Technology of Liberation or Control?: The Asymmetric Effects of the Internet on Political Conflict*

Alexander Fertig and Megan Ryan

University of Michigan

October 20, 2025

CLICK HERE FOR MOST RECENT VERSION

Abstract

Over the past two decades, the internet and social media have expanded rapidly to all corners of the world. While these new technologies have liberalized access to information and communication channels, they have also introduced new platforms for surveillance and propaganda. As such, the internet can be characterized as a "double-edged sword" for society, introducing new freedoms as well as oppressions. This duality is perhaps most evident in the case of Myanmar, where a majority of the population was first exposed to the internet within the past ten years. In this paper, we estimate the effects of the internet empirically by exploiting geographic variation in access as well as temporal shocks to exposure. We find that reducing internet access leads to a reduction in the prevalence of demonstrations—but not other forms of political conflict—during the months following a military coup. However, as internet freedoms are eroded, the effect on protest activity disappears, and we argue that this shift can be explained by a change in the nature of political discourse online. Moreover, in the long run, we find that internet access is associated with more political violence and higher levels of military control across Myanmar. Taken together, our results show that the internet can serve as both a tool of liberalization and oppression, conditional on the government's capacity to monitor and exert influence over the network. These findings are especially relevant for developing economies in which widespread internet access is relatively recent, as these advances may not necessarily be beneficial for democracy movements.

*Contact: afertig@umich.edu. We would like to thank Dean Yang, Achyuta Adhvaryu, Yusuf Neggers, and Kevin Carney for their advising and support on this project. We also thank seminar and conference participants at the University of Michigan and Northwestern University, as well as discussants and participants at the Empirical Studies of Conflict (ESOC) Research and Policy Conference, for their helpful comments and feedback. This research has been supported in part by two Rackham Graduate Student Research Grants from the University of Michigan, an Innovations for Poverty Action (IPA) Peace and Recovery Program Exploratory Grant, and an NICHD training grant to the Population Studies Center at the University of Michigan (T32HD007339).

1 Introduction

How does the internet affect political conflict? Since the proliferation of internet access to all countries of the world, and the rise of social media in particular, much has been made of the connection between these new digital communication technologies and successful social movements against oppressive governments. The Arab Spring protests of the early 2010s are cited as one of the first examples of this phenomenon, during which new social media platforms such as Facebook and Twitter provided an alternative mode of communication to circumvent state-controlled media (Howard and Hussain 2011). More recently, observers of the 2019 Hong Kong protests and 2022 protests in Iran described ways in which social media contributed not only to internal mobilization, but helped share evidence of power abuses with an international audience (Amidi 2022; Haas 2019). In the U.S., social media has been credited with fueling recent protest movements like Occupy Wall Street and Black Lives Matter (Chang et al. 2022; Suh et al. 2017).

However, the same technologies that inspire these narratives have also been charged with enabling oppression and restricting social movements. For example, China's government has long been accused of heavy surveillance and censorship of social media services, using the internet as a tool to track dissidence and spread pro-government messaging (Griffiths 2020). Even in countries with seemingly high levels of internet freedom, such as the United States, social media has provoked criticism for spreading misinformation—as during the 2016 presidential elections—and inciting violence—as on January 6$^{\text{th}}$, 2021 (Allcott and Gentzkow 2017; Li et al. 2025).

In this paper, we estimate the internet's effect on political conflict using evidence from Myanmar, a country that was recently subjected to a nationwide internet shutdown in the wake of the 2021 coup d'état. We compare spatial patterns of conflict directly before the shutdown with patterns of conflict in the following weeks to estimate how exposure to the internet effects both the frequency and intensity of conflict in Myanmar. A Difference-in-Differences (DiD) framework allows us to estimate a treatment effect of the shutdown—which can be interpreted causally under certain assumptions—by calculating changes in conflict for high internet access areas relative to changes in conflict for low access areas.

We estimate these effects by combining three separate empirical components. First, we locate the incidence of political conflict since 2021 within specific townships in Myanmar. Second, we generate a map of cellular signal strength using a standard wave propagation model[1] that predicts how radio waves are impacted by fluctuations in physical terrain. Though signal strength is measured in units of power along a continuous spectrum, there is an approximate power threshold below which a standard smartphone is not able to reliably transmit data. We use this threshold to define all pixels on the map as areas in which an

[1] We use the Irregular Terrain Model (ITM), as in e.g. Gonzalez and Maffioli (2024).

individual either receives mobile data or does not, i.e. internet *access* or *non-access* areas. By aggregating this binary access variable at the township level, we generate a continuous variable that estimates the total number of people in each township that reliably receives internet access where they live. Finally, to build a deeper understanding of the mechanisms through which conflict is driven by internet access, we take advantage of a rich set of public data from township-specific Facebook groups that were active in Myanmar around the time of the coup. This data contains text content of all posts made to these groups from January 2021 through September 2022, as well as the total number of reactions and comments that each post received. Using simple natural language processing, we can individually identify posts that are political in nature, allowing us to show how political Facebook content corresponds with actual incidents of conflict and violence.

Myanmar presents an ideal context in which to study modern political conflict, as it has received international attention for two major conflict episodes in the last decade. In both conflicts, the media focused attention on the role of Facebook, the country's most popular social media platform, though for very different reasons. In 2017, the state military initiated a violent campaign against the Rohingya ethnic group in western Myanmar, which was labeled a genocide by many human rights groups and international agencies (Wilkinson 2018). As the crisis was unfolding, international news outlets described how hate speech, spread online through Facebook, was exacerbating violence and enabling the government's goal of ethnic cleansing (Stecklow 2018).

Several years later, the military sparked another political crisis after they staged a coup to unseat the nation's democratically elected leaders. As before, Facebook received international attention for it's role in the conflict, but for very different reasons. This time—initially at least—coverage focused on the social media platform's role in mobilizing mass resistance against the military's abuse of power. Peaceful protesters used Facebook and Twitter to generate outrage against military abuses and to coordinate resistance activities (Jordt et al. 2021; Oo 2021; Prasse-Freeman 2023; Ryan et al. 2024; Whong 2021), while armed groups utilized social media platforms to build pan-ethnic solidarity among resistance actors (Ryan et al. 2024). Much as with the Arab Spring a decade earlier, social media was hailed as a tool for liberation from authoritarian rule (Wells and Deejay 2021).

However, even as the press was reporting on these stories of digital resistance, another narrative began taking shape simultaneously. The Myanmar military was using Facebook and other social media platforms to monitor citizens and target dissidents, often releasing their personal information online (Myint 2023; Whong and Foster 2023). Graphic messages and imagery, promoting violence and retaliation against protesters, were spreading throughout public groups and pages (Ratcliffe 2021).

Given the conflicting narratives about the role of the internet in modern political conflict, we hope to provide a better understanding of these complex relationships. Is the internet a technology that helps bring about social change? Does social media contribute to the aims of civil society by enabling resistance against authoritarian rule? Or do these technologies instead serve the rulers by providing an expansive network for surveillance and propaganda? If both narratives contain some degree of truth, which is more salient in today's world, and what are the conditions under which these effects manifest?

In our analysis of post-coup Myanmar, we find that the nationwide internet shutdown was effective in disconnecting citizens from digital communication channels. The number of daily Facebook posts observed in our data drops by nearly 50 percent post shutdown, and we also observe a reduction in protest activity over the same period. In the DiD results, we estimate that, for every additional 10,000 people connected to the internet, the shutdown reduces the number of Facebook posts by an additional 3 percent, and the number of protests by nearly 10 percent. If a relative reduction in internet exposure reduces protest activity, then the inverse should also be true: increasing internet exposure will lead to an increase in protest activity. At the same time, we see no significant effect on any form of violent political conflict. This evidence supports the narrative that the internet, and social media in particular, is a tool that enables mass mobilization and aids resistance movements.

Communication is critical to political mobilization. State leaders, rebel groups, and military commanders communicate logistics and tactics and signal intentions, goals, and capacity to a variety of audiences, from allies to adversaries (Smith and Stam 2004). These same leaders also seek to coordinate public behavior through mass communication channels, such as television, radio, print media, and social media (Howard 2010; Zeitzoff 2017).[2] Our findings align with early research on the interaction between conflict and digital communication technologies—e.g., the internet, cell phones, social media—which describes these advances as "liberation technologies" due to the speed of communication and peer-to-peer networking capabilities (Diamond and Plattner 2012; Milner 2006), emphasizing how digital tools helped activists communicate, coordinate, and raise awareness of shared grievances without a prior organizational base (Bradshaw et al. 2021; Fisher 2011; Howard and Hussain 2013; Little 2016; Margetts et al. 2016; Ruijgrok 2017; Steinert-Threlkeld 2017; Tufekci and Wilson 2012; Weidmann and Rød 2019). Conflict scholars have emphasized how insurgents use ICTs to coordinate their activities and recruit insurgents, and how protest movements rely on these technologies to build a following and organize demonstrations (Dafoe and Lyall 2015). In both cases, new ICTs operate along the same mechanism: lowering the costs of collective action and facilitating the spread of important information—such as human rights abuses—that may have a mobilizing effect (Bailard 2015; Pierskalla and Hollenbach 2013; Shapiro and Siegel 2015).[3]

[2]In economics, there is a longstanding literature on the persuasive effects of traditional media on behavior, summarized in DellaVigna and Gentzkow (2010), which covers general classifications for theoretical models of persuasion.

[3]The most common communication technologies studied in the conflict literature are cell phones and

Our estimates suggest that access to social media and the internet has an asymmetric affect on different types of contentious mobilization. The peer-to-peer broadcasting capacity of social media is more relevant for mass protest mobilization because activists need to reach a wider network of participants, more rapidly, to coordinate protests than insurgents need to coordinate violent attacks. While both insurgents and activists need to overcome coordination and collective action problems, protests are powerful because they demonstrate the voice of the masses, and thus generally require a larger number of participants than armed insurgency in order to be effective. While insurgents still require some participation and tacit public support for their survival, the coordination of an insurgent attack depends less on mass participation than on careful plotting, planning, timing, and coordination of movement (Pierskalla and Hollenbach 2013). Since these attacks only involve coordination between close contacts within a chain of command, the internet is a sufficient, but not necessary technology.

While the early findings in the literature apply primarily to contentious political mobilization between 2000 and 2010 (Dafoe and Lyall 2015), more recent scholarship takes a more skeptical approach to the "liberation technology" framework, emphasizing the limitations of new ICTs for actors challenging a powerful state. In Myanmar, not long after the first nationwide shutdown, the government continued to employ internet restrictions as a military strategy over the ensuing years. These subsequent shutdowns lasted anywhere from a single day to several months, and were targeted at specific townships or groups of townships. We use a similar DiD approach to estimate the impact of these events on political conflict, and find a different set of results. While we still find that, after a shutdown event, Facebook activity decreases relatively in townships with higher levels of ex-ante internet access, we no longer see a corresponding decrease in protest activity. (As before, we observe no significant effect on other forms of political conflict as well.)

Why do we observe such a large discrepancy in the effect of internet shutdowns on protest activity, even within the same country and context? There is a growing literature documenting the conditions under which autocrats use ICTs and the internet to successfully surveil, censor, and repress anti-regime dissent (Gohdes 2015; King et al. 2017; Mattingly and Yao 2022; Morozov 2011; Qin et al. 2017; Roberts 2018; Shapiro and Siegel 2015; Weidmann and Rød 2019; Xu 2021). In the same way that new ICTs can reduce coordination costs for non-state actors, they are also likely to strengthen the

social media. Traditional mass media is unidirectional in that it allows elites to communicate directly to the masses, who passively receive the information (Zeitzoff 2017), but cell phones disrupt the state advantage of traditional media by introducing improved peer-to-peer communication technology. By allowing individuals to communicate more frequently over longer distances, cell phones undermine the effects of government propaganda facilitated by traditional media and help overcome collective action and coordination problems (Pierskalla and Hollenbach 2013). Social media, on the other hand, not only facilitates a more frequent and diffuse style of communication than was ever possible before, but also allows individuals to broadcast their own messages among online social networks and to interact with others in a public digital space. Social media thus facilitates the spread of information beyond an individual's direct contacts, by sharing content among those with whom they have weaker ties (Steinert-Threlkeld 2017).

state's attempts to surveil and repress any challenges to its authority. For example, in the context of Iraq—a state with a strong central intelligence capacity—the introduction of new cell phone towers reduced insurgent violence by improving the state's surveillance and counterinsurgency abilities (Shapiro and Siegel 2015). Other examples highlight how the internet can help state actors to engage in more targeted repression of challengers (Gohdes 2015), and censor online content that would otherwise facilitate the mobilization of collective action (King et al. 2017). In some cases, while ICTs may *initially* benefit the efforts of resistance movements and insurgents against strong states and/or authoritarian regimes, these benefits could wane and even reverse over time, as governments strategically adapt to their dynamic information environments (Weidmann and Rød 2019). In this sense, the internet may be viewed as a "double-edged sword" for the growth of democratic movements worldwide.

Through our textual analysis of Facebook posts, we find that there was a marked evolution in the way that Myanmar users interacted with the social media platform as the political crisis unfolded. Specifically, we find that the percentage of posts in our dataset that are explicitly political in nature decreases dramatically over time. During the first nationwide internet shutdown—which occurred less than two months after the outbreak of the coup—political posts made up nearly one-third of total observed posts, but by the time of the first targeted shutdown, this ratio was reduced by half, and continues to decrease over time. While the reasons for this change in Facebook usage may be varied, we note an increase in the number of arrests that were related to online dissident activity, a trend which coincides with the reduction in political Facebook content. We conclude that, as a government's ability and/or willingness to monitor online activists and control social media messaging increases, the effect of the internet on political conflict will also change. As governments learn and adapt to the importance of new communication technologies, these tools can shift from a role of enabling resistance movements to one that is neutral or even undermining.

This paper contributes to the existing literature on conflict and media in several ways. First, Despite recent advances in our understanding of the effects that modern ICTs have on political conflict, we know little about how various types of non-state actors and forms of conflict might react differently to these effects, as the literature tends to be siloed within a specific type of actor or form of conflict. For instance, researchers tend to look exclusively at violent crime/conflict *or* protest as an outcome, depending on the regional and historical context of the study. For examples of the former, see Yanagizawa-Drott (2014), which finds that radio propaganda led to a significant number of excess deaths during the Rwandan genocide in the mid-90s, Adena et al. (2015), which shows that exposure to Nazi radio led to higher rates of anti-Semitic violence in pre-war Germany, Bursztyn et al. (2019), which shows that use of the Russian social media site VK led to higher rates of ethnic hate crimes, and Müller and Schwarz (2021), which finds a similar effect of Facebook exposure on anti-immigrant hate crimes in Germany. For an example of the latter, Enikolopov et al. (2020) finds that penetration of VK led to a significant increase in both the probability of a protest occurring and the number of protesters involved, and Qin et al. (2024) shows how the expansion of social media in China has had a sizable impact on the geographical spread of protests. Inevitably, one of the primary challenges in assessing effects across different forms of conflict is that they do not often overlap, and thus are rarely exposed to the same shocks. In the case of current day Myanmar, however, the coincidence of peaceful demonstrations and armed conflict during the period following the coup provides an ideal natural experiment in which to estimate these effects. In our analysis, we are able to look separately at trends in non-violent protests and violent expressions of conflict.

Next, ours is the first paper in this literature to look at the effect of intentional internet shutdowns on conflict outcomes. Since causal identification depends on plausibly exogenous variation in exposure, most researchers rely on random factors that determine the penetration of new internet services (Bursztyn et al. 2019; Fergusson and Molina 2019; Fujiwara et al. 2024; Müller and Schwarz 2023) or exposure to existing services (Müller and Schwarz 2021). Estimating the effect of intentional communication blackouts, on the other hand, is inherently difficult, as these events are by definition not randomly occurring. However, there is reason to believe that they effect internet users in fundamentally different ways compared to supposedly random changes in access, so studying shutdowns as unique political phenomena is important in its own right. It has been widely observed that internet shutdowns are used by governments as reactionary measures to control information and prevent the spread of protest activity (Selva 2019; Stremlau 2025), and while much of this research suggests that shutdowns do in fact inhibit protest and other forms of civil resistance, descriptive results suggest a range of more complex interactions. For example, Rydzak et al. (2020) observes cases in which protest *escalates* following a network disruption due to a political backlash effect, and Ruohonen (2024) notes that shutdowns can undermine a government's ability to control resistance mobilization due to a reduction in their capacity to monitor online activity. Given our data and the context of this study, we have the unique opportunity to not only causally identify the effects of strategic shutdowns, but shed further light on the political conditions under which these events occur, and how they ultimately impact various conflict actors.[4]

Additionally, we make an important data-related contribution to the literature on social media and conflict by leveraging the location and full text content of social media posts. Due to evolving data privacy laws and increasingly restrictive policies being imposed by companies like Meta and Google, access to post-level social media data has become

[4]Understanding the impact of internet restrictions is particularly important given the increasing willingness and capability of governments to resort to these measures in recent years. In 2024, 296 shutdowns were documented across 54 countries, the highest annual total ever recorded (Rosson et al. 2025).

more scarce for researchers in recent years. Even when such granular data is publicly available, linking these data to specific individuals or locations can be difficult, especially in the case of Facebook and other Meta-owned services. Though imperfect, our data structure allows us to roughly assign Facebook posts to a given township through the identification of public groups that are targeted at (but not restricted to) residents of that township. Other papers, such as Müller and Schwarz (2021) and Fujiwara et al. (2024), have made use of more precise geo-coded Twitter data that was gathered between 2014 and 2015, though subsequent changes to the ways in which location information is collected and shared mean that such data is not available for more recent time periods. Aside from Twitter, micro-data from other social media services has been even more restrictive, and many papers in the literature thus do not incorporate post or user-level data at all (Bursztyn et al. 2019; Enikolopov et al. 2020; Fergusson and Molina 2019). In these papers, the authors rely on a reduced form analysis, regressing the outcome on some predictor of social media exposure, but do not have access to an explicit measure of internet or social media activity.

Even among the papers that do draw from social media data directly, it is rare to see any sophisticated content analysis. Müller and Schwarz (2021, 2023) categorize twitter posts based on specific hashtag or keyword searches, and Qin et al. (2024) do the same for posts on the Chinese social media service Weibo. However, due to recent advances in the sophistication and accessibility of large language models (LLMs), natural language processing (NLP) tasks are now inexpensive and relatively easy to adapt to custom applications. We take advantage of these technologies to perform text classification that goes beyond simple keyword search. This allows us to be much more comprehensive in classifying political posts within our Facebook data. Instead of collecting only posts that contain one or more common political hashtag, we can expand the collection to include all posts interpreted as political by a trained LLM. The applications of this approach are also extremely flexible, as text can be classified according to virtually any criteria, as long as a human reader can first make accurate classifications on a very small subsample of data.

Finally, our work builds on the writings of many scholars that have previously discussed the internet in terms of liberation and control. However, while existing work tends to argue for a narrative of liberation (Diamond and Plattner 2012; Milner 2006) *or* control (Morozov 2011; Qin et al. 2017; Roberts 2018; Shapiro and Siegel 2015; Xu 2021), this paper shows evidence of both, describing the internet as a technology that can affect political outcomes in both directions, depending critically on the current state of internet freedoms.

The remainder of the paper is organized as follows. In Section 2, we begin by providing context for the setting of the study, then describe our data sources in Section 3. Next, we provide some descriptive results in Section 4 before explaining the primary empirical methodology in Section 5. We discuss results in Section 6, and Section 7 concludes.

2 Background

2.1 2021 Myanmar Coup d'État

Our research focuses on early 2021, a time in which life for the average citizen of Myanmar changed dramatically overnight. On the morning of February 1st, the military—led by Senior General Min Aung Hlaing—detained leaders of the National League for Democracy (NLD), Myanmar's elected governing party, effectively ending the nation's decade-long experiment with democracy. These arrests included President Win Myint and State Counsellor Aung San Suu Kyi, along with cabinet ministers, regional governors, activists, and journalists. Military leadership justified their actions on national television later that day, repeating unverified claims of corruption in recent national elections which the NLD carried in a landslide, and citing a clause in the constitution granting them the power to declare a national emergency in such situations. They assured the public that this was a temporary measure, until fair elections could be held, though at the time of writing—over four years later—the military shows no intention of ceding power willingly. (Goldman 2022).

After detaining opposition leaders, the military's priority was to gain control of key infrastructure, including communications systems and information sources. In the days following the coup, the civilian response was swift and nearly universal in its condemnation of the military's actions, resulting in the birth of a large and well-organized Civil Disobedience Movement (CDM), which eventually turned violent after military crackdowns were enforced (Paddock 2022).[5] As the CDM began organizing protests and labor strikes in major cities, the military moved to restrict telephone and internet access in an effort to inhibit large-scale demonstration. On February 6th, the military shut down all internet access, including both mobile and broadband connections, for a period of 30 hours. Shortly thereafter, starting around February 15th, daily internet "curfews" kept the total blackout in place between midnight and 9 a.m. (Januta and Funakoshi 2021). Finally, on March 15th, about six weeks after the coup, the military imposed the most drastic internet restriction to date—a full suppression of mobile internet connections that remained in place until at least the beginning of May (Myanmar Internet Project 2022).

While the March 15th restriction did not cover broadband connections, the impact was still nearly universal for a nation in which mobile networks accounted for nearly 75 percent of all internet traffic and over 99 percent of social media users (Kemp 2021). In addition to the small number of broadband connections, some citizens were also able to evade restrictions by installing virtual private networks (VPNs) or purchasing SIM cards from

[5] This escalation of violence resulted in the formation of the People's Defense Force (PDF), an armed resistance group that has since joined forces with existing ethnic armed organizations (EAOs) in a multi-front struggle against military rule (Hein and Myers 2021). Four years later, the region remains unstable, and the military and resistance forces are still locked in an armed battle over the future of the nation (Myers 2023).

telecommunications companies—based primarily in Thailand—that did not comply with the military's shutdown directives (Januta and Funakoshi 2021). Nonetheless, the overall effects of the shutdown were dramatic, with the majority of citizens losing reliable internet access overnight. According to Google monitoring data, search traffic dropped by over 50 percent on the morning of March 15th, and did not return to prior levels until after May 10th. (Google Transparency Report 2025) The internet watchdog organization NetBlocks also reported cellular connectivity levels dropping to around 6 percent of their normal levels on March 15th and remaining low until at least the beginning of May. (NetBlocks 2021).

While the restriction was intended to thwart resistance mobilization, activists and insurgents in Myanmar are adaptable and have experience resisting military rule during periods of high censorship and repression. Furthermore, as the military uses the internet as a surveillance tool, the shutdown may have also hindered their capacity to gather key intelligence. An important question, then, is whether the internet shutdown was effective in demobilizing the resistance—or whether resistance forces adapted and found alternative communication strategies—and whether the shutdown negatively impacted the military's ability to conduct operations against the resistance. Given the varied types of conflict occurring in Myanmar during this period, observers may also wonder if the internet shutdown had different effects on different types of conflict events (e.g., peaceful protest versus violent conflict).

2.2 The Internet in Myanmar

As Myanmar began to liberalize in 2011 (emerging from half a century of repression under the same generals that led the 2021 coup), and the telecommunications industry was opened to international competition, the cost of a SIM card dropped from hundreds of dollars to under a dollar. For the first time, reliable internet access was available to more than just the economic elite, and internet use exploded over the next decade. Since affordable access had arrived so late relative to the rest of the world, broadband connections were not common for the average business or household, and wired technology was superseded by the convenience and affordability of wireless (cellular) internet access. At the time of the coup, nearly 75 percent of internet users connected via mobile devices, and 99 percent of social media traffic was transmitted through mobile data (Kemp 2021).

As millions of people were able to access the internet for the first time, Facebook—which came pre-loaded by local phone retailers and initially allowed its smartphone app to be used without incurring data charges—quickly became the most popular social media and messaging service in Myanmar (Asher 2021). Before the coup, Facebook was used by over half of the country's 54 million people (Januta and Funakoshi 2021), and four years later, though other platforms—such as Twitter and the Russian service VKontakte—are used as well, Facebook remains dominant, accounting for over 90 percent of all social media traffic (Kemp 2024).

It is commonly said that in Myanmar, "Facebook is the internet," as its use has become so ubiquitous that many people use Facebook not only as a traditional social media service, but as a search tool, instant messenger, online marketplace, etc. (Asher 2021). In fact, the estimated 27 million Facebook users in Myanmar at the beginning of 2021 is actually *higher* than the estimated number of total internet users from the same period, indicating that everyone on the internet is also using Facebook in some form (Kemp 2021).

Due in part to its meteoric rise, Facebook was slow to realize that they lacked the capacity to adequately monitor the use of its platform in the Myanmar market. This deficiency was dramatically exposed in 2017 during the Rohingya Genocide, when the U.N. and various human rights groups documented thousands of instances of vicious hate speech against the Rohingya Muslim minority circulating freely across public Facebook groups and private messages. In the wake of the 2017 atrocities, it was widely believed that Facebook played an important role in exacerbating a conflict that led to the forced displacement of over 700,000 members of the Rohingya community in western Myanmar and the deaths of countless thousands more (Stecklow 2018).

The public pressure on Facebook was strong enough to force CEO Mark Zuckerberg to appear in front of U.S. Senators in April of 2018, promising to increase his company's capacity to monitor and regulate content in Myanmar. To Facebook's credit, they did take several positive steps in this direction, including the removal of military leader Min Aung Hlaing's personal account in August of 2018, along with the accounts of several other military and religious officials known for making derogatory posts targeted at Muslims and other minority ethnic groups. They also began actively removing individual posts containing hate speech—using a combination of human observers and automated algorithms—though their capacity to identify such content remains severely constrained (Solon 2018). In 2021, the period on which this paper focuses, hate speech was still common and widespread across public Facebook groups and pages in Myanmar.

In the days following the coup, and despite the military's efforts to suppress internet access, Facebook was used widely as a tool for organizing peaceful demonstrations—with public groups/pages serving a critical function for the resistance. In one notable example, a public page called "Civil Disobedience Movement," which was created on February 2nd and attracted nearly 200,000 followers in a single day, encouraged members to engage in forms of peaceful resistance. As the conflict expanded and evolved, use of the platform similarly evolved to encourage more violent forms of conflict. For example, "Social Punishment" posts—in which the relatives and associates of military leaders were publicly exposed—began appearing on Facebook and other social media platforms. These posts typically encouraged others to shame, ostracize, and boycott the targeted individual, and in some cases explicitly encouraged violence (Myint 2021).

On the other side of the conflict, the military also used social media to promote violence. In fact, an investigation conducted by the rights group Global Witness revealed that Facebook's own algorithm was actively recommending pro-military content that incited violence against protesters, even after the company had banned military owned and affiliated accounts from its platform. In one example, an image shows a bloody man with a rope around his neck, with the caption "This is how you should arrest them," referring to protesters. In another, a pro-military account states that "they [protesters] all need to be killed so that the children will not have the wrong role models (Ratcliffe 2021)." Anecdotally, there is evidence that posts such as these have led to extrajudicial killings of civilians on both sides of the conflict.

3 Data

3.1 Irregular Terrain Model

In order to measure geographic variation in access to the internet, our identification strategy requires an estimation of cellular signal strength on the ground. To generate accurate predictions of cell coverage with sufficient spatial resolution, we follow existing literature and employ a specific type of wave propagation model known as the Irregular Terrain Model (ITM). The ITM requires two primary inputs: 1) a high-resolution digital surface model (DSM), or topographic map, that defines elevation above sea level for all surface points in Myanmar, and 2) information on the precise location and technical characteristics of all cell towers within the country. Physical properties of electromagnetic waves determine the way in which these waves propagate outward from a point source, given local topography and climate as well as the power and orientation of the transmitting antenna. By modeling this process, proprietary software[6] can be used to generate a map of estimated signal strength across a given surface.

To generate an ITM for Myanmar, we use a DSM with 30-meter resolution[7] and cell tower coordinates from a public, user-generated database. Since accessing official records from the Myanmar government or telecommunications companies has proven to be impossible given the current political situation, this dataset—provided by an organization called OpenCellid—offers a suitable alternative: location coordinates that are generated from measurements automatically and regularly collected from users all over the world, then combined to triangulate locations of the cell towers to which these users are connecting. The OpenCellid data for Myanmar includes coordinates for over 33,000 unique tower sites,

[6]There are several software options available, but we use a tool called Cloud-RF due to its user-friendly interface and flexible feature set.

[7]The ALOS Global Digital Surface Model from the Japan Aerospace Exploration Agency (JAXA) is based on satellite imagery taken between 2006 and 2011.

a number which closely matches official estimates. For our analysis, we use data that was downloaded in November of 2023, and which contains measurements received up until that date.[8]

In addition to the coordinates of each tower, the estimation software takes an exhaustive list of input parameters to refine its predictions as much as the available data will allow. While it would not be feasible to identify all of these parameters for all towers, we have made an effort to compile as much verified information as possible, and make reasonable assumptions where necessary. For example, though we do not know the exact broadcast frequency of each antenna, we assume the industry standard for every network protocol, a variable which is identified for each tower in the OpenCellid data (e.g., GSM, LTE, UMTS, CDMA). From various telecommunications companies, we have also obtained country averages for technical antenna characteristics such as height, positioning, gain, and output power. Finally, the software provides default configurations for climate and landcover that are based on regional characteristics. Taking all of this data as input, the model estimates separate coverage maps for each cell tower, which we then merge into a single raster image file. This raster contains the average predicted signal strength on the ground (measured in decibel-milliwatts, or dBm) for each 300×300 meter grid cell within Myanmar's national boundary.

3.2 Political Conflict

Measurements of our outcome variable, political conflict, come from the Armed Conflict Location & Event Data Project (ACLED), a global research effort that collects the dates, actors, locations, fatalities, and types of all reported political violence and protest events around the world. While the full ACLED dataset is a cross-national panel, their subnational coverage is quite extensive. For Myanmar, the data contains 87,479 unique events from 2010 through 2024, though the majority of these (over 87 percent) occurred in 2021 and after.[9] See Table 1 for a partial breakdown of recorded events by type. (For most of the subsequent analysis, we focus on the frequency of *Demonstrations* and *Political Violence*, two aggregated categories of conflict, as defined in Table 1.)

For each observation, the data provides a precise geo-tagged location, date of occurrence, event type (e.g., battle, explosion, protest, etc.), description of major actors (e.g., military, militia, civilians, etc.), number of recorded fatalities, and news source.[10] ACLED regularly

[8]To be precise, the OpenCellid data includes 33,462 observations as of November 2023, while official documents report 29,508 towers as of February 2020. Since these latter documents are now several years old, this represents a lower bound for the true number, and the current total is likely closer to the OpenCellid estimate.

[9]ACLED regularly updates their records with newly recorded observations. The data used in this paper was downloaded in March 2025.

[10]For a full description of conflict classification, including detailed descriptions of each event and sub-event type, see the methodology section of ACLED's website: `https://acleddata.com/conflict-data/knowledge-base/methodology`.

draws from over 1,000 news sources when compiling and updating the Myanmar data, only about 2 percent of which are international. The vast majority of reports come from national and regional news sources, reducing the likelihood of selective reporting driven by the preferences of an international audience.

Table 1: Types of Conflict

	2021		2022	
	Frequency	%	Frequency	%
Demonstrations				
Peaceful	7008	37.66	3368	17.30
Violent	780	4.19	27	0.14
Political Violence				
Explosions / Remote Violence	2859	15.36	4400	22.60
Battles	2355	12.66	4438	22.80
Violence Against Civilians	2041	10.97	2482	12.75
Looting / Property Destruction	730	3.92	1942	9.98
Other	455	2.45	557	2.86
Arrests & Other Non-Violent Events	2381	12.79	2253	11.57
Total	18609		19467	

Notes: Table shows the number of unique events (by type) found in ACLED data for Myanmar in 2021 and 2022. The categories shown here correspond to ACLED's documented Sub-event types as follows: Peaceful Demonstrations include only the "Peaceful protest" category, while Violent Demonstrations include "Excessive force against protesters," "Protest with intervention," "Violent demonstration," and "Mob violence." Explosions / Remote Violence combine the "Chemical weapon," Air/drone strike," "Suicide bomb," "Shelling/artillery/missile attack," "Remote explosive/landmine/IED," and "Grenade" Sub-event types. Battles include the "Government regains territory," "Non-state actor overtakes territory," and "Armed clash" Sub-event types. Violence Against Civilians comprises "Sexual violence," "Attack," and "Abduction/forced disappearance," while Looting / Property Destruction includes only the "Looting/property destruction" Sub-event type. Finally, Other includes "Disrupted weapons use" and the "Other" Sub-event type, while Arrests & Other Non-Violent Events includes all remaining ACLED Sub-event types, classified in ACLED as "Strategic developments" ("Agreement," "Arrests," "Change to group/activity," "Headquarters or base established," and "Non-violent transfer of territory").

We also use information on arrests for a secondary analysis, and though ACLED documents politically motivated arrests in Myanmar, we supplement this data with a local source. The Assistance Association for Political Prisoners (AAPP), a Myanmar based human rights organization, has been keeping detailed records on arrests, detentions, and killings of political activists since the start of the coup, including information about the date, location, and government's stated justification for an arrest (when available). We use this data to help separately identify cases in which individuals were arrested for posting or engaging with political content online.

3.3 Covariates

Along with the primary variables of interest described above, we draw from a combination of spacial and administrative data to construct the covariates in our analysis. First, we use WorldPop estimates of the spatial distribution of Myanmar's population for 2020, which measure the approximate number of people living within each 100×100 meter grid cell.[11] Next, we take an annual composite of nighttime lights for the same year, which averages the full year of nightly observations by the Visible and Infrared Imaging Suite (VIIRS), providing a spatial map of average light radiance[12] that covers all of Myanmar at a resolution of 500×500 meters.[13] In the subsequent analysis, population estimates will be useful for normalizing variables in per-capita terms, while light radiance correlates with local electricity consumption, and thus can be interpreted as a proxy for economic activity, as is commonly done in the literature (Henderson et al. 2012).

Additional spacial covariates are also generated from previously described data. First, we calculate the number of cell towers within any given township, as well as its average elevation and terrain slope (the latter two of which are derived from the same DSM used to produce the ITM model). Next, we construct "free space" estimates of signal strength, which are analogous to the ITM estimates (and modeled by the same software), but assume flat topography and no obstacles between tower and receiver. The "free space" model gives, for each grid cell, the expected signal strength for a receiver living in a completely flat environment, determined only by distance to the nearest tower. Note that for any given grid cell, the ITM estimate depends on four factors: 1) the number of towers in the vicinity, 2) the placement of these towers, 3) the topography of the grid cell, and 4) the topography along a line-of-sight between the grid cell and tower.[14] Factors 1) and 2) are clearly endogenous, as tower construction is strategically determined by telecommunications planners, and factor 3) is also likely to be correlated with local economic and political conditions. Thus the purpose of the four spacial variables described above is to control for these endogenous factors, isolating variation caused by factor 4), which has been argued to be a plausibly exogenous, or randomly determined component of signal strength (Gonzalez and Maffioli 2024; Yanagizawa-Drott 2014).

Finally, to control for other township level factors that may be endogenous to our outcome variable, we draw from the Myanmar Population and Housing Census, the most recent of which was conducted in 2014. This data source includes basic demographic information at the township level, including population count, urbanization, literacy rate, public health status, and a variety of crude indicators for household wealth (e.g., phone ownership, sanitation quality, etc.).

To define our unit of analysis, we use GIS shapefiles mapping Myanmar's national and sub-national boundaries.[15] Myanmar's largest administrative divisions are states

[11] WorldPop uses a combination of census data, satellite imagery, and prediction algorithms to determine each grid cell estimate, which are then normalized at the country level to match the latest UNDP population figures.

[12] Radiance is measured in nanowatts per square radian per square centimeter ($nW \cdot sr^{-1} \cdot cm^{-2}$).

[13] This data is maintained by the Earth Observation Group at the Colorado School of Mines.

[14] The ITM is also a function of technical tower characteristics and environmental factors, but since we take these to be mostly constant in our model, their relevance here is marginal.

[15] These files are provided by the Myanmar Information Management Unit (MIMU), a UN-supported organization that maintains national statistics and GIS resources for public use.

and regions,[16] which are further divided into districts, townships, village tracts, and (less formally) villages. All subsequent analysis is conducted at the township level (of which there are 330 in total), as this is the smallest geographic unit for which ACLED observations are consistently defined. The shapefiles allow us to aggregate all other spatial data (which exist in various native resolutions) to the township level.

3.4 Facebook Data

To directly observe social media activity during the period of analysis, we downloaded a large sample of Facebook posts using Meta's API. In Myanmar, it is common to find public Facebook groups created by and for people living in a specific township, serving various functions for the community. Some of these groups are for sharing local news and information, some serve as marketplaces for goods and services, and others are specific to local associations or interest groups (e.g., university students, auto enthusiasts, farmers associations, etc.). Many groups serve multiple functions at once. To identify as many relevant groups as possible, we searched all Myanmar township names in Facebook's online research tool[17] and recorded those that could be confidently associated with a specific township (based on the group name or description). In the end, we identified 7,177 groups—covering 325 out of 330 townships in Myanmar—for which we downloaded all posts made between January 1, 2021 and December 31, 2022. The download includes not only the text content of the more than 17 million posts, but also tracks the number of comments, shares, likes, and other reactions[18] on each individual post.

Since the downloaded content does not contain location data for the individual user, we cannot geolocate Facebook activity to the same degree that ACLED allows us to do for conflict activity. By restricting the set of groups to those which could be verifiably linked to a specific township, we thus make our best attempt to estimate locations. Specifically, we assign each post in our data to the township of the group in which it was posted. We therefore make the implicit assumption that the post was made by a user located in that township, and that all likes, comments, shares, and reactions also came from users located in the same township.

As the posts in our dataset come primarily from non-political groups, we know that not all observed Facebook activity will be directly relevant to conflict outcomes. We thus

[16] Both states and regions have an equivalent status within the national government, but were given different naming conventions in recognition of the fact that states contain large ethnic minority populations, while regions are populated mainly by the national majority Bamar peoples.

[17] To access Facebook content we used CrowdTangle—a data tool formerly maintained by Facebook's parent company, Meta, that allowed researchers to download and analyze public content on Facebook, Instagram, and Reddit. While CrowdTangle did not give access to user demographics, or track any activity from private accounts, it did include all historical content from posts made to public pages and groups. As of August 14, 2024, CrowdTangle is no longer available.

[18] In addition to "liking" a post, Facebook users are given the option to respond with one of the following reaction emojis: "love," "care," "haha," "wow," "sad," or "angry."

need a method for separately identifying posts with political content, and with millions of posts to analyze, this cannot feasibly be done manually. Instead, we use a Natural Language Processing (NLP) technique to automatically categorize all posts, which consists of first manually classifying a small random subset of posts, then applying a pre-trained LLM to predict the most likely classification (political vs. non-political) for the remaining data. A more detailed explanation of this process is provided in Appendix B.

4 Descriptive Results

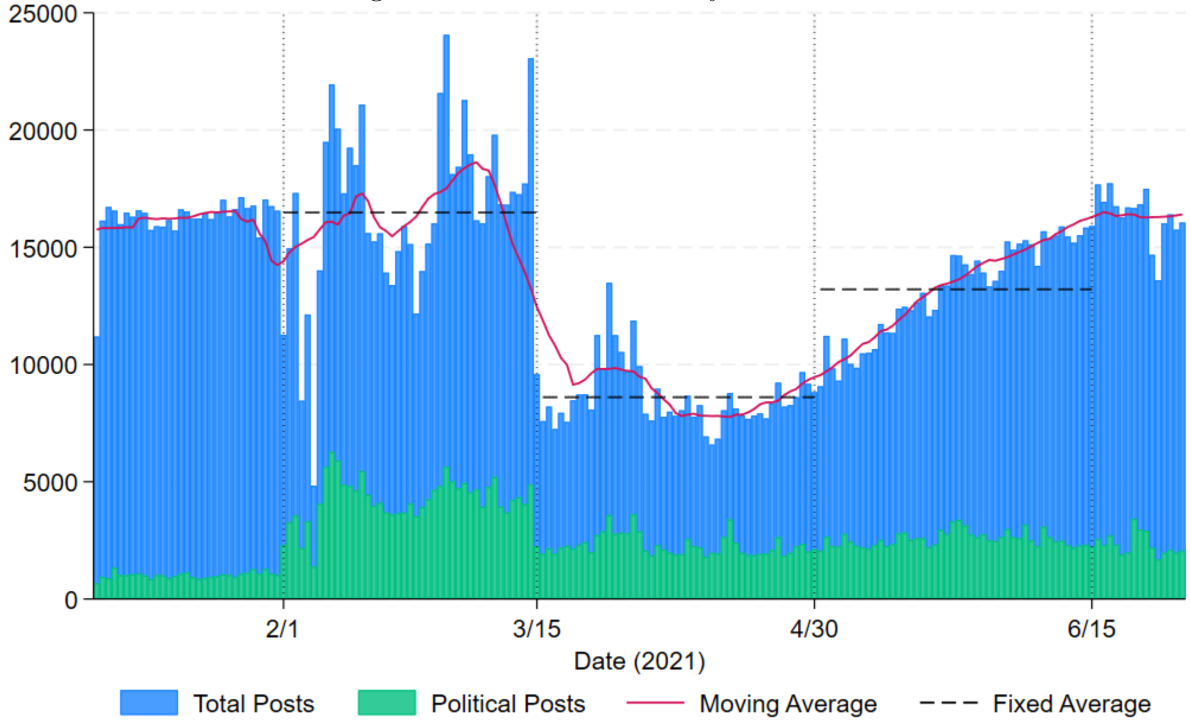
4.1 Social Media and Conflict Trends

In the following section, we show descriptively how trends in both conflict and Facebook activity changed in the periods before and after the March 15th internet shutdown. In Figure 1, we plot the daily number of Facebook posts appearing in our sample for the first half of 2021. It is immediately obvious from the figure that Facebook activity drops dramatically on the first day of the internet outage. The average number of daily posts made during the outage is just over half the average from the previous period—between the start of the coup on February 1st and the first day of the outage on March 15th. After the beginning of May, when mobile connectivity was reportedly restored to most of the nation, we see Facebook activity gradually increase again until the middle of the year, when the number of daily posts reaches a level comparable to the pre-coup baseline. As such, we observe about 5,000 more posts per day in the 1.5 months after the outage compared to the outage period itself.

In addition to total posts, Figure 1 also plots the daily number of political posts made over the same period (see Appendix B for a description of the text classification process). Unsurprisingly, the number of political posts observed before the coup is very low, constituting less than 7 percent of total posts made over the first month of 2021. However, both the level and proportion of political content jumps dramatically after February 1st, as the coup induces a higher increase in political posts relative to total posts (political posts represent over 25 percent of all posts made between February 1st and March 15th). After the start of the internet restriction, the number of political posts drops off abruptly, but in proportion with the general drop in Facebook activity (maintaining the same 1:4 ratio with total posts through the duration of the internet outage). Interestingly, however, after the restriction is lifted around the end of April and early May, the number of political posts does not increase along with the general increase in Facebook activity, but remains remarkably stable through at least the month of June.

Next, we plot the total number of conflict events from ACLED over the same time period (Figure 2). Not surprisingly, there is a very low baseline level of political conflict before the coup, but activity spikes in the days after February 1st, reaching a maximum

Figure 1: Facebook Activity Trends



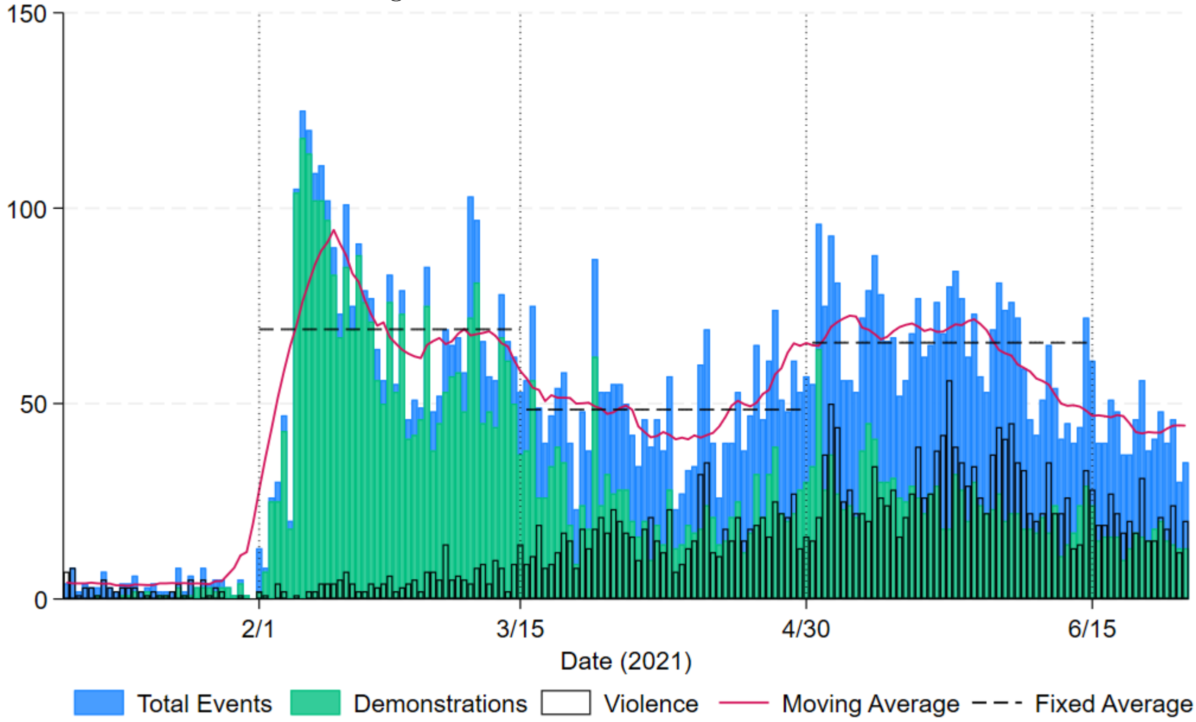
Notes: Bar height represents the number of posts in our Facebook database for each day over the first half of 2021, for both the total set of posts as well as the subset that were classified as political in nature (see Appendix B). The Solid line plots a two-week moving average (including 6 lagged and 7 lead values), while the dashed lines indicate the average number of daily posts during the period spanned by the line's length (both averages are calculated for the total number of posts).

during the second week of February. Looking across the same three time periods as defined in Figure 1, we notice a similar—though less dramatic—pattern in trends. During the 1.5 month duration of the internet restriction, the average daily number of conflict events is lower than the 1.5 months directly preceding and following. Compared with the period between the start of the coup and the beginning of the internet outage, we observe about 25 percent fewer events per day during the extent of the outage itself.[19]

Taken together, these figures provide suggestive evidence that the restriction on internet access, beginning in mid March and ending in early May, had a measurable effect on both online Facebook activity and real-world conflict. At the very least, the start of the internet outage coincided with a reduction in the daily number of Facebook posts as well as the incidence of conflict events, and the end of the outage period coincided with a subsequent rise in both measures.

[19]The frequency of demonstrations actually experiences a more pronounced drop during the period of the internet restriction, but this trend is partially offset by the frequency of violent events, which is steadily growing over the same period.

Figure 2: Political Conflict Trends



Notes: Bar height represents the number of conflict events appearing in our ACLED sample for each day over the first half of 2021. In addition to the total number of events, demonstrations and incidents of political violence are represented separately (see Table 1 for a summary of event type definitions). The solid red line plots a two-week moving average (including 6 lagged and 7 lead values), while the dashed lines indicate the average number of daily conflict events during the period spanned by the line's length (both averages are calculated for total events).

4.2 Correlation Between Conflict and Social Media Activity

Next, using a standard two-way fixed effects regression, we show how social media activity is correlated with political conflict at the township-week level, i.e.,

$$Y_{i,t} = \alpha_i + \delta_t + \eta_{sr,t} + \beta \cdot FB_{i,t} + \epsilon_{i,t}, \tag{1}$$

where $Y_{i,t}$ represents some conflict outcome in township i during week t, α_i and δ_t represent township, week, and state/region by week fixed effects,[20] respectively, $FB_{i,t}$ denotes the number of Facebook posts observed in township i during week t (measured in 1,000s of posts), and $\epsilon_{i,t}$ is the idiosyncratic error term. The latter set of fixed effects absorb any endogenous factors that vary across time and state/region, restricting cross-sectional comparisons to townships within the same state/region.

We estimate values of β using the Poisson pseudo maximum likelihood (PPML) estimator—first proposed by Gourieroux et al. (1984) and popularized by Silva and Tenreyro (2006) as an alternative to using log-linearized OLS for constant-elasticity models—which is particularly well suited for count data in which the outcome takes on

[20]The state/region is the first level administrative division in Myanmar, of which there are 15, though we further divide Bago region into two subdivisions and Shan state into 3 subdivisions to create 18 fixed effects, containing anywhere from 7–45 townships each.

many non-positive (zero) values. Since many townships in Myanmar have experienced no incidents of certain types of conflict over the past four years, this is an attractive estimator for our context. (For relatively small values of β, PPML coefficients can be interpreted in the same way as coefficients in log-linear models, i.e. "a one unit change in X is associated with a $\beta \times 100$ percent change in Y.") We present PPML estimates of β in Table 2, in which we show coefficients on total posts as well as political posts, for both demonstrations and incidents of political violence as the dependent variable.

Table 2: Social Media and Conflict – Correlations over Time

	Pre-Restriction		Mid-Restriction		Post-Restriction	
	(1) Dem.	(2) P.V.	(3) Dem.	(4) P.V.	(5) Dem.	(6) P.V.
Total Posts	0.0989**	0.4836*	0.0438	0.2549	-0.1068	0.0654
	(0.0424)	(0.2553)	(0.0702)	(0.1862)	(0.0961)	(0.0518)
Political Posts	0.4021**	3.1384*	0.1714	1.2044	-0.2724	0.9819***
	(0.1969)	(1.7485)	(0.3338)	(0.9801)	(0.3111)	(0.2486)
1 Period Lag						
Total Posts	0.0336	-0.0814	-0.0231	0.1780*	-0.1240	0.0255
	(0.0281)	(0.1950)	(0.0440)	(0.0909)	(0.1040)	(0.0389)
Political Posts	0.2861**	0.5474	-0.1156	0.7455*	-0.0944	0.5147**
	(0.1351)	(1.1496)	(0.1768)	(0.4355)	(0.3156)	(0.2177)
1 Period Lead						
Total Posts	0.0553**	0.1671	0.0148	-0.0016	-0.0611	0.0446
	(0.0266)	(0.3271)	(0.0863)	(0.1620)	(0.0787)	(0.0401)
Political Posts	0.2899*	0.3378	0.1808	-0.1275	-0.3959	0.7020**
	(0.1581)	(1.0855)	(0.3063)	(0.6451)	(0.3485)	(0.3368)
Dep. Var. Mean	1.213	0.083	0.568	0.341	0.350	0.642
Observations	1980	1980	2310	2310	11550	11550

Notes: *** p<0.01, ** p<0.05, * p<0.10. Reported coefficients are PPML estimates of β from Equation 1, with standard errors—clustered at the township level—displayed in parentheses. Odd numbered columns show results from specifications in which the number of demonstrations is taken as dependent variable Y, while political violence is taken as the dependent variable in even-numbered columns. In the first two columns, our sample is limited to weeks occurring before the nationwide internet restriction (between February 1st and March 15th, 2021), while the sample is restricted to weeks occurring during the internet restriction in the next two columns (March 16th to April 30th, 2021), and weeks occurring in the months after the end of the restriction in the final two columns (May 1st to December 31st, 2021). We show coefficients for both the total number of posts in our sample as well as the subset of posts classified as political by the language model (measured in 1,000s of posts). For both categories of Facebook post, we first present results from a specification in which we regress $Y_{i,t}$ on $FB_{i,t}$, then a specification in which we regress $Y_{i,t}$ on $FB_{i,t-1}$ (1 period lag), and finally one in which we regress $Y_{i,t}$ on $FB_{i,t+1}$ (1 period lead).

We note that there is a significant and positive correlation between Facebook activity and all forms of conflict in the 1.5 months between the coup and the March 15th internet restriction. Specifically, an additional 1,000 posts observed in a given township and week is associated with an expected 10 percent increase in the number of demonstrations occurring in that township during the same week. Not surprisingly, an additional 1,000 political posts is associated with an even larger, 40 percent increase. We also observe strong positive correlations between Facebook posts—both total and political—and incidents of political violence. These coefficients are even larger (though less significant) than the previous set, including a massive expected increase associated with political posts.

In the second panel of Table 2, we show estimates from a model with a 1 period lag, that is, a model in which we replace $FB_{i,t}$ with $FB_{i,t-1}$, but which is otherwise the same. In this regression, β now estimates the relationship between political conflict and Facebook activity in the preceding week, which gives some insight into the direction of causality between the dependent and independent variables. We can see that there is no significant relationship between the aggregate number of Facebook posts and the intensity of violent conflict one week later, but we do estimate a significantly positive effect on the number of demonstrations—when looking at strictly political posts. While this result doesn't prove a causal relationship, since the error term may still include endogenous correlates (including a lagged term of the dependent variable), it is suggestive. Since the only significant estimate is for the effect of political posts on demonstrations, this could be interpreted as evidence of a coordination effect, i.e., individuals in a given township are sharing information about the political crisis, possibly planning in-person resistance, which leads to a higher likelihood of future protest. There is no analogous effect on other forms of political conflict, however, possibly because the types of events categorized as "Political Violence" (see Table 1) do not require mass coordination of previously unassociated actors. These events are either planned formally through military command structures, or can be carried out by small groups / individuals.

Finally, in the last panel of Table 2, we replace the 1 period lag with a 1 period lead ($FB_{i,t-1}$ with $FB_{i,t+1}$), and display the resulting β estimates. These results describe how Facebook activity in a given week is related to political conflict in the preceding week, and provide evidence of the reverse causal direction. We note that higher levels of Facebook activity are correlated with a higher frequency of *prior* demonstrations, both because the error term (likely) contains a lead term of the dependent variable itself, and because protest activity results in a sustained increase in online activity that lasts longer than a single week. The fact that the coefficients on political violence are again insignificant implies that these kind of conflict events cause social media spikes that, while large in the immediate term, are ultimately short-lived.

Moving beyond the initial period of conflict occurring between the start of the coup and the nationwide internet shutdown, the size and significance of our estimates falls in most specifications. In the period during which the internet shutdown is ongoing (March 15^{th} to April 30^{th}), we observe no significant correlations between Facebook activity and conflict (aside from two marginally significant estimates in the second panel). However, during the final 8 months of 2021, after the internet restriction has ended, we estimate strong positive coefficients on political violence and *negative* (though insignificant) effects on demonstrations. This is essentially a reversal of the estimates from the pre-shutdown period, during which we observed positive effects on demonstrations and largely null effects on political violence. Again, though we cannot rule out the existence of confounding factors or reverse causality, the fact that this set of coefficients changes dramatically across the selected time periods suggests a meaningful shift in the relationship between the internet and real-world conflict as the political crisis unfolded.

5 Methodology

Our primary empirical strategy relies on the timing of the nationwide mobile internet blackout imposed by the military government on March 15$^{\text{th}}$, 2021. If this action was indeed successful in restricting citizens' access to the internet and social media, we expect it would have had a larger impact in areas with higher levels of access *ex ante*. In the extreme case, the blackout would be toothless in areas with no internet access to begin with, while having a universal impact in areas where everyone has access to a mobile network. In this sense, we can use a DiD model to estimate the effect of the blackout by defining high access areas as the treated group and low access areas as control (defined along a continuous spectrum of treatment intensity, or exposure).

5.1 Variation in Treatment Exposure

Recall that the unit of analysis in this study is the township, Myanmar's third level administrative division, of which there are 330 nationwide. The military's mobile internet blackout extended uniformly to all townships in Myanmar in the sense that the issued mandate and physical network restrictions did not vary by location, i.e., everyone was subjected to the same policy change. However, when considering the blackout's potential *impact*, we must acknowledge geographic differences in what we can call *exposure* to the policy change. In our setting, we characterize exposure as the degree to which an individual (or geographic cluster of individuals) was able to access the internet prior to any restrictions, which we estimate by calculating the percentage of people living in each township that receive a sufficient cellular signal to send and receive mobile data.

To characterize what constitutes a "sufficient" signal, we borrow a standard definition from the technical literature on radio wave propagation, which commonly sets a sensitivity threshold of -95 dBm for network functionality on a modern smartphone (Farahani 2008).[21] In other words, we define any grid cell[22] within Myanmar's national boundary as receiving

[21]dBm = decibel-milliwatt is a standard unit of absolute power used to describe radio, microwave, and fiber-optic communication networks.

[22]Recall that the resolution of the ITM is 300 × 300 meters.

a sufficient level of cellular signal if the ITM estimates a signal strength greater than or equal to -95 dBm for that cell, and any grid cell for which the ITM estimate is below -95 dBm as receiving an insufficient signal.[23] From an individual mobile user's perspective, moving from an area that receives -95 dBm power to an area that receives, say, -96 dBm would result in a change from "one bar" to "no bars," i.e., from a weak but functional signal to no signal. If this individual was browsing Facebook in the first area, their feed would stop loading data in the new area.

Having thus characterized each grid cell as either having data access or not, we construct a township level variable for the number of people that fall within grid cells above the sensitivity threshold, as well as the *fraction* of each township's total population that live in these cells.[24] This latter variable ranges from 0 to 1, with an average value of about 0.7, indicating the existence of some townships that receive no cell coverage and some that enjoy universal coverage, while just under 70 percent of a township's population lives within signal range on average (Figure 3). As a measure of internet access, this variable is imperfect for two main reasons: 1) for people that do not own internet-capable devices, living in an area with mobile internet connectivity does not equate to access; and 2) people that live in areas without mobile connectivity could still be accessing the internet through broadband connections. However, due to a high rate of internet penetration (over 43 percent of Myanmar's population reportedly used the internet in 2021), and low broadband usage (over 73 percent of these internet users connected via mobile device) we believe these concerns to be relatively benign in our context (Kemp 2021).

5.2 Simple Difference-in-Differences

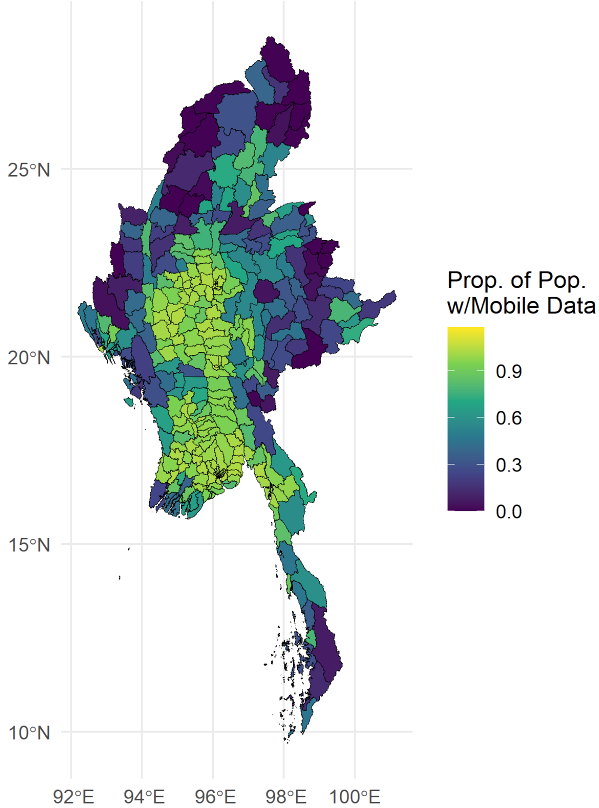
We now turn to the primary empirical strategy of this paper, which leverages cross-sectional variation in the level of internet access as well as the timing of the internet shutdown—a universal shock to access that was implemented uniformly across all townships. As discussed above, this empirical approach relies on a DiD comparison of conflict before and after the implementation of the mobile internet outage, and therefore a causal interpretation does not require exogeneity in access *levels* across townships, but merely exogeneity in trends—i.e., that temporal changes in conflict across townships would not be correlated with internet access in the absence of the shutdown event. Since nearly all townships were affected by the ban to some extent,[25] we use a continuous treatment variable that measures exposure to the outage, rather than a dichotomous treatment indicator. This is an *ex ante* measure of the degree to which an individual township is

[23]See Data section for a complete description of the ITM and our method of estimating signal strength for any discrete grid cell.

[24]Since our population data is spatially distributed at an even higher resolution than the ITM, we can easily calculate the total population in each grid cell that falls above the threshold.

[25]Only 5 out of 330 townships could be considered *untreated*, i.e., have an estimated internet access level (treatment exposure) of 0.

Figure 3: Mobile Data Access



Notes: Map shows the estimated proportion of each township's population that lives in an area with "sufficient" cellular signal strength (at least -95 dBm) to access the internet via mobile data network.

expected to be impacted by the internet restriction, described above as the number of people in a township that live within areas of adequate cellular signal strength.

After combining data sources and creating a balanced panel of bi-weekly[26] observations grouped at the township level, we define our basic specification: a simple DiD regression of the outcome variable on the interaction of the access variable and a post-treatment indicator, i.e.,

$$Y_{i,t} = \alpha_i + \delta_t + \eta_{sr,t} + \beta\,(access_i \times post_t) + \epsilon_{i,t}, \tag{2}$$

where $Y_{i,t}$ represents some measure of conflict in township i during the two-week period t. Township fixed effects, bi-week fixed effects, and state/region by time fixed effects are represented by α_i, δ_t, and $\eta_{sr,t}$, respectively, while $\epsilon_{i,t}$ denotes the idiosyncratic error term. The $access_i$ term is the treatment variable, measuring internet access at the township level (in terms of 10,000s of people with internet access), and $post_t$ is an indicator equal to one for any two-week period t that comes after the implementation of the internet outage. A similar approach, in which the continuous treatment variable represents each unit's exposure to some policy change, is taken in Finkelstein (2007). In this study of Medicare expansion, the treatment is defined by pre-expansion levels of private insurance

[26]We define each period length as two weeks since there is high variance in the outcome variable across weeks and thus estimating weekly changes results in large standard errors.

coverage for each U.S. state, as states with higher levels of existing health care coverage are less affected by changes to Medicare eligibility. More recent examples of this estimation strategy can also be found in Dube and Vargas (2013) and Nunn and Qian (2011).

The coefficient of interest in this regression is β, which we interpret as the effect of internet exposure on conflict. Specifically, it tells us how conflict trends diverge for townships with different levels of internet access, so a negative coefficient would imply that the internet outage reduced conflict by a greater degree in areas with higher levels of ex ante access. If a larger *negative* shock to internet exposure leads to a greater *reduction* in conflict, then inversely, a larger *positive* shock should lead to a greater *increase* in conflict levels. Simply put, we would interpret a negative coefficient on β as evidence that internet exposure is increasing the prevalence of political conflict in Myanmar.

One potential confounder of the causal interpretation of these results is the existence of any factors that are correlated with the $access_i$ variable and over which conflict trends also diverge after the outage. For example, internet access is correlated with population, since more populous areas attract a higher density of communications infrastructure. If some nationwide event, roughly coinciding with the internet blackout, caused conflict to decrease more in highly populated townships relative to sparsely populated townships, β could be capturing a spurious effect. While the township fixed effects absorb any time-invariant, township level covariates, they do not control for these differential trends. Therefore, in our preferred specification, we also include an interaction between the $post_t$ indicator and a vector of time-invariant, township level controls, as in

$$Y_{i,t} = \alpha_i + \delta_t + \eta_{sr,t} + \beta\,(access_i \times post_t) + \gamma\,(\chi_i \times post_t) + \epsilon_{i,t}, \tag{3}$$

where the vector χ_i accounts for population, percent urban population, literacy rate, sanitation quality, nighttime lights radiance, number of cellular towers, average elevation, average slope, "free space" signal strength,[27] historical conflict trends, and historical trends in Facebook activity.[28]

5.3 Event Study

Treatment effects from the simple DiD specification can be interpreted causally only under the assumption of parallel trends, i.e., that differential changes in conflict across townships would be uncorrelated with internet access in the absence of the internet

[27]See Section 3.3 for a description of the "free space" estimates. We construct this control variable by calculating a population weighted, township average of all grid cell level estimates of signal strength as predicted by the "free space" model. We use population weights to construct this measure since the *access* variable also accounts for the geographic distribution of population within a township.

[28]For historical controls, we calculate township level averages for the number of historical conflict incidents from January 1st, 2010 (the starting date for ACLED) to December 31st, 2020, and the number of Facebook posts in our dataset made between January 1st and February 1st, 2021 (prior to the coup).

outage event. Obviously we cannot observe this counterfactual condition directly, but we can use pre-treatment periods to conduct an indirect test: if we observe no correlation between conflict trends and internet access during pre-treatment periods, we assume that these parallel trends would continue in the counterfactual. To test for the existence of pre-treatment parallel trends, and to track the evolution of treatment effects as we move further away from the start of the treatment period, we employ an event-study model that allows us to assess the evolution of relative outcomes while controlling for fixed conditions within townships and national trends over time, as well as differential time trends across a set of covariates:

$$\begin{aligned} Y_{i,t} = \alpha_i + \delta_t + \eta_{sr,t} + & \left(access_i \times \sum_{\substack{k=-m \\ k \neq -1}}^{n} \beta_k I\left(t - t^* = k\right) \right) \\ & + \left(\chi_i \times \sum_{\substack{k=-m \\ k \neq -1}}^{n} \gamma_k I\left(t - t^* = k\right) \right) + \epsilon_{i,t}. \end{aligned} \tag{4}$$

The terms in Equation 4 have the same interpretation as in Equation 3, but we exchange the static interaction term for a series of interactions between the access variable and an indicator measuring time relative to the implementation of the outage. (We also add a similar set of interactions for the vector of control variables χ_i.[29]) In our notation, t^* represents the treatment period, in which $k = 0$ (implying that k is positive for all post-treatment periods and negative for pre-treatment periods). The limits of the summation term, m and n, represent the number of bi-weeks before and after the treatment period, respectively, that are used in the estimation. By omitting the interaction term for $k = -1$, Equation 4 estimates $m + n$ individual β_k coefficients, each representing the difference in outcome trends (between the period directly before treatment and k periods after treatment) for townships with one additional unit of $access_i$ (10,000 additional people with internet access). If pre-treatment trends in violence are not correlated with internet access (i.e., the parallel trends assumption holds), we expect β_k to be statistically indistinguishable from 0 for all $k \leq -2$.

[29]Including all γ_k terms for each covariate uses up many degrees of freedom in this model, and can be computationally intensive. To reduce burden on the data and increase statistical power, we pool adjacent time periods when creating the indicators to interact with χ_i (such that we only create one indicator for each of $k \in \{-2, -1\}$, $k \in \{0, 1\}$, $k \in \{2, 3\}$, etc.). In practice, there are thus only half as many γ_k for each covariate as there are β_k.

6 Results

6.1 Simple DiD Results

Table 3 reports values of β from the specification in Equation 3 for several different outcomes. In all models, we estimate heteroskedasticity-robust standard errors clustered at the township level. Here we define March 15^{th} as the start date of the internet outage, taking the 10 weeks prior as the pre-treatment period and subsequent 10 weeks as the post-treatment period. Since we again use PPML to estimate our model, the coefficients presented in Table 3 represent the difference in the percent change of the outcome variable (after the internet outage) between townships separated by an additional 10,000 people with internet access. In other words, we are estimating the percent difference in the effect of the internet outage associated with a 1 unit increase in the internet access variable ($access_i$).

Moving from left to right, the columns in Table 3 are ordered in terms of increasing covariate dimensionality. In column (1), the regression specification controls only for population, which is necessary to normalize the treatment variable (since *access* is measured in raw population numbers). The next specification adds “free space” signal strength, average elevation, average slope, and number of cell towers (referred to collectively as “free space” controls), while the third specification adds nighttime lights, literacy, urbanization, and sanitation quality, (referred to collectively as demographic controls). The model in column (4) adds historical controls for conflict and Facebook activity trends, and the final specification also includes state/region by time fixed effects, restricting cross-sectional variation to inter-state/region comparisons.

In the first regression, controlling only for township population, we estimate negative effects on total conflict, Facebook posts, and demonstrations, but a positive effect on violent conflict (though the latter three coefficients are insignificant). After adding the “free space” set of controls in column (2), the negative coefficients increase substantially in magnitude and become statistically significant, while the estimated effect on violent conflict remains largely unchanged. Adding demographic controls in column (3) and historical controls in column (4) does not have a meaningful impact on coefficient estimates for Facebook posts or demonstrations, but coefficients for total and violent conflict are attenuated toward zero.

Finally, after adding state/region by time fixed effects in column (5), coefficients on total and violent conflict both attenuate further toward zero and neither remains statistically significant, while the estimated effect on demonstrations remains relatively unchanged from previous specifications. Adding this last set of fixed effects has a small impact on all conflict coefficients, but a more dramatic effect on the coefficient for Facebook activity, which loses statistical significance. This is likely an artifact of data-quality issues

Table 3: Simple DiD Results

	(1)	(2)	(3)	(4)	(5)
FB Posts	-0.003	-0.042**	-0.035**	-0.030**	-0.021
	(0.013)	(0.017)	(0.015)	(0.015)	(0.018)
Mean	554.093	554.093	554.093	554.093	554.093
Total Conflict	-0.024**	-0.045***	-0.049***	-0.034*	-0.026
	(0.010)	(0.016)	(0.017)	(0.020)	(0.018)
Mean	2.096	2.096	2.096	2.096	2.096
Demonstrations	-0.017	-0.077***	-0.095***	-0.083***	-0.080***
	(0.014)	(0.017)	(0.018)	(0.021)	(0.019)
Mean	1.355	1.355	1.355	1.355	1.355
Political Violence	0.023	0.032	0.063*	0.041	0.042
	(0.017)	(0.026)	(0.033)	(0.036)	(0.031)
Mean	0.521	0.521	0.521	0.521	0.521
Observations	3300	3300	3300	3300	3300
Free Space Controls	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Hist. Conflict & FB Controls	No	No	No	Yes	Yes
State/Region by Time FEs	No	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.10. Reported coefficients are estimates of β from Equation 3, with dependent variable Y listed in the first column. All coefficients are estimated using PPML, and robust standard errors (clustered at the township level) are displayed in parentheses. Data is structured as a township-(bi-week) panel, running from January 4$^{\text{th}}$ to May 23$^{\text{rd}}$, 2021 (from 10 weeks before the internet outage on March 15$^{\text{th}}$ to 10 weeks after), so the regression identifies effects from average biweekly outcomes during the 10-week post-treatment period relative to the 10-week pre-treatment period. The construction of outcome variables "Demonstrations" and "Political Violence" is described in Table 1, and "Total Conflict" is the sum of all conflict events recorded in ACLED for a given township and time period. "FB Posts" represents the total number of Facebook posts made to all groups in our Facebook dataset associated with a given township during a given time period. In column (1), results are reported from a specification that includes only population (measured in 2020) in the vector of time-invariant control variables χ_i. Results from columns (2) through (4) are estimated after additionally adding the following time-invariant control variables to χ_i: (2) average "free space" signal strength, average elevation, average slope, and total number of cell towers; (3) all previous controls as well as average nighttime lights intensity (2020), adult literacy rate (2014), percent urban population (2014), and percent of households with safe sanitation (2014); (4) all previous controls as well as the number of historical conflict incidents (prior to 2021) and the number of Facebook posts in our dataset made between January 1st and February 1st, 2021 (prior to the coup). Finally, results in column (5) are estimated from a regression that includes this full set of controls as well as state/region by time fixed effects.

inherent to our set of Facebook posts, as we are not able to perfectly measure township level Facebook activity, particularly among townships located in close proximity of one another. As such, variation in Facebook activity *across* townships, but *within* state/region, will be a particularly noisy measure, and likely the reason that adding state/region by time fixed effects absorbs some of the magnitude on this coefficient.[30]

6.2 Event Study Results

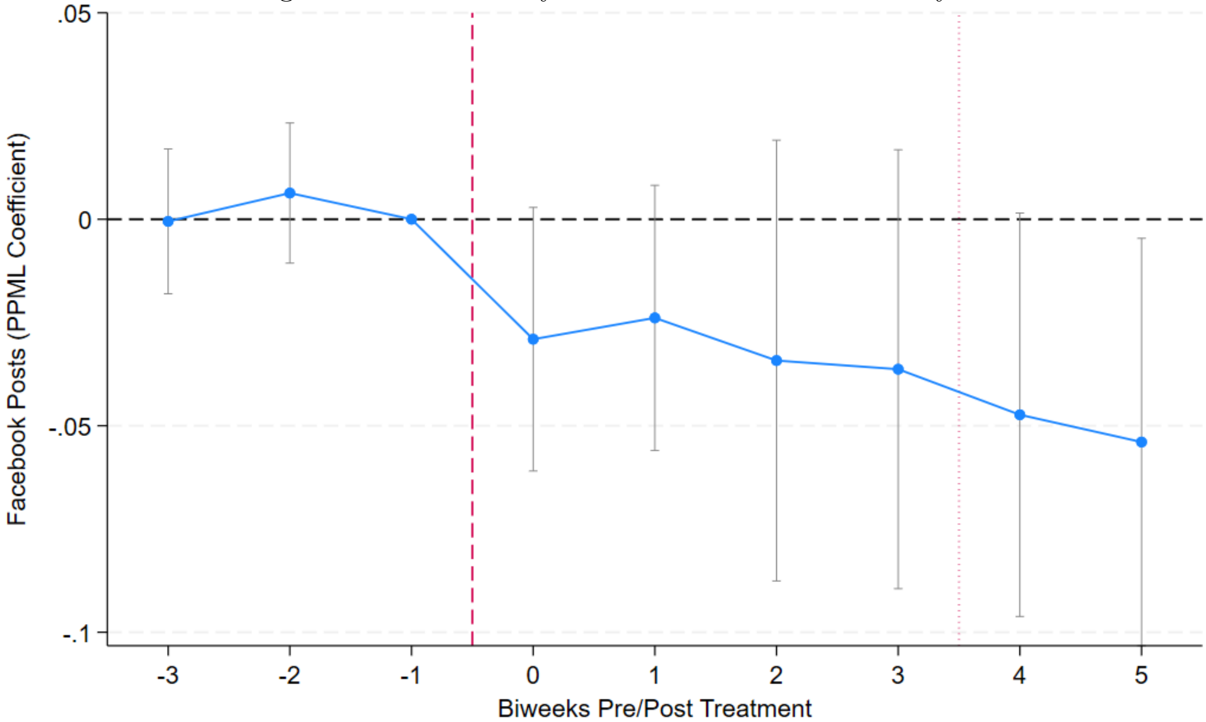
We now present results from the event study model, separately plotting each β_k from three periods before the beginning of the internet restriction until six periods after. First, in Figure 4 we show effects on the number of Facebook posts per township (using the specification from the second to last column of Table 3), where we see evidence of an immediate trend break on Facebook activity in the first post-restriction period. Though the estimates are noisy, this negative effect is sustained through the end of May 2021. In the three bi-weeks (six weeks) before the start of the internet outage, coefficients are small and statistically indistinguishable from zero, suggesting the existence of parallel trends across different levels of treatment exposure.

Next we present dynamic results for conflict outcomes. In Figure 5, we track treatment effects on the number of conflict incidents per township, separately plotting the effects for demonstrations and other forms of political violence in the same figure (using the specification from the last column in Table 3). We note remarkably parallel pre-trends for demonstrations, but evidence of a slight positive (though statistically insignificant) pre-trend for political violence (see Section 6.3.4 for further discussion). Immediately after the start of the internet restriction, we observe a relatively small—and not statistically significant—positive effect on political violence, but a significant negative effect on demonstrations, which becomes more negative in the second post-treatment period and reaches it's minimum by the third post-treatment period (six weeks after the start of the internet restriction). This negative effect on the occurrence of demonstrations remains significant and relatively stable through at least the end of May. In contrast, the estimated effect on other forms of political violence is statistically insignificant and close to zero throughout the entirety of the period presented here.

These dynamic results corroborate the interpretation of the static DiD and provide additional insight. First, the ability to track coefficients in the periods before the imple-

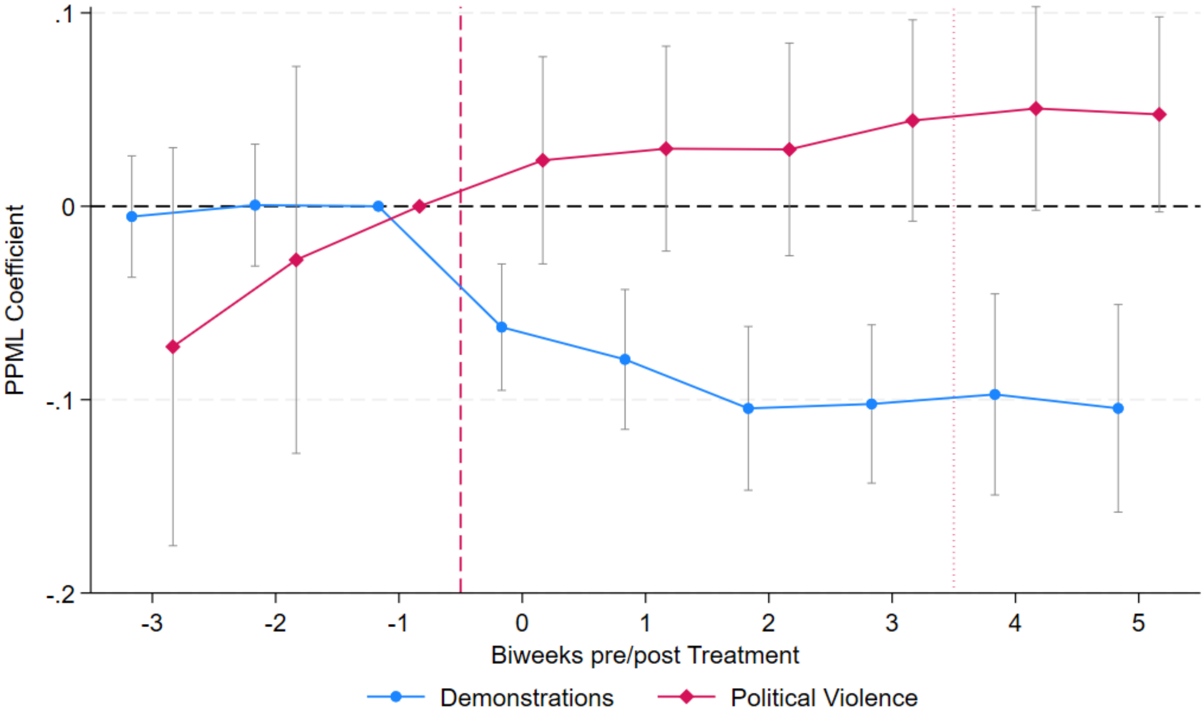
[30]Recall that we only observe posts made to public groups that are associated with specific townships, making the implicit assumption that all posts are made by a user presently located in that township. However, since these public groups are not restricted to local residents, we should expect at least some degree of contamination, i.e., posts appearing in a given township group that were made by a user located elsewhere. This introduces measurement error in our Facebook data, as some posts are undoubtedly attributed to the wrong township. Additionally, it seems likely that this kind of classification error will mostly occur between nearby or neighboring townships (within the same state/region), as the likelihood of engaging with content intended for a specific township's residents should be decreasing with distance.

Figure 4: Event Study Results – Facebook Activity



Notes: Points represent PPML estimates of β_k from Equation 4, with dependent variable defined as the total number of Facebook Posts, for each two-week period between February 1st and June 6th, 2021 (including 90% confidence bands based on robust standard errors clustered at the township level). The internet restriction was in place between the vertical dotted lines (March 15th to approximately the beginning of May), and all effects are estimated in reference to the two-week period directly before the implementation of the restriction. The coefficients represented here are estimated from the specification of Equation 4 that does not include state/region by time fixed effects, but does include all other township-level controls: average "free space" signal strength, average elevation, average slope, total number of cell towers, average nighttime lights intensity (2020), total population (2020), adult literacy rate (2014), percent urban population (2014), percent of households with safe sanitation (2014), number of historical conflict incidents (prior to 2021), and the number of Facebook posts in our dataset made between January 1st and February 1st, 2021 (prior to the coup).

Figure 5: Event Study Results – Demonstrations vs. Political Violence



Notes: Points represent PPML estimates of β_k from Equation 4, with dependent variable defined as either the number of demonstrations or other forms of political violence (see Table 1), for each two-week period between February 1$^{\text{st}}$ and June 6$^{\text{th}}$, 2021 (including 90% confidence bands based on robust standard errors clustered at the township level). The internet restriction was in place between the vertical dotted lines (March 15$^{\text{th}}$ to approximately the beginning of May), and all effects are estimated in reference to the two-week period directly before the implementation of the restriction. The coefficients represented here are estimated from the specification of Equation 4 that includes state/region by time fixed effects in addition to all other township-level controls: average "free space" signal strength, average elevation, average slope, total number of cell towers, average nighttime lights intensity (2020), total population (2020), adult literacy rate (2014), percent urban population (2014), percent of households with safe sanitation (2014), number of historical conflict incidents (prior to 2021), and the number of Facebook posts in our dataset made between January 1$^{\text{st}}$ and February 1$^{\text{st}}$, 2021 (prior to the coup).

mentation of the internet restriction provides plausible justification of the parallel trends assumption: that without the implementation of the restriction, townships with different levels of $access_i$ would not have experienced differential changes in conflict during the post restriction periods.[31] While it is impossible to test for conditions under the unobserved counterfactual, the estimates of β_k in pre-treatment periods provide the best alternative metric, i.e., the difference in outcome evolutions for townships with different *access* levels at a time when no townships have yet been treated. Since the dynamic plots for all outcomes show estimates that are statistically indistinguishable from 0 across the six-week period between the coup and the start of the internet restriction, we invoke parallel trends through the implicit assumption that these null effects would have continued beyond March 15th in the absence of any internet restrictions.

Second, the dynamic plots provide important insight into the evolution of treatment effects in post-treatment periods as well. While the static DiD results simply estimate a negative effect on the number of demonstrations, the dynamic specification shows that the full impact of the internet restriction is not observed until three periods (six weeks) after the first post-treatment period. This suggests that internet exposure has a delayed effect on conflict outcomes, a result that is not altogether surprising, since it necessarily takes time for information to be acted upon. Though the coefficient on the first post-treatment period is already negative, this effect grows over the two subsequent periods before reaching a minimum in the third post-treatment period. This effect size is then sustained over the next three periods, even after the internet restriction was supposedly lifted in the first weeks of May.

6.3 Robustness

Our primary results suggest that the internet was an important driver of protest activity in post-coup Myanmar, evidenced by what we observe when the internet is effectively shut down for a period of time. In the six weeks after the shut down, townships in which an additional 10,000 people had internet access experienced an approximately 8 percent relative reduction in demonstration activity, and a 2–3 percent reduction in Facebook activity. Thus, in the event of an exogenous shock that reduces Facebook access by a certain amount, we may expect an even larger negative response in protest activity. Inversely, we can say that *increasing* Facebook access by the same amount would lead

[31] In the case of a continuous treatment DiD, the standard parallel trends assumption needs to be strengthened from the binary treatment case. Under standard parallel trends, one assumes that the evolution of outcomes that treated units would have experienced in the absence of treatment is the same as the evolution of outcomes that untreated units actually experience. Under "strong" parellel trends, this assumption is extended to all values of the treatment variable, i.e., that the average evolution of outcomes for the entire population, had they experienced treatment level d, would have been the same as the path of outcomes for the group that actually experienced d (for all d in the domain of the treatment variable) (Callaway et al. 2024).

to a corresponding *increase* in Demonstrations. Of course, other online services are also affected during a general internet shutdown, but due to the dominance of the Facebook platform in Myanmar (see Section 2.2), we believe that the primary effects of the internet shutdown are driven by changes in Facebook exposure.

In this section, we consider potential threats to this causal interpretation of treatment effects and present evidence against alternative explanations.

6.3.1 Alternate Specifications

We have already shown that our primary estimates are stable across specifications including a range of covariates (see Table 3). After the addition of "free space" controls, effects on conflict outcomes do not change significantly upon further controlling for demographics, historical trends, and state/region by time fixed effects. This stability helps to establish our identifying assumption that effects are driven by differences in internet access rather than some other combination of correlated factors.

Additionally, we can show that our estimates are not unique to the estimation method. Though we believe that PPML estimation is most appropriate given the nature of our data, one could alternatively estimate OLS coefficients using a binary outcome, i.e., a linear probability model (LPM). In this case, estimates of β would represent differential changes in the likelihood of observing any conflict event, rather than changes in the relative number of events. Though the LPM generates estimates on the external margin, we would still expect the direction and significance of coefficients to match our primary PPML specification. In Table A.1, we present OLS estimates of Equation 3 for both a binary and continuous dependent variable,[32] showing that results for both specifications are mostly consistent with PPML estimates.

6.3.2 Military Strategy

Since the event study plots show promising evidence of parallel trends in pre-treatment periods, we believe that the observed effects are not driven by differential outcome trends across townships before the implementation of the internet restriction. However, this does not rule out the possibility of confounding effects concurrent with the treatment itself. For example, if the military government introduced other changes on March 15$^{\text{th}}$, which differentially affected townships based on internet access levels, we may be identifying treatment effects from a mix of different shocks, and not purely an internet effect. While we have not found specific mention in news articles of such policy changes occurring around the time of the internet shutdown, the military may have implemented strategic changes unannounced.

[32] The binary dependent variable, $Y_{i,t}$ is defined to equal 1 when we observe at least one instance of conflict type Y in township i during period t, and 0 otherwise. For the continuous dependent variable, we simply use the observed count of a given conflict type.

To test whether this may have occurred, we can re-run the regression in Equation 3, but with a different set of outcome variables. Instead of looking at protest events or political violence in general, we look specifically at actions taken by the military. In ACLED, multiple actors are coded for each conflict event, and for this test we are interested in all events for which state forces are listed as the first, or primary actor. Though ACLED does not formally specify which actor is the aggressor, or instigator of a conflict event, the primary actor field typically signals this role.[33] We call these events "Military Actions," since they represent all conflict events driven by state forces, which could include air strikes, artillery attacks, armed clashes, arrests, attacks on civilians, and property destruction. These are strategic and targeted actions, and if the military altered their implementation of these tactics around the beginning of the internet restriction, in such a way that was also correlated with the *access* variable, it could cause bias in our estimates of the internet shutdown effect. However, as seen in Table 4, none of the specifications of Equation 3 yield significant estimates on combined military actions, implying that military activity is not confounding the effects of the internet restriction. In addition to the aggregate number of military actions, we also look separately at the number of arrests[34] and civilian attacks perpetrated by state forces.

While estimates for civilian attacks are similarly null, we find significant negative effects on military arrests, indicating that the number of arrests decreases in high internet access areas relative to low access areas after the internet shutdown. This coefficient goes in the same direction as the effect on demonstrations, so if we interpret it as a possible *cause* of the demonstration effect, it implies that fewer arrests somehow lead to fewer demonstrations. If demonstrations are a response to military abuse and overstep, then this causal chain would make sense, but in that case we'd also expect to see negative effects on other types of military actions as well. However, since there is no differential effect on total military action or violence against civilians, the idea that people in low internet access areas have less reason to protest is not supported by the data.

Instead, we interpret the negative coefficient on arrests as an endogenous response to the decrease in protests. Simply put, arrests decrease by a relatively higher margin in high access areas after the internet shutdown because protest activity also decreases more

[33]For example, the following two conflict events are both coded as "Armed Clashes" between "State Forces" and "Political Militias" in ACLED, but the "Actor 1" field is coded differently in each. The first event, in which "Actor 1" is specified to be "State Forces," is described as follows: "On 27 December 2022, in Tei Pin village of Shwebo township (Shwebo district, Sagaing region), about 50 Myanmar military troops from Kyauk Myaung raided a security gate of a local People's Defense Force (PDF). Six members of local PDF died in the raid." The second event, in which "Political Militias" are designated as "Actor 1," is described in the following way: "On 26 December 2022, in Shwe Pan Kone village of Wetlet township (Shwebo district, Sagaing region), joint local defense forces in Wetlet township carried out parcel mine against the Myanmar military troops from Shwe Pan Kone Police Station who went to a market at around 8 am. The Myanmar military troops randomly fired back. Casualties unknown."

[34]This is not necessarily the total number of people arrested, but the number of distinct events or occasions in which at least one individual was arrested.

in these areas, and fewer protests means that there are fewer reasons to arrest civilians. Since the military's decisions are always made in response to events on the ground, all types of military action will be at least partially endogenous to demonstration activity and other forms of political conflict, and thus we interpret the results in Table 4 with some caution. Since the dependent variables presented therein may themselves be affected by the internet restriction, these regressions are still an imperfect method for identifying possible confounding strategy changes concurrent with the timing of the restriction.

Table 4: Changes in Military Activity

	(1)	(2)	(3)	(4)	(5)
Total Military Actions	-0.030	0.008	0.027	0.010	0.004
	(0.028)	(0.020)	(0.032)	(0.037)	(0.037)
Mean	0.472	0.472	0.472	0.472	0.472
Military Arrests	-0.060*	-0.055	-0.061	-0.096*	-0.140**
	(0.036)	(0.035)	(0.054)	(0.055)	(0.060)
Mean	0.157	0.157	0.157	0.157	0.157
Military Attacks on Civilians	-0.008	0.086	0.092	0.078	0.049
	(0.045)	(0.062)	(0.057)	(0.062)	(0.070)
Mean	0.087	0.087	0.087	0.087	0.087
Observations	3300	3300	3300	3300	3300
Free Space Controls	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Hist. Conflict & FB Controls	No	No	No	Yes	Yes
State/Region by Time FEs	No	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.10. Reported coefficients are estimates of β from Equation 3, with dependent variable Y listed in the first column. All coefficients are estimated using PPML, and robust standard errors (clustered at the township level) are displayed in parentheses. Data is structured as a township-(bi-week) panel, running from January 4th to May 23rd, 2021 (from 10 weeks before the internet outage on March 15th to 10 weeks after), so the regression identifies effects from average biweekly outcomes during the 10-week post-treatment period relative to the 10-week pre-treatment period. "Total Military Actions" are defined as any conflict event in which "State Forces" are designated in the "Actor 1" field in ACLED, while "Military Arrests" and "Military Attacks on Civilians" are subsets of these events. Arrests constitute any detainment of an individual or group, and attacks constitute any violent action taken against civilians. See Table 3 for a description of the regression specifications used in columns (1)–(5).

6.3.3 Measurement Error in Dependent Variable

Another potential threat to our identifying assumptions comes from the way in which conflict outcomes are measured. Though ACLED provides one of the most comprehensive databases of modern conflict events, it will invariably contain some omissions. Since their information is sourced from news reports, the quality of data for a given place and time can only be as good as the quality of news coverage. Even a small degree of measurement error caused by under/mis-reporting can become a concern for identification if this error is

Table 5: Simple DiD Results by Reporting Scale

	International/Regional/National Sources					Subnational/Local Sources				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Total Conflict	-0.014	-0.027	-0.023	-0.022	-0.031	-0.072***	-0.114***	-0.137***	-0.061*	-0.017
	(0.012)	(0.018)	(0.021)	(0.024)	(0.022)	(0.020)	(0.035)	(0.043)	(0.037)	(0.023)
Mean	1.593	1.593	1.593	1.593	1.593	0.503	0.503	0.503	0.503	0.503
Demonstrations	-0.008	-0.060***	-0.078***	-0.077***	-0.088***	-0.073	-0.218***	-0.235**	-0.173*	-0.123***
	(0.017)	(0.018)	(0.021)	(0.023)	(0.022)	(0.045)	(0.079)	(0.093)	(0.097)	(0.045)
Mean	1.053	1.053	1.053	1.053	1.053	0.302	0.302	0.302	0.302	0.302
Political Violence	0.026*	0.043	0.080**	0.046	0.044	0.005	0.030	0.071	0.078*	0.043
	(0.016)	(0.029)	(0.038)	(0.042)	(0.036)	(0.031)	(0.043)	(0.046)	(0.048)	(0.048)
Mean	0.368	0.368	0.368	0.368	0.368	0.153	0.153	0.153	0.153	0.153
Observations	3300	3300	3300	3300	3300	3300	3300	3300	3300	3300
Free Space Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Hist. Conflict & FB Controls	No	No	No	Yes	Yes	No	No	No	Yes	Yes
State/Region by Time FEs	No	No	No	No	Yes	No	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.10. Reported coefficients are PPML estimates of β from Equation 3. All conflict outcomes are calculated as counts of either the subset of events reported by international/national/regional sources (columns 1–5) or the subset of events reported by subnational/local sources (columns 6–10). For more information on the regression specifications presented herein, see notes to Table 3.

correlated with the treatment variable, and it seems plausible that this could be the case in our context, as internet access may affect the media's ability to cover local politics. If measurement error in our dependent variable is correlated with internet access, specifically if changes in the error of measuring $Y_{i,t}$ between $t = -1$ and $t = 0$ are correlated with $acces_i$, then our estimated treatment effects may be explained partially or totally by this error. In other words, the negative treatment effect on demonstration frequency as reported in Table 3 may simply be a reflection of the fact that the availability of news reports decreases by a greater margin in high access relative to low access townships. Though it's not possible to estimate the effect of measurement error if the form of the error is unknown, we provide suggestive evidence that measurement error is not a source of significant bias in our model.

In the process of compiling their conflict database, ACLED researchers draw from a range of media sources operating at the local, subnational, national, regional, and international level. For each documented event, the database includes a variable indicating the geographic scale of the media source from which the report was gathered, meaning that we can separately run any regression on the subset of conflict events sourced from a given geographic scale. If internet access has a significant effect on reporting quality, it seems that the magnitude of this effect would vary by geographic scale, since international outlets are more dependent on remote communication to monitor events. Local outlets, on the other hand, are more likely to have a physical presence on the ground, able to report on protest activity and violent conflict directly.

In Table 5 we re-report the estimates of β from Equation 3 on distinct samples separated by reporting scale. In columns (1)–(5), reported coefficients are estimated from a regression in which $Y_{i,t}$ only includes conflict events reported by international, regional, or national sources, while coefficients in columns (6)–(10) are estimated from a regression in which the dependent variable only includes events reported by subnational or local sources. If measurement error was indeed a significant source of bias on these effects, we would expect differences by reporting scale, but the sign and magnitude of coefficients is similar across columns. Our assumptions about reporting differences imply that measurement bias would be larger for international sources, as these outlets are more reliant on internet access, though if anything we observe slightly larger negative coefficients from locally reported conflict. We take these results as evidence that imperfect measurement of conflict is not a significant source of bias in our estimates.

6.3.4 Pre-Trend Analysis

While we observe evidence of nearly perfect parallel pre-trends in event study regressions for both Facebook posts and demonstrations, recall that pre-treatment values of β_k show evidence of differential trends in violent conflict. The presence of pre-trends, even weak or noisy as they are here, poses a risk to identification if one assumes a natural extension of these differential trends into the post-treatment period. In such cases, estimated treatment effects are biased by pre-existing differences unrelated to the impact of the treatment itself. Given the visual evidence of pre-trends in violent conflict (see Figure 5), one may wonder about the validity of our treatment effect estimates.

To address this concern, we implement the sensitivity analysis procedure described in Rambachan and Roth (2023). Essentially, their method examines how treatment effects would change after accounting for reasonable extrapolations of the observed pre-trends into the post-treatment period. One reasonable extrapolation, for instance, assumes that the slope of the pre-trend can change by no more than some parameter M across consecutive periods. In Figure A.1, we plot the observed treatment effect on political violence (averaged across the first six post-treatment periods) along with simulated effects over increasing values of the parameter M. We see that there is no M such that our estimated effect on violence would be significant, as the simulations only attenuate the point estimate further towards zero and increase the size of confidence intervals as M increases. If anything, the observed effect on political violence may be overstating its magnitude, and thus we remain confident in stating that internet access does not have a significant effect on violent conflict in the short run.

6.4 Targeted Internet Shutdowns

In addition to the nationwide internet shutdown imposed during the early months of the conflict, the military has also engaged in more localized restrictions that target specific townships.[35] Since the middle of 2021, 461 of these targeted restrictions have been reported across 202 (out of 330) townships, each one affecting the local community for nearly 200 days on average.[36] In Figure 6, we map the fraction of such days experienced by all townships during each year from 2021 to 2024, making two trends immediately apparent.[37] First, we see that both the length and scope of localized internet restrictions has steadily increased since the start of the coup, as both the number of townships experiencing any interruption and the number of interrupted days the average township experiences has increased each year through 2024. Second, we note that the military has concentrated their efforts in the Northwest of the country, as these townships have experienced the majority of total outage days over these four years. This area coincides with Chin state and Sagaing region, which together have experienced over half of the total number of township-days[38] under communications blackout nationwide. Though smaller than either of these, Kayah state in eastern-central Myanmar has also been targeted to such an extent that over 75 percent of all township-days from 2021 to 2024 were under communications blackout, making it, by this measure, the most affected state or region in the country.

Since these blackouts were locally targeted, the military likely had strategic goals for implementation. Based on interviews with people in affected areas, communications blackouts often preceded or were preceded by military ground attacks and airstrikes. It seems that the military cut internet and phone networks to disrupt the opposition's ability to communicate with one another and call for outside support during periods of conflict, thereby weakening local resistance (Athan 2022). As such, we would expect areas that experienced heavy internet restrictions to have also been subjected to high levels of conflict. Unsurprisingly, Kayah, Chin, and Sagaing experienced the highest level of conflict incidents per capita out of all states/regions from 2021 to 2024. In Figure A.2, which maps the total number of conflict incidents recorded in each township across the same four years, we see that the townships that experienced the most conflict tend to coincide with areas that were targeted most heavily by the military's communications blackouts.

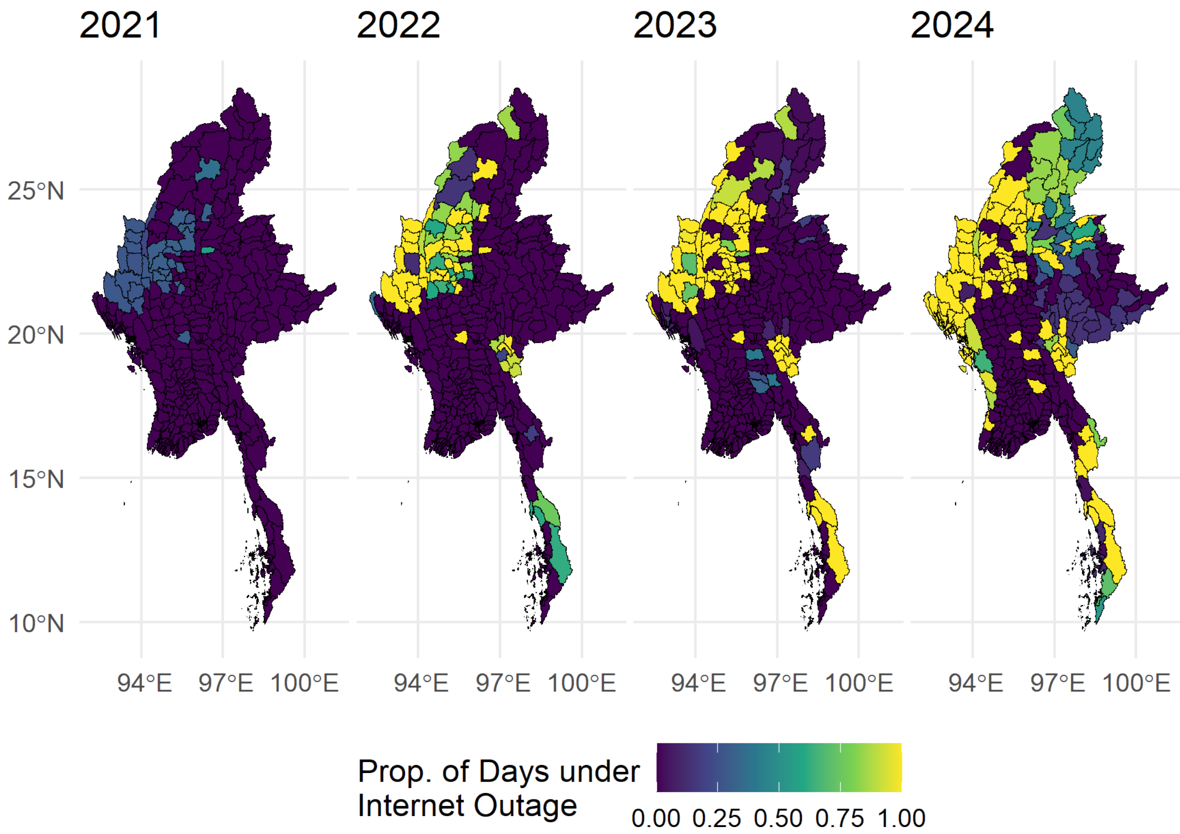
[35] In this section, while we refer to these events broadly as internet restrictions or communications blackouts, there is some variation in the type of communication technologies that were targeted. In some cases, the military was able to shut down mobile internet, traditional cellular service, and broadband connections at the same time, while in other cases only one or two of these services were targeted. Over 99 percent of all reported outage days affected mobile internet, and around 60 percent of these days also impacted traditional cell service. Only a small percentage (~ 7) of reported outage days involved broadband connections.

[36] For those living in an affected township during a communications blackout, limited options for internet access remained available to some (e.g., an expensive StarLink, or satellite connection), but for most people the restriction on connectivity was near total (Myanmar Internet Project 2024b).

[37] All data on reported communications outages is made publicly available by Athan, a non-profit organization that monitors freedom of expression and civil liberties in Myanmar (see `https://athanmyanmar.org/myanmar-communication-blackout-2021-to-present/`).

[38] A township-day is one day experienced by a single township. If a region comprises 10 townships, each of which experienced one day of communications blackout, the region would have experienced a blackout of 10 township-days, regardless of whether the blackouts occurred simultaneously or consecutively.

Figure 6: Localized Internet Restrictions



Notes: This map displays the proportion of all days in a given year during which government-imposed internet outages were reported in a given township. Internet outage location and duration estimates are based on data collected by Athan, a Myanmar non-profit organization: `https://athanmyanmar.org/myanmar-communication-blackout-2021-to-present/`.

We can more rigorously analyze the effect of these targeted blackouts through an event study framework, similar to what we used to estimate the effects of the nationwide restriction. However, since in this case the introduction of the blackouts (the "treatment") is staggered, we use a slightly different approach. Because the treatment is not introduced to all townships at once, we can construct control groups for each "cohort"[39] in our sample, composed of townships that have either not yet experienced a communications blackout or that never will. Under certain assumptions (parallel trends and no-anticipation[40]), treatment effects can be estimated by comparing the evolution of the outcome variable between treated and control groups, and aggregating these estimates across cohorts to calculate average effects. In Appendix C, we follow Chaisemartin and D'Haultfœuille (2024) to calculate these effects, but as we explain below, this method does not produce causal estimates.

6.4.1 Identifying Causal Effects

Though the analysis in Appendix C illustrates how townships are impacted in the weeks following an internet outage, the effects we identify can not be interpreted as internet shutdown effects due to obvious selection, i.e., there is strong evidence that the military imposes communications blackouts in townships with strategic importance, so treated and untreated townships are not comparable.

In this section, we attempt to recover causal effects from the rolling blackouts by using a stacked DiD approach.[41] This method reformulates our staggered treatment adoption design into a series of sub-experiments, which we then concatenate vertically to estimate an average treatment effect (see Wing et al. 2024). In other words, instead of comparing treated townships with already-treated and never-treated townships, we make comparisons across townships that are treated at the same time (i.e. within the same *cohort*). Since we are leveraging only within cohort comparisons, we lose the advantage of a pure control group, but can still rely on variation in internet access, as before. Essentially, this is equivalent to separately estimating Equation 4 for each cohort, and taking a weighted average of these estimates. Using our preferred notation, the stacked formulation can be

[39]In this setting, a cohort is defined as the set of all townships that were first exposed to the treatment (i.e., first targeted by a communications blackout) during the same time period.

[40]See Chaisemartin and D'Haultfœuille (2024) for a formal definition of these conditions.

[41]Unlike Chaisemartin and D'Haultfœuille (2024), this method does not allow for non-absorbing treatment, meaning that once a township is first targeted with an internet blackout, it is assumed to remain under blackout for the duration of our data period. This may be a problematic assumption for townships that experience more than one blackout, especially when a brief shutdown is followed later by a sustained one, as the stacked DiD approach would fail to identify any effect from the latter, more significant shutdown. To minimize this issue, we remove all prior shutdown instances for townships which experience a longer shutdown at a later date. In effect, this guarantees that the first recorded internet blackout for each township in our data is the longest such instance experienced by that township across all periods.

expressed as

$$
\begin{aligned}
Y_{a,i,e} = \alpha_i + \delta_{a,e} + \eta_{sr,a,e} &+ \left(access_i \times \sum_{\substack{k=-m \\ k\neq -1}}^{n} \beta_k I\left(e = k\right) \right) + \left(\chi_i \times \sum_{\substack{k=-m \\ k\neq -1}}^{n} \gamma_k I\left(e = k\right) \right) \\
&+ \left(access_i \times \beta_{n^+} I\left(e > n\right)\right) + \left(access_i \times \beta_{m^-} I\left(e < -m\right)\right) + \left(\chi_i \times \gamma_{n^+} I\left(e > n\right)\right) \\
&+ \left(\chi_i \times \gamma_{m^-} I\left(e < -m\right)\right) + \epsilon_{a,i,e},
\end{aligned}
\tag{5}
$$

where a now indexes the sub-experiment and e indexes event time. Each sub-experiment a represents a unique set of townships (cohort) that is first treated during the same (bi-week) calendar period,[42] and each event time period e represents a bi-week period relative to a township's specific treatment period (treatment occurs at $e = 0$). We retain township fixed effects α_i, but replace the previous calendar time fixed effects with $\delta_{a,e}$, which represent sub-experiment by event time fixed effects. This latter set of fixed effects restricts comparisons to be made within cohorts, precluding comparisons between early and late treated townships, which may be undesirable if the implementation of internet shutdowns changed over time. As before, we want to additionally restrict comparisons to be made within state/region, so we add a set of state/region by sub-experiment by event time fixed effects ($\eta_{sr,a,e}$).

Since our data covers a period spanning 4 years of calendar time (2021 through 2024), but we only want to estimate treatment effects for a small number of event time periods before and after treatment ($n+1$ post-treatment periods and $m-1$ pre-treatment periods), we estimate separate effects for any periods outside of the $[-m, n]$ treatment window. The aggregate effect for all e more than n periods after the treatment period is represented by β_{n^+} (and γ_{n^+} for time-invariant control trends), while the aggregate effect for all e more than m periods before the treatment period is represented by β_{m^-} and (and γ_{m^-} for time-invariant control trends).[43]

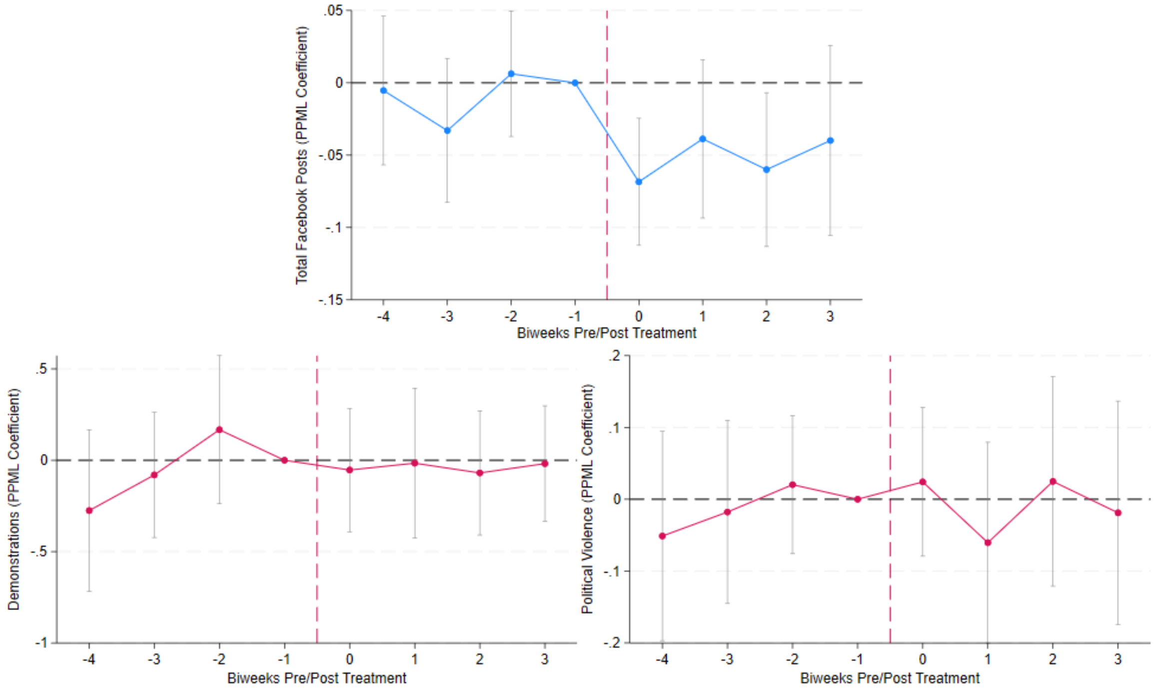
In Figure 7, we plot estimates of β_k from Equation 5 for Facebook posts, demonstrations, and political violence. We first note that there is still a significant decline in Facebook activity following a shutdown (as we also show in Figure C.1 when comparing targeted townships directly to untargeted townships). In percentage terms, this effect is even larger than what we observed following the nationwide shutdown (greater than 5 percent compared to around 3 percent, as seen in Figure 4), and extends across at least four post-treatment periods. However, unlike what we observe in Figure C.1, we do not see an immediate spike in violence among treated townships. If anything, we document a slight

[42]In total, our data contains 37 township cohorts, beginning with townships that first experienced a targeted blackout during the second week of June 2021, and ending with townships that were first targeted in December 2024.

[43]This method implicitly assumes that effects are constant outside of the treatment window.

decrease in violence that occurs one period after the first post-treatment period, though this is not significant and not sustained. The fact that there is no significant effect on violent conflict in this model is reassuring, as it suggests that our identification is not affected by the same selection issues as in the direct comparison of treated and untreated (or not-yet treated) townships (Figure C.1). In other words, military response no longer seems to be positively correlated with exposure to the internet shutdown effect.

Figure 7: Event Study Results – Targeted Internet Shutdowns



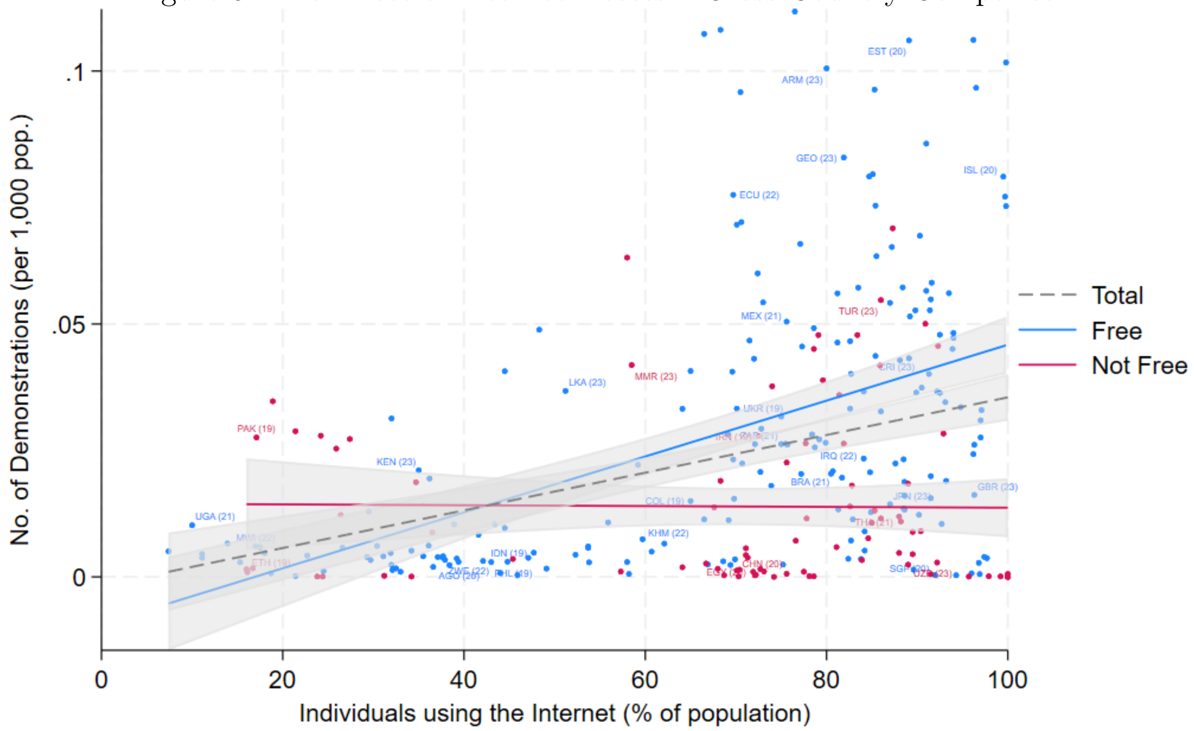
Notes: Points represent PPML estimates of β_k from Equation 5 for 4 pre-treatment periods and 4 post-treatment periods, with outcome variable Y defined as either the number of Facebook posts, number of demonstrations, or number of incidents of political violence (including 90% confidence bands based on robust standard errors clustered at the township level). All specifications include the following township-level controls as part of χ_i: average "free space" signal strength, average elevation, average slope, total number of cell towers, average nighttime lights intensity (2020), total population (2020), adult literacy rate (2014), percent urban population (2014), percent of households with safe sanitation (2014). Each k represents a period in event time, which are measured in relation to the start of each township's treatment date, and thus do not correspond with a specific calendar date. Our data runs from January 1st, 2021 through December 31st, 2024, and each period spans two weeks. (For Facebook outcomes, data only runs through the end of 2022.) The first internet shutdown occurs on June 10th, 2021, and these shutdowns continue on a rolling basis through the end of 2024.

Finally, we note that the targeted shutdowns have no significant effect on the number of demonstrations, an unexpected result considering the strong negative effect observed after the earlier nationwide shutdown. Though the effect on Facebook activity is just as strong (if not stronger) for these later shutdowns, the subsequent effect on demonstrations has disappeared.

6.5 Technology of Liberation or Control?

In the preceding sections, we have looked at multiple internet shutdown events, estimating their impact on subsequent conflict trends. In the first analysis, we show that a nationwide

Figure 8: The Effect of Internet Access – Cross-Country Comparison



Notes: For an unbalanced panel of 72 countries between 2019 and 2024, this figure plots annual number of demonstrations (per 1,000 people) as a function of the percent of total population using the internet (data on demonstrations is from ACLED, while data on internet usage comes from the World Bank). Blue dots indicate countries that were categorized as "Free" or "Partly Free" in Freedom House's annual "Freedom on the Net" report, while red dots indicate countries categorized as "Not Free." Each line plots predicted values from a linear regression of y on x (with 95% CI). Colored lines represent predicted values from a regression restricted to the corresponding subsample of country-years, while the gray dotted line is derived from a regression on the entire sample.

shutdown, implemented in early 2021, led to a significant decrease in protest activity. Not only did overall protest levels drop after the shutdown, but they dropped relatively more in townships with higher exposure to the shutdown (higher ex-ante internet access levels). Next, we analyze the impact of rolling internet shutdowns that temporarily impacted the majority of townships at some point between late 2021 and 2024. We find that the average effect of these events is not consistent with that of the nationwide shutdown, as we estimate null effects on demonstrations from this second event study.

A majority of existing studies seem to corroborate our first result, arguing that the internet enables coordination and communication among potential protesters, and thus exposure to the internet (and social media in particular) will lead to more frequent and larger protests. However, others have pointed out that this is not an inevitable outcome for societies with expanding internet access, citing features of the internet that may actually make it easier for governments to repress dissent and discourage protests. Looking across the experiences of 72 nations[44] since 2019, we see that both arguments may be consistent with descriptive evidence.

In Figure 8 we represent this evidence graphically, plotting the number of demonstra-

[44]These represent the set of countries included in at least one of the annual "Freedom on the Net" reports published by Freedom House between 2019 and 2024.

tions (per 1,000 people) that a given country experiences in a given year against the percent of the population that uses the internet. In other words, we show how the prevalence of political resistance correlates with internet access. Across the full set of countries, the evidence supports an interpretation of the internet as a technology of liberation, as higher levels of internet exposure are broadly associated with more protest activity. However, this aggregate comparison belies an important heterogeneity across countries with different standards of internet freedom. After categorizing each country-year observation as either "Free" or "Not Free" (according to Freedom House's annual "Freedom on the Net" index[45]), we show that this positive correlation only holds for countries that enjoy relative internet freedoms. For those in which online activity is more restricted, the connection between protest activity and internet access disappears entirely. For these countries, internet access does not seem to have an obvious liberation effect, and in the most extreme cases may be co-opted by the government as a means of control.

These cross-country comparisons provide useful context for the case of Myanmar, which has experienced a rapid decline in internet freedoms since the start of the coup.[46] This decline is analogous to a shift from the blue line to the red line in Figure 8, and provides a plausible explanation for the internet's evolving role in political conflict. In the following sections, we explore the political conditions in Myanmar that may have brought about this evolution, and discuss how it shifted the internet from a technology of liberation to one of control.

6.5.1 Increasing Surveillance and Arrests

Though it may seem obvious, the way that internet access interacts with conflict will be determined largely by the content to which people are exposed. If, for example, the only content available on the internet is completely apolitical, then we wouldn't expect exposure to have any impact on political behavior. It is through information and communication that the internet can effect political conflict, but only if people are sharing information that is related to political events.

In Myanmar, we know that the internet is an important platform for political content, and we have shown that even in our limited sample of Facebook data—taken from public groups that are generally not explicitly political—up to a third of daily posts were related to the political crisis in the period following the coup. However, as we noted earlier in Figure 1, the percentage of political posts changes noticeably over time. Before the start

[45]The "Freedom on the Net" index assigns each country a score from 0–100 (a lower score equates to less internet freedom) based on three general categories: 1) obstacles to access, 2) limits on content, and 3) violations of user rights. For more information on Freedom House's methodology, see the full report for 2024 (Funk et al. 2024).

[46]In 2019, Myanmar received a score of 36 on the "Freedom on the Net" index, four points shy of a "Partly Free" rating. In 2020, their score dropped to 31 before plummeting to 17 the following year in response to the coup. Their performance continued to fall over the following three years, reaching its lowest point in 2024 with a score of 9.

of the coup on February 1st, 2021, political posts account for only about 5 percent of all posts, but this increases by 5 times, to nearly 30 percent, after the start of the conflict. The concentration of political content ranges between 30 percent and 25 percent for the following three months, until May 2021, at which point it drops by two-thirds (to 10 percent), which is maintained over the course of the following year.[47]

In Figure 9, we plot these trends explicitly over the entirety of 2021, along with the overall number of demonstrations observed in ACLED. We also plot the constant average proportion of political posts for two time periods: (1) The duration of the nationwide internet shutdown, from March 15th to April 30th, 2021; (2) the period during which rolling blackouts were targeted at specific townships, from June 10th through the end of 2021.[48] The figure makes stark the difference in online political activity between these two periods. At the time of the nationwide shutdown in March, over 25 percent of Facebook posts in our sample were political in nature, but this proportion reduces by a third during the period of targeted blackouts.

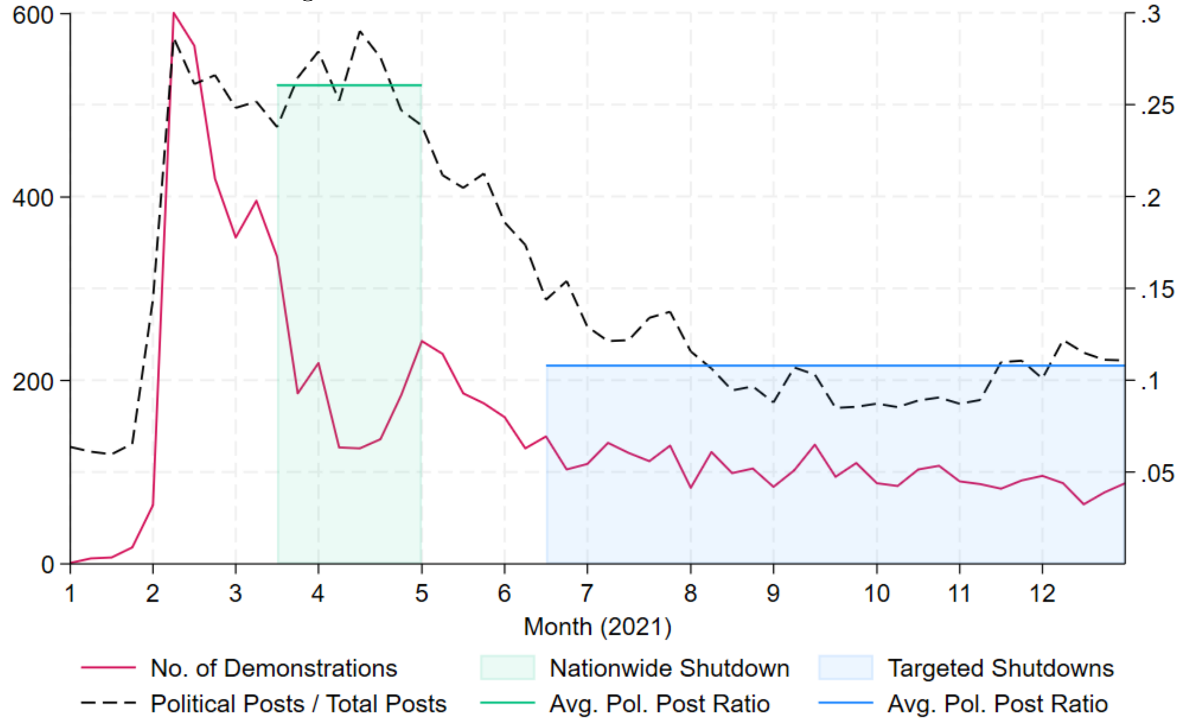
What could be the cause of this relatively sudden shift in the composition of Facebook content? One potential explanation is that the country experienced a general decline in political activism as the military cracked down on demonstrations and other forms of resistance. In other words, as political engagement decreases among the population, we observe a natural decrease in the likelihood that people write about the conflict online. While there is almost certainly some truth to this—indeed we observe a general correlation between the frequency of demonstrations and the proportion of political Facebook content—it cannot explain the observed trend entirely, since political content maintains a high ratio for several months even as demonstration frequency declines between February and May.

Another explanation for the shift in online behavior could be related to changes in the way that the military responded to online activism. According to sources close to the military, its leaders were surprised by the overwhelming civilian response to their unexpected and undemocratic ouster of the elected government. They reportedly believed that people would quickly abandon organized resistance if they arrested key dissidents and made threats of retaliation. After all, these tactics were successful in swiftly suppressing previous resistance movements in 1988, 2007, and 2008 (Lederer 2021). However, the internet was not widely available during any of these past conflicts, and the military seems to have learned in real time to adapt to a new form of digital resistance. Critically, they began targeting online activism directly by arresting individuals purely for content created

[47]Crucially, the ratio of political posts did not fall after the nationwide internet shutdown on March 15th, consistent with all reporting that this event was an indiscriminate restriction on all internet access, not a selective censorship. It also supports an implicit assumption that the shutdown was not accompanied by other government actions that could have induced a change in political activity, which is important for the validity of treatment effect estimates.

[48]While these targeted blackouts continued through 2024, here we only plot trends for the first year. The proportion of political posts remained relatively constant across 2022, the final year for which we have Facebook data.

Figure 9: Trends in Political Facebook Content



The solid red line plots the number of weekly demonstration events occurring throughout Myanmar during 2021, while the dashed black line plots the ratio of political posts to all posts observed in our sample of Facebook data over the same time period. The height of the green area shows the average proportion of political posts across the duration of the nationwide internet shutdown (March 15th to April 30th), while the height of the blue area shows this average between mid-June 2021 (when the military started employing targeted internet shutdowns) and the end of the year.

or shared on social media.

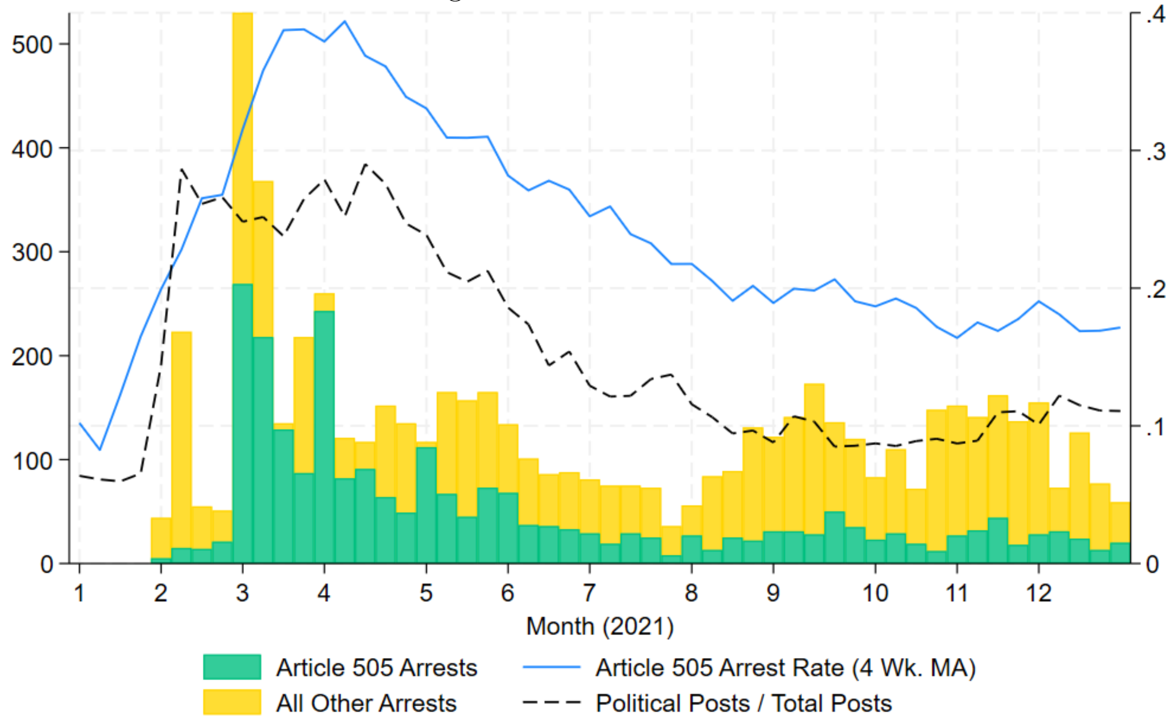
Among various other changes that eroded basic rights and legal protections for Myanmar citizens, the military made two amendments to the penal code on February 14th—two weeks after the 2021 coup—that gave authorities a legal justification for prosecuting online activism. Together, the confusingly designated Articles 505(a) and 505A allow for the arrest of anyone that "causes or intends to cause fear," "causes or intends to spread false news," or "causes or intends to commit or to agitate directly or indirectly criminal offence [sic] against a government employee." The definition of such criminal offenses was also expanded to include vague descriptions like "affecting," "disturbing," or "damaging motivation" of any government employee (Free Expression Myanmar 2022).

Under the jurisdiction of these new legal provisions, the military began monitoring online platforms and conducting physical searches at security checkpoints to arrest alleged perpetrators (The Irrawaddy 2024). Using arrest records provided by the Assistance Association for Political Prisoners (AAPP), a human rights organization based in Myanmar, we can track these arrests from the start of the coup. Though our data does not specify the exact circumstances or charges related to each arrest, it often provides a description of the specific law under which the arrest was made, allowing us to separately identify arrests attributed to Articles 505(a) and 505A.[49] Though not all detainees charged under

[49]After removing from the data all arrest records that are missing either date or location information,

these articles were arrested for online activity, and these articles were not cited in all such arrests, we interpret the mention of either article as a good indicator that an arrest was related to online activism.

Figure 10: Arrest Trends



The green bars show the number of weekly arrests made under Article 505(a) or 505A from the amended penal code, while the yellow bars show the number of all other arrests (not attributed to these specific articles) made during 2021. The dashed black line plots the ratio of political posts to all posts observed in our sample of Facebook data over the same time period, while the solid blue line shows the proportion of total arrests that were attributed to Articles 505(a) and 505A (plotted as a 4 week moving average).

In Figure 10, we plot weekly arrest numbers separately for online arrests and all other arrests in the data, observing clearly that the military used Article 505 sparingly in February (when the total number of arrests was already high), but dramatically ramped up its prosecution of online resistance actors in March. The number of arrests made under Article 505 remained relatively high in April and May but then started to reduce gradually such that, by the middle of 2021, these arrests once again represented only a small proportion of total arrests. We also plot a moving average trend line that represents this proportion directly, noting that the ratio starts low, peaks around April, then gradually decreases through the remainder of the year—a pattern that closely follows the trend in political Facebook posts. Political Facebook activity grows rapidly during the first month of the coup—when Article 505 arrests are relatively uncommon—but as the military leans more heavily on the amended penal code in March, the proportion of arrests targeted at online activists grows dramatically, followed by a subsequent decline in political posts.

---

we are left with 8,759 arrests observed during 2021. To identify arrests made under Articles 505(a) or 505A, we search for any mention of "505" in the text variable that describes the legal justification for each arrest, resulting in 2,450 entries, over 25 percent of total arrests.

As online political content becomes scarcer over the next few months, the military's prosecution of internet offenses declines at a similar rate.

6.5.2 Pro-Military Content

The evidence suggests that the military's expanding capability—and willingness—to arrest its citizens for online speech resulted in a dramatic shift in the way that the resistance movement used the internet. Fear of prosecution or retaliation has had a "chilling effect" on peoples' inclination to express themselves publicly, creating an environment of fear that led to Myanmar being named by Freedom House as the world's second worst violator of internet freedoms in their 2023 report (RFA Burmese 2023).

In addition to this expanding surveillance capacity, there is further evidence of the military's attempts to manipulate the narrative across Myanmar's public online spaces. Though official military accounts were banned from Meta-owned services (i.e., Facebook and Instagram) shortly after the coup (BBC 2021), the company could not police the thousands of unofficial pro-military accounts created by soldiers or other paid affiliates. Such accounts were reportedly engaged in a kind of coordinated "information combat" that sought to not only spread the military's views, but to attack and discredit resistance actors (Potkin and Lone 2021).

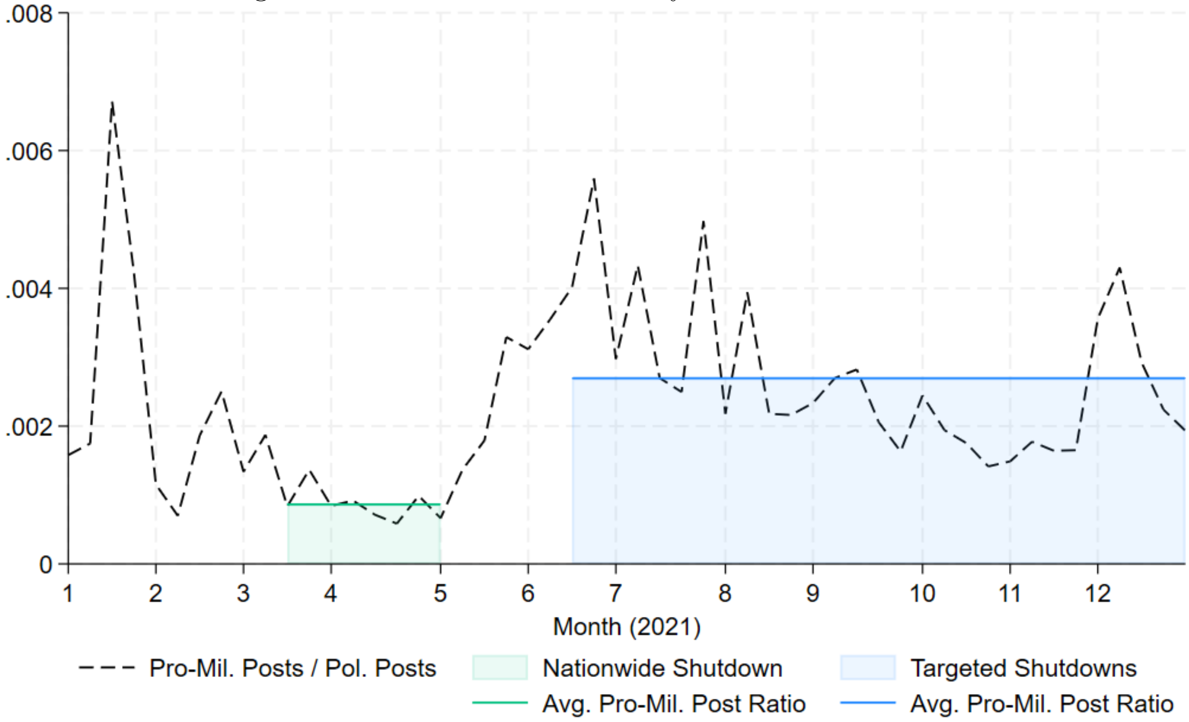
Compared to the overwhelming initial response of anti-coup sentiment that spread across the internet, pro-military posts were admittedly scarce. In an analysis of a random sample of Facebook posts captured between March and May of 2021, Ryan and Tran (2024) find that only 2.7 percent of coup-related posts—and 1 percent of all posts—can be interpreted as pro-military. However, they also find that the prevalence of pro-resistance content, and the average level of engagement with this content, decreased over time, while the prevalence of—and engagement with—pro-military content increased. Using our own sample of Facebook data, we find a similar trend.

In Figure 11, we plot these trends in pro-military content—defined as any post containing at least one of a predefined set of keywords—for all of 2021.[50] The percentage of political posts classified as pro-military ranges from less than .1 percent to around .7 percent, lower than the 2.7 percent identified by Ryan and Tran (2024) (likely because our keyword search does not capture the full scope of pro-military language[51]), but we note, as they do, that pro-military content appears at a particularly low rate in the early months of the coup—especially during the period of the nationwide internet outage—before rising

[50]To identify posts, we search for 9 keywords in total, 8 of which are derogatory terms for supporters of Aung San Suu Kyi or her political party (the NLD), and the last of which refers to student-led terrorist activity.

[51]It is not feasible to use natural language processing—as we do with political posts in general—to identify specifically pro-military posts due to the low rate at which they are found in the data. The frequency of positive cases is so low that it becomes impossible to generate a random sample small enough to allow for manual classification, yet large enough to guarantee a sufficient number of pro-military posts for model training.

Figure 11: Trends in Pro-Military Facebook Content



The dashed line plots the weekly proportion of "pro-military" posts among all political posts in our sample of Facebook data, for all of 2021. The height of the green area shows the average value of this proportion across the duration of the nationwide internet shutdown (March 15th to April 30th), while the height of the blue area shows the same average between mid-June 2021 (when the military started employing targeted internet shutdowns) and the end of the year. "Pro-military" posts are identified by searching for the presence of one or more keywords that are associated with military propaganda and/or the writings of military sympathizers.

dramatically in the second half of 2021.[52] If we again compare prevailing conditions during the time of the nationwide internet outage with the later period of targeted shutdowns, we find that the average rate of pro-military content was more than twice as high during the latter period.

As we've shown, the empirical evidence suggests a dramatically different political environment at play between the two periods of interest in our analysis. In the weeks immediately following the February coup, the military faced an unprecedented level of social resistance, fueled in part by widespread internet access and social media use that had not yet been a factor in previous civil conflicts. The military was surprised by and completely unprepared for the scope and energy of the blowback, which led to a short period of relatively free online expression, during which arrests for social media activity were relatively uncommon and the volume of political messaging encountered by internet users relatively high. This was the prevailing environment in Myanmar at the time of the nationwide internet shutdown running from March 15th to April 30th, 2021. However, the military quickly adapted to the new digital politics and instituted several strategies to more directly target online dissent. First, after enacting revised laws to expand their power

[52] Pro-military posts are also found in high proportion during January of 2021, before the outbreak of the coup, but this is mostly explained by a low number of political posts in general (denominator) rather than an exceptionally high number of pro-military posts (numerator).

of detention, the military began aggressively arresting people for their online activity during March and April, which preceded a dramatic decline in the relative frequency of political content on social media. Next, after their official accounts were banned from Myanmar's most popular social media platforms, the military ramped up their online presence through networks of coordinated but unofficial content creators, a strategy of "information combat" that led to a measurable increase in pro-military messaging across a range of Facebook groups and pages.

6.5.3 Heterogeneous Effects

Together, these trends can explain the difference in treatment effects estimated across different time periods. During the initial nationwide internet shutdown, social media provided a widely accessible forum for expression and coordination, and thus the shutdown, by reducing access to this open platform, caused a reduction in resistance activity. However, during the later period of rolling shutdowns, after internet freedoms had been eroded by the military in several ways, social media no longer served the same political function. People were deterred from participating in online resistance for fear of arrest, and the relative frequency of pro-military content had more than doubled since the start of the coup. In this context, the internet had stopped serving the same coordinating function, so the targeted shutdowns no longer caused reductions in protest activity. The case of Myanmar illustrates how, in the course of less than a year, the internet can shift from a technology of liberation to one of oppression, depending critically on government capacity and the prevailing internet freedoms of the time.

We end this discussion with one additional empirical test. If the internet's effect on conflict is conditional on free political expression, as we have argued, then we should find heterogeneous effects across townships with different rates of online political content. Though internet freedoms do not necessarily vary by township, the average level of online activism among people in that township will vary based on local factors, and these differences can be measured by the proportion of political posts observed across the Facebook groups associated with each township. For townships in which we observe a relatively high baseline proportion of political posts, we expect internet access to be more closely connected to conflict outcomes, as the internet plays a more relevant role in political discourse. For townships producing relatively fewer political posts, on the other hand, we expect the internet to have a smaller effect on conflict. Though people may be equally or even more politically active in these townships, they rely less on the internet for their political expression.

To test whether the internet's effect is greater in townships with higher levels of online activism, we return to the March 15$^{\text{th}}$ internet shutdown, estimating Equation 3 separately for townships that are above/below the median proportion of political Facebook posts.[53] In the first panel of Table 6, we show that the magnitude of the negative effect on demonstrations is twice as large for townships above the median compared to those below, and the below median effect is no longer statistically significant. In the second panel, we split the sample along a different dimension, this time by the proportion of political posts that we've identified as pro-military.[54] Here we observe the opposite pattern; townships with below median levels of pro-military posts experience a slightly stronger internet shutdown effect than those above the median.

These results are consistent with our previous findings, as they suggest that the effect of the nationwide shutdown was conditional on the relative prominence of social media in politics. After the March 15$^{\text{th}}$ event, townships in which politics were more openly discussed *online* experienced a greater decline in *offline* protest activity. Similarly, townships in which political content was less likely to be pro-military in nature also experienced a greater decline in demonstrations. These cross-sectional heterogeneities mirror the differences observed over the course of the conflict: as online content became less political and more pro-military over time, its effect on political resistance faded.

6.6 Long Term Results

Shutdowns provide an opportunity to estimate well-identified effects within our empirical framework. However, due to the transient nature of these events, they cannot tell us anything about longer term outcomes. In this section, we condense our panel data into repeated annual cross-sections to estimate effects over several years. To the extent that the access variable, as defined earlier, is conditionally exogenous, we interpret these results as causal, but with admittedly strong assumptions.[55] Consider the following simple regression:

$$Y_i = \eta_{sr} + \beta \cdot access_i + \gamma \cdot \chi_i + \epsilon_i, \tag{6}$$

in which Y_i represents some conflict outcome in township i, $access_i$ represents the estimated number of people in township i with mobile internet access (measured in 10,000s of people), η_{sr} are state/region-level fixed effects, and χ_i represents the same vector of control variables used in the DiD regressions. In the second panel of Table 7, we present estimates of β from Equation 1 for the total incidence of political violence, fatalities, and two of the

[53]Specifically, this is calculated from the proportion of all posts made by each township between the beginning of 2021 and March 15$^{\text{th}}$ (prior to the national shutdown) that are classified as political.

[54]This is calculated from the proportion of political posts made by each township between the beginning of 2021 and March 15$^{\text{th}}$ (prior to the national shutdown) that contain at least one of the pro-military keywords previously mentioned.

[55]Within the structure of the DiD, $access_{i,t}$ is only required to be exogenous in trends, but causal identification in the cross-section requires exogeneity in levels, i.e., $access_i$ must be uncorrelated with ϵ_i in Equation 6. This is a stronger assumption, but we have previously argued why $access_i$ is considered conditionally exogenous (Section 3.3), and point out that much previous use of the ITM in economics only uses cross-sectional data (Bursztyn and Cantoni 2016; Olken 2009; Yanagizawa-Drott 2014).

Table 6: Heterogeneous Results by Online Political Activity

	(1)	(2)	(3)	(4)	(5)
Demonstrations (by % Political Posts)					
Below Median	-0.005	-0.039	-0.069*	-0.058	-0.061
	(0.027)	(0.038)	(0.041)	(0.042)	(0.042)
Mean	1.333	1.333	1.333	1.333	1.333
Above Median	-0.018	-0.118***	-0.132***	-0.117***	-0.121***
	(0.014)	(0.022)	(0.024)	(0.027)	(0.023)
Mean	1.377	1.377	1.377	1.377	1.377
Demonstrations (by % Pro-Military Posts)					
Below Median	-0.011	-0.059**	-0.073***	-0.062*	-0.069*
	(0.013)	(0.027)	(0.027)	(0.035)	(0.036)
Mean	1.123	1.123	1.123	1.123	1.123
Above Median	0.024	-0.032	-0.047	-0.042	-0.060
	(0.028)	(0.041)	(0.045)	(0.044)	(0.042)
Mean	1.819	1.819	1.819	1.819	1.819
Observations	1650	1650	1650	1650	1650
Free Space Controls	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Hist. Conflict & FB Controls	No	No	No	Yes	Yes
State/Region by Time FEs	No	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients are PPML estimates of β from Equation 3, estimated separately for townships in which the ratio of 1) political Facebook posts to total posts or 2) pro-military posts to political posts is below/above the median value. The dependent variable in all regressions is demonstration count. For each township, we calculate the total number of Facebook posts observed between the beginning of 2021 and March 15$^{\text{th}}$, as well as the number of political posts and the number of posts containing a pro-military keyword over the same time period. We divide the number of political posts by total posts to calculate the baseline proportion of political posts, and the number of pro-military posts by political posts to calculate the baseline proportion of pro-military posts for each township. The median of the former determines the sample division in the first panel, while the median of the latter determines the division in the second panel. For more information on the regression specifications presented herein, see notes to Table 3.

violent conflict event types as described in Table 1.[56] For each outcome, we calculate the total number of events per township during every year from 2021 to 2024 and regress this on $access_i$ (separately for each year), using the PPML estimator as before.

These cross-sectional estimates show evidence of an evolving relationship between conflict and internet access across the four years of data. In 2021, the first year of post-coup Myanmar, we observe negative (though statistically insignificant) coefficients on the total number of violent conflict incidents, as well as the number of fatalities, battles, and incidents of remote violence[57]. These coefficients increase across 2022 and 2023, and in 2024, nearly four years after the start of the coup, the coefficients on fatalities, battles, remote violence, and total violent conflict are all positive (with the latter two also being statistically significant). Specifically, by 2024, the number of incidents of violent political conflict recorded in a township increases by 3.8 percent for every additional 10,000 people with internet access.

Next, in the first panel of Table 7, we show results for an additional outcome variable related to military control.[58] This measures is taken from the Special Advisory Council for Myanmar (SAC-M), an independent group of security experts that produces periodic reports on the political situation in Myanmar. In reports for 2022 and 2024, they provide an assessment of effective control for each township, determined around the middle of the year. This measure is represented along an 8-category scale ranging from full resistance control (=1) to stable military control (=8), and captures the degree to which the military junta (or resistance movement) has consolidated its control over a township's territory, population, and the administrative functions of local government (Special Advisory Council for Myanmar 2022).[59] Figure 12 maps these assessments in a side-by-side comparison of 2022 and 2024, illustrating clearly that while the junta gained control of some townships in southern and central regions of the country, they ceded much more territory in peripheral townships of the northern and western regions. Overall, the military's outlook seems to have worsened since 2022.

While correlation between internet access and military control[60] is weak and not statistically significant in 2022, this estimate grows in magnitude and becomes more significant over the subsequent two years. By 2024, the likelihood that a township is under

[56]The number of battles and incidents of remote violence are both subsets of the total count of political violence.

[57]Remote violence is defined as an incident "in which one side uses weapons types that, by their nature, are at range and widely destructive." This includes military airstrikes, artillery or missile attacks, landmines, and attacks involving any other kind of explosive device.

[58]Effects on military control are estimated using OLS, and interpretable as an LPM due to the binary construction of the dependent variable.

[59]In the published SAC-M report, full resistance control is assigned the highest value of 8, while stable military control is assigned the lowest value of 1. We take the additive inverse in our analysis so that higher numbers align with a higher degree of military control.

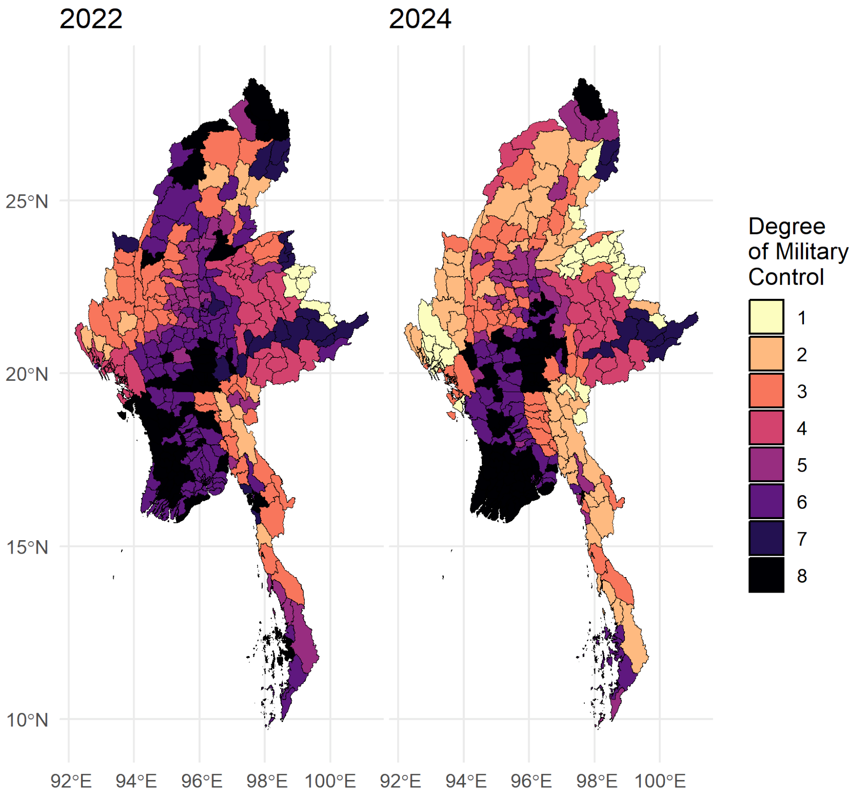
[60]In the regression, we convert SAC-M's categorical control scale to a binary indicator for military control, which is equal to 1 where the categorical variable is greater than or equal to 5, and 0 otherwise.

Table 7: Long Term Effects of Internet Access

	2021	2022	2023	2024
Military Control		0.007		0.013**
		(0.006)		(0.005)
Mean		0.712		0.558
Political Violence	-0.003	0.014	0.035*	0.038**
	(0.016)	(0.020)	(0.019)	(0.018)
Mean	25.376	41.770	45.345	42.739
Fatalities	-0.029	0.003	0.031	0.029
	(0.029)	(0.023)	(0.020)	(0.025)
Mean	35.085	70.882	64.309	59.524
Battles	-0.025	0.008	0.021	0.027
	(0.022)	(0.022)	(0.019)	(0.021)
Mean	7.100	13.394	15.755	15.276
Remote Violence	-0.005	0.017	0.060**	0.054***
	(0.016)	(0.021)	(0.024)	(0.021)
Mean	8.567	13.297	16.115	18.009
Observations	330	330	330	330
Controls	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.10. Reported coefficients are estimates of β from Equation 6, with dependent variable Y listed in the first column. Heteroskedasticity robust standard errors are displayed in parentheses. "Military Control" is a binary variable that equals 1 for townships in which the military control measure is in the upper half of the scale defined in Figure 12 (≥ 5). "Fatalities" indicates the annual number of deaths resulting from all conflict events in each township. "Battles" and "Remote Violence" are both sub-categories of "Political Violence" (see Table 1 for a description of all conflict outcomes). For "Military Control", coefficients are estimated with OLS, while PPML is used to estimate coefficients for the remaining outcomes. All results are estimated from a regression specification that includes state/region-level fixed effects as well as the following township-level control variables: average "free space" signal strength, average elevation, average slope, total number of cell towers, average nighttime lights intensity (2020), total population (2020), adult literacy rate (2014), percent urban population (2014), percent of households with safe sanitation (2014), number of historical conflict incidents (prior to 2021), and the number of Facebook posts in our dataset made between January 1st and February 1st, 2021 (prior to the coup).

Figure 12: Evolution of Military Control by Township



Notes: This map displays the degree of military control reported for each township in Myanmar during the first half of 2022 and 2024. Values run from 1 to 8 in order of increasing military control, and have the following interpretation: 1 = Full resistance control & local administration – whole township; 2 = Strong resistance control & local administration – 90%+ of township; 3 = Junta control receding, resistance defending increasing territories & asserting local administration; 4 = Limited junta movement, dependent on ceasefires; 5 = Resistance controls growing territory but still cannot consolidate fuller control; 6 = Junta forces under regular attack from resistance forces, administration functions remain weak; 7 = Junta dependent on local proxy militias for control; 8 = Stable junta control. Source: SAC-M Briefing Paper – Effective Control in Myanmar.

military control increases by 1.3 percentage points for every additional 10,000 people with internet access. Taken together, the evidence in the first and second panels of Table 7 show how the internet may evolve over the span of a conflict, shifting from a potential "liberation technology" to one that actively works against political freedoms. While one could argue for unobservable factors that jointly explain the increase in military control, violent conflict, and internet access, the fact that access conditionally predicts military control in 2024 but not 2022 is a compelling piece of evidence. Overall, the military loses ground between 2022 and 2024, as evidenced by a 15 percentage point reduction in the number of townships under their de facto control, and these territorial losses are necessarily concentrated in townships with lower internet access levels, as evidenced by the increase in our point estimate of β. In other words, the military was more likely to retain control of high access townships, suggesting that the internet had become a useful tool of control by this stage of the conflict.

7 Conclusion

Should the internet be considered a tool of liberation or control? In this paper, we argue that either interpretation may be correct, depending critically on the current state of internet freedoms. In Myanmar, reliable internet access has only become available to most people within the last ten years, and the social media platform Facebook has since become an integral part of social and political life. Facebook's rapid growth as a tool for communication and information sharing has been at the center of several recent episodes of conflict, but its actual effect on these conflicts—as either a boon to resistance movements or an instrument of government power—has been debated.

By leveraging shocks to access that were induced by a series of government mandated internet shutdowns, we estimate the aggregate effects of the internet on political conflict. We show that there is a negligible effect on violent conflict in the short-run, but that more exposure to the internet leads to significantly higher levels of protest, as social media allows resistance actors to coordinate mass activities and share information with a wide audience. However, this positive effect on protest is only sustained as long as citizens feel free to voice dissent online. As internet freedoms are eroded, political posts on social media become less common, and the relationship between internet access and resistance activity disappears. In the long run, we show suggestive evidence that higher levels of internet access may even undermine resistance efforts and expand government control.

In general, these findings have important implications for our understanding of the internet and social media in the context of economic development. As countries grow richer, internet access expands and becomes affordable to more and more people. It is tempting to assume that this expansion will always bring desirable political outcomes—such as the weakening of autocratic regimes and the growth of democratic movements—and indeed this has often been true, as in the case of the Arab Spring. However, we hope that our research makes a strong case for a more nuanced interpretation of these technologies. Internet expansion may lead to democratization when a certain level of freedoms are guaranteed, but is more likely to cause democratic backsliding when the ability of internet users to express dissent is circumscribed.

References

Adena, Maja, Ruben Enikolopov, Maria Petrova, Veronica Santarosa, and Ekaterina Zhuravskaya (2015). "Radio and the Rise of The Nazis in Prewar Germany". In: *The Quarterly Journal of Economics* 130.4, pp. 1885–1939.

Allcott, Hunt and Matthew Gentzkow (2017). "Social Media and Fake News in the 2016 Election". In: *Journal of Economic Perspectives* 31.2, pp. 211–236.

Amidi, Feranak (2022). "Hashtags, a viral song and memes empower Iran's protesters". In.

Asher, Saira (2021). "Myanmar coup: How Facebook became the 'digital tea shop'". In: *BBC News*.

Athan (2022). *Internet Access Amid Darkness and Lives Amid Threats: Analysis of Internet Blackout*.

Bailard, Catie Snow (2015). "Ethnic conflict goes mobile: Mobile technology's effect on the opportunities and motivations for violent collective action". In: *Journal of Peace Research* 52.3, pp. 323–337.

BBC (2021). "Myanmar coup: Facebook, Instagram place immediate ban on military". In.

Bradshaw, Samantha, Hannah Bailey, and Philip N. Howard (2021). *Industrialized Disinformation: 2020 Global Inventory of Organized Social Media Manipulation*. Oxford, UK: Programme on Democracy & Technology, p. 26.

Bursztyn, Leonardo and Davide Cantoni (2016). "A Tear in the Iron Curtain: The Impact of Western Television on Consumption Behavior". In: *The Review of Economics and Statistics* 98.1, pp. 25–41.

Bursztyn, Leonardo, Georgy Egorov, Ruben Enikolopov, and Maria Petrova (2019). *Social Media and Xenophobia: Evidence from Russia*. w26567. National Bureau of Economic Research.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro H. C. Sant'Anna (2024). *Difference-in-Differences with a Continuous Treatment*.

Chaisemartin, Clément de and Xavier D'Haultfœuille (2024). "Difference-in-Differences Estimators of Intertemporal Treatment Effects". In: *The Review of Economics and Statistics*, pp. 1–45.

Chang, Ho-Chun Herbert, Allissa Richardson, and Emilio Ferrara (2022). "#Justicefor-GeorgeFloyd: How Instagram facilitated the 2020 Black Lives Matter protests". In: *PLOS ONE* 17.12, e0277864.

Dafoe, Allan and Jason Lyall (2015). "From cell phones to conflict? Reflections on the emerging ICT – political conflict research agenda". In: *Journal of Peace Research* 52.3, pp. 401–413.

DellaVigna, Stefano and Matthew Gentzkow (2010). "Persuasion: Empirical Evidence". In: *Annual Review of Economics* 2.1, pp. 643–669.

Diamond, Larry and Marc F. Plattner (2012). *Liberation Technology: Social Media and the Struggle for Democracy*. Johns Hopkins University Press. 176 pp.

Dube, Oeindrila and Juan F. Vargas (2013). "Commodity Price Shocks and Civil Conflict: Evidence from Colombia". In: *The Review of Economic Studies* 80.4, pp. 1384–1421.

Enikolopov, Ruben, Alexey Makarin, and Maria Petrova (2020). "Social Media & Protest Participation: Evidence From Russia". In: *Econometrica* 88.4, pp. 1479–1514.

Farahani, Shahin (2008). "Chapter 4 - Transceiver Requirements". In: *ZigBee Wireless Networks and Transceivers*. Ed. by Shahin Farahani. Burlington: Newnes, pp. 137–170.

Fergusson, Leopoldo and Carlos Molina (2019). *Facebook Causes Protests*. Rochester, NY.

Finkelstein, Amy (2007). "The Aggregate Effects of Health Insurance: Evidence from the Introduction of Medicare". In: *The Quarterly Journal of Economics* 122.1, pp. 1–37.

Fisher, Max (2011). *The Arab Spring, Year One, in Videos*. The Atlantic. URL: `https://www.theatlantic.com/international/archive/2011/12/the-arab-spring-year-one-in-videos/250559/` (visited on 02/06/2025).

Free Expression Myanmar (2022). *505A - Act of Revenge: Review of Myanmar Coup Speech 'Crimes'*.

Fujiwara, Thomas, Karsten Müller, and Carlo Schwarz (2024). "The Effect of Social Media on Elections: Evidence from The United States". In: *Journal of the European Economic Association* 22.3, pp. 1495–1539.

Funk, Allie, Kian Vesteinsson, and Grant Baker (2024). *Freedom on the Net 2024: The Struggle for Trust Online*. Freedom House.

Gohdes, Anita R (2015). "Pulling the plug: Network disruptions and violence in civil conflict". In: *Journal of Peace Research* 52.3, pp. 352–367.

Goldman, Russell (2022). "Myanmar's Coup, Explained". In: *The New York Times*.

Gonzalez, Robert and Elisa M. Maffioli (2024). "Is the phone mightier than the virus? Cellphone access and epidemic containment efforts". In: *Journal of Development Economics* 167, p. 103228.

Google Transparency Report (2025). *Traffic and disruptions to Google*.

Gourieroux, C., A. Monfort, and A. Trognon (1984). "Pseudo Maximum Likelihood Methods: Applications to Poisson Models". In: *Econometrica* 52.3, pp. 701–720.

Griffiths, James (2020). *The Great Firewall of China: How to Build and Control an Alternative Version of the Internet*. London, England: Zed Books. 400 pp.

Haas, Benjamin (2019). "The new battle in Hong Kong isn't on the streets; it's in the apps". In: *MIT Technology Review*.

Hein, Ye Myo and Lucas Myers (2021). *Seizing the State: The Emergence of a Distinct Security Actor in Myanmar*. Wilson Center.

Henderson, J. Vernon, Adam Storeygard, and David N. Weil (2012). "Measuring Economic Growth from Outer Space". In: *American Economic Review* 102.2, pp. 994–1028.

Howard, Philip N. (2010). *The Digital Origins of Dictatorship and Democracy: Information Technology and Political Islam*. Oxford University Press.

Howard, Philip N. and Muzammil M. Hussain (2011). "The Upheavals in Egypt and Tunisia: The Role of Digital Media". In: *Journal of Democracy* 22.3, pp. 35–48.

— (2013). *Democracy's Fourth Wave? Digital Media and the Arab Spring*. Oxford University Press.

Januta, Andrea and Minami Funakoshi (2021). "Myanmar's internet suppression". In: *Reuters*.

Jordt, Ingrid, Tharaphi Than, and Sue Ye Lin (2021). "How Generation Z Galvanized a Revolutionary Movement against Myanmar's 2021 Military Coup". In: *How Generation Z Galvanized a Revolutionary Movement against Myanmar's 2021 Military Coup*. Ed. by Ingrid Jordt, Sue Ye Lin, and Tharaphi Than. ISEAS–Yusof Ishak Institute, pp. vii–viii.

Kemp, Simon (2021). *Digital 2021: Myanmar*. DataReportal. URL: `https://www.datareportal.com/reports/digital-2021-myanmar` (visited on 02/10/2025).

— (2024). *Digital 2024: Myanmar*. DataReportal. URL: `https://www.datareportal.com/reports/digital-2024-myanmar` (visited on 02/11/2025).

King, Gary, Jennifer Pan, and Margaret E. Roberts (2017). "How the Chinese Government Fabricates Social Media Posts for Strategic Distraction, Not Engaged Argument". In: *American Political Science Review* 111.3, pp. 484–501.

Lederer, Edith M. (2021). "UN envoy: Myanmar army is 'surprised' at opposition to coup". In: *The Associated Press*.

Li, Qinghua, Brayden G King, and Brian Uzzi (2025). "Quantifying social media predictors of violence during the 6 January US Capitol insurrection using Granger causality". In: *Journal of the Royal Society Interface* 21.220 (), p. 20240314.

Little, Andrew T. (2016). "Communication Technology and Protest". In: *The Journal of Politics* 78.1, pp. 152–166.

Margetts, Helen, Peter John, Scott Hale, and Taha Yasseri (2016). *Political Turbulence: How Social Media Shape Collective Action*. Princeton University Press.

Mattingly, Daniel C. and Elaine Yao (2022). "How Soft Propaganda Persuades". In: *Comparative Political Studies* 55.9, pp. 1569–1594.

Milner, Helen V. (2006). "The Digital Divide: The Role of Political Institutions in Technology Diffusion". In: *Comparative Political Studies* 39.2, pp. 176–199.

Morozov, Evgeny (2011). *The Net Delusion: The Dark Side of Internet Freedom*. New York: PublicAffairs. 432 pp.

Müller, Karsten and Carlo Schwarz (2021). "Fanning the Flames of Hate: Social Media and Hate Crime". In: *Journal of the European Economic Association* 19.4, pp. 2131–2167.

Müller, Karsten and Carlo Schwarz (2023). “From Hashtag to Hate Crime: Twitter and Antiminority Sentiment”. In: *American Economic Journal: Applied Economics* 15.3, pp. 270–312.

Myanmar Internet Project (2022). *DIGITAL COUP QUARTERLY (February 2021 to April 2021)*. URL: https://www.myanmarinternet.info/post/quartely_report_001 (visited on 02/10/2025).

— (2024a). *Myanmar Digital Coup Quarterly: May 2024 to July 2024*. Myanmar Internet. URL: https://www.myanmarinternet.info/post/myanmar-digital-coup-quarterly-may-2024-to-july-2024 (visited on 07/10/2025).

— (2024b). *No End in Sight : Situation of Internet Shutdown and Infrastructure Damages in Myanmar*. URL: https://www.myanmarinternet.info/post/no-end-in-sight-situation-of-internet-shutdown-and-infrastructure-damages-in-myanmar (visited on 05/01/2025).

Myers, Lucas (2023). *The Myanmar Military Is Facing Death by a Thousand Cuts*. War on the Rocks. URL: https://warontherocks.com/2023/11/the-myanmar-military-is-facing-death-by-a-thousand-cuts/ (visited on 02/06/2025).

Myint, Ma Wai Phyo (2023). “Myanmar Junta Planning a Digital Coup”. In: *The Irrawaddy*.

Myint, Sithu Aung (2021). “‘Social punishment’ campaign turns the tables on military elite”. In: *Frontier Myanmar*.

NetBlocks (2021). *Internet disrupted in Myanmar amid apparent military uprising*. Reports. URL: https://netblocks.org/reports/internet-disrupted-in-myanmar-amid-apparent-military-uprising-JBZrmlB6 (visited on 02/10/2025).

Nunn, Nathan and Nancy Qian (2011). “The Potato’s Contribution to Population and Urbanization: Evidence From A Historical Experiment”. In: *The Quarterly Journal of Economics* 126.2, pp. 593–650.

Olken, Benjamin A. (2009). “Do Television and Radio Destroy Social Capital? Evidence from Indonesian Villages”. In: *American Economic Journal: Applied Economics* 1.4, pp. 1–33.

Oo, Phyu Phyu (2021). *The importance of Myanmar’s pots and pans protests — Lowy Institute*. The Interpreter. URL: https://www.lowyinstitute.org/the-interpreter/importance-myanmar-s-pots-pans-protests (visited on 02/06/2025).

Paddock, Richard C. (2022). “Myanmar’s Coup and Its Aftermath, Explained”. In: *The New York Times*.

Pierskalla, Jan H. and Florian M. Hollenbach (2013). “Technology and Collective Action: The Effect of Cell Phone Coverage on Political Violence in Africa”. In: *American Political Science Review* 107.2, pp. 207–224.

Potkin, Fanny and Wa Lone (2021). “Insight: ’Information combat’: Inside the fight for Myanmar’s soul”. In: *Reuters*.

Prasse-Freeman, Elliott (2023). “Bullets and Boomerangs: Proleptic Uses of Failure in Myanmar’s Anti-coup Uprising”. In: *Public Culture* 35.1, pp. 73–112.

Qin, Bei, David Strömberg, and Yanhui Wu (2017). “Why Does China Allow Freer Social Media? Protests versus Surveillance and Propaganda”. In: *Journal of Economic Perspectives* 31.1, pp. 117–140.

— (2024). “Social Media and Collective Action in China”. In: *Econometrica* 92.6, pp. 1993–2026.

Rambachan, Ashesh and Jonathan Roth (2023). “A More Credible Approach to Parallel Trends”. In: *The Review of Economic Studies* 90.5, pp. 2555–2591.

Ratcliffe, Rebecca (2021). “Myanmar: Facebook promotes content urging violence against coup protesters – study”. In: *The Guardian*.

RFA Burmese (2023). “Myanmar No. 2 in world for violations of internet freedom: report”. In: *Radio Free Asia*.

Roberts, Margaret E. (2018). *Censored: Distraction and Diversion Inside China’s Great Firewall*. Princeton University Press.

Rosson, Zach, Felicia Anthonio, and Carolyn Tackett (2025). *Emboldened offenders, endangered communities: internet shutdowns in 2024*. Access Now.

Ruijgrok, Kris (2017). “From the web to the streets: internet and protests under authoritarian regimes”. In: *Democratization* 24.3, pp. 498–520.

Ruohonen, Jukka (2024). “A comparative study of online disinformation and offline protests”. In: *SN Social Sciences* 4.12, p. 232.

Ryan, Megan and Mai Van Tran (2024). “Democratic backsliding disrupted: The role of digitalized resistance in Myanmar”. In: *Asian Journal of Comparative Politics* 9.1, pp. 133–158.

Ryan, Megan, Mai Van Tran, and Swan Ye Htut (2024). “Strange Bedfellows or Trusted Comrades? Digital Solidarity Building among Myanmar’s Revolutionaries”. In: *Journal of Contemporary Asia* 54.5, pp. 887–912.

Rydzak, Jan, Moses Karanja, and Nicholas Opiyo (2020). “Dissent Does Not Die in Darkness: Network Shutdowns and Collective Action in African Countries”. In: *International Journal of Communication* 14.0, p. 24.

Selva, Meera (2019). “Reaching for the off switch: Internet shutdowns are growing as nations seek to control public access to information”. In: *Index on Censorship* 48.3, pp. 19–22.

Shapiro, Jacob N and David A Siegel (2015). “Coordination and security: How mobile communications affect insurgency”. In: *Journal of Peace Research* 52.3, pp. 312–322.

Silva, J. M. C. Santos and Silvana Tenreyro (2006). “The Log of Gravity”. In: *The Review of Economics and Statistics* 88.4, pp. 641–658.

Smith, Alastair and Allan C. Stam (2004). “Bargaining and the Nature of War”. In: *Journal of Conflict Resolution* 48.6, pp. 783–813.

Solon, Olivia (2018). “Facebook’s failure in Myanmar is the work of a blundering toddler”. In: *The Guardian*.

Special Advisory Council for Myanmar (2022). *Effective Control in Myanmar*.

Stecklow, Steve (2018). “Why Facebook is losing the war on hate speech in Myanmar”. In: *Reuters*.

Steinert-Threlkeld, Zachary C. (2017). “Spontaneous Collective Action: Peripheral Mobilization During the Arab Spring”. In: *American Political Science Review* 111.2, pp. 379–403.

Stremlau, Nicole (2025). “Internet Shutdowns, Sovereignty, and the Postcolonial State in Africa”. In: *Global Policy* 16.2, pp. 372–379.

Suh, Chan S., Ion Bogdan Vasi, and Paul Y. Chang (2017). “How social media matter: Repression and the diffusion of the Occupy Wall Street movement”. In: *Social Science Research* 65, pp. 282–293.

The Irrawaddy (2024). “Myanmar Junta Swells Arrest Log for Online Criticism”. In.

Tufekci, Zeynep and Christopher Wilson (2012). “Social Media and the Decision to Participate in Political Protest: Observations From Tahrir Square”. In: *Journal of Communication* 62.2, pp. 363–379.

Weidmann, Nils B. and Espen Geelmuyden Rød (2019). *The Internet and Political Protest in Autocracies*. Oxford University Press.

Wells, Tamas and Aleks Deejay (2021). “How activists are using Facebook in Myanmar for democratic ends, but Facebook itself also facilitated hate speech”. In: *Melbourne Asia Review* (Edition 7, 2021).

Whong, Eugene (2021). *Myanmar Junta Charges 100 Healthcare Pros for Work Stoppage Protests*. Radio Free Asia. URL: https://www.rfa.org/english/news/myanmar/doctors-04192021190924.html (visited on 02/06/2025).

Whong, Eugene and Malcolm Foster (2023). *Myanmar junta arrested more than 1,300 people for online criticism*. Radio Free Asia. URL: https://www.rfa.org/english/news/myanmar/arrests-10132023171012.html (visited on 02/06/2025).

Wilkinson, Bard (2018). “UN official convinced of Myanmar Rohingya ‘genocide’”. In: *CNN*.

Wing, Coady, Seth M. Freedman, and Alex Hollingsworth (2024). *Stacked Difference-in-Differences*.

Xu, Xu (2021). “To Repress or to Co-opt? Authoritarian Control in the Age of Digital Surveillance”. In: *American Journal of Political Science* 65.2, pp. 309–325.

Yanagizawa-Drott, David (2014). “Propaganda and Conflict: Evidence from the Rwandan Genocide”. In: *The Quarterly Journal of Economics* 129.4, pp. 1947–1994.

Zeitzoff, Thomas (2017). “How Social Media Is Changing Conflict”. In: *Journal of Conflict Resolution* 61.9, pp. 1970–1991.

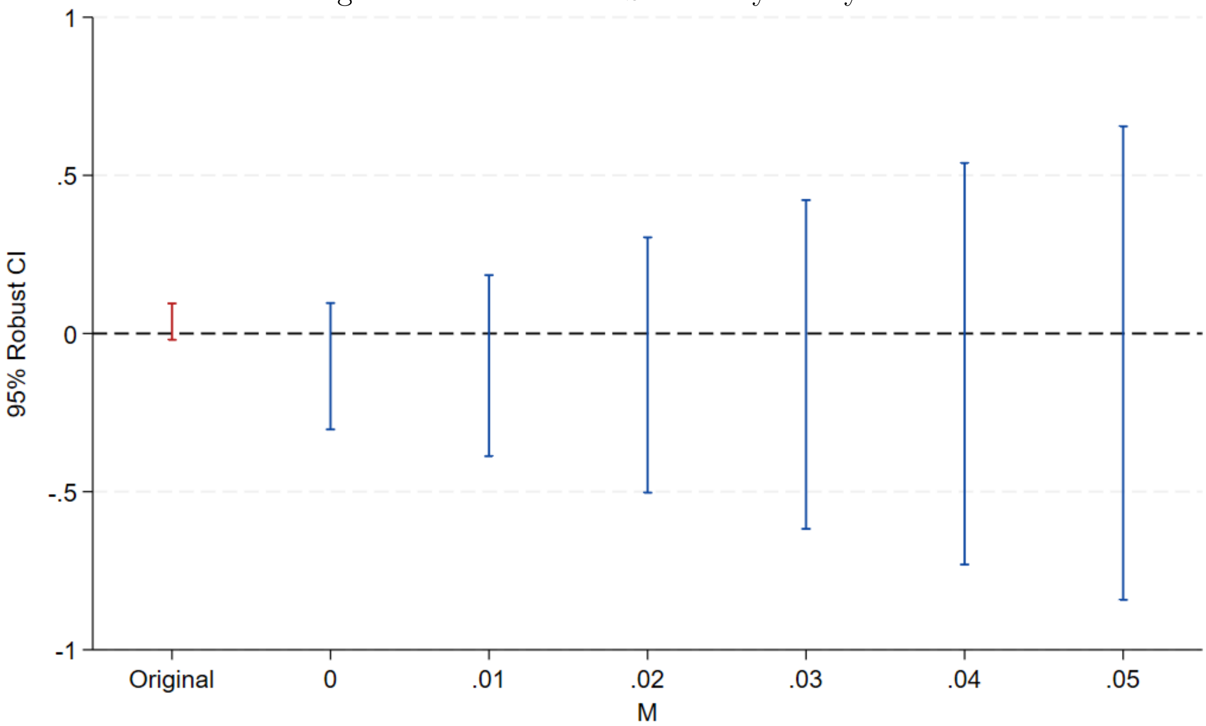
A Appendix: Additional Tables and Figures

Table A.1: Simple DiD Results: OLS

	Binary Outcomes					Continuous Outcomes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FB Posts	-19.921**	-35.444**	-29.613**	-29.606**	-35.169	-19.921**	-35.444**	-29.613**	-29.606**	-35.169
	(9.273)	(13.702)	(14.214)	(14.556)	(21.436)	(9.273)	(13.702)	(14.214)	(14.556)	(21.436)
Mean	554.093	554.093	554.093	554.093	554.093	554.093	554.093	554.093	554.093	554.093
Total Conflict	0.006*	-0.003	-0.004	-0.004	-0.005	-0.081	-0.147**	-0.162**	-0.116*	-0.119*
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.058)	(0.071)	(0.073)	(0.062)	(0.066)
Mean	0.441	0.441	0.441	0.441	0.441	2.096	2.096	2.096	2.096	2.096
Demonstrations	0.004	-0.010**	-0.011***	-0.010**	-0.006	-0.041	-0.096**	-0.100**	-0.088**	-0.058
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.032)	(0.042)	(0.045)	(0.041)	(0.051)
Mean	0.343	0.343	0.343	0.343	0.343	1.355	1.355	1.355	1.355	1.355
Political Violence	0.007**	0.002	0.003	0.003	0.000	-0.036	-0.045	-0.055*	-0.021	-0.049**
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.027)	(0.032)	(0.032)	(0.025)	(0.021)
Mean	0.190	0.190	0.190	0.190	0.190	0.521	0.521	0.521	0.521	0.521
Observations	3300	3300	3300	3300	3300	3300	3300	3300	3300	3300
Free Space Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Hist. Conflict & FB Controls	No	No	No	Yes	Yes	No	No	No	Yes	Yes
State/Region by Time FEs	No	No	No	No	Yes	No	No	No	No	Yes

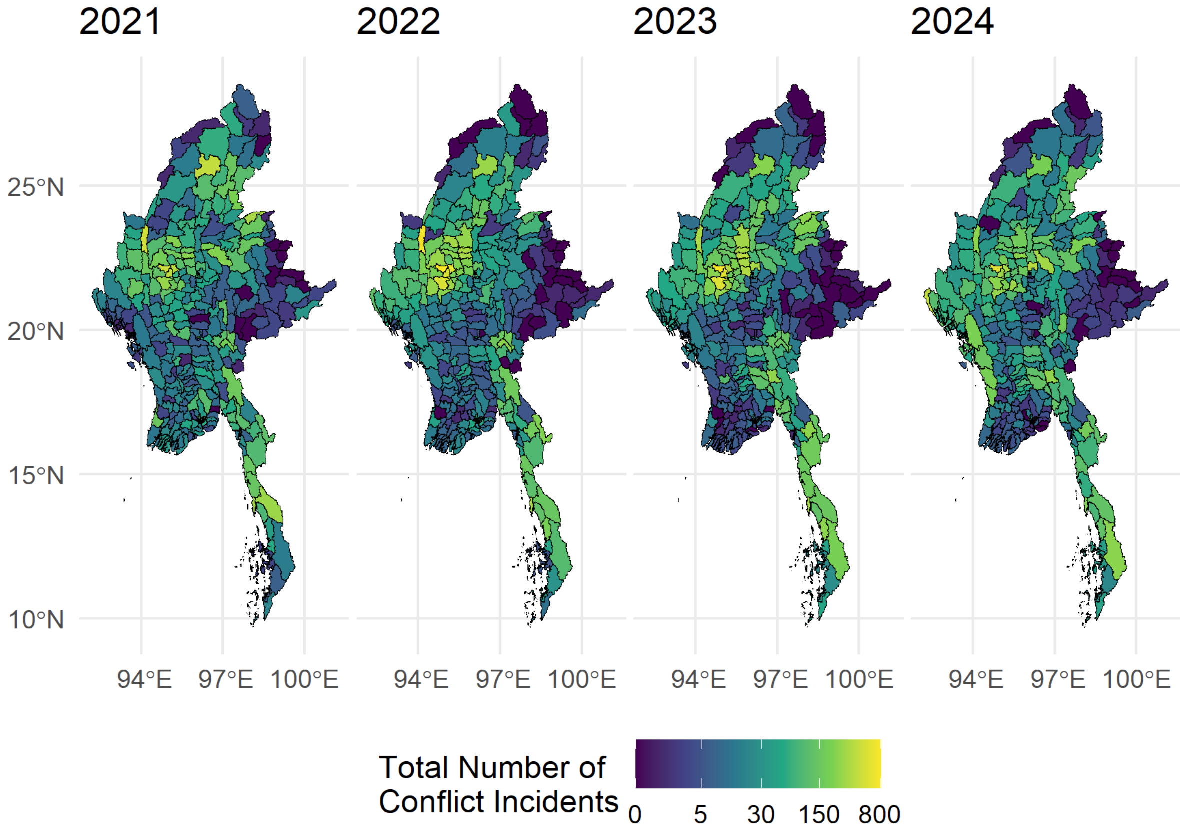
Notes: *** p<0.01, ** p<0.05, * p<0.10. Reported coefficients are OLS estimates of β from Equation 3. In the first row, the outcome variable is calculated as a count of Facebook posts across all columns, while conflict outcomes are converted to binary indicators (columns 1–5) or left as count variables (columns 6–10). The binary indicator is defined as dependent variable $Y_{i,t} = 1$ when we observe at least one instance of conflict type Y in township i during period t, and 0 otherwise. For more information on the regression specifications presented herein, see notes to Table 3.

Figure A.1: Pre-Trend Sensitivity Analysis



Notes: This figure shows sensitivity analysis for the treatment effect on political violence (averaged across all β_k s.t. $k \in [0, 5]$). Following Rambachan and Roth (2023), we plot the 95% confidence interval of the original (observed) effect, as well as 6 simulated confidence intervals corresponding to different assumptions on the parameter M. Each simulation is an estimate of the hypothetical confidence interval given an extrapolation of the observed pre-trend into the post-treatment period, where M indicates the maximum allowable difference in the slope of the pre-trend across consecutive periods (e.g., $M = 0$ corresponds with a linear extrapolation of pre-trends).

Figure A.2: Annual Conflict



Notes: This map displays the total number of conflict incidents recorded per township in a given year, based on total incident counts from ACLED data.

B Appendix: Automated Classification of Facebook Posts

The following steps describe the text classification procedure that was used to identify political Facebook content.

1. **Manual Labeling**: We first read through a small random sample of posts ($n = 1,000$) to identify those containing content related to the political crisis in Myanmar, broadly defined to include any mention of the coup, the military, or resistance actors/activity. While the majority of political posts in our sample are from news sources (both formal news outlets and informal citizen journalists), there are also many non-media posts containing a range of opinions and communication tactics, e.g., expressing approval/disapproval of the military or resistance, encouraging or coordinating collective action, inciting violence and/or spreading hateful speech, exposing personal information and encouraging retaliation against individuals suspected of wrongdoing.

2. **Model Tuning**: We then fine-tuned a large language model on the sample of manually labeled posts, using a multilingual version of the popular BERT model, which is pre-trained on a large corpus of Myanmar language text.[61] In this step, the model learns about language features that are common to all the posts labeled as political in the sample set.

3. **Automated Classification**: Finally, we introduced the full set of unlabeled Facebook posts to the fine-tuned model, asking it to predict, for each post, whether or not the content is political in nature. From what it has learned during the fine-tuning phase, the model can make a probabilistic determination about the true classification of each post. Based on validation exercises against a small and previously hidden subset of manually labeled posts, the model performs quite well, providing an accurate classification in over 95 percent of cases.

Though we currently use this method only to separate political and non-political posts, it is a flexible approach that can be applied to any classification task of arbitrary complexity, and in which the main constraint is a human's ability to perform the initial manual classification on a sufficiently large sample of positive cases.[62] For example, within the set of political posts, one could further categorize text by the author's method of

[61]For both tokenization and fine-tuning, we used the BERT multilingual base model (uncased), available at `https://huggingface.co/google-bert/bert-base-multilingual-uncased`.

[62]In order to achieve accurate model predictions, fine-tuning must be performed on a labeled sample with a sufficient number of cases for each classification category. Thus in cases where certain categories appear only rarely in the data, it may be impossible to obtain sufficient representation of these categories from a small random sample.

communication, or “rhetorical” strategy. (Does the post use hateful speech or graphic imagery to make an emotional appeal? Does the post encourage participation in or contain information about a future protest?) These types of classifications could help to further uncover some of the specific mechanisms underlying the connection between internet access and political conflict.

C Appendix: Staggered DiD Estimation of Targeted Internet Shutdowns

We follow the specific procedure outlined in Chaisemartin and D'Haultfœuille (2024) to estimate dynamic effects of the internet shutdowns for up to eight weeks after implementation. To be precise, we calculate the following DiD estimator separately for each post-treatment period in each township:

$$D_{i,l} = Y_{i,F_i-1+l} - Y_{i,F_i-1} - \frac{1}{N^{i'_l}} \sum_{j \in i'_l} \left(Y_{j,F_i-1+l} - Y_{j,F_i-1} \right). \tag{C.1}$$

In Equation C.1, F_i represents the time period in which township i was first treated (first exposed to an internet blackout), and l is a fixed integer, so Y_{i,F_i-1+l} represents the value of the outcome variable Y for township i measured l periods after the period directly before initial treatment exposure. Next, i'_l represents the set of all townships that have not yet changed their treatment status through period $F_i - 1 + l$, and $N^{i'_l}$ represents the number of townships in this set. In other words, $D_{i,l}$ calculates the difference between the following two values: (1) the change in the outcome variable observed in township i from the period directly before Treatment to $l-1$ periods after and (2) the average change in the outcome variable across the same stretch of time for the group of townships that have not yet changed their treatment status as of $l-1$ periods after township i's initial exposure.[63]

While Equation C.1 estimates separate effects for each township and relative period,[64] we can aggregate these effects across all townships to generate a single average effect for each l:

$$D_l = \frac{1}{N^{i''_l}} \sum_{j \in i''_l} D_{j,l}. \tag{C.2}$$

Here we denote the set of all townships i for which $D_{i,l}$ can be estimated as i''_l,[65] and $N^{i''_l}$ represents the number of townships contained in this set. D_l thus represents a simple average of all $D_{i,l}$ across the full set of townships for which $D_{i,l}$ can be estimated.

These D_l are analogous to β_k in Equation 4, in that they estimate the average effect of the treatment across all treated townships, i.e., the average effect of restricting internet access. However, in the case of the nationwide shutdown, β_k represents the marginal effect

[63] Though we do not show it here, $D_{i,l}$ is then normalized by the total number of periods between F_i and $F_i - 1 + l$ during which township i remains treated.

[64] Since each l represents a distance from the period in which a township was first treated, this represents a different calendar period for each cohort. As such, we are not estimating effects by calendar time, but by event time, i.e. time relative to initial treatment period.

[65] Since $D_{i,l}$ can only be estimated for townships in which a sufficient number of periods are observed, we say that i''_l contains all townships i for which $F_i - 1 + l \leq T_i$, where T_i is the total number of periods for which the outcome variable and treatment status are observed for township i.

of treatment for townships with varying levels of internet access (or treatment *exposure*), and due to the universal coverage and non-staggered timing of the shutdown, we are not able to leverage comparisons with a true control, or untreated group. In the case of the rolling blackouts, however, we can always compare treated townships with untreated and not-yet treated townships—since internet restrictions do not affect all townships at once, and some townships are never targeted during our period of coverage—so D_l represents a standard binary treatment effect.

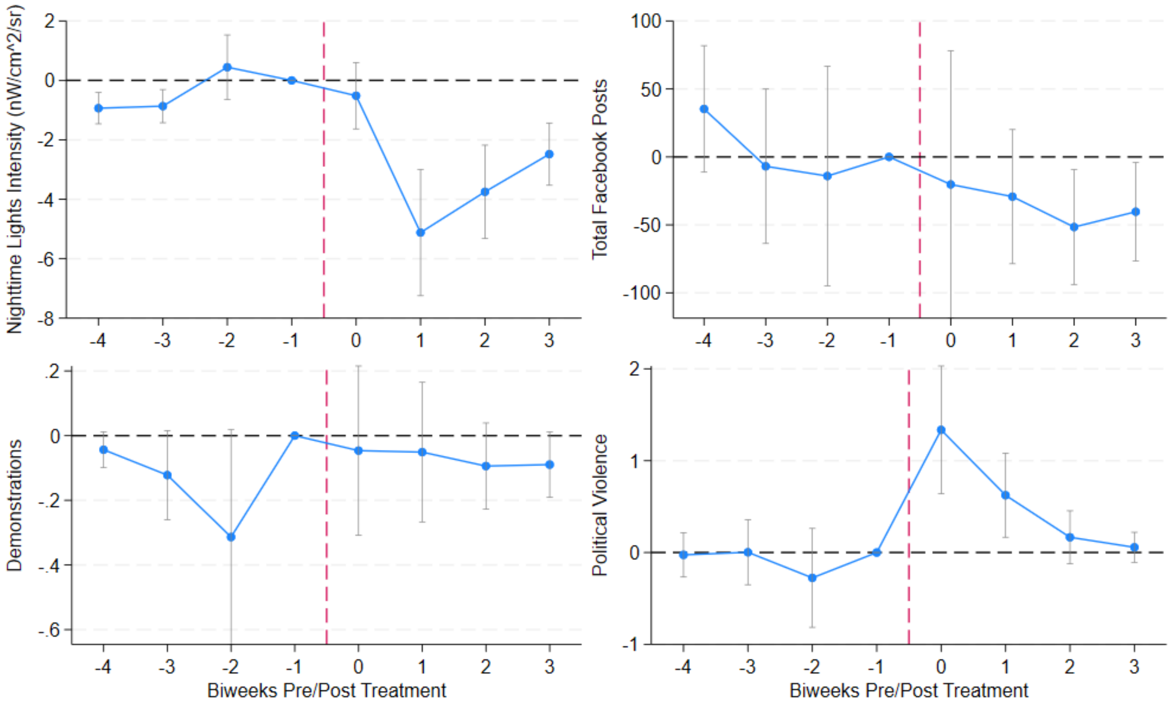
In Figure C.1 we plot D_l for a range of outcome variables, across 4 pre-treatment and 4 post-treatment periods each.[66] Turning first to the upper right panel, we note that these internet shutdowns lead to a reduction of up to 50 Facebook posts per two-week period. However, unlike before, we do not see this translate to any real effect on the number of demonstrations.[67] Instead, we observe a large immediate effect on violent conflict, but this is not surprising considering what we know about the timing of targeted internet shutdowns. This effect is consistent with reports that military attacks are often preceded by communication blackouts in an attempt to weaken resistance and coordination among rebel groups.

Finally, the upper left panel of Figure C.1 plots treatment effects on our satellite measure of nightly light intensity, showing a strong and sustained effect starting one period after the start of an internet shutdown. This sudden and dramatic fall in nighttime lights is likely due to a combination of two factors. First, the increase in violent conflict that we observe in the first period after an internet shutdown should have a direct effect on light intensity due to infrastructure and property destruction, especially in the case of artillery attacks and air strikes, both of which have been reported as common offensive strategies in the wake of communications blackouts (Athan 2022). Second, there have also been many documented cases of targeted cuts to electricity, often occurring in conjunction with or directly after internet shutdowns, as a further attempt to soften resistance (Myanmar Internet Project 2024a). As with property damage caused by military strikes, electricity cuts will have an obvious direct effect on measured nighttime lights intensity in affected townships.

[66]Note that we are no longer estimating a PPML model, as in Equations 3 and 4. D_l simply represents an average difference in the outcome variable, so effect sizes in Figure C.1 can be interpreted as level changes.

[67]We see a significant pre-trend on demonstrations, suggesting the possibility of anticipation on the part of treated townships or increased military activity in advance of treatment.

Figure C.1: Targeted Internet Shutdowns – Chaisemartin and D'Haultfœuille (2024)



Notes: Points represent D_l from Equation C.2 for all $\{l \in \mathbb{Z} \mid -3 \leq l \leq 4\}$, i.e., up to 4 pre-treatment periods and 4 post-treatment periods, with outcome variable Y defined as either nighttime lights intensity, number of Facebook posts, number of demonstrations, or number of incidents of political violence (including 90% confidence bands based on robust standard errors clustered at the township level). Each l represents a period in event time, which are measured in relation to the start of each township's treatment date, and thus do not correspond with a specific calendar date. Our data runs from January 1$^{\text{st}}$, 2021 through December 31$^{\text{st}}$, 2024, and each period spans two weeks. (For Facebook outcomes, data only runs through the end of 2022.) The first internet shutdown occurs on June 10$^{\text{th}}$, 2021, and these shutdowns continue on a rolling basis through the end of 2024. In calculating D_l, observations are weighted by township population.