

From cell phones to conflict? Reflections on the emerging ICT–political conflict research agenda

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Abstract

From mobilizing masses to monitoring rebels, information and communication technologies (ICT) are transforming political conflict. We reflect on the contributions made by the articles of this special issue to the emerging ICT–political conflict research agenda, highlighting strengths of these articles, and offering suggestions for moving forward. Elaborate theory is crucial: it informs our standards of evidence, our choice of statistical models, our tests of competing theories, and our efforts to draw appropriate generalizations. Qualitative data is often neglected as a source of evidence, especially for evaluating the many competing mechanisms in this literature. Alternative explanations for results should be taken seriously, especially more mundane ones like confounding, measurement, and selection biases. We discuss in detail the risk that measurement bias could account for the prominent association between cellular coverage and (reported) conflict, and recommend several ways of evaluating and bounding this risk. We discuss the problem of temporal and spatial dependence for statistical inference – a problem that is often present for studies of ICTs – and point out that methodological solutions rely on (rarely stated) causal assumptions. Finally, we highlight key areas for future research, recommend a commitment to transparency best practices, and conclude with a discussion of the policy implications of this research.

Keywords

communication, conflict, information technology, methods, theory

Introduction

Distributed globally via social media, graphic images of 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 of politics that will rival the introduction 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 include both peaceful and violent challenges to state authority. As a whole, these articles find that ICT has diverse effects,

¹ See, for example, Google's Project Loon (<http://www.google.com/loon/>) and 'Now Facebook has a drone plan', *New York Times* 4 March 2014.

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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 articles draw on diverse data sources to measure ICT's effects, including cross-national and subnational data, surveys, and Twitter data. The authors do not, however, necessarily agree on the nature and magnitude of these effects, outlining the broad contours of a productive research program.

In this article we review some of the contributions of these articles and the broader opportunities and challenges facing researchers in the emerging ICT–political conflict research program. We first direct attention to the value of rich theory: theory that articulates many testable implications, that explicitly theorizes the conditions that amplify or attenuate the various cross-cutting effects, and that embraces the dynamic nature of ICT effects. We highlight the gains that can be achieved through the direct testing of favored theories against alternative explanations with research designs that are capable of addressing possible confounding, selection, and measurement biases. In addition, qualitative process tracing can be used to explore and substantiate the mechanisms that underpin observed relationships. We then examine the deep statistical challenge facing studies of ICT and conflict about how to deal with temporal and spatial dependence; we remind scholars of the role of unexamined causal assumptions underlying widely used methods and recommend ensuring that inferences are not sensitive to unjustified assumptions. We then turn to a discussion of future directions that the ICT–political conflict research program could take: greater examination of how ICTs affect the dynamics (onset, duration, and termination) of conflict; a focus on the micro-processes of battlefield and protest behavior; further utilization of the new forms of data becoming available, including satellite imagery and Twitter sentiment analysis; a commitment to best practices in making research transparent and replicable. Finally, we discuss how these findings have important policy implications, underscoring the need for interchange of ideas between the technology community, scholars, NGOs, and other relevant parties.

Rich theory: Elaborate, conditional, dynamic

Social scientists and policymakers are concerned about causal questions. Does cellular phone infrastructure promote development and security? How would the Arab Spring have been different had Twitter and SMS not been available? Our ability to answer these questions depends crucially on theory. Theory tells us what to look

for and where, whether we could be 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. Our answers to these causal questions will be most confident and useful when we can rely on 'rich theory', by which we mean theory that is elaborate (has many testable implications), explicit, precise, logically developed, informed by intuition from extensive field experience, and grounded in the body of empirical findings.

Determining what constitutes a 'causal effect' can be more difficult than it might seem at first. Even experimentally identified causal effects are of little value without theory to interpret them and inform our generalizations to other empirical domains (Stokes, 2014). Effects are the product of multiple mechanisms, often cross-cutting and indirect; theory is needed to separate these, interpret our findings in light of each of them, and then draw appropriate generalizations. For example, Rød & Weidmann (2015) theorize that the effect of the expansion of Internet access on support for autocratic regimes depends on the resources available to the government. Shapiro & Siegel (2015) unpack and mathematically model distinct mechanisms by which cell phone coverage affects insurgent violence: cell phone coverage is expected to lower the costs of organization, but also makes it easier for the group to be monitored by signals and human intelligence.

Statistical models employed by social scientists typically assume 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 complex, 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 until some culminating point is reached. Effects may change in sign over time, as actors strategically adapt to their new information environment. The most appropriate (and powerful) statistical lenses for observing effects will be calibrated to the specific temporal dynamics implied by theory.

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 can spread via cell phones 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 (Crabtree, Darmofal & Kern, 2015; Warren, 2015) or networks of communication (Weidmann, 2015; Zeitzoff, Kelly & Lotan, 2015).

Moreover, causal effects are 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. Further, ICTs are often deployed strategically to promote certain interests, and thus the effects of ICTs could depend on which groups are in power (Rød & Weidmann, 2015). Understanding and explicitly theorizing this conditionality will allow researchers to gain traction on why and when specific technologies do and do not matter.

Indeed, many of the authors of this special issue have already begun to theorize and investigate the conditionality of ICT effects. A partial list of conditioning factors cited by these authors includes: (1) the type and density of networks; (2) ethnicity and other group-specific characteristics (size, spatial concentration); (3) pre-existing grievances and prior history of group conflict; (4) pre-existing institutional capacity to organize, including relative willingness to pick up technology, itself possibly tied to age/generational/socio-economic 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.

Alternative explanations

Causal inference in most of these articles involved estimating average causal effects from observational data. 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, measurement bias, 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 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 and evaluation of alternative explanations, including especially the possibilities of confounding and measurement error. To illustrate this opportunity, we devote the remainder of this section to discussion of some puzzles in 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: 207) 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 & Weidmann (2015), 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.

Now the fact that Shapiro & Weidmann (2015) find a negative association need not cast doubt on studies finding a positive association, since these studies differed from each other in many respects, including the empirical domain. Causal effects may simply be heterogeneous (though if they are, it would be valuable to understand why). We propose that an alternative explanation for this pattern of results is that measurement error induces a positive bias for studies that use news reports for measuring conflict. Specifically, cellular coverage will make it more likely that a fatal event will be reported by news media, 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 should thus affect the *reporting of violence*, inducing a positive bias. This alternative explanation for the observed association

implies distinct testable implications. Future work could articulate and evaluate these, thereby increasing our confidence in our causal inference.

For example, if measurement error is inducing a positive bias to these estimates, then the observed association should become weaker whenever the measurement error is expected to be weaker, such as in domains with higher quality data. Specifically, (T1) the association should be less positive for studies based on measures of violence that are less dependent on news reporting, as would be the case for data collected directly by the military such as the 'Significant Activity' (SIGACT) reports. In fact, the results to date are consistent with this explanation: Shapiro & Weidmann find a negative association using SIGACT data based on a dependent variable, whereas the other studies using news-based dependent variables find a positive association.

A second testable implication (T2) arises from the fact 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 third possibility (T3) is suggested by Weidmann's (forthcoming; Table II & III) finding that location and casualty news reporting is less accurate for battle events than for one-sided attacks. If the cellular coverage association is biased by measurement error, then the association should be more positive for the more noisy battle events than for one-sided attacks.

A last testable implication (T4) 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 (2015) reports evidence consistent with this: the association is more positive in rural regions and for groups that have lower population density.

In fact, Weidmann (2014) looks at some of these proposed testable implications and others, and finds substantial evidence consistent with severe measurement bias. By comparing media-reported violence in Afghanistan to more complete SIGACT data, Weidmann shows that a conflict event is more likely to be reported by international news where there is cellular coverage, when the event involves high casualties, and when the event occurs close to a town or city. Applying the design of Pierskalla & Hollenbach (2013) to Afghanistan, Weidmann shows how cellular coverage has a positive

association with news media reported events, but a zero association with SIGACT recorded events, consistent with the former being driven by measurement bias. Finally, Weidmann shows how the Pierskalla & Hollenbach (2013) results become weaker when subsetting on more severe conflict events (greater fatalities), with the association nearly disappearing for the most severe conflicts. Given that measurement error seems a plausible explanation for the associations found in the literature, future studies will strengthen our causal understanding by directly evaluating whether, and to what extent, measurement error is biasing estimates, as well as other possible sources of confounding, measurement, and selection bias.

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 (2002, 2010) 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 be the effect of this confounding factor on the causal factor and outcome in order 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.

Another, easy to implement strategy for calibrating (confounding) bias is employed by Weidmann (2015) in this special issue. This procedure (Alonji, Elder & Taber, 2005; Bellows & Miguel, 2009) provides an estimate of the size of unobserved confounding needed to account for one's result, relative to the change in the estimated association from controlling for observed control variables. Specifically, the scholar will estimate a 'full model' with all appropriate control variables, and a restricted model without some or all control variables. The ratio $\hat{\beta}_F / (\hat{\beta}_R - \hat{\beta}_F)$ is reported, where $\hat{\beta}_F$ and $\hat{\beta}_R$ are the estimated coefficients for the parameter of interest from the full and restricted models. A large ratio arises if we observe a big association (large $\hat{\beta}_F$) and/or the inclusion of the set of control variables does not change the estimated association by much (small $\hat{\beta}_R - \hat{\beta}_F$). Therefore, larger ratios imply that for unobserved confounding to account for the association, they would have to have a larger effect on the estimated association, relative to the effect of the observed control variables on the estimated association. 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 *p*-value plots (see Figure 1 in Dafoe, Oneal & Russett, 2013) or using Bayes Model Averaging (Montgomery & Nyhan, 2010).

Sensitivity analysis can be further improved by leveraging insight into the likely character and magnitude of possible biases. Notably in this regard, Weidmann has examined the kinds of measurement error (forthcoming) and measurement biases (2014) present in media-based datasets by comparing their coding of conflict events to those available in the US 'Significant Activities' military database, finding that only $\approx 30\%$ of insurgent-initiated fatality events were reported in the news, a magnitude of error that could generate large measurement biases for factors that are associated with it, such as cellular coverage.

To summarize, we recommend that future studies of ICTs, and studies of cellular coverage in particular, interrogate plausible sources of bias. 'Interrogation' in this sense involves first acknowledging possible sources of biases from confounding, selection, or measurement error, and then performing sensitivity analyses to determine the magnitude of errors that would be necessary to account for one's estimated causal effect. In addition, future work would do well to consciously build research designs that can tease apart, or are robust against, the most plausible sources of bias.

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. To draw the additional inference that this is evidence for our specific preferred causal explanation we must rule out other plausible causal explanations that predict a similar sized effect. In general there are multiple causal explanations tying a causal factor to some outcome. Further, there are often subtly different versions of any particular causal explanation. Inference is strongest when we articulate all plausible relevant causal explanations, then interpret evidence with respect to each of them while (ideally) designing our studies to 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 the 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 & Siegel use a mathematical model to theorize about the countervailing effects of cellular coverage, an effort that could lead scholars to novel testable implications to better tease apart these alternative explanations.

The above explanations emphasize the effects of cellular coverage on the costs and viability of collective action. By contrast, Warren (2015) offers a novel explanation for the positive association related to the relative ease of constructing integrative versus divisive appeals. Warren argues that cellular coverage promotes 'divisive appeals', as opposed to 'appeals to national unity' that are more easily produced with technologies such as radio. Consistent with this, Warren (2015) also finds that penetration of radio transmission capability is negatively associated with collective violence.

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 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 explanations. Future work could search for other distinctive implications of E1a and E1b: changes in motivation could reveal themselves in surveys of reported grievances by potential insurgents, for example, while changes in opportunity for collective action could be measured using different patterns of collective action such as quicker mobilizations or more spatially diffuse actions. Warren's explanation is especially distinctive, and could be evaluated by looking for its distinct implications, such as 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 responsible for generating the observed effect. Though rarely used in civil war studies to date (Lyll, 2014; Wood, 2008), a reliance on CPOs and process tracing of how causal effects are actually produced would address several gaps in existing studies.

In particular, 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). At times, the depiction of technology's effects in these articles borders on 'magical', with new technologies suddenly and effortlessly lowering barriers to collective action that once stood immovable. These findings would lead us to conclude that peaceful and violent movements alike have increased in number over time. Yet the macro-level trends for both types of campaign suggest that their frequency has actually declined since the 1980–89 era (Chenoweth & Stepan, 2011: 7–8).

A host of questions thus remain unresolved. 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?² It strikes us that there is enormous opportunity to marry econometric research designs that identify effects with fieldwork and historical qualitative research to explore the individual- and group-level mechanisms underpinning the association between independent and dependent variables.

Integrating levels of analysis

A specific opportunity for this emerging research agenda is to reconcile competing empirical findings from macro-

level (e.g. cross-national) and micro-level (e.g. subnational) studies. All of the cross-national studies that look at the effects of communication technology on conflict (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 micro level, however. Several scholars (e.g. Gohdes, 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; for example, Crabtree, Darmofal & Kern (2015) find that media (in this case, television) had no discernible effect on protest participation or size in East Germany.

The next wave of research on these topics will benefit from attempting to reconcile these findings within a coherent theoretical framework. Macro-level theories should be able to 'scale-down' to the subnational level, while micro-level theories should 'scale-up' to address broader patterns at the regional or national level. How should macro-level theories 'scale-down'? Researchers testing their arguments at the cross-national 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, micro-level research designs should also consider how local effects 'scale-up' to produce aggregate effects at the macro level. While micro-level 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.

Dealing with dependence

Most of the articles in this special issue involved data with some temporal and/or spatial structure. Some of the

² On this last point, see Lyll, Shiraito & Imai (2014) who find that coethnicity shapes the willingness to support information-sharing about insurgent behavior among Tajik and Pashtun respondents in Afghanistan.

units of observation were: the ethnic-group-year (Bailard, 2015), the country-year (Weidmann, 2015; Rød & Weidmann, 2015), the (German) county-day (Crabtree, Darmofal & Kern, 2015), the newspaper-day (Baum & Zhukov, 2015), the country-day (Baum & Zhukov, 2015; 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 article that estimated causal effects adopted some form of correction. In addition to the respective control variables, some articles controlled for unit-specific effects assumed to come from a normal distribution ('random effects' or the 'conditional frailty model') (Baum & Zhukov, 2015; Crabtree, Darmofal & Kern, 2015). All articles conditioned on a function of lags of the outcome to address temporal dependence (Bailard, 2015; Weidmann, 2015; Baum & Zhukov, 2015; Gohdes, 2015; Crabtree, Darmofal & Kern, 2015; Rød & Weidmann, 2015) and/or spatial dependence (Baum & Zhukov, 2015; Crabtree, Darmofal & Kern, 2015; Warren, 2015). Some studies employed an estimator for the standard errors that allows for clustering within spatial units (Weidmann, 2015; Rød & Weidmann, 2015).

Given how substantial the temporal and spatial dependence can be 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. Our confidence in our inferences should depend on how confident we are about the causal process that generated the dependence. For each of the above techniques there exist causal processes under which the technique is an appropriate correction, and there exist causal processes under which the technique is not appropriate for identification of causal effects. In particular, methods that condition on a lag of the outcome can induce bias into estimates that were otherwise unbiased. Rather than improving estimates, these methods can actually worsen them. Dafoe (2014a) and Glynn & Quinn (2013) discuss the kinds of causal processes for which conditioning on a lag of the outcome is appropriate or not appropriate for (non-parametric) causal identification; Keele & Kelly (2006) and Wilson & Butler (2007) evaluate similar issues for certain parametric estimators, which generally rely on similar causal assumptions as well as assumptions about functional form

such as that all covariates and disturbances have linear additive effects.

Absent confident knowledge about the causal processes generating dependence in their data, scholars should not rely on any one correction. Instead scholars can estimate models with and without lags of the outcome; if the inference is robust we gain confidence that the estimate is not being driven by bias. When an inference is sensitive to the temporal or spatial specification then we know that our inference depends on the assumptions underlying our specification and that future progress on this question will come about from better understanding of these temporal and spatial processes.

Another strategy for reducing our reliance on strong causal assumptions is to change the unit of analysis to one that is thought to have less dependence (Bertrand, Duflo & Mullainathan, 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 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, substantially increasing our confidence in the validity of their result.

Dependence is a symptom of potentially deep problems in our estimation of causal effects. Temporal and spatial dependence tend to be especially prevalent in studies of information and communication technologies because ICTs shape the temporal and spatial dynamics of political behavior, often in difficult to measure ways. 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 and spatial dependence.

Future directions

Taken together, these articles illustrate the promising research agenda that lies at the intersection of ICT and

conflict studies. We consider here four future directions for this agenda, 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, Gleditsch & Buhaug, 2013; Cederman, Wimmer & Min, 2010). The steady proliferation of cheap telecommunications and alternative media outlets can 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) could dispel misperceptions about the relative balance of power.

The diffusion of cell phones and social media outlets such as Skype, Twitter, and Facebook 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 could shorten wars by facilitating the defection of state supporters, including military forces, leading to regime collapse as it becomes less costly to narrowcast messages to particular audiences.

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 ICTs 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 (Lyll &

Wilson, 2009). Indeed, while ICTs 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, 1997, 2002) by improving the monitoring of would-be spoilers. ICTs might therefore have effects both on the nature of the victory and on 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 interference.

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 have evolved a fairly sophisticated media strategy that partly hinges on the use of these same towers for SMS messaging, particularly about the civilian casualties inflicted by the International Security Assistance Force (ISAF) or Afghan National Security Forces (ANSF). 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.³

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 intragroup 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.⁴ 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.⁵ Similarly, exploiting (hopefully exogenous) subnational variation in the introduction or withdrawal of ICT would also be 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, Imai & Lyall, 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.

We would be remiss, however, if we ignored how these new sources of ICT data also pose new problems for replication. The rise of Facebook, Twitter, and other social media platforms means that required data are often collected and disseminated by private (commercial) providers rather than traditional state or NGO sources. Issues of data privacy, proprietary software and data collection techniques, and non-disclosure agreements can all conspire to reduce data access and transparency. In one notable example, Pierskalla & Hollenbach’s (2013) central finding cannot be directly replicated since the data collector (GSMA) prohibited further dissemination of cell phone coverage data. More generally, prominent studies drawing on provider data cannot be replicated due to access restrictions (see, for example, Eagle, Macy & Claxton, 2010; Simini et al., 2012). These developments are worrisome. Funders, journal editors, and scholars have increasingly come to appreciate the importance of transparency and replicability for science (see Dafoe, 2014b; Miguel et al., 2014; and footnote 6). ICT scholars should engage the emerging best practices, which explicitly consider issues of confidential data and proprietary techniques.⁶ Such a

³ See, for example, ‘Cell carriers bow to Taliban threat’, *Wall Street Journal* 22 March 2010 and ‘Afghan Taliban use phones for propaganda’, *BBC News* 30 March 2012.

⁴ See, for example, the Satellite Sentinel Project in Sudan and South Sudan (<http://www.satsentinel.org/>) and Hsiang, Burke & Miguel (2013).

⁵ Lengthening 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).

⁶ Statements of best practices in social science are emerging from groups such as BITSS and the Center for Open Science: <http://bitss.org/2014/11/06/creating-standards-for-reproducible-research-overview-of-cos-meeting/> and <http://osc.centerforopenscience.org/2014/11/12/facilitating-radical-change/>.

commitment to transparency best practices, as exemplified by the policies of the *Journal of Peace Research*, will increase the credibility of the results produced by this fast-moving field.

Policy relevance

Curiously, 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.⁷ Attention has also increasingly turned to the use of predictive models that estimate the likelihood of disruptive events such as famine.⁸

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.

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.⁹ Many large-scale national development programs now explicitly identify telecommunications as a key engine of economic growth and political progress. And the argument is a compelling one: shared communications can create new market opportunities, bolster perceptions that the national government is effective, and create a greater sense of collective identity. The globalization of communications technology – by promoting trade, investment, empathy, and reducing misunderstandings – seems to have played a similar role in underpinning more peaceful relations between states (Pinker, 2011: § 4–5).¹⁰

If, however, the research presented by this special issue (Bailard, 2015; Warren, 2015; Weidmann, 2015; Rød & Weidmann, 2015) and elsewhere (Pierskalla & Hallenbach, 2013) is correct, these same media technologies are generating a host of unintended consequences. Above all, forging a new national telecommunications network may accelerate, rather than dampen, instability and even armed challenges against the state. There are a host of potential mechanisms at work here. Modern social media can exacerbate perceived inequality among groups within society, fueling anger that could motivate individuals to seek redress violently. In addition to facilitating mobilization, these technologies may also speed the adoption of violent tactics by frustrated groups seeking to circumvent state censorship and disruption. New connections to the outside world foster the exchange of ideas, funds, and technology, which can create more lethal organizations.

The introduction of new technologies will have cross-cutting effects, and these will often go unrecognized by implementing agencies. It is uncommon, for example, for these informational infrastructure programs to assess their impact in terms of violence or, more broadly, political conflict. Similarly, a narrow focus on counter-programming against violence and extremism tends to focus solely on traditional indicators – poverty and literacy rates, for example – and fails to recognize that macro-level processes influenced by new technologies may be adversely affecting programming. More generally, there is a need for program evaluations that

⁷ See, for example, Ushahidi (<http://www.ushahidi.com/>) and First Mile Geo (https://www.firstmilegeo.com/case_studies/aleppo).

⁸ 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/>).

⁹ The World Bank has spent \$50 million since 2011 in Afghanistan alone on its ICT Sector Development Project. See: <http://www.worldbank.org/projects/P121755/afghanistan-ict-sector-development-project?lang=en>.

¹⁰ We thank Nils Petter Gleditsch for raising this point.

explicitly measure the effects of technology on collective action, including the possibility that the positive association between technology and political conflict is an artifact of better reporting rather than a causal relationship.

Prevailing conventional wisdom that the empowering effects of these new technologies largely accrue to activists, not states, may also need to be revisited. In fact, as Ghodes (2015) demonstrates, Syrian authorities have turned their informational infrastructure (specifically, access to the Internet) into a weapon that degrades insurgent capabilities. China, too, has managed the threat of Internet-fueled opposition handily so far, allowing citizens to express grievances – and thus provide useful information about potential regime weaknesses – while preventing collective action through a rigorous application of censorship (King, Pan & Roberts, 2013). Such efforts are not the sole preserve of strong states: Azerbaijan, for example, has also proven adept at using social media to discredit would-be opponents, contributing to their marginalization and the absence of antiregime protest (Pearce, 2014). It remains an open question whether these technologies translate into greater political freedom or even strengthened civil societies.

Scholars are well placed to identify and test these potentially cross-cutting effects. This is especially true since the study of technology and its effects crosses a number of artificial divides that separate scholars (themselves often divided) and practitioners. Development and security are not separate issues but rather interlinked processes, especially when considering the policy impacts of new technologies. This special issue thus outlines an agenda for both scholarly and applied research that can help identify the conditions under which technology does, and does not, influence political conflict in strong and fragile states around the world.

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 to highlight several recommendations that we believe will advance 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 knowledge 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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