The Monitoring Role of Social Media

Jonas Heese
Harvard Business School
jheese@hbs.edu

Joseph Pacelli
Harvard Business School
jpacelli@hbs.edu

Abstract

In this study, we examine whether social media activity can reduce corporate misconduct. We use the staggered introduction of 3G mobile broadband access across the United States to identify exogenous increases in social media activity and test whether access to 3G reduces misconduct. We find that facilities reduce violations by 1.8% and penalties by 13% following the introduction of 3G in a local area. To validate social media activity as the underlying mechanism, we show that 3G access results in sharp increases in Tweet volume and that facilities located in areas with high Tweet volume engage in less misconduct. The effect of 3G access on misconduct is stronger for facilities of more visible firms and concentrated in non-financial violations, such as those involving unsafe workplace conditions and inappropriate treatment of employees and customers. Overall, our results demonstrate that social media plays an important role in monitoring corporate misconduct.

Keywords: corporate misconduct; social media; mobile broadband.

JEL Classifications: M40, M41

Acknowledgements: This paper has benefited from the suggestions of Francois Brochet (discussant), Thomas Bourveau (discussant), Patricia Dechow (discussant), Alexander Dyck (discussant), Travis Dyer (discussant), Brady Twedt and participants at Brunel University, 2022 CETAFE, 2022 FARS Mid-Year Meeting, Florida State University, Kent State University, 2022 London Business School Accounting Symposium, London School of Economics and Political Science, 2022 MIT Asia Conference, 2022 PAC Conference, University of Texas – Austin, and Virtual Accounting and Finance Seminar Series of University of Bielefeld, University of Bonn, University of Dortmund, WHU, and University of Wuppertal. We thank Karsten Müller for sharing data on Twitter users around the 2007 SXSW festival. We also thank Tony Liu and Dolly Yu for excellent research assistance.

1. Introduction

> *All companies now operate in a world that's closely watching their policies, actions, and how they handle themselves when things go wrong. When literally anyone can simultaneously act as a customer, a protester, a critic, and a muckraking reporter with a video camera, executives have zero room for error.*
>
> – Andrew Winston, Harvard Business Review, 2017

The rapid growth in social media use over the past two decades has presented firms with a significant challenge. Users on social media platforms can share and disseminate damaging information with potentially adverse consequences for firms (Miller and Skinner, 2015). Prominent examples of damaging content that went "viral" on social media include accounts of racist and sexist treatment of employees at Walmart, unsafe working conditions at Amazon, unfair pay practices at Chipotle, and environmental violations at Nestlé (Chaudhari and Purkayastha, 2011; Mui, 2011; Jennings, 2020; Carman and Heil, 2021). As noted by social psychologist Takuya Sawaoka, "[t]he internet now allows […] thousands of people to participate in collective […] [monitoring], in a way that wasn't possible before" (Meinch, 2021). These anecdotes suggest an important, yet unexplored, role of social media in monitoring corporate misconduct. The objective of this study is to examine whether and to what extent social media activity reduces corporate misconduct.

Examining the effects of social media on corporate misconduct poses several empirical challenges. Most importantly, many forms of social media activity are unobservable and endogenous to misconduct.[1] We attempt to overcome these challenges through various complementary empirical approaches. Our primary empirical methodology follows Guriev et al. (2021) and exploits the staggered introduction of third-generation (3G) mobile broadband networks across the United States to identify exogenous increases in social media activity.[2] 3G

[1] For example, larger firms may simultaneously generate more social media activity and commit more violations, given the complexity and size of their operations.
[2] We further discuss and validate this proxy in more detail below.

access provides users with faster data transfers and the ability to share content on their mobile devices and has been a key driver of the rapid growth in the use of popular social media applications, such as Twitter, Facebook and YouTube (Rainie and Wellman, 2012). We thus examine whether 3G access is associated with reductions in local misconduct levels.

While the 3G instrument alleviates many endogeneity concerns, it admittedly provides a less direct linkage between social media activity and misconduct. Consequently, we also use the popular social media platform, Twitter, as a setting for conducting a rich set of additional analyses to strengthen the link between social media activity and misconduct. In particular, we utilize detailed data from Twitter to both validate the 3G instrument and also assess the direct association between Tweet volume and misconduct. We also examine the effects of exogenous increases of Twitter activity following the 2007 South by Southwest (SXSW) festival, which has been shown to drive growth on the platform (Müller and Schwarz, 2020; Fujiwara et al., 2021). The Twitter setting is not without its limitations, as it represents activity on only *one* social media platform, thus sacrificing external validity. We therefore view our various empirical strategies as complementary and as facing orthogonal limitations.

The final notable feature of our empirical framework is the misconduct data we utilize from Violation Tracker. These data comprise a wide set of violations and penalties related to issues that are regularly exposed on social media, including financial issues as well as non-financial issues related to workplace safety, employee discrimination, labor relations, and environmental violations.

Ex ante, the effects of social media activity on corporate misconduct are unclear. On the one hand, social media platforms provide a mechanism for local citizens to expose and disseminate information about misconduct to the general public, regulators, and other monitors. According to Becker's (1968) model of crime, the exposure and dissemination of such information can increase the expected costs of engaging in misconduct for firms, which in turn generates incentives for managers to reduce misconduct (e.g., Dyck et al., 2008).

On the other hand, there are valid reasons to suggest that social media activity may have no effect on corporate misconduct. First, the costs associated with negative social media content may not be substantial enough to deter misconduct because viral web content is often short-lived, with an average half-life of only one day (Maiaroto, 2013). Second, the internet often hosts "fake news," and outsiders may not be able to determine whether negative social media coverage truly reveals misconduct at a firm.[3] Finally, even if social media can expose misconduct, firms still may not reduce it. Instead, firms may respond with less costly activities, such as issuing a public apology which, given the limited attention of users, may be sufficient. Ultimately, whether and the extent to which social media activity impacts corporate misconduct are empirical questions.

As indicated above, our primary empirical strategy follows Guriev et al. (2021) and relies on a difference-in-differences (DiD) analysis exploiting the staggered rollout of 3G networks across the United States to identify exogenous increases in social media activity. We begin by first obtaining digital maps of 3G network coverage from Collins Bartholomew's Mobile Coverage Explorer and then supplement this dataset by manually collecting news articles indicating the date of 3G expansion in various localities. Using these data, we determine the year in which 3G coverage becomes available in a zip code. We then incorporate data from Violation Tracker containing detailed facility-level violations and penalties issued by 44 regulatory agencies. Our sample includes 11,508 violations perpetrated by 10,590 unique facilities of 1,360 Compustat firms, including approximately 80% of Fortune 100 and Fortune

[3] Zhuravskaya et al. (2020) provide a review of the literature and summarize the well-documented evidence of false stories being shared on social media.

500 firms, for the period 2000 to 2017. We examine how both the number of violations and resulting penalties change in the three-year period following 3G implementation. Our analyses control for facility-level controls (employee headcounts and sales), firm-level controls (size, leverage, and profitability), and county-level controls (labor force participation and unemployment rate) that may relate to local misconduct and 3G availability. Finally, we also incorporate facility fixed effects and state-year fixed effects to control for time-invariant characteristics of the facility as well as time trends in the facility's state.

Our main results show that 3G access leads to a decline in both penalties and violations in the three years after a 3G rollout. In terms of economic significance, our estimates indicate that 3G access decreases facility-level penalties by nearly 13% and the number of violations by 1.8%. These estimates likely represent a lower-bound effect, because they are based only on observed misconduct and do not capture reputational costs. Overall, these findings are consistent with the view that social media is an effective monitor of corporate misconduct.

Having established a robust relationship between 3G access and misconduct, we next introduce a set of analyses relying on Twitter data to better establish the social media mechanism underpinning our findings. These analyses help support our claim that 3G access is an appropriate proxy for increased social media activity and also assess whether our effects vary meaningfully with social media activity. We utilize data from Twitter as it is one of the most popular social media applications, with more than 330 million monthly active users. In addition, Twitter is one of the only social media platforms that provides "geotags." These data allow us to obtain the precise geographical origin of Tweets as of 2010, and thus map Tweets directly to local 3G rollouts and facility-level misconduct.

In our first analysis, we examine how Twitter activity changes in a locality following the introduction of 3G. This is an important step in our analyses as it is not clear whether 3G should have a meaningful impact on social media activity in our setting (compared to Guriev et al.'s (2021) international setting) given the prevalence of home broadband internet in the U.S. We examine regressions of the number of Tweets in a zip code on our 3G indicator and include zip code and year fixed effects, making this test akin to a generalized DiD model. Our results indicate a sharp increase in Tweets in a zip code following 3G access. This evidence validates a necessary underlying assumption in our earlier analysis as it establishes that 3G access does indeed increase social media activity.

Next, we attempt to establish a more direct link between social media activity and misconduct. As discussed above, our primary analyses utilize 3G access as an instrument, given that prior research provides a precedent for considering 3G as a plausibly exogenous shock to social media activity (Guriev et al., 2021). To support the underlying social media mechanism, we repeat our main analysis and use an indicator for high social media activity (defined as above the median number of Tweets in a zip code) as an alternative treatment variable. Using this alternative specification, we find that facilities located in zip codes with an above median number of Tweets are associated with reduced misconduct levels. This result further supports the argument that social media activity helps curtail misconduct.

The Twitter setting also provides us with an opportunity to consider an alternative identification strategy. Following Fujiwara et al. (2021) and Müller and Schwarz (2020), we utilize the SXSW festival as a shock that spurred the use of Twitter across the U.S. We find that facilities located in counties with interest in SXSW that experienced an increase in the number of Twitter users following the festival (but not before the festival) significantly reduce misconduct compared to facilities located in counties with interest in SXSW that did not experience an increase in Twitter users following the festival.

Our evidence thus far suggests that social media activity is associated with reductions in misconduct. We next conduct a set of cross-sectional analyses that consider whether the effects of 3G vary based on firm visibility. In the case of social media, more visible firms are expected to respond more to 3G as social media content will be consumed by more viewers across the country. As per Becker (1968), increased viewership should be associated with higher expected costs, as it increases the chance of negative content going viral and generating reputational damage or attracting regulatory scrutiny. We use three proxies to capture firm visibility: firm size, firm Twitter followers, and firm media coverage. Consistent with our expectation, we find that 3G reduces misconduct more for facilities of large firms, facilities of firms with higher levels of Twitter followers, and facilities of firms with more media coverage.

We conclude by conducting a battery of exploratory analyses and robustness tests that further enrichen our study. More specifically, we (i) demonstrate that the effect of 3G access on misconduct is only observed among non-financial violations; (ii) conduct analyses that rule out pre-trends and anticipatory effects; (iii) demonstrate the robustness of our results to controlling for unobserved heterogeneity at the county-year, firm-year, or industry-year fixed level; and (iv) consider alternative sampling choices, research methodologies, and standard error clustering choices. We also consider an alternative explanation in which 3G facilitates increases in IT investments that help reduce misconduct. We find no evidence to suggest that 3G increases such investments, adding additional credence to our claim that social media monitoring reduces misconduct. Finally, we explore intra-firm dynamics and consider the possibility that other facilities within a firm also update their monitoring and improve their compliance procedures. We find no evidence to suggest that 3G rollouts are associated with intra-firm spillover effects, suggesting that 3G access does not prompt firm-wide changes in compliance practices. In sum, our results indicate a significant and robust relationship between

3G rollout and facility-level misconduct, and suggest an important role for social media in monitoring firm misconduct.

Our contribution to the literature is threefold. First, we extend the burgeoning literature examining the implications of social media for corporations, which has primarily examined social media's *informational* role in capital markets (e.g., Blankespoor et al., 2014; Lee et al., 2015; Curtis et al., 2016; Drake et al., 2017; Gomez et al., 2018; Cao et al., 2021).[4] We extend this literature by providing evidence consistent with social media serving as a *monitoring* role. Our findings show that increased social media activity helps deter a wide range of corporate violations, particularly those that are non-financial in nature and potentially of interest to ESG investors (e.g., environmental violations, consumer-protection violations, and workplace safety violations). Our effect sizes suggest a meaningful monitoring role for social media as access to 3G access reduces local penalties for non-financial violations by up to 20%.

Second, our study also contributes to the accounting literature examining the role of external monitors in shaping agents' propensity to engage in misconduct. Prior work, for example, examines the monitoring role of the traditional media (Miller, 2006; Heese et al., 2022), regulators (Correia, 2014; Duro et al., 2019; Heese, 2019, 2022; Bourveau et al., 2021), or whistleblowers (Heese and Pérez-Cavazos 2019, 2021; Heese et al., 2021; Berger and Lee, 2022; Dey et al., 2022). Our study provides evidence that social media can play an important role in reducing firm misconduct in part because it allows a broader set of viewers to become aware of local misconduct. In addition, our findings also suggest that social and traditional

[4] One notable exception is the study by Dube and Zhu (2021) which shows that firms improve their workplace practices in response to Glassdoor reviews.

media play a complementary role in deterring corporate misconduct – a particularly important insight in light of the demise of traditional media in the U.S. and elsewhere.[5]

Finally, our results can be of interest to policy-makers and regulators concerned about the role of social media and corporate misconduct. While the current public debate primarily focuses on the negative effects of social media, for example, by spreading "fake news" and disinformation, our findings highlight a "bright side" of social media use in that it can reduce corporate misconduct, in part because it empowers citizens to monitor firms' behavior.

2. Background and Related Literature

2.1 Social Media in the United States

Social media encompasses the set of web-based technologies that allow people to share information in virtual communities and networks (Lee et al., 2015). The history of social media in the United States can be traced back to the mid-1990s, with the emergence of online communication services such as CompuServe, America Online, and Prodigy.[6] Subsequently, there was rapid growth in platforms that targeted specific user needs, such as blogging (Live Journal in 1999), professional networking (LinkedIn in 2002), and social networking (MySpace in 2003). As noted by Lee et al. (2015), Twitter and Facebook represented the next evolution of social media. Gaining popularity in the late 2000s, these platforms substantially increased the ease with which users could share updates through their user-friendly platforms and "one-click" tools for sharing.

As social media platforms proliferated, the United States was simultaneously overhauling its mobile broadband infrastructure to support the increased demand for

[5] In the U.S., for example, the circulation of local newspapers has decreased by nearly 50% over the last two decades (Pew Research Center, 2019).
[6] For a brief review of social media history, please see https://online.maryville.edu/blog/evolution-social-media.

technology. 3G, or third-generation, wireless mobile telecommunications emerged towards the end of the 20th century with the promise of satisfying users' demand for greater data capabilities and services. The technology represented a significant improvement to existing 2G networks, which offered theoretical maximum download speeds of 0.3 mbps. 3G networks offered download speeds of up to 43 mbps, which could better support typical social media usage (requiring 3-10 mbps).[7]

Indeed, evidence suggests that most social media activity occurs on mobile devices and that 3G has accelerated the spread of social media (Guriev et al., 2021). Notably, Kemp (2018) finds that nearly three billion users (i.e., 93% of total users) accessed social media via mobile devices in 2017. Similarly, social media outlets such as YouTube and Twitter frequently conduct reviews of their user bases and note similar trends. For example, YouTube indicates that more than 70% of its users watch videos from mobile devices, and Twitter reports that mobile users represented 80% of its total users in 2015.[8]

2.2 Prior Literature

Our study is motivated by the idea that social media can play an important role in monitoring firm misconduct, as social media platforms allow users to share and disseminate evidence of firm misconduct. In other words, we examine whether social media can serve as a monitor of corporate misconduct.

Despite the potential influence that social media can have on firm misconduct, our understanding of the monitoring role of social media is limited. Prior studies largely focus on

---

[7] See for example https://kenstechtips.com/index.php/download-speeds-2g-3g-and-4g-actual-meaning and https://www.move.org/how-much-internet-speed/. The limitations of 2G for social media were substantial enough to motivate Twitter to introduce a lite version of its app in 2017 to international markets relying on 2G (https://blog.twitter.com/en_us/topics/product/2017/introducing-twitter-lite).
[8] See, for example, https://www.youtube.com/intl/en-GB/about/press/ and https://www.statista.com/chart/1520/number-of-monthly-active-twitter-users/.

the informational role of social media in capital markets, exploring a wide range of platforms including Seeking Alpha, Twitter, and Estimize. For example, several studies examine how social media relates to earnings announcements (EAs) and forecasting.[9] Another set of studies examines how firms use social media (e.g., Twitter) to disseminate news and manage expectations (e.g., Blankespoor et al., 2014; Lee et al., 2015; Cao et al., 2021). These studies ultimately provide a strong foundation for establishing social media's informational role. As noted by both Miller and Skiller (2015) and Blankespoor et al. (2020), however, the social media literature is still nascent. Our objective is to examine whether social media can also serve as a monitor of corporate misconduct.

Our study is grounded in a growing literature in economics that studies the role of social media in political contexts using the introduction of mobile broadband technology as an instrument for social media activity. As Guriev et al. (2021) explain, 3G access allows users to freely browse the internet from a smartphone and to use social media applications. As a result, 3G access increases the likelihood of using social media, the number of hours spent on social media, and the range of types of shareable content. Using global survey data, they find that expansion of 3G mobile networks reduces government approval and helps expose government corruption. Similarly, Donati (2019) shows that 3G internet coverage influenced political outcomes in South African municipal elections between 2011 and 2016, as 3G increased the spread of social media. Relatedly, recent research also demonstrates the impact of social media, with evidence that social media can help coordinate protest activity and reduce corruption

[9] Gomez et al. (2018) find that SeekingAlpha content reduces information asymmetry prior to the EA and Curtis et al. (2016) find that social media activity increases EA price responses. Drake et al. (2017), however, show that the informational value of social media depends on the content, with less sophisticated blogs reducing the speed of the EA price response. Jame et al. (2016) demonstrate the value of forecasts on Estimize.

(Fergusson and Molina, 2019; Enikolopov et al., 2018, 2020). Collectively, these studies suggest that social media can expose and reduce political corruption.

Our study also relates to the literature focused on understanding the efficacy of traditional media (e.g., newspapers and the business press) in monitoring corporate misconduct. For example, Miller (2006) provides evidence of the press exposing accounting fraud through their investigations. Other studies provide mixed evidence on the ability of the press to monitor misconduct. Dyck et al. (2008) document governance changes in Russian firms in response to international press coverage, which is consistent with traditional media playing a monitoring role, while Core et al. (2008) find no evidence that the media curtails excessive compensation. More recently, Heese et al. (2022) address this question by focusing on the local press. They find that closures of local newspapers are associated with increased local firm misconduct, which is consistent with the idea that traditional media serves a monitoring role. Collectively, these studies enhance our understanding of the role of traditional media outlets in monitoring corporate misconduct.

We note that there are several important differences between traditional media and social media that warrant an independent investigation of social media's role in monitoring misconduct. First, social media allows citizens and other stakeholders to share information with the general public largely without editorial approval or other forms of constraint. As such, social media may enable more extensive and comprehensive sharing of information about potential misconduct in contrast to traditional media outlets that only cover stories approved by editors. Second, the traditional media likely faces different incentives that may influence its monitoring efforts. For example, the traditional media may face conflicts of interest related to their relationships with firms (through advertising partnerships) or consumers (through subscription income) (e.g., Mullainathan and Shleifer, 2005; Gurun and Butler, 2012; Shapira and Zingales, 2017). In fact, research (e.g., Miller, 2006; Stäbler and Fischer, 2020) shows that these and other considerations affect the traditional media's coverage of corporate misconduct. These monetary incentives are largely non-existent for social media users. Instead, social media users generally post and share content for intrinsic factors, such as the desire for recognition or the sense of efficacy (Amarashinghe, 2010). Third, social media users have far fewer resources than traditional media outlets, which maintain large staffs of reporters. For example, large media companies such as Dow Jones generate over a billion dollars a year in revenue and employ thousands of individuals. Thus, it is unclear whether social media users face strong enough incentives and have substantial enough resources to uncover misconduct. Finally, while traditional media has been declining over recent decades, social media shows strong growth, emphasizing the importance of understanding the monitoring role of this type of media.

2.3 The Role of Social Media in Reducing Firm Misconduct

Ex ante, it is unclear whether and to what extent social media can reduce firm misconduct. According to Becker's (1968) model of crime, a firm's decision to engage in fraud is shaped by the expected benefits and expected costs associated with such behavior. In particular, the model demonstrates that firms are less likely to engage in fraud when the expected costs, which are a function of the probability of being caught and the size of penalties, exceed the expected benefits of misconduct. Social media applications make it easier for employees, customers, or other stakeholders to share potentially damaging information about firms, increasing the probability that inappropriate behavior becomes known to a larger audience, which in turn carries reputational and legal costs (e.g., Baloria and Heese, 2018; Dyck et al., 2008).

As discussed earlier, there are numerous examples of viral posts exposing firms' inappropriate behavior, including examples of poor employment practices, unsafe working conditions, unfair pay, and environmental violations. The costs associated with negative social media coverage are large enough that firms are increasingly hiring third-party professional monitoring services to identify viral posts that can harm stock price (Atkinson, 2019). Social media posts about potential misconduct can also trigger costs by increasing regulatory investigations, as law enforcement agencies are increasingly monitoring social media to identify enforcement targets (Tau, 2021). Similarly, the traditional media can pick up damaging posts, which further disseminates reports of inappropriate behavior to a larger audience. In fact, surveys indicate that an "overwhelming majority of reporters and editors use social media sources for researching their stories" (Bunz, 2010).

Taken together, these arguments suggest that social media can increase the expected costs of engaging in misconduct for a firm, leading firms to reduce misconduct. In fact, there is some evidence that firms actively seek to address problems reported on social media. For example, the findings of Dube and Zhu (2021) suggest that firms learn from Glassdoor reviews and improve their workplace practices in response to being reviewed. Thus, under this view, we would expect that social media effectively monitors corporate misconduct.

We note, however, that it is also possible that social media has no effect on firm misconduct. First, the damage from social media posts may not be substantial enough to warrant a reduction in misconduct, as viral web content is generally short-lived, with an average half-life of only one day (Maiaroto, 2013). Second, the internet contains a significant amount of "misinformation" or "fake news." Thus, outsiders may not be able to determine whether negative social media posts truly reveal inappropriate behavior, which can dilute reputational damage and generate fewer incentives for firms to respond. Third, it is also possible that firms would choose alternative, less costly reputation-repairing responses to a damaging social media campaign, if one were to arise. For example, a "public apology" is relatively costless, effective, and possibly quicker strategy for diffusing negative sentiment (Winston, 2017). In such cases, however, the underlying misconduct would not change. Ultimately, whether and the extent to which social media is an effective monitor of corporate misconduct are empirical questions to explore.

3. Empirical Methodology and Data

3.1 Data

3.1.1 Violation Tracker Data

We obtain data from two primary sources. First, we collect corporate-misconduct data from Violation Tracker for the period 2000 (the first year this data is available) to 2017. This data is maintained by Good Jobs First, a non-profit organization focused on promoting corporate and government accountability. The Violation Tracker database is advertised as being the first wide-ranging database on corporate misconduct.

Violation Tracker includes 67,000 violations for our sample period. From these data, we retain all observations in which the parent company is a publicly traded firm and drop violations from financial institutions. We focus on public firms in our main analyses as they are subject to a different set of regulations and engage in the majority of misconduct (70% of violations in our sample).[10] Note that Violation Tracker matches facilities to the *current* parent company, even if the facilities were part of a different parent company at the time of the violation. We adjust for this choice by matching facilities to their *historical* parent over time.[11] Violations in which the location of the misconduct is ambiguous or not available are matched to a firm's headquarters location.[12] As we describe in more detail in Section 3.2, we use a six-

[10] We also consider private firms in a robustness analysis (discussed in more detail below).
[11] The results are robust when we do not adjust for this choice.
[12] The results are robust when excluding those violations (see Section 6.2).

year window around the treatment date and drop all other years of treated facilities.[13] Our final sample includes 11,508 violations with $8.5 billion in penalties sanctioned against 1,360 unique firms, including approximately 80% of Fortune 100 and Fortune 500 firms. Table 1, Panel A describes our sample composition in more detail.

The Violation Tracker database only includes facilities with at least one violation during our sample period but does not include facilities that never had a violation between 2000 and 2017. To account for inherent differences between facilities that have violations and those that do not, we run our primary analyses using facilities that have at least one violation at any point during our sample period. In additional analyses, we also re-examine our primary results using a sample that also includes non-violation facilities that report sales (see Table 9, Panel E). For these analyses, we obtain information on the location of non-violation facilities from the Dun & Bradstreet Historical Duns Marketing Information (DMI) Files.

Panel B provides summary statistics about the violations in the sample. Violations are somewhat rare, with the average firm having approximately one violation per year and mean penalties of $715,888. Further, the average facility in our sample has approximately 0.17 violations per year, with mean penalties of $133,656. We note that we winsorize our dependent variables at the 99th percentile to mitigate the concern that outliers affect the estimates of the economic magnitudes. Panel C provides an overview of the number of violations and penalties by year. Violations vary over time in terms of both the frequency and the associated penalties. We note that violations peak in 2010 (14.8% of total) and 2006 (21.7% of total). On the other hand, violations are less frequent in earlier years in our sample, and there is also a dip in 2014 (0.4% of total). Overall, these descriptive trends indicate substantial variation over time.

[13] In additional analyses, we show that our results are robust to a five-year event window and to including all post-event years in the treatment.

As noted above, Violation Tracker contains data on a wide range of corporate misconduct. For example, these offense types may relate to workplace safety or health violations, environmental violations, labor relations violations, and securities violations, among others. In Panel D, we describe the common offense types in our sample in terms of the number of violations and total dollar value of penalties. Workplace safety or health violations are the most common, in terms of the number of violations, as they account for 66.1% of total violations. These violations appear relevant in light of the aforementioned examples of negative social media coverage indicating poor working conditions (e.g., Amazon's failure to provide safe COVID working conditions). Environmental violations and railroad safety violations also account for a substantial portion of our sample, representing 8.8% and 7.3% of total violations, respectively. In terms of penalties, False Claims Act, environmental, and securities violations are the largest violation categories representing 30.7%, 26.8%, and 17.4% of total penalties, respectively. Overall, the data show that our sample includes a broad set of violations.

– Insert Table 1 here –

3.1.2 3G Coverage Data

Our second data source relates to the measurement of 3G introduction. We follow Guriev et al. (2021) and obtain digital maps of domestic 3G network coverage from Collins Bartholomew's Mobile Coverage Explorer. Collins Bartholomew has provided map products for nearly 200 years and is the creator and publisher of the Times World Atlas range. The Mobile Coverage Explorer product includes maps that are assembled using data submitted from mobile network operators and covers the period from 2007 to 2017. We translate the maps into useable data by coding whether each zip code has available 3G infrastructure in a given year.

The Mobile Coverage Explorer data only begins in 2007. However, 3G began to rollout in the United States as early as 2002.[14] Thus, for all zip codes that already have 3G as of 2007, as per the Mobile Coverage Explorer, we manually search for news articles to indicate the launch of the first available 3G network in the respective zip code.

Figure 1 illustrates the geographical expansion of 3G within the United States. The figure presents indicators illustrating whether 3G is available in a particular county at three points in time during our sample period, i.e., in 2004 (marked in blue), in 2010 (marked in green), and in 2017 (marked in yellow). The data indicate several interesting trends. First, as of 2004, very few zip codes had 3G available. These zip codes are concentrated in several parts of the New York-New Jersey-Pennsylvania tri-state area as well as the West Coast. By 2007, however, 3G access had expanded substantially, as indicated by the green markers on the map. We note that even by the end of our sample period (2017), many areas still do not have 3G access, as indicated by the portions of the map with no color.

Table 2 provides additional detail on 3G rollout per zip code across our sample period. In this table, we display the number of treated zip codes by year. In total, 3,636 zip codes received access to 3G during our sample period. Access to 3G is well distributed across our sample period, with 2011 being the year with the largest number of treated zip codes (942), representing approximately 26% of all treated zip codes.

– Insert Figure 1 and Table 2 here –

3.1.3 Other Data Sources

[14] 3G began rolling out in the United States in 2002, with Verizon offering services in three areas: a corridor between Norfolk, Virginia to Portland, Maine; the Salt Lake City area; and the San Francisco/Silicon Valley area (CNN, 2002). Consistent with recent economic studies (e.g., Guriev et al., 2021), we focus on 3G rollout as opposed to broadband internet access or earlier advancements in mobile technology (e.g., 2G). We expect 3G internet access to be most relevant for our setting as this is the first mobile technology that provided sufficient bandwidth to use popular social media applications.

In addition to the violations and 3G coverage data, we also collect facility-level, firm-level, and county-level control variables from Dun & Bradstreet DMI files (which include annual establishment information), Compustat, and the Bureau of Labor Statistics (BLS), respectively. From Dun and Bradstreet, we collect the number of employees per facility (*Employees_Facility*) and the total sales per facility (*Sales_Facility*). From Compustat, we collect data on firm's total assets (*Size*), the ratio of liabilities to total equity (*Leverage*), and profitability (*ROA*). From BLS, we collect the total labor force per county (*Labor_Force*) and the unemployment rate (*Unemployment_Rate*). These variables are described in more detail in Appendix A. Our primary sample after requiring non-missing data for variables of interest and controls contains 10,590 facilities and 63,687 facility-year observations.

3.2 Empirical Methodology

Our baseline regression model examines the effect of 3G access on facility-level misconduct using the following generalized DiD framework:

$$Y_{i,j,l,t} = \beta 3G_{l,t} + \text{Controls} + \gamma_i + \delta_{s,t} + \varepsilon_{i,j,l,t}, \quad (1)$$

where *i* indexes a facility, *j* indexes a firm (to which the facility belongs), *l* indicates zip code, *s* indicates state, and *t* indicates year. The dependent variable is either the natural logarithm of one plus the number of violations (*Violations*) or the natural logarithm of one plus the penalty amounts (*Penalties*) in a facility-year. The treatment variable, *3G*, is an indicator variable that takes the value of one for the three years after 3G becomes available in a zip code and zero in the three years prior to 3G access. This implies that treated facilities are included from three years before 3G access to three years after 3G access but are excluded for all other years (i.e., dropped from the sample).

Our generalized DiD methodology allows us to exploit the staggered availability of 3G at the zip-code level across the U.S. over time. The first difference is the change in misconduct as measured in terms of the total penalties or number of violations in each facility prior to and following the rollout of 3G in a zip code. The control group at time *t* consists of all facilities located in areas that do not yet have 3G access. The second difference is the change in misconduct within this control group.[15] Therefore, the effect of 3G access on facility-level misconduct is estimated as the difference in those two differences and is reflected in β in the above regression. If increased social media access reduces misconduct at the facility level, we expect the dollar amount of penalties and the number of violations per facility to decrease following the availability of 3G (i.e., $\beta < 0$).

As noted above, we also control for factors at the facility-, firm-, and county-level that may influence corporate misconduct (*Controls*). At the facility level, we control for total employees and sales, both of which proxy for the size of the facility. At the firm level, we also control for size, leverage, and profitability. At the county level, we control for the size of the labor force and the unemployment rate. These factors account for macro-economic conditions that may also influence 3G rollout. In additional analyses, we also consider a stricter set of time-varying county fixed effects that control for all unobservable macro-economic conditions. All control variables are winsorized at the 1st and 99th percentile. Our DiD specification also includes two sets of fixed effects. Facility fixed effects (γ_i) control for time-invariant heterogeneity across facilities and state-year interactive fixed effects ($\delta_{s,t}$) control for time-varying differences across states.[16] Finally, standard errors are clustered by facility.[17]

Figure 2 provides a graphical summary of our research design using 3G access in three zip codes with Walmart facilities as examples. Two of the Walmart facilities are located in

[15] In additional analyses, we consider the robustness of our results to alternative control group assumptions, following Barrios (2021) and Baker et al. (2022).
[16] As described later, the results are robust to adding industry-year, firm-year, and county-year fixed effects.
[17] As described later, the results are robust to clustering by state as well as by state and year.

Bucks County, Pennsylvania, and the third in Union County, Georgia. While 3G became available in zip code 19030 (Bucks County) in 2007, it only became available in zip codes 18951 (which is also part of Bucks County) and 30512 (Union County) in 2009 and 2012, respectively. Facilities of public companies, such as Walmart, are treated in the year 3G becomes available in their zip code.

– Insert Figure 2 here –

Table 3 provides descriptive statistics on the variables included in our tests. 46.3% of all facility observations are located in an area with 3G access. The average facility employs 574 employees and generates $3.9 million in sales. Facilities in our sample belong to firms that, on average, have $31 billion in assets, return on assets of 4.5%, leverage of 33%, and are located in areas with an average labor force of approximately 458,000 people and an unemployment rate of 6.8%.

– Insert Table 3 here –

4. Main Results

We begin our analyses by examining the effect of 3G access on corporate misconduct. Table 4 provides the results from estimating equation (1). In Columns (1) through (3), we present the results for the natural log of one plus the total dollar value of penalties (*Penalties*). In Columns (4) through (6), we present the results for the natural log of one plus the number of violations (*Number_Violations*). In each set of results (Column (1) and (4)), we first present the results without control variables, only including the baseline fixed effects (facility and state-year fixed effects). We then layer on firm and facility controls in Columns (2) and (4). Finally, in Columns (3) and (6), we further control for county-level factors (i.e., labor force and unemployment).

The results from Table 4 indicate a negative coefficient on *3G* in each specification. In all specifications, the coefficient on *3G* is significant at the 1% level of significance. In terms of economic significance, the effect sizes are also economically important. With respect to *Penalties*, we find that the availability of 3G decreases penalties by approximately 13% in our most stringent specification (Column (3)). For *Number_Violations*, the introduction of 3G reduces violations by 1.8%, based on the results in Column (6). This represents approximately 0.45% of facility-level sales. We note that this estimate likely represents the lower bound on the effect of 3G on misconduct penalties, because it is based only on regulatory penalties and does not capture litigation and reputational costs.[18] In terms of control variables, we find that most controls do not load significantly, with the exception of employees. The coefficient on *Employees_Facility* is positive and significant, suggesting that larger facilities tend to have more violations and incur greater fines.

Overall, these results indicate that 3G access reduces misconduct in facilities. Both the number of detected violations and total penalties decline after 3G is introduced in a locality. These results support the view that social media functions as an effective monitor of firm misconduct.

– Insert Table 4 here –

5. Social Media as Economic Mechanism

Having documented a robust relationship between 3G access and misconduct, we next conduct additional analyses to explore the mechanisms underlying this relationship. These tests include (i) examining how 3G access influences social media activity and (ii) examining the

[18] Prior research has documented substantial additional costs related to the violations included in our sample. For example, workplace safety violations can trigger litigation and wage demands, while also damaging the firm's reputation with investors, customers, and employees (e.g., Caskey and Ozel 2017; Viscusi 2010; Wei 2007).

relationship between social media activity and misconduct. In these analyses, we rely on Twitter as a laboratory for examining social media effects. Twitter offers several key advantages. First, it is one of the most popular social media applications, with more than 330 million monthly users. Importantly, Twitter also makes detailed micro-data data available and provides an application programming interface (API) for analyzing large-scale data. This data features detailed geotags that allow us to track the exact location from which a user Tweets. Finally, Twitter has also been featured in prior accounting studies as a prominent social media information intermediary (e.g., Blankespoor et al., 2014; Bartov et al., 2018).

5.1 The Effects of 3G Access on Twitter Activity

Our first analysis examines the effect of 3G access on Twitter activity. The objective of this test is to help validate the underlying assumption of our study, which is that 3G access facilitates misconduct reduction through increased social media access. While Guriev et al.'s (2021) findings imply this relation in the international setting, it is not clear whether 3G will impact social media in the U.S. market, where home broadband internet is more prevalent. To test this assumption, we examine whether the number of Tweets increases in a zip code after the introduction of 3G in that specific zip code. In this test, the dependent variable is the natural logarithm of one plus the number of Tweets per zip code and year. Consistent with our primary research design, the treatment variable, *3G*, is an indicator variable that takes the value of one for the three years after 3G becomes available in a zip code and zero in the three years prior to 3G access.[19] We include zip code and year fixed effects, making this analysis akin to a generalized DiD, as the unit of observation is at the zip code-year level. The sample for this

[19] We find consistent results when we use one-year or two-year treatment and control windows (untabulated).

analysis spans the years 2010 to 2017 (as the API data is only available as of 2010) and includes more than 1,000 zip codes where 3G became available after 2010.

Table 5, Panel A provides the results from these tests. As shown, we find significant increases of tweets in a zip code after 3G becomes available ($p<0.01$). These findings help validate our assumption that 3G increases social media activity in the U.S.

5.2 The Relationship between Twitter Activity and Misconduct

Next, we examine the association between zip-code-level Twitter activity and misconduct. Our primary analyses offer more causal evidence on social media's effects as we rely on a plausibly exogenous shock to social media activity. The goal of this additional analysis is to establish a more direct link between social media usage and firm misconduct, outside of the 3G setting. Specifically, we test whether zip-code-level Twitter activity is associated with less misconduct. We re-estimate equation (1) but replace *3G* with *High_Twitter_Activity*, an indicator variable that takes the value of one if the number of Tweets per zip code is above the median number across all zip codes and zero otherwise. The sample for this analysis again spans the years 2010 to 2017, given the availability of Twitter API data. The results, provided in Panel B, indicate that increased Twitter activity is indeed associated with reductions in misconduct. In particular, facilities located in zip codes with high Twitter activity have approximately 20% lower penalties and 1.8% fewer violations. These economic magnitudes are consistent with those presented in Table 4. Overall, these results help validate our claim that social media activity can deter firm misconduct.

– Insert Table 5 here –

6. Cross-Sectional Tests based on Firms' Visibility

Our evidence thus far suggests that social media is associated with reductions in misconduct. In this section, we present a set of cross-sectional analyses to assess whether our effects vary with visibility, as this should increase the expected costs of misconduct in the Becker (1968) framework. In the case of social media, more visible firms are expected to respond more to 3G as social media content will be consumed by more viewers across the country, increasing reputational damage or attracting regulatory scrutiny. To capture firm visibility, our cross-sectional tests focus on firm size, firm Twitter following, and firm media coverage. In these tests, we study the interactive effects of our different proxies for firm visibility and 3G access on misconduct using the following regression:

$$Y_{i,j,l,t} = \beta_1 3G_{l,t} \text{ x Firm Visibility}_{j,t} + \beta_2 3G_{l,t} + \text{Controls} + \gamma_i + \delta_{s,t} + \varepsilon_{i,j,l,t}. \qquad (2)$$

If firm visibility increases the effect of 3G on misconduct, we expect a negative loading on β_1. Econometrically, these tests also help address any limitations in our ability to control for time-varying local characteristics in our baseline model. Since these tests rely on cross-sectional variation at the firm level (and our empirical tests are run at the facility level), any meaningful cross-sectional differences cannot be easily explained by variation in local characteristics, as facilities of firms with high and low visibility can be located in the same local areas. We discuss each of these tests in more detail in the following sections.

6.1 Firm Size

We begin by examining whether the effects of 3G on misconduct are more pronounced for facilities that belong to large firms. To implement this test, we test the interactive effects of firm size and 3G access on misconduct using equation (2). In particular, we set *Large_Firm* to one if the facility belongs to a firm with above the median level of total assets, and zero otherwise. The results from this test are provided in Table 6, Panel A. We document a negative and significant coefficient on *3G x Large_Firm* using either *Penalties* ($p<0.10$) or *Number_Violations* ($p<0.05$) as dependent variables. We also find that the coefficient on 3G is negative and statistically significant when *Number_Violations* is the dependent variable and that the F-Test yields significance at the 1% level.

6.2 Firms' Social Media Visibility

In our second test, we examine whether the effects of 3G on misconduct are more pronounced for firms that have large Twitter followings. To implement this test, we first collect data from firm's Twitter webpages containing the current number of followers and use this as a proxy for social media visibility. We then test the interactive effects of social media visibility and 3G access on misconduct using equation (2). In particular, we interact *3G* with *Many_Followers,* while also controlling for each main effect. We set *Many_Followers* to one if the facility belongs to a firm with above the median level of Twitter following, and zero otherwise. Two points are noteworthy for this test. First, *Many_Followers* is time invariant and therefore absorbed by the fixed effects. Second, we set 3G introductions before 2009 to zero as Twitter was founded in 2006 and only reached one million users in 2008.[20]

The results from the Twitter cross-sectional test are provided in Table 6, Panel B. We document a negative and significant coefficient on *3G x Many_Followers* ($p<0.10$) using either *Penalties* or *Number_Violations* as dependent variables. We also find that the coefficient on *3G* is negative and statistically significant when *Number_Violations* is the dependent variable and that the F-Test yields significance at the 1% level.

6.3 Firm Media Coverage

In our final cross-sectional test, we examine whether the effects of 3G on misconduct are more pronounced for firms that have large media coverage. To implement this test, we first obtain data on newspaper articles from Ravenpack. We then test the interactive effects of media

[20] The results are qualitatively similar if we set 3G introductions before 2006 to zero as Twitter did not exist before 2006 (untabulated). Our main results also persist with this alternative definition of 3G introduction (untabulated).

coverage and 3G access on misconduct using equation (2). In particular, we interact *3G* with *High_Coverage*, while also controlling for each main effect. We set *High_Coverage* to one if the facility belongs to a firm with above median level of newspaper articles, and zero otherwise.

The results from the media cross-sectional test are provided in Table 6, Panel C. We document a negative and significant coefficient on *3G x High_Coverage* ($p<0.10$) using either *Penalties* or *Number_Violations* as dependent variables. We also find that the coefficient on *3G* is negative and statistically significant when *Number_Violations* is the dependent variable and that the F-Test yields significance at the 1% level.

Overall, the results in Table 6 are consistent with our expectation that increased social media activity has a stronger effect for facilities belonging to more visible firms, as the costs for engaging in misconduct is higher for these facilities, leading to a more pronounced reduction in misconduct once 3G becomes available.

– Insert Table 6 here –

7. Additional Analyses

In this section, we present a wide set of tests that further examine the robustness of our results. In these tests, we consider different types of corporate misconduct and assess pre-trends and dynamic effects. We also examine alternative fixed effects, treatment windows, sampling procedures, research methodologies, explanations, and clustering choices. We discuss each of these issues in more detail below.

7.1 Financial versus Non-Financial Fraud

A question that arises from our results is whether social media is more effective in monitoring certain types of corporate misconduct. For example, the anecdotes above suggest that social media may be particularly effective in exposing non-financial violations, such as those related to unsafe working conditions or unfair treatment of employees or customers. In contrast, social media may be less effective in exposing financial violations, such as those related to accounting manipulation or overcharging the government and other customers.

We thus repeat our primary tests separately for financial as well as non-financial violations. We define financial violations as those pertaining to accounting fraud, anti-money laundering deficiencies, economic sanction violations, tax fraud, and government contracting fraud, and we classify all other violations as non-financial violations. There are 255 financial violations with total penalties of approximately $4.2 billion in our sample. Non-financial violations account for all remaining violations in our sample.

Table 7 presents the results. Columns (1) and (3) provide the results for *Non_Financial_Penalties* and *Non_Financial_Violations*, and Columns (2) and (4) provide the results for *Financial_Penalties* and *Financial_Violations*. As shown in Columns (1) and (3) of Table 6, the coefficient on the treatment variable continues to load negatively and significantly for both *Non_Financial_Penalties* (Column (1)) and *Non_Financial_Violations* (Column (3)). In contrast, the coefficient on the treatment variable is statistically insignificant for both *Financial_Penalties* (Column (2)) and *Financial_Violations* (Column (4)). We also find that the coefficients on *3G* differ significantly across Columns (1) and (2) as well as Columns (3) and (4). We find that the effect of 3G access on corporate misconduct is concentrated in non-financial violations, suggesting that social media is particularly effective in curtailing non-financial fraud. This result helps validate our claim that social media reduces misconduct, as the effects we document are concentrated in violations most observable to social media users.

– Insert Table 7 here –

7.2 Timing and Dynamic Effects

We next examine how the effect of 3G availability on facility-level misconduct evolves in the years surrounding 3G access. Doing so allows us to test for pre-trends and also examine how the effect manifests in each year following the event. Specifically, we re-estimate equation (1) but decompose the main effect into single-year treatment windows that range from one to three years prior to 3G access to one to two years after 3G access, using the third year before 3G access as the baseline.

Table 8 presents the results from our dynamic effects analysis. Column (1) presents the results for *Penalties*, and Column (2) presents the results for *Number_Violations*. We find that the coefficients on $3G_{t-2}$ and $3G_{t-1}$ are statistically insignificant, indicating that treated and control facilities are indistinguishable from each other before 3G access. This result reduces concerns related to pre-trends, further mitigating the concern of correlated omitted variables driving 3G access and facility-level misconduct. We also find a negative and significant coefficient on $3G_t$ to $3G_{t+2}$, indicating that the effect of 3G access on facility-level misconduct occurs after such access and persists for at least three years.

– Insert Table 8 here –

7.3 Alternative Shock

In our main tests, we use the local adoption of 3G as a shock to social media activity. One concern with this shock is that it may also improve access to the internet more broadly (and not just access to social media). To alleviate this concern, we follow recent studies (Fujiwara et al. 2021; Müller and Schwarz 2020) and use the increase in the number of Twitter users following the 2007 SXSW festival as an alternative shock for an increase in social media activity. The SXSW festival was a key event in Twitter's rise to popularity and also contributed to Twitter's geographical diffusion. In particular, counties with more SXSW followers who joined Twitter during the 2007 festival saw disproportionately higher growth of Twitter adoption compared to counties with SXSW followers who already joined Twitter before the festival, leading to a persistent difference in Twitter use (Fujiwara et al., 2021; Müller and Schwarz, 2020).

For these tests, we limit our sample to facilities located in counties whose population exhibit interest in the SXSW festival (and its Twitter account) as these counties may be systematically different from other counties (see Fujiwara et al., 2021; Müller and Schwarz, 2020). Thus, our empirical design exploits variation in the timing of when Twitter users interested in SXSW joined Twitter across counties. In particular, the treatment group consists of facilities located in counties with interest in SXSW that experienced an increase in the number of Twitter users following the festival (but not before the festival) and the control group consists of facilities located in counties with interest in SXSW that did not experience an increase in Twitter users following the festival.

In these tests, we include year and firm fixed effects as the inclusion of facility fixed effects would absorb the time-invariant SXSW treatment variable. As shown in Table 9, Panel A, we find consistent results using this alternative shock to social media activity. In particular, facilities located in counties with an increase in Twitter activity following the SXSW festival have approximately 5.5% lower penalties and 0.3% fewer violations. While these magnitudes are smaller in magnitude compared to the results tabulated in Table 4, it is important to note this sample contains all post-event years. As discussed in the following section, the magnitude from our main 3G tests becomes comparable when we use all years.

7.4 Alternative Fixed Effects

Our baseline analyses include facility and state-year fixed effects and a rich set of control variables. To further alleviate the concern that unobservable factors explain our results, we consider three alternative fixed-effects structures. First, we replace state-year fixed effects with county-year fixed effects, exploiting the fact that 3G access is captured at the zip-code level as opposed to the county-level. We note that this test results in a significant loss of sample (~21% of observations), as many counties only have one facility and this fixed effect becomes subsumed by the treatment (which varies at the facility-year level). Second, we add industry-year fixed effects to assess whether our results are driven by industry trends. Finally, we add firm-year fixed effects to assess whether our results are driven by firm-wide changes, such as new policies that might reduce corporate misconduct.

Table 9, Panel B provides the results from our alternative fixed-effects structure analyses. Columns (1) to (3) provide the results for *Penalties*, and Columns (4) to (6) provide the results for *Number_Violations*. As shown in Columns (1) and (4), our results are robust to the inclusion of county-year fixed effects. The treatment variable continues to load negatively and maintains significance for both *Penalties* (Column (1)) and *Number_Violations* (Column (3)). In Columns (2) and (5), we add industry-year fixed effects to rule out the possibility that unobservable changes at the industry level affecting facility-level misconduct drive our results. Our inferences continue to hold. In Columns (3) and (6), we add firm-year fixed effects to rule out the possibility that unobservable changes at the firm level drive our results. Our inferences continue to hold. Overall, the results from our alternative fixed effects analysis provide further evidence that our findings do not appear to be influenced by unobservable local-, firm-, or industry-level heterogeneity.

7.5 Alternative Treatments

In our main tests, we focus on a six-year window around the availability of 3G access. The advantage of this narrower window is that it is more likely to capture changes in facility-level misconduct that are driven by 3G access. However, we also re-examine our main tests using longer windows, including either a ten-year centered window around 3G introduction or all years before and after 3G access.

Table 9, Panel C provides the results for our alternative treatments. We present the results for *Penalties* in Columns (1) and (2) and the results for *Number_Violations* in Columns (3) and (4). As shown in Table 9, Panel C, we find consistent results using these longer windows. In terms of economic magnitude, the ten-year centered window generates similar effect sizes as our baseline analyses, with 3G decreasing penalties by roughly 8.5% (Column (1)) and violations by approximately 1.36% (Column (3)). We note that the magnitudes are more modest when we consider all post-event years. In Columns (2) and (4), 3G access is associated with a 4.6% reduction in penalties and a 0.8% reduction in violations. These smaller effect sizes suggest that the effects of 3G access on corporate misconduct diminish over time.

7.6 Alternative Research Designs

A potential concern with our primary research design is that our estimated effects could be biased due to the observations that form the control group. In our context, 3G rollout was staggered, creating the concern that, for late treatments, the control group consists not only of "not-yet-treated" zip codes but also zip codes that had 3G rollout in earlier periods.[21] To alleviate this concern, we follow Baker et al. (2022) and Barrios (2021) and adjust for the use of prior treated units as effective comparison units by running stacked regressions. In particular, we create a separate dataset for each 3G rollout year and only use not-yet-treated facilities as controls. Consistent with our primary research design, we use a 6-year estimation window around 3G access and then stack these datasets to calculate average treatment effects across the events. As shown in Table 9, Panel D, our results hold using this alternative estimation technique. In addition, the economic magnitudes are similar to those reported in Table 4.

[21] It is important to note that this concern is not particularly severe in our setting as we only include facility-year observations in a six-year window around the treatment in our analyses, reducing the possible impact of already-treated observations.

An additional, related concern in our setting is that there could be an anticipatory effect, i.e., once nearby zip codes have 3G access, firms might expect 3G to also rollout in their zip code. This anticipation effect could violate the Stable Unit Treatment Value Assumption (SUTVA). To further alleviate the concern that our results are driven by a SUTVA violation, we follow Bellemare and Nguyen (2018) and include either the natural logarithm of the number of zip codes within 50 km that already have 3G access or the proportion of zip codes within 50 km that already have 3G access as additional controls. As shown in Table 9, Panel D, Columns (2)-(3) and Columns (5)-(6), our results persist.

7.7 Alternative Samples

In our main analyses, we make three sample selection choices. First, we allocate violations with ambiguous or unavailable location information to a firm's headquarters location. Second, we run our tests using facilities with violations but do not include facilities without violations. Third, we exclude private firms, as they are not subject to the same regulations as public firms and have fewer violations. We next conduct robustness tests to examine the sensitivity of our main results to these three choices.

First, we modify our assumption regarding the assignment of ambiguous locations by excluding all violations with ambiguous or unavailable location information from the sample, rather than assigning them to the firm's headquarters location. Doing so reduces the number of violations to 9,116 and total penalties to approximately $2 billion. As shown in Table 9, Panel E, Columns (1) and (4), the results continue to hold using this alternative sample.

Second, we also alter our design choice related to only focusing on firms with violations. To do so, we collect data from Dun & Bradstreet that allow us to identify 24,248 non-violation facilities that report sales at least once during our sample period. We re-estimate our primary model using the sample of violation and non-violation facilities, resulting in a much larger panel of 216,923 observations. Note that the average non-violation facility has sales of approximately $279,000, which is more than ten times smaller than the average facility in the violation sample (see Table 3). This validates our earlier argument that violation and non-violation facilities are inherently different. We repeat our main analyses using penalties (Column (2)) and the number of violations (Column (5)) as the dependent variables. As shown in Table 9, Panel E, Columns (2) and (5), we find a negative and significant coefficient on *3G* in both models. In terms of economic magnitude, the results indicate that 3G access decreases the dollar penalties and the number of violations in treated facilities by approximately 5.1% and 0.8%, similar to those reported in Table 4.

Third, we also include private firms with violations in our sample. To do so, we obtain data on private firms' violations from Violation Tracker. We identify 73,837 unique private firm facilities with at least one violation. Adding those observations to our primary sample increases the sample to 521,191 facility-year observations. We repeat our main analyses using penalties (Column (3)) and the number of violations (Column (6)) as the dependent variables.

As shown in Table 9, Panel E, Columns (3) and (6), we find a negative and significant coefficient on *3G* in both models. In terms of economic magnitude, the results indicate that 3G access decreases the dollar penalties and the number of violations in treated facilities by approximately 2.2% and 0.3%. These effects are smaller than those reported in Table 4, as private firms have fewer violations. In particular, while the average facility of a public firm in our sample has a violation approximately once every five years, the average facility of a private firm in our sample has a violation approximately once every twenty years. Independent of our sampling choices, we find that 3G reduces corporate misconduct.

7.8 IT Investments

One plausible alternative explanation for our findings is that 3G access may motivate increased levels of IT investments. Facilities located in areas with mobile broadband internet may invest in new technologies that help monitor and reduce misconduct. We note that this explanation does not easily explain the relation between Twitter activity and misconduct. Nevertheless, we conduct an additional analysis to address this explanation. Specifically, we collect data on facility-level IT investments from Aberdeen's Computer Intelligence Technology Database (CiTDB) and re-estimate our baseline regression where the dependent variable is *IT_Investments*, the natural logarithm of a facility's annual IT budget. We note that the sample for these tests is smaller as Aberdeen only provides information on IT budgets starting in 2010 and does not cover all facilities in our sample. Table 9, Panel F provides the results from this analysis. We do not find evidence that *3G* loads significantly, thus rendering this explanation less plausible.

7.9 Alternative Clustering

In our primary tests, we cluster the standard errors by facility. We also rerun our main tests clustering by state, state and year, or zip code. As shown in Table 9, Panel G, we find consistent results using these alternative clustering approaches.

– Insert Table 9 here –

7.10 Firm-Level Analysis

We also examine the effect of 3G access on misconduct at the firm level. As a larger number of a firm's facilities are exposed to 3G over time, overall firm misconduct could decrease. To examine this, we use *3G_Exposure*, which captures the fraction of a firm's facilities located in areas with 3G access over time, as our treatment variable and aggregate total penalties and number of violations at the firm level (denoted *Penalties_Firm* and *Number_Violations_Firm*). As shown in Table 10, the coefficient on *3G_Exposure* is negative and significant when either penalties or number of violations are the dependent variables. These results demonstrate that overall firm misconduct decreases as a larger fraction of facilities are exposed to 3G.

– Insert Table 10 here –

7.11 Intra-Firm Spillover Effects

A related question is whether 3G access results in intra-firm spillover effects. Intra-firm spillover effects may occur if facilities adopt new compliance technologies in response to 3G that are then subsequently adopted more broadly by the firm. To examine this question, we assess whether there are significant changes in corporate misconduct at other facilities of the same firm in the three years after either more than 10% or more than 50% of a firm's facilities are located in areas with 3G access compared to the three years prior. In untabulated tests, we do not find a statistically significant change in misconduct using either type of treatment. These results suggest no evidence of intra-firm spillover effects in misconduct in response to 3G access. It is possible that neighboring facilities (or headquarters) face frictions in learning about how a local facility combats misconduct or do not face strong enough incentives to respond to such technology across the firm.

8. Conclusion

This paper examines the effect of social media on firm misconduct through multiple empirical strategies. Our primary analyses exploit the staggered introduction of 3G mobile broadband access across the United States. Mobile broadband access, and 3G internet in particular, is a key driver of growth in the use of social media applications. Our results indicate that facilities reduce violations by 1.8% and penalties by 13% in the three-year period following the introduction of 3G. The effects are more pronounced for visible firms that face higher expected costs associated with misconduct. Using Twitter as a setting, we complement the 3G identification strategy with a wide set of analyses validating 3G as an instrument for social media use, employing an alternative shock, and demonstrating that social media activity is an underlying mechanism for our findings. Overall, our findings suggest that social media is an effective monitor of corporate misconduct, providing some of the first evidence on this role of social media.

We acknowledge several caveats to our findings and also offer some suggestions for future research. While our study documents a novel monitoring role of social media, much of our causal evidence is indirect, relying on instruments for social media activity. Future research can consider alternative methodologies, including field experiments, to further assess how firms respond to specific types of social media activity, particularly “viral” events. Our study also does not speak to the effects of alternative technologies, including surveillance technologies adopted by firms or other forms of local technology (e.g., home broadband internet, 4G, 5G). We suspect that the continued expansion of mobile broadband technology coupled with growth in social media platforms will continue to deter misconduct. Finally, future research can further examine the managerial implications of increased social media monitoring, including the role of organizational structure and centralization in prompting firm-wide improvements in compliance.

References

Amarasinghe, A., 2010. What Motivates People to Participate in Social Media? *Social Media Today* https://www.socialmediatoday.com/content/what-motivates-people-participate-social-media (accessed June 7, 2021).

Atkinson, C., 2019. Fake news can cause 'irreversible damage' to companies – and sink their stock price. https://www.nbcnews.com/business/business-news/fake-news-can-cause-irreversible-damage-companies-sink-their-stock-n995436 (accessed March 23, 2022).

Baker, A., D. Larcker, and C. Wang, 2022. How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics* 144(2): 370–395.

Baloria, V. P., and J. Heese. 2018. The effects of media slant on firm behavior. *Journal of Financial Economics* 129(1): 184–202.

Barrios, J., 2021. Staggeringly problematic: A primer on staggered DiD for accounting researchers. Working paper.

Bartov, E., L. Faurel, and P. S. Mohanram, 2018. Can Twitter help predict firm-level earnings and stock returns? *The Accounting Review* 93(3): 25–57.

Becker, G., 1968. Crime and punishment: An economic approach. *Journal of Political Economy* 76(2): 169–217.

Bellemare, M. F., and N. Nguyen, 2018. Farmers markets and food-borne illness. *American Journal of Agricultural Economics* 100(3): 676–690.

Berger, P. G., and H. Lee, 2022. Did the Dodd-Frank whistleblower provision deter accounting fraud? *Journal of Accounting Research* 60(4): 1337–1378.

Blankespoor, E., Miller, G. S., and H. D. White, 2014. The Role of Dissemination in Market Liquidity: Evidence from Firms' Use of Twitter™. *The Accounting Review* 89: 79–112.

Blankespoor, E., deHaan, E. and I. Marinovic, 2020. Disclosure processing costs, investors' information choice, and equity market outcomes: A review. *Journal of Accounting and Economics* 70 (2-3).

Bourveau, T., Coulomb, R., and M. Sangnier, 2021. Political connections and white-collar crime: Evidence from insider trading in France. *Journal of the European Economic Association* 19(5): 2543–2576.

Bunz, M., 2010. Most journalists use social media such as Twitter and Facebook as a source. The Guardian. https://www.theguardian.com/media/pda/2010/feb/15/journalists-social-music-twitter-facebook (accessed September 13, 2021).

Cao, S. S., Fang, V. W., and L. G. Lei, 2021. Negative peer disclosure. *Journal of Financial Economics* 140(3): 815–837.

Carman, T., and E. Heil, 2021. Why restaurant workers are demanding better wages and working conditions. *Washington Post*. https://www.washingtonpost.com/food/2021/05/28/restaurant-workers-demands/ (accessed March 11, 2022).

Caskey, J., and N. B. Ozel. 2017. Earnings expectations and employee safety. *Journal of Accounting and Economics* 63 (1): 121–141.

Chaudhari, A., and E. Purkayastha, 2011. Greenpeace, Nestlé and the palm oil controversy: Social media driving change? IBS Center for Management Research. https://www.washingtonpost.com/food/2021/05/28/restaurant-workers-demands/ (accessed March 11, 2022).

CNN, 2002. Verizon launches first U.S. '3G' network. https://www.cnn.com/2002/TECH/ptech/01/28/verizon.3g/#:~:text=CNN.com%20%2D%20Verizon%20launches%20first,'%20network%20%2D%20January%2028%2C%202002&text=U.S.,-WEATHER (accessed April 1, 2021).

Core, J. E., Guay, W., and D. F. Larcker, 2008. The power of the pen and executive compensation. *Journal of Financial Economics* 88: 1–25.

Correia, M., 2014, Political connections and SEC enforcement. *Journal of Accounting and Economics* 57(2-3): 241–262.

Curtis, A., Richardson, V. J., and R. Schmardebeck, 2016. Social Media Attention and the Pricing of Earnings *News. Handbook of Sentiment Analysis in Finance*.

Dey, A., Heese, J. and G. Pérez-Cavazos, 2021. Cash-for-information whistleblower programs: Effects on whistleblowing and consequences for whistleblowers. *Journal of Accounting Research* 59(5): 1689–1740.

Donati, D., 2019. Mobile Internet access and political outcomes: Evidence from South Africa. Working paper, Universitat Pompeu Fabra.

Drake, M. S., Thornock, J. R., and B. J. Twedt, 2017. The Internet as an information intermediary. *Review of Accounting Studies* 22: 543–576.

Dube, S., and C. Zhu, 2021. The disciplinary effect of social media: Evidence from firms' responses to Glassdoor reviews. *Journal of Accounting Research* 59(5): 1783–1825.

Duro, M., Heese, J., and G. Ormazabal, 2019. The effect of enforcement transparency: Evidence from SEC comment-letter reviews. *Review of Accounting Studies* 24(3): 780–823.

Dyck, A., Volchkova, N., and L. Zingales, 2008. The corporate governance role of the media: evidence from Russia. *Journal of Finance* 63: 1093–1135.

Dyck, A., Morse, A., and L. Zingales, 2010. Who blows the whistle on corporate fraud? *Journal of Finance* 65: 2213–2253.

Enikolopov, R., Petrova, M. and K. Sonin, 2018. Social media and corruption, American Economic Journal: Applied Microeconomics 10: 150–174.

Enikolopov, R., Makarin, A. and M. Petrova, 2020. Social media and protest participation: Evidence from Russia. *Econometrica* 88: 1479–1514.

Fergusson, L., and C. Molina, 2019. Facebook causes protests. Working paper, Universidad de los Andes.

Fujiwara, T., Müller, K., and C. Schwarz, 2021. The effect of social media on elections: Evidence from the United States. Working paper.

Gomez, E., Heflin, F., Moon, J. and J. Warren, 2018. Crowdsourced financial analysis and information asymmetry at earnings announcements. *Unpublished Working Paper*.

Guriev, S., Melnikov, N., and E. Zhuravskaya, 2021. 3G internet and confidence in government. *Quarterly Journal of Economics* 1236(4): 2533–2613.

Gurun, U. G., and A. W. Butler, 2012. Don’t believe the hype: Local media slant, local advertising, and firm value. *Journal of Finance* 67: 561–598.

Heese, J., 2019. The political influence of voters' interests on SEC enforcement. *Contemporary Accounting Research* 36(2): 869–903.

Heese, J., 2022. Does industry employment of active regulators weaken oversight? *Management Science*, forthcoming.

Heese, J., and G. Pérez-Cavazos, 2019. Fraud allegations and government contracting. *Journal of Accounting Research* 57(3): 675–719.

Heese, J., and G. Pérez-Cavazos, 2021. The effect of retaliation costs on employee whistleblowing. *Journal of Accounting and Economics* 71(2-3).

Heese, J., Krishnan, R., and H. Ramasubramanian, 2021. The Department of Justice as a gatekeeper in whistleblower-initiated corporate fraud enforcement: Drivers and consequences." *Journal of Accounting and Economics* 71(1).

Heese, J., Pérez-Cavazos, G., and C. D. Peter, 2022. When the local newspaper leaves town: The effects of local newspaper closures on corporate misconduct. *Journal of Financial Economics* 145(2): 445–463.

Jame, R., Johnston, R., Markov, S. and M. C. Wolfe, 2016. The value of crowdsourced earnings forecasts. *Journal of Accounting Research* 54: 1077–1110.

Jennings, R., 2020. This week in TikTok: When Walmart and Amazon employees go viral. *Vox* https://www.vox.com/the-goods/2020/12/15/22174581/walmart-amazon-tiktok-employee-worker-influencer (accessed April 1, 2021).

Kemp, S., 2018. "Digital in 2018: Essential Insights into Internet, Social media, Mobile and Ecommerce Use around the World," Technical report, We Are Social and Hootsuite, https://wearesocial.com/blog/2018/01/global-digital-report-2018 (accessed June 7, 2021).

Kottasova, I., 2017. United loses $250 million of its market value. CNN. https://money.cnn.com/2017/04/11/investing/united-airlines-stock-passenger-flight-video/ (accessed March 11, 2022).

Lee, L. F., Hutton, A. P. and S. Shu, 2015. The role of social media in the capital market: Evidence from consumer product recalls. *Journal of Accounting Research* 53: 367–404.

Maiaroto, T., 2013. The average lifespan of viral content. https://www.socialmediatoday.com/content/average-life-span-viral-web-content (accessed April 1, 2021).

Meinch, T., 2021. Shame and the Rise of the Social Media Outrage Machine https://www.discovermagazine.com/the-sciences/shame-and-the-rise-of-the-social-media-outrage-machine (accessed April 1, 2021).

Miller, G. S., 2006. The press as a watchdog for accounting fraud. *Journal of Accounting Research* 44(5): 1001–1033.

Miller, G. S., and D. J. Skinner, 2015. The evolving disclosure landscape: How changes in technology, the media and capital markets are affecting disclosure. *Journal of Accounting Research* 53(2): 221–239.

Mui, C., 2011. The 5 Most Brand-Damaging Viral Videos of 2011. *Forbes* https://www.forbes.com/sites/chunkamui/2011/12/28/the-5-most-brand-damaging-viral-videos-of-2011/?sh=22e17753b1fe (accessed May 29, 2021)

Mullainathan, S. and A. Shleifer, 2005. The market for news. *American Economic Review* 95: 1031–1053.

Müller, K., and C. Schwarz, 2020. From hashtag to hate crime: Twitter and anti-minority sentiment. Working paper.

Ostrower, J., 2017. United CEO apologizes for 'truly horrific' passenger incident. CNN. https://money.cnn.com/2017/04/11/news/companies/united-munoz-apology/index.html?iid=EL (accessed March 11, 2022).

Pew Research Center, 2019. Newspapers fact sheet. Available at: https://www.journalism.org/fact-sheet/newspapers/

Rainie, L., and B. Wellman, 2012. Networked – The new social operating system. Cambridge, MA: The MIT Press, 2012.

Shapira, R., and L. Zingales, 2017. Is pollution value-maximizing? The DuPont case. *Unpublished working paper*.

Stäbler, S., and M. Fischer, 2020. When does corporate social irresponsibility become news? Evidence from more than 1,000 brand transgressions across five countries. *Journal of Marketing* 84(3): 46–67.

Tau, B., 2021. Law enforcement's use of commercial phone data stirs surveillance fight. The *Wall Street Journal*. https://www.wsj.com/articles/law-enforcements-use-of-commercial-phone-data-stirs-surveillance-fight-11631707201?mod=hp_lead_pos10 (accessed October 6, 2021).

Viscusi, W. 2010. The heterogeneity of the value of statistical life: Introduction and overview. *Journal of Risk Uncertainty* 40 (1): 1–13.

Wei, X. 2007. Wage compensation for job-related illness: Evidence from a matched employer and employee survey in the UK. *Journal of Risk Uncertainty* 34 (1): 85–98.

Winston, A., 2017. Pepsi, United, and the Speed of Corporate Shame. https://hbr.org/2017/04/pepsi-united-and-the-speed-of-corporate-shame (accessed April 1, 2021).

Zhuravskaya, E., Petrova, M., and R. Enikolopov, 2020. Political Effects of the Internet and Social Media. *Annual Review of Economics* 12: 415–438.

Appendix A. Variable Definitions

The following variables are constructed using data from Violation Tracker's dataset of corporate misconduct [VT], data on facilities from Dun and Bradstreet DMI files [D&B], Compustat [C], data on county characteristics from the Bureau of Labor Statistics [BLS], data on 3G introduction per zip code from Collins Bartholomew's Mobile Coverage Explorer and newspaper articles [CB], data on Twitter following from Twitter [TWTR], Tweet data from Twitter's API database [API], data on newspaper articles from Ravenpack [RP], data on Twitter users around the 2007 SXSW festival from Müller and Schwarz (2020) [MS], and data on facilities' IT investments from Aberdeen's Computer Intelligence Technology Database [CiTDB].

A. Variables of Interest

Penalties	The natural logarithm of one plus total penalties for misconduct per facility and year winsorized at the 99th percentile. [VT]
Penalties_Firm	The natural logarithm of one plus total penalties for misconduct per firm and year winsorized at the 99th percentile. [VT]
Number_Violations	The natural logarithm of one plus the number of violations per facility and year winsorized at the 99th percentile. [VT]
Number_Violations_Firm	The natural logarithm of one plus the number of violations per firm and year winsorized at the 99th percentile. [VT]
Number_Tweets	The natural logarithm of one plus the number of Tweets per zip code and year. [API]
3G	Indicator variable that is set to 1 in the three years following introduction of 3G in a zip code and 0 in the three years prior to the introduction of 3G. [CB]
SXSW	Indicator variable that is set to 1 for facilities located in counties that experienced an increase in Twitter users following the SXSW festival (but no increase before the festival), and to 0 for facilities located in counties that experienced an increase in Twitter users before the SXSW festival. [MS]
3G_Exposure	The fraction of a firm's facilities located in areas with 3G access per year. [CB]
High_Twitter_Activity	Indicator variable that is set to 1 if the number of Tweets per zip code is larger than the median number of Tweets across all zip codes and 0 otherwise. We obtain data on Tweets from Twitter's API database. [API]
Large_Firm	Indicator variable that is set to 1 if the facility is part of a firm with assets larger than the median and 0 otherwise. [C]
Many_Followers	Indicator variable that is set to 1 if the facility is part of a firm with Twitter followers larger than the median number of Twitter followers and 0 otherwise. [TWTR]
High_Coverage	Indicator variable that is set to 1 if the facility is part of a firm with above median coverage in the number of newspaper articles per year and 0 otherwise. [RP]
IT_Investments	The natural logarithm of one plus a facility's annual IT budget. This data is available as of 2010. [CiTDB]
B. Controls	
Employees_Facility	The natural logarithm of one plus the number of employees per facility. [D&B]
Sales_Facility	The natural logarithm of one plus sales per facility (in thousands of dollars). [D&B]

Size	The natural logarithm of one the firm's asset size (in millions of dollars) at the beginning of the year. [C]
Leverage	The ratio of total liabilities to total equity. [C]
ROA	Net income scaled by total assets. [C]
Labor_Force	The natural logarithm of the labor force per county. [BLS]
Unemployment_Rate	The unemployment rate per county. [BLS]
Number_Nearby_Treated_Zip_Codes	The natural logarithm of one plus the number of zip codes within a 50 km radius with 3G access. [CB]
Proportion_Nearby_Treated_Zip_Codes	The proportion of the number of zip codes within a 50 km radius with 3G access. [CB]

Figure 1. Map of 3G Introduction in the United States

This map shows the geographic distribution of 3G introduction across the United States during the period 2000-2017. The blue, green, and yellow grids indicate counties in which 3G was available as of 2004, 2010, and 2017, respectively.

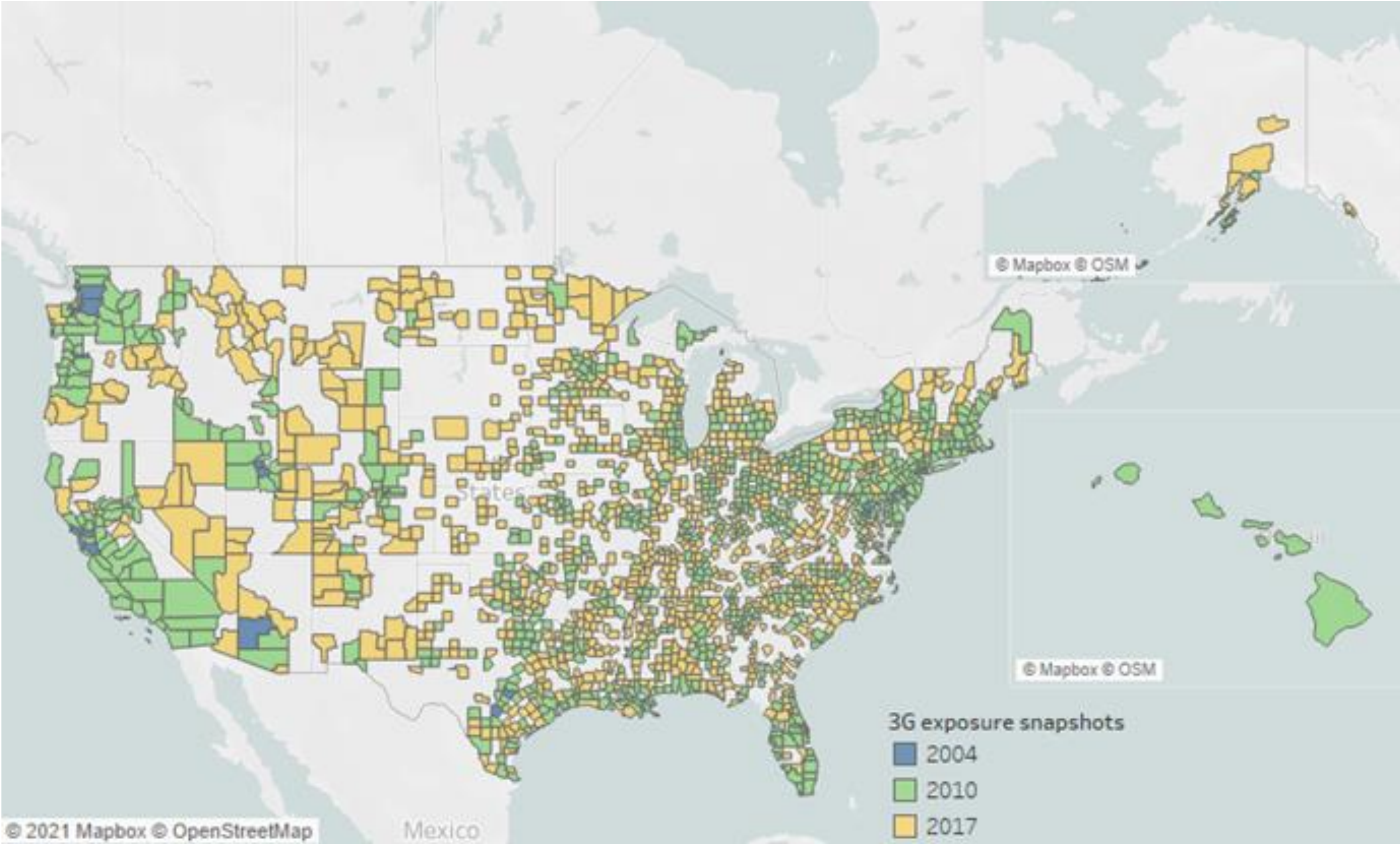

Figure 2. Research Design

This figure provides an example from our sample to better illustrate how we code *3G* for our analyses. Consider three facilities of Walmart located in different zip codes. In 2007 (therefore 2007 is the first treatment year), 3G became available in zip code 19030, which is part of Bucks County, Pennsylvania, treating the Walmart facility located in that zip code. We use a 6-year window around the treatment date, meaning treated facilities are included from three years before the treatment to three years after the treatment. Other facilities of the same firm or some other firm located elsewhere form the control group. For example, another Walmart facility, which is also located in Bucks County, is only treated in 2009 when 3G became available in zip code 18951. 3G access occurs at different points in time for our sample firms, affecting the time series of *3G*. Each 0/1 coded cell (emphasized in bold) represents a facility-year observation included in our analysis.

Facility	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
Facility of Walmart located in zip code 19030, Bucks County, Pennsylvania	**0**	**0**	**0**	**1**	**1**	**1**					
Facility of Walmart located in zip code 18951, Bucks County, Pennsylvania			**0**	**0**	**0**	**1**	**1**	**1**			
Facility of Walmart located in zip code 30512, Union County, Georgia						**0**	**0**	**0**	**1**	**1**	**1**

Table 1. Sample

Panel A. Sample Composition

This table presents the sample composition for the period 2000-2017.

	Number of Violations (1)	Number of Firms (2)	Number of Facilities (3)
Violation Tracker sample	67,000	2,875	
Less: Private companies	(23,637)	(1,362)	
Less: Financial industry	(5,231)	(130)	
Less: Missing control variables	(9,261)	(23)	
Less: Outside treatment window	(17,363)	(0)	
Final sample	11,508	1,360	10,590

Panel B. Summary Statistics Violations by Facilities and Firms

This table presents the summary statistics on the number of violations and penalties by facilities and firms for the period 2000-2017.

	Facility-Years Sample (N=63,687)										
	Mean	Std.	Min.	5th	10th	25th	Median	75th	90th	95th	Max.
Number of Violations	0.172	1.762	0	0	0	0	0	0	0	1	121
Penalties (in $)	133,656	6,222,901	0	0	0	0	0	0	0	10,675	700,000,000
	Firm-Years (N=7,566)										
	Mean	Std.	Min.	5th	10th	25th	Median	75th	90th	95th	Max.
Number of Violations	1.001	8.432	0	0	0	0	0	1	2	3	358
Penalties (in $)	715,888	15,619,820	0	0	0	0	0	5,000	48,381	190,000	900,000,000

Panel C. Sample Composition by Year

This table presents the distribution of violations and penalties in our sample for the period 2000-2017 by year.

Year	Number of Violations	% of Total	Penalties ($m)	% of Total
2000	55	0.5%	56.9	0.7%
2001	47	0.4%	5.8	0.1%
2002	188	1.6%	135.1	1.6%
2003	174	1.5%	461.7	5.4%
2004	401	3.5%	1,464.3	17.2%
2005	533	4.6%	1,051.0	12.3%
2006	738	6.4%	1,851.1	21.7%
2007	1,093	9.5%	513.1	6.0%
2008	1,227	10.7%	178.7	2.1%
2009	1,610	14.0%	511.5	6.0%
2010	1,698	14.8%	1,153.9	13.6%
2011	1,548	13.5%	649.0	7.6%
2012	986	8.6%	38.3	0.4%
2013	576	5.0%	220.9	2.6%
2014	278	2.4%	34.8	0.4%
2015	126	1.1%	81.1	1.0%
2016	142	1.2%	102.9	1.2%
2017	88	0.8%	1.8	0.0%
Total	11,508	100%	8,511.9	100%

Panel D. Sample Composition by Offense Type

This table presents the sample composition for the period 2000-2017 by offense type.

Offense Type	Number of Violations	% of Total	Penalties ($m)	% of Total
Workplace safety or health violation	7,608	66.1%	336.0	3.9%
Environmental violation	1,009	8.8%	2,280.0	26.8%
Railroad safety violation	839	7.3%	8.2	0.1%
Wage and hour violation	639	5.6%	189.0	2.2%
Labor relations violation	504	4.4%	159.0	1.9%
Aviation safety violation	273	2.4%	17.7	0.2%
Motor vehicle safety violation	143	1.2%	3.2	0.0%
Employment discrimination	74	0.6%	338.0	4.0%
Family and Medical Leave Act violation	44	0.4%	0.6	0.0%
Securities violation	37	0.3%	1,478.5	17.4%
False Claims Act violation	32	0.3%	2,610.0	30.7%
Other	306	2.7%	1,091.7	12.8%
Total	11,508	100%	8,511.9	100%

Table 2. Summary Statistics 3G Introduction

This table presents the distribution of 3G mobile broadband penetration in our sample for the period 2000-2017 by year and zip code.

Year	Number of Treated Zip Codes	% of Total
2000	-	-
2001	-	-
2002	111	3.1%
2003	25	0.7%
2004	24	0.7%
2005	193	5.3%
2006	22	0.6%
2007	400	11.0%
2008	594	16.3%
2009	760	20.9%
2010	278	7.6%
2011	942	25.9%
2012	200	5.5%
2013	1	0.0%
2014	64	1.8%
2015	21	0.6%
2016	1	0.0%
2017	-	-
Total	3,636	100%

Table 3. Summary Statistics Facilities

This table reports the summary statistics, on an annual basis, of the variables used in our analyses. All variables are defined in Appendix A.

	Facility-Years Sample (N = 63,687)				
Variable	Mean	Std.	Min.	Median	Max.
3G	0.463	0.499	0	0	1
Employees_Facility	574	1,811	1	150	58,507
Sales_Facility (in thousands)	3,867	17,037	0.11	64.5	273,005
Size (in millions)	30,589	75,174	146	6,962	552,257
Leverage	0.327	0.436	0	0.243	2.715
ROA	0.045	0.069	–0.238	0.049	0.213
Labor_Force	458,143	854,393	3,111	172,437	4,914,702
Unemployment_Rate (in %)	6.79	2.62	2.50	6.10	13.80

Table 4. 3G Introduction and Facility-Level Misconduct

This table reports the estimation results from linear regressions of the following form:

$$Y_{i,j,l,t} = \alpha_0 + \alpha_1\, 3G_{l,t} + \phi\ Controls + \gamma_i + \delta_{s,t} + \varepsilon_{i,j,l,t}$$

Y is either the natural logarithm of one plus the dollar amount of penalties per facility and year (Columns 1-3) or the natural logarithm of one plus the number of violations per facility and year (Columns 4-6). Columns 1 and 4 report results without *Controls*. Columns 2 and 5 report results with facility-level and firm-level *Controls*. Columns 3 and 6 report results with facility-level, firm-level, and county-level *Controls*. Our main explanatory variable is *3G*, which takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Penalties			Number_Violations		
Variables	Pred.	(1)	(2)	(3)	(4)	(5)	(6)
3G	–	–0.1291***	–0.1298***	–0.1298***	–0.0176***	–0.0177***	–0.0178***
		(0.0477)	(0.0476)	(0.0476)	(0.0056)	(0.0056)	(0.0056)
Employees_Facility			0.0766***	0.0768***		0.0072***	0.0073***
			(0.0125)	(0.0125)		(0.0015)	(0.0015)
Sales_Facility			–0.0035	–0.0035		–0.0002	–0.0002
			(0.0103)	(0.0103)		(0.0011)	(0.0011)
Size			–0.0031	–0.0030		0.0024	0.0024
			(0.0163)	(0.0163)		(0.0015)	(0.0015)
Leverage			0.0468	0.0473		0.0056	0.0056
			(0.1025)	(0.1026)		(0.0100)	(0.0100)
ROA			0.2150	0.2052		0.0269	0.0240
			(0.2254)	(0.2250)		(0.0222)	(0.0220)
Labor_Force				–0.3614			–0.0932**
				(0.4235)			(0.0470)
Unemployment_Rate				–0.0283			–0.0084***
				(0.0262)			(0.0031)
Facility FE		Yes	Yes	Yes	Yes	Yes	Yes
Year FE x State FE		Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-square		0.101	0.101	0.101	0.266	0.266	0.266
Observations		63,687	63,687	63,687	63,687	63,687	63,687

Table 5. Economic Mechanism

Panel A. 3G Access and Twitter Activity

This table examines the effect of 3G access on Twitter activity. *3G* takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. The dependent variable is the natural logarithm of one plus the number of Tweets per zip code and year. *Controls* includes *Labor_Force* and *Unemployment_Rate*. Column 1 reports results without *Controls*. Column 2 reports results with *Controls*. All variables are defined in Appendix A, and the sample spans the period 2010-2017. Standard errors are clustered by zip code. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Number_Tweets	
Variables	(1)	(2)
3G	0.1882***	0.1890***
	(0.0548)	(0.0550)
Controls	No	Yes
Zip Code FE	Yes	Yes
Year FE	Yes	Yes
Adj. R-square	0.926	0.926
Observations	4,526	4,526

Panel B. Twitter Activity and Facility-Level Misconduct

This table reports the estimation results from linear regressions of the association between Twitter activity and firm misconduct. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (Column 1) or the natural logarithm of one plus the number of violations per facility and year (Column 2). Our main explanatory variable is *High_Twitter_Activity*, which takes the value of 1 if the number of Tweets per zip code is larger than the median number of Tweets across all zip codes and 0 otherwise. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2010-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Penalties	Number_Violations
Variables	Pred.	(1)	(2)
High_Twitter_Activity	–	–0.2005*	–0.0183*
		(0.1062)	(0.0102)
Controls		Yes	Yes
Facility FE		Yes	Yes
Year FE x State FE		Yes	Yes
Adj. R-square		0.093	0.342
Observations		13,142	13,142

Table 6. Cross-Sectional Tests

Panel A. Firm Size

This table analyzes cross-sectional variation in the results of Table 4. *Large_Firm* equals 1 if a firm's assets are above the median and 0 otherwise. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Column 1) or the natural logarithm of one plus the number of violations (i.e., Column 2). Our main explanatory variable is *3G*, which takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. We use an F-test to test whether the sum of the coefficients ($\beta_1 + \beta_3$) is greater than 0 and report the p-values in square brackets. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variables		Penalties	Number_Violations
Variables		(1)	(2)
3G x Large_Firm	β_1	–0.0842*	–0.0106**
		(0.0467)	(0.0042)
3G	β_3	–0.0444	–0.0091*
		(0.0537)	(0.0055)
Large_Firm		0.1643**	0.0239**
		(0.0814)	(0.0098)
F-Test: $\beta_1 + \beta_3 > 0$		–0.1286**	–0.0197***
		[0.01]	[0.00]
Controls		Yes	Yes
Facility FE		Yes	Yes
Year x State FE		Yes	Yes
Adj. R-square		0.106	0.266
Observations		63,687	63,687

Panel B. Twitter Followers

This table analyzes cross-sectional variation in the results of Table 4. *Many_Followers* equals 1 if a firm's number of Twitter followers is above the median and 0 otherwise. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Column 1) or the natural logarithm of one plus the number of violations (i.e., Column 2). Our main explanatory variable is *3G*, which takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet (treatments before 2009 are set to zero). We use an F-test to test whether the sum of the coefficients ($\beta_1 + \beta_3$) is greater than 0 and report the p-values in square brackets. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. *Many_Followers* is time invariant and is hence absorbed by the fixed effects. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variables		Penalties	Number_Violations
Variables		(1)	(2)
3G x Many_Followers	β_1	–0.0877*	–0.0095*
		(0.0539)	(0.0049)
3G	β_3	–0.0862	–0.0139*
		(0.0670)	(0.0074)
F-Test: $\beta_1 + \beta_3 > 0$		–0.1739***	–0.0234***
		[0.006]	[0.001]
Controls		Yes	Yes
Facility FE		Yes	Yes
Year x State FE		Yes	Yes
Adj. R-square		0.101	0.266
Observations		63,687	63,687

Panel C. Media Coverage

This table analyzes cross-sectional variation in the results of Table 4. *High_Coverage* equals 1 if a firm has above median coverage in the number of newspaper articles per year. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Column 1) or the natural logarithm of one plus the number of violations (i.e., Column 2). Our main explanatory variable is *3G*, which takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. We use an F-test to test whether the sum of the coefficients ($\beta_1 + \beta_3$) is greater than 0 and report the p-values in square brackets. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variables		Penalties	Number_Violations
Variables		(1)	(2)
3G x High_Coverage	β_1	–0.0945*	–0.0092*
		(0.0537)	(0.0050)
3G	β_3	–0.0749	–0.0124**
		(0.0560)	(0.0060)
High_Coverage		0.0729	0.0102**
		(0.0474)	(0.0049)
F-Test: $\beta_1 + \beta_3 > 0$		–0.1694**	–0.0216***
		[0.01]	[0.00]
Controls		Yes	Yes
Facility FE		Yes	Yes
Year FE		No	No
Year x State FE		Yes	Yes
Adj. R-square		0.101	0.267
Observations		63,687	63,687

Table 7. Financial vs. Non-Financial Misconduct

This table reports the estimation results from linear regressions of the following form:

$$Y_{i,j,l,t} = \alpha_0 + \alpha_1\, 3G_{l,t} + \phi\ Controls + \gamma_i + \delta_{s,t} + \varepsilon_{i,j,l,t}$$

Y is either the natural logarithm of one plus the dollar amount of non-financial penalties per facility and year (Column 1), the natural logarithm of one plus the dollar amount of financial penalties per facility and year (Column 2), the natural logarithm of one plus the number of non-financial violations per facility and year (Column 3), or the natural logarithm of one plus the number of non-financial violations per facility and year (Column 4). Our main explanatory variable is *3G*, which takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Non-Financial Penalties	Financial Penalties	Non-Financial Penalties	Financial Penalties
Variables	(1)	(2)	(3)	(4)
3G	–0.1276***	0.0035	–0.0177***	0.0002
	(0.0474)	(0.0056)	(0.0056)	(0.0003)
Controls	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes
Year FE x State FE	Yes	Yes	Yes	Yes
Adj. R-square	0.097	0.148	0.266	0.133
Observations	63,687	63,687	63,687	63,687
H_0	$\alpha_{1\_Non\text{-}Financial} > \alpha_{1\_Financial}$		$\alpha_{1\_Non\text{-}Financial} > \alpha_{1\_Financial}$	
p-value	0.001		0.001	

Table 8. Dynamic Effects

This table reports the estimation results from linear regressions of the following form:

$$Y_{i,j,l,t} = \alpha_0 + \alpha_1\, 3G_{l,t} + \phi\, Controls + \gamma_i + \delta_{s,t} + \varepsilon_{i,j,l,t}$$

Y is either the natural logarithm of one plus the dollar amount of penalties per facility and year (Column 1) or the natural logarithm of one plus the number of violations per facility and year (Column 2). The main explanatory variables are single-year treatment windows that range from 3 years before the introduction of 3G mobile internet to 3 years after the introduction of 3G internet. These treatment windows are benchmarked against the year *t-3* before the introduction of 3G mobile internet. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties	Number_Violations
Variables	(1)	(2)
$3G_{t-2}$	−0.0820	−0.0084
	(0.0501)	(0.0053)
$3G_{t-1}$	−0.0509	−0.0104
	(0.0586)	(0.0071)
$3G_{t}$	−0.1842***	−0.0253***
	(0.0692)	(0.0093)
$3G_{t+1}$	−0.1819**	−0.0306***
	(0.0841)	(0.0117)
$3G_{t+2}$	−0.2804***	−0.0380***
	(0.0987)	(0.0139)
Controls	Yes	Yes
Facility FE	Yes	Yes
Year FE x State FE	Yes	Yes
Adj. R-square	0.091	0.267
Observations	63,687	63,687

Table 9. Additional Tests

Panel A. Alternative Shock

This table examines the robustness to our primary results tabulated in Table 4 using *SXSW* an alternative shock for increased social media activity. *SXSW* is set to 1 for facilities located in counties that experienced an increase in Twitter users following the SXSW festival (but no increase before the festival), and to 0 for facilities located in counties that experienced an increase in Twitter users before the SXSW festival. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (Columns 1 and 2) or the natural logarithm of one plus the number of violations per facility and year (Columns 3 and 4). *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate.* All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties		Number_Violations	
Variables	(1)	(2)	(3)	(4)
SXSW	−0.0637***	−0.0552***	−0.0037***	−0.0029*
	(0.0164)	(0.0176)	(0.0014)	(0.0015)
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adj. R-square	0.005	0.006	0.020	0.020
Observations	69,191	69,191	69,191	69,191

Panel B. Alternative Fixed Effects

This table examines the robustness to our primary results tabulated in Table 4 to different fixed effects. Columns 1 and 4 report results with county-year fixed effects. Columns 2 and 5 report results with state-year and industry-year fixed effects. Columns 3 and 6 report results with state-year and firm-year fixed effects. *3G* takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Columns 1-2) or the natural logarithm of one plus the number of violations (i.e., Columns 3-4). *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate* (in Columns 1 and 3, *Labor_Force*, and *Unemployment_Rate* are subsumed by the county-year fixed effects). All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties			Number_Violations		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
3G	−0.2760*	−0.0806*	−0.0833*	−0.0403**	−0.0094*	−0.0097*
	(0.1577)	(0.0475)	(0.0500)	(0.0196)	(0.0048)	(0.0059)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x County FE	Yes	No	No	Yes	No	No
Year x State FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	No	Yes	No	No	Yes	No
Year x Firm FE	No	No	Yes	No	No	Yes
Adj. R-square	0.102	0.062	0.109	0.302	0.219	0.276
Observations	50,515	59,283	58,514	50,515	59,283	58,514

Panel C. Alternative Treatment Windows

This table examines the robustness to our primary results tabulated in Table 4 using alternative treatment windows. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (Columns 1 and 2) or the natural logarithm of one plus the number of violations per facility and year (Columns 3 and 4). In Columns 1 and 3, *3G* is set to 1 for the five years after the introduction of 3G mobile internet and 0 in the five years prior to the introduction of 3G mobile internet. In Columns 2 and 4, *3G* is set to 1 for all years after the introduction of 3G mobile internet and 0 in all years prior to the introduction of 3G mobile internet. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate.* All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties		Number_Violations	
Treatment Window	10 years	All years	10 years	All years
Variables	(1)	(2)	(3)	(4)
3G	–0.0851**	–0.0460*	–0.0136***	–0.0080**
	(0.0385)	(0.0279)	(0.0042)	(0.0031)
Controls	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes
Year FE x State FE	Yes	Yes	Yes	Yes
Adj. R-square	0.090	0.081	0.250	0.206
Observations	101,903	184,346	101,903	184,346

Panel D. Alternative Research Designs

This table examines the robustness to our primary results tabulated in Table 4 using alternative research designs. Columns 1 and 4 use stacked regressions and Columns 2-3 and 5-6 include additional controls. *3G* takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Columns 1-3) or the natural logarithm of one plus the number of violations (i.e., Columns 4-6). *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. *Group* marks each subsample for each 3G rollout year, which includes only not-yet-treated facilities as controls. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Penalties			Number_Violations	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
3G	–0.1495***	–0.1540***	–0.1600***	–0.0264***	–0.0205***	–0.0196***
	(0.0566)	(0.0555)	(0.0515)	(0.0066)	(0.0061)	(0.0059)
Number_Nearby_Treated_Zip_Codes		0.0243			0.0028	
		(0.0292)			(0.0031)	
Proportion_Nearby_Treated_Zip_Codes			0.4119			0.0250
			(0.2928)			(0.0326)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facility x Group FE	Yes	No	No	Yes	No	No
Year x Group FE	Yes	No	No	Yes	No	No
Facility FE	No	Yes	Yes	No	Yes	Yes
Year FE x State FE	No	Yes	Yes	No	Yes	Yes
Adj. R-square	0.281	0.101	0.101	0.451	0.266	0.266
Observations	85,333	63,687	63,687	85,333	63,687	63,687

Panel E. Alternative Sample

This table examines the robustness to our primary results tabulated in Table 4 to different samples. Columns 1 and 4 report results excluding violations and penalties that cannot be unambiguously assigned to a facility. Columns 2 and 5 report results including facilities without violations. Columns 3 and 6 report results including facilities of private firms with violations. *3G* takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Columns 1-3) or the natural logarithm of one plus the number of violations (i.e., Columns 4-6). In Columns 1-2 and 4-5, *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. In Columns 3 and 6, *Controls* includes *Labor_Force* and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties			Number_Violations		
Sample	Without Ambiguous Violations	With No Violation Facility	Private and Public Facilities	Without Ambiguous Violations	With No Violation Facility	Private and Public Facilities
Variables	(1)	(2)	(3)	(4)	(5)	(6)
3G	−0.0899*	−0.0511***	−0.0221*	−0.0165***	−0.0084***	−0.0034***
	(0.0497)	(0.0163)	(0.0134)	(0.0056)	(0.0019)	(0.0013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-square	0.054	0.087	0.001	0.221	0.232	0.074
Observations	58,111	216,923	521,191	58,111	216,923	521,191

Panel F. 3G Access and IT Investments

This table examines changes in facilities' IT investments after 3G adoption. The dependent variable, *IT_Investment*, is the natural logarithm of the dollar amount of a facility's annual IT budget. Column 1 reports results without *Controls*. Column 2 reports results with *Controls*. Our main explanatory variable is *3G*, which takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2010-2017. Standard errors are clustered by facility. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	IT_Investments	
Variables	(1)	(2)
3G	0.0464	0.0487
	(0.0541)	(0.0542)
Controls	No	Yes
Facility FE	Yes	Yes
Year x State FE	Yes	Yes
Adj. R-square	0.714	0.714
Observations	8,763	8,763

Panel G. Alternative Clustering

This table examines the robustness to our primary results tabulated in Table 4 to different clustering of standard errors. Columns 1 and 4 report results with standard errors clustered by state. Columns 2 and 5 report results with standard errors two-way clustered by state and year. Columns 3 and 6 report results with standard errors clustered by zip code. *3G* takes the value of 1 for the three years after the introduction of 3G mobile internet and 0 in the three years prior to the introduction of 3G mobile internet. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Columns 1-3) or the natural logarithm of one plus the number of violations (i.e., Columns 4-6). *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties			Number_Violations		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
3G	–0.1298**	–0.1298**	–0.1298*	–0.0178**	–0.0178**	–0.0178*
	(0.0489)	(0.0468)	(0.0162)	(0.0069)	(0.0070)	(0.0019)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered by	State	State and Year	Zip Code	State	State and Year	Zip Code
Adj. R-square	0.101	0.101	0.101	0.266	0.266	0.266
Observations	63,687	63,687	63,687	63,687	63,687	63,687

Table 10. Firm-Level Analysis

This table reports the estimation results from linear regressions of the following form:

$$Y_{i,t} = \alpha_0 + \alpha_1\, 3G\_Exposure_{i,t} + \phi\, Controls + \gamma_i + \delta_t + \varepsilon_{i,t}$$

Y is either the natural logarithm of one plus the dollar amount of penalties per firm and year (Column 1) or the natural logarithm of one plus the number of violations per firm and year (Column 2). Our main explanatory variable is *3G_Exposure*, which captures the fraction of a firm's facilities located in areas with 3G access per year. *Controls* includes *Size*, *Leverage*, and *ROA*. All variables are defined in Appendix A, and the sample spans the period 2000-2017. Standard errors are clustered by firm. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties_Firm	Number_Violations_Firm
Variables	(1)	(3)
3G_Exposure	−0.2993**	−0.0457***
	(0.1524)	(0.0168)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Adj. R-square	0.334	0.508
Observations	23,772	23,772