The Unintrusive Nature of Digital Surveillance and Its Social Consequences

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Abstract

The world is witnessing an explosion of digital surveillance in recent years. Yet, we rarely saw massive surveillance states before the digital age. This paper examines citizens' responses to digital surveillance versus in-person surveillance in dictatorships to identify potential causes of digital surveillance expansion. I argue that digital surveillance is less offensive than in-person surveillance because it does not entail human intrusion into citizens' private lives. I manipulate information about surveillance operations in a field survey experiment on college students in two regions of China. I find that digital surveillance is less likely to undermine interpersonal trust and regime legitimacy than in-person surveillance. But both types of surveillance are effective in deterring political participation. I further establish the external validity of the experimental findings by using a nationally representative survey and a natural experiment caused by the 2015 Tianjin explosion. Overall, digital surveillance suppresses political participation, and the unintrusive nature of digital surveillance implies that it can expand rapidly without facing much resistance from society.

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1 Introduction

Rarely in history could any autocratic regimes surveil citizens at a scale as large as those achieved by today's digital surveillance states. For instance, China has used cutting-edge surveillance technology to monitor hundreds of millions of netizens. It has also built the world's largest video surveillance networks, some are AI-powered, to monitor citizens' everyday activities (Liu and Wang 2017). The Iranian and Syrian governments have developed sophisticated digital surveillance systems to identify and track opposition members (Gunitsky 2015). By 2015, at least 30 countries have employed digital surveillance to spy on citizens, and more than half of them are autocracies (Valentino-DeVries, Vo and Yadron 2015). This trend is rapidly evolving through the export of surveillance technology from China, Israel, the US, and the UK to less developed countries such as Uganda, Zimbabwe, Angola, Bahrain, Kazakhstan, Mozambique, Nicaragua, and Saudi Arabia, among others (Feldstein 2019). Further, the COVID-19 pandemic ushered in a new era of digital surveillance as numerous countries have imposed surveillance tools to track individuals' health status. The omnipresence of digital surveillance in today's dictatorships and even in democracies raises an interesting question: why did we see massive surveillance expansion in the digital age but rarely in history?

All regimes need information in order to identify potential threats to their rule. Traditionally, autocratic regimes rely on secret police and informers to spy on citizens. The scales of traditional surveillance states were usually small. The only exception was East Germany's Stasi (short for *Staatssicherheitsdienst*, or State Security Service). The Stasi at its peak employed over 90 thousand employees and nearly 170 thousand informers who collected vast amounts of information to intimidate citizens and eliminate regime opposition. Counting part-time informers, the Stasi had 1 collaborator per 80 to 160 inhabitants depending on the region (Gieseke 2014). In the digital age, states do not rely on human agents to collect information directly from citizens. Instead, citizens transmit information in an electronic form on the internet and social media that can be accessed and analyzed by automatic algorithms and machines with minimal human assistance. Video surveillance powered by artificial intelligence is also widely employed in many countries. For example, in 2018, China had 350 million surveillance cameras installed or one camera for every 4.1 citizens. The number of installed cameras is expected to rise to over 560 million cameras by 2021, which means one camera for every 2.5 citizens. The different techniques in information collection suggest that digital surveillance may receive different responses from citizens compared with in-person surveillance.

In this paper, I argue that digital surveillance by nature is less intrusive than in-person surveillance. For traditional, in-person surveillance to work, surveillance agents must penetrate citizens' close social networks to collect information and identify dissidents. This has important implications for interpersonal trust and regime legitimacy. Humans' privacy concerns have evolutionary roots: privacy is the selective, self-interested opening and closing of the self to other individuals (Acquisti, Brandimarte and Hancock 2022). In other words, humans react to sensorial cues that suggest the presence of others. In societies penetrated by human agents who gather information directly from observing and interacting with their targets, citizens sense the invasion of privacy and are atomized from each other. They hide their true anti-regime sentiments and exhibit low levels of interpersonal trust (Blaydes 2018). In-person surveillance also undermines regime legitimacy. Legitimacy is the belief on the part of citizens that the dictates of the state are right and proper (Hechter 2009). Citizens may consider a regime less legitimate when in-person surveillance conducted by the regime intrudes privacy and fosters betrayal, sabotage, and unethical exchanges of information for personal gains. On the contrary, digital surveillance does not entail human-agent intrusion into citizens' private lives. Although some of its data layers still rely on human inputs, digital surveillance avoids sensorial cues of human presence and the intervention of subjective, self-interested human informers. Thus, digital surveillance is *less* likely to decrease trust and regime legitimacy than in-person surveillance.

The unintrusive nature of digital surveillance does not mean it is ineffective in deterring anti-regime mobilization. Regardless of its types, surveillance helps dictators selectively repress citizens who are most likely to pursue anti-regime activities. When faced with the prospect of repression, citizens under surveillance have an incentive to refrain from expression (Kuran 1991). Surveillance also deters protest coordination because, under repression threat, individuals expect a higher cost of political participation – for themselves and others – and therefore anticipate fewer people to participate. Thus, both digital surveillance and in-person surveillance deter free speech and political participation.

To test the above hypotheses, I use a field survey experiment with a sample of over 500 students in two universities in North and West China to examine people's responses to digital surveillance and in-person surveillance. As students are the most active social group concerning protests and political movements, finding a deterrence effect of surveillance on a student sample implies an even stronger effect on the general population. In the experiment, respondents are randomly assigned to read information about a digital surveillance scenario, an in-person surveillance scenario, and a no-surveillance control scenario regarding an issue that is very pertinent to their campus life.

I find that digital surveillance is indeed less intrusive to citizens than in-person surveillance. In-person surveillance significantly reduces interpersonal trust and regime legitimacy, but digital surveillance only has a small, negative effect on trust and a negative but statistically insignificant effect on legitimacy. These differences suggest that digital surveillance may not arouse the same level of negative sentiment among citizens that in-person surveillance inflames – reflected by decreased interpersonal trust and regime legitimacy. The practice of in-person surveillance in traditional surveillance states usually encounters strong resistance from the public (Gieseke 2014). Sometimes, it even stirs up hatred and sparks anti-regime protests (Hager and Krakowski 2021). The unintrusive nature of digital surveillance implies that it may expand rapidly without facing much resistance from society.

Moreover, I find that both types of surveillance negatively influence respondents' political expression and intent to protest. In addition, both types of surveillance reduce respondents' beliefs about how many other individuals would participate in “anti-regime” collective action. Causal mediation analysis indicates that both types of surveillance reduce individuals’ protest intention through influencing their beliefs about others’ participation in protest rather than through influencing interpersonal trust. These findings suggest that digital surveillance is as effective as in-person in deterring anti-regime coordination and aiding authoritarian survival.

To establish the external validity of the experimental findings on *digital surveillance*, I use the 2015 Chinese General Social Survey with a nationally representative sample of nearly 11 thousand respondents and an interrupted time series design that exploits an exogenous shock to digital surveillance caused by the 2015 Tianjin Explosions in China. At the midnight of August 12, 2015, a series of massive explosions in the port of Tianjin killed 173 people and injured nearly a thousand (Merchant 2017). Online surveillance and censorship increased tenfold in China immediately afterward (Dou 2015). By comparing respondents surveyed just before the explosions with those surveyed just afterward, I show that the intensified digital surveillance after the explosions decreases citizens’ confidence in free speech and petitioning the government but has no effect on interpersonal trust. There is also a decrease in regime legitimacy, but this is likely due to the accident itself rather than the aftermath surveillance. To further show that digital surveillance is the mechanism underpinning these results, I explore provincial-level differences in surveillance capacity, using the number of pilot counties for China’s digital surveillance system (the Golden Shield Project, developed by the Ministry of Public Security). I show that citizens feel less secure about free speech and petitioning the government in provinces with greater surveillance capacity.

This paper highlights the unintrusive nature of digital surveillance that might have contributed to the global expansion of surveillance states in the digital age. Exiting literature attributes the expansion to public support for state surveillance and coercion. A classic argument is that people are willing to sacrifice privacy and liberties for public safety (Davis and Silver 2004). Citizens support state surveillance if surveillance measures target potential criminals instead of all citizens and if safety threats are salient (Ziller and Helbling

2021). Amid the COVID pandemic, people all around the world have been willing to tolerate surveillance measures for better health conditions (Alsan et al. 2020). Unlike previous studies, this paper argues that citizens may not view digital surveillance as a particular threat to privacy because digital surveillance lacks sensorial cues that suggest the presence of other humans (as in-person surveillance does). The findings that digital surveillance has little impact on interpersonal trust and regime legitimacy highlight the unintrusive nature of digital surveillance, which provides a new explanation for the rapid expansion of digital surveillance around the globe.

This paper also identifies the deterrence effect of digital surveillance on citizens' political participation. It contributes to a growing body of literature on information technology and authoritarian survival. Many early proponents of Internet development believed that information technologies would spread freedom and spur democratization. Yet, two decades after the advent of the digital era, we have not observed widespread authoritarian collapse. Existing studies explore how authoritarian governments use the Internet and ICT to censor and repress online expressions (King, Pan and Roberts 2013), collect information about citizen preferences (Gunitsky 2015), monitor local politicians (Qin, Strömberg and Wu 2017), distract or guide public opinion (King, Pan and Roberts 2017; Roberts 2018), and identify political opponents for targeted repression (Xu 2020). This paper contributes to the literature by emphasizing the role of digital surveillance in deterring mass mobilization. Indeed, truthful communication and beliefs in others' participation may be necessary for citizens to coordinate a successful protest (Chwe 2013; Edmond 2013). For this reason, this paper explores how digital surveillance prevents mobilization in authoritarian countries by discouraging expression, reducing willingness to protest, and decreasing beliefs about others' willingness to participate in anti-regime collective action.

This paper also examines how surveillance influences interpersonal trust and political participation – two cornerstones of free societies and the building blocks of open markets. Interpersonal trust is essential for facilitating trade and a well-functioning market economy (Greif 1989; Algan and Cahuc 2010). Political participation, particularly as the basis for coordinating citizens' anti-government behavior, is a central part of toppling dictatorships peacefully and establishing free and democratic societies (Kendall-Taylor and Frantz 2014). Though studies of both topics are numerous (e.g., Almond and Verba 1963; Putnam, Leonardi and Nanetti 1994), few have considered surveillance, especially digital surveillance, as a determinant of trust and political participation. In addition, recent studies find that autocratic rule decreases interpersonal trust and civic engagement generations after individuals migrate to democratic countries (Xu and Jin 2018), but the exact sources of social distrust and isolation in autocracies are not well understood. This paper contributes to this literature by identifying one channel through which autocratic rule damages the fabric of society: state surveillance.

Finally, this paper complements historical research on traditional, informer-based surveillance (e.g., Bruce 2010; Gieseke 2014; Blaydes 2018). While the pernicious social consequences of in-person surveillance are well-documented, quantitative research on state surveillance is rare. This paper adds to the thin literature on the causal analysis of the consequences of in-person surveillance (e.g., Lichter, Loeffler and Siegloch 2019; Hager and Krakowski 2021). More importantly, it compares the social costs of digital surveillance to those of in-person surveillance. My findings, therefore, have important implications for understanding authoritarian control in the digital age. In particular, digital surveillance yields many of the same benefits as in-person surveillance (decreasing citizen coordination) without some of the costly byproducts of in-person surveillance, namely decreased interpersonal trust and regime legitimacy.

2 In-Person Surveillance vs. Digital Surveillance

Surveillance is a common tool for information collection and political control in dictatorships (Greitens 2016). Unlike democratic leaders, dictators are inherently uninformed because citizens in dictatorships often hide their true anti-regime sentiments when faced with the prospect of state repression (Kuran 1991). To gauge public opinion for policymaking and to contain threats before they spread, dictators historically rely on human security agents and/or informers to collect information from citizens. In the digital age, information technology expands dictators' information-collection toolkit. As citizens move to online media for socializing, networking, communicating, shopping, and expressing opinions, computers and algorithms replace human agents as tools for governments to collect information from citizens. This shift from human to digital surveillance has important implications for interpersonal trust and political participation in modern dictatorships.

2.1 In-person Surveillance

Human societies have a long tradition of in-person surveillance. In BC 839, King Li of Zhou Empire in China asked his wizards to spy on the people and kill those who criticized his tyranny (Zuo 1998). The infamous Jinyiwei (Embroidered Uniform Guard) was founded in the 1360s by the Hongwu Emperor of the Ming Dynasty and served as the dynasty's secret police until the collapse of Ming in 1644. In Europe, secret police organizations emerged after the French Revolution in the 18th-century. Hitler's regime in Germany (1933–1945) utilized the Gestapo to eliminate opponents. East Germany (1945–1990) created the Stasi with unparalleled social penetration. Other dictatorships such as Iraq under Saddam Hussein, Chile under Pinochet, Peru under Fujimori, Philippine under Marcos, and North Korea under the Kims also used secret police organizations to control society (Greitens 2016; McMillan and Zoido 2004).

In-person surveillance is innately intrusive to citizens. To obtain precise information about opposition groups, a traditional surveillance apparatus needs security agents and, particularly, informers to penetrate citizens' social networks and private lives. Informers are ordinary citizens but use their professional and social networks to gather information about their targets. Research from history, anthropology, and ethnography suggests that humans are sensitive to the presence of others because privacy evolved from physical needs for security and self-interest (Westin 1968; Klopfer and Rubenstein 1977). By distinguishing kin from strangers and adapting behavior from openness to protection, humans (and other species) enhance their survival and evolutionary success (Acquisti, Brandimarte and Hancock 2022). The evolutionary roots of private suggest that citizens' reaction to surveillance depends, in part, on sensorial cues to detect others. Knowing that some of their colleagues, neighbors, friends, and even family members might be watching them in an in-person surveillance regime, citizens develop negative sentiment against the surveillance apparatus and the regime.

In-person surveillance also encourages betrayal, sabotage, and unethical exchanges of information for personal gains. Informers betray the trust of friends, neighbors, colleagues, relatives, and even family members to collect information (Ash 1998). Societal penetration and betrayal thus generate widespread suspicion and a deep sense of mistrust within society (Blaydes 2018). In addition, In-person surveillance relies on self-interested human agents and informers who use subjective assessment of other citizens' loyalty to the regime. To provide information, these self-interested informers may demand benefits from the regime, such as government jobs, opportunities to travel abroad, or monetary compensation. They may also maliciously target "innocent" people to resolve personal disputes (Kalyvas 2006). Potential informers may be "tricked" by the government to provide others' information to clear up their own "blemishes" (Ash 1998). To gain rewards or prove their innocence, self-interested agents may misreport or sabotage their fellow citizens. The potential power abuse by agents and informers associated with in-person surveillance further foments distrust and anti-regime sentiments in society. Moreover, in-person surveillance often involves unethical exchanges as informers trade others' secrets to the regime in exchange for material or non-material gains. These unethical exchanges could further upset people, thereby reducing trust and regime legitimacy.

Based on the above discussion, I derive the following testable implications.

Trust hypothesis (human): In-person surveillance reduces interpersonal trust.

Legitimacy hypothesis (human): In-person surveillance reduces regime legitimacy.

2.2 Digital Surveillance

Since the advent of the information era, authoritarian governments have increasingly adopted digital surveillance for social control. They use malware to spy on opposition leaders and journalists (Deibert 2017), collect metadata from social media to keep tabs on political opponents (Qin, Strömberg and Wu 2017), and employ high-resolution digital cameras and facial recognition technologies to identify dissidents (Liu and Wang 2017). Recent advances in artificial intelligence detect suspicious movements in crowds, identify thousands of people at once, and recognize citizens who attempt to conceal their identities by wearing hats, sunglasses, or scarves to cover their faces (Intel 2017; Singh et al. 2017).

Unlike in-person surveillance, digital surveillance does not entail human intrusion into citizens' private lives. In the digital age, citizens communicate and spend a substantial amount of time online, leaving personal digital information for governments and tech companies to access and analyze. Digital surveillance relies on digital infrastructures such as computers, software, algorithms, cameras, cables, routers, servers, and data storage centers rather than human informers. Offline sensorial cues that humans depend upon for private protection may be absent. The lack of sensorial cues in the digital world may explain seemingly careless online behaviors by individuals who claim to care about their privacy (Acquisti, Brandimarte and Hancock 2022). Further, digital surveillance allows governments to monitor a large population and reach the most private part of people's lives with minimal human assistance.[1] Computers and algorithms also yield more accurate and objective data than human agents who often misreport or intentionally sabotage "loyal" citizens for personal gains. Thus, the lack of sensorial cues and the absence of human betrayal and sabotage in the operation of

[1]For example, digital cameras allow governments to analyze population who do not use the Internet; Data from search engines reveal people's private preferences such as pornography that they would not even tell their most close friends.

digital surveillance suggests that it is less likely to reduce interpersonal trust than in-person surveillance.

To sum up, we expect the following testable implications.

Trust hypothesis (digital): Digital surveillance is less likely to reduce interpersonal trust than in-person surveillance.

Legitimacy hypothesis (digital): Digital surveillance is less likely to reduce regime legitimacy than in-person surveillance.

2.3 Surveillance and Political Participation

Surveillance, no matter digital or in-person, discourages political participation because it entails preemptive, targeted repression against regime opponents (Dimitrov and Sassoon 2014; Xu 2020). In dictatorships where meaningful elections and other representative channels of political expression are often unavailable, political participation takes the forms of petitions, protests, and even violent revolts. These actions disrupt social order and may threaten autocratic survival. Dictators thus use repression as a strategy to ensure political stability and avoid revolution (Wintrobe 2000). Surveillance enables dictators to find dissidents for targeted repression, thereby discouraging citizens' anti-regime political expression and protest participation.This suggests that both types of surveillance discourage political expression and protest participation.

Expression hypothesis: Both in-person and digital surveillance deter political expression.

Protest hypothesis: Both in-person and digital surveillance deter protest participation.

To mount a protest, participants must coordinate their actions (Chwe 2013). Surveillance may deter protest coordination via two channels. First, interpersonal trust may help induce protest participation by increasing individuals' belief that protest participation will be *safe* and *worthwhile* (Benson and Rochon 2004). Surveillance lowers interpersonal trust, thereby reducing individuals' willingness to participate. Second, strategic considerations are another important determinant of protest participation: an individual's behavior is shaped by beliefs about the participation of others (Edmond 2013). Thus, surveillance may also deter participation by influencing individuals' beliefs about how many others will participate. The following hypotheses examines these two potential channels through which surveillance deters protest participation.

Trust-protest hypothesis: Surveillance deters protest participation by lowering inter-personal trust among citizens.

Coordination-protest hypothesis: Surveillance deters protest participation by influencing individuals' belief about others' participation.

3 Experimental Design

To test the hypotheses, I pursue two strategies. First, I use an in-the-field survey experiment to compare the social consequences of in-person surveillance with those of digital surveillance. Second, I address the external validity of the experimental findings on digital surveillance by analyzing a nationally representative survey in an interrupted time-series (ITS) setting. This section explains the details of the experimental design. The ITS design comprises Section 5.

3.1 Design, Randomization, and Implementation

I conducted an in-the-field survey experiment with a sample of 539 Chinese university students in March 2019. Surveying respondents on a potentially sensitive topic in the field circumvents state censorship that may be present in China-based online survey platforms. It also helps create trust and cooperation between enumerators and respondents. More importantly, the *digital-surveillance* treatment in this study may induce respondents' self-censorship in *online* surveys. An in-the-field survey experiment avoids this problem because respondents answer questions on paper questionnaires. Students were recruited in dining halls and on main roads in universities. Online Appendix A1 discusses survey implementation and ethics in more detail.

I recruited students in two universities in North and West China to broaden sample representativeness. As Figure 1 shows, the home provinces of the student sample cover most regions in China. I choose university students because they are the most active social group in political participation. For example, in the 1960s, students initiated the anti-war movement and actively participated in the Vietnam War protests in the U.S. (Moore 1999). In 1989, hundreds of thousands of student protesters occupied the Tiananmen square to demand democracy in China, sparking large-scale student protests throughout the country (Zhao 2004). During the 2014 Hong Kong protests, students were also at the heart of the "Umbrella Revolution" protests (Cantoni et al. 2019). If we can find a deterrence effect of surveillance on political participation in the demographic group most likely to protest, we are likely to observe a stronger effect in less active demographics. Thus, examining a student sample can shed light on a much larger population.

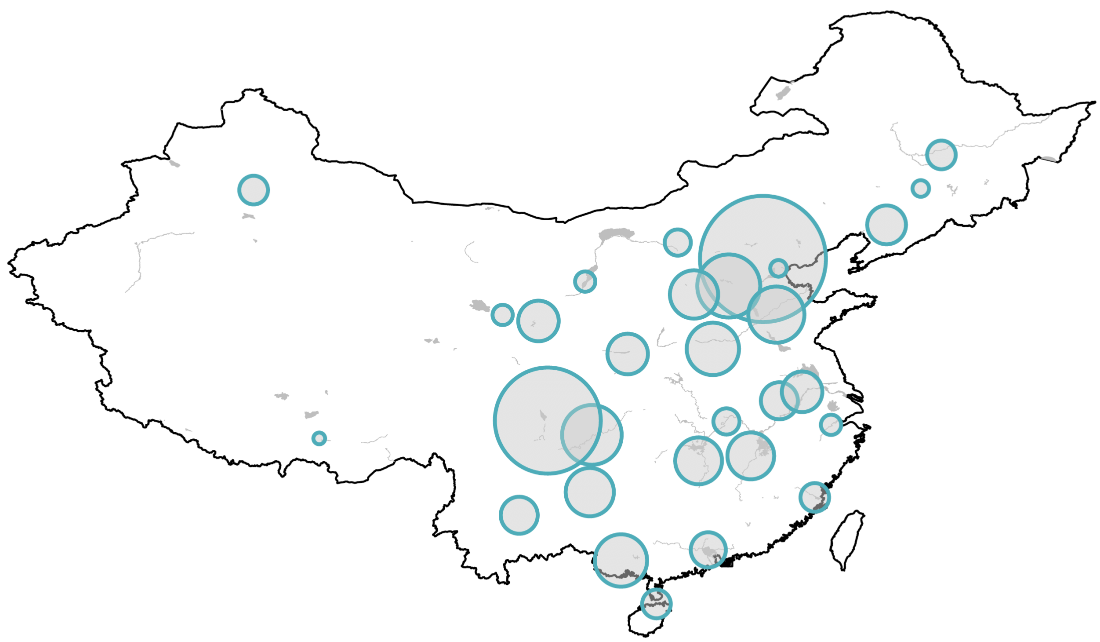

Figure 1: Distribution of Respondents by Home Province

I use a within-subject design to compare interpersonal trust, political participation, and regime support among three groups: a treated group with in-person surveillance, a treated group with digital surveillance, and a control group without surveillance.[2] To assess possible

[2]A pilot study on a sample of 214 college students revealed large standard deviations in

experimenter demand effects of the within-subject design, I also embed a between-subject design for comparison. This two-by-three design yields six experimental groups. Each student has a 2/3 chance to be assigned to the within-subject design group and a 1/3 chance to be assigned to the between-subject design group.[3] Within each of the two design groups, students have an equal chance to be assigned to the control condition or one of the two treatment conditions. In total, 353 students were randomly assigned to the three experimental groups in the within-subject design, and 186 students were randomly assigned to the three experimental groups in the between-subject design.

During the survey, all respondents first answer background questions about age, gender, income, party affiliation, social distrust, general civil participation, and media usage. They then read a descriptive vignette about an issue concerning their campus life. Respondents in the within-subject design group first answer questions about willingness to make public political expressions and protest, trust toward fellow students, as well as approval of the university authority over this issue. After receiving information about the school authority's in-person surveillance, digital surveillance, or no surveillance operation, the respondents answer the same set of questions for the second time. This within-subject design allows me to differentiate out respondents' intrinsic attitudes that are difficult to manipulate in a short experiment. Alternatively, respondents in the between-subject design group directly receive information about surveillance without the pretest questions. Table 1 shows the structure of the design. Table A.3 in the Online Appendix shows that covariates are balanced across control and treatment groups.

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trust and participation variables. As trust and political participation are intrinsic values, it would require a very large sample to observe significant effects of manipulations from a between-subject design. On a relatively small sample, I thus implement a within-subject design that differentiates respondents' intrinsic values, following Wiswall and Zafar (2014).

[3]To balance students' gender, I use a block random assignment procedure whereby complete random assignment occurs within the male block and the female block.

Table 1: Experimental Design for In-person vs. Digital Surveillance

	Within-Subjects Design			Between-Subjects Design		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Groups:	Control	Treatment 1	Treatment 2	Control	Treatment 1	Treatment 2
Background Qs:	Yes	Yes	Yes	Yes	Yes	Yes
Scenario:	Yes	Yes	Yes	Yes	Yes	Yes
Pre-test:	Yes	Yes	Yes	No	No	No
Treatment:	No Surveil.	In-person	Digital	No Surveil.	In-person	Digital
Post-test:	Yes	Yes	Yes	Yes	Yes	Yes
N of Obs.:	124	103	126	61	64	61

3.2 Treatments and Measures

Protest Scenario and Surveillance Treatments

To reduce sensitivity and protect respondents, I design a hypothetical scenario under which students confront the university authority for an unfair housing policy. In this scenario, students were forced to move from a new dormitory to an old one with bad living conditions, and the university refused to refund the price differentials. Students were discussing the means to fight for their rights, including filing a petition to the Ministry of Education. This scenario corresponds to a typical real-world confrontation between citizens and the government but with less political risk. Students' appeals to the Ministry of Education also mimic citizens' petitions to upper-level administrations for justice – a common phenomenon in authoritarian countries (Lorentzen 2013). To simulate real-world protest coordination, I remind the respondents that more participants lead to a higher chance of petition success. In addition, I made punishment upfront by reminding the respondents of the costly consequences of protest participation. This punishment reminder also helps respondents relate the surveillance scenarios to repression.

One may argue that protesting against a university authority is different from confronting a political authority. But the fact is that most universities in China are state entities, and they often directly engage in political repression. For example, during the 2018 Jasic labor rights conflict in Shenzhen, several universities including Peking University and Renmin

University were involved in cracking down on student activists who supported the movement (Yang 2019). In addition, the hypothetical scenario mentions interrogation and recording of a demerit as punishment for protest participation, which are very serious to college students. This level of threat to a student is very close to the level of a typical repression threat to a citizen. More importantly, even if protesting a university authority is less risky than protesting a political authority, the survey results concerning campus protests have general implications for political protests. This is because if surveillance can deter less risky protests, it will have even stronger deterrence effects on riskier political participation. In short, this experimental scenario is very relevant to campus life to elicit students' truthful responses, while representing a common situation of contentious politics in authoritarian countries.

In the experiment, I carefully differentiate the scenarios for in-person surveillance, digital surveillance, and no surveillance. The in-person surveillance scenario mimics the traditional, Stasi-style surveillance that relies on human informers to spy on fellow citizens. It reminds correspondents of the key features of in-person surveillance: societal penetration, betrayal, and the unethical exchanges of information for personal gains. The digital surveillance scenario is similar to real-world online surveillance conducted by authoritarian governments with no human informers involved. Because respondents' prior experience of surveillance may influence their responses even if they do not receive any new information about surveillance, I specify in the control condition that "the university does not know who participate in the protest" to "reset" respondents' prior beliefs about surveillance operations.

Measurement

In the survey, I ask respondents' willingness to express their discontent both in front of their fellow students and online. For protest participation, I elicit respondents' willingness to file an online complaint to the Ministry of Education and their beliefs regarding other students' participation (percentage points). The responses for willingness to express and protest are recorded on a scale from 1 to 4, with 4 indicating the most affirmative answer. Interpersonal trust is measured by respondents' trust toward other students in the same residential hall on a scale from 0 to 10, with 10 indicating the highest level of trust.[4] Regime legitimacy is citizens' belief about the right and acceptance of an authority. One basis for the belief is government performance (Levi, Sacks and Tyler 2009), which is particularly important in China since satisfying people's needs for a decent livelihood has roots in Chinese traditional political culture (Perry 2008). Thus, I measure legitimacy by the extent to which respondents *generally* approve what the university authority does concerning student affairs, also on a scale from 0 to 10. See Table A.3 in Online Appendix for detailed questions concerning these outcome variables.

4 Experimental Findings

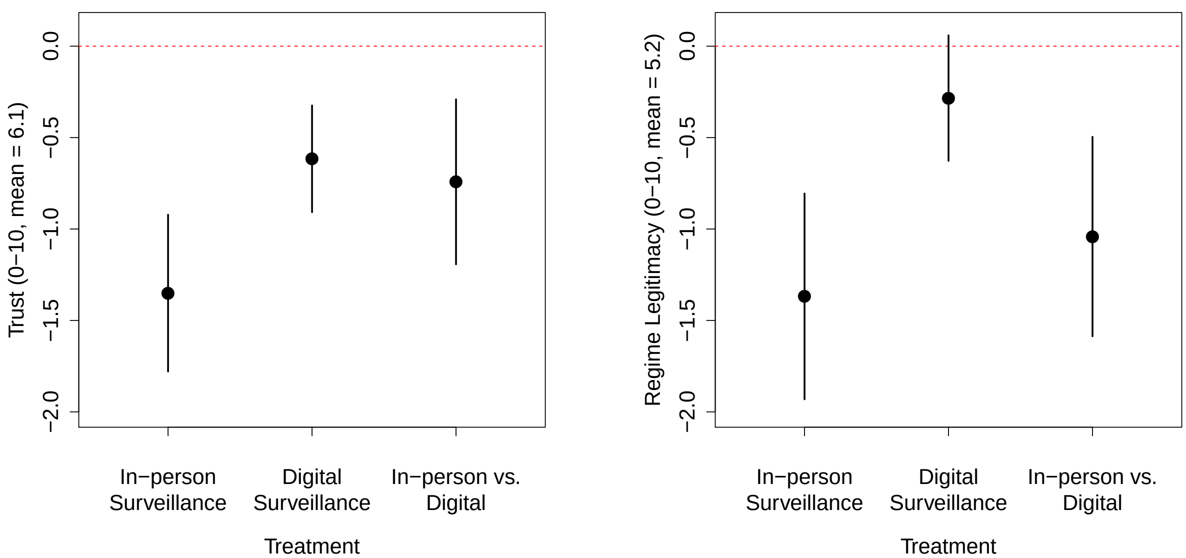
This section presents the results of the survey experiment based on the within-subject design. I take the differences between post-test answers and pre-test answers to generate the outcome variables of trust, regime legitimacy, expression, and protest participation. For each outcome, I first examine the effects of in-person surveillance and digital surveillance using no-surveillance as the comparison group. I then compare in-person surveillance with digital surveillance using the latter as the comparison group. The statistical inferences are based on standard comparisons of means using OLS estimation.[5]

[4]Scholars find that the 11-point scale consistently outperforms the dichotomous counterpart for measuring trust in surveys (Lundmark, Gilljam and Dahlberg 2015).

[5]As Table A.4 in Online Appendix shows slight differences (though statistically insignificant) in social distrust between three groups, I control for this variable in all specifications. The results are robust with the randomization inference approach. In addition, I fit Ordered Probit models for expression and protest participation. The effects are similar and statistically more significant (Table A.9 in Online Appendix). Nevertheless, I use the more conservative results from OLS models.

4.1 Trust and Regime Legitimacy

I begin by presenting the effects of in-person and digital surveillance on interpersonal trust and regime legitimacy. My theory suggests that in-person surveillance reduces trust and legitimacy, while digital surveillance is less likely to have impacts. In addition, in-person surveillance is more likely to reduce trust and legitimacy than digital surveillance. The evidence from the survey experiment is consistent with these predictions.



(a) Interpersonal Trust

(b) Regime Legitimacy

(Notes: OLS estimates with 95% Confidence Intervals. See Online Appendix B.1 for the regression results underlying these figures.)

Figure 2: Trust and Legitimacy

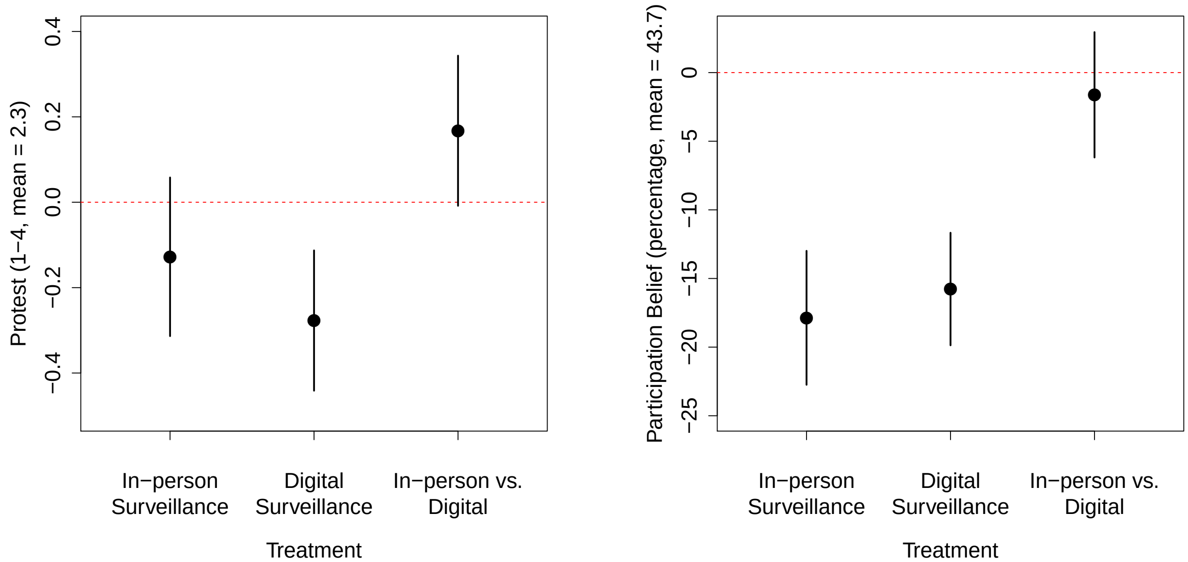
As Figure 2a shows, in-person surveillance largely reduces interpersonal trust. Recall that interpersonal trust is measured on a scale of 0 to 10, with 10 indicating the highest level of trust. Given that the sample mean is 6.1 and the standard deviation is 2.1, a 1.4 decrease in trust scale is quite substantial (23 percent of the mean). Digital surveillance only slightly reduces trust (0.6 or 10 percent of the mean). Both effects are statistically significant at the 0.001 level. More importantly, compared with digital surveillance, in-person surveillance further reduces trust by 13 percent, and the effect is statistically significant. Figure 2b examines regime legitimacy (on a scale of 0 to 10) and shows that in-person surveillance reduces legitimacy by a large margin (1.4 or 26 percent, significant at the 0.001 level) whereas digital surveillance has a negative but statistically insignificant effect. The negative effect of in-person surveillance is 19 percent larger than that of digital surveillance and is statistically significant at the 0.001 level.

4.2 Expression and Political Participation

Next, I examine how in-person and digital surveillance influence various aspects of political participation. As discussed in the theoretical part, I expect both types of surveillance to discourage expression and protest participation. Figure 3a shows that both digital surveillance and in-person surveillance deters protest participation and the effect of digital surveillance is stronger. In particular, digital surveillance decreases respondents' willingness to participate by 0.28. Given that the sample mean is 2.3 and the standard deviation is 1, the negative effect is substantial. In-person surveillance also negatively affects protest participation, but the effect is statistically insignificant in OLS models.[6] This is likely due to my framing of the protest as an online protest instead of a street protest. In theory, digital surveillance deters online protest whereas in-person surveillance may not because citizens can hide their online activities from their friends, colleagues, or family members. Nevertheless, the negative effects of both types of surveillance are consistent with the theoretical predictions.

Figure 3b presents the effects of surveillance on individuals' beliefs about others' protest participation. On average, respondents believe that 43.7 percent of students in the residence hall will participate in the protest. As we can see, in-person surveillance and digital surveillance reduce the belief by 17.9 and 15.8 percentage points respectively, which are about 41 percent and 36 percent decreases. These large, negative effects are highly significant, and there is no statistically significant difference between two types of surveillance in affecting

[6]Note that the effect is statistically significant in Ordered Probit models, see Table A.9 in Online Appendix.



(a) Protest Participation (b) Blief in Others' Participation

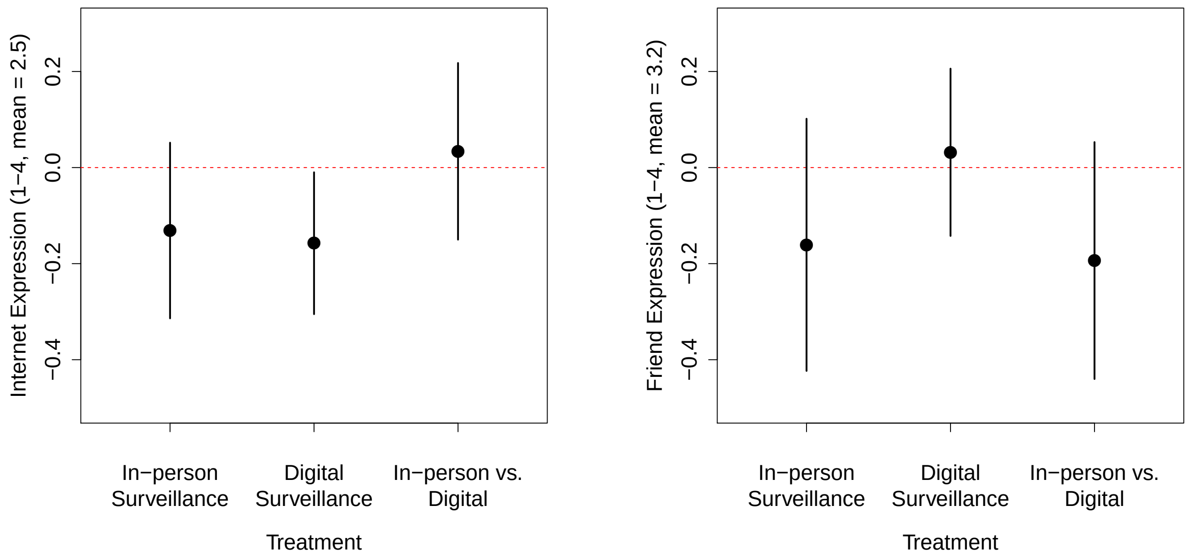
(Notes: OLS estimates with 95% Confidence Intervals. See Online Appendix B.1 for the regression tables underlying these figures.)

Figure 3: Protest Participation and Beliefs

respondents' beliefs.

Figure 4 plots the effects of digital and in-person surveillance on the online and offline expression of discontent. Both types of surveillance reduce respondents' willingness to express their discontent online and the effect of digital surveillance is statistically significant. The difference between the two types of surveillance is statistically insignificant. With regard to expression in front of fellow students, in-person surveillance has a negative but insignificant effect whereas the effect of digital surveillance is close to zero. The findings that digital surveillance discourages online expression but not offline expression meet common expectations.[7]

[7]Note that the results from Ordered Probit models (Table A.9 in Online Appendix) suggest the effects of both types of surveillance on online expression are statistically significant, and the effect of human surveillance on offline expression is also statistically significant.



(a) Online Expression (b) Expression to Fellow Students

(Notes: OLS estimates with 95% Confidence Intervals. See Online Appendix B.1 for the regression tables underlying these figures.)

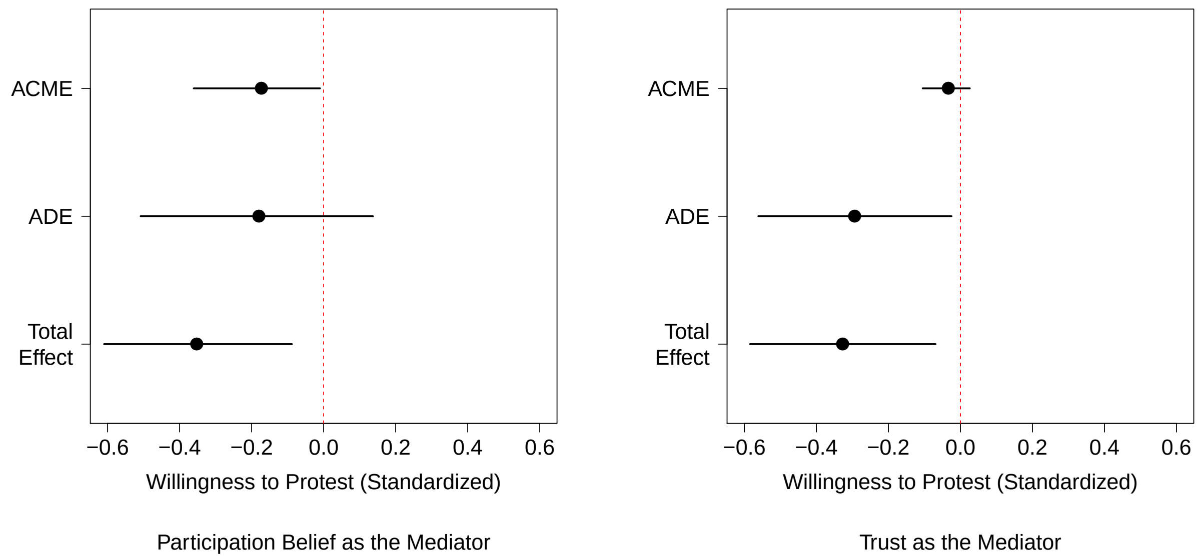
Figure 4: Online and Offline Expression

4.3 Causal Mediation Tests

I further examine the mechanisms through which surveillance deters protest participation using causal mediation analysis developed by Imai, Keele and Tingley (2010). As I mentioned in the theory section, both interpersonal trust and beliefs about others' participation could reduce individuals' willingness to protest. Thus, I use interpersonal trust and respondents' beliefs about others' turnout as mediators.

There are two major assumptions underlying the causal mediation test in identifying the mediation effect. First, there should be no unmeasured confounders between surveillance and the willingness to protest. Second, there should be no unmeasured confounders between the mediator and willingness to protest. The first assumption holds since the surveillance treatments are randomized. To address the second assumption, I control for as many covariates as possible, including age, gender, family income, income satisfaction, party affiliation, membership in official school organization, membership in student organizations, interest in discussing politics, media usage, and social distrust.

Figure 5 plots the results from the causal mediation analysis concerning digital surveillance. Individuals' belief about others' participation has a positive and statistically significant average causal mediation effect (ACME) on their own willingness to participate in the protest. In contrast, the ACME of trust is close to 0 and statistically insignificant.

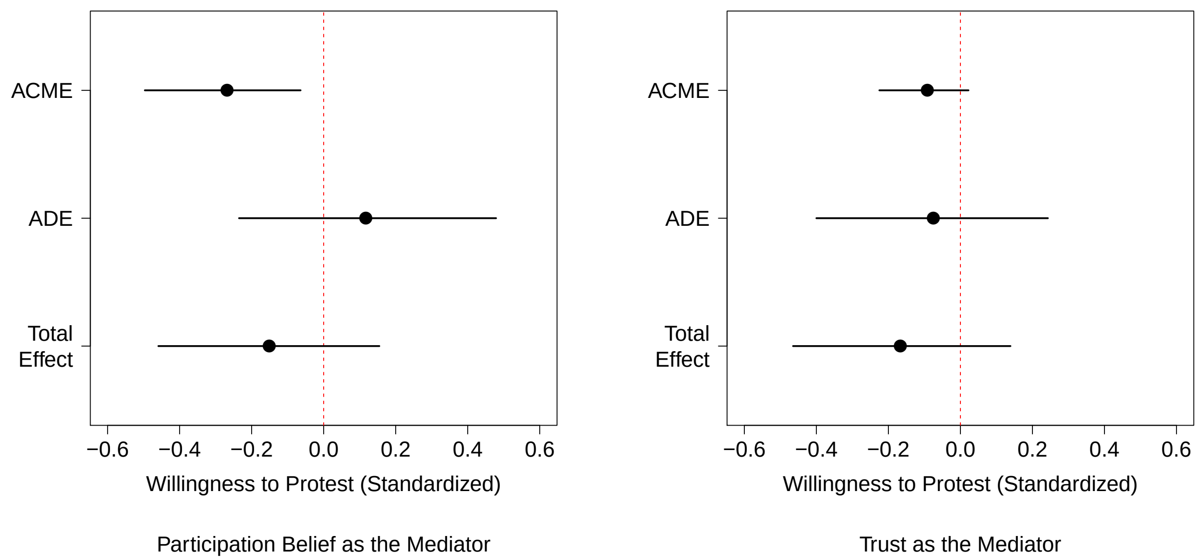


(Notes: 95% Confidence Intervals obtained via bootstrapping with 1000 resamples.)

Figure 5: Mediators between Digital Surveillance and Protest Participation

Figure 6 presents the causal mediation analysis concerning in-person surveillance. Although the total effect is not statistically significant, the average causal mediation effect of coordination beliefs on protest participation is still negative and statistically significant. The results from Figure 5 and Figure 6 suggest that surveillance discourages protest participation mainly through influencing individuals' strategic considerations about others' participation rather than their trust levels.[8]

[8]Note that belief about others' turnout is different from generalized trust. An individual may trust others but still think others would not participate under repression threat. The fact that participation belief serves as a mediator rather than trust suggests that the relationship between participation beliefs and trust is weak.



(Notes: 95% Confidence Intervals obtained via bootstrapping with 1000 resamples.)

Figure 6: Mediators between In-person Surveillance and Protest Participation

4.4 Discussion

Experimenter demand effects may bias estimates from a within-subject design because experimental subjects may tailor their responses to conform to their perceptions of the researcher's hypothesis due to answering the same question twice (before and after the treatment). However, recent literature (e.g., Mummolo and Peterson 2019) demonstrates that demand effects are typically modest and usually do not alter the treatment effects in survey and field experiments since research participants often exhibit a limited ability to infer researchers' expectations. In addition, experimenter demand effects caused by the within-subject design would likely produce similar outcomes for both the digital and the in-person surveillance treatment groups since respondents in both groups answer the repeated questions in the same way. The different findings between these two groups suggest the demand effects from the within-subject design are not very likely to be a concern. Moreover, for demand effects, if existed, to bias the results, respondents would have to be able to infer the researcher's intentions. But most of the respondents spent only about 5 minutes to complete the survey. It would be difficult for them to infer the researcher's intentions in such a short period, especially given that respondents in one group do not know the treatment condition of the other group.

I formally assess potential demand effects by comparing post-treatment responses between the within-subjects design group and the between-subjects design group. Because demand effects are less likely to present in the between-subjects design that has no repeated questions, if there are any demand effects, we would observe systematic differences in post-treatment responses between these two design groups. Figure A.1 in Online Appendix presents the details of the comparison, which shows that the differences in regime legitimacy, online expression, and protest participation between the two design groups are statistically *insignificant*. The difference in beliefs about others' participation between the two design groups is statistically insignificant for the in-person surveillance treatment group but significant for the digital surveillance treatment group, which are inconsistent. Only does trust show systematic difference between the two design groups. But this is also likely due to the fact that asking the trust question again makes respondents think about others' trustworthiness more carefully.

Information spillovers of the treatments are also unlikely to bias the survey results. First, the survey was not conducted in classrooms and dormitories where respondents would more likely be classmates or roommates. The survey enumerators randomly approached individual students on campus roads or in dining halls. Occasionally, respondents came in groups, but the survey enumerators made sure that the respondents did not communicate during the survey. This strategy reduces the likelihood of spillovers. Practically, if there were information spilling over from the treatment groups to the control group, such spillovers would bias estimates towards 0. Moreover, information spillovers between treatment groups would lead to similarities in the effects of in-person and digital surveillance. However, we see significant differences between the two treated groups, suggesting that information spillovers are unlikely to be a concern.

Finally, one might be concerned about the statistical inferences based on standard comparisons of means using the OLS estimation. As an alternative, I use the Randomization Inference approach to examine the statistical significance of the results (Gerber and Green 2012). I randomly assign (fictional) treatment status and estimate treatment effects 1,000 times. I then calculate the p-values of the estimated treatment effects from the actual treatment assignment based on the sampling distribution of the fictional treatment assignments. Two-tailed tests find very similar p-values for the treatment effects of surveillance on trust, legitimacy, and protest participation as those from the OLS estimation. See Table A.8 in Online Appendix for details.

5 Interrupted Time Series Design

To establish the external validity of my experimental results concerning digital surveillance, I provide additional evidence using a nationally representative sample of Chinese citizens. To be specific, I use the 2015 Chinese General Social Survey with a sample of 10,968 respondents and an interrupted time series design that exploits an exogenous shock to the Chinese government's digital surveillance operation caused by the Tianjin explosion in 2015. Due to the limitation of the observational data, I am not able to compare digital surveillance with in-person surveillance in a real-world setting. Nevertheless, recent empirical studies find that traditional, Stasi-style surveillance has long-lasting negative effects on interpersonal trust, institutional trust (i.e., regime legitimacy), and election participation (e.g., Lichter, Loeffler and Siegloch 2019), which lend external validity to my experimental findings on in-person surveillance.

5.1 2015 Tianjin Explosions and Government Surveillance

On August 12, 2015, a series of blasts in a Sinochem subsidiary's warehouse in the port of Tianjin killed 173 people and injured nearly a thousand (Merchant 2017). More than 17,000 housing units were damaged by the explosion, and 779 businesses suffered property damages. The two major explosions were caused by combustible fertilizer ammonium nitrate, detonated by fire and small explosions due to the misuse of firewater sprinklers on some chemicals (Huang and Zhang 2015). According to the earthquake waveform records, the first major explosion occurred at 11:34:06 pm, and the local earthquake magnitude (ML) was about 2.3. The second major explosion occurred 30 seconds later, and the ML was about 2.9. The resulting fireballs reached hundreds of meters high. The second explosion was estimated to be 336 tons TNT equivalent (Huang and Zhang 2015). Days later, local authorities ordered the evacuation of residents within a 3-kilometer (1.9-mile) radius of the blast site, prompted by the threat of "toxic substances", including sodium cyanide (Ryan 2015).

Immediately after the explosions, information on the event, including blast videos, was released over social media platforms like Weibo and WeChat. This accident drew a great deal of attention among Chinese netizens, with the topic racking up more views on Weibo than the country's total population of nearly 1.4 billion (Dou 2015). Figure 7 shows the temporal distribution of the Baidu Index from Mainland China using "Tianjin" as the keyword. We can see the search intensity peaked in the two weeks immediately following the accident.

Figure 7: Baidu Index on Tianjin Explosions

The devastating explosions raised serious questions about corruption, industrial safety, and emergency responses in China (Merchant 2017; Dou 2015). As soon as discussions and rumors went viral on the Internet and social media, the country's Internet surveillance and censorship machines operated at full capacity to control information and silence discussions. Data from the censorship tracker Weiboscope, developed by the Department of Journalism at the University of Hong Kong, shows that surveillance and censorship rates on Weibo were up tenfold after the explosions compared with earlier in the month (Dou 2015). A large number of posts and discussions were rapidly deleted on the Internet and social media. These include facts about the casualties, pictures, and videos of the explosions and the site afterward, investigative reports, as well as comments that question the political ties of the warehouse owners, criticize the government's responses, and discuss the chemicals inside the warehouse (Dou 2015; Hanrahan 2015).

One may argue that the aftermath of this event is a manifestation of government censorship instead of surveillance. However, conceptually, censorship is the combination of digital surveillance and online repression – censors need to identify the objectionable posts first and then delete them. Netizens who see posts being censored, even if they did not re-post or comment on them, certainly know that the government is monitoring the Internet and repressing online expression. As I discussed in the theory section, surveillance deters political participation because surveillance is associated with targeted repression. This is also the logic behind online censorship. The Tianjin accident creates an exogenous shock to online censorship and hence can be used to identify the causal effects of digital surveillance (and online repression) on individuals' trust and political participation.

5.2 Empirical Strategy and Data

My empirical strategy takes advantage of the coincidence that the 2015 Chinese General Social Survey (CGSS) was being conducted across China around the time of the Tianjin blasts. The CGSS is a nationwide survey ran every other year by the China Survey and Data Center at Renmin University – one of the top research universities in China. It participates in the International Social Survey Program and is the most reliable social survey in China. Figure 8 presents the time, location, and the number of individuals surveyed in the 2015

CGSS, with the dashed line indicating the time of Tianjin blasts.

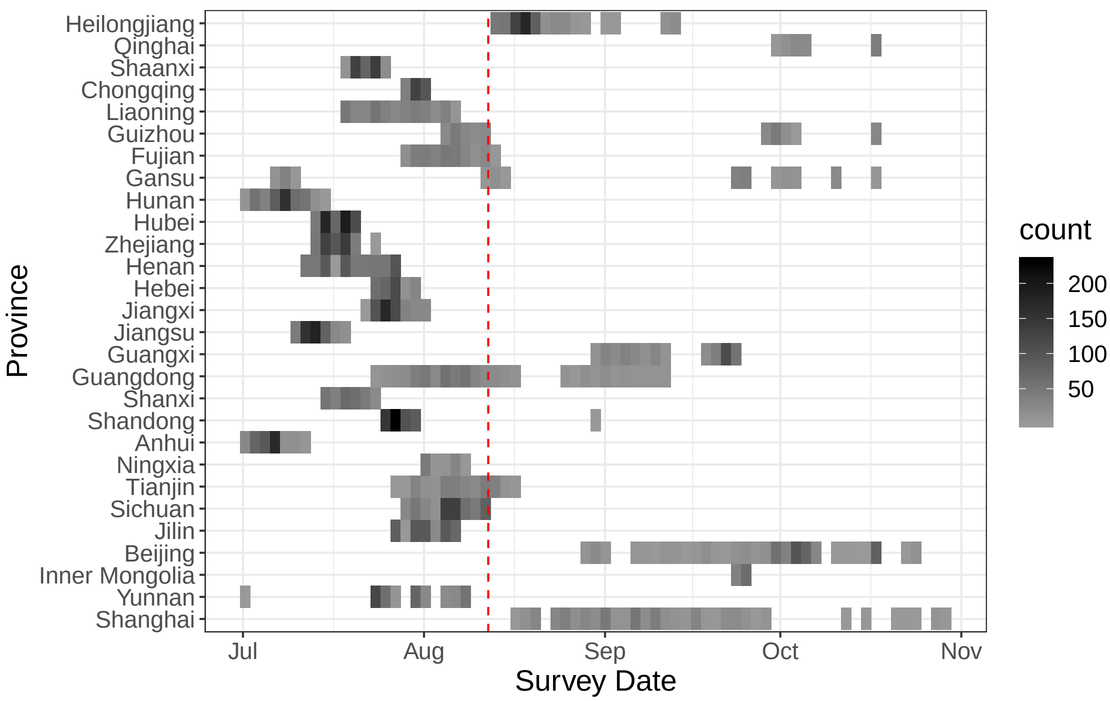


Figure 8: Distribution of Survey Respondents in the 2015 CGSS

Following Mummolo (2018), I use the interrupted time series (ITS) approach to compare individuals surveyed right before the Tianjin accident with individuals surveyed right after it.[9] As shown in Figure 8, some provinces do not have observations either before or after the accident. Because China is a large country where people in different regions may hold systematically different beliefs, the ITS estimate could be biased by regional differences. Thus, I include province fixed effects in the model. Specifically, I estimate the following equation:[10]

[9]Scholars also refer this approach to a Regression Discontinuity in Time design (e.g., Hausman and Rapson 2018).

[10]I use a parametric linear model as the main specification because this model allows me to add interactions for mechanism tests (see Section 5.3). It also allows selecting appropriate sample windows around the cutoff time to avoid the events that could bias the results. I also use the local polynomial RD models developed by Calonico, Cattaneo and Titiunik

$$Y_{ip} = \alpha + \delta\, cutoff_{ip} + \pi\, min_{ip} + \lambda\, cutoff_{ip} \cdot min_{ip} + X'_{ip}\,\Psi + province_p + \epsilon_{ip} \tag{1}$$

where i indexes the respondent and p the province; $cutoff_{ip}$ is a binary variable that takes the value of 1 if the respondent was interviewed after 11:59 pm on August 12, 2015 and 0 otherwise; min_{ip} is the running variable – the minute when the respondent was interviewed; X'_{ip} is a set of individual controls; $province_p$ is the province fixed effects. For Y_{ip}, I use four questions measuring interpersonal trust, regime legitimacy, views about expression, and views on petitioning (See Table B.1 in Online Appendix for the survey questions).

Non-netizens might not feel the intensified online surveillance after the Tianjin accident. I use individuals' Internet usage (including WAP phone services) to identify and exclude those who do not use the Internet (approximately half of the sample).[11] In addition, the identifying assumption of the ITS approach requires the groups of individuals surveyed before and after the time cutoff to be identical. As other concurrent events might influence respondents surveyed much earlier or much later, I further restrict the samples to one-week, two-week, and three-week windows (i.e., one week before and one week after, so on and so forth). I use the two-week window sample for the main analysis because people's interests peaked within two weeks after the Tianjin accident (Figure 7). Different time windows also serve as robustness checks for the ITS estimates. Table B.2 in Online Appendix shows the summary statistics of the samples.

The ITS design would be weakened if there were "precise" sorting of the survey respondents around the cutoff time to imbalance the treated and control groups. This is unlikely the case since, first, respondents could not decide when they were interviewed, and, second,

(2014) and find similar results (Panal A in Table B.8 in Online Appendix). Panel B and C in Table B.8 also show that the results are largely robust to nonlinear global polynomial specifications.

[11]After sample restriction, it is still much more representative than the student sample in the survey experiment.

survey organizers could not change the predetermined sampling scheme to select a biased sample after the accident. In addition, survey interviewers all over the country had neither incentives nor capabilities to select a systematically biased sample in terms of trust and participation right after the event. I further test the assumption of local randomization by looking at whether baseline covariates are balanced. Table B.3 in Online Appendix shows that a number of covariates have no statistically significant changes around the cutoff time.[12]

5.3 Specification for Mechanism Testing

One concern is whether the effects identified by the ITS approach are due to surveillance caused by the Tianjin Explosions or just due to the accident itself. Other unknown concurrent events could also bias the results. To examine the mechanism of digital surveillance, I construct a measure of provincial-level surveillance intensity using the number of pilot counties for China's Golden Shield Project (GSP)[13] – a domestic digital surveillance and content filtering system that integrates online government databases with an all-encompassing surveillance network developed by the Ministry of Public Security (Walton 2001).

The phase-in GSP was implemented in small scale in some prefectures in 2000s but carried over in large scale in early 2010s, especially the "3111" Initiative that built local networks of digital surveillance tools with integrated street surveillance cameras. Under the "3111" Initiative, a total of 660 pilot counties/districts were selected by provincial governments between 2008 and 2012 to build the surveillance camera systems (Li and Hikvision Digital Technology Co. 2015). The ratio of pilot counties to the total counties in a province reflects

[12] I check for discontinuities in other covariates at the cutoff instead of using the McCrary density test to examine the sorting problem because the density of the running variable (time) is uniform, which renders the test for discontinuities in its conditional density irrelevant (Hausman and Rapson 2018).

[13] I aggregate the measure at the provincial level because the CGSS only provides province identifiers.

the strength of digital surveillance in that province because the operation of surveillance camera systems requires well-developed surveillance infrastructures, integrated surveillance platforms, and sufficient security personnel. It also reflects the effort in surveillance operations made by security agencies in a province. In addition, although digital surveillance and censorship were mainly operated online, local security force is required to enforce punishment such as intimidation, harassment, and detention (Mozur 2019). Thus, despite that the "3111" Initiative aimed to build surveillance camera systems, the ratio of "3111" counties reflects the capacity of digital surveillance in a province.

Following Fitzpatrick (2010), I include an interaction term into Equation (1) to examine how the effects of surveillance and censorship caused by the Tianjin accident vary at different level of surveillance capacity. This method is an extension of Model (1) since it assumes the effects of digital surveillance on civil participation are conditional on surveillance capacity. In particular, I estimate the following specification.

$$\begin{aligned} Y_{ip} = \alpha + \delta\, cutoff_{ip} + \pi\, min_{ip} + \beta\, intensity_p + \gamma\, cutoff_{ip} \cdot intensity_p \\ + \lambda\, cutoff_{ip} \cdot min_{ip} + X'_{ip}\, \Psi + province_p + \epsilon_{ip} \end{aligned} \quad (2)$$

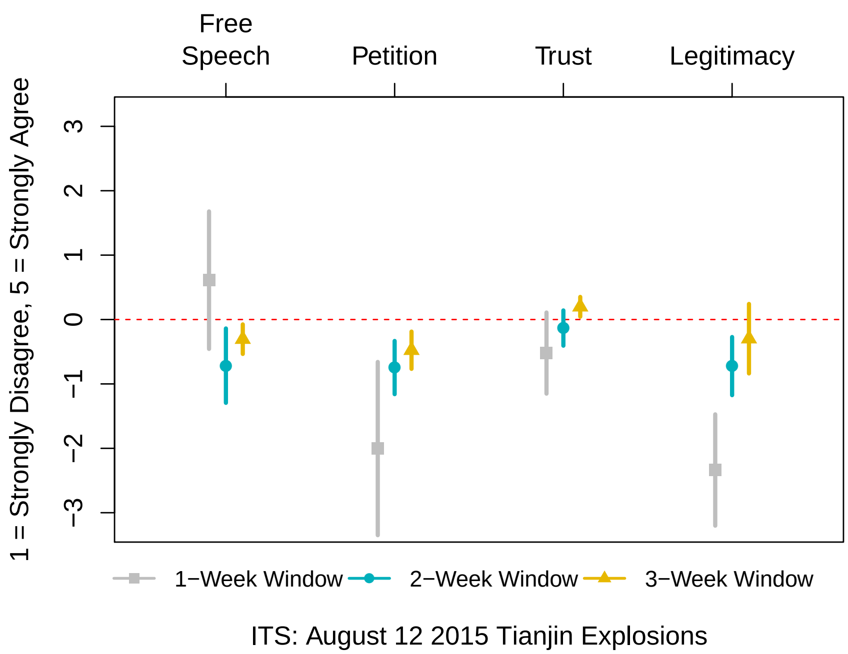
where $intensity_p$ is the ratio of "3111" pilot counties to the total counties in a province and $cutoff_{ip} \cdot intensity_p$ the interaction term. I expect γ to be negative and δ to be negative and statistically significant within certain ranges of $intensity_p$.

6 Findings from the 2015 CGSS Sample

This section presents the results of the Interrupted Time Series design using the 2015 CGSS data. I first present the main effects of the Tianjin Explosions on trust and political participation. Then, I show how the effects vary with the level of surveillance capacity.

6.1 Main Effects

Figure 9 plots the effects of digital surveillance (caused by the Tianjin Explosions) on individuals' view of free speech, perceived risk of petitioning to the government, interpersonal trust, and regime support. Under intensified digital surveillance, individuals are less likely to think that the right to criticize the government publicly is protected by law. They are more likely to think that petitions would be obstructed by the government. The effect on the view of petitioning is statistically significant on samples within the one-week, two-week, and three-week windows. The effect on the view of free speech is statistically significant on samples within the two-week and three-week windows. On the other hand, the effect of digital surveillance on trust is negative but statistically insignificant. These findings are consistent with the experimental results.



(Notes: This figure shows OLS estimates with 95% Confidence Intervals. Standard errors are clustered on prefectures. See Online Appendix B.3 for the regression tables underlying this figure.)

Figure 9: Tianjin Explosions on Political Participation, Trust, and Legitimacy

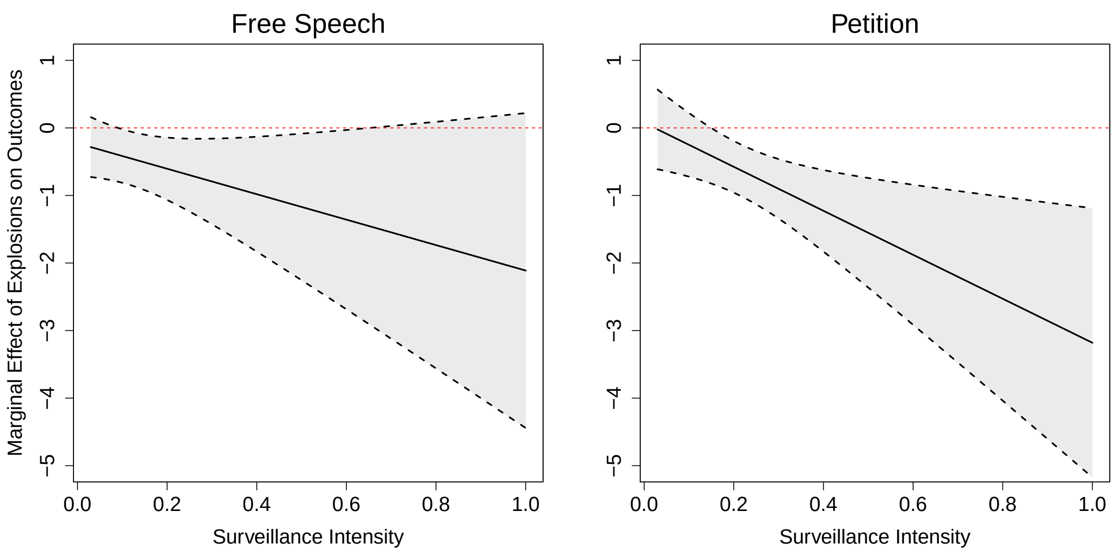
The theory and the experimental findings suggest that digital surveillance should not reduce regime legitimacy much. However, I find a negative effect of the Tianjin accident on regime legitimacy in the one-week and two-week windows. This inconsistency is likely due to the negative effect of the accident itself: after the horrifying explosions, people blamed the government for its failures to contain corruption, ensure industrial safety, and respond to emergencies, which, in turn, lower regime legitimacy.

6.2 Mechanism Testing

I further examine whether the negative effects of Tianjin Explosions on individuals' beliefs are conditional on the capacity of digital surveillance. In Figure 10, I plot the results of the ITS model with interaction between the time cutoff and surveillance capacity. The left panel shows that the marginal effect of Tianjin Explosions on individuals' views of free speech decreases with higher surveillance capacity and the effect is statistically significant when the capacity is within the 0.1 – 0.7 range. The right panel shows that the marginal effect of explosions on the view of petitioning also decreases with higher surveillance capacity and is statistically significant at most of the surveillance capacity levels. These findings provide strong evidence that digital surveillance is the driving force behind the decreased confidence in political participation after the Tianjin Explosions.

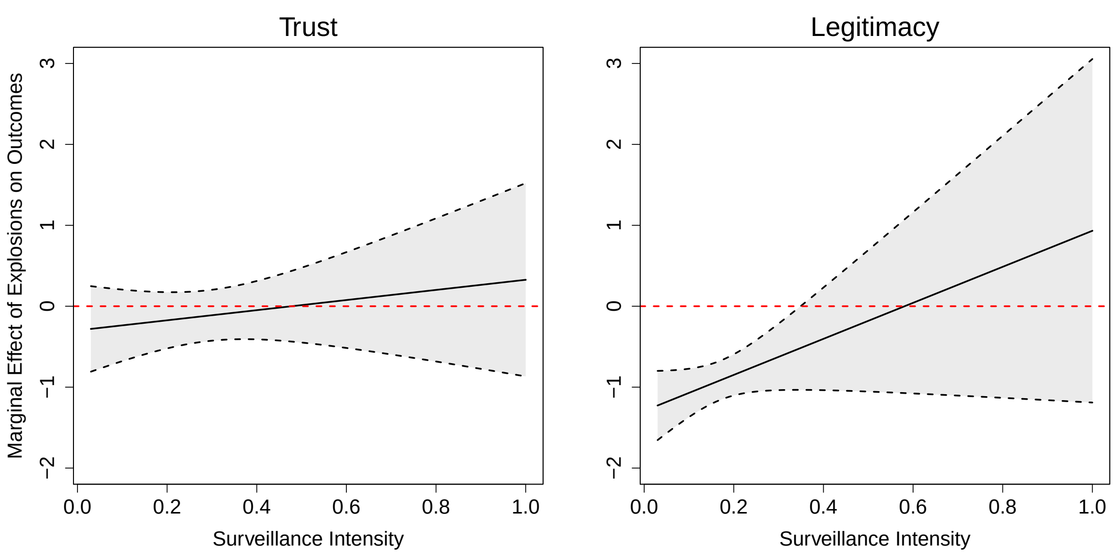
Figure 11 plots the marginal effects of the Tianjin accident on interpersonal trust and regime legitimacy. Consistent with our expectation, the marginal effect on trust is statistically insignificant. Interestingly, the marginal effect of the accident on regime legitimacy increases with higher surveillance capacity and is statistically significant when this capacity is within the 0 – 0.4 range. This provides further evidence that the reduced legitimacy is due to the Tianjin accident itself because stronger surveillance and censorship prevent the information about the accident from spreading to decrease citizens' regime approval.

The above findings from the 2015 CGSS sample are largely consistent with the experimental findings, which lend further weight to the main theoretical claims.



(Notes: OLS estimates with 95% Confidence Intervals. Standard errors clustered at the prefecture level. See Online Appendix B.3 for the regression results underlying these figures.)

Figure 10: Marginal Effects of Explosions on Political Participation



(Notes: OLS estimates with 95% Confidence Intervals. Standard errors clustered at prefecture level. See Online Appendix B.3 for the regression results underlying these figures.)

Figure 11: Marginal Effects of Explosions on Trust and Legitimacy

6.3 Robustness Tests

Some concurrent events could potentially bias the estimates of the ITS approach. Fortunately, during the four-week period, there were no big events or incidents that could invalidate the surveillance treatment caused by the Tianjin Explosions. On July 26, the death of a 31-year-old mother on an escalator set off a furor online, but this incident did not trigger surveillance and censorship. Even if there were censorship, it would lead to underestimation because censorship on this incident would have occurred before Tianjin Explosions. There were a few small incidents such as a chemical explosion in Shandong (August 31, 5 death) and landslide in Shaanxi (August 12, 26 missings), but none of them caught nationwide attention in China. There are two relatively large events around Tianjin Explosions: a stock market rollercoaster (June 12 – July 10) and the 70th Anniversary Parade of China's Victory over Japan Day (September 3). But both events were not in the four-week treatment window. Even if they had any influence on citizens during the four-week window, the stock market crash would have reduced trust and regime legitimacy before the treatment time, and the anniversary parade would have increased regime legitimacy after the treatment time. Thus, both events would lead to underestimation of the treatment effects. Figure B.1 in Online Appendix plots Google search trends for a number of events; none of them could threat the validity of the ITS design.

Another concern is that the results could be driven by the differences between individuals surveyed before and after the Tianjin incident. Individuals from different regions may hold different opinions to bias the ITS estimates if the pre- and post-treatment samples were drawn from different regions. I control for province fixed effects to reduce this potential bias. Further, I conduct placebo tests on a series of outcomes that should not be affected by Tianjin Explosions. The logic is that, if it were regional differences that drive the differences in the outcomes of interest, we would observe similar patterns in other attitudes and behavior. However, I did not find significant differences between the pre- and post-treatment groups in terms of randomly picked questions such as attitudes and behavior concerning inequality, gender role, housework, voting, one-child policy, and homosexuality (Table B.7 in Online Appendix). Another concern is that the Tianjin incident could cause some people to become new Internet users afterward, changing the post-treatment sample. Although I cannot rule out this possibility, the fact that the covariates and other outcomes are balanced between the pre- and post-treatment groups suggests that self-selection is not a concern.

Moreover, the mechanism test that the Tianjin Explosions event has stronger effects in areas with higher surveillance capacity provides further evidence for the theoretical argument, which suggests that concurrent events and sample selection are unlikely to invalidate the findings.

7 Conclusion

Dictatorships have a long history of using surveillance to collect information and control society. Yet, we rarely saw traditional surveillance states as massive as today's digital surveillance states. This paper compares digital surveillance with in-person surveillance in terms of citizens' responses. Using a field survey experiment in China, I show that digital surveillance is less intrusive than in-person surveillance in the sense that it is less likely to lower interpersonal trust and regime legitimacy. On the other hand, digital surveillance is as effective as in-person surveillance in deterring political participation. Evidence from a natural experiment using the 2015 Chinese General Social Survey is consistent with the experimental findings. To sum up, digital surveillance deters expression and protest participation and, meanwhile, does not have the intrusiveness of in-person surveillance to prevent its expansion. The unintrusive nature of digital surveillance, in part, explains why state surveillance expands so rapidly in the digital age, which makes it a more dangerous tool of authoritarian control than in-person surveillance.

This paper highlights the unintrusive nature of digital surveillance as an explanation for the rapid expansion of digital surveillance around the world. This is certainly not the only reason. The development of surveillance states in the digital age may also depends on governments' information scarcity, resources, and technological know-how, as well as citizens' support for the governments and ignorance of the repressive nature of surveillance. Public security incidents or potential threats such as terrorist attacks, natural disasters, the COVID pandemic, and other health crises may also increase states' digital surveillance powers. Besides, states may exploit citizens' misperceptions about potential threats to justify the expansion of surveillance capacity. Nevertheless, the lack of sensorial cues in digital surveillance and its unintrusive nature imply double risks to citizens: they are not only less likely to resist the expansion of digital surveillance but also less cautious about privacy protection. This makes citizens in digital surveillance states even more vulnerable.

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Online Appendix

A Experiment: Implementation, Data, and Findings

A.1 Implementation and Ethics

Implementation

Conducting the survey experiment on a potentially sensitive topic in the field circumvents state censorship that may be present in China-based online survey platforms. It also helps create trust and cooperation from respondents. More importantly, since one of the treatment conditions is online surveillance, it may induce respondents' self-censorship in online surveys. An in-the-field survey experiment avoids this problem because respondents answer questions on paper questionnaires.

The enumerators conducted the survey in dining halls and main roads between classroom buildings and residential halls. For a convenience sample, respondents were recruited in those areas to represent the student population better than in dormitories or classrooms because all students come to dining halls and main roads regardless of their major, gender, and year at university. Survey questionnaires require five to ten minutes to complete. Respondents were requested to complete the questionnaire independently to minimize potential spillover effects of the treatments. Each student received five Chinese Yuan (about 0.75 USD) as compensation. The six different versions of questionnaires were placed in random order. The enumerators asked students whether they were willing to participate in an *anonymous* survey first, and if they agreed, the enumerators then presented the five-Yuan compensation to them and gave them the questionnaires in the random order. Roughly 50 percent of the students approached by enumerators agreed to participate. This response rate is within the normal range for a field survey. In addition, most of the non-respondents refused before the enumerators explained the survey topic to them – their unwillingness to participate was thus not due to the content of the survey but unrelated excuses including "no time", "hungry", "too busy", etc. Thus, it is unlikely that non-response is related to potential outcomes that would bias results.

Ethical Considerations

I sought and obtained research approval for the study from the Institutional Review Board (IRB) at XXX. Given that this is essentially a five-minute opinion survey with minimal risk, it received an IRB exemption. I further take the following efforts to protect the rights and wellbeing of research participants and field staff.

First, I design the entire questionnaire to focus on students' campus lives without mentioning any sensitive political issues in China. This framing strategy reduces the risk of participating in the survey. In addition, I ask students' attitudes toward the school authority rather than the government. The hypothetical confrontation is between students and the school authority instead of between citizens and the state. Such questions are safe in the political context of China. During the recruitment and survey procedures, no student refused participation because of the content of the questionnaire. Second, I use an online petition instead of a street protest because stating participation in the former is less sensitive and safer for respondents, field staff, and the researcher. Third, in each university, I consulted with several faculty members and students to proofread the questionnaire to ensure it did not incur any risk to respondents. Finally, the survey is anonymous and the enumerators were requested to stay away from respondents when respondents were filling in the questionnaire. A questionnaire that does *not* collect personal information reduces the risk of a loss of confidentiality and any other potential risks to the subjects. These strategies not only reduce potential risks to respondents, but also minimize respondents' social desirability bias and self-censorship in answering questions.

A.2 Scenarios and Treatments

Table A.1 shows the vignette of the hypothetical scenario under which students confront the university authority for an unfair policy involving changing dormitories. Table A.2

shows the scenarios for in-person surveillance treatment, digital surveillance treatment, and control (no surveillance) conditions.

Table A.1: Scenario of Political Participation

Imagining the following scenario:

Without holding any public hearings or conducting any opinion surveys among the students, the university unilaterally notifies you and other students who live in a new residence hall to move to one of the university's oldest and poorly maintained dormitories. Furthermore, your housing rates are not reduced. The new residence hall that you are currently living in will be freed up to accommodate an increased number of freshmen due to the university's recent enrollment expansion. You and the other students in your residence hall are very upset with the university's decision and are complaining about this change. You and others are considering filing official complaints to the Ministry of Education who oversees your university, using the Ministry's online mailbox. The more students participate, the more likely you will push the ministry to change the university's decision. But once the university finds out who participated, it may interrogate the participants or even record a demerit.

Table A.2: Treatment and Control Scenarios

Treatment 1: In-person Surveillance

The Ministry of Education's online mailbox can be filed anonymously. However, the university authority approached some of the students in your residence hall and promised them some benefits (you do not know who those students are and what benefits will they receive). In exchange, those students agreed to secretly investigate and report the names of the students who participate in the filing process as well as the students who promoted this protest.

Treatment 2: Digital Surveillance

The Ministry of Education's online mailbox can be filed anonymously. However, the university authority can monitor students' online activities (on social media/apps, websites, forums, etc.) through the university's Internet servers to identify the students who participate in the filing process as well as the students who promoted this protest.

Control: No Surveillance

Because the Ministry of Education's online mailbox can be filed anonymously, the university does not know who participates in the protest.

A.3 Outcome Questions

Table A.3 lists the outcome questions concerning political participation and interpersonal trust. As mentioned in the main text, the responses for willingness to express and protest take a ordinal scale from 1 to 4, with 4 indicating the most affirmative answer. Interpersonal trust and regime legitimacy are measured on an ordinal scale from 0 to 10, with 10 indicating the highest level of trust or approval. The belief in other's participation is a percentage value ranging from 0% to 100% at a 10% step.

Table A.3: Outcome Questions in the Survey

Trust: To what extent do you think the students in your residential hall can be trusted? **Legitimacy:** Do you approve of the university's general policies, rules, and actions? **Protest Participation:** Are you planning to file a complaint to the Ministry of Education? **Belief in Others' Participation:** Please guess what percentage of the students from your current dormitory will file a complaint. **Expression to Fellow Students:** Will you express your discontent concerning the university's policy in front of your college peers? **Online Expression:** Will you express your discontent concerning the university's policy on the Internet, such as social media, online forums, etc.?

A.4 Balance Tests

Table A.4 reports the covariate balance across control and treatment groups on a number of background questions, including age, gender, family income, income satisfaction, party affiliation, membership in official university organizations, membership in student societies, community service, interest in discussing politics, media usage, social distrust, online expression. As shown in Table A.4, randomization is successful and the treatment is balanced across all these covariates. Note that the *Distrust* variable is slightly unbalanced. Thus, I control for this variable in all model specifications.

Table A.4: Balance Check, The Within-Subject Design Sample

	obs.	Control	Human	Digital	p-value
Age	337	20.41	20.47	20.60	0.772
Female (F=1)	341	0.52	0.48	0.51	0.831
Income (1-9)	338	6.66	6.59	6.81	0.644
Income Sat. (0-10)	342	6.62	6.96	6.80	0.582
Party (Yes=1)	343	0.09	0.11	0.11	0.845
Offical Org. (Yes=1)	343	0.46	0.48	0.54	0.501
Stud. Org. (Yes=1)	342	0.64	0.67	0.61	0.700
Commu. Serv. (1-5)	343	2.52	2.61	2.60	0.710
Speech (1-5)	342	3.11	3.06	3.07	0.896
Media: News (1-5)	334	2.08	2.10	2.12	0.925
Media: TV (1-5)	337	3.02	2.91	2.91	0.638
Media: Phone (1-5)	343	4.72	4.73	4.75	0.896
Distrust (0-10)	342	4.22	4.57	3.81	0.175
Diss. Politics (1-5)	341	2.30	2.13	2.25	0.442

A.5 Experimental Findings

Results from OLS regressions

The following tables report the results from OLS regressions. Table A.5 presents the effects of different types of surveillance on interpersonal trust and regime legitimacy. Table A.6 shows the results concerning protest participation and belief in others' participation. Table A.7 reports the results regarding online expression and expression to fellow students.

Table A.5: Trust and Legitimacy

	Trust			*Legitimacy*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat.Human	-1.350^{***}			-1.368^{***}		
	(0.219)			(0.287)		
Treat.Digital		-0.616^{***}			-0.284	
		(0.149)			(0.175)	
Human vs. Digital			-0.742^{***}			-1.041^{***}
			(0.230)			(0.278)
Distrust	0.003	-0.013	0.002	-0.011	0.056	-0.060
	(0.035)	(0.028)	(0.037)	(0.052)	(0.036)	(0.042)
Univ. FEs	-0.103	-0.152	-0.210	0.317	0.229	-0.226
	(0.205)	(0.145)	(0.225)	(0.268)	(0.185)	(0.265)
Constant	0.390^{*}	0.482^{***}	-0.159	-0.178	-0.419^{**}	-0.010
	(0.204)	(0.176)	(0.198)	(0.274)	(0.196)	(0.259)
Observations	220	239	213	221	239	213
R^2	0.166	0.075	0.051	0.113	0.032	0.051
Adjusted R^2	0.154	0.063	0.038	0.100	0.019	0.038

Note: Robust standard errors are clustered on universities. *p<0.1; **p<0.05; ***p<0.01

Table A.6: Protest Participation and Beliefs

	Protest Participation			*Belief in Others' Participation*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat.Human	-0.131			-0.161		
	(0.093)			(0.134)		
Treat.Digital		-0.158^{**}			0.032	
		(0.075)			(0.089)	
Human vs. Digital			0.034			-0.194
			(0.094)			(0.126)
Distrust	-0.004	0.009	-0.012	-0.026	-0.003	-0.015
	(0.016)	(0.013)	(0.015)	(0.020)	(0.014)	(0.019)
Univ. FEs	-0.137	-0.044	-0.119	-0.082	0.067	0.026
	(0.091)	(0.076)	(0.094)	(0.129)	(0.091)	(0.124)
Constant	0.086	-0.018	-0.052	0.005	-0.167^{*}	-0.068
	(0.115)	(0.100)	(0.111)	(0.124)	(0.088)	(0.120)
Observations	220	239	215	221	240	215
R^2	0.020	0.023	0.011	0.016	0.003	0.016
Adjusted R^2	0.006	0.011	-0.003	0.002	-0.009	0.002

Note: Robust standard errors are clustered on universities. *p<0.1; **p<0.05; ***p<0.01

Table A.7: Online and Offline Expression

	Online Expression			*Expression to Fellow Students*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat.Human	−0.131			−0.161		
	(0.093)			(0.134)		
Treat.Digital		−0.158**			0.032	
		(0.075)			(0.089)	
Human vs. Digital			0.034			−0.194
			(0.094)			(0.126)
Distrust	−0.004	0.009	−0.012	−0.026	−0.003	−0.015
	(0.016)	(0.013)	(0.015)	(0.020)	(0.014)	(0.019)
Univ. FEs	−0.137	−0.044	−0.119	−0.082	0.067	0.026
	(0.091)	(0.076)	(0.094)	(0.129)	(0.091)	(0.124)
Constant	0.086	−0.018	−0.052	0.005	−0.167*	−0.068
	(0.115)	(0.100)	(0.111)	(0.124)	(0.088)	(0.120)
Observations	220	239	215	221	240	215
R^2	0.020	0.023	0.011	0.016	0.003	0.016
Adjusted R^2	0.006	0.011	−0.003	0.002	−0.009	0.002

Note: Robust standard errors are clustered on universities. *p<0.1; **p<0.05; ***p<0.01

Results from Randomization Inference Methods

Table A.8 shows the results from the randomization inference approach. The test statistics were calculated from randomly assigning (fictional) treatment status and estimating treatment effects 1,000 times. The statistical significance of the estimates is consistent with that of the OLS estimation. Note that, following Gerber and Green (2012), the random assignment of fictional treatment status is nested within the male and female sample blocks because I used a block random assignment procedure to balance students' gender in the original experimental design.

Table A.8: Test Statistics from the Randomization Inference Approach

	Estimate	Two-Tailed P-Value	N of Obs.
	Trust		
Treat Human	-1.375	0.000	219
Treat Digital	-0.598	0.000	239
Human vs. Digital	-0.767	0.004	212
	Legitimacy		
Treat Human	-1.390	0.000	220
Treat Digital	-0.325	0.116	239
Human vs. Digital	-1.060	0.001	213
	Protest Participation		
Treat Human	-0.123	0.281	220
Treat Digital	-0.280	0.002	239
Human vs. Digital	0.158	0.146	213
	Blief in Others' Participation		
Treat Human	-17.963	0.000	218
Treat Digital	-16.174	0.000	238
Human vs. Digital	-1.739	0.546	212
	Online Expression		
Treat Human	-0.137	0.235	219
Treat Digital	-0.155	0.092	239
Human vs. Digital	0.018	0.912	214
	Expression to Fellow Students		
Treat Human	-0.179	0.240	220
Treat Digital	0.036	0.711	240
Human vs. Digital	-0.212	0.139	214

Note: The estimates are slightly different from OLS coefficients because the OLS specifications include a few control variables and drop several observations.

Results from Ordered Probit Models

Table A.9 shows the results regarding online expression, offline expression, and protest participation using Ordered Probit Models. We can see that both types of surveillance lower willingness to protest and express online and the effects are statistically significant. Human surveillance deters expression to fellow students whereas digital surveillance does not. Also, there are no statistically significant differences between human and digital surveillance in affecting these outcomes. The results are largely consistent with the OLS results and have better statistical significance. Note that the measures of trust and legitimacy take a scale of more than 10 points so that I do not use Ordered Probit models for those variables.

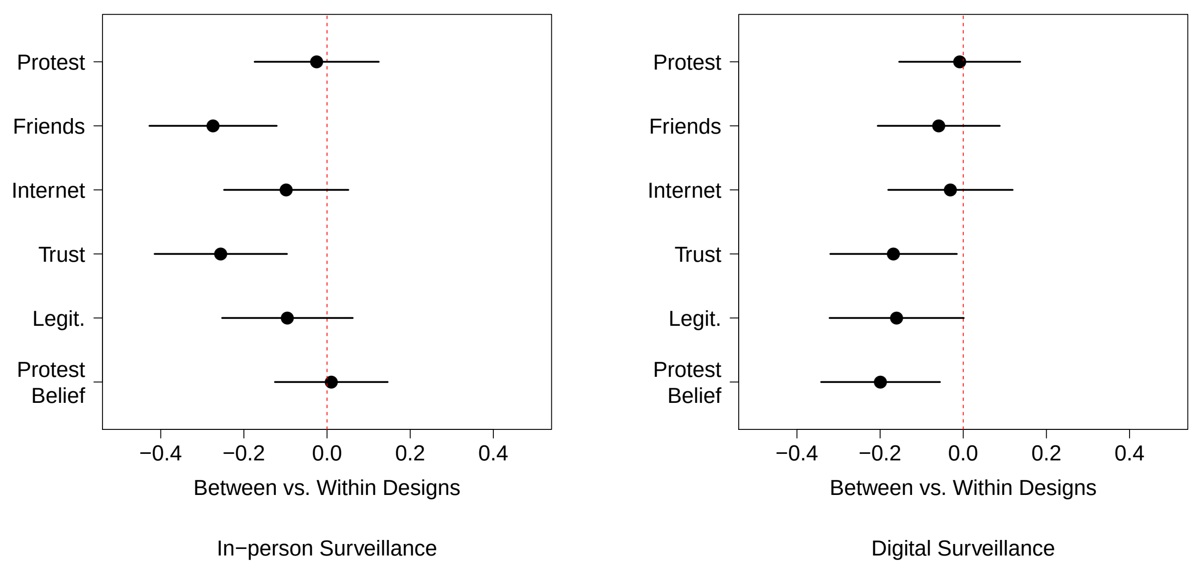
Table A.9: Online/Offline Expression & Political Participation (Ordered Probit)

	Protest Participation			Online Expression			Expression to Fellow Students		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Human Suv.	-0.264***			-0.216**			-0.164**		
	(0.098)			(0.089)			(0.071)		
Digital Suv.		-0.544***			-0.357***			0.000	
		(0.031)			(0.038)			(0.097)	
Hum. vs. Digi.			0.291			0.120			-0.165
			(0.180)			(0.138)			(0.173)
Gen. Distrust	0.028***	0.034	-0.018	-0.020	0.012	-0.019	-0.024***	0.002	-0.006
	(0.011)	(0.062)	(0.055)	(0.013)	(0.035)	(0.011)	(0.001)	(0.007)	(0.008)
Univ. Fixed	-0.036***	0.064*	-0.050***	-0.256***	-0.115***	-0.201***	-0.087***	0.092***	0.008*
	(0.004)	(0.033)	(0.004)	(0.019)	(0.014)	(0.004)	(0.008)	(0.005)	(0.005)
Constant cut1	-2.787***	-2.913***	-2.359***	-2.724***	-3.001***	-2.653***	-2.361***	-2.485***	-2.228***
	(0.356)	(0.030)	(0.301)	(0.371)	(0.258)	(0.127)	(0.045)	(0.050)	(0.065)
Constant cut2	-2.225***	-2.462***	-1.259***	-2.458***	-2.362***	-2.123***	-1.735***	-1.745***	-1.788***
	(0.194)	(0.264)	(0.204)	(0.243)	(0.219)	(0.008)	(0.066)	(0.087)	(0.002)
Constant cut3	-1.664***	-1.589***	1.425***	-1.659***	-1.479***	-1.248***	-1.235***	-1.125***	-1.069***
	(0.017)	(0.301)	(0.068)	(0.094)	(0.167)	(0.012)	(0.109)	(0.048)	(0.157)
Constant cut4	1.075***	1.160***	2.168***	1.181***	1.409***	1.537***	1.284***	1.628***	1.376***
	(0.030)	(0.241)	(0.079)	(0.125)	(0.143)	(0.107)	(0.041)	(0.112)	(0.091)
Constant cut5	1.943***	2.050***		1.979***	2.390***	2.168***	1.720***	2.344***	1.940***
	(0.138)	(0.523)		(0.180)	(0.464)	(0.252)	(0.069)	(0.081)	(0.017)
Constant cut6							2.289***		2.405***
							(0.077)		(0.048)
Observations	225	246	223	226	246	224	224	244	222
R-squared	0.155	0.051	0.046	0.008	0.050	0.019	0.189	0.206	0.003

Robust standard errors are clustered on universities.
*** p<0.01, ** p<0.05, * p<0.1

A.6 Demand Effect Tests

Figure A.1 shows the differences in outcome variables between the between-subject design and within-subject design (also in Table A.10 and Table A.11). I standardize the scale of the outcome variables for comparison. With regard to protest participation, expression to fellow students, online expression, and regime legitimacy, the differences between the two designs are either statistically insignificant or in the opposite directions from the main results. The difference in respondents' belief in others' participation is negative and statistically significant in the digital surveillance sample. However, as discussed in the main text, experimenter demand effects are not of much concern if the in-person surveillance sample and the digital surveillance sample show different patterns. Only does interpersonal trust show systematic differences between the two designs for both the in-person and digital surveillance samples. This is likely due to the fact that asking the trust question again makes respondents think about their interpersonal trust more carefully. Above all, the results suggest that the demand effect is unlikely a concern, though we cannot fully rule it out.



(Notes: OLS estimates with 95% Confidence Intervals. See Table A.9 and A.10 for the regression results underlying these figures.)

Figure A.1: Demand Effect Tests

Table A.10: Demand Effect Tests for Human Surveillance Treatment

	Dependent variables:					
	(1)	(2)	(3)	(4)	(5)	(6)
	Protest	Exp.Online	Exp.Offline	Trust	Legitimacy	Protest Belief
Pre/Post-test	−0.023	-0.263^{***}	−0.094	-0.248^{***}	−0.094	0.017
	(0.073)	(0.074)	(0.077)	(0.076)	(0.079)	(0.066)
Party Member	0.129	0.013	−0.078	−0.079	−0.092	0.057
	(0.089)	(0.067)	(0.086)	(0.104)	(0.096)	(0.069)
Distrust	0.008	0.083	0.008	−0.085	-0.192^{**}	0.179^{***}
	(0.078)	(0.083)	(0.081)	(0.080)	(0.082)	(0.067)
Univ. FEs	0.111	0.109	−0.013	−0.103	0.021	−0.082
	(0.160)	(0.169)	(0.161)	(0.169)	(0.164)	(0.144)
Constant	−0.157	-0.281^{**}	−0.059	−0.135	−0.167	-0.242^{***}
	(0.106)	(0.114)	(0.108)	(0.125)	(0.110)	(0.093)
Observations	161	161	161	160	160	158
R^2	0.016	0.073	0.017	0.071	0.050	0.047
Adjusted R^2	−0.010	0.049	−0.009	0.047	0.026	0.022

Note: Robust standard errors are clustered on universities. *p<0.1; **p<0.05; ***p<0.01

Table A.11: Demand Effect Tests for Digital Surveillance Treatment

	Dependent variables:					
	(1)	(2)	(3)	(4)	(5)	(6)
	Protest	Exp.Online	Exp.Offline	Trust	Legitimacy	Protest Belief
Pre/Post-test	−0.013	−0.060	−0.034	-0.166^{**}	-0.163^{**}	-0.197^{***}
	(0.077)	(0.073)	(0.077)	(0.074)	(0.081)	(0.072)
Party Member	−0.078	−0.096	-0.154^{**}	0.003	−0.001	-0.145^{*}
	(0.077)	(0.088)	(0.070)	(0.084)	(0.091)	(0.082)
Distrust	−0.014	−0.050	−0.060	−0.091	−0.038	0.086
	(0.075)	(0.082)	(0.078)	(0.079)	(0.083)	(0.070)
Univ. FEs	0.085	0.142	−0.011	−0.048	−0.080	−0.149
	(0.157)	(0.160)	(0.162)	(0.161)	(0.168)	(0.149)
Constant	−0.106	−0.037	−0.044	0.050	0.058	0.010
	(0.107)	(0.110)	(0.115)	(0.126)	(0.122)	(0.106)
Observations	175	176	176	175	175	173
R^2	0.011	0.026	0.028	0.036	0.026	0.074
Adjusted R^2	−0.013	0.003	0.005	0.013	0.004	0.052

Note: Robust standard errors are clustered on universities. *p<0.1; **p<0.05; ***p<0.01

B Interrupted Time Series Analysis

B.1 Outcome Questions

Table B.1 shows the questions measuring interpersonal trust, regime legitimacy, views about expression, and views on petitioning from the 2015 Chinese General Social Survey (CGSS).

Table B.1: Measures of Trust, Legitimacy, and Participation in the CGSS

View on Free Speech: To what extent does the following statement reflect the reality in China? The right to criticize the government publicly is protected by law. **View on Petition:** Do you agree or disagree with the following statement concerning petitions in China? Petitions are not obstructed. **Interpersonal Trust:** Generally speaking, do you agree that most people can be trusted in society? **Regime Legitimacy:** Do you agree with the following statement? Some policy reforms of the government bodies violate current laws but those policies have good intentions and work well. Such government reforms deserve recognition and appraisal.

B.2 Summary Statistics and Balance Tests

Table B.2 shows the summary statistics for the samples of CGSS respondents surveyed within two weeks, one week, and three weeks around the time of the Tianjin Explosions. The variable *Cutoff* takes a value 1 if the respondent is surveyed after 09:07 PM on August 12, 2015, and 0 otherwise. The "treated" individuals constitute 23 percent, 33 percent, or 20 percent of the two-week, one-week, or three-week samples respectively.

To identify the causal effect of Tianjin Explosions, the Interrupted Time Series Design requires the observations in the pre-event group are identical to those in the post-event group. Table B.3 shows the balance tests of a series of covariates for the two-week window sample. We can see that the pre-event and post-event groups are balanced in terms of gender, age, ethnicity, income, education, rural-urban composition. These two groups only show a

Table B.2: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Two-Week Window							
Cutoff	1,576	0.23	0.4	0	0	0	1
Age	1,576	39.5	14.3	18	28	50	93
Minority	1,575	0.1	0.2	0.0	0.0	0.0	1.0
Education	1,571	6.6	3.3	1.0	4.0	10.0	13.0
Female	1,576	0.5	0.5	0	0	1	1
Income(log)	1,454	8.4	4.6	−2.3	9.2	10.8	15.4
Rural	1,576	0.8	0.4	0	1	1	1
Party	1,571	0.1	0.3	0.0	0.0	0.0	1.0
Trust	1,573	3.4	0.9	1.0	3.0	4.0	5.0
Petition	517	2.5	1.0	1.0	2.0	3.0	5.0
Free spch.	510	3.0	0.9	1.0	2.0	4.0	5.0
Legitimacy	502	3.4	0.8	1.0	3.0	4.0	5.0
One-Week Window							
Cutoff	718	0.33	0.5	0	0	1	1
Age	718	40.0	14.1	18	28.2	49	93
Minority	717	0.1	0.2	0.0	0.0	0.0	1.0
Education	717	6.5	3.3	1.0	4.0	10.0	13.0
Female	718	0.5	0.5	0	0	1	1
Income(log)	661	8.7	4.4	−2.3	9.2	10.8	15.4
Rural	718	0.7	0.4	0	0	1	1
Party	715	0.1	0.3	0.0	0.0	0.0	1.0
Trust	716	3.4	0.9	1.0	3.0	4.0	5.0
Petition	223	2.4	1.0	1.0	2.0	3.0	5.0
Free spch.	220	2.9	0.9	1.0	2.0	4.0	5.0
Legitimacy	221	3.3	0.8	1.0	3.0	4.0	5.0
Three-Week Window							
Cutoff	2,692	0.20	0.4	0	0	0	1
Age	2,692	39.3	13.9	18	28	48	93
Minority	2,690	0.1	0.2	0.0	0.0	0.0	1.0
Education	2,686	6.6	3.2	1.0	4.0	10.0	13.0
Female	2,692	0.5	0.5	0	0	1	1
Income(log)	2,524	8.3	4.7	−2.3	9.2	10.8	16.1
Rural	2,692	0.7	0.4	0	0	1	1
Party	2,686	0.1	0.3	0.0	0.0	0.0	1.0
Trust	2,687	3.4	1.0	1.0	3.0	4.0	5.0
Petition	844	2.4	1.1	1.0	2.0	3.0	5.0
Freespch	835	3.0	0.9	1.0	2.0	4.0	5.0
Legitimacy	829	3.5	0.8	1.0	3.0	4.0	5.0

difference in individuals' party membership. Nevertheless, in the empirical specifications, I control for all these covariates to further account for a potential imbalance between the two groups.

Table B.3: Covariate Balance around the Cutoff (2-week Window)

VARIABLES	(1) Female	(2) Age	(3) Minority	(4) Income (Log)	(5) Education	(6) Party	(7) Rural
Cutoff	0.011	2.677	-0.049	0.266	0.246	0.061**	0.028
	(0.039)	(2.315)	(0.030)	(0.426)	(0.419)	(0.024)	(0.073)
Constant	0.435***	40.149***	0.072**	8.538***	6.681***	0.117***	0.803***
	(0.016)	(0.855)	(0.028)	(0.237)	(0.260)	(0.013)	(0.047)
Observations	1,566	1,566	1,565	1,444	1,561	1,562	1,566
R-squared	0.000	0.006	0.008	0.001	0.001	0.006	0.001

Robust standard errors are clustered on prefectures. All models use weighted data.
*** p<0.01, ** p<0.05, * p<0.1

B.3 Main Results

Table B.4 presents the OLS results of the interrupted time series analysis using the two-week window sample. Table B.5 shows the results using the one-week window and three-week window samples. Table B.6 presents the effect of Tianjin Explosions on political participation, trust, and regime legitimacy conditional on surveillance intensity.

Table B.4: Political Participation, Trust, and Legitimacy (2-week Window)

VARIABLES	(1) Free spch.	(2) Petition	(3) Trust	(4) Legitimacy
Cutoff	-0.716**	-0.746***	-0.132	-0.723***
	(0.295)	(0.211)	(0.140)	(0.230)
Time(minute)	-0.441	-1.190***	0.252	-0.451
	(0.359)	(0.266)	(0.237)	(0.439)
Cutoff*Time	1.705**	1.624**	0.229	1.398**
	(0.748)	(0.611)	(0.387)	(0.588)
Indiv. Controls	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes
Constant	2.718***	3.943***	2.534***	3.335***
	(0.411)	(0.405)	(0.202)	(0.310)
Observations	470	479	1,433	461
R-squared	0.058	0.124	0.066	0.101

Robust standard errors are clustered on prefectures. All models use weighted data.
*** p<0.01, ** p<0.05, * p<0.1

Table B.5: Political Participation, Trust, and Legitimacy (1-week & 3-week Windows)

	1-week Window				3-week Window			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Free spch.	Petition	Trust	Legitimacy	Free spch.	Petition	Trust	Legitimacy
Cutoff	0.611	-2.004***	-0.521	-2.337***	-0.305**	-0.477***	0.198**	-0.298
	(0.545)	(0.686)	(0.321)	(0.441)	(0.117)	(0.148)	(0.0782)	(0.275)
Time(minute)	1.178	-1.906**	0.132	-2.092*	0.307	-0.311	0.319*	-0.350
	(1.459)	(0.788)	(0.636)	(1.105)	(0.256)	(0.235)	(0.186)	(0.294)
Cutoff*Time	-2.654	5.815**	1.244	6.644***	0.197	0.316	-0.798***	0.412
	(1.876)	(2.193)	(0.925)	(1.421)	(0.357)	(0.314)	(0.249)	(0.395)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.232***	3.172***	2.409***	2.671***	2.809***	3.685***	2.847***	3.441***
	(0.774)	(0.406)	(0.295)	(0.440)	(0.230)	(0.234)	(0.169)	(0.301)
Observations	203	208	652	204	781	795	2,496	774
R-squared	0.121	0.134	0.074	0.207	0.038	0.092	0.051	0.079

Robust standard errors are clustered on prefectures. All models use weighted data.
*** p<0.01, ** p<0.05, * p<0.1

Table B.6: Political Participation, Trust, and Legitimacy (2-week Window), Conditional on Surveillance Intensity

	(1)	(2)	(3)	(4)
VARIABLES	Free spch.	Petition	Trust	Legitimacy
Cutoff	-0.484**	-0.428**	-0.063	-1.074***
	(0.204)	(0.199)	(0.124)	(0.119)
Time(minute)	-0.433	-1.173***	0.256	-0.461
	(0.362)	(0.268)	(0.238)	(0.440)
Cutoff*Time	1.815**	1.800***	0.247	1.226**
	(0.737)	(0.532)	(0.385)	(0.513)
Suv.Intensity	0.024	-0.109***	0.004	0.076***
	(0.050)	(0.024)	(0.015)	(0.027)
Cutoff*Suv.	-0.005	-0.007**	-0.001	0.007*
	(0.003)	(0.003)	(0.002)	(0.004)
Distance.Tianjin	-0.000	0.003***	-0.001**	-0.001**
	(0.001)	(0.001)	(0.000)	(0.001)
Indiv. Controls	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes
Constant	0.236	18.131***	1.357	-5.140
	(5.921)	(3.167)	(1.784)	(3.088)
Observations	470	479	1,433	461
R-squared	0.060	0.126	0.066	0.105

Robust standard errors are clustered on prefectures. All models use weighted data. Because surveillance intensity is measured at the province level, I include a variable of each province's distance to Tianjin to control for potential geographically varying impacts of Tianjin Explosions.
*** p<0.01, ** p<0.05, * p<0.1

B.4 Robustness Checks

Figure B.1 shows the temporal distribution of the Google search Index from Mainland China using "Tianjin (天津)", "Football (足球)", "Military Parade (阅兵)", "Landslide (滑坡)", and "Stock Market (股市)" as the keywords. The Google search trends show no major events that occurred around the time of the Tianjin Explosion. The 70th Anniversary Parade of China's Victory over Japan Day (September 3) was a big event as shown by the distribution of the keyword "Military Parade (阅兵)". But public interests in this event occurred three weeks later than the Tianjin Explosion. Thus, none of these events could invalidate the ITS design.

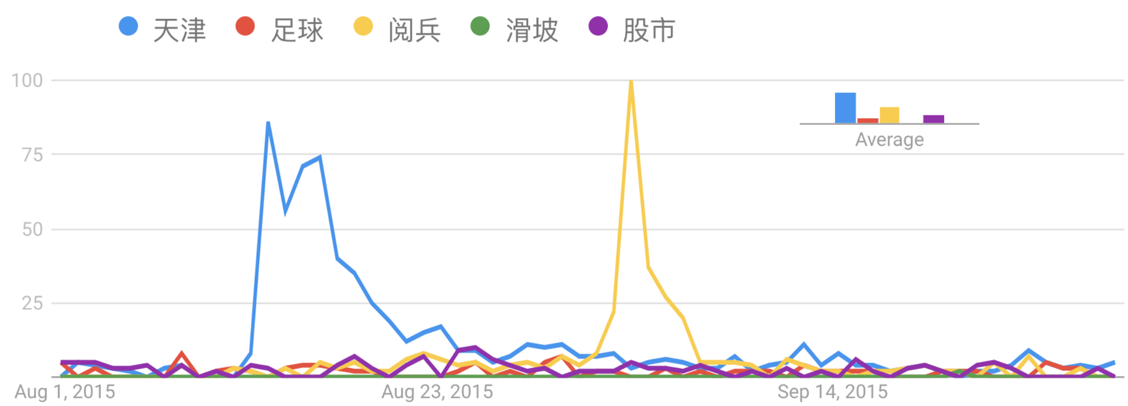


Figure B.1: Google Trends on Tianjin and Other Events

Table B.7 shows the results of the placebo tests. I examine the discontinuities in individuals' attitudes and behavior that should not be affected by the Tianjin Explosions in theory. There are no discontinuities around the cutoff in terms of attitudes and behavior regarding inequality, gender role, housework, voting, one-child policy, and homosexuality.

The main results of the interrupted time series analysis are based on linear OLS regression models. I further use local polynomial regressions developed by Calonico, Cattaneo and Titiunik (2014) to test the arguments and the results are shown in Panel A in Table B.8. In the local polynomial models, I control for the same individual characteristics and province

Table B.7: Discontinuities in Other Variables around the Cutoff, 2-week Window

VARIABLES	(1) Inequality	(2) Housework	(3) Voted	(4) Sexism	(5) Homo Sex	(6) Child Policy
Cutoff	0.000755	0.594	0.134	0.00677	0.250	-0.372
	(0.429)	(0.612)	(0.119)	(0.175)	(0.197)	(0.222)
Time(minute)	-0.519	0.141	0.137	-0.0970	0.0593	0.681**
	(0.595)	(0.502)	(0.207)	(0.377)	(0.345)	(0.319)
Cutoff*Time	0.893	-1.924	-0.128	-0.217	-0.644	0.877
	(1.375)	(1.571)	(0.312)	(0.572)	(0.573)	(0.560)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.232***	3.172***	2.409***	0.573	2.809***	3.685***
	(0.774)	(0.406)	(0.295)	(1.237)	(0.230)	(0.234)
Observations	220	215	1,435	1,434	1,392	1,431
R-squared	0.152	0.394	0.228	0.095	0.146	0.094

Robust standard errors are clustered on prefectures. All models use weighted data.
*** p<0.01, ** p<0.05, * p<0.1

fixed effects.[1] The results are largely consistent with those from linear regression models. In particular, after the Tianjin explosions, individuals show lower confidence in free speech and regime approval and these effects are statistically significant. There is also a decrease in individuals' confidence in petitioning though the effect is statistically insignificant. I further tested 2nd-order and 3rd-order polynomial regressions and the results are still robust (Panel B and C). Note that the optimal bandwidth selection algorithms in the local polynomial regressions may choose a much larger window (bandwidth) than the period of intensified surveillance (Figure 7 in the main text), which covers some important events that could change people's attitudes such as the 70th Anniversary of Anti-Japanese War. Thus, we should interpret these results with caution. On the other hand, the OLS results in Table B.4 are more reliable.

[1] I choose provinces where surveys were mostly conducted around the cutoff time, including Yunnan, Jilin, Sichuan, Tianjin, Ningxia, Guangdong, Gansu, Fujian, Guizhou, Heilongjiang.

Table B.8: Results from Local Polynomial Regression

	Coef.	Std. Err.	z	P> $\|z\|$	[95% CI]		N of Obs.	Effective N
Panel A: 1st Order Polynomial								
	Free Speech							
Conventional	-0.375***	0.143	-2.627	0.009	-0.655	-0.095	L:2375 R:769	L:779 R:201
Robust	-0.375**	0.143	-2.092	0.036	-0.688	-0.022	L:2375 R:769	L:779 R:201
	Petition							
Conventional	-0.100	0.229	-0.438	0.661	-0.550	0.349	L:2372 R:785	L:431 R:174
Robust	-0.100	0.229	-0.176	0.860	-0.567	0.474	L:2372 R:785	L:431 R:174
	Trust							
Conventional	-0.140	0.127	-1.109	0.268	-0.388	0.108	L:7721 R:2543	L:867 R:508
Robust	-0.140	0.127	-1.239	0.215	-0.471	0.106	L:7721 R:2543	L:867 R:508
	Legitimacy							
Conventional	-0.451**	0.180	-2.502	0.012	-0.804	-0.098	L:2309 R:755	L:323 R:150
Robust	-0.451***	0.180	-2.674	0.007	-0.955	-0.147	L:2309 R:755	L:323 R:150
Panel B: 2nd Order Polynomial								
	Free Speech							
Conventional	-0.363*	0.194	-1.871	0.061	-0.744	0.017	L:2375 R:769	L:1337 R:246
Robust	-0.363	0.194	-1.607	0.108	-0.797	0.079	L:2375 R:769	L:1337 R:246
	Petition							
Conventional	0.039	0.260	0.151	0.880	-0.470	0.548	L:2372 R:785	L:1222 R:254
Robust	0.039	0.260	0.255	0.799	-0.482	0.626	L:2372 R:785	L:1222 R:254
	Trust							
Conventional	-0.198	0.144	-1.368	0.171	-0.481	0.085	L:7721 R:2543	L:1888 R:686
Robust	-0.198	0.144	-1.221	0.222	-0.519	0.120	L:7721 R:2543	L:1888 R:686
	Legitimacy							
Conventional	-0.634***	0.199	-3.184	0.001	-1.024	-0.244	L:2309 R:755	L:836 R:205
Robust	-0.634***	0.199	-3.098	0.002	-1.154	-0.260	L:2309 R:755	L:836 R:205
Panel C: 3rd Order Polynomial								
	Free Speech							
Conventional	-0.368	0.246	-1.497	0.134	-0.849	0.114	L:2375 R:769	L:1580 R:288
Robust	-0.368	0.246	-1.433	0.152	-0.904	0.140	L:2375 R:769	L:1580 R:288
	Petition							
Conventional	0.155	0.369	0.421	0.674	-0.568	0.879	L:2372 R:785	L:1083 R:243
Robust	0.155	0.369	0.455	0.649	-0.612	0.982	L:2372 R:785	L:1083 R:243
	Trust							
Conventional	-0.239	0.155	-1.539	0.124	-0.543	0.065	L:7721 R:2543	L:3715 R:842
Robust	-0.239	0.155	-1.418	0.156	-0.571	0.092	L:7721 R:2543	L:3715 R:842
	Legitimacy							
Conventional	-0.590**	0.252	-2.338	0.019	-1.085	-0.095	L:2309 R:755	L:1046 R:217
Robust	-0.590**	0.252	-2.031	0.042	-1.133	-0.020	L:2309 R:755	L:1046 R:217

Covariate-adjusted sharp RD estimates are generated from local polynomial regression based on Mean-Square-Error-optimal bandwidth selector and triangular Kernel function. Robust standard errors are clustered on prefectures. "Effective N" means the number of observations calculated by the Mean Square Error optimal bandwidth algorithms. L indicates left of cutoff; R indicates right of cutoff.

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