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LIBERATION TECHNOLOGY: MOBILE PHONES AND POLITICAL MOBILIZATION IN AFRICA

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Can digital information and communication technology foster mass political mobilization? We use a novel georeferenced data set for the entire African continent between 1998 and 2012 on the coverage of mobile phone signal together with georeferenced data from multiple sources on the occurrence of protests and on individual participation in protests to bring this argument to empirical scrutiny. We find that while mobile phones are instrumental to mass mobilization, this only happens during economic downturns, when reasons for grievance emerge and the cost of participation falls. The results are in line with insights from a network model with imperfect information and strategic complementarities in protest occurrence. Mobile phones make individuals more responsive to both changes in economic conditions—a mechanism that we ascribe to *enhanced information*—and to their neighbors' participation—a mechanism that we ascribe to *enhanced coordination*.

KEYWORDS: Protests, politics, Africa, mobile phones.

1. INTRODUCTION

IN THIS PAPER, we use a variety of georeferenced data for the whole of Africa covering a span of 15 years to investigate whether mobile phone technology has the potential to foster mass political mobilization and to explore the underlying channels of impact.

The spread of digital information and communication technology (ICT) has fed a wave of optimism and extensive rhetoric about its use as a "liberation technology" capable of helping the oppressed and disenfranchised around the world. According to this argument, popularized by political sociologists and media scholars alike (Diamond (2010), Shirky (2011)), mobile phones and the Internet, thanks to the opportunity they offer for two-way, multi-way, and mass communication in addition to their low-cost, decentralized, open-access nature, have the potential to foster citizens' political activism and even lead to

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mass political mobilization, especially when civic forms of political participation are de facto or lawfully prevented.[1]

This claim appears particularly appealing for Africa. Over the period of analysis (1998–2012), the continent experienced a rapid spread of mobile phone technology: while in 1999 an estimated 80 million African citizens had access to mobile phones, by 2008 this number had risen to 477 million or around 60 percent of the continent's population (Aker and Mbiti (2010)). The diffusion of mobile technology across Africa took place against the backdrop of a very limited, and in some countries practically nonexistent, fixed-line telephone infrastructure. Precisely due to this context, it has been argued that this expansion has had considerable economic and social effects on the lives of its citizens, particularly the poor and very poor. The ubiquitous use of mobile phones across the continent has also led to the emergence of a number of creative applications and technological developments, such as short message service-based (SMS-based) election monitoring and health information campaigns, disaster relief operations, and mobile banking (Jack and Suri (2014), Aker, Collier, and Vicente (2017)). Due to the lack of fixed phone lines and high-speed Internet cabling, mobile phones are also the most commonly used way to access the Internet and social media (Stork, Calandro, and Gillwald (2013)), greatly enhancing their information and communication potential. Consistent with the liberation technology hypothesis, over the last decade Africa has witnessed some of the most spectacular episodes of mass mobilization. Food riots swept the continent between 2007 and 2008, while mass civil unrest—the Arab Spring—erupted in the northern countries between 2010 and 2012.

Simple economic reasoning, which we formalize below, suggests that increased information and communication enabled by mobile phones have the potential to trigger collective action. More specifically, this technology can help individuals acquire and spread *information* on issues and reasons for grievance and, by fostering communication, improve *coordination*, which is key to protest provision. Due to its open-source and open-content nature and, hence, by granting access to unadulterated information, digital ICT also has the potential to offset government propaganda, which can curb discontent via misinformation and persuasion. This is especially true when traditional media are under the control of the government or in the hands of powerful interest groups (Edmond (2013)).

Note that these arguments focus on the role of information provision in terms of citizens' private incentives to participate via its effect on perceived individual costs and returns. Yet when strategic complementarities in the occurrence of protests exist, that is, when the returns to political activism increase or the costs of participation decrease with the number of others participating (Barbera and Jackson (2017), Passarelli and Tabellini (2017)), mobile phone technology can also induce mass mobilization through its ability to promote coordination. Knowledge, albeit imperfect, of others' likelihood of participating can, in particular, foster individuals' willingness to participate and lead to the emergence of protests in equilibrium, an outcome that would not occur in a world where individuals act atomistically.

Despite, however, the popularity of the liberation technology argument, there are reasons for skepticism. First, governments can use this technology as a control, surveil-

[1]As early as 2007, *The Economist* highlighted the role of mobile phone technology in fostering political activism worldwide, launching the term "mobile activism" (*The Economist* 2007). Digital ICT and new media, including blogging and Twitter, are also claimed to have been instrumental in what appears to be a recent surge of protests worldwide, from the Arab Spring in North Africa and the Middle East to the Occupy Wall Street movement in the United States, the *indignados* in Spain, and the Iranian Green Revolution (Ortiz, Burke, Berrada, and Cortés (2013)).

lance or propaganda tool, hence, making protests less, rather than more, likely (Morozov (2012)). This effect is enhanced by the nature of the technology, which makes centralized control possible, an effect that is magnified by the fact that physical infrastructures and market regulation of ICT are often directly controlled by governments.

A second often-heard counterargument against the liberation technology hypothesis is that digital ICT can discourage social capital accumulation and the establishment of "strong ties" that are thought to be instrumental to mass mobilization (Gladwell (2010)), ultimately leading to political apathy rather than mobilization.

Yet another reason why digital ICT might ultimately not lead to the emergence of mass mobilization is that, not dissimilar from traditional media, this technology has the potential to effectively increase government accountability through the spread of information and greater transparency (Guriev, Melnikov, and Zhuravskaya (2019), Snyder and Strömberg (2010)). In addition, mobile phones have the potential to directly improve living standards, thus weakening the main rationale for mass political mobilization, which is widespread discontent with the perceived state of the economy and politics (Aker and Mbiti (2010)).

In sum, while there has been a great deal of enthusiasm and a plethora of anecdotal evidence concerning the role played by digital ICT—and particularly mobile phones—in fostering mass political mobilization, there are good reasons to question the role actually played by this technology, especially as the evidence remains scant. The mechanisms of impact are also poorly understood. This paper aims precisely to investigate these questions.

In its simplest form, the liberation technology argument suggests that protests should arise in response to the availability of mobile phones. However, an established body of evidence that we confirm based on our data shows that the incidence of protests is negatively correlated with economic conditions, as a worsening of the latter is associated with lower private opportunity costs of participation and provides a rationale for widespread grievances (for all, see Campante and Chor (2012)).[2] This paves the way for a "nuanced" version of the liberation technology hypothesis: while mobile phones may play a role in fostering protest provision, this effect would be expected to emerge largely during recessions, when an independent trigger for protests exists.

We empirically assess this qualified version of the liberation technology argument by investigating the heterogenous effect of mobile phone coverage over the business cycle. Our setting offers the opportunity to perform this exercise since, despite sustained continental economic growth, over the 15 years of analysis a number of countries across the continent experienced outright recessions and reasons for grievance abounded.

In order to perform our analysis, we use several data sets for the whole of Africa on the spread of mobile phone technology and on protest activity, respectively. The geographical level of detail of these different data sets makes them especially appealing, and allows examination of the spread of protests and mobile phone technology over time across small areas within countries.

Data on local mobile phone coverage come from the Global System for Mobile Communications (GSM) Association, which collects this information for the purpose of creating roaming maps for use by customers and providers worldwide. The data report the

[2]This parallels findings that worse economic conditions are typically associated with greater incidence or risk of conflict and insurgency (see Blattman and Miguel (2010), Harari and La Ferrara (2018)). Related literature also emphasizes the role of protests and revolution threats during poor economic times as triggers for political changes and democratization (Acemoglu and Robinson (2006), Brückner and Ciccone (2011)).

availability of signal for the whole of continental Africa (with the exception of Somalia) between 1998 and 2012 at a level of geographical precision of between approximately 1 and 23 km^2 on the ground, depending on the country. GSM technology accounts for around 80 percent of mobile technology worldwide and almost 100 percent in Africa. Over the period of observation, most of the variation in mobile phone coverage refers to second generation (2G) technology, which allows for voice and SMS services and basic Internet access.

In order to measure the incidence of protests, we employ three data sets on individual protest events, all largely based on compilations of news wires. First, we use information from a very large, open-source data set, which relies on an automated textual analysis of news sources: the Global Database on Events, Location and Tone (GDELT, Leetaru and Schrodt (2013)). We complement this information with data from two manually compiled but much smaller data sets: the Armed Conflict Location and Event Data Project (ACLED, Raleigh, Linke, Hegre, and Karlsen (2010)) and the Climate Change and African Political Stability (CCPAS) Social Conflict Analysis Database (SCAD, Salehyan et al. (2012)).

The very detailed level of geographical disaggregation of the data allows us to compare changes in the incidence of protests in areas within the same country that experienced differential changes in the coverage of mobile technology. Moreover, in addition to estimating an average effect of coverage on protests across all countries and periods, the availability of observations on countries at different points of the business cycle also permits an identification of the effect of mobile phones separately at these various points.

For most of the analysis, we focus on cells of $0.5° \times 0.5°$ resolution, corresponding approximately to areas of 55×55 km. We focus on within- rather than between-country variation in the incidence of protests and the spread of ICT. This alleviates the obvious concern—and the ensuing bias in the estimates of impact—that ICT adoption and the incidences of protests are correlated due to country-specific trends or shocks in unobservable variables, such as the state of the economic cycle. This also allows us to investigate several dimensions of heterogeneity based on area characteristics.

The concern remains, however, that even within countries, protests and ICT adoption are correlated for reasons other than the causal effect of the latter on the former. For this reason, we use an instrumental variable strategy that exploits differential rates of adoption of mobile phones across areas characterized by different average incidence of lightning strikes. Frequent electrostatic discharges during storms are known to damage mobile phone infrastructures and negatively affect connectivity, acting on both demand and supply (Andersen, Bentzen, Dalgaard, and Selaya (2012), ITU (2003)). Using National Aeronautics and Space Administration (NASA) satellite-generated data on the incidence of lightning for the entirety of Africa, we show that, in fact, areas with higher than average incidences of lightning display slower adoption of mobile phone technology over the period under examination: conditional on a large number of cell-level controls, a 1 standard deviation (s.d.) increase in lightning intensity leads to a lower penetration rate of mobile phone technology of approximately 0.25 percentage points (p.p.) per year or 7 percent of the overall continental growth.

In turning to the two-stage least squares (2SLS) estimates of the effect of mobile phone coverage on protests, we find strong evidence that mobile phones are instrumental to mass political mobilization, although this only occurs when the economy deteriorates. Our estimates suggest that a fall in national gross domestic product (GDP) growth of 4 p.p. (approximately 1 s.d.) leads to a differential increase in protests per capita between an area with full mobile phone coverage compared to an area with no coverage of between

8 and 23 percent. Effects manifest during recessions, while we find no effects of mobile phones on protest occurrence during good economic times. Since the continent experienced sustained economic growth during the period, this also means that we do not find an effect of mobile phone coverage on protests per se, that is, on average in our sample of countries/years. These findings lend support to a qualified version of the liberation technology argument: mobile phones are instrumental to mass political mobilization provided sufficient reasons for grievance exist.

To address concerns about the validity of the identification assumption, we perform a placebo test and show that there is no correlation between the instrument and the outcome variable in periods when mobile phone technology was unavailable. We also perform a number of additional tests that rule out a direct or indirect effect of the instrument on protests other than via mobile phone coverage.

Our results are robust to restricting to specific sample periods and geographical areas, to alternative definitions of the variables used, to the weighting scheme, to specific parametric assumptions, and to the level of geographical aggregation. We also show that our results are not driven by access to the Internet, be it via mobile phones or broadband. Importantly, our results are not explained by selection, whereby mobile phones make protests more likely to be reported as opposed to more likely to happen. The effects are, however, particularly pronounced in urban areas, in areas with a legacy of conflict, in nondemocratic countries, and when traditional media are captured by the state.

We complement the analysis using microdata from the Afrobarometer, which collects, among others, information on protest participation. Self-reported participation follows a similar pattern to protest occurrence, increasing more during periods of economic downturn in covered relative to uncovered areas, further reinforcing our claim that the results are not driven by selective news reporting. A major additional advantage of microdata on protest participation is that they also allow us to shed some light on the mechanisms through which mobile phones affect political mobilization.

In order to investigate these mechanisms, we borrow from and extend the network model with imperfect information of Jackson and Yariv (2007). In its barest form, the model assumes that agents maximize the payoff of taking a certain action (in the present case, protesting), which depends positively on the number of connections taking that same action through strategic complementarities and depends negatively on the cost of participation. The latter in turn depends positively on economic conditions, as worse economic conditions reduce the opportunity cost of participating in a protest or increase reasons for grievance. Although individuals do not know which actions their connections will take, they can make educated guesses based on the distribution of connectedness in the population, which is publicly known. At the stable equilibrium, the level of protests is higher the lower is the GDP growth. There are two mechanisms at work. For one, since worse economic conditions reduce the individual cost of participation or increase grievances, then the occurrence of protests will mechanically increase. This is a first-round effect. If, though, strategic complementarities are at work, this mechanism is enhanced, as individuals iterate over their neighbors' best responses knowing that when the economy does poorly, their neighbors will be more likely to participate, leading to a second-round increase in protest occurrence in equilibrium. Both of these effects are enhanced by greater connectedness in society. If individuals with mobile phones, which we understand to increase connectedness, are more likely to participate when the economy deteriorates—an effect that we ascribe to *enhanced information*—or if they are more responsive to changes in their neighbors' propensity to participate—an effect that we ascribe to *enhanced coordination*—then worse economic conditions will unambiguously lead to a greater increase in protest participation when mobile phone coverage increases.

Regressions estimates based on aggregate data from GDELT, ACLED, and SCAD potentially subsume both the information and the coordination mechanisms. We show, however, that one can use information on mobile phone use and individual protest participation from the Afrobarometer to separately identify these two effects.

Consistent with this model, data from the Afrobarometer show that individuals are more likely to participate in protests during poor economic times. We also observe that individuals are more likely to participate the higher is the fraction of others in society participating, even in areas with no coverage. Our estimates imply that a 10 p.p. increase in the fraction of fellow citizens participating increases each individual's probability of participation by around 6.5 p.p., providing evidence of strategic complementarities in the occurrence of protests. We find suggestive evidence that mobile phones enhance both of these effects, as those with mobile phones appear more likely to respond to changes in both economic conditions and in the fraction of fellow citizens participating.

Our paper is related to different strands of literature. An influential body of work focuses on the determinants of conflict in Africa (e.g., Berman, Couttenier, Rohner, and Thoenig (2017), Besley and Reynal-Querol (2014), König, Rohner, Thoenig, and Zilibotti (2017)). Protests are often considered to be precursors of such events, serving as a focal point for unmet grievances and allowing violent actors to subsequently build an armed opposition (Gurr (2000)).

In parallel, a separate body of literature focuses on the impact of traditional media on civic forms of political participation, largely in western countries. While newspapers seem to foster political participation and turnout (Gentzkow, Shapiro, and Sinkinson (2011)), television (TV) and the Internet seem to have the opposite effect (Falck, Gold, and Heblich (2014), Gentzkow (2006)). Another strand of work emphasizes the role of media in voters' political alignment through persuasion (for an overview, see DellaVigna and Gentzkow (2010)). Particularly relevant in the African setting is Yanagizawa-Drott (2014), who studies the role of radio propaganda in fostering mass killings during the Rwandan genocide. Additional evidence from Africa argues that information campaigns and social media increase accountability and reduce corruption (Acemoglu, Hassan, and Tahoun (2017), Reinikka and Svensson (2011)).

A number of recent papers, not specifically on Africa, focus on the determinants of protests. Cantoni, Yang, Yuchtman, and Zhang (2019), for example, examine the Hong Kong protests and find evidence of strategic substitutability in protest participation. Particularly relevant for our setting are studies of the role of new media in protest participation. Battaglini (2017) shows theoretically how social media can enhance the effectiveness of protests via information aggregation, while Enikolopov, Makarin, and Petrova (2019) find empirical evidence that digital ICT affects protest participation in Russia.

The rest of the paper is organized as follows. Section 2 presents preliminary anecdotal evidence on the role of mobile phones in triggering protests in Africa during periods of economic slowdown. Section 3 introduces the data and Section 4 discusses the descriptive statistics. Section 5 builds on the theoretical model, which is described in detail in Appendix A.1 of the Supplemental Material (Manacorda and Tesei (2020)) and lays out the empirical strategy. Section 6 presents the empirical results and Section 7 concludes.

2. PRELIMINARY EVIDENCE: ARAB SPRING AND THE FOOD RIOTS

Before turning to a formal empirical analysis of the relationship between protests, mobile phones, and the economic cycle, in this section we present anecdotal evidence that mobile phones might have been instrumental to mass political mobilization in Africa during periods of economic crises.[3]

Certainly, the most well-known episode of mass mobilization in Africa over the last two decades is the Arab Spring, which was sparked by the self-immolation of Tunisian street vendor Mohammed Bouazizi in December 2010 to protest authorities' seizure of his goods after refusing to pay a bribe. The day after Bouazizi set himself on fire, a video recorded with a mobile phone started circulating that showed a small crowd that had gathered outside the city's town hall to protest the maltreatment of vendors. The video was posted online and quickly became so popular that the TV network Al-Jazeera retransmitted it repeatedly. Two days after the video went viral, fuelled by widespread discontent with unemployment, low living standards, and government corruption, protests spread to the entire country, eventually reaching the capital, Tunis, in early January 2011 and leading to the ousting of President Ben Ali on January 14.

Despite the reluctance of other countries' state media to cover Ben Ali's departure, the news spread rapidly across the region, most notably in Egypt, where mobile phone penetration was among the highest in the region. Facebook and Twitter proved crucial in providing information and coordinating participants in the mass protests that started on January 25 throughout the country. In Cairo, people gathered in Tahrir Square to protest high unemployment and food-price inflation and to demand the resignation of President Mubarak. YouTube became a particularly important tool for spreading news about the uprising around the world in the form of user-generated videos. After several attempts to curb the protest, including a complete Internet shutdown and heavy-handed police intervention that caused almost 1000 deaths, President Mubarak was forced to resign on February 11. In the months that followed, protests mounted in several countries in the region, in some cases escalating to full-fledged civil wars.

The Arab Spring is not, however, the only episode of mass mobilization in Africa where digital ICT has played a crucial role. Moreover, these uprisings took place in countries of North Africa, which are hardly representative of the entire continent, and during a period of fast diffusion of the Internet and social media. For these reasons, the remainder of this section turns to one of the most severe—although possibly less known—episodes of social conflict in post-colonial Africa, the "food riots," which took place in several countries in the continent starting in 2007, so before the availability of mobile Internet.

Between 2007 and 2012, 14 African countries were affected by food riots in at least one of these years.[4] In Mozambique, for example, waves of violent popular protests against the rising cost of living broke out in 2008 and then in 2010. The protests were met with strong police repression, leaving several dead and hundreds injured. Similar riots erupted in West Africa. In Senegal, Burkina Faso, and Ivory Coast, youths and urban poor took to the streets demanding government action to curb food and fuel prices. A strong police reaction led to multiple victims and hundreds jailed.

Extensive anecdotal evidence points to the key role of mobile phones in fostering and sustaining these protests. Accounts in the press reported that the 2008 riots in Mozambique happened after several days of widely circulating text messages calling for "a great day of strike" to protest the increase in energy, water, minibus taxi, and bread prices. In

[3]References to the episodes discussed here are reported in Appendix B.6, which is located in the replication file (Manacorda and Tesei (2020)).

[4]According to Sneyd, Legwegoh, and Fraser (2013) these countries were Algeria, Burkina Faso, Cameroon, Egypt, Guinea, Ivory Coast, Madagascar, Mauritania, Morocco, Mozambique, Senegal, Somalia, Tunisia, and Uganda.

addition, evidence of police repression recorded on mobile phones attracted international attention, leading the government to suspend SMS services. Similarly, mobilization in Burkina Faso was preceded by text message exchanges among citizens unrelated through formal organizational structures.

A comparison of neighboring West African countries, Senegal and Mali, shows that these two countries experienced similar increases in food prices over the period (12 percent in 2008) and associated economic slowdowns (both countries lost approximately 1 p.p. in GDP growth between 2007 and 2009 relative to the previous three years). However, while Senegal—the country with the highest mobile phone coverage in the region—experienced food riots in both 2007 and 2008, Mali, which had virtually no mobile phone coverage, had no food riots in either of the two years. More generally, protests took place by and large only in countries covered by mobile phones. A simple mean comparison between countries that did and did not experience food riots in West Africa indicates that the former had 60 percent higher mobile phone coverage than the latter.[5]

Overall, this anecdotal evidence suggests that mobile phones played a key role in mobilizing citizens in Africa in the face of widespread grievances. Importantly, there is also evidence that mobile phones where instrumental to mass political mobilization before the introduction of third generation (3G) technology and the associated spread of mobile Internet and social media. We now move beyond anecdotal evidence, turning to a quantitative analysis of the relationship between mobile phone availability, economic conditions, and protests.

3. DATA

In this section we introduce the main sources of data used in the analysis. We start by presenting georeferenced data on mobile phone coverage, and then data on protest occurrence and participation. We subsequently describe the large array of additional socioeconomic, geographic, and climatic variables employed in the analysis, including, importantly, lightning strike intensity, used to construct an instrument for mobile phone coverage. Our data cover the entire continent (with the exception of Somalia, for which we have no information on mobile phone coverage) over a period of 15 years, from 1998 to 2012.[6]

Our primary geographical units of observation in the analysis are cells of $0.5° \times 0.5°$ resolution, corresponding to areas of approximately 55×55 km at the Equator. This is the finest level of geographical disaggregation available for a few of the key variables used in the analysis (i.e., lightning strikes, temperature, and rainfall). Overall, we split the continent into 10,409 cells. At a continent population of about 885 million, each cell accounts for around 84,000 individuals. Since the contours of cells do not typically correspond to a country's political borders, we assign cells spanning more than one country to the nation occupied by the largest area of any given cell.

While disaggregation at the $0.5° \times 0.5°$ level allows us to examine the relationship between ICT adoption and the spread of protests at the finest possible level of geographical detail warranted by our data, a concern is that these units are artificial, as they potentially cut through uniform administrative units or aggregate across very heterogenous

[5]For example, coverage in Senegal, Burkina Faso, Mauritania, and Ivory Coast was, respectively, 74, 58, 43, and 41 percent, while coverage in Mali and Liberia was just 7 and 12 percent, respectively.

[6]We drop the small island nations of Comoros, Mauritius, Sao Tome, and Principe, Seychelles, as these are likely outliers. In order to keep the data set balanced, we also do not account for the creation of South Sudan in 2011, treating Sudan as a single country throughout the entire sample period.

areas. Moreover, some of the variables we use (e.g., population) are obtained through geographical interpolation across larger areas, possibly inducing measurement error in the estimates of these variables for small geographical units. For these reasons, we also experiment below with coarser aggregations that correspond to administrative divisions rather than cells.

3.1. *Mobile Phone Coverage: GSM Association Data*

Data on mobile phone coverage are collected by the GSM Association (GSMA), the association that represents the interests of the mobile phone industry worldwide, in partnership with Collins Bartholomew, a digital mapping provider. The data come from submissions made directly by mobile operators for the purposes of constructing roaming coverage maps for end users.

The coverage refers to the GSM network, which is the dominant standard in Africa, with around 96 percent of the market share. The data licensed to us for this analysis provide, for all years between 1998 and 2012, yearly geo-located information on mobile phone coverage.[7] The data report separate information on the availability of 2G, 3G, and fourth generation (4G) technologies, although most of the variation over the sample period refers to the adoption of 2G technology.[8]

The data allow us to measure the adoption of mobile phone technology at a finely disaggregated geographical level. More specifically, the geographical precision of the original data submissions ranges between 1 km^2 on the ground (for high-quality submissions based on a geographic information system (GIS) vector format) to 15–23 km^2 (for submissions based on the location of antennas and their corresponding radius of coverage) (GSMA (2012)).[9]

Our data represent a considerable improvement over similar sources of information used in previous studies. Most cross-country studies typically employ measures of mobile subscription or penetration, which vary only at the country level. On the contrary, studies at finer levels of geographical detail commonly focus on just one country (e.g., Jensen (2007)). The only work we are aware of that uses detailed information on mobile phone availability at a fine level of geographical detail for more than one country is Buys, Dasgupta, Thomas, and Wheeler (2009) and Pierskalla and Hollenbach (2013), although these studies cover limited time spans (respectively 1999–2006 and 2007–2009).

3.2. *Protest Occurrence: Aggregate Data From GDELT, ACLED, and SCAD*

Our first source of data on political mobilization is GDELT (Leetaru and Schrodt (2013)), an open-access database that, through an automated coding of news wires, collects information on the occurrence and location of political events, including protests,

[7] Since data on coverage are not available for 2005 and 2010, we interpolate linearly across neighboring years to derive an estimate for these two years.

[8] Over the period, less than 2 percent of the continent's population was in reach of a 3G signal and there was virtually no 4G signal available.

[9] GSMA data provide information on signal availability rather than actual subscriptions, which are not available at this level of geographical detail. We aggregate our measure of mobile phone coverage at the country/year level and compare it with data on the number of subscribers by country and year from the International Telecommunications Union (ITU (2014)). A regression of the fraction of log subscribers over total population on the log fraction of individuals covered by mobile phone signal, controlling for country and year fixed effects, shows that a 10 percent increase in coverage is associated with a 3 percent increase in mobile phone subscriptions.

worldwide. The data set contains an average of 8.3 million fully geo-coded records of daily events per year for the entire world. Events in GDELT come from both digitalized newspapers and news agencies (e.g., Africa News, Agence France-Presse, The Associated Press, Xinhua, BBC Monitoring, The Washington Post, The New York Times, etc.) as well as from web-based news aggregators such as Google News, which gathers around 4000 media outlets. The data are extracted using an open-source coding algorithm called Textual Analysis By Augmented Replacement Instructions (TABARI). The algorithm sifts through news articles in search of actions and actors available in Conflict And Mediation Event Observations (CAMEO), a widely used coding system in the field of political science that provides a list of approximately 15,000 actions and 60,000 political actors. A precise location at the city or landmark level is assigned to each event using the GeoNames gazetteer, which includes over 10 million toponyms for 9 million places with 5.5 million alternate names in up to 200 languages (www.GeoNames.org). The data also report information on the number of sources and articles that refer to the same event as well as on the actors involved, although the latter information is missing for a large portion of the events. Out of the 20 primary event categories in the data, we focus on "protests," which are defined as "civilian demonstrations and other collective actions carried out as a sign of protest against a target." Importantly, the data do not provide any information on the issue at stake, the number of participants, or the original news sources.[10]

In order to probe the robustness of our analysis to the measures of protests used, we complement the analysis with two additional, manually compiled, data sets: ACLED (Raleigh et al. (2010)) and SCAD (Salehyan et al. (2012)). ACLED provides information on political violence during civil wars or episodes of instability and state failure starting from 1997, and has been used widely in the literature on civil conflict (e.g., Harari and La Ferrara (2018), Michalopoulos and Papaioannou (2015), Pierskalla and Hollenbach (2013)). Importantly, events that are potential precursors or critical junctures of conflict, like protests and riots during peaceful times, are also recorded. We focus on these events, which represent around 20 percent of the total number of records in ACLED. Events are manually compiled from local, regional, national, and continental media, and are supplemented by non-governmental organization (NGO) reports. As in GDELT, no information is available on either the issue or the number of participants. SCAD data provide information on social conflict events across Africa, including riots, strikes, protests, coups, and communal violence, starting from 1990. The data are compiled based on reports by the Associated Press and Agence France-Presse. Although SCAD is less widely used than ACLED, it has the advantage of providing, for each protest, information on the number of participants, which we also use to investigate whether our estimates suffer from a news-reporting effect.

3.3. *Protest Participation: Individual Level Data From Afrobarometer*

All of the data sets described above refer to protest occurrence and are derived from news reports. We complement this information with data from the Afrobarometer, a public attitude survey on governance and economic conditions in Africa (Afrobarometer (2011)). These data have been widely used for research in economics and political science (e.g., Michalopoulos and Papaioannou (2013), Nunn and Wantchekon (2011), Rohner, Thoenig, and Zilibotti (2013a)). Notably, in addition to a large array of socioeconomic

[10]In a comprehensive study of protests worldwide, Ortiz et al. (2013) list, in order of frequency, the following causes of protest that occurred between 2006 and 2013: economic justice and anti-austerity, failure of political representation, global justice, and rights of people. Most of these protests were against national governments.

variables, rounds 3–5 of the Afrobarometer provide individual-level information on participation in protests across 27 African countries between 2005 and 2012.[11] The data also provide information on mobile phone use.[12]

The version of Afrobarometer data made available to us also contains information on individuals' locality of residence. This allows, albeit with a certain degree of approximation, assigning individuals to the $0.5° \times 0.5°$ cells. To this end, we match localities in the Afrobarometer to data from GeoNames and, via this, to cells. In total, we are able to assign 78,167 individuals (81 percent of total respondents). Further details on the assignment procedure are reported in Appendix B.3 of the Supplemental Material. One caveat of the Afrobarometer survey compared to GDELT, ACLED, and SCAD is that, apart from the data only covering 27 out of the 48 countries in GSMA, the time span is also more limited, and a reduced number of cells per country are covered.

3.4. *Cell-Level Characteristics*

We use data from multiple sources to compute a large array of cell characteristics. These include population, climatic variables (temperature and rainfall), natural resources (fraction of the cell's area covered by oilfields, presence of mineral and diamond mines), geography (fraction of the cell's area covered by mountains and forests, latitude and longitude of the cell centroid, cell area, distance of the centroid to the coast, and whether the cell is on the coast), administrative features (whether it hosts the country's capital, distance to capital, whether on the border and distance to the border, number of cities in the cell, first- and second-order administrative division the majority of the cell belongs to), infrastructures (km of primary roads, km of electrical grid), and measures of socioeconomic development (infant mortality rate and night light intensity). Note that, with the exception of population, temperature, rainfall, and night lights, all other variables are time-invariant. Definitions and original sources are reported in Table A.I of the Supplemental Material.

One important additional variable that we use in the analysis is lightning strike intensity. These data come from the Global Hydrology Resource Center, which makes data collected by the NASA through space-based sensors publicly available. In particular, we use average lightning strike intensity between 1995 and 2010 in $0.5° \times 0.5°$ cells (Cecil, Buechler, and Blakeslee (2014)).[13] Further information on the measurement of lightning strikes is reported in Appendix A.2 of the Supplemental Material.

4. DESCRIPTIVE STATISTICS

In this section we provide descriptive evidence on the spread of mobile technology and mass political mobilization throughout Africa.

Figure 1 shows a map of mobile phone coverage over the entire continent at 5-year intervals. While in 1998 only 3 percent of the African territory was covered by mobile

[11]Countries are Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Ghana, Guinea, Ivory Coast, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, Sierra Leone, South Africa, Swaziland, Tanzania, Togo, Uganda, Zambia, and Zimbabwe.

[12]Information on mobile phone use is only available in rounds 4 and 5. Based on socioeconomic characteristics of the respondent and coverage in the cell, we predict mobile phone use for each individual, including those in round 3. The exact procedure is discussed in Appendix B.3 of the Supplemental Material.

[13]Importantly, NASA does not provide yearly information on lightning strike intensity at the level of geographical detail of the $0.5° \times 0.5°$ cells. However, there is robust evidence that local lightning strikes activity is very persistent over time (Andersen et al. (2012)).

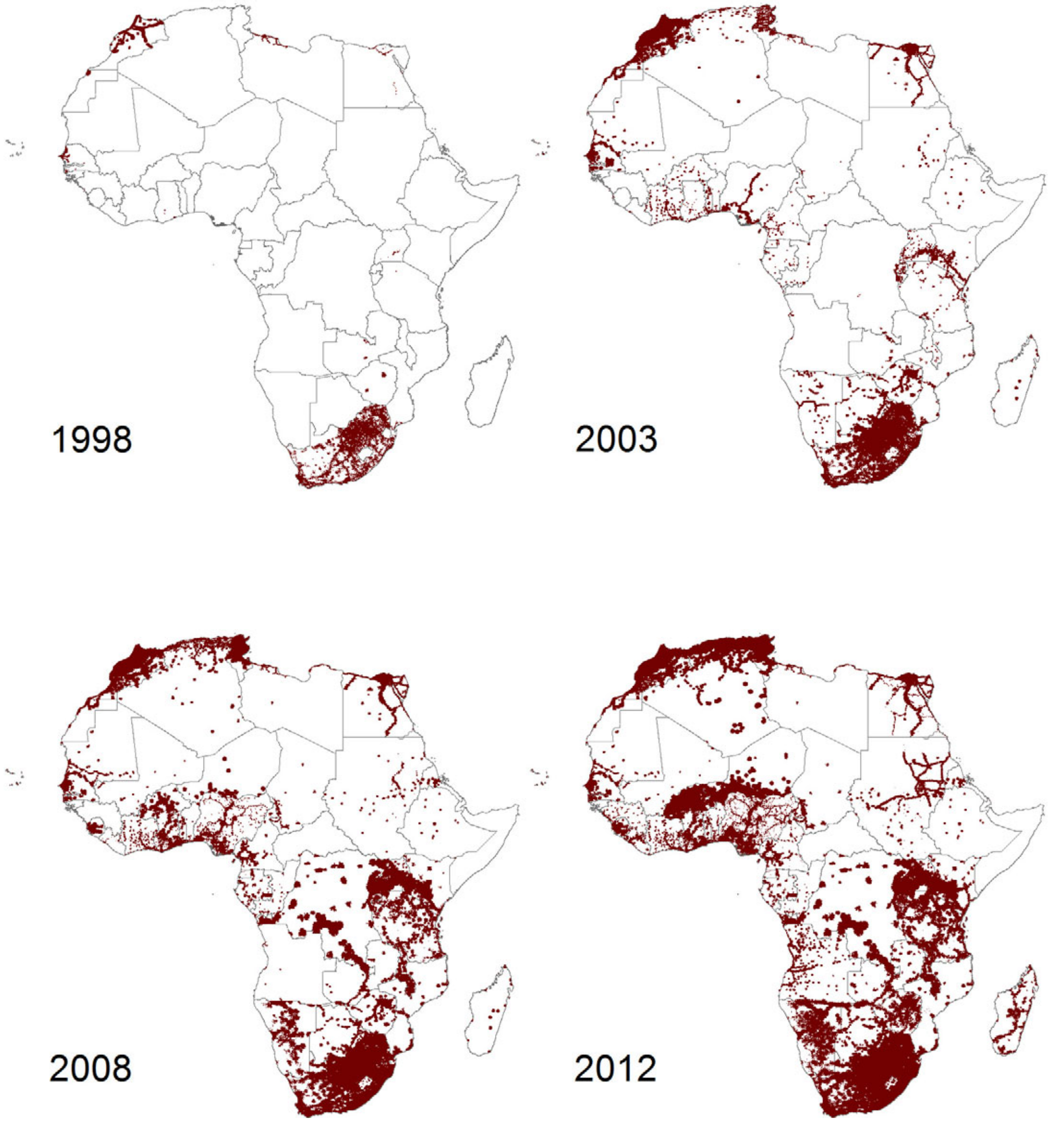


FIGURE 1.—Mobile phone coverage diffusion in Africa: 1998–2012. The figure reports georeferenced data on mobile phone coverage for all of Africa at 5-year intervals between 1998 and 2012. Sourcc: GSMA.

phone signal, by 2012 this figure had risen to 27 percent. Figure A.1 in the Supplemental Material zooms onto Nigeria, superimposing the lattice of $0.5° \times 0.5°$ grid cells, showing the level of geographical detail allowed by our data together with the very rapid expansion of mobile phone coverage over the period.

These figures clearly do not provide information on the fraction of the population covered, as coverage is higher in more populated areas. We hence use information on the share of each cell's area that is covered by mobile phone technology and on population by cell, and we assume that population is uniformly distributed within cells in order to compute the fraction of individuals reached by the mobile phone signal in each cell/year. In the rest of the paper, we use this as our primary measure of mobile phone penetration. We aggregate across cells using population weights to obtain country-level or continent-level measures of mobile phone penetration. For robustness, in the analysis we also experiment with measures of coverage by cell that take into account the precise distribution of mobile phone signal and population within cells.

The average population-weighted mobile phone coverage throughout the 1998–2012 period across the entire continent is 0.43 (see the first row of Table A.II of the Supplemental Material). Over the period, continent-wide coverage increases considerably, from 8.8 percent in 1998 to 64 percent in 2012. This rapid continental growth masks, however, large differences across countries. While among early adopters such as Morocco and South Africa, coverage was virtually ubiquitous by the end of the period, in countries like Ethiopia and Mali, still less than 10 percent of the population was covered in 2012 (see Appendix B.1 of the Supplemental Material for further details).

Turning to the information on protests, Figure A.2 in the Supplemental Material reports GDELT data for Cairo in 2011 and shows the level of geographical detail allowed by our data. There are as many as 70 different landmarks identified, with the size of the circles indicating the number of days of protest in each precise location. While events in Tahrir Square and Cairo University are easily recognizable, less well-known episodes, such as the recurrent strikes in the industrial district of Helwan in the southern suburbs of the city, are also identified.

In order to combine information on protests with information on coverage of mobile phone technology, we compute the total number of protest events falling in each cell in each year and we standardize this number to the cell's population (in 100,000s). On average, over the entire continent, GDELT records 1.33 yearly protests per 100,000 individuals.

Trends in protests across the continent are shown in Figure 2, which reports the evolution of protests per capita over the entire continent. There is a pronounced positive trend

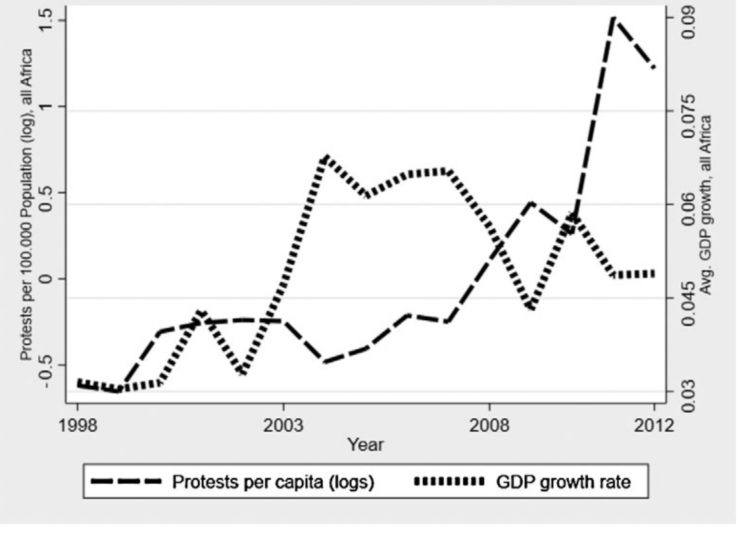


FIGURE 2.—The evolution of GDP growth and protests over time in Africa. The figure reports continent-wide log protests per 100,000 individuals (dashed line) and the rate of GDP growth (dotted line) as a function of time. Continent-wide GDP growth is obtained as a population-weighted average of GDP growth in each country.

in the incidence of protests, with an overall increase of around 200 log points over the period. A temporary increase occurred in 2008–2009, when the food riots erupted; then a very pronounced rise in 2010–2012 when the Arab Spring swept through the northern part of the continent. Alongside trends in log protests per capita, Figure 2 reports average GDP growth across Africa during this period (the dotted line).[14] A remarkable feature of the data is that protests are strongly countercyclical, in line with the literature cited in the Introduction that suggested that protests are more likely to occur when reasons for grievance abound and when the opportunity cost of participation falls, both of which are more likely to occur during recessions.

Data from ACLED and SCAD provide estimates of the incidence of protests per 100,000 individuals on the order of 0.09 and 0.06, respectively, that is, between 1/15 and 1/22 of what is found in GDELT (see the second, third, and fourth rows of Table A.II of the Supplemental Material). One possible reason why the number of protests in GDELT is much greater than in ACLED and SCAD is that GDELT data are less likely to suffer from type-1 error, whereby truly occurring protests are not reported. In particular, small mobilization events might be less likely to be recorded in ACLED and SCAD compared to GDELT. On the other hand, given the automated coding, it is possible that GDELT suffers from a higher rate of type-2 error compared to ACLED and SCAD, whereby events that are not genuine protests are incorrectly classified as such. A related problem is that, although every attempt is made in GDELT to collapse multiple reports of a unique event into a single record, the algorithm might fail to do so if the variables that uniquely identify an event differ across articles and news wires. We return to this issue when we present our regression estimates.

We also investigate the correlation between GDELT, ACLED, and SCAD. Despite marked differences in the number of reported protests, the incidence of protests across countries and over time, as well as within countries, is very highly correlated across the three data sets (see Appendix B.2 of the Supplemental Material).

Turning to the individual-level data, during the period of observation, on average 11 percent of individuals in the Afrobarometer report having participated in at least one protest in the previous year and between 7 and 15 percent report strongly disapproving and not trusting the president at all. At 68 percent, cells in the Afrobarometer display higher than average mobile phone coverage. The fraction of the population reporting using a mobile phone at least once a day is consistently 69 percent. Further descriptive statistics on the Afrobarometer data are reported in Appendix B.3 of the Supplemental Material.

Finally, focusing on the instrument, Figure A.3 in the Supplemental Material reports the average number of ground lightning strikes between 1995 and 2010 for each of the $0.5° \times 0.5°$ cells. Africa has the highest lightning density on Earth, with an average of 17.3 lightning strikes per square kilometer per year, compared to the world average of 2.9 (Cecil, Buechler, and Blakeslee (2014)). The highest annual number of lightning strikes is found in the Democratic Republic of Congo, with almost half a million lightning strikes per year in each cell or about one strike every 2 days for each square kilometer. Notably, even within countries, there is substantial variation in lightning intensity across areas, suggesting that this instrument has the potential to generate useful variation in the rate of mobile phone adoption across cells.

[14]This is a weighted average of countries' GDP growth using cell population as weights. GDP growth is taken from the World Development Indicators (World Bank (2012)) and represents the annual percentage growth rate of real GDP at constant 2005 national prices (in million 2005 USD).

5. ECONOMETRIC MODEL

As shown in Figure 2, protests respond to the state of the economic cycle, increasing during recessions and falling during booms. Worsening economic conditions can increase the incidence of protests because they provide reasons for grievance and because they reduce the opportunity cost of participating in mass mobilization. In Section 5.1, we present a regression model that expresses protest occurrence as a function of mobile phone diffusion and its interaction with the state of the economic cycle. In Section 5.2, we turn to the micro-founded model that underlies this aggregate model. We show how data on protest participation and mobile phone use at the individual level, both of which are available from the Afrobarometer, can be employed not only to validate results based on aggregate data, but also in an attempt to disentangle and quantify the different mechanisms of impact.

5.1. *Aggregate Outcomes: Protest Occurrence*

We start by modelling the occurrence of protests in a cell as a function of mobile phone availability. We also allow for the effect of mobile phone coverage to vary as a function of economic conditions.

If we denote a generic cell by j, with $j \in c$, where c denotes a country and t denotes a generic year, and ignore other controls, our regression model is

$$\bar{y}_{jct} = \beta_0 + \beta_1 \mathrm{Cov}_{jct} + \beta_2 \Delta\mathrm{GDP}_{ct}\mathrm{Cov}_{jct} + f_j + f_{ct} + u_{jct}, \tag{1}$$

where $\bar{y}_{jct}$ denotes the incidence of protests, Cov_{jct} is a measure of local mobile phone coverage, $\Delta\mathrm{GDP}_{ct}$ is a measure of the country's economic growth, f_j and f_{ct} are, respectively, cell fixed effects and country $\times$ year effects, and u_{jct} denotes the error term. As we condition on cell and country $\times$ year fixed effects, parameter estimates capture the average (across countries) effect of the explanatory variables on the differential growth in protests across cells in the same country. The coefficient β_1 in equation (1) captures the effect of mobile phone coverage on protests at zero GDP growth, while β_2 measures how country-level economic booms and downturns translate into differential protest activity in areas with different mobile phone coverage. If mobile phones magnify the effect of economic downturns on protests, this coefficient will be negative. Below, we also present more restrictive specifications where we constrain the coefficient β_2 to 0, in which case the parameter β_1 captures the effect of mobile phones on protests at *average* growth.[15]

A concern with the estimates of model (1) is that coverage is unlikely to be randomly allocated across areas, potentially generating a bias in the estimates of model parameters. In order to deal with this concern, we use an instrumental variable strategy that exploits differential rates of adoption of mobile phones across areas characterized by different incidences of lightning strikes. Frequent electrostatic discharges during storms are known to damage mobile phone infrastructures and, in particular, antennas on the ground that transmit the signal in their vicinity, thus negatively affecting connectivity. As a consequence, this reduces both the supply of (as power surge protection is costly and poor connectivity makes the investment in technology less profitable) and the demand for (as

[15]Clearly, by including country $\times$ year effects, we are unable to identify the effect of GDP growth on protests per se. We have also experimented with regressions that include additive country and year effects in addition to the countries' GDP growth. Estimates of β_1 and β_2 remain effectively unchanged, while we consistently find a negative effect of GDP growth on protests at average coverage.

the risk of intermittent communications discourages adoption of) mobile phone services (ITU (2003)).[16] Hence, one would expect to see slower adoption of mobile phone technology in areas subject to higher incidence of lightning strikes.

In practice, we instrument mobile phone coverage with the interaction between the average number of lightning strikes in a cell over the period 1995–2010, denoted by lightning_{jc} and a linear time trend t that captures the generalized increase in mobile phone adoption across the continent. In formulas, our first-stage equations are

$$\text{Cov}_{jct} = \delta_0 + \delta_1 Z_{jct} + \delta_2 \Delta\text{GDP}_{ct} Z_{jct} + f_j + f_{ct} + \eta_{jct}, \quad (2)$$

$$\Delta\text{GDP}_{ct}\,\text{Cov}_{jct} = \theta_0 + \theta_1 Z_{jct} + \theta_2 \Delta\text{GDP}_{ct} Z_{jct} + f_j + f_{ct} + \mu_{jct}, \quad (3)$$

where $Z_{jct} = \text{lightning}_{jc} \times t$.

Consistency of the 2SLS estimates relies on the assumption that, other than because of differences in mobile phone coverage and its differential effect over the business cycle, protest activity does not vary differentially over time across cells depending on average lightning strike intensity. This identification assumption might not hold unconditionally, as lightning strikes could be correlated with geographical variables (i.e., distance to the coast or longitude and latitude), climatic variables (i.e., rain and temperature), or the availability of other infrastructures or services (i.e., electricity) that might have an independent effect on protests. We temper these concerns by including in all regressions the available time-varying cell-level characteristics (log local population, log yearly temperature, log rainfall, and log night light intensity) as well as a large number of cross-sectional cell characteristics interacted with a linear time trend (see the footnotes to Table I). In Section 6.2 we also present a battery of tests in support of our identification assumption.

5.2. *A Micro-Founded Model: Mechanisms of Impact*

In this section we introduce a micro-founded empirical model of protest participation that is consistent with the aggregate model in Section 5.1. Compared to the aggregate model, the advantage of this model is that it allows us to investigate the channels through which mobile phones may affect protest participation.

The underlying theoretical model is described in detail in Appendix A.1 of the Supplemental Material. It is worth emphasizing that several models can deliver similar implications to our own and that our estimates are not meant to be interpreted as structural estimates of the model parameters.

In the model, the private cost of participation in a protest falls when the economy deteriorates, and the individual utility from participation increases with the fraction of connected individuals participating. Individuals make educated guesses about the probability of their connections participating given the degree of connectedness in society, which is publicly known. For convenience, we focus on the stable equilibrium. The best-guess estimate of the probability of participation of each individual's connections is the same for all individuals, irrespective of their degree of connectedness, and this also turns out to be the fraction of individuals participating in equilibrium. Worse economic conditions increase participation through two channels. First, they increase everybody's willingness to participate, a mechanical or purely compositional effect that we attribute to individuals' information about the state of the economy; second, via a spillover effect that results from

[16]We provide additional background information on the effect of lightning strikes on mobile phone functionality in Appendix A.2 of the Supplemental Material.

strategic complementarities in protest occurrence, an effect that we attribute to coordination among individuals.[17]

Mobile phones have the potential to affect both margins of response, namely to make individuals more responsive to variations in economic conditions—an effect we label *enhanced information*—and to changes in others' willingness to participate—an effect we label *enhanced coordination*.

The micro-founded model of behavior predicts in particular that individual i's protest participation y_{ijct} will depend on the state of the economy $\Delta\mathrm{GDP}_{ct}$ and on the average protest participation in the cell $\bar{y}_{jct}$. Mobile phone use, denoted by d_i, can potentially affect both the intercept and the slope coefficients. In formula form,

$$y_{ijct} = \gamma_0 + \gamma_1 d_i + \gamma_2 \Delta\mathrm{GDP}_{ct} d_i + \gamma_3 \bar{y}_{jct} + \gamma_4 \bar{y}_{jct} d_i + f_j + f_{ct} + u_{ijct}. \tag{4}$$

The parameter γ_1 provides a measure of the differential protest activity between people with and without mobile phones, irrespective of GDP growth and others' propensity to participate. The parameter γ_2 provides a measure of the differential response to changes in economic conditions among those who have mobile phones relative to those who do not. The parameter γ_3 provides a measure of the response to changes in others' participation, while γ_4 measures the differential response to this spillover effect among those who are connected.

Note that aggregating equation (4) across individuals by cell and assuming, for simplicity, that the fraction of people with mobile phones in a cell ($\bar{d}_{jct}$) equals the fraction of people covered by the signal (Cov_{jct}) gives precisely equation (1), where $\beta_2 \approx \frac{\gamma_2}{(1-\gamma_3-\gamma_4\bar{d})}$ and $\bar{d}$ is the overall fraction of individuals using a mobile phone in the economy.

The more mobile phones make individuals responsive to the state of the economic cycle (γ_2) or to their fellow citizens' propensity to participate (γ_4), the greater the effect of mobile phone coverage on protests during recessions (β_2). If one is able to identify the parameters in equation (4), then one will be able to decompose the effect of mobile phone coverage on protest activity in response to changes in economic conditions into a compositional (*enhanced information*) effect and a spillover (*enhanced coordination*) effect. Identification of model (4) involves some challenges, though. Even ignoring the possibility of nonrandom allocation of mobile phones across areas and individuals, estimates of the model will still potentially be plagued by a classical reflexivity problem (Manski (1993)), as individual i's protest activity, y_{ijct}, will also affect others' propensity to protest, $\bar{y}_{jct}$.

However, aggregation of equation (4) across individuals suggests that one can control for this source of endogeneity by instrumenting average participation in the economy, $\bar{y}_{jct}$, with the fraction of people using mobile phones, $\bar{d}_{jct}$, and its interaction with GDP growth. Intuitively, we rely on the assumption that, conditional on d_i, our admittedly restrictive model implies that the fraction of those covered in society will only matter for individual participation through the spillover effect.

6. EMPIRICAL RESULTS

In this section we turn to the empirical analysis. We start by presenting 2SLS estimates of equation (1), which relates the incidence of protests by cell and time to mobile phone

[17]While most of the recent theoretical work cited in the Introduction focuses on strategic complementarities, an earlier literature emphasizes the public good nature of collective action, which may lead to free riding in protest participation (Olson (1965)).

coverage and its interaction with GDP growth. We subsequently turn to the microdata from the Afrobarometer and present estimates of equation (4).

6.1. *Aggregate Outcomes*

Table I presents first-stage and 2SLS estimates of the model (equations (2), (3), and (1)). All regressions are weighted by population size, although we also experiment below with unweighted regressions. In order to allow for unrestricted correlation in the error term across observations in the same country, we cluster standard errors at the country level. Because of the limited number of clusters, we use wild cluster bootstrap standard errors (Cameron, Gelbach, and Miller (2008)), although below we also present results based on alternative clustering schemes.[18]

Columns 1 and 2 of Table I report estimates of the first-stage equations. Estimates in column 1 show that greater lightning strike activity leads to a slower adoption of mobile phone technology: a 1 s.d. increase in the number of lightning strikes per square kilometer (0.42) leads to lower growth in coverage of around 0.25 p.p. a year (-0.006×0.42), that is, a differential growth of 3.75 p.p. over the entire 15-year period. Column 2 reports regression estimates where the dependent variable is the interaction between coverage and ΔGDP. For the model to be well specified, the coefficient of Z in column 1 is expected to be similar to the coefficient of $Z\Delta$GDP in column 2, which is indeed the case (-0.006 compared to -0.015). At the bottom of the table we also report Sanderson and Windmeijer's (2016) conditional first-stage F-statistics for the validity of the instruments. As the Stock–Yogo 10 percent and 15 percent critical values for a perfectly identified model with two endogenous variables are, respectively, 7.03 and 4.58, it appears that we can reject that the instruments are weak.

The 2SLS estimates are reported in columns 3–8 of Table I. The dependent variable in all regressions is the log number of protests (plus 1 to account for zeros) per 100,000 individuals. The odd-numbered columns present estimates of model (1), where we include only mobile phone coverage, that is, we constrain the coefficient β_2 to zero, while the even-numbered columns present regressions that also include the interaction term between coverage and GDP growth.

We start by focusing on results from GDELT. When we restrict the parameter β_2 to zero in column 3, we find a small and statistically insignificant effect of mobile phone coverage on protests, while the results from the unrestricted model in column 4 show that protests respond more to economic downturns in covered than in uncovered areas: a 1 s.d. fall in GDP growth leads to an increase of 23 percent (-5.776×0.04) in the per capita protest differential between areas with full coverage and with no coverage. We also find no statistically significant effect of coverage at zero GDP growth (the coefficient on the variable coverage in column 4).

In order to validate the results from GDELT, in the remaining columns we report the same regression estimates based on ACLED and SCAD, respectively. The patterns of estimates are very similar to those found for GDELT. Results show once more that there is no average effect of mobile phones on protests (columns 5 and 7), while mobile phones amplify the effect of economic downturns on protests (columns 6 and 8). The coefficient

[18]As mentioned, we have a total of 10,409 cells and 156,135 cells $\times$ year observations. In the analysis we exclude 3105 cell $\times$ year observations with missing GDP growth (Djibouti from 2008 to 2012, and Libya from 1998 to 1999 and from 2010 to 2012) and 2147 observations with missing temperature. This gives a total of 150,883 observations.

TABLE I

MOBILE PHONES AND PROTESTS: 2SLS[a]

	First Stage		2SLS					
	Coverage	ΔGDP × Coverage	Protests	GDELT	Protests	ACLED	Protests	SCAD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Z	−0.006	0.001						
	[0.05]	[0.12]						
ΔGDP × Z	0.008	−0.015						
	[0.67]	[0.08]						
Coverage			−0.876	−0.493	−0.079	0.063	−0.187	−0.062
			[0.15]	[0.18]	[0.80]	[0.85]	[0.26]	[0.63]
ΔGDP × coverage				−5.776		−2.151		−1.886
				[0.03]		[0.02]		[0.02]
SW $F - Z$	11.580							
SW $F - \Delta$GDP × Z	6.968							
Endogeneity test p-value			0.24	0.18	0.77	0.16	0.33	0.02
Observations	150,883	150,883	150,883	150,883	150,883	150,883	150,883	150,883

[a]Columns 1 and 2 report first-stage regressions of coverage and ΔGDP × coverage on average lightning intensity in a cell interacted with a linear time trend (Z), and the interaction of this variable with GDP growth, equations (2) and (3). Columns 3–8 report 2SLS estimates of equation (1) based on GDELT, ACLED, and SCAD, respectively. Specifications in columns 3, 5, and 7 constrain the coefficient β_2 to zero. All specifications include cell and country × year fixed effects, plus the time-varying controls log population, log rainfall, log temperature, and log night lights plus the interaction between a linear time trend and the following cross-sectional cell characteristics: fraction of the cell's area covered by mountains and forests, oilfields, presence of mineral and diamond mines, latitude and longitude of the cell centroid, cell area, distance of the centroid to the coast and whether the cell is on the coast, whether the cell hosts the country's capital, distance to capital, whether on the border and distance to the border, number of cities in the cell, dummies for first-order administrative division the majority of the cell belong to, kilometers of primary roads, kilometers of electrical grid, infant mortality rate, and dummies for missing values of all these variables. Summary statistics for these variables are reported in Table A.II of the Supplemental Material. All regressions are weighted by cell population. The p-values for wild cluster bootstrap standard errors at the level of country are reported below each coefficient. The value of the Sanderson and Windmeijer (2016) conditional first-stage F-statistics for the validity of the instruments is also reported in columns 1 and 2. The p-values for a test of the joint endogeneity of the regressors are reported in columns 3–8.

on the interaction term between GDP growth and coverage is negative and significant for both ACLED (−2.151) and SCAD (−1.886), implying that a 1 s.d. fall in GDP growth is associated with an increase in the differential incidence of protests between an area with full coverage and an area without coverage of between 7.5 and 8.6 percent, around 1/3 of the effect found in GDELT.

Although point estimates based on ACLED and SCAD are smaller in magnitude than those based on GDELT, remarkably the results based on the three data sets are qualitatively similar. In all cases we conclude that in our sample of countries and years, characterized by strong average economic growth (4.9 percent), greater coverage did not lead per se to greater protest incidence. We do find, however, that mobile phone coverage played a significant role in magnifying the effect of recessions on protest occurrence, with an effect that is both statistically and economically significant.[19]

In the regressions we constrain the effect of coverage to vary with GDP growth in a linear fashion. One might object that our results may be driven by this functional form assumption. A related issue is that estimates in Table I imply, by extrapolation, that the incidence of protests may be lower in covered compared to uncovered areas during economic booms. In order to address both of these issues and to add further transparency to our regression analysis, in Figure 3 we report separate 2SLS estimates of the effect of coverage on protests by percentile of the distribution of ΔGDP. In practice, we estimate 50 parameters by groups of two percentiles (1–2, 3–4, ..., 99–100). Point estimates are reported in the figures as dots. We superimpose on this graph a kernel-weighted local polynomial regression. As is evident from the figure, significant effects are only found at the bottom of the GDP growth distribution, while the effects at the top of the GDP growth distribution are very close to zero, confirming that the effect of mobile phones on protests manifests largely during economic downturns.[20]

Before closing this section, and in order to add transparency to the identification strategy, we conclude by presenting graphical evidence on the reduced-form relationship between protests and the instrument. For protests to respond negatively to the state of the economic cycle when coverage increases ($\beta_2 < 0$ in equation (1)), and given that coverage varies negatively with the instrument ($\delta_1 < 0$ in equation (2)), one would expect the protest differential between areas with high and low lightning intensity to be positively correlated with GDP growth. Figure 4 reports the within-country change in the differential in log GDELT protests between high (in the top quartile of the continent distribution) and low (in the bottom quartile) lightning intensity areas alongside average growth in GDP. Consistent with the regression estimates, there is indeed evidence of a positive correlation between the two series.

[19]For comparison, we report ordinary least squares (OLS) estimates of model (1) in Table A.III of the Supplemental Material. Although the endogeneity test at the bottom of Table I suggests that the OLS and 2SLS are not statistically different, if anything, it appears that the former provides conservative estimates of the parameter of interest (β_2). Differences are likely due to measurement error and to the fact that observations affected by the instrument (i.e., the compliers) are more highly populated places (Figure A.4 in the Supplemental Material), where the impact of mobile phone usage on protests is also higher, potentially due to agglomeration effects. A third class of explanations has to do with omitted variables. Suppose that covered areas respond less to aggregate economic fluctuations than areas with no coverage; for example, because the former display greater diversification in production than the latter. In this case, a rise in coverage in the face of an aggregate economic slowdown will lead to an attenuated effect on protests.

[20]The corresponding figure for the OLS is reported in Figure A.5 of the Supplemental Material. One can still observe a negative gradient in the coefficients across levels of economic growth similar to the results for the 2SLS. As expected, estimates are more precise than the corresponding 2SLS.

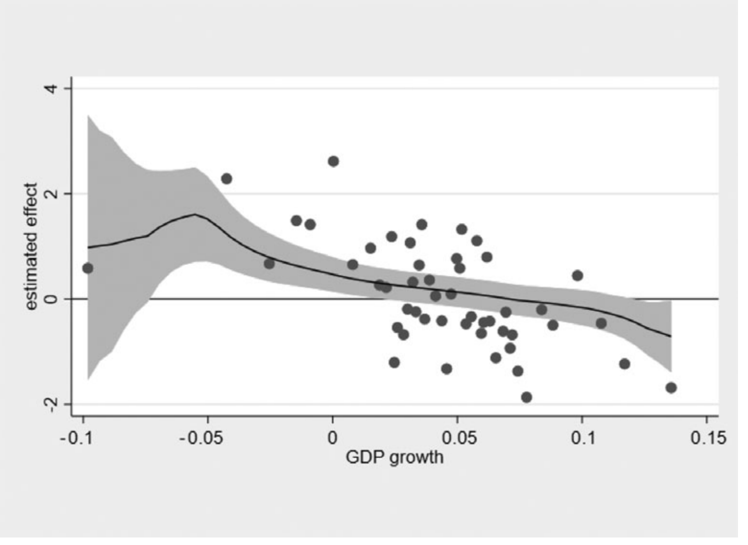


FIGURE 3.—The effect of coverage on protests at different levels of GDP growth: 2SLS. The figure reports 2SLS estimates of the effect of coverage on protests by pairs of percentiles of the Δ GDP distribution, estimated nonparametrically. Point estimates are reported in the figures as dots. We superimpose a kernel-weighted local polynomial regression where each observation is weighted by the inverse of the square of the standard error of the associated estimate. We use a polynomial of degree 0 and an Epanechnikov kernel function, with a "rule-of-thumb" bandwidth. The graph reports this estimated regression fit as well as the 95 percent confidence interval around the prediction.

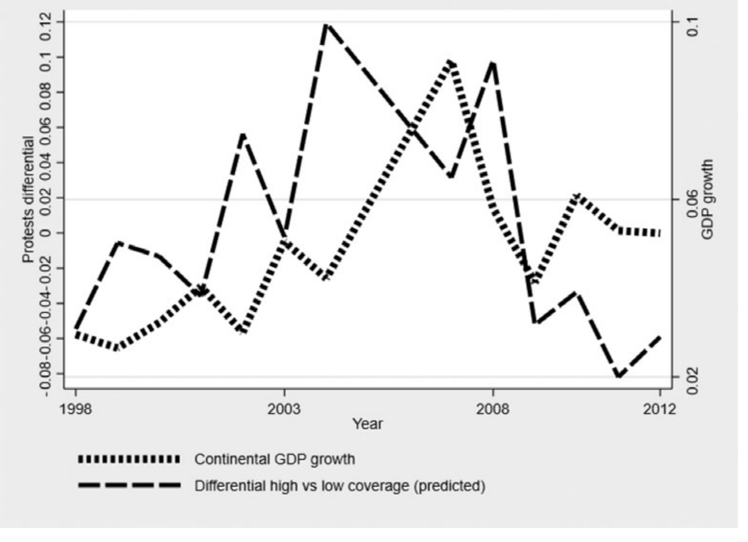


FIGURE 4.—Protests differential between high- and low-lightning-intensity areas and GDP growth. The figure reports the within-country trend in the log protest differential between high- arid low-lightning-intensity areas (dashed line) and the continent-wide rate of GDP growth (dotted line). High- (low-) lightning intensity refers to observations in the top (bottom) quartile of the continent distribution. The series are population-weighted averages across countries. Data used only refer to countries-years with observations in both the top and bottom quartile of the lightning intensity distribution.

6.2. *Evidence in Support of the Identification Assumption*

In this section, we present evidence in favor of our identification assumption. First, if, as assumed, lightning strikes and their interaction with GDP growth affect protests only through their impact on mobile phone coverage, then one would expect no correlation between the outcome variable and these variables in periods when mobile phone technology was not available. We test for this using data on protests from GDELT, which are available since 1990, that is, before the spread of mobile phone technology in Africa. The top panel of Figure 5 reports average mobile phone coverage across the continent between 1990 and 2012.[21] Coverage is zero in 1990 and begins to rise starting in 1996. Growth is then basically linear, with a slight slowdown beginning in the mid-2000s.

The bottom panel of Figure 5 presents estimates of the reduced-form equation, where the dependent variable is protests from GDELT and the regressors are the instrument (Z_{jct}) and its interaction with GDP growth ($\Delta\text{GDP}_{ct} Z_{jct}$). In the figure, we focus on the coefficient on the interaction term, estimated separately by 3-year subperiods. We use the same specification as in columns 1 and 2 of Table I, that is, with the inclusion of cell fixed effects, country $\times$ year effects, cell-level time-varying controls, and all baseline characteristics interacted with a linear time trend. Consistent with the identification assumption, one can see that there is no effect of the instrument interacted with GDP growth on protests in the early period, that is, effectively up until the late 1990s. Point estimates are small and not statistically significant at conventional levels. Positive effects tend to manifest from the early 2000s, when coverage starts to increase, and like the spread of coverage, these effects follow an upward trend, with the gradient once more flattening toward the end of the period.[22]

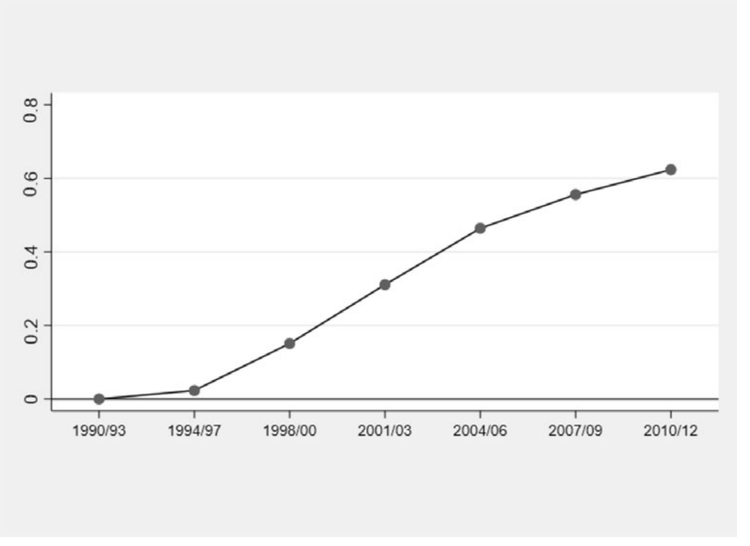
As an additional check, we test whether our instrument is correlated with other observed potential determinants of protests. A concern, in particular, could be that trends in lightning strikes and their interaction with GDP growth may be correlated with local economic conditions and, via this, affect the incidence of protests in a cell. Similarly, the instrument may be correlated with changes in weather patterns and, via this, with patterns of urbanization and desertification, leading to spurious correlation between protests and coverage. In column 1 of Table A.IV of the Supplemental Material, we present estimates of the reduced-form equation, where the dependent variable is now a measure of local economic development. To this end, we use the yearly growth rate in light intensity measured by satellites at night. Night lights are a widely used measure in the literature (Henderson, Storeygard, and Weil (2012)) and have been shown to proxy well for local economic activity. Importantly, we find that the latter seems not to vary with the instrument and its interaction with GDP growth. In columns 2 and 3, we report reduced-form equations where the dependent variables are now measures of log population and desertification, respectively. We find that neither of these variables appears to be correlated with the instrument and its interaction with GDP growth.

As a final test for the validity of the exclusion restriction, we restrict to protests occurring during the months when lightning intensity is at is lowest and close to zero (June, July, and August for countries south of the equator; December, January, and February

[21]To obtain this series, we use information on coverage from GSMA (available since 1998) as well as exploit the fact that 2G technology was not available in Africa until 1995, deriving for each cell a predicted measure of coverage by linear interpolation between 1995 and 1998. The series plots the population-weighted average coverage across the continent in each year.

[22]Results are similar, but more imprecise, using SCAD. Note that we cannot perform this exercise using data from ACLED, which began only in 1997.

Panel A: Mobile phone coverage by subperiods



Panel B: Reduced-form coefficients of $Z\ X\ \Delta GDP$ by subperiods

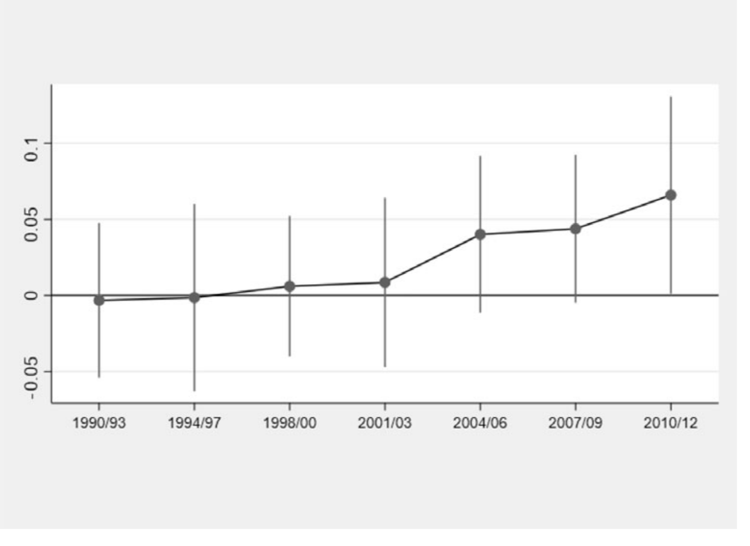


FIGURE 5.—Placebo test. Panel A reports the continental trend in mobile phone coverage by 3-year subperiods. Coverage is set at 0 for all cells before 1995 (the year in which mobile phone technology was first introduced in Africa) and is linearly interpolated at the cell level between 1995 and 1998 (the first year in our data). Panel B reports the estimated coefficients from the reduced-form regression of log protests per 100,000 people (winsorized at the 99th percentile) on the variable $Z \times \Delta$GDP by 3-year subpcriods and the corresponding 90 percent confidence intervals.

for countries north of the equator) (Christian et al. (2003)). This exercise aims to alleviate concerns that lightning directly discourages protest participation, leading to a spurious correlation between our instrumented measure of coverage and protest activity. Regression results in Table A.V of the Supplemental Material show qualitatively similar results to those in Table I, implying that our findings are not driven by a direct effect of lightning on protests.

6.3. *Additional Results*

In this section we present a number of checks meant to probe the robustness of our findings. We start by investigating the robustness of our baseline results to alternative choices of the level of clustering of the standard errors. We compute standard errors clustered by country (with no wild bootstrap) and by first- and second-order administrative divisions, hence allowing for an unrestricted cross- and autocorrelation among observations in the same administrative unit. We also compute two-way clustered standard errors by administrative units and country $\times$ year, thus additionally allowing for an arbitrary cross-correlation in the error terms among observations in the same country and year, but in different administrative units. These results are reported in Table A.VI of the Supplemental Material. The p-values typically become smaller the finer is the level of clustering. Overall, the coefficient on the interaction term is always significant at conventional levels, while the coefficient on coverage is consistently insignificant. In that table, we also report Ibragimov and Müller (2016) confidence intervals assuming clustering at the country level. These confidence intervals perform well even under cluster heterogeneity, which cannot be ruled out by assumption. Once more, we find no significant effect of coverage at zero GDP growth irrespective of the data used, while we find a significant negative effect of the interaction term, except for SCAD.

We next analyze the robustness of our results to the inclusion of additional controls, the level of geographical aggregation, the weighting scheme, transformations of the dependent variable, and different samples. A first concern could be that our model assigns all the differential variation across cells in protest activity over the business cycle to differences in lightning intensity. However, this will not be the case if, for example, areas with greater lightning intensity experience differential growth in population and population affects the responsiveness of protests to economic conditions. To test for this, Table II presents 2SLS estimates of the parameters of the model where we include, in addition to the interaction of GDP growth with mobile phone coverage, the interaction of GDP growth with log population (column 1), as well as a fully saturated specification that includes the interaction of GDP growth with all cell level time-varying characteristics (log rainfall, log temperature, log night lights, and log population) (column 2). Inclusion of the interaction between GDP growth and log population makes virtually no difference to our results. If anything, estimates of the parameter on the interaction term become slightly larger (by around 10 percent), although it should be noted that measurement error in population can attenuate its use as a control. Once we include all time-varying controls interacted with GDP growth, estimates of the coefficient on the interaction term remain negative and large, although they are no longer significant at conventional levels except for SCAD.

In column 3 we report 2SLS estimates where we aggregate our data at the level of the 6352 second-order administrative divisions as opposed to $0.5° \times 0.5°$ cells. Point estimates of the coefficient on the interaction term are qualitatively similar to those obtained by aggregating data by grid cells, although typically larger in absolute value and, other than for ACLED, significant at conventional levels. That the point estimates are marginally larger than those in our specification by cell might be interpreted as evidence of positive geographical spillovers in protest participation, although one would probably be unable to reject that the effects are the same for different levels of spatial aggregation. Note,

TABLE II

ROBUSTNESS CHECKS: 2SLS[a]

	+ Pop. × ΔGDP (1)	+ Controls × Δ*GDP* (2)	Aggr. at Admin2 (3)	Within-Cell Pop. Distr. (4)	One per Day (5)	High Precision (6)	Unweighted (7)	Number Protesters (8)	Pre-2011 (9)	No Middle Class (10)	Pre-Internet (11)	Pre-3G (12)
						GDELT						
Coverage	−0.528	−0.226	1.978	−1.035	−0.047	0.446	−0.128		−0.444	−0.362	−0.298	−0.491
	[0.22]	[0.52]	[0.18]	[0.19]	[0.83]	[0.25]	[0.75]		[0.34]	[0.77]	[0.41]	[0.22]
ΔGDP × coverage	−6.259	−11.321	−8.987	−5.903	−4.534	−4.838	−7.088		−5.447	−3.101	−9.125	−5.780
	[0.04]	[0.18]	[0.10]	[0.09]	[0.02]	[0.02]	[0.20]		[0.05]	[0.07]	[0.03]	[0.03]
						ACLED						
Coverage	0.055	0.172	0.399	−0.013	0.103	0.168	0.173		−0.264	0.120	0.081	−0.156
	[0.90]	[0.49]	[0.29]	[0.97]	[0.70]	[0.44]	[0.47]		[0.35]	[0.29]	[0.51]	[0.57]
ΔGDP × coverage	−2.262	−4.232	−6.769	−2.045	−2.086	−1.690	−2.666		−1.779	−1.645	−2.239	−1.309
	[0.08]	[0.14]	[0.14]	[0.06]	[0.01]	[0.01]	[0.35]		[0.23]	[0.08]	[0.25]	[0.12]
						SCAD						
Coverage	−0.079	0.012	0.022	−0.189	−0.065	−0.052	0.056	1.413	−0.064	0.041	0.029	−0.052
	[0.58]	[0.95]	[0.91]	[0.39]	[0.62]	[0.71]	[0.65]	[0.30]	[0.64]	[0.81]	[0.77]	[0.66]
ΔGDP × coverage	−2.119	−3.572	−3.424	−1.866	−1.853	−2.075	−1.771	−15.328	−2.148	−1.682	−2.699	−1.832
	[0.01]	[0.05]	[0.06]	[0.04]	[0.02]	[0.02]	[0.41]	[0.02]	[0.07]	[0.06]	[0.04]	[0.03]
SW $F - Z$	8.329	11.110	1.150	1.618	11.580	11.580	3.618	11.580	5.675	26.410	24.100	12.390
SW $F - \Delta\text{GDP} \times Z$	13.980	8.871	0.940	3.230	6.968	6.968	3.137	6.968	3.266	13.100	4.030	5.888
Observations	150,883	150,883	93,875	150,883	150,883	150,883	150,883	150,883	131,375	68,800	96,770	148,670

[a]The table reports 2SLS estimates of equation (1). The upper panel refers to GDELT, the middle panel to ACLED, and the bottom panel to SCAD. All specifications include cell and country × year fixed effects plus the entire set of cell-level controls described in the footnotes to Table I. Column 1 includes additionally the interaction of log population × ΔGDP. Column 2 also includes all other time-varying cell characteristics (log night lights, log temperature, log rainfall) interacted with ΔGDP. Column 3 reports estimates based on data aggregated at the level of second-order administrative divisions. Column 4 reports estimates based on the exact distribution of population and coverage within cells (see text for details). In column 5 the dependent variable is the log number of days of protests per capita in a given location/year. In column 6 we restrict to protests with at least a 3-digit precision in both geographical coordinates. Column 7 reports estimates for regressions not weighted by cell population. Column 8 present regressions where the dependent variable is the log number of protesters (for SCAD only). Column 9 restricts to the period before the Arab Spring (1998–2010). Column 10 restricts to countries where the middle class represents less than 8 percent of the population as of 2010 (see text for details). Column 11 restricts to periods of no Internet availability in the country, based on data from the World Development Indicators (World Bank (2012)). Internet availability is defined as penetration greater than or equal to 3 percent of the population. Column 12 excludes cells/years with availability of 3G mobile phone technology in a cell, based on data from the GSMA. The p-values for wild cluster bootstrap standard errors at the level of country are reported below each coefficient. See also the notes to Table I.

though, that the conditional first stage F-statistics are now below standard Stock–Yogo critical values, possibly due to the smaller number of observations.

In column 4 we present estimates using a measure of mobile phone coverage that takes into account the precise distribution of the population within cells. In practice, rather than using the fraction of land covered by the signal in each cell, we exploit information on the exact distribution of population and mobile phone coverage within cells in order to derive a measure of the fraction of the population in that cell covered by mobile phone signal. Point estimates are effectively insensitive to the use of this alternative measure of coverage.

In column 5 we address the concern that the data might fail to successfully de-duplicate protests when the latter are reported in different articles or outlets, hence increasing the rate of false positives. We thus construct an alternative measure of protests, that is, a variable that takes a value of 1 if at least one protest event is recorded in a certain location on a specific day, treating events in the same location but classified as different in the data as a single event. This makes no substantial difference to our results, which remain largely in line with those in Table I, both in terms of magnitude and statistical significance.

In column 6 we restrict to protests with at least a 3-decimal digit precision in both of their geographical coordinates, out of a concern that the locations of some of the events in the data might not be precisely identified. The results for the subsample of these more precisely identified protests (which represent between 70 and 80 percent of the total sample depending on the data set used) are very similar to those in Table I.

Regressions in Table I are weighted by cell population. In column 7 we report results for unweighted regressions. By failing to weight by population in our baseline specification, clearly, we recover effects for an average cell rather than for an average person in the population. In any case, point estimates from the unweighted regressions are very similar to our main results but imprecisely estimated and not statistically significant at conventional levels, while the Sanderson-Windmeijer (SW) test marginally fails.

In column 8 we examine the effect of mobile phone coverage on the intensive margin of protest occurrence that we measure as the log number of protesters (plus 1 to account for zeros) in each cell $\times$ year using SCAD data.[23] In line with results in Table I, we again find a negative coefficient on the interaction term. This also eases the concern that mobile phones increase the probability that an event is reported in the data and hence that our estimates capture a selective reporting effect rather than a genuine effect of mobile phones on protests.[24]

Finally, we show that our findings are not driven by specific samples or periods. In column 9 we restrict to the pre-2011 period out of a concern that our results are driven exclusively by the Arab Spring, something for which we find no evidence. In column 10 we exclude countries that experienced a rapid rise in the middle class. An influential body of literature suggests that by increasing government accountability, the middle class is instrumental to democratic progress (e.g., Moore (1966)). The potential criticism here is that

[23]As the number of protesters in SCAD is classified in bands (<10, 11–100, 101–1000, 1001–10,000, 10,001–100,000, 100,000–1,000,000, and $>1{,}000{,}000$), we assign to each band the expected number of protesters assuming that log protesters is a normally distributed variable. To do so, we fit an ordered probit model to the data and derive estimates of the mean and standard deviation of the latent variable log protesters. We use these estimates and standard formulas for a normal distribution to derive the expected number of protesters in each band (these are, respectively, 3, 38, 327, 2820, 24,505, 215,784, and 1,932,666).

[24]Suppose, in particular, that selective reporting implies that marginal (i.e., smaller) protests are reported during economic downturns in covered areas. In this case, one would expect the coefficient on the interaction term to be positive, which is the opposite of what we find.

mobile phone adoption might be correlated to the rise of the middle class and that the latter and not the former is responsible for the observed increase in protests.[25] Irrespective of the data set used, results remain largely stable across specifications and similar to those reported in Table I. In the last two columns, we instead address the potential concern that areas with a high level of mobile phone coverage also have a high degree of access to the Internet. In column 11 we restrict to the pre-Internet period, while in column 12 we exclude cells/years with 3G technology.[26] In both cases, the coefficient of interest remains negative and statistically significant, confirming that our results are not driven by Internet availability.

In Table A.VII in the Supplemental Material we also consider additional transformations of the dependent variable. One relevant feature of the protest data is that their distribution is highly skewed to the right, with a few cells displaying a very high number of protests. For this reason, our baseline dependent variable is in logs. However, as alternative checks, in columns 1 and 2 we present estimates where, respectively, we trim and winsorize the log number of protests per capita at the top percentile, while in column 3 we use the square root of protests per capita, which is similar to the log transformation but does not suffer from the presence of zeros. As an additional check, in column 4 we also experiment with the number (instead of the log) of protests per capita. All these checks make no substantial difference to our results and, once more, we obtain negative and significant estimates of the parameter on the interaction term, irrespective of the data used.

In an attempt to further assess the impact of mobile phones on protests, which may differ depending on country and area characteristics, in Table III we investigate several dimensions of heterogeneity. We start by focusing on measures of institutional quality. The point estimates on the interaction term are consistently larger in absolute value and more precisely estimated in countries/periods characterized by autocratic (column 2) compared to democratic (column 1) regimes and when the traditional media are captured (column 4) with respect to when they are free (column 3). This suggests that mobile phones may be particularly effective in fostering protests when governments control traditional media and when citizens do not have the option of expressing their dissent via traditional forms of participation, such as voting. We also find that the effects are stronger in more relative to less populated cells (columns 5 and 6), possibly indicating that lower communication costs in urban compared to rural areas enhance coordination in protest participation. Finally, we investigate how a legacy of past violence may affect the response to economic downturns in areas with different levels of mobile phone coverage. A well-documented feature of violent conflicts is that they have long-term consequences on those affected, undermining national identity, generating social and economic tensions, and eroding trust across communities and toward institutions (Besley and Reynal-Querol (2014), Rohner, Thoenig, and Zilibotti (2013a,b)). Speculatively, this suggests that

[25]We use African Development Bank estimates (Ncube, Lufumpa, and Kayizzi-Mugerwa (2011)) of the fraction of the middle class by country as of 2010. We refer to measures that exclude the "floating" middle class and restrict to nations with a share of the middle class below 8 percent. This group encompasses the poorest countries in Africa (Burkina Faso, Burundi, Central African Republic, Chad, Democratic Republic of Congo, Equatorial Guinea, Eritrea, Guinea, Guinea Bissau, Liberia, Madagascar, Malawi, Mauritania, Mozambique, Niger, Rwanda, Sierra Leone, Sudan, Tanzania, Zambia, and Zimbabwe) and accounts for around 30 percent of the continent's population.

[26]Internet availability is defined as penetration greater than or equal to 3 percent of the population, based on data from the World Development Indicators (World Bank (2012)). The 3G mobile phone technology is calculated for each cell, based on data from the GMSA.

TABLE III

HETEROGENEOUS EFFECTS OF MOBILE PHONES AND PROTESTS: 2SLS[a]

	Institutions		Media		Pop. Density		Past Conflict	
	Democr. (1)	Autocr. (2)	Free (3)	Captured (4)	Low (5)	High (6)	No Violence (7)	Violence (8)
				GDELT				
Coverage	−0.543	−1.006	1.118	−0.455	−2.186	−1.599	1.285	−2.795
	[0.78]	[0.57]	[0.20]	[0.17]	[0.14]	[0.30]	[0.07]	[0.24]
ΔGDP × coverage	−3.055	−7.007	2.359	−5.973	3.878	−10.299	−3.466	−2.367
	[0.72]	[0.16]	[0.82]	[0.03]	[0.56]	[0.11]	[0.27]	[0.10]
				ACLED				
Coverage	2.577	−0.770	1.219	−0.231	−0.374	0.001	0.278	−0.010
	[0.49]	[0.61]	[0.08]	[0.46]	[0.43]	[0.99]	[0.25]	[0.99]
ΔGDP × coverage	0.761	−2.232	−2.930	−1.790	3.554	−3.076	−0.987	−3.040
	[0.94]	[0.35]	[0.39]	[0.10]	[0.38]	[0.06]	[0.20]	[0.04]
				SCAD				
Coverage	0.706	0.233	0.354	−0.063	−0.600	0.073	−0.068	−0.241
	[0.36]	[0.49]	[0.31]	[0.60]	[0.18]	[0.77]	[0.56]	[0.18]
ΔGDP × coverage	−0.103	−1.464	0.485	−1.923	0.973	−2.684	−1.451	−1.793
	[0.97]	[0.04]	[0.86]	[0.02]	[0.58]	[0.08]	[0.06]	[0.08]
Observations	47,939	101,977	52,995	97,888	117,651	33,172	131,100	19,683

[a]The table reports 2SLS estimates of equation (1) across different subsamples. The upper panel refers to GDELT, the middle panel to ACLED, and the bottom panel to SCAD. All regressions include cell and country × year fixed effects, plus the entire set of cell-level controls described in the footnotes to Table I. Columns 1 and 2 report separate regressions for democratic and autocratic regimes, based on the Polity Index (http://www.systemicpeace.org/polity/polity4.htm). Democracy is defined for Polity scores greater or equal to 5. Columns 3 and 4 report separate regressions based on media freedom. Countries with captured media are those with a value on the Reporters Without Borders World Press Freedom Index (https://index.rsf.org) below the worldwide median. Columns 5 and 6 report separate regressions by population density. Low-density cells are those with population below the sample mean. Columns 7 and 8 report separate regressions based on cells that did and did not experience at least one episode of violence between 1989 and 1997 based on the UCDP Georeferenced Event Dataset (https://ucdp.uu.se/). All regressions are weighted by cell population. The p-values for wild cluster bootstrap standard errors at the level of country are reported below each coefficient. See also the notes to Table I.

citizens in high conflict regions may be more responsive to downturns, either due to latent grievances or because they do not trust the government's handling of the economy. Consistent with this conjecture, we typically find larger effects in cells with a past legacy of conflict relative to peaceful areas (columns 7 and 8).[27]

In Table A.VIII of the Supplemental Material we also investigate the possibility that individuals respond to local as opposed to national economic conditions. We report 2SLS regressions where, similar to Section 6.2, we measure economic growth based on the per capita growth rate in night light intensity, which allow us to derive consistent measures of both national and local economic growth. In column 1 we report estimates of regression (1), where we measure economic growth by the national growth rate in night light intensity. Similar to Table I, we find no effect of mobile phones at zero economic growth and a negative gradient over the economic cycle. This further reinforces the main finding of

[27]Values of the Polity2 score less than 5 identify autocracies and anocracies; values greater than or equal to 5 identify established democracies. Media are considered captured if their score falls below the world median in the Reporters Without Borders World Press Freedom Index. Highly densely populated cells are those with a population above the mean across cells. Areas with a legacy of conflict are those that experienced at least one episode of violence between 1989 and 1997 according to the UCDP Georeferenced Event Dataset.

the paper. In column 2 we interact coverage with local as opposed to national economic growth. We use growth rate at the level of first-order administrative divisions (rather than of cells) as a measure of local economic conditions. In these regressions as well as in all other regressions in the table, we also include the interaction between first-order administrative division $\times$ year effects. Results suggest that regional economic shocks also matter for the effect of mobile phones on protests. Point estimates on the interaction term are negative, although small in magnitude compared to national shocks and not statistically significant at conventional levels other than for SCAD. Once we include both interaction terms in column 3, the coefficient on the interaction between regional shocks and coverage becomes virtually zero, while the coefficient on the interaction between coverage and national economic shocks remains practically unchanged, although, compared to column 1, point estimates are somewhat less precise and below conventional significance levels for GDELT and SCAD. One interpretation of these findings is that mobile phones make citizens informed about reasons for grievance that they would not otherwise be aware of. This seems likely when shocks hit other areas and less likely when shocks are local. This conclusion is, however, tempered by the fact that measurement error in night lights may be considerable and possibly more so for local relative to national measures, which could in turn lead to attenuated estimates of the parameter on the interaction between coverage and local economic shocks.

6.4. *Individual Participation in Protests: Channels of Impact*

In this section we finally turn to individual data from the Afrobarometer survey to further investigate the effect of coverage and its interaction with GDP growth on participation in protests. Microdata from the Afrobarometer have two major advantages relative to the data on protest occurrence used in the previous sections. First, by including information on self-reported individual participation in protests, they allow validation of results from GDELT, ACLED, and SCAD, and, in particular, to further rule out that these results are driven by systematic reporting in the news. Second, by providing information on both protest activity and mobile phone use, they allow shedding some light on potential mechanisms of impact.

Note that the results in this section should be taken with caution, as we ignore the potential nonrandom allocation of coverage across areas. The reason for this is that data from the Afrobarometer only span a limited number of cells/years over which trends in lightning strikes have relatively little power in predicting variations in coverage.

As preliminary evidence, the top panel of Table IV reports regressions of a number of dependent variables that reflect individuals' knowledge and perception of economic and political conditions on individual mobile phone use and its interaction with GDP growth. Regressions in this and the following table include all controls as in Table I, as well as individual-level covariates available in the Afrobarometer, and are weighted by sampling weights.[28] Again, standard errors are wild cluster bootstrapped at the country level. The dependent variable in column 1 is a dummy for the respondent's self-reported economic status, as proxied by non-employment. Dependent variables in columns 2 and 3 are, respectively, dummies if the respondent's self-reported perceptions of his own and the country's economic conditions are much worse compared to 12 months before. The dependent variable in column 4 is a dummy if the respondent reports not trusting the

[28]The individual controls are age and age squared, a gender dummy, educational dummies, a dummy for urban residence, dummies for religion, and number of adults in the household.

TABLE IV

MOBILE PHONE USE, ECONOMIC CONDITIONS, AND POLITICAL OPINIONS: INDIVIDUAL LEVEL REGRESSIONS[a]

	Worse Economic Conditions				
	Individual		Country	Opinions of President	
	Actual (1)	Perceived (2)	Perceived (3)	Distrust (4)	Disapprove (5)
			Mobile phones only		
Mobile	−0.001	0.007	0.037	0.047	0.033
	[0.95]	[0.47]	[0.01]	[0.01]	[0.07]
ΔGDP × mobile	0.137	−0.150	−0.509	−0.777	−0.524
	[0.52]	[0.28]	[0.01]	[0.01]	[0.05]
Observations	75,680	75,680	75,680	72,643	72,781
			All media types		
Mobile	0.016	0.009	0.035	0.043	0.032
	[0.38]	[0.42]	[0.01]	[0.01]	[0.07]
ΔGDP × mobile	0.015	−0.155	−0.452	−0.715	−0.502
	[0.94]	[0.35]	[0.01]	[0.01]	[0.04]
Observations	75,670	75,670	75,670	72,633	72,771
			All media types + socioeconomic char		
Mobile	0.032	0.012	0.027	0.034	0.025
	[0.16]	[0.42]	[0.05]	[0.04]	[0.04]
ΔGDP × mobile	−0.272	−0.203	−0.314	−0.555	−0.385
	[0.34]	[0.39]	[0.21]	[0.01]	[0.01]
Observations	75,669	75,669	75,669	72,632	72,770

[a]The table reports estimated coefficients based on individual-level OLS regressions using data from Afrobarometer, rounds 3 to 5. Mobile is a dummy for mobile phone use. See Appendix B.3 of the Supplemental Material for the method used to construct this variable. The dependent variable is a dummy equal to 1 if the respondent is unemployed (column 1), thinks his own economic conditions are much worse compared to 12 months before (column 2), thinks the country's economic conditions are much worse compared to 12 months before (column 3), does not trust the president at all (column 4), and strongly disapproves of the president's performance (column 5). All regressions include cell and country × year fixed effects, the entire set of cell-level controls described in footnotes to Table I, plus the individual controls age and its square, gender, rural/urban status, education level, dummies for religion, and number of adults in the household. The middle panel additionally includes dummies for ownership of radio and TV and use of Internet and the corresponding interactions with ΔGDP; the lower panel further includes all the interactions of the individual socioeconomic characteristics with ΔGDP. All regressions are weighted by sampling weights. The p-values for wild cluster bootstrap standard errors at the level of country are reported below each coefficient. See also notes to Table I.

country's president at all, while the dependent variable in column 5 is a dummy if the individual strongly disapproves of the actions of the president.

Several findings emerge. First, there is no evidence that individuals with mobile phones are more vulnerable to economic conditions than those without (columns 1 and 2). However, when the economy deteriorates, and compared to those with no mobile phones, these individuals are more likely to report that the economy is doing much worse than before (column 3) and to distrust and disapprove of the president (columns 4 and 5). It is, however, possible that these results are not due to mobile phone usage but to its correlation with the availability of other media and, more generally, with socioeconomic status. For this reason, in the middle panel of the table we control additionally for individual ownership of TV and radio as well as for Internet use, including both main effects as well as their interactions with GDP growth, while in the bottom panel we further include the interaction of all individual socioeconomic characteristics with GDP growth. Point estimates are largely unaffected by the inclusion of additional controls. However, in the most saturated specification, the estimated effect on the interaction term in column 3, where the perceived state of the economy is used as the dependent variable, is no longer significant at conventional levels (p-value 0.2).

Taken together, the results in Table IV suggest that mobile phones make individuals more critical of government performance when the economy deteriorates. On the other hand, these individuals do not appear to be themselves more vulnerable to economic conditions. There is also suggestive evidence that mobile phones make individuals more informed about the true state of the economy, although part of this effect seems to be ascribed to the nonrandom allocation of mobile phones across individuals.

With this preliminary evidence at hand, we now turn to the effect of mobile phones on individual protest participation. Table V reports 2SLS estimates of equation (4), that is, a regression of a dummy for individual protest participation on a dummy for mobile phone use, the fraction of individuals in a cell protesting, and the interaction of a dummy for mobile phone use with this latter variable and with GDP growth. As discussed in Section 5.2, both the fraction of individuals protesting as well as its interaction with a dummy for mobile phone use are potentially endogenous. We instrument both variables with the fraction of people using mobile phones in the cell and its interaction with GDP growth, as well as with further interactions of these two variables with a dummy for mobile phone use. The first-stage estimates, reported in Table A.IX of the Supplemental Material, are interesting in their own right. They suggest that a 1 s.d. fall in GDP growth is associated with an increase of around 2.8 p.p. in the protest participation differential between areas with full and zero mobile phone usage (-0.710×0.04). At a baseline protest participation of around 11 p.p., this is equivalent to an increase of around 25 percent. Importantly, this result, which is based on self-reported protest participation, is in line with those from

TABLE V

MOBILE PHONES AND PROTESTS: INDIVIDUAL LEVEL REGRESSIONS[a]

	Indiv. Participation (0/1)		
	(1)	(2)	(3)
Mobile	−0.022	−0.016	−0.020
	[0.35]	[0.42]	[0.26]
ΔGDP × mobile	−0.420	−0.360	−0.216
	[0.05]	[0.06]	[0.10]
% Participating	0.564	0.512	0.545
	[0.15]	[0.04]	[0.04]
% Participating × mobile	0.386	0.314	0.281
	[0.12]	[0.15]	[0.19]
Media interactions	No	Yes	Yes
Socioeconomic interactions	No	No	Yes
Observations	73,781	73,622	73,622

[a]The table reports 2SLS estimates of equation (4) using Afrobarometer data. The regression in column 1 includes cell and country × year fixed effects, the entire set of cell-level controls described in the footnotes to Table I, plus the individual controls age and its square, gender, rural/urban status, education level, dummies for religion, and number of adults in the household. Column 2 includes additionally dummies for ownership of radio, TV and use of Internet and the corresponding interactions with ΔGDP. Column 3 further interacts all socioeconomic characteristics with ΔGDP. First-stage estimates are reported in Table A.IX of the Supplemental Material. All regressions are weighted by sampling weights. The p-values for wild cluster bootstrap standard errors at the level of country are reported below each coefficient. See also the notes to Table I.

GDELT, ACLED, and SCAD, further indicating that systematic news reporting is not explaining our aggregate estimates in Section 6.1.[29]

Turning to the 2SLS estimates in column 1 of Table V, there are several important findings that emerge. First, conditional on all other controls, individuals with mobile phones are effectively as likely to protest as those without mobile phones ($\gamma_1 = 0$). Second, individuals with mobile phones are more likely to respond to changes in economic conditions than those without mobile phones ($\gamma_2 < 0$). At given average protest participation, a 1 s.d. fall in GDP growth leads to a differential increase in protest participation among those with mobile phones compared to those without of around 1.7 p.p. (-0.420×0.04). Third, there is evidence of positive spillovers in the occurrence of protests ($\gamma_3 > 0$). We estimate that a 10 p.p. increase in average protest participation in society leads to an increase in protest participation among those without mobile phones of around 5.6 p.p. Finally, there is some evidence that those with mobile phones are more responsive to an increase in others' protest participation than those without mobile phones ($\gamma_4 > 0$), with a differential effect of around 3.9 p.p., although the p-value associated to this coefficient is marginally above conventional levels (0.12). This seems to suggest that mobile phones are complementary to others' participation in the decision to join a protest.

As additional robustness checks, in column 2 of Table V we control for individual ownership of TV and radio, Internet use, and the corresponding interactions with GDP growth, while in column 3 we further include the interaction of all individual socioeconomic characteristics with GDP growth. In both cases, results remain essentially unchanged.

Taken together, and with the caveats highlighted above, Tables IV and V provide suggestive evidence in favor of the hypothesis that mobile phones affect protest participation via both an information and a coordination effect. Back-of-the-envelope estimates suggest that around half of the effect is due to increased coordination and about half to increased information (see Appendix A.1.6 of the Supplemental Material for further details on the calculations).

7. CONCLUSIONS

In this paper we provide novel systematic evidence on the impact of mobile phone technology on mass political mobilization. Using detailed georeferenced data for Africa from different sources on protest incidence and self-reported protest participation, we find strong and robust evidence in support of a nuanced and qualified version of the liberation technology argument. Mobile phones are indeed instrumental to political mobilization, but this occurs in periods of economic downturn when reasons for grievance emerge or the opportunity cost of protest participation falls.

While over the period of analysis the African continent experienced on average robust growth, several countries suffered outright recessions: 60 percent of the countries experienced at least 1 year of negative income growth, and 25 percent experienced at least 3 years. Our results imply that, in the face of these adverse shocks, absent mobile phones, one would not have seen the emergence and scale of protests that did in fact occur.

Using a combination of theory and data we attempt to shed light on the behavioral channels behind this empirical result. We argue that mobile phones foster political par-

[29] In Section B.3 of the online Appendix we show that OLS estimates of model (1) based on GDELT, ACLED and SCAD data on the restricted sample of Afrobarometer cells/years are qualitatively similar to the ones obtained based on the entire sample of cells/years.

ticipation during economic downturns in two ways. They appear to both make individuals more informed about the state of the economy and make people more responsive to changes in others' participation, which is key in determining the equilibrium level of protests via strategic complementarities.

Our results refer to a period when the only available technology was effectively 2G. Increasing access to 3G and 4G technologies and the associated advent of social media, both of which seem to further facilitate coordination among citizens, suggests that the potential for digital ICT to foster mass political movements might persist beyond the period of analysis. Our findings indicate that mobile phones are particularly effective in fostering mobilization in autocratic regimes and where traditional media are captured, suggesting that this technology may play a key role in advancing political freedom in the long term. However, the results mainly refer to poor countries with low state capacity and may not apply to advanced, tech-savvy autocracies that are found in other regions of the world. The related issue of whether digital ICT will continue to sustain rising levels of direct participation and even possibly promote democratic progress or whether ultimately governments will appropriate this technology for their own ends remains a first order question for future research.

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