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# Urban Geography and Protest Mobilization in Africa

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## Abstract

Urbanisation has long been seen by scholars and policymakers as a disruptive process that can contribute to social and political unrest, yet there is little cross-national quantitative empirical research on the topic. In this paper we provide a comprehensive analysis of the links between urban geography and the incidence of protests (i.e. demonstrations, riots and strikes) in African countries since 1990. In contrast to previous studies, we are careful to distinguish between urban population scale effects, urban population ratio effects, population rate-of-change effects and urban population distribution effects. We also provide an explicit test of the long-standing hypothesis that ‘over-urbanization’ increases the risk of civil unrest. Employing multilevel negative binomial models that control for key political and economic variables we find that urban population size and the number of large cities in a country are both positively and significantly associated protest incidence. By contrast, we find that a country’s level of urbanization is negatively associated with protest incidence and reject the over-urbanization hypothesis: higher levels of urbanization are associated with less frequent protests at all income levels. We find no evidence that the pace of urban population growth or urban primacy significantly influence protest mobilization. In sum, our results provide a nuanced picture of the relationship between urban geography and protest incidence that challenges conventional wisdom and contemporary hyperbole about the dangers of ‘rapid urbanization’ in Africa in particular, and developing countries more generally.

## Key words

Urbanization, protest, civil unrest, democratization, economic development, Africa

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## INTRODUCTION

The process of urbanisation has long been seen by scholars and policymakers as a disruptive process that can contribute to social and political unrest (see Huntington 1968; Cornelius 1969; Pye 1969; Hibbs 1973; Walton and Ragin 1990; Goldstone 2010). In his classic 1968 work *Political Order in Changing Societies* Samuel Huntington argued that “rapid urbanization leads to social dislocation and political instability” in cities in developing countries (2006 [1968], 299). More recently Jack Goldstone has identified urbanization in poor countries as one of the key ‘mega-trends’ shaping global security risks in the twenty first century, claiming that “the more heavily urbanized, the more [poor countries] are likely to experience Dickensian poverty and anarchic violence” (2010, 39). Yet despite this longstanding interest in the links between urbanization and civil unrest there has been little cross-national quantitative empirical research on the topic. In this paper we make a contribution to addressing this gap in the literature by examining the complex relationships between urban geography and protest mobilisation in African countries between 1990 and 2013. We focus on Africa for two reasons. First, countries across the continent have experienced the highest rates of urban population growth on average in the world in recent decades. Second, it is the only continent for which comprehensive, comparable and transparent national-level data on protest activity are available.

Our analysis offers two contributions to the literature. First, we provide a comprehensive discussion and empirical investigation of the links between various aspects of urban geography and the likelihood of protests, including public demonstrations, riots and strikes. In contrast to previous studies of contentious collective action events such as protests, which often incorporate one or two urban demographic variables without controlling for others, we are careful to explicitly distinguish between urban population scale effects, urban population ratio effects, population rate-of-change effects and population distribution effects. We also provide a

direct test of the long-standing hypothesis that ‘over-urbanization’ (i.e. urbanization without economic development) increases the likelihood of outbreaks of civil unrest.

Our second innovation is the use of a multilevel modelling strategy that allows us to distinguish between a) the effects of changes in individual variables within countries over time, and b) the effects of variation in key explanatory variables across countries. This is in contrast to previous empirical studies, which generally employ a country fixed-effects approach. Fixed effects models only consider within country difference, and do not allow differences between countries to be considered, meaning their presentation of the processes at hand are always incomplete in comparison to our approach.

Multilevel negative binomial models controlling for key political and economic variables show that urban population size and the number of large cities in a country are both positively and significantly associated with the frequency of protest events, as expected. Conversely, we find that a country’s level of urbanization is negatively and significantly associated with protest incidence. We also reject the over-urbanization hypothesis (at least with regard to protest activity): interaction terms designed to explicitly test this hypothesis indicate that levels of urbanization are associated with less frequent outbreaks of unrest at every level of income. We find no evidence that the pace of urban population growth or urban primacy significantly influence the frequency of protest mobilization. In sum, our results provide a nuanced picture of the relationship between urban geography and protest incidence that challenges conventional wisdom and contemporary hyperbole about the dangers of rapid ‘urbanization’ in Africa in particular, and developing countries more generally. This more nuanced perspective hinges on recognising the substantive difference between population ratio, scale, rate-of-change and distribution effects.

The remainder of the paper is organised as follows. Section two reviews the existing theoretical and empirical literature on the links between urban geography and civil unrest. It also provides a cursory review of key political and economic variables associated with civil unrest. Section

three summarises the variables used in our models and describes our multilevel binomial estimation strategy. Section four summaries our key results and section five concludes.

## URBAN GEOGRAPHY & PROTEST MOBILIZATION: A REVIEW OF THEORY AND EVIDENCE

Existing literature on the causes of contentious collective action events such as protests offers a diverse range of theories which can roughly be grouped into four categories: grievance-based approaches, resource mobilization theory, political opportunity approaches, and modernization theory (Chenoweth and Ulfelder 2015). In turn, these four broad approaches can be intuitively summarised as seeking to identify how the motives, means and opportunities for contentious collective action shape the frequency and intensity of events such as protests.

Theoretically there are many ways in which the size and distribution of populations, as well as changes in these variables, may affect the motives, means and opportunities for collective mobilization. In order to provide a structured approach to analysing these relationships we distinguish between four separate types of effects: population ratio effects, population scale effects, rate of population change effects and population distribution effects.

### Urbanization and Ratio Effects

The term 'urbanization' is used somewhat carelessly in the literature to refer to a range of related but distinct phenomena. As a result, it is conceptualised and operationalised in a variety of ways in empirical research. Here we use the term in the way it is deployed by professional demographers: urbanization refers specifically to the proportion of a country's total population living in urban areas ('level of urbanization'), or the rate at which this proportion is changing ('rate of urbanization'). However, the fact that many authors use the term in a more generic way to refer to the demographic growth and physical expansion of towns and cities can lead to some confusion about causal mechanisms.

Theoretically, the association between a country's level of urbanization and contentious collective action is ambiguous. From a resource mobilization perspective, population concentration mitigates the perennial 'time-distance' costs associated with coordinating collective action thereby making it easier to organise a protest and hence increasing the probability of such an event (Sewell 2001; see also Walton and Ragin 1990; Glaeser and DiPasquale 1998; Herbst 2009; Staniland 2010; Wallace 2013). In more urbanized countries there may also be a lower probability of being detected or punished by a repressive political regime than in a less urbanized country, which might reduce the opportunity costs of participation. From a grievance perspective, population concentration creates challenges in terms of public goods delivery and the management of conflicts between diverse groups while at the same time bringing the prosperous and poor into close proximity and throwing socioeconomic inequality into stark relief (Huntington 1968; Cornelius 1969; Walton and Ragin 1990; Blanco and Grier 2009; Goldstone 2010; Wallace 2014). And from a modernization perspective, urbanization is traditionally associated with the emergence of a middle-class that is likely to agitate for enhanced political and economic rights or take to the streets to express their grievances (Huntington 1968; Reissman 1970; Chenoweth and Ulfelder 2015). Through these resource mobilization, grievance and social modernization mechanisms, we might therefore expect a country with a highly urbanized population to experience more protest events than an identical country with a lower level of urbanization.

On the other hand, high levels of urbanisation could plausibly reduce the incidence of contentious mobilization. While population concentration creates challenges, it also yields economies of scale in the provision of public goods (thereby reducing grievances or motives for protest) and facilitates government monitoring and strategic repression, which can reduce opportunities for mobilisation and raises the costs of doing so (Herbst 2000; Collier and Hoeffler 2004; Staniland 2010). Urbanisation can also encourage social integration and the emergence of a unifying nationalist sentiment by bringing members of diverse and geographically dispersed communities into close physical contact (Reissman 1970). This may

have the effect of attenuating inter-communal tensions by cultivating personal friendships, intermarriages and economic interdependences between groups. For example, Green (2013) shows that urbanisation has had a statistically significant negative effect on ethnic diversity in Africa, and ethnic diversity is often cited as a structural factor that may increase the likelihood of conflict or clashes (e.g. ethnic riots) between groups.

An increase in the proportion of a population living in urban areas could also affect the composition and behaviour of key political actors in ways that render protests less likely. Huntington (1968) argued that “[sustained] urbanization not only increases the number of slumdwellers, but it also expands and diversifies the middle class, bringing into existence new, more conservative middle-class strata” (301) that may be less likely to take to the streets. In other words, the first generation of urban middle classes may agitate in the streets, but the second is likely to have more to lose by doing so. Moreover, as the share of a nation’s population living in urban areas increases it is rational for political elites in power to cater to urban preferences in order to a) build a broad base of constituents and/or b) mitigate the risk of outbreaks of urban unrest, which could undermine the authority or legitimacy of a ruling regime. The threat of urban unrest has been a concern for rulers since the birth of cities, hence the frequent recourse throughout history to ‘bread and circuses’ to appease urban masses (Ades and Glaeser 1995; Wallace 2013; Anthony and Crenshaw 2014). In the contemporary era of at least nominally democratic mass politics in Africa, the ratio of urban dwellers may well factor into the political calculus of incumbent elites or their opponents. A study of urban political attitudes in Africa by Harding (2010) offers some indirect support for this hypothesis. Using Afrobarometer data, Harding found that urbanites in Africa generally have a more negative view of incumbent political parties than their rural counterparts, but this bias appears to be inversely correlated with the percentage of the population living in urban areas, suggesting that urbanisation has a positive effect on citizens’ political attitudes toward incumbent regimes. Harding speculates that politicians shift their platforms in favour of urban voters as these voters become increasingly (quantitatively) important constituencies.

Empirical evidence on the relationship between levels of urbanisation and protest activity is very thin as the majority of existing studies do not actually directly test this relationship. There are three studies that provide some evidence of a positive relationship between a nation's level of urbanisation and social unrest (see Walton and Ragin 1990; Glaeser and DiPasquale 1998; Chenoweth and Ulfelder 2015). However, the estimation strategies employed in the first two of these studies are questionably simple (both employ tobit models and exclude key political and economic variables), and in all three cases the authors fail to control for urban population *size*, creating ambiguity in the interpretation of the 'urbanization' coefficient. This is evident in the results of a recent study by Anthony and Crenshaw (2014), which finds a U-shaped association between levels of urbanisation and anti-government demonstrations when controlling for national population—a close proxy for urban population. This finding suggests that the frequency of such demonstrations *decreases* with urbanisation up to a point, and then increases at higher levels. Finally, Chenoweth and Ulfelder (2015) find that level of urbanization helps to predict major nonviolent uprisings but do not specify whether the association is positive or negative in their forecasting model.

In sum, the potential effect of urbanization is theoretically ambiguous. In general it is assumed that urbanization increases the likelihood of contentious collective action, but this assumption largely rests on urban population scale effects rather than ratio effects *per se*. In other words, past theorising on the topic has often conflated two distinct phenomena: the growth of urban populations in *relative* terms and the scale and growth of urban populations in *absolute* terms. Indeed, most of the hypothesised positive effects of 'urbanisation' on civil unrest in the literature are better understood as population *scale* effects than population *ratio* effects *per se*.

### Urban Population and Scale Effects

There are very strong theoretical reasons for expecting the absolute size of a country's urban population (and by extension the number of large cities in a country) to have a positive and



significant effect on the frequency of certain kinds of contentious collective action. From a resource mobilisation standpoint, urban population size directly determines the number of people in close proximity and potentially available to organise or participate in a collective action event. Absolute population size also has a more direct potential effect on the probability of detection. The same consideration holds for hypothesised grievance mechanisms. The challenges of providing services, amenities and opportunities in an urban area are more directly linked to the absolute size of the population living in such areas than their share of the national population, and (potentially volatile) socio-cultural diversity is more likely to be correlated with population size than population ratios.

Measures of urbanisation (as a ratio) only indirectly capture information about the scale of urban populations. For example, in a country of 50 million people with 20 percent living in urban areas there are 10 million potential protestors in urban areas; in a country of 200 million people and just 15 percent living in urban areas, there would be 30 million potential protestors living in close proximity. Given the hypothesised mechanisms linking urban population to contentious collective action, we would expect the latter, less-urbanised country to experience more frequent protests due to sheer scale effects.

Taken together, these arguments support the proposition that the size of a nation's urban population exerts a significant positive effect on the frequency of protests in any given country and year by influencing the motives and means for protest mobilisation, as well as lowering the individual costs of participation.

There is fairly robust evidence of such population scale effects. Population size has been shown to be correlated with various indicators of contentious collective action events at the national level (Hibbs 1973; Arce and Bellinger 2007; Collier and Rohner 2008; Anthony and Crenshaw 2014), at the national *urban* level (Hibbs 1973; Kurtz 2004) and with outbreaks of violent conflict at the local level in Africa (Raleigh and Hegre 2009). Studies have also shown that larger

cities tend to have more contentious collective action events than smaller cities (Eisinger 1973; Urdal and Hoelscher 2012; Buhaug and Urdal 2013).

Given the strong theoretical basis for expecting a positive relationship between the size of urban populations and the frequency of contentious collective action events, we expect measures of both urban population size and the number of large cities in a country to be positively and significantly correlated with protest frequency.

### Urban Population Growth and Rate-of-Change Effects

Often when authors discuss links between 'urbanization' and civil unrest the implicit or explicit mechanism cited is social strain and intensification of resource competition due to rapid population growth in urban areas (e.g. Goldstone 2010; Buhaug and Urdal 2013). In other words, it is the rate of change in the urban population (rather than the ratio or size of urban populations), that serves as a source of potential grievance that may stimulate unrest. However, the few published studies that have explicitly tested this 'demographic strain' hypothesis have failed to find evidence of a significant correlation. In an early quantitative study, Hibbs (1973) found no association between rates of urbanisation and an indicator of collective protest in a sample of 58 countries. Walton and Ragin (1990) found urban population growth to be negatively but insignificantly correlated with protest events in a study of austerity protests in developing countries in the 1980s. In a more recent study of political instability in Latin America, Blanco and Grier (2009) found that a country's rate of urbanisation is negatively and significantly correlated with an index of political instability that includes demonstrations, riots and strikes. Most recently, Buhaug and Urdal (2013) find no evidence that population pressure in urban areas is associated with increased risk of urban social disorder events and some evidence of a *negative* association. The authors speculate that there may be an endogenous relationship between these variables: population pressure may be a source of grievance, but

civic disorder may also discourage or prevent people from moving to cities, thereby serving as a countervailing mechanism.

While the failure to find clear empirical support for the demographic strain hypothesis may be due to an endogenous relationship, it could also be a consequence of the way in which this effect has been tested statistically. For example, a country's rate of urbanization (used by Hibbs), which is a measure of the rate of change in a ratio, does not contain explicit information on the actual scale of urban population change. Consequently, this variable does not directly reflect actual population pressure, particularly between countries. Similarly, a country's rate of urban growth could be very high, but the actual number of people added to urban areas comparatively modest. For example, in a country with an urban population of 1 million, a 5% increase in urban population size translates into an additional 50,000 people needing shelter, water, sanitation and employment. By contrast, in a country with an urban population of 70 million (such as Nigeria), an increase of 5% represents an additional 3.5 million urbanites—a profoundly more challenging situation. In the models presented below, we use urban growth as one measure of relative population pressure, but also introduce an alternative measure that explicitly accounts for the absolute size of urban population change to address this potential problem with previous empirical efforts.

### Urban Concentration and Distribution Effects

Several recent studies have argued that urban primacy or a high degree of urban concentration also influences the frequency of contentious collective action events. Generally speaking, urban primacy is measured as the percentage of a nation's population living in the largest (or capital) city. Alternative measures of urban concentration (a more expansive concept) use a subset of the national urban population instead (e.g. the population of the largest city divided by sum of the population of the next four largest cities). Anthony and Crenshaw (2014) argue that "urban primacy promotes political mobilization via the concentration of national economic and political

life” (15) and that the size of a country’s largest city positively affects the likelihood of political mobilization due to the agglomeration of resources and grievances, and by creating a more favourable political opportunity structure for potential protesters. Similarly, Wallace (2014) argues that urban concentration increases the likelihood of contentious collective action events because “Atomized populations, whether in many different cities or rural areas, make coordination and hence collective action against [a] regime less likely” (2014, 24).

Empirically, the evidence is inconclusive. Anthony and Crenshaw (2014) actually find a negative and significant correlation between a measure of urban primacy and the frequency of anti-government demonstrations. However, the relationship appears to be curvilinear: the frequency of demonstrations increases at very high levels of primacy. By contrast, Wallace (2013) finds that the proportion of the urban population concentrated in the largest city is positively associated with the frequency of urban social disturbance events in a selection of African and Asian cities, and with the frequency of contentious collective action events catalogued in the Cross-National Time Series Archive of Arthur Banks (2011). Both Anthony and Crenshaw (2014) and Wallace (2014) also find a positive and significant correlation between the size of a country’s largest city and the frequency of contentious collective action events and claim that this provides corroborating evidence of the primacy/urban concentration hypothesis.

The inconsistency of results across studies is likely due to different models and somewhat dependent variables. Wallace (2013, 2014) only controls for GDP per capita and GDP growth. Such a parsimonious model is problematic given the potential for omitted variable bias. Anthony and Crenshaw provide a more complete model with controls for a range of demographic, economic and political variables, but clear inference is confounded by collinearity of explanatory variables.

On further reflection the hypothesised relationships between primacy or urban concentration on the one hand and contentious collective action on the other essentially hinge on simple scale effects, which need to be accounted for in an empirical model. As noted above, larger cities are

likely to have more protests than smaller ones, and countries with larger urban populations in general are likely to experience more collective action events than demographically smaller ones. Urban concentration may increase the size of a country's primary city, and hence increase the likelihood of collective action events in that particular city, but it doesn't logically follow that the number of events overall will be higher than in an identical country with a more evenly distributed urban population. To illustrate the point, consider the four hypothetical countries in Table 1.

**TABLE 1 Urban primacy and protest: a thought experiment**

Country A and Country B have identical urban populations of 5 million people but different degrees of primacy. In Country A 25 percent of the urban population is in the largest city and the remainder distributed across other urban centres; in Country B 50 percent are concentrated in the largest city. While Country B may experience more protests in the largest city than Country A, in the latter case there are more people spread across other towns and cities than in Country B, which raises the probability of events in those other urban centres. While the difference in population concentration may influence the *geography* of contentious collective action, it is not clear that it should affect the overall *frequency* of such events. This ambiguity regarding the significance of urban concentration on overall event frequency is further illustrated by Country C, which has a lower level of primacy than Country B but the same size primary city and many more people in other urban centres. This country is likely to experience more protests than Country B despite a much lower degree of urban concentration. Finally, Country D is roughly modelled on Nigeria, which has the second highest average number of protest events per annum in our dataset (after Egypt) and yet one of the lowest levels of urban concentration in Africa. As this thought experiment illustrates, once scale effects are factored

into the analysis, urban primacy or urban concentration is very unlikely to affect the overall probability of events in any given country.

In sum, protests are more likely in large cities than small ones and may be even more likely in cities that have special political significance (e.g. capitals). But it doesn't logically follow that the distribution of urban populations will exert a highly significant effect on the overall frequency of contentious collective action events in a country.

### Economic development and 'over-urbanization'

One of the most consistent findings in empirical studies of contentious collective action is an inverse correlation between a country's rate of economic growth and the probability of contentious events ranging from full blown armed rebellions to urban demonstrations and riots (see Glaeser and DiPasquale 1998; Campos and Nugent 2002; Collier and Hoeffler 2004; Kurtz 2004; Miguel, Satyanath and Sergenti 2004; Arce and Bellinger 2007; Blattman and Miguel 2010; Bohlken and Sergenti 2010; Urdal and Hoelscher 2012; Wallace 2013; Wallace 2014). The explanation for this is intuitive: robust growth is associated with favourable economic conditions at household level (hence reducing grievances) and increases the opportunity costs of engaging in contentious activities. It also generates more government revenues that can be used for (popular) public expenditure or bolstering resources for state security (in autocratic regimes). Conversely, low or negative growth decreases opportunity costs, intensifies social strain and may stimulate divisive political mobilization (Bohlken and Sergenti 2010), while potentially undermining a state's ability to deliver services or repress dissent. However, the relationship between *levels* of economic development and contentious collective action is theoretically and empirically less clear.

In the modernization school of thought there is a longstanding view that economic development, when viewed over the long run, is a socially and politically destabilising process (Huntington 1968; Hibbs 1973; Harms and Zink 2005; Chenoweth and Ulfelder 2015). This view

is based on the theory that quantitative and qualitative shifts in economic output erode 'traditional' hierarchies and values, exacerbate socioeconomic stratification and stimulate popular demands for the redistribution of wealth and political authority, particularly among emergent urban middle classes, which (contrary to popular perceptions) is far more likely to organize and participate in protest than the urban poor (Huntington 1968; Nelson 1979). From a resource mobilisation point of view, rising income and the emergence of an organised urban workforce provide the means for effective collective action (Chenoweth and Ulfelder 2015).

Some empirical support for these arguments can be found in recent studies that have assessed the determinants of individual propensities to engage in protest. For example, in a study of self-reported protest participation based on Afrobarometer data from 18 African countries Pilati (2011) found that educational attainment, employment and membership in a trade union, professional association or community development association—factors associated with middle-class status—are all positively and significantly correlated with protest participation. In a similar study of protest participation in Latin America, Machado, Scartascini and Tommasi (2011) found education, personal income and 'civic engagement' to be positively associated with protest participation.

However, from a grievance point of view, higher incomes might be expected to exert a negative effect on contentious collective action and there is clear evidence in studies of armed conflict that this is the case. Scores of studies have consistently demonstrated that income level and the incidence of armed conflict are negatively correlated (Blattman and Miguel 2010). But the evidence is less clear-cut in the case of other forms of contentious collective action. While Glaeser and DiPasquale (1998) report a negative and significant association between income and riots in a cross-country analysis, Collier and Rohner (2008) show a positive and significant correlation between GDP per capita on the one hand and demonstrations, riots and strikes on the other. Buhaug and Urdal (2013) show mixed results, with GDP per capita negatively and significantly associated with lethal urban social disturbance events but uncorrelated with non-

lethal events, while Anthony and Crenshaw (2014) find a curvilinear relationship between income and anti-government demonstrations. Other studies have generally found no statistically significant correlation between income and the incidence of various forms of contentious collective action other than armed rebellions (Walton and Ragin 1990; Bratton and van de Walle 1997; Bohlken and Sergenti 2010; Cunningham and Lemke 2011; Urdal and Hoelscher 2012; Wallace 2014).

The lack of a clear correlation may be due to a tendency to conflate different types of contentious collective action events into a single dependent variable. For example, the most popular dataset of contentious events other than civil wars has been the Banks (2011) data, and many authors combine the reported number of strikes, demonstrations, riots, coups, assassinations, purges, government crises, revolutions and anti-government demonstrations. Similarly, the urban social disorder dataset employed by Urdal and Hoelscher (2012) and Buhaug and Urdal (2013) combines events such as demonstrations, riots, terrorist attacks and armed conflict. We hypothesise that the relationship between levels of income and contentious collective action is contingent on both the type of action and level of economic development. Where participation involves a sustained commitment to organised violence (e.g. armed rebellion), income is negatively correlated with the incidence of such events. By contrast, where participation is spontaneous and the probable level of violence is less than lethal, we expect income to exert a positive effect at low levels of development (through the resource mobilization channel) but taper off at higher levels of development, as accumulated wealth attenuates grievances and raises the opportunity costs of taking to the streets.

We are also interested in a hypothesised interaction effect between levels of urbanisation and levels of economic development on the incidence of protest mobilisation. Put simply, it has long been argued that increases in urbanization without commensurate economic development – or ‘over-urbanization’ – creates a uniquely volatile situation that raises the likelihood of contentious political mobilisation and instability (Huntington 1968; Goldstone 2002). However,



we found no direct test of this hypothesis in the published quantitative empirical literature. We therefore explicitly test this theory in our empirical analysis below.

### Political Institutions and Processes

In our core models we include a suite of variables to capture the influence of political institutions and processes, which play a pivotal role in shaping the opportunities and motives for contentious collective action. In countries with autocratic regimes, where popular participation in political affairs and open competition for political office are not permitted, protests are likely to be rare given the lack of organizations available to mobilise interest groups and the high opportunity costs facing potential participants (e.g. imprisonment, torture or death). By contrast, the 'political opportunity structure' is more favourable to protests in democratic countries where civil society organizations are allowed to operate independently, political competition is permitted, and free speech is protected (Tilly and Tarrow 2007; Chenoweth and Stephan 2011). All other things equal, democracies are therefore likely to be more prone to protests than autocracies. There is some evidence to support this proposition: in cross-country quantitative studies Glaeser and DiPasquale (1998) found that dictatorships experience fewer riots than non-dictatorships, while Collier and Rohner (2008) show that democracies generally have more demonstrations, riots and strikes than non-democracies, but that wealthier democracies have fewer protests than poorer ones.

There are, however, a diverse range of political regime types that exist in the space between archetypal autocracies and democracies. Nearly all countries in sub-Saharan Africa fall somewhere in between these extremes, and the 'hybrid' nature of these regimes can provide motivation for collective mobilisation. In countries with nascent democratic institutions, which are not yet perceived to be wholly legitimate, the mismatch between public expectations and the actual performance of *de jure* institutions may drive citizens into the street (Machado, Scartascini and Tomassi 2011; Pilati 2011). Consequently, countries with hybrid regimes are

theoretically likely to experience more protests than 'strong' or stable autocracies or democracies, and the existing evidence supports this proposition. Cunningham and Lemke (2011) find evidence that hybrid regimes have a higher risk of riots than strong democracies and strong autocracies; Urdal and Hoelscher (2012) show a similar result with urban social disturbance events; and complimentary studies of demonstrations and riots in Latin America by Kurtz (2004) and Arce and Bellinger (2007) show that hybrid democracies are more prone to unrest than fully institutionalised ones. This inverted-U has even been demonstrated at the city level. In a study of protest events in American cities in 1968, Eisinger (1973) found that protest was more common in cities where "the political opportunity structure is characterized by a mix of open and closed variables" (17) in terms of perceived access to city councillors and executives.

Political opportunity structures are also shaped by process and events that increase the possibility and potential returns of collective mobilisation at particular moments in time. Elections provide important focal points for political contestation and mobilisation, particularly in weakly institutionalised democracies where the legitimacy of electoral processes and outcomes are contested (Chenoweth and Ulfelder 2015). Similarly, during periods of political reform, when the 'rules of the game' governing important political issues such as the architecture of authority and the terms of political competition are in flux, collective mobilisation can be used as a tactic to influence decision makers or signal public preferences (ibid). We control for both election effects and political regime instability in our empirical models below.

## METHODOLOGY

### Dependent variable

A protest can broadly be defined as an expression of dissent or discontent. For the purposes of the present investigation a protest is defined more narrowly as a public demonstration, riot or

strike. The empirical data on urban protest events used in this chapter were derived from the Social Conflict in Africa Database (SCAD) (see Hendrix and Salehyan 2012). The SCAD database, which covers all African countries with a population of 1 million or more over the years 1990-2013, contains information on a variety of event types, including demonstrations, riots, strikes, inter-communal conflict, government violence against civilians, as well as other types of 'social conflict' events that are generally excluded from armed conflict databases. Data were compiled from Associated Press and Agence France Press wires using Lexis-Nexis. The database contains information on the date, magnitude and location of each event. Utilising the event type information we created a dataset that includes a measure of protest incidence consisting of the sum of all demonstrations, riots and strikes (*etype 1-6*). Demonstrations are defined as generally peaceful actions; riots involve intentional physical injury or damage to property; strikes involve the partial or complete abandonment of workplaces by those belonging to an organization or union. Models were additionally run with demonstrations, riots and strikes as separate dependent variables in separate models with similar results, which are available on request. The dataset covers 46 countries between 1990 and 2013 and includes a total of 1128 country-year observations. Observations for Eritrea begin in 1993 when the country became independent. Somalia and South Sudan have been excluded from the sample due to lack of socioeconomic data. Models incorporating Freedom of the Press scores only include data from 1993. There are some other country-year observations with missing data points resulting in a slightly reduced sample size in some specifications. Descriptive statistics are provided in the appendix.

An important concern with data derived from media outlets is that there may be a systematic bias in coverage associated with press freedom and resources. To some extent these concerns are mitigated by the fact that a) the events investigated here are public, collective and generally occurring in urban areas and therefore difficult to hide from foreign correspondents, b) the sources are international news wires, which are less susceptible to censorship than domestic news outlets, and c) wire services offer greater geographical coverage and face fewer space

constraints than newspapers, which are also used to produce event datasets of this kind (Salehyan et al. 2012). Indeed, comparisons of the SCAD dataset with that of Banks (2011), which has been the most commonly employed source of data on protests for quantitative studies, clearly indicates more comprehensive event coding in the former (Salehyan et al. 2012). Nevertheless, we empirically address the potential for bias in our core models by incorporating a measure of press freedom in our models derived from the Freedom House (2015) Freedom of the Press database. This provides an index of press freedom for each country and year based on an annual survey of print, broadcast and internet freedom. Values range from 0-100, with higher numbers representing lower degrees of freedom. To simplify the visual interpretation of the index the scale has been inverted by subtracting each country's score from 100 so that higher values represent greater levels of press freedom (0=not free; 100=completely free). The series begins in 1993.

It should be noted that there is an alternative and popular dataset available for measuring protest/riot events in the Africa: the Armed Conflict Location and Event Database (ACLED). However, this dataset has two disadvantages for our purposes. First, in episodes where governments crack down on protesters, the event is classified as 'Violence against Civilians' rather than a protest, which confuses matters. Second, and more importantly, in the ACLED database every day of a protest/riot is coded as a separate event. For example, a demonstration that begins on a Monday and ends on a Friday is coded as five separate events. In contrast, this would be coded as a single event in the SCAD database. As we are substantively interested in the frequency with which people mobilise, rather than for how long, the SCAD dataset was deemed more appropriate.

### Explanatory Variables

Most of our core demographic variables are derived from UN Population Division estimates (see United Nations 2014). These include the natural log of the urban population (*Ln urban*

*population*) and the log of level of urbanisation (*Ln urbanisation*) for each country-year. In some models we also incorporate the size of the population in the largest city (*Ln pop. Largest city*) and number of large cities (*N big cities* - i.e. those with a population of 300,000+). Logged values of population variables are used to account for non-normal distributions. Our measure of urban primacy was calculated as the percentage of the total urban population living the largest city from UN data. In some cases—particularly for the smallest countries—UN estimates were unavailable for individual settlements and supplementary data were used from Thomas Brinkhoff's estimates published online ([citypopulation.de](http://citypopulation.de)). To estimate the effects of urban population change we use two separate measures: the average annual rate of change in the size of the urban population ( $\Delta$  *urban pop*) and the natural log of the absolute increase in the number of people living in urban areas (*Urban pop growth*). We introduce this latter measure to compensate of the fact that  $\Delta$  *urban pop* only captures the rate of urban population change, not the scale of such change, which we would expect to be the more significant factor. We have excluded *total national population* due to its exact collinearity with urban population size and urbanisation. Including it would therefore obscure our ability to empirically distinguish between the urban population scale and ratio effects that we are interested in.

In our core models we also include a vector of political variables to control for the influence of institutional conditions and political processes linked to protest. The first is an indicator of the number of national elections (i.e. presidential, parliamentary or constitutional referenda) for each country-year (*N elections*) taken from Nunley (2013). The second is a measure of the depth of democratic institutions drawn from the Polity IV dataset. We use the *Democracy* score (a subcomponent of the overall *Polity* score) for each country-year. This is a composite indicator combining measures of the competitiveness of political participation, the openness and competitiveness of executive recruitment and constraints on the chief executive for each country and year. The indicator takes values between 0 and 10, with 0 representing no democratic characteristics and 10 representing strong democratic institutions. Given that the literature also indicates a non-linear (i.e. an inverted-U) shaped relationship between

democratic institutions and various types of civil unrest, with 'hybrid' regimes most prone to contentious collective action, we also include a *Democracy*<sup>2</sup> term in our models. Finally, we include a dummy variable to capture the potential effects of political instability on protest. This variable, *Regime change*, takes a value of 1 if a country experienced a polity score change of 3-points or more in that year. The vast majority of regime changes recorded in the dataset are positive.

To reduce the prospect of bias due to reverse causality the *Democracy* and *Democracy*<sup>2</sup> variables are lagged by one year. With regard to elections and *Regime change*, the expected influence is contemporaneous—i.e. the holding of an election or a significant change in political institutions may create motives for protest before, during and after such events as groups seek to influence outcomes. As a result, these variables are not lagged, which renders clear identification of the direction of causality more difficult. We tested our core models with lagged values of both variables to ensure the robustness of our other findings. These models can be found in the online data appendix.

For economic controls we include *GDP per capita* (log transformed) and *GDP growth* for each country-year. Both indicators are drawn from the World Bank World Development Indicators database and are measured in constant 2011 international dollars adjusted for purchasing power parity.

Finally, in our core models we include a dummy variable for North Africa to account for the substantial historical, political and economic differences between countries on either side of the Sahara, as well as an Arab Spring dummy to capture the effects of the unique sequence of events that led to a dramatic increase in protest activity in the region after 2010. This latter variable takes a value of 1 for all North African countries from the year 2010. Year dummy variables for all countries were also included to account for changes that were the result of unobserved global or continental shifts that affected all countries equally.

In addition to these variables, we also tested the robustness of our results to the inclusion of a number of other control variables: the proportion of the country under 25, inequality (measured by Gini coefficient), ethnic polarisation and fractionalisation (between countries only), ethnic exclusion and ethnic minority dominance (using data from the Ethnic Power Relations dataset of Wimmer, Cederman and Min (2009)), mortality, manufacturing as a percentage of GDP, and industry as a percentage of GDP. None of these changed our substantive result. We therefore excluded these from the analysis because missing data for these variables significantly reduced sample sizes.

### Model Estimation

Our explanatory variables are incorporated into a series of negative binomial models with country random effects, and specified using the within-between formulation suggested by Bell and Jones (2015). A negative binomial model is the most appropriate estimation strategy given the skewed and highly dispersed distribution of the protest data and has become the standard estimation strategy for analyses of similar event datasets (see Kurtz 2004; Shatzman 2005; Arce and Bellinger 2007; Bohlken and Sergenti 2010; O'Hara and Kotze 2010; Urdal and Hoelscher 2012). The general form of our core model can be written as:

$Protests_{it} \sim \text{Negative binomial}(\pi_{it})$

$$\log(\pi_{it}) = \beta_0 + \sum_1^k \beta_{Wk}(X_{itk} - \bar{X}_i) + \sum_1^k \beta_{Bk}\bar{X}_{Bi} + u_i + y_t$$

Here,  $Protests_{it}$  is the number of protest events in year  $t$  and country  $i$ , which is assumed to have a negative binomial distribution with an underlying rate of  $\pi_{it}$ .  $X_{itk}$  is a series of  $k$  covariates, representing various demographic, political and economic characteristics of country  $i$  on occasion  $t$ . The effects of these variables are divided into 'within' effects ( $\beta_{Wk}$ ), where only the within-country variation is considered, and 'between' effects ( $\beta_{Bk}$ ) which only consider

differences between countries.  $u_i$  is a series of country random effects, where  $u_i \sim N(0, \sigma_u^2)$ , with  $\sigma_u^2$  being estimated.  $y_t$  is a series of year dummy variables. The variance of  $Protests_{it}$  that is not accounted for by the variables in the model is equal to  $\pi_{it} + \pi_{it}^2 * r$ , where  $r$  is an overdispersion parameter that is estimated. The use of the negative binomial link function is appropriate when modelling counts (in this case, of protest events) where the data may be overdispersed; if  $r$  is found to be statistically significant, it suggests that the use of the simpler Poisson model would be inappropriate.

The use of a random effects (RE) model may seem a controversial choice, given its perceived inability to control for unobserved country level factors when compared to the more often used fixed effects (FE) approach. However there are both technical and substantive reasons for making this choice. First, for negative binomial models, there is no conditional maximum likelihood estimation method that fully accounts for unobserved country effects (Allison and Waterman 2002; Allison 2009, 2012; Greene 2007; Guimarães 2008). Second, the dividing of parameters into within and between effects in a RE model means that the within effects will be equivalent to fixed effects estimates, because the higher level has been apportioned out into the between effect (Allison 2009; Bell and Jones 2015). Within effects represent the effect of an unusually high (or low) level of X for a given country, whilst the between effect represents the effect of a country generally having a high (or low) level of X. Whilst often the interest is primarily in the within effect (this is closer to the 'causal' effect of a change in X, although this interpretation is problematic in the presence of omitted time-varying variables), the between effect can also reveal interesting associations that operate at the country level and that can be very different from their time varying counterparts. A FE approach would not be able to uncover these between-country associations, because any country-level variation in the dependent variable is conditioned out. Whilst it would tell us how changes in demographic and democratic characteristics within countries affects protest incidence, it would tell us nothing about how those characteristics lead to differences between countries in protest incidence. Of course, care should be taken in interpreting *either* within or between effects as causal, since



both could be subject to bias due to reverse causality or omitted variables. The models were estimated in Stata using the `xtnbreg` command. Code to replicate the study can be found in this article's online appendix.

## RESULTS

Tables 2-4 summarise our key empirical results. We begin in Table 2 by examining the links between urban primacy and protest mobilisation as we expect this dimension of urban geography to have no substantive effect on national trends once other demographic variables are incorporated into the model. In these models we use the sum of protest events for each country over a five year period as the dependent variable as primacy data are only available in 5-year intervals. While this yields only 4 observations per country we prefer fewer observations over an artificially inflated dataset employing interpolated values, which can bias results. In column 1 we present the most parsimonious model mirroring Wallace (2013). In contrast to his results we find no effect within countries and a negative and significant effect between countries. However, once we control for level of urbanization and urban population size (column 2) the effects disappears entirely. Column 3 incorporates a squared primacy term and the natural log of the population living in the largest city, following Anthony and Crenshaw (2014), and returns a similar non-result. Indeed, we tested a wide variety of models and found primacy to be consistently insignificant once basic demographic controls were introduced. We therefore have excluded the primacy variable from the remainder of the models presented here. This non-result is consistent with our theoretical expectations: if primacy influences the frequency of protest mobilisation, the effect is marginal at best and very possibly a statistical artefact of omitted variables bias.

**TABLE 2. Urban primacy and protest incidence**

In Table 3 we introduce our core model and test the remainder of our urban geography variables with political and economic controls. In these models the dependent variable is the number of protest events in each country-year (rather than a 5-year summary). This annual approach allows us to control more precisely for volatile political and economic conditions than is possible with the five-year summary DV presented in Table 2. The model in column 1 of Table 3 reveals a counterintuitive result: a country's level of urbanisation is negatively and significantly correlated with the frequency of protest mobilization. This finding proved highly robust across the wide range of specifications that we tested and directly contradicts the conventional view that urbanization in developing countries increases the risks of protests.

However, we find that urban population size is positively and significantly correlated with protest incidence between countries, but not within countries, and this result is similarly robust across a wide range of specifications. As an alternative test of scale effects we introduce the number of large cities as an explanatory variable in column 2 and find a similar positive and significant result.

Taken together, these results indicate that an increase in a country's urban population in *relative* terms has a mitigating effect on protest frequency, and that countries with larger-than-average urban populations have more protests. However, it is not clear whether or not an increase in urban populations *within* a country raises the likelihood of protest mobilisation. This may in part be because of the high degree of auto-correlation between urban population and urbanisation at the within-country level (see appendix table A5). We suspect that a substantial increase in the size of a country's urban population over a significant period of time may increase the likelihood of protest mobilization, but from year-to-year, or even over five year periods, changes in urban growth appear to have no substantive effect.

**TABLE 3. Urban geography and protest incidence: core models**

This hypothesis finds some support in the results presented in columns 3-5, which examine the association between urban population change and protest incidence. In columns 3 and 4 we use the standard measure of the percentage change in the size of the urban population; in column 5 we use the log of the absolute increase in the size of the urban population. The hypothesis that urban population growth may stoke civil unrest is not supported: we find a negative and generally insignificant correlation between our two measures of urban growth and protest incidence. This is consistent with results reported by Blanco and Grier (2009) and Buhaug and Urdal (2013) and may reflect reverse causality – i.e. people may not move to cities in periods of intense civil unrest. Given that the correlation is weak and highly susceptible to changes in our model specification we omit it from the remainder of the empirical analysis.

Our political and economic controls are generally consistent with our expectations. Within countries, protests are more likely in election years and during episodes of regime instability. We also find a weakly positive and non-linear association between democracy and protest, which is consistent with the political opportunity structure theory of mobilisation and previous empirical research. The non-linear association between press freedom and protest within countries is more robust and warrants further research, as this is a unique finding. GDP per capita is generally positively associated with protest incidence, which is consistent with resource mobilization theories, and GDP growth is generally negative, as expected. Finally, all other things equal, North African countries appear to have been significantly less likely to experience protest mobilization than those south of the Sahara (at least when the later, Arab Spring years are controlled for), and there is a clear Arab Spring effect.

Table 4 builds on our baseline model (column 1 of Table 4) to explore interaction effects. We are particularly interested in directly testing the ‘over-urbanization’ hypothesis here by introducing interaction terms between GDP per capita and urban population variables, and alternatively interaction terms between a dummy variable that takes a variable of 1 for

countries classified as 'low income' by the World Bank and our urban population variables. The same political and economic controls reported in Table 4 are included in the models, however we do not report them in Table 5 in order to save space.

**TABLE 4. 'Over-urbanization' and protest incidence**

The results show some evidence of a significant interaction between urbanisation and income, but these interactions are not large enough to support the 'over-urbanisation' hypothesis. In column 1 of table 5 we find a negative and significant association between our GDP per capita\*Urbanization term and protest incidence, indicating that the within-country association between urbanisation and protest is more negative in richer countries. However this is not a strong enough effect size to reverse the directionality of the line – although less strong, low income countries are still (if anything) protected from protest incidents by higher levels of urbanisation. This is illustrated in figure 1 (which is based on the interaction in model 3). Although the apparent protective effect of urbanisation is smaller for low-income countries, it remains negative. If the over-urbanisation hypothesis were true, we would expect low income countries to experience more protests at higher levels of urbanisation, because they do not have concomitant growth to help deal with the problems that urbanisation poses. This does not appear to be the case. There is no evidence of any interactions between urban population size and GDP per capita.

**FIGURE 1. Interaction effect: urbanisation, income level and protest incidence**

## CONCLUSION

Countries with large urban populations experience more protests than those with small urban populations. However, this does not mean that urbanization contributes to civil unrest. In fact, we find robust evidence to the contrary: at any income level, an increase in a country's level of urbanization is associated with a decrease in the probable number of protest events in any given year. Similarly, rapid urban population growth is negatively correlated with protest incidence, suggesting that urban population pressure is not a significant risk factor. We also find no robust evidence of a link between urban primacy and protest once appropriate demographic controls are introduced.

These nuanced findings arise from an explicit recognition of the complexity of urban demographic processes. Separating population ratio, scale, rate-of-change and distribution effects provides a substantially more nuanced interpretation of the significance of urban population change in developing countries than that which emerges from vague theorising about the dangers of rapid 'urbanization'. This is of more than academic interest. A failure to distinguish between the related but distinct processes of urban growth and urbanisation has been particularly problematic in past research and arguably influenced flawed policies in Africa and other developing regions, such as disinvestment in cities and efforts to restrict rural-urban migration (see Fox 2014; Fox and Goodfellow 2016). It is therefore important to be more careful about theorising and modelling urban demographic processes and their relationship to social and political phenomena of interest.

Indeed, our results suggest that widespread concern about the disruptive consequences of rapid 'urbanization' appear to be somewhat misplaced—at least with regard to the likelihood of protest events such as demonstrations, riots and strikes. However, it is possible that urban demographic processes influence collective contentious action differentially along the spectrum of violence (e.g. effects on protest mobilization may differ from those on insurgency and civil

war)—a possibility warranting further research. Finally, the conclusions drawn from a study such as this, where data are aggregated at the national level, need to be treated with caution. Further subnational research is needed to parse out substantive causal mechanisms—research that has so far been constrained by the dearth of reliable, comparable subnational data in most developing countries.

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**TABLE 1. Urban primacy and protest: a thought experiment**

	<b>Country A</b>	<b>Country B</b>	<b>Country C</b>	<b>Country D</b>
Urban population	5 million	5 million	10 million	70 million
Primacy score	25%	50%	25%	15%
Population in largest city	1.25 million	2.5 million	2.5 million	10.5 million
Residual urban population	3.75 million	2.5 million	7.5 million	59.5 million

**TABLE 2. Urban primacy and protest incidence**

	Dep. Var. = 5-year sum of protest events per country, 1990-2010					
	(1)		(2)		(3)	
	within	between	within	between	within	between
Urban Primacy	-0.0148 (0.0168)	-0.0251*** (0.00727)	-0.00632 (0.0192)	-0.00535 (0.00696)	-0.153 (0.163)	0.0236 (0.123)
Urban Primacy <sup>2</sup>					0.000688 (0.00110)	-0.000394 (0.000990)
Ln Pop. Largest city					3.535 (2.699)	0.0299 (1.654)
Ln Urban Population			1.972*** (0.422)	0.440*** (0.0834)	-1.400 (2.683)	0.403 (1.664)
Ln Urbanisation			-3.414*** (0.779)	-0.491* (0.255)	-3.651*** (0.791)	-0.380 (0.271)
Ln GDP per capita	2.604 (1.893)	-1.065* (0.632)	1.081 (1.981)	0.465 (0.833)	2.116 (2.064)	0.0898 (0.902)
GDP growth	0.0171 (0.0154)	-0.0877 (0.0590)	0.000367 (0.0193)	-0.120** (0.0497)	0.000537 (0.0211)	-0.110** (0.0521)
<i>N</i>	179		179		179	

*Note:* Standard error in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$

**TABLE 3. Urban geography and protest incidence: core models**

	Dependent variable = annual number of protest events per country, 1993-2013									
	(1)		(2)		(3)		(4)		(5)	
	within	between	within	between	within	between	within	between	within	between
Ln urbanisation	-2.590** (0.911)	-0.564** (0.217)	-2.661** (0.897)	-0.386 (0.220)	-1.591** (0.500)	-0.228 (0.264)	-2.436* (0.953)	-0.740*** (0.224)	-2.812** (0.946)	-0.717*** (0.217)
Ln urban pop.	0.625 (0.644)	0.381*** (0.0615)	0.607 (0.642)				0.761 (0.660)	0.395*** (0.0599)	0.803 (0.702)	0.807*** (0.241)
N big cities				0.145*** (0.0306)						
Urban pop. growth					-0.000577 (0.0321)	-0.104 (0.0834)	-0.0174 (0.0366)	-0.154* (0.0683)		
Δ in urban pop									-0.133 (0.111)	-0.394 (0.225)
N elections	0.0896* (0.0368)	-1.314 (0.726)	0.0839* (0.0369)	-0.937 (0.742)	0.0792* (0.0375)	-1.420 (0.812)	0.0885* (0.0365)	-1.115 (0.713)	0.0809* (0.0373)	-0.949 (0.717)
Regime change	0.633*** (0.0849)	0.521 (0.670)	0.626*** (0.0860)	0.857 (0.670)	0.682*** (0.0838)	1.080 (0.754)	0.637*** (0.0845)	0.798 (0.656)	0.659*** (0.0850)	0.814 (0.649)
Democracy <sub>t-1</sub>	0.130** (0.0493)	0.0454 (0.143)	0.144** (0.0489)	0.0549 (0.149)	0.131* (0.0508)	0.0824 (0.177)	0.121* (0.0496)	0.0912 (0.140)	0.126* (0.0498)	0.0400 (0.141)
Democracy <sup>2</sup> <sub>t-1</sub>	-0.0107 (0.00610)	-0.00675 (0.0171)	-0.0120* (0.00613)	-0.00875 (0.0180)	-0.0112 (0.00636)	-0.0102 (0.0231)	-0.00989 (0.00614)	-0.0174 (0.0172)	-0.0102 (0.00615)	-0.00874 (0.0177)
FOTP score	0.0358*** (0.0104)	0.0744* (0.0290)	0.0383*** (0.0102)	0.0694* (0.0300)	0.0360** (0.0111)	0.0568 (0.0336)	0.0355*** (0.0105)	0.0617* (0.0289)	0.0451*** (0.0109)	0.0650* (0.0284)
FOTP squared	-0.000686*** (0.000133)	-0.000767* (0.000316)	-0.000707*** (0.000134)	-0.000696* (0.000326)	-0.000676*** (0.000137)	-0.000561 (0.000375)	-0.00684*** (0.000133)	-0.000568 (0.000322)	-0.000790*** (0.000136)	-0.000616* (0.000314)
Ln GDP per capita	2.320 (1.256)	1.542 (0.839)	1.513 (1.213)	0.901 (0.856)	2.282 (1.169)	0.658 (0.990)	2.562* (1.252)	1.549 (0.808)	4.300** (1.374)	1.636* (0.805)

GDP growth	0.000293 (0.00310)	-0.142* (0.0675)	0.00137 (0.00281)	-0.103 (0.0673)	0.000324 (0.00280)	0.00368 (0.0772)	-0.0000755 (0.00311)	-0.138* (0.0661)	0.000970 (0.00300)	-0.145* (0.0648)
N. Africa dummy		-0.776** (0.257)		-0.335 (0.261)		-0.695* (0.300)		-0.929*** (0.264)		-0.812** (0.258)
Arab Spring		1.179*** (0.161)		1.222*** (0.163)		1.175*** (0.162)		1.261*** (0.164)		1.193*** (0.161)
Constant		0.273 (0.212)		0.242 (0.212)		0.318 (0.187)		0.308 (0.212)		0.253 (0.215)
N		957		957		957		957		930
Log Likelihood		-2326.2		-2331.0		-2341.5		-2323.8		-2269.9

*Note:* Standard error in parentheses. \*\*\* $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$

**TABLE 4. 'Over-urbanization' and protest incidence**

	Dependent variable = annual number of protest events per country, 1993-2013							
	(1)		(2)		(3)		(4)	
	within	between	within	between	within	between	within	between
Ln Urbanisation	4.832 (3.079)	0.0974 (1.551)	-2.797*** (0.970)	-0.558** (0.217)	-4.499*** (0.983)	-0.356 (0.303)	-2.493*** (0.903)	-0.263 (0.170)
Ln urban pop.	0.0183 (0.686)	0.397*** (0.0615)	0.268 (0.991)	-0.280 (0.719)	0.0527 (0.644)	0.386*** (0.0626)	0.112 (0.818)	0.314*** (0.0709)
Ln GDP per cap	16.21*** (5.663)	3.147 (3.537)	-0.0882 (4.980)	-1.117 (2.969)				
GDPpc * Urbanisation	-3.842** (1.527)	-0.381 (0.882)						
GDPpc * Urban Pop			0.275 (0.560)	0.341 (0.373)				
Low Income dummy					-0.0970 (0.136)		-0.0728 (0.140)	
Low Income * Urbanisation					3.116*** (0.878)	0.155 (0.312)		
Low Income * Urban Pop							0.580 (0.378)	0.124 (0.0755)
Constant	0.139 (0.219)		0.297 (0.215)		0.203 (0.236)		0.294 (0.231)	
N	957		957		957		957	
Log Likelihood	-2322.9		-2325.7		-2322.4		-2325.6	

*Note:* Standard error in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . The political and economic controls reported in Table 4 were included in these models. They are omitted from this table to conserve space and are available on request.



**FIGURE 1. Interaction effect: urbanisation, income & protest**

