risk of backlash and, thus, makes more brutal forms of repression appealing. Again, as the audience of bystanders shrinks to zero, all executives opt for more severe repression, as this demobilizes the vanguard without risking escalation (if $n = 0$, then all types maximize their payoffs by setting $r = \overline{r}$). This equilibrium shift then motivates a second hypothesis:

H2: *Increasing the population of bystanders reduces the severity of repression should it be employed.*

A.3 Proofs

Proposition 1. *Assuming conditions (1) and (2) below hold, the following Perfect Bayesian Equilibrium exists:*

$$\left(s_{\theta_B} = s_{\theta_G} = \underline{r},\ s_i = \begin{cases} Join & v_i > q/\alpha \\ \sim Join & o.w. \end{cases} \textit{ and } \beta(r) = \begin{cases} 1 & r > \underline{r} \\ \alpha & r = \underline{r} \\ 0 & r < \underline{r} \end{cases} \right)$$

where s_x represents the equilibrium strategy of player x; $\beta(r)$ represents i's belief that the executive is a bad type after observing r.

Proof. If both types pool on $\underline{r}$, then bystander i can not update their prior belief and chooses to join the protest only if their expected return to joining is greater than their reservation utility, $v_i\alpha > q$. The proportion of all bystanders, n, that choose to join is the proportion for which $v_i > q/\alpha$ or $1 - F(q/\alpha)$.

Suppose off-the-path beliefs are such that if a bystander observes repression in excess of non-lethal repression, then they believe that they are certainly facing a brutal type. However, if they witness no repression, then the bystander believes that they are definitely facing a good type. In terms of the notation used above, the posterior belief, $\beta(r)$, equals one for $r > \underline{r}$ and zero for $r < \underline{r}$. More generally, in any pooling equilibrium on r^*, bystanders who observe repression less severe than r^* infer that they face a good type; repression more brutal than r^* conveys to bystanders that they are definitely confronting a bad type.

Both types choose non-lethal repression ($s_{\theta_B} = s_{\theta_G} = \underline{r}$) in this pooling equilibrium, and receive: $u_j = p\theta_j\underline{r} - nc[1 - F(q/\alpha)]$ for $j \in \{G, B\}$. These strategies are incentive compatible if

$$\begin{aligned} p\theta_G\underline{r} - nc[1 - F(q/\alpha)] &> 0 \\ p\theta_G\underline{r} &> nc[1 - F(q/\alpha)] \end{aligned} \tag{2}$$

$$\begin{aligned} p\theta_B\underline{r} - nc[1 - F(q/\alpha)] &> p\theta_B\overline{r} - nc[1 - F(q)] \\ nc[F(q/\alpha) - F(q)] &> p\theta_B(\overline{r} - \underline{r}) \end{aligned} \tag{3}$$

Recall that $\theta_B > \theta_G > 0$, so if (1) is satisfied for the good type, it will also be satisfied for the bad type. By the same logic, if (2) is satisfied for the bad type, it will also be satisfied for the good type. $\square$

B. Tables for Alternative Explanations

B.1 Reporting Bias

News Coverage by Repression and Location

Table 6: Pr(Multiple News Sources) Given Repression and Location
Lethal repression does not increase the likelihood that rural events are covered by both news sources.

	No Repression	Non-Lethal	Lethal
Rural	0.39	0.45	0.48
Urban	0.40	0.48	0.67

Cells report the probability that an event is covered by both the AP and AFP news wires depending on where they occur (rows) and how they are repressed (columns).

Table 7: Pr(Multiple Sources) by Lethal Repression and Location
Lethal repression only affects the likelihood that both sources cover urban events.

	Dependent variable:	
	$\mathbb{1}$(Multiple News Sources)	
	(1)	(2)
$\mathbb{1}$(Urban)	0.034	0.057
	(0.063)	(0.056)
$\mathbb{1}$(Lethal)	-0.009	0.015
	(0.042)	(0.039)
$\mathbb{1}$(Urban)$\times\mathbb{1}$(Lethal)	0.180^{*}	$0.126^{\dagger}$
	(0.069)	(0.068)
Year FEs	23	23
Country FEs	48	48
Leader FEs		146
Event Controls		✓
Observations	1,845	1,837
Note:	Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$	

Columns 1-2: linear-probability models, where the dependent variable is an indicator for whether a social conflict is covered by both the AP and AFP news wires. Models include country and year fixed effects; model 2 also includes leader fixed effects and event controls. The unit of analysis is the social conflict. Data sources are outlined in section 4.

Distance to Reporting Resources

Table 8: Pr(Lethal Repression) by Distance to Reporting Resources

Conditioning on distance to known reporting locations does not wipe out the relationship between population density and lethal repression.

	Dependent variable:			
	$\mathbb{1}$(Lethal Repression)			
	1	2	3	4
Log(Pop. Density)	-0.026^{*}	-0.022^{*}	-0.023^{*}	-0.020^{*}
	(0.007)	(0.006)	(0.006)	(0.005)
Dist. AP Reporting Hub	0.0002^{*}	0.0003^{*}		
	(0.0001)	(0.0001)		
Dist. AP Filing Loc.			0.0004^{*}	0.0004^{*}
			(0.0001)	(0.0001)
Year FEs	23	23	23	23
Country FEs	48	48	48	48
Leader FEs		146		146
Event Controls		✓		✓
Observations	1,842	1,834	1,842	1,834

Note: Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$

Columns 1-4: linear-probability models (see equation 1), where the sample has been restricted to only those events involving some form of repression. All models include country and year fixed effects; models 2 and 4 also include fixed effects and event controls. Dist. AP Reporting Hub: distance from event to the closest reporting hub (defined as a location where AP reporters filed an average of at least five stories in each of the previous three years). Dist. AP Filing Loc.: distance from event to the closest place where an AP reporter filed a story in the previous year. The unit of analysis is the social conflict. Data sources are outlined in section 4.

Table 9: Summary Statistics: Pr(Lethal) by Distance to Reporting Resources

Statistic	N	Mean	St. Dev.	Min	Max
Dist. AP Reporting Hub	1,842	136.483	226.815	0.001	1,338.764
Dist. AP Filing Loc.	1,842	72.203	143.485	0.001	1,004.232

Bounding

It is possible to bound any reporting bias — that is, to determine how large the bias would have to be to account for the observed difference in the conditional probability of lethal repression across urban and rural areas reported in table 1. To calculate this bound, I first make the most penalizing assumptions:

A1: All events in urban areas are reported with probability 1.

A2: All events involving lethal repression are reported with probability 1 regardless of location.

A3: There is no difference in the probability of *non*-lethal repression across urban and rural areas given that some repression occurs. That is, for the subset of events involving repression, Pr(Non-lethal | Urban) = Pr(Non-lethal | Rural).

I then solve for the number of events involving non-lethal repression that would have to have occurred in rural areas to satisfy this equality.

$$\begin{aligned} \Pr(\text{Non-lethal}|\text{Rural}) &= \Pr(\text{Non-lethal}|\text{Urban}) \\ \frac{x}{142+x} &= \frac{1145}{1504} \\ x &= 452 \end{aligned}$$

If there were no difference in the conditional probability of lethal repression across urban and rural areas, then we should observe 452 events in rural areas involving non-lethal repression. Yet, we only observe 199 in the sample. This level of reporting bias implies that the news wires miss over 55 percent of the rural events in which the state violently represses protesters but no participant dies.

B.2 International Sanctioning

Table 10: International Aid and Repression

Foreign aid does not reduce the likelihood that states employ repression or use lethal repression in cities.

	Dependent variable:			
	$\mathbb{1}$(Repression)		$\mathbb{1}$(Lethal Repression)	
	1	2	3	4
ODA % GDP	0.00002*		−0.00001*	
	(0.00000)		(0.00000)	
ODA % Govt. Rev.		−0.001		−0.003
		(0.001)		(0.003)
Log(Pop. Density)			−0.048*	−0.037*
			(0.012)	(0.009)
Log(Pop. Density) × ODA % GDP			0.00000*	
			(0.00000)	
Log(Pop. Density) × ODA % Govt. Rev.				0.00003
				(0.0004)
Country FEs	31	44	30	44
Year FEs	20	20	20	20
Observations	1,512	4,644	393	1,152

Note: Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$

Columns 1-4: linear-probability models (see equation 1). In columns 3-4, the sample has been restricted to events involving some repression. All models include country and year fixed effects. ODA: Official Development Assistance. The unit of analysis is the social conflict. Data sources are outlined in section 4.

Table 11: Summary Statistics: International Aid

Statistic	N	Mean	St. Dev.	Min	Max
ODA % GDP	1,512	441.440	2,901.237	0.038	40,745.360
ODA % Govt. Rev.	4,644	8.625	11.615	−0.198	125.174

B.3 History of Armed Conflict

Table 12: Pr(Lethal Repression) Given History of Armed Conflict
Conditioning on proximity to past armed conflicts does not affect the correlation between population density and lethal repression.

	Dependent variable:			
	$\mathbb{1}$(Lethal Repression)			
	(1)	(2)	(3)	(4)
Log(Pop. Density)	-0.033^{*}	-0.034^{*}	-0.033^{*}	-0.028^{*}
	(0.005)	(0.006)	(0.005)	(0.004)
Σ Conflicts (50km, 5yr)	-0.0001			-0.0001
	(0.0002)			(0.0002)
$\mathbb{1}$(Conflict) (50km, 5yr)		0.021		
		(0.041)		
Σ Deaths (50km, 5yr)			-0.00001	
			(0.00002)	
Year FEs	23	23	23	23
Country FEs	48	48	48	43
Leader FEs				128
Event Controls				✓
Observations	1,842	1,842	1,842	1,834
Note:	Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$			

Columns 1-4: linear-probability models (see equation 1), where the sample has been restricted to only those events involving some form of repression. All models include country and year fixed effects; model 4 also includes leader fixed effects and event controls. The unit of analysis is the social conflict. Data sources are outlined in section 4.

Table 13: Summary Statistics: Pr(Lethal) Given History of Armed Conflict

Statistic	N	Mean	St. Dev.	Min	Max
Σ Conflicts (50km, 5yr)	1,842	19.250	75.250	0	914
Σ Deaths (50km, 5yr)	1,842	207.159	841.517	0	12,551

B.4 Proximity to Natural Resources

Table 14: Pr(Repression) or Pr(Lethal) Given Proximity to Natural Resources
Conditioning on the local presence of diamonds or oil does not affect the correlations between population density and repression.

	Dependent variable:				
	$\mathbb{1}$(Repression)		$\mathbb{1}$(Lethal Repression)		
	(1)	(2)	(3)	(4)	(5)
Log(Pop. Density)	0.011*	0.008*	-0.032^{*}	-0.028^{*}	-0.028^{*}
	(0.004)	(0.002)	(0.005)	(0.004)	(0.004)
$\mathbb{1}$(Diamonds) (50km)	0.049	0.014	0.077	0.070	
	(0.044)	(0.057)	(0.049)	(0.047)	
$\mathbb{1}$(Oil) (50km)	0.071†	0.031	0.014	0.0003	
	(0.037)	(0.039)	(0.096)	(0.091)	
Σ Diamonds (50km)					0.026
					(0.019)
Σ Oil (50km)					0.00000
					(0.091)
Year FEs	23	23	23	23	23
Country FEs	48	43	48	43	43
Leader FEs		149		128	128
Event Controls		✓		✓	✓
Observations	6,737	6,429	1,842	1,834	1,834

Note: Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$

Columns 1-5: linear-probability models (see equation 1). In columns 3-5, the sample has been restricted to events involving some repression. All models include country and year fixed effects; leader fixed effects and event controls are added to models 2, 4, and 5. The unit of analysis is the social conflict. Data sources are outlined in section 4.

Table 15: Summary Statistics: Pr(Repression) or Pr(Lethal) Given Proximity to Natural Resources

Statistic	N	Mean	St. Dev.	Min	Max
$\mathbb{1}$(Diamonds) (50km)	6,737	0.074	0.261	0	1
$\mathbb{1}$(Oil) (50km, 5yr)	6,737	0.033	0.177	0	1
Σ Diamonds (50km)	6,737	0.158	0.893	0	19
Σ Oil (50km)	6,737	0.033	0.183	0	2

B.5 Identities of Protesters and Repressers

Table 16: Addressing Concerns about Variation in Identities of Protesters and Repressers

	Dependent variable:					
	$\mathbb{1}$(Lethal Repression)					
	1	No Rebel	No Military	No Capitals	Students	Citizens
Log(Pop. Density)	-0.033^{*}	-0.032^{*}	-0.031^{*}	-0.016^{*}	-0.039^{*}	-0.033^{*}
	(0.005)	(0.007)	(0.005)	(0.007)	(0.017)	(0.013)
Year FEs	23	23	23	23	23	23
Country FEs	48	48	48	45	39	37
Observations	1,842	1,737	1,777	991	392	320

Note: Robust std. errors clustered on country; $^{\dagger}p < 0.1$, $^{*}p < 0.05$