Online Appendix

To Repress or To Co-opt? Authoritarian Control in the Age of Digital Surveillance

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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. Further, local security officials boost preventive coercion during sensitive times, such as the Tiananmen Square Memorial Day (Truex 2019).

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 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).[1] 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).

One concern is that cross-sectional measurement errors may occur because local governments' capacities to fund their coercive agents vary across cities and regions (Greitens 2017). However, most of 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 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

[1]China.com (2003), The Golden Shield Project, http://www.china.com.cn/chinese/zhuanti/283732.htm. Accessed July 4, 2019.

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. I further address the concern of under-reporting in Section B.6.

In the main text, I code the number of county-level political prisoners using the locations of detention complimented by their counties of residence. Though most of the dissidents are detained in prisons or detention centers close to the places where they were arrested and live, 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 in prisons and search every prisoner's arrest location online because the information of current prisoners are more available online than ex-prisoners. The current prisoner data is obtained from the Political Prisoner Database collected by the Congressional-Executive Commission on China.[2] Among 1,193 prisoners whose locations are identifiable in both the main sample and the subsample, 759 of them (63%) have same locations

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

of arrests and detentions, and 944 of them (80%) are located in same prefectures. I aggregate the number of current prisoners in each county-year unit based on the searched locations of arrests. Appendix B.3 reports the results obtained from using this current prisoner measure. The estimates remain statistically significant at 0.01 level.

A.3 Summary Statistics

Table A.1: Summary Statistics, Public Security Dataset

Variable	**Unit**	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
Expenditure - Public Security	10,000 Yuan	36,185	1,641.9	3,453.7	0	139,932
Expenditure - Social Welfare	10,000 Yuan	25,911	1,530.9	4,405.9	0	265,206
Expenditure - Education	10,000 Yuan	36,247	6,161.4	8,297.1	0	331,798
Expenditure - Agriculture	10,000 Yuan	27,426	797.9	1,511.0	0	44,578
Expenditure - Administration	10,000 Yuan	28,376	2,598.0	3,220.1	17	163,536
Expenditure - Total	10,000 Yuan	36,282	26,259.9	47,398.2	162	2,225,040
GDP	10,000 Yuan	26,929	364,974.0	652,030.2	0	29,200,000
Population	10,000 Persons	37,018	45.1	48.1	0	5,022
Urbanization Ratio	0-1	35,722	0.25	0.24	0	1
Golden Shield	(County-year)	37,349	0.10	0.30	0	1
Year	(Year)	37,349	2001	4.0	1994	2007

Table A.2: Summary Statistics, Political Prisoners Dataset

Variable	**Unit**	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
Arrests of Political Activists	Person	34,325	0.132	1.630	0	165
Surveillance Camera System	(County-year)	35,275	0.139	0.346	0	1
Year	(Year)	35,275	2011	3.4	2006	2017
Primary Industry Value Added	10,000 Yuan	20,130	177,816.0	160,460.0	84	1,399,516
Secondary industry Value Added	10,000 Yuan	20,132	624,798.1	1,064,072.0	13	17,000,000
Population	10,000 Persons	20,154	70.0	1,456.1	1	115,453
Land Area	Square km	20,150	4,283.3	9,970.2	56	202,298
Expenditure - Total	10,000 Yuan	20,143	168,598.2	168,685.4	214	3,889,833
Welfare Center	Number	19,510	16.1	24.0	0	1,474
Welfare Bed	Bed	19,483	1,164.3	1,461.1	0	20,790
Hospital Bed	Bed	20,112	1,269.6	1,183.0	10	37,846
Elementary School Students	Person	20,153	35,632.6	33,324.9	482	1,600,272
Middle School Students	Person	20,140	25,888.0	22,265.2	106	224,076
Grain	Ton	18,392	256,490.7	287,939.5	7	3,349,885
Cotton	Ton	8,552	6,948.3	21,132.6	1	390,177
Oil Crop	Ton	19,312	14,653.3	26,095.5	1	381,336

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.

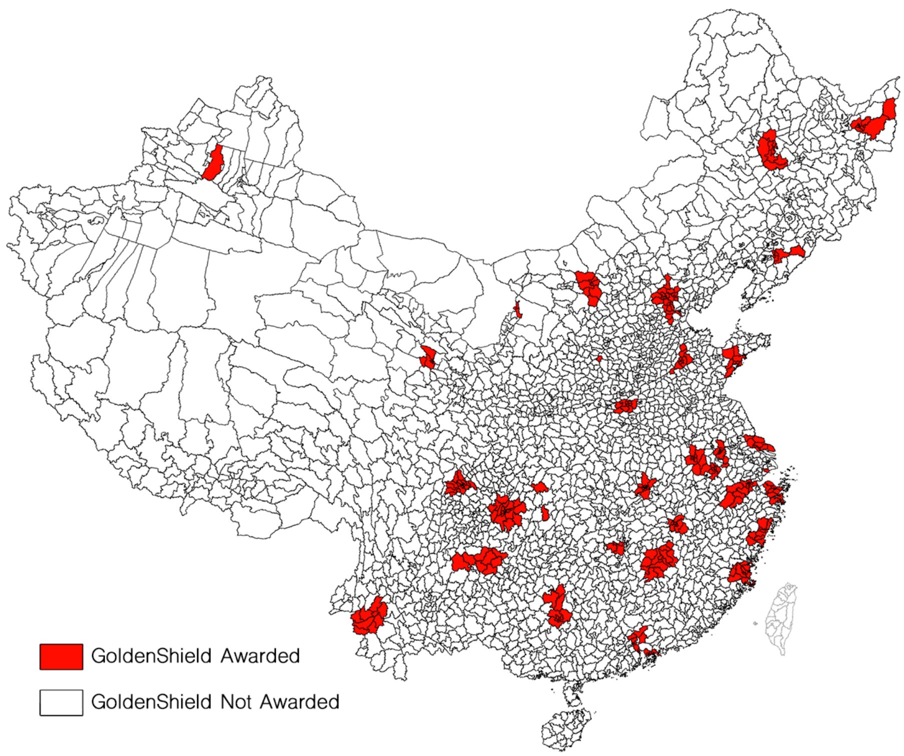

Figure A.1: The Distribution of Golden Shield Award Prefectures
Source: Ministry of Public Security Official Website

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.

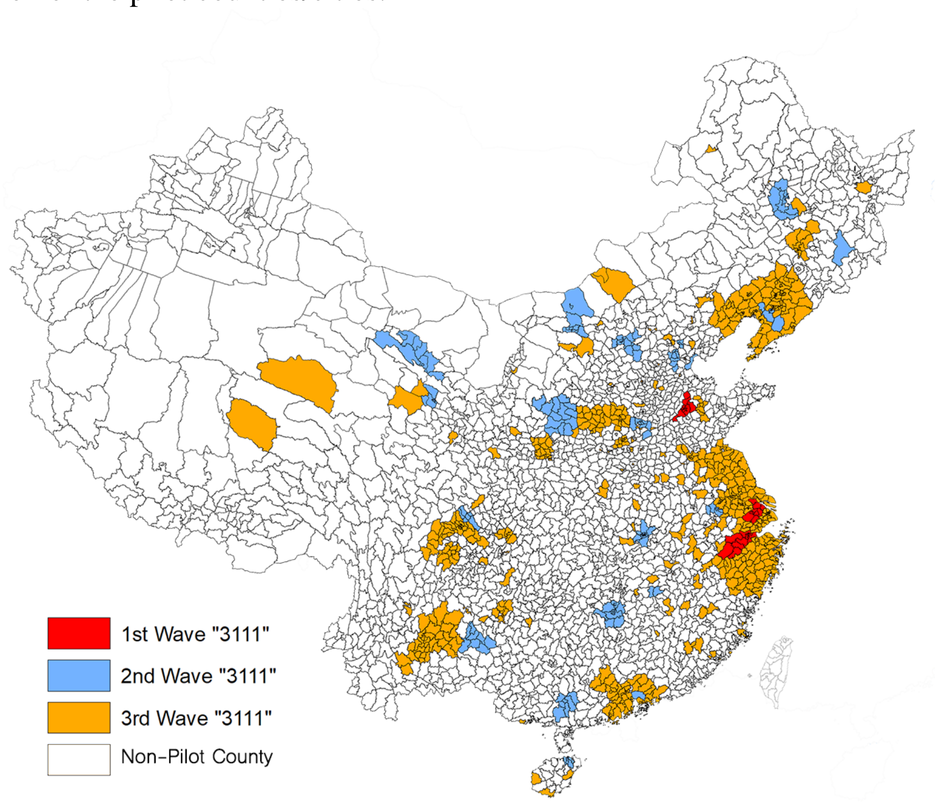

Figure A.2: The Distribution of "3111" Pilot Counties/Cities
(Xinjiang, Tibet, and Beijing Excluded)
Source: Ministry of Public Security No.1 Research Institute

Correlation Matrix for Past Repression and Surveillance Counties 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 Counties	0.0479	0.0471	-	-
3111 Initiative Counties	-	-	0.0165	0.0133

B Additional Empirical Analyses

B.1 Public Security Expenditure Data

I include control variables such as local GDP and local governments' other administrative expenditure. 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. In Table 1 of the main text, the number of observations are different between Fixed Effect models and Lagged DV models because the lags automatically drop observations in the first year. Table B.4 shows that the results remain similar and robust when matching the sample of the Fixed Effect model with the Lagged DV model. Note that I do not restrict all the model specifications on the same sample because the GDP variable is missing for all counties/cities in 1994, 1995, 1996, and 2007, and the administrative expenditure variable is missing for all counties/cities in 2004 and 2005.

Table B.5 reports the results on repression-cooptation comparison using log-transformed variables. 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

Table B.4: 1st-Phase GSP and Public Security Expenditure, Sample Size Matched

	Fixed Eff.	Lagged DV	Fixed Eff.	Lagged DV	Fixed Eff.	Lagged DV
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Security (log)	Security (log)	Security (log)	Security (log)	Security (log)	Security (log)
Golden Shield×Time	0.109***	0.0363**	0.0941***	0.0297**	0.121***	0.0338**
	(0.0283)	(0.0141)	(0.0298)	(0.0122)	(0.0362)	(0.0139)
Time: Post-2005	0.228***		0.0366***		0.170***	
	(0.0235)		(0.0129)		(0.0363)	
Lagged DV		0.754***		0.773***		0.755***
		(0.0118)		(0.0126)		(0.0138)
Exp. (log)	0.608***	0.263***	0.567***	0.237***	0.501***	0.223***
	(0.0393)	(0.0132)	(0.0502)	(0.0146)	(0.0339)	(0.0145)
Pop. (Log)	0.0431	-0.00982*	0.0556	-0.0153***	0.0449	-0.00173
	(0.0302)	(0.00536)	(0.0403)	(0.00550)	(0.0324)	(0.00587)
Urb. Ratio	-0.00644	0.0460***	-0.00703	0.0411***	-0.00121	0.0619***
	(0.0129)	(0.00996)	(0.0161)	(0.00974)	(0.0136)	(0.0118)
GDP (log)			0.00162	0.0112***		
			(0.00590)	(0.00373)		
Admin. Exp. (log)					0.135**	0.0465**
					(0.0590)	(0.0215)
County FEs	Yes	No	Yes	No	Yes	No
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Constant	1.004**	-0.590***	1.365***	-0.741***	0.832	-0.771***
	(0.410)	(0.0751)	(0.500)	(0.0785)	(0.532)	(0.103)
Observations	31,632	31,632	24,827	24,827	24,489	24,489
R-squared	0.877	0.953	0.828	0.948	0.843	0.948
N of County	2,728		2,717		2,728	

Model (1), (3), and (5) use two-way fixed effects models with cluster-bootstrap variance matrix developed by Bertrand, Duflo and Mullainathan (2004). Model (2), (4), and (6) use lagged dependent variable models. Robust standard errors clustered at the prefecture level.
*** p<0.01, ** p<0.05, * p<0.1

(a proxy for telcommunication infrastructures), land area, population, primary industry value increase, secondary industry value increase, and local fiscal expenditure. These controls along with 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 estimate much.

Count Data Models

Due to the panel structure of the prisoner data, I fit linear fixed effects models as main analyses.

Table B.5: 1st-Phase GSP, Public Security, and Redistribution Expenditures (Log Transformation)

	Fixed Eff.	Fixed Eff.	Fixed Eff.	Fixed Eff.
	(1)	(2)	(3)	(4)
VARIABLES	Security (log)	Welfare (log)	Education (log)	Agriculture (log)
Golden Shield×Time	0.181***	-0.131**	0.0194	-0.162***
	(0.0226)	(0.0649)	(0.0166)	(0.0496)
Time: 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

SUR Models. Robust standard errors clustered at the prefecture level.
*** p<0.01, ** p<0.05, * p<0.1

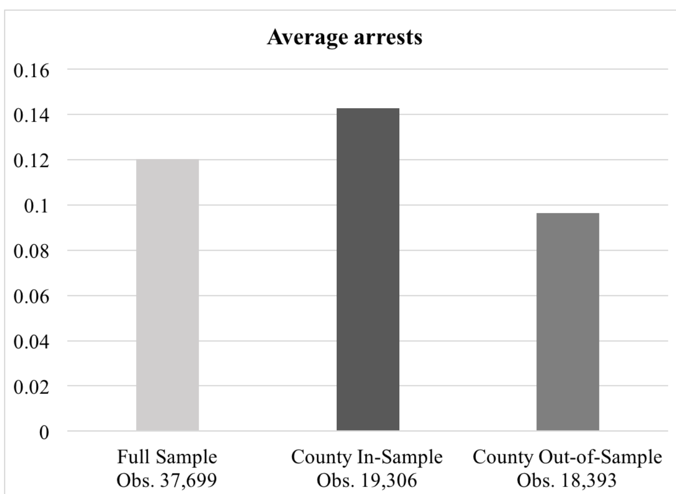

Figure B.3: Average Arrests in Samples With and Without Controls

Since the outcome variable is a count of political arrests with discrete and non-negative values, I 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 remain substantial and statistically significant. Moreover, I use Zero-inflated Negative Binomial (ZINB)

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

	Fixed Eff.	Lagged DV	Fixed Eff.	Lagged DV
	(1)	(2)	(3)	(4)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners
3111 Pilot Counties	0.0687**	0.0555**	0.0703**	0.0572**
	(0.0268)	(0.0247)	(0.0278)	(0.0245)
Lagged Prisoners		0.131***		0.131***
		(0.0107)		(0.0107)
Tel User (Log)			0.0198	-0.00518
			(0.0376)	(0.0224)
Land Area (Log)	0.0612	0.0571***	0.0693	0.0587***
	(0.0877)	(0.0182)	(0.0937)	(0.0172)
Population (Log)	0.0987	-0.160***	0.0786	-0.153***
	(0.344)	(0.0517)	(0.362)	(0.0540)
1st Industry (Log)	0.0256	0.0308	0.00195	0.0306
	(0.0656)	(0.0247)	(0.0719)	(0.0244)
2nd Industry (Log)	-0.114*	-0.163**	-0.106*	-0.160**
	(0.0671)	(0.0659)	(0.0638)	(0.0664)
Fiscal Income (Log)	0.0415	0.282***	0.0279	0.277**
	(0.0781)	(0.108)	(0.0799)	(0.109)
County Fixed Effect	Yes	No	Yes	No
Year Fixed Effect	Yes	Yes	Yes	Yes
Constant	-0.195	-1.285***	0.720	-1.233***
	(1.265)	(0.469)	(1.566)	(0.465)
Observations	17,918	16,172	17,761	16,023
R-squared	0.002	0.031	0.002	0.031
Number of County	1,847		1,844	

Robust standard errors clustered at the county level. Samples exclude counties in Beijing, Tibet, and Xinjiang.
*** p<0.01, ** p<0.05, * p<0.1

models for the lagged DV estimation to account for the excessive zeros in the political arrests variable. The results remain large and statistically significant. Surveillance increases political arrests by 1.5 to 1.7 more persons (Table B.7). Given that the average number of prisoners in counties with non-zero arrests is 3.1, 1.5 - 1.7 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. Thus, excessive zeros in the dependent variable is not of big concern.

"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. Note that log transformation in Table 4 in the main text is preferable to using

Table B.7: "3111" Initiative (2nd-Phase GSP) and Political Prisoners, Count Data Models

	Fixed Eff. Neg. Bino.	Neg. Bino. Lag-DV	ZINB Lag-DV
	(1)	(2)	(3)
VARIABLES	Prisoners	Prisoners	Prisoners
3111 Pilot Counties	0.415***	0.531***	0.377***
	(0.111)	(0.118)	(0.146)
Lagged Prisoners		0.652***	0.119***
		(0.107)	(0.0261)
County Fixed Effect	Yes	No	No
Year Fixed Effect	Yes	Yes	Yes
Constant	-3.116***	-2.744***	-0.717***
	(0.151)	(0.145)	(0.191)
Observations	9,744	31,460	31,460
Number of County	812		

Robust standard errors are clustered at the county level.
*** p<0.01, ** p<0.05, * p<0.1

the original unit due to data skewness and large outliers.

Table B.8: "3111" Initiative (2nd-Phase GSP) and Public Goods Provision, Original Units

	Welfare			Education		Agriculture		
	Fixed E.	Fixed E.	Fixed E.	Fixed E.	Fixed E.	Fixed E.	Fixed E.	Fixed E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Welfare Centers	Welfare Beds	Hospital Beds	Primary Stdts	Middle Stdts	Grain Prodct	Cotton Prodct	Oil Crop Prodct
3111 Counties	-0.103	61.95	-14.74	1,276***	323.4	-5,033*	-1,189**	-1,445***
	(0.537)	(47.16)	(25.54)	(459.2)	(365.1)	(2,885)	(522.9)	(336.3)
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	13.13	357.7	53.97	38,808	29,024	201,328	-28,977	9,215
	(2.536)	(124.3)	(226.4)	(2,272)	(2,034)	(5,932)	(22,386)	(1,395)
Observations	19,477	19,450	20,068	20,109	20,101	18,362	8,544	19,277
R-squared	0.020	0.273	0.572	0.047	0.314	0.153	0.093	0.047
N of admcode	2,081	2,081	2,086	2,086	2,086	2,066	1,093	2,041

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

B.3 Currently Detained Prisoners with Arrest Information Searched Online

As mentioned in Section A.2, 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 currently detain 0.04 more activists than counties without surveillance. The average number of current prisoners per county is about 0.038 persons, so the number of arrests is more than doubled. Column (2) reports the result of the lagged DV model, which is also large and statistically significant. Excluding counties from Beijing, Tibet, and Xinjiang does not change the statistically significance.

Table B.9: "3111" Initiative (2nd-Phase GSP) and Political Prisoners, In-prison Sample

	All Provinces		Tibet, Xinjiang & Beijing Excluded	
	Fixed Eff.	Lagged DV	Fixed Eff.	Lagged DV
	(1)	(2)	(3)	(4)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners
3111 Pilot Counties	0.0398***	0.0412***	0.0330***	0.0198**
	(0.0136)	(0.0115)	(0.00914)	(0.00881)
Lagged Prisoners		0.151***		0.0868
		(0.0474)		(0.0534)
County Fixed Effect	Yes	No	Yes	No
Year Fixed Effect	Yes	Yes	Yes	Yes
Constant	0.00165*	0.000837	0.000836	0.000684
	(0.000870)	(0.000622)	(0.000550)	(0.000533)
Observations	34,326	31,460	32,031	29,359
R-squared	0.007	0.028	0.007	0.013
Number of County	2,866		2,672	

Robust standard errors clustered at the county level. The lagged DV model has slightly larger coefficient than the fixed-effect model (Column [1]). This is likely due to the fact that currently detained prisoners only constitute a small subsample of total prisoners, so that outlier repressive counties may have a greater influence on the coefficient. Column (3) and (4) show that the fixed effect model has a larger coefficient than the lagged DV model when excluding repressive counties.
*** 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 (Hazlett and Xu 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***	0.01983	328	2734

Yearly Average Treatment Effect on the Treated:

Year	ATT	S.E.	N.Treated	N.Control
2007	0.083***	0.024	164	1367
2006	0.049***	0.020	164	1367
2005	0.008	0.012	164	1367
2004	0.003	0.012	164	1367
2003	0.018	0.013	164	1367
2002	-0.007	0.013	164	1367
2001	-0.018	0.013	164	1367
2000	0.002	0.013	164	1367
1999	0.032	0.013	164	1367
1998	0.015	0.012	164	1367
1997	0.005	0.011	164	1367
1996	-0.005	0.012	164	1367
1995	-0.001	0.013	164	1367
1994	0.010	0.011	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 by Panel Matching Approach

Mean Balance of Security Expenditure

Time to Treatment	-1 year	0 year
Log(Lagged Security Spending) (SD)	0.132	0.183
Log(Population) (SD)	-0.048	0.0452
Log(Budget Expenditure) (SD)	0.103	0.115
Log(Urbanization Ratio) (SD)	0.006	0.014

Table B.12 presents the matching results. Comparing with counties without completed GS system, counties with completed GS systems increase public security expenditure by 4.2 percent in 2006 and 6.4 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)	0.042	0.064
Standard Error(s)	0.016	0.039
Lower Limit of 95 % Regular Confidence Interval	0.010	-0.018
Upper Limit of 95 % Regular Confidence Interval	0.071	0.133

Weighted Difference-in-Differences with Mahalanobis Distance. 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.

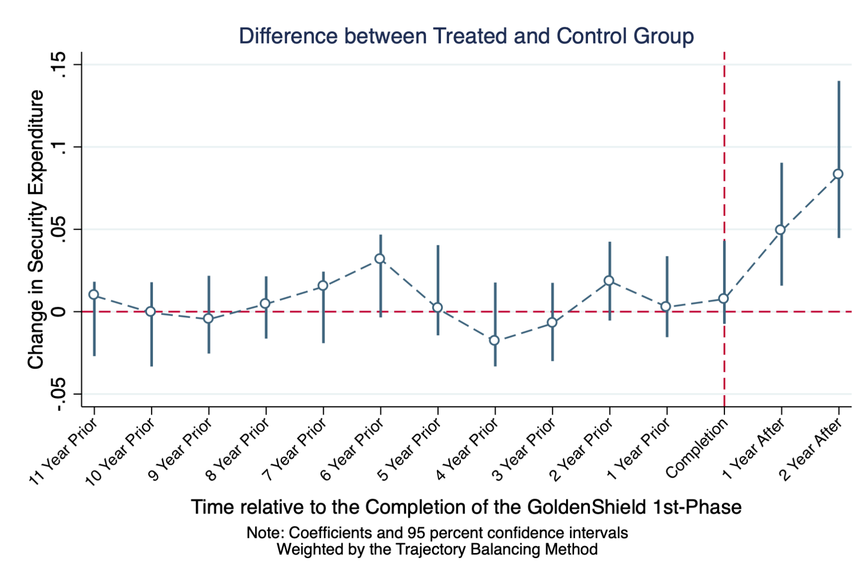

Figure B.4: Pre- and Post-GS Estimates, Public Security Expenditure, DiD, 1994-2007

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

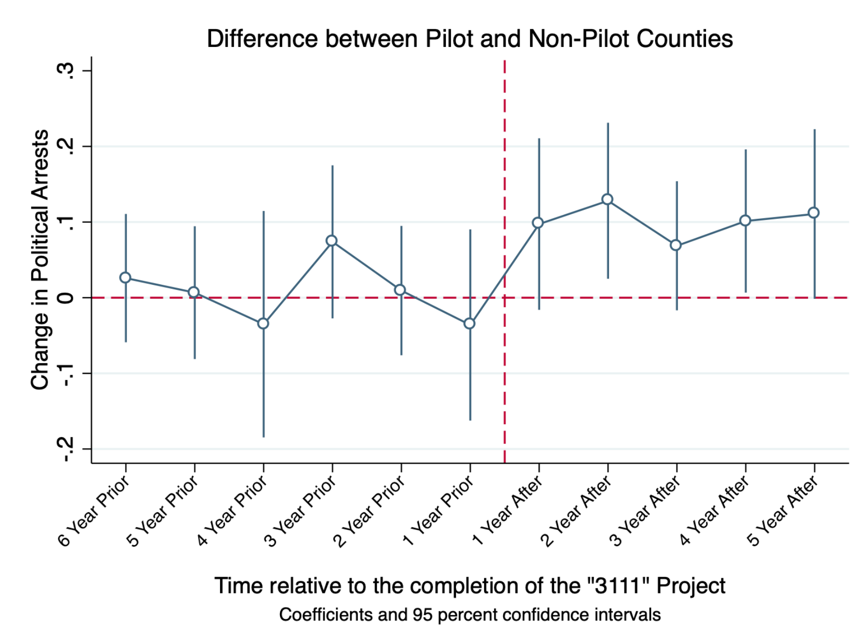

Figure B.5: Pre- and Post-3111 Estimates, Political Prisoners, DiD specification, 2006-2017

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.[3] Had the Internet or social

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

Table B.13: "3111" Initiative (2nd-Phase GSP) and Political Prisoners, No Internet-Related Arrests

	All Provinces		Tibet, Xinjiang & Beijing Excluded	
	Fixed Eff.	Lagged DV	Fixed Eff.	Lagged DV
	(1)	(2)	(3)	(4)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners
3111 Pilot Counties	0.0833***	0.0627***	0.101***	0.0305**
	(0.0273)	(0.0185)	(0.0238)	(0.0127)
Lagged Prisoners		0.150***		0.140***
		(0.0107)		(0.0117)
County Fixed Effect	Yes	No	Yes	No
Year Fixed Effect	Yes	Yes	Yes	Yes
Constant	0.0354***	0.0629***	0.0319***	0.0634***
	(0.00682)	(0.00992)	(0.00635)	(0.0104)
Observations	34,324	31,460	32,030	29,359
R-squared	0.002	0.023	0.002	0.020
Number of County	2,864		2,671	

Robust standard errors clustered at the county level.
*** 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 each sample. 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 (315 versus 2,553). 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 (Column [1] - [4]), 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 (2nd-Phase GSP) and Political Prisoners
(By Treated, Non-treated Counties in the 1st Phase, and GuangDong Province)

	1st Phase GS Counties Excluded		1st Phase GS Counties Only		Guangdong Province	
	Fixed Eff.	Lagged DV	Fixed Eff.	Lagged DV	Fixed Eff.	Lagged DV
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners	Prisoners
3111 Pilot Counties	0.0735**	0.0385*	0.106**	0.206***	0.267**	0.292***
	(0.0323)	(0.0209)	(0.0485)	(0.0465)	(0.107)	(0.103)
Lagged Prisoners		0.153***		0.252***		0.132**
		(0.0109)		(0.0622)		(0.0547)
County Fixed	Yes	No	Yes	No	Yes	No
Year Fixed	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0364***	0.0600***	0.0519***	0.0992***	0.131**	0.0564*
	(0.00759)	(0.0104)	(0.0194)	(0.0345)	(0.0652)	(0.0305)
Observations	30,581	28,028	3,747	3,432	1,464	1,342
R-squared	0.002	0.024	0.020	0.087	0.024	0.046
N of County	2,553		315		122	

Robust standard errors clustered at the county level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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. Column (5) and (6) in Table B.14 show that 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 are less reporting biases.

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