From Cell Phones to Conflict?  
Reflections on the Emerging  
ICT-Political Conflict Research Agenda  

Allan Dafoe¹ and Jason Lyall¹  
¹Department of Political Science, Yale University  

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Abstract  
From mobilizing masses to monitoring rebels, information and communication technologies (ICT) can have potentially transformative effects on political conflict. We reflect on the contributions made by the articles of this special issue to the emerging ICT-political conflict research agenda. We focus in particular on the role of elaborate theorizing and alternative hypothesis testing when teasing out the multiple, sometimes cross-cutting, effects of ICT. These effects are likely conditional and dynamic, suggesting the need to specify scope conditions and to engage in close-range analysis of the mechanisms that are producing these effects. We also consider the issue of temporal and spatial dependence, a pitfall of the types of data typically used by ICT studies to date. Finally, we highlight key areas for future research and conclude with a discussion of the policy implications of this research.  

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1 Introduction

Captured and then distributed globally via social media, graphic images from bloody clashes between Twitter-mobilized protesters and government forces in Egypt, Turkey, and Ukraine offer a searing example of technology’s role in facilitating mass mobilization. Scenes from the contemporary battlefield, whether Syrian rebels using Google Maps to correct mortar fire or the Taliban using SMS to narrowcast propaganda in Afghanistan, similarly illustrate the role of modern technology in changing the dynamics of conflict. This democratization of information and communications technology (ICT)—taken here to mean the Internet, cellular phone networks, and social media platforms such as Facebook, Twitter, and Instagram—is transforming the nature of political conflict. And the pace of change is only accelerating: both Facebook and Google are deploying unmanned aerial vehicles designed to deliver the Internet to the last inaccessible populations around the world.¹

We appear, then, to be standing at the threshold of an ICT-driven transformation that will rival the impact of earlier technologies such as the telegraph, newspaper, radio, and television. The articles in this special issue wrestle with the potentially transformative effects of ICT on political conflict, which we define as encompassing both peaceful and violent mobilization against a state’s central or local authorities. As a whole, these articles find that ICT has diverse effects, ranging from the rise of new political actors, identities, and audiences to lowered barriers to (violent) collective action and the altering of the power balance between regimes and rebels. These papers draw on diverse analytical traditions—from crossnational and subnational regressions to surveys and Twitter data—to measure

¹See, for example, Google’s Project Loon (http://www.google.com/loon/) and “Now Facebook Has a Drone Plan,” The New York Times, 4 March 2014.
ICT’s effects. The authors do not, however, necessarily agree on the nature and magnitude of these effects, fueling several important debates for future research.

Our article draws on these papers as a springboard for discussing a broad set of conceptual, theoretical, and methodological opportunities and challenges facing researchers in this emerging ICT-political conflict research program. We first state the case for adopting elaborate theories that (1) make multiple testable implications and (2) explicitly theorize the conditional and dynamic nature of ICT effects (section 2). We then discuss the need for directly testing favored theories against alternative explanations (section 3), including the possibilities of confounding, selection, and measurement biases. Qualitative process tracing can also be used to explore the mechanisms that underpin observed relationships. We also examine a prominent problem endemic to statistical studies of ICT and conflict: temporal and spatial dependence (section 4). We argue that dependence poses a deep problem; we should be cautious in drawing inferences from findings that are not robust to multiple approaches to dealing with dependence. We then explore future directions in the emerging ICT-political conflict research program (Section 5). Finally, we discuss how these findings have important, if often unexplored, policy implications, underscoring the need for interchange of ideas between the technology community, scholars, NGOs, and other relevant parties.

2 Rich Theory: Elaborate, Conditional, Dynamic

Nearly all of the papers in this special issue, as with most of social science, are engaged in causal inference. Persuasive causal inference depends crucially on theory. It tells us what to look for, where, whether we could mistaken in drawing a causal inference, how confident we can be in our inference, and the extent to which causal effects generalize to other domains. To the extent possible, we advocate “rich theory”, by which we mean theory that is more elaborate (has more testable implications), explicit, precise, logically developed, grounded in other well-established findings and intuition from extensive field experience.

What is a “causal effect”? This is harder to determine than might first seem. Even experimentally identified causal effects are near meaningless without theory to interpret them. Heterogeneity of causal effects means that we need theory to tell us how our findings will generalize to other, even only slightly different, empirical domains (Stokes, 2014). Effects typically consist of a multitude of often cross-cutting direct and indirect mechanisms; theory is needed to separate these, interpret our findings in light of each of them, and then draw appropriate generalizations. For example, Shapiro & Siegel (2015) unpack and mathematically model distinct mechanisms by which cell phone coverage affects insurgent violence: it may lower the costs of organization, but also make it easier for the group to be monitored by signals and human intelligence.

Statistical models employed by social scientists typically assume very simplistic temporal and spatial dynamics, namely that effects operate instantly or with a one period lag. But rich causal processes such as those involving ICT can have a variety of interesting dynamics. Effects may be largest at the beginning, or when the technology is first introduced, and then fade over time. Effects may increase as a cumulative process. Effects may not arise

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2Predictive inference is another distinct scientific goal, though pursued much less often in the social sciences. Descriptive inference is generally regarded as valuable to the extent that it contributes to causal or predictive inference.
until some culminating point is reached. Effects may change in sign over time, as actors strategically adapt to their new information environment.

Similarly, spatial dynamics need to be theorized. Weidmann (2015) shows how conflicts may diffuse more as a function of communication “distance” than physical distance or cultural distance. An airstrike in one village may have direct effects in that location, but knowledge of it and any attendant civilian casualties or property damage may spread via cellphones and SMS technology throughout a district, province, or even across the country and beyond. A potentially productive move is towards quantification of space less in terms of administrative boundaries, and more towards fields of cell/television accessibility (Darmofal et al., 2015; Warren, 2015) or networks of communication (Weidmann, 2015; Zeitzoff et al., 2015).

Moreover, causal effects unlikely to be additive and independent in the manner we conveniently assume for our statistical models. Instead, causal effects are conditioned by other factors. This is especially true for ICTs which tend to influence politics through their amplification and suppression of other processes related to communication, coordination, and monitoring. Understanding and explicitly theorizing this conditionality will allow researchers to gain traction on why and when technology does (and does not) matter (and which technologies).

Indeed, many of the authors of this special issue implicitly recognize that ICT effects are conditional. This is perhaps evidenced best by the sheer number of conditioning factors drawn on by these authors to explain observed outcomes. A partial list of these conditioning factors includes: (1) the type and density of networks; (2) ethnicity and other group specific characteristics (size, spatial concentration); (3) preexisting grievances and prior history of group conflict; (4) preexisting institutional capacity to organize, including relative willingness to pick up technology, itself possibly tied to age/generational/socioeconomic factors; (5) technology costs; (6) the prior degree of ICT penetration; (7) citizens’ relative support for government; (8) the distribution and strength of government capacity in a given area (including the entire country); (9) the distribution of agents’ threshold for conversion to a cause; (10) diaspora linkages; and (11) the goals, preferences, and strategies of the actors themselves.

Possibly the most important role for theory in causal inference, however, is in the specification of the set of plausible alternative explanations and their specific observable implications. The next section discusses how greater consideration of alternative explanations presents an opportunity for the future study of ICTs.

3 Alternative Explanations

Causal inference in most of these papers, as is common for much of quantitative social science, largely consisted of estimating average causal effects from observed variation. If we observe a statistically significant association in the direction predicted by our theory, we generally conclude that we have evidence for our theorized causal process. Such an inference depends on two deep conditions. First, we must have confidence that the estimated effect is in fact causal, and thus is unlikely to have arisen by a confounding factor, selection bias, or another problem in the design. Second, even if we are confident that our estimate reveals a causal effect, there must not be other plausible causal explanations that could predict a
similar association. Articulating and ruling out alternative explanations for an empirical finding is a challenge facing most social science. In particular we believe there is a great opportunity in the study of ICTs and political conflict for more explicit theorizing of and evaluation of alternative explanations, including especially the possibilities of confounding and measurement error.

Consider the study of the effect of cell phones on collective violence. A number of scholars have examined whether cellular coverage is associated with violence. Pierskalla & Hollenbach (2013) report a robust finding, based on data of conflict events in Africa, that the “availability of cell phone coverage significantly and substantially increases the probability of violent conflict.” Bailard (2015) examines this association on data from a broader set of countries and based on the ethnic-group as the unit of analysis; she finds a similar positive association. Warren (2015) studies the effect of cell phone coverage on collective violence in Africa, using different data for cellular coverage and a different statistical model; Warren similarly finds a positive association. The three preceding studies each used a similar dependent variable: measures of collective violence based on news reporting of fatality events. By contrast, Shapiro and Weidmann (Forthcoming), looking at Iraq and using “significant activity” reports of attacks against Coalition and Iraqi government forces, find a negative association between cellular coverage and attacks. Could some of these results be attributable to measurement error or other biases? If not, what causal processes should we infer are responsible for these effects?

3.1 Confounding, Measurement Error, Selection

An ever present alternative explanation is that the association is due to some problem with the research design, such as confounding with some other causal process, reverse causation, selection bias, or measurement error. Disentangling causes, effects, confounders, and other sources of bias pose a deep challenge. In the study of cellular coverage, one plausible source of bias is that cellular coverage may make it more likely that a fatal event will be reported by newsmedia, which makes it more likely that an act of collective violence will be coded as occurring in the datasets used in these studies. Cellular coverage could thus affect the reporting of violence, inducing a spurious association.

Measurement error as an alternative explanation for the observed association implies a number of distinct testable implications, which can be used to evaluate it and improve our causal inference. Future work could articulate and evaluate these. For example, the first broad testable implication (T1) is that the measurement error, and therefore the observed association, should become weaker for higher quality data. (T1a) A specific implication is that the association should be less positive for studies based on measures of violence that are less dependent on news-reporting, as is the case for data collected directly by the military such as the “Significant Activity” reports. This measurement error explanation is in fact consistent with Shapiro and Weidmann’s anomalous finding of a negative association (since they used SIGACT data for their DV), and the others’ findings of a positive association (based on news-based data for their DV). (T1b) A second specific implication is that measurement error is likely to be smaller for high-fatality events, since it is reasonable to think that high-fatality events are more likely to be reported irrespective of cellular coverage. We are not aware of any studies that have looked at this association for different fatality levels, though this would be straightforward to do.
A second broad testable implication (T2) is that the measurement error should be more severe, and thus the positive bias in the association stronger, in areas where information about fatalities would otherwise be less likely to be reported by the news, such as rural regions, regions with low population density, and in otherwise inaccessible areas. Bailard (2014) in fact reports evidence consistent with this: the association is more positive in rural regions and for groups that have lower population density. It thus appears that measurement error should be regarded as a plausible explanation for the associations found in this literature.

3.2 Bounding Bias

It is generally not possible to rule out all sources of bias. However, it is always possible to calibrate and bound the bias under different assumptions. Rosenbaum (2010; 2002) advocates a general strategy of sensitivity analysis for observational studies in which some unobserved confounding factor is conjectured to exist; the scholar then examines how strong must this factor’s effect on the causal factor and outcome be to account for the observed association. Manski (1990) recommends a non-parametric form of bounds in which one examines the range of possible causal effects consistent with the data if one imputes the missing potential outcomes in a worst case manner. Unfortunately the assumption free character of these bounds comes at a price: Manski bounds always cover 0 and are completely uninformative when the outcome variable has no restrictions on its range. Though with additional assumptions, perhaps leverage qualitative observation of potential confounders (Glynn & Ichino, 2014), these bounds can be made less conservative.

Another, easy to implement, strategy for calibrating (confounding) bias is employed by Weidmann (2015) in this special issue; this procedure (Alonji et al., 2005; Bellows & Miguel, 2009) provides an estimate of the relative size of unobserved confounding that would be required to explain away one’s result, relative to the change in the estimated association after controlling for the observed control variables. The larger is the ratio of the estimated association, over the change in the estimated association from including control variables, then the greater must be the effect of unobserved confounds on the association relative to the effect of the observed controls in order to explain away one’s finding. In addition to all of the above excellent strategies for sensitivity analysis, one can increase confidence in an inference by showing it to be robust to a variety of plausible specifications, ideally with all results reported in a systematic way such as through techniques such as p-value plots (see Fig 1 in Dafoe et al., 2013) or Bayes Model Averaging (Montgomery & Nyhan, 2010).

Sensitivity analysis will be improved if scholars invest effort in directly studying the kinds of bias that might be present in their studies. Notably in this regard, Weidmann (2015) has examined the possible bias from measurement error by comparing the coding of conflict events in media-based datasets with the more complete data (for insurgent-initiated fatality events) available in the US “Significant Activities” military database (p 13). Weidmann finds that only ≈ 30% of insurgent-initiated fatality events were reported in the news, a magnitude of misclassification error that could generate large measurement biases for factors that are associated with it, such as cellular coverage. Further, consistent with our conjectures in T1b and T2, Weidmann finds that this misclassification error is greater for less fatal events and more spatially remote events. An extension of Weidmann’s analysis could be used to estimate the bias likely to arise in the study of cellular coverage from measurement error.
To summarize, we recommend that future studies of cellular coverage specifically, and ICTs more generally, interrogate plausible sources of bias. This involves (R1) acknowledging and articulating these possible sources of bias, such as confounding, selection, and measurement error, (R2) examining the available evidence in light of these alternative explanations, (R3) performing sensitivity analysis to investigate the magnitude of confounding, selection, or measurement error that would be required to explain away one’s causal effect. In addition, future work would do well to (R4) consciously build research designs that can tease apart, or are robust against, the most plausible sources of bias.

3.3 Alternative Causal Pathways

Suppose now we have interrogated plausible sources of bias, and we are convinced that the estimated associations reflect correctly signed causal effects. We can only infer that this is evidence for our specific preferred causal explanation if there are no other plausible causal explanations that predict a similar sized effect. In general there are multiple plausible causal explanations tying a causal factor to some outcome. Further, there are often multiple subtly different versions of any particular causal explanation. Inference is strongest when we articulate all these different causal explanations, we interpret evidence with respect to each of them, and ideally design our studies to best discriminate between them.

Consider again the case of the effects of cellular coverage. Pierskalla & Hollenbach (2013), Bailard (2015), and Shapiro & Siegel (2015) argue that cellular coverage promotes violent behavior through its facilitation of collective action. Bailard (p 2) exemplifies our above recommendation by additionally theorizing two variants of this explanation: cellular coverage may increase opportunities for and/or increase motivation for collective violence. Similarly, Shapiro and Siegel also exemplify the above recommendation by theorizing through a mathematical model about the countervailing effects of cellular coverage, an effort that could lead scholars to novel testable implications to better tease apart these alternative explanations.

By contrast with the above Olsonian explanations, Warren (2015) offers a novel explanation for the positive association related to the relative ease of constructing inclusive versus divisive appeals. Warren argues that cellular coverage promotes “divisive appeals”, as opposed to the “appeals to national unity” that are more easily produced with technologies such as radio.3

We thus have a number of alternative explanations for the positive association of cellular coverage and collective violence: (E1a) increases in opportunities for collective action, (E1b) increases in motivations for collective action, and (E2) increases in divisive appeals. It is likely possible to think of additional variants of these, and other plausible explanations. An opportunity for future scholarship would be to more consciously search for evidence that could discriminate between these and other plausible explanations.

To do so requires articulating the full set of other plausible explanations, evaluating the evidence in light of these other explanations, and revising research design and searching for evidence (often qualitative) that will most likely discriminate between these explanations. Bailard (2015), for example, looks to see whether the association varies by characteristics of the ethnic groups in ways, she argues, that will help discriminate between her two ex-

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3Consistent with this, Warren (2015) also finds that penetration of radio transmission capability is negatively associated with collective violence.
planations. Future work could search for other distinctive implications of E1a and E1b: changes in motivation could reveal itself in surveys of reported grievances by potential insurgents, changes in opportunity for collective action could reveal itself in different patterns of collective action such as quicker mobilizations or more spatially diffuse actions. Warren’s explanation is especially distinctive, and could be specifically evaluated by looking for its distinct implications, such as perhaps changes in measures of individuals’ identity and a time-lag in the observed effects consistent with the slower process of identity transformation.

Causal process observations (CPOs) can play an important role in adjudicating between competing accounts, particularly if they rely on different mechanisms. Typically qualitative in nature, a CPO is an observation “that provides information about context, process, or mechanism” (Brady & Collier, 2010, 2). These observations help flesh out whether the proposed causal pathway(s) between independent and dependent variables are both present and responsible for generating the observed effect. Though rarely used in civil war studies to date (Lyall, 2014; Wood, 2008), a reliance on CPOs and the process tracing of how causal effects are actually produced would address several gaps in existing studies.

Indeed, the discussion of technology’s effects in these articles sometimes verges on the invocation of magic: ICTs appear to knock down hitherto unscalable barriers to collective action, heighten the lethality of insurgent organizations, sharpen grievances, and do so on a (very) compressed time scale, often measured in weeks or months. At times, one wonders how insurgents (or governments, for that matter) made due without cellular phones or the Internet when prosecuting their wars. These findings would lead us to conclude that peaceful and violent movements alike have increased in number over time. Yet the macrolevel trends for both types of campaign suggest that their frequency has actually declined since the 1980-89 era (Chenoweth & Stepan, 2011, 7-8).

More generally, close-range study of how these actors actually use ICTs is essential if we are to parse out why we observe these trends between the introduction of ICTs and violence (to cite one example). How exactly do cellular phones and social media lower obstacles to collective action? What separates organizations that adopt these practices from those that do not? Are ICTs substitutes or complements for earlier forms of information-sharing and mobilization? Do organizations experiment by switching between different types of ICT at different stages of a protest or conflict cycle? How important is the content of the messages sent via these media? Can we close the gap between the introduction of ICTs and the individual-level decisions to join a movement, provide tips to local authorities, or take up arms against the state? How exactly do identity considerations such as coethnicity condition the flow of information? It strikes us that there is enormous opportunity to marry econometric research designs that identify effects with fieldwork and historical qualitative research that explore the individual- and group-level mechanisms underpinning the association between independent and dependent variables.

As is common for the complex phenomena of interest to social scientists, for most of the studies in this special issue there were a variety of plausible causal explanations for their estimated causal effect. We recommend scholars make their theories elaborate and precise, and articulate possible alternative explanations; consciously build research designs so as

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4On this last point, see Lyall et al. 2014, who find that coethnicity shapes the willingness to support information-sharing about insurgent behavior among Tajik and Pashtun respondents in Afghanistan.
to tease apart the most important alternative explanations; examine evidence in light of these alternative explanations, including especially causal process observations; and perform sensitivity analysis under different assumptions about the alternative explanations.

### 3.4 Integrating Levels of Analysis

A specific opportunity for this emerging research agenda is to try to reconcile competing empirical findings from macrolevel (e.g., crossnational) and microlevel (e.g., subnational) studies. All of the crossnational studies that look at the effects of a communication technology (Bailard, 2015; Warren, 2015; Weidmann, 2015) in this special issue converge on the same central finding, namely, that the introduction of new ICT (at least in the 2000-10 time frame) is associated with an uptick in insurgent violence in the immediate aftermath. With the exception of Warren (2015), this empirical claim is underpinned by a belief that the (net) effect of ICT’s introduction is to lower barriers to collective action, resulting in more attacks.

The picture is more mixed at the microlevel, however. Several scholars (Gohdes, 2015; Cutler et al., 2015) suggest that net gains in relative power accrue to the state, not insurgents. It is also unclear whether ICTs always bolster recruitment by reducing collective action problems: Darmofal et al. (2015) find that media (in this case, television) had no discernible effect on protest participation or size in East Germany, while Hassanpour (2015) finds some evidence that exposure to foreign media was associated with (some) increased turnout in Egypt.

The next wave of research on these topics will benefit from attempting to reconcile these findings within a coherent theoretical framework. Macrolevel theories should be able to “scale-down” to the subnational level, while microlevel theories should “scale-up” to address broader patterns at the regional or national level. How should macrolevel theories “scale-down?” Researchers testing their arguments at the crossnational level could specify subnational indicators—perhaps at the group or regional level—in order to identify what types of evidence would be consistent with the proposed theoretical framework. This moves scholars away from a single, typically aggregated, dependent variable, and toward a range of different observable implications, quantitative and qualitative, to distinguish competing arguments.

Similarly, microlevel research designs should also consider how local effects “scale-up” to produce aggregate effects at the macro-level. While microlevel studies generally yield more credible causal inferences, the often-unique nature of their settings, conditioning factors, and data collection efforts can frustrate efforts at drawing even limited generalizations. Yet this should be the goal. Explicitly theorizing about how local processes scale up to broader levels will help us adjudicate between competing explanations of these broader patterns.

### 4 Dealing with Dependence

Most of the papers in this special issue involved data with some temporal and/or spatial structure. Some of the units of observation were: the ethnic-group-year (Bailard, 2015), the country-year (Weidmann, 2015), the (German) county-day (Darmofal et al., 2015), the newspaper-day or country-day (Baum & Zhukov, 2015), (Syrian) days (Gohdes, 2015), and a continuous space model (Warren, 2015). In general, the smaller the scale of the unit
of observation the less plausible is the crucial condition for statistical inference that the observations are independent. We tend to think that countries are more independent than country-years, than country-months, than country-days. Though even the largest scales—countries or country-years—are likely to be dependent.

To address this dependence, every paper engaged in causal estimates adopted some form of correction. In addition to the respective control variables, some papers controlled for unit-specific effects assumed to come from a normal distribution (“random effects” or the “conditional frailty model”) (Baum & Zhukov, 2015; Darmofal et al., 2015). All papers controlled for a function of lags of the outcome to address temporal dependence (Bailard, 2015; Weidmann, 2015; Baum & Zhukov, 2015; Gohdes, 2015; Darmofal et al., 2015) and/or spatial dependence (Warren, 2015; Baum & Zhukov, 2015). One study employed an estimator for the standard errors that allows for clustering within spatial units (Weidmann, 2015).

Given how substantial can be the temporal and spatial dependence in these domains, it is worth asking how confident we can be about the appropriateness of the adopted solutions. This issue is not well appreciated by social scientists, and is developed in more detail in separate work of ours (Dafoe, 2014). The answer depends on how confident we are about the causal process that generated the dependence. For each of the above techniques there exist data generating processes under which the technique is an appropriate correction, and there exist data generating processes under which the technique is not appropriate. In particular, “corrections” that condition on a lag of the outcome can induce bias into estimates. Rather than improving estimates, these models can in fact worsen estimates. Every study cited in this section controlled for a function of lags of the outcome, so this issue is worth exploring.

When will conditioning on a function of lags of the outcome harm estimates? Dafoe (2014) considers this question for nonparametric estimation; for parametric estimation the answer can be more complicated since it will depend on the nature of the functional misspecification. For non-parametric estimation, a bias will be introduced from conditioning on lags of the outcome to the extent that there exist (C1) common causes of the causal factor of interest and the lagged outcome, and (C2) unobserved persistent causes of the outcome. Since (C2) is almost always true, the key condition is (C1). (C1) is more likely to be true to the extent that lags of the causal factor of interest are not adequately controlled for (most studies do not control for lags of the causal factor of interest) and to the extent that the current research design fails to completely address confounding.\(^5\) On the other hand, **not conditioning** on lags of the outcome will induce a bias, roughly speaking, to the extent (C3) that there is reverse causation (the lagged outcome affects the causal factor of interest) or (C4) there is auto-causation (the lagged outcome affects the outcome).\(^6\) To justify basing one’s inference on estimates that always condition on lags of the outcome, therefore, requires a substantive argument against unobserved common causes of the causal factor of interest and the lagged outcome (C1), and in favor of reverse causation (C3) or auto-causation (C4). These substantive arguments are almost never made.

Absent confident knowledge about the causal processes generating dependence in their

\(^5\)This is because causes of the causal factor of interest and the outcome are also likely to be causes of the lagged outcome, making C1 true.

\(^6\)Formally, Dafoe (2014) notes three possible sources of bias present when not conditioning on lags of the outcome. Each of these requires either reverse causation or auto-causation, though only one source of bias requires both.
data, the above argument suggests that scholars should not rely just on models that control for lags of the outcome, nor should they rely just on models that don’t control for lags of the outcome. Instead we recommend that scholars routinely estimate models with and without lags of the outcome. If one’s inference is robust then we gain confidence that the estimate is not being driven by bias. In addition, for some data generating processes it is the case that the expected values of the estimates with and without controls for lags of the outcome will bound the true value.\(^7\) If an inference changes after controlling, or removing, lags of the outcome, then that tells us that the causal process of interest is deeply temporal/spatial, that we cannot interpret the estimates without making assumptions about the causal processes that generate the temporal and spatial dependence, and that progress on this question could come about from better understanding of these temporal and spatial processes.

Another strategy for avoiding relying on strong causal assumptions for our inferences is to change the unit of analysis to one that is thought to have less dependence (Bertrand et al., 2002, §IV.C.). Baum & Zhukov (2015) exemplify this strategy. After estimating a series of complex econometric models based on newspaper-days \((n \approx 680,000)\) and country-days \((n \approx 33,000)\), they also evaluate their question using just countries as the unit of observation \((n \approx 100)\). While the latter specification has a much smaller nominal \(n\), it also avoids assuming independence across newspapers and across days. They find continued support for their finding on this more aggregated empirical domain, increasing our confidence in the validity of their result.

In conclusion, dependence is a symptom of potentially deep problems in our estimation of causal effects. Most econometric solutions to this problem assume a specific causal process that generates the dependence. Scholars rarely articulate these assumptions, let alone defend them. If our assumptions are mistaken, then our “corrections” can induce bias and other problems. We recommend ensuring that results are robust to different specifications, specifically models with and without lags of the outcome, and, when possible, examining more aggregated data for which dependence is generally less of a problem. Non-robust results are not necessarily wrong, but our confidence in them is limited by our understanding of the causal processes that generated the temporal or spatial dependence.

5 Future Directions

Taken together, these articles illustrate the promising research agenda that lies at the intersection of ICT and conflict studies. We consider four broad future directions for this agenda here, though this list is not exhaustive.

First, many questions remain about how ICT affects the onset, duration, and termination of civil wars and mass protest movements. Take, for example, recent research that links civil war onset to group-based political and economic inequalities (Cederman et al., 2013, 2010). The steady proliferation of cheap telecommunications and alternative media outlets might exacerbate these grievances by publicizing the extent of these inequalities, helping foment opposition to the regime even among dispersed populations. Yet these same processes might actually deter groups from challenging the state since improvement in monitoring capabilities (on both sides) might dispel misperceptions about the relative balance of power.

The diffusion of cellphones and social media outlets such as Skype, Twitter, and Face-

\(^7\)An example of such a data generating process is one in which all effects are monotonic additive positive.
book may facilitate updating among combatants about the likelihood of victory once war has begun. Bargaining theories of war, for example, suggest that combatants update beliefs about war outcomes through fighting. Combat reveals the true distribution of military strength, enabling combatants to strike bargains that were impossible given prewar disagreements about the relative power balance (Fearon, 1995; Powell, 2006; Reiter, 2003). Increased reliance on ICT by each side might therefore shorten wars by providing faster, potentially more accurate, information about battlefield progress. Similarly, these technologies might shorten wars by facilitating the defection of state supporters, including military forces, leading to regime collapse as narrowcasting messages to particular audiences becomes a cheap, effective tool of war-fighting.

At the same time, however, these same technologies might prolong the war. Defection from the state may bolster rebel capabilities, enabling them to survive state campaigns of violence that would have otherwise destroyed them. Moreover, ICT can promote the diffusion of knowledge and skills that empower the rebels relative to the state by increasing their combat power, including the sophistication and lethality of their tactics. It is likely that ICT also facilitate fund-raising from external powers, again lengthening the war through the provision of badly-needed funds and recruits.

Given the potentially disruptive effects of ICT on state-insurgent power balances, it is likely that patterns of war outcomes will also be affected. For example, it is possible that ICT’s effects on insurgent combat capabilities might help explain the marked recent downturn in the ability of states to defeat insurgent foes (Lyall & Wilson, 2009). Indeed, while ICT may bolster some state capacity (e.g., intelligence-gathering through signals intelligence, or SIGINT), the net effect of these technologies may be an uptick in insurgent resiliency. Yet the advent of ICT innovations, including satellite imagery, might also help mitigate, if not resolve, commitment problems that often sabotage peace settlements (Walter, 2002, 1997) by improving the monitoring of would-be spoilers. ICTs might therefore have effects both on the nature of the victory and the durability of the postwar settlement.

Second, battlefield and mass protest dynamics offer fertile ground for studying ICT effects. It is likely, for example, that ICTs will alter processes of recruitment and group cohesion for protest and insurgent organizations. These effects may be ambiguous, however. The skillful use of ICT may help leaders exercise greater supervision over their recruits, easing principal-agent problems and, in so doing, improving organizational effectiveness. ICTs may also aid leaders in fostering group solidarity—based on the judicious manipulation of existing or manufactured grievances—that supplant material incentives (Weinstein, 2007). At the same time, ICTs may also promote fractionalization if they lower the start-up costs of creating new organizations and of attracting like-minded recruits.

Group fractionalization may in turn have important consequences for rebel governance in civil wars (Arjona, 2010; Staniland, 2012). To cite one example, the introduction of ICT may embolden civilians to challenge rebel governance structures, if only indirectly. For example, ICT and related media platforms may allow civilians to better publicize rebel excesses and to punish them by providing tips to state authorities. Yet monitoring capabilities can cut both ways: insurgents may also be better positioned to control population movements and actions, including exploiting SIM cards that log phone numbers called. Moreover, insurgents’ taxation could also be strengthened by ICT by improving their ability to record data and rapidly disseminate it among insurgent cadres, say about crop yields or market conditions. Cell
phones and SMS messaging also provide a cheap means for disseminating rebel propaganda to large audiences with little or no state inference.

More generally, variation in the adoption and use of these technologies by different organizations remains an important empirical puzzle. Insurgent organizations can even display considerable spatial and temporal variation in their engagement with ICT. The Taliban in Afghanistan offer an illustration of this within-group variation. In some cases, local Taliban have destroyed cell phone towers, either out of security considerations or from economic motives such as efforts to coerce telecommunications companies. In other regions, the Taliban have moderated their stance, allowing cell phone towers to operate during the day if they are turned off nightly. The Taliban has evolved a fairly sophisticated media strategy that partly hinges on the use of these same towers for SMS messaging, particularly about ISAF or ANSF-inflicted civilian casualties. In still other locations, the Taliban do not interfere with these towers at all. Instead, they have struck local bargains with these companies to receive funds — often in the guise of grants, work programs, or “protection money” — in exchange for their cooperation in leaving the transmitters alone.8

Seizing on these twin research opportunities will require movement away from the prevailing unitary actor assumption in current theorizing, however. Nearly all of the articles in this special issue rely on theories or models that invoke “governments,” “insurgents,” or “communities” as if they represented single actors. While the parsimony and modeling simplicity of such a stance is productive, it comes at the cost of foreclosing study of intra-group process over time. This is probably too high a price to pay if the majority of ICT effects are found within groups, not across them.

Third, substantial room still exists to draw on additional forms of technology to strengthen causal inferences through careful measurement strategies. Satellite imagery, for example, is rapidly changing our understanding of conflict dynamics—particularly the linkage between food production, scarcity, and violence—by greatly improving our ability to collect data passively in denied areas.9 In addition, mixing methods to account for varying strengths and weaknesses of each approach should be pursued. Is attitudinal data collected via Twitter and SMS-polling comparable to randomized household surveys, for example? Are event data captured via Ushahidi and similar platforms comparable to data collected using more traditional newspaper records?

Finally, but perhaps most importantly, scholars should embrace conditional and elaborate theories that are tested competitively with other explanations. Rather than rely on a single measure of “effect,” researchers should propose multiple observable indicators for their arguments and then test for congruence across these measures. Both qualitative and quantitative data and methods should be marshaled in this endeavor. Crafting research designs that draw on data over longer time periods (i.e. panel data) will also enable scholars to move closer to causal inference.10 Similarly, exploiting (hopefully exogenous) subnational

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9See, for example, the Satellite Sentinel Project in Sudan and South Sudan (http://www.satsentinel.org/) and Hsiang et al. 2013.

10Lengthening time frames would also allow scholars to investigate the long-term effects of technology on war and, conversely, of war on technology (which is often cited as a key driver of a country’s economic development).
variation in the introduction or withdrawal of ICT would also highly beneficial, as would field experiments that explicitly test the mechanisms underpinning ICT’s presumed effects. Scholars may also be able to draw on “Big Data” from Twitter, phone call logs, and other media to engage in out-of-sample testing (Hirose et al., 2013). Predictive prowess is now (re)emerging as an important consideration of an argument’s validity, and scholars working with large datasets—sometimes recording millions of observations—are well-positioned to forecast.

6 Policy Relevance

Nearly all of the articles in the special issue avoid discussion of the policy relevance of their findings. Yet the combination of the rapid diffusion of ICTs and their presumed conflict-inducing properties raises a host of policy-related questions with few easy answers. Should ICT promotion continue to be a central plank of development agencies—notably the World Bank and the US Agency for International Development (USAID)—if it plausibly increases conflict and instability? What if these ICTs actually facilitate the ability of dictatorial leaders to monitor their populations or subvert initial democratic openings? If ICTs do lower barriers to collective action for terrorists and insurgents, then what level of monitoring (and outright disruption) is warranted in a bid to thwart these challenges?

More specifically, there are at least two ways in which further research on ICT and conflict can affect policy debates. Scholars can, for example, harness new ICTs to facilitate near-real time data collection from various social media platforms in areas with poor coverage by traditional media. Great strides have already been made in documenting human rights abuses from open-source media in conflicts as diverse as Syria, Liberia, and South Sudan. Attention has also increasingly turned to the use of predictive models that estimate the likelihood of disruptive events such as famine.

Though these efforts are not without shortcomings, their ability to collect data from denied areas helps throw into relief changing patterns of behavior that are often missed by data collection methods that rest on traditional media. The more these innovative ICTs can be marshaled for these reporting purposes—and the more arresting their visual display—the more these approaches can inform policy discussions about the nature of the problem at hand and possible options for mitigating it. Landman & Crook (2015) in this special issue provide an example of just this kind of innovation.

There is room for more active engagement with the policy community, however. To date, the World Bank, USAID, and other organizations have spent hundreds of millions of dollars creating “informational infrastructures” in (post)conflict countries such as Afghanistan, Liberia, and Pakistan. These efforts, including programs to create national telecommunication and payment systems (such as M-PAISA/M-Pesa), have been deemed successful and adopted as models in other countries. If the research examined here is correct, then these


12 The Famine Early Warning Systems Networks (FEWS NET) is one example of a platform that integrates multiple streams of data to generate predictions about acute food insecurity (http://www.fews.net/).

same programs may actually exacerbate violence. Scholars are well-placed to identify and test these potentially cross-cutting effects. Doing so would be a boon to policymakers and would help bridge the artificial divide between “development” and “security” scholars that has frustrated study of the interplay between ICTs, development, and security.

7 Conclusion

Seizing the opportunity created by the remarkable pervasiveness of modern information and communications technology, these authors collectively have charted the broad outlines of an exciting research agenda on the links between ICTs and political conflict. Our ambition here has been modest: to encourage these authors and others working in this emerging field to adopt several “best practices” that will further our understanding of how, and when, ICTs affect patterns of mobilization and violence. Embracing elaborate theories that specify multiple indicators of “effects,” and then crafting research designs that facilitate competitive hypothesis testing, is one avenue for ensuring that progress is cumulative. In particular, important theoretical and empirical gains can be made by examining the mechanisms that underpin these processes at close-range. Paying close attention to the demands of causal identification, including addressing spatial and temporal dependence, is also likely to pay dividends. Finally, a closer dialogue with Silicon Valley and technology users “in the field” — whether citizens, rebels, governments, companies, or NGOs — would help ground our studies in real-world processes and problems. The end goal should be a research program that not only sheds light on how technology affects these processes but also informs policy discussions about political conflicts and their possible resolution.
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