Can You Hear Me Now?: How Communication Technology Affects Protest and Repression*

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Abstract

Commentators covering recent social movements, such as the Arab Spring, have claimed that cell phones and social media enable collective action. We develop a theoretical model to illustrate why, focusing on two mechanisms: first, by enabling communication among would-be protesters, cell phones lower the costs of coordination; second, these technologies broadcast information about whether a protest is repressed. Knowing that a large audience will now witness, and may be enraged by repression, governments refrain from squashing demonstrations, lowering the cost of protesting. We evaluate the model's predictions using high-resolution global data on the expansion of cell phone coverage and the incidence of protest from 2007-2014. Our difference-in-differences estimates indicate that cell phone coverage increases the probability of protest by over half the mean. Consistent with our second mechanism, we also find that gaining coverage has a larger effect when it connects a locality to a large proportion of other citizens.

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1. Introduction

Media coverage of recent social movements—the Arab Spring, the Green Movement in Iran, and the Occupy movements in the U.S. and Turkey—frequently claim that new communication technologies facilitate protests. Headlines proclaim that cell phones and social media "fuel protests in Iran, Bahrain, and Yemen" (ABC News, 2011), "give Wall Street Protests a Global Reach" (Preston, 2011), and are "key to [Turkey's] 'Occupy Gezi' protests" (Dorsey, 2013). What is missing from the public and scholarly debate is an explanation for *why* these technologies affect collective action, and evidence that they have a *causal effect* on the incidence of protest. This paper helps fill both of those gaps.

Many have focused on the role that specific platforms (e.g., Twitter) play in organizing protests (Enikolopov et al., 2015; Fowler et al., 2014). Our first contribution is to take a step back and develop a more general framework for thinking about why communication technologies—such as cell phones—affect the interaction between protesters, their government, and the mass public. We formally model two mechanisms through which these technologies may affect protest: first, they lower the costs of coordination; and second, they increase the visibility of government repression should it occur.

First, cell phones enable would-be protesters to communicate, allowing them to share information about, for example, when and where a protest will occur (Little, 2014). This facilitates the creation of (almost) common-knowledge about their intentions, which helps would-be demonstrators overcome the coordination problem inherent in protest.

A second, complementary mechanism highlights the role of cell phones in broadcasting information about government repression. Where a large proportion of citizens have access to cell phones, the government knows that the mass public will witness, and may be enraged by, repression. Fearing that repression could spark escalation, government may soften its response. As the expected level of repression falls, protests become less costly and, thus, even more likely. In the recent pro-democracy protests in Hong Kong, police were caught on video beating an activist. Leung (2014) writes that "For the neutrals, this episode could well be the tipping point…[A]fter such a brutal beating—which we know happens all the time behind closed doors …but just never in public—it's become harder for many to just sit on the fence. Indeed, more people are back out on the streets …and angrier than ever." By documenting and widely disseminating evidence of police brutality, protesters translated repression into additional support.

The second contribution of this paper is empirical. We leverage high resolution geo-spatial data on the expansion of cell phone networks and protest activity around the globe from 2007 to 2014 to evaluate the empirical relationship between cell phone access and the occurrence of protest. We find that gaining coverage increases the probability of protest—an effect that is roughly half the baseline probability. Furthermore, we find evidence consistent with our theoretical model: the effect of gaining access on protest is largest where joining the network connects a locality to a large share of their fellow citizens; we also find more direct evidence that cell phone access reduces the use of repression. Both pieces of evidence suggest that cell phones not only enable protesters to coordinate, but also temper the government's response by raising the visibility of repression.

To bolster our findings, we perform placebo tests to ensure that differential trends prior to the extension of coverage do not explain our findings, and directly show that pre-coverage trends in protest are in fact parallel. Second, we show that our main result holds across different event datasets that employ different methods to code and geo-locate protests. Finally, we find no evidence of reporting bias in areas receiving cell phone coverage: the number of sources or articles covering protest events does not increase with our treatment. Moreover, a bounding exercise suggests that the reporting bias would have to be large to explain away our main effects.

By employing expansive data and a difference-in-differences design, our approach overcomes limitations of past empirical work. Several past studies focus on already extant social movements where ICT is suspected to have catalyzed protests (e.g., Howard et al., 2011; Khamis and Vaughn, 2011; Caren and Gaby, 2012). While these studies are rich in detail, by selecting on the dependent variable, they can not rule out the possibility that these technologies have no effect—that there are contexts with comparable cell phone penetration that have seen no change in protest activity. Other studies rely heavily on cross-sectional data, comparing protest activity in areas with and without coverage (e.g., Pierskalla and Hollenbach, 2013, present primary results that are based on cross-sectional data). Such studies struggle to account for differences between the localities that do and do not receive coverage that may also affect the incidence of protest, such as, distance from the capital, ethnic composition, or economic activity.

The remainder of this paper proceeds as follows. We review past work on the determinants of protest. These prior studies motivate the model we present in section 3. We then present hypotheses that translate the comparative statics of the model into specific empirical predictions. We outline our empirical strategy, data, and present results before concluding.

2. Extant Work on the Coordination and Containment of Protest

Organizing a protest requires overcoming formidable challenges. Protesting imposes private costs on participants: they have to gather information about the event, take time away from work, and risk being repressed. Even if an individual cares deeply about a cause, he or she may only be willing to bear these costs if they are confident that others will join them. Why are individuals' payoffs to protesting dependent on what others do? As the protest increases in size, its probability of success grows, and each individual demonstrator's likelihood of being targeted for repression declines (Kuran, 1991, p. 18). Thus, the returns to protesting are increasing in the number of other individuals that choose to participate. This type of strategic problem is commonly referred to as a coordination problem (Chwe, 2001, p. 12).[1]

How do individuals solve these coordination problems? Consider the problem from the perspective of a single individual. A potential protester p wants her compatriots to know that she is planning to protest at a specific place and time. Knowing this, they may also want to participate, as their returns to protesting are higher if p turns out. But before p wants to follow through with her stated plan, she needs to know that her compatriots have heard her, and, furthermore, they need to know that she knows that they have heard her plans, and so on. That is, p's protest plan needs to be common knowledge (Aumann, 1976). Several scholars have clarified the important role that public rituals and organized religion can play in the development of common knowledge (Chwe, 2001; Patel, 2007). We focus here on the role of communication technologies, such as cell phones, in generating common knowledge or "almost" common knowledge.[2,3]

First, in order for p to transmit her plan to protest, she needs to be able to communicate with her compatriots. Better still, they should be able to communicate back and confirm that they heard

[1] This is distinct from a free-rider problem. Although some past work asserts that cell phones help groups detect and discipline free-riders, we focus on coordination problems, because of the many case studies illustrating how cell phones help individuals communicate about their plans to protest (see Kelly Garrett, 2006, for a review).

[2] Fowler et al. (2014, p. 5) observe that true common knowledge (with all of the implied higher order beliefs) rarely, if ever, exists in reality. They focus instead on what they call "almost common knowledge," a concept developed in Rubinstein (1989).

[3] Cell phones are not the only technology that can serve this function: centralized mass media, such as radio and television, can aid in coordination; however, these outlets are also more easily captured by the state (e.g., Kern and Hainmueller, 2009; Warren, 2015; Yanagizawa-Drott, 2014).

her message. The ability to (reliably) transmit messages is then a necessary, if not sufficient, condition for generating common knowledge about would-be protesters' intentions. (If protesters prefer to share their intentions shortly before protesting to avoid preemptive arrests, then it also helps if they can communicate quickly.) Second, social media, which is increasingly accessed through mobile phones, provides a platform for users to share information about protests and know that others have seen their posts (e.g., the "Like" button on Facebook).[4] Tufekci and Wilson (2012, p. 369) report that, in their sample of Egyptian protesters, just over 80% used their phones to communicate about the protests, roughly 50% used Facebook, and another 13% used Twitter. And this use of social media appears to have increased protest activity: Fowler et al. (2014) find, for example, that protests are more likely after popular Twitter users publicize information about grievances or protest logistics. In Russia, Enikolopov et al. (2015) find that social network penetration leads to an increased probability of protest, as well as to larger demonstrations. Recent work also suggests that governments are concerned about how social media can enable collective action. King et al. (2013) present evidence that Chinese censors do not worry about critical comments, but focus their attention on posts that could lead to social mobilization, revealing the government's concern about the role that social media can play in catalyzing protests or other forms of collective dissent.

While most work on this topic argues that cell phones help groups generate common knowledge and, thus, coordinate protests, a smaller number of studies suggests that these platforms can actually reduce certain forms of collective action. In their study of insurgent violence in Iraq, Shapiro and Weidmann (2015) find that better cell phone coverage leads to a reduction in attacks at the district level. They argue that the most consequential effect of cell phones in Iraq is to enable more effective surveillance of rebel activity—an insight that is then formalized in Shapiro and Siegel (2015).[5] Closer to our own focus on protest, Hassanpour (2012, p. 4) argues that cell phones and social media might "discourage face-to-face communication and mass presence in the streets … [and] create greater awareness of risks involved in protests, which in turn can discourage people from taking part in demonstrations." He shows that a sudden country-wide disruption of communications networks

[4]As of the second quarter of 2014, Facebook announced that 60% of its ad revenues were generated through mobile, and 30% of its users only access the service through their phones (Hamburger, 2014).

[5]While insurgency and protest are both forms of political violence, they impose different costs on citizens. In the model developed in Shapiro and Siegel (2015, 316), the community wants to aid in surveillance if this shields them from intense insurgent violence. However, unlike insurgency, protests do not often result in violence against non-participants. Thus, citizens do not have the same strong incentive to actively collaborate with the government against potential protesters.

in Egypt led to increased dispersion of protests in Cairo. Given these findings, it remains an open empirical question whether access to cell phone technology increases the probability of protest.

Faced with a protest, how will the government (or its agents) respond and, in particular, when will they employ repression? In earlier work (from the 1950s to 1970s), repression was not regarded as a choice, but rather as a characteristic of certain types of regimes. Davenport (2007, p. 4) notes that repression was seen as a "pathology . . . that political leaders were simply compelled to take because of some system deficiency." More recent theoretical work treats governments as rational decision-makers, weighing the benefits and risks associated with repression.

This more recent work seems to agree on why regimes may want to employ repression. First and foremost, repression imposes a cost on its targets and can, thus, deter or demobilize dissidents. This argument appears in some form in nearly every model: repression is either assumed to be effective in generating short-term reductions in dissent (Balbus, 1973; Lichbach, 1984), or it imposes an additional cost on protesters, discouraging demonstrations (Opp and Roehl, 1990; Pierskalla, 2010; Magaloni, Kricheli, and Livne, Magaloni et al.). A second common argument contends that repression serves as a signal of either the government's resolve or strength. Walter (2006), for example, argues that states wage costly wars against separatist movements to develop a reputation for toughness and discourage future challengers. Pierskalla (2010) instead focuses on what the decision to repress signals about the government's strength (rather than their willingness to fight): in his game of incomplete information, governments opt for repression, because they worry that challengers will view the decision to accommodate protesters as a sign of weakness (see proposition 8). From the government's perspective, repression can demobilize protesters and, by some accounts, signal its willingness or ability to fend off future challengers.

Given these upsides, why do governments ever exercise restraint? Repression may simply be costly: protest policing requires equipment and personnel, and governments have finite budgets. Other scholars, particularly in international relations, argue that governments pay costs for violating international laws and norms against human rights abuses (Hafner-Burton, 2005; Hendrix and Wong, 2012). However, the most widespread explanation for restraint does not focus on these costs, but rather on the possibility that repression will actually inflame dissent and, thus, fail to serve its intended purpose. Goldstone and Tilly (2009, p. 181) summarize a number of case studies, which find evidence that repression backfired:

> "Khawaja's (1993) study of Palestinian protest in the West Bank, Rasler's (1996) study of Iranian protests in 1977-79, Francisco's (1996) study of protest in Germany, and Olivier's

(1990) study of Black protest in South Africa all find, as the latter clearly states, that 'the effect of repression on the rate [of collective action] is not negative! Repression led to a significant increase in the rate of collective action."[6]

Scholars have rationalized this finding by arguing that repression can push other, previously docile citizens to openly oppose the government. Opp and Roehl (1990, p. 524) summarize several reasons why repression might engender a backlash. First, "repression may thus be regarded as immoral, and individuals who are exposed to repression or who know about it may feel a moral obligation to support a movement's cause and even to regard violence as justified." Second, "repression may cause system alienation, i.e., discontent with a society's political institutions, which will in turn lead to more protest if persons believe they can change these conditions by means of protest." This work suggests that repression is a double-edged sword: it both discourages dissent and further justifies political opposition.

Less work has been done to enumerate the conditions under which repression extinguishes or exacerbates protest. Siegel (2011) provides a notable exception. The findings from his computational model help explain when repression will backfire and, of particularly importance for this paper, how that relates to communication. If the targets of repression do not have many ties that extend beyond their village, then outrage is unlikely to spread beyond the confines of their locality: "anger has little aggregate effect when network structure doesn't allow it to spread. However, once there is a sufficient number of weak ties, anger-driven participation can spread throughout the network rapidly enough to overwhelm repression and trigger a backlash" (p. 1005). By this logic, governments should worry more about generating a backlash when information about their use of repression can spread quickly and widely throughout the polity.

3. Model of Coordination, Repression, and Escalation

When deciding whether to stage a protest, individuals consider each others' decisions about participating, the costs of coordinating, and the risk of repression. Each potential protester cares about what others do, because there is strength and protection in numbers.

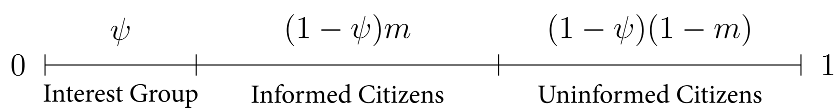
The government, unwilling or unable to immediately concede to the protesters' demands, can choose to repress, raising the costs of protesting. However, repression can also outrage citizens and

[6] Lawrence (2013) provides more recent evidence from Morocco that information about police brutality increased support for the movement's vanguard.

escalate protests. The government must then weigh the deterrent effect of repression against the risk of escalation.

How does communication technology affect the decision calculus of these players? We are not claiming that technology alone incites protests; demonstrators have political or economic motivations that we do not model. Rather, we argue that technology reduces the costs of collective action, where groups want to mobilize. To summarize our theoretical results, first, it allows protesters to coordinate, lowering their costs to demonstrating. Second, by linking citizens across a country and making any acts of repression visible to a larger audience, cell phones increase the risk of escalation and, thus, can cause the government to reconsider its use of repression. These two mechanisms are formalized below.[7]

We model a game between three sets of actors: (1) an interest group considering whether to protest, (2) the government, and (3) a mass of citizens. In a population of measure 1, let ψ belong to the interest group and $1-\psi$ represent other citizens. (We use p to refer to an interest group member and i to refer to a citizen.) Among these $1-\psi$ let $m \in [0,1]$ have access to information about whether a protest happens and any government response. All players know the distribution of the population (ψ and m) and each others' payoff functions.



The sequence of play is as follows:

(1) Before any protest is organized, the government (G) chooses whether to repress in the event of a demonstration ($r \in \{0,1\}$).[8] The government pays a direct (linear) cost for deploying repression ($R_G \in \mathbb{R}^1_+$). This choice is immediately observed by all members of the interest group (all p).

[7] We assume that cell phones allow citizens to learn accurate information about protests and repression. This may not be true of all types of communication technologies: traditional news media, for example, may be controlled by government and, thus, less likely broadcast evidence of repression. Such technologies may not then enable escalation.

[8] In our one-shot game, allowing the government move first allows it to credibly commit to repressing without complicating the model by introducing repeated play.

(2) Every interest group member (p) eventually makes two choices: (i) whether to protest, and (ii) what tactic to select.[9] However, before making these decisions, interest group members discuss the plans for a demonstration. Formally, each p receives a vector of S private signals ($\vec{s}_p$) about when or where the protest will take place if it occurs. While the distribution of the signals are common knowledge, each p's signals are private and not observed by the government, citizens, or other interest group members. All private signals are independent and identically distributed with each $s_k \sim \mathcal{N}(T, 1/\beta_s)$, where T is the actual tactic selected by the protest's organizers.[10] T is an exogenous parameter in this model; it represents the time or location for the protest chosen by the group's leadership.

Using these signals, each p updates their prior belief $T \sim \mathcal{N}(0, 1/\beta_0)$.[11] Each p's posterior belief about tactics is then

$$E[T|\vec{s}_p] = \mu_p \sim \mathcal{N}\left(\frac{\beta_s \sum_{p=1}^{S} s_p}{\beta_0 + S\beta_s}, \frac{1}{\beta_0 + S\beta_s}\right).$$

To save space, we define $\overline{\beta} = \beta_0 + S\beta_s$ as the precision of this posterior belief.

(3) With this new information in hand, each p then decides whether to protest ($d_p \in \{0, 1\}$) and also selects a tactic ($t \in \mathbb{R}^1$). These choices are observed by the government and *informed* citizens. Furthermore if any p protests and G represses, this repression is then observed by all informed citizens. Any p that protests pays a cost for selecting a tactic that differs from the organizers' plans (T). Furthermore, this cost is larger when the government has chosen to repress demonstrators; botched coordination is especially costly when p shows up at the wrong time and faces the police with few compatriots. Specifically, we assume the cost function $(k + rR_p)(t - T)^2$, where $k \in \mathbb{R}^1_+$ scales the cost of botched coordination even absent repression, $r \in \{0, 1\}$ is the government's choice of repression, and $R_p \sim U[0, 1]$ is each p's cost to being repressed. Should they succeed, each p benefits from the policy concession, receiving $c \in \mathbb{R}^1_+$.

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[9] We draw upon a recent global game by Little (2014), who presents a thoughtful and tractable approach for modeling protesters' coordination problem. This approach builds upon work by Morris and Shin (2002).

[10] Alternatively, we could allow for S rounds of communication, in which each interest group member receives a signal from another member of the group and then updates their posterior belief. This process generates an even more precise posterior distribution than i.i.d. signals for $S > 2$.

[11] The precision parameters β_0 and β_s are assumed to be known to all players.

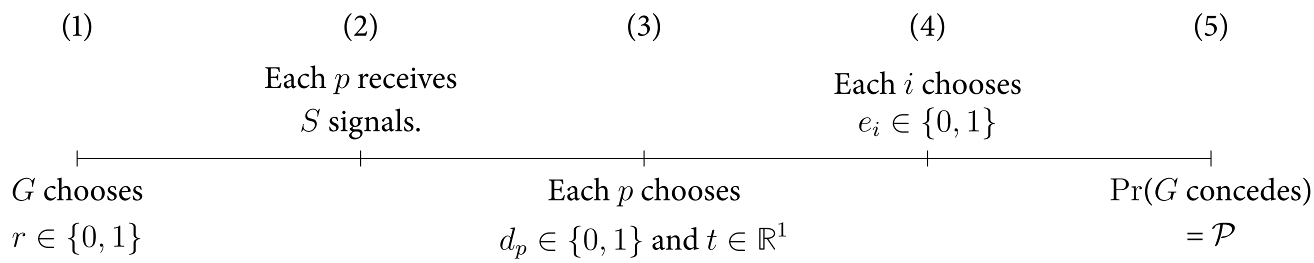
If no p protests, then the game ends with the government retaining the concession without incurring the cost of repression, all p getting nothing, and all citizens receiving their reservation value $q \in \mathbb{R}^1_+$.

(4) If a protest does occur, each citizen (i) decides whether to punish the government; i.e., join or support the protesters ($e_i \in \{0, 1\}$). Each i responds differently upon witnessing repression—some may be outraged, others cowed. If i is informed and observes repression, they receive $v_i \in \mathbb{R}^1$ for choosing $e_i = 1$ and their reservation value (q) for $e_i = 0$.[12]

(5) The game ends with a lottery in which the government concedes with a probability that increases in the measure of protesters and citizens that punish ($\mathcal{P} = \text{measure}[p \mid d_p = 1 \cup i \mid e_i = 1]$). For convenience, we assume that the probability of concession is simply equal to this measure $\mathcal{P}$.

If the government prevails, it keeps the concession c. However, if the protest succeeds, then the concession is granted to the interest group members. The rest of the citizens get rv_i if they punish and q if not.

The following figure summarizes the timing of the game:



We can now define each player's expected payoffs both in words and using the notation introduced above:

$$G\text{: } E[u_G(r)] = E(\text{Concession}) - \mathbb{1}(\text{Repress}) * \text{Cost of Repressing} = c(1 - \mathcal{P}) - rR_G$$

$$p\text{: } E[u_p(d, t)] = \mathbb{1}(\text{Protest}) * E(\text{Concession} - \text{Coordination Cost}) = d[c\mathcal{P} - (k + rR_p)E(t - T)^2]$$

$$i\text{: } E[u_i(e)] = \mathbb{1}(\text{Repress, Punish}) * \text{Outrage} + \mathbb{1}(\sim \text{Punish}) * \text{Res. Value} = e\, rv_i + (1 - e)q.$$

[12] This assumes that i does not directly value the concession. We can relax this assumption and allow c to enter i's utility, increasing the measure of citizens that escalate for any level of repression.

3.1 Equilibrium Characterization and Comparative Statics

We derive the equilibrium through backwards induction, starting with the citizens' decision to escalate, then the interest group members' decision to protest, and, finally, the government's initial choice of repression.

First, consider the decision of an informed citizen. Citizens react to what they see transpire in the streets. Did the government repress demonstrators, and is the citizen angered enough by this repression to want to take action? The case studies and survey evidence cited above suggest that witnessing repressive acts can mobilize some citizens to sympathize with protesters. A citizen will choose to punish the government if their outrage, upon observing repression, exceeds their payoff from remaining neutral. If no repression occurs, then nothing incites citizens, and no escalation occurs.[13]

Second, interest group members have to evaluate whether the expected value of the policy concession exceeds the costs of protesting.[14] Their expected benefits ($\mathcal{V}$) from protesting will depend on what proportion of their own group members protest ($\psi\overline{R}$) and what proportion of citizens (if any) choose to punish ($\mathcal{E}$). In short, the more people that demonstrate or punish, the better the chances that the government will be forced to concede.[15] Each potential protester's cost to demonstrating depends on their choice of tactic. As this choice is symmetric and does not depend on others' actions, we can immediately solve for each interest group member's optimal tactic: they simply choose their best guess about where or when the protest will happen based on the signals they received, i.e.,

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[13]One could, alternatively, allow citizens to experience outrage (i.e., positive v_i) even absent repression. A measure of informed citizens may then punish, regardless of the government's action. This amendment would allow for a direct effect of m on protest, in addition to the indirect effect that runs through citizens' reactions to repression.

[14]Readers familiar with global games will recognize that our model does not generate multiple equilibria in the complete information setting, a common characteristic of global games. However, a slight and reasonable change to p's utility function (namely, making some cost of repression unrelated to p's tactical decision) restores this multiplicity, leaves us grasping for an argument about equilibrium selection, and motivates our use of the global game.

[15]We define the expected value of the concession as $\mathcal{V}$, which is equal to $c\psi$ if no repression occurs and $c[\mathcal{E}+\psi\overline{R}]$ if the government intervenes, where $\mathcal{E}$ (defined below) represents the measure of citizens that punish after observing repression, and $\overline{R}$ identifies the interest group member that is indifferent between protesting and not. $\overline{R} = \arg_{R_p} \left\{c[\mathcal{E} + \psi R_p] = (k + rR_p)(t - T)^2\right\}$.

their posterior belief (proof in Appendix A.1). This optimal behavior yields the following expected utility to protesting for every protester:[16]

$$E[u_p(\mu_p)] = d[\mathcal{V} - (k + rR_p)/\overline{\beta}]$$

As is already apparent from this expression, the costs of coordination decrease as p receives more information about the logistics of protest (because $\overline{\beta}$ is increasing in the number of signals, S).

Finally, the government has to decide whether to repress. The government wants to repress only when the expected deterrent or demobilizing effects of repression outweigh the costs associated with alienating citizens. We define $\mathcal{E}$ as the increased probability that the government will be forced to concede if escalation occurs.[17]

The preceding paragraphs are summarized in the following proposition:

Proposition 1. ***(Equilibrium Characterization)*** *There exists a unique Perfect Bayesian Equilibrium. In it, the following properties hold:*

(i) Protests never occur if the expected value of the concession, absent any escalation by other citizens, does not exceed the cost of coordination ($\mathcal{V} < k/\overline{\beta}$).

(ii) However, if this first condition does not hold, the government faces the possibiilty of protest and represses if the deterrent value of repression exceeds the direct cost of repression, as well as the cost of any escalation ($\psi(1 - \overline{R}) \geq \mathcal{E} + R_G/c$).

(iii) An interest group member will protest if the expected value of the concession exceeds their costs of coordination and repression. If this is not true for any member of the interest group, then no protest occurs. (An interest group member p whose cost to being repressed is R_p will protest if $\mathcal{V} \geq (k + R_p)/\overline{\beta}$.)

(iv) A citizen punishes the government if he observes repression and his outrage exceeds his reservation payoff ($v_i \geq q$).

[16] The expectation simplifies because $E(\mu_p - T)^2 = 1/\overline{\beta}$. Conveniently, $E[(\mu_p - T)^2]$ is simply the variance of the posterior μ_p or $1/\overline{\beta}$.

[17] Let $\mathcal{E} = (1 - \psi)m(1 - F\{q\})$. This is simply the measure of informed citizens, whose outrage exceeds their reservation value (i.e., for whom $v_i > q$).

Proof: See Appendix A.2. □

We focus on two comparative statics. First, how does the equilibrium change if we allow interest group members to more intensely communicate? If we allow each member of the interest group to receive more signals (increasing S), this will diminish the possibility of mis-coordinating (e.g., showing up at the wrong place or time). When an interest group member is more confident that he or she will choose the correct tactic, their costs to protesting decline regardless of the government's choice of repression. This makes protest more likely.

Second, what if we expand the audience of informed citizens that observes the government's choice of repression (expand m)? Increasing the proportion of informed citizens amplifies the government's downside risk if it represses, making it less likely to intervene. As the expected level of repression falls, so too does the cost of protesting for interest group members.

These results are now collected in the following proposition:

Proposition 2. ***(Comparative Statics)*** *The unique PBE, characterized in Proposition 1 above, has the following comparative statics:*

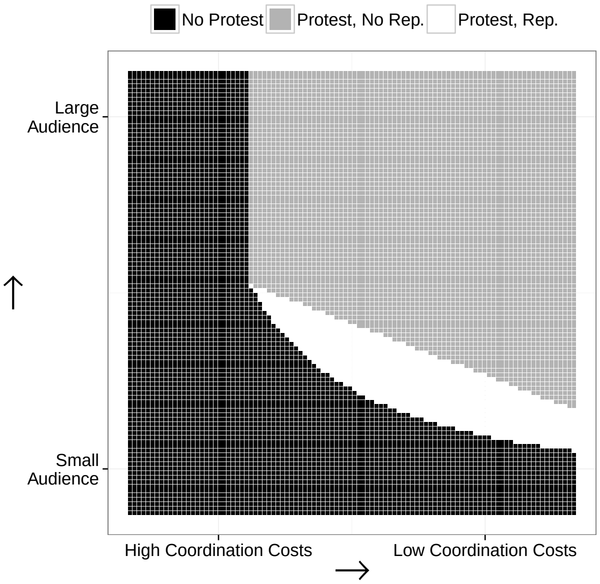
(i) Protest is more likely when interest group members are increasingly confident that they will select the correct tactic, and, thus, face lower expected costs to demonstrating. An interest group member's posterior belief concentrates around the truth as his or her intensity of communication increases (i.e., as their number of signals, S, increases).

(ii) If the expected value of the concession without escalation exceeds the cost of coordination ($\mathcal{V} \geq k/\overline{\beta}$), then repression is less likely as the audience of informed citizens (m) increases. This further reduces the costs of demonstrating and thus increases the likelihood of protest.

Proof: See Appendix A.3. □

A simple way to present these comparative statics is to map out the equilibrium reached for different costs to coordinating (which are a function of $\overline{\beta}$) and audience sizes (m), holding the other parameters fixed. As is apparent in figure 1, if coordination costs are too high protest is not possible. However, below this threshold, the likelihood of protest is increasing as coordination costs fall and the audience size increases.

Figure 1: Equilibrium as Coordination Costs, Audience Size Change
Lowering coordination costs and increasing audience size increases Pr(Protest).



We map the equilibrium reached at different values of $\overline{\beta}$ and m, the two parameters in our model that we relate to cell phone access. To create this figure, we set ψ = .05, c = 1.5, $F\{q\}$ = .9, R_G = 0.001, and k = .2.

4. Hypotheses

We focus on two predictions from our model: first, gaining cell phone access increases protest; and second, this effect should be largest where a large proportion of the population already accesses the network. We quickly review the intuition for these claims, which are stated more formally in proposition 2.

First, cell phones reduce the costs of coordination. Where potential protesters can quickly exchange information about where or when a demonstration will be staged, they reduce uncertainty about how to participate. This reduces the costs of turning out and, thus, increases the probability of protest (proposition 2(i)).

Second, where the cell network is extensive, gaining coverage connects a community to a large proportion of their fellow citizens. If a protest occurs in this newly covered community, information about any government response can now be widely broadcast. Following past work on how repression can inflame dissent, we argue that some citizens will sympathize with protesters and punish the government if they witness harsh repression. Anticipating this potential backlash, governments will exercise greater restraint in the newly covered community. This reduces protesters' expected costs of repression and, thus, further increases the probability of protest. Hence, the effect of gaining coverage on protest will be greater where a large proportion of citizens are connected to the cell phone network, i.e., where a bigger audience bears witness to any repression (proposition 2(*ii*)).

We take these two predictions to the data:

(H1) Gaining access to cell phone networks increases the probability of protest.

(H2) This effect is larger when a greater share of the population already has access to the cell phone network.

We also look for more direct evidence that the introduction of cell phones reduces the probability of repression. Our prediction is that cell phones should reduce the use of repression, though this is a more difficult hypothesis to empirically evaluate given sample selection concerns discussed below.

5. Empirical Strategy

5.1 Estimating the Effect on Protest

To evaluate the first hypothesis, we look for changes in the probability of protest after an area receives access to a cell phone network and compare these changes to trends in localities that remain outside of the network. Put more technically, we estimate the difference-in-differences between areas that receive coverage during our study period and those that do not, using the following specification:

$$y_{it} = \alpha_i + \beta_t + \gamma D_{it} + \delta X_{it} + \varepsilon_{it}, \tag{1}$$

where i indexes a locality, t indexes years, D_{it} is an indicator variable for whether a locality is covered in year t, and X_{it} is a matrix of time-varying covariates. α_i and β_t are locality and year-specific intercepts.[18] Our dependent variable, y_{it}, is an indicator for whether area i had a protest in year t. If gaining access to cell phone networks increases the probability of protest, then γ should be positive, indicating that the likelihood of protest increases by a larger magnitude after localities receive coverage relative to the change observed in uncovered areas.

[18] In addition to the standard difference-in-differences approach, we also estimate models that, in addition to locality intercepts, include country-year fixed effects. These models flexibly account for country-specific trends, in addition to the flexible global time trend already included in the sparser model.

Our second prediction is that gaining access to a cell phone network should have a larger effect on the probability of protest when the proportion of citizens already connected to the network (m) is large. In short, if an area is suddenly able to communicate with most of the country by virtue of its inclusion in the communication network, we expect that access to the network will have a larger impact on protest activity. To estimate this heterogeneous effect, we amend equation (1) slightly:

$$y_{it} = \alpha_i + \beta_t + \gamma D_{it} + \zeta m_{ct} + \eta D_{it} * m_{ct} + \delta X_{it} + \varepsilon_{it}, \tag{2}$$

where m_{ct} is the proportion of people in i's country c that are covered in time t. The second hypothesis suggests that coefficient η should be positive—the effect of coverage should be more pronounced if it connects to a higher proportion of citizens. In estimating all of these models, we cluster our standard errors at the locality level unless otherwise noted.

Our empirical strategy does not rely on the random assignment of cell phone coverage. We do, however, have to make milder assumptions to obtain estimates of γ and η that are consistent for the average treatment effect on the treated of cell phone coverage.[19] To recover the causal effect of cell phone coverage, we need (1) the areas that do and do not receive treatment to follow parallel trends in the absence of treatment, (2) that cell phone coverage affects all places in a similar way, and (3) that coverage expansion into one area does not affect protest or repression in other areas. We do not find evidence of differential pre-trends, lending credibility to the first assumption. Furthermore, we address concerns about non-constant treatment effects and violations of SUTVA through the specific functional form in equation 2. This specification allows for both the heterogeneous treatment effects and the specific form of spillover suggested by our formal model.

5.2 Estimating the Effect on Repression

Finally, if cell phones expand the number of citizens that witness repression and, thus, discourage authorities from clashing with demonstrators, then the frequency of repression should decline as areas transition into cell phone coverage. We estimate:

$$r_{it} = \alpha_i + \beta_t + \tau D_{it} + \delta X_{it} + \varepsilon_{it}, \tag{3}$$

where r_{it} is an indicator for repression in locality i in year t. Even granting the standard difference-in-differences assumptions above, estimating the effect of coverage on repression remains challenging. This is the case because *repression is only observed when a protest actually takes place and not*

[19] Specifically, we require that $E(\varepsilon_{it}|D_{it}, \alpha_i, \beta_t) = 0$, the presence of constant treatment effects, and the stable unit treatment value assumption (SUTVA).

when a protest that would have been repressed never materializes (i.e., when repression effectively deters protest). If we could somehow observe every instance where repression would have been employed whether or not a protest took place, we expect that $\tau < 0$.

Fortunately, our theoretical model allows us to make empirical progress. Assuming our model is correct, we show in appendix B that our estimate of τ will *understate* the true reduction in repression if we exclude localities where the costs of staging a protest are prohibitively high. To remove such places, we drop localities that never experience a protest between 2000 and 2012 (or their first year of treatment, whichever comes first). Estimating equation 3 using the resulting sample, we feel more confident about interpreting our estimate of τ as an underestimate of the negative effect of coverage on repression.

6. Data

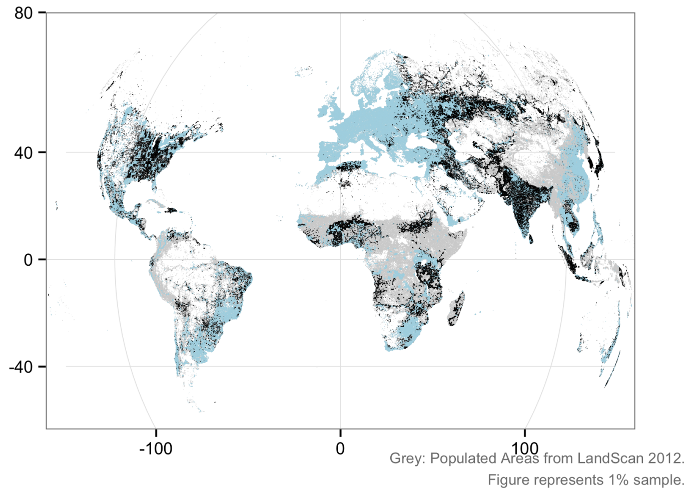
6.1 Cell Phone Coverage

To measure cell phone coverage over time, we rely on the Collins Mobile Coverage Explorer database, which is based on submissions made by telecom operators around the world. The data has a nominal resolution of approximately 1km on the ground, and is available yearly for the period 2007-2014, except for 2010.[20] Pierskalla and Hollenbach (2013) employ data from the same source, albeit for a shorter time span and only for African countries.

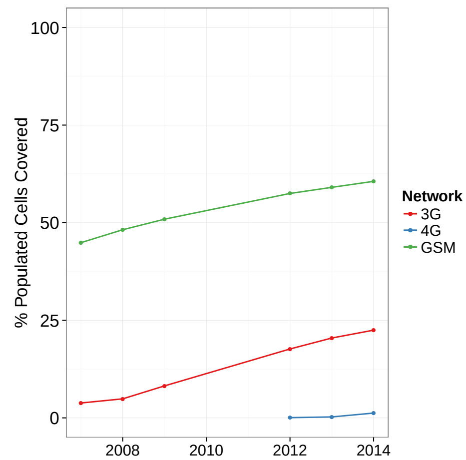
As figure 2 shows, cell phone coverage increased substantially during the 2007-2014 period, though larger urban areas and developed countries already had (near) complete coverage prior to 2007. In the empirical analysis, we leverage variation from the areas that undergo a change in their coverage status during the period of study (marked in black) and exclude areas that are covered

[20] Our maps indicate coverage areas in quarter 1 (Q1) of 2007, Q1 2008, Q1 2009, Q4 2011, Q4 2012, Q4 2013. We use the 2007, 2008, and 2009 maps to code treatment in those years. However, for the 2011, 2012, and 2013 data, we use these maps to code treatment in the following year. That is, if an area has coverage in the last quarter of 2011, we code it as treated from 2012 forward. This decision avoids coding areas as treated before they actually transition into coverage. However, it comes at the cost of coding some of our areas as control when they had access to the cell network for part of the year. If cell phones do induce increased protest activity, this coding decision should make it harder to find such an effect.

Figure 2: Expansion of Cell Phone Networks, 2007-14
Cell phone networks expanded, esp. in low- and middle-income countries.



(a) Expansion of All Networks, 2007-14.



(b) Proportion of Populated Cells

These figures are based on the Collins Mobile Coverage Explorer database. We restrict attention to those areas that, according to data from Landscan, are populated. In the figure on the left, **light blue** indicates areas that are covered throughout the study period, **black** represents areas that receive coverage between 2007 and 2014, and **grey** areas remain uncovered as of 2014. This map is based on a 1% sample from the Collins Mobile Coverage Explorer database.

throughout the entire study period.[21] In appendix D.1, we perform a validation check by comparing the proportion of the population covered in every country-year according to the Collins Mobile Coverage Explorer database with data on cell phones per capita from Banks and Wilson (2014). Reassuringly, these variables are correlated at 0.62, indicating a strong positive association between access to and uptake of mobile technology.

Our geographic unit is the 6 km^2 grid cell (at the equator). We discuss this aggregation decision below, which is motivated by our recognition that protest events are often geo-coded using

[21]The cell phone coverage data includes information for GSM (2G), 3G and 4G mobile standards. Some countries—notably the US—phased into GSM from a different standard (CDMA/IS-95) at the beginning of the period of study. For these areas, we could incorrectly assign a transition into cell phone coverage, when in fact the data simply reflects a change in standards (e.g., from CDMA to GSM). This problem affects very few countries. In Africa, for example, GSM accounted for 90% of market share by 1999 (Selian, 2001). Given that our results hold in a sample of African countries and when we exclude 2007 (the year of greatest concern), we feel more confident that changes in mobile conventions are not driving our findings. Our analysis is also robust to removing any given country from the sample.

cities or towns, which can span multiple 1 km^2 cells. We code units as treated if at least half of their area is covered in a given year. Alternatively, we can code units as treated if any of it is covered; this decision does not affect our results.

6.2 Protest Events

6.2.1 Global Database of Events, Location, and Tone

The Global Database of Events, Location, and Tone (GDELT) uses tools from text analysis to machine code events from a wide array of news sources (Leetaru and Schrodt, 2013). GDELT includes a number of different types of events, but we only extract the protests which occurred between 2007 and August 2014 and can be geo-located based on the name of specific city or landmark. That is, we only retain protest events with the most precise geo-codes.[22]

GDELT errs on the side of inclusion and, thus, contains more false positives than other event databases. However, we do not believe this introduces any bias into our analysis. First, we show that our results hold using the Social Conflict in Africa Database, which is hand-coded. Second, our empirical strategy leverages trends and not level-differences in protest activity, and head-to-head comparisons suggest that GDELT captures important *changes* in protest activity (Steinart-Threlkeld, 2014; Ward et al., 2013). Ward et al. (2013) look at events in Egypt, Syria, and Turkey as reported in GDELT and ICEWS, a warning system used by the US government. They find that "the volume of GDELT data is very much larger than the corresponding ICEWS data, but they both pick up the same basic protests in Egypt and Turkey, and the same fighting in Syria" (p. 10). Finally, we include both locality and year (or country-year) fixed effects in our models. These absorb any time-invariant variation in protest levels at the grid cell level (e.g., due to geography), as well as global trends in protest incidence (e.g., due to changes in the corpus of news sources used to code GDELT events).

As with most geo-coded databases, protest events are typically assigned coordinates based on the town or city that they occur in. According to Oak Ridge National Laboratory (2012), the median area of major towns or cities is 37 kilometers squared. For this reason, we employ grid cells that are 6×6 kilometers in dimension. We recognize that the geo-coding procedure may amplify protest counts in some cells (e.g., the centroids of towns). Such level-differences across grid cells will be absorbed by our fixed effects and, thus, not affect our estimates.

Our results are robust to different grid cell sizes: our effect sizes are the same if we use either smaller (1 km^2) or larger (24 km^2) grid cells (available upon request). In section D.4, we restrict attention to major cities and find support for our hypotheses at the city-level.

[22] GDELT avoids double-counting by aggregating stories covering the same event. We also employ a binary indicator for protest as the dependent variable to limit concerns about over-counting.

6.2.2 Social Conflict in Africa Database

We also use event data on protests, riots, and strikes from the Social Conflict in Africa Database (SCAD) (Hendrix and Salehyan, 2012). The SCAD is culled from *Associated Press* and *Agence France Presse* news wire stories for African countries (1990-2011). A pool of stories that contain key words associated with mobilization or violence are sorted, read, and hand-coded. Events only enter the data one time, but multiple locations (e.g., a simultaneous protest across different cities) receive separate entries with distinct coordinates. The SCAD excludes all events that take place within the context of an armed civil conflict (as defined by the start and end dates in the Uppsala Armed Conflict Database). As with GDELT, we only use those protests with precise geo-codings.

The SCAD is especially useful for our purposes, because it includes variables for whether the event was repressed. We use these variables to assess whether cell phone coverage reduces the probability of repression.

6.3 Other Covariates

Oak Ridge National Laboratory (2012) provides global population estimates at the 1 km^2 resolution. We employ the 2012 data in our analysis. Ideally, we would have population data for each grid cell-year in our panel. However, we heed the advice of the data creators, who caution against over time comparisons at the grid cell-level. We use this population data, first, to remove grid cells with zero population and, second, to calculate m_{ct}, the proportion of citizens covered by the cell phone network in country c in year t.[23]

If cell phone expansion is driven by demand, then coverage may follow economic development. These economic changes could increase the likelihood of both coverage and protest, confounding our estimates. While yearly income or consumption data does not exist for every square kilometer of the globe, we can use information on nighttime lights collected by the Defense Meteorological Satellite Program's Operational Linescan System (DMS-OLS) at the 1 km^2 resolution from 2006 to 2013. A number of studies have demonstrated a robust positive correlation between nighttime lights and other indicators of development (Chen and Nordhaus, 2011; Doll et al., 2006); others still have deployed this data for purposes similar to our own (Pinkovskiy, 2013; Michalopoulos and Papaioannou, 2012, e.g.,). We employ the "Average Lights x Pct" measures, which, unlike the

[23] Suppose that there are N grid cells in country c. We calculate $m_{ct} = \left(\sum_{i=1}^{N} \mathbb{1}(\text{Covered})_{it} \times \text{Pop}_i\right) / \left(\sum_{i=1}^{N} \text{Pop}_i\right)$.

"Nighttime Lights Composite" is available through 2013. In this data, each cell is assigned a digital number from 0 to 63, representing its luminosity, multiplied by the percent frequency of light detection. To calculate the luminosity within our larger, 6 km^2 grid cells, we simply take the average across the nested 1 km^2 grid cells.

7. Results

7.1 Cell Coverage and the Probability of Protest

We evaluate our first two hypotheses by estimating equations 1 and 2 using both the GDELT and SCAD. To recap, we expect that cell phone coverage increases the probability of protest and that this effect will be largest where a large proportion of the citizenry is already a part of the network (i.e., where cell phones connect localities to a larger audience).

Before presenting the main estimates, we start by reporting the probability of protest for three groups in table 1: (a) grid cells that never receive coverage, (b) grid cells that receive coverage but have not yet, and (c) grid cells that receive coverage after they have gained access to the network. Among those areas that eventually receive coverage, the probability of protest is over twice as large after they transition into coverage. These simple comparisons foreshadow our regression results. This table also highlights an important feature of the data: we are looking at the probability of protest in a given 6 km^2 grid cell in a given year. There are over two million populated grid cells in our sample, so that probability is small in absolute terms. In interpreting the magnitude of our effects, it is important to keep in mind this low baseline probability.

Table 1: Pr(Protest) by Coverage; GDELT Data
Pr(Protest) doubles pre-/post-coverage in cells that eventually receive treatment.

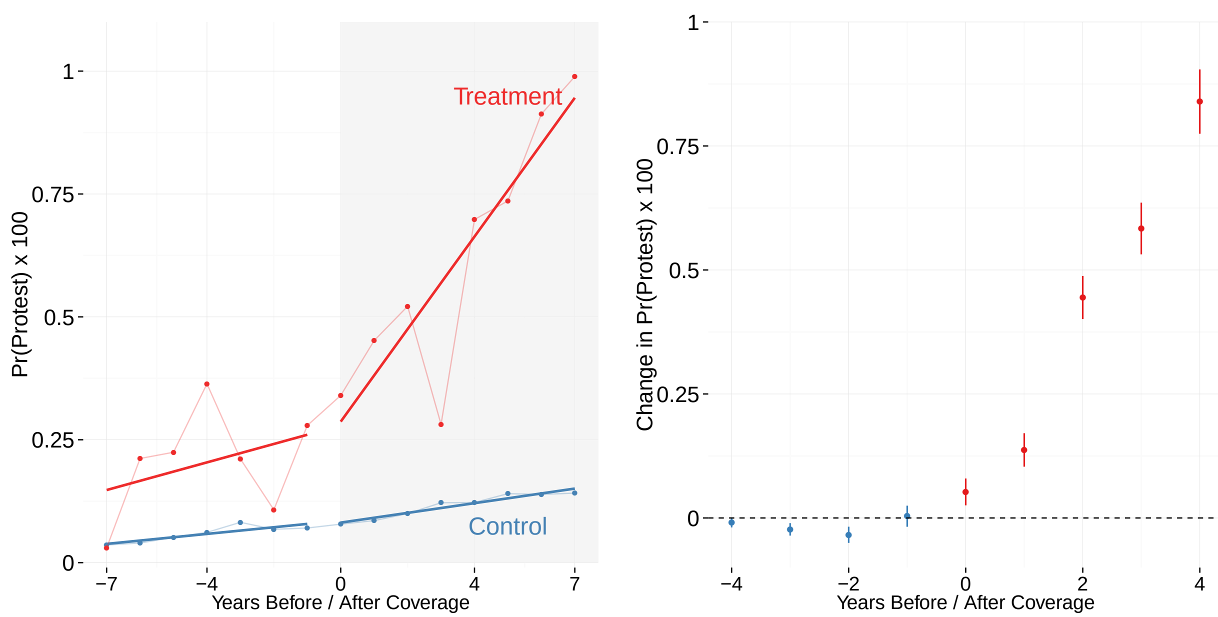
Never Covered	$\mathbb{1}$(Covered)	Pr(Protest) $\times$ 100	St. Dev.
1	0	0.050	2.228
0	0	0.185	4.302
0	1	0.458	6.750

In figure 3, we present the main results visually. In the left panel, we graph the trends in the probability of protest in both control and treatment grid cells.[24] This figure shows, first, that prior to

[24]To construct the figure, we estimate the probability of protest in the control grid cells associated with each treated grid cell for each period. We then collapse the treated units by the relative year of transition, generating average protest rates for both treatment and relevant control groups and for each period of time.

transitioning into coverage, both groups follow roughly parallel trends. Second, after receiving cell phone coverage, the probability of protest increases substantially more in treated grid cells relative to control areas. In the right panel, we estimate the probability of protest in the years before and after grid cells transition to coverage. To estimate this model, we include both leads and lags of our treatment variable in equation 1 (See Autor Fig. 3 2003, for an early implementation of this strategy). This figure conveys two similar points. First, as with the simple difference-in-differences visualization, there is no evidence that the probability of protest was increasing prior to coverage in the grid cells that eventually receive treatment. Finding no evidence of anticipatory effects bolsters the identifying assumption that treatment and control areas would have followed parallel trends in the absence of treatment. Second, the treatment effect is not immediate, but rather increases with time. We do not expect the introduction of cell phone coverage to immediately incite protest; only after citizens adopt the technology can it have the effect of enabling collective action.

Figure 3: Effect of Coverage Expansion on Pr(Protest); GDELT Data
Trends in Pr(Protest) are parallel prior to treatment, but Pr(Protest) increases after cell phone coverage.



(a) Visualizing Difference-in-Differences　　(b) Leads/Lags Plot

Estimated impact of treatment on the probability of protest for years both before and after the change in coverage status. The figure on the **left** plots the probability of protest in the years before and after coverage. The figure on the **right** displays the point estimates and 95% confidence intervals on four leads and lags of our treatment variable. We use protest information from 2000-14 to construct the lead/lags to avoid losing observations. The final lag is equal to 1 for every year beginning with the fourth year after coverage. The sample used is limited to grid cells that experience a change in treatment status.

In table 3, we report the estimates from equations 1 and 2.[25] The first two models estimate the most straightforward difference-in-differences, only including an indicator for whether a grid cell has access to the cell phone network in a given year. The first model includes grid cell and year fixed effects, while the second model substitutes the year fixed effects for country$\times$year fixed effects. This second model flexibly accounts for country-specific trends in the probability of protest. The difference-in-differences estimate from model 1 implies that the transition to coverage increases the probability of protest by roughly half the baseline probability in treated areas. Model 4 demonstrates that this result is robust to including our proxy for economic development (logged luminosity, lagged one year), suggesting that the effect is not driven by modernization that both generates demand for coverage and also generates protest.

Table 2: Summary Statistics: GDELT Data

Statistic	N	Mean	St. Dev.	Min	Max
$\mathbb{1}$(Protest) $\times$ 100	12,661,254	0.150	3.867	0	100
$\mathbb{1}$(Covered)	12,661,254	0.178	0.383	0	1
m	12,661,254	0.758	0.247	0.000	1.000
Log Luminosity$_{t-1}$	12,661,254	0.284	0.573	0.000	4.159

Our second hypothesis states that the effect of cell phone coverage should be larger where access to the cell network connects a locality to a large proportion of their fellow citizens. We expect the interaction of our coverage indicator and the proportion of each country's population connected to the cell phone network (m_{ct}) to be positive. In both models 3 and 5, we find that the coefficient on the interaction term is both positive and significant. Our linear interaction term in model 3 implies that the effect of coverage on protest is positive when m_{ct} exceeds 0.7, which occurs around the 6th percentile of m_{ct} for the covered cells in our sample. We caution against reading too much into the implied effect of coverage at low-levels of m_{ct}. First, there are not many treated cells in this range. Second, when we look at the effect of coverage on protest for cells that fall below the median level of m, we find that the effect is smaller but still positive.

We conduct a falsification test to alleviate concerns concerns that our effects are driven by differential trends prior to the expansion of coverage. We artificially assign coverage eight years

[25] As noted above, the sample in these tables includes grid cells that never transition or eventually transition into coverage.

Table 3: Coverage Expansion and Pr(Protest); GDELT Data
Cell phone coverage increases Pr(Protest), esp. where audience (m_{ct}) is large.

	Dependent variable:				
	$\mathbb{1}$(Protest) $\times$ 100				
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Covered)	0.088*	0.037*	−0.251*	0.085*	−0.237*
	(0.006)	(0.006)	(0.053)	(0.006)	(0.053)
m			0.096*		0.097*
			(0.021)		(0.021)
$\mathbb{1}$(Covered) $\times$ m			0.362*		0.344*
			(0.056)		(0.056)
Log Luminosity$_{t-1}$				0.033*	0.028*
				(0.007)	(0.007)
Cell FEs	2,110,209	2,110,209	2,110,209	2,110,209	2,110,209
Year FEs	6		6	6	6
Country$\times$Year FEs		1,236			
Observations	12,661,254	12,661,254	12,661,254	12,661,254	12,661,254

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

Notes: columns 1-5: linear probability model regressions, where the dependent variable has been multiplied by 100. See equations 1 and 2 for the econometric specifications. The unit-of-analysis is the grid cell-year (grid cells measure 6x6 km at the equator). Grid cells with no population according to the LandScan data in 2012 are excluded from the sample, as are all grid cells covered throughout the study period. Data for the dependent variable comes from GDELT from 2007-09 and 2012-14; only protests with precise geo-codes are used. Information on mobile coverage is taken from the Collins Mobile Coverage Explorer database. Luminosity data (lagged one year) comes from the Defense Meteorological Operational Linescan System.

before it actually occurred (table C.1).[26] Our estimates using this placebo treatment are relatively precisely estimated zeros, which are roughly an order of magnitude smaller than our effects using the actual date of treatment.

We also perform a number of robustness checks. First, o address potential spatial dependence, we cluster our standard errors on larger geographic units, such as 24 km^2 grid cells (see section D.3).

[26] We exclude 2006 from this analysis to avoid wrongly coding areas as untreated when, in fact, they transitioned to coverage during 2006 but are first reported as covered in Q1 2007.

Our inferences are unchanged. Second, we also estimate the overall effect of coverage using the SCAD (see appendix D.5). This demonstrates that our findings are robust to using an alternative (hand-coded) measure of social conflict and shows that the results hold in African countries, where there are no concerns about changes in mobile standards (from CDMA to GSM) contaminating treatment assignment. Table D.6 reports results that confirm what we found using the GDELT data. As a percentage of the baseline probability, these effect sizes are actually larger. Finally, in table D.7, we find that cell phones per capita are associated with a higher probability of protest and number of protests at the country level.

7.2 Cell Coverage and Repression

We find that the effect of cell phone access on the probability of protest is greater where gaining access to the network connects a locality to a larger proportion of the citizenry. This supports the logic of our model: governments should be less inclined to repress a protest if they know that protesters can rapidly share brutality with a large audience of their fellow citizens. Anticipating less repression, protesters are then more willing to demonstrate. In this section we look for more direct evidence that the use of repression declines in areas that have received coverage.

The analysis in this section requires a few additional caveats. First, we are limited to the SCAD, which only includes African countries (with populations over one million) and does not contain information on social conflict beyond 2012. This lops off a large non-random chunk of our sample. Second, and perhaps more importantly, we only observe repression that occurs in response to protests. If no protest occurs in a cell-year, then (in this data) the government never has an opportunity to use repression, which induces the selection problem described in section 5.2. By removing observations where no protest takes place in the recent past (between 2000 and 2012 or the year of treatment, whichever comes first), we can obtain an estimate of a lower bound of the effect of coverage on repression (see section B for the logic behind this subgroup analysis). That is, if the model correctly describes the effect of cell phone coverage on repression decisions, the estimated effect understates the true reduction in repression.

Table 4: Summary Statistics: Coverage Expansion and Repression; SCAD

Statistic	N	Mean	St. Dev.	Min	Max
$\mathbb{1}$(Repressed)	1,976	0.0142	0.1182	0	1
$\mathbb{1}$(Covered)	1,976	0.0693	0.2541	0	1
m	1,976	0.4149	0.3011	0.0018	0.9978

Figure 4: Pr(Repress) Pre-/Post-Coverage
Decline in repression only after coverage.

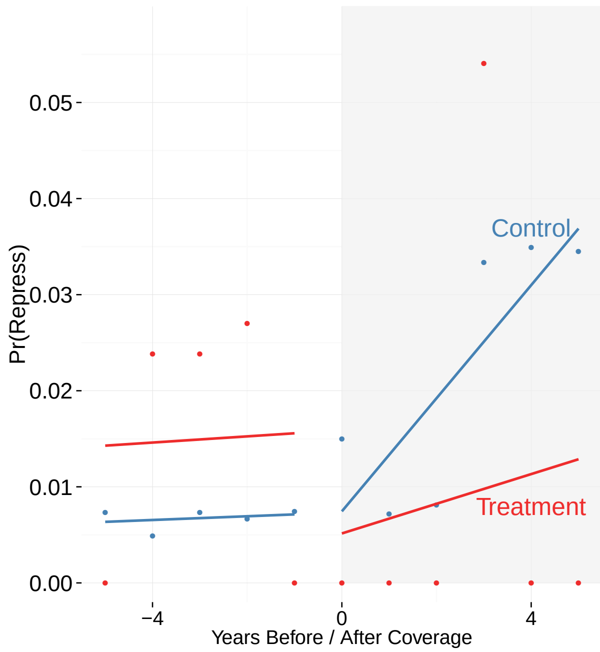


Table 5: Coverage and Repression; SCAD
Pr(Repress) falls after coverage.

	Dependent variable:		
	$\mathbb{1}$(Repressed)		
	(1)	(2)	(3)
$\mathbb{1}$(Covered)	−0.022	−0.022	−0.018
	(0.016)	(0.018)	(0.078)
m			−0.023
			(0.059)
$\mathbb{1}$(Covered) $\times\, m$			−0.002
			(0.100)
Cell FEs	494	494	494
Year FEs	4		4
Country×Year FEs		148	
Observations	1,976	1,976	1,976
Note:	Robust std. errors clustered on grid-cell; $^{\dagger}p < 0.1$, $^{*}p < 0.05$		

The figure on the **left** plots the probability of repression in the years before and after coverage. The table on the **right** includes linear probability models, as specified in equation 3. Data on repression comes from SCAD, and information on cell phone coverage is taken from the Collins Mobile Coverage Explorer database. Per section B, the sample is limited to grid cells that experienced a protest between 2000 and 2012 or prior to treatment (whichever comes first).

We start by presenting these results graphically in figure 4: while the probability of repression appears to follow parallel trends in treatment and control areas prior to the expansion of coverage, the likelihood of repression falls considerably in treated areas. This decrease is especially striking given the increasing probability of repression observed in uncovered areas. The results from equation 3 are presented in table 5. Our difference-in-differences estimates suggest that the probability of repression is considerably lower after grid cells gain access to a cell phone network.[27] We regard these results as suggestive of the second mechanism highlighted by the model, though they are not statistically significant ($p \approx 0.2$, for the first two models). When we interact coverage with the proportion of the population covered by the network, the coefficient is negative, as expected, but also very imprecisely estimated.

[27] Including logged luminosity has no effect on these point estimates.

7.3 Cell Phone Coverage and Reporting Bias

Readers may be concerned that cell phones enable journalists to learn about and report on protests. As a result, protests in areas with cell networks may receive more coverage and, thus, be more likely to appear in our event datasets, which are based on news reports. In a recent article, Weidmann (2015, 6-7) provides evidence that cell phone coverage increases the probability that international news outlets report armed conflicts in Afghanistan.

We take a number of steps to ameliorate concerns that such reporting bias could drive the effects we detect. Two features of our empirical design address potential reporting bias. First, unlike cross-sectional studies, we control for all features of grid cells that do not vary between 2007 and 2014. We are not worried then about reporting biases that are driven by geography, distance to a major city or border, or the language spoken in a particular place. Second, we include a time-varying measure of development, luminosity. This addresses the concern that as areas develop, they are more likely to garner reporters' attention.

We go further and look at whether the average number of articles or sources reporting on protests increase when locations transition into coverage. That is, we run our same difference-in-differences (equation 1) but use the average number of articles or sources per protest (from GDELT) as the dependent variable. Our estimates are *negative* and small relative to the mean. These results suggest that the intensity of media coverage did not meaningfully change when areas transitioned into cell phone coverage, providing more direct evidence that reporting bias is not in play. The number of observations drops in these regressions, as these only include cell-years that have protests.

Table 6: Summary Statistics: Media Coverage; GDELT Data

Statistic	N	Mean	St. Dev.	Min	Max
Mean(Articles)	17,158	6.023	7.767	1.000	531.000
Mean(Sources)	17,158	1.211	1.145	1.000	57.000
$\mathbb{1}$(Covered)	17,158	0.574	0.494	0	1
Log Luminosity$_{t-1}$	17,158	1.549	1.473	0.000	4.159

Finally, we pursue a bounding approach and find that reporting bias would need to be large to generate our effects (see appendix D.2). This bounding approach (summarized by figure D.2) indicates the the probability of reporting in treated and untreated areas would have to differ by

Table 7: Coverage Expansion and Media Coverage; GDELT Data
Cell phone coverage does not increase reporting on protests.

	Dependent variable:			
	Mean(Articles/Protest)		Mean(Sources/Protest)	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Covered)	-0.237	-0.237	-0.018	-0.019
	(0.329)	(0.329)	(0.047)	(0.047)
Log Luminosity$_{t-1}$		0.051		0.114
		(0.496)		(0.110)
Cell FEs	2,946	2,946	2,946	2,946
Year FEs	6	6	6	6
Observations	17,158	17,158	17,158	17,158
Note:	Robust std. errors clustered on grid cell; $^{\dagger}p < 0.1$, $^{*}p < 0.05$			

Notes: columns 1-4: OLS regressions, where the dependent variable is the average number of news articles or news sources reporting on each protest within a grid-cell-year. The unit-of-analysis is the grid cell-year (grid cells measure 6x6 km at the equator). This analysis uses the same sample of grid cells as table 3. However, the outcome variable can not be measured in grid cell-years that do not experience protest; hence, the considerably reduced sample. See table 3 for notes on other data sources.

more than 15 percentage points to explain away our effects. This seems unreasonable given that Weidmann's estimates place this bias at around six percentage points in Afghanistan—a war zone where reporting challenges are extreme.

Any data set built on media or third-party reports will suffer underreporting. However, we do not find evidence that cell phone coverage increases the resources devoted to reporting on protests. Moreover, we find that the reporting bias would have to more than double what Weidmann (2015) finds to completely account for our effects. Given these two pieces of evidence, we feel confident that our results are not explained by increased media attention post-treatment.

8. Conclusion

This paper addresses an ongoing debate about whether and why cell phones affect protest activity around the world. We make two advances. The first is theoretical: we present a formal logic for how cell phones both reduce coordination costs and deter repression. Our second contribution is empirical: we find that gaining access to the cell phone network increases the probability of protest by more than half the baseline probability of protest. Furthermore, this effect is larger in cases where a large proportion of citizens already have access to the network—a finding consistent with our argument that cell phones increase the risk of escalation and thus deter repression. We also find suggestive evidence that the probability of repression declines after an area gains access to the cell network, though these estimates are imprecise and plausibly a lower bound of the true effect.

This paper helps resolve an ongoing debate about whether and why cell phones affect protest. More broadly, we address questions about how citizens coordinate to assert their demands, and when such mobilization will be tolerated or met with brutal repression. Cell phones are simply a technology—albeit an important one—that enables individuals to quickly disseminate information both about their political intentions and any government response. While nearly every country constitutionally recognizes citizens' rights to freely associate, fewer honor this right in practice (Christensen and Weinstein, 2013). This paper provides a model (supported by empirical evidence) for thinking about when governments will allow citizens to engage in public dissent—not because of the undeniable normative appeal of free association but because cracking down is counter-productive.

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Appendices

Contents

A. Proofs

A.1 Proof of Protester's Tactical Decision

Proof. Each protester chooses the tactic that maximizes her expected utility, given her signals, $\vec{s}$:

$$\begin{aligned} t^* &= \arg\max_t E_T[u_p(t)|\vec{s}] \\ &= \arg\max_t E_T[c\mathcal{P} - (k + R_p)(t - T)^2|\vec{s}] \\ &= E_T[T|\vec{s}] = \mu_p \end{aligned}$$

$\square$

A.2 Proof of Proposition 1 (Equilibrium Characterization)

Proof. An informed citizen i will never want to punish if no repression occurs, as $0 < q$. However, if i observes repression and $v_i > q$, then they will punish the government. This implies that the government will alienate a proportion $(1 - \psi)m[1 - F\{q\}]$ of citizens by choosing to repress.

If the government does not repress at all, no citizens will punish, and an interest group member p will only protest if $c\psi \geq k/\overline{\beta}$.

Suppose that $c\psi < k/\overline{\beta}$. The government can ensure their maximum payoff c by not repressing. If the government represses, then $(1 - \psi)m[1 - F\{q\}]$ of citizens will punish, reducing the government's expected payoff to $c(1 - \psi)m[1 - F\{q\}] \leq c$. Thus, if $c\psi < k/\overline{\beta}$, then no protests occur, the regime never represses, and no citizens punish.

Suppose instead that $c\psi \geq k/\overline{\beta}$. If the government represses, then they will be punished by $(1 - \psi)m[1 - F\{q\}]$ citizens, and p will want to protest if their expected utility to protesting is greater than their status quo payoff:

$$\underbrace{c[(1 - \psi)m[1 - F\{q\}] + \psi R_p]}_{\text{Expected Benefit}} - \underbrace{(k + R_p)/\overline{\beta}}_{\text{Expected Cost}} \geq 0.$$

If $\overline{R}$ represents the R_p for which this condition is satisfied with equality, then we know that any p with $R_p < \overline{R}$ will protest. Under the assumption that $R_p \sim U[0, 1]$, $\Pr(R_p < \overline{R}) = \overline{R}$. If $\overline{R} = 0$, then no p protests. The government will repress only if

$$\begin{aligned} c[1 - (1 - \psi)m[1 - F\{q\}] - \psi\overline{R}] - R_G &\geq c[1 - \psi] \\ \underbrace{\psi(1 - \overline{R})}_{\text{Deterred Protesters}} &\geq \underbrace{(1 - \psi)m[1 - F\{q\}]}_{\text{Alienated Citizens}} + \frac{R_G}{c}. \end{aligned}$$

$\square$

A.3 Proof of Proposition 2 (Comparative Statics)

Proof. If the government does not repress, then an interest group member p will protest if $c\psi \geq k/(\beta_0 + S\beta_s)$. This condition is more likely to hold as $S\beta_s$ increases.

If an interest group member p anticipates repression, then they will protest if $c[(1-\psi)m[1-F\{q\}] + \psi R_p] \geq (k + R_p)/(\beta_0 + S\beta_s)$. A protest only occurs if this condition is satisfied for the p with the smallest $R_p > 0$, and this condition is more likely to be satisfied for any p as $S\beta_s$ increases.

The government wants to repress if $\psi(1-\overline{R}) \geq (1-\psi)m[1-F\{q\}] + R_G/c$. This inequality is less likely to hold as m increases. The government will never repress if this condition does not hold, regardless of whether protests actually take place.

Suppose that $c\psi \geq k/\overline{\beta}$ but $c[(1-\psi)m[1-F\{q\}] + \psi R_p] < (k + R_p)/\overline{\beta}$. If an increase in m shifts the government's decision from repression to no repression, then we move from a region in which no p protests to one in which all p protest. Thus, by disincentivizing repression, increasing m can increase the likelihood of protest. $\square$

B. Resolving Selection Problem for Repression Analysis

Estimating the effect of coverage on repression remains challenging. This is the case, because *repression is only observed when a protest actually takes place and not when a protest that would have been repressed never materializes* (i.e., when repression effectively deters protest).

Our theory helps reveal the thorniness of this selection problem, which can lead us to over- or under-estimate the true effect of cell phone coverage on the government's propensity to repress. Recall that there are four equilibrium outcomes in our model: (A) no protest, and government would not repress; (B) no protest, and government would repress; (C) protest, and government represses; and (D) protest, and government does not repress. If our argument is correct and cell phones reduce coordination costs and increase the visibility of repression, then receiving coverage can change the equilibrium in a locality in one of six ways. These are listed in the first column of table B.1.

Table B.1: The Selection Problem Related to Repression

Equilibrium Shift:			Actual Change:	Observed Change:	Proportion of
$D_i = 0$		$D_i = 1$	$\tau_i = R_i(1) - R_i(0)$	$\widetilde{\tau}_i = \widetilde{R}_i(1) - \widetilde{R}_i(0)$	Observations:
A	$\rightarrow$	B	1	0	p_{AB}
A	$\rightarrow$	C	1	1	p_{AC}
A	$\rightarrow$	D	0	0	p_{AD}
B	$\rightarrow$	C	0	1	p_{BC}
B	$\rightarrow$	D	-1	0	p_{BD}
C	$\rightarrow$	D	-1	-1	p_{CD}

(A) No protest, government would not repress; (B) No protest, government would repress; (C) Protest, government represses; (D) Protest, government does not repress.

How does true and observed use of repression change with each of these equilibrium shifts? Let $R_i(D_i)$ be the government's true decision about whether to employ repression in locality i as a function of i's treatment status, $D_i \in \{0, 1\}$. What we actually observe is $\widetilde{R}_i(D_i)$, which is one if a protest occurs in locality i and is repressed and zero otherwise. The second and third columns of table B.1 show the change in the true and observed use of repression, respectively. Taking the first row of the table as an example, when gaining cell phone coverage shifts an area from equilibrium A to equilibrium B, the government's decision to repress changes from 0 to 1 ($\tau = R(1) - R(0) = 1 - 0 = 1$), but we do not observe this change in repression because protest is deterred in equilibrium B ($\widetilde{\tau} = \widetilde{R}(1) - \widetilde{R}(0) = 0 - 0 = 0$). The final column of the table indicates the proportion of observations that experience this equilibrium shift (e.g., p_{AB} is the proportion of localities that shift from A $\rightarrow$ B).

After weighting the actual change in repression by these proportions, the true average effect of cell phone coverage on repression can be written as:

$$\tau = p_{AB} + p_{AC} - p_{BD} - p_{CD}.$$

However, what we actually observe is:

$$\widetilde{\tau} = p_{AC} + p_{BC} - p_{CD}.$$

The true decrease in the use of repression will be larger in magnitude than the observed reduction when the following condition holds:

$$\tau < \widetilde{\tau} \iff p_{AB} < p_{BC} + p_{BD}.$$

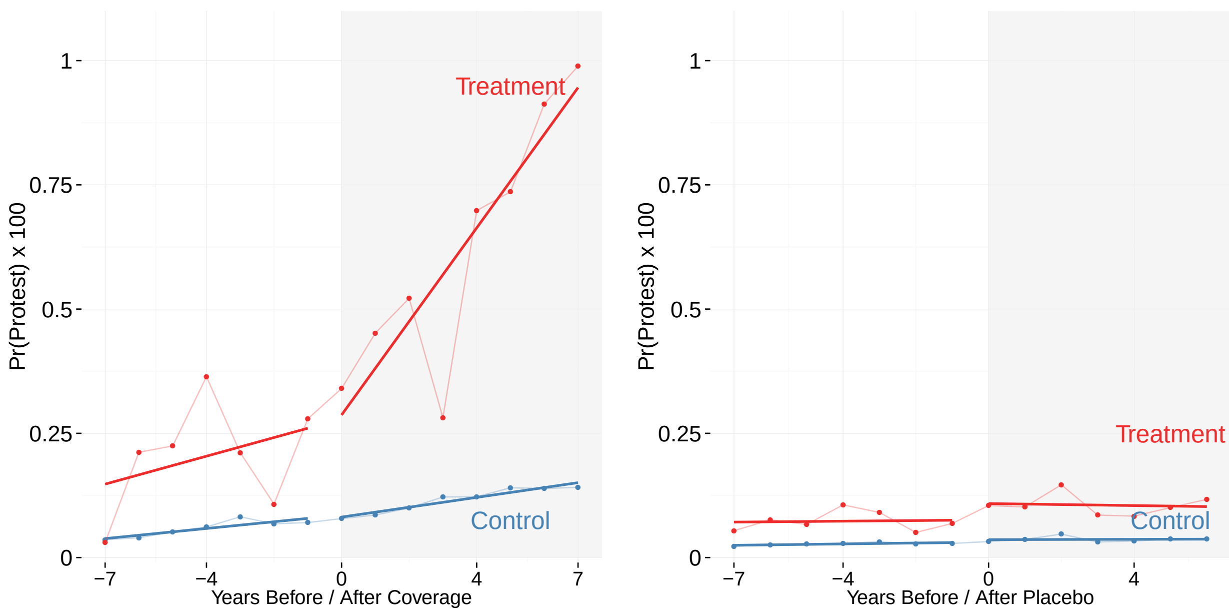
Put differently, when this condition holds, the selection problem makes it tougher to find evidence supporting our hypothesis that repression declines following the expansion of coverage.

This insight allows us to make *some* empirical progress. If we can remove the observations that make up p_{AB} from our sample, thus satisfying the condition above, then (assuming our model is correct) our estimate will *understate* the true reduction in repression that results from treatment. Equilibrium (A) (i.e., no protest, government would not repress) results when the costs of staging a protest are prohibitively high, regardless of the government's response. In an attempt to exclude all such places, we drop localities that never experience a protest between 2000 and 2012 (or their first year of treatment, whichever comes first). Estimating equation 3 using the resulting sample, we feel more confident about interpreting the estimate of τ as understating the true reduction in repression that results from the introduction of cell phone coverage. This strategy allows us to plausibly recover a lower bound on the effect of coverage on repression.

C. Placebo Results (GDELT)

As a further check that trends in the treatment and control areas are parallel prior to the expansion of cell coverage, we conduct a falsification test. First, we re-assign treatment—transition into cell-phone coverage—to eight years before the actual rollout, and then estimate the difference-in-differences (equation 1) using data on protest from 1999-2006 period. For example, a place that receives coverage in 2012 is assigned placebo coverage starting in 2004. Under the parallel trends assumption, we expect no effect of this placebo treatment on the probability of protest.

Figure C.1: Difference-in-Differences using Actual and Placebo Treatments



(a) Actual Coverage (2007-2014) (b) Placebo Coverage (1999-2005)

Estimated impact of treatment on the probability of protest for years both before and after the change in coverage status. The figure on the **left** plots the probability of protest in the years before and after coverage. The figure on the **right** plots the probability of protest in the years before and after a placebo treatment that occurs eight years prior to the actual treatment.

Using GDELT data, figure C.1 compares the probability of protest in each year before and after transition into coverage for the actual period of transition (left panel), and with the placebo transition (right panel). The levels in the panels are different, suggesting a general upward trend in the overall probability of protest over time. Crucially, while the actual treatment generates a substantial increase in the probability of protest following coverage, the placebo does not.

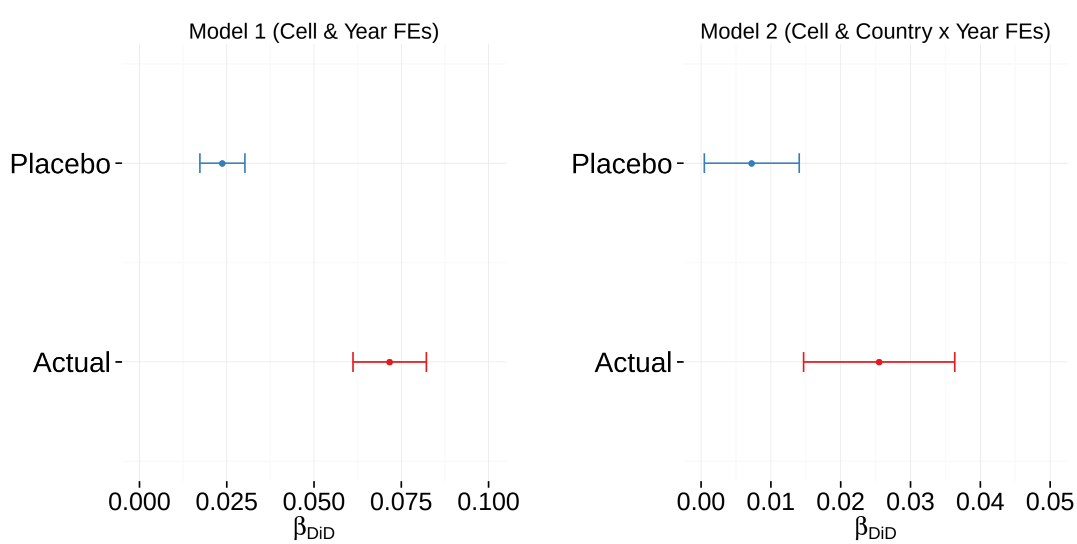
The pattern revealed in the figure is confirmed in table C.1, where we repeat our main analysis with the placebo treatment and estimate equation 1. The point estimate of placebo coverage is precisely estimated and close to zero in all models. For instance, model 2, which includes country-year fixed effects in addition to grid cell fixed effects, indicates that the magnitude of the estimated effect is over eighty times larger using the real treatment as compared to using the placebo. The size of the audience does not change these results; at different levels of m, the placebo treatment does not follow a discernible pattern and is never statistically distinguishable from zero.

Table C.1: Placebo Treatment and Pr(Protest); GDELT Data

	Dependent variable:	
	$\mathbb{1}$(Protest) $\times$ 100	
	(1)	(2)
$\mathbb{1}$(Covered)	0.0111*	0.0024
	(0.0037)	(0.0041)
Cell FEs	2,110,209	2,110,209
Year FEs	5	
Country$\times$Year FEs		1,030
Observations	10,551,045	10,551,045
Note:	Robust std. errors clustered on grid cell; $^{\dagger}p < 0.1$, $^{*}p < 0.05$	

Notes: columns 1-2: linear probability models. See equation 1 for the specification and table 3 for notes on data sources. Data for the dependent variable comes from GDELT from 1999-2001 and 2004-2005.

Figure C.2: Results using Actual vs. Placebo Treatment



D. Supporting Information (Online)

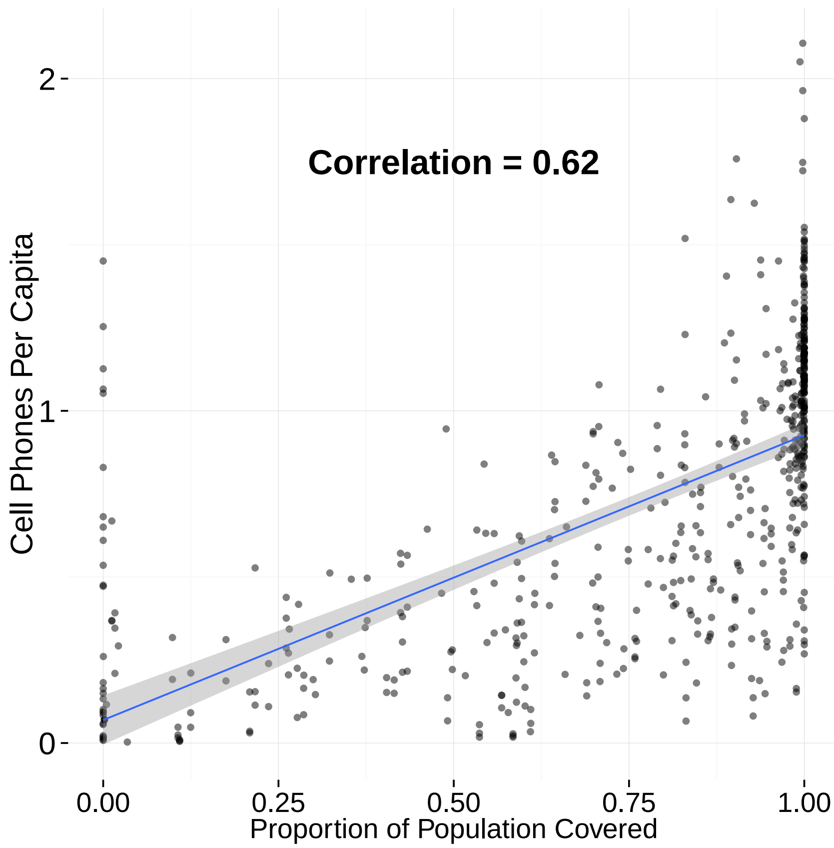
Can You Hear Me Now?:
How Communication Technology Affects Protest and Repression

Following text to be published online.

D.1 Comparing Cell Phone Coverage with Mobile Phone Ownership

The Collins Mobile Coverage Explorer database is compiled from submissions by telecom operators around the World. To check that reported expansions in coverage correspond to increases in cell phone use, figure D.1 compares the proportion of the population covered by the cell phone network (according to the Collins Mobile Coverage Explorer database) with data on cell phone ownership per capita from Banks and Wilson (2014). As expected, we find that the two are highly positively correlated ($\rho = 0.62$).

Figure D.1: Cell Phone Coverage vs. Cell Phone Ownership Per Capita



We calculate the proportion of the population covered by the cell phone network using the formula in section 6.3 and data from the Collins Mobile Coverage Explorer database and LandScan. Data on cell phone ownership per capita come from Banks and Wilson (2014). Note that cell phone ownership per capita can exceed one if the average individual owns multiple phones.

There are some very small countries (e.g., the Bahamas, Djibouti, Kiribati) where ownership is high despite minimal coverage. In particular, there are 40 country-years where the proportion of the population covered is less than 0.05, yet per capita ownership exceeds 0.25. This suggests that we may be wrongly classifying some areas as "control" when they, in fact, enjoy some access. Comfortingly, this works against rejecting the null. Furthermore, such observations make up less than 1% of our sample and, thus, do not meaningfully impact our results.

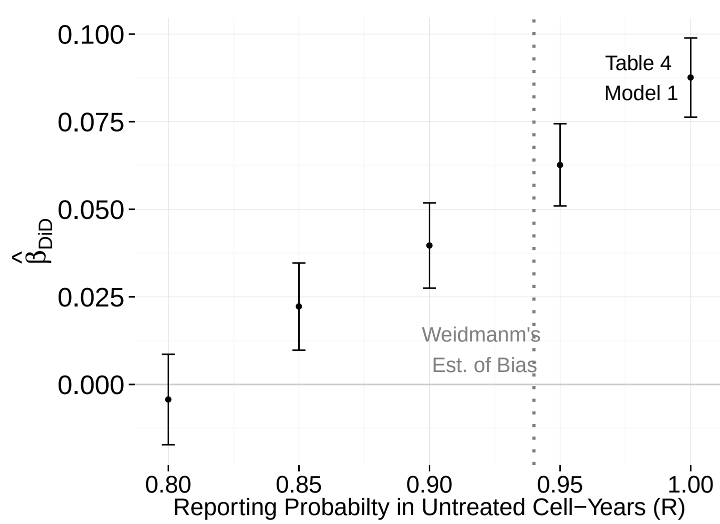
D.2 Bounding Reporting Bias

Figure D.2 indicates the the probability of reporting in treated and untreated areas would have to differ by more than 15 percentage points to explain away our effects. This is more than double the reporting bias that Weidmann (2015) estimates using data from Afghanistan.

For the purposes of this bounding exercise, we assume that (1) there is no underreporting in treated grid cell-years; and (2) the null hypothesis that there is no difference in the probability of protest in areas with and without cell phone coverage is true. These assumptions imply that we can estimate the average probability of protest in all cells by just looking at treated cell-years. Call this probability $\mathcal{P} = \Pr(\text{Protest}|\text{Treated}) = \Pr(\text{Protest}|\text{Untreated})$. Let $\mathcal{R}$ be the probability that a protest is reported on if it occurs; our second assumption implies that $\mathcal{R} = 1$ in treated grid cells. With these assumptions and notation in hand, we then proceed as follows:

- We retain the outcome information of treated grid cell-years, which is assumed complete.
- If a grid cell-year does not get coverage but reports a protest, we retain their outcome data.
- If a grid cell-year does not get coverage *and* does not report a protest, then we assume that a protest occurred with probability $\mathcal{P}$ and was reported on with probability $\mathcal{R}$. We thus assign new outcomes to these cells by drawing from $\{0, 1\}$ with probabilities $\{1 - \widehat{\mathcal{P}}\mathcal{R}, \widehat{\mathcal{P}}\mathcal{R}\}$. ($\widehat{\mathcal{P}}$ is simply the estimated probability of protest in the grid cell-years receiving treatment.)
- We use this adjusted outcome vector to reestimate our difference-in-differences (eqn. 1) with country-year fixed effects. We report results for different levels of reporting bias ($\mathcal{R} \in [0.8, 1]$).

Figure D.2: Reporting Bias Required to Explain Away Our Results



Estimated impact and 95% confidence intervals using (eqn. 1) of cell coverage on the probability of protest assuming different levels of underreporting in uncovered areas relative to covered areas. Note that Weidmann (2015) estimates this bias at 0.06 in Afghanistan, which is indicated with the dashed vertical line on the figure.

D.3 Robustness to Clustering on Larger Geographic Units

In the primary analysis, we cluster our standard errors on grid cell to account for temporal dependence. To account for possible spatial dependence, we also nest each of our 6×6 km cells in larger 24×24 km cells. Table D.1 replicates table 3 but clusters the standard errors on these larger (24×24 km) units. Our inferences are unchanged.

Table D.1: Coverage Expansion and Pr(Protest), Clustering on Larger Geographies; GDELT Data

	Dependent variable:				
	$\mathbb{1}$(Protest) $\times$ 100				
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Covered)	0.088*	0.037*	−0.251*	0.085*	−0.237*
	(0.006)	(0.006)	(0.055)	(0.006)	(0.055)
m			0.096*		0.097*
			(0.021)		(0.021)
$\mathbb{1}$(Covered) $\times$ m			0.362*		0.344*
			(0.058)		(0.058)
Log Luminosity$_{t-1}$				0.033*	0.028*
				(0.007)	(0.007)
Cell FEs	2110209	2110209	2110209	2110209	2110209
Year FEs	6		6	6	6
Country$\times$Year FEs		1236			
Observations	12,661,254	12,661,254	12,661,254	12,661,254	12,661,254
Note:	Robust std. errors clustered on grid-cell; $^{\dagger}p < 0.1$, $^{*}p < 0.05$				

D.4 Robustness to Using Cities as Unit of Analysis

In geo-coding events, GDELT assigns them to the town or city of occurrence. For this reason, our main analysis uses a grid with cells sized to correspond to the median city's area (6×6 km). We corroborate our results using a lower resolution (24×24 km). In this section, we also present a city-level analysis, in which the geographic units of analysis are contiguous areas with 200 people per km^2 or more according to Oak Ridge National Laboratory (2012). Of the 5793 cities, our sample comprises the 927 cities that were not covered throughout the period of analysis. We code a city as covered by a cell phone network if any of its area is covered by a network in a given year. Results in table D.3 support our previous findings. When we include country-specific flexible time trends, we find that the direct effect of coverage is positive (if slightly smaller in magnitude). Moreover, we find strong evidence that the likelihod of protest increases as the size of the audience grows; at $m_{ct} = 0.78$ — which falls at the 17th percentile of covered cities — the effect becomes positive.

Table D.2: Summary Statistics: City-Level GDELT Data

Statistic	N	Mean	St. Dev.	Min	Max
$\mathbb{1}$(Protest) $\times$ 100	5,562	17.080	37.640	0	100
$\mathbb{1}$(Covered)	5,562	0.401	0.490	0	1
m	5,562	0.677	0.320	0.000	1.000

Table D.3: Coverage Expansion and Pr(Protest); GDELT City-Level Data

	Dependent variable:		
	$\mathbb{1}$(Protest) $\times$ 100		
	(1)	(2)	(3)
$\mathbb{1}$(Covered)	-0.129	3.962*	-17.460^{*}
	(1.178)	(1.982)	(5.704)
m			-5.568
			(4.732)
$\mathbb{1}$(Covered) $\times$ m			22.500*
			(6.420)
Cell FEs	927	927	927
Year FEs	6		6
Country$\times$Year FEs		540	
Observations	5,562	5,562	5,562
Note:	Robust std. errors clustered on grid-cell; $^{\dagger}p < 0.1$, $^{*}p < 0.05$		

D.5 Effect of Coverage on Protest using SCAD Data

Table D.4: Pr(Protest) by Coverage; SCAD Data

Never Covered	$\mathbb{1}$(Covered)	$\Pr$(Soc. Conf.) $\times$ 100	SD
1	0	0.009	0.946
0	0	0.006	0.743
0	1	0.025	1.579

Table D.5: Summary Statistics: SCAD Data

Statistic	N	Mean	St. Dev.	Min	Max
$\mathbb{1}$(Soc. Conf.) $\times$ 100	1,992,524	0.009	0.969	0	100
$\mathbb{1}$(Covered)	1,992,524	0.054	0.227	0	1
Log Luminosity$_{t-1}$	1,992,524	0.308	0.385	0.000	4.157

Table D.6: Coverage Expansion and Pr(Soc. Conf.); SCAD Data

	Dependent variable:		
	$\mathbb{1}$(Soc. Conf.) $\times$ 100		
	(1)	(2)	(3)
$\mathbb{1}$(Covered)	0.0189*	0.0244*	0.0189*
	(0.0073)	(0.0094)	(0.0073)
Log Luminosity$_{t-1}$			-0.0008
			(0.0053)
Cell FEs	498131	498131	498131
Year FEs	4		4
Country$\times$Year FEs		228	
Observations	2,553,544	2,553,544	2,553,544
Note:	Robust std. errors clustered on grid-cell; $^{\dagger}p < 0.1$, $^{*}p < 0.05$		

D.6 Cross-National Results

While our high-resolution data allow us to employ a more credible empirical strategy than past work, our basic findings are not driven by our decision to focus on a much smaller unit of analysis (the grid cell) than is typical in cross-national comparative projects. In table D.7 we use the well-known Cross-National Time-Series Data Archive from Banks and Wilson (2014) to replicate our first result. Employing a country-year panel from 1991-2011, we find that cell phones per capita (lagged one year) are associated with a higher probability of protest and a higher number of protests (where protests include anti-government demonstrations, strikes, and riots). These models include country and year fixed effects, country-specific linear time trends, and controls for logged GDP and logged population.

Table D.7: Cross-national Correlations of Cell Phones (per capita) and Protest

	Dependent variable:			
	$\mathbb{1}$(Protest)	$\mathbb{1}$(Protest)	Σ Protests	Σ Protests
	(1)	(2)	(3)	(4)
Cell phones / Pop. (lag)	0.14	0.086	1.78	1.29
	(0.048)	(0.053)	(0.66)	(0.54)
Log of GDP (lag)		0.020		0.42
		(0.032)		(0.29)
Log of Pop. (lag)		0.12		1.60
		(0.23)		(4.53)
Country & Year FE	✓	✓	✓	✓
Country Time-Trends	✓	✓	✓	✓
R^2	0.37	0.37	0.31	0.27
Observations	3859	3668	3873	3678
Number of countries	197	195	197	195

Robust standard errors clustered on country.

Note: columns 1-2: linear probability models. Columns 3-4: OLS regressions with the number of protests used as the dependent variable. Data for all variables is taken from the CNTS Data Archive from 1991-2011.

In all of the specifications, the correlations between cell phones per capita and our protest variables are positive; the relationship is statistically significant (or nearly significant) in all four models. The first model implies that one within-country standard deviation increase in cell phones per capita (0.16) is associated with a two percentage point increase in the probability of protest (or 14 percent of a within-country standard deviation of the dependent variable). While we are comforted by finding a similar correlation between cell phone penetration and protest activity at the country-level, this analysis is more likely confounded by omitted variables than our early results that leverage over time variation within very small geographic units. Furthermore, these country-level data does not allow us to evaluate our second hypothesis.