Can you hear me now? Good?? The Effect of Mobile Phones on Collective Violent Action in the Libyan Revolution

Absher, Samuel and Grier, Kevin

Rand Corporation, Texas Tech University

4 March 2019

Can You Hear Me Now? … Good?

The Effect of Mobile Phones on Collective Violent Action in the Libyan Revolution

Dr. Sam Absher
Associate Economist
RAND Corporation

Dr. Kevin Grier*
Gordon Tullock Professor of Political Economy and
Professor of Political Science
Texas Tech University

ABSTRACT

We explore the effect of mobile phone and internet access on levels of collective violent action within the Libyan Revolution. Eastern Libya experienced a state-implemented blackout shortly after widespread riots and protests began. However, with luck, ingenuity, and foreign aid, Libyan rebels forged an independent mobile phone network. We exploit the exogeneity of the timing of the network's reactivation and use a variation of difference-in-differences (DID) to measure the effect on the frequency of collective violent action. While the dominant view in the literature is that cell access increases violence by lowering the costs of organizing, we find that the reactivation of the mobile phone network reduced violent collective action by 21%. We find this negative effect for all conflicts and for conflicts that can be identified as initiated by non-state actors. We also study mobile phone's effect on collective deadly action and fatalities using a different source for conflicts, finding similar negative effects. We propose mechanisms that may explain the aggregate negative effect: (1) substitution of physical protests to digital protests, (3) the reduction of dissatisfaction toward the state, and (3) the use of mobile phones to avoid conflict with state actors.

*Corresponding Author. We thank, Robin Grier, Daniel Hicks, Kevin Munger, Jamie Pavlik, Alex Salter, Le Wang, and Andrew Young for helpful comments and suggestions.

"Those who make peaceful revolution impossible will make violent revolution inevitable."
John F. Kennedy

1. Introduction

A rough consensus has emerged that horizontal media such as cell phones and the internet are a big positive in the operation of markets in the developing world (Jensen (2008), Aker (2010), Muto & Yamano (2009)). However, no such consensus exists when it comes to the role of horizontal media in the political arena. As we will discuss in depth later, some studies claim that expansion of cell coverage increases violent collective action and political unrest, while others find the opposite. The debate has become contentious. Our contributions in this paper are to (1) broaden the theoretical perspective on the mechanisms of how horizontal media affect political conflict and (2) present new evidence from a case drawn from the Libyan Civil War.

As we will discuss in our theory section, the main mechanism posited for horizontal media to influence political disorder is by lowering the costs for rebels, protesters, and insurgents to organize their efforts and thus creates more protests, violence, and deaths. This is undeniable. Yet, we believe there are other mechanisms at play that cut in the opposite direction, especially in our case, where Gaddafi severed cell communications in East Libya, but rebels, with the help of expat engineers and foreign aid, managed to reactivate one of the two downed networks after 31 days of blackout.

Clearly the state cut cell service to aid their cause in the conflict and hurt the rebels. But the reactivation was a surprise to the state, and quasi-random in its exact timing. We exploit the quasi-random elements which determined ICT access during and around the reactivation (timing and network selection) to gain a cleaner look at the causal effects than has been achieved so far in the literature.

We believe that there are three overlapping mechanisms that work in the opposite direction of the "cell access increases political violence because it lowers the cost of organizing" at play in our case.

First, the cessation of cell service makes people angry, and angry people often protest. When the cell service resumes, protests by that segment of the population could actually fall. Second, if you were already anti-state, it was possible for you to indulge your activism indoors via test messages, tweets, Facebook posts, etc. until cell service was cut. At that point, a person would have to take their discontent into meat space to express it. Resumption of cell service means that, on the margin, activists could re-convert to slacktivism. Third, the resumption of cell service means that prudent protesters or rebels could more easily know when and where state forces were located and avoid them if so desired. That is to say, cell access can lower the cost of organizing activities in a way that avoids direct conflict with state authorities if so desired. In the blackout period, protesters and rebels were "flying blind" with respect to the location of state authorities and subject to increased danger. In sum, we believe it entirely possible, without denying the truth of the lower organizing costs mechanism that the reactivation of cell service in our case at least could reduce physical political unrest and violent collective action.

Indeed, in our statistical analysis, we find that in windows as small as 7 days on either side of the reactivation in East Libya, political unrest and violent collective action significantly decreased after the reactivation, which we take to mean that the mechanisms we posited above (and discuss further in our theory section) outweigh the dominant mechanism in the current literature, at least in this specific case. Our results stand as one significant counterexample to the pessimistic position that access to horizontal media raises violent political action in the developing world.

In Section 2, we begin by telling the story of the East Libyan cell blackout and subsequent partial reactivation. Section 3 considers the mechanisms of how horizontal media can affect violent collective action in a short theory section. Section 4 describes our data sources and how we constructed a grid level dataset on cell coverage and political unrest in Libya. In Section 5, we cover the details of our identification strategy and empirical model and in Section 6 we present our results.

We initially use a 30-day window on each side of the reactivation and a panel negative binomial estimator, and show that the reactivation reduced violent collective action in the affected area.

Section 7 presents a variety of robustness tests including (a) tightening the window using a grid level fixed effects model, (b) a placebo where we falsely assume that the other cell network was the one reactivated, (c) a change in the source of the data for our dependent variable, (d) a restriction of the sample to just the parts of East Libya that had cell coverage at one point, and finally (e) an alternative way to construct standard errors via a permutation test approach. Altogether the results indicate that overall violent collective action fell, that rebel and citizen initiated violent collective action fell and that fatalities also fell after the surprise reactivation of one cell network in East Libya. Section 8 changes the dependent variable to collective violent action and fatalities, where we continue to observe substantial negative effects. Section 9 concludes.

2. Background

The Libyan Revolution belongs to the broader Arab Spring movement. Aside from regional similarities, this series of protests, riots, and rebellions, share another characteristic: the conjectured role of mobile phone and internet technologies in both spreading dissent and coordinating collective action. Such an argument is defensible, based on the prevalence of Libya's ICT infrastructure and first-hand accounts from dissident organizers. "We use Facebook to schedule the protests, Twitter to coordinate, and YouTube to tell the world," tweeted one Arab Spring activist (Howard, 2011). What's more, Libya's telecommunication infrastructure is impressive. Figures 1 and 2 show the 2G coverage of the Qam and Horrya networks in Libya, the two mobile phone service providers during the revolution. As of 2012, there were 9.6 million cellular subscriptions, which comes to 148.19 per 100 inhabitants. 19.9% has internet access (ITU, 2012). Meanwhile, the United States has 317.4 million cellular subscriptions, 100 per 100 inhabitants, and 86.8% have access to the internet (The World Factbook, 2014). Obviously, telecommunication technologies are pervasive within Libyan society and, if they do in fact enable (or discourage) collective violent action, have ample opportunity to do so in Libya's revolution.

Like many pundits, journalists, and protestors, the potential influence of horizontal media did not escape Muammar Gaddafi, Libya's long-reigning military dictator. Shortly after intensifying protests in Benghazi launched the revolution (February 15th, 2011), Gaddafi's forces severed the fiber optics cable, which connected the East Libya to the Tripoli-based telecom infrastructure. The blackout occurred on approximately March 1st , "somewhere between Misurata and Khomas" (Hill, 2011). Both Horrya and Qam no longer functioned east of this interval. Figure 3 depicts the aggregate mobile phone coverage prior to the blackout, while Figure 4 approximates the 2G access following Gaddafi's blackout imposed on eastern Libya, based on Hill's report.

According to first-hand accounts, this restriction of telecommunications access had a drastic and disastrous effect upon rebels and their ability to coordinate military strategy. "'We went to fight with flags: Yellow meant retreat, green meant advance,' said Gen. Ahmed al-Ghatrani, a rebel commander in Benghazi. 'Gadhafi forced us back to the stone age.'" (Coker & Levinson, 2011). This setback stalled the rebel military advance. Gadhafi's forces even began pushing the rebels back towards Benghazi, the rebel capital.

The growing turmoil drew attention from the international community. Among the humanitarians seeking to provide aid to the fledgling rebel community, was Ousama Abushagur, a 31-year old Libyan telecom executive raised in the US (CL, 2011). Abushagur and his team prepared and delivered the first of many aid convoys on Feb. 23rd, 2011, shortly after protests had erupted into outright rebellion.

While in eastern Libya, the team discovered the lack of cell and satellite phone service, which inspired Abushagur to hatch a plan for the creation of a telecommunication provider entirely independent from Gadhafi's centrally planned mobile phone and internet infrastructure, while exploiting much of the existing, but dormant infrastructure.

Throughout the process of reactivating portions of the Horrya network, Abushagur and his team relied on the international community to provide crucial hardware components and services. Some countries and organizations balked at the proposal to support a rebellion. Abushagur initially planned to acquire hardware from Huawei Technologies, a Chinese telecom company and original contractor for the Libyan networks, but the company ultimately refused, choosing to avoid political affiliations (CL, 2011).

Since the rebels saw little chance of convincing other major telecom companies to sell sophisticated hardware to a regime, which lacked international recognition, they turned to other Persian Gulf nations for assistance. "'The Emirates government and [its telecommunications company] Etisalat helped us by providing the equipment we needed to operate [Horrya] at full capacity,' said Faisal al-Safi, a Benghazi official who oversees transportation and communications issues" (CL, 2011). The equipment arrived in the U.A.E by March 21st, but still required transport to Benghazi to operate the dormant mobile phone network infrastructure in east Libya. To minimize risk of capture or destruction of the newly acquired equipment, Abushagur's team flew the hardware to Egypt, rather than directly to Benghazi. Finally, the rebels launched the new Horrya network on April 2nd. Figure 5 shows the impact of the reactivation. As the reader can see, significant portions of eastern Libya regained mobile phone access. However, all areas previously covered by the Qam network remained without coverage. The rebels lacked vital hardware to reactivate Qam and the state left the network inactive. A timeline of significant and relevant events of the Libyan Revolution can be found in Figure 6.

3. Mechanisms of Horizontal Media's Effect on Collective Violent Action

In this section, we discuss the proposed mechanisms linking horizontal media and violence in the literature and elaborate on our own ideas. It is both useful and humbling to point out at the outset that all of the literature is reduced form and doesn't explicitly model the causal mechanisms. The various papers study the effect that expanding or cutting off or in our case restoring cell phone access has on different types of violence. In other words, we are estimating the net effect of all relevant mechanisms. In our case, we believe we are isolating the overall causal effect, our identification strategy and empirical model (difference-in-differences) is a reduced form approach.

As noted in the introduction. The most well-known mechanism for why cell access affects violence is the argument of PH and Warren that it lowers the cost of organizing violent collective action. We agree that this is undeniably true. It is easier to get a group together through SMS than by going door to door. Ghodes shows that in the Syrian Civil War, cell blackouts are positively correlated with "significantly higher levels state repression". She argues that the state blocks cell communications as a strategy to "target and weaken opposition groups". In other words, she is implicitly taking the same position as PH and Warren. They say access strengthens opposition groups, Ghodes says blackouts hurt opposition groups. It is worth pointing out that Ghodes explicitly studies violence by the state. She is not directly showing that the opposition was weaker, simply that the blackouts and state repression are correlated. PH study the effect of cell phone access on the frequency of organized violent events, not distinguishing between state violence and opposition violence.

SW make the opposite argument. They study the effect of cell phone access on insurgent violence in Iraq and find a significant negative correlation. They argue the cell access allows average citizens to report insurgent locations to state authorities, thus limiting insurgents' abilities to coordinate violent action.

In the Libyan Revolution, some residents of East Libya had cell access, and then it was cut off by Gadhafi presumably to aid his efforts to calm the rebellion, just like Ghodes' case. However, what we study is not that blackout, which seems clearly endogenous with planned state violence, but the unexpected and randomly timed partial reinstatement of cell service to East Libya. Our cell phone access is not expansion of new towers (the location and timing of which could also be endogenous to forces that affect violence), but the reactivation of an existing service. As such, we believe there are other relevant mechanisms for how cell access would affect violence.

First, it could deter excesses by state forces as reporting of atrocities or violence against civilians is more likely if rebels have cell access. But on the non-state actor side, we also think there are additional relevant factors beyond the specified, "easier to organize" mechanism of PH and Warren. For example, easier to organize may not automatically lead to greater violence. Citizens could use the technology to disseminate information about the location of state actors to avoid violent confrontations (kind of a reverse version of the SW argument). In addition, there is a large literature claiming that horizontal media keeps people acting in the virtual world instead of in the physical one. Regaining cell access could reduce on the street group activities that lead to violence. In our case we think this tendency could be amplified by anger against the state for cutting off the service to begin with. Think of that as yet another grievance against the state that on the margin would put more citizens and angrier citizens out on the street. The underlying theme of our proposed mechanisms is that digital expression is a substitute for physical expression and at the margin, (re)introducing digital communication technology can reduce physical expression of political views.

Because we are using ACLED data, our events include riots and protests that do not necessarily create fatalities and we believe it very reasonable to consider that restored cell access could diminish these activities. While our model is at best estimating the causal effect of all mechanisms combined, another advantage of the ACLED data is that for a bit over two thirds of the cases, the instigator is identified. Given that we estimate models explaining the overall level of conflicts, and additional models explaining conflicts initiated by citizens (riots and protests) as well by rebel groups. This is a cleaner test of how cell access affects citizen and rebel actions than what PH are able to do with UCDP data as it does not identify the initiating party.

4. Data

We construct a spatially referenced dataset from a number of sources. For our measure of collective violent action, we use the Armed Location and Event Database (ACLED). ACLED is a conflict-level dataset that contains descriptors of both conflict time and location, among other covariates. Many of the conflicts report the initiating and defending party. From these variables, we identify rebel- and citizen-initiated conflicts, which allows us to test hypotheses about the behavior of political dissidents. During the 2011 revolution, ACLED reports 674 total conflicts. Of these, we identify 158 that fall under the "Riots, Protests, & Rebel-Initiated" subcategory.

PRIO-Grid is a spatially referenced dataset, which we employ as a framework where other geocoded data (i.e. ACLED, 2G data, and blackout areas) can be tied together. Since most of the PRIO-Grid data varies at large intervals or is time invariant altogether, we use PRIO-Grid primarily for the spatial cell structure. The data uses a global raster of .5 by .5 decimal degree cells. At this spatial level, we plot the ACLED data and sum the conflicts which take place in each grid-day observation.

We use journalistic reports to determine the time and location of the blackout. Al Jazeera (Hill, 2012) and Wall Street Journal (Coker & Levinson, 2011) both covered the severance of the fiber optic cable which provided cell phone and internet access to eastern Libya and the ensuing scramble to re-establish a replacement mobile phone network. Hill (2011) gives a fairly precise account of the blackout's location, "between the cities of Misurata and Khomas". WSJ reports the rebels placed the first call on the network on April 2nd 2011, the date of reactivation. Figure 7 maps the 631 PRIO-Grid cells and their categorization into blackout or non-blackout regions, based on the journalistic accounts.

We acquire individual cellular network coverage maps of both the Horrya and Qam network from Collins Bartholomew. Each map displays the precise coverage regions for each, respective network. Figures 2 and 3 shows both network coverage areas in Libya. Our data is, to our knowledge, the first in the literature to include multiple providers, while maintaining the ability to identify coverage regions specific to each provider. To merge the data into the PRIO-Grid framework, we calculate the percentage area of Qam and Horrya network coverage for each PRIO-Grid cell.

There is a total of 632 PRIO-Grid cells within Libya. In the primary specification, we use only observations occurring 30 days before and 30 days after the rebel network's reactivation, as well as the day of the reactivation (i.e. 61 days). The final dataset then contains a total of 38,552 cell-day observations.

5. Empirical Strategy

To identify the causal effect of the 2G mobile phone and internet access, we exploit the sources of exogeneity created in the blackout and ensuing scramble to reactivate available infrastructure. We observe two periods of rapid change in access to 2G mobile phone and internet infrastructure: the initial, region-wide blackout (March 1st), which affects both the Qam and Horrya networks, and the reactivation of the Horrya infrastructure (April 2nd). Given the quasi-experiment's division of Libya into treated and control regions, we implement a difference-in-differences (DID) strategy to estimate the effect of mobile phone and internet access. Because Libya underwent two different instances of

ICT shocks, we are faced with decisions on how to include or manipulate them in our econometric framework to best estimate the causal effect of 2G and internet access on collective violent action.

DID eliminates any time-invariant heterogeneity between the treated (eastern Libya) and control (western) regions. Potential confounders that remain must vary across both time and region. Thus, our choice of which shock to exploit rests on the context of the shock and how well it satisfies the conditions that the DID framework requires. In the initial blackout, state forces select both the timing and placement of 2G and internet access. The entirety of eastern Libya (from Khomas, eastward) experiences the blackout. Gadhafi and his forces did not select this cutoff at random; certain regions were denied access to communication technologies in order to disrupt the organization of protests, riots, and rebel activity. Thus, during the initial blackout period, the determinants of horizontal ICT access are network coverage infrastructure (endogenously selected at installation) and state-selected blackout regions (endogenous). The blackout also begins at a time selected by the state (endogenous). Our primary concern is that state forces severed access in anticipation of increasing violence in eastern Libya, a time- and region-varying determinant of anti-state collective action.

Once the blackout began, rebel forces recognized immediately the crucial role of mobile phone access in organization. If possible, the rebels would have reactivated both networks immediately. The constraints which delayed this process represent the determinants of the *timing* of the access, which, if exogenous, mean that the DID framework can identify the causal effect of horizontal ICT access. Treated and control groups may remain non-randomly sorted; their time-invariant differences accounted for through the model. From the Coker & Levinson (2011), we know that the rebels encountered many delays (bureaucratic, technical, financial, customs, etc.) in the creation of the independent network, but there is little evidence to suggest that these determinants are correlated with political disruption in the region. Instead, the eventual reactivation rested on forgotten, abandoned hardware and the willingness (or unwillingness) of outside, foreign parties to offer technical or financial assistance to the rebel government. Given this evidence, we argue that the reactivation occurred exogenously to other determinants of conflict and so choose to focus our analysis around this event.

It is also important to note that the rebels were only able to reactivate one of the two known mobile coverage providers. So, even within the blackout region, the fraction of each cell with access to any 2G or internet coverage changes. Regions that had access to the Qam network, but not the Horrya, would not see mobile phone access until after the revolution's end. We further exploit this random variation in access by including time-invariant indicators for both networks. The specification is a variation of the difference-in-differences framework:

$$y_{it} = \alpha + \beta_1(2G)_{it} + \beta_2(BR)_i + \beta_3(RP)_t + \beta_4(Horrya)_i + \beta_5(Qam)_i + \beta_6(NATO)_t + \epsilon_{it}$$

The violence in cell i at time t is a function of the 2G variable, which corresponds to the interaction term in a standard DID model; it represents the percentage of the 2G coverage in the eastern Libya, within the Horrya region following the reactivation. In this way, the model departs from a traditional DID in which the interaction term is binary. $(NATO)_t$ is an indicator for the period in which NATO intervened during the revolution. Indicators are also included for the blackout region and reactivation period, $(BR)_i$ and $(RP)_t$, respectively. Unlike a simple DID, we also include indicators for mobile phone access: $(Horrya)_i$ describes the fraction of each cell that lies within the Horrya coverage region. The same is true for the $(Qam)_i$ variable. The addition of these predictors slightly complicates the identification strategy compared to a standard DID model, so we explain how our model identifies the effect of mobile phone access and draw a parallel to the simple DID specification below.

(β_1) is the coefficient of interest in our model. If the specification excluded (β_1), the model would still predict outcomes for all combinations of network access (i.e. Horrya or Qam), region (east or west), and time (pre- or post-reactivation) dummies (hereafter, "*NRT* combinations"). The addition of (β_1) captures how these predictions shift when the eastern Horrya region regained mobile phone access.[1] Although more complex through the addition of a dimension in the data (i.e. Horrya and Qam variables) the underlying identification in our model mimics that from the simple DID specification. What's more, the complexity limits the number of potential confounders. Any heterogeneity that might otherwise explain the coefficient of interest, must vary (simultaneously) at the network-, region-, and time-level, as the telecommunication access does.

To exploit the exogeneity observed in the latter ICT shock, our primary specification restricts the data to observations that occur within a 30-day window on either side of the reactivation. We face a tradeoff in the selection of the time window. Expanding the bandwidth increases the number of available observations, but increases the potential for network-, region-, or time-varying confounders to contaminate estimates of the treatment effect. Given the count nature of the data, we use a negative binomial model and estimate the parameters through iterative maximum likelihood.[2] However, we include OLS estimates of a fixed effects model in Section 7. In the OLS models, we also vary the data specification, by reducing the "*x*-day window" around the reactivation to test whether the results are driven primarily by the data's proximity to the original, endogenous shock to ICT access.

6. Results

[1] In the appendix, we change the value of this variable, allowing it to take a positive value in all *NRT* combinations that have mobile phone access. We count 5 such *NRT* combinations: Qam-West-Pre, Qam-West-Post, Horrya-West-Pre, Horrya-West-Post, & Horrya-East-Post. This allows all relevant *NRT* combinations to contribute toward the estimation of the coefficient of interest. Results remain unchanged and statistically significant.

[2] The data overwhelmingly reject a constant dispersion parameter so we model dispersion via random effects in the negative binomial modelling.

Table 1 presents the results for the primary specification, which restricts the cell-day observations to only those within a 30-day window on either side of the Horrya network's reactivation. Columns (1) and (2) list coefficients for identical models but use both dependent variables: ACLED Conflicts and Riots, Protests, & Rebel-Initiated events respectively. In column (1), we observe the coefficient of interest to be both negative and statistically significant at the 5% level. These findings suggest that the reactivation of the horizontal ICT infrastructure decreased levels of violence.

Coefficients in the negative binomial model (which are provided in Table 1) show the difference in the log of expected counts when there is a one unit increase in *x*. This particular interpretation can be unintuitive, so we instead discuss results using the incidence rate ratios (IRR), which are the original coefficients, but exponentiated. The IRR coefficients represent the factor by which the rate of the *y*-variable is multiplied given a one-unit increase in *x*. For column (1), the IRR coefficient is 0.364. However, our 2G variable is not simply binary, but represents the fraction of the cell's area with mobile phone coverage. Among the 116 affected cells (i.e. those with non-zero Horrya coverage in eastern Libya), the average level of exposure to the Horrya network is approximately 33%. Taking this partial coverage into account, our model predicts the Horrya network's reactivation to have reduced conflicts by 21% on average within the affected regions.[3]

We are particularly concerned with the response of civilians and rebels to changes in mobile phone and internet access, as this allows us to check for evidence of a substitution effect. For this reason, we systematically categorize ACLED events as either riots, protests, or rebel-initiated conflicts and estimate the effects of the reactivation on these two dependent variables. Column (2) gives the estimates of the model coefficients for "Riots, Protests, and Rebel-Initiated" events. We observe a

[3] An IRR coefficient of 0.364 corresponds to an approximately 64% decrease in the rate of the dependent variable given a one-unit change in *x* (i.e. conflict rate is .364 of original predicted rate). We multiply by the average exposure to the Horrya treatment (.33) to obtain the average treatment effect.

larger and statistically significant (i.e. at the 5% level), effect in this data specification. Using the IRR interpretation, the model estimates that the reactivation reduced the rate of conflicts by around 30%.

The estimates suggest that the rate of conflict falls significantly when a region has access (or in this case, regains access) to mobile phone and internet networks compared to regions with previous access where there was no re-activation. The biggest effect is on rebel-initiated events which we categorized as protest, riots, or fighting started by a rebel military group. This finding is consistent with the substitution channel we propose outweighing the lowering cost of coordination channel most discussed in the literature. The channels of horizontal media that reduce collective action, in this context, outweigh the positive effect of coordination cost reduction. Given this statements holds when the data is parsed down to only protestor- and rebel-initiated actions, we interpret this to strongly evidence the channels we propose in Section 3. After the treatment, the cost to organizing collective violent action has been theoretically reduced, but the likelihood of rebel-initiated conflict falls significantly.

7. Robustness

7.1. Restricting the Window

The validity of the identification strategy rests on the exogenous nature of the reactivation's timing. As the data strays further from this point, more doubt is cast on the causality of the findings. The nearness of the initial blackout (March 1st) to the reactivation (April 2nd) exacerbates this concern in our quasi-experiment. We address this issue by restricting the window closer around the reactivation. Note this procedure introduces some tradeoffs. Similar to a regression discontinuity design, a tighter bandwidth reduces potential bias, but reduces the number of observation with which to estimate the parameters of the model, thus increasing the estimated coefficient's standard error. Reducing the time dimension in the negative binomial model is particularly troublesome as it increases the likelihood of convergence errors during the estimation process. To address these two problems, we (1) use a variety of bandwidths and (2) replace the negative binomial model with OLS.[4] In the OLS estimates, we include fixed effects at the PRIO-Grid cell level. The empirical specification remains otherwise unchanged from Section 5.

Table 2 presents the results of the robustness test. We select three, increasingly restrictive, bandwidths. Column (1) uses the same data specification (i.e. 30 day pre- & post-reactivation bandwidth) as the primary analysis, demonstrating that the estimates remain statistically significant, even in a fixed effect, linear model. Column (2) restricts the data further, to a 15-day bandwidth. Despite the growing standard errors, both dependent variables remain statistically significant. We find the coefficient on the "Riots, Protests, & Rebel-Initiated" increases substantially in the data specification. This phenomenon continues in column (3), where, after restricting the data to a 7-day bandwidth, "Riots, Protests, & Rebel-Initiated" estimate grows from -.0216 to -.0371. "ACLED Conflicts" is essentially unchanged and both increase in statistical significance to the 5% level. From this test, we conclude that the Section 6 findings are unlikely to be driven by the data's proximity to the original, endogenous blackout or other, unobservable confounders. Instead, we find statistically and economically significant effects, as the data draws tighter around the unanticipated mobile phone reactivation.

7.2 Placebo Test by Falsely Reactivating "Qam" Network

In this robustness test, we code a false reactivation of the Qam network, Libya's primary, state-controlled mobile phone provider. Like the Horrya network, Gadhafi's forces deactivated Qam's infrastructure east of the region between Misurata and Khomas. However, unlike Horrya, the rebels were unable to reactivate any of the Qam infrastructure.

[4] We were unable to get negative binomial models to converge in these shorter samples.

We manipulate the data to create a false reactivation of the Qam network in rebel-controlled, eastern Libya. The placebo takes place at the same time as the true establishment of the Horrya network, April 2nd. We also repeat the methodology used in Section 5; the data specification, empirical specification, negative binomial model, and maximum likelihood estimation technique remain unchanged. If the tests return statistically significant or coefficients of comparable magnitude, it might indicate that there exist some unobserved, time-varying determinants of collective violent action between regions with mobile phone access and those without. However, if the treatment coefficient is attenuated and insignificant, the test evidences the causality of the true, network-specific reactivation effect.

The results for this placebo test can be found in Table 3, where the placebo treatment coefficient is statistically insignificant for both dependent variables. "ACLED Conflicts" and "Riots, Protests, & Rebel-Initiated" are approximately half as large as their "true treatment" counterparts. Both the magnitude and significance of the coefficients witness substantial decreases relative to the results from the true, Horrya reactivation. The Qam network covers much of the Horrya network, so we expect the coefficients to resemble one another. Despite this correlation, the Qam placebo is far from statistically significant, indicating the only Horrya network witnesses reduced violence after the reactivation.

7.3 Standard Difference-in-Differences for Connected East Libya

In the primary specification, the coefficient of interest measures the deviation between a prediction for the treated *NRT* combination (i.e. generated by the estimated coefficients from *Horrya*, *East*, & *Post-Reactivation*) and the actual, observed counts. Each element in the specification (i.e. *NRT*) contributes to the estimate of the treatment coefficient. In this robustness test, we limit the data to eastern Libya cells with non-zero mobile phone coverage. Using this data, we perform a standard DID between the Qam and Horrya cells. Although this reduces the number of observations, the specification strictly measures how differences in collective action between east Libyan Qam cells and east Libyan Horrya cells changes across the exogenous network reactivation. The former is an appropriate counterfactual for the latter, as both groups were selected for the original 2G network access and into the blackout. Data from cells that lack 2G access or from western Libya are omitted and therefore play no role in the estimation of the treatment group's predicted outcome. Ultimately, this robustness exercise tests whether the inclusion of more dissimilar regions (i.e. non-2G, western Libya) in our empirical methodology drives the estimated treatment effect.

The specification is:

$$y_{it} = \alpha + \beta_1(2G)_{it} + +\beta_2(RP)_t + \beta_3(Horrya)_i + \beta_4(Qam)_i + \beta_5(NATO)_t + \epsilon_{it}$$

The first three parameters of the model (β_1, β_2, and β_3) are the standard parameters in a DID specification. We continue to control for the time-invariant level of Qam coverage and the NATO no-fly period. Similar to the primary empirical test, we estimate the parameters of this specification in a negative binomial model, using iterative maximum likelihood. Table 4 contains the estimated parameters for models of both dependent variables. We observe increases in the both estimated treatment coefficient, relative to the results in Section 6. ACLED conflicts (1) grows from -1.010 to -1.365, and riots, protests, & rebel-initiated conflicts (2) increases in magnitude from -2.550 to -2.941. When measuring the effect of telecommunication access on all ACLED conflicts, we also find the coefficient to be statistically significant at the 1% level.

7.4 Alternative Estimator of Standard Errors

Calculating valid standard errors in DID models is challenging. We use a generalized DID and a negative binomial estimator, but our results do not escape the challenge.[5] In order to address this issue,

[5] Bertrand, Duflo, Mullainathan (2004) were among the first to point out this issue.

some researchers have turned to constructing a distribution of the model's treatment effect through a variant of Fisher's (1935) randomization test, on uncontaminated data.[6] In this subsection, we use data from the Libyan Civil War as the setting for our randomization test.

We repeat the model and estimation procedure described in Section 5 on a series of "placebo" interventions which are placed at every possible day within the civil war. Because the original data specification uses the 30-day window format, we exclude the first and last 30 days of the civil war, as they are unable to match the original data specification. There are a total of 1,266 "placebo" interventions which fit these criteria.[7] After the coefficients are estimated and stored, we append the true estimated coefficient from Section 6. We then replace all coefficients with their absolute value and sort them by magnitude. The *p*-value is the number of placebos with a larger magnitude divided by the total number of placebos.

Figure 4 contains four panels that describe the distributions of the randomization coefficients. Panels A and C plot the absolute values of the "ACLED Conflicts" and "Riots, Protests, & Rebel-Initiated" coefficients. A red, dashed and dotted line indicates the location of the true treatment effect estimate from Section 6. These two panels visualize the *p*-value calculation process of our randomization test. Panels B and D show the behavior of the placebo coefficients as the placebo date varies over the course of the Libyan Civil War. Here, the raw coefficient is used, not the absolute value.

Using the new distribution and standard error estimates, we calculate the effect of mobile phone access on "ACLED Conflicts" to have a *p*-value of .095, while the treatment effect on "Riots, Protests, & Rebel-Initiated" has a *p*-value of .045. From this exercise, we conclude that the significance of our

[6] See for example, Buchmueller et al. (2011), and Bunnenberg & Meyer (2017).

[7] There are 595 days of Libyan Civil War data in ACLED. However, to properly mimic the specification of the primary analysis, where 61-days around the reactivation window are used, we omit false reactivations that do not have 30 days of data preceding or following that date.

primary estimates are likely not driven by overly-conservative estimates of the coefficient of interest's standard error.

8. Collective *Deadly* Action

In contrast to much of the established literature, our analysis uses data on collective action, both deadly and non-deadly.[8] We chose to study the impact of ICT on all collective action, because non-deadly political dissent can meaningfully impact political and economic outcomes. However, the question remains whether mobile phone and internet access affects actions that incur fatalities differently than all actions. In this section, we use data on collective deadly violent action from the Uppsala Conflict Data Program (UCDP) and measure the causal effect of the mobile phone reactivation on this dependent variable.

We also study the reactivation's effect on fatalities, an outcome which has, to our knowledge, gone un-studied in the literature. Doing so could reveal additional insight into the mechanism of the effect. For example, it is possible that when the rebels reactivate the mobile phone network they organize more effective (or larger) events of collective violent action. The count of conflicts may fall while fatalities increase, in which case concluding that mobile phones mitigate violence would be, at best, a half-truth. This section tests for evidence of that possibility.

We employ the same empirical methodology (i.e. generalized DID specification in a negative binomial model) used in Section 6, but the dependent variables are UCDP conflicts and UCDP total fatalities. The results of this estimation procedure are found in Table 5. In column (1) presents estimates of the treatment coefficient that are slightly larger (-1.511) in magnitude and comparable in significance to those from the primarily analysis, found in Table 1. To interpret and compare, we find the mobile

[8] UCDP, the data most often used in the "mobile phones and collective violent action" literature, requires a conflict result in at least 25 deaths to appear in the data. ACLED does not share this requirement.

phone reactivation reduced ACLED conflicts by 21% and UCDP conflicts by 26%.[9] Similarly, column (2) lists the coefficient estimates for the model of total fatalities. The reactivation's effect is significant at the .05 level and is estimated to have reduced fatalities by approximately 30%.

These findings suggest that the negative effect of ICT access on collective action extends to deadly action as well. We also reject the hypothesis that the decreasing number of ACLED and UCDP conflicts is driven by changing "size" of conflicts; there is no evidence that the conflicts become more deadly. In fact, our model estimates a negative treatment effect on total fatalities as well.

9. Conclusion

In this paper, we exploit a plausibly exogenous reactivation of a previously deactivated Libyan mobile phone network. We find that mobile phone access significantly reduced levels of collective violent action within the Libyan Revolution. When categorized data into "Riots, Protests, & Rebel-Initiated" conflicts, the strong, negative relationship between horizontal media and this type of political disorder endures. We also conduct a series of robustness tests, including (1) a restriction of bandwidth around the reactivation used to estimate the treatment effect, (2) a placebo test, in which we falsely reactivate the Qam region, (3) a more traditional DID model using only East Libya regions, and (4) use a variant of the randomization test to construct a more-accurate measure of the DID standard errors; the tests produce no outcomes that jeopardize the results of the original empirical strategy. We also study the impact of the mobile phone and internet reactivation on the count of collective violent action and fatalities. Estimates of the treatment coefficient in both models are statistically and politically significant, suggesting that the "size" of conflicts are not increasing while their total count decrease.

[9] An IRR coefficient of 0.221 corresponds to an approximately 78% decrease in the rate of the dependent variable given a one-unit change in x (i.e. conflict rate is .221 of original predicted rate). We multiply by the average exposure to the Horrya treatment (.33) to obtain the average treatment effect.

This article demonstrates that, within an ongoing conflict, access to horizontal media may play a more complex role than originally thought. Although undeniably crucial in the convenient organization of collective action, horizontal media may also (1) serve as a means for protestors and rioters to avoid conflict with the state, (2) distract political dissidents from contributing more actively in physical protests or rebel activity, and/or (3) reduce dissatisfaction with the regime. In this setting, we find the latter channels stronger than the former.

The quasi-experimental setting limits the external validity of our findings. Through it, we better understand the causal effect of ICT on collective violent action within an ongoing conflict, but not how horizontal media shapes attitudes towards regimes prior to conflict. Future work should attempt to identify the causal effect of ICT on collective violent action in this context, though exogenous changes in mobile phone and internet access are more difficult to find in a peaceful setting.

References

Adena, M., Enikolopov, R., Petrova, M., Santarosa, V., & Zhuravskaya, E. (2015). Radio and the Rise of The Nazis in Prewar Germany. The Quarterly Journal of Economics, 130(4), 1885-1939. doi:10.1093/qje/qjv030

Aker, J. C. (2010, July). Information from Markets Near and Far: Mobile Phones and Agricultural Markets in Niger. American Economic Journal: Applied Economics, 2(3), 46-59. doi:10.1257/app.2.3.46

Blakely, Rhys. 2005. "Terrorists 'threaten' Iraq mobile operators." The Times, July 22. http://business.timesonline.co.uk/tol/business/industry_sectors/telecoms/article546896.ece

Brand, Jon. 2007. "Iraqi Insurgents Target Water and Electricity, But Spare the Cell Phone." Newshour, January 29. http://www.pbs.org/newshour/extra/features/janjune07/infrastructure_1-29.html

Cameron, A. C., & Miller, D. L. (2015). A Practitioner's Guide to Cluster-Robust Inference. Journal of Human Resources J. Human Resources, 50(2), 317-372. doi:10.3368/jhr.50.2.317

Cameron, A. C., & Trivedi, P. K. (1999, June 30). Essentials of Count Data Regression. A Companion to Theoretical Econometrics, 331-348. doi:10.1002/9780470996249.ch16

Cerra, V., & Saxena, S. C. (2008). Growth Dynamics: The Myth of Economic Recovery. American Economic Review, 98(1), 439-457. doi:10.1257/aer.98.1.439

Coker, M., & Levinson, C. (2011, April 13). Rebels Hijack Gadhafi's Phone Network. The Wall Street Journal. Retrieved from http://www.wsj.com/articles/SB10001424052748703841904576256512991215284

Crost, B., Felter, J., & Johnston, P. (2014). Aid Under Fire: Development Projects and Civil Conflict. American Economic Review, 104(6), 1833-1856. doi:10.1257/aer.104.6.1833

Dell, Melissa, Benjamin F. Jones and Benjamin A. Olken. 2014. "What Do We Learn from the Weather? The New Climate-Economy Literature." Journal of Economic Literature, 52(3): 740-98.

Dube, O., & Vargas, J. F. (2013). Commodity Price Shocks and Civil Conflict: Evidence from Colombia. The Review of Economic Studies, 80(4), 1384-1421. doi:10.1093/restud/rdt009

Dunne, J. P., & Tian, N. (2013). Military Expenditure, Economic Growth and Heterogeneity. Defence and Peace Economics, 26(1), 15-31. doi:10.1080/10242694.2013.848575

Esteban, J., Mayoral, L., & Ray, D. (2012). Ethnicity and Conflict: An Empirical Study. American Economic Review, 102(4), 1310-1342. doi:10.1257/aer.102.4.1310

Fearon, James D.. 2010. Governance and civil war onset. World Development Report background papers ; 2011. Washington, DC: World Bank. http://documents.worldbank.org/curated/en/119531468181479612/Governance-and-civil-war-onset

Gohdes, A. R. (2015). Pulling the plug: Network disruptions and violence in civil conflict. Journal of Peace Research, 52(3), 352-367. doi:10.1177/0022343314551398

Goyal, A. (2010, July). Information, Direct Access To Farmers, And Rural Market Performance In Central India. American Economic Journal: Applied Economics, 2(3). doi:10.1596/1813-9450-5315

Hill, E. (2012, April 23). How 'rebel' phone network evaded shutdown. Retrieved June 27, 2016, from http://www.aljazeera.com/indepth/features/2011/04/20114233530919767.html

Hijmans, R. (n.d.). Free Spatial Data. Retrieved August 22, 2016, from http://www.diva-gis.org/Data

Howard, P. N. (2011, February 23). The Arab Spring's Cascading Effects. Retrieved from https://psmag.com/economics/the-cascading-effects-of-the-arab-spring-28575

Hsiang, S. M., Burke, M., & Miguel, E. (2013). Quantifying the Influence of Climate on Human Conflict. Science, 341(6151), 1235367-1235367. doi:10.1126/science.1235367

International Telecommunications Union (ITU). Mobile-cellular subscriptions. Retrieved July 11, 2016, from http://www.itu.int/en/ITU-D/Statistics/Pages/stat/default.aspx

Jensen, R. (2010). The (Perceived) Returns to Education and the Demand for Schooling. Quarterly Journal of Economics, 125(2), 515-548. doi:10.1162/qjec.2010.125.2.515

Justino, Patricia, and Philip Verwimp. 2006. "Poverty Dynamics, Violent Conflict and Convergence in Rwanda." Households in Conflict Network Working Paper 16.

Kassim, S. (2012). Twitter Revolution: How the Arab Spring Was Helped By Social Media. Retrieved June 27, 2016, from https://mic.com/articles/10642/twitter-revolution-how-the-arab-spring-was-helped-by-social-media#.blODQp6a2

Khanna, G., & Zimmermann, L. (2017). Guns and butter? Fighting violence with the promise of development. Journal of Development Economics, 124, 120-141. doi:10.1016/j.jdeveco.2016.09.006

Madden, D., Nolan, A., & Nolan, B. (2005, March). GP reimbursement and visiting behaviour in Ireland. Health Economics, 14(10), 1047-1060. doi:10.1002/hec.995

Munger, Kevin. (2017). Experimentally Reducing Partisan Incivility on Twitter, NYU Working Paper

Muto, M., & Yamano, T. (2009, December). The Impact of Mobile Phone Coverage Expansion on Market Participation: Panel Data Evidence from Uganda. World Development, 37(12), 1887-1896. doi:10.1016/j.worlddev.2009.05.004

Nunn, N., & Qian, N. (2014). US Food Aid and Civil Conflict†. American Economic Review, 104(6), 1630-1666. doi:10.1257/aer.104.6.1630

Pierskalla, J. H., & Hollenbach, F. M. (2013). Technology and Collective Action: The Effect of Cell Phone Coverage on Political Violence in Africa. Am Polit Sci Rev American Political Science Review, 107(02), 207-224. doi:10.1017/s0003055413000075

Raleigh, Clionadh, Andrew Linke, Håvard Hegre and Joakim Karlsen. 2010. Introducing ACLED-Armed Conflict Location and Event Data. Journal of Peace Research 47(5) 651-660.

Rodrik, D. (1998). Where Did All The Growth Go? External Shocks, Social Conflict, and Growth Collapses. doi:10.3386/w6350

Scarborough, R. (2005, March 7). Cell-Phone Technology an Explosive Tool for Insurgents; Rebels Set off Bombs, Send Terror Messages. The Washington Times (Washington, DC). Retrieved 2016, from http://www.highbeam.com/doc/1G1-129818654.html?refid=easy_hf

Schreyögg, J., & Grabka, M. M. (2009). Copayments for ambulatory care in Germany: A natural experiment using a difference-in-difference approach. The European Journal of Health Economics, 11(3), 331-341. doi:10.1007/s10198-009-0179-9

Shapiro, J. N., & Weidmann, N. B. (2015). Is the Phone Mightier Than the Sword? Cell phones and Insurgent Violence in Iraq. International Organization Int Org, 69(02), 247-274. doi:10.1017/s0020818314000423

Stata Annotated Output: Negative Binomial Regression. UCLA: Statistical Consulting Group. from http://www.ats.ucla.edu/stat/stata/output/stata_nbreg_output.htm (accessed August 16, 2016).

Svensson, J., & Yanagizawa, D. (2009, April 5). Getting Prices Right: The Impact of the Market Information Service in Uganda. Journal of the European Economic Association, 7(2-3), 435-445. doi:10.1162/jeea.2009.7.2-3.435

Warren, T. C. "Explosive Connections? Mass Media, Social Media, and the Geography of Collective Violence in African States." Journal of Peace Research 52.3 (2015): 297-311. Web.

The World Factbook: United States. (2016). Retrieved July 11, 2016, from https://www.cia.gov/library/publications/the-world-factbook/geos/us.html

Yanagizawa-Drott, D. (2014). Propaganda and Conflict: Evidence from the Rwandan Genocide. The Quarterly Journal of Economics, 129(4), 1947-1994. doi:10.1093/qje/qju020