

News Media Reporting Patterns and our Biased Understanding of Global Unrest*

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News reports of political violence are systematically compiled into large global conflict-event datasets used by academics, governments, and international organizations. These datasets present opportunities to examine the micro-dynamics of conflict but are often systematically skewed. We compare various news-report based datasets to high quality administrative records from Afghanistan, Iraq, the Philippines, South Africa, and Syria to identify sources of systematic missingness in the former. We identify underreporting biases related to violence intensity, weaponry, target, perpetrator, and non-deadly violence. In a large replication exercise, we show that media-based data fail to uncover the results reported in leading economics/political science journal articles.

“If one Iraqi person killed, not a story. Five, not a story. Ten, maybe. Twenty or more, that is probably a story... If an American soldier is killed, that’s a story.”

former New York Times reporter

“There is a lot of violence that happens and it can’t all be written about because not everything should be written about. We are not just a chronicle of all the violent acts that took place over the course of a day.”

current staff writer at The New York Times Magazine

INTRODUCTION

From Taliban bombings of civilian targets to election riots in South Africa, details of individual incidents of political violence and unrest published by the news media have been systematically compiled into large conflict event datasets. Datasets constructed largely or wholly from news reports track multiple forms of political violence including terrorist activity, civil war violence, attacks against peacekeeping forces, drone strikes, suicide bombings, and more (Shaver et al. 2021). These datasets have become workhorse tools for academic researchers to understand political violence around the world (Berman et al. 2018). Citations of journal articles introducing these datasets and their respective codebooks number in the thousands, while similar methodological approaches are now being used to track non-violent activity during periods of conflict (e.g. Chenoweth et al. 2019). These datasets have become particularly valuable because there exist few alternatives for scholars interested in making comparisons across countries.

The scope of these datasets – tracking millions of incidents over decades – also make them attractive to policymakers. Media-based datasets have been widely used and funded by government and non-governmental organizations, orienting decision-makers within the United Nations, the World Bank, and the U.S. Departments of Defense, Homeland Security, and State, among others.

While they provide an unprecedented opportunity to learn about the micro-dynamics of conflict and social unrest, much remains unknown about the quality of these datasets, and whether potential sources of error and systematic missingness may skew our understanding of global conflict and unrest. For example, if there exists an endogenous relationship between violence and government efforts to limit reporting, data drawn from media reports may represent a systematically biased sample of violent events. These concerns are not merely hypothetical. As protests broke out in Kazakhstan in early 2022, the government imposed an internet blackout, directly limiting news reports as violence intensified (Anna 2021). As violence raged in Ethiopia’s Tigray region, the government likewise imposed an internet blackout, inhibiting the flow of information from the region (Solomon 2020a). Fearing that reports of violence would deter voting, the Afghan government restricted media reporting on Taliban attacks ahead of the 2009 presidential elections (Tran

2009). Governments elsewhere have responded to violence with measures that produce electricity shortages, limiting the media's ability to operate: responding to attacks by Hamas, Israel cut off fuel shipments to the Gaza Strip's sole power plant (al Mughrabi 2020).

The aforementioned examples are the tip of the iceberg: bias can and does arise for many more reasons, raising fundamental questions about whether media-based datasets accurately reflect patterns of violence, or whether they are systematically biased. We use the term "bias" to describe any deviation away from some base rate or underlying trend or pattern. Our emphasis is on deviations from "perfect" reporting, in which all relevant events that occur in a conflict or setting of social unrest are documented. We identify a non-exhaustive list of biases, including systematic under-reporting relating to violence intensity, electricity outages, weaponry employed, target, perpetrator, and deadly versus non-deadly violence used, among others. If the news media systematically under-reports particular types of violent events, then media-based conflict event datasets will do the same. Variables generated from the datasets may thus suffer from significant biased sampling, undermining the validity of inferences using them.

As Figure 1 shows, countries with higher levels of political violence are also more likely to suppress media reporting; the relationship is prominent even though the violence data are derived primarily from news reports and therefore likely represents a lower bound on the extent of this bias.

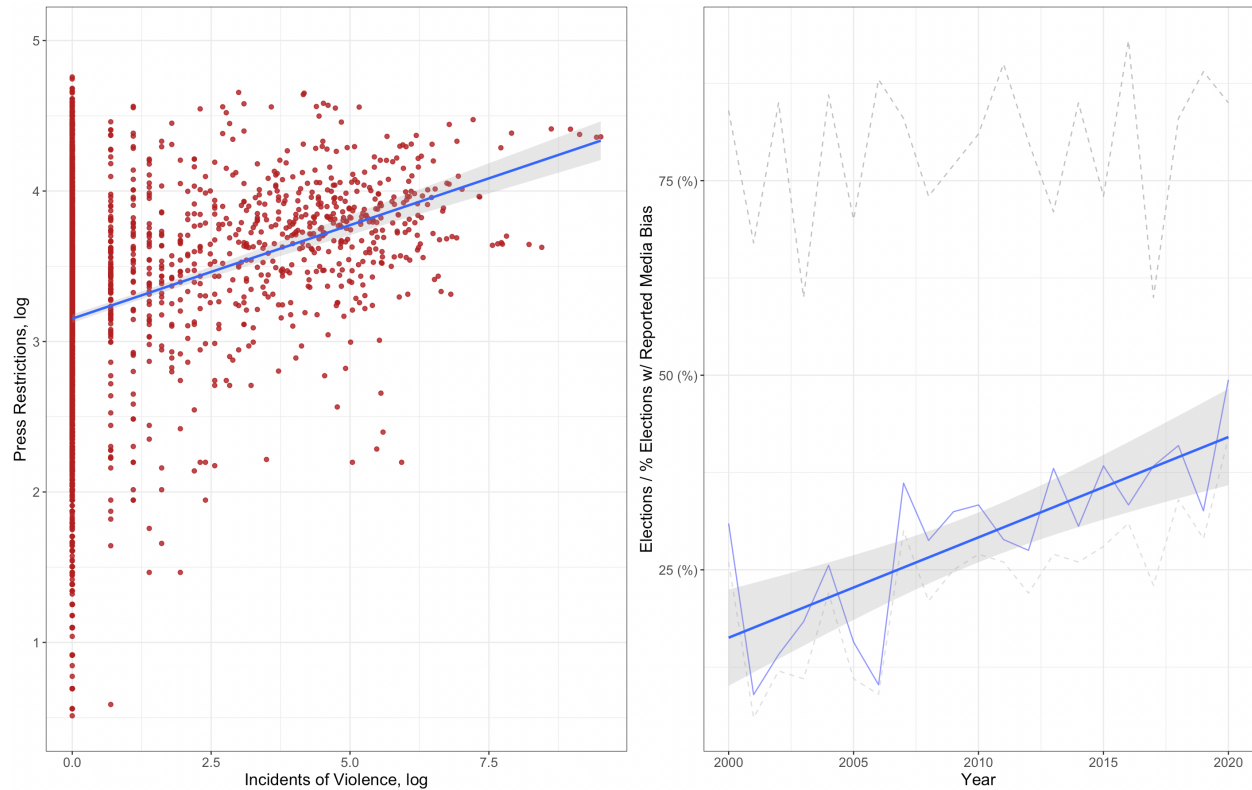


Figure 1: Comparison of incidents of violence per country per year (derived from news reports) against measures of press restrictions (left panel) and number of elections where media bias has been reported over time (right panel). Sources are UCDP and Reporters Sans Frontières (left panel) and National Elections Across Democracy and Autocracy (NELDA) (right panel).

From our analysis of the existing literature on media reporting biases and through one dozen in-depth interviews with journalists who have reported on conflict for major news organizations and wire agencies such as the *British Broadcasting Corporation (BBC)*, *BuzzFeed*, *France 24*, *The New York Times*, *Public Radio International*, *Reuters*, and *The Wall Street Journal* – and whose reporting experience/expertise on violent unrest includes Afghanistan, Burkina Faso, China, Colombia, El Salvador, Iraq, Israel, Libya, Mexico, the Palestinian Territories, the Philippines, South Korea, Sudan, Syria, Venezuela, and Yemen – we focus on two general sources of media under-reporting. First, the news media are constrained in their ability to report on particular types of violent events. When this results in systematic under-reporting of particular types of events, this produces *capability bias*. Second, the news media actively choose not to report certain events. When this results in systematic under-reporting of particular types of events, this produces *editorial bias*. We then derive observable implications of these biases and generate evidence for and against them by comparing conflict event data coded using media reports against data gathered by security forces across five separate cases: the U.S. led wars in Afghanistan and, separately, Iraq, insurgency in the Philippines, contemporary protest and riot activity in South Africa, and the international

campaign against ISIS in Iraq and Syria.

Our findings are fourfold. First, media-based datasets are significantly more likely to under-report incidents of violence and civil unrest as conflict intensity or unrest increases. Second, media-based datasets significantly under-report non-lethal attacks by combatants – although such violence may still result in significant cost and/or be strategically important. Third, bombings and explosions, which are more likely to leave lasting signatures, are more likely to be reported by the news media, despite the fact that they are often less prevalent than attacks employing small arms. Fourth, the probability of an event’s inclusion in media-based datasets depends upon who the attack targeted and who perpetrated it.

Does this matter? We conduct a large “reverse” replication exercise, assessing whether findings published in leading economics and political science journals using high-quality administrative datasets of political violence can be recovered using existing media-based data. In most cases we find that they cannot. Of the total 271 tests we conduct, only 81 (or 30%) generate results in which both the direction of the coefficient and statistical significance is maintained. The biases inherent to media-based datasets are so significant that they generate findings inconsistent with those produced using more comprehensive security force data sources.

Given the importance of news media in informing our understanding of global politics, our paper raises and begins to answer fundamental questions about how our understanding of violence and politics more broadly may be systematically skewed. Media-based datasets are used to study multiple facets of political life, and our findings raise concerns about what inferences we can draw from such sources. In the specific case of political violence, we demonstrate how patterns of systematic missingness in media-based datasets produce divergences from actual patterns of violence. By cataloguing and quantifying these biases, we offer estimates about the likely frequency of particular events, even when access to more “perfect” data is infeasible, opening up possibilities for researchers to “de-bias” their datasets or probe the sensitivity of their findings. Finally, our findings carry implications for policymakers, given that institutions such as the U.S. Department of State, the Political Instability Task Force, the United Nations, and the World Bank all use and in some cases fund media-based datasets. Our findings should prompt a rethinking of policymakers’ reliance on such datasets to determine programmatic priorities and assess program results.

SOURCES OF MEDIA BIAS IN CONFLICT REPORTING

Scholars have long studied which events become news and why (Galtung and Ruge 1965). Attention has recently turned to sources of reporting bias on conflict, typically focused on the causes of systematic under-reporting of particular types of events, and less often on over-reporting. Kalyvas (2004) describes why civil war violence is more likely to be reported in urban settings.

von Borzyskowski and Wahman (2019) study incidents of violence close to elections and find that media based data under-report events in more remote areas and where violence was less expected to occur. Weidmann (2016) finds that conflict events in areas without cellular reception are more likely to be under-reported. Donnay and Filimonov (2014) find that media report based data are more likely to under-report events resulting in fewer casualties. Media-based data from the South Sudanese civil war demonstrates significant casualty under-reporting (Dawkins 2020). Baum and Zhukov (2015) find that the media tend to report large-scale events and that under-reporting varies by government regime type: under authoritarian governments the news media are less likely to report on violence related to state legitimacy, such as protests and collective action by regime opponents (Baum and Zhukov 2015: p. 1).

Other scholars have focused on accuracy. Weidmann (2015) finds that events observed by fewer individuals are less likely to be accurately reported. Drawing on interviews, Hoiby and Ottosen (2019) show that news media outlets are increasingly reluctant to deploy reporters to conflict zones, potentially affecting reporting quality: “staff reporters are protected and held back from conflict hotspots [and] freelancers, stringers or local journalists and fixers are left to take the highest risk” (Hoiby and Ottosen 2019: p. 83). Absar et al. (2015: p. 10) identifies a number of potential media reporting biases, including selection, significance, omission, proximity/access, event size, source, and media fatigue biases.

We build on this work, grouping together some biases that the aforementioned authors identify while adding others. We suggest two broad categories of under-reporting bias. First, news media may suffer from *capability bias*, which we define as under-reporting of individual conflict events given restrictions on reporters’ ability to report on a conflict. Second, news media may suffer from *editorial bias*, which we define as under-reporting of individual conflict events when reporters’ or news organizations’ choices of what to report either explicitly or implicitly excludes particular types of events. Below we describe specific sources of each category of bias.

CAPABILITY BIAS

Various factors impede journalists from reporting on events during conflict or social unrest. These are listed in Table 1. The first is violence intensity, which may affect the news media’s ability to report on events by both threatening the safety of media personnel (e.g. reporters, stringers, or locals who provide reporters with leads) and placing demands on scarce resources available to news media to pursue leads.

Even if journalists were able to operate freely during periods of intense fighting, they may nevertheless lack the time and financial resources to report on the multitude of events occurring. Practical limitations on journalists’ ability to pursue leads are likely to bind beyond some level

of violence. Our interviews with journalists highlight the importance of logistical obstacles and cost considerations in generating differences in reporting both *across* and *within* countries. As a former journalist of a major wire agency described, reporters' access in Yemen and Syria has been far more limited than in Ukraine, with implications for what is covered: "we never got a visa [to Yemen] and we never got to go. [T]his [was] the beginning of the ... Saudi intervention in Yemen. So it was a very important bloody period a lot of people were ... being killed, and ... it was very difficult to report on."¹ Another interviewee (#12) describes very similar dynamics with respect to differences between Libya, on the one hand, and Afghanistan, Iraq, and Syria, on the other:

[With] Iraq and Afghanistan, the threshold to actually gain access to these [countries] were a lot higher because you need to be embedded; because it was very expensive; because you needed networks, and so on; [With] Libya, you could just fly to Cairo or to Tunis, and you could be there very quickly.

The big frustration with Syria is that at some point we started being cut off [from the country] and apart from speaking with refugees or people who were in and out at the border, we were losing any visibility as to what was going on inside...

Obstacles to reporting within a given country can be just as significant. Interviewee (#3) describes the difficulty of reporting on events in Gaza when compared to the West Bank, where access is much easier:

[I]f you're not a recognized journalist with the Israeli system, you can't just go there ... you have to go across [three] checkpoints, [Israeli, Palestinian Authority, and Hamas] ... [I]f you were a correspondent based in Jerusalem or Tel Aviv, you couldn't just go to Gaza for a day ... you had to go through logistics to get in, to stay over ... [You have to] get a Hamas visa; you have to get a fixer, who then vouches for you... then you have to pay a fixer every time you come in. You have to pay for a place to stay; you have to pay for all of this travel ... if you're going for three days, you must have done enough reporting for four or five stories to make the whole thing pay for itself. So it's like an investment.²

A second factor is observability. Particular types of events are less likely to be observed by members of the media, either directly or indirectly (e.g. via civilian reporting), and are therefore less likely to be reported. Violence is less observable in remote regions with limited media presence that frequently are the sites of violence. Similarly, violence carried out in restricted areas—including rocket or mortar attacks (indirect fire) targeting secure military installations—may go

¹Interviewee 3.

²Ibid.

unobserved by the media, unless such actions are reported by combatants themselves. From China to Colombia to Iraq, multiple interviewees described related issues. As a major wire service journalist in Colombia told us, “[t]here are certainly places where we don’t go because they are just not safe for my team,” and, in particular, they would not travel to areas under insurgent (FARC) control “without a lot of prior assurances from that group.”³

Yet, just as combatants can hinder reporting, they can facilitate it as well. A particularly striking example comes from Libya, which, as one journalist described, “generated so much reporting [because] access was easy as long as you were [on the] rebel side of the conflict... [Those on the rebel side] were very willing to take you everywhere you wanted... Like, for them, there was a clear equation between: the more media attention there is, the more the West is going to help...”⁴

Constraints on observability may apply directly to media outlets themselves, or indirectly to sources of information upon which the news media rely. For instance, some outlets solicit “tips” from the public, with some events less likely than others to be reported. This might occur because members of the public do not observe the violent event in the first place (e.g. attacks carried out in remote locations where civilians are not present) or because the public chooses not to report certain events to the media (e.g. civilians fearing retaliation).

EDITORIAL BIAS

Editorial bias refers to biases stemming from the autonomy that the media have in choosing which events to cover. We consider various reasons why journalists and their employers may opt not to report particular events, although our list is not exhaustive. Sources of editorial biases are summarized in Table 2.

Summary bias is likely to arise as violence in a given conflict intensifies. Amidst intense violence, a news report may simply explain that a significant number of attacks occurred at a particular time or place. Consider, for instance, how journalists Yee and Saad (2019) described

³Interviewee 4.

⁴Interviewee 12.

Table 1: Hypothesized Capability Biases

Type of Capability Bias		Attacks...	Example
Violence Intensity	<i>Safety</i>	...restrict the movement of fixers, stringers, international correspondents. ...exceed time, financial resources required to capture them all.	"The volatile security situation in Iraq means that few journalists are willing to travel to the country and report on one of the most important stories of our time" (Graff 2007).
	<i>Resource Scarcity</i>		"Most newspapers covered [the Iraq War] episodically. This was partly because reporting wars is always very expensive and is particularly costly in Iraq and Afghanistan because of the need to pay security companies" (Cockburn 2010).
Observability	<i>Remoteness</i>	...occur along border regions where journalists and/or civilians are unlikely to observe them.	"[T]he government of Sudan has intensified its deliberate bombing attacks on civilian and humanitarian targets... [yet] it is believed that additional bombings go unreported because international aid workers are generally absent from the Nuba Mountains and areas east of Khartoum" (USCIRF 2001).
	<i>Accessibility</i>	...occur in areas in which access by journalists and/or civilians is restricted (e.g. rocket or mortar fire fired into/against military installations).	"But it's the government of Bashar al-Assad that's incredibly restrictive about when it lets reporters in. And that's why there's been such a narrow and sporadic view of the conflict from that side" (NPR 2016).
	<i>Information Flow</i>	...occur during periods of electricity and/or internet shortages/outages restricting the flow of information to/from media professionals about these attacks.	"As fighting erupted in the Ethiopian region of Tigray in early November, the northern part of the country was cut from internet, mobile phone and landline communications. Journalists say the government-imposed blackout made it virtually impossible to get accurate information about the conflict" (Solomon 2020b).

Table 2: Hypothesized Editorial Biases

Type of Editorial Bias		Attacks...	Example
Violence Intensity	<i>Summary</i>	...are so numerous the media tend to report on them as a group rather than individually, omitting individual event details necessary for the construction of event datasets.	"There were scores of attacks on election targets on Saturday that killed police officers and wounded civilians" (Gibbons-Neff and Rahim 2019).
Normative	<i>Deadly vs. non-deadly</i>	...that result in death(s) are more likely to be reported.	"There seems to be a symbiotic relationship between the mainstream media and ISIS: the organization receives the publicity it needs, and these networks increase their audiences and advertising revenue" (Rafizadeh 2015).
	<i>Perpetrator</i>	...carried out by particular militants (e.g. Al-Qaeda) are more likely to be reported.	
	<i>Target</i>	...carried out against particular targets (e.g. U.S. forces) are more likely to be reported.	"[A]t least 20 times as many Iraqis have died as Coalition members, but in virtually all American papers there are far more articles describing [the latter]" (Henderson et al. 2009).
	<i>Salience (cultural, political, strategic, etc.)</i>	...carried out in location (e.g. capital city) or against a target (e.g. electricity infrastructure) thought to signify greater strategic importance is more likely to be reported.	"In a highly publicized attack... residents of Fallujah... attacked the vehicle of four US private security contractors, dragged their burned and mutilated bodies through the streets and strung them up on a bridge" (UNHCR 2005).
Coercion		...that put journalists at risk of retaliation if they are reported.	Somali journalists say "many more attacks go unreported because journalists fear further reprisals" (CPJ 2004).
Anticipation		...at particular times during which violence is expected (e.g. during or in the immediate lead up to elections) are more likely to be covered.	"The U.S. expects attacks in Iraq to continue to spike as the holy month of Ramadan begins this week..." (Arraf 2010).
Audience		...are less likely to be reported as public interest in a conflict wanes.	"[S]ometimes you can hear about a car bombing and there is a sameness to it all. And you wonder, okay, what's this going to add to tomorrow's coverage? Does this deserve to be put on page one? And sometimes now, you say no" (Pesca 2006).

fighting during Syria's civil war:

As many as 100,000 people have been forced to flee their homes in northwestern Syria as the government of President Bashar al-Assad and his patron, Russia, have intensified a military offensive there in recent days ... Drivers and cars were scarce as people hurried to find a way out of Maarat al Noaman, which Russian and Syrian government warplanes have been bombarding relentlessly during the latest assault.

For conflict event datasets, such summaries pose a significant problem. Given that observations correspond to individual conflict incidents (e.g. roadside bomb attack on civilians; direct fire attack on a checkpoint), summaries of violence typically cannot be translated into individual incidents. In the above example, the number of Russian and Syrian bombings and when they occurred, among other details, are not printed and therefore are omitted from conflict event datasets drawing on these summaries.

The media may also opt not to report conflict events for other reasons. For example, the media may make normative judgments about the types of events that are newsworthy. Our interviews highlight the importance of novelty: “[S]upposing there’s a protest in a place where there’s always protests, right, like you wouldn’t necessarily write about something like that. So it has to be there has to be some novelty value to it.”⁵ Particular targets may arouse less interest among the media. Individual targets’ nationality, gender, ethnicity, religion and other characteristics may alter reporting likelihood, as reflected in the epigraph to this paper. Another interviewee described a similar phenomenon within Israel and the Palestinian Territories:

[T]here was a disparity [in the likelihood of a news report being generated] if a Palestinian is killed and Israeli is killed ... but even just looking at the violence against Israelis ... it made a huge difference *where* it happened ... If it happened inside of “green line” Israel ... particularly if it happened in Jerusalem or Tel Aviv, it got a lot more coverage than if it happened in the West Bank ... if a settler was attacked outside of a settlement, it would not be a big story outside of the Israeli/Palestinian media ... ⁶

Our interviews also reveal significant differences in the type of attacks that are covered. One interviewee described how widely under-reported small arms fire is in Colombia:

⁵Interviewee 1.

⁶Interviewee 3.

Generally [attacks] involving guns [tend to involve] battles between FARC dissidents and a crime gang that they are fighting ... or the government versus the ELN [a major leftist guerrilla organization] ... different combinations of the various armed groups ... or in targeted killings of social leaders ... **And we don't generally cover individual attacks** unless it's someone who is very famous already or ... who is connected to some larger piece of newsworthiness ... maybe they are a political candidate or already a senator.⁷

Attacks carried out by particular rebel organizations may be more or less likely to be covered. For instance, as we show below, rebel organizations in the Philippines with Islamic affiliations received far more coverage than communist rebels, even though the latter were responsible for a greater percentage of violence in that country during the study period. This resonates with existing scholarship, which shows that attacks by Muslim perpetrators receive on average 357% more coverage than other attacks (Kearns et al. 2019), and that news outlets use pre-existing interpretive structures to place certain events in their broader context, with consequences for what events get reported and how (Norris et al. 2004).

For similar reasons, events deemed to have greater social, political, or strategic importance may be more likely to receive coverage. Attacks carried out on election days, or days prior to elections, may receive greater attention than those carried out on other days. Similarly, attacks against particular targets or in certain regions may receive more attention. In other cases, journalists may not report on events that put them at risk for retaliation. As one interviewee told us, there exists “[t]he potential for retaliation from some of these groups to track you down or track your family down and exercise some sort of retaliation or revenge on you. That’s always at the back of your mind when you’re reporting on these issues.”⁸ They may also decline to report on events on which they have strong personal views or even local “dogs in the fight.”

Finally, as audiences grow tired of particular topics, media outlets may limit coverage. As Perez-Pena (2008) reported in the spring of 2008, once violence in Iraq had begun to wane, “more than 50 percent of Americans said they followed events in Iraq ‘very closely’ in the months just before and after the war began, but that slid to an average of 40 percent in 2006, and has been

⁷Interviewee 4. Emphasis is ours.

⁸Interviewee 6.

running below 30 percent since last fall,” which he associated with a concomitant drop in the war’s news coverage. Fatigue alone may not account for diminished reporting: competing global events too may convince the media to cease or reorient coverage. As Perez-Pena (2008) further notes in the Iraq case, a “flagging economy and the most competitive presidential campaign in memory have diverted attention and resources” away from Iraq. Conflict actors may even strategically time unpopular decisions to coincide with events that occupy the attention of the news media (Durante and Zhuravskaya 2018).

We do not render normative judgments about these reporting decisions: the media often highlights for its audiences notable developments that might otherwise be passed over but for selective reporting. Problems arise, however, when media reports are assumed to represent an unbiased sample of violent events occurring within a given conflict and are then compiled into violent events datasets. A much more detailed set of interviewee quotes demonstrating these dynamics can be found in Appendix sub-section A.3.

HYPOTHESES

Our descriptions of capability and editorial biases imply a set of hypotheses, each with testable implications. Here we summarize the hypotheses and in the empirical section test them using data from Afghanistan, Iraq, the Philippines, South Africa, and Syria.

H1: As violence within a civil war or context of civil unrest intensifies, under-reporting of violent events will increase.

H2: Conflict events or incidents of social unrest that are less likely to be discovered or observed by the news media are less likely to be reported.

H3: Conflict events considered less relevant by the news media are less likely to be reported.

EMPIRICAL STRATEGY

Capability and editorial biases likely influence reporting on political violence in multiple ways. Our goal is not to identify all of their consequences, but rather to provide evidence of plausible sources of systematic under-reporting and to quantify their magnitudes. Having done so, we will have demonstrated that media-based datasets are prone to under-reporting in at least as many ways as we identify, and likely more.

Are capability and editorial biases sufficiently large to meaningfully influence inferences drawn using media-based data? We carry out a set of “reverse” replications to test whether prominent results in the literature that use high-quality administrative records of political violence can be replicated substituting media-based data. We generally find that they cannot. Whereas our primary empirical strategy establishes the existence of systematic under-reporting, our reverse replications demonstrate that – whatever the total set of under-reporting processes at play, and however they interact – these biases substantially limit what we can learn from media-based datasets.

IDENTIFICATION AND DATA

We study patterns of missingness within conflict event datasets that are consistent with expected patterns of under-reporting by the media and, where we identify such patterns, seek to quantify their magnitude. We leverage the highly organized record keeping system that security forces in Afghanistan, Iraq, and Syria employed throughout the U.S.-led/supported wars in those countries, and similar systems employed by security forces in the Philippines and South Africa. All five administrative datasets were developed and used for internal purposes. In Afghanistan, Iraq, and Syria, some or all of these records were classified until their release, burnishing our confidence in the data’s accuracy since they were intended for access and use exclusively by officials with appropriate security clearance.

Past studies have used high-quality administrative records as the benchmark for media-based event datasets: Weidmann (2016: 211) manually matches all events from news reports in the

GED dataset to military records from Afghanistan to establish that “only about 2% (8/362) of all insurgent-initiated events could not be matched [which] suggests that the available SIGACTS version has almost perfect coverage of violent action initiated by insurgent groups ... and can serve as an approximation of the ‘universe’ of violence.” We build on this approach, comparing like events in the SIGACTs datasets to media report-based datasets for the entire war periods in Afghanistan and Iraq. While this approach limits external validity — we are unsure if our findings accurately describe systematic sources of under-reporting related to inter-rebel violence, for example — it significantly increases internal validity.⁹ For the Philippines and South Africa, we also compare like events.

In addition to the quality of the comparison datasets, the four cases allow us to assess whether expected patterns of under-reporting manifest across diverse settings of political violence. The settings vary in terms of intensity; weapons and tactics employed by participants; attention paid to them by media outlets; and so forth. Identifying systematic under-reporting across these five cases would suggest that patterns of under-reporting are highly likely to manifest elsewhere. (Our interviews also make this very clear.)

Here we describe in turn each of these administrative datasets: 1) Iraq and Syria “Significant Activities” (or SIGACTs) corresponding to the international campaign of mostly aerial bombardment against ISIS targets; 2) Afghanistan War SIGACTs; 3) Iraq War SIGACTs; 4) Armed Forces of the Philippines Joint Operations Center (JOC) Conflict Records; and 5) and South African Police Service (SAPS) Protest and Riot Dataset.

OIR, OEF, and OIF SIGACTs Datasets

Throughout the Afghanistan and Iraq wars,¹⁰ U.S. and partner forces collected detailed data on violent activities and other wartime outcomes (e.g. the discovery of weapons caches). U.S. forces

⁹Other important classes of political violence likely are also subject to reporting biases. In some cases, pressures to under-report may be even greater: rebel-on-rebel violence may be of less interest to the news media than attacks on civilians or security forces and obtaining details about rebel-on-rebel violence may be associated with a greater degree of risk, for example.

¹⁰Formally, “Operation Enduring Freedom” and “Operation Iraqi Freedom.”

also tracked individual cases of bombings and close air support bombings and shootings conducted during “Operation Inherent Resolve” in Iraq and Syria. These forces collectively logged nearly one million incidents, which were geo-referenced and time stamped. These events were logged using specific military protocols and technologies, ensuring a high degree of accuracy and uniformity across space and time.

Data for the campaign against ISIS cover the period February 2016 through mid-March 2017, and include approximately 7,808 individual instances of bombings and close air support in which U.S. forces engaged using bombs.¹¹ The Afghanistan and Iraq SIGACTs datasets cover the periods January 2002 to January 2015, and December 2003 to December 2011, respectively. These datasets include details on weaponry used by government forces or insurgents in attacks, as well as their targets. The Iraq-Syria and Afghanistan data also include details on attack outcomes, including the number of individuals estimated killed or wounded (for Iraq and Syria) and whether a given attack resulted in fatalities, injuries, or damage to security force equipment or infrastructure (for Afghanistan). The data for the campaign against ISIS were collected and prepared by one of the authors (Shaver) and are being used/released for the first time with this article. Data for Afghanistan were collected and prepared by Shaver and Wright (2017), while data for Iraq were collected and prepared by Shaver and Bollfrass (Conditionally Accepted).

Because incidents of wartime violence were systematically logged by security forces, reporting biases that we expect to affect media-based datasets generally should not apply to the SIGACTs datasets. In particular, standard operating procedures should ensure incidents are generally reported, regardless of time and place. The use of advanced technology by these forces, which helped automate the record generating process, also increases our confidence in these records. The level of detail of these records can be seen in Figure 3. Equally important, the records were intended for internal use only when created: some or all of the documents were classified secret until their release. SIGACTs data is arguably less biased than media-coded data, although it might still have (some) similar biases. Hence this paper identifies a lower bound to the bias incorporated in

¹¹Additional cases of close air support are reported in the data but not included in our analysis, as these involved the use of rotary cannons against targets, for example, rather than bombs.

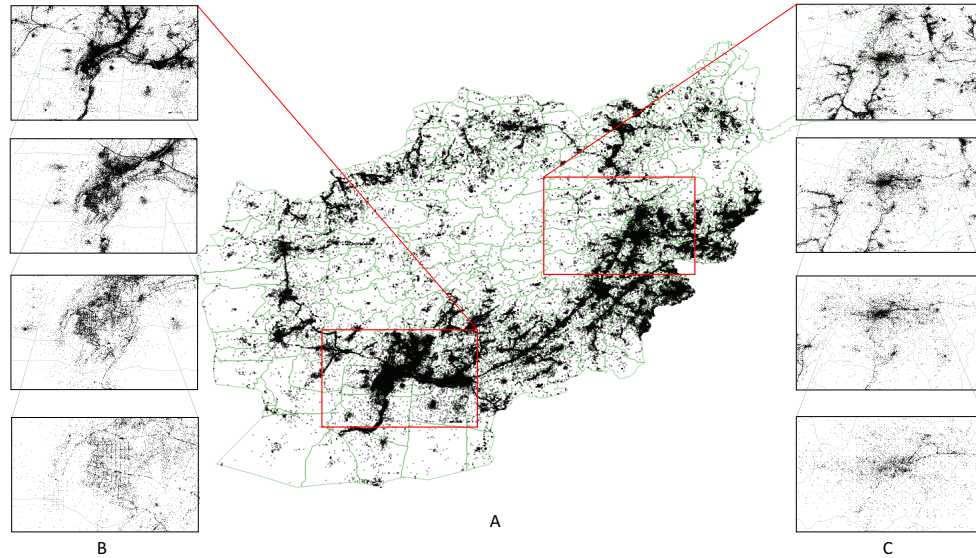


Figure 2: Distribution of SIGACTS across Afghanistan, compiled by US and partner forces. Source is Shaver and Wright (2017).

media-coded data.¹²

Furthermore, in the case of the campaign against ISIS in Iraq and Syria, the per unit cost of bombs dropped by U.S. forces often ranged from tens to hundreds of thousands of U.S. dollars.¹³ It is unlikely that strikes were successfully and consistently concealed or fabricated by particular individuals or entities within the DoD from others in the department.¹⁴ We cannot be certain, however, that the data supplied by the U.S. Defense Department is complete: some records may have been omitted, for example, if they were not located by the release authority.¹⁵

Armed Forces of the Philippines Joint Operations Center (JOC) Conflict Records

Similar to the SIGACTs datasets, security forces in the Philippines kept detailed logs of their engagement with militants from organizations including the New People’s Army and Abu Sayyaf.

¹²We thank the ESOC referee for this suggested wording.

¹³For instance, some of the most commonly used bombs and missiles include the GBU-38, GBU-31, GBU-39, and AGM-114 (Hellfire II). Per unit prices of many of these are published by the U.S. Department of the Airforce on its website: <https://www.af.mil>.

¹⁴The consideration is not about potential incentives to misrepresent data to the public, but rather the potential for military actors to conceal or fabricate bombings before others within the DoD.

¹⁵Substantially fewer strikes are reported before February 2016, for example; whether this reflects a major change in military response between the Obama and Trump administrations or a change in record keeping/systems, we cannot be sure. We therefore subset the data to the period from February 2016 onwards.

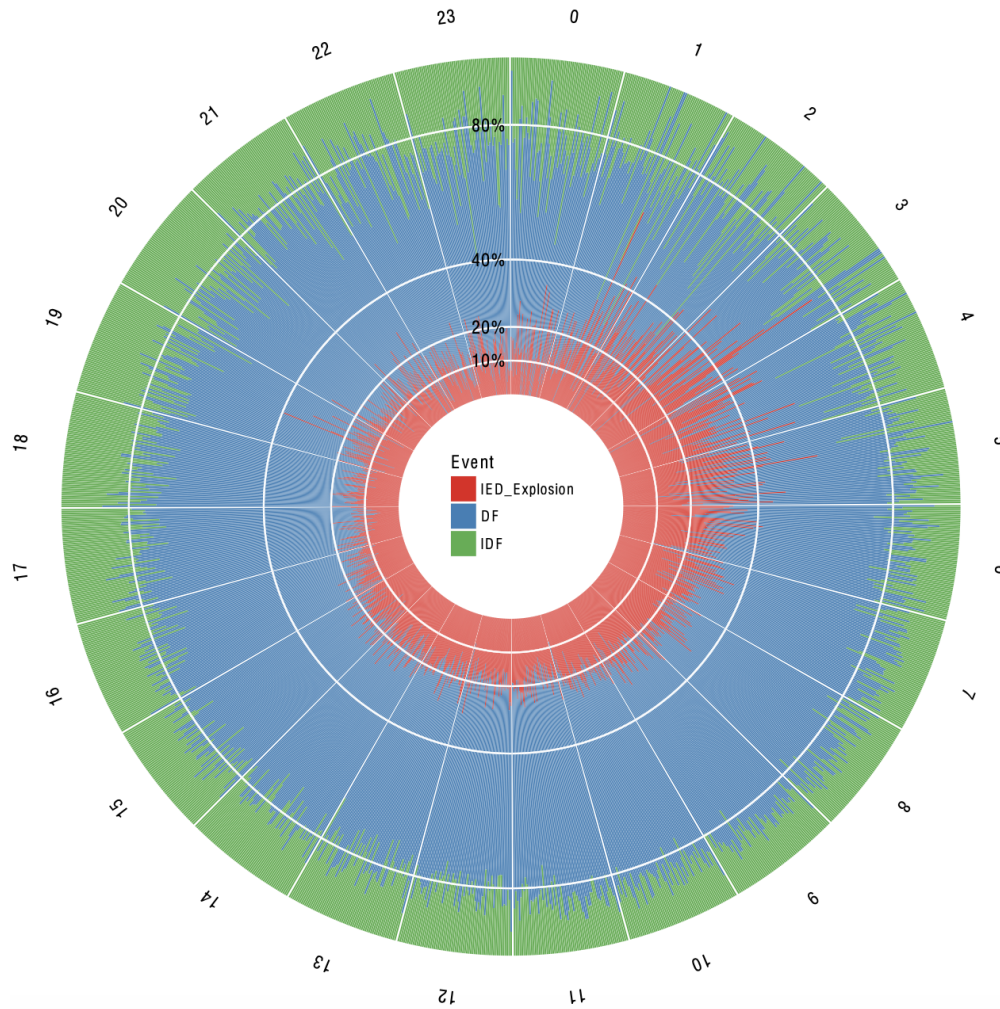


Figure 3: Average proportion of attacks by type that occurred during each minute throughout the war: improvised explosive device attacks (red), direct fire attacks (blue), and indirect fire attacks (green). Source is Shaver and Wright (2017).

Like the SIGACTs data, their accuracy is high given the nature of their use. As the authors responsible for retrieving these data explain:

[T]he field reports underlying our data are only for internal use by the AFP [Armed Forces of the Philippines] and were not used to make public claims about the program. Their main purpose is to chronicle military activities and assist in developing intelligence assessments and planning future AFP operations. Inaccurate information included in field reports might jeopardize future military operations and put the lives of AFP troops at risk, so the AFP’s chain of command has a strong incentive to enforce accurate reporting (Croft et al. 2014: 1841).

The data are extensive and cover the period 1975 through 2012. We subset the Philippines data to 1999 through 2012, a period of intense fighting.

South African Police Service (SAPS) Protest and Riot Dataset

We use detailed data on protest and riot activity from South Africa, drawn from the Incident Registration Information System (IRIS), a database of all police related incidents in South Africa. The SAPS data consist of 232,242 incidents of mainly protests and riots between January 1, 1997 and January 18, 2018. We use a subset of the data labelled “crowd incident (peaceful or unrest),” which consist of instances when crowd control was utilized by the police. The data include information about event location, along with over 150 unique types of motivation. We obtained these data from two Promotion of Access to Information Act (PAIA) requests to the South African Police Service (SAPS), similar to Freedom of Information Act (FOIA) requests. We remove approximately 14,000 observations where the “motivation” of the protest is listed as unknown, which produces a total of 70,955 observations: 13,310 associated with grievances over service delivery; 20,896 over labor issues; 3,594 over education; 4,262 over crime; 1,926 over housing; and 3,005 over local municipality administration.¹⁶

Descriptions of the media report based event datasets that we use—the Georeferenced Events Database (GED), the Global Terrorism Database (GTD), the Integrated Crisis Early Warning Sys-

¹⁶These classifications were made by Daniel de Kadt, Ada Nina Johnson-Kanu, and Neelum Maqsood for work on another project that uses these data.

tem (ICEWS) database, and the Social Conflict Analysis Database (SCAD)¹⁷—appear in Appendix Sub-Section 1.

OBSERVABLE IMPLICATIONS

To test our hypotheses, we compare the number of relevant incidents reported in the media report based datasets with those in the administrative datasets. The former have specific criteria for events included, and to make direct comparisons between the two we only include comparable events. The hypotheses presented above are general, and may vary across conflict types. Hypothesis 2, which indicates that wartime events that are less observable by the media will be under-reported. However, the type of events that escape the media’s attention may vary across conflicts. In Iraq, for instance, the United States maintained large military bases, which insurgents often targeted with indirect fire. Many of these attacks are unlikely to have been detected by the media, as evidence of their occurrence would be known only to combatants.

Here we present observable implications specific to each of the five contexts we study and describe our strategies for testing for them. We explore whether the media-based datasets: 1) are more likely to under-report events as the intensity of conflict or social unrest increases; 2) are less likely to report events in areas or during periods of diminished nighttime radiance (intended to proxy levels of electricity, internet availability, etc.) 3) tend to under-report particular types of attacks; 4) are less likely to under-report violence on significant dates; 5) are more likely to under-report non-fatal violence; and, finally, 6) are more likely to under-report particular targets or perpetrators of attacks.

Capability Bias

Violence intensity. Do media-based datasets under-report violence or social unrest as violence increases? We begin by independently aggregating incidents of violence from each of the datasets

¹⁷We do not present results using the Armed Conflict Location and Event Data (ACLED), given unsettled discussions about whether comparing these data to administrative records represents a violation of their terms of agreement.

to the district (d)-month (m).¹⁸ We then generate estimates of under-reporting for each district-month observation for each dataset, subtracting media report based incidents of violence from those tracked by security forces:

$$\widehat{U}_{d,m} = S_{d,m} - M_{d,m},$$

Because under-reporting returns to violence intensity may not be linear, we generate primary results using ordinary least squares regression in a series of linear and higher order polynomial specifications given by the response function $f(T_{d,m})$.¹⁹ We also generate results using additive modeling techniques.²⁰

$$\widehat{U}_{d,m} = f(T_{d,m}) + v_d + vt + \epsilon_{d,m}$$

where t denotes unique years or months, depending on the model.

Nighttime lights. Limited access to electricity, the internet, and cellular telephone coverage likely reduces news reporting, for example by restricting the flow of information about attacks to media professionals. From Ghana to Yemen to Venezuela, electricity outages have negatively affected news media operations, particularly local outfits (Malsin 2015; Gonzalez 2020; House 2016). Lack of electricity and internet may also prompt local media to change how they report, in ways that could reduce the probability that events find their way into articles by wire services and other online news sources used to construct conflict event datasets. For instance, Issa (2016) notes in Syria the “proliferation of traditional media forms such as newspapers and radio, as opposed to online media” in response to electricity and internet shortages. For media-based datasets that rely primarily or exclusively on online news content, this is particularly problematic.

¹⁸For Afghanistan, Iraq, and Syria, we adopt the district. For the Philippines and South Africa, we adopt the province and municipality, respectively. For compactness, “district” refers to our subnational units of analysis.

¹⁹Given the potential for over fitting, we limit specifications to cubic terms and below.

²⁰As a robustness check, we generate measures of violence intensity, grouping attacks into N bins of ten, and regress estimated under-reporting on a series of binary indicators represented by the bins, including district and month or year fixed effects: $\widehat{U}_{i,t} = \sum_{n=1}^N \gamma_n I_{i,t}^n + \xi_d + v_t + \epsilon_{i,t}$. Results are consistent across methods.

To capture fluctuations in available electricity, we associate estimated under-reporting with monthly average nighttime lights across subnational units for four of our cases (Elvidge et al. 1997).²¹ We also add year and, separately, month fixed effects. These tests cover most months over approximately eight years per country.²² Figure 4 shows estimated monthly under-reporting per district on district-month average nighttime lights.

We also utilize previously sequestered, high-frequency time series data from the U.S. Department of State on the number of hours of electricity available to residents of Baghdad and Basrah in Iraq during the Iraq War to assess the relationship between access to power and estimated under-reporting (e.g. Shaver and Tenorio 2014). Due to space restrictions, details and results appear in Appendix Sub-Section 3.

Attack by type. Attacks using particular types of weapons may be less likely to be observed than others. We distinguish specifically between armed assaults and, separately, bombings and explosions.²³ We also distinguish separate direct fire attacks – typically involving the use of small arms – and bombings. In the Afghanistan and Iraq cases, these roadside bomb attacks typically used improvised explosive devices. Direct fire attacks are less likely than bombings to leave lasting signatures that can be detected by and reported to the media: roadside bombings often leave disabled vehicles on public roadways, blast marks on roads, or render roadways entirely impassable, while

²¹Monthly nighttime lights data are unavailable after the first few months of 2014, so we are unable to include the ISIS campaign in this particular analysis. In the Appendix we also show results for models that include unit fixed effects, to estimate within-district changes in nighttime lights.

²²The start date is the same for all countries – 01/01/2005 – but the end date varies. For Afghanistan the end date is 12/31/2013, the last complete year for which monthly nighttime satellite data is available; for Iraq the end date is 12/31/2011, which is the end of Operation Iraqi Freedom; for the Philippines the end date is 12/31/2012, which is the last year for which data is available for the Philippines; and for South Africa the end date is 12/31/2013, the last complete year for which monthly nighttime satellite data is available. Not all months are included, given gaps in satellite data coverage: of the 103 months available for South Africa, 70 are complete and 33 are incomplete, given that its territory is sometimes only partially visible to the satellite. We exclude incomplete data from the analysis.

²³Ideally, we would distinguish more specifically between attack types, comparing separately attacks involving small arms, projectiles fired over long distances (e.g. rockets and mortars), roadside bombings, etc. Unfortunately, GTD often lacks weapon type specifics (e.g. “Unknown Explosive Type”). Where it does classify attacks, its organizing principles aggregate attacks that we would distinguish from one another: GTD tracks “projectile” attacks, which include rockets, mortars, and RPGs, the latter of which typically requires a line of sight on a target and is likely to be associated with very different wartime dynamics than indirect fire. GED does not provide information on specific weaponry used in attacks that would permit comparisons with that dataset.

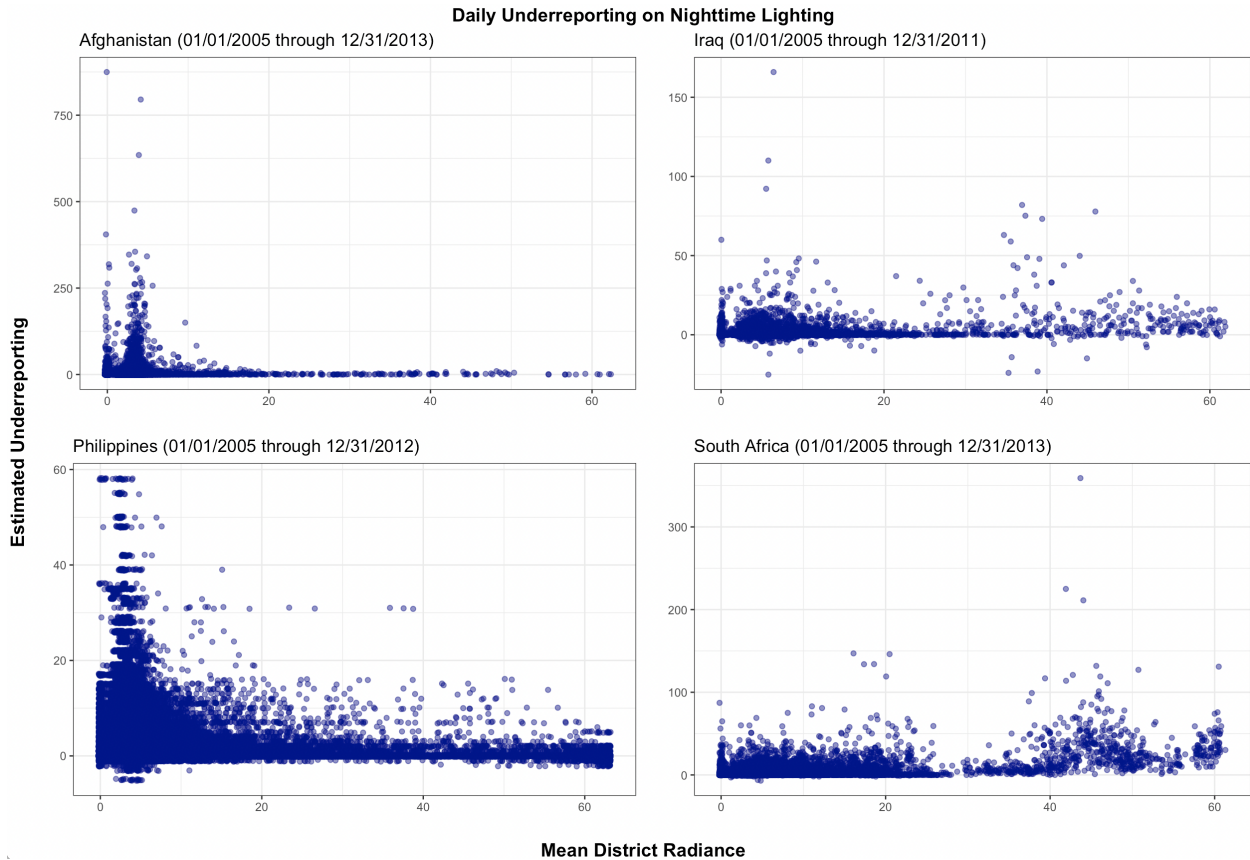


Figure 4: Mean district (Afghanistan and Iraq), province (Philippines), and municipal (South Africa) nighttime radiance against $\widehat{U}_{d,m}$ for each country. Source for nighttime radiance data is Elvidge et al. (1997).

direct fire attacks are less likely to leave behind such evidence of their occurrence.

We aggregate attacks by type across the SIGACTs, JOC, and GTD datasets, and then plot monthly country-level time series of each type to determine whether relative levels of reporting by attack type diverge between the two kinds of datasets. One concern is that attacks with particular weapon types may be more or less likely to result in casualties, and that casualty likelihood drives reporting frequency of different types of attacks. In a second test we condition on fatalities, generating the same time series plots while subsetting the data to fatal events only.

Editorial Bias

The observable implications of editorial bias are multiple. Here we consider several criteria and associated tests.

Politically important events. Wartime events deemed by the media to be of social, political or strategic importance may be more likely to be reported. Although we are unable to determine the political or strategic salience of individual attacks, we examine whether the probability that a given attack is reported depends upon whether it a) took place within or near politically important cities, or b) was carried out on meaningful days in a given country.

We compare estimated under-reporting in capital and other major cities when compared with other regions. Specifically, we identify districts representing capital and major cities and estimate the following month fixed effect model, $\widehat{U}_{d,w} = \beta \mathbb{1}[d \in \text{major city}] + v_m + \epsilon_{d,w}$, where d, w are subset to only cases that experienced violence.²⁴

As a complementary test, we also re-estimate results using the nighttime lights data, in this instance including month fixed effects (the temporal unit of observation and dropping district indicators) in order to compare across-district differences in nighttime lights and estimated under-reporting. This second test more generally compares levels of under-reporting across areas with varying levels of development (which removes some of the subjectivity of our assessments of what constitutes a “significant” city).

We aggregate to the conflict-day, creating an indicator variable that takes a value of 1 for days on which a major religious (e.g. Eid-al-Adha), social (e.g. national holidays), political (e.g. presidential elections), or military (e.g. the Taliban announcing the beginning of a spring military offensive) event occurs. To do so, we constructed separate datasets for all four countries in which we identify important events and associate them with calendar days. We then regress “significant days” on estimated under-reporting. Because some dates are likely to attract media attention

²⁴During the campaign against ISIS, the U.S. did not carry out any airstrikes against targets in Damascus, thus for Syria we limit ourselves to contested cities (e.g. Aleppo and Raqqa).

in their lead up and/or immediate aftermath (e.g. national elections), we also test an alternative model that regresses all days within a “significant week” (formally, the set of days $D \in [d_i \pm 1, d_i \pm 2, d_i \pm 3] \forall$ significant date i) on estimated under-reporting.²⁵

Fatal and non-fatal events. Media-based datasets may under-report non-fatal events more often than fatal ones. We restrict our focus to Afghanistan and the Philippines: for any given attack our security force datasets record details about both the targeted actor(s) and whether one or more of the targeted actors were killed. We consider whether attacks that produced fatalities, regardless of the victims’ identity, were more likely to appear in the media-based data than those that did not. We again generate measures of \hat{U} (at the country-month level) for fatal and non-fatal events, and plot these against one another.

One might expect a large number of non-fatal attacks carried out by insurgents against American forces in Afghanistan, given significant power asymmetries between combatants. As a robustness check, we replicate this analysis while restricting observations to attacks carried out by the Taliban against Afghan police forces: the power asymmetry between these two actors is more limited and may be more reflective of combat dynamics between insurgent and state forces in other conflicts. Results for this supplemental exercise are consistent with those reported below.

Target and/or perpetrator identity. We consider whether the media report based datasets show greater levels of under-reporting when attacks are either carried out against particular targets or, separately, when conducted by particular militant organizations. To identify differences in reporting patterns by target, we use the Afghanistan SIGACTs data, comparing reports of fatal incidents in which either security forces or insurgents were killed to those in media based datasets.

²⁵We do not include the campaign against ISIS targets: during the period of intense fighting for which we have data there were no elections or other clearly predictable social or political events. We carry out an alternative test, identifying events during the study period in which ISIS was mentioned in at least 2% of major news articles on a given day, determined using Roberts et al. (2021). These events were when ISIS claimed responsibility for a mass shooting in Orlando, FL on June 13, 2016, and when former U.S. President Trump referred to former U.S. President Obama as “the founder of ISIS” on August 11, 2016. Given that the media could not have anticipated these events, we analyze whether underreporting on airstrikes against ISIS targets in Iraq/Syria was less likely during the one week period following these news events. The results are inconclusive, so we not report them fully here.

To identify possible differences in reporting by perpetrator, we carry out separate analyses for the Philippines and South Africa. For the former, the security logs specify the militant organization associated with each event and we compare the sums of attacks, fatal events, and casualties associated with each of the three most active rebel organizations during the study period: Abu Sayyaf; the Moro Islamic Liberation Front; and the New People’s Army. Given research that reveals differences in how the media and the public treat violence perpetrated by actors who claim an association with Islam and those who do not (e.g. American white nationalists), we explore whether attacks by two aforementioned Islamist organizations were more likely to be covered by the media than those attacks perpetrated by the militant wing of the Communist Party of the Philippines.

Using highly (geospatially) detailed 2011 census data from nearly 90,000 subdivisions of South Africa, we calculate for each of the country’s 1152 police precincts its shares of minority (all non-white) and, separately, “Black African”, “Coloured”, “Indian or Asian”, and “Other” residents.²⁶ The data include demographic characteristics for 51,759,163 South African residents. Controlling for each precinct’s share of non-urban area, average age, share of high income earners,²⁷ share of individuals with no schooling,²⁸ and unemployment rate, we then estimate whether estimated underreporting varies with the racial/ethnic makeup of the precinct. Specifically, similar to our approach associating violence intensity with underreporting, we calculate results for a series of linear and higher order polynomial specifications given by the response function $f(T_p)$ as well as with additive modeling techniques. Given that the census data are for a single year, we sum underreporting for our full study period ($\sum_{1997}^{2017} \widehat{U}_p$), generating cross-sectional results. Additionally, we separately estimate results for each individual year. For all tests, we include district and, separately, province fixed effects. (Results are largely unchanged regardless of the choice.)

²⁶These are the formal racial/ethnic divisions used in the South African census.

²⁷Unfortunately, respondents’ incomes are reported in the data by the income bins to which they belong. Furthermore, the bins vary in terms of the number of South African Rand they represent. Thus, calculating a more direct measure of average income per precinct is not possible. We, therefore, instead use the four highest income bins to calculate the precinct share of high income earners.

²⁸Given the variety of educational outcomes tracked through the census, rather than distinguish between different educational types, we calculate for each precinct the share of individuals having completed no schooling to proxy for general educational differences across units.

RESULTS

We first compare the security force and media-based datasets to one another given their over-all levels of reporting, comparing similar events. In Figure 5 we plot the comparable time series against one another: media-based datasets tend to be much less comprehensive than security force datasets. This is attenuated somewhat when comparing only fatal events, a first indication that non-fatal violent events may be more likely to go unreported.

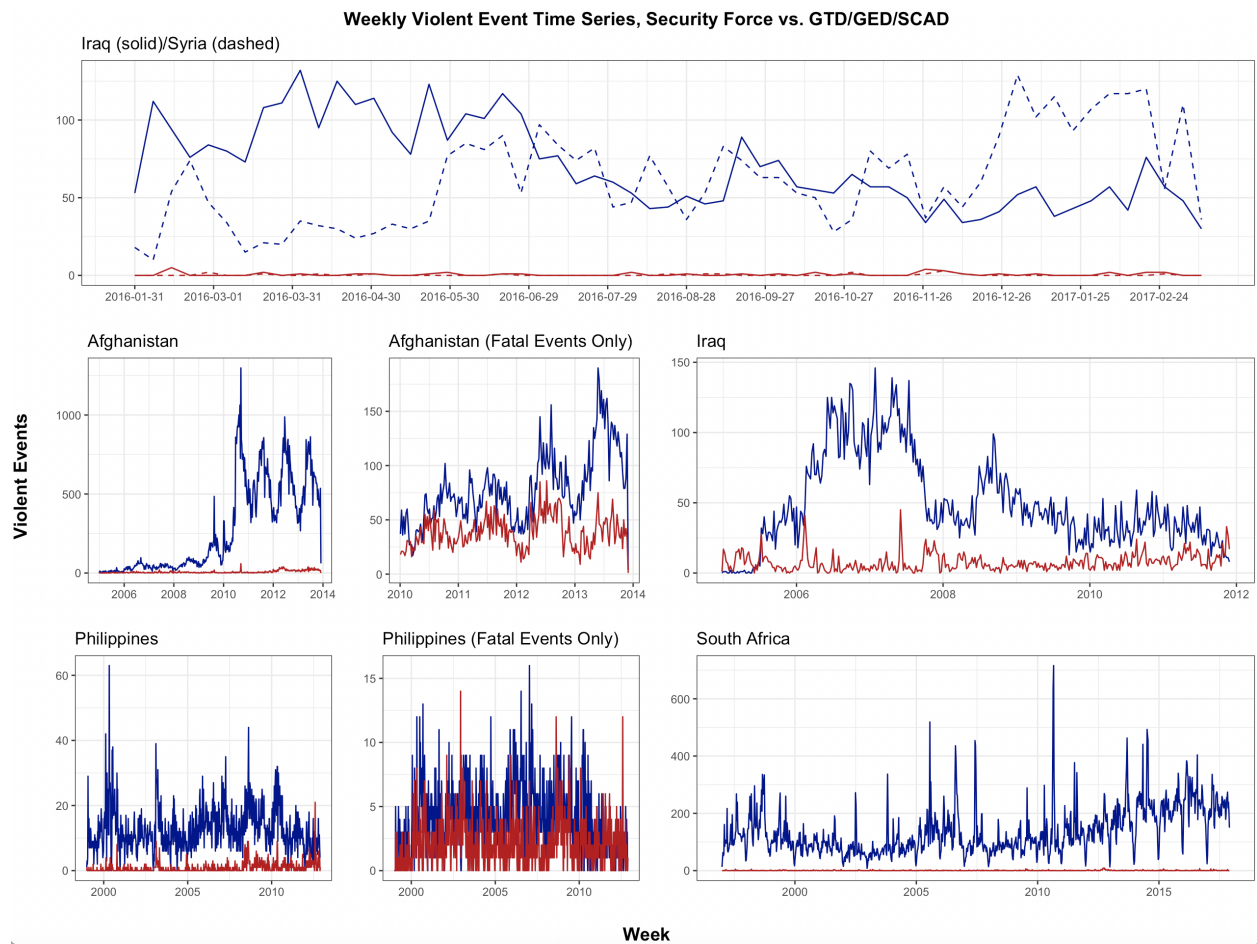


Figure 5: Comparison of GED, GTD and SCAD (red) and security force data (blue) for Syria/Iraq, Afghanistan War, Iraq War, the Philippines, and South Africa. Data are subsetted so that included events are comparable. Fatal event comparisons are shown for only Afghanistan and the Philippines, as the other datasets do not contain such details.

Violence Intensity

The results indicate that as the number of violent incidents increases, so too does under-reporting. Generalized additive model results are presented in Figure 6. Additional regression results are presented in Appendix Sub-Section 8.2.

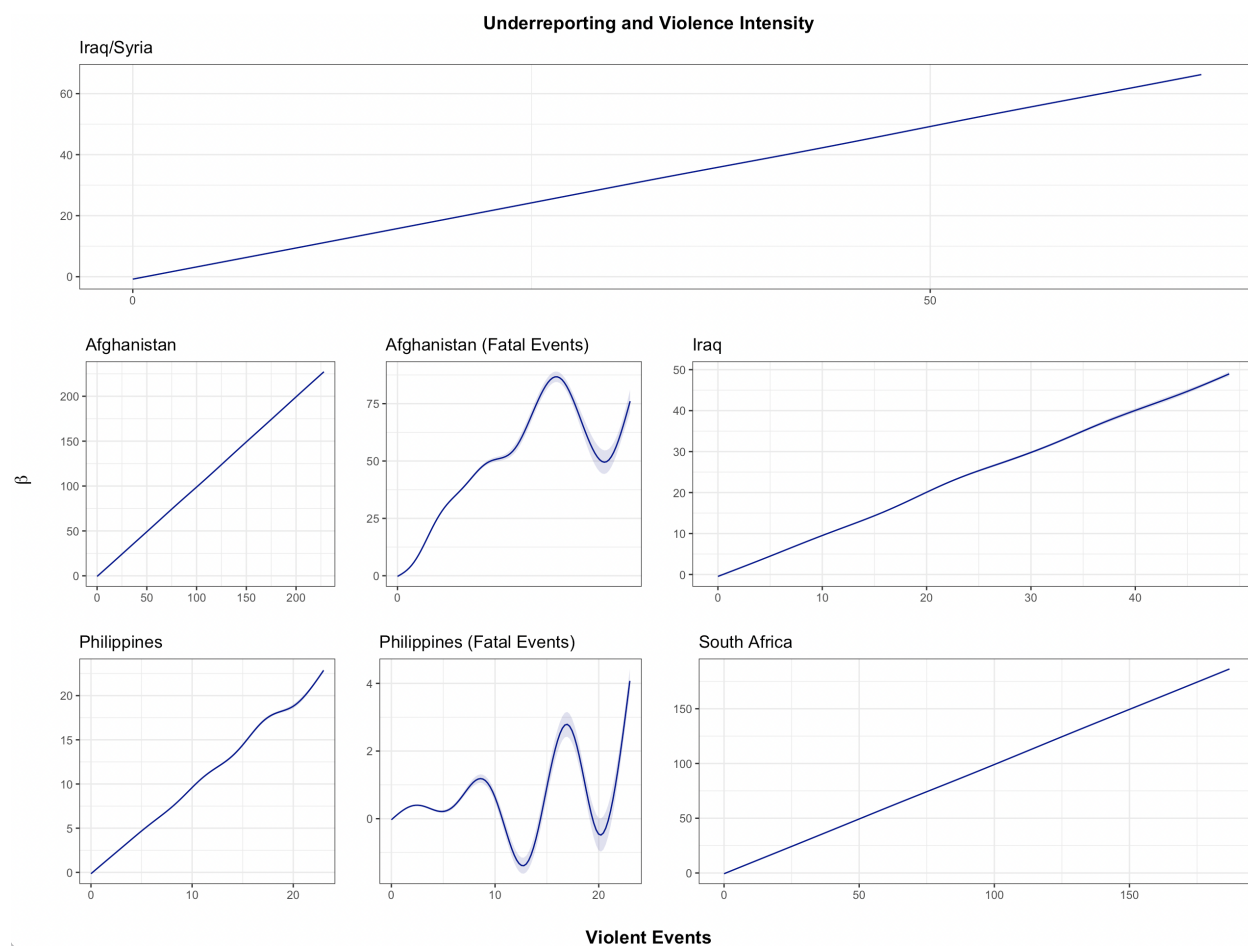


Figure 6: Generalized additive models of underreporting and violence intensity. Results show that, across cases, estimated underreporting consistently increases at the district-week level as the level of violence/unrest increases. All models include district and month fixed effects.

Nighttime Lighting

Generalized additive model results associating monthly nighttime lights and estimated under-reporting are displayed in Figure 7. The results are inconclusive: while estimates from Afghanistan and Iraq show that under-reporting tends to increase when district-level radiance is reduced, the

Philippines and South Africa show the opposite. These mixed results suggest that sources of under-reporting may differ across countries, raising further concerns about cross-country comparisons when using media-based datasets.

Results from the daily hours-without-power analysis (reported in Appendix Sub-Section 3) show that for a one standard deviation (σ) increase in hours without power across Iraq (≈ 2.62), we estimate an increase in under-reporting of approximately half of one attack (≈ 0.42). Given Iraq's mean daily hours without power (≈ 11.5), approximately two attacks per day went unreported due to lack of electricity or correlated factors. However, hours of power varied over time: Baghdad saw significant fluctuations ($\sigma \approx 4.34$ hours), suggesting that under-reporting varied widely across both time and space during the war.

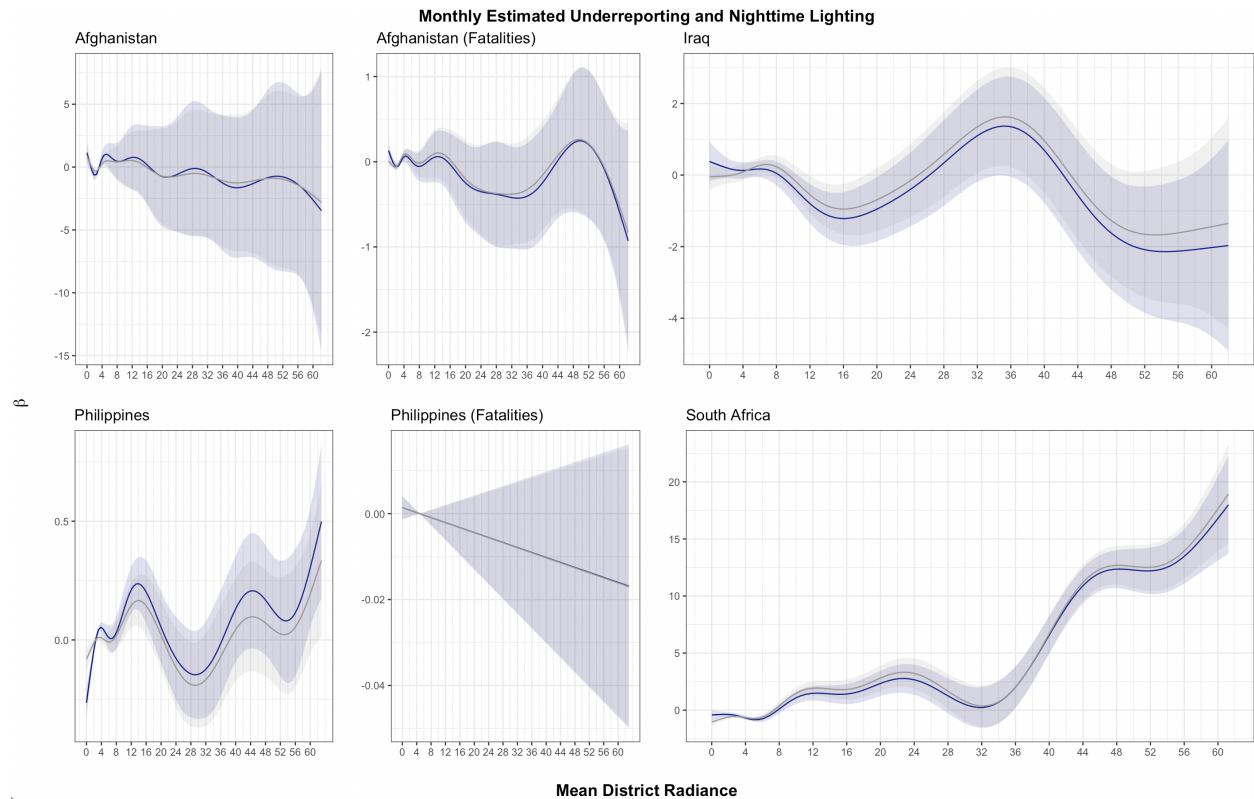


Figure 7: Generalized additive models of monthly nighttime lights and estimated underreporting.

Attack by Type

The results in Figure 8 provide strong evidence that bombings are more likely to be reported than armed assaults. In Afghanistan, armed assaults were nearly twice as common as bombings and explosions (improvised explosive devices and indirect fire) (1.71:1). As expected, however, GTD reports a substantially greater number of the latter (0.49:1). Similarly, in the Philippines, armed assaults were more than seven times more likely (7.42:1), yet we observe that they are reported at roughly the same rate by GTD as bombings and explosions (1.21:1). Iraq is the exception in this case: according to security force records, ≈ 0.375 armed assaults were carried out for every bombing or explosion, while GTD shows very similar patterns (0.41:1). The Iraq results are likely driven by the high frequency of mortar and rocket fire by insurgents against military installations.²⁹

Significant Cities and Days

Estimated underreporting in capital (and other major) city regions was typically significantly different from that in other areas. (See Appendix A.2.1. for the full results, including alternative models with week fixed effects, which produce nearly identical results.) Yet the direction of the relationship is not the same across conflict settings. While we expect from our in-depth interviews attacks in capital cities (or other politically important locations) to be more likely to be reported, in some countries such cities were also disproportionately affected by violence. Editorial bias that prioritizes reporting on events in cities may be diminished or offset by a violence intensity bias. Consistent with this expectation, we find that underreporting of events in Kabul, a region that experienced relatively less violence during the Afghanistan War than other parts of the country, was significantly smaller than in other parts of the country. In contrast, we find the reverse for Iraq: underreporting in Baghdad, which experienced a great deal of the wartime violence, is greater than

²⁹Rocket and mortar attacks were far less common in Afghanistan and the Philippines than in Iraq. Because we are unable to disaggregate attacks classified by GTD as either bombings or explosions, we cannot distinguish between bombings and rocket and mortar fire. We expect the are relatively unlikely to have been reported, given that targets of these attacks were large military installations to which journalists and civilians had limited or no access. Underreporting of armed assaults in Iraq is likely to have been more or less matched by under-reporting of rocket and mortar fire, concealing the higher likelihood of reporting of bombings.

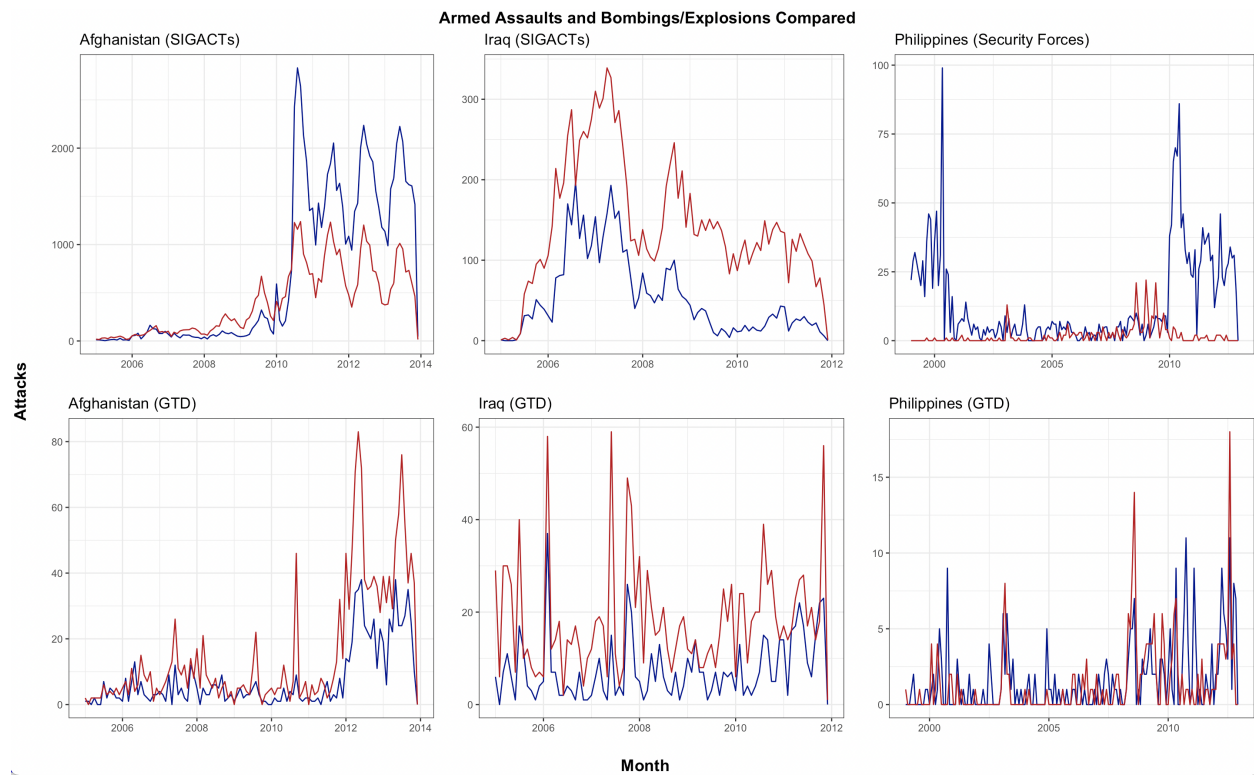


Figure 8: Monthly attacks recorded by security forces and GTD, disaggregated by type: armed assaults (blue) and bombings/explosions (red).

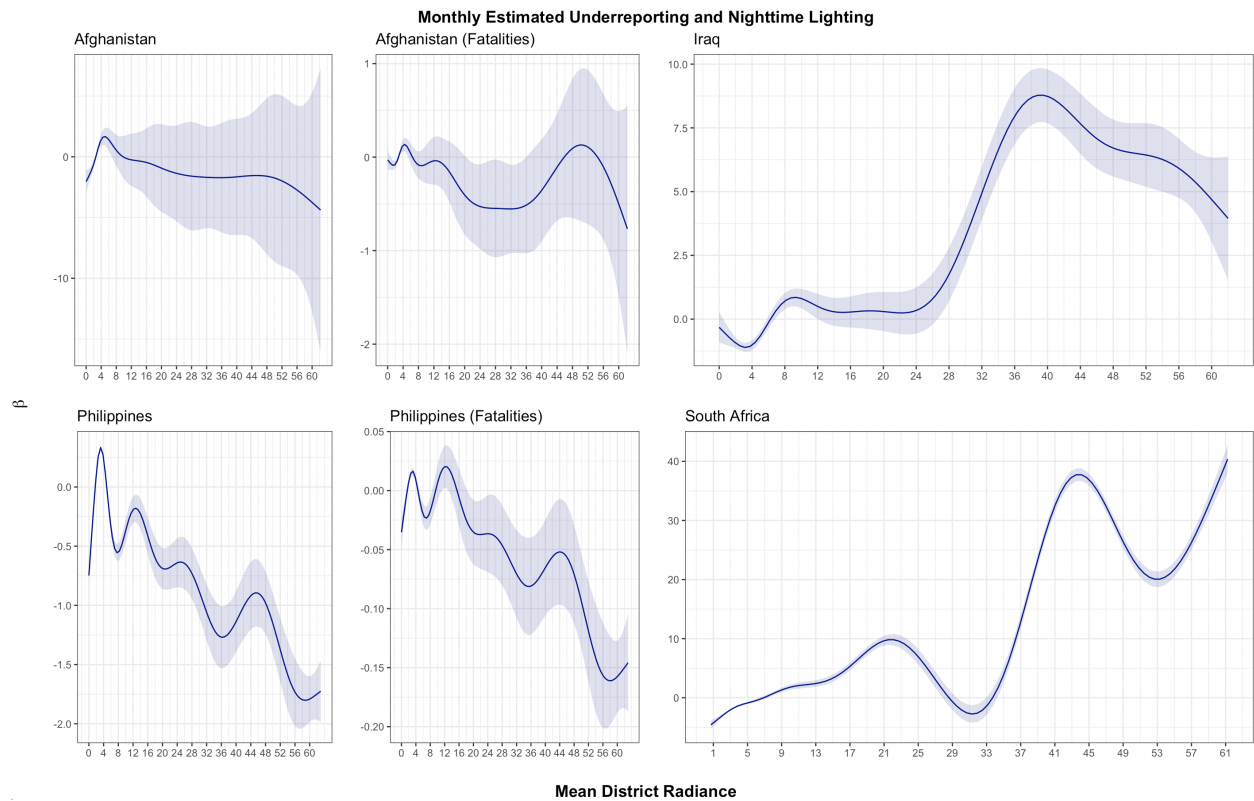


Figure 9: Nighttime Lights and Under-reporting Across Districts

for other areas of the country.

Results relating to under-reporting and significant days are suggestive yet inconclusive. As expected, levels of under-reported are negatively correlated with significant days (and significant weeks, in the alternative specification). However, results are not consistently statistically significant, and we find no evidence in the Philippines of less under-reporting on significant days. Regression results appear in Tables 3 (significant days) and 4 (significant weeks).

Table 3: Significant Days and Under-reporting

	Estimated under-reporting											
	(1) Afghanistan	(2) Afghanistan	(3) Afghanistan - Fatal	(4) Afghanistan - Fatal	(5) Iraq	(6) Iraq	(7) Philippines	(8) Philippines	(9) Philippines - Fatal	(10) Philippines - Fatal	(11) South Africa	(12) South Africa
Significant Date	-1.273 (1.306)	-1.717 (1.308)	-0.494 (0.307)	-0.491 (0.313)	-0.330 (0.381)	-0.341 (0.388)	-0.011 (0.107)	0.155 (0.118)	-0.130** (0.061)	-0.121* (0.068)	-2.169*** (0.763)	0.227 (0.802)
Constant	0.944 (2.479)	3.503 (2.942)	-0.468 (0.583)	-0.606 (0.704)	-1.441** (0.639)	-2.525*** (0.803)	1.324*** (0.291)	0.735** (0.330)	0.109 (0.166)	-0.112 (0.189)	7.102*** (2.098)	0.584 (2.252)
Month Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Week-of-Year Fixed Effects	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	3,257	3,257	3,257	3,257	2,526	2,526	5,084	5,084	5,084	5,084	7,640	7,640
R ²	0.895	0.901	0.650	0.656	0.643	0.654	0.170	0.191	0.074	0.088	0.440	0.467
Adjusted R ²	0.891	0.895	0.638	0.639	0.631	0.634	0.141	0.155	0.042	0.047	0.421	0.445

Notes: Effects of significant days on the extent of under-reporting in Afghanistan (models 1 and 2); Afghanistan only fatal events (models 3 and 4); Iraq (models 5 and 6); the Philippines (models 7 and 8); the Philippines only fatal events (models 9 and 10); and South Africa (models 11 and 12). All specifications include month fixed effects, while even columns also include week-of-year fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Significant Weeks and Under-reporting

	Estimated under-reporting											
	(1) Afghanistan	(2) Afghanistan	(3) Afghanistan - Fatal	(4) Afghanistan - Fatal	(5) Iraq	(6) Iraq	(7) Philippines	(8) Philippines	(9) Philippines - Fatal	(10) Philippines - Fatal	(11) South Africa	(12) South Africa
Significant Date	-1.750*** (0.621)	-2.951*** (0.664)	-0.255* (0.146)	-0.325** (0.159)	-0.056 (0.181)	-0.254 (0.200)	0.041 (0.056)	0.133* (0.073)	-0.004 (0.032)	0.039 (0.042)	-2.012*** (0.385)	-0.065 (0.631)
Constant	1.298 (2.480)	4.036 (2.936)	-0.426 (0.584)	-0.556 (0.704)	-1.439** (0.640)	-2.490*** (0.803)	1.295*** (0.294)	0.628* (0.337)	0.100 (0.167)	-0.173 (0.193)	7.292*** (2.095)	0.665 (2.287)
Month Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Week-of-Year Fixed Effects	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	3,257	3,257	3,257	3,257	2,526	2,526	5,084	5,084	5,084	5,084	7,640	7,640
R ²	0.895	0.901	0.650	0.657	0.643	0.654	0.170	0.191	0.073	0.087	0.442	0.467
Adjusted R ²	0.892	0.896	0.638	0.639	0.630	0.634	0.141	0.155	0.041	0.046	0.423	0.445

Notes: Effects of significant weeks on the extent of under-reporting in Afghanistan (models 1 and 2); Afghanistan only fatal events (models 3 and 4); Iraq (models 5 and 6); the Philippines (models 7 and 8); the Philippines only fatal events (models 9 and 10); and South Africa (models 11 and 12). All specifications include month fixed effects, while even columns also include week-of-year fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Fatal and Non-Fatal Events

We find significant evidence of under-reporting of non-fatal violent events relative to those causing fatalities. These results can be seen in Figure 10. In Afghanistan, security force records indicate that ≈ 6.74 non-fatal events occurred for every fatal event. In contrast, the media-based data report ≈ 0.37 non-fatal events for every fatal event, a staggering difference. In the Philippines, we observe a similar though attenuated pattern: the JOC data show ≈ 2.18 non-fatal violent events for every fatal attack, whereas the media-based data give a ratio of $\approx 0.99:1$. Given that the majority of wartime attacks in Afghanistan and the Philippines involving security forces did not result in deaths, our findings suggest that the bulk of wartime violence goes unreported by the media.

Target and Perpetrator Identity

We find significant evidence of differences in reporting across both targets and perpetrators of violence. Results from Afghanistan show significant disparities between the number of fatal events tracked by ISAF/Afghan security forces and those in GED. Figure 11 shows a clear pattern of under-reporting of security force fatalities in the media data. The period when under-reporting of security force fatalities begins to increase substantially as Afghan forces start to assume a greater role over the country's security affairs, once the U.S. military ends its troop surge. One possible explanation for this under-reporting is that the instructions for reporting fatalities to the media occurred concurrently with this transition.

Results from the Philippines show significant differences in reporting depending upon perpetrator identity, as Figure 13 indicates. (Regression results can be found in Appendix Sub-Section 4.) Attacks carried out by the New People's Army (NPA), the rebel group responsible for the greatest number of attacks during the study period, were significantly less likely to be captured by news report-based datasets than those carried out by Islamic groups, Abu Sayyaf and the Moro Islamic Liberation Front (MILF). For every fatal attack involving these groups, the media based datasets were approximately three and four times more likely to report those by MILF and Abu

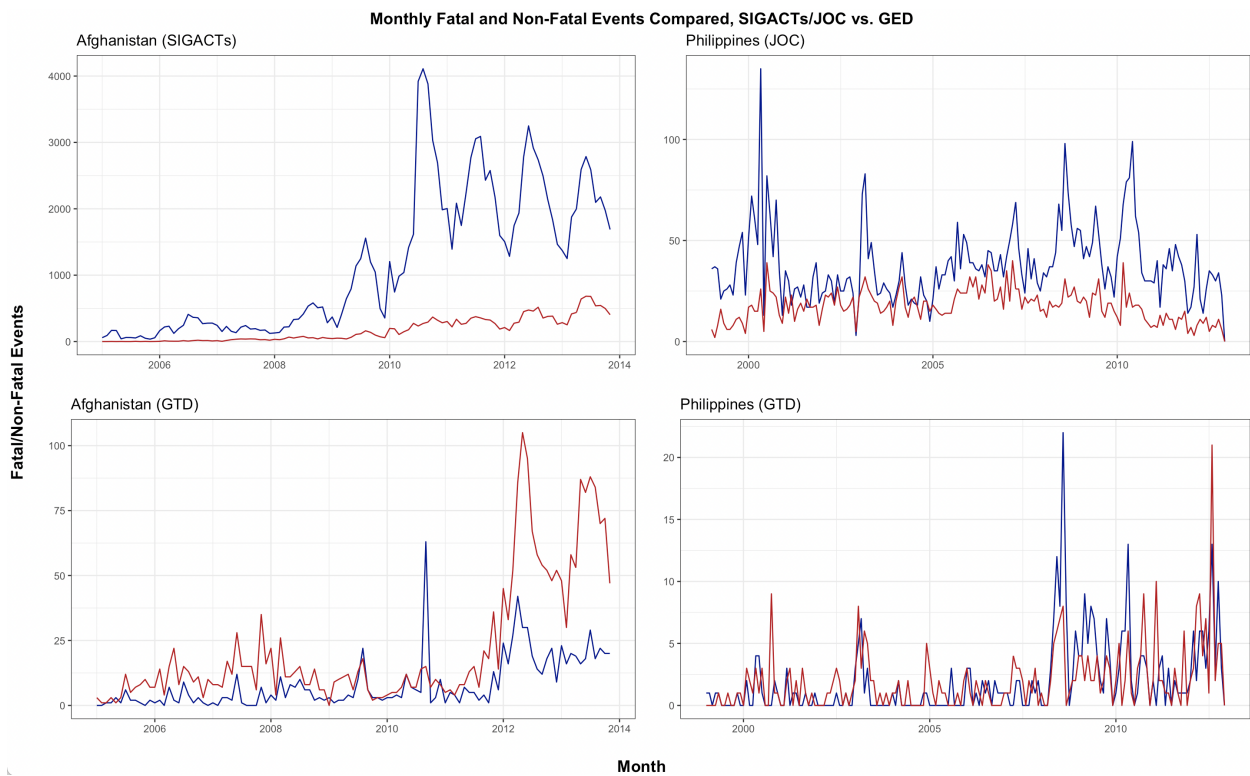


Figure 10: Monthly SIGACTs/JOC and GTD time series of wartime attacks in Afghanistan and the Philippines, disaggregated by outcome: fatal (red) and non-fatal (blue).

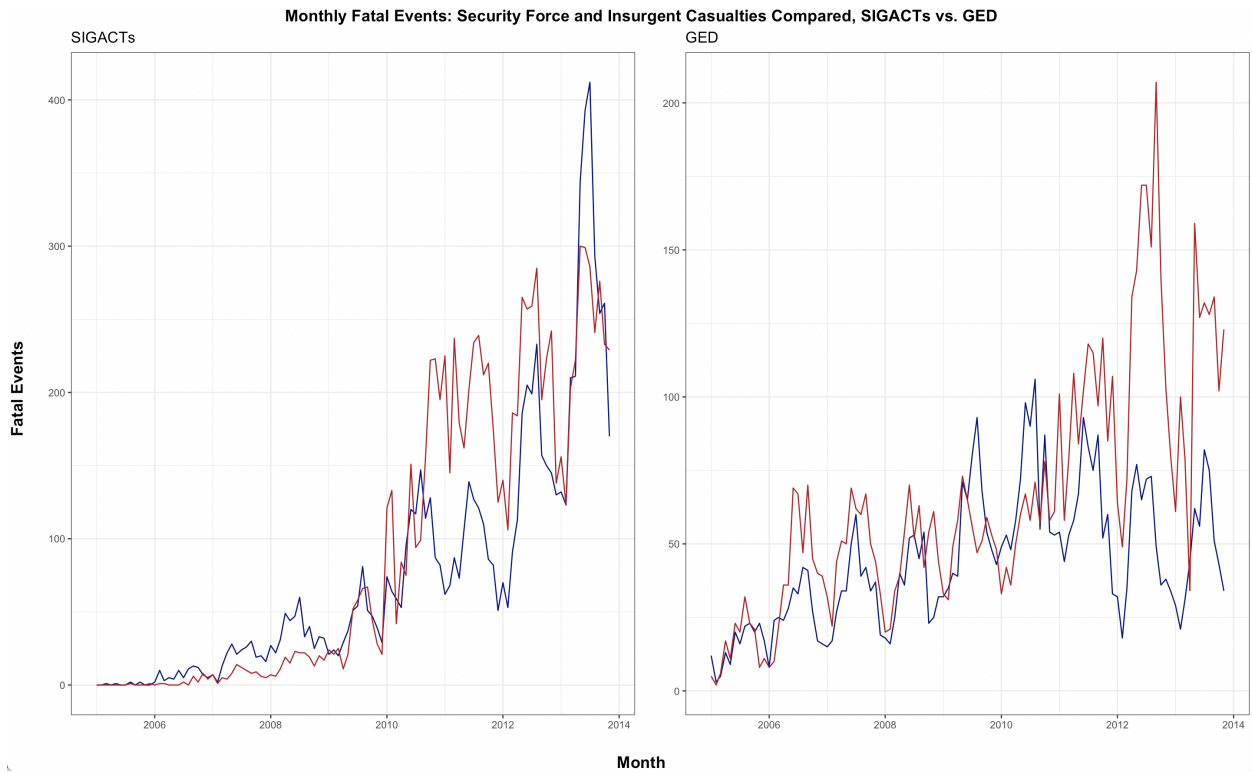


Figure 11: Differences in the number of fatal events tracked by SIGACTS (left panel) and GED (right panel), disaggregated by attacks killing members of security forces (blue) and insurgents (red).

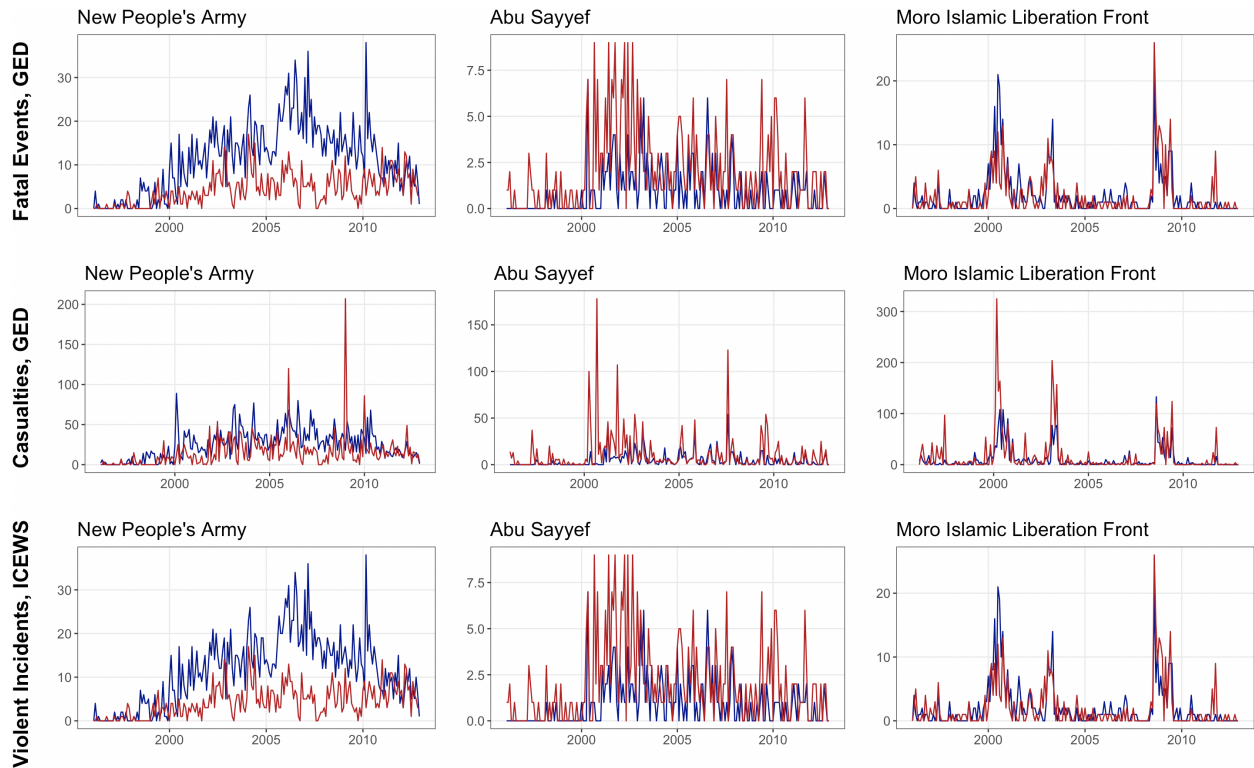


Figure 12: Differences in fatal events (top three panels); casualties (middle three panels); and violent incidents (bottom three panels), as tracked by Filipino security forces (blue) and news report-based datasets (red). The media based datasets appear to do a far better job tracking violence perpetrated by those organizations that identify with Islam: Abu Sayyaf and the Moro Islamic Liberation Front (MILF).

Sayyaf, respectively, than those perpetrated by the NPA. Individual casualties involving MILF and Abu Sayyaf were ≈ 2.9 times more likely to be reported. Finally, individual attacks, whether resulting in fatalities or not, were between ≈ 6 (MILF) and ≈ 29 (Abu Sayyaf) times more likely to be reported. While striking, these patterns are limited to the Philippines, for which comprehensive data on perpetrators is available. Future work could study whether similar patterns are observed in other settings.

Finally, results from South Africa show significant increases in the likelihood of news media reporting on protests and riots when they occurs in areas consisting predominantly of “Black African” and, separately, “Coloured” residents.³⁰ We cannot be sure that such patterns are driven

³⁰Results show similar but less well pronounced results for “Asians and Indians” (and results for “Other” show the opposite pattern; though, we note that such individuals make up approximately half of one percent of the population).

by race and ethnicity themselves. For instance, if such areas were less likely to experience protest/riot activity, estimated decreases might reflect (the absence or reduced influence of) a capability bias in such cases: with fewer activities to report, journalists may do a better job of capturing them in the first place. This was not the case in areas with significant shares of “Black African” populations (> 65%), which had higher average levels of unrest during the study period. It was, however, with areas with significant shares of “Coloured” populations. Furthermore, we note that our specifications controls for other factors like unemployment rate, which are likely to capture at least some variation in the likelihood of unrest. If we control directly for the previous year’s violence, generalized additive model results continue to show a strong negative relationship whereas the other results are ambiguous.³¹ Yet, regardless of whether the patterns we observe reflect a direct causal effect of race/ethnicity on reporting patterns, whether race/ethnicity proxy for an observed factor(s), or both, the implications for conflict event data derived from news media reports are the same: underreporting clearly systematically varies with the types of variables of interest to social scientists studying political violence in a manner that significantly threatens inferences derived relating to both the causes and consequences of political violence.

“REVERSE” REPLICATIONS

We have shown that datasets constructed from news reports systematically under-report particular types of violent events. Do such omissions matter, or do existing media-based datasets sufficiently resemble security force records in ways that make the former reasonable substitutes for the latter?

We consider whether prominent studies using the security force datasets (e.g. SIGACTs) would have produced similar results had media-based datasets been used in their place. In addition to the “reverse replications,” we also carry out one traditional replication. In total, we replicate eleven studies: we “reverse replicate” Berman et al. (2011); Condra and Shapiro (2012); Berman et al. (2013); Weidmann and Callen (2013); Crost et al. (2014); Shapiro and Weidmann (2015); Crost

³¹For cross-sectional models, we derive a lagged violence measure by summing the yearly lag of security force recorded activity in the panel dataset.

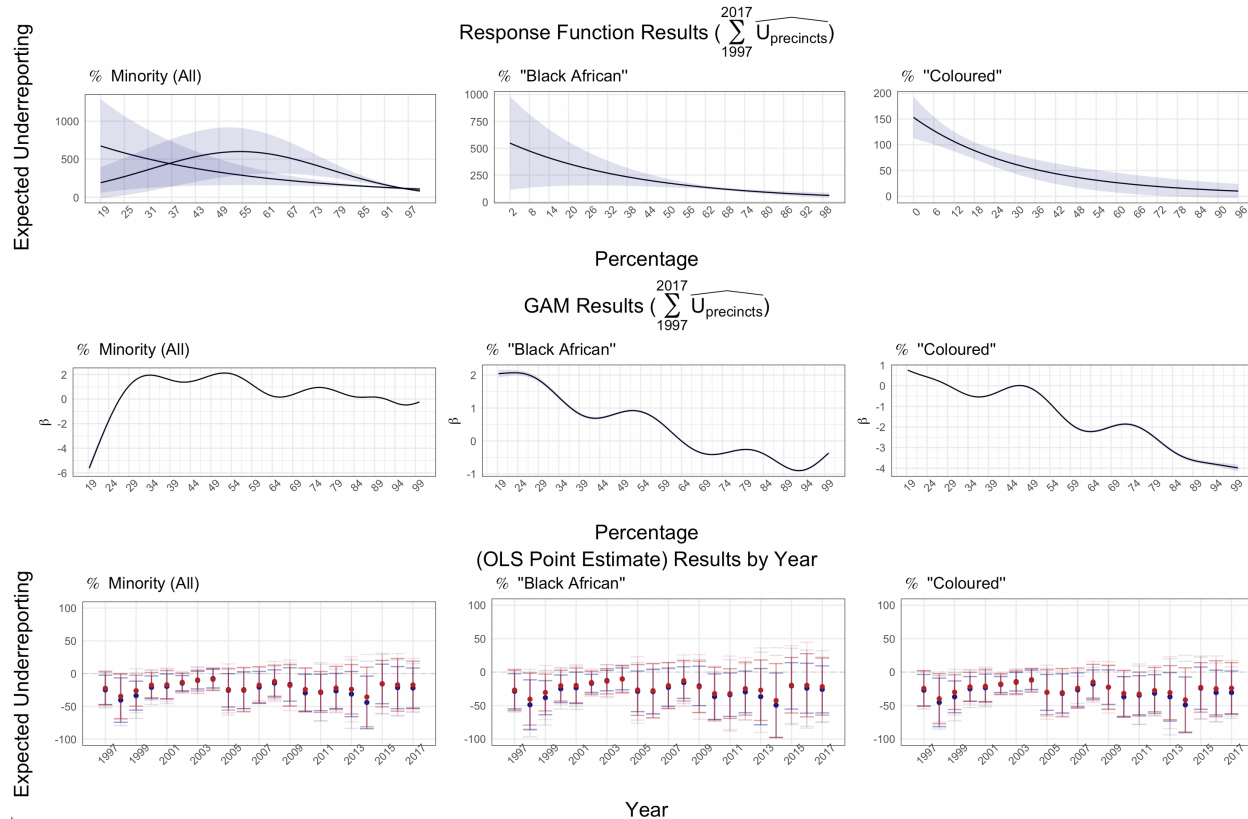


Figure 13: Estimated changes in under-reporting as a function of the racial/ethnic share of police precincts in South Africa. Those with the highest makeups of “Black African” and “Coloured” residents are far more likely to see coverage of protest and riot activity.

et al. (2016, 2018); Lyall (2019); Crost and Felter (2020), and directly replicate Reese et al. (2017). These articles appear in leading political science and economics journals including the *American Political Science Review*, *American Journal of Political Science*, and *American Economic Review*, and together account for 2,056 total citations. Two use data from Afghanistan; five from Iraq; and four from the Philippines. We replicate these eleven studies in particular given that they adopt temporal and spatial units of analysis in some or all of their analyses that can be replicated using media-based data.

For some articles, we directly replicate the studies, substituting the relevant dependent or independent variable with its media report-derived equivalent. In other cases, perfect substitutions are not possible, forcing us to construct and use similar measures.³² For completeness, we also analyze alternative variables where direct comparisons are available, which may work to the benefit of media-based datasets.³³ Additional replication details appear in the Appendix.

Results from the replication exercise appear in Figure 14. In most cases, results either fail to replicate altogether or only partially replicate. Of the total 271 of tests we carry out, only 81 (or 30%) generate results in which both the direction of the coefficient and statistical significance is maintained.³⁴ Given that we do not test whether the substantive magnitudes of effects change in ways that might indicate failure to replicate, the 30% figure is conservative: if we considered meaningful changes in magnitude, even fewer tests would replicate.

³²For instance, Berman et al. (2013) study insurgent attacks, which may or may not result in fatalities. Because GED only tracks events that result in one or more fatalities, a direct replication using GED is not possible. GED users may note that some non-fatal events do actually appear in that dataset. Given that GED's codebook defines an event as one involving a fatality, we are unsure why a small number of non-fatal events appear in the dataset.

³³For instance, when we reverse-replicate Crost et al. (2014) using measures of conflict casualties, we find that results mostly do not replicate. Yet when we use the related measure of fatal events (aggregating events that resulted in one or more casualty, rather than casualties themselves), results using GED do replicate. This might be evidence that in capturing fatality-causing events, datasets like GED do better than capturing the actual number of fatalities associated with such events.

³⁴If a test continues to generate results that are significant at conventional levels, then we confirm that statistical significance has been maintained.

No.	Article Name	Journal (Year)	Authors	Country and Study Period	Key independent variable	Dependent variable	Replicates with GED?	Replicates with GTD?	Replicates with ICEWS?
1	"Aid Under Fire: Development Projects and Civil Conflict"	<i>American Economic Review</i> (2014)	Benjamin Crost, Joseph Felter, and Patrick Johnston	Philippines (2002-2005)	Participation in development project	Conflict casualties	2/8 with casualties	2/8 with casualties	Not possible
							8/8 with fatal events	0/8 with violent events	0/8 with violent events
2	"Modest, Secure, and Informed: Successful Development in Conflict Zones"	<i>American Economic Review</i> (2013)	Eli Berman, Joseph H. Felter, Jacob N. Shapiro, and Erin Troland	Iraq (2004-2008)	Development spending	Changes in Insurgent attacks (Incidents)	5/7 with changes in fatal events	4/7 with changes in insurgent attacks	2/7 with changes in violent events
							5/7 with changes in casualties	5/7 with changes in casualties	Not possible
3	"Who Takes the Blame? The Strategic Effects of Collateral Damage"	<i>American Journal of Political Science</i> (2011)	Luke N. Condra, Jacob N. Shapiro	Iraq (2004-2009)	Killings (lagged difference) by different actors	Changes in Insurgent attacks per capita	3/6 with changes in fatal events per capita	2/6 with changes in insurgent attacks per capita	3/6 with changes in violent events per capita
							4/6 with changes in casualties per capita	2/6 with changes in casualties per capita	Not possible
4	"Days of Action or Restraint? How the Islamic Calendar Impacts Violence"	<i>American Political Science Review</i> (2017)	Michael J. Reese, Keven G. Ruby, Robert A. Pape	Iraq (2004-2011)	Both Religious Day and Public Holiday	Incidents of political violence	1/1 with SIGACTs violence		
5	"Civilian Casualties, Humanitarian Aid, and Insurgent Violence in Civil Wars"	<i>International Organization</i> (2019)	Jason Lyall	Afghanistan (2011-2013)	Eligibility	Insurgent attacks on civilians	1/4 with fatal events	2/4 with insurgent attacks on civilians	2/4 with insurgent attacks on civilians
							1/4 with civilian casualties	4/4 with civilian casualties	Not possible
6	"Is the Phone Mightier Than the Sword? Cellphones and Insurgent Violence in Iraq"	<i>International Organization</i> (2015)	Jacob N. Shapiro, Nils B. Weidmann	Iraq (2004-2009)	Additional cellphone coverage	First difference of insurgent violence per capita	1/7 with first difference of fatal events per capita	0/7 with first difference of violent incidents per capita	2/7 with first difference of violent incidents per capita
							2/7 with with first difference of casualties per capita	0/7 with first difference of casualties per capita	Not possible
7	"Conditional Cash Transfers, Civil Conflict and Insurgent Influence: Experimental Evidence from the Philippines"	<i>Journal of Development Economics</i> (2016)	Benjamin Crost, Joseph H. Felter, Patrick B. Johnston	Philippines (2001-2010)	Program treatment	Violent incidents	0/4 with fatal events	0/4 with violent incidents	0/4 with violent incidents
							0/4 with casualties	0/4 with casualties	Not possible
8	"Export Crops and Civil Conflict"	<i>Journal of the European Economic Association</i> (2020)	Benjamin Crost, Joseph Felter	Philippines (2001-2009)	Value of banana production	Violent incidents (defined as incidents resulting in at least one casualty)	0/4 with violent incidents	0/4 with violent incidents	4/4 with violent incidents
							0/4 with casualties	0/4 with casualties	Not possible
9	"Can Hearts and Minds Be Bought? The Economics of Counterinsurgency in Iraq"	<i>Journal of Political Economy</i> (2011)	Eli Berman, Jacob N. Shapiro, Joseph H. Felter	Iraq (2004-2008)	CERP spending	intensity of insurgent activity measured as attacks per capita	0/6 with fatal events per capita	3/6 with violent incidents per capita	0/6 with violent incidents per capita
							1/6 with casualties per capita	2/6 with casualties per capita	Not possible
10	"Violence and Election Fraud: Evidence from Afghanistan"	<i>British Journal of Political Science</i> (2013)	Nils B. Weidmann, Michael Callen	Afghanistan (2009)	Violence (number of attacks per capita against ISAF)	Fraud	5/8 with fatal events per capita	0/8 with violent incidents per capita	3/8 violent incidents per capita against the International Security Assistance Force
11	"Climate Change, Agricultural Production and Civil conflict: Evidence from the Philippines"	<i>Journal of Environmental Economics and Management</i> (2018)	Benjamin Crost, Claire Duquenois, Joseph H. Felter, Daniel Rees	Philippines (2001-2009)	Casualties and violent incidents	Precipitation	0/2 with casualties	0/2 with casualties	Not possible
							0/4 with violent incidents	0/4 with violent incidents	0/4 with violent incidents

Figure 14: "Reverse" and standard replication results.

CONCLUSION

We have shown that violent event datasets constructed from media reports exhibit systematic biases. In a large-scale “reverse replication” exercise, these datasets fail to produce similar results to those in existing studies using more complete security force data. What alternative methods of sourcing data exist? (Due to space restrictions, a more complete discussion of these alternatives appears in the Appendix.)

One obvious substitute for media-based event datasets are those built by state security forces, such as those used in this study. Yet these datasets are exceedingly rare, are often difficult to access, and in low-capacity countries may themselves suffer from reporting biases. Another substitute could be surveys of civilian populations, yet errors of recall (Öztaş Ayhan and İşiksal 2005), social desirability bias (DeMaio 1984) and “logistical and ethical issues that accompany survey research in a wartime setting” (Lyll et al. 2013) render surveys an impractical substitute for violent events data. (In the Appendix we discuss questions we embedded in a 2021 Mercy Corps survey of 500+ households in Iraq’s Sinjar District. The exercise demonstrates highly problematic levels of item non-response to questions about exposure to violence.)

Remote sensing technologies – whereby satellite imagery gathers information from hard-to-reach locations – could represent another alternative. However, using satellite images to sense the details of a particular event (i.e. casualties, type of attack) is rarely feasible. Crowdsourcing – wherein organizations offer civilians an opportunity to report violent incidents that they witness – is another potential source of data, yet crowdsourcing can be biased due to the civilian population’s awareness of the crowdsourcing operation (De Juan and Bank 2015; Bailard and Livingston 2014), as well as significant under-reporting (De Juan and Bank 2015; Wirtschafter 2021). Investigations by truth and reconciliation commissions (TRCs) could provide another alternative, yet the accuracy of their conclusions has been questioned: the correction of biased and missing data in Peru, for example, likely led to *overestimations* of deaths, in addition to wrongful attribution of deaths to particular actors (Rendon 2019a,b). Relying more on local reporters might reasonably improve the quality of media-based datasets, yet doing so would likely shift capability and editorial biases

affecting violent event datasets, rather than eliminate them.

Given the centrality of media-based datasets, and the difficulty of developing alternatives, we believe that future work might instead develop formal solutions (“corrections”) to help control for systematic missingness in media datasets. The editorial and capability biases that influence the kinds of violent events the media report on are severe. We have demonstrated empirically – systematically comparing news report-based datasets to high quality administrative records of wartime violence and social unrest in Afghanistan, Iraq, the Philippines, South Africa, and Syria – that these biases affect the quality of inferences drawn from media-based datasets. This empirical exercise helps quantify the magnitude of specific sources of systematic missingness in media reports of conflict and the datasets that use them. As we have shown, reporting biases may manifest differently across particular contexts. Failing to acknowledge these biases threatens both knowledge accumulation in the social sciences and hampers effective decision-making at the national and international levels, which has come to rely on media-based data. Without access to more comprehensive and reliable data, both social scientists and policymakers will be hamstrung in their efforts to resolve intractable conflicts and to mitigate the suffering that accompany them.

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APPENDIX

FOR ONLINE PUBLICATION

A.1 NEWS MEDIA REPORT BASED DATASETS

Georeferenced Events Database (GED)

A collaboration between Uppsala Conflict Data Program and Peace Research Institute Oslo, the Georeferenced Events Database compiles incidents of fatal violence globally from January 1, 1989 through December 31, 2020 (partially excluding Syria). The GED defines an event as an incident “where armed force was [used] by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death” (Croicu and Sundberg 2015). Additionally, an incident must include specific time and location information, otherwise it is excluded. The GED collects information on potential “events” using global newswire reporting, the monitoring of global BBC local reporting and other local media, plus NGO and field reports. It relies on human coders to discern if an incident qualifies as an event or not (Croicu and Sundberg 2015).

Global Terrorism Database (GTD)

Funded by the Study of Terrorism and Responses to Terrorism (START) at the University of Maryland, College Park, the Global Terrorism Database (GTD) is an ongoing collection effort that includes global incidents from January 1, 1970 through December 31, 2019. The GTD defines a terrorist event as “the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation” (Codebook 2015). The GTD draws on public, open-source materials, such as news archives, existing conflict datasets, and books and journals.¹ The GTD codes an attack as an “event” if it 1) is intentional; 2) is violent or has an immediate threat of violence; and 3) is conducted by sub-national actors (Codebook 2015). Additionally, the event must be committed with the goal of

¹The GTD includes several media-based conflict datasets, one of the most notable being the Worldwide Incidents Tracking System (WITS). Discontinued in 2012, the WITS dataset used media collection methods to compile a collection of violent incidents around the world. After 2012, its relevant events were folded into the GTD.

attaining political, religious, social, or systemic economic change, and there must be evidence that it was committed with the goal of coercing, intimidating, or conveying a message to an audience broader than the immediate victims. The attack must also be committed outside the context of “legitimate warfare activities” (Codebook 2015).

Integrated Crisis Early Warning System (ICEWS)

The Integrated Crisis Early Warning System (ICEWS) database (O’Brien 2010) “captures and processes millions of pieces of data from digitized news media, social media and other sources to predict, track and respond to events around the world ...” The database includes incidents from January 1, 1995 through April 30, 2020. The data “consists of coded interactions between socio-political actors (i.e., cooperative or hostile actions between individuals, groups, sectors and nation states). Events are automatically identified and extracted from news articles by the BBN ACCENT event coder” (ICEWS 2016).

The Social Conflict Analysis Database (SCAD)

The Social Conflict Analysis Database (SCAD) dataset (Salehyan et al. 2012) tracks “protests, riots, strikes, inter-communal conflict, government violence against civilians, and other forms of social conflict not systematically tracked in other conflict datasets” (Strauss 2010).² This dataset is somewhat more limited in country coverage but has been expanding and currently includes “all of Africa ... Mexico, Central America, and the Caribbean” (Salehyan and Hendrix 2017). Coverage runs from January 1, 1990 to December 31, 2017.

²This dataset was previously called the Social Conflict in Africa Dataset, using the same acronym, SCAD.

A.2 ADDITIONAL REGRESSION RESULTS

A.2.1 Violence Intensity

Table 1: Violence Intensity Results, Iraq-Syria

	Estimated Under-Reporting		
	(1)	(2)	(3)
Violent Events	1.000*** (0.0003)	0.998*** (0.001)	0.997*** (0.001)
Violent Events²		0.0001*** (0.00001)	0.0001 (0.0001)
Violent Events³			-0.00000 (0.00000)
Constant	0.006 (0.012)	0.014 (0.012)	0.015 (0.012)
District Fixed Effects	Y	Y	Y
Month Fixed Effects	Y	Y	Y
Observations	9,794	9,794	9,794
R ²	1.000	1.000	1.000
Adjusted R ²	1.000	1.000	1.000

Notes: Effects of violent events (column 1), violent events squared (column 2), and violent events cubed (column 3) on the extent of under-reporting in Iraq/Syria. All specifications include month fixed effects and district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Violence Intensity Results, Afghanistan

	Estimated Under-Reporting					
	(1)	(2)	(3)	(4)	(5)	(6)
Violent Events	0.996*** (0.0001)	0.993*** (0.0002)	0.990*** (0.0003)			
Violent Events²		0.00004*** (0.00000)	0.0001*** (0.00001)			
Violent Events³			-0.00000*** (0.00000)			
Fatal Events				0.855*** (0.001)	0.819*** (0.002)	0.733*** (0.003)
Fatal Events²					0.005*** (0.0002)	0.028*** (0.001)
Fatal Events³						-0.001*** (0.00002)
Constant	-0.259*** (0.010)	-0.258*** (0.010)	-0.257*** (0.010)	-0.214*** (0.019)	-0.210*** (0.019)	-0.201*** (0.019)
District Fixed Effects	Y	Y	Y	Y	Y	Y
Month Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	185,866	185,866	185,866	185,866	185,866	185,866
R ²	0.999	0.999	0.999	0.724	0.725	0.728
Adjusted R ²	0.999	0.999	0.999	0.724	0.724	0.728

Notes: Effects of violent events (column 1), violent events squared (column 2), violent events cubed (column 3), fatal events (column 4), fatal events squared (column 5), and fatal events cubed (column 6) on the extent of under-reporting in Afghanistan. All specifications include month fixed effects and district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Violence Intensity Results, Iraq

	Estimated under-reporting		
	(1)	(2)	(3)
Violent Events	1.002*** (0.002)	0.989*** (0.002)	0.974*** (0.003)
Violent Events²		0.001*** (0.0001)	0.003*** (0.0004)
Violent Events³			-0.00004*** (0.00001)
Constant	0.061 (0.041)	0.058 (0.041)	0.057 (0.041)
District Fixed Effects	Y	Y	Y
Month Fixed Effects	Y	Y	Y
Observations	37,648	37,648	37,648
R ²	0.944	0.944	0.944
Adjusted R ²	0.944	0.944	0.944

Notes: Effects of violent events (column 1), violent events squared (column 2), and violent events cubed (column 3) on the extent of under-reporting in Iraq. All specifications include month fixed effects and district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Violence Intensity Results, the Philippines

	Estimated under-reporting					
	(1)	(2)	(3)	(4)	(5)	(6)
Violent Events	0.971*** (0.001)	0.966*** (0.001)	0.978*** (0.002)			
Violent Events²		0.001*** (0.0001)	-0.004*** (0.001)			
Violent Events³			0.0002*** (0.00002)			
Fatal Events				0.811*** (0.003)	0.857*** (0.005)	0.888*** (0.010)
Fatal Events²					-0.025*** (0.002)	-0.055*** (0.008)
Fatal Events³						0.004*** (0.001)
Constant	0.002 (0.014)	0.002 (0.014)	0.002 (0.014)	0.004 (0.021)	0.004 (0.021)	0.004 (0.021)
District Fixed Effects	Y	Y	Y	Y	Y	Y
Month Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	59,614	59,614	59,614	59,614	59,614	59,614
R ²	0.957	0.957	0.957	0.559	0.560	0.560
Adjusted R ²	0.957	0.957	0.957	0.557	0.558	0.558

Notes: Effects of violent events (column 1), violent events squared (column 2), violent events cubed (column 3), fatal events (column 4), fatal events squared (column 5), and fatal events cubed (column 6) on the extent of under-reporting in the Philippines. All specifications include month fixed effects and district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Violence Intensity Results, South Africa

	Estimated under-reporting		
	(1)	(2)	(3)
Violent Events	0.998*** (0.0001)	0.998*** (0.0001)	0.998*** (0.0001)
Violent Events²		0.00001*** (0.00000)	0.00000 (0.00000)
Violent Events³			0.00000* (0.00000)
Constant	0.002 (0.004)	0.001 (0.004)	0.001 (0.004)
District Fixed Effects	Y	Y	Y
Month Fixed Effects	Y	Y	Y
Observations	226,044	226,044	226,044
R ²	0.999	0.999	0.999
Adjusted R ²	0.999	0.999	0.999

Notes: Effects of violent events (column 1), violent events squared (column 2), and violent events cubed (column 3) on the extent of under-reporting in South Africa. All specifications include month fixed effects and district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Nighttime Light Results

	Estimated Underreporting			(4) Afgh. Casualties	under_cas			(7) Iraq	(8) Iraq	(9) Iraq	under_all			(13) Phil. Casualties	under_cas		(16) South Africa	under_all	
	(1) Afghanistan	(2) Afghanistan	(3) Afghanistan		(5) Afgh. Casualties	(6) Afgh. Casualties	(10) Philippines				(11) Philippines	(12) Philippines	(14) Phil. Casualties		(15) Phil. Casualties	(17) South Africa		(18) South Africa	
Mean Nighttime Lighting	-0.056 (0.051)	-0.052 (0.095)	-0.081 (0.135)	-0.010 (0.006)	-0.018* (0.011)	-0.024 (0.016)	-0.033** (0.016)	-0.021 (0.037)	-0.114* (0.061)	0.005*** (0.002)	0.013*** (0.004)	0.047*** (0.007)	-0.0003 (0.0003)	-0.0003 (0.001)	0.0004 (0.001)	0.226*** (0.016)	-0.006 (0.037)	-0.019 (0.058)	
Mean Nighttime Lighting ²		-0.0001 (0.002)	0.002 (0.008)		0.0002 (0.0002)	0.001 (0.001)		-0.0002 (0.001)	0.005* (0.003)		-0.0002** (0.0001)	-0.002*** (0.0004)		0.00000 (0.00001)	-0.00004 (0.0001)		0.005*** (0.001)	0.006** (0.003)	
Mean Nighttime Lighting ³			-0.00003 (0.0001)			-0.00001 (0.00001)		-0.0001* (0.00004)				0.00002*** (0.00000)			0.00000 (0.00000)			-0.00001 (0.00004)	
Constant	0.855 (2.226)	0.842 (2.244)	0.566 (2.418)	-0.196 (0.257)	-0.165 (0.259)	-0.219 (0.280)	-1.347* (0.773)	-1.434* (0.813)	-1.059 (0.836)	1.327*** (0.101)	1.315*** (0.101)	1.261*** (0.102)	0.058*** (0.018)	0.058*** (0.018)	0.057*** (0.018)	-0.826 (0.745)	-0.987 (0.744)	-0.987 (0.744)	
District Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Month Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Observations	27,860	27,860	27,860	27,860	27,860	27,860	5,824	5,824	5,824	148,230	148,230	148,230	148,230	148,230	148,230	19,251	19,251	19,251	
R ²	0.352	0.352	0.352	0.252	0.252	0.252	0.398	0.398	0.398	0.343	0.343	0.343	0.139	0.139	0.139	0.540	0.541	0.541	
Adjusted R ²	0.341	0.341	0.341	0.239	0.239	0.239	0.381	0.381	0.381	0.342	0.342	0.343	0.138	0.138	0.138	0.533	0.534	0.534	

Notes: Effects of mean nighttime lighting (columns 1, 4, 7, 10, 13, and 16), mean nighttime lighting squared (columns 2, 5, 8, 11, 14, and 17), and mean nighttime lights cubed (column 3, 6, 9, 12, 15, and 18) on the extent of under-reporting in Afghanistan (columns 1, 2, 3, 4, 5, and 6); Iraq (columns 7, 8 and 9); Philippines (columns 10, 11, and 12) and South Africa (columns 16, 17, and 18). All specifications include month fixed effects and district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effects of Nighttime Light on Estimated Under-Reporting

	Estimated under-reporting											
	(1) Afghanistan	(2) Afghanistan	(3) Afghanistan	(4) Iraq	(5) Iraq	(6) Iraq	(7) Philippines	(8) Philippines	(9) Philippines	(10) South Africa	(11) South Africa	(12) South Africa
Mean Nighttime Lighting	-0.056 (0.051)	-0.052 (0.095)	-0.081 (0.135)	-0.033** (0.016)	-0.021 (0.037)	-0.114* (0.061)	0.005*** (0.002)	0.013*** (0.004)	0.047*** (0.007)	0.226*** (0.016)	-0.006 (0.037)	-0.019 (0.058)
Mean Nighttime Lighting ²		-0.0001 (0.002)	0.002 (0.008)		-0.0002 (0.001)	0.005* (0.003)		-0.0002** (0.0001)	-0.002*** (0.0004)		0.005*** (0.001)	0.006** (0.003)
Mean Nighttime Lighting ³			-0.00003 (0.0001)			-0.0001* (0.00004)			0.00002*** (0.00000)			-0.00001 (0.00004)
Constant	0.855 (2.226)	0.842 (2.244)	0.566 (2.418)	-1.347* (0.773)	-1.434* (0.813)	-1.059 (0.836)	1.327*** (0.101)	1.315*** (0.101)	1.261*** (0.102)	-0.826 (0.745)	-0.987 (0.744)	-0.987 (0.744)
District Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	27,860	27,860	27,860	5,824	5,824	5,824	148,230	148,230	148,230	19,251	19,251	19,251
R ²	0.352	0.352	0.352	0.398	0.398	0.398	0.343	0.343	0.343	0.540	0.541	0.541
Adjusted R ²	0.341	0.341	0.341	0.381	0.381	0.381	0.342	0.342	0.343	0.533	0.534	0.534

Notes: Effects of mean nighttime lighting (columns 1, 4, 7, 10, 13, and 16), mean nighttime lighting squared (columns 2, 5, 8, 11, 14, and 17), and mean nighttime lights cubed (column 3, 6, 9, 12, 15, and 18) on the extent of under-reporting in Afghanistan (columns 1, 2, 3, 4, 5, and 6); Iraq (columns 7, 8 and 9); Philippines (columns 10, 11, and 12) and South Africa (columns 16, 17, and 18). All specifications include month fixed effects and district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Additional Electricity Results: Iraq War Electricity Shortages

As a supplement to the nighttime light analysis, we use daily data on the numbers of hours of electricity available Iraq and, separately, to Baghdad and Basrah specifically (Shaver and Tenorio 2014), to construct a measure of hours without electricity per day for each. We then regress estimated under-reporting per day (\widehat{U}_d) on the number of hours of without power using 1) time series data for all of Iraq, and 2) a panel consisting of Baghdad and Basrah.³ We also test specifications that include month and, separately, week fixed effects to account for possible time-varying confounders that might correlate with electricity supply and effect under-reporting.

Figure 14 shows the distributions of hours without power for all of Iraq and Baghdad, and Basrah specifically. The average number of hours without power nation wide was ≈ 11.48 . In Baghdad and Basrah, these averages were ≈ 13.5 and ≈ 8.93 hours, respectively.

For a one standard deviation (σ) increase in hours without power across Iraq (≈ 2.62), using results from the week fixed effects model, the estimated change in under-reporting is approximately half of one attack (≈ 0.42). Given Iraq's mean daily hours without power of ≈ 11.5 , the results suggest that approximately two attacks per day went unreported for lack of electricity (or some factor correlated with it). Importantly, however, hours of power varied significantly throughout the conflict. Baghdad, in particular, saw particularly significant fluctuations ($\sigma \approx 4.34$ hours), suggesting that under-reporting also varied widely across time and space during the war.⁴ Results are shown in Figure 15 and Table 10.

³We adopt these two units of analysis given that the State Department's data estimates hours of electricity per day at only these three levels: national, Baghdad, and Basrah.

⁴Any identified relationship between hours of power and under-reporting may reflect either a direct effect of lack of electricity on under-reporting by the media, or the role of some other factor (e.g. violence intensity) that is responsible for under-reporting. Violence (through direct or collateral attacks on electricity infrastructure) may reduce electricity levels, meaning that an effect of violence intensity on under-reporting may be mediated through effects on electricity supply. We are agnostic about the actual causal processes at work and are primarily interested in showing that reporting on violence by the news media follows predictable patterns that reveal systematic under-reporting.

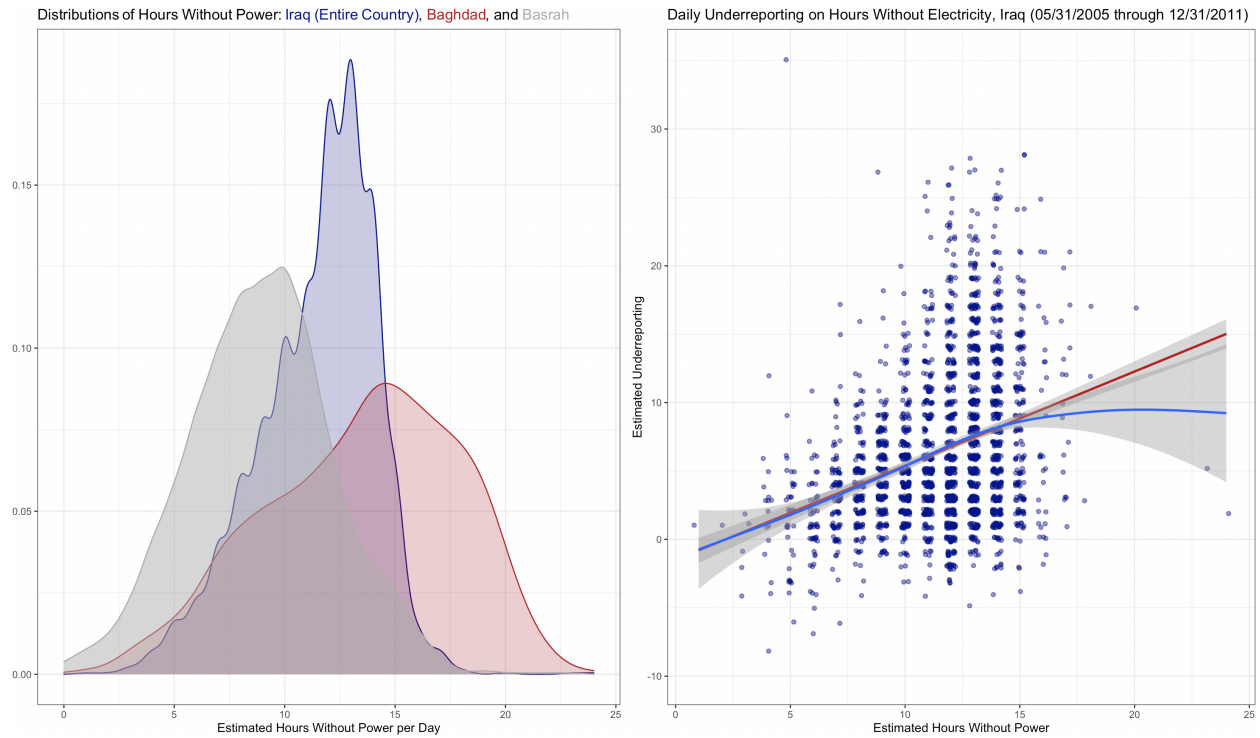


Figure 1: Distributions of hours without power for all of Iraq, Baghdad, and Basrah (left) and daily hours without power for all of Iraq (right) against \widehat{U}_d for the entire country. Data for Iraq, Baghdad, and Basrah cover the periods 2005-05-31 through 2011-12-31; 2005-01-01 through 2011-12-31; 2005-07-04 through 2011-12-31, respectively. OLS and GAM models estimate the relationship between these variables. Given overlapping data points, noise is applied for legibility.

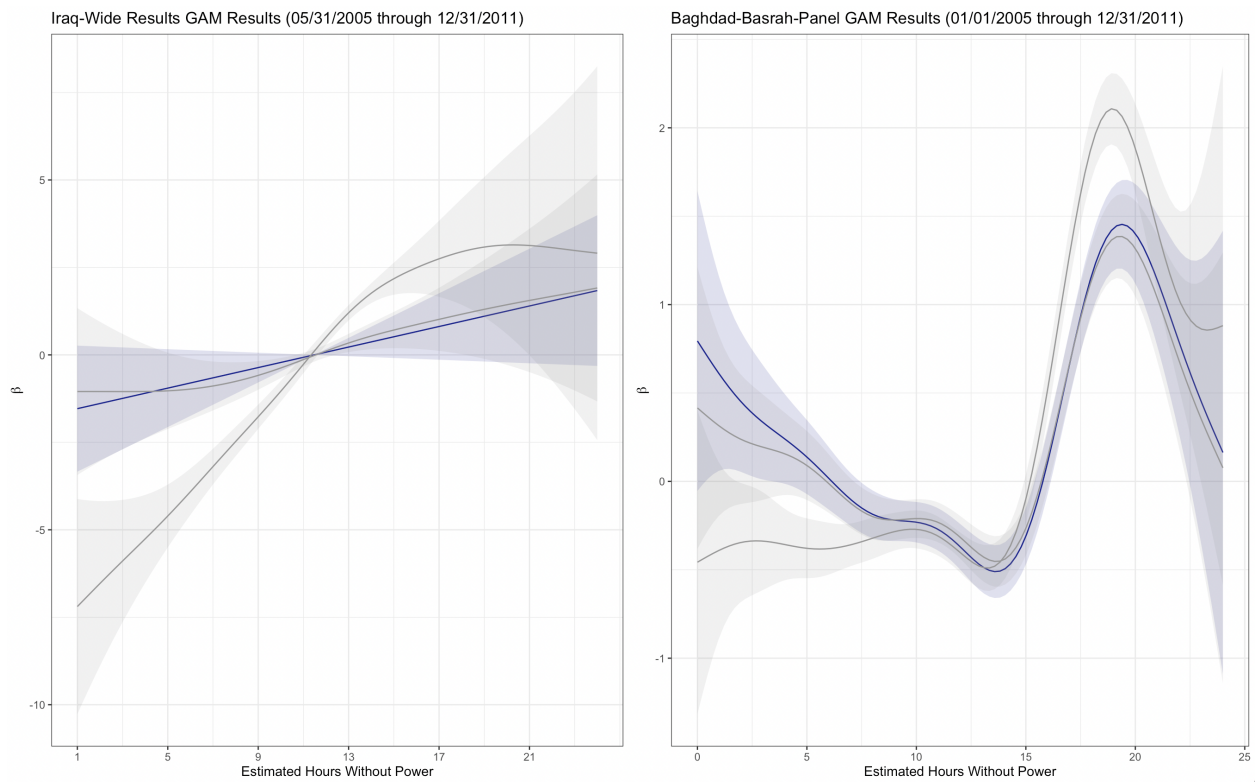


Figure 2: Generalized additive models of estimated hours without power in Iraq (left) and Baghdad

Table 8: Effects of Hours without Electricity on Estimated Under-reporting

	Estimated under-reporting					
	(1) (Iraq Time Series)	(2) (Baghdad-Basrah Panel)	(3) (Iraq Time Series)	(4) (Baghdad-Basrah Panel)	(5) (Iraq Time Series)	(6) (Baghdad-Basrah Panel)
Hours without Electricity	0.688*** (0.043)	0.110*** (0.007)	0.196*** (0.068)	0.059*** (0.011)	0.147* (0.086)	0.064*** (0.012)
Constant	-1.494*** (0.505)	1.174*** (0.109)	-2.143 (3.800)	-1.381*** (0.368)	-1.607 (2.238)	-2.024 (1.800)
Month Fixed Effects	N	N	Y	Y	N	N
Week Fixed Effects	N	N	N	N	Y	Y
Governorate Fixed Effects	N	Y	N	Y	N	Y
Observations	2,362	4,726	2,362	4,726	2,362	4,726
R ²	0.098	0.307	0.614	0.437	0.681	0.475
Adjusted R ²	0.098	0.306	0.600	0.427	0.627	0.433

Notes:

*p<0.1; **p<0.05; ***p<0.01

Coverage by Perpetrator

Table 9: Effects of Casualties, Fatal Events, and Violent Events on Estimated Under-reporting in the Philippines

	GED			GED			ICEWS		
	(NPA)	(ASG)	(MILF)	(NPA)	(ASG)	(MILF)	(NPA)	(ASG)	(MILF)
Casualties	0.227*** (0.028)	0.920*** (0.103)	0.713*** (0.048)						
Fatal Events				0.452*** (0.070)	1.295*** (0.172)	1.298*** (0.097)			
Violent Events							0.051*** (0.006)	1.482*** (0.219)	0.307*** (0.019)
Constant	1.799*** (0.390)	1.217*** (0.163)	0.457** (0.198)	3.575 (2.193)	4.752*** (1.519)	3.407 (2.236)	1.068* (0.610)	0.495 (2.552)	-0.928 (0.706)
Observations	203	203	203	203	203	203	203	203	203
R ²	0.244	0.285	0.523	0.172	0.221	0.474	0.287	0.186	0.576
Adjusted R ²	0.240	0.282	0.520	0.168	0.217	0.471	0.284	0.182	0.574

Notes:

*p<0.1; **p<0.05; ***p<0.01

A.2.2 Capital/Major City Analysis

Table 10: Capital/Major City Results

	(IQ/SY)	(AF)	(AF - Fatal)	(IQ)	(PH)	(PH - Fatal)	(SA)
Capital/Major City	4.536*** (0.511)	-2.305*** (0.328)	-0.536*** (0.124)	1.113*** (0.074)	-0.332 (0.274)	0.197 (0.279)	5.984*** (0.062)
Constant	1.676 (1.984)	1.000 (8.338)	1.000 (1.614)	0.333 (1.618)	1.000 (1.116)	1.250*** (0.302)	0.877 (1.349)
Month Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Observations	1,307	31,877	10,593	6,856	6,372	2,573	49,618
R ²	0.071	0.035	0.046	0.114	0.072	0.121	0.186
Adjusted R ²	0.061	0.032	0.037	0.103	0.046	0.060	0.182

Note:

*p<0.1; **p<0.05; ***p<0.01

A.2.3 Reverse Replications

This section provides more details about the reverse replications discussed in the manuscript.

Crost and Felter (2020)

Crost and Felter (2020) use violent incidents (defined as incidents resulting in at least one casualty) as the dependent variable of their analysis. We replicated their main results displayed in Table 2 using GED, GTD, and ICEWS data. For both GED and GTD data, we created a variable of violent incidents defined in the same way as Crost and Felter (2020). For ICEWS, we created a variable of both fatal and non-fatal violent incidents.

Crost et al. (2014)

Crost et al. (2014) use conflict casualties as the dependent variable of their different specifications contained in Table 4. We replicated these results using GED and GTD data. For both GED and GTD data, we used the same independent variable (casualties) to replicate their main results. For ICEWS data, we could not replicate the results using Crost et al. (2014)'s definition of the dependent variable as data on casualties is not available. However, we replicated the main results using violent incidents.

Berman et al. (2013)

Berman et al. (2013) use changes in insurgent attacks (incidents) as their dependent variable to measure the effect of aid spending on violence. We replicated their main results displayed on Table 1 using GED, GTD, and ICEWS data. In the case of GED, we created a variable that captures the changes in insurgent attacks, defined as incidents resulting in at least one casualty. Additionally, as our violent incidents variable differs from the exact definition of Berman et al. (2013)'s independent variable, we also replicated the results with a variable that capture the changes in the number of casualties. For GTD data, we created a variable of changes in insurgent attacks, both fatal and non-fatal, defined in the same way as Berman et al. (2013). For ICEWS, used a variable

that captures the changes on violent incidents, both fatal and non-fatal.

Condra and Shapiro (2012)

Condra and Shapiro (2012) use the difference in the number of insurgent attacks per capita as their dependent variable. We replicated the results contained in Table 1 using GED, GTD, and ICEWS data. For GED, we used the difference in the number of violent incidents per capita, defined as incidents resulting in at least one casualty, as our dependent variable. As our definition of the dependent variable do not match exactly the one of Condra and Shapiro (2012), we performed another replication using the difference in the number of casualties per capita as the dependent variable. In the case of GTD data, we used as our dependent variable the difference in the number of insurgent attacks per capita, following Condra and Shapiro (2012)'s same definition. For ICEWS, we created a variable that captures the changes in violent events per capita.

Reese et al. (2017)

Reese et al. (2017) use the incidences of political violence as the dependent variable to test the effect of the Islamic calendar on this particular type of violence. We replicated the results contained in Table 5 – Model 4 for Iraq using SIGACTs dataset. We created a variable to capture all violent incidents and used it as our dependent variable.

Lyall (2019)

Lyall (2019) uses insurgent attacks on civilians as the dependent variable to measure the effect of aid on violence. We replicated these results using GED, GTD and ICEWS data. For GED we created a variable to capture all violent incidents of Taliban against civilians, defined as incidents resulting in at least one casualty. We also replicated the results contained in Figure 4, Panel C using civilian casualties as the dependent variable. For GTD and ICEWS, we created a variable to capture all insurgent attacks (fatal and non-fatal) against civilians, following Lyall's (2019) definition.

Shapiro and Weidmann (2015)

Shapiro and Weidmann (2015) use the first difference of insurgent violence per capita (that includes attacks against Coalition and Iraqi government forces) as their dependent variable. We replicated the results contained in Table 1 using GED, GTD and ICEWS data. For GED, we created a variable that captures the first difference of all violent incidents per capita, defined as incidents resulting in at least one casualty, and used it as the dependent variable in our replication. For GTD and ICEWS, we created a variable to capture the difference of all violent incidents (both fatal and non-fatal) per capita, and replicated their main results.

Crost et al. (2016)

Crost et al. (2016) use the number of violent incidents (models 1 and 2 in Table 5) and any violent incidents (models 3 and 4 in Table 6) as their dependent variables to capture the causal effect of CCT programs on violence in the Philippines. We replicated these results using GED, GTD, and ICEWS data. For GED, we created a variable that capture all violent incidents, defined as incidents resulting in at least one casualty. For GTD and ICEWS, we created a variable to capture all violent incidents (both fatal and non-fatal).

Berman et al. (2011)

Berman et al. (2011) use attacks per capita against Coalition and Iraqi government forces as their dependent variable to evaluate the effect of CERP spending on violence. We replicated the results contained in Table 4 using GED, GTD and ICEWS data. For GED, we created a variable that captures all violent incidents per capita, defined as incidents resulting in at least one casualty. We also replicated the results using casualties per capita as our dependent variable. For GTD and ICEWS, we created a variable that capture all violent incidents (both fatal and non-fatal) per capita and replicated the results of Table 4.

Weidmann and Callen (2013)

Weidmann and Callen (2013) use a measure of violence that captures the number of attacks per capita against the International Security Assistance Force (ISAF) as their independent variable. We replicated the results contained in Table 2 using GED, GTD, and ICEWS data. For GED, we created a variable that captures all violent incidents (defined as incidents resulting in at least one casualty) per capita. We also replicated the results using casualties per capita as our independent variable. For GTD, we created a variable that capture all violent incidents (both fatal and non-fatal) per capita and use it to replicate their main results. In the case of ICEWS, we used Weidmann Callen (2013) same definition of the independent variable (number of attacks per capita against the International Security Assistance Force).

Crost et al. (2018)

Crost et al. (2018) use the relationship between rainfall and agriculture to measure the impact of rainfall on casualties and violent incidents in the Philippines. We replicated the results contained in Table 5 using GED, GTD and ICEWS data. For GED, we created a variable that captures all violent incidents (defined as incidents resulting in at least one casualty) and another variable that measures the number of conflict-related casualties. For GTD, we also created two variables: a variable on violent incidents (both fatal and non-fatal) and another one on conflict casualties. In the case of ICEWS data, we created a variable that capture all violent incidents (both fatal and non-fatal).

A.3 INTERVIEWEE QUOTES

To supplement our research, we carried one dozen in-depth interviews with news media professionals who have reported on conflict and/or social unrest across the globe with many of the world's largest news organizations and wire agencies. These individuals include current and former media professionals (international correspondents and a cable news executive) from (but not limited to⁵) the *Canadian Broadcasting Corporation (CBC)*, the *British Broadcasting Corporation (BBC)*, *BuzzFeed*, *France 24*, *The New York Times*, *Public Radio International*, *Reuters*, and *The Wall Street Journal*. These professionals have reporting experience/expertise on violent unrest that has occurred around the world, including in (but, again, not limited to) Afghanistan, Burkina Faso, China, Colombia, El Salvador, Iraq, Israel, Libya, Mexico, the Palestinian Territories, the Philippines, South Korea, Sudan, Syria, Venezuela, and Yemen.

Below, we provide quotes from the interviews that speak directly to the reporting processes and decisions that call into significant question the use of media reports as a representative sample of violent events taking place both between and within countries around the world. These interviews highlight reporting tendencies that span space and time and almost surely significantly limit the value of conflict event dataset derived mostly or entirely from news media reports.

Reporting on Political Violence in General

“There is a lot of violence that happens and it can’t all be written about because not everything should be written about. We are not just a chronicle of all the violent acts that took place over the course of a day.” (Interviewee 7)

Speaking about incidents of violence and unrest: *“We are not covering everything that takes place ... there are constraints that control what is reported.” (Interviewee 7)*

“All news is local... it’s the same thing with the international media in a war zone. If there are Americans or Westerners who are killed, that is there first default thinking as an editor, as a reporter. They tend to think about their own audience and their own editorial makeup. So, if there’s an IED that goes off and it kills two soldiers in a tank, and they are from California and Texas... that is going to be a major story... if a house is bombed and a woman and child are killed, that’s collateral of war... Their names aren’t printed. The details of their lives aren’t printed. It’s not

⁵Some of our interviewees requested anonymity. So their current/former organizations are not listed here.

considered worthy because their value is lower to an American audience. That's just a fact. I mean, I hate to be blunt about it, but all lives are not considered equal in the eyes of journalists from the lowest level to the highest... And I am just speaking factually not passing judgment on the morality of it.” (Interviewee 11)

Restricted Access to Violent Events

–Differences in Reporting Restrictions Across Countries

“I worked on Yemen for 18 months, and I was never able to get a visa to go, and we were applying for 12 months or something, but we never got a visa and we never I never got to go. [T]his [was] the beginning of the ... Saudi intervention in Yemen. So it was a very important bloody period a lot of people were ... being killed, and ... it was very difficult to report on; whereas, if you look at Ukraine, there are probably three thousand foreign journalists in Ukraine right now ... I could tomorrow... get a flight to Poland, and then cross over the border relatively easily. No one's going to cause me too many issues. [But] getting to some ... parts of the Middle East is a real challenge ... I would say at no point during the war in Syria have then been ... except right at the beginning ... more than a 100 [journalists] or so ... and in recent years it's been very few ...” (Interviewee 3)

Interviewee 9, who asked not be directly quoted, noted the effects of increasing censorship by authoritarian governments, from Russia to Myanmar, that are making it more difficult for journalists to operate and cover relevant events.

“The big frustration with Syria is that at some point we started being cut off [from the country] and apart from speaking with refugees or people who were in and out at the border, we were losing any visibility as to what was going on inside... you [then] had this tendency of covering everything from afar and from second and third hand witnesses... it was going so counter the basic rule of journalism which is to see it for yourself or to interview people who are there themselves at the very least that it was getting more and more to establish facts...” (Interviewee 12)

“[With] Iraq and Afghanistan, the threshold to actually gain access to these [countries] were a lot higher because you need to be embedded; because it was very expensive; because you needed networks, and so on; [With] Libya, you could just fly to Cairo or to Tunis, and you could be there very quickly.” (Interviewee 12)

–Reporting Restrictions Within Countries

“There were a series of knife and bomb attacks that the government blamed on Uyghur separatists in China in ... 2013/14 and it was basically impossible to cover them on the ground because the whole thing would be cleaned up immediately and they would just sort of bar you from getting anywhere near the scene ... ” (Interviewee 1)

Speaking about other news organizations with personnel in Iraq: *“They didn't go out that much. All of their reporting [was] based on either stringers or the wires. So, they are in Baghdad but they don't really leave the hotel.” (Interviewee 2)*

During the Iraq War: *“There are areas that just can’t be reached ... and Mosul was one of them.”* (Interviewee 2)

“[I]f you’re not a recognized journalist with the Israeli system, you can’t just go [to the Gaza Strip] ... So it is very difficult in terms of that access. [In terms of] the logistics of going there: you have to go across [three] checkpoints, [Israeli, Palestinian Authority, and Hamas] ... [I]f you were a correspondent based in Jerusalem or Tel Aviv, you couldn’t just go to Gaza for a day ... you had to go through logistics to get in, to stay over ... [You have to] get a Hamas visa; you have to get a fixer, who then vouches for you ... then you have to pay a fixer every time you come in. You have to pay for a place to stay; you have to pay for all of this travel ... if you’re going for three days, you must have done enough reporting for four or five stories to make the whole thing pay for itself. So it’s like an investment.” (Interviewee 3)

“There are certainly places where we don’t go [in Colombia] because are just not safe for my team. We would not travel to areas under FARC [Revolutionary Armed Forces of Colombia] control without a lot of prior assurances from that group.” (Interviewee 4)

Interviewee 9, who asked not be directly quoted, describing reporting on violence in El Salvador during the civil war: frequently, they access areas of the country because of fighting. Though, they further noted that information and communication technologies potential ease such restrictions by permitting more remote reporting.

Speaking about the early period of the Iraq War, *“[g]enerally, journalists in these situations are embedded with the military... to have access and to go in safely, especially where, in situations in which the military is controlling how the story is going to involved because their decisions about what they are going to bomb determine the stories from there... now, to do a story about a house that was bombed, and family members that were killed, is tremendously risky because you are essentially going directly to an area... that is being targeted. So, it is dangerous for the reporter; hard to get access... so, [reports on] the impacts on people usual come later and they usually are less frequent... for reasons of access, for reasons of safety... it takes a lot longer to get to place, physically get there and actually sit down with someone... and dissect the story. It just takes a lot longer, and journalists frequently aren’t given that kind of latitude by the editors.”* (Interviewee 11)

Threats to Journalists

“Above all, there is the threat factor. The potential for retaliation from some of these groups to track you down or track your family down and exercise some sort of retaliation or revenge on you. That’s always at the back of your mind when you’re reporting on these issues. And sadly in some places around the region, especially in Colombia and Venezuela, that can come from any side of the conflict. [T]hat can come from paramilitary groups. It can come from guerillas. It can come from the government itself, which has a history of targeting people who work [on] human rights issues.” (Interviewee 6)

“Local reporters are at far more risk of such retaliation than foreign correspondents are. The numbers speak for themselves. I’ve covered some of these episodes sadly in these countries where journalists have been targeted and killed or family members have been killed. So, local reporters face far more risks than than we do. That doesn’t mean that we don’t face any though and you know, International correspondence have been kidnapped. They’ve been abducted. They’ve been jailed. They’ve been pressured in in various parts of the region for their for their work.” (Interviewee 6)

“There’s also the issue of online harassment coming from various different parties. So, I think that that does cause some people to self-censure at times, to hold back on their reporting, to hold back on maybe how they’re positioning or framing a certain type of story ... and it’s a problem that’s only gotten worse, right? It’s not necessarily the threat of bodily harm to you or your loved ones, but it certainly does makes make for some very unpleasant days, when you have to be deal with an onslaught of harassment on social media ...” (Interviewee 6)

Interviewee 9, who asked not be directly quoted, recounting their experience reporting on violence in El Salvador during the civil war, described how high levels of wartime violence significantly impeded the ability of Salvadoran reporters from covering violence in any significant way. As an effect, unlike in some other country settings where American journalists could rely on local journalists for much of their information, such information flow was largely absent in the El Salvador case.

After describing their ability to report from most parts of the country: *“I am very cautious about doing reporting in militia controlled territory. That’s because it’s quite dangerous. They have a reputation for not, let’s say, sparing anyone who is trying to dig into their business and they don’t care if you are a journalist or an NGO observer or whatever ... we wouldn’t go working in militia controlled territory without a really good reason and I would want security there, and it would be tricky business to set up security for that.” (Interviewee 10)*

Reporting on Deadly, Large, or Novel Events or those Situated within Broader Political Contexts

“[W]e looked a lot at death toll ... if something is like very deadly, it obviously becomes more newsworthy because it has a greater impact on people” (Interviewee 1)

“[T]here’s a calculus of whether something is newsworthy. So like supposing there’s a protest in a place where there’s always protests right like that you wouldn’t necessarily write about something like that. So it has to be there has to be some novelty value to it.” (Interviewee 1)

“Already you can see that [reporting on] Ukraine ... is a bit less ... times that by twelve years nearly for Syria and you can see ... why it doesn’t get much coverage anymore. I think once editors start to think of something as intractable ... sometimes there a real challenge to justify why we should do this story now and not in three months time because things will be roughly the same in three months time.” (Interviewee 3)

“We limit the number and type [of protests] we report on because of their newsworthiness.” Referring to recent protest in Bogotá, “if those protests had gotten really violent or they had tens of thousands of people ... we probably would have reported them. But because it was like three [or] five thousand people and all that it caused was traffic delays, we didn’t report that story. So, there are a lot of protests that we don’t even cover because they don’t rate for us as an international outlet ...” (Interviewee 4)

“If it’s hundreds of people [protesting in Sudan], we will rarely cover that unless it’s a kind of a very specific lawyers protests [or] teachers protest, but if it’s [a] mass public protest and it’s only hundreds, we don’t cover that. But in Sudan, it’s been a situation where it’s constantly thousands... or more, so we do typically do a story. But sometimes we do kind of get push back from our editors just saying, you know, people don’t want to read the same story twice a week ... so we do have to work on diversifying our stories ...” (Interviewee 5)

Describing protests in Sudan: *“If there’s violence and if someone dies in a protest, that like raises our chances of covering it quite a bit.” (Interviewee 5)*

Speaking about political violence, *“[o]ne of the biggest factors in determining whether something gets covered or not often is not always the magnitude ... but whether what took place was new.” (Interviewee 7)*

“One factor in terms of whether we work on a story... or not has to do with [the question]: Does this violence reflect something larger going on the country that we writing about? [D]oes it reflect something that maybe is going on in other countries too ...? [The] Arab Spring is a good example of that. Very violent; a lot going on. But at the center of it, it wasn’t just violence. It was states being violent against protestors. It was democracy or autocracy ... it was kind of the big issues of the world – whether there is going to be democracy. Where there is going to be authoritarianism.” (Interviewee 7)

“I would hate to invoke this and have it be taken out of context but there is the old journalistic maxim: ‘if it bleeds it leads.’ I wouldn’t go so far as to say that is what we follow. But, certainly above a certain level of deaths, you are going to be doing a story. There is no number ... If people do not die, [there is] much less chance that we are going to be writing about it.” (Interviewee 10)

“Before you write a story about [a protest,] you’d want there to be a reason and the reason ... is usually ... that it represents a large portion of society going out to the streets. It doesn’t have to be, you know, hundreds of thousands of people. But you’d want thousands of people, at least, representing the grievances of many more thousands of people.” (Interviewee 10)

“Now, when it comes to repression of protesters with violence, that instantly makes it more newsworthy ... if you have police cracking skulls, they need to held to account for what they are doing, which is often, if not illegal, an overstepping of their training.” (Interviewee 10)

Reporting by Targets

“In [the] Iraq War, if five American soldiers [were] killed in one day, that would be a major news. But if five Iraqi soldier are killed in the ... same conflict, that’s not major news ... It has to be ... something between 40 and 50 [Iraqis killed] until you are absolutely sure that there will be a story.” (Interviewee 2)

“Yes, there’s a disparity between if a Palestinian is killed [and reported on relative to if] an Israeli is killed, because sadly, many more Palestinians are killed than Israelis.” (Interviewee 3)

“[A] bombing of a police station ... next door to the presidential palace is probably more newsworthy to us because it shows a larger security issue than the bombing of a police station in a small town in an active conflict zone where those things happen frequently.” (Interviewee 4)

“Generally here [in Colombia, attacks] involving guns [tend to involve] battles between FARC dissidents and a crime gang that they are fighting... or the government versus the ELN [a major leftist guerrilla organization]... different combinations of the various armed groups ... or in targeted killings of social leaders ... And we don’t generally cover individual attacks unless it’s someone who is very famous already or... who is connected to some larger piece of newsworthiness ... maybe they are a political candidate or already a senator.” (Interviewee 4)

Interviewee 9, who asked not be directly quoted, explained that the Western news media prioritize reporting on bombings in Western countries like Sweden over bombings in South Asia, where they are much more frequent and produce many more deaths. Such judgments, they explain, are the historical norm and continue to take place.

“If a civilian is [being targeted], this is something that is newsworthy. If the general public is suddenly at risk of having something happen to them, we would be more likely to cover that than say ... gangs vs. police or something like that ... I guess civilians suffering violence would have more likelihood of being covered ... it’s more in the public good to know.” (Interviewee 10)

Speaking about fighting in Burkina Faso, “[a]ll of the attacks that are against security forces... get absolutely no traction. First of all, it happens too regularly unfortunately. So, it’s more another day where there is another attack. Sometimes it’s six people. Sometimes it’s ten... It needs to be civilians [to be reported]. And then it’s just a matter of how many people were killed... When it’s big [and] it’s civilians, it will get some traction. But anything in between, honestly, [isn’t likely to be covered]... [I]n Burkina [Faso], there’s barely a week that goes by without at least a few civilians killed in an attack... and it’s not getting covered.” (Interviewee 12)

Effects of Resource Limitations and Existing Infrastructure

“We had a verbal rule ... that was in early 2009 and they said: Okay, we are changing the coverage because Obama is in power, and also the New York Times is almost broke. So we are minimizing the paper size itself, so we have less space for Iraq ... we’re just going to change the way we covered things. You still have to tell us everything, but we probably can tell you in most times: Thank you but no.” (Interviewee 2)

“Syria was covered really aggressively largely because there were many news organizations who had people in Beirut at the time. And it’s right next door. Whereas, [countries experiencing conflict] often don’t have a big [news media presence] ... The Times [for instance] only has three offices in Africa – in east Africa, in west Africa and in south Africa.” (Interviewee 7)

“Ever since the Ukraine crisis, we have not had the ability to cover ... all but one of the protests that we’ve had in Sudan with video and photos because the budget just isn’t there.” (Interviewee 5)

“The main constraint we could face is that ... the country I cover is very large, and much of our personnel are based in the major cities. And information in these very far flung spots is rare sometimes. If violence happens, it’s not either or quick that the information comes out, or when it comes out, it’s murky who’s actually done what. And getting to the bottom of it is either too difficult or too costly ... there are financial and logistical constraints just because of how big this country is.” (Interviewee 10)

“There were definitely constraints on [traveling to unsafe areas]. There was a lot of push back any time you wanted to do a story that involved spending money and that was not directly related to economic policy or something along the lines of [financial] news reporting ... There was one story that I wanted to pursue ... about land disputes and [assassinations] ... and [my employer] would only allow me to go [given the need for] a big investment in security ... so big, in fact, that it made the pitch unviable.” (Interviewee 10)

Referring to reporting on Syria’s civil war, “[w]hat started to happen is that news organizations, especially the American and English news organizations, started to require a risk advisor to be traveling with you on location, and a risk advisor not only costs a crazy amount of money but also comes from usually Western companies that are based in the States or in the UK. And... I was feeling that that was making me more of a target.. and that really started to take place at some point in 2013, and after a few journalists got kidnapped and an American journalist got kidnapped... a lot of us lost access or when you could access, there were all of these limitations as to where you could go because some of these decisions were taken by the security adviser and were not necessarily guided by editorial needs.” (Interviewee 12)

On Combats Strategic Timing/Use of Violence

“I remember during COVID there were these crackdowns that were going on from the Cuban government against dissidents. The Cuban government is very good at timing what they do to coincide with world events that are distracting people in other places. So, in 2003, for instance, during the invasion of Iraq, they had a really big crackdown on dissents as well. So, it wasn’t surprising that they timed what they were doing [during COVID] to a time when everyone was looking in the other direction.” (Interviewee 7)

“In Libya... it generated so much reporting, at least in 2011, [because] access was easy as long as you were remaining on the... rebel side of the conflict. There [were] no protection issues [for] journalist unless you were in the wrong place at the wrong time” But there was no kidnapping within the opposition side, and people were very willing to take you everywhere you wanted”

Those who were not fighting or were not busy with the hospital and so on, they just wanted to be helpful. They also just somehow saw the journalists as the more articles that are written, the more bombs are going to fall on Qaddafi's forces. Like, for them, there was a clear equation between: the more media attention there is, the more the West is going to help so people were helping and there were... very little security risks..." (Interviewee 12)

Politically/Strategically Salient Locations/Events/Actors

"If ten Iraqi people get killed, that's nothing. That is not even worth a story in the New York Times. And that's [even more the case] if they are killed in remote parts [of the country]." (Interviewee 2)

"On just looking at [violence] against Israelis ... it made a huge difference where it happened more than anything. If it happened inside what's called "green line" Israel ... and particularly if it happened in Jerusalem or Tel Aviv, it got a lot more coverage than if it happened in the West Bank. If a settler was attacked outside of a settlement, it would not be a big story outside of the Israeli-Palestinian media." (Interviewee 3)

"Location also matters to an extent, a bombing of a police station in, you know, next door to the Presidential palace is probably more newsworthy to us, because it shows a larger sort of security issue than the bombing of a police station in a small town in an active conflict zone, where those things happen frequently. So I think the newsworthiness, besides scale also plays into sort of like a why does it matter larger context." (Interviewee 4)

"The other type of protest that might not cover that closer are those that ... for example, [by] teachers or doctors or workers at a particular factory ... when it's not part of a wide protest movement, where it's something a little more narrow – and honestly you could probably add like women's groups to this ..." (Interviewee 5)

"[W]hen there is a general protest movement in Sudan, it's not just in the capital Khartoum, which is where our team is based, it's also in cities all across the country. It's a vast country... [It's] very hard to cover all those. So we typically tend to kind of give a detailed coverage of what happens in the capital, and then we kind of say ... parallel protests were seen ... on social media ... Sometimes – honestly rarely – we'll see you'll kind of call people up and ask for detail. But we generally don't have the time necessarily to do that, nor is there necessarily the interest given that kind of people tend to focus on the capital, and the capital is the most politically consequential [location of a] protest, because it's right at certain government buildings, it ... has more symbolic significance ..." (Interviewee 5)

"We wouldn't go out [to the Korean countryside] if it was going to be a little demonstration. We would go out if they contacts we have are telling us it's going to be a big one." (Interviewee 8)

"Compared to your standard Free Syrian Army group speaking in a fairly radical language [Kurdish fighters] openness and Western-like take on a lot of things was one of the reasons why the Western media [gave] the story of Kurdish fighters so much traction." (Interviewee 12)

Summaries of Events/Trends Versus Reporting on Individual Events

“So, we have a protest in [the greater] Khartoum area... and we’ll have detail and quotes from [the capital area] ... and then we’ll [reference] protests in [other] cities ... particularly on days where it’s larger protests where you are looking at tens or hundreds of thousands of people, we will kind of say, you know, ‘hundreds of thousands have marched across Sudan.’ And so, the purpose of us getting these cities in the story is just to give an idea of the breadth of the protest movement – basically, that it’s not just something that is happening in the capital and it’s happening across the country.” Then, responding to whether the the other cities referenced are a sample of those affected or all affected: “No, it’s definitely samples ... so, it depends on the size of the protests ... sometimes if its a bigger protest, I’ll try to include more cities, but I tend to try to take, like, a city from the west ... a city from the east of the country, a city from the south of the country if any of them are protesting, the north of the country. I kinda try to do major cities everywhere just to give a sense that it is happening in a diverse array of areas.” (Interviewee 5)

Regarding how violent events in Afghanistan around the same time would be covered: *“You put them all into one story ... There was a period [in Afghanistan or another country with significant violence] where we basically did a running tally [of deaths] because those acts of violence were happening every single day. We made an editorial decision that we... are at least going to record that they died ... with a number. Kind of like with the coronavirus. You can’t report every single COVID death but you can keep a running tally ... there is at least some acknowledgment that people are dying ... What you want to try to do is explain a story, not just give a bunch of numbers or just sort of make everyone sound the same. You want to put numbers in context ... You are explaining the tragedy that Afghans are suffering from much more clearly rather than saying this person died and this person died ...” (Interviewee 8)*

ALTERNATIVES TO MEDIA BASED DATASETS

In the Conclusion we mention alternatives to media-based datasets. Due to space constraints we do not fully elaborate in the manuscript, and so include a richer discussion of these issues here.

Security Force Administrative Records

One obvious substitute for media-based event datasets are those built by state security forces, such as those used in this study. Such datasets are not without their own limitations. Security force records may not capture events that do involve security forces, e.g. rebel targeting of civilians or fighting among rebels groups. Additionally, the quality or availability of such datasets depends upon the level of organizational and technological sophistication of the force to be able to consistently and accurately track individual incidents.

Such datasets are also exceedingly rare. For many beleaguered governments, such records are unlikely to exist at all, or may be of poor quality, or may even be systematically bias. Even where they exist, they may be impossible to access. Given that they are intended for internal use, access may only be obtained through democratic procedures such as the United States' Freedom of Information Act. Many governments most vulnerable to conflict are precisely those with significant restrictions on internal documents. If security forces anticipate that their records may be made publicly available, they may have incentives to misrepresent the true incidence of violent events.

Civilian Surveys

Surveys of civilian populations could theoretically provide an alternative method for measuring wartime activity, yet biases abound. Retrospective questions reflect errors of recall, which may be increasing in time since an event occurred (Öztaş Ayhan and Işıksal 2005).⁶ Survey respondents may

⁶Other cognitive psychological effects on memory recall include *the telescoping effect*, wherein people struggle to create a timeline of events, as they perceive recent events as more distant than they are and distant events as being more recent than they are, or *perceptual load effects*, wherein the complexity of a perceived event can cause people to misremember the event and even increase their susceptibility to leading questions about it (Neter 1970; Murphy and Greene 2016).

alter their answers due to social desirability (DeMaio 1984), which may be particularly pronounced in highly politicized environments or where intergroup relations are strained and coethnic bias exists (O’Loughlin and Toal 2020; Lyall et al. 2015). In fact, those who respond “I don’t know” or decline to answer often reflect a political stance through their refusal to answer (Berinsky 2002). Refusals to answer can be a form of “strategic hedging,” whereby those who fear that their support of or opposition to a certain group will affect them adversely (O’Loughlin and Toal 2020). A variety of “logistical and ethical issues that accompany survey research in a wartime setting” (Lyall et al. 2013) render surveys an impractical substitute for violent events data.

To explore this possibility more directly, we advised Mercy Corps on several questions embedded in a survey administered to more than 500 households in 2021 in Iraq’s Sinjar District, which in 2014 was considered “the epicenter of ISIS’s attacks on the Yazidi population” (Lichtenheld and Brooke 2021: p. 17). Each household respondent was asked to recount the frequency with which they witnessed or were personally exposed to each of the following types of violence: physical assault, indirect fire, small arms fire, bombings, or abduction, imprisonment or forced labor.⁷ We examine rates of item non-response (don’t know or refuse to answer) across exposure to different kinds of violence. These rates of non-response appear in Figure 3. Across violence types, they ranged from 54% for small arms fire to 78% for physical assault.⁸

⁷Each violence type was described as: “Physical assault”, “Shots fired by small arms (e.g., pistols, rifles)”, “Bombings (e.g., the detonation of car bombs, improvised explosive devices)”, and “Instances of abduction, imprisonment, or forced labor” (Lichtenheld and Brooke 2021: p. 39).

⁸These rates of non-response are related to questions about violence exposure. For sake of comparison, we present here the rates of non-response (“Don’t know or unsure” or “Did not answer”) for other questions:

- *Should Sunni Muslims be fully welcomed into [your village/Markez Sinjar/IDP camp]? 5.5%*
- *If Sunni Muslim IDPs from Sinjar moved to your village, how likely is it that the community will accept them and allow them to be integrated? 0%*
- *Please tell me the degree to which you agree or disagree with the following statement: ‘The government cares about my opinions.’ 0%*
- *How much confidence do you have in the Iraqi government’s security forces (military and police)? 7%*
- *Some people think that the tactic of using arms and violence (armed resistance) against other people for a political cause is justified. Other people believe that, no matter what the reason, this kind of violence is never justified. Do you personally feel that this kind of violence is: often justified, sometimes justified, rarely justified, or never justified? 10%*
- *Please tell me the degree to which you agree or disagree with the following statement: ‘If there is violence between Yazidis and Sunni Muslims, I should offer support to the other group if members of my group are the*

Whether individuals feared repercussions for providing honest answers; sought to avoid the psychological trauma of recalling instances of violence; legitimately could not remember; or chose not to supply details for some other reason, civilian reports of violence from this survey could not be used to credibly estimate levels or types of violence.

	Physical assault		Rocket or mortar fire		Small arms fire		Bombings		Abduction, imprisonment, etc.	
	When ISIS came	After leaving home	When ISIS came	After leaving home	When ISIS came	After leaving home	When ISIS came	After leaving home	When ISIS came	After leaving home
Once a day	—	58 (11%)	103 (20%)	87 (17%)	154 (30%)	136 (27%)	57 (11%)	35 (7%)	63 (12%)	57 (11%)
Once a week	—	28 (5%)	32 (6%)	56 (11%)	28 (6%)	32 (6%)	31 (6%)	33 (6%)	27 (5%)	30 (6%)
Once a month	—	18 (3%)	38 (7%)	34 (7%)	30 (6%)	39 (8%)	38 (8%)	37 (7%)	24 (5%)	25 (5%)
Once a year	—	8 (2%)	34 (7%)	47 (9%)	22 (4%)	28 (5%)	32 (6%)	44 (9%)	19 (4%)	26 (5%)
Don't know or unsure	—	43 (8%)	79 (16%)	84 (16%)	89 (17%)	87 (17%)	46 (9%)	46 (9%)	38 (7%)	37 (7%)
Did not answer	—	356 (70%)	225 (44%)	203 (40%)	188 (37%)	189 (37%)	307 (60%)	316 (62%)	340 (67%)	336 (66%)
Total	—	511 (100%)	511 (100%)	511 (100%)	511 (100%)	511 (100%)	511 (100%)	511 (100%)	511 (100%)	511 (100%)

Figure 3: Survey responses from Iraqi Yazidis regarding exposure to violence following the ISIS invasion of Sinjar District. Rates of non-response (“Don’t know or unsure” + “Did not answer”) are high. *Source: Lichtenheld and Brooke (2021).*

aggressors.’ 0%

Among those respondents who did supply a frequency, self-reported exposure to small arms fire was significantly higher than to bombings, consistent with our findings from Afghanistan and the Philippines.

Remote Sensing

Remote sensing technologies – whereby satellite imagery is used to gather information from even the most hard-to-reach locations – could represent yet another alternative. Indeed, remote sensing technologies have been used to detect the Darfurian genocide and to document human trafficking in Afghanistan (Brown 2010; Witmer 2015). Remote sensing, however, is limited in its capacity to gather sufficiently fine-grained data on violent events. A core problem is the reconciliation of temporal and spatial data: a satellite only captures images of an area at one point in time and therefore is prone to missing critically important events (Aplin 2006). While remote sensing works well to identify changes in an area over time, sensing the details of a particular event (i.e. casualties, type of attack) is rarely feasible (Witmer 2015). Further, although satellite imagery can grant access to remote or dangerous areas, without on-the-ground corroboration of what occurred in the field, the basic details of these events may be inaccurately reported (Raymond et al. 2013). It may be necessary to combine remotely sensed imaging with other types of big data, such as the conflict event datasets discussed by Levin et al. (2018).

Crowdsourcing

Crowdsourcing – wherein organizations offer civilians an opportunity to report violent incidents that they witness – is another potential source of data. This method has been used to track political violence, including during the Syrian civil war and in the aftermath of Nigerian elections in 2011 (De Juan and Bank 2015; Bailard and Livingston 2014). Yet crowdsourcing has proven an imperfect data collection tool, as both practical and ethical concerns limit its usefulness. Data collected through crowdsourcing can be biased due to the organization sourcing the information and the civilian population's awareness of the crowdsourcing operation (De Juan and Bank 2015; Bailard and Livingston 2014). Further, crowdsourcing is subject to vast under-reporting, as the accuracy of the data requires live witnesses to violence and access to rural areas (De Juan and Bank 2015; Wirtschafter 2021). Finally, crowdsourcing raises ethical issues, as tactics used to gain

information from a crowd can often be exploitative of vulnerable populations (Busarovs 2013; Martin-Shields 2013).

Truth and Reconciliation Commissions

Investigations conducted by truth and reconciliation commissions (TRCs) provide an opportunity to gather vast troves of conflict-related information. In the aftermath of conflict, TRCs use quasi-judicial restorative justice methodologies to uncover the human rights atrocities and violations faced by civilians in wartime. In fulfilling their functions, TRCs have recently turned to data collection, to highlight the scope of violence in wartime and to document its patterns. For example, in Peru, the TRC documented 69,000 deaths in Peru during the internal conflict of the 1980s and 1990s (United States Institute of Peace 2018). Using data from three main sources (civilian interviews, data gathered from NGOs, and testimonies to the Public Ministry), the TRC used a capture-recapture method of data analysis (multiple systems estimation) to compile all events into a single dataset and fill in gaps within the data (Ball et al. 2003). Both Guatemala and Kosovo's TRCs employed similar methodologies (Ball et al. 2003). Yet the accuracy of the Peruvian TRC's conclusions has been questioned, as the correction of biased and missing data may have led to *overestimations* of deaths, in addition to wrongful attribution of deaths to particular actors (Rendon 2019a,b), which can produce flawed portrayals of conflict events (Zwierzchowski and Tabeau 2010).

Greater Reliance on Local Reporting?

Would an increased reliance on local news reports improve the quality of media-based datasets? Local reporters are more likely to be aware of local security conditions and may be less likely to attract the attention of combatants than foreign reporters. They may also be less subject to certain kinds of editorial bias. Yet reliance on local sources would simply shift the set of capability and editorial biases affecting conflict event datasets. For example, while editorial decisions may

not seek to appeal to the whims of foreign audiences, media outlets would still seek to appeal to national audiences, which may be skewed towards events occurring in urban areas, or those targeting populations that resemble the bulk of their viewers.

The Committee to Project Journalists, which tracks data on harm to journalists and media workers, documents a greater number of work-related deaths suffered by local media professionals than their foreign counterparts.⁹ Dangers faced by local media are substantial, with clear implications for capability biases. Furthermore, some conflict settings lack local media professionals. This dynamic is widely discussed in Syria, where citizen journalists, who lack training and are often activists with their own agendas, have supplied much of the country's news:

... it's just incredible to watch how Syrians from all walks of life have bravely turned to journalism as a public service during a period of acute crisis for their country. But... there are some downsides to the kind of confluence of political activism and citizen journalism in the sense that it has generated doubts about the accuracy of the reporting. If you have a political agenda, how accurate is what you're reporting? And that's just not a concern for Syrians trying to track the news, but also foreign media Abouzeid et al. (2001).

This can manifest in different ways, with incentives to both over- and under-report on particular events. Ibrahim al-Assil describes the former: “[s]ometimes you have people, for example, in [a] besieged area and they get emotional and they think like, ‘if I say a hundred people got killed, nobody is moved. Let’s try to say 200’” (Abouzeid et al. 2001). In contrast, Rania Abouzeid, a foreign correspondent, who has reported extensively from Syria, describes pressures on local sources to *under-report*:

It’s very difficult [for them] to push back against armed forces. For example, to criticize your own side, when your own side might be very small militias, and when you’re in a fragmented war zone, where every man with a gun has become an authority, it’s a very difficult thing to criticize the mistakes or the crimes in some cases of your own side Abouzeid et al. (2001).

One possibility to improve the quality and coverage of media-based datasets lies in greater engagement between dataset developers and media outlets. Media professionals collect data on

⁹See: <https://cpj.org/data>

many more conflict incidents than those on which they ultimately report. A partnership, perhaps partially financed by the same governmental/inter-governmental bodies that presently fund and consume media-based data, could help incentivize media professionals to share details of violent events that would otherwise be left out of their stories. While such an arrangement would not resolve systematic missingness resulting from capability issues, it may attenuate editorial biases.

ETHICAL CONSIDERATIONS

The semi-structured interviews we conducted for this research were approved in advance by the University of California's Institutional Review Board (UCM2021-125). Each interviewee provided informed and voluntary consent. Participants were informed ahead of the interview and before providing informed consent that they were free to withdraw from the study at any time. They were also asked directly about their preferred manner of identification in research products. Although not required under the IRB protocol, we do not directly associate interview quotes and material with former or current professional affiliations, to further anonymize respondents. Furthermore, we offered each interviewees the opportunity to follow-up with us within one week after the interview to request any edits to the interview transcript, or should they seek to redact particular portions. This ensured that if interviewees were uncomfortable with any statements made during the interview they would have the opportunity to revise (or altogether) delete these. Interview materials have been saved securely, in line with the IRB protocol we established.