Supplementary Materials for:
The Politics of Police Data: State Legislative Capacity and the Transparency of State and Substate Agencies

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A Investigatory and Coercive Powers

Central to a legislature's ability to gather information is its ability to compel testimony.[1] For example, in 2015, the California Assembly's Committee on Public Safety held open hearings under the title "Law Enforcement and Community Trust in California" (CPS 2015). These hearings were held largely in response to public complaints over widely publicized incidents of police violence, and the poor quality of public information on the prevalence and distribution of these incidents. Ultimately, fourth-fifths of the docket were directed toward data, transparency, and internal scrutiny. In these hearings, the committee solicited testimony from agency representatives at the state (Bureau of Criminal Information and Analysis, California Department of Justice), county (Alameda and Stanislaus County Sheriff's Departments and the Los Angeles County District Attorney's Office), and municipal (Los Angeles, Richmond, and San Jose Police Departments) levels. Likewise, in 2014, the Judicial Committee of the Connecticut General Assembly summoned the Cromwell Chief of Police (who also served as representative of the Connecticut Police Chiefs Association) to offer testimony on public observance and recording (film and video) of officers during the performance of their duties (CGA 2014).

Just as a legislature may compel testimony, it may also subpoena documents, including records of expenditures and assets, formal and informal procedures, internal communications, etc. This type of information can be critical to oversight, and, fortunately, the powers of legislatures to produce this information are expansive. For example, the standing rules of the Illinois House of Representatives state that the house speaker and committee chairpersons "may administer oaths and may compel, by subpoena, any person to appear and give testimony as a witness before the standing committee and produce papers, documents, and other materials" (Illinois General Assembly 2020, 20-21). Importantly, legislatures have the power of contempt at their disposal to enforce their subpoenas. The Florida House, for example, may fine those not complying with a subpoena $1,000 per day and jail them as long as the House is in session (Koh 2018). In another example, while the Illinois

1. Of course, legislatures may also compel testimony with the intent of grandstanding or embarrassing agency representatives.

House cannot issue fines, it has the the power to *indefinitely* jail those who fail to comply with subpoenas. These are well-designed coercive instruments and, as such, the threat of subpoena is typically sufficient for compliance with the legislature's demands.

In the context of policing, however, there are numerous examples of state legislatures issuing subpoenas to law enforcement agencies at various levels of government, demonstrating that the threat of subpoena is very real. In 2018, the Maryland General Assembly formed a special committee to investigate the Baltimore (city) Police Department's gun-tracing task force that used its subpoena power extensively (Fenton 2020). That same year, the Florida House issued a series of subpoenas to the Broward County and Palm Beach County Sheriff's Offices in its investigation of police conduct during the Parkland shooting (Koh 2018), and the Nebraska Legislature subpoenaed the head of its State Department of Correctional Services to deliver extensive information and testimony regarding its lethal injection protocol (Duggan 2018). A particularly interesting legislative investigation played out in 2014 regarding New Jersey Governor Chris Christie's so-called "bridgegate" scandal. Here, the New Jersey Legislature issued subpoenas to not only the New Jersey State Police, but also the Port Authority of New York and New Jersey, an interstate executive agency with its own police force. In sum, though threat of subpoena is often sufficiently coercive, state legislatures do regularly serve subpoenas to municipal, county, state, and even interstate law enforcement agencies. Indeed, state legislative subpoena powers even extend to state judiciary (DiPippa 2016) and federal officials (Vitiello 1983). Even in the case where local, institutionalized oversight exists, as in the case of civilian review boards, these institutions often lack resources, expertise, or *de jure* sanctioning capacity, such that they must ask the state legislature to step in to conduct its own audit, as was recently the case in San Diego County (McDonald and Davis 2021).

The legislature may also commission investigation and reporting from internal research services to collect, digest, and analyze relevant information. For example, the Texas House of Representatives used its House Research Organization to compile information on asset seizure by state and local police agencies when considering changes to the status quo policy (Dworaczyk 2018). Similarly, the Michigan House of Representatives asked its internal research group, called the House Fiscal

Agency, to research the transit police employed by the Detroit Transit Corporation (the public corporation that operates the Detroit People Mover) and summarize relevant information regarding their training, core competencies, and performance, which informed the Judiciary Committee's deliberation on a proposal to subject these officers to state mandated law enforcement training and standards (Smith, Hamilton, and Coffin 2020).

When internal research services are insufficient, legislatures may also farm investigation or analysis out to external experts. While there are some legislatures with the convention of forming special committees with external experts sitting on them for this purpose—for example, the Massachusetts General Court established a commission to study and recommend police "standards and training" including representatives of various state and municipal police agencies in addition to the FBI (Holmes 2020)—most often, reports are just commissioned directly from consulting firms, think tanks, academics, etc.

On occasion, these investigations reveals shortcomings that must be resolved legislatively in order to provide agencies with the resources necessary to faithfully execute policy, close statutory loopholes that allow intransigent agencies to resist the legislature's demands, or otherwise constrain future behavior. For example, the Michigan House's "sexual assault kit evidence submission act"—which established and appropriated sufficient operational funding for a central body to process rape kits for all Michigan police agencies—was passed after it was revealed that the Detroit Police Department had a backlog of over 11,000 unprocessed rape kits (Walsh 2014). A New York Assembly proposal to indefinitely suspend without pay New York City Police officers accused of violent misconduct until the charge is adjudicated was submitted following the revelation that particular officers racked up multiple accusations of abuse while misconduct proceedings were already in progress (Perry 2019). In another case, after investigation revealed pervasive shortcomings in police agencies' record-keeping of misconduct accusations, a Maryland General Assembly proposal to establish a database detailing all allegations of violent misconduct filed against all police agencies in the state was initiated (Benson 2015). Should agencies remain recalcitrant after investigation reveals their misbehavior and the relevant loopholes have been closed or resources have been provided, the legislature may employ its

ultimate coercive power and threaten to slash budgets. Importantly, all of this legislative activity requires legislative capacity—time and resources for legislative work.

Rather than rely on anecdotes alone, we briefly examine the relationship between capacity and state legislative responsiveness to police and policing with a bit more quantitative rigor to provide some prime facie evidence for our argument. The killing of George Floyd by a Minneapolis Police officer on May 25, 2020 sparked a series of nationwide protests in multiple cities in *every* state in the country demanding police reform (Burch et al. 2020).[2] The protests were very effective in swaying public opinion and broadening the base of support for police reform (Cohen and Quealy 2020) and, more importantly, prompted real action in the policymaking process. In the month following Floyd's killing (May 25 to June 24), 19 state legislatures proposed at least one bill meant to reform policing and several states saw dozens of legislative proposals. We expect this flurry of legislative activity to be strongly correlated with legislative capacity—without time and resources for information gathering and bill-authoring, legislators cannot efficiently respond to shocks in the political environment that require action. To assess this relationship, we compare a state's legislative capacity to the number of legislative proposals involving policing submitted in that state's legislature in the month following Floyd's death: May 25 through June 24.

The proposal data were gathered by the National Conference of State Legislatures (2020)[3] and we use the adaptation of the Squire index (Squire 2017) described in the article main text as our measure of legislative capacity. The Squire index is the "industry standard" measure of capacity, an index of the institutional endowments discussed in the main text: legislator compensation, length of legislative sessions, and legislative staff. We gather the data on legislator compensation, session length, and staff expenditures and scale the values into a summary measure using the factor analytic model proposed by Quinn (2004).

Table A.1 displays the results of 4 models regressing a state's legislative capacity on the number

2. This is not an exaggeration. The New York Times documented, for example, protests in 16 cities in Alaska and 11 cities in Wyoming in addition to scores of cities in more populous states like California and Texas (Burch et al. 2020).

3. There were a handful of false positives in the database that were removed prior to analysis.

Table A.1: Effect of legislative capacity on legislative proposals on policing in the month following the death of George Floyd

	Whole counts (NB)		Log counts (linear)	
	All	Omit MN	All	Omit MN
	(1)	(2)	(3)	(4)
Capacity	1.438	1.441	0.696	0.684
	(0.560)	(0.578)	(0.241)	(0.216)
Republican legislature	−1.617	−0.825	−0.521	−0.318
	(1.035)	(1.002)	(0.441)	(0.399)
Republican governor	−1.241	−0.437	−0.043	0.153
	(1.317)	(1.271)	(0.546)	(0.492)
Republican legislature × Republican governor	1.882	1.096	−0.005	−0.206
	(1.641)	(1.578)	(0.675)	(0.607)
Constant	2.259	1.474	1.346	1.140
	(0.578)	(0.577)	(0.257)	(0.237)
Observations	50	49	50	49
$log(likelihood)$	−88.548	−82.271		
R^2			0.415	0.254

of proposals on policing initiated in that 30-day period, controlling for the partisan control of the legislature and governorship. The first two models (1 and 2) use the counts of the legislative proposals as the outcome, and are estimated via negative binomial regression. The second two models (3 and 4) instead use the log of the proposal counts (plus 1) as the outcome, and are estimated using ordinary least squares. Each of the models reveal a positive and statistically significant correlation between legislative capacity and the submission of police reform legislation. Indeed, the data suggest that capacity is a substantially better predictor of legislative action than partisan control of government.[4] That said, this analysis is merely suggestive (as intended), as researchers interested in identifying the precise causal effect of George Floyd's killing on subsequent legislative action would, at minimum, need a more complete model specification than offered here. However, even our pared down analyses indicate support for the relationship of interest, as we find a strong, positive correlation between legislative capacity and responsiveness to a pervasive call for intercession into policing.

4. These results are robust to the inclusion (or omission) of Minnesota, the state where George Floyd lived and was killed, in which largest number of proposals (76) was submitted.

B Compliance Study

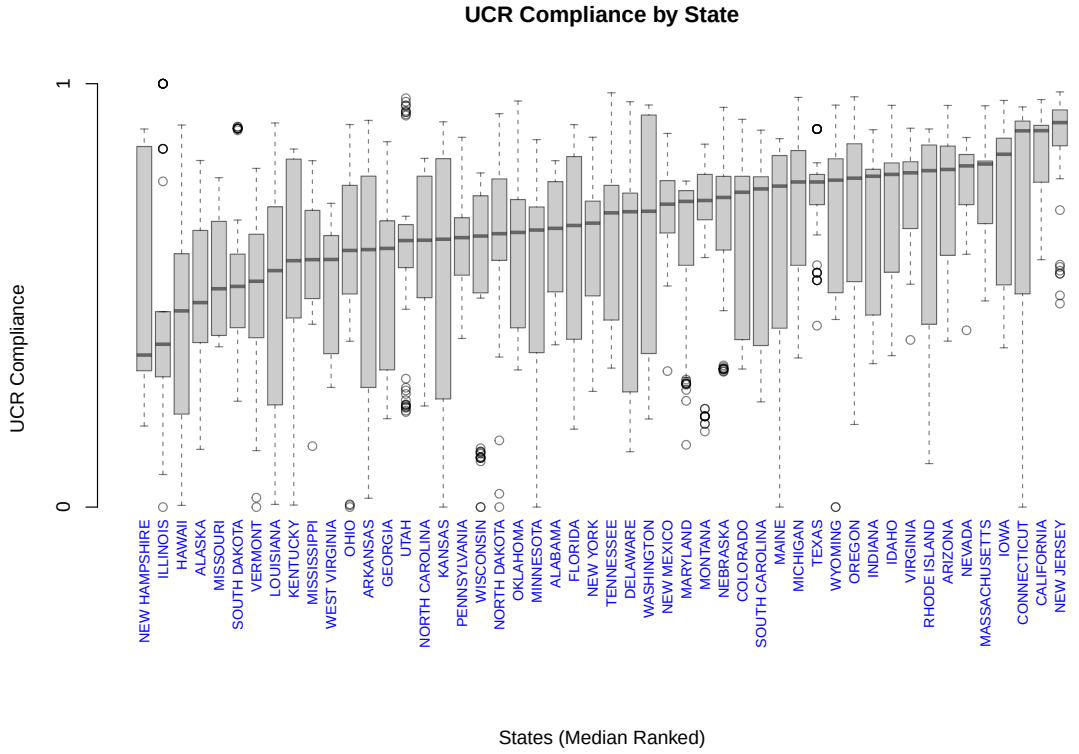
B.1 Outcome variable

Data for the compliance study were obtained from the Department of Justice through the FBI's Crime Stats group. While the more recent data (1995-2017) were available on the FBI's website (compiled, clean, and including some other useful information about the agencies, like the number of officers employed, etc.), the older data (1960-1994) had to be requested in writing and obtained in physical media. Those data included all UCR information sent by compliers, in unusually formatted or unformatted text, but no information regarding non-compliers. We also obtained from the FBI an accounting of all police agencies in the United States, including some relevant information about them, for all years in our sample periods. These data include each agency's Originating Agency Identification (ORI), which is also included in the UCR information we were sent for the older sample period. This allowed us to write a program to query each year of the UCR data for each of the live ORI's in that year to produce the dependent variable (i.e., compliance) for our analysis.

Unfortunately, at least one of the original data files was corrupted or incomplete. The 1977 file contains only 10% of the information as the 1976 file (comparing total file sizes). Likewise, parsing the data shows positive returns for only about 3% of agencies in 1977, but 39% in 1976. This was the only clearly outlying year, and dropping this year from the analysis does not substantively change the results, but it does demonstrate that the older sample is not as reliable as the newer sample, which was complied from the original records by the DOJ in the post-Paper Reduction Act period. To be clear, just as we stated in the main text, we are less confident in the accuracy of the older sample than we are in the accuracy of the newer sample. That said, the analysis below shows a good deal agreement in results between the two samples.

We plot the distribution of average annual UCR compliance for all but 1977 (the tainted year) across all states in Figure A1 below.

Figure A1: Distribution of Outcome Variable Across States

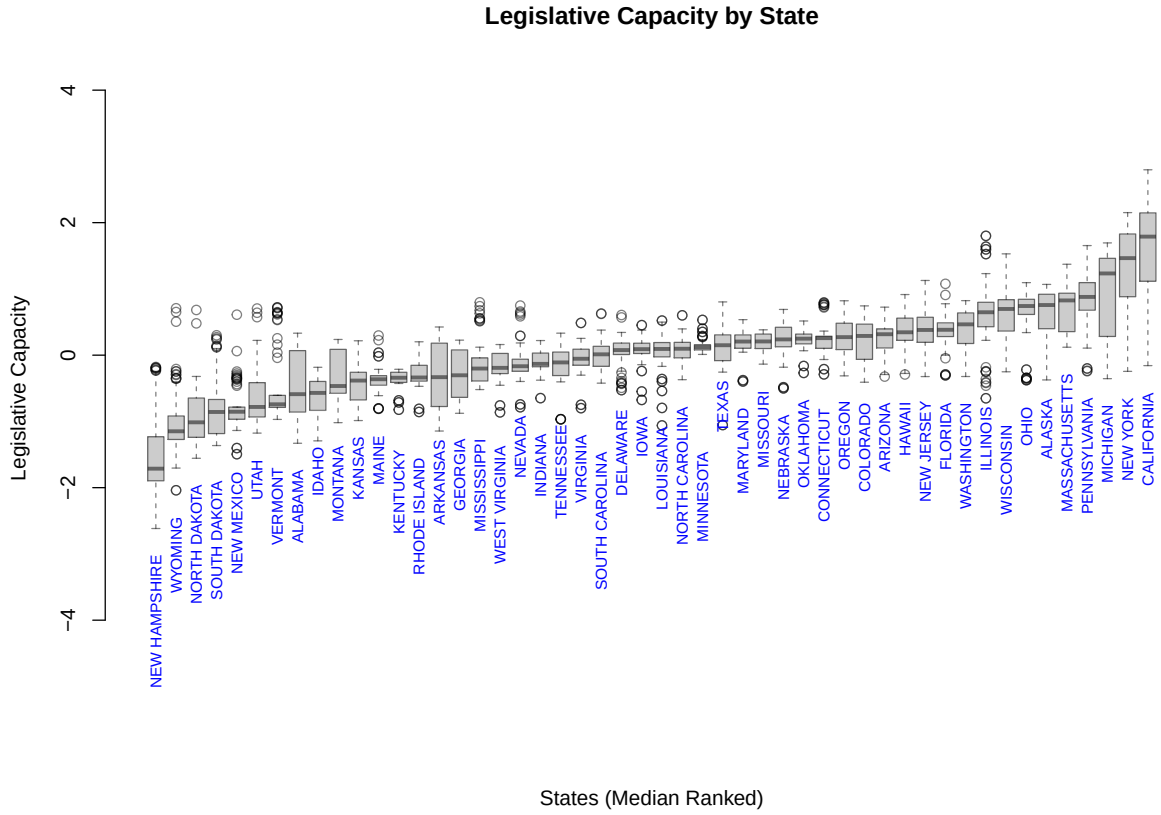


B.2 Treatment variables

We use two variables to capture legislative capacity: a version of the Squire Index and the imposition of term limits. Because the original Squire Index is measured intermittently and always relative to Congress, it is not appropriate for dynamic analyses. To construct a more appropriate measure, we use information on state legislatures' session days, staff budgets, and member salaries (gathered and standardized to 2010 dollars by Bowen and Greene (2014)), and scale the components together using a factor analytic model developed for mixed data by Quinn (2004). Because the dollar amounts are inflation adjusted and we scale all the information together, rather than arbitrarily tying them to some benchmark, the estimates are appropriate for comparison over time and space. We summarize

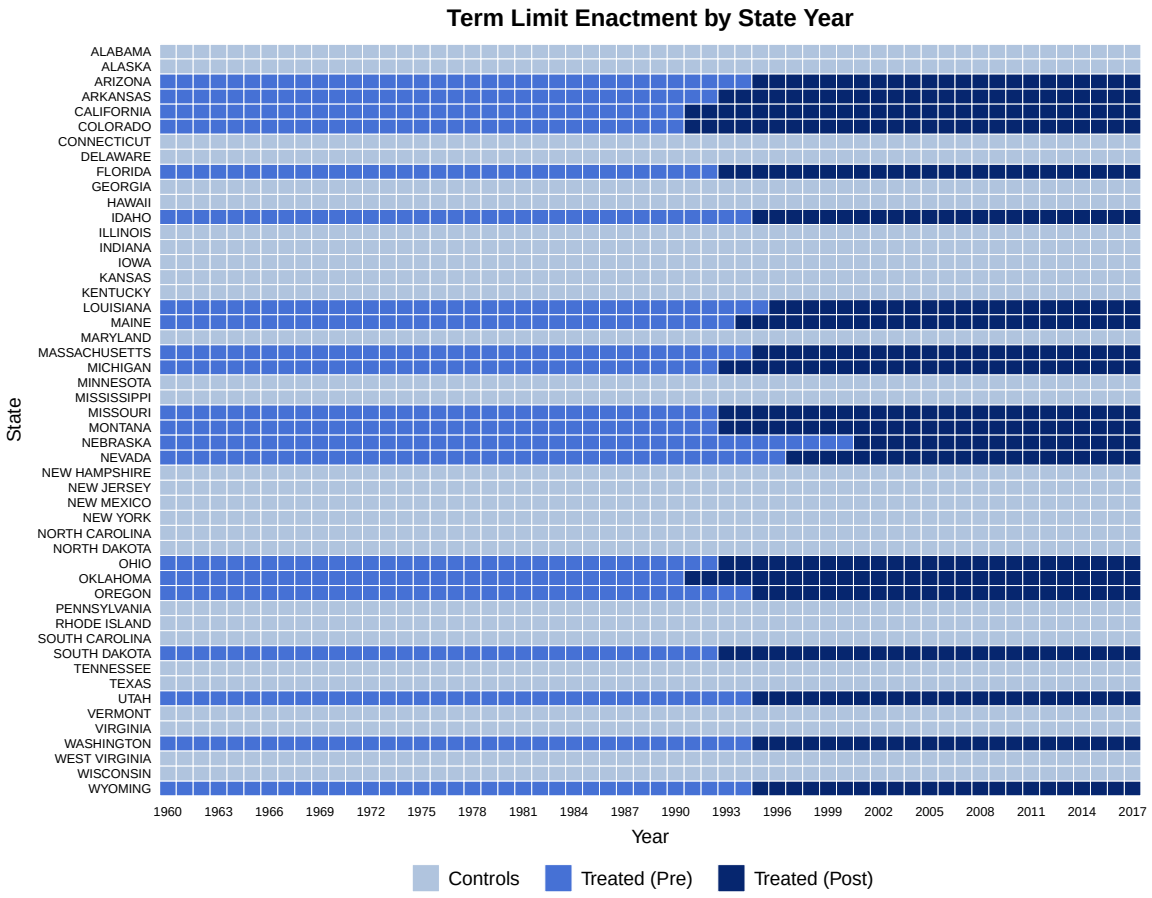
these values by state in Figure A2 below.

Figure A2: Distribution of Legislative Capacity Across States



We gathered information on term limit enactment from the National Conference of State Legislatures. The treatment status of all states is given in Figure A3 below.

Figure A3: Term Limit Imposition Across States



B.3 Descriptive statistics

Table A.2 displays descriptive statistics for all covariates used in our UCR compliance analysis. Note that employee counts and population enter the models after being rescaled to standard normal.

Table A.2: Descriptive statistics for UCR compliance study

	1960 - 1994		1995 - 2017	
Variable	Mean	SD	Mean	SD
UCR comply	0.565	0.496	0.697	0.460
Squire capacity	0.212	0.755	0.319	0.819
VAM constraint	0.009	0.014	0.012	0.014
Elected head	0.163	0.370	0.164	0.370
Term limits	0.019	0.136	0.369	0.482
Democratic legislature	0.613	0.487	0.319	0.466
Republican legislature	0.185	0.389	0.455	0.498
Democratic governor	0.576	0.494	0.418	0.493
Republican governor	0.422	0.494	0.574	0.495
County agency	0.164	0.371	0.165	0.371
State agency	0.100	0.299	0.100	0.299
Agency employees	21.060	333.092	50.069	475.727
Population	12152.360	82756.920	15936.650	88730.970
Percentage Black population	0.111	0.083	0.118	0.081

B.4 Missingness

Several values of agency-service population and agency employee counts are missing in the raw data. Further, state legislative capacity information is unavailable for years prior to 1974. We impute these missing values using the software developed by Honaker, King, Blackwell, et al. 2011. We do not compute Rubin's errors for the models presented in the main text, however, we simply use one imputed data frame and report the standard errors directly from the model estimation. We choose the simpler route because there is effectively zero variance in the parameter and standard error estimates on our key covariates; no non-negligible differences. We note here that interpolating the missing values, rather than imputing, does not substantively change the results. Our posted replication materials contain both the raw and imputed data and discuss the imputation.

B.5 Main Text Table 1 Discussion

The analyses we present here are given for two key reasons. First, we simply provide the results of the Main Text Table models with all control variables included. Those can bee seen in Table A.3.

Next, we want to show that our results are robust to alternative measures of capacity. Here, we use the level of executive constraint estimated by Vannoni, Ash, and Morelli (2020). This is not capacity *per se*, rather an estimate of the degree to which statutory language produced by legislatures in passed laws constrains executive action. Capacity itself is an antecedent factor to this value. We also want to show that our results hold when analyzing state, county, and municipal-level agencies separately, or, when allowing the effect of capacity to vary by party control.

We replicate the main text analysis using an alternative measure of oversight capacity, the level of executive constraint estimated by Vannoni, Ash, and Morelli (2020). The measure is derived from a text analysis of state-level legislation that estimates the level of constraint the legislative language places on the executive in policy implementation. As noted, this is not legislative capacity *per se*, but an estimate of ex-ante constraint. As such it does not capture the entirety of our conceptual argument, because, of course, ex-ante constraint and ex-post oversight may be additive *or* complementary—it may be the case that legislatures write more constrictive bills when they lack

the resources or will for ex-post oversight. Indeed, regressing capacity on the constraint variable reveals that the two measures are uncorrelated. This can be seen in Table A.4. Nonetheless, the executive constraint measure does provide us an estimate of a kind of agency management (even it only captures half of the legislative toolbox) and as such is worth evaluating.

Table A.5 replicates the main text models using the constraint measure and displays them side-by-side with estimates from models using the Squire-derived measure that we employ in the main text. The core result holds with the alternate measure: when legislatures allow agencies less wiggle-room in implementation, agencies are more likely to comply with UCR requests. There is an oddity, however, in the interaction terms. Specifically, in the latter period, the effect of the measure is insignificantly exacerbated (rather than significantly ameliorated) by elections. Though the total effect of the variable is as expected, this positive interactive effect is the opposite of what we would expect from a proper measure of capacity. As we discuss above, however, this is *not* a measure of capacity, but rather a measure of ex-ante constraint, so, while we believe this should predict compliance in general, it is unclear from our capacity-focused argument how the constraint effects should be moderated in the presence of elected agency heads.

Next, we analyze the three different agency levels separately to demonstrate that the results from the pooled models are not driven by, for example, only state-level agencies. These results are given in Table A.6, which displays each of the three levels for both time periods, including control variables.

Finally, we allow for the effects of capacity to be moderated by party control of the legislature. The results in Table A.7 show that there are significant differences in the capacity under different partisan control regimes. These differences, however, are unstable across samples, with capacity having a greater effect under divided control (the baseline category) in the early sample, but a lesser effect in the late sample. Most importantly, the capacity effect remains large, significant, and impactful under all partisan regimes. The story with term limits is largely similar, however, in the earlier sample (and only in the earlier sample), it appears that the total effect of term limit imposition can be positive under Republican majorities. This is likely an artifact of the small

amount of variation in term limits in this earlier sample: only Colorado enters term limit treatment under a Republican Legislature in the early sample.

Table A.3: Main text Table 1 models with all control variables.

	1960-1994			1995-2017		
Capacity	0.013***		0.036***	0.047***		0.050***
	(0.001)		(0.001)	(0.003)		(0.003)
Term limits		−0.070***	−0.082***		−0.170***	−0.172***
		(0.005)	(0.005)		(0.010)	(0.011)
Election			0.419***			0.444***
			(0.017)			(0.024)
Capacity × election			−0.036***			−0.021***
			(0.002)			(0.002)
Term limits × election			0.001			0.089***
			(0.011)			(0.004)
Democratic legislature			0.045***			−0.058***
			(0.007)			(0.008)
Republican legislature			0.047***			−0.136***
			(0.011)			(0.008)
Democratic governor			−0.011			−0.053***
			(0.012)			(0.008)
Republican governor			−0.026**			−0.048***
			(0.012)			(0.008)
County agency			−0.012			−0.276***
			(0.017)			(0.024)
State agency			−0.219***			−0.174***
			(0.002)			(0.002)
Sheriff's office in constitution			−0.186***			−0.064***
			(0.003)			(0.004)
Total agency employees			−0.030***			−0.049***
			(0.001)			(0.001)
Population			0.058***			0.071***
			(0.001)			(0.001)
Percentage Black population			−1.116***			0.264
			(0.064)			(0.204)
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Party × year FE			✓			✓
Observations	668,325	668,325	668,325	452,226	452,226	452,226
R^2	0.210	0.211	0.283	0.115	0.115	0.158

Table A.4: Relationship between legislative capacity and executive constraint

	1960 - 1994	1995 - 2017
Squire Capacity	−0.0002	0.0002
	(0.001)	(0.001)
Constant	0.012***	0.008***
	(0.0004)	(0.0003)
Observations	1,150	1,750
R^2	0.0001	0.0001
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A.5: Full set of difference-in-differences models of UCR compliance, including alternative capacity measure and control variables. Baseline categories: municipal agencies, divided control legislatures, and independent governors.

	1960 - 1994				1995 - 2017			
Squire capacity	0.013***		0.037***		0.047***		0.050***	
	(0.001)		(0.001)		(0.003)		(0.003)	
VAM constraint		0.238***		0.432***		1.171***		1.252***
		(0.044)		(0.047)		(0.056)		(0.060)
Term limits			−0.081***	−0.075***			−0.172***	−0.200***
			(0.005)	(0.005)			(0.011)	(0.011)
Election			0.422***	0.423***			0.444***	0.439***
			(0.003)	(0.017)			(0.024)	(0.024)
Squire capacity × election			−0.036***				−0.021***	
			(0.002)				(0.002)	
VAM constraint × election				−1.038***				0.191
				(0.094)				(0.121)
Term limits × election			−0.008	−0.012			0.089***	0.084***
			(0.011)	(0.011)			(0.004)	(0.004)
Democratic legislature			0.045***	0.024***			−0.058***	−0.052***
			(0.007)	(0.007)			(0.008)	(0.008)
Republican legislature			0.048***	0.022**			−0.136***	−0.121***
			(0.011)	(0.011)			(0.008)	(0.008)
Democratic governor			−0.011	−0.007			−0.053***	−0.037***
			(0.012)	(0.012)			(0.008)	(0.008)
Republican governor			−0.026**	−0.022*			−0.048***	−0.031***
			(0.012)	(0.012)			(0.008)	(0.008)
County agency				−0.006			−0.276***	−0.274***
				(0.017)			(0.024)	(0.024)
State agency				−0.219***			−0.174***	−0.174***
				(0.002)			(0.002)	(0.002)
Sheriff's office in constitution			−0.174***	−0.191***			−0.064***	−0.064***
			(0.003)	(0.004)			(0.004)	(0.004)
Total agency employees			−0.036***	−0.031***			−0.049***	−0.049***
			(0.001)	(0.001)			(0.001)	(0.001)
Population			0.066***	0.060***			0.071***	0.071***
			(0.001)	(0.001)			(0.001)	(0.001)
Percentage Black population			−1.123***	−0.616***			0.264	0.126
			(0.065)	(0.062)			(0.204)	(0.203)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Party×year FE			✓	✓			✓	✓
Observations	668,325	668,325	668,325	668,325	452,226	452,226	452,226	452,226
R^2	0.210	0.210	0.267	0.281	0.115	0.115	0.158	0.158

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.6: Difference-in-differences models of UCR compliance, analyzing city, county, and state agencies separately.

	1960 - 1994			1995 - 2017		
	City	County	State	City	County	State
Capacity	0.021***	0.014***	0.012***	0.039***	0.067***	0.072***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.006)	(0.009)
Term limits	−0.072***	−0.088***	−0.061***	−0.150***	−0.244***	−0.038**
	(0.005)	(0.009)	(0.014)	(0.015)	(0.014)	(0.019)
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	491,995	109,795	66,535	333,000	74,481	44,745
R^2	0.242	0.393	0.402	0.122	0.307	0.392

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.7: Reduced form and fully specified models of UCR compliance allowing for moderation of main effect by party control.

	1960-1994			1995-2017		
Capacity	0.013***		0.067***	0.047***		0.047***
	(0.001)		(0.002)	(0.003)		(0.003)
Term limits		−0.070***	−0.049***		−0.170***	−0.168***
		(0.005)	(0.009)		(0.010)	(0.011)
Elected head			0.408***			0.442***
			(0.017)			(0.024)
Democratic legislature			0.047***			−0.041***
			(0.007)			(0.008)
Republican legislature			0.071***			−0.166***
			(0.011)			(0.008)
Capacity × elected head			−0.034***			−0.023***
			(0.002)			(0.002)
Capacity × Democratic legislature			−0.050***			0.013***
			(0.002)			(0.003)
Capacity × Republican legislature			−0.029***			0.012***
			(0.003)			(0.003)
Term limits × elected head			0.004			0.101***
			(0.011)			(0.004)
Term limits × Democratic legislature			−0.058***			−0.084***
			(0.010)			(0.005)
Term limits × Republican legislature			0.081***			0.088***
			(0.017)			(0.004)
Sheriff's office in constitution			−0.187***			−0.064***
			(0.003)			(0.004)
County agency			−0.011			−0.278***
			(0.017)			(0.024)
State agency			−0.219***			−0.174***
			(0.002)			(0.002)
Total agency employees			−0.030***			−0.049***
			(0.001)			(0.001)
Population			0.059***			0.071***
			(0.001)			(0.001)
Percentage Black population			−1.176***			0.609***
			(0.064)			(0.205)
Democratic governor			−0.023*			−0.061***
			(0.012)			(0.008)
Republican governor			−0.036***			−0.056***
			(0.012)			(0.008)
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Party × year FE			✓			✓
Observations	668,325	668,325	668,325	452,226	452,226	452,226
R^2	0.210	0.211	0.280	0.115	0.115	0.160

C Underreporting Study

Data on police killings:

- FBI's Uniform Crime Reports: Data are voluntarily reported by local and state police agencies as "justifiable homicides," defined as "the killing of a felon [by an officer] in the line of duty." Includes information on location, agency, offender, and victim. Data are available from 1993 to 2016.[5]

- *The Washington Post* Fatal Force is a database of "every fatal shooting in the United States by a police officer in the line of duty..." gathered by "culling local news reports, law enforcement websites and social media and by monitoring independent databases such as Killed by Police and Fatal Encounters." Includes information on the location, the circumstances of the shooting (victim armed, victim exhibiting signs of mental illness, victim fleeing), and victim characteristics (race, gender). Data are available from 2015 to 2018.

- *The Guardian* The Counted: "we count with traditional reporting on police reports and witness statements, by monitoring regional news outlets, research groups and open-source reporting projects such as the websites Fatal Encounters and Killed by Police." Includes information on the location, victim characteristics (age, gender, race or ethnicity), and the law enforcement agency. Data are available for 2015 and 2016.

- Mapping Police Violence: Original data sourced from Fatal Force, Killed by Police, and the U.S. Police Shootings Database. Additional original work is then done to improve the quality and completeness of the data by searching "searching social media, obituaries, criminal records databases, police reports and other sources." Includes information on location, victim characteristics, whether the victim was armed. Data are available from 2013 to present.

- Killed by Police (this site's license has lapsed since we gathered compiled its data, but the raw entries are cached here): Compiled local news accounts of police killings solicited via an

5. Annual estimates are available for 2017 as well, but not the incident level data necessary for analysis.

active Facebook community. While operating, the project logged an entry for each reported police killing containing the data of the event, its state, the victim's gender and race, the (inclusive) cause of death (gunfire, taser, restraint/physical force, chemical, vehicle, other), as well as links to the initial Facebook posting, the news story, as well as, on occasion, links to audio/video of the incident itself (from surveillance cameras, dash cams, body cams, or uploaded bystander video) or links to media updates on the incident.

- Fatal Encounters: Truly crowd-sourced. Volunteers are able to upload incidents through a upload form, corrections can be made to existing data via a corrections forms. Includes information on location, victim characteristics (ex. race gender), agency involved in death. Data are available from 2000 to 2017 (However the accuracy is questionable before 2013).

C.1 Main text Table 2 discussion

In the following we undertake a series of robustness tests to ensure the results presented in the main text are not an artifact of model specification, sample selection, or the method of analysis. For each set of results, a brief description of analysis and results is provided.

Table A.8 gives the results for police killings reported by each of the sources separately, unlike the differenced results provided in the text. Here we see that if a researcher was to use the FBI's UCR data (Model 1), they would obtain the wrong sign on both *Capacity* and *Income*, and conclude that *Police Ideology* was not significantly related to police killings. The crowd-sourced data (Models 2-6) indicate that such conclusions would be an artifact of the data collection method, not true determinants of police killings.

In Table A.9 we include year fixed effects, with 2015 selected as the reference case (as it appears in all sources). The results on *Capacity* remain the same. In Model 3 we see some evidence that the discrepancy between the MPV data and the FBI's UCR data was significantly lower in 2013 and 2014 than it was in 2015 and 2016. This may be a consequence of improvements to the crowd-sourced data collection over time, though it was difficult to say with certainty.

In Tables A.10 through A.13 we analyze individual cross-sections of data. As you will see, the

Table A.8: Reported Deaths (OLS) – All

	Dependent variable:					
	FBI	WaPost	Guardian	MPV	KBP	Fatal
	(1)	(2)	(3)	(4)	(5)	(6)
Capacity	0.001	−0.063***	−0.060**	−0.047***	−0.048***	−0.030
	(0.011)	(0.024)	(0.024)	(0.014)	(0.016)	(0.019)
Income	0.014	−0.030	−0.055**	−0.036**	−0.053***	−0.073***
	(0.014)	(0.023)	(0.024)	(0.017)	(0.019)	(0.022)
Employees	0.052***	0.068***	0.073***	0.068***	0.059***	0.055**
	(0.015)	(0.019)	(0.019)	(0.017)	(0.018)	(0.022)
Police Ideology	0.027*	0.103***	0.109***	0.091***	0.092***	0.114***
	(0.015)	(0.020)	(0.020)	(0.018)	(0.018)	(0.022)
Democratic Legislature	0.038	−0.103	−0.075	0.005	−0.002	0.001
	(0.030)	(0.066)	(0.066)	(0.041)	(0.046)	(0.055)
Republican Legislature	0.047	−0.065	−0.064	0.021	−0.025	0.080
	(0.034)	(0.057)	(0.057)	(0.043)	(0.048)	(0.057)
Democratic Governor	−0.136**	−0.502***	−0.640***	−0.270***	−0.641***	−0.312***
	(0.063)	(0.149)	(0.149)	(0.087)	(0.122)	(0.118)
Republican Governor	−0.134**	−0.476***	−0.626***	−0.255***	−0.609***	−0.298**
	(0.063)	(0.148)	(0.148)	(0.087)	(0.120)	(0.117)
Constant	0.226***	0.853***	1.035***	0.609***	0.964***	0.780***
	(0.069)	(0.156)	(0.155)	(0.094)	(0.128)	(0.126)
Observations	200	100	100	200	150	200
Log Likelihood	141.496	34.645	26.902	84.490	58.384	28.461

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.9: Unreported by FBI (Random Effects) – w/ Time Fixed Effects

	Dependent variable:				
	WaPost	Guardian	MPV	KBP	Fatal
	(1)	(2)	(3)	(4)	(5)
Capacity	−0.075***	−0.072***	−0.041***	−0.054***	−0.040**
	(0.022)	(0.023)	(0.015)	(0.017)	(0.020)
Income	−0.047**	−0.070***	−0.064***	−0.052***	−0.088***
	(0.023)	(0.023)	(0.017)	(0.018)	(0.021)
Employees	0.011	0.016	0.016	0.013	0.008
	(0.018)	(0.018)	(0.015)	(0.016)	(0.019)
Police Ideology	0.092***	0.098***	0.063***	0.072***	0.089***
	(0.019)	(0.019)	(0.015)	(0.016)	(0.019)
Democratic Legislature	0.002	0.029	−0.008	−0.039	−0.020
	(0.062)	(0.063)	(0.040)	(0.044)	(0.052)
Republican Legislature	−0.031	−0.030	−0.011	−0.030	0.046
	(0.053)	(0.054)	(0.040)	(0.044)	(0.052)
Democratic Governor	−0.122	−0.255*	−0.123	−0.210*	−0.146
	(0.140)	(0.142)	(0.085)	(0.113)	(0.112)
Republican Governor	−0.100	−0.244*	−0.124	−0.195*	−0.142
	(0.140)	(0.142)	(0.085)	(0.111)	(0.112)
2013			−0.049		−0.007
			(0.031)		(0.040)
2014			−0.009	0.002	−0.002
			(0.029)	(0.029)	(0.038)
2015	−0.005	−0.006	0.001	−0.023	−0.034
	(0.021)	(0.024)	(0.025)	(0.023)	(0.032)
Constant	0.308**	0.485***	0.376***	0.436***	0.528***
	(0.148)	(0.150)	(0.093)	(0.119)	(0.121)
Observations	100	100	200	150	200
Log Likelihood	38.011	30.095	85.121	63.295	34.378

Note: *p<0.1; **p<0.05; ***p<0.01

results hold in year-by-year analysis, with the exception of the Fatal Forces data, which, as we discussed in the main text, are biased toward over-counting, because they also include "deaths in custody." On the whole, these models demonstrate that the results provided in the main text are not a consequence of efficiency gains realized from pooling the data—each year stands on its own.

Table A.10: Unreported by FBI - 2013

	Dependent variable:	
	MPV	Fatal
	(1)	(2)
Capacity	-0.039^{**}	-0.039
	(0.019)	(0.030)
Income	-0.068^{***}	-0.122^{***}
	(0.022)	(0.035)
Employees	0.008	-0.005
	(0.020)	(0.031)
Police Ideology	0.021	0.037
	(0.018)	(0.028)
Democratic Legislature	0.036	0.018
	(0.072)	(0.113)
Republican Legislature	0.049	0.083
	(0.072)	(0.113)
Democratic Governor	0.114	0.222
	(0.127)	(0.198)
Republican Governor	0.005	0.147
	(0.132)	(0.206)
Constant	0.108	0.156
	(0.143)	(0.224)
Observations	50	50
R^2	0.363	0.423
Adjusted R^2	0.239	0.311

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.11: Unreported by FBI - 2014

	Dependent variable:		
	KBP	MPV	Fatal
	(1)	(2)	(3)
Capacity	−0.054**	−0.047**	−0.022
	(0.020)	(0.021)	(0.031)
Income	−0.069***	−0.070***	−0.096***
	(0.021)	(0.022)	(0.033)
Employees	0.036*	0.057**	0.046
	(0.021)	(0.022)	(0.032)
Police Ideology	0.037*	0.036*	0.059**
	(0.019)	(0.019)	(0.029)
Democratic Legislature	−0.032	−0.064	−0.097
	(0.084)	(0.087)	(0.130)
Republican Legislature	0.013	−0.00001	0.014
	(0.083)	(0.087)	(0.130)
Republican Governor	−0.031	−0.025	0.007
	(0.049)	(0.051)	(0.076)
Constant	0.224***	0.275***	0.416***
	(0.076)	(0.080)	(0.119)
Observations	50	50	50
R^2	0.430	0.430	0.411
Adjusted R^2	0.335	0.335	0.312

Note: *p<0.1; **p<0.05; ***p<0.01 .

Table A.12: Unreported by FBI - 2015

	Dependent variable:				
	WaPost	Guardian	MPV	KBP	Fatal
	(1)	(2)	(3)	(4)	(5)
Capacity	−0.083***	−0.076**	−0.075***	−0.075**	−0.077**
	(0.027)	(0.030)	(0.028)	(0.029)	(0.033)
Income	−0.023	−0.040	−0.027	−0.028	−0.062*
	(0.027)	(0.030)	(0.028)	(0.029)	(0.033)
Employees	−0.006	0.006	−0.007	−0.001	−0.014
	(0.024)	(0.027)	(0.025)	(0.026)	(0.030)
Police Ideology	0.106***	0.106***	0.107***	0.109***	0.134***
	(0.022)	(0.024)	(0.023)	(0.024)	(0.027)
Democratic Legislature	−0.002	0.024	0.009	0.031	0.084
	(0.074)	(0.082)	(0.077)	(0.080)	(0.092)
Republican Legislature	−0.014	−0.015	−0.008	0.021	0.081
	(0.064)	(0.071)	(0.067)	(0.070)	(0.080)
Democratic Governor	−0.134	−0.248	−0.134	−0.212	−0.131
	(0.166)	(0.185)	(0.173)	(0.180)	(0.207)
Republican Governor	−0.105	−0.247	−0.106	−0.187	−0.088
	(0.165)	(0.184)	(0.172)	(0.179)	(0.205)
Constant	0.299*	0.469**	0.299	0.408**	0.398*
	(0.172)	(0.192)	(0.180)	(0.187)	(0.215)
Observations	50	50	50	50	50
R^2	0.509	0.436	0.482	0.472	0.553
Adjusted R^2	0.413	0.326	0.381	0.369	0.466

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.13: Unreported by FBI - 2016

	Dependent variable:				
	WaPost	Guardian	MPV	KBP	Fatal
	(1)	(2)	(3)	(4)	(5)
Capacity	−0.064**	−0.066**	−0.069***	−0.064**	−0.079**
	(0.024)	(0.025)	(0.025)	(0.025)	(0.032)
Income	−0.098***	−0.117***	−0.100***	−0.114***	−0.114***
	(0.026)	(0.027)	(0.027)	(0.027)	(0.035)
Employees	0.020	0.026	0.019	0.026	0.017
	(0.018)	(0.018)	(0.019)	(0.018)	(0.024)
Police Ideology	0.073***	0.086***	0.077***	0.086***	0.122***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.027)
Democratic Legislature	0.026	0.043	0.025	0.056	0.007
	(0.068)	(0.069)	(0.071)	(0.070)	(0.090)
Republican Legislature	−0.051	−0.044	−0.059	−0.041	−0.045
	(0.058)	(0.059)	(0.060)	(0.060)	(0.076)
Democratic Governor	−0.147	−0.281*	−0.279*	−0.142	−0.391*
	(0.153)	(0.156)	(0.159)	(0.158)	(0.202)
Republican Governor	−0.151	−0.277*	−0.270*	−0.120	−0.340
	(0.154)	(0.157)	(0.160)	(0.159)	(0.203)
Constant	0.371**	0.533***	0.515***	0.387**	0.781***
	(0.162)	(0.165)	(0.168)	(0.167)	(0.213)
Observations	50	50	50	50	50
R^2	0.504	0.575	0.509	0.566	0.577
Adjusted R^2	0.407	0.492	0.413	0.481	0.494

Note: *p<0.1; **p<0.05; ***p<0.01

In Table A.14 and A.15 we relax the linearity assumption of the results in the main text, estimating count regression models. Because the crowd-sourced data are frequently, but not uniformly, greater than the UCR data, we are now forced to censor the differenced results from below (constraining them to zero). Additionally, rather than deflating by population, we now include this as an exposure variable, which is analogous to modeling this as a rate as in the main text. The results on *Capacity* remain negative and significant.

Table A.14: Unreported by FBI (Poisson)

	Dependent variable:				
	WaPost	Guardian	MPV	KBP	Fatal
	(1)	(2)	(3)	(4)	(5)
Capacity	-0.330^{***}	-0.295^{***}	-0.257^{***}	-0.270^{***}	-0.092^{***}
	(0.071)	(0.064)	(0.031)	(0.041)	(0.022)
Income	-0.371^{***}	-0.377^{***}	-0.347^{***}	-0.394^{***}	-0.256^{***}
	(0.049)	(0.044)	(0.028)	(0.037)	(0.022)
Employees	0.075^{*}	0.071^{*}	0.120^{***}	0.102^{***}	0.019
	(0.041)	(0.037)	(0.024)	(0.031)	(0.020)
Police Ideology	0.206^{***}	0.189^{***}	0.094^{***}	0.152^{***}	0.160^{***}
	(0.026)	(0.024)	(0.017)	(0.021)	(0.012)
Democratic Legislature	0.281^{**}	0.306^{***}	0.228^{***}	0.165	0.200^{***}
	(0.136)	(0.118)	(0.088)	(0.109)	(0.069)
Republican Legislature	-0.005	-0.040	0.094	-0.001	0.303^{***}
	(0.110)	(0.098)	(0.082)	(0.096)	(0.065)
Democratic Governor	-1.025^{*}	-1.338^{***}	-0.415	-1.291^{**}	-0.174
	(0.614)	(0.485)	(0.454)	(0.525)	(0.360)
Republican Governor	-0.864	-1.253^{**}	-0.405	-1.193^{**}	-0.101
	(0.617)	(0.488)	(0.455)	(0.526)	(0.361)
log(Population / 100k)	0.938^{***}	0.964^{***}	1.008^{***}	0.965^{***}	0.942^{***}
	(0.058)	(0.053)	(0.033)	(0.042)	(0.025)
Constant	-0.724	-0.220	-1.300^{***}	-0.430	-0.891^{**}
	(0.600)	(0.471)	(0.461)	(0.517)	(0.363)
Observations	100	100	200	150	200
Log Likelihood	-337.630	-334.259	-702.165	-502.741	-714.960
Akaike Inf. Crit.	695.259	688.519	1,424.329	1,025.482	1,449.921

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.15: Unreported by FBI (Negative Binomial)

	Dependent variable:				
	WaPost	Guardian	MPV	KBP	Fatal
	(1)	(2)	(3)	(4)	(5)
Capacity	−0.301**	−0.259**	−0.248***	−0.251***	−0.083**
	(0.140)	(0.116)	(0.059)	(0.081)	(0.042)
Income	−0.258***	−0.295***	−0.256***	−0.296***	−0.225***
	(0.091)	(0.076)	(0.049)	(0.066)	(0.035)
Employees	0.022	0.055	0.090*	0.063	0.008
	(0.080)	(0.064)	(0.046)	(0.062)	(0.035)
Police Ideology	0.338***	0.296***	0.211***	0.269***	0.183***
	(0.067)	(0.054)	(0.039)	(0.051)	(0.028)
Democratic Legislature	0.143	0.215	0.162	0.046	0.085
	(0.252)	(0.206)	(0.151)	(0.195)	(0.110)
Republican Legislature	0.027	−0.019	0.113	0.022	0.233**
	(0.203)	(0.168)	(0.138)	(0.170)	(0.100)
Democratic Governor	−0.728	−1.088*	−0.381	−0.965	−0.147
	(0.797)	(0.630)	(0.529)	(0.668)	(0.406)
Republican Governor	−0.561	−1.002	−0.391	−0.863	−0.091
	(0.803)	(0.637)	(0.531)	(0.671)	(0.407)
log(Population / 100k)	0.959***	0.964***	0.996***	0.956***	0.942***
	(0.120)	(0.099)	(0.061)	(0.081)	(0.044)
Constant	−1.108	−0.470	−1.252**	−0.701	−0.823**
	(0.765)	(0.602)	(0.544)	(0.664)	(0.416)
Observations	100	100	200	150	200
Log Likelihood	−288.153	−296.483	−591.243	−432.608	−645.003
θ	3.546*** (0.835)	5.735*** (1.433)	5.413*** (0.906)	4.081*** (0.824)	11.281*** (1.994)
Akaike Inf. Crit.	596.306	612.966	1,202.485	885.215	1,310.006

Note: *p<0.1; **p<0.05; ***p<0.01

C.2 Main text Table 3 discussion

As of 2019, 8 states have enacted policies that make explicit demands of police agencies when investigating officer-involved deaths.[6] These laws, California (Penal Code §§917, 919) Colorado (§§16-2.5-301, 20-1-114) Connecticut (§51-277a), §15-12-71), Hawaii (§§28-151 through 153), Illinois (50 §727/ 1-10), New York (EO 147), Utah (§76-2-408) and Wisconsin (§175.47), mandate the investigation of excessive use-of-force allegations and officer-involved deaths. Note that Hawaii's law did not go into effect until 2017, so it is outside of our sample period. Note also that New York's policy is an executive order, rather than a law. Policies come in two primary forms, either a) mandating a protocol for local investigation, nearly always requiring delegation of the investigation to a separate law enforcement agency (i.e., county or state agency) and *always* requiring collaboration with a separate entity, or b) mandating an investigation by state entities (state department of justice, or a special officer review board). Because these policies not only mandate investigation and but also mandate, in some form or another, alerting a separate government entity about the incident, these are, in effect, transparency enforcement devices. As such, at a minimum, these policies reduce the potential gains from failing to report or inaccurately reporting police lethality.

While these policies are still new (the oldest dating to 2011), they may still allow us to evaluate whether or not state intercession into local law enforcement is able to increase transparency. Figure A4 shows the enactment of these laws across states and over time. Six of the seven treated units receive treatment during our observational period (note that Hawaii's law was not in effect until 2017, after our sample period).

Before estimating the effect of these policies, we first note that states with high legislative capacity appear significantly more likely to enact such policies (p-value of 0.029 in a bivariate logistic regression), with a one-standard deviation increase in legislative capacity increasing the probability of treatment from 1.96% to 11.67%. Of course, this is in itself support for our central

6. Georgia has also passed a law permitting grand jury review of officer-involved deaths, but place no explicit demands on police agencies in their investigation of such offenses. In other words, the law has no mandate for transparency and therefore no teeth as far as our study is concerned.

argument.

Figure A4: Distribution of State Investigation Laws

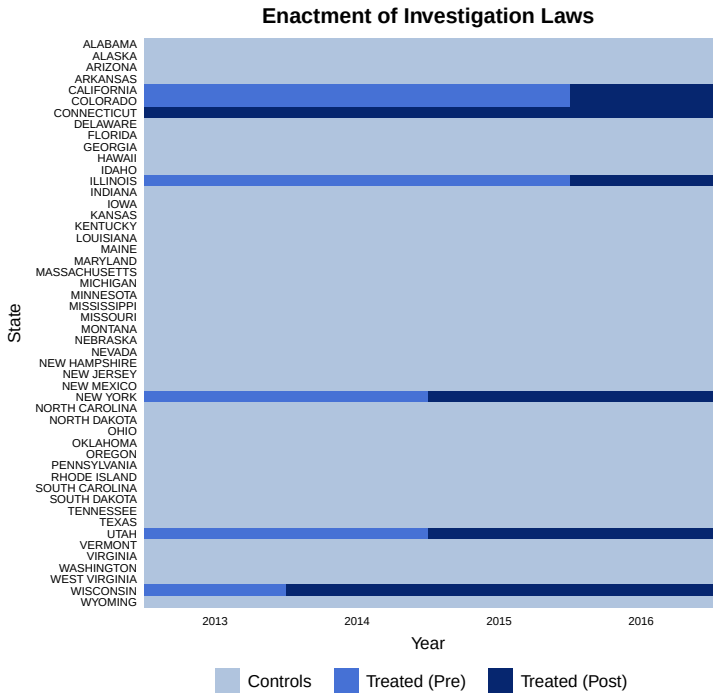


Table A.16 presents the same difference-in-differences analysis we discussed in the main text. In each model, we observe a large and statistically significant reduction in the number of unreported police killings following the implementation of the investigation law.

Table A.17 presents the difference-in-differences analysis, but with the addition of control variables. The central results hold. Entering treatment reduces the number of unreported police killings. Though we report the control variable results for transparency, it is important to remember that this model is not structured to identify any effect other than the investigation policy and the correlations on the variables are not to be substantively interpreted.

Table A.16: Diff-in-Diff Analysis of Unreported Police Killings and State Investigation Laws

	WaPost	Guardian	MPV	KBP	Fatal
Investigation Policy	-8.149^{***}	-7.411^{**}	-5.096^{**}	-6.641^{***}	-9.961^{***}
	(2.920)	(3.179)	(2.255)	(2.255)	(3.100)
State Effects	✓	✓	✓	✓	✓
Year Effects	✓	✓	✓	✓	✓
Observations	100	100	200	150	200
R^2	0.964	0.973	0.941	0.947	0.964

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.17: Diff-in-Diff Analysis of Unreported Police Killings and State Investigation Laws w/ Control Variables

	WaPost	Guardian	MPV	KBP	Fatal
Investigation Policy	-7.297^{**}	-6.556^{*}	-4.716^{*}	-7.274^{***}	-8.314^{***}
	(3.009)	(3.296)	(2.402)	(2.435)	(3.083)
Capacity	102.052^{**}	102.459^{*}	1.536	1.008	0.876
	(46.695)	(51.137)	(1.685)	(1.685)	(2.162)
Income	−0.019	−0.479	−0.822	2.083	−0.974
	(2.774)	(3.038)	(1.823)	(1.969)	(2.340)
Employees	0.622	0.123	−1.281	−0.926	−0.080
	(2.276)	(2.492)	(2.351)	(2.140)	(3.018)
Police Ideology	32.564^{**}	33.728^{**}	−8.450	−34.216	-191.368^{***}
	(12.242)	(13.407)	(23.274)	(31.029)	(29.872)
Democratic Legislature	75.629^{**}	75.667^{**}	1.617	−0.665	0.204
	(29.358)	(32.150)	(2.056)	(2.144)	(2.639)
Republican Legislature	41.144^{**}	36.729^{*}	−0.984	−1.922	2.414
	(18.427)	(20.179)	(2.819)	(3.191)	(3.618)
Democratic Governor	188.747^{**}	184.903^{*}	−2.673	−2.168	−1.847
	(90.848)	(99.489)	(4.036)	(5.645)	(5.180)
Republican Governor	188.259^{**}	185.868^{*}	−0.806	0.089	−2.031
	(90.387)	(98.984)	(3.965)	(5.216)	(5.089)
Population / 100k	−1.322	−1.280	0.261	−0.103	-2.206^{***}
	(0.824)	(0.902)	(0.352)	(0.470)	(0.452)
State Effects	✓	✓	✓	✓	✓
Year Effects	✓	✓	✓	✓	✓
Observations	100	100	200	150	200
R^2	0.966	0.974	0.943	0.949	0.970
Adjusted R^2	0.924	0.941	0.918	0.915	0.956

Note: *p<0.1; **p<0.05; ***p<0.01

D LEMAS replication

Here, we replicate our main findings by examining another federal law enforcement data-gathering project, the BJS's Law Enforcement Management and Administrative Statistics survey. LEMAS collects data on "... agency responsibilities, operating expenditures, job functions of sworn and civilian employees, officer salaries and special pay, demographic characteristics of officers, weapons and armor policies, education and training requirements, computers and information systems, vehicles, special units, and community policing activities." Like the UCR, this is an official request for data from a federal agency. Unlike UCR, LEMAS asks for management-related information, rather than information on crime. With the help of Elizabeth Davis, the statistician that runs the project, we were able to recover full information on non-response from the 2016 iteration of the survey. LEMAS codes any survey with less than 60% completion as "nonresponse," however, Davis reported that only 5 of the 692 officially non-responding agencies (about 20% of the 3,470 sampled agencies) submitted partial information and dropping these observations or recoding them as responding does not meaningfully change estimation results. In Table A.18, we regress LEMAS survey response on state legislative capacity, as well as the presence of term limits, to establish a cross-sectional correlation. The results comport with the analyses that we present in the main text for legislative capacity: response probability is significantly increasing with legislative capacity. There is no relationship, however, between term limits and LEMAS response in this cross-section.

Table A.18: Legislative capacity and LEMAS response

Capacity	0.032***		0.033***
	(0.011)		(0.011)
Term limits		0.001	−0.004
		(0.014)	(0.014)
Constant	0.806***	0.800***	0.807***
	(0.007)	(0.009)	(0.009)
Observations	3,470	3,470	3,470
R^2	0.003	0.00000	0.003

Note: *p<0.1; **p<0.05; ***p<0.01

E Sheriffs

E.1 Accounting of constitutional foundations

The interdisciplinary literature on police and policing has produced a fair amount of research suggesting that sheriffs (and elected sheriffs in particular) are fundamentally different police administrators because they are elected or because their office is mandated by the state constitution (e.g., Falcone and Wells 1995; Tomberlin 2018; DeHart 2020). There is some evidence that sheriffs behave differently than their appointed, municipal agency heads counterparts. For example, they engage in civil asset seizures less often (Mughan, Li, and Nicholson-Crotty 2020). On the other hand, their approach to budgeting processes or interaction with legislators is no different than their appointed counterparts (LaFrance 2012), despite previous scholarship implying that there may be differences there (LaFrance and Placide 2010). In the main text, we compare the effect of capacity on elected and appointed sheriffs using the excellent data collected by DeHart (2020) and find, as many scholars would expect, that elected sheriffs are less sensitive to changes in legislative capacity than their appointed counterparts—although the baseline effect of electing sheriffs appears to increase their overall rate of compliance, implying a level of *political* accountability to voters (Romzek and Dubnick 1987). Here, we 1) document which states mention sheriffs in their constitutions, 2) document funding mandates for sheriffs, and 3) assess the effect of constitutionality and funding mandates by analyzing within-state variation in Wyoming and Alabama.

Table A.19 catalogues the states with constitutions that refer to the sheriff's office in any way. We also do our best to identify which entity controls the compensation for the sheriff, though this is unclear in many cases from the constitutional text and we urge caution to any scholars who may be interested in using these categorizations. We finally note that we did not detect any significant behavioral differences when comparing across compensation control regimes.

Forty-one constitutions have ever noted the office while thirty-seven retain that note as of September 2021. These numbers disagree with the figure reported by Falcone and Wells (1995), which they attribute to a sheriff's association document. We came by our figure by reading the

constitutions. We also note that the amount of text dedicated to sheriffs is minimal and nearly always the same or very similar across documents. Most often, there is prescription for elections to be held, as in this text from the Massachusetts Constitution,

> "The legislature shall prescribe, by general law, for the election of sheriffs, registers of probate, [commissioners of insolvency,] and clerks of the courts, by the people of the several counties, and that district-attorneys shall be chosen by the people of the several districts, for such term of office as the legislature shall prescribe."

and also a prohibition against individuals holding multiple offices (again Massachusetts)

> "No person shall be capable of holding or exercising at the same time, within this state more than one of the following offices, viz. – judge of probate – sheriff – register of probate – or register of deeds – and never more than any two offices which are to be held by appointment of the governor, or the governor and council, or the senate, or the house of representatives, or by the election of the people of the state at large, or of the people of any county, military offices and the offices of justices of the peace excepted, shall be held by one person."

In many cases, that is the totality of language dedicated to the sheriff and we do not interpret this as a constitutional mandate that would endow sheriffs with special authority, legitimacy, powers, etc. that would justify different management behaviors from other agency heads *apart* from the fact that nearly all sheriffs are elected and therefore serve a different principal (voters) from their appointed counterparts (who serve legislatures or chief executives). As such, we believe it is not the office of the sheriff that is due special consideration or even whether or not the office is written into the text of the constitution, but rather whether or not the sheriff is *elected* that is important. Of course, in the main text, leveraging both within- and across-state and year variation in sheriffs' selection institutions, we provide strong evidence that elected sheriffs are, in fact, more resistant to legislative oversight than their appointed counterparts.

Table A.19: Constitutional parameters of sheriffs. States not listed have no reference to sheriff in constitution

State	Salary control	Funding mandate	Note
Alabama	legislature	Geneva County: $20 document fee (per pleading) Lamar County: $100-500 fine (drug conviction) Pickens County: $10 court fee (all cases) St. Clair County: 10% of $0.10 bingo card taxes	
Arkansas	legislature	NA	
Arizona	county†	NA	
California	legislature	NA	
Colorado	legislature	NA	
Connecticut	legislature	"sufficient sureties... for the faithful discharge of ... duties"	elected sheriff replaced by state marshal via amendment in 2000
Delaware	legislature	NA	
Florida	legislature	NA	
Georgia	legislature	NA	
Idaho	county	NA	
Illinois	county†	NA	county may abolish via referendum
Indiana	county†	NA	
Kansas	legislature	NA	sheriff removed from constitution via Amendment 17, 1902
Kentucky	legislature	NA	
Louisiana	legislature‡	NA	
Maryland	legislature	"... such expenses necessary to the conduct of his office..."	
Massachusetts	unclear	NA	
Maine	unclear	NA	sheriff may be deselected by governor
Michigan	county†	NA	
Mississippi	legislature	NA	
Montana	legislature	NA	
North Carolina	legislature	NA	
North Dakota	county	NA	
New Hampshire	unclear	NA	
New Jersey	legislature	NA	
New York	legislature	NA	
New Mexico	county	NA	new legislated services must be coupled with funds
Nevada	legislature	NA	
Ohio	county	NA	sheriff removed in Amendment 100 (1933), devloving all county organization
Oklahoma	legislature	NA	legislature can eliminate via regular procedure
Oregon	legislature‡	NA	
Pennsylvania	legislature‡	NA	
South Carolina	legislature	NA	
South Dakota	legislature‡	NA	
Tennessee	legislature‡	NA	
Texas	legislature	NA	
Vermont	unclear	NA	
Virginia	legislature	NA	
West Virginia	legislature	NA	
Wisconsin	unclear	NA	sheriff may be deselected by governor
Wyoming	legislature	NA	sheriff removed from constitution via Amendment (1990)

† = legislature may supercede county

‡ = home-rule counties have compensation authority

E.2 Null effect of constitutional description

How can we be sure election is the central institutional factor driving behavioral differences? Fortunately, one state, Wyoming, amended its constitution within our sample period (1990) removing any mention of sheriff's office. This allows us assess whether or not constitutional mention has a causal impact on sheriff behavior. In Figure A5, we plot the average UCR compliance rate for all 24 Wyoming sheriff's offices before and after the constitutional change. While compliance is, on average, higher after the constitutional change, the pattern of the data make clear that the amendment was in no way causal to this average increase. Indeed, statistical analysis in Table A.20 comparing the pre-post reform behavior of sheriffs to state and municipal agency heads implies that, if anything, sheriffs became *less* likely to comply with UCR requests, relative to appointed agency heads, after being struck from the constitution. While we still include an indicator for constitutions mentioning the sheriff's office in the main text analysis, this investigation implies to us that constitutional reference to the sheriff's office is not a significant behavioral modifier.

Figure A5: Constitutional change in Wyoming

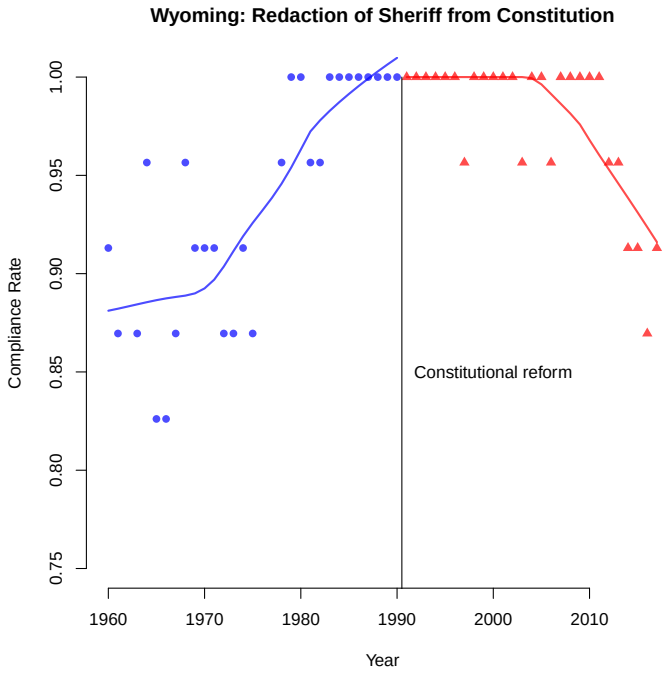


Table A.20: Effect of reform removing sheriff from Wyoming constitution on UCR compliance

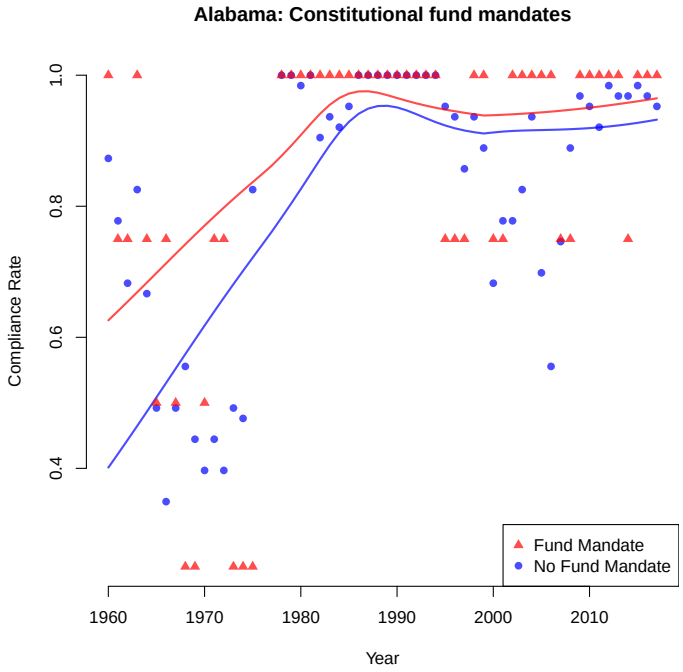
Sheriff	0.357^{***}
	(0.017)
Post-reform	0.218^{***}
	(0.061)
Sheriff × post-reform	-0.097^{***}
	(0.025)
Year FE	✓
Observations	4,582
R^2	0.339
Note:	*p<0.1; **p<0.05; ***p<0.01

E.3 Null effect of funding mandate

The table also identifies funding mandates and we were surprised to find that they are quite rare—just three states, one of which eliminated the office in 2000, write explicit funding mandates into their constitutions—despite several academic articles asserting that they are pervasive. Though many states may have *statutory* funding mandates, these are, of course, laws that the legislature can change if it so desires, just as the legislature may change the budget if it so desires. For the specific constitutional mandate in Maryland, we could find no instance of a sheriff bringing suit against the legislature for insufficient funds. Of course, this could mean that the legislature always provides ample funding, but we find the language of the mandate vague and believe most would agree that it provides substantial discretion to the legislature in its budgeting choices. As such, we do not believe this funding mandate is likely to be a salient behavioral modifier for either the legislature or sheriffs. One potential exception is Alabama. The Alabama constitution contains explicit funding guidelines for sheriff's offices in four counties: Geneva, Lamar, Pickens, and St. Clair. It is possible that these explicit funding guidelines endow sheriffs in these counties with a degree of insulation from oversight by providing at least some guaranteed revenue. We assess this by comparing the average UCR compliance of these four counties to the average compliance of the other 63 counties over time in Figure A6. As the figure shows, the counties with funding mandates have slightly higher (but not statistically differentiable) UCR compliance. The data suggest that the funding mandates are not salient behavioral modifiers for sheriffs or the legislature—transparency patterns for sheriffs with and without constitutionally guaranteed revenue are non-differentiable.

We do not deny that sheriffs' offices are different from municipal and state police agencies. Indeed, as an anonymous reviewer points out, sheriffs' responsibilities are often more diverse or diffuse than municipal agency heads, as sheriffs are often tasked with running jail systems and providing courthouse security and services in addition to providing regular police services. Nonetheless, at least in the case of data transparency, our analyses suggests that what really sets sheriffs apart is their (typical) method of selection. We find no evidence from a constitutional reform in Wyoming that having the office written into the state constitution or not changes sheriffs' behavior—and we

Figure A6: Funding mandates in Alabama



note that, after reading all the state constitutions, this is not a surprise; the typical language used in reference to sheriffs is cursory and bland and almost never defines features that may insulate sheriffs from legislative scrutiny *apart* from their selection and retention method. Neither do we find evidence that special funding carveouts change behavior by examining the behavior of Alabama sheriffs, the one state that has written explicit funding mandates for sheriffs' offices into its constitution. There is a lot of room for good research on sheriffs vis-á-vis state and municipal police agency heads, but in the case of transparency, what seems to matter most is whether they are elected or appointed. Finally, we also catalogue in Table A.19 when mention of the office was struck from the constitution in the few cases it has been so, and note interesting removal procedures, such as the unilateral power of the governor in Maine and Wisconsin to dismiss a sitting sheriff. We hope our colleagues find these data useful.

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