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Abstract

We show that drought-induced changes in the intensity of riots lead to moves towards democracy in sub-Saharan Africa, and that these changes are often a result of concessions made as a result of the riots. This provides evidence that low-intensity conflict can have a substantial short-run impact on democratic change, and supports the .window of opportunity. hypothesis: droughts lead to an increase in the threat of conflict, and incumbents often respond by making democratic concessions.

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Keywords: riots, drought, transitions, democracy, autocracy.

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1 Introduction

What determines a country's political institutions, and in particular, the extent to which they are democratic?[1] An important set of explanations has focused on the idea that conflict, or the possibility of conflict, induces leaders to promote institutional change. Tilly (1990), Besley and Persson (2008, 2009) and Dincecco and Prado (2012) argue that conflict, and in particular wars between countries, created the setting for Western European nations to build institutions that would enable the enforcement of contracts and collection of taxes. Conflict also plays an important role in Acemoglu and Robinson's (2000, 2001, 2006) theory of democratization; they emphasize how the *threat* of conflict, in the form of a revolution, induces autocrats to make democratic concessions in an attempt to defuse that threat. In their theory revolutions are more likely in times of economic hardship, so negative economic shocks open a "window of opportunity" that can lead to a peaceful transition towards democracy.

In this paper we test the hypothesis that low-intensity conflict (riots) leads to democratic reform, and that this often happens through concessions made in response to the riots. The events in Togo in 1991 illustrate this mechanism. As Piot (2010) explains, "[i]n Togo in summer 1991, after months of clashes on the streets between dissidents and the military, Eyadéma capitulated to calls for a national conference to discuss steps toward democratization" (p.31). Meredith (2005) concurs that the riots put pressure on president Eyadéma: "[a]fter months of strikes, demonstrations and violence, Eyadéma agreed in April 1991 to allow opposition parties to operate and in July yielded to demands for a national conference" (p.397). The concessions were partial, however: "[a]t the end of the national conference in 1991, Eyadéma cleverly headed off an attempt by the political opposition to strip him of power ... but agreed to hold presidential elections" (Piot, p.33). Fearing electoral fraud, the opposition boycotted the election and allowed Eyadéma to win with 96 percent of the vote (Piot, 33).

The main difficulty in testing whether conflict opens a "window of opportunity" is that riots are rarely exogenous: there might be problems of reverse causality because the expectation of political change might itself lead to riots, and there might be unobservable omitted variables that cause both riots and political change. We address these technical problems by using droughts to create an instrumental variable for riots, as there is a considerable amount of case study evidence that shows that droughts often trigger social unrest in poor countries (see, for example, Walton and Seddon (1994)).[2]

To test the hypothesis that riots lead to democratic change we focus on sub-Saharan Africa in the period between 1990 and 2007. This choice is motivated by the large number of democratic (and anti-democratic) transitions that took place in that part of world during the sample period, and by the fact that drought is a particularly relevant instrument for this subset of countries.[3] We use data on democratic change from the Polity IV project (Marshall and Jaggers, 2005) and geographical data on riots from the Social Conflict in Africa Database (Hendrix and Salehyan, 2011).

Our instrumental variables estimates show a strong first stage relationship between drought and riots, and represent empirical evidence that droughts lead to an increase in riots. We then find a significant second stage relationship between (instrumented) riots and democratic change. The magnitude of the effect is substantial; for example, some of our estimates suggest that the probability of a democratic change in the average country in the sample increases by 16.7 percentage points (from a baseline probability of 5.7%) as a consequence of the impact a drought has on riots. Naturally, finding that riots lead to changes in democracy does not show that this is because governments feel threatened and make concessions, as suggested by the theory and the events in Togo discussed above. To address this concern, we restrict our attention to instances of democratic change that can be explicitly associated with concessions made by the incumbent government.[4] This provides strong evidence that riots can lead to democratic change because incumbent governments are induced to make democratic concessions. This channel is different from that emphasized in the previous literature (e.g. Burke and Leigh, 2010; Brückner and Ciccone, 2011), where democratic change happens when its opportunity cost is low, as measured by GDP per capita.

These results provide new evidence that low-intensity conflict can lead to democratic change over relatively short periods of time. These results are consistent with the causal mechanism underlying Acemoglu and Robinson's (2000, 2001, 2006) theory of political transitions, where the threat of a revolution induces autocratic rulers to make democratic concessions. Rioting and popular protest represent a threat to autocratic rulers, and may signal that the democratic "window of opportunity" is open. This might be because riots could unintentionally degenerate into a regime challenge (e.g., if rioters storm the presidential palace), or because the political opposition might use them for this purpose (e.g. to try to start a revolution). In either case, the incumbent government might react to this increased threat by making democratic concessions.

Our paper is related to a large literature that examines the relationship between international conflict and institutions. Tilly (1990), Hoffman and Rosenthal (1997) and O'Brien (2011) argue that Western European institutions for tax collection were created as a result of the need to pay for the costs of war. More recently, Besley and Persson (2008, 2009, 2010, 2011) have developed a theory of institutional development where conflict plays a central role. Dincecco, Federico and Vindigni (2011) and Dincecco and Prado (2012) provide empirical support for the link between conflict, fiscal capacity and institutional change. Our paper contributes to this literature, but differs in that it focuses on Africa (while most of the literature focuses on Western Europe), looks at a more recent historical period, examines the short-term rather than long-term impact of conflict on institutional change, and emphasizes the role of the threat of internal conflict instead of the threat of international wars.

This paper also relates to a small but growing empirical literature on the threat of revolution and democratization. Przeworski (2009) uses data on riots, demonstrations and strikes to study the correlation between franchise extensions and the threat of revolution in a broad world sample starting in 1918. Aidt and Jensen (2011) take a longer historical perspective and use the international diffusion of information about revolutionary events in Europe between 1820 and 1938 to study the causal link between revolutionary threats and suffrage reform. Both studies find evidence supporting Acemoglu and Robinson's (2000) theory of democratization. None of these papers, however, explore the association between temporary economic shocks, riots, and democratization.[5] Burke and Leigh (2010) use information about anti-government protests reported in the New York Times to study the link between riots and democratic change in a world sample of countries, but cannot properly identify the effect. Using more detailed riots data for Africa and an instrumental variables approach, we can identify the causal impact riots have on democratic change.

Finally, our paper is also related to the large literature on adverse economic conditions and political change (e.g. Burke and Leigh, 2010; Brückner and Ciccone, 2011), and on economic shocks and civil conflict (e.g. Collier and Hoeffler, 1998, 2004; Miguel, Satyanath and Sergenti, 2004; Jensen and Gleditsch, 2009; Brückner and Ciccone, 2010). Brückner and Ciccone (2011) and Burke and Leigh (2010) use rainfall shocks to identify when and where the democratic "window of opportunity" might be open. Brückner and Ciccone (2011) establish a causal link between negative rainfall shocks and democratic change in sub-Saharan Africa, suggesting that this happens because rainfall shocks reduce real GDP per capita, which in turn reduces the opportunity cost of contesting power; Barron, Miguel and Satyanath (2013) discuss the robustness of these results. Burke and Leigh (2010) study a broader sample of countries and find results similar to those in Brückner and Ciccone (2011).[6] Miguel, Satyanath and Sergenti (2004) use rainfall as an instrument for economic growth to establish a causal link between (the lack of) economic growth and civil conflict.[7]

The rest of this paper is organized as follows. Section 2 discusses the theoretical framework. Section 3 presents the data, while section 4 lays out our empirical strategy. Section 5 presents and discusses our main results, and section 6 concludes.

2 Theoretical Framework

The idea that the threat of social conflict is linked in a causal way to democratic change has a long history in political economy, and has recently gained renewed currency through the work of Acemoglu and Robinson (2000, 2001, 2006) and Boix (2003). The formalization of this idea in the work by Acemoglu and Robinson emphasizes the notion of a "window of opportunity" for democratic reform. Their theory starts from the premise that incumbent rulers are unwilling to share power with other groups because this compromises their policy objectives. Consequently, democratic reform only happens in situations where opposition groups pose a threat to the status quo that is seen as credible by the incumbents. In some cases the incumbents perceive a need to act preemptively in order to avoid radical political change. Sharing power through democratic reform is, of course, only one alternative amongst many open to the incumbents. Repression or policy concessions are often sufficient and typically preferable, but sometimes more durable institutional change is required as the lesser of multiple evils. A key element in this theory is that institutional reforms are durable, so that they cannot be easily reversed once the threat that triggered them has subsided. History contains numerous instances in which reforms were undertaken under threat but were subsequently rolled back; the "Arab Spring" is perhaps the most recent example. These instances do not undermine the logic of the theory; what matters is that at the time they were made, the concessions were considered of *sufficient* duration to dissuade opposition groups from overthrowing the regime. Acemoglu and Robinson (2001) develop this logic in a formal model where democratic concessions can be reversed; in practice what makes democratic change more durable than other types of concessions is that it involves a change in institutions. This does not make it impossible to revert to the *ex-ante* status quo, but it does increase the cost of doing so.

An important insight that follows from this theory is that the threat posed by opposition groups is not always credible, but when it is, a "window of opportunity" is open. Acemoglu and Robinson (2001), Burke and Leigh (2010), and Brückner and Ciccone (2011) associate the "window of opportunity" with temporary adverse economic shocks that lower the cost of contesting power, allowing opposition groups to credibly threaten to overthrow the incumbents. We build on this idea but add an important new dimension that is at the center of our empirical analysis. Acemoglu and Robinson (2000, 2001, 2006) assume that whether the "window of opportunity" is open is common knowledge to all parties. This assumption is challenged in the work by Andrews and Jackson (2005), among others, who point out that institutional reform typically takes place under conditions of extreme uncertainty. In essence this means that nobody can be entirely certain that the "window of opportunity" is open, and at any given point in time incumbents and opposition groups must act taking cues from events as they unfold. This insight, on the one hand, allows us to conjecture a link between these cues and democratic change: we argue that riots which have not yet escalated into a full-blown regime challenge but which have the potential do so are important cues that may induce democratic concessions.[8] On the other hand, allowing for uncertainty introduces the new and important possibility that riots may develop into full-blown revolutions or civil wars; this may happen, for example, if incumbents underestimate the threat and fail to act in time to avoid it.[9]

We argue that riots, although often triggered by adverse economic shocks (e.g. droughts), can affect regime transitions through channels other than the opportunity cost mechanism stressed in the previous literature (and captured by fluctuations in official GDP per capita data). For example, in poor countries with large informal sectors where a large fraction of the population lives near the subsistence level in the countryside, negative weather shocks can lead to large population movements that might trigger riots and induce democratic concessions.

In short, we can summarize our theory as follows: incumbents will only share power if they perceive that opposition groups pose a credible threat. They use cues from unfolding events to assess whether they need to act in order to preserve their power. One important cue is low-intensity social unrest (e.g. riots) triggered by droughts. Based on this logic, we hypothesize that riots induced by drought may result in democratic concessions even if GDP per capita stays constant. We interpret this as a more refined version of the "window of opportunity" theory of democratic change than the one tested elsewhere in the literature.

3 Data

Our dataset combines information on democratic change, riots, droughts, and economic conditions for a sample of 41 sub-Saharan African countries over the period 1990 to 2007.[10] As many previous studies, we draw on the Polity IV database to measure changes in democracy (Marshall and Jaggers, 2005).[11] We use this database to construct three different measures of democratic change. The starting point for our first two measures is the variable *regtrans*. This variable indicates whether a regime transition has begun in a given year; it can take a number of different values, with the value repeated in every year of the transition. Following Burke and Leigh (2010), we only count transitions in the first year in which they occur, setting the *regtrans* values to zero in the later years. We then transform this adjusted version of *regtrans* into a binary variable that equals one if any change (pro- or anti-democratic) started in a given year, and zero otherwise. We call this new variable *transition, binary*. This variable tells us whether a transition has started and allows us to test whether riots have a *short-run* impact on democratic change, but gives us no information about the direction or magnitude of the change.[12] We address this limitation by using a count version of *transition, binary* which we call *transition, count*. It takes values in the set {-2,-1,0,1,2,3} as coded in the Polity IV database. Positive values denote pro-democratic changes (i.e. towards a higher Polity IV index score) of different magnitudes, while negative values denote anti-democratic changes; zero denotes no change. We also use detailed case study evidence we collected on the 43 regime transitions recorded by the Polity IV database as having taken place in sub-Saharan Africa during our sample period. We use this information to identify those cases in which concessions by the incumbent government played a role in the transition (so that it was not simply the result of a coup or an election), which allows us to restrict our regressions to these cases only. This information was collected primarily from the Encyclopaedia Britannica, supplemented with information from other sources.[13]

Naturally, these measures involve a loss of information, both because they do not capture the transition period in full (as they look only at the first year of a transition), and because the magnitude of the change is not captured accurately. To account for this, we use the *polity*2 variable from the Polity IV database, which is a version of the *Polity IV index* that has been corrected to allow for use in time series analysis. However, periods of interregnum are coded as zero, which would lead us to mistakenly interpret instances in which a country with a negative *polity*2 score falls into interregnum (and so its *polity*2 score increases to 0) as democratic improvements. We avoid this problem by following Brückner and Ciccone (2011) and setting *polity*2 equal to missing in all interregnum years and years immediately following an interregnum.

The data on riots is from the Social Conflict in Africa Database version 2.0, updated on July 12, 2011 (Hendrix and Idean, 2011). This is a comprehensive database of protests, riots, strikes and other social disturbances in Africa from 1990 to 2010, and it is constructed from Lexis-Nexis searches.[14] The riots data include geographic coordinates, which we use to construct Map 1.[15] We include all riots, regardless of whether they eventually led to a civil war.[16] We create the variable *riots* to capture the intensity of the protests; it is calculated by adding the duration (in days) of all riots that happened in a given year and in a given country. Different riots are counted individually even if they occurred on the same day. This coding has the advantage that both riots that last more days and days with more riots contribute more to the total.[17]

[Map 1: Riots in Africa, 1990-2007]

We use rainfall data from the Global Precipitation Climatology Project (GPCP) to identify countries and years with droughts between 1990 and 2007.[18] We say that a country experienced a drought in a given year if its annual rainfall level was below the 20th percentile. We create a binary variable *drought* that equals one in country-years where rainfall fell below this threshold, and zero otherwise.[19] This is our main instrumental variable for riots.

Our measure of growth in per capita income, which we refer to as *gdp per capita growth*, is calculated using data on GDP per capita from the World Development Indicators (2011). Deaton and Miller (1995) and Deaton (1999) show that decreases in commodity prices lead to slower (or negative) GDP growth in Africa. Brückner and Ciccone (2010) use monthly commodity price data for 19 commodities to calculate an annual price, and then use information in Deaton (1999) and the UN Commodity Trade Statistics Database to construct a commodity exports basket for each country. This then allows them to construct a country-specific index.[20] We follow them and use a growth version of this index, which we call *commodity price index growth*, as an instrument for *gdp per capita growth* in some of our specifications.[21][22]

Figure 1 presents preliminary evidence of the relationship between riots and democratic change. Year 0 is defined as the year in which a transition begins (as recorded by *transition, binary*) and the axis to the left (right) of that point measures the time before (after) the start of the transition. The y-axis records the average across countries of the log of *riots* in that year. The graph clearly shows that riots increase in the run-up to a transition, and that once a transition has begun the average number of riots declines considerably.

[Figure 1: Pre- and post-transition riot activity]

4 Empirical Specification

Our main empirical specification is:

$$transition_{i,t} = \alpha + \beta \ln(riots)_{i,t-1} + \gamma \ln(gdp\ per\ capita\ growth)_{i,t-1} + \iota_i + \theta_t + u_{i,t}, \quad (1)$$

where $transition_{i,t}$ is one of the three measures of democratic change introduced above. We lag *riots* and *gdp per capita growth* by one year because it is unlikely that their effect on democratic change is immediate.[23] We include country (ι_i) and year (θ_t) effects, and we also include a lagged dependent variable in the specifications with *polity2* as the dependent variable. This partial adjustment model allows us to estimate the short-run and long-run impact of riots on democratic change in a more flexible way.[24]

We include *gdp per capita growth* in our regressions for three reasons. First, it is likely that fluctuations in recorded national income capture changes in the opportunity cost of a regime challenge, and so they might directly lead to preemptive democratization. This is the idea behind the specifications used by Brückner and Ciccone (2011). Second, including *gdp per capita growth* is necessary for drought to satisfy the exclusion restriction as an instrumental variable for *riots*. If *riots* remains statistically significant in specifications that hold *gdp per capita growth* constant, then riots caused by drought affect democratic change through channels other than *gdp per capita growth* (e.g. through shocks to the informal sector, migration, etc.). Finally, including *gdp per capita growth* allows us to rule out that shocks lead to democratic change because they limit an autocrat's ability to use public funds to "buy off" opposition groups (Haggard and Kaufman, 1997), or because they create opportunities for peaceful constitutional exchange unrelated to the threat of conflict (Congleton, 2007, 2011).

The main problem associated with the estimation of equation (1) is that *riots* and *gdp per capita growth* are likely to be endogenous, as a large number of factors, some of them unobservable, might impact on these variables and democratic change. To be able to make a causal claim we must instrument for *riots* and *gdp per capita growth*, and we do so by using *drought* and *commodity price index growth* as instruments. For *drought* to be a relevant instrument it needs to be strongly correlated with *riots*, and we later show that this is the case. To be valid, it must satisfy the independence condition and the exclusion restriction. The independence condition requires that the "treatment" assignment not be determined by the outcome. In our case, whether there is a drought cannot be a function of whether institutional change is about to take place (or not). Since drought (and rain more generally) is not determined by human activities or decisions, at least in the short-run, this condition is satisfied. The exclusion restriction requires that the instrument not belong in the structural equation (1). Specifically, this means that drought should not affect democratic change through channels other than riots once we have controlled for country fixed effects, time effects, and *gdp per capita growth*. Analogously, for *commodity price index growth* to satisfy the independence condition it must be that political change does not trigger changes in this index; in other words, that it does not affect the growth of international commodity prices. This is true since the countries in our dataset are small players in international markets. The exclusion restriction requires that *commodity price index growth* only affects democratic change through its impact on *gdp per capita growth* (once we control for riots), which seems likely to be the case for small commodity exporters.

5 Main Empirical Results

We present our results in four tables. Table 1 shows OLS and conditional logit estimates of the structural equation (1), as well as estimates of the reduced form where we associate the instrument directly with our measures of democratic change. Table 2 shows the main results from the instrumental variables estimation. Table 3 presents the results when we only consider transitions that involved concessions. Table 4 presents the results for a dynamic panel specification.

5.1 Benchmark Estimates

As a benchmark, section A of Table 1 reports OLS and conditional logit estimates of the structural equation (1). The outcome variable in column 1 is *transition, binary*, and we find that a one standard deviation increase in the log of *riots* leads to an increase in *transition, binary* of 0.024.[25] The mean value of this variable across countries and time is 0.057, so this is a substantial effect. In column 2, the outcome variable is *transition, count*, which makes a distinction between transitions to and away from democracy. The results are similar, and we conclude that riots are positively correlated with the likelihood of short-run democratic change. Column 3 shows a specification with *polity2* in levels and a lagged dependent variable; the coefficient estimate on *riots* is positive but not significant.[26] Finally, column 4 shows the results from a fixed effects conditional logit estimation with *transition, binary* as the outcome variable. The coefficient on *riots* is positive but not significant.[27] All these correlations are consistent with the hypothesis that riots trigger democratic change.[28]

[Table 1: Structural and Reduced Form Regressions]

5.2 Reduced Form Estimates

As we noted above, endogeneity is a serious concern, and so the OLS (and logit) estimates cannot be given a causal interpretation. Before we report the instrumental variables estimates we present evidence on the reduced form, where we regress the measures of democratic change directly on *drought*.[29] This gives us an indication of whether the causal relationship we have hypothesized is present. Column 1 in section B of Table 1 reports on the specification with *transition, binary* as the outcome variable and we find a positive and significant relationship with *drought*. Column 2 shows that *drought* is also significant in the regression using *transition, count* as the measure of democratic change. Column 3 reports the results for the partial adjustment model with *polity2*; *drought* is not significantly correlated with *polity2*, but the sign is as expected.

5.3 IV Estimates

In columns 1 and 2 of Table 2 we present results where we instrument for *riots* using *drought*. In section B of the table we report the first stage results where we regress *riots* on *drought*. In both columns *drought* at time $t-2$ has a significant impact on *riots* at time $t-1$. Section A of the table presents the second stage results using the 2SLS estimator. The outcome variables are *transition, binary* and *transition, count* (the results for *polity2* are shown in Table 4). Since the F-statistics are below 10, we use weak-instrument robust inference p-values (in square brackets) to assess significance.

The point estimate on *riots* is positive and statistically significant in both columns 1 and 2. Taking the point estimates from column 1, a drought leads to an increase of 0.380 in the log of *riots*, while a one unit increase in the log of *riots* leads to an increase of 0.440 in the probability of a transition. It then follows that a drought leads to an increase of $0.440 \times 0.380 = 0.167$, or 16.7 percentage points, in the probability of a transition. Since the average *transition, binary* value is 0.057, this is a considerable increase. The outcome variable in column 2 is *transition, count*, and again we find a positive and significant effect of *riots* on democratic change. The estimated coefficients are large, but this is not due to the relative weakness of the drought instrument; since the specifications are just-identified, the 2SLS estimate is median-unbiased (see Angrist and Pischke, 2009, p. 213).

[Table 2: 2SLS, Riots and Democratic Change]

It is plausible that the impact of riots on democratic change is heterogeneous across countries. In this case, the instrumental variable estimate corresponds to an average causal response, and should be interpreted as the impact on democratic change of riots that happen because a country has switched into a state of drought. In other words, the coefficient on *riots* captures the average democratic change that follows from riots that were triggered by drought. For example, the estimate in column 1 tells us that a drought at time $t-2$ increases the log of *riots* at time $t-1$ by 0.380, so that there is an increase of 16.7 percentage points in the probability of democratic change.

The average causal response is often uninformative because it is instrument-specific and captures only the impact on "compliers" whose treatment status changes as a result of a change in the instrument (Heckman, 1997). In our case, however, the average causal response allows us to establish that the democratic change we observe is a consequence of the increase in riots that resulted from a drought. This proves particularly useful because it isolates the impact that comes from a change in drought status, allowing us to show that a specific shock, drought, can lead to an improvement in democracy because it increases the threat of social conflict, as captured by riots.

For our interpretation of the estimated IV coefficients as average causal responses to be valid, the monotonicity condition must hold (see Imbens and Angrist, 1994; Angrist and Imbens, 1995). This condition states that the impact of drought on the intensity of riots must always be in the same direction, i.e. riots cannot become less intense because there is a drought (relative to the counterfactual intensity of riots in the case of no drought). In essence, we need to rule out the possibility that droughts inhibit riots. This assumption cannot be tested, but we view it as being fairly uncontroversial.

In columns 3 and 4 of Table 2 we present IV results using *drought* and the *commodities price index growth* as instruments for *riots* and *gdp per capita growth.* Section B of the table shows the first stage for *riots,* while Section C shows the first stage for *gdp per capita growth.* Section A presents the second stage results using the 2SLS estimator. The F-statistics are again below 10, and so we use weak-instrument robust inference p-values (in square brackets) to assess significance. Looking at the top row in column 3, we observe that the point estimate on *riots* in the specification with *transition, binary* is positive and statistically significant (according to two of the three p-values). The outcome variable in column 4 is *transition, count*, and again we find a large positive effect of *riots* on democratic change, although it is no longer significant.

In Table 3 we repeat the regressions from Table 2 but we set *transition, binary* and *transition, count* to 0 in instances where the transition did not involve concessions by the incumbent. In short, transitions triggered by coups/revolutions or elections are not recorded as democratic changes in these specifications. If the results in Table 2 had been driven by transitions that were due to coups/revolutions or elections, then these results would not show up in Table 3. However, the results are very similar to those in Table 2; if anything, those in Table 3 are stronger. This shows that our results are indeed consistent with the "window of opportunity" hypothesis: riots lead to democractic change because they induce democratic concessions by the incumbent government.[30]

[Table 3: 2SLS, Riots and Democratic Change, Concessions Only]

5.4 The Partial Adjustment Model

Table 4 reports the results for the partial adjustment specification with *polity2* as the dependent variable. We include a lagged dependent variable, which introduces what is known as the Nickell bias (see Nickell, 1981). We address this problem by using the Arellano-Bond GMM estimator (Arellano and Bond, 1991).[31] Column 1 reports the results from a specification where we use *drought* and lagged right-hand side variables as instruments for the differenced equation. We find the coefficient on *riots* not to be significant.[32] In column 2 we repeat these regressions but treat *gdp per capita growth* as endogenous and include the *commodity price index growth* as an additional instrument in the Arellano-Bond estimation procedure. The coefficient on *riots* is significant at the 10% level.

[Table 4: Dynamic Specification with Polity2]

The Arellano-Bond GMM estimator is intended for cases in which n (i.e. countries) is large and t (i.e. years) is small. However, our panel has at most 41 countries and 18 years, so that the Arellano-Bond estimates could be severely biased. An alternative is to use the bias-corrected least-squares dummy variable (LSDV) estimator (see Bruno, 2005), which performs better than the Arellano-Bond GMM estimator when the number of cross-sectional units is small. The LSDV results are presented in column 3. The point estimate is somewhat smaller than with Arellano-Bond, and significant at the 10 percent level. Focusing on the point estimate obtained with the LSDV estimator, a one standard deviation increase in the log of *riots* leads to a short-run increase of $1.744 \times 0.131 = 0.229$ points in the *polity2* score, while the long-run impact is considerably larger and equal to 1.16 points.[33]

5.5 Discussion of the Results

Taken together, our results suggest that riots trigger democratic change, providing new evidence for the causal mechanism that underlies Acemoglu and Robinson's (2000, 2001, 2006) theory of political transitions: adverse economic shocks generate social unrest and riots, which in turn lead incumbent governments to make democratic concessions. Moreover, the size of the effect is large and of economic and political importance.

Acemoglu and Robinson's (2000, 2001, 2006) theory of political transitions acknowledges that incumbents might use strategies other than concessions to avoid being overthrown.[34] For example, they may invest in repression or offer temporary transfers to those who pose a threat to their rule, thus eliminating their incentive to participate in a revolt.[35] In our setting, food subsidies could be used to mitigate the incentive to riot in the face of a drought, while international food aid could unintentionally serve the same function. It is not possible for us to control for these alternative strategies, and so they remain as potential sources of statistical bias in the estimation of β in equation (1). To establish the direction of this bias, let us consider the consequences of omitting spending on repression from equation (1).[36] The theory predicts that repression is negatively correlated with democratic change and positively correlated with *riots*. Consequently, failing to control for repression biases the estimate of β towards zero, which would work against us finding a coefficient that is significantly different from zero. This means that our estimate of the effect of *riots* on democratic change should be viewed as a lower bound on the true causal effect.

A related issue is that rain might discourage or stop riots; for example, travel may become more difficult and people may dislike getting wet. One might then think that drought is correlated with riots not because economic conditions provide a reason to protest, but because the lack of rain makes riots possible (given some other underlying cause for the riots). However, this is unlikely to be a problem at an annual level of aggregation. Even in years with heavy rainfall, there will be periods without rain; riots could happen in those periods even if rain, in general, deters riots.

6 Conclusions

This paper has shown that riots triggered by drought lead to democratic change. In doing so, it provides new evidence that conflict can lead to institutional change. While most work in this area has focused on international wars and their impact on institutional change over long periods of time, we have shown that low intensity conflict can also lead to change in relatively short periods of time.

We have shown that droughts increase riots, which in turn lead to more democratic institutions. These results are driven by cases in which democratic concessions were made as a result of riots, and so they support the theory of democratic change formulated by Acemoglu and Robinson (2000, 2001, 2006). Although our focus on the recent experience of sub-Saharan Africa is somewhat specific, our study adds to a growing literature that finds evidence in support of this theory: Aidt and Jensen (2011) find evidence of this mechanism when they look at the international diffusion of information about revolutionary events in Europe in the period 1820-1938; Aidt and Franck (2014) establish a link between local riots and support for the Great Reform Act of 1832 in Britain; Przeworski (2009) studies the correlation between franchise extensions and the "threat of revolution" in a large sample of countries. Overall, this body of work provides compelling evidence in support of the central mechanism in Acemoglu and Robinson's (2000, 2001, 2006) theory of political transitions, as many episodes of democratic change, both today and in the past, are the result of concessions made by governing elites in response to what they perceived to be threats to the established order.

Notes

[1]This question has received a considerable amount of attention in political science and political economy, with seminal contributions by Lipset (1959), Moore (1966), and more recently Przeworski et al. (2000), Rueschemeyer et al. (1992), Boix (2003), Acemoglu and Robinson (2006), and Congleton (2011).

[2]We focus on droughts (large negative rainfall shocks) rather than on rainfall levels because we expect a link between rainfall and riots only in years of drought; that is, the relationship between rain and riots is highly non-linear. For example, we do not expect that a rainfall shock that reduces the rainfall level from above to just below the median will trigger riots, as agricultural production would only be marginally affected. Riots and social unrest are triggered by large negative shocks that seriously disrupt agricultural production.

[3]In the context of the 1974 revolution in Ethiopia, Berhanu (1998, p. 82) explains that "the progressive deterioration of the quality of life for many Ethiopians was aggravated by a major drought-induced famine (1973-74) that killed hundreds of thousands. This increased the unpopularity of the regime..." Similarly, Appiah and Gates (2010, p. 234) explain the causes of the 1974 revolt against Diori in Niger by arguing that "Diori managed to maintain order until the devastating drought reached its peak in 1973. Popular unrest escalated into riots when evidence emerged that Diori and members of his administration were enriching themselves with diverted foreign food aid." More recently, referring to the 1990 Sudan famine, de Waal (1997, p. 103) explains that "[f]oreign NGOs planned to distribute food in rural areas: the government wanted to divert it to the towns. In one instance, after a food riot in Um Ruwaba town, NGO relief food intended for displaced Southerners was commandeered at gunpoint and distributed to the town's residents."

[4]Using data from the Encyclopaedia Britannica and other sources, we found that 28 of the 43 large changes in the democracy measure in our sample involved democratic concessions by the incument government.

[5]The work by Berger and Spoerer (2001), however, indicates that there was a strong link between poor economic conditions and the European revolutions of 1848.

[6]Franck (2012) establishes a causal link between short-term variations in local tax income (instrumented by rainfall levels) in 19th century France and electoral support for democratic institutions.

[7]See Ciccone (2011) and Miguel and Satyanath (2011) for a discussion of these results.

[8]There are several other reasons why the window of opportunity may be open in situations where

riots are widespread, which do not require that the initial riots be directed at regime change. First, an effective regime challenge requires coordination, and riots may serve as a focal point or a public signal that, in the spirit of Ellis and Fender (2011), helps overcome this coordination problem. Second, as stressed by Tullock (1974) and Kuran (1989), revolt requires leadership, and opposition leaders might be able to redirect the dissatisfaction with economic conditions to dissatisfaction with the existing regime. This line of reasoning also makes it clear that preemptive democratic change need not (and often does not) result in voting rights and increased political influence being granted to those who riot. All that is required is for those who might take advantage of the situation or provide leadership for a regime challenge to be granted increased and lasting political rights.

[9] Aidt and Jensen (2011) present a mathematical model of regime transitions in the tradition of Acemoglu and Robinson (2000, 2006) that formalizes these predictions and shows the conditions under which revolution can happen on the equilibrium path.

[10] The countries are listed in Table A1 in the online appendix.

[11] A major advantage of the Polity IV data is that they are coded in a systematic way for a large number of countries for a long time span. However, it has been noted that they do not capture aspects of democratization related to suffrage reforms very well (Aidt and Eterovic, 2011), and that changes within the middle range of the scale of the Polity IV index are difficult to interpret (Cheibub, Gandhi, Vreeland, 2010).

[12] We note that of 43 regime changes observed in the data, 7 were anti-democratic and 36 were pro-democratic. Hence, we feel it is reasonable to think of this binary variable as a measure of democratic change. We decided against dropping instances of anti-democratic change because that would have introduced a selection problem.

[13] More details on the coding can be found in the online appendix.

[14] These involved searching for each country name and the codewords "protest" or "strike" or "riot" or "violence" or "attack" in the following wire services: Agence France Presse - English, The Associated Press, Associated Press Online, and Associated Press Worldstream. The articles were then inspected to determine the type of incident they referred to and to avoid double-counting. For more details, see www.scaddata.org.

[15] All GIS data uses the world equidistant-cylindrical projection.

[16] We exclude 75 riots which are recorded as being part of an ongoing civil war. The original number of riots in the dataset is 7,337, so that we are left with a net total of 7,262.

[17] There are a total of 7,262 events in the dataset (after excluding events that are part of an ongoing

civil war), of which only 67 ended in a year different from their start year. In these cases we allocated them to the year in which they started.

[18]To ensure consistency with previous work, we draw on data from Ciccone (2011) and Miguel and Satyanath (2011).

[19]Benson and Clay (1998) propose several different definitions of drought. We follow Brückner and Ciccone (2010) and adopt a physical (rather than economic) definition. We use a 20th percentile threshold, so that the country-years with the 20% lowest rainfall levels will be considered to be experiencing a drought. The results are similar if we use a 15th percentile threshold.

[20]They calculate the price index for country c in year t as follows:

$$index_{ct} = \sum_{i=1}^{19} \omega_{ci} P_{it},$$

where ω_{ci} is the export share of commodity i (assumed to be fixed over time) and P_{it} is its price. For more details, see Brückner and Ciccone (2010).

[21]We are grateful to Markus Brückner and Antonio Ciccone for making their data available to us.

[22]Table A2 in the online appendix shows the summary statistics for the main variables.

[23]Our results are robust to using contemporaneous growth in GDP per capita, as suggested by Barron, Miguel and Satyanath (2013), and to using growth in GDP per capita lagged twice. See Table A3 in the online appendix for the details.

[24]This introduces what is known as Nickell bias (Nickell, 1981); we return to this issue when we discuss our results.

[25]Table A2 in the online appendix shows that the standard deviation is 1.744, so $1.744 \times 0.014 = 0.024$

[26]The difference in the number of observations in columns 2 and 3 is due to differences in how Burke and Leigh (2010) and Brückner and Ciccone (2011) deal with years in which a country was in interregnum. Burke and Leigh (2010) set the transition variable to 0, while Brückner and Ciccone (2011) set the *polity2* variable to missing.

[27]We lose some observations because conditional logit estimation uses only countries with variation in the *transitions, binary* variable (i.e. that experience both years with a transition and years without a transition) during the sample period.

[28]We report estimates using GDP data from the World Development Indicators (2011).

[29]We include *drought* lagged twice. This is because we use variables dated t-2 to instrument for

riots at t-1, which is the variable that appears in equation (1).

[30] In the online appendix we present a number of results where we allow riots to have a different impact on democratic change depending on whether they happen in urban or rural areas. We find no large differences in the impact different types of riots have on democratic change.

[31] We adopt the Arellano-Bond GMM estimator instead of the system estimator because it involves fewer internal instruments.

[32] The Arellano-Bond test for AR(1) errors rejects the null hypothesis of no autocorrelation, while the test for AR(2) errors does not reject the null. Moreover, the Hansen test does not reject the null hypothesis of valid instruments.

[33] In the long-run, *polity2* reaches a steady-state, i.e. $polity2_t = polity2_{t-1}$, and so the long-run effect is $\frac{0.229}{1-0.803} = 1.162$ (on a scale from -10 to 10).

[34] It is also possible, as suggested in Aidt and Albornoz (2011), that foreign governments might induce or encourage regime transitions in the wake of adverse economic shocks.

[35] The drawback of this strategy is that it is only credible when the “threat of revolution” is perceived to be real. For this reason, it is often insufficient to avoid a regime challenge, and the incumbent must then resort to either democratic reform or repression.

[36] The logic with regards to omitted food subsidies and international food aid is the same.

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Map 1: Riots in Africa, 1990-2007

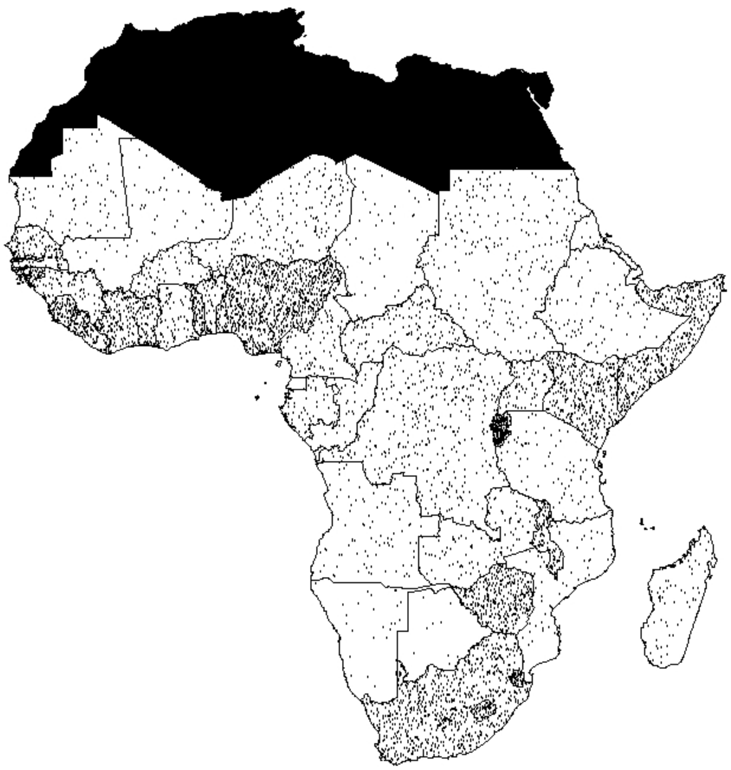
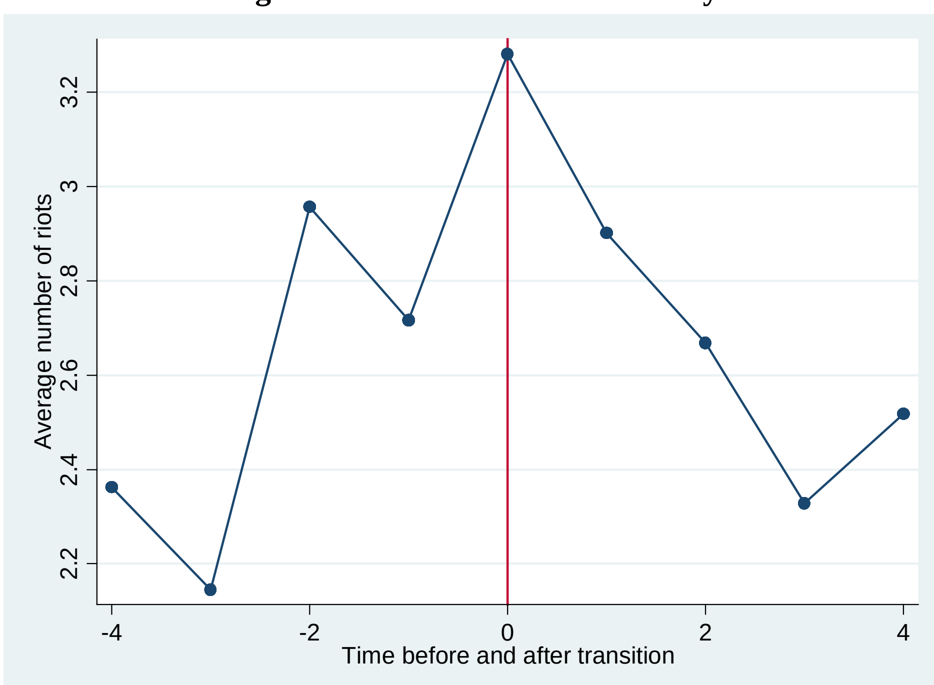


Figure 1: Pre-transition riot activity



Notes: Average number of riots is the log of riots averaged across the countries in the sample. *Transition, binary* is used to identify when *t=0*. The number of observations is not the same in all years.

Table 1: Structural and Reduced Form Regressions

(A) Structural Regressions: Riots and Democratic Change

	(1)	(2)	(3)	(4)
	Transition, $binary_t$	Transition, $count_t$	$Polity2_t$	Transition, $binary_t$
	OLS	OLS	OLS	Conditional Logit
Riots $_{t-1}$, logs	0.014†	0.036**	0.117	0.260
	(0.007)	(0.015)	(0.076)	(0.162)
GDP per capita $growth_{t-1}$	-0.111	0.239	2.453	-2.709
	(0.132)	(0.373)	(1.567)	(4.012)
Polity2 $_{t-1}$			0.710***	
			(0.032)	
Country fixed effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Observations	624	625	597	442

(B) Reduced Form: Drought and Democratic Change

	(1)	(2)	(3)
	Transition, $binary_t$	Transition, $count_t$	$Polity2_t$
	OLS	OLS	OLS
<u>Instrument:</u>			
$Drought_{t-2}$	0.159***	0.339**	0.599
	(0.056)	(0.149)	(0.511)
<u>Controls:</u>			
GDP per capita $growth_{t-1}$	-0.153	0.121	2.434
	(0.128)	(0.376)	(1.753)
$Polity2_{t-1}$			0.713***
			(0.033)
Country fixed effects	Y	Y	Y
Year effects	Y	Y	Y
Observations	640	641	605

Notes: † significance at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors are clustered by country and reported in parentheses. Column 4 in section (A) reports conditional logit coefficients.

Table 2: 2SLS, Riots and Democratic Change

	(1)	(2)	(3)	(4)
	Transition, $binary_t$	Transition, $count_t$	Transition, $binary_t$	Transition, $count_t$
	(A) 2SLS			
Riots $_{t-1}$, logs (instrumented)	0.440	0.936	0.483	1.004
GDP per capita growth $_{t-1}$ (instrumented)	-	-	-0.607	2.119
A-R Wald, F (p-value)	[0.016]	[0.055]	[0.052]	[0.155]
A-R Wald, Chi-sq (p-value)	[0.007]	[0.035]	[0.027]	[0.109]
Stock-Wright LM (p-value)	[0.054]	[0.091]	[0.114]	[0.238]
GDP per capita growth $_{t-1}$	Y	Y	-	-
Country fixed effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Observations	624	625	603	604
	(B) First Stage for Riots $_{t-1}$, logs			
$Drought_{t-2}$	0.380**	0.378**	0.349**	0.346**
	(0.152)	(0.164)	(0.158)	(0.157)
Commodity price index growth $_{t-1}$	-	-	0.043	0.055
			(0.515)	(0.514)
GDP per capita growth $_{t-1}$	Y	Y	-	-
Country fixed effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
F-statistic for instrument; Angrist-Pischke F test when multiple instruments (p-value in parentheses)	6.22 (0.017)	6.15 (0.018)	4.76 (0.036)	4.75 (0.036)
Observations	624	625	603	604
	(C) First Stage for GDP per capita $growth_{t-1}$			
$Drought_{t-2}$			0.003	0.003
			(0.006)	(0.006)
Commodity price index growth $_{t-1}$			-0.039	-0.039
			(0.036)	(0.036)
Country fixed effects			Y	Y
Year effects			Y	Y
Angrist-Pischke F test for multiple instruments (p-value in parentheses)			1.15 (0.290)	1.13 (0.294)
Observations			603	604

Notes: In part (A) we report p-values for three significance tests in brackets; these are tests of the significance of the endogenous regressor(s) in the structural equation, where the null is that it equals (they jointly equal) zero and that the over-identifying restrictions (where relevant) are valid. These tests are robust to weak instruments, and the versions we implement are robust to both heteroskedasticity and within-country correlation in the errors. In parts (B) and (C) standard errors are clustered and reported in parentheses (); † indicates significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 3: 2SLS, Riots and Democratic Change, Concessions Only

	(1)	(2)	(3)	(4)
	Transition, $binary_t$	Transition, $count_t$	Transition, $binary_t$	Transition, $count_t$
	(A) 2SLS			
Riots $_{t-1}$, logs (instrumented)	0.399	0.942	0.435	1.021
GDP per capita $growth_{t-1}$ (instrumented)	-	-	-0.169	0.187
A-R Wald, F (p-value)	[0.026]	[0.057]	[0.082]	[0.135]
A-R Wald, Chi-sq (p-value)	[0.014]	[0.037]	[0.048]	[0.092]
Stock-Wright LM (p-value)	[0.074]	[0.093]	[0.175]	[0.200]
GDP per capita $growth_{t-1}$	Y	Y	-	-
Country fixed effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Observations	624	625	603	604
	(B) First Stage for Riots $_{t-1}$, logs			
$Drought_{t-2}$	0.380**	0.378**	0.349**	0.346**
	(0.152)	(0.152)	(0.158)	(0.157)
Commodity price index growth $_{t-1}$	-	-	0.043	0.055
			(0.515)	(0.514)
GDP per capita $growth_{t-1}$	Y	Y	-	-
Country fixed effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
F-statistic for instrument; Angrist-Pischke F test when multiple instruments (p-value in parentheses)	6.22	6.15	4.76	4.75
	(0.017)	(0.018)	(0.036)	(0.036)
Observations	624	625	603	604
	(C) First Stage for GDP per capita $growth_{t-1}$			
$Drought_{t-2}$			0.003	0.003
			(0.006)	(0.006)
Commodity price index growth $_{t-1}$			-0.039	-0.039
			(0.036)	(0.036)
Country fixed effects			Y	Y
Year effects			Y	Y
Angrist-Pischke F test for multiple instruments (p-value in parentheses)			1.15	1.13
			(0.290)	(0.204)
Observations			603	604

Notes: In this table we only consider transitions where concessions were made by the incumbent; in all other cases the dependent variable is set equal to 0. In part (A) we report p-values for three significance tests in brackets; these are tests of the significance of the endogenous regressor(s) in the structural equation, where the null is that it equals (they jointly equal) zero and that the over-identifying restrictions (where relevant) are valid. These tests are robust to weak instruments, and the versions we implement are robust to both heteroskedasticity and within-country correlation in the errors. In parts (B) and (C) standard errors are clustered and reported in parentheses (); † indicates significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 4: Dynamic Specification with Polity2

	(1)	(2)	(3)
	$Polity2_t$	$Polity2_t$	$Polity2_t$
	Arellano-Bond GMM	**Arellano-Bond GMM**	**LSDV**
Riots $_{t-1}$, logs	0.329	0.492†	0.131†
	(0.236)	(0.292)	(0.067)
GDP per capita growth $_{t-1}$	3.513	21.668	2.851
	(2.477)	(15.938)	(1.828)
$Polity2_{t-1}$	0.916***	0.975***	0.803***
	(0.089)	(0.114)	(0.036)
Country fixed effect	Y	Y	Y
Year effect	Y	Y	Y
Arellano-Bond AR(1) test, p-value	0.000	0.001	0.000
Arellano-Bond AR(2) test, p-value	0.826	0.649	0.812
Excluded Instrument(s):	$Drought_{t-2}$	$Drought_{t-2}$ Commodity price index growth $_{t-1}$	-
Sargan overid test, p-value	-	-	0.990
Hansen overid test, p-value	0.983	0.571	-
Observations	546	529	546

Notes: † significance at the 10% level, ** at the 5% level, *** at the 1% level. The standard errors in the Arellano-Bond specification are consistent in the presence of any pattern of heteroskedasticity and autocorrelation. The Arellano-Bond result is based on using one lag in levels as instruments. The Arellano-Bond (1991) tests for autocorrelation test the null hypothesis of no first and second order serial autocorrelation in the errors against the alternative of AR(1) and AR(2), respectively. LSDV standard errors are bootstrapped with 100 repetitions (but the coefficient on riots stays significant with 10, 20 and 50 repetitions). The LSDV estimation uses Arellano-Bond and not system GMM.

Online Appendix

The Democratic Window of Opportunity: Evidence from Riots in sub-Saharan Africa

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1 Political transition variables

We use the Polity IV database to construct three different and complementary measures of political change. The Polity IV database contains a number of different indicators of political authority, and is built around the *polity IV index*. This index codes three key aspects of a country's political system: i) competitiveness and openness in the process of executive recruitment, ii) constraints on the chief executive, and iii) competitiveness and regulation of political participation. A weighted sum of the components is used to construct two summary variables, measuring democracy on a scale of 0 to 10 (the

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DEMOC score) and autocracy on a scale of -10 to 0 (the AUTOC score). The *polity IV index* is the sum of these two sub-indexes.

We use this database to construct three different measures of democratic change. Our first two variables, $transition, binary$ and $transition, count$, are based on the *regtrans* variable. The Polity IV manual (p.35) defines *regtrans* as follows: "The Polity has undergone a substantive regime transition between period $t-1$ and t, defined as a 'regime change'. A 'regime change' is defined simply as a three-point change in either the polity's DEMOC or AUTOC score." A score of +3 corresponds to a "major democratic transition" defined as an increase of six points or more in the Polity IV score over a period of three years or less, including a shift from an autocratic Polity IV value (-10 to 0) to a partial democratic Polity IV value (+1 to +6) or to a full democratic Polity IV value (+7 to +10); or a shift from a partial democratic value to a full democratic value. A score of +2 corresponds to a "minor regime change", defined as a three to five point increase in the Polity IV index over a period of three years or less, including a shift from autocratic to partial democratic. A score of +1 corresponds to a "positive regime change", defined as an increase of three or more points in the Polity IV score without a shift in regime type, as defined for scores of +2 and +3. A score of -1 corresponds to a "negative regime change", defined as a 3 to 5 point drop in the Polity IV index. A score of -2 corresponds to an "adverse regime transition", defined as a six or more point decrease in the Polity IV index, or a collapse of central state authority or a revolutionary transformation in the mode of governance (not a democratic transition). We also use the $polity2$ variable, which is a version of the *Polity IV index* that has been corrected to allow for use in time series analysis.

2 Urban Riots

2.1 Baseline

In our paper we argue that riots threaten the incumbent regime, which may then respond by making democratic concessions. Urban riots are often viewed as a much more serious threat to the government than rural riots, and so we check whether our results are robust to focusing only on that type of riot. If our results go away, then this would raise some doubts regarding the interepretation of our results. To determine which riots happen in urban areas, we use the GIS map "1km 2010 Africa population distribution" from Afripop, which is a digital map with 1 square kilometer cells that record the population in that area. These data are shown in Map A1. Using our GIS riots data, we can then assign a population value to each riot, which we then turn into a binary variable where an area is considered to be urban if the population is more than 100 individuals per square kilometer.

[Map A1: Population Density in Africa, 2010]

Our results are presented in Table A4. We find that the coefficient on urban riots in column 1 is similar to those in column 3 in Table 2, and that the endogenous variables are jointly significant according to two of the three p-values reported. The results in column 2 are also similar to those in column 4 in Table 2. In Table A5 we run the Arellano-Bond GMM and the LSDV, and the coefficients are positive, although not significant.

[Table A4: 2SLS, Urban Riots]

[Table A5: Dynamic Specification, Urban Riots]

However, it is unclear where the boundary between urban and rural should lie, and in fact it is likely that such discrete labelling is not appropriate. To address this issue we re-estimate our specification using three different weighted riots measures, where more weight is given to riots that happen in more populated areas, closer to the capital city, or in countries where the population is more concentrated around the capital.

2.2 Weighted Riots Results

We first create a riot measure where riots are given weights that depend on the population of the location in which they occur. As a result, riots that happen in more urbanized areas carry a larger weight. We again use the GIS map "1km 2010 Africa population distribution" from Afripop, and assign a population density to each riot, with the log of population density becoming their weight.[1]

Table A6 shows the results for the 2SLS. The middle of the table shows the first stage results for riots weighted by population density, where the F-stat is quite low; the bottom of the table shows the first stage for GDP per capita growth. The top of the table presents the results for the 2SLS: when using the binary variable, the coefficient is 0.594, which is similar to what we found before, and significant according to two of the three p-values. The second column, which looks at the count variable, has a large coefficient. Table A7 presents the results for the dynamic specification using polity2, where little changes relative to Table 4 in the paper.

[Table A6: Riots weighted by Population Density]

[Table A7: Dynamics]

However, riots in and near the capital city may be more threatening to the regime

[1] We use logs because population density is highly skewed, with a few cities (e.g., Lagos) having very large values.

than riots further away, even if some of these regions are heavily populated. To consider this issue we create a new weighted riots variable, where the weights are now given by the inverse of the log distance between the location of the riots and the capital city of the country in which they happen. Riots that happen closer to the capital city receive a larger weight. Data for the location of the capital cities are from the CShapes dataset (Wedmann, Kuse and Gleditsch, 2010). Table A8 presents the results: although the first stage results are quite similar, the coefficients on the second stage are twice as large. Table A9 presents the results for the dynamic specification using polity2, where the coefficients are twice as large as those in Table A7.

[Table A8: Riots weighted by Distance to Capital City]

[Table A9: Dynamics]

Finally, Do and Campante (2009) argue that the concentration of population around the capital city can act as a substitute for governance institutions, providing checks on the behavior of (autocratic) rulers.[2] The theory is that population concentration in and around the capital makes it easier for opposition groups to channel popular unrest into a regime challenge. Building on this logic, we conjecture that riots represent a greater threat in countries where people live closer to the center of power. To capture this empirically, we create a weighted riots measure where the weight is equal to the gravity-based measure of population concentration around the capital city, *centrality*, constructed by Campante and Do (2010).[3] The results are presented in Tables A10

[2] To substantiate this claim, Do and Campante (2009) report a positive association between population concentration near the capital city and better governance and more redistribution.

[3] Campante and Do (2010) construct two centered indices of spatial concentration, PCI_1 and PCI_2. We use PCI_2, which is normalized by population size and the maximum distance within each country. Using PCI_1 instead does not change our qualitative results. The index is available for 1990, 1995 and 2000, but shows little within country variation over time, so we use the index value in 1990 in the estimations.

and A11, and are similar to those when riots are weighted by their distance to the capital (Tables A8 and A9).

[Table A10: Riots weighted by Centrality]

[Table A11: Dynamics]

References

[1] Do, Quoc-Anh and Filipe Campante, 2009. "Keeping Dictators Honest: The Role of Population". Research Collection, School of Economics, Paper 1135.

[2] Campante, Filipe and Quoc-Anh Do, 2010. "A Centered Index of Spatial Concentration: Expected Influence Approach and Application to Population and Capital Cities". Unpublished manuscript.

[3] Weidmann, Nils B., Doreen Kuse and Kristian Skrede Gleditsch. 2010. "The Geography of the International System: TheCShapes Dataset." *International Interactions* 36(1).

ONLINE APPENDIX

Table A1: Countries in Dataset

Angola	Ghana	Nigeria
Benin	Guinea	Rwanda
Botswana	Guinea-Bissau	Senegal
Burkina Faso	Ivory Coast	Sierra Leone
Burundi	Kenya	Somalia
Central African Rep	Lesotho	South Africa
Cameroon	Liberia	Sudan
Chad	Madagascar	Swaziland
Republic of Congo	Malawi	Togo
DR Congo	Mali	Tanzania
Djibouti	Mauritania	Uganda
Ethiopia	Mozambique	Zambia
Gabon	Namibia	Zimbabwe
The Gambia	Niger	

Table A2: Summary Statistics

	Mean	Standard Deviation	Min.	Max.	Observations
Transition, binary	0.057	0.232	0	1	757
Transition, count	0.083	0.531	-2	3	758
Polity2	-0.476	5.616	-10	9	712
Riots, logs	2.509	1.744	0	7.249	700
Drought	0.199	0.400	0	1	758
GDP per capita growth	0.005	0.063	-0.699	0.316	680
Commodity price index growth	0.023	0.175	-0.382	0.744	637

Notes: *Transition, binary* refers to a modified version of the *regtrans* variable in the Polity IV database, and it equals one if a political change was initiated in that year (as captured by *regtrans*), and zero otherwise. *Transition, count* takes an integer value between -2 and 3 and captures the direction and size of the transition, but only in its first year. *Riots* are calculated by adding up the duration (in days) of all riots that happened in a given year; different riots are counted individually even if they occurred on the same day. *Drought* is a dummy variable equal to one if the average rainfall in the year was in the bottom quintile of the rain distribution for Sub-Saharan Africa. *GDP per capita growth* is calculated using data from the World Development Indicators (2011), while *commodity price index growth* is from Brückner and Ciccone (2010) and is calculated using country-specific baskets of exported commodities. We include data from 1989 for the variables for which they are available (i.e., not for riots).

Table A3: Robustness to using different lags of GDP per capita growth

	(1)	(2)	(3)	(4)
	Transition, $binary_t$	Transition, $count_t$	Transition, $binary_t$	Transition, $count_t$
	All	All	Only Concessions	Only Concessions
	(A) Robustness to using GDP per capita growth at *t*			
	2SLS			
Riots $_{t-1}$, logs (instrumented)	0.440	0.935	0.399	0.942
A-R Wald, F (p-val)	[0.016]	[0.054]	[0.026]	[0.057]
A-R Wald, Chi-sq (p-val)	[0.007]	[0.035]	[0.014]	[0.037]
Stock-Wright LM (p-val)	[0.054]	[0.091]	[0.074]	[0.093]
GDP p/cap_t growth	Y	Y	Y	Y
Country Fixed Effects	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Observations	624	625	624	625
	First Stage for Riots $_{t-1}$, logs			
$Drought_{t-2}$	0.380**	0.377**	0.380**	0.377**
GDP p/cap_t growth	Y	Y	Y	Y
Country Fixed Effects	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
F statistic for instrument	6.13	6.06	6.13	6.06
(p-val in parentheses)	(0.018)	(0.019)	(0.018)	(0.019)
Observations	624	625	624	625
	(B) Robustness to using GDP per capita growth at *t-2*			
	2SLS			
Riots $_{t-1}$, logs (instrumented)	0.391	0.811	0.360	0.834
A-R Wald, F (p-val)	[0.052]	[0.151]	[0.077]	[0.143]
A-R Wald, Chi-sq (p-val)	[0.033]	[0.119]	[0.053]	[0.111]
Stock-Wright LM (p-val)	[0.090]	[0.169]	[0.120]	[0.163]
GDP p/cap_{t-2} growth	Y	Y	Y	Y
Country Fixed Effects	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Observations	586	587	586	587
	First Stage for Riots $_{t-1}$, logs			
$Drought_{t-2}$	0.353†	0.350†	0.353†	0.350†
	(0.184)	(0.184)	(0.184)	(0.184)
GDP p/cap_{t-2} growth	Y	Y	Y	Y
Country Fixed Effects	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
F statistic for instrument	3.70	3.64	3.70	3.64
(p-val in parentheses)	(0.062)	(0.064)	(0.062)	(0.064)
Observations	586	587	586	587

Notes: † significance at the 10% level, ** at the 5% level, *** at the 1% level. Clustered standard errors in parentheses (). We report p-values for three significance tests in brackets; these are tests of the joint significance of the endogenous regressor in the structural equation, where the null is that it equals zero and that the over-identifying restrictions (where relevant) are valid. Their primary appeal in our case lies in that they are robust to weak instruments, and the versions we implement are robust to both heteroskedasticity and within-country correlation in the errors.

Map A1: Population Density in Africa, 2010

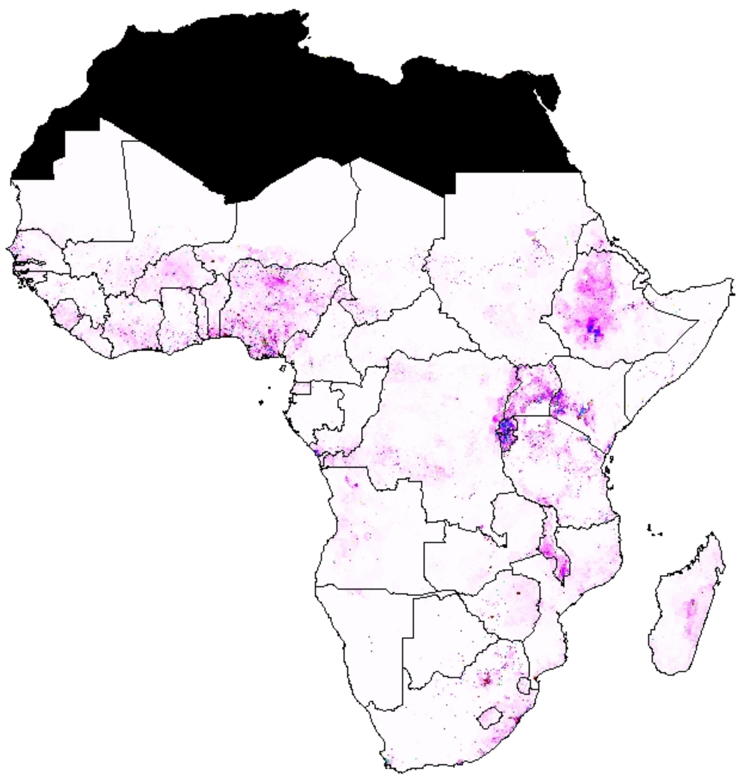

Table A4: 2SLS, Urban Riots

	(1)	(2)	(3)	(4)
	Transition, $binary_t$	Transition, $count_t$	Transition, $binary_t$	Transition, $count_t$
	All	All	Only Concessions	Only Concessions
	2SLS			
Urban Riots $_{t-1}$, logs (instrumented)	0.474	0.983	0.427	1.001
GDP p/cap_{t-1} growth (instrumented)	2.209	7.806	2.367	5.975
A-R Wald, F (p-val)	[0.052]	[0.155]	[0.082]	[0.135]
A-R Wald, Chi-sq (p-val)	[0.027]	[0.109]	[0.048]	[0.092]
Stock-Wright LM (p-val)	[0.114]	[0.238]	[0.175]	[0.200]
Country Fixed Effects	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Observations	603	604	603	604
	First Stage for Urban Riots $_{t-1}$, logs			
$Drought_{t-2}$	0.339	0.338	0.339	0.338
	(0.211)	(0.211)	(0.211)	(0.211)
Commodity Price Index growth $_{t-1}$	.276	0.279	0.276	0.279
	(0.423)	(0.424)	(0.423)	(0.424)
Country Fixed Effects	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Angrist-Pischke F test for excluded instruments (p-val in parentheses)	2.80	2.78	2.80	2.78
	(0.103)	(0.104)	(0.103)	(0.104)
Observations	603	604	603	604
	First Stage for GDP p/cap $growth_{t-1}$			
$Drought_{t-2}$	.003	0.003	0.003	0.003
	(.006)	(0.006)	(0.006)	(0.006)
Commodity Price Index growth $_{t-1}$	-.039	-0.039	-0.039	-0.039
	(.036)	(0.036)	(0.036)	(0.036)
Country Fixed Effects	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Angrist-Pischke F test for excluded instruments (p-val in parentheses)	1.17	1.14	1.17	1.14
	(.287)	(.292)	(0.287)	(0.292)
Observations	603	604	603	604

Notes: † significance at the 10% level, ** at the 5% level, *** at the 1% level. Clustered standard errors in parentheses (). We report p-values for three significance tests in brackets; these are tests of the joint significance of the endogenous regressors in the structural equation, where the null is that they equal zero and that the over-identifying restrictions (where relevant) are valid. Their primary appeal in our case lies in that they are robust to weak instruments, and the versions we implement are robust to both heteroskedasticity and within-country correlation in the errors.

Table A5: Dynamic Specification, Urban Riots

	(1)	(2)
	Polity2$_t$	Polity2$_t$
	Arellano-Bond GMM	**LSDV**
Urban Riots $_{t-1}$, logs	0.707	0.053
	(0.605)	(0.130)
GDP p/cap growth $_{t-1}$	18.872	2.625
	(13.801)	(1.809)
Polity2$_{t-1}$	1.020***	0.803***
	(0.126)	(0.036)
Country Fixed Effect	Y	Y
Year Effect	Y	Y
Arellano-Bond AR(1) test	p-val: 0.001	p-val: 0.000
Arellano-Bond AR(2) test	p-val: 0.758	p-val: 0.797
Excluded Instrument:	Drought$_{t-2}$ Commodity Price Index growth $_{t-1}$	
Sargan overid test, p-val	-	0.988
Hansen overid test, p-val	0.442	-
Observations	529	546

Notes: † significance at the 10% level, ** at the 5% level, *** at the 1% level. The standard errors in the Arellano-Bond specification are consistent in the presence of any pattern of heteroskedasticity and autocorrelation. The Arellano-Bond result is based on using one lag in levels as instruments. The Arellano-Bond (1991) tests for autocorrelation test the null hypothesis of no first and second order serial autocorrelation in the errors against the alternative of AR(1) and AR(2), respectively. LSDV standard errors are bootstrapped with 100 repetitions (but the coefficient on riots stays significant with 10, 20 and 50 repetitions). The LSDV estimation uses A-B and not system GMM.

Table A6: 2SLS, Riots weighted by Population and Democratic Change

	(1)	(2)
	Transition, $binary_t$	Transition, $count_t$
	2SLS	
Riots weighted by pop $_{t-1}$, logs (instrumented)	0.594	1.239
GDP p/cap growth $_{t-1}$ (instrumented)	3.690	11.288
A-R Wald, F (p-val)	[0.052]	[0.155]
A-R Wald, Chi-sq (p-val)	[0.027]	[0.109]
Stock-Wright LM (p-val)	[0.114]	[0.238]
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Observations	603	604
	First Stage for Riots weighted by population $_{t-1}$, logs	
$Drought_{t-2}$	0.264	0.261
	(0.175)	(0.175)
Commodity Price Index growth $_{t-1}$	0.317	0.330
	(0.565)	(0.563)
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Angrist-Pischke F test for excluded instruments (p-val in parentheses)	2.64 (0.113)	2.60 (0.115)
Observations	603	604
	First Stage for GDP p/cap growth $_{t-1}$	
$Drought_{t-2}$	0.003	0.003
	(0.006)	(0.006)
Commodity Price Index growth $_{t-1}$	-0.039	-0.039***
	(0.036)	(0.036)
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Angrist-Pischke F test for excluded instruments (p-val in parentheses)	1.16 (0.288)	1.14 (0.294)
Observations	603	604

Notes: †significance at the 10% level, ** at the 5% level, *** at the 1% level. Clustered standard errors in parentheses (). Riots are weighted by population density: we use the log of the population density in the location of the riot as a weight, with greater population resulting in a larger weight. We report p-values for three significance tests in brackets; these are tests of the joint significance of the endogenous regressors in the structural equation, where the null is that they equal zero and that the over-identifying restrictions (where relevant) are valid. Their primary appeal in our case lies in that they are robust to weak instruments, and the versions we implement are robust to both heteroskedasticity and within-country correlation in the errors.

Table A7: Dynamic Specification,
Riots weighted by Population and Democratic Change

	(1)	(2)
	Polity2$_t$	Polity2$_t$
	Arellano-Bond GMM	**LSDV**
Riots $_{t-1}$, logs	0.478†	0.128**
	(.286)	(0.062)
GDP p/cap growth $_{t-1}$	21.362	2.823
	(15.910)	(1.814)
Polity2$_{t-1}$	0.994***	0.804***
	(0.114)	(0.036)
Country Fixed Effect	Y	Y
Year Effect	Y	Y
Arellano-Bond AR(1) test	p-val: 0.001	p-val: 0.000
Arellano-Bond AR(2) test	p-val: 0.601	p-val: 0.797
Excluded Instrument:	Drought$_{t-2}$ Commodity Price Index growth $_{t-1}$	
Sargan overid test	-	p-val: 0.990
Hansen overid test	p-val: 0.599	-
Observations	529	546

Notes: †significance at the 10% level, ** at the 5% level, *** at the 1% level. The standard errors in the Arellano-Bond specification are consistent in the presence of any pattern of heteroskedasticity and autocorrelation. The Arellano-Bond result is based on using one lag in levels as instruments. The Arellano-Bond (1991) tests for autocorrelation test the null hypothesis of no first and second order serial autocorrelation in the errors against the alternative of AR(1) and AR(2), respectively. LSDV standard errors are bootstrapped with 100 repetitions (but the coefficient on riots stays significant with 10, 20 and 50 repetitions). The LSDV estimation uses A-B and not system GMM.

Table A8: 2SLS, Riots weighted by Distance to Capital City and Democratic Change

	(1)	(2)
	Transition, $binary_t$	Transition, $count_t$
	2SLS	
Riots weighted by $dist_{t-1}$, logs (instrumented)	1.132	2.370
GDP p/cap growth $_{t-1}$ (instrumented)	3.291	10.793
A-R Wald, F (p-val)	[0.052]	[0.155]
A-R Wald, Chi-sq (p-val)	[0.027]	[0.109]
Stock-Wright LM (p-val)	[0.114]	[0.238]
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Observations	603	604
	First Stage for Riots weighted by distance $_{t-1}$, logs	
$Drought_{t-2}$	0.139†	0.137†
	(0.074)	(0.074)
Commodity Price Index growth $_{t-1}$	0.153	0.164
	(0.304)	(0.303)
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Angrist-Pischke F test for excluded instruments (p-val in parentheses)	3.87	3.78
	(0.057)	(0.060)
Observations	603	604
	First Stage for GDP p/cap growth $_{t-1}$	
$Drought_{t-2}$	0.003	0.003
	(0.006)	(0.006)
Commodity Price Index growth $_{t-1}$	-0.039	-0.039
	(0.036)	(0.036)
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Angrist-Pischke F test for excluded instruments (p-val in parentheses)	1.16	1.14
	(0.288)	(0.293)
Observations	603	604

Notes: †significance at the 10% level, ** at the 5% level, *** at the 1% level. Clustered standard errors in parentheses (). Riots are weighted by distance to the capital city: we use the inverse of the log distance to the capital as the weight, with a greater distance resulting in a lower weight. We report p-values for three significance tests in brackets; these are tests of the joint significance of the endogenous regressors in the structural equation, where the null is that they equal zero and that the over-identifying restrictions (where relevant) are valid. Their primary appeal in our case lies in that they are robust to weak instruments, and the versions we implement are robust to both heteroskedasticity and within-country correlation in the errors.

Table A9: Dynamic Specification, Riots weighted by Distance to Capital City and Democratic Change

	(1)	(2)
	Polity2$_t$	Polity2$_t$
	Arellano-Bond GMM	**LSDV**
Riots $_{t-1}$, logs	1.015	0.216†
	(0.646)	(0.127)
GDP p/cap growth $_{t-1}$	21.954	2.859
	(15.433)	(1.830)
Polity2$_{t-1}$	0.959***	0.803***
	(0.120)	(0.036)
Country Fixed Effect	Y	Y
Year Effect	Y	Y
Arellano-Bond AR(1) test	p-val: 0.001	p-val: 0.000
Arellano-Bond AR(2) test	p-val: 0.656	p-val: 0.786
Excluded Instrument:	Drought$_{t-2}$ Commodity Price Index growth $_{t-1}$	
Sargan overid test	-	p-val: 0.989
Hansen overid test	p-val: 0.591	-
Observations	529	546

Notes: †significance at the 10% level, ** at the 5% level, *** at the 1% level. The standard errors in the Arellano-Bond specification are consistent in the presence of any pattern of heteroskedasticity and autocorrelation. The Arellano-Bond result is based on using one lag in levels as instruments. The Arellano-Bond (1991) tests for autocorrelation test the null hypothesis of no first and second order serial autocorrelation in the errors against the alternative of AR(1) and AR(2), respectively. LSDV standard errors are bootstrapped with 100 repetitions (but the coefficient on riots stays significant with 10, 20 and 50 repetitions). The LSDV estimation uses A-B and not system GMM.

Table A10: 2SLS, Riots weighted by Centrality and Democratic Change

	(1)	(2)
	Transition, binary$_t$	Transition, count$_t$
	2SLS	
Riots weighted by cent$_{t-1}$, logs (instrumented)	0.797	1.662
GDP p/cap growth $_{t-1}$ (instrumented)	0.857	5.440
A-R Wald, F (p-val)	[0.052]	[0.155]
A-R Wald, Chi-sq (p-val)	[0.027]	[0.109]
Stock-Wright LM (p-val)	[0.114]	[0.238]
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Observations	603	604
	First Stage for Riots weighted by centrality $_{t-1}$, logs	
Drought$_{t-2}$	0.206**	0.204**
	(0.098)	(0.098)
Commodity Price Index growth $_{t-1}$	0.098	0.110
	(0.383)	(0.382)
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Angrist-Pischke F test for excluded	4.52	4.46
instruments (p-val in parentheses)	(0.041)	(0.042)
Observations	603	604
	First Stage for GDP p/cap growth $_{t-1}$	
Drought$_{t-2}$	0.003	0.003
	(0.006)	(0.006)
Commodity Price Index growth $_{t-1}$	-0.039	-0.039
	(0.036)	(0.036)
Country Fixed Effects	Y	Y
Year Effects	Y	Y
Angrist-Pischke F test for excluded	1.16	1.14
instruments (p-val in parentheses)	(0.288)	(0.293)
Observations	603	604

Notes: †significance at the 10% level, ** at the 5% level, *** at the 1% level. Clustered standard errors in parentheses (). Riots are weighted by centrality, where the weight is set equal to centrality. Since centrality is a country-specific and time-invariant measure, all riots within a country get the same weight, with a higher centrality resulting in a larger weight. We report p-values for three significance tests in brackets; these are tests of the joint significance of the endogenous regressors in the structural equation, where the null is that they equal zero and that the over-identifying restrictions (where relevant) are valid. Their primary appeal in our case lies in that they are robust to weak instruments, and the versions we implement are robust to both heteroskedasticity and within-country correlation in the errors.

Table A11: Dynamic Specification, Riots weighted by Centrality and Democratic Change

	(1)	(2)
	$Polity2_t$	$Polity2_t$
	Arellano-Bond GMM	**LSDV**
Riots $_{t-1}$, logs	0.707	0.167†
	(0.434)	(0.094)
GDP p/cap growth $_{t-1}$	21.609	2.858
	(15.652)	(1.826)
$Polity2_{t-1}$	0.969***	0.803***
	(0.116)	(0.036)
Country Fixed Effect	Y	Y
Year Effect	Y	Y
Arellano-Bond AR(1) test	p-val: 0.001	p-val: 0.000
Arellano-Bond AR(2) test	p-val: 0.606	p-val: 0.787
Excluded Instrument:	$Drought_{t-2}$ Commodity Price Index growth $_{t-1}$	
Sargan overid test	-	p-val: 0.990
Hansen overid test	p-val: 0.569	-
Observations	529	546

Notes: †significance at the 10% level, ** at the 5% level, *** at the 1% level. The standard errors in the Arellano-Bond specification are consistent in the presence of any pattern of heteroskedasticity and autocorrelation. The Arellano-Bond result is based on using one lag in levels as instruments. The Arellano-Bond (1991) tests for autocorrelation test the null hypothesis of no first and second order serial autocorrelation in the errors against the alternative of AR(1) and AR(2), respectively. LSDV standard errors are bootstrapped with 100 repetitions (but the coefficient on riots stays significant with 10, 20 and 50 repetitions). The LSDV estimation uses A-B and not system GMM.