How Corruption Investigations Undermine Regime Support
Evidence from China

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Abstract

Populist leaders around the world often fight against corruption in an effort to win public support. Conventional wisdom holds that this strategy works because leaders can signal their benevolent intentions by removing corrupt officials. We argue that fighting against corruption can produce unintended consequences. By revealing scandals of corrupt officials, anti-corruption campaigns can alter citizens' beliefs about public officials and lead to disenchantment about political institutions. We test this argument by examining how China's current anti-corruption campaign has changed citizens' public support for the government and the Communist Party. We analyze the results of two surveys conducted before and during the campaign, and employ a difference-in-differences strategy to show that corruption investigations decrease respondents' support for the central government and party. We also examine our respondents' prior and posterior beliefs, and the results support our updating mechanism.

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Corruption is the world's most frequently discussed problem.[3] Populist leaders, such as Venezuela's Hugo Chávez, India's Narendra Modi, Turkey's Recep Tayyip Erdogan, Hungary's Viktor Orban, and China's Xi Jinping, have launched anti-corruption campaigns—a series of intense political operations to arrest corrupt officials—in order to win public support.[4] But do anti-corruption campaigns actually score points with the public?

A distinguished literature shows that citizens are antagonistic to government corruption: the more corrupt the citizens perceive the incumbents to be, the more likely they are to punish the incumbent government in elections (Ferraz and Finan 2008; Chang, Golden, and Hill 2010; Winters and Weitz-Shapiro 2013; Chong et al. 2015). Cross-national studies also show that political corruption erodes trust in political institutions, increases anti-government protest, and undermines the regime's long-term legitimacy (Seligson 2002; Anderson and Tverdova 2003; Chang and Chu 2006; Gingerich 2009; Morris and Klesner 2010).

When leaders fight corruption, as they fight poverty and unemployment, they hope to signal their responsiveness and benevolent intentions to the public, and therefore to garner more public support. As Chinese President Xi Jinping said in a speech in 2014 to justify his anti-corruption drive: "People hate corruption the most, so we must be determined to fight against corruption to win support from the people."[5]

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[3] BBC, "Global Poll: Corruption is world's most talked about problem," December 9, 2010. http://www.bbc.co.uk/pressoffice/pressreleases/stories/2010/12_december/09/corruption.shtml (accessed July 25, 2017).

[4] For Chávez, see https://goo.gl/ySfMME; for Modi, see https://goo.gl/yU3fF1; for Erdogan, see https://goo.gl/54BuqR; for Orban, see https://goo.gl/o9Yzs6; for Xi, see https://goo.gl/AUZd14 (all accessed July 27, 2017).

[5] See http://paper.people.com.cn/rmrbhwb/html/2016-05/25/content_1682162.htm (accessed July 25, 2017).

There is, however, very little research on the effects of anti-corruption campaigns on public support, especially in authoritarian regimes. We do not know how citizens react to information disclosed during anti-corruption campaigns. Do they perceive the campaign as its leaders intend—as a genuine effort by the regime to curb corruption? Or are they shocked by the excessive corruption revealed in the investigations and become disenchanted with the regime? Empirically, it is challenging to estimate the causal effects of anti-corruption campaigns, because they are usually not randomized.[6]

In this article, we argue that anti-corruption campaigns may produce unintended consequences. We start with the assumption that citizens have *prior beliefs* about the integrity of government officials, which affects their degree of support for the regime. When political leaders launch an anti-corruption campaign, scandals of politicians' corrupt activities are revealed to the public. Citizens use this new information to update their level of regime support. If the corrupt behavior revealed during the campaign exceeds citizens' expectations, they are surprised by the excessive government corruption. They then start to question the integrity of public officials in general, and reevaluate their support for the regime. As a result, anti-corruption campaigns may lead to citizens' "informed disenchantment"[7] and undermine regime support.

We substantiate our arguments by examining one of the most intense anti-corruption drives in the world—the on-going anti-corruption campaign in China. We use a difference-in-differences (DID) strategy to analyze the results of two original surveys that we conducted before and during the campaign, and demonstrate strong and highly

[6] There have been efforts to randomize the information of incumbent malfeasance or social corruption, please see Chong et al. (2015), de Figueiredo et al. (2014), Larreguy, Marshall, and Snyder (2017), and Corbacho et al. (2016).

[7] Gallagher (2006) coined the term "informed disenchantment" to describe litigants' disenchantment with the legal system after having direct contact with the system.

robust evidence that citizens who are exposed to more corruption investigations exhibit lower support for the regime.

Using our fine-grained survey data, we also explicitly test the updating mechanism. Employing a DID framework, we first demonstrate that while the respondents, on average, had the same *prior beliefs* about officials' integrity in our baseline survey, those who were exposed to more corruption investigations updated their beliefs to exhibit lower *posterior beliefs* about officials' integrity in the follow-up survey. Consistent with our updating theory, we find that the impact of corruption investigations on people's public support depends on how the investigations relate to their prior beliefs. Interacting respondents' prior beliefs with the intensity of corruption investigations, we show that investigations only have a significantly negative impact on people's public support when their priors are high; the effect is precisely zero when their priors are low. This indicates that the anti-corruption campaign provided new information that shocked respondents who had strong prior beliefs about officials' integrity. As a result, the outrageous level of corruption revealed by the campaign has decreased citizens' regime support in China—the opposite of the intended effect.

Our key contribution is to provide the first, direct quasi-experimental evidence of citizen updating during an anti-corruption campaign in an autocracy. Several recent studies have examined how incumbent corruption information influences how individuals vote in subsequent elections (Ferraz and Finan 2008; Chong et al. 2015; Arias et al. 2017). For example, Ferraz and Finan (2008) show that incumbents in Brazil were severely punished in elections if their corruption violations were revealed to the public, a result the authors attribute, without direct evidence, to voters' updating of beliefs (705).

In another study, Chong et al. (2015) find that incumbent corruption information decreases incumbent party support in local elections in Mexico and erodes partisan attachments. Their theory suggests that voters' prior expectations are important (64), but budgetary constraints prevented them from conducting a baseline survey to measure beliefs prior to their intervention (61). Similarly, Arias et al. (2017) explicitly test a Bayesian model of learning using a field experiment in Mexico. But due to financial constraints, they did not conduct a baseline survey and had to use the control group's post-treatment beliefs to proxy for the (pre-treatment) prior beliefs of treated voters (3). These studies posit the same updating mechanism as we do, but do not directly test for people's prior beliefs.

Our study is hence the first to use two original surveys before and after the treatment to measure both prior and posterior beliefs and directly test citizen updating. While most existing work focuses on democracies, we highlight the unintended consequences of revealing corruption information in an autocracy, joining a recent literature that shows the surprising consequences of transparency or "the adverse effects of sunshine" in authoritarian regimes (Malesky, Schuler, and Tran 2012; Hollyer, Rosendorff, and Vreeland 2015).

We believe our findings are applicable to other regimes that rely on campaigns (rather than the rule of law) to tackle corruption, and to those that have a low-information environment. Anti-corruption campaigns usually involve a series of intense political operations within a short period of time, so a large amount of information on corrupt activities is disclosed to the public, which is more likely to constitute a shock. Another key contextual condition that is necessary for our updating mechanism to work is citizens' incomplete or inaccurate prior beliefs about public officials. Survey research has shown that developing countries, especially authoritarian regimes, tend to have a low-information environment due to the opacity of the political system (Zhu, Lü, and Shi 2012; Larreguy, Marshall, and Snyder 2017; Huang 2017). As we discuss in our concluding remarks, such regimes are likely to face this dilemma: the more the regime publicly punishes corrupt officials, the more the public punishes the regime.

THEORY AND BACKGROUND

In this section, we elaborate on our theory, which is based on a learning model of the citizens, introduce China's current anti-corruption campaign, and derive several testable hypotheses.

Information about Corruption and Public Support

We start with the premise that citizens' support for the regime is in part a function of their perceptions of the degree of corruption in the political system. They prefer (receive higher expressive utility) to support a government with less corrupt officials. This is consistent with the empirical literature that shows a negative association between perceived corruption and public support (Seligson 2002; Chang and Chu 2006; Ferraz and Finan 2008; Chang, Golden, and Hill 2010; Winters and Weitz-Shapiro 2013; Chong et al. 2015).

Since citizens have *incomplete information* about how corrupt government officials are, they develop *prior beliefs* based on officials' *revealed corrupt behavior* (corruption scandals, personal experience, and rumors, etc.). A recent strand of research shows that citizens usually have misinformation about specific sociopolitical events and misperceptions about socioeconomic facts in democracies and autocracies alike (Berinsky 2017; Huang 2017). Because of the covert nature of corruption, citizens usually have imperfect knowledge about how corrupt their public officials are. This is consistent with much survey research in developing countries (Zhu, Lü, and Shi 2012; Corbacho et al. 2016; Larreguy, Marshall, and Snyder 2017).

Anti-corruption campaigns involve investigating corrupt officials and revealing corrupt activities. Leaders of the campaign, in order to justify their actions, publicize the details of the corrupt activities of the arrested individuals. Citizens then update their *posterior beliefs* about the integrity of officials in the regime based on informational signals in a Bayesian fashion. The campaigns usually reveal new information that causes citizens to believe that public officials are more corrupt than they previously thought (informational signals more negative than citizens' mean prior belief). This updated information leads citizens to decrease their support for the regime.[8]

The key insight is that the effect of anti-corruption depends on how the new information relates to citizens' prior beliefs. If they hold low priors on officials' integrity (e.g., they already thought most officials were dishonest), new revelations of corrupt behavior will not change their level of support. In order to update their priors, citizens must receive signals that are different from their priors.

An intense anti-corruption drive that reveals that many public officials are corrupt is likely to alter citizens' prior beliefs about officials' integrity and decrease their regime

[8] Exposed to new information that officials in the incumbent government are corrupt, citizens will not only punish the incumbents with lower support, they may even lose trust in the opposition and withdraw from the political process altogether (Chong et al. 2015) or be more inclined to engage in political protest (Gingerich 2009).

support. When citizens are "shocked" by the information revealed in anti-corruption campaigns, they start to wonder why there is so much corruption in the system and question the integrity of the whole body of government officials.

Our simple theoretical framework is consistent with emerging empirical evidence on how new information changes citizens' support for the incumbent government (Humphreys and Weinstein 2012; Larreguy, Marshall, and Snyder 2017) and is based on more formalized models in Kendall, Nannicini, and Trebbi (2015) and Arias et al. (2017).

So although leaders intend to garner more public support by arresting corrupt officials and publicizing corrupt activities, anti-corruption campaigns may unintentionally lead to decreased public support. While such campaigns may help them achieve other goals—such as eliminating rivals, signaling strength, and consolidating power—they do so at the expense of the legitimacy of their regime.

China's Anti-Corruption Campaign

Consistent with the cross-national evidence, corrupt officials are Chinese citizens' top concern (ahead of inequality, crime, food safety, and pollution).[9] Scholars have pointed to the weak institutional design of corruption investigations as one explanation for China's endemic corruption (Manion 2004, 2016; Sun 2004; Gong 2015). Other scholars emphasize that the country's weak property rights regime incentivizes firms to engage in corrupt practices in order to gain market advantages (Truex 2014; Wang 2014).

Starting in 2012, after Xi Jinping took power, the Chinese Communist Party (CCP) launched an anti-corruption campaign with the stated goal of eliminating "tigers"

[9] See http://www.pewglobal.org/2016/10/05/chinese-public-sees-more-powerful-role-in-world-names-u-s-as-top-threat/10-4-2016-9-39-43-am/ (accessed July 25, 2017).

(high-ranking corrupt officials) and "flies" (low-ranking corrupt officials). By October 2017, a total of 350,000 officials had been investigated for corruption.[10] Many believe that Xi's anti-corruption campaign is the most intense and protracted in the People's Republic's history (e.g., Wedeman 2016).

There is speculation that the current campaign is less a genuine effort to reduce corruption than a politically motivated effort by Xi to weaken his opponents; some evidence suggests there are fewer investigations in provinces where Xi previously served (Murong 2015; Zhu and Zhang 2017). Other scholars argue that the effort has been largely genuine and has significantly changed the structure of Party and government incentives so as to reduce bureaucratic opportunities for corruption and structural obstacles to anti-corruption enforcement (Manion 2016; Lü and Lorentzen 2016). Recent events seem to lend more support to the former interpretation, as Xi has successfully abolished the two-term limit on the presidency with hardly any public dissent from the elite.[11] Our stance on this debate is that it is still too early to tell, and it is difficult to identify the motives of the political leaders behind the current campaign. Instead, we are primarily interested in the campaign's *effects* on public opinion.

The CCP has gone to great lengths to collect and publicize the corrupt activities of investigated officials during the campaign. Many high-ranking officials' trials were made public or broadcast live on TV (such as Bo Xilai's). Many details of their corruption, such as bribery, business deals, kickbacks, mansions, and mistresses, have been disclosed to the public.

[10] See http://news.xinhuanet.com/mrdx/2017-10/21/c_136695470.htm (accessed November 29, 2017).
[11] See https://www.nytimes.com/2018/03/11/world/asia/china-xi-constitution-term-limits.html (accessed March 13, 2018).

These informational signals are expected to change people's beliefs about public officials and their support for the regime. Given the intensity of the campaign, we expect the campaign to have a negative average treatment effect on people's public support: respondents who are exposed to more corruption investigations should, on average, exhibit lower public support for the regime.

> **H1 (Average Treatment Effect)**: Corruption investigations reduce citizens' support for the central government and Party, *ceteris paribus*.

We argue that citizen updating is the main mechanism driving the results. Specifically, we expect that the revelations of endemic corruption at all levels of the party, government, and military cause the public to update its beliefs about officials' integrity: people exposed to more investigations are less likely to believe in the integrity of public officials, which decreases their public support.

> **H2 (Updating Beliefs)**: Corruption investigations weaken citizens' beliefs about the integrity of public officials, *ceteris paribus*.

Our theory also predicts that the campaign has heterogeneous effects: corruption investigations should have no effect on people's regime support if they already believe officials' integrity is low; corruption investigations should decrease people's regime support when their prior beliefs about officials' integrity are high.

> **H3 (Heterogeneous Effects)**: The marginal effects of corruption investigations depend on citizens' prior beliefs. Such investigations should have no effect on people's regime support if their prior beliefs about officials' integrity are low,

while investigations should decrease people's regime support when their prior beliefs about officials' integrity are high.

In addition to our proposed *updating mechanism*, an *economic mechanism* might also be at work. Removing a large number of officials might disrupt government activities and bring political instability, which harms economic growth (Alesina et al. 1996). If the CCP's legitimacy has primarily been based on its effectiveness in promoting economic growth (Zhao 2009), the lower economic performance might decrease regime support. We test this alternative mechanism and show that our evidence is consistent with only the *updating mechanism*.

RESEARCH DESIGN

In this section, we discuss our identification strategy and show that our data meet the key identification assumption that is required to make causal inference.

Data

In 2010 and 2014, respectively, we designed and conducted two original surveys in China. The two surveys used the same sampling design (spatial sampling with the same primary sampling units, see Landry and Shen [2005]) and questionnaire, and both were implemented by the Research Center for Contemporary China (RCCC) at Peking University. Both surveys interviewed adult citizens in the same 49 counties across China's 25 provinces or provincial-level cities.[12] The 2010 baseline survey interviewed 3,874 respondents, and the 2014 follow-up survey interviewed 4,128 respondents.

[12] Provinces not surveyed include Inner Mongolia, Hainan, Guizhou, and Ningxia. These provinces were not included in the sample because the surveys used the probability proportionate to size method, meaning

Our key outcome variable is regime support, which we define in terms of what Easton (1975: 436-437) refers to as "diffuse support"—citizens' support for the regime's political institutions rather than for the incumbents or their policies. We asked respondents about their levels of *support* and *trust* in the most important political institutions (the central government and party) on a scale from 0 (no support/trust) to 10 (high level of support/trust). We therefore have four variables: *Trust Central Government*, *Trust Central Party*, *Support Central Government*, and *Support Central Party*.[13] In our main regression analysis, we use ***Support Central Party***—the most relevant indicator of the CCP's legitimacy—to make our presentation parsimonious, but as we show in the Appendix, our results are consistent using the other three measures.

Our key independent variable is the number of corruption investigations during the period between the two surveys. We collected the data from Tencent—the largest Internet company in China. In March 2016, Tencent launched a searchable online database of all corruption investigations across China since 2011.[14] Based on information provided by Party disciplinary committees, courts, and procuratorates from the central to local levels, Tencent's database includes each official's name, position, locality, rank, and reason for investigation. In August 2016, we used Python to scrape Tencent's website and organized the database in an analysis-ready format. To verify this database

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that localities with large populations had a higher probability of being selected than smaller localities, and these provinces have relatively small populations. Tibet and Xinjiang were also not part of the survey because of recent political tensions.

[13] These measures are widely used in the study of public support in China. See Dickson (2016) and Dickson, Shen, and Yan (2017) for a discussion of the concept of regime support and its measurement; see Li (2013) for a dissenting view on whether survey respondents' reported political trust is a reliable indicator of regime support. Other scholars have used different measures of regime support (see, for example, Chen 2013; Shi 2015; Tang 2016).

[14] See http://news.qq.com/zt2016/fanfu_ccdi/index.htm (accessed February 7, 2017).

and ensure that every probe was made public, we ran an Internet search on every name to find its original source and record the date of the announcement.

The Tencent database has two advantages. First, it is the most comprehensive, public database on China's corruption investigations. It synthesizes information from official statistics at all levels of government and from all branches. Second, Tencent has provided this online database for Internet users to search how many officials in their hometowns have been investigated for corruption. By clicking on their hometowns (province, prefecture, or county) on a drop-down list, this online interface reports the total number of probes. This is the only place Chinese citizens can find out this number in a single click, and the database is widely circulated via Tencent's app—WeChat—China's most popular social network app, which has over 800 million users.

While we believe information on corruption investigations is widely circulated, we nevertheless cannot directly observe the extent to which our respondents received the information. So our analysis focuses on estimating intention-to-treat (ITT) effects, which are the quantity of interest in some recent studies (e.g., Arias et al. 2017) and the most policy relevant.

Theoretically, every citizen's exposure to corruption investigations consists of two components. The first component is the investigations that involve *central* officials. Between the two surveys, there were 64 central investigations. The second component is the *local* investigations in the respondent's province. Citizens pay attention to investigations in their own province because the local media cover these cases extensively. Some media outlets frequently rank the provinces according to the number of corruption investigations.[15]

To measure each respondent's exposure to corruption investigations, we therefore need to calculate the number of both types of investigations. Mathematically, however, unless central investigations exert differential effects on people in different provinces, the number of central investigations will be "netted out" in our DID framework.[16] There are two possible ways in which central investigations can have differential effects on different provinces. First, citizens in an official's hometown province may be more sensitive to news about that official's corruption. Second, most central officials worked in local government before being promoted to the center, so citizens may pay more attention to central officials who used to work in their own province. We test these two possibilities by coding the biographies of corrupt central officials, but find no evidence to support either (Appendix Table A1.5). Regardless of whether we use the sheer number of central investigations or weight the number by corrupt officials' rank,[17] central investigations have no differential effects on different provinces.

Central investigations do not have differential effects on different provinces; the number of *local* investigations drives local variation in exposure to the campaign. Our main independent variable is therefore ***Number of Corruption Investigations***, which is the total number of corruption probes *in a province* between January 2011 (after our

[15] This is one example: http://history.sina.com.cn/cul/zl/2014-05-09/094790321.shtml (accessed July 24, 2017).

[16] To see this, respondent *i* in province *j* is exposed to X_{ij} investigations, and $X_{ij}=X_c+X_j$, where X_c is the number of central investigations and X_j is the number of local investigations. Similarly, respondent *h* in province *k* is exposed to X_{hk} investigations, and $X_{hk}=X_c+X_k$. So the difference in exposure between these two respondents is $X_{ij}-X_{hk}=(X_c+X_j)-(X_c+X_k)=X_j-X_k$.

[17] Officials' rank is coded using China's civil service code, which ranks from 1 (state level) to 9 (deputy office level). ***Number of Corrupt Central Officials (Weighted)*** is calculated using the following formula: $\sum \frac{1}{\text{rank}_i}$, which is the sum of the inverted rank of each official. This number is higher when corrupt officials have higher ranks.

baseline survey) and July 2014 (before our follow-up survey). We employ this simple measure because it is the most intuitive number that could create an impression in people's heads without requiring much cognitive burden. In the robustness checks, we also use ***Number of Corruption Investigations (Weighted)*** (which takes into account the varying bureaucratic ranks of corrupt officials), ***Numbers of Tigers and Flies*** (which separates high- and low-ranking officials), and ***Number of Corruption Investigations Per Million*** (which considers population size in each province).

In this period, there were 4,352 corruption probes, which cover all of China's 31 provincial-level units.[18] We aggregate local corruption cases at the provincial level[19] because these are the cases that the public media focus on when reporting on corruption probes, and therefore the level that ordinary citizens pay attention to. This is corroborated by our data: we do not obtain the same results when we use corruption measures at lower levels (Appendix Table A2.2).

We also consider several ***Demographic Controls*** that can influence regime support, including ***Male***, ***Age***, ***Years of Education***, ***Urban,***[20] ***Han***, ***Party Member***, and ***Per Capita Family Income (log)***. Appendix Table A1.2 presents these variables' measures and summary statistics.

Identification Strategy

Because our two surveys used the same sampling design and questionnaire and were implemented by the same survey institute, we can treat them as *repeated cross-sections*

[18] Appendix Table A1.1 shows the distribution of corruption probes across provinces.
[19] The measure therefore includes all corruption cases within the province at all levels.
[20] This refers to the official household (*hukou*) registration of respondents. Rural migrants living in cities typically do not have an urban hukou. This variable is included to capture a potentially important subset of the urban population.

(Abadie 2005: 2). If we detect any changes in respondents' regime support, we can attribute these changes to factors *outside* the survey.[21]

Employing a DID strategy, we can then estimate how corruption probes occurring in different provinces from 2011 to 2014 changed citizens' regime support. Specifically, the *first difference* is the temporal difference: the extent to which respondents changed their level of regime support from 2010 to 2014. The *second difference* is the regional difference: the extent to which respondents changed their level of regime support due to exposure to different numbers of corruption probes in their provinces.

Formally, assuming that some provinces had very few (below average) corruption probes, let us call these provinces the *Control* group; some provinces had many (above average) corruption probes, let us call these provinces the *Treatment* group. Before the anti-corruption campaign, respondents' regime support in the Control group is denoted by Y_1^C; during the campaign, respondents' regime support in the Control group is Y_2^C. Meanwhile, before the campaign, respondents' regime support in the Treatment group is Y_1^T; during the campaign, their support is Y_2^T. The DID estimator is defined as follows:

$$DID = (Y_2^T - Y_2^C) - (Y_1^T - Y_1^C) \quad (1)$$

The identification assumption is that, in the absence of an anti-corruption campaign, average regime support for the Control and Treatment groups would have followed a *common trend*. As a result, the Control group can be used to infer the

[21] We check whether there are compositional changes in the sample between the two periods and present the *t*-tests of key demographic variables in Appendix Table A1.3. While most demographic variables are balanced between the two periods in most provinces, there are significant differences in several covariates, such as *Years of Education*, *Urban*, and *Per Capita Family Income (log)* in provinces like Heilongjiang, Shandong, Hubei, Guangdong, and Guangxi. We hence control for these demographics in our analyses and conduct *conditional* DID estimators.

counterfactual evolution of average regime support for the Treatment group in the absence of a campaign.

One possible violation of the common trends assumption is that the provinces that experienced many corruption investigations were systematically different from those that had few. The campaign might have targeted certain provinces *because* they have low levels of public support or slow economic growth. So although we might find an association between corruption investigations and lower levels of public support, the results might suffer from reverse causality or omitted variable bias.

To rule out this possibility, we correlate a province's *pretreatment* levels of public support and economic development with the number of corruption investigations. As Figure 1 shows, a province's average level of public support (measured by *Trust Central Government*, *Trust Central Party*, *Support Central Government*, and *Support Central Party*) and economic development (measured by *Per Capita GDP* and *GDP Growth Rate*) in 2010 are not significantly correlated with the number of corruption investigations they experienced during 2011–2014. Regression results further confirm these results (Appendix Table A1.4).

The common trends assumption cannot be directly tested with data from only two periods. But if we have more than one *pretreatment* period for which data are available, pre-existing differences in the trends of the outcome variable between the Treatment and Control groups can be detected by applying the DID estimator to pretreatment data (Abadie 2005: 2).

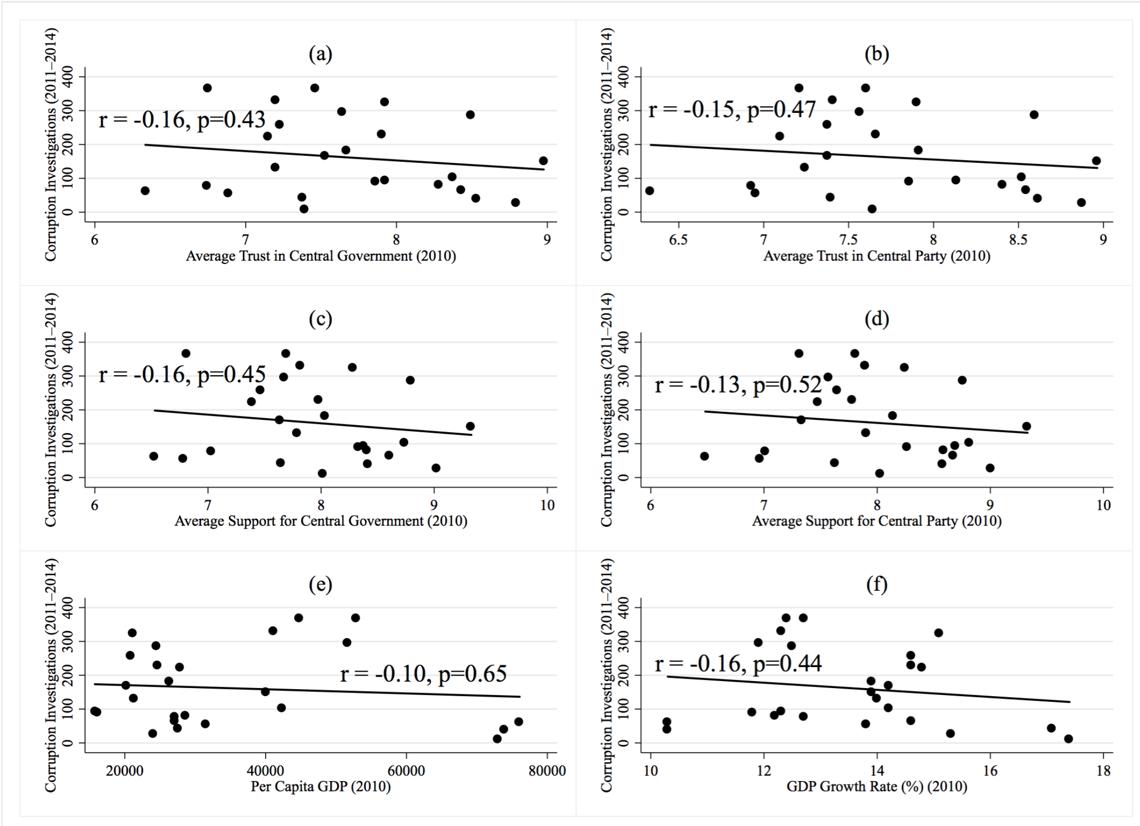

Figure 1: Correlates of Corruption Investigations

Notes: These graphs plot the bivariate correlations between the level of public support and economic development in 2010 and the number of corruption investigations during 2011–2014. The dots indicate Chinese provinces.

To test the common trends assumption directly, we compare our 2010 baseline survey with another *pretreatment* survey—*Attitudes Towards Citizenship in China*—which was conducted in 2008, used the same sampling design (spatial sampling), and was also implemented by RCCC.[22] If the DID assumption holds, the Treatment and Control groups should follow a common trend from 2008 to 2010. Specifically, the DID estimator should be zero for the two pretreatment periods.

[22] The survey data is publicly available at Peking University's dataverse. Please see https://goo.gl/jiKF7J (accessed February 13, 2017).

Table 1: Testing Common Trends Using Two Pretreatment Surveys (OLS Regressions)

Outcome Variable	(1) **Trust Central Government (z-score)**	(2) **Trust Central Party (z-score)**
	Coefficient **(C.S.E.)** **(Bootstrap C.S.E.)**	**Coefficient** **(C.S.E.)** **(Bootstrap C.S.E.)**
Year 2010 * N. of Corruption Investigations	-0.0002	-0.0001
	(0.0005)	(0.0005)
	(0.0004)	(0.0005)
Outcome Variable Mean	0	0
Outcome Variable S.D.	1	1
Outcome Variable Range	[-2.254, 1.754]	[-2.216, 1.722]
Year 2010	YES	YES
N. of Corruption Investigations	YES	YES
Province F.E.	YES	YES
Intercept	YES	YES
Observations	6,452	6,371
N. of Clusters	20	20
R^2	0.675	0.685

Notes: Here we use two pretreatment surveys (conducted in 2008 and 2010) to test the common trends assumption. *N. of Corruption Investigations* is the total number of corruption investigations in a province during the anti-corruption campaign between 2011 and 2014. All specifications include provincial fixed effects, and we report both clustered standard errors and clustered bootstrap standard errors (both clustered at the provincial level). *p*-values are based on a two-tailed test: * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 1 shows the DID results with ordinary least squares (OLS) estimates using the 2008 and 2010 data. The outcome variables are ***Trust Central Government*** and ***Trust Central Party***, which are measured using the same questions in the 2008 and 2010 surveys. The 2008 survey, however, used a 1–4 scale to measure respondents' responses, while the 2010 survey used a 0–10 scale. We thus standardize the variables by taking their z-scores to assure comparability between the two surveys.[23] ***Year 2010*** is an indicator for the 2010 survey (2008 as the reference group), ***Number of Corruption Investigations*** is the total number of corruption probes in a province during 2011–2014, and ***Year 2010*** $\times$ ***Number of Corruption Investigations*** is the interaction between the two. The models control for provincial fixed effects, capturing any time-invariant historical, institutional, and cultural covariates at the provincial level. We report both clustered standard errors and clustered bootstrap standard errors, both at the treatment level (provincial level). We employ bootstrapping to deal with the potential downward bias caused by the small number (20) of clusters (Cameron, Gelbach, and Miller 2008, 414). As shown, the coefficient on the interaction term (the DID estimator) is very small and indistinguishable from zero, which supports the common trends assumption.

So far, we have provided ample evidence that the common trends assumption plausibly holds, so the DID approach is a valid way to evaluate the ITT of corruption investigations. Next, we present a simplified case in which our respondents are "assigned" to either a Control group (provinces with below-average corruption probes) or a Treatment group (provinces with above-average corruption probes), as defined in Equation (1). Please note that our data lacks a perfect control group with no corruption investigations; we use this simplified approach only to provide a visualization of the DID estimator.

Figure 2 shows standard DID plots, visualizing the changes in regime support from 2010 to 2014 in the Treatment and Control groups. The four graphs use the four different outcome variables measuring regime support. The figure shows that the levels

[23] The z-score is a measure of how many standard deviations below or above the population mean a raw score is, so the transformed variable has a mean of zero and a standard deviation of one.

of regime support are similar in 2010. From 2010 to 2014, the respondents' regime support increased in the Control group.[24] Without an anti-corruption campaign, the respondents in the Treatment group should have followed a common trend (the dotted line) and increased their regime support. However, their regime support declined in 2014, and is about 0.6 points lower than the counterfactual (on a 0–10 scale). This change (DID) is statistically significant at the 0.01 level. Appendix Table A1.6 presents the estimates.

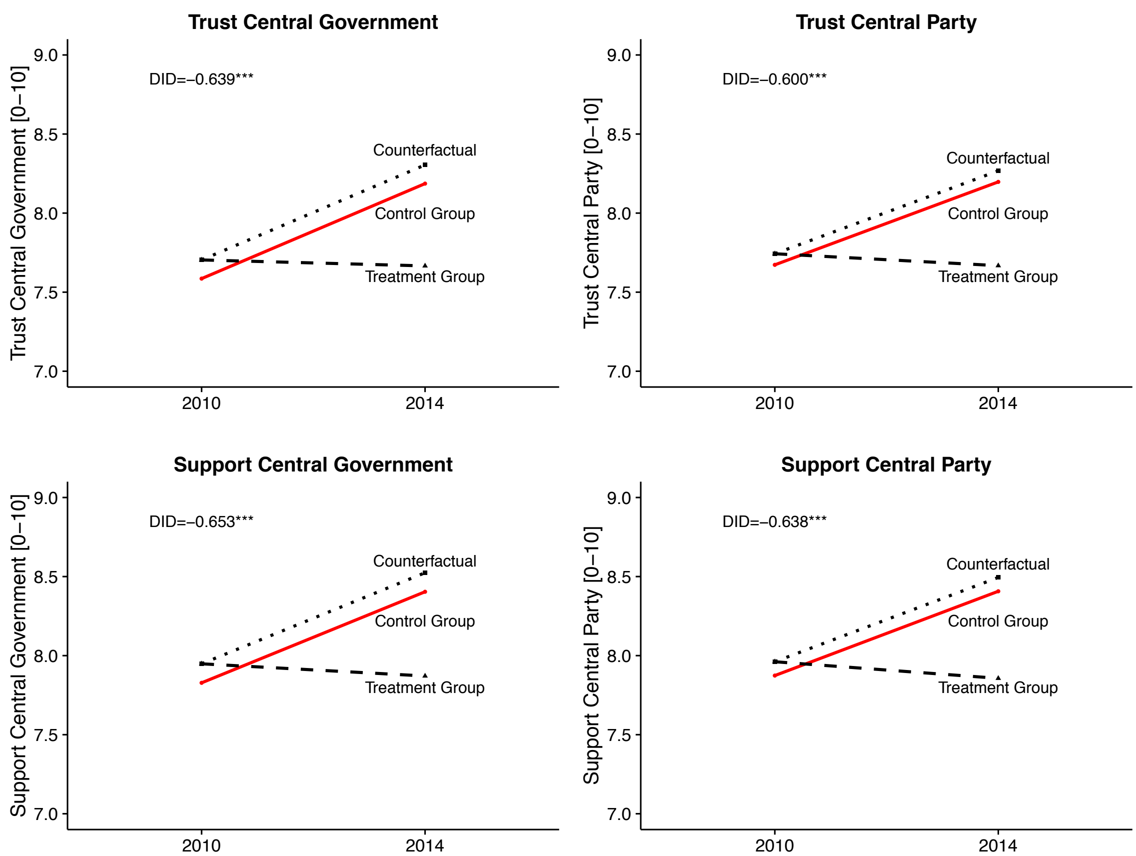

Figure 2: Difference-in-Differences Plots of Regime Support

[24] The increase ranges from 0.5 to 0.6, depending on which outcome variable we use, and is statistically significant at the 0.01 level. The increase of public support in the Control group might be due to a higher level of income, quality of life, or changes in individual well-being. Examining this secular trend is beyond the scope of this study, because we are interested in the *relative* difference between the parallel trend of the Control and Treatment groups.

Notes: These graphs plot the DID estimates by "assigning" respondents to either the Treatment or Control group. The (black) dashed line indicates average regime support among respondents in provinces that have an above-average number of corruption investigations between 2011 and 2014, the (red) solid line indicates average regime support among respondents in provinces that have a below-average number of corruption investigations, and the (black) dotted line indicates the parallel trend of the Control group.

While this lends preliminary support to our hypothesis (H1), the number of corruption investigations is a continuous rather than a dichotomous variable. We can think of provinces with different numbers of investigations as treated with different "doses." We now depart from our simplified model and use a continuous variable to conduct our DID estimates.

EMPIRICAL RESULTS

In this section, we present our main empirical results and show that they are highly robust. We then provide evidence to show that people are updating their beliefs based on new information disclosed in the campaign, and that the effects of corruption investigations depend on people's prior beliefs. We also examine an alternative mechanism.

Average ITT Effects

We use OLS to fit the following equation to the repeated cross-section data file that combines the 2010 and 2014 surveys:

$$
\begin{aligned}
&Support\ Central\ Party_{ijt} \\
&\qquad = \alpha_{ijt} + \beta_1 Year2014_t \times Number\ of\ Corruption\ Investigations_{jt} \\
&\qquad + \beta_2 Year2014_t + \beta_3 Number\ of\ Corruption\ Investigations_{jt} + X\mathrm{B} \\
&\qquad + \mu_j + \varepsilon_{ijt} \qquad (\mathbf{2})
\end{aligned}
$$

where ***Support Central Party**$_{ijt}$* is province *j*'s respondent *i*'s level of support for the central Party in year *t*, ***Year2014**$_t$* is an indicator for the respondents in the 2014 follow-up survey (2010 is the baseline), and ***Number of Corruption Investigations**$_{jt}$* is the total number of corruption probes in province *j* from January 2011 to July 2014. β_1 is the DID estimator, which is expected to be negative. We also control for several ***Demographic Controls***, including ***Male***, ***Age***, ***Years of Education***, ***Urban***, ***Han***, and ***Party Member*** in ***X*** and provincial fixed effects μ_j. We exclude ***Per Capita Family Income (log)*** for now because of the large amount of missing data, but will include it in the robustness checks.[25] We cluster standard errors at the treatment (provincial) level.

[25] About one-third of our respondents did not report their family income.

Table 2: DID Estimates of the Average ITT of Corruption Investigations on Regime Support (OLS Regressions)

	(1)	(2)	(3)
Outcome Variable		**Support Central Party**	
	Coefficient (C.S.E.) (Bootstrap C.S.E.)	**Coefficient (C.S.E.) (Bootstrap C.S.E.)**	**Coefficient (C.S.E.) (Bootstrap C.S.E.)**
Year 2014 * N. of Corruption Investigations	-0.002	-0.002	-0.002
	(0.001)*	(0.001)**	(0.001)**
	(0.001)*	(0.001)**	(0.001)**
Outcome Variable Mean	8.006	8.006	8.011
Outcome Variable S.D.	1.833	1.833	1.830
Outcome Variable Range	[0, 10]	[0, 10]	[0, 10]
Year 2014	YES	YES	YES
N. of Corruption Probes	YES	YES	YES
Demographic Controls	NO	NO	YES
Province F.E.	NO	YES	YES
Intercept	YES	YES	YES
Observations	7,773	7,773	7,274
N. of Clusters	25	25	25
R^2	0.010	0.097	0.121

Notes: This table presents the benchmark results. Appendix Table A2.1 shows the full results. We report both clustered standard errors and clustered bootstrap standard errors (both at the provincial level). *p*-values are based on a two-tailed test*: * p < 0.1, ** p < 0.5, *** p < 0.01.*

Table 2 presents the estimates. We first present the most parsimonious specification in Column (1), add provincial fixed effects in Column (2), and then include ***Demographic Controls*** (potentially post-treatment) in Column (3). Regardless of which specification we use, the coefficient on the interaction term is consistently negative and significant.[26] The effect of corruption probes is striking: holding everything else constant, every 200 corruption probes decrease citizen support by 0.4 on a 0–10 scale (5% of the mean). Many provinces, including Jiangsu, Guangdong, Shandong, and Sichuan, had

[26] Appendix Table A2.1 presents the full table.

conducted nearly 400 probes before July 2014, and more investigations occurred after that.

Our results are highly robust, as shown in a wide range of robustness checks. For example, one concern regarding survey research in authoritarian regimes is that the respondents might over-report their regime support due to political fears (Kuran 1991). Jiang and Yang (2016) show that after the removal of a powerful local politician, Chinese respondents become more willing to express their criticism of the regime. It is therefore possible that the lower public support that we detect is a result of decreased political fear. In our surveys, we asked respondents how much they feared the central government when discussing politics (***Political Fear***). Obviously, this measure is imperfect because respondents with political fear might be too afraid to say so. Thus we also examine whether they responded to this question (***Response to Political Fear***). As survey methodologists show, respondents usually avoid a sensitive question by selecting “Don’t Know” or “No Response” (Presser et al. 2004). In one of the robustness checks, we first use the DID framework to examine whether the campaign changes respondents’ political fear (measured by ***Political Fear*** and ***Response to Political Fear***) but find no evidence. We then control for ***Political Fear*** or ***Response to Political Fear*** and obtain the same estimates (Appendix Tables A3.7–A3.8).

In nine other robustness checks, we use alternative measures of the outcome variable (*Trust Central Government*, *Trust Central Party*, *Support Central Government,* and dichotomous coding of these variables as suggested by Lü and Dickson (2017)), alternative measures of the independent variables (*Number of Corruption Probes (Weighted), Number of Corruption Probes Per Million*, and separating “tigers” and "flies"), control for *Per Capita Family Income (log)*, drop new migrants, conduct placebo tests by creating 100 "fake" anti-corruption campaigns in which the number of corruption probes in each province is drawn randomly from a uniform distribution, and drop one province at a time. None of these tests significantly changes our original results (Appendix Section III).

Evidence on Updating

So far, we have established a negative relationship between corruption investigations and citizens' regime support. Now we provide direct evidence on our updating mechanism that new information disclosed during the campaign has updated citizens' beliefs about public officials.

Our theory predicts that respondents who are exposed to more investigations start to doubt the integrity of public officials. To measure citizens' beliefs about the integrity of public officials (***Beliefs in Officials' Integrity***), we use a question in the surveys asking the respondents' opinion on the statement "In general, public officials are honest," which is scaled from 1 (Strongly Disagree) to 4 (Strongly Agree). *Beliefs in Officials' Integrity* is estimated to have a strong positive effect on people's regime support (Appendix Table A4.1).

We first present a graphic analysis. Again, we "assign" our respondents to either the Treatment Group (with an above-average number of corruption investigations) or the Control Group (with a below-average number of corruption investigations).

Figure 3 presents the DID plot using *Beliefs in Officials' Integrity* as the outcome variable. It is striking that respondents in the Treatment and Control groups, on average, had the same level of *prior beliefs* about officials' integrity in the 2010 baseline survey. But while the Control group updated positively during the campaign, the Treatment group updated negatively. Comparing the *posterior beliefs* in the Treatment group and its counterfactual, the DID estimate is -0.284 ($p<0.01$).

Our regression results confirm this simplified graphic analysis. Using the same DID framework, Table 3 presents the ITT of corruption investigations on *Beliefs in Officials' Integrity*. Consistent with H2, respondents living in provinces that had more corruption probes have lower posterior beliefs about officials' integrity. For every 200 investigations, respondents' *Beliefs in Officials' Integrity* decreases by 0.16 on a 1–4 scale (6.8% of the mean).

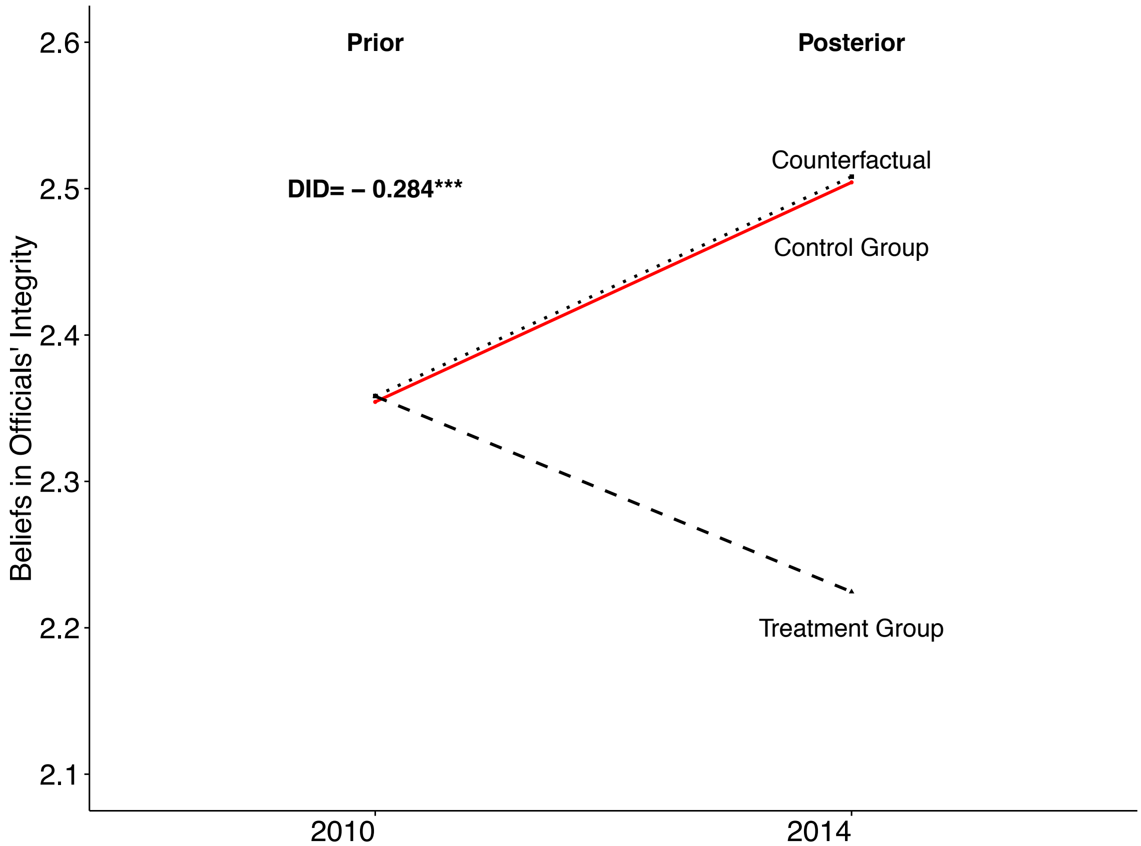

Figure 3: Difference-in-Differences Plot of *Beliefs in Officials' Integrity*

Notes: This graph plots the DID estimates by "assigning" respondents to either the Treatment or Control group. The (black) dashed line indicates average beliefs in officials' integrity among respondents in provinces that had an above-average number of corruption investigations between 2011 and 2014, the (red) solid line indicates average beliefs in officials' integrity among respondents in provinces that had a below-average number of corruption investigations, and the (black) dotted line indicates the parallel trend of the Control group. Appendix Table A4.2 presents the estimates.

Table 3: Difference-in-Differences Estimates of the Average ITT of Corruption Investigations on *Beliefs in Officials' Integrity* (OLS Regressions)

	(1)	(2)	(3)
Outcome Variable	**Beliefs in Officials' Integrity**		
	Coefficient (C.S.E.) (Bootstrap C.S.E.)	**Coefficient (C.S.E.) (Bootstrap C.S.E.)**	**Coefficient (C.S.E.) (Bootstrap C.S.E.)**
Year 2014 * N. of Corruption Investigations	-0.0009	-0.0008	-0.0008
	(0.0004)**	(0.0004)**	(0.0004)*
	(0.0003)***	(0.0004)*	(0.0004)**
Outcome Variable Mean	2.347	2.347	2.344
Outcome Variable S.D.	0.818	0.818	0.815
Outcome Variable Range	[1, 4]	[1, 4]	[1, 4]
Year 2014	YES	YES	YES
N. of Corruption Probes	YES	YES	YES
Demographic Controls	NO	NO	YES
Province F.E.	NO	YES	YES
Intercept	YES	YES	YES
Observations	7,204	7,204	6,754
N. of Clusters	25	25	25
R^2	0.012	0.045	0.067

Notes: This table tests the updating mechanism. Appendix Table A4.3 shows the full results. We report both clustered standard errors and clustered bootstrap standard errors (both at the provincial level). *p*-values are based on a two-tailed test: * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Heterogeneous Effects

The core insight of our theory is that the effects of corruption investigations should be conditional on people's prior beliefs. If they already have a low opinion of officials' integrity, the revealed corruption during the campaign should not surprise them; if their baseline belief is that officials are largely honest, they should be surprised by the rampant corruption revealed during the campaign and reduce their regime support accordingly. This suggests the utility of specifying a model with a triple interaction term among *Year2014*, *Number of Corruption Investigations,* and people's prior beliefs.

We use the provincial mean of *Beliefs in Officials' Integrity* in the 2010 baseline survey to measure average prior beliefs in each province. Using this aggregate measure will enable us to retain our DID framework, considering that we do not have a panel to measure respondents' priors at the individual level. The coefficient on the triple interaction (difference in difference in differences estimator) is then the marginal effect of corruption investigations in provinces with different levels of prior beliefs.

Hainmueller, Mummolo, and Xu (Forthcoming) have proposed the current best-practice method and a more flexible approach to estimating an interaction model. It does not rely on a linear interaction effect and can reliably estimate the conditional effects of the independent variable at values of the moderator that have sufficient common support. Using a binning estimator to divide the provinces into three groups (low, medium, and high) based on their average prior beliefs, Figure 4 shows the estimates of the marginal effect of corruption investigations on *Support Central Party* at different levels of prior beliefs.[27]

[27] I use the interflex function in Stata 14.2.

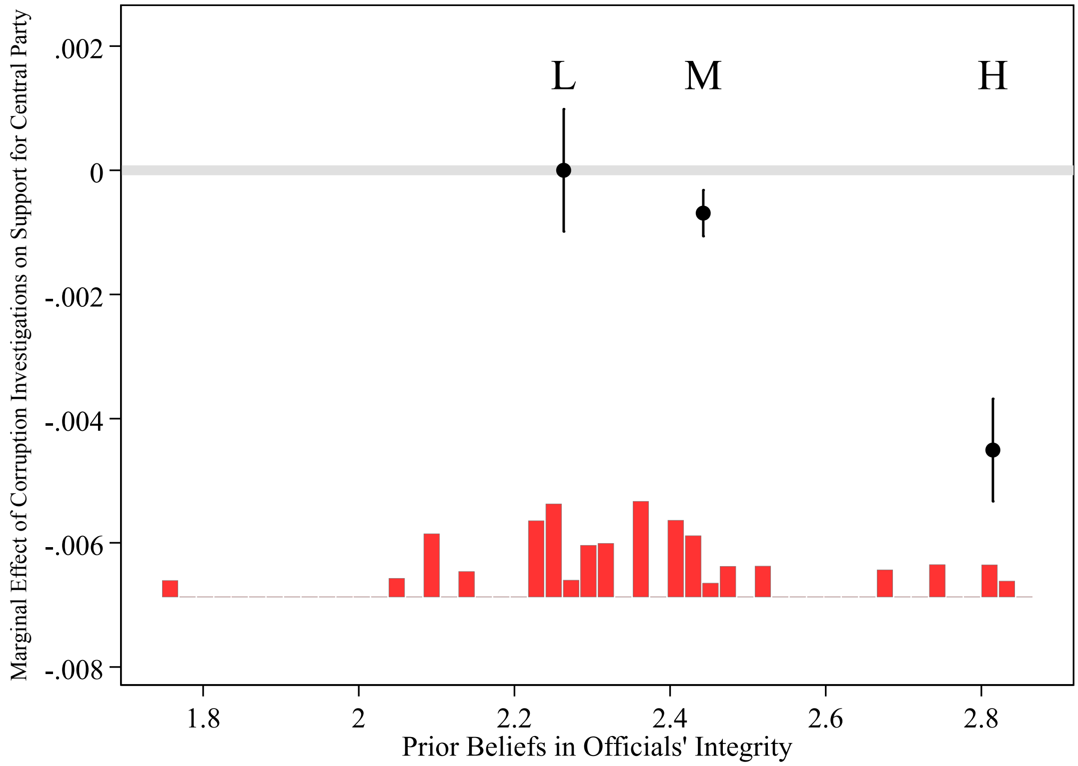

Figure 4: Marginal Effect of Corruption Investigations at Different Levels of Prior Beliefs in Officials' Integrity

Notes: This graph plots the marginal effects (with 95% confidence intervals) of corruption investigations on *Support Central Party* at three different levels of prior beliefs. The bars refer to the distribution of the moderator.

Consistent with H3, corruption investigations have precisely zero effect for respondents who already had a low opinion of officials' integrity, but as people's prior beliefs in officials' integrity become stronger, the marginal effect of corruption investigations becomes statistically significant and negative. At the medium level of priors, the marginal effect of corruption investigations is -0.001 ($p<0.01$); at the high level it is -0.005 ($p<0.01$). Appendix Table A4.4 shows the full estimates, with graphs showing all four outcome variables (Appendix Figures A4.1–A4.3).

An Alternative Mechanism

Above we have provided strong evidence supporting all three hypotheses. Here we examine an alternative (economic) explanation to provide further support for our theory. During the anti-corruption campaign, a large number of officials were removed from office. This political instability might have led to lower economic performance because of frequent government turnovers and high political uncertainties. However, we find no evidence that the campaign had affected economic performance, at least by 2014 when the second wave of our survey was conducted. Using a DID strategy and provincial-level indicators of economic performance, including GDP, per capita GDP, GDP growth rate, and per capita GDP growth rate, we find no evidence that the provinces that experienced more corruption investigations had lower economic performance (Appendix Table A5.1).

DISCUSSION AND CONCLUSION

Corruption is a global disease: it impedes economic growth, increases inequality, and erodes political legitimacy (Bardhan 1997; Seligson 2002). While some countries establish independent institutions, such as anti-corruption agencies or a judiciary, to control corruption, many resort to periodic campaigns to crack down on corruption.

Regimes that rely on anti-corruption campaigns, however, face a dilemma. To convince the public that the investigations are legitimate, they need to publicize details of the investigations, including corrupt officials' personal properties, bribery records, and corruption networks. But in places where the investigations are extensive, this publicity *de*legitimizes the regime by revealing too much corruption to the public, especially if the public did not already know the regime was so corrupt. The leaders of the campaign, however, can still benefit, for example by eliminating their enemies and consolidating their power, but the institutions they lead have to pay a public opinion cost. (Unfortunately, there are no approval ratings for individual leaders in China, so we cannot determine what the public thinks of Xi Jinping.)

Using the case of China's current anti-corruption campaign, we show that citizens update their beliefs about public officials based on new information revealed during such campaigns. The more corruption the campaign reveals, the more it decreases citizen support for the regime. We substantiate our arguments by applying a DID strategy to two national surveys.

To our knowledge, this is the first quasi-experimental evidence of how anti-corruption campaigns affect public support in an authoritarian regime. Our study makes three contributions to the literatures on information, public goods provision, and corruption.

First, there is emerging empirical evidence that information about incumbent malfeasance can change electoral results. Media revelations of incumbent corruption reduce the likelihood of re-election and suppress turnout (Ferraz and Finan 2008; Chang, Golden, and Hill 2010; Chong et al. 2015; Larreguy, Marshall, and Snyder 2017). Our study explicitly takes into account the role of prior beliefs and shows that the way in which information changes regime support depends on citizens' prior beliefs. Our results indicate that an anti-corruption campaign may decrease regime support if the revealed corruption exceeds people's prior beliefs. We join a new line of research that examines how prior assumptions condition the effect of new information (Kendall, Nannicini, and

Trebbi 2015; Arias et al. 2017) and present the first direct evidence of citizen updating using a baseline survey and a follow-up survey.

Second, much research has established a positive association between public goods provision and political support (Bueno de Mesquita et al. 2005; Zhao 2009; Dickson, Shen, and Yan 2017). If we consider clean government to be a public good, by removing corrupt officials, leaders of anti-corruption campaigns are providing a public good to the whole society. But as we have shown, the provision of this type of public good does not necessarily translate into higher levels of regime support. Our study points to the importance of examining what the regime reveals in the process of providing such a public good. If the provision of clean government entails disclosing previously hidden corruption, attempts to increase political support will backfire. Our findings are consistent with other research that shows how public goods provision can have the unintended consequence of raising expectations in ways that can be detrimental to the regime (e.g., Lü 2014).

Last, there is a large literature on the political economy of corruption, but most studies focus on the causes of corruption (Rose-Ackerman 1999; Treisman 2000; Montinola and Jackman 2002; Malesky, Gueorguiev, and Jensen 2015; Zhu 2017). Research on the consequences of corruption primarily examines its effect on economic development and other socio-economic outcomes (Bardhan 1997; Mauro 1998). We join recent efforts to causally assess the effects of corruption or anti-corruption on political attitudes and behavior (Gingerich 2009; Chong et al. 2015; Corbacho et al. 2016). But we differ from most recent research in that our evidence is from an authoritarian regime where the leaders have an urgent and frequent need to legitimize their rule by publicly fighting corruption.

Our findings hence identify a crucial dilemma for authoritarian leaders. If they target corruption with a public campaign, their efforts may backfire on them: instead of building popular support, revelations of extensive corruption cause the public to update their beliefs and consequently reduce their support for the regime. But if leaders do not expose and punish corruption, it is likely to fester and spread, leading to detrimental impacts on the economy and state-society relations. Anti-corruption campaigns may be welcomed by the public, but the public does not necessarily reward the regime for launching the campaign. Anti-corruption campaigns may have other benefits, such as eliminating political rivals and enhancing the reputation of the top leader, but the unintended consequence—reduced regime support—should also be recognized. Our findings reveal a bitter irony: corruption negatively impacts regime support, but fighting corruption is no panacea. In terms of public opinion, the cure may be as bad as the disease.

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