Supporting Information

Contents

A Data and Summary Statistics

A.1 Public Security Expenditure Measure

I use local public security expenditure as a measure for preventive repression because Chinese local governments intensively use local police force, procuratorates, and courts for repression. In China, Career advancement is one of the most important concerns for local officials (e.g., Edin 2003). Among all criteria for cadre evaluation, local instability, such as large-scale protests and riots, serves as "one-strike" veto to local officials' promotion (Edin 2003; Li 2014). Thus, local officials have a strong incentive to repress dissidents, petitioners, and protesters to prevent mass demonstrations (Li 2014). During my fieldwork in China in 2015, a local Street Office Director stated that their office often helps local police prevent petitioners and protesters from congregating on the street or traveling to the province or Beijing to appeal to upper-level governments.[1] Further, local security officials boost preventive coercion during sensitive times, such as the Tiananmen Square Memorial Day (Truex 2018).

Archival documents show that police spending usually constitutes more than 60 percent of total public security expenditure, far greater than other repressive bodies' spending combined (e.g., Yin and Shandong Provincial Department of Finance 2011). Importantly, local public security expenditure does not include the spending on the Armed Police, allowing the following tests to isolate investment in preemptive repression conducted by local police rather than mass repression perpetrated by the armed police at the behest of provincial governments or the central government.

Abundant archival evidence suggests that local public security expenditure is used for maintaining social stability (Zhou and Anhui Provincial Department of Finance 2006). For example, the Anqin City Bureau of Finance mentions in their 2005 annual report that "... we further increased *the 'stability maintenance funding'* by providing an annual public security expenditure of 17,050,000 Yuan (about 2.12 million USD in 2005), which is an increase of 17.6 percent from

[1]An Interview with a sub-district office director in Sichuan Province in June, 2015.

2004. This funding effectively helped maintain social stability and build a harmonious society..." (Zhou and Anhui Provincial Department of Finance 2006).

Further, local public security expenditure does not include spending on the GS Project, which is mainly funded by the central government through the National Planning Commission (currently, the National Development and Reform Commission).[2] In addition, evidence from provincial fiscal yearbooks shows that the expense of Golden Shield constitutes less than 0.5 percent of provincial public security expenditure (e.g., 0.4 percent in Gansu Province and 0.3 percent in Shandong Province in 2010). And data from prefecture fiscal yearbooks further shows that prefecture public security expenditure does not include the expense of the local GS system. See, Gansu Provincial Department of Finance (2011), Yin and Shandong Provincial Department of Finance (2011), and Bai and Heichi City Bureau of Finance (2014).

Note that scholars may have different opinions towards using local public security expenditure as a *cross-sectional* proxy of coercive capacity between regions, but most of them agree that its *temporal changes* reflect the strengthening of police power and repression within each locality. Cross-sectional measurement errors may occur because local governments' capacities to fund their coercive agents vary across cities and regions (Greitens 2017). However, scholars agree that the Chinese Communist Party has indeed attempted to strengthen its coercive capacity in recent decades, as reflected by an increase in security expenditure (Greitens 2017), the empowering of the public security chiefs (Wang 2014), and a strong correlation between these two developments (Wang and Minzner 2015). Thus, temporal changes in security funding over time within counties can reflect changes in local repression. Since the DiD approach in my paper exploits temporal variation rather than cross-sectional variation, it is less prone to measurement errors caused by local variation in funding capacity.

A.2 Political Prisoner Measure

The date of detention started in 1981 but the data quality appears worse for early years because the Commission started to record Chinese prisoners in 2004 and data entries in earlier years were

[2]China.com (2003), The Golden Shield Project, http://www.china.com.cn/chinese/zhuanti/283732.htm.

retrospective. To get a longer pretreatment period and avoid using early-year entries, my empirical strategy for this prisoner data is based on the second phase of the Golden Shield project finished in 2012, whereas the first phase of the project was finished in 2005. Considering the possibility that the effect of the first-phase Golden Shield project could contaminate the effect of the second-phase project, I exclude all entries prior to 2006.

Between 2006 and 2017, there were 5,007 entries in the CECC-PPD. 52 percent of all arrests are related to association, 62 percent of them are related to speech, and 11 percent are related to spread of information (categories are not mutually exclusive). Excluded double counted entries, 82 percent of all arrests are related to association, speech, and spread of information that often facilitates protests. The rest 18 percent of arrests are related to purely religious and ethnic issues, which often trigger protests or even violent actions. Thus, the political prisoner measure well captures local preventive repression in China.

One may be concerned that reporting of prisoners can be affected by investments in public security or in the Golden Shield Project since political imprisonment is a sensitive topic where media report is suppressed in China (but many NGOs and human right groups still make great efforts to collect and report information on political imprisonment) (Gueorguiev 2017). However, if the reporting of imprisonment is affected by investments in local public security, counties with more security spending will have stronger media repression and hence less reporting on political arrests, which will bias the DiD estimate downward. In other words, the positive, significant effect of surveillance on political prisoners will be underestimated. Thus, removing reporting bias can make the results even stronger.

In the main text, I code the number of county-level political prisoners using prisoners' detention locations. Though most of the dissidents are detained in prisons or detention centers close to the places where they were arrested, it is possible that the places of arrest are different from the places of detention. To address this concern, I focus on a subsample of 1,414 dissidents currently under detention and search every prisoner's arrest location online. The current prisoner data is obtained from the Political Prisoner Database collected by the Congressional-Executive Commission on

China.[3] Among 1,193 prisoners whose locations are identifiable in both the main sample and the subsample, 759 of them (63%) have same locations of arrests and detentions, and 944 of them (80%) are located in same prefectures. I aggregate the number of arrests in each county-year unit based on the searched locations. Appendix C.3 reports the results obtained from using this new measure. The estimates remain statistically significant at 0.01 level.

A.3 Summary Statistics

Table A.1: Summary Statistics of the Public Security Data

Variable	Unit	Obs	Mean	Std. Dev.	Min	Max
Expenditure - Public Security	10,000 Yuan	36185	1641.905	3453.701	0	139932
Expenditure - Social Welfare	10,000 Yuan	25911	1530.886	4405.911	0	265206
Expenditure - Education	10,000 Yuan	36247	6161.444	8297.082	0	331798
Expenditure - Agriculture	10,000 Yuan	27426	797.897	1511.043	0	44578
Expenditure - Administration	10,000 Yuan	28376	2598.018	3220.07	17	163536
Expenditure - Total	10,000 Yuan	36282	26259.86	47398.21	162	2225040
GDP	10,000 Yuan	26929	364974	652030.2	0	29200000
Population	10,000 Persons	37018	45.06952	48.10466	0	5022
Urbanization Ratio	0-1	35722	0.2506099	0.2357159	0	1
Golden Shield	(County-year)	37349	0.1019037	0.3025256	0	1
Year	(Year)	37349	2000.731	3.970078	1994	2007

Table A.2: Summary Statistics of the Political Prisoner Data

Variable	Unit	Obs	Mean	Std. Dev.	Min	Max
Arrests of Political Activists	Person	37696	0.1201984	1.555844	0	165
Surveillance Camera System	(County-year)	37696	0.1398557	0.3468418	0	1
Year	(Year)	37696	2011.5	3.452088	2006	2017
Primary Industry Value Added	10,000 Yuan	19298	177712.8	160373	84	1399516
Secondary industry Value Added	10,000 Yuan	19300	616356.2	1056598	13	17000000
Population	10,000 Persons	19305	70.79207	1487.714	0.7	115453
Land Area	Square km	19303	455009.8	62600000	56	8700000000
Expenditure - Total	10,000 Yuan	19294	167531.7	167237.2	214	3889833
Grain	Ton	17576	257715.2	288328.8	7	3349885
Cotton	Ton	8336	6988.293	21329.66	1	390177
Oil Crop	Ton	18479	14634.78	26281.02	1	381336
Welfare Center	Number	18714	16.11505	24.17274	0	1474
Welfare Bed	Bed	18689	1161.222	1456.733	0	20790
Hospital Bed	Bed	19267	1266.968	1177.316	10	37846
Elementary School Students	Person	19304	35313	32907.62	482	1600272
Middle School Students	Person	19291	25664.31	21991.58	106	224076

[3] Data available at: https://www.cecc.gov/resources/political-prisoner-database

A.4 Golden Shield Award Prefectures and "3111" Initiative Pilot Counties

Awarded Prefectures for the 1st phase of the GS Project

In 2006, the Ministry of Public Security awarded about 40 prefectural-level Bureaus for their excellent work in completing the first phase of this project. As shown in Figure A.1, the distribution of these prefecture is even in China.

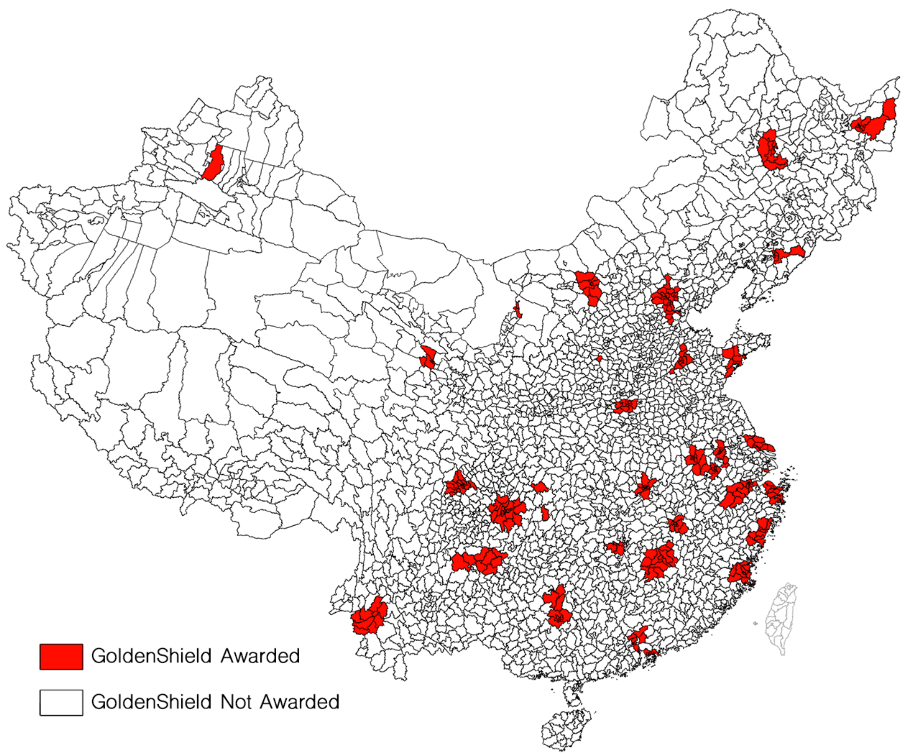

Figure A.1: The Distribution of Golden Shield Award Prefectures
Source: Ministry of Public Security Official Website

Criteria for Selecting "3111" pilot counties/cities – 2nd Phase of the GS Project

As discussed in the main text, about 660 pilot cities/counties all over China were selected in three waves to install and operate the "3111" street surveillance camera and alarm systems. The materials collected during my fieldwork also provide some information regarding how those "3111" pilot counties were selected. In particular, in the interviews with the managers of several security product companies conducted by the China Public Security journal, one manager mentioned that "many '3111' projects were constructed based on the current telecommunication networks provided by major Telecom operators. Thus, it is very important for us to cooperate with those operators"(Ding 2007). Because the *Notice on Construction of City Security Alarm System (2005)* states that "Pilot counties start first ... provinces should promote security alarm system in pilot areas with *suitable conditions* and use those areas as examples for other cities/counties to replicate the security alarm system", we can infer that areas with *suitable conditions* are counties with better telecommunication infrastructures, better economic conditions, and larger populations, etc. We can also infer that the selected pilot counties are evenly distributed within China's 31 provinces to serve as examples for other counties to replicate their success, which means the endogeneity between repression and surveillance should not be a major concern. Figure A.2 shows the distribution of the pilot counties/cities.

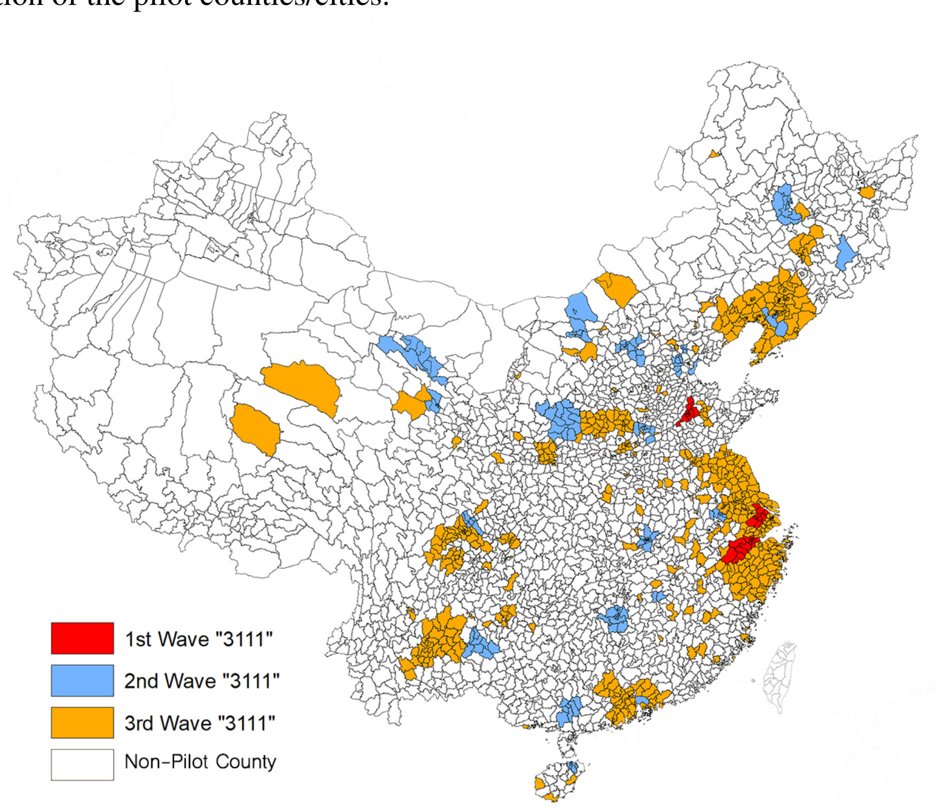

Figure A.2: The Distribution of 3111 Pilot Counties/Cities
(Xinjiang, Tibet, and Beijing Excluded)
Source: Ministry of Public Security No.1 Research Institute

Correlation Matrix for Past Repression and Surveillance City Selection

Table A.3 shows that past public security expenditure and past political prisoners are not correlated with the selection of Golden Shield prefectures and the 3111 counties.

Table A.3: Correlations between past repression and selections of surveillance counties

	Public Security Expenditure		Political Arrests	
	1-year Lag	2-year Lag	1-year Lag	2-year Lag
Golden Shield Cities	0.0479	0.0471	-	-
3111 Initiative Cities	-	-	0.0616	0.0496

B Additional Empirical Analyses

B.1 Public Security Expenditure Data

I include more control variables such as local GDP and local governments' other administrative expenditure for all model specifications. The former control for local economic development level, although this is addressed by controlling for total government expenditure and urbanization ratio in the main models; the latter addresses measurement problem since non-repressive operating expenses of local public security bureaus are highly correlated with other administrative expenditure of local governments. As shown in Table B.4, the effect of surveillance on public security expenditure remains large and statistically significant across all specifications. Note that I do not include these two controls in the main models because the GDP variable misses all counties/cities in 1994, 1995, 1996, and 2007, and the administrative expenditure variable misses all counties/cities in 2004 and 2005.

Table B.5 reports the results on repression-cooptation comparison using a log scale. The results are similar to those of Table 3 in the main text.

B.2 Political Prisoner Data

Additional Controls (Sample with Missing Data)

The models in Table 2 in the main text do not include social-economic control variables because the social-economic data from the County Statistical Yearbooks misses all the county-level districts in cities (about two-fifths of the total county/district-level units, Figure B.3).

I further address potential selection biases by controlling for the number of telephone users (a proxy for telcommunication infrastructures), land area, population, primary industry value increase, secondary industry value increase, and local fiscal expenditure. These controls along with

Table B.4: Surveillance and Public Security Expenditure, with More Controls

	Fixed Eff.	Lagged DV	Lagged DV	Fixed Eff.	Lagged DV	Lagged DV
VARIABLES	(1) Security (log)	(2) Security (log)	(3) Security (log)	(4) Security (log)	(5) Security (log)	(6) Security (log)
GS Completed County	0.0995***	0.0297**	0.0551***	0.128***	0.0338**	0.0485**
	(0.0303)	(0.0122)	(0.0208)	(0.0369)	(0.0139)	(0.0214)
Post-2005	0.0387***			0.552***		
	(0.0128)			(0.102)		
Lagged DV		0.773***			0.755***	
		(0.0126)			(0.0138)	
2yr-Lagged DV			0.662***			0.645***
			(0.0154)			(0.0188)
GDP (log)	0.00205	0.0112***	0.0187***			
	(0.00588)	(0.00373)	(0.00512)			
Admin. Exp. (log)				0.149***	0.0465**	0.0735***
				(0.0531)	(0.0215)	(0.0267)
Total Exp. (log)	0.570***	0.237***	0.356***	0.531***	0.223***	0.326***
	(0.0492)	(0.0146)	(0.0189)	(0.0315)	(0.0145)	(0.0195)
Population (Log)	0.0445	-0.0153***	-0.0275***	0.0450	-0.00173	-0.00771
	(0.0401)	(0.00550)	(0.00828)	(0.0298)	(0.00587)	(0.00898)
Urbanization Ratio	-0.00247	0.0411***	0.0578***	-0.00100	0.0619***	0.0864***
	(0.0163)	(0.00974)	(0.0144)	(0.0132)	(0.0118)	(0.0176)
County Fixed Effect	Yes	No	No	Yes	No	No
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Constant	1.363***	-0.778***	-1.059***	0.000178	-0.696***	-0.962***
	(0.488)	(0.0669)	(0.0951)	(0.407)	(0.0857)	(0.136)
Observations	25,340	24,827	24,525	27,329	24,489	21,768
R-squared	0.826	0.948	0.927	0.858	0.948	0.924
Number of County	2,731			2,742		

Model (1) and (4) use DiD models with prefecture-level clustered SEs using cluster-bootstrap variance matrix developed by Bertrand, Duflo, and Mullainathan (2004). Model (2), (3), (5) and (6) use lagged dependent variable models with robust standard errors clustered on prefecture level.
*** p<0.01, ** p<0.05, * p<0.1

the lagged outcome variable model can address potential endogeneity between surveillance and repression. As Table B.6 shows, the effect of "3111" Initiative on political prisoners remains large and statistically significant, and including the telecommunication variable does not change the magnitude of the DiD estimate much.

Count Data Models

Due to the panel structure of the prisoner data, I fit linear fixed effects models as main analyses. Since the outcome variable is a count of political arrests with discrete and non-negative values, I

Table B.5: Golden Shield, Repression, and Co-optation

	Fixed Eff.	Fixed Eff.	Fixed Eff.	Fixed Eff.
	(1)	(2)	(3)	(4)
VARIABLES	Security (log)	Welfare (log)	Education (log)	Agriculture (log)
GS Completed County	0.181***	-0.131**	0.0194	-0.162***
	(0.0226)	(0.0649)	(0.0166)	(0.0496)
Post-2005	-0.0501***	0.107***	0.0413***	-0.0142
	(0.0103)	(0.0297)	(0.00759)	(0.0227)
Total Expenditure (log)	0.971***	0.910***	0.704***	1.102***
	(0.00454)	(0.0130)	(0.00333)	(0.00994)
Population (Log)	0.0599***	0.316***	0.288***	-0.0599***
	(0.00456)	(0.0131)	(0.00335)	(0.00999)
Urbanization Ratio	0.200***	0.171***	-0.228***	-0.811***
	(0.00941)	(0.0270)	(0.00690)	(0.0206)
County Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Constant	-2.882***	-4.151***	0.543***	-3.806***
	(0.0333)	(0.0956)	(0.0245)	(0.0730)
Observations	19,714	19,714	19,714	19,714
R-squared	0.922	0.705	0.952	0.754

Model (1) - (4) use two-way fixed effect models. Robust standard errors clustered on prefecture are shown in parentheses.
*** p¡0.01, ** p¡0.05, * p¡0.1

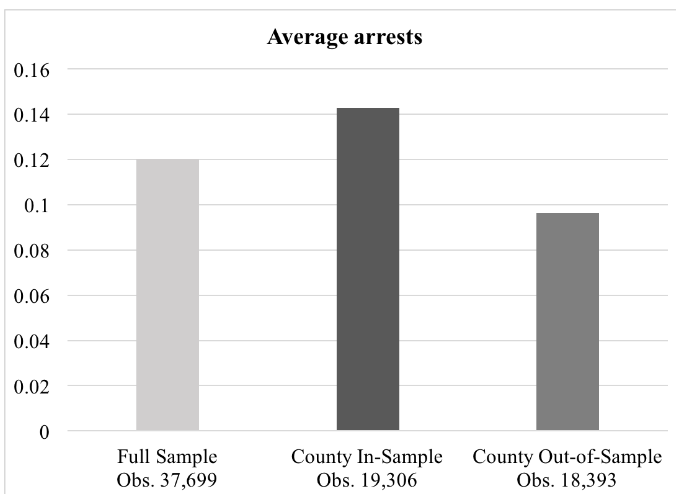

Figure B.3: Average arrests in samples with and without controls

also fit the data using Fixed-Effects Negative Binomial models for difference-in-differences estimations and Negative Binomial (NB) models for lagged DV estimations. The effects are substantial and statistically significant. Moreover, I use Zero-inflated Negative Binomial (ZINB) models for

Table B.6: "3111" Initiative (2nd Phase GS) and Political Prisoners, with Controls

	Two-way	Two-way	Lagged DV	Lagged DV
	(1)	(2)	(3)	(4)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners
"3111" Pilot Counties	0.0689**	0.0706**	0.0565**	0.0756**
	(0.0269)	(0.0279)	(0.0244)	(0.0304)
Lagged Prisoners			0.131***	
			(0.0107)	
2yr-Lagged Prisoners				0.0724***
				(0.0150)
Tel User (Log)		0.0198	-0.00570	-0.00840
		(0.0376)	(0.0224)	(0.0255)
Land Area (Log)	-0.0455	-0.0425	0.0559***	0.0651***
	(0.0391)	(0.0414)	(0.0167)	(0.0203)
Population (Log)	0.103	0.0836	-0.154***	-0.172***
	(0.344)	(0.361)	(0.0542)	(0.0639)
1st Industry (Log)	0.0292	0.00571	0.0318	0.0336
	(0.0652)	(0.0710)	(0.0245)	(0.0291)
2nd Industry (Log)	-0.114*	-0.106*	-0.161**	-0.190**
	(0.0671)	(0.0638)	(0.0664)	(0.0807)
Fiscal Budget (Log)	0.0418	0.0282	0.279**	0.310**
	(0.0781)	(0.0799)	(0.109)	(0.127)
County Fixed Effect	Yes	No	No	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Constant	0.559	0.720	-1.233***	-1.042**
	(1.398)	(1.566)	(0.465)	(0.487)
Observations	17,919	17,762	16,024	14,206
R-squared	0.002	0.002	0.031	0.020
Number of County	1,847	1,844		

Model (1) and (2) use two-way fixed effect models with cluster-bootstrap variance matrix. Model (3) and (4), (5) use lagged outcome models. Robust standard errors are clustered on county. Samples exclude counties in Beijing, Tibet, and Xinjiang.
*** p<0.01, ** p<0.05, * p<0.1

the lagged DV estimation to account for the excessive zeros in the political arrests variable. The results remain large and statistically significant. In particular, surveillance increases political arrests by 1.5 to 2.1 more persons (Table B.7). Given that the average number of prisoners in counties with non-zero arrests is 3.1, 1.5 - 2.1 increase is substantial. Akaike's information criterion (AIC) and Bayesian information criterion (BIC) tests suggest that ZINB models have slightly smaller AICs and BICs than NB models, which means that excessive zeros in the dependent variable is not of big concern (Table B.8).

Table B.7: Surveillance and Political Prisoners, Count Data Models

	Fixed Eff. Neg. Bino.	Neg. Bino. Lag-DV	Neg. Bino. Lag-DV	ZINB Lag-DV	ZINB Lag-DV
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners
3111 Pilot Counties	0.412***	0.536***	0.720***	0.374**	0.455***
	(0.108)	(0.119)	(0.136)	(0.146)	(0.162)
Lagged Prisoners		0.703***		0.374**	
		(0.115)		(0.146)	
2yr-Lagged Prisoners			0.671***		0.180***
			(0.0669)		(0.0387)
County Fixed Effect	Yes	No	No	No	No
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Constant	-3.116***	-2.842***	-1.739***	-0.718***	0.160
	(0.161)	(0.146)	(0.168)	(0.191)	(0.261)
Observations	9,756	34,551	31,410	34,551	31,410
Number of County	813				

Model (1) use two-way fixed-effect negative binomial model with cluster-bootstrap variance matrix. Model (2), (3) use negative binomial model with lagged dependent variable. Model (4) and (5) use Zero-inflated Negative Binomial model with lagged dependent variable. Robust standard errors are clustered on county for all models.
*** p<0.01, ** p<0.05, * p<0.1

"3111" Initiative and Co-optation, Original Units

Table B.8 reports the effect of the "3111" Initiative on public goods provision using the original units of those goods.

B.3 Currently Detained Prisoners with Arrest Information Searched Online

As mentioned above, I focus on a subsample of 1,414 dissidents currently under detention and search every prisoner's arrest location online to address the concern that the places of arrest might be different from the places of detention. The results from this new data are very close to the main results. Table B.9 Column (1) reports the difference-in-differences estimates. The result indicates that counties with advanced surveillance camera system arrest 0.042 more activists than counties without surveillance. Average arrests per county are about 0.034 persons, so the number of arrests is more than doubled compared with that in surveillance counties. Column (2) and (3) reports the lagged DV models. The results are also large and statistically significant. Excluding counties from

Table B.8: “3111” Initiative and Co-optation

	Welfare			Education		Agriculture		
	Fixed	Fixed	Fixed	Fixed	Fixed	Fixed	Fixed	Fixed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Welfare Center (N)	Welfare Bed (N)	Hospital Bed (N)	Primary Stdt (Person)	Middle Stdt (Person)	Grain Prodct (Ton)	Cotton Prodct (Ton)	Oil Crop Prodct (Ton)
3111 Pilot	-0.191	60.95	-26.71	1,291***	433.8	-5,615*	-1,282**	-1,514***
	(0.564)	(48.22)	(26.06)	(471.9)	(379.3)	(3,024)	(527.5)	(350.0)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Eff.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Eff.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	12.51	148.6	-10.49	38,502	28,706	181,230	-25,280	9,831
	(2.691)	(140.4)	(230.9)	(3,273)	(2,330)	(9,084)	(20,500)	(1,350)
Observations	21,373	21,328	22,098	22,139	22,127	20,366	9,484	21,240
R-squared	0.019	0.310	0.593	0.060	0.322	0.178	0.078	0.044
N of admcode	2,085	2,084	2,089	2,089	2,089	2,069	1,112	2,049

Model (1) - (8) use two-way fixed effect models with robust standard errors clustered on county level.
*** p<0.01, ** p<0.05, * p<0.1

Beijing, Tibet, and Xinjiang does not change the results.

Table B.9: “3111” Initiative and Current Political Prisoners

	All Provinces			Tibet, Xinjiang & Beijing Excluded		
	Two-way	Lagged DV	Lagged DV	Two-way	Lagged DV	Lagged DV
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners
3111 Pilot Counties after 2012	0.0415***	0.0401***	0.0432***	0.0289***	0.0182**	0.0198**
	(0.0130)	(0.0114)	(0.0131)	(0.00941)	(0.00873)	(0.00827)
Lagged Prisoners		0.157***			0.0866	
		(0.0490)			(0.0533)	
2yr-Lagged Prisoners			0.137**			0.151**
			(0.0551)			(0.0741)
County Fixed Effect	Yes	No	No	Yes	No	No
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.00175*	0.00112	0.000810	0.00150*	0.000619	0.000523
	(0.000919)	(0.000874)	(0.000627)	(0.000903)	(0.000542)	(0.000558)
Observations	37,167	34,308	31,449	34,736	32,064	29,392
R-squared	0.008	0.030	0.022	0.007	0.013	0.024
Number of County	2,859			2,672		

Model (1) and (4) use two-way fixed effect models with cluster-bootstrap variance matrix. Model (2), (3), (5), and (6) use lagged dependent variable models. Robust standard errors clustered on county are shown in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

B.4 Trajectory Balancing and Matching for Security Spending Data

I use recently developed trajectory balancing and matching approaches to address potential time-varying omitted biases. The trajectory balancing approach (Xu and Hazlett 2018) uses kernel balancing to weight control observations to match the treated observations in terms of the pre-treatment trends (i.e., high-order "trajectory") of outcome and covariates. This approach handles time-vary confounders by assuming some time-fixed linear combination of those time-varying confounders. Because pretreatment outcomes contain information of those confounders, balancing control observations with the treated on pretreatment outcomes helps difference out those time-varying confounders. Table B.10 shows the results from the Trajectory Balancing approach for the security expenditure data. In total, surveillance leads to an 6.6 percent increase in public security spending and the effect is statistically significant at 0.01 level. The yearly average effects show a 5 percent increase in 2006 and an 8.3 increase in 2007. The pretreatment effects are not statistically significant whereas the post treatment effects are significant at 0.01 level.

Table B.10: Trajectory Balancing Approach

Total Average Treatment Effect on the Treated:

	ATT	S.E.	N.Treated	N.Control
	0.06624***	0.02088	328	2734

Yearly Average Treatment Effect on the Treated:

Year	ATT	S.E.	N.Treated	N.Control
2007	0.0833443***	0.02535	164	1367
2006	0.0491447***	0.02068	164	1367
2005	0.0076821	0.01292	164	1367
2004	0.0027995	0.01278	164	1367
2003	0.0184354	0.01399	164	1367
2002	-0.0071704	0.01479	164	1367
2001	-0.0180269	0.01472	164	1367
2000	0.001773	0.01383	164	1367
1999	0.0318249	0.0131	164	1367
1998	0.015156	0.0124	164	1367
1997	0.0046964	0.0114	164	1367
1996	-0.0046143	0.01131	164	1367
1995	-0.0006649	0.0129	164	1367
1994	0.0096204	0.01154	164	1367

I further employ a matching method for panel data developed by Imai, Kim and Wang (2018). I select 5 control observations for each treatment observation among observations with identical treatment history for one year ahead, based on the outcome variable, public security expenditure, and counties' population (i.e., controlling for city size). Table B.11 checks the balance between the treated group and the control group in the matched sample. Balance is assessed by taking the difference between the values of the outcome variable in the treated unit and the weighted average of that across all the control units in each matched set, divided by one standard deviation of the values of the outcome variable across all treated units of all matched sets. A smaller value indicates better balance. The results suggests that the treated group and the control group are rather close.

Table B.11: Balance Table for Matched Sample

Mean Balance of Security Expenditure

Time to Treatment	-1	0	1
balance	0.1049316	0.1478327	0.1895312

Table B.12 presents the matching results. Comparing with counties without completed GS system, counties with completed GS systems increase public security expenditure by 4 percent in 2006 and 7.6 percent in 2007. The effect in 2006 is statistically significant at 0.05 level.

Table B.12: Estimate of Average Treatment Effect on the Treated (ATT) by Period

	t+0	t+1
Point Estimate(s)	0.0386453	0.0751547
Standard Error(s)	0.0187202	0.0481432
Lower Limit of 95 % Regular Confidence Interval	0.0002853	-0.026908
Upper Limit of 95 % Regular Confidence Interval	0.0721407	0.1623591
Bias-corrected Estimate(s)	0.0392122	0.0761045
Lower Limit of 95 % Bias-corrected Confidence Interval	0.0051499	-0.0120496
Upper Limit of 95 % Bias-corrected Confidence Interval	0.0770053	0.1772175

Weighted Difference-in-Differences with Propensity Score. Matches created with 1 lags. Standard errors computed with 500 Weighted bootstrap samples.

B.5 DiD Falsification Tests

To examine whether other possible (but unidentified) differences might drive the repression level higher in counties with a completed surveillance system, I examine if there exist some pretrend differences. This exercise provides a general falsification test to check whether the differential changes in repression link to surveillance projects or other confounders. Figure B.4 plots the effects of pre- and post-GS project on public security expenditure based on the Trajectory Balancing approach, running from two years ahead and eleven years behind. The estimates show no discernible differences in pretrends.

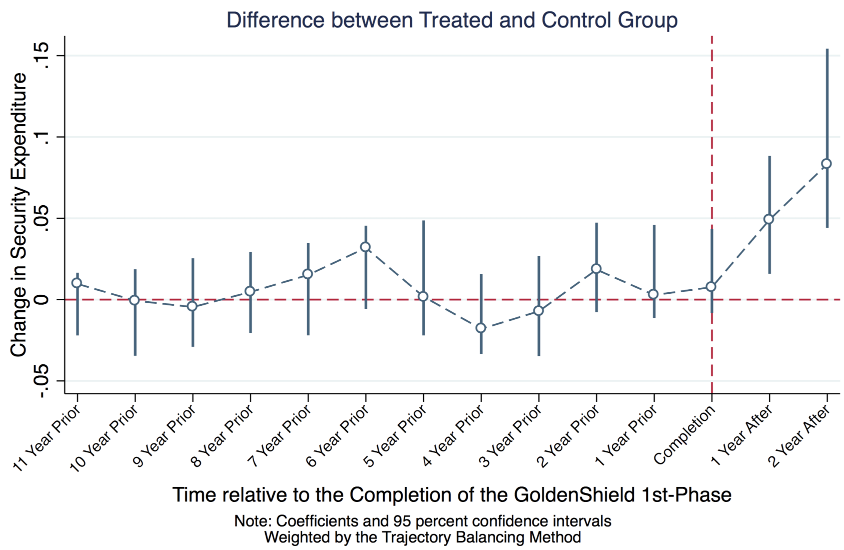

Figure B.4: Pre- and Post-GS Estimates, Public Security Expenditure, DiD, 1994-2007

Figure B.5 plots the effects of pre- and post-3111 Initiative treatments on the number of political prisoners. The estimates show no effects in the six years before the completion of the "3111" Initiative, with sharply increasing effects on arrests in the years following the completion. The common pretreatment trends in both figures provide evidence consistent with the parallel-trends assumption necessary for a causal interpretation of the surveillance estimate.

B.6 Alternative Mechanisms

"Redefining" radicals vs. "identifying" radicals?

It is possible that digital surveillance could lower dictators' repression thresholds to include citizens' browsing preferences. Thus, the positive effect of surveillance on repression could be due to an increase in "defining" radicals instead of improved information for identifying radicals. To address this concern, I examine the 5007 prisoners to identify and exclude all Internet and social

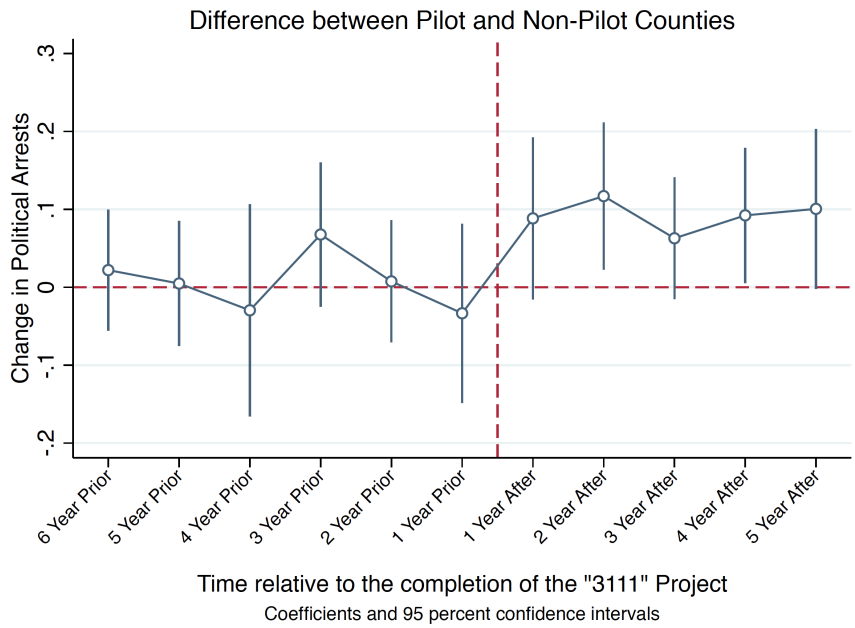

Figure B.5: Pre- and Post-3111 Estimates, Political Prisoners, DiD specification, 2006-2017

media related arrests (hereafter, ISM-related). The CECC-PPD prisoner data includes information on the reasons for imprisonment. I identify ISM-related cases by the following keywords: "internet", "wechat", "qq", "online", "weibo", "msn", "tencent", "skype", "google", "twitter", "facebook", "whatsapp", "messenger", "website", "webpage", "web", "bbs", and "forum", etc. Among the 5007 cases, 10.6% are ISM-related. I then examine the effect of surveillance on political prisoners based on a subsample that excludes all ISM-related arrests. As Table B.13 shows, the results based on this subsample remain large, positive, and statistically significant. The significant effect suggests that even if dictators might lower their repression thresholds under digital surveillance, better information for identifying radicals still leads to an increase in repression in dictatorships.

It should be noted that arresting online activists does not necessarily means the government has lower repression thresholds. Careful examination of the ISM-related cases suggests that most of the online activists used the Internet as a platform for anti-regime mobilization. For example, Ou Quanjiang (Case 2016-00330) along with two others were arrested for "using a WeChat group to organize protests in Ningxiang" during a large June 27 protest in front of Ningxiang government offices against the planned construction of a waste incineration plant.[4] Had the Internet or social

[4]See China's Political Prisoner Database, *Congressional-Executive Commission*. Nov. 5, 2017.

Table B.13: 3111 Initiative and Political Prisoner, No Internet-related Arrests

	All Provinces			Tibet, Xinjiang & Beijing Excluded		
	Two-way	Lagged	Lagged	Two-way	Lagged	Lagged
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners
"3111" Pilot	0.0707***	0.0517***	0.0581***	0.0863***	0.0237**	0.0269**
	(0.0248)	(0.0164)	(0.0178)	(0.0213)	(0.0110)	(0.0115)
Lagged DV		0.149***			0.138***	
		(0.0106)			(0.0119)	
2yr-Lagged			0.0845***			0.0832***
			(0.0125)			(0.0131)
County Fixed	Yes	No	No	Yes	No	No
Year Fixed	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0319***	0.0574***	0.230***	0.0286***	0.0575***	0.199***
	(0.00636)	(0.00905)	(0.0594)	(0.00599)	(0.00944)	(0.0616)
Observations	37,699	34,551	31,410	35,369	32,417	29,470
R-squared	0.001	0.023	0.008	0.001	0.020	0.008
N of County	3,148			2,952		

Model (1) and (4) use two-way fixed effect models with cluster-bootstrap variance matrix. Model (2), (3), (5), and (6) use lagged outcome models. Robust standard errors are clustered on county.
*** p<0.01, ** p<0.05, * p<0.1

media been unavailable, those radicals would use other means to mobilize or just hide. Although it is difficult to draw a clear boundary between detecting radicals and "redefining" radicals under digital surveillance, the above evidence suggests that the former mechanism is clearly in play.

More arrests because of increased security expenditure?

One may be concerned that the increase in political prisoners is related to increased security expenditures in the 1st Phase of Golden Shield Project. This is not likely the case since, as discussed in Section A.2, I exclude all prisoners data prior to 2006 when the 1st-phase Golden Shield project had completed. I further address this concern by separating the 2006-2017 sample (with political prisoner measure) by 1st-phase Golden Shield *treated* and *non-treated* counties to examine the effect of the "3111" Initiative on political prisoners in separated samples. The logic of this test is that, if the increases in political prisoners were mainly driven by the increase in security expenditure in the 1st phase of the Golden Shield project, we would expect the "3111" Initiative to have little effect in a sample of counties without expenditure increase or in a sample of counties that had all experienced expenditure increase.

Table B.14 shows that a very small proportion of counties had experienced an increase in security expenditures in the 1st phase (343 versus 2,805). It further shows that the effect of the "3111" Initiative on the number of political prisoners is statistically significant across different models and different samples, which suggests that the increase in political prisoners is unlikely to be driven by the increase in security expenditures during the 1st phase Golden Shield Project.

Table B.14: "3111" Initiative and Political Prisoners

(By Treated and Non-treated Counties in the 1st Phase)

	1st Phase GS Non-treated Counties			1st Phase GS Treated Counties		
	Two-way	Lagged	Lagged	Two-way	Lagged	Lagged
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners
"3111" Pilot	0.0666**	0.0356*	0.0411**	0.0989**	0.192***	0.217***
	(0.0290)	(0.0191)	(0.0208)	(0.0437)	(0.0433)	(0.0520)
Lagged DV		0.153***			0.256***	
		(0.0110)			(0.0621)	
2yr-Lagged			0.0889***			0.202***
			(0.0142)			(0.0656)
County Fixed	Yes	No	No	Yes	No	No
Year Fixed	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0331***	0.0546***	0.260***	0.0477***	0.0909***	0.0611***
	(0.00693)	(0.00951)	(0.0684)	(0.0181)	(0.0317)	(0.0191)
Observations	33,616	30,811	28,010	4,083	3,740	3,400
R-squared	0.002	0.024	0.009	0.019	0.087	0.059
N of County	2,805			343		

Model (1) and (4) use two-way fixed effect models with cluster-bootstrap variance matrix. Model (2), (3), (5), and (6) use lagged outcome models. Robust standard errors are clustered on county.
*** p<0.01, ** p<0.05, * p<0.1

Under-reporting on political prisoners?

One concern is that the political prisoner data might be under-reported. However, the staggered DiD design based on three waves of "3111" pilot counties are less sensitive to reporting bias: it is unlikely that the systematic bias in reporting, if there were any, would overlap exactly with the multiple time periods and multiple groups of counties to bias the results. I further address this under-reporting concern by analyzing the effect of surveillance on political prisoners using a sample of counties in Guangdong Province only. Adjacent to Hong Kong and Macau, Guangdong

Province is known for its economic prosperity and its openness to international influences. Due to the impact of NGOs and activists from nearby Hong Kong, voluntary organizations in Guangdong have developed earlier and more rapidly than in any other areas of China. More importantly, many Hong Kong-based organizations closely observe human rights violations in Guangdong Province. Thus, the under-reporting problem is likely less severe in Guangdong than in the entire country. Table B.15 shows the effect of surveillance on political prisoners is about 2 times larger than that from the full sample. Although this larger effect could be due to heavier repression in Guangdong, the statistical significance suggests that the effect is robust when there is less reporting bias.

Table B.15: 3111 Initiative and Political Prisoners, Guangdong Province

	Guangdong Province		
	Two-way	Lagged DV	Lagged DV
	(1)	(2)	(3)
VARIABLES	Prisoners	Prisoners	Prisoners
"3111" Pilot Counties	0.233***	0.254***	0.273***
	(0.0891)	(0.0903)	(0.103)
Lagged Prisoners		0.140**	
		(0.0545)	
2yr-Lagged Prisoners			0.0875**
			(0.0384)
County Fixed Effect	Yes	No	No
Year Fixed Effect	Yes	Yes	Yes
Constant	0.113**	0.0480*	0.0255
	(0.0485)	(0.0262)	(0.0231)
Observations	1,692	1,551	1,410
R-squared	0.021	0.044	0.033
Number of County	141		

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

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