Hierarchical Accountability in Government: Theory and Evidence*

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Abstract

Does an indirectly-elected policymaker face incentives similar to a politician or a bureaucrat? This paper develops a voter-intermediary-policymaker model of hierarchical agency with adverse selection and moral hazard. In equilibrium a policymaker accountable to the voter through an intermediary behaves either as an insulated bureaucrat or a pandering politician. Which behavior dominates depends on policymaker congruence and public opinion strength. We examine the model's predictions for U.S. city governments and find: (i) indirectly-elected city managers choose popular police employment policies less often than directly-elected mayors, and (ii) this police employment differential varies according to the model's political and informational mechanisms.

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1 Introduction

A recent body of theoretical and empirical research in political economy has advanced our understanding of how elected policymakers - politicians - differ from non-elected policymakers - bureaucrats - in terms of either extrinsic motivation (incentives) or intrinsic motivation (type).[1] Less attention has been given to *indirectly-elected policymakers*, despite the pervasiveness of this accountability form. For instance, about half of U.S. cities operate under a manager charter, where the chief executive is hired/fired by a directly-elected city council and not by city voters. Unlike a politician, a city manager is not answerable to voters; unlike a typical bureaucrat, he is not protected by civil service tenure.[2]

Does an indirectly-elected policymaker face incentives more similar to those of a politician or a bureaucrat? At first sight, transferring policymaker control to an *intermediary* seems to disconnect policymaker choices from voter preferences, reducing policymaker responsiveness. However, if the voter is informed he can exploit the intermediary's and policymaker's reelection motivations to obtain desired policies. In a complete information environment indirect control can be as effective as direct control (Persson, Roland and Tabellini 1997). An indirectly-elected policymaker, such as a city manager, should then track the median voter's preferences just as a directly-elected policymaker (Deno and Mehay 1987).[3]

In an asymmetric information environment, where the policymaker knows more about policy optimality than the voter, direct accountability to the voter creates pandering incentives (Canes-Wrone, Herron and Shotts 2001, Maskin and Tirole 2004). That is, the policymaker follows popular opinion by choosing popular policies, instead of using his expertise to choose optimal policies. Even if the voter's best interest is to limit policymaker pandering a commitment problem renders him unable to do so: Ex-ante the voter may prefer to insulate the policymaker but ex-post he is better off firing an unpopular policymaker.[4]

When accountability to an uninformed voter distorts the policymaker's incentives in this

[1] Examples include: Besley and Coate (2003), Maskin and Tirole (2004), Alesina and Tabellini (2007), Iaryczower, Lewis and Shum (2010).

[2] "If the manager is not responsive to the governing body, it has the authority to terminate the manager at any time." (ICMA 2007, p. 2) Manager government is also used by U.S. counties, as well as cities and counties in Canada, Ireland, and other developed countries. Similarly, U.S. public school superintendents are accountable to a popularly-elected school board in about 80 percent of independent public school districts.

[3] One caveat is possible policymaker-intermediary collusion. In that case, an indirectly-elected policymaker has more discretion to pursue rent-seeking than a directly-elected policymaker. Persson, Roland and Tabellini (2000) apply these ideas to fiscal policy in parliamentary and presidential regimes.

[4] Besley and Smart (2007) and Smart and Sturm (2011) notice the committment issue in the context of fiscal rules and term limits, respectively. Non-elected officials such as life-tenured judges are immune to pandering incentives (Maskin and Tirole 2004), while term limits (Smart and Sturm 2011) and informative media (Ashworth and Shotts 2010) can limit politician pandering.

way, delegating policymaker accountability to an informed intermediary seems justifiable. The case for delegation is less clear-cut if the intermediary may have preferences different from the voter's, however. In that case the voter needs to provide incentives for the intermediary to act in the voter's interest. To what extent can an informed intermediary help the voter insulate the policymaker from pandering incentives? If insulation is achievable, is it complete, allowing the policymaker to always act on his expertise as a typical bureaucrat would, or partial, preserving some dependence on public opinion?

This paper studies the incentives of indirectly-elected policymakers in an asymmetric expertise setting. We develop a hierarchical agency model to understand the political and informational conditions under which an indirectly-elected policymaker is insulated from popular pressure. We then estimate policy differences between indirectly-elected and directly-elected U.S. city executives (managers vs. mayors) during 1960-2000, addressing the potential endogeneity of accountability form with a new instrument for manager government. We also examine the mechanisms behind the estimated effects.

In the model, policymaker accountability to the voter is mediated through an intermediary. Every period the policymaker chooses between a popular and an unpopular policy, and the intermediary keeps or fires the policymaker. Elections take place every other period, when the voter, having observed two periods of policymaker and intermediary actions, retains or replaces the intermediary. Thus, strictly speaking, *hierarchical accountability* is a form of *indirect accountability* where the intermediary can fire the policymaker between elections, and policymaker accountability is the intermediary's primary responsibility.[5] We introduce two informational problems: (i) policymaker and intermediary preferences are private information and can be either congruent with or dissonant from the voter's preferences (adverse selection), and (ii) each period the voter is uncertain about the popular's policy optimality (moral hazard), although a priori it is more likely optimal.

The model's key result is that under hierarchical accountability policymaker insulation can be an equilibrium phenomenon. This happens when policymaker congruence is high and public opinion is weak. In this setting keeping an unpopular policymaker signals a congruent intermediary because unpopular policymakers are more likely congruent than dissonant. Thus the voter's best response is to retain an intermediary who keeps the policymaker. This insulates the policymaker, allowing him to follow his preferences and expertise regardless of public opinion. Conversely, when policymaker congruence is low and public opinion is strong keeping an unpopular policymaker signals a dissonant intermediary. The voter then

[5]For instance, a council-elected city manager serves at the council's pleasure, whereas a pre-17th Amendment U.S. senator, although indirectly-elected through the state legislature, had a fixed term of six years.

gives the intermediary the incentive to fire an unpopular policymaker, which in turn gives the policymaker the incentive to pander, i.e., choose the popular policy regardless of its optimality to the voter.[6]

Apart from giving pre-election *incentives* to the intermediary, which in turn shapes the incentives to the policymaker, the voter's equilibrium strategy also performs between-terms *selection* of more congruent intermediaries, who in turn perform within-term selection of policymakers. Policymaker selection by the intermediary is precise because the intermediary is informed about policymaker preferences, but can backfire if the intermediary is dissonant.

By contrast, under direct accountability the voter could not commit to keep an unpopular policymaker because an unpopular policy always signals a dissonant policymaker. Thus, directly-elected policymakers would always have pandering incentives when public opinion favors a particular policy. Also, selection would occur only between-terms, and not within a term, and would be based on the voter's limited policy expertise, therefore less precise.

The model thus predicts that indirectly-elected policymakers choose different policies than directly-elected policymakers. The nature of policy differences depends on whether incentives or selection are at play. Incentives-driven differences imply a lower frequency of popular policies under hierarchical than under direct accountability, due to reduced pandering. Selection-driven differences depend on the outcome of hierarchical selection. Popular policies are more frequent if hierarchical selection is superior to direct selection, or less frequent if hierarchical selection is less effective. The model also predicts no policy differences on policy issues over which public opinion is neutral.

Our empirical application examines how policymaking depends on accountability form in U.S. cities. There are several reasons why this setting is a suitable testing ground for our theory. First, a large number of U.S. cities are run by city managers, indirectly-elected chief executives with the same major policy responsibilities as directly-elected mayors, i.e. writing the city budget and hiring personnel.[7] Second, because crime has consistently ranked among the top two concerns of city residents since Gallup started to survey local attitudes in 1959 (see Gallup 2000), police employment is susceptible to pandering incentives. We can thus test whether managers employ fewer police officers; for comparison, we can also test whether

[6]That both insulation and pandering are equilibrium outcomes is consistent with anecdotal accounts of city manager behavior. Interviews with U.S. city managers reveal that having to respond to the dual expectations of neutral expertise and public acceptance leads managers to "[...] cautiously and continuously tread the middle ground between the two poles of politics and expertise." (Stillman 1977, p. 666.)

[7]Mayor government has historically been the traditional form of government in U.S. cities. Manager government was first adopted in Staunton, Virginia in 1908 and spread rapidly during the Progressive Era and later in the 20th century. See Knocke (1982) for an analysis.

a similar differential exists for policies that do not elicit a clear popular preference, such as employment of police civilians, i.e. administrators, attorneys, dispatchers, etc.

We find that managers employ fewer police officers per capita, equivalent to 7% of average police department spending, and 0.5% of average city spending, but a comparable number of police civilians per capita. This pattern is robust across a number of specifications. First, the results hold up when we control for an extensive array of geographic, demographic, and institutional factors, including related Progressive reforms. Second, the results are robust to instrumenting for government form using early 20th century precipitation shocks that influenced manager charter adoptions for reasons obsolete today. Third, the pattern survives extensive sensitivity analysis.

We empirically examine the behavior behind this effect by introducing measures of the theoretical model's mechanisms: motivation, congruence and popularity. Overall the results are consistent with an incentive effect. We find that the officer employment differential increases in retention motivation, measured by policymaker salary. The officer employment differential increases in policymaker congruence, measured by low police unionization, and decreases in police popularity, measured by local newspaper sales, consistent with insulation of managers in high-congruence low-popularity cities. The officer differential is also more pronounced in election years, when incentives should be sharper.[8] We argue that these patterns cannot be explained by alternative mechanisms, such as patronage motivations or policymaker type selection.

Our paper is related to several strands of literature. First, it directly relates to the small, and mostly empirical, literature examining indirectly-elected officials: federal agency heads elected by Congress (Weingast and Moran 1983); prime-ministers elected by parliament (Persson, Roland and Tabellini 1997); pre-17th Amendment U.S. senators elected by state legislatures (Gailmard and Jenkins 2009); senior judges promoted by a UK government commission (Blanes i Vidal and Leaver 2011); and village heads appointed by district heads in new democracies (Martinez-Bravo 2011). Our paper contributes to this literature by introducing expertise asymmetry and theoretically characterizing the tension between insulation and pandering in the incentives of indirectly-elected policymakers.[9]

[8] The evidence for electoral cycles in public policies is mostly at the national and state level, and is generally weaker for developed countries. For references see Drazen (2000) and Dahlberg and Mork (2011).

[9] Hierarchical agency (principal-supervisor-agent) models in contract theory and corporate finance focus on financial, rather than political, incentives. Strausz (1997) finds that under moral hazard the principal can improve both incentives and his ability to commit to a broader range of wage structures by delegating agent monitoring to an intermediary. Park (2000) argues that the optimal firm debt contract should delegate monitoring to a single senior lender who is allowed to appropriate the full return from his monitoring activities.

Second, our paper relates to the literature on the role of career concerns in distorting the incentives of asymmetrically informed experts. Examples include: Prendergast (1993), Morris (2001), Ashworth (2005), Prat (2005), Besley (2006), Fox (2007), Levy (2007), Morelli and Van Weelden (2011), and Che, Dessein and Kartik (2012). We contribute to this literature by showing how incentive distortions can be alleviated by institutional constraints, i.e., hierarchical accountability allows voters to reduce pandering when it becomes detrimental to their welfare. We also provide evidence for this type of behavior and its sources.[10]

Third, our paper relates to the large empirical literature on U.S. city managers, most of it in public administration and political science. This literature has placed disproportionate emphasis on differences in public spending from city mayors; results have been mixed. Coate and Knight (2011) survey this literature and provide evidence that managers outspend mayors.[11] A smaller literature has looked at other policy outcomes: Managers are more likely to privatize city services (Levin and Tadelis 2010) and reduce full-time city employment (Enikopolov 2010). In contrast, we document differences in police employment. Also, while the previous literature has treated city government form as exogenous, we address potential endogeneity in government form with a new instrumental variable.[12]

The rest of the paper is organized as follows. Section 2 presents the model. Section 3 provides historical background and introduces the data. Section 4 presents the empirical strategy and results. Section 5 concludes.

2 Model

We develop a stylized political agency model to understand how hierarchical accountability may shape policymaking behavior.[13] To more easily connect the model with our empirical application to police employment this section uses terminology specific to this particular policy domain. The model, however, can be readily adapted to other settings featuring an indirectly-elected policymaker with asymmetric expertise.

[10] The only other emprirical work on pandering incentives we are aware of is Besley and Payne (2003) for elected judges, and Besley and Coate (2003) for elected regulators. Pandering incentives are not confined to electoral settings. See Dasgupta and Prat (2006) for a model of conformism of fund managers with career concerns, and Panova (2006) for a model of pandering by the media.

[11] Their paper is the only one we are aware of that provides a theoretical model of manager-mayor differences in fiscal policies. Their citizen-candidate model attributes differences in spending not to incentives, but to voters electing different types of council-members.

[12] An exception is the cross-sectional analysis in Baqir (2002) who uses past city government institutions as an instrument for current government institutions. The focus in that paper is on city council size.

[13] The agency approach to political accountability originated with Barro (1973) and Ferejohn (1986). See Besley (2006) for a synthesis of the political agency literature.

Setup. A policy choice x_t needs to be made at time t from the set of policy alternatives $\{0,1\}$. For instance, the policy issue can be police employment, in which case $x_t = 1$ can stand for "high police" and $x_t = 0$ for "low police." The policy's effect depends on the state of the world $s_t \in \{0,1\}$ prevailing in that period. In the illustration state $s_t = 1$ can represent "high crime" and state $s_t = 0$ can represent "low crime." The state is i.i.d. across periods. Let $p = \mathbb{P}\{s_t = 1\}$ denote the probability of the "high crime" state. Assume $p > \frac{1}{2}$.

There are three kinds of players: a voter, an intermediary and a policymaker. The voter gets a unit of payoff when the policy matches the state, zero otherwise: $v(x_t, s_t) = \mathbf{1}\{x_t = s_t\}$. Notice that based on the crime prior p the voter prefers policy $x_t = 1$. Using Maskin and Tirole's (2004) terminology, we say that "high police" is the *popular* policy.

The voter delegates policymaking to a policymaker and delegates policymaker accountability to an intermediary. These two agents are both policy-motivated and office-motivated. Their policy motivation depends on their type, congruent or dissonant: $\theta^I, \theta^P \in \{C, D\}$. A congruent agent has the same preferences as the voter: $u(x_t, s_t|C) = \mathbf{1}\{x_t = s_t\}$. A dissonant agent has preferences opposed to the voter's: $u(x_t, s_t|D) = \mathbf{1}\{x_t = 1 - s_t\}$.[14]

Following Rogoff (1990) we model the notion that more recent agent choices are stronger signals of agent preferences by assuming that agent types are partially persistent.[15] Let η be a preference shock with $\mathbb{P}\{\eta = C\} = 1 - \mathbb{P}\{\eta = D\} = \gamma$. Assume $\gamma > \frac{1}{2}$. Let (η_t^I, η_t^P) be a pair of independent draws from η, independently drawn across periods.

Policymaker and intermediary types are based either on last period's preference shock or the current period's shock in the following way. Types (θ_t^I, θ_t^P) evolve independently according to: $\mathbb{P}\left\{\theta_t^I = \eta_{t-1}^I\right\} = 1 - \mathbb{P}\left\{\theta_t^I = \eta_t^I\right\} = \lambda^I$ and $\mathbb{P}\left\{\theta_t^P = \eta_{t-1}^P\right\} = 1 - \mathbb{P}\left\{\theta_t^P = \eta_t^P\right\} = \lambda^P$, where $0 < \lambda^I, \lambda^P < 1$. This process for types implies that initially intermediary and policymaker types are independent. Correlation between their types can nevertheless develop during the game due to retention decisions made in equilibrium. The agents observe their types but not the underlying preference shocks.

The timing of the game is as follows. Every period $t = 1, 2, ...$ the policymaker observes the state $s_t \in \{0,1\}$ and chooses between the unpopular/popular policy: $x_t \in \{0,1\}$; the intermediary observes (x_t, θ_t^P) and decides whether to fire/keep the policymaker: $y_t \in \{0,1\}$. Intermediary elections take place every other period, say even periods. At election time the voter observes $[s_{t-1}, (x_{t-1}, y_{t-1}), (x_t, y_t)]$ and decides to replace/retain the intermediary: $z_t \in \{0,1\}$. An agent exiting at t is succeeded by an agent (challenger) whose type is a new

[14] The analysis would be similar if we assumed the dissonant type were biased against the popular policy: $u(x_t, s_t|D) = \mathbf{1}\{x_t = 0\}$.

[15] Shi and Svensson (2006) use a similar approach.

draw $\theta^I_{t+1} = \eta^I_{t+1}, \theta^P_{t+1} = \eta^P_{t+1}$ from the distribution η of preferences. An exiting agent cannot run for office again.

The policymaker cares about policies he himself chooses. The intermediary cares about the choices of the policymaker he had a role in bringing into office. Formally, lifetime utility for a policymaker at $t-1$ is $u(x_{t-1}, s_{t-1}|\theta^P_{t-1}) + \sum_{i=t}^{\infty} \beta^{i-t+1} u(x_i, s_i|\theta^P_i) \prod_{j=t}^{i} y_{j-1}$; for an intermediary it is $[u(x_t, s_t|\theta^I_{t-1}) + \beta u(x_{t+1}, s_{t+1}|\theta^I_t)] + \sum_{i=t}^{\infty} \beta^{2i-2t+2} [u(x_{2i-t+2}, s_{2i-t+2}|\theta^I_{2i-t+1}) + \beta u(x_{2i-t+3}, s_{2i-t+3}|\theta^I_{2i-t+2})] \prod_{j=t}^{i} z_{2j-t}$. Here β is a time discount factor, with $0 < \beta < 1$.

We denote by $r \in (0,1)$ the fraction of agents whose office motivation dominates their policy motivation, i.e. when there is a conflict between the two they choose to pursue office. These are referred to as *responsive* agents. The remaining fraction are *ideological* agents who always act on their policy motivation. For simplicity we assume that either both the policymaker and the intermediary are responsive or both are ideological.[16]

We denote by $\tilde{\theta}^I_t = \mathbb{P}\left\{\theta^I_t = C|\mathcal{I}_t\right\}$ and $\tilde{\theta}^P_t = \mathbb{P}\left\{\theta^P_t = C|\mathcal{I}_t\right\}$ the voter's beliefs about intermediary and policymaker types, given the voter's information at time t. Following standard practice, we refer to voter beliefs about an agent's type as that agent's "reputation."

Discussion. In this environment the voter faces two informational problems: incomplete information about agents' policy preferences, i.e. adverse selection, and imperfect information about the policy's effect, i.e. moral hazard.

We use the standard assumption that the policymaker is a policy expert who knows the state and thus which policy is optimal in a given period. The intermediary, on the other hand, is a political expert, in the sense of Gailmard and Jenkins (2009), who knows the policymaker's preferences (type). This assumption seems reasonable in settings where the intermediary has frequent contact with the policymaker. That is the case of a city council since the policymaker they choose, the city manager, has to attend all city council meetings and sometimes committee meetings.

The voter, on the other hand, knows neither if the popular policy is optimal nor the policymaker's preferences. Thus, hierarchical accountability features two forms of the classic delegation tradeoff: by delegating policymaking to the policymaker the voter may benefit from more informed policy choices but may suffer from the policymaker's dissonance; by

[16] One can think of responsive agents as those whose marginal rate of substitution between current and future payoffs approaches zero, or discount factor approaches one, and ideological agents as those whose marginal rate of substitution approaches infinity, or discount factor approaches zero. While in practice the relative strength of the office motivation vs. the policy motivation may be partly endogenous, depending on the costs of rent extraction, for simplicity we assume it is exogenous. That the policymaker and intermediary are similarly motivated is true if the the prestige (ego rent) or salary associated with the office are correlated between the two agents.

delegating policymaker accountability to the intermediary the voter may benefit from more informed firing decisions but may suffer from the intermediary's dissonance.[17]

A key feature of hierarchical accountability is that the indirectly-elected policymaker can be fired between elections. A city manager, for example, serves at the city council's pleasure. His "job tenure is only secure until the next council meeting" (Stillman 1977, p. 664). The model captures this feature by assuming that the intermediary can fire the policymaker during the course of an electoral term.

We model the intensity of the retention motivation by the parameter r measuring the fraction of responsive agents. Retention value can originate in ego rents (Rogoff 1990) such as the power and prestige of the office, or can be due to a high salary (Besley 2004).

The parameter γ measures the degree of agent congruence with the voter. Congruent agents benefit from the voter's preferred policy. Dissonant agents oppose the voter's preferred policy. This assumption seems appropriate if there are interest groups on both sides of a policy issue. For example, public safety advocacy groups pushing for more police vs. police unions protecting their benefits (see, e.g., Rynecki and Morse 1981, Carter and Sapp 1992 on economic benefits as police unions' primary focus in collective bargaining, and Trejo 1991, Valletta 1993 for evidence of positive wage effects and negative employment effects of police unionization in U.S. cities).[18]

The voter leans toward the popular policy because it is optimal with probability $p > \frac{1}{2}$. The parameter p captures the voter's prior about the level of crime. Stucky (2005) reviews the literature on how the public's crime prior (also known as "fear of crime" in the criminology literature) affects popular demand for police.[19]

We assume that the state is i.i.d. across periods. That means that past policy choices do not affect the voter's current crime prior. This seems to be a good approximation if police

[17] The admittedly stark informational asymmetries can be relaxed by assuming that the agents are only relatively more informed compared to the voter.

[18] The parameter γ can be interpreted as the fraction of voters favoring state-matching policies. Thus $\gamma > \frac{1}{2}$ means that a majority has this preference. Then, an entering agent can be thought of as a random draw from the electorate.

[19] Gallup's annual Crime Survey asks the question "Is there more crime in your area than there was a year ago, or less?" Since the survey started in 1973 the percentage of respondents who say "More" exceeded those that say "Less" except for three years 1998, 2000, 2001 (see Gallup 2010). The biennial American National Elections Study (ANES) surveys for 1966-1976 contain "thermometer" questions measuring public sentiment toward social groups. On a scale from 0 to 100, with 50 measuring a "neutral" feeling, policemen's reading is between 75.22-80.46, with an average over the six surveys of 78.17. By comparison, lawyers score 65.38, city/county officials 63.26, and politicians 52.76. From a different angle, an ABC News/*Washington Post* national poll from March 2011, on possible solutions to the current state and local budget crises, finds that popular opposition to cutbacks was "broadest and deepest" for firefighters, teachers, and police officers, with 89 (75), 86 (74), 86 (70) percent, respectively, opposing (strongly opposing) cuts.

have only a short-term effect on crime, i.e., long-term crime is driven by more fundamental forces like economic inequality, economic growth, and race relations.

The parameters r, γ, and p reflect long-term features of the political environment that may differ across cities.[20] In what follows we show that equilibrium outcomes critically depend on these parameters. In the empirical application we provide evidence for these predictions of the model.

Equilibrium. The model described above is a principal-intermediary-agent model with asymmetric information. The voter is the principal and the policymaker is the agent, whereas the intermediary is both a principal (to the policymaker) and an agent (of the voter). We solve for the Perfect Bayesian Equilibrium of the game, namely: (i) policymaker, intermediary and voter strategies (x_t, y_t, z_t) that are sequentially rational, and (ii) voter beliefs $[\tilde{\theta}_t^I(x_t, y_t), \tilde{\theta}_t^P(x_t, y_t)]$ that are consistent with agent strategies. We restrict attention to pure strategies.[21]

A key feature of hierarchical accountability is that it does not permit the voter to fire the policymaker directly. The voter, however, can strategically remove the intermediary on the basis of intermediary actions. To what extent does this affect policymaker behavior?

Proposition 1 *Hierarchical accountability insulates the policymaker pre-election, if $\gamma > p$, creates policymaker pandering incentives pre-election, otherwise, and increases intermediary congruence post-election.*

Proof See the Theory Appendix.

Accountability outcomes in this model are shaped by the voter's behavior under limited information. The voter's future payoff depends on intermediary type because the intermediary can affect policymaker type between the current and the next election. In particular, in the post-election period the intermediary keeps only policymakers of his own type.[22]

The credibility of the voter's strategy thus critically depends on what pre-election actions (x_t, y_t) signal about the incumbent intermediary's preference type θ_t^I. The voter's interpretation of the signal contained in (x_t, y_t) depends on the parameters γ and p. First, the popular policy $x_t = 1$ signals a congruent policymaker since congruent types want the popular policy

[20] For example, the General Social Survey (GSS) measure of fear of crime, based on a national sample of respondents during 1973-2006, had a coefficient of variation of only 0.10.

[21] The assumed process for types (θ_t^I, θ_t^P) makes the environment stationary by "breaking structural links between elections" (Rogoff 1990) thus precluding the need for stronger solution concepts such as Markov Perfect Equilibrium.

[22] This requires that λ^I and λ^P are sufficently close to a half, so that the intermediary's decision is driven by the event that the intermediary's type persists together with the policymaker's, and less by the event that the intermediary's type changes while the policymaker's persists.

more often. Second, keeping the policymaker $y_t = 1$ signals a congruent intermediary *if* the policymaker is popular, i.e., chose the popular policy. However, it may signal different things if the policymaker is unpopular.

There are two cases. (i) If the fraction of congruent unpopular policymakers $\gamma(1-p)$ is larger than the fraction of dissonant unpopular policymakers $(1-\gamma)\,p$, keeping an unpopular policymaker is more likely the action of a congruent intermediary, causing the voter to "retain the intermediary iff he keeps the policymaker." This gives responsive intermediaries incentives to always keep the policymaker. (ii) Conversely, if the fraction of congruent unpopular policymakers is smaller than the fraction of dissonant unpopular policymakers keeping an unpopular policymaker is more likely the action of a dissonant intermediary, causing the voter to "retain the intermediary if and only if he keeps a popular policymaker and fires an unpopular policymaker." This gives responsive intermediaries incentives to "keep the policymaker iff policy popular."[23]

The equilibrium is unique. By the above argument its form depends on how the congruence ratio $\frac{\gamma}{1-\gamma}$ compares to the popularity ratio $\frac{p}{1-p}$. If agent congruence exceeds policy popularity the unique equilibrium is *insulating*: the voter's strategy insulates the policymaker; otherwise, if popularity dominates congruence, the unique equilibrium is *pandering*: the voter transmits pandering incentives through a responsive intermediary.[24]

In equilibrium the voter's strategy not only affects incentives but also performs *intermediary selection*. Since the voter retains only intermediaries with above-average reputation, the voter selects more congruent intermediaries into the post-election period. Post-election average intermediary congruence increases among those that have behaved ideologically. The change in intermediary congruence is given by:

$$\begin{aligned}\bar{\theta}^I_{t+1} - \gamma &= \mathbb{P}\left\{\theta^I_t = \theta^I_{t+1} = \eta^I_t\right\} \sum_{(x_t,y_t)} \mathbb{P}\left\{x_t, y_t\right\} \left[\tilde{\theta}^I_t(x_t, y_t) - \gamma\right] \\ &= \lambda^I(1-\lambda^I)\gamma(1-\gamma)\left[(2\gamma-1)\mathbf{1}\left\{\gamma > p\right\} + (2p-1)\mathbf{1}\left\{\gamma < p\right\}\right] \qquad (1)\end{aligned}$$

where in the summation $(x_t, y_t) \in \{(0,1),(1,1)\}$ in the insulating equilibrium, and $(x_t, y_t) \in \{(0,0),(1,1)\}$ in the pandering equilibrium. Note that $\bar{\theta}^I_{t+1} - \gamma$ is always positive since

[23] These two types of intermediary behavior are consistent with case studies of U.S. city councils that distinguish between council strategies that insulate the manager, termed "blind faith," versus council strategies that transmit popular preferences, termed "political." See Stillman (1977).

[24] While closely related to the concept of populism (Acemoglu, Egorov and Sonin 2011), pandering differs in at least two respects: first, it is not necessarily associated with economic inequality and redistribution, and second, it affects public officials of all ideological stripes, not only those who are naturally inclined to serve the cause of the "common man," e.g., working-class politicians.

$\gamma, p > \frac{1}{2}$. Equation (1) reflects that this type of selection occurs when the intermediary's type persists between pre-election and post-election, $\theta_t^I = \theta_{t+1}^I = \eta_t^I$, which happens with probability $\lambda^I(1-\lambda^I)$. It also captures the notion that in the insulating equilibrium intermediary actions are stronger signals of their type, making selection dependent on intermediary congruence γ, while in the pandering equilibrium policymaker actions are stronger signals of intermediary types, making selection dependent on policy popularity p.

Another type of selection occurs under hierarchical accountability, namely *policymaker selection* by the intermediary. This occurs both between terms, t to $t+1$, just like intermediary selection by the voter, but also within a term, $t+1$ to $t+2$. Policymaker average congruence after selection by an intermediary of reputation $\bar{\theta}_{t+1}^I$ changes by:

$$\begin{aligned} \bar{\theta}_{t+2}^P(\bar{\theta}_{t+1}^I) - \gamma &= \mathbb{P}\left\{\theta_{t+1}^P = \theta_{t+2}^P = \eta_{t+1}^P\right\} \mathbb{P}\left\{y_{t+1} = 1\right\} \left[\frac{\gamma\bar{\theta}_{t+1}^I}{\gamma\bar{\theta}_{t+1}^I + (1-\gamma)\left(1-\bar{\theta}_{t+1}^I\right)} - \gamma\right] \\ &= \lambda^P(1-\lambda^P)\gamma(1-\gamma)(2\bar{\theta}_{t+1}^I - 1) \end{aligned} \tag{2}$$

which is positive since $\bar{\theta}_{t+1}^I > \gamma > \frac{1}{2}$, by equation (1). The expression reflects that within-term selection occurs when the policymaker's type persists between two periods, $\theta_{t+1}^P = \theta_{t+2}^P = \eta_{t+1}^P$, which happens with probability $\lambda^P(1-\lambda^P)$. The term in square brackets represents the increase in the reputation of a policymaker kept by the intermediary.

To summarize, the voter's strategy performs two functions: gives pre-election *incentives* to the intermediary, who in turn gives incentives to the policymaker; and performs post-election *selection* of more congruent intermediaries, who in turn perform within-term selection of policymakers. In terms of qualitative outcomes, an indirectly-elected policymaker may behave either as an insulated bureaucrat or as a pandering politician.[25]

To better understand the voter's ability to exert accountability hierarchically, consider a setting where the voter can hold the policymaker directly accountable. Dropping the intermediary from the model, what are the policymaker's incentives under direct accountability? Moreover, how effectively can the voter select policymakers given that, unlike the intermediary, he does not observe policymaker types?

Proposition 2 *Direct accountability creates pandering incentives pre-election and increases policymaker congruence post-election.*

Proof See the Theory Appendix.

[25] Note that pre-election voter welfare under insulation is γ whereas under pandering it is p. An indirectly-elected policymaker's role thus endogenously varies between that of "politician" and "judge" (in Maskin and Tirole's 2004 terminology) to maximize pre-election voter welfare.

Direct accountability produces pandering incentives at election time because the voter cannot credibly commit to keep an unpopular policymaker. An unpopular policy signals a dissonant policymaker, reducing the incumbent policymaker's reputation below the γ of the average policymaker, no matter how high that is. The voter's only credible strategy then is to "keep the policymaker iff policy popular." This gives responsive policymakers the incentive to pander pre-election and at the same time selects more congruent policymakers into the post-election period.

The voter strategy of keeping only popular policymakers increases policymaker congruence post-election for those that have behaved ideologically. Post-election average policymaker congruence changes by:

$$\begin{aligned} \bar{\theta}^P_{t+1} - \gamma &= \mathbb{P}\left\{\theta^P_t = \theta^P_{t+1} = \eta^P_t\right\} \mathbb{P}\left\{x_t = 1\right\} \left[\frac{\gamma p}{\gamma p + (1-\gamma)(1-p)} - \gamma\right] \\ &= \lambda^P(1-\lambda^P)\gamma(1-\gamma)(2p-1) \end{aligned} \tag{3}$$

which is always positive since $p > \frac{1}{2}$. The term in square brackets represents the increase in the reputation of a policymaker choosing the popular policy.

Notice that hierarchical policymaker selection in equation (2) is more effective than direct selection in equation (3) iff $\bar{\theta}^I_{t+1} > p$. Intuitively, hierarchical selection is more precise than direct selection because of the intermediary's superior information, but may backfire if the intermediary is dissonant. If post-election intermediary reputation $\bar{\theta}^I_{t+1}$ is high enough, then hierarchical selection on net is superior to direct selection.

Neutral Issues. One may argue that another way in which indirectly-elected policymakers differ from directly-elected policymakers is that they have weaker motivations to deliver political patronage to the voter. While patronage could potentially affect popular policies, such as crime fighting or low taxes, it also typically influences *neutral policies* such as in-kind transfers or city jobs that do not elicit a clear popular preference. For example, a large city bureaucracy may indicate a thriving city or may convey waste.

Suppose that $p = \frac{1}{2}$ so that the voter does not prefer either policy a priori. What does the model imply for policymaking on such neutral issues?

Proposition 3 *For neutral policy issues policymaking outcomes do not vary with the accountability form.*

Proof See the Theory Appendix.

In this setup neither policy signals a congruent type. This allows the voter to credibly insulate the policymaker under direct accountability. The voter preferes insulation because

the policymaker is more likely congruent than dissonant. Insulation yields an expected payoff of γ every period, whereas prescribing a particular policy would yield $\frac{1}{2}$ in expectation.[26] Under hierarchical accountability the equilibrium is now always insulating because keeping the policymaker, who is more likely congruent, signals a congruent intermediary. Thus the voter's strategy is to "retain the intermediary iff he keeps policymaker." This result implies that incentives on neutral issues should not differ by accountability form. A patronage motivation, in contrast, would imply that, all else equal, indirectly-elected policymakers deliver less patronage on popular issues as well as on neutral issues.

Policy Differentials. Having solved for the equilibrium of the game we can now derive implications for observable policy outcomes. Here we explore in detail how policy differences between accountability forms vary with the model's parameters r, γ, and p.

Let $\bar{x}_t$ denote the equilibrium frequency of the popular policy in period t. If the policymaker were simply following his preferences the frequency of the popular policy over T periods would be:

$$\bar{x}\left(\gamma\right) = \sum_{t=1}^{T} \left[\gamma s_t + \left(1-\gamma\right)\left(1-s_t\right)\right] \tag{4}$$

which approaches $\gamma p + \left(1-\gamma\right)\left(1-p\right)$ as T becomes large.

Based on Proposition 1 hierarchical accountability is characterized by the following policy frequencies pre-election and post-election:

$$\bar{x}^H_{pre}(r,\gamma,p) = r\left[\bar{x}[\bar{\theta}^P(\gamma)]\mathbf{1}\left\{\gamma > p\right\} + \mathbf{1}\left\{\gamma < p\right\}\right] + (1-r)\bar{x}[\bar{\theta}^P(\bar{\theta}^I)] \tag{5}$$

$$\bar{x}^H_{post}(r,\gamma,p) = r\bar{x}\left(\gamma\right) + (1-r)\bar{x}[\bar{\theta}^P(\gamma)] \tag{6}$$

where $\bar{\theta}^I$ and $\bar{\theta}^P()$ are defined in equations (1) and (2). Notice how selection is reflected both in pre-election and post-election policy choices. Pre-election outcomes are influenced by within-term policymaker selection by the intermediary. Post-election outcomes are influenced by across-terms policymaker selection by the intermediary. The term $\bar{x}[\bar{\theta}^P(\gamma)]$ in equation (5) reflects the fact that when the intermediary is responsive, the voter cannot perform intermediary selection; the term $\bar{x}[\bar{\theta}^P(\bar{\theta}^I)]$ reflects the fact that when the intermediary is ideological, intermediary selection by the voter increases intermediary congruence to $\bar{\theta}^I$, which in turn increases policymaker congruence to $\bar{\theta}^P(\bar{\theta}^I)$.

Based on Proposition 2 direct accountability is characterized by the following policy

[26] Due to the symmetry of the environment, the direct accountability equilibrium is no longer unique. To make the argument, we use the standard equilibrium selection criterion of ex-ente voter welfare.

frequencies pre-election and post-election:

$$\bar{x}^D_{pre}(r,\gamma,p) = r + (1-r)\bar{x}\,(\gamma) \quad (7)$$

$$\bar{x}^D_{post}(r,\gamma,p) = r\bar{x}\,(\gamma) + (1-r)\bar{x}[\bar{\theta}^P(p)] \quad (8)$$

where $\bar{\theta}^P()$ is defined in equation (3). Note that pre-election policy is affected by responsive policymakers' pandering incentives, yielding r, and post-election policy is affected by policymaker selection among those that acted on preferences before the election, yielding $(1-r)\bar{x}[\bar{\theta}^P(p)]$.

The theoretical results in Propositions 1-3 then imply that, either because of incentive or selection differences, we should expect policy differences between accountability forms. They also identify mechanisms that explain these differences. We state the key implications in the following hypotheses. They emerge directly from equations (5)-(8).[27]

First, a basic insight of the model is that preference signaling is only effective on popular policy issues. This translates into policy differences on popular issues that are not present on neutral issues.

H1 *A policy differential* $\Delta_t = \bar{x}^H_t - \bar{x}^D_t$ *exists between hierarchical and direct accountability for popular issues. No policy differential exists for neutral issues.*

The model also demonstrates that incentives and selection have opposing implications for the popular policy differential, both on average and at different points within a term. Let $\bar{\Delta} = avg(\Delta_{pre}, \Delta_{post})$ denote an average policy differential in a term.

H2 *Incentive differences generate* $\bar{\Delta}\,|_{r=1} < 0$, *if* $\gamma > p$, *and* $\bar{\Delta}\,|_{r=1} = 0$, *if* $\gamma < p$.

H3 *Selection differences generate* $\bar{\Delta}\,|_{r=0} > 0$, *if* $\gamma > p$, *and* $\bar{\Delta}\,|_{r=0} < 0$, *if* $\gamma < p$.

If policy is driven by incentives ($r = 1$) then high policymaker congruence and a low crime prior cause the voter to hierarchically insulate the policymaker. This reduces the prevalence of the popular policy relative to direct accountability, generating a negative differential. If policy is driven by selection ($r = 0$) the differential is due to hierarchical selection being more precise than direct selection, but also with potential to backfire. If intermediary congruence is high then hierarchical selection increases policymaker congruence more, resulting in a positive differential. In contrast, if intermediary congruence is low equilibrium policymaker congruence is also low, resulting in a negative differential.[28]

The incentive and selection effects also work differently pre-election and post-election.

[27] Comparative statics within each accountability form are generally ambiguous and are relegated to the Theory Appendix.

[28] Strictly speaking, when $r = 1$ the equilibrium is no longer unique. However, one could argue that the most plausible one is the limit of the sequence of unique equilibria as r approaches 1.

Thus the policy differential depends on the timing relative to the election.

H4 *Incentives differences generate* $\Delta_{pre} \mid_{r=1} < \Delta_{post} \mid_{r=1}$.

H5 *Selection differences generate* $\Delta_{pre} \mid_{r=0} > \Delta_{post} \mid_{r=0}$.

Intuitively, hierarchical accountability restrains pandering incentives, reducing the frequency of the popular policy pre-election. Post-election there are no incentives under either accountability form. Thus, if incentives dominate ($r = 1$) they should reflect themselves in a negative pre-election differential. Hierarchical policymaker selection is more effective pre-election than it is post-election, because it follows after intermediary selection. The opposite is true of direct selection, where policymaker selection occurs only post-election. Thus, on net, if selection drives behavior ($r = 0$) the policy differential is larger pre-election.

3 Empirical Application

Our empirical application examines popular (police officer employment) and neutral (police civilian employment) policy issues in U.S. cities where chief executives are either indirectly-elected (managers) or directly-elected (mayors). This section overviews historical facts that motivate our empirical strategies and discusses measures of key theoretical variables.

Historical Background. Early U.S. city executives were appointed by state governors or city councils. After major cities like Boston and St. Louis started to popularly elect their mayors in 1822, popular election became nearly universal.[29] By the end of the 19th century city politics had became dominated by "machine" politicians drawing their support from workers and immigrants, and often using illegal means to stay in office. This crisis in city hall accountability sparked an urban reform movement that coincided with broader social reforms taking place during the Progressive Era (1890-1930). Initially reformers targeted cities' electoral systems: from district-based partisan to at-large non-partisan elections. The goal was to dilute the power of minorities and parties on which the machines thrived.

The Progressives, who wanted not only cleaner government but also more efficient government, were later inspired by the "efficiency movement"'s success with the new corporate form and sought to apply the model to city governance. Manager government, first experimented with in the small city of Staunton, Virginia in 1908, attained broad recognition when the National Municipal League made it their recommended government form in the

[29] Historians note that the trend toward direct elections coincided with the explosive growth of cities and the emergence of a mass electorate (Judd and Swanstrom 2010).

1915 edition of the *Model City Charter*. During the 1910s and 1920s most major cities, including New York City, were debating switching to a manager charter.

Despite strong opposition from incumbent mayors and party bosses manager government advanced steadily.[30] Knoke (1982) attributes successful switches to manager to strong business interests, weak unions, high population mobility, small immigrant population, and small city size. As these factors are likely to persist and independently affect policy today, empirically identifying the effect of manager government seems challenging.

Manager government, however, also filled a growing need for technical expertise at the top of city government, a need felt more acutely in times of crisis. The technological boom of the early 20th century had created a demand for technologically-intensive public services, such as paved roads, streetlights, and sewers. The new infrastructure often amplified a crisis. Dayton, Ohio provides an illustration. In March 1913 after days of heavy rainfall the Great Miami River overflowed the city's levees causing a flood that destroyed over 20,000 homes. In the immediate aftermath local leaders sought to rebuild the flood control system with a large public works campaign. Expediency dictated the adoption of a manager charter so that an engineer could be appointed to lead the reconstruction effort.[31]

After the Ohio Flood subsequent crises such as the Great Mississippi Flood of 1927 and the Northeast Flood of 1936 resulted in substantial losses across multiple local jurisdictions and helped swing the balance toward federal takeover of flood control from local authorities. In 1936 Congress passed the Flood Control Act (FCA) that assigned responsibility for flood prevention and management to the Army Corps of Engineers. The hundreds of miles of levees and 375 major reservoirs constructed by the Army Corps of Engineers after 1936 significantly weakened the link between heavy precipitation and the incidence of floods (Arnold 1988).

The connections between natural hazards, infrastructure crises and public demand for technical expertise, combined with the federal takeover of flood control in 1936, suggest an instrumental variable identification strategy: using precipitation shocks during the local flood control era (1900-1936), when they were more likely to have triggered an infrastructure crisis, to isolate exogenous variation in manager government.[32]

[30] A number of 87 cities adopted a manager charter between 1913-1918, another 153 between 1918-1923, and 84 more between 1923-1928 (Judd and Swanstrom 2010).

[31] After Dayton's first choice for the position – George Washington Goethals, the engineer overseeing the Panama Canal – declined, the engineer Henry M. Waite became the city's first manager.

[32] We subject our instrument to formal validity and exogeneity tests in the next section. Here we note that trends in city managers' educational backgrounds provide additional support for our identification strategy. In the first ten years 95 percent of city managers were engineers. Twenty years later 75 percent had an engineering degree (Stone, Price, and Stone 1944, p. 265). The shift in flood control coincided with a dramatic change in city manager expertise. By 1971 only 33 percent had degrees in engineering. Stillman

Data. Our sample consists of all U.S. cities with 1900 Census population over 17,500 residents. This sample selection criterion is not affected by government form choice, since manager reforms do not occur until the small city of Staunton, Virginia (pop. 7,289 in 1900) starts experimenting with it in 1908. After dropping Washington DC, since it has a federally-appointed city government until 1973, a number of 248 present-day cities satisfy this criterion.[33] The sample period for our panel is 1960-2000.

City government form is collected by the International City/County Management Association (ICMA) through surveys sent every five years to municipal officials. The survey results are reported in their *Municipal Year Book*.[34] We point out two features of government form variation in our sample. First, the maps in Figures A1 and A2 display little geographic clustering in manager government. Second, as Figure A3 shows, most manager charter adoptions in our sample occur before 1960. In fact, the majority of adoptions occurred before the federal takeover of flood control in 1936, and only ten occurred after 1960.

We measure local policymaking behavior using police employment. This policy area has the attractive feature that it allows us to distinguish popular from neutral policy issues by disaggregating police employment into officer and civilian employees, according to the distinction made in the FBI's *Uniform Crime Reports*. Police per capita displays wide variation both across and within cities.

The adoption of a manager charter during the Progressive Era often followed electoral reforms. To separate the effect of manager government from at-large and non-partisan electoral reforms, we include these institutions in the analysis as controls. The data comes from the *Municipal Year Book*. We also include a measure of early civil service reform, i.e., as of 1937, available in *Civil Service Agencies in the U.S.*[35]

The incentive effect we have characterized theoretically works through two political mechanisms (retention motivation and policymaker congruence) and two informational mechanisms (voter crime prior and election proximity). We measure the strength of the retention motivation with policymaker salary, available in the ICMA *Salaries of Municipal Officials*, although only after 1992. We measure policymaker congruence using the fraction of policemen that are members of a nationally-affiliated union or employee association, available in

(1974, p. 89) notes: "The turbulence of the cities has increasingly caused managers to turn to sociologists and political scientists for answers to complex urban issues."

[33] Two intervening annexations and one merger slightly alter the sample: Pittsburgh, PA annexed Allegheny, PA in 1907; Omaha, NE annexed South Omaha, NE in 1915; and West Hoboken, NJ merged with Union Hill, NJ to form Union City, NJ in 1925.

[34] If government form changes between survey years, we date it using newspaper sources or city charters.

[35] With the introduction of Social Security in 1939 essentially all local governments had set up a civil service agency as it was a federal condition for disbursing Social Security payments to city residents.

the 1968 edition of the *Municipal Year Book*. For informational mechanisms we use sales of local newspapers, from George and Waldfogel (2006), as a measure of voter crime priors and election years as an indicator of pre-election periods.[36] City elections data are not available on a systematic basis in any one source and discrepancies among existing sources are prevalent. To reduce errors we corroborated multiple sources (see Data Appendix).

We construct our instrumental variable using 1900-2000 weather reports from the U.S. Historical Climatology Network's *Daily Temperature, Precipitation, and Snow Data*. This dataset contains daily readings for rainfall, snowfall, and temperature extremes collected from weather stations throughout the U.S. We match a city to the closest weather station based on geographic coordinates.[37] Our main precipitation measure is the yearly sum of rainfall and density-adjusted snowfall at the station level.[38] We define an extreme precipitation event as a year when precipitation exceeds the 99th percentile of the national 20th century yearly precipitation distribution. Our cross-sectional measure of precipitation shocks for a given city is the frequency of extreme precipitation events in a given period, referred to below as *precipitation shocks*. For instance, precipitation shocks in the local flood control period (1900-1936) are referred to as *LFC precipitation shocks*.

In addition to these key variables, we work with an extensive set of geographic, demographic, economic, crime and policymaker characteristics controls. The Data Appendix provides the complete list of variables, with details about their sources and measurement.

Table 1 reports descriptive statistics for our major variables, overall and by government form. Panel A shows that manager governments employ on average about 19% fewer officers per capita than mayor governments, and virtually the same number of civilians per capita. Interestingly, Panel E suggests that before the advent of the manager plan there were no major differences in either type of police employment. The other statistically significant differences are as follows: manager cities have on average longer distance to nearest river, higher education, higher property and total crime (Panel B), higher incidence of Progressive electoral institutions (Panel C), and higher policymaker salary (Panel D).[39]

[36] Heath and Gilbert (1996) review research on the link between newspaper consumption and popular fear of crime. More direct fear of crime measures, e.g. GSS, are survey-based and cannot be aggregated up to a reasonably large number of cities.

[37] The U.S. has 126 weather stations reporting in 1900. The median distance to the closest weather station for our sample cities is 47 miles. As the opening of new stations could be related to changes in local weather or local economy we keep every city matched to the same station over time.

[38] We adjust snowfall for water density by dividing it by ten, as suggested by the U.S. Department of Agriculture. See http://www.ak.nrcs.usda.gov/snow/data.

[39] If police reduces crime, our model implies higher crime under manager government. While Table 1 appears to contradict this potential implication in the case of violent crime, Table A6 in the Appendix shows that after controlling for year effects, geographic and demographic factors the correlation is positive.

Figure A4 provides a first look at how manager government affected police employment historically. Manager governments employ fewer officers from 1960 onward, but not before; the same cannot be said for civilians. In the bottom panel we divide cities based on whether they experienced LFC precipitation shocks. Cities hit by manager-inducing shocks have lower officer employment after 1960 though the difference attenuates over time.[40] Again, civilian employment does not follow this same pattern.

4 Empirical Strategy and Results

We start by estimating a baseline model, study its robustness and sensitivity, and then explore evidence for the political and informational mechanisms of the theory model.

Ordinary Least Squares. We first estimate a simple model of the form:

$$\log(Police_{i,t}) = \beta_1 Manager_{i,t} + \beta_2 X_{i,t} + \tau_t + \epsilon_{i,t} \tag{9}$$

where $Police_{i,t}$ is either officer or civilian employment per capita for city i in year t, $Manager_{i,t}$ is a dummy variable indicating manager form of city government, $X_{i,t}$ is a set of controls, τ_t's are year fixed effects, and $\epsilon_{i,t}$ is the error term. The coefficient β_1 measures the conditional difference in mean police employment between manager and mayor cities. According to hypothesis (H2) if policymaking is driven by incentives, $\beta_1 < 0$ for officer employment. According to (H1), $\beta_1 = 0$ for civilian employment.

This model allows us to account for measurable city geographic, demographic, economic, and political characteristics, as well as policymaker characteristics, and to control for national trends affecting local police employment. As government form changes infrequently during 1960-2000 and the observations are unlikely to be independent within a city we cluster the standard errors at the city level.[41]

Table 2 presents OLS estimates of β_1. Accounting only for year effects column (1) shows that manager governments employ significantly fewer officers. We next add geographic and demographic controls as voter preferences could affect both government form and policy. For example, Van Weelden and Morelli (2011) argue that pandering can depend on how divisive an issue is for the electorate; the distribution of voter preferences may also affect the choice of institutions (Aghion, Alesina, and Trebbi 2004). Adding controls for geography

[40] Federal grants to fund police hiring through the Community Oriented Police Services (COPS) program established by Congress in 1994 may have reduced local discretion over police employment.

[41] See Table 4 Panel B for alternative standard error definitions.

and population characteristics in columns (2) and (3) does not alter the sign of the point estimate, and it remains statistically significant at the 5% level.

Manager government may be related to other institutional and political factors that could affect policy. The descriptive statistics in Table 1 reflect the historical fact that the manager charter reform often came on the heels of other urban reforms: non-partisan ballots, at-large elections, and civil service rules. The literature has sometimes packaged these reforms together with manager reform, distinguishing only between "traditional" and "reformed" cities (Stucky 2005). Manager cities may also differ in police union strength and media penetration, factors that according to the theoretical model affect policymaker preferences and behavior. Adding these institutional and political controls in column (4) does little to alter the point estimate, although statistical significance decreases.[42]

Finally, column (5) shows that the officer differential is robust to controls for policymaker salary, gender, and race. Salary could affect policymaker retention motivation and skill.[43] According to the summary statistics in Table 1 Panel D managers earn higher salaries than mayors, at least in the decade of our sample for which salary data is available. However, policymaker gender and race are not correlated with government form in our sample.[44]

Since the conceptual source of the officer differential is an informational distortion, it is useful to compare its magnitude with the impact of information on government policy from prior work. For instance, Stromberg's (2004) estimated 0.201 elasticity of federal unemployment relief spending with respect to radio penetration (see his Table II column IV) would imply a 20.1% upper bound, in absolute value, on the officer differential, corresponding to a change from zero radio penetration to full penetration. All the estimates of the officer differential in Table 2 are below this upper bound. Our -8% point estimate of the officer differential in column (2) is equivalent to about 7% of police department and 0.5% of city government spending respectively.[45]

[42] City-level police unionization data is very limited. The *Municipal Year Book* reports the presence/absence of a police union in its 1961 and 1962 editions, and the fraction of unionized police employees in its 1968 edition. We choose the latter variable since it is more informative.

[43] Ferraz and Finan (2011) and Gagliarducci and Nannicini (2012) find that higher salaries increase the productivity of Brazilian city council members and Italian mayors by attracting more competent types. Salary may be endogenous to policy outcomes. For example, Enikopolov (2011) finds that growing cities reward managers with a higher salary. In Table A7 of the Appendix we find little evidence that policymakers who reduce crime are rewarded with higher salaries.

[44] Gender and race have been shown to affect policy in local government, e.g., Beaman, Chattopadhyay, Duflo, Pande and Topalova (2009), Nye, Rainer and Stratmann (2010), Ferreira and Gyourko (2011).

[45] Police departments spend an average of $105,515 (2000$) per officer during our sample period. The -8% officer differential translates into (2.07/1,000)×214,647×0.08, or 35.55 officers. Thus, the implied difference in police department spending between the two government forms is $3,751,058. This represents about 7% of police department spending (mean = $56,328,770) and 0.5% of city spending (mean = $683,043,100).

The second half of Table 2 changes the dependent variable from police officers to police civilians. We find little evidence of a similar statistically significant relationship between government form and police civilians. The point estimates in columns (6)-(10) are positive, closer to zero, and not statistically significant. While the lower number of officers in manager cities may be consistent with managers' weaker patronage motivations (Enikolopov 2010) the lack of a similar pattern for civilians seems to belie this explanation. In fact, if the educational requirements and background checks for hiring a police officer are stricter than for a civilian any patronage effect should be more pronounced in civilian employment, all else equal, e.g., labor supply for these positions, state mandates of minimum forensic staff. The officer and civilian estimates are consistent with our theoretical model's predicted differences in pandering incentives, which should be present in popular policy issues, such as police officers, but absent in neutral policy issues, such as police civilians.

Instrumental Variables. While the results thus far show clear evidence of a police officer differential, OLS estimates may still be biased by reverse causality. Despite the infrequency of actual changes in city institutions (e.g., 0.8% of 1,420 U.S. cities had changed their government form between 1980-90 according to Table 1 in Baqir 2002) city charters are endogenous by virtue of being subject to revision by popular referendum. Measurement error is another potential source of bias in OLS estimates leading to attenuation. As manager cities typically have an honorary mayor, city clerks in these cities sometimes mistakenly report a mayor form of government on ICMA survey forms (see Coate and Knight 2011).

To address concerns with bias in the OLS estimates we develop an IV approach. Our strategy is based on the observation that infrastructure crises often triggered early 20th century switches to manager government because they facilitated the ascension of engineers into top executive office. Floods caused by extreme precipitation were one such crisis. The demand for engineering-trained city executives was stronger when flood control was a local responsibility, i.e., before the federal government establishes the Army Corps of Engineers in 1936.[46] We thus instrument for manager government using the frequency of precipitation shocks in the local flood control era, *LFC precipitation shocks*.[47]

Our identification strategy requires that, conditional on typical local precipitation patterns: (i) cities hit by LFC precipitation shocks have the same average unobserved charac-

[46] Table A1 in the Appendix shows that the relationship between extreme precipitation and flood incidence is markedly stronger in the local flood control period (1900-1936).

[47] In a related IV strategy using country-level data Bruckner and Ciccone (2011) exploit the fact that in non-democratic societies the cost of popular opposition to an authoritarian regime is lower during times of economic distress, rendering negative rainfall shocks democracy biased.

teristics as spared cities, and (ii) LFC precipitation shocks affect police employment decades later only through their effect on government form. In the Appendix we provide supportive evidence for these identifying assumptions.[48]

Table 3 presents IV estimates of the employment differentials controlling for typical local precipitation patterns.[49] In columns (1) and (3) the only additional controls are year effects. In columns (2) and (4) we use within-Census-division variation and control for geographic factors. The first stage shows a strong relationship between LFC precipitation shocks and present-day manager government: the F-statistic exceeds the critical value of 10 below which finite-sample weak-instrument bias could be a concern (Bound, Jaeger and Baker 1995). In the second stage we find significantly lower officer employment in manager cities, and mixed evidence of a civilian employment differential. The larger point estimates relative to the baseline OLS results suggest that measurement error in government form might be present in our sample (Coate and Knight 2011). Overall the IV results uphold the substantive conclusions derived from the OLS estimates.[50]

Whether the identified effects have external validity is worth discussing. If the costs of changing government form are heterogeneous, the identified effects will be local to a subset of cities and potentially different from the population-wide treatment effect. For example, Acemoglu, Robinson, and Torvik (2011) argue that voters dismantle exogenously imposed checks and balances when politician rents are low and special interests are strong. One way to explore this issue is to examine how sensitive the IV estimates are to changes in instrument construction. If the effects of government form are heterogeneous and highly specific to cities induced to change government form by the specific instrument we constructed, alternative

[48] In support of instrument validity and the exclusion restriction we find that: (i) LFC precipitation shocks are not correlated with early city characteristics (see Table A2); (ii.a) in contrast to LFC precipitation shocks, federal flood control (FFC) precipitation shocks and LFC temperature shocks are not related to police employment today (see Table A3); and (ii.b) LFC precipitation shocks are not related to other present-day city institutions (see Table A4).

[49] Formally, the first stage model is:

$$\begin{aligned} Manager_{i,t} &= \alpha_1 LFC\_Precipitation\_Shocks_i + \alpha_2 Century\_Precipitation\_Shocks_i + \\ &+\alpha_3 Median\_Precipitation_i + \alpha_4 X_{i,t} + \tau_t + \epsilon_{i,t} \end{aligned} \quad (10)$$

where $LFC\_Precipitation\_Shocks_i$ is the frequency of precipitation shocks in the local flood control era for city i, $Century\_Precipitation\_Shocks_i$ is the frequency of precipitation shocks during the 20th century, $Median\_Precipitation_i$ is median annual precipitation during the 20th century, and the remaining variables are from equation (9). We include $Century\_Precipitation\_Shocks_i$ to make our exclusion restriction credible as previous research has found evidence that climate affects economic growth (Dell, Jones and Olken 2008), crime (Jacob, Lefgren and Moretti 2007), conflict (Miguel, Satyanath and Sergenti 2004), and the origins of trust (Durante 2010).

[50] Baqir (2002) and Whalley (2012) also find little evidence that institutional endogeneity contaminates OLS results in the context of U.S. local governments.

instrument definitions would likely change the magnitude of the estimated effects. Table A5 in the Appendix shows that the IV results are robust to alternative instrument definitions, strengthening their external validity.[51]

Robustness. Table 4 reports sensitivity checks for the parsimonious OLS and IV specifications, i.e., the model that does not include geography and Census division fixed effects. Panel A checks the sensitivity of our results to minor sample alterations: excluding extremely large/small cities, dropping dependent variable outliers, dropping Census divisions with few observations, excluding post-1994 years, when the federal government intervened more forcefully in local police employment through COPS grants, dropping cities with the most years of missing precipitation data, and dropping cities far away from weather stations. Overall, the OLS and the IV results maintain their prior patterns.

Table 4 Panel B varies the inference procedure. We examine three sets of alternative standard errors: clustering on both city and year, clustering on weather station (as this is the unit of observation for the weather variables), and accounting for spatial correlation (Conley 1999).[52] While the OLS estimates are unchanged for both officers and civilians, and the IV estimates for officers are stable, the IV estimates for civilians remain positive and sometimes lose their statistical significance, failing to support a negative civilian employment differential similar to the officer differential.

Thus far we have scaled the number of police employees by population, however a more relevant measure may be the crime level if large cities have more crime. A high officer-crime ratio may also better capture deviations from policy optimality, by providing a measure of "excess police." Table 4 Panel C presents estimates with employment scaled by three different crime measures. In both OLS and IV specifications manager cities have fewer officers per crime than mayor cities (though scaling by violent crime leads to imprecise estimates), but not fewer civilians per crime.

Political and Informational Mechanisms. Across a range of approaches and measures we have found that manager cities employ fewer police officers, but not civilians. This finding is consistent with hierarchical incentives being less linked to popular opinion (see

[51] An additional external validity question is whether the identified effects apply to policy issues other than police employment. Identifying other local policy issues susceptible to pandering incentives is an interesting question for future research.

[52] To implement Conley standard errors we allow for a spatial dependence of up to 0.5 degrees latitude and longitude, which corresponds to about 65 miles, quite close to the 58 miles mean distance of a city from the nearest weather station. We do not include year effects in this model as the unbalanced nature of the panel impedes the estimation of the Conley procedure. This slight change accounts for the difference between the point estimates in row (9) and previous ones.

H2). However, according to our theoretical model, hierarchical selection can also give rise to a negative policy differential. This happens for low-congruence high-prior cities (see H3). To shed further light on whether the results thus far are driven by incentives or selection we examine how the policy differential depends on these factors.

In Tables 5 and 6 we add to equation (9) an interaction of $Manager_{i,t}$ with each theoretically characterized factor. We first examine whether the negative officer differential becomes larger in cities with stronger incentives, using policymaker salary as a proxy for retention motivation. Table 5 columns (1)-(3) show that cities with high policymaker salary have a larger negative officer differential. A similar pattern is not present for the civilian differential. These results are consistent with an incentive effect dominating any selection effect.

To further distinguish between the two effects, we explore the differential's dependence on congruence and popularity. According to the theoretical model, the negative popular policy differential increases with policymaker congruence and falls with police popularity when incentives are at play (see H2). Conversely, when selection drives behavior (see H3) the popular policy differential becomes positive with increasing congruence and becomes negative with increasing crime prior.

We test for these predictions in Table 6, using low police unionization as a measure of policymaker congruence and high newspaper sales as a measure of a high crime prior. The point estimates in columns (1)-(3) further support an incentive effect. Low police unionization increases the negative officer differential by between 11 and 14 percentage points. High newspaper sales reduce the negative officer differential by between 4 and 17 percentage points, although only one estimate is statistically significant. Similar patterns are not present for police civilians in columns (4)-(6). Thus Table 6 provides further support for the incentives-based hypothesis in (H2).

Electoral Effects. The theoretical model also predicts that pandering incentives are strongest pre-election because the voter relies more on recent information about policy choices. We measure election proximity by coding the year before the election date as an election year.[53] To estimate electoral effects we add to equation (9) the election indicator $Election_{i,t}$ and the interaction $Manager_{i,t} \times Election_{i,t}$, as well as city fixed effects. In this model $\beta_{Manager}$ measures the manager-mayor policy differential in off-election years, while $\beta_{Manager} + \beta_{Manager \times Election}$ measures the manager-mayor policy differential in election years. Incentives-based hypothesis (H4) states that the election year differential is smaller than the

[53]Election dates are set in the city charter; special elections outside the regular election cycle, say to replace a mayor resigning before the end of the term, are infrequent and we ignore them. For manager governments we use council election dates.

off-election year differential, which means $\beta_{Manager \times Election} < 0$. Selection-based hypothesis (H5) makes the reverse prediction.

Table 7 column (1) shows that mayors employ 0.6% more officers in election years, suggesting pandering ahead of an election, although the estimate is statistically weak. To increase precision columns (2)-(4) restrict the sample to the subset of 174 cities with population over 50,000 in 1960.[54] The coefficient pattern from column (1) reappears in columns (2) and (3) and is now also statistically significant at conventional levels. The further addition of a policymaker gender control in column (4) has little effect on the results. The interaction coefficient is negative and highly statistically significant, lending support to an incentive effect (see H4) as opposed to selection effect (see H5). The point estimates for civilians in columns (5)-(8) are all closer to zero and never statistically significant. Overall these results also imply that mayor cities display an electoral cycle in officer employment that is not present in the average manager city.[55]

5 Conclusion

This paper sheds light on the effects of hierarchical accountability, a prevalent and increasingly common accountability form in governments such as counties, cities and school districts. Our theory uses a principal-intermediary-agent model where informational asymmetries play a central role. The theoretical model shows that under hierarchical accountability policymaker insulation, i.e., a policymaker following his policy expertise, can be an equilibrium phenomenon. This happens when policymaker congruence is high or public opinion is weak. Otherwise, the policymaker has pandering incentives, i.e., follows public opinion regardless of his own information.

The theoretical model guides our empirical analysis of U.S. city managers in a salient area of local policy, law enforcement. The estimates for the main institutional effect and its theoretical mechanisms support our agency model of incentives and survive a number of robustness checks, including possible endogeneity of city government form.

Our analysis raises a number of theoretical and empirical issues that can be addressed in further work. Our model emphasizes the importance of informational asymmetries, yet

[54] Levitt (1997) argues that electoral cycles should be more pronounced in larger cities. He exploits the correlation between election years and police hiring in a panel of 59 large U.S. cities with directly-elected mayors to identify the effect of police on crime. In a follow-up analysis McCrary (2002) uses the same sample but recodes election dates.

[55] In unreported analysis we have also estimated models with police *hiring*, measured as $\Delta \log(Police_{i,t})$, rather than *employment*, as the dependent variable. The results are stronger and available upon request.

for simplicity information is assumed exogenous to the institutional environment. One can imagine that voter and policymaker motivations in acquiring information depend on the accountability form. On the one hand, the voter might be less interested in acquiring information if he cannot directly control the policymaker. On the other hand, an indirectly-elected policymaker, who enjoys more discretion, has an incentive to acquire more policy information. Another extension would be to introduce policymaker-intermediary collusion, which contract theory models suggest may weaken incentives. Empirically, it would be interesting to test the model's predictions in alternative governmental environments, such as county executives or central bank governors, where popular policies take different forms. Another avenue would be to test for additional implications of the model, for instance how voter and intermediary strategies adjust to the political and informational environment in order to optimally incentivize an indirectly-elected policymaker.

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TABLE 1: Descriptive Statistics, By City Government Form

Sample:	Full	Manager Cities	Mayor Cities	Difference t-stat [p-value]
	(1)	(2)	(3)	(4)
Panel A: Police Employment (per 1,000), 1960-2000				
Officers Per Capita	2.07	1.85	2.20	-4.41
	(0.97)	(0.55)	(1.12)	[0.000]
Civilians Per Capita	0.37	0.38	0.37	0.31
	(0.32)	(0.24)	(0.36)	[0.760]
Panel B: City Geographic, Demographic, Economic, and Crime Characteristics, 1960-2000				
Distance to Nearest River	32	40	28	2.01
	(42)	(55)	(31)	[0.046]
Population	214,647	146,198	252,521	-1.79
	(584,486)	(184,417)	(712,626)	[0.074]
Fraction Non-White	0.20	0.19	0.20	-0.13
	(0.18)	(0.15)	(0.20)	[0.900]
Fraction College Graduate	0.13	0.15	0.13	2.34
	(0.08)	(0.08)	(0.08)	[0.020]
Household Income	28,387	28,653	28,241	0.70
	(7,493)	(7,477)	(7,498)	[0.487]
Violent Crime Rate	3.76	3.35	4.00	-1.71
(1975-2000)	(3.70)	(2.82)	(4.09)	[0.089]
Property Crime Rate	77.57	85.36	73.21	3.21
(1975-2000)	(38.06)	(35.94)	(39.03)	[0.001]
Total Index Crime Rate	81.33	88.71	77.20	2.83
(1975-2000)	(40.60)	(36.86)	(42.00)	[0.005]
Panel C: City Government Characteristics				
Non-Partisan Elections 1960	0.65	0.85	0.53	6.27
	(0.48)	(0.35)	(0.50)	[0.000]
Fraction At-Large Seats 1960	0.65	0.83	0.55	5.86
	(0.42)	(0.34)	(0.43)	[0.000]
Early Civil Service	0.70	0.69	0.70	-0.14
	(0.46)	(0.46)	(0.46)	[0.886]
Low Police Unionization	0.60	0.52	0.64	-1.85
in 1968	(0.49)	(0.50)	(0.48)	[0.066]
High Local Newspaper Sales	0.50	0.55	0.47	1.22
over 1990-2000	(0.50)	(0.50)	(0.50)	[0.224]
City Government Spending	2.17	2.11	2.21	-0.60
(1972-2000)	(2.65)	(2.58)	(2.68)	[0.548]
Panel D: Policymaker Characteristics				
Annual Salary	81,210	100,096	66,687	9.14
(1992-1993,1995-2000)	(32,125)	(27,760)	(27,360)	[0.000]

White	0.78	0.80	0.77	0.63
(1992-1993,1995-2000)	(0.41)	(0.40)	(0.42)	[0.526]
Male	0.85	0.88	0.84	1.12
(1992-1993,1995-2000)	(0.35)	(0.33)	(0.37)	[0.262]
Male (name-based imputation)	0.93	0.92	0.93	-0.76
	(0.26)	(0.27)	(0.25)	[0.450]
Panel E: Police Department Characteristics (per 1,000), as of 1900				
Officers Per Capita	1.10	1.10	1.10	-0.08
	(0.42)	(0.34)	(0.46)	[0.935]
Civilians Per Capita	0.07	0.08	0.06	1.79
	(0.09)	(0.11)	(0.06)	[0.074]
Arrests Per Capita	0.06	0.07	0.05	2.30
	(0.05)	(0.05)	(0.04)	[0.022]
Number of Observations	10,168	3,622	6,546	

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of largest 248 self-governing cities in the United States in 1900. Each entry in columns (1) - (3) presents the mean for the indicated variable, with the standard deviation in parentheses. The entries in column (4) are test statistics (and p-values in brackets) for the hypothesis that the difference between the variables in columns (2) and (3) is zero, using standard errors clustered at the city level. The number of observations reflects the maximum sample size across all the reported variables. All monetary variables are expressed in 2000$.

TABLE 2: City Government Form and Police Employment: OLS Estimates

Dependent Variable =	Log (Officers Per Capita)					Log (Civilians Per Capita)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Manager	-0.16***	-0.08**	-0.06**	-0.05*	-0.07**	0.09	0.02	0.05	0.00	0.03
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.07)	(0.07)	(0.06)	(0.07)	(0.08)
Additional Controls:										
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography & Division	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Demographic	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Institutional & Political	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Policymaker	No	No	No	No	Yes	No	No	No	No	Yes
Sample Years	Full	Full	Full	Full	1992+	Full	Full	Full	Full	1992+
Number of Observations	9,974	9,974	9,974	9,974	1,164	9,850	9,850	9,850	9,850	1,156
Number of Clusters	248	248	248	248	236	248	248	248	248	235

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix, except for columns (5) and (10) where the sample is 1992-1993, 1995-2000. The unit of observation is a city-year for the sample of the largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. The main entries in the columns (1)-(10) report an OLS estimate of β_1 from equation (9) in the text. Standard errors clustered at the city level reported in parentheses. The models in columns (1) and (6) do not include any additional controls. The models in columns (2) and (7) also include the following geographic controls: *elevation minimum*, *elevation maximum, latitude, longitude, a latitude-longitude interaction, distance to nearest river*, *presence of large river*, *presence of swamp*, *located on the coast*, *percentage of clay in the soil*, and Census division fixed effects. The models in columns (3) and (8) also include the following demographic controls: *population, fraction of population non-white, fraction of population college graduate*, and *median household income.* The models in columns (4) and (9) also include controls for *Non-Partisan Elections 1960*, *Fraction At-Large Seats 1960*, *Early Civil Service*, *Low Police Unionization 1968*, and *High Local Newspaper Sales 1990-2000.* The models in columns (5) and (10) also control for *log(Policymaker Salary)*, *Policymaker Male,* and *Policymaker White.* * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE 3: City Government Form and Police Employment: IV Estimates

Dependent Variable =	Log (Officers Per Capita)		Log (Civilians Per Capita)	
	(1)	(2)	(3)	(4)
	Panel A: Second Stage *Dependent Variable = Log (Police Employment Per Capita)*			
Manager	-0.28**	-0.50**	0.69**	0.19
	(0.14)	(0.21)	(0.35)	(0.43)
Century Precipitation Shocks	-0.27	-1.78	7.11***	2.23
	(0.87)	(1.54)	(1.82)	(2.36)
Median Precipitation 1900-2000	0.55**	0.38	-1.41**	-1.41
	(0.22)	(0.61)	(0.60)	(1.12)
	Panel B : First Stage *Dependent Variable = Manager*			
Excluded Instrument:				
LFC Precipitation Shocks	11.67***	9.79***	11.69***	9.85***
	(2.85)	(2.54)	(2.85)	(2.54)
Controls:				
Century Precipitation Shocks	-6.90***	-9.86***	-6.95***	-9.87***
	(2.07)	(2.29)	(2.07)	(2.30)
Median Precipitation 1900-2000	-0.54	1.41*	-0.53	1.42*
	(0.45)	(0.81)	(0.45)	(0.81)
Excluded Instrument F-Statistic	16.76	14.88	16.83	14.99
[p-value]	[0.0001]	[0.0001]	[0.0001]	[0.0001]
Additional Controls:				
Year Fixed Effects	Yes	Yes	Yes	Yes
Geography & Division	No	Yes	No	Yes
Sample Years	Full	Full	Full	Full
Number of Observations	9,974	9,974	9,850	9,850
Number of Clusters	248	248	248	248

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. The main entries in the first row of columns (1)-(4) report an IV estimate of β_1 from equation (9) in the text with the first stage given by equation (10) and estimated in Panel B. Standard errors clustered at the city level reported in parentheses. The excluded instrument is *LFC Precipitation Shocks*. The models in columns (1) and (3) do not include any additional controls. The models in columns (2) and (4) also include the following geographic controls: *elevation minimum*, *elevation maximum, latitude, longitude, a latitude-longitude interaction, distance to nearest river*, *presence of large river*, *presence of swamp*, *located on the coast*, *percentage of clay in the soil*, and Census division fixed effects. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE 4: City Government Form and Police Employment: Robustness

Dependent Variable=	Log (Officers Per Capita)		Log (Civilians Per Capita)	
Model:	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Baseline	-0.16***	-0.28**	0.09	0.69**
	(0.03)	(0.14)	(0.07)	(0.35)
Panel A: Alternative Samples				
(1) Drop Very Small and Very Large Cities	-0.13***	-0.28**	0.14*	0.68*
	(0.04)	(0.14)	(0.07)	(0.35)
(2) Drop Dependent Variable Outliers	-0.08***	-0.21*	0.18**	0.79**
	(0.03)	(0.11)	(0.07)	(0.34)
(3) Drop Low Government Concentration Census Divisions	-0.15***	-0.19*	0.12	0.80**
	(0.04)	(0.11)	(0.08)	(0.31)
(4) Drop post-COPS Program Years	-0.17***	-0.35**	0.10	0.70**
	(0.04)	(0.15)	(0.07)	(0.35)
(5) Drop High Missing Precipitation Cities	-0.18***	-0.26*	0.05	0.95**
	(0.04)	(0.15)	(0.07)	(0.43)
(6) Drop Far from Weather Station Cities	-0.17***	-0.28*	0.13*	0.79**
	(0.04)	(0.16)	(0.08)	(0.40)
Panel B: Alternative Standard Error Construction and Inferences				
(7) Cluster on both City and Year	-0.16***	-0.28**	0.09	0.69**
	(0.03)	(0.14)	(0.07)	(0.34)
(8) Cluster on Weather Station	-0.16***	-0.28**	0.09	0.69
	(0.04)	(0.13)	(0.09)	(0.55)
(9) Conley Standard Errors (No Year Fixed Effects)	-0.16***	-0.29**	0.09	0.66
	(0.04)	(0.14)	(0.07)	(0.41)
Panel C: Alternative Police Employment Scaling				
(10) Per Property Crime	-0.35***	-0.64**	-0.07	0.22
	(0.05)	(0.25)	(0.06)	(0.23)
(11) Per Violent Crime	-0.16	-1.02*	0.11	-0.16
	(0.10)	(0.53)	(0.10)	(0.40)
(12) Per Index Crime	-0.34***	-0.66**	-0.06	0.20
	(0.05)	(0.26)	(0.06)	(0.22)
Additional Controls:				
Year Fixed Effects	Yes	Yes	Yes	Yes
Geography & Division	No	No	No	No

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of largest 248 self-governing cities in the United States in 1900. The main entries report estimates of β_1 in equations (9) and (10). Standard errors clustered at the city level reported in parentheses (except in Panel B). See text for detailed descriptions of the specifications in panels A, B, and C. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE 5: City Government Form and Police Employment: Policymaker Salary Interactions

Dependent Variable=	Log (Officers Per Capita)			Log (Civilians Per Capita)		
	(1)	(2)	(3)	(4)	(5)	(6)
Manager	-0.08	-0.02	0.02	-0.09	-0.14	-0.06
	(0.05)	(0.04)	(0.04)	(0.12)	(0.11)	(0.11)
Manager × High Policymaker Salary 1992+	-0.26***	-0.22***	-0.20***	-0.16	-0.05	-0.02
	(0.08)	(0.06)	(0.05)	(0.15)	(0.14)	(0.13)
High Policymaker Salary 1992+	0.25***	0.27***	0.16**	0.65***	0.48***	0.30***
	(0.05)	(0.04)	(0.04)	(0.09)	(0.09)	(0.09)
Additional Controls:						
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geography & Division	No	Yes	Yes	No	Yes	Yes
Demographics	No	No	Yes	No	No	Yes
Sample Years	Full	Full	Full	Full	Full	Full
Number of Observations	9,974	9,974	9,974	9,850	9,850	9,850
Number of Clusters	248	248	248	248	248	248

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. The main entries in the columns (1)-(6) report OLS estimates of equation (9) in the text augmented with the salary variable and an interaction term. The models also include an indicator variable for missing *High Policymaker Salary 1992+* and its interaction with *Manager*. Standard errors clustered at the city level reported in parentheses. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE 6: City Government Form and Police Employment: Unionization and Newspaper Interactions

Dependent Variable=	Log (Officers Per Capita)			Log (Civilians Per Capita)		
	(1)	(2)	(3)	(4)	(5)	(6)
Manager	-0.18**	-0.05	-0.04	0.00	0.03	0.03
	(0.06)	(0.05)	(0.04)	(0.13)	(0.13)	(0.11)
Manager × Low Police Unionization 1968	-0.14*	-0.12*	-0.11**	0.07	-0.03	-0.01
	(0.08)	(0.07)	(0.05)	(0.15)	(0.15)	(0.13)
Manager × High Local Newspaper Sales 1990-2000	0.17**	0.04	0.04	0.02	-0.08	-0.07
	(0.07)	(0.06)	(0.05)	(0.14)	(0.14)	(0.12)
Low Police Unionization 1968	0.10*	0.10**	0.07	-0.04	0.09	0.02
	(0.06)	(0.05)	(0.04)	(0.11)	(0.10)	(0.09)
High Local Newspaper Sales 1990-2000	-0.09*	0.04	0.01	0.03	0.04	-0.02
	(0.05)	(0.04)	(0.03)	(0.10)	(0.09)	(0.08)
Additional Controls:						
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Geography & Division	No	Yes	Yes	No	Yes	Yes
Demographics	No	No	Yes	No	No	Yes
Sample Years	Full	Full	Full	Full	Full	Full
Number of Observations	9,974	9,974	9,974	9,850	9,850	9,850
Number of Clusters	248	248	248	248	248	248

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. The main entries in the columns (1)-(6) report OLS estimates of equation (9) in the text augmented with an additional variable and an interaction term. Standard errors clustered at the city level reported in parentheses. All models include an indicator variable for missing *High Police Unionization 1968* and its interaction with *Manager*. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE 7: City Government Form and Police Employment: Electoral Effects

Dependent Variable =	Log (Officers Per Capita)				Log (Civilians Per Capita)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election	0.006	0.010**	0.009**	0.009**	0.008	0.006	0.005	0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)	(0.009)	(0.008)	(0.008)
Manager	-0.078	-0.064	-0.044	-0.045	0.009	-0.040	-0.034	-0.035
	(0.056)	(0.084)	(0.047)	(0.047)	(0.095)	(0.110)	(0.085)	(0.085)
Manager × Election	-0.009*	-0.015**	-0.013***	-0.013***	-0.001	-0.009	-0.006	-0.006
	(0.005)	(0.007)	(0.005)	(0.005)	(0.013)	(0.015)	(0.014)	(0.014)
Additional Controls:								
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic	No	No	Yes	Yes	No	No	Yes	Yes
Policymaker	No	No	No	Yes	No	No	No	Yes
Sample Cities	Full	Large Cities	Large Cities	Large Cities	Full	Large Cities	Large Cities	Large Cities
Sample Years	Full	Full	Full	Full	Full	Full	Full	Full
Number of Observations	9,974	7,033	7,033	7,033	9,850	7,017	7,017	7,017
Number of Clusters	248	174	174	174	248	174	174	174

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix. The unit of observation is a city-year. In columns (1) and (5) the sample is the largest 248 self-governing cities in the United States in 1900. In the remaining columns the sample is the 174 cities that also have at least 50,000 residents in 1960. Each column reports the results from one regression. The main entries in columns (1)-(8) report OLS estimates of equation (9) in the text augmented with *Election* and *Manager*×*Election*. Standard errors clustered at the city level reported in parentheses. Additional controls included as indicated. The models in columns (3) and (7) also include demographic controls: *population, fraction of population non-white, fraction of population college graduate*, and *median household income*. The models in columns (4) and (8) also include *Policymaker Male* (name-based imputation) and an indicator variable for *Missing Policymaker Male* (name based imputation). * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

Appendix - *For Online Publication*

A.1 Theory Appendix

Proof of Proposition 1 We show that there is a unique set of strategies and beliefs that satisfy the Perfect Bayesian Equilibrium conditions. The proof has two parts. First, we show how voter beliefs and actions depend on intermediary and policymaker strategies. Second, we show that intermediary actions are unique best responses to voter strategies, and policymaker actions are unique best responses to intermediary strategies.

At election time t the voter can affect the intermediary's future type by retaining/replacing the incumbent intermediary. The incumbent intermediary's type θ_t^I is signaled by his pre-election response y_t to the policymaker action x_t. Earlier responses are based on preference shocks at and before $t-1$, which are not correlated with type at t. Thus the intermediary follows his preferences in a post-election period. This means that he "keeps policymaker iff $\theta_{t+1}^P = \theta_{t+1}^I$." This affects the policymaker's type in the pre-election period $t+2$.

Pre-election, in any equilibrium the signal contained in (x_t, y_t) must depend on ideological intermediaries' preferences because responsive intermediaries' actions do not depend on their type. Since $\gamma p > (1-\gamma)(1-p)$ congruent popular policymakers are more common than dissonant popular ones. Thus keeping a popular policymaker is more likely a congruent intermediary action; firing a popular policymaker more likely a dissonant intermediary action. Which type is more likely to keep an unpopular policymaker depends on how $\gamma(1-p)$ compares to $(1-\gamma)p$. If $\gamma > p$ it is the congruent intermediary type, otherwise the dissonant type. Pre-election equilibrium voter beliefs $\tilde{\theta}_t^I(x_t, y_t) = \mathbb{P}\left\{\theta_t^I = C | x_t, y_t\right\}$ take the form:

$$\tilde{\theta}_t^I(x_t, y_t) = \frac{\gamma \mathbb{P}\left\{x_t, y_t | \theta_t^I = \eta_{t-1}^P\right\} \lambda^I + \mathbb{P}\left\{x_t, y_t, \eta_t^I = C | \theta_t^I = \eta_t^I\right\}\left(1-\lambda^I\right)}{\mathbb{P}\left\{x_t, y_t | \theta_t^I = \eta_{t-1}^I\right\} \lambda^I + \mathbb{P}\left\{x_t, y_t | \theta_t^I = \eta_t^I\right\}\left(1-\lambda^I\right)} \tag{A1}$$

where:

$$\begin{aligned} &\mathbb{P}\left\{x_t, , y_t, \eta_t^I = C | \theta_t^I = \eta_t^I\right\} = \\ &= \begin{cases} r\{[\gamma p + (1-\gamma)(1-p)]\gamma \mathbf{1}\{\gamma > p\} + & \\ +\gamma \mathbf{1}\{\gamma < p\}\} + (1-r)\gamma^2 p & \text{if } (x_t, y_t) = (1,1) \\ (1-r)(1-\gamma)(1-p)\gamma, & \text{if } (x_t, y_t) = (1,0) \\ r\left[\gamma(1-p) + (1-\gamma)p\right]\gamma \mathbf{1}\{\gamma > p\} + & \\ +(1-r)\gamma^2(1-p), & \text{if } (x_t, y_t) = (0,1) \\ (1-r)(1-\gamma)p\gamma, & \text{if } (x_t, y_t) = (0,0) \end{cases} \end{aligned} \tag{A2}$$

$$\mathbb{P}\left\{x_t, y_t | \theta_t^I = \eta_t^I\right\} = \tag{A3}$$

$$= \begin{cases} r\left\{\left[\gamma p + (1-\gamma)(1-p)\right]\mathbf{1}\left\{\gamma > p\right\} + \mathbf{1}\left\{\gamma < p\right\}\right\} + \\ + (1-r)\left[\gamma^2 p + (1-\gamma)^2(1-p)\right], & \text{if } (x_t, y_t) = (1,1) \\ (1-r)\left[(1-\gamma)(1-p)\gamma + \gamma p(1-\gamma)\right], & \text{if } (x_t, y_t) = (1,0) \\ r\left[\gamma(1-p) + (1-\gamma)p\right]\mathbf{1}\left\{\gamma > p\right\} + \\ + (1-r)\left[\gamma^2(1-p) + (1-\gamma)^2 p\right], & \text{if } (x_t, y_t) = (0,1) \\ (1-r)\left[(1-\gamma)p\gamma + \gamma(1-p)(1-\gamma)\right], & \text{if } (x_t, y_t) = (0,0) \end{cases}$$

are equilibrium probabilities in the event that current type θ_t^I is based on the current period's preference shock η_t^I. Notice that $\tilde{\theta}_t^I(1,1) > \gamma > \tilde{\theta}_t^I(1,0)$ when $\gamma > \frac{1}{2}$. Also note that $\tilde{\theta}_t^I(0,1) > \gamma > \tilde{\theta}_t^I(0,0)$ when $\gamma > p$, and $\tilde{\theta}_t^I(0,1) < \gamma < \tilde{\theta}_t^I(0,0)$ when $\gamma < p$.

Since in the next (post-election) period the intermediary follows his preferences, and the challenger's reputation is γ, the only sequentially rational voter response is to (i) keep the intermediary after $(x_t, y_t) = (1,1), (0,1)$, and fire the intermediary after $(x_t, y_t) = (1,0), (0,0)$, when $\gamma > p$, and (ii) keep the intermediary after $(x_t, y_t) = (1,1), (0,0)$, and fire the intermediary after $(x_t, y_t) = (1,0), (0,1)$, when $\gamma < p$. In the first case, the voter's only sequentially rational strategy is to "retain intermediary iff keeps policymaker." In the second case, the voter's only sequentially rational strategy is to "retain intermediary iff keeps popular policymaker and fires unpopular policymaker." The first strategy insulates responsive policymakers. The second strategy causes them to pander pre-election. ■

Proof of Proposition 2 The strategy for identifying the unique equilibrium is analogous to the proof for Proposition 1. First, we show how voter beliefs and actions depend on policymaker strategies. Second, we show that policymaker actions are unique best responses to voter strategies.

At election time t the voter can affect the policymaker's future type by keeping/firing the incumbent policymaker. The incumbent policymaker's type θ_t^P is signaled by his pre-election policy choice x_t. Earlier choices are based on preference shocks at and before $t-1$, which are not correlated with type at t. Thus the policymaker follows his preferences in a post-election period.

Pre-election, in any equilibrium the signal contained in x_t must depend on ideological policymakers' preferences because responsive policymakers' actions do not depend on their type. Since a congruent policymaker prefers the popular policy more often (p) than a dissonant policymaker ($1-p$) a popular policy signals a congruent type and an unpopular policy signals a dissonant type. Pre-election equilibrium voter beliefs $\tilde{\theta}_t^P(x_t) = \mathbb{P}\left\{\theta_t^P = C | x_t\right\}$ take

the form:

$$\tilde{\theta}_t^P(x_t) = \frac{\gamma \mathbb{P}\left\{x_t|\theta_t^P = \eta_{t-1}^P\right\}\lambda^P + \mathbb{P}\left\{x_t, \eta_t^P = C|\theta_t^P = \eta_t^P\right\}\left(1-\lambda^P\right)}{\mathbb{P}\left\{x_t|\theta_t^P = \eta_{t-1}^P\right\}\lambda^P + \mathbb{P}\left\{x_t|\theta_t^P = \eta_t^P\right\}\left(1-\lambda^P\right)} \tag{A4}$$

where:

$$\mathbb{P}\left\{x_t, \eta_t^P = C|\theta_t^P = \eta_t^P\right\} = \begin{cases} r\gamma + (1-r)\gamma p, & \text{if } x_t = 1 \\ (1-r)\gamma(1-p), & \text{if } x_t = 0 \end{cases} \tag{A5}$$

$$\mathbb{P}\left\{x_t|\theta_t^P = \eta_t^P\right\} = \begin{cases} r + (1-r)\left[\gamma p + (1-\gamma)(1-p)\right], & \text{if } x_t = 1 \\ (1-r)\left[\gamma(1-p) + (1-\gamma)p\right], & \text{if } x_t = 0 \end{cases} \tag{A6}$$

are equilibrium probabilities in the event that current type θ_t^P is based on the current period's preference shock η_t^P.

Notice that $\tilde{\theta}_t^P(1) > \gamma > \tilde{\theta}_t^P(0)$ when $p > \frac{1}{2}$. Since in the next (post-election) period the policymaker follows his preferences, and the challenger's reputation is γ, the only sequentially rational voter response is to keep the policymaker after $x_t = 1$ and to fire the policymaker after $x_t = 0$, i.e., the voter's only sequentially rational strategy is to "keep policymaker iff policy popular." The voter's strategy thus causes responsive policymakers to pander pre-election. ■

Proof of Proposition 3 The equilibrium argument follows by substituting $p = \frac{1}{2}$ in equations (A1)-(A3) and (A4)-(A6). Doing so yields policymaker insulation in equilbirium under both hierarchical and direct accountability. Due to the symmetric environment the direct accountability equilibrium is no longer unique; as is standard, we use ex-ante voter welfare as the equilibrium selection criterion.

Under hierarchical accountability keeping the policymaker signals a congruent intermediary because $\gamma > \frac{1}{2}$, i.e., there are more congruent than dissonant policymakers. Voter equilibrium beliefs satisfy $\tilde{\theta}_t^I(1,1) = \tilde{\theta}_t^I(0,1) > \gamma > \tilde{\theta}_t^I(1,0) = \tilde{\theta}_t^I(0,0) > \frac{1}{2}$. Therefore, the only sequentially rational voter strategy is to "retain intermediary iff keeps policymaker." A responsive intermediary then insulates the policymaker. Between terms the voter performs selection of the intermediary, as in equation (1). Within terms the intermediary performs selection of the policymaker, based on type, as in equation (2). Even though both types of selection increase voter welfare, there is no change in the distribution of policy outcomes.

Under direct accountability neither policy can signal the policymaker's type. Thus any voter strategy is sequentially rational. The strategy that maximizes ex-ante voter welfare

is to insulate the policymaker because that produces expected voter welfare of γ, whereas pandering on a particular policy produces expected voter welfare of $\frac{1}{2}$. There is no selection of policymakers. ■

Additional Comparative Statics Section 2 presents comparative statics on the hierarchical-direct policy differential. Comparative statics can also be performed within each accountability form. Here the effects are more ambiguous. The results below follow from equations (5)-(8).

Consider, first, hierarchical accountability. With respect to the retention motivation (r) the popular policy's frequency pre-election can increase or decrease depending on whether the equilibrium is insulating or pandering.

$$\frac{\partial}{\partial r}\bar{x}^H_{pre}(r,\gamma,p) = \left\{\bar{x}[\bar{\theta}^P(\gamma)] - \bar{x}[\bar{\theta}^P(\bar{\theta}^I)]\right\}\mathbf{1}\{\gamma > p\} + \left\{1 - \bar{x}[\bar{\theta}^P(\bar{\theta}^I)]\right\}\mathbf{1}\{\gamma < p\} \quad \text{(A7)}$$

$$\frac{\partial}{\partial r}\bar{x}^H_{post}(r,\gamma,p) = \bar{x}(\gamma) - \bar{x}[\bar{\theta}^P(\gamma)] \quad \text{(A8)}$$

The sign of the pre-election effect thus depends on whether the equilibrium is insulating or pandering. Post-election the popular policy's frequency decreases in r because responsive intermediaries do not perform beneficial policymaker selection between terms.

An increase in γ has two opposing effects. A higher γ increases policymaker selection. A higher γ also makes the insulating equilibrium more likely, reducing $\bar{x}^H_{pre}$. Thus there is non-monotonicity at the point where the equilibrium switches from insulating to pandering. Post-election a higher γ increases $\bar{x}^H_{post}$ through increased policymaker selection. A higher p makes the pandering equilibrium more likely, thus increasing $\bar{x}^H_{pre}$, but not affecting $\bar{x}^H_{post}$.

Under direct accountability the popular policy is more common when more policymakers are responsive pre-election because they respond to pandering incentives, and less common post-election because of reduced selection.

$$\frac{\partial}{\partial r}\bar{x}^D_{pre}(r,\gamma,p) = 1 - \bar{x}(\gamma) \quad \text{(A9)}$$

$$\frac{\partial}{\partial r}\bar{x}^D_{post}(r,\gamma,p) = \bar{x}(\gamma) - \bar{x}[\bar{\theta}^P(p)] \quad \text{(A10)}$$

Pre-election, policymakers acting on preferences choose the popular policy more often when γ increases. Post-election a higher γ increases the popular policy's frequency, though at a decreasing rate, due to the selection performed between terms. A higher p increases policymaker selection which increases $\bar{x}^D_{post}$.

A.2 Data Appendix

Police and Crime

Officers Per Capita: Number of sworn police officers in city police department. *Sources:* Uniform Crime Reports (1960-2000).
Civilians Per Capita: Number of non-sworn employees in city police department. *Sources:* Uniform Crime Reports (1960-2000).
Violent Crime Rate: Total number of murder, rape, and robbery crimes reported in city in a given year, per 1000 population. *Sources:* Uniform Crime Reports (1975-2000).
Property Crime Rate: Total number of motor vehicle theft, larceny, burglary, and assault crimes in a city reported in a given year, per 1000 population. *Sources:* Uniform Crime Reports (1975-2000).
Total Index Crime Rate: Sum of violent and property crime rates. *Sources:* Uniform Crime Reports (1975-2000).
Low Police Unionization 1968: Indicator variable equal to 1 for cities with below median fraction of the police department unionized in 1968, and 0 otherwise. *Source:* Municipal Year Book (1968).

Geography

Sources: Fishback, Horrace, and Kantor (2005, 2006). The data are reported at the county level. We match it to our sample cities.
Elevation Minimum: The minimum elevation in the county.
Elevation Maximum: The maximum elevation in the county.
Latitude: Latitude of the county seat.
Longitude: Longitude of the county seat.
Presence of Very Large River: County has a river that goes through more than 50 counties.
Presence of Large River: County has a river that goes through 21 to 50 counties.
Presence of Small River: County has a river that goes through 11 to 20 counties.
Distance to Nearest River: Minimum distance to a county with a small, large or very large river.
Presence of Swamp: County has a swamp.
Located on the Coast: County is located on the coast.
Percentage of Clay in the Soil: Based on contemporary surveys by USDA soil scientists.

Soil Indicated Flood Frequency Index: The index is based on the average flood class of the county standardized to a variable with a mean of zero and a standard deviation of one. Based on contemporary surveys by USDA soil scientists.

Demographics

Population: Number of city residents, in thousands. Based on 1960, 1970, 1980, 1990, and 2000 Census of Population numbers, linearly interpolated in intercensal years. *Sources:* U.S. Census Bureau, Census of Population (various years).

Fraction Non-White: Fraction of city population that are non-white. Based on 1960, 1970, 1980, 1990, and 2000 Census of Population numbers, linearly interpolated in intercensal years. *Sources:* U.S. Census Bureau, Census of Population (various years).

Fraction College Graduate: Fraction of city population that are college graduates. Based on 1960, 1970, 1980, 1990, and 2000 Census of Population numbers, linearly interpolated in intercensal years. *Sources:* U.S. Census Bureau, Census of Population (various years).

Household Income: Median household income of city residents, in 2000$. Based on 1960, 1970, 1980, 1990, and 2000 Census of Population numbers, linearly interpolated in intercensal years. *Sources:* U.S. Census Bureau, Census of Population (various years).

City Institutions

Manager: Indicator variable equal to 1 in a year when the city has a manager form of government, and 0 otherwise. *Sources:* Municipal Year Book (1960-2000).

Non-Partisan Elections 1960: Indicator variable equal to 1 if the city charter in effect in 1960 mandates non-partisan elections, and 0 otherwise. *Source:* Municipal Year Book (1960).

Fraction At-Large Seats 1960: Fraction of city council seats that elected at-large in 1960. *Source:* Municipal Year Book (1960).

Early Civil Service: Indicator variable equal to 1 if the city has a non-political civil service before 1937, and 0 otherwise. *Source:* Civil Service Assembly (1938).

Election: Indicator variable equal to 1 in an election year, and 0 otherwise. If the election takes place before July 31 of the year, the previous year is coded as an election year. Election years are coded based on mayor elections for mayor cities and based on city council elections for manager cities. *Sources:* Municipal Year Book (various years), World Almanac (various years), www.ourcampaigns.com, city charters, newspaper articles.

City Government Spending: Total expenditure by city government, in thousands of 2000$. *Sources:* Census of Governments, City Government Finances (1972-2000).
High Local Newspaper Sales 1990-2000: Indicator variable equal to 1 for cities with an above median fraction of local newspaper sales per capita during the 1990 to 2000 period, and 0 otherwise. *Source:* George and Waldfogel (2006).

Policymaker Salary and Characteristics

Annual Salary (survey reported): Salary of manager or mayor in 2000$. *Sources:* ICMA Salaries of Municipal Officials (1992-1993,1995-2000).
High Policymaker Salary 1992+: Indicator variable equal to 1 for cities with an above median average salary of manager or mayor during the 1992-1993,1995-2000 period, and 0 otherwise. *Sources:* ICMA Salaries of Municipal Officials (1992-1993,1995-2000).
Male (survey reported): Manager or mayor is male. *Sources:* ICMA Salaries of Municipal Officials (1992-1993,1995-2000).
White (survey reported): Manager or mayor is white. *Sources:* ICMA Salaries of Municipal Officials (1992-1993,1995-2000).
Male (name-based imputation): Manager or mayor is male. For imputation procedure see section A.3. *Sources:* World Almanac and Book of Facts (1960-2000).

1900 City Outcomes

Sources: U.S. Census Bureau (1905) and U.S. Census Bureau (1906).
Officers Per Capita: Number of sworn police employees per resident.
Civilians Per Capita: Number of non-sworn police employees per resident.
Arrests Per Capita: Number of police department arrests per resident.
Miles of Paved Roads Per Square Mile: Miles of paved road per square mile.
Miles of Sewers Per Square Mile: Miles of sewers per square mile.
Population: Number of city residents.

Weather

Sources: The U.S. Historical Climatology Network's *Daily Temperature, Precipitation, and Snow Data* contains daily readings for precipitation, snowfall, and temperature extremes collected from weather stations throughout the U.S. We construct yearly variables based on this dataset.

LFC Precipitation Shocks: Fraction of years in the Local Flood Control Era, from 1900 to 1936, with annual city precipitation in the top 1 percent of the national precipitation distribution.

Century Precipitation Shocks: Fraction of years from 1900 to 2000 with annual city precipitation in the top 1 percent of the national precipitation distribution.

Median Precipitation 1900-2000: Median annual city precipitation from 1900 to 2000.

Fraction of Years in Top 1 Percent 1900-2000: Fraction of years from 1900 to 2000 with annual city precipitation in the top 1 percent of the national precipitation distribution

Fraction of Years in Top 5 Percent 1900-2000: Fraction of years from 1900 to 2000 with annual city precipitation in the top 5 percent of the national precipitation distribution

Fraction of Years in Top 10 Percent 1900-2000: Fraction of years from 1900 to 2000 with annual city precipitation in the top 10 percent of the national precipitation distribution

FFC Precipitation Shocks: Fraction of years in the Federal Flood Control Era, from 1937 to 1960, with annual city precipitation in the top 1 percent of the national precipitation distribution.

LFC Drought Shocks: Fraction of years in the Local Flood Control Era, from 1900 to 1936, with annual city precipitation in the bottom 1 percent of the national precipitation distribution.

Century Drought Shocks: Fraction of years from 1900 to 2000 with annual city precipitation in the bottom 1 percent of the national precipitation distribution.

LFC Hot Shocks: Fraction of years in the Local Flood Control Era, from 1900 to 1936, with city annual high temperature in the top 1 percent of the national high temperature distribution.

Century Hot Shocks: Fraction of years from 1900 to 2000 with city annual high temperature in the top 1 percent of the national high temperature distribution.

Median High Temperature 1900-2000: Median annual high temperature in a city from 1900 to 2000.

LFC Cold Shocks: Fraction of years in the Local Flood Control Era, from 1900 to 1936, with city annual low temperature in the bottom 1 percent of the national high temperature distribution.

Century Cold Shocks: Fraction of years from 1900 to 2000 with city annual low temperature in the bottom 1 percent of the national high temperature distribution.

Median Low Temperature 1900-2000: Median annual low temperature in a city from 1900 to 2000.

A.3 Data References

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Williams, C.N., M.J. Menne, R.S. Vose and D.R. Easterling (2006) "U.S. Historical Climatology Network Daily Temperature, Precipitation, and Snow Data," Oak Ridge National Laboratory.

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A.4 Name-Based Imputation of Policymaker Gender

To obtain a measure of policymaker gender for the full sample period 1960-2000 we use the manager and mayor names reported in the *World Almanac*. To impute gender we use the ICMA *Salaries of Municipal Officials* dataset to identify the modal gender for each policymaker first name. We then use this predicted gender with the actual first name of the policymaker from the *World Almanac* to assign gender. The imputation of gender from the policymaker name may be imperfect. We are unable to impute a policymaker gender when (i) the policymaker name in the *World Almanac* does not exist in the ICMA salary dataset, (ii) the policymaker name in the *World Almanac* is reported as an initial, not full first name, and (iii) the policymaker name is not reported in the *World Almanac*. Ultimately, we are able to impute gender for the policymaker in this fashion for 69.78 percent of the city-year observations in our data. We did not use similar name-based imputation for policymaker race due to the low degree of correlation found.[1]

[1]While it is possible in principle to impute race from policymaker names in a similar fashion in practice many policymaker names are less informative about race then they are about gender. For example, the fraction of policymakers who are directly reported as male in the ICMA salary data is 0.86. This is very

A.5 Instrument Validity and Robustness

As explained in Section 4 of the paper we obtain climate measures by first aggregating daily weather data to the yearly level. We then define an extreme weather event as a year when a city's weather measure is in the upper nth percentile of the national weather-years 1900-2000 distribution. We then calculate the fraction of years that a city has an extreme weather event during the local flood control era (1900-1936) and for the full century. We create these cross-sectional variables for high and low precipitation, and hot and cold temperatures. In addition, to control for differences in typical weather across cities we calculate median precipitation, high temperature and low temperature for each city using data from 1900-2000. Tables A1 to A5 report results relevant to the validity and robustness of our IV approach.

Precipitation Shocks and Flood Risk. We use precipitation to measure the timing of flood crises because comprehensive local data on the occurrence of floods does not exist in the local flood control period and flood occurrence may partially reflect the choice of flood control technology. To provide a first check of our identification strategy we examine whether the relationship between extreme precipitation and flood incidence was stronger before 1936 as the historical record would suggest. We use a measure of flood frequency at the local level based on soil surveys by contemporary USDA soil scientists.[2] This index measure is based on local flood frequency class as determined by the interpretation of soil properties and other evidence gathered during soil survey fieldwork.

Table A1 examines the relationship between soil indicated flood frequency and three candidate measures of precipitation shock: the fraction of years that city precipitation is in the top 1st, 5th, and 10th percentile of the national precipitation distribution for the century.[3] In principle any of these measures could represent a promising instrument. To avoid concerns about weak instruments leading to finite sample bias in our IV estimates we seek the strongest possible predictor of flood frequency.

All Local Flood Control (LFC) precipitation shocks measures predict flood frequency.

close to the 0.93 fraction imputed to being male based on the policymaker name and the twocorrelation between the two measures is 0.53 for the years when both measures exist. In contrast, the fraction white from the name-based measure is 0.29, far lower than the 0.79 fraction white in the ICMA salary data. In addition, the correlation between the two policymaker race measures is only 0.03 for the years when both measures exist.

[2]The data are based on maps of annual flood frequency regions averaged at the county level.

[3]Our analysis is necessarily based on the use of national percentiles to define extreme precipitation years. If we instead used city-specific percentiles to define extreme precipitation years we would obtain essentially the same fraction of years above a given percentile cutoff for every city. This is one way in which our precipitation shock cohort approach differs from the within country rainfall approach of Bruckner and Ciccone (2011).

However, comparing the different measures indicates that the shocks based on the 1 percent definition have the most power in explaining flood frequency. Furthermore, when we add geographic controls in columns (4)-(6) the shocks based on the 5 and 10 percent definitions have substantially less power in explaining the occurrence of floods. In contrast, the second half of the table shows that century precipitation shocks have little relationship with flood risk. This suggests that flood control technology post-1936 had become more effective in reducing flood risk.

Validity and Exclusion Restriction. We next estimate models that probe the validity and exclusion restriction of our IV strategy. One threat to validity would be that the Flood Control Act was passed in 1936 precisely because politically powerful cities particularly suffered in the Great Flood of 1936. If these cities also employ more police today regardless of government form we would estimate a negative effect of manager government even if no effect existed. To shed light on this issue we examine whether cities affected by LFC shocks were already different in 1900 before the shocks occurred. The results in Table A2 show that cities experiencing precipitation shocks in the local flood control era are very similar across a number of observables to cities that do not.

In Table A3 we examine whether other climate shocks that have local effects but are less likely to generate infrastructure crises during the local flood control era have similar effects on manager government adoption and police employment. We present the results of reduced form models of the relationship between climate shocks and manager government, and between climate shocks and police officer employment. If our exclusion restriction is valid we would expect that the more recent federal flood control (FFC) precipitation shocks would not predict either manager government or police officer employment. The results in columns (2) and (7) confirm this expectation.

During the local flood control era cities were often also responsible for providing water to city residents and engineering expertise was also a key input into effective water supply. Thus, cities experiencing negative precipitation (drought) shocks are also more likely to adopt manager government. We find some evidence of this effect in column (3). Lastly, we would not expect extreme temperature shocks during the local flood control era to affect either government form or police employment today. Columns (4), (9) and (5), (10) show little relationship between extreme temperature shocks and manager government adoption or police employment.[4]

[4]In unreported analysis we have estimated the models in column (4) adding geographic controls and found a statistically insignificant relationship between LFC hot shocks and manager government.

Another potential concern with our IV approach is that LFC shocks might lead to crises with persistent effects on policy making, regardless of government form, in violation of our exclusion restriction. We test for clear violations of our exclusion restriction by examining whether LFC shocks lead to the adoption of other political institutions that also affect policy. The results in Table A4 reveal little relationship between LFC shocks and the three other Progressive reforms commonly associated with manager reform. These findings may be expected as these other institutional changes have little to do with flood control policy.

Alternative Instruments. As precipitation shocks are by definition a relatively rare event our IV estimates may be local to the set of cities induced by LFC shocks to adopt manager government. To probe the external validity of our results we next examine whether our estimates change with alternative versions of the instrument. We consider three different types of instrument specifications.[5]

First, instead of using the fraction of years with a positive precipitation shock we instead use only whether a city has any positive precipitation shocks during the local flood control era. We examine two candidate definitions for a positive precipitation shock, a year with precipitation ever in the top 1 percent or top 5 percent of the national distribution.[6] Second, we choose different time periods when positive precipitation shocks would be manager biased. Lastly, we use negative precipitation shocks as city governments were frequently involved in the supply of water to city residents and thus droughts increased the demand for engineering skills in government in a fashion similar to flood risk.[7]

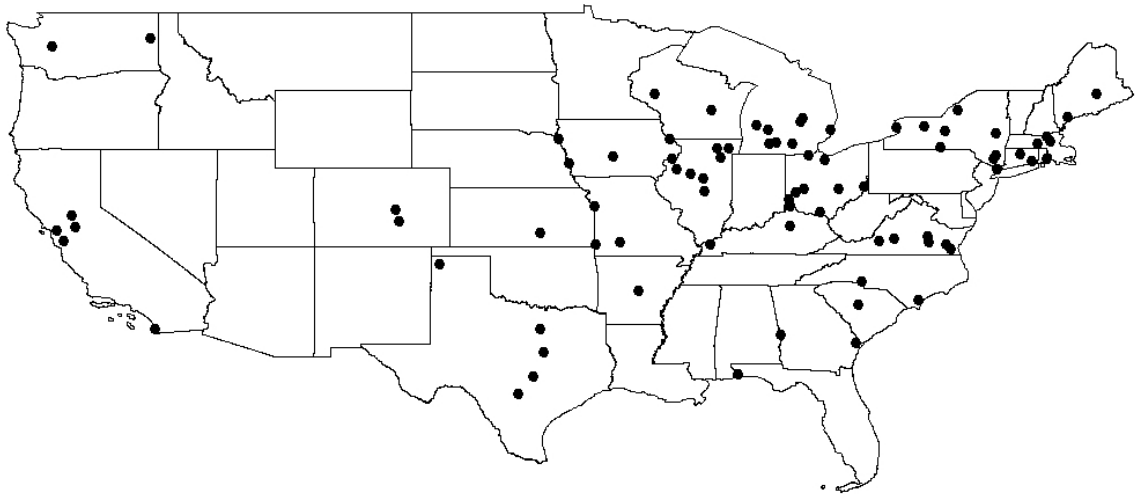
The results in Table A5 reveal that our central results above change little when we use these alternative instruments. In columns (1) to (5) the negative effect of manager government on police officer employment remains statistically significant at the 5% level in all the specifications (with the exception of column (4) where significance is at 10%) and the point estimates are of similar magnitude to the estimates in our baseline analysis. In columns (6) to (10) while the estimates of the manager coefficient for civilian employment are less stable, none of the estimates indicate a statistically significant negative manager effect.

[5] While there are many candidates for alternative instruments we focus on the ones that have sufficient power in explaining manager form of government, i.e. first-stage F-statistic close to or exceeding 10, to mitigate finite sample issues with the IV estimates.

[6] In unreported analysis we have also estimated the baseline models above with precipitation shocks defined by the top 5 percent. We obtain very similar results with a point estimate on the manager coefficient of -0.77 and a city-clustered standard error of 0.34. However, the first stage F-statistic is only 7.39 and so weak instrument bias may be a concern.

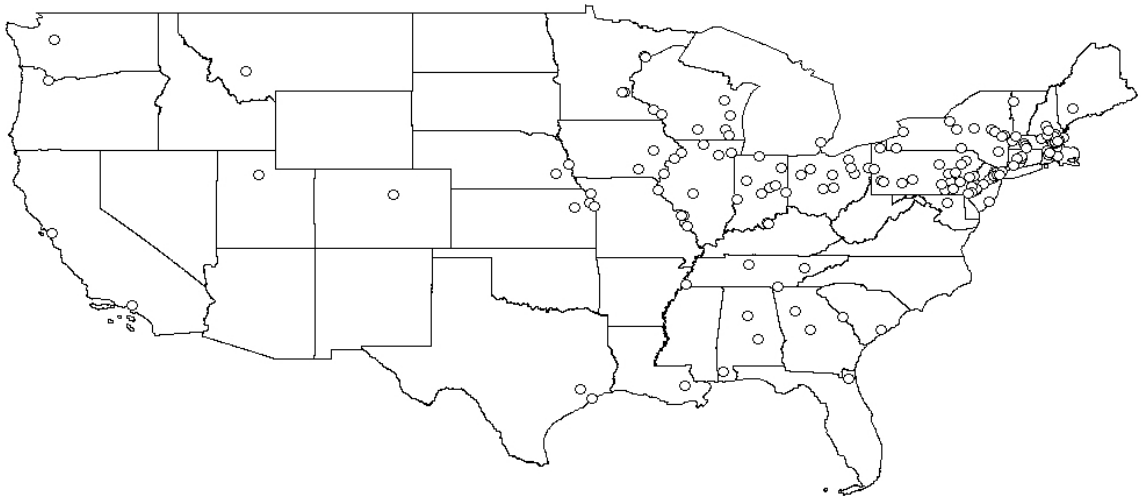
[7] We obtain additional first stage strength by adding water availablility controls: *presence of very large river*, *presence of large river*, *presence of small river*, and *located on the coast*.

FIGURE A1: Manager Cities, 1960



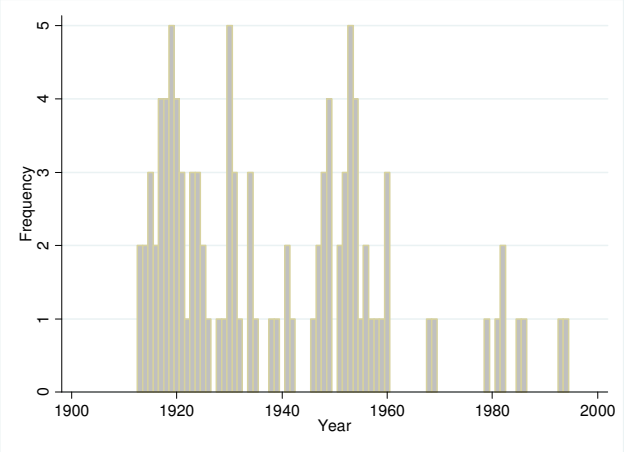
Notes: Authors' calculations for the sample of the 248 largest self-governing cities in 1900. Manager government locations are plotted based on 1960 form of government. The map reflects state jurisdictional boundaries.

FIGURE A2: Mayor Cities, 1960



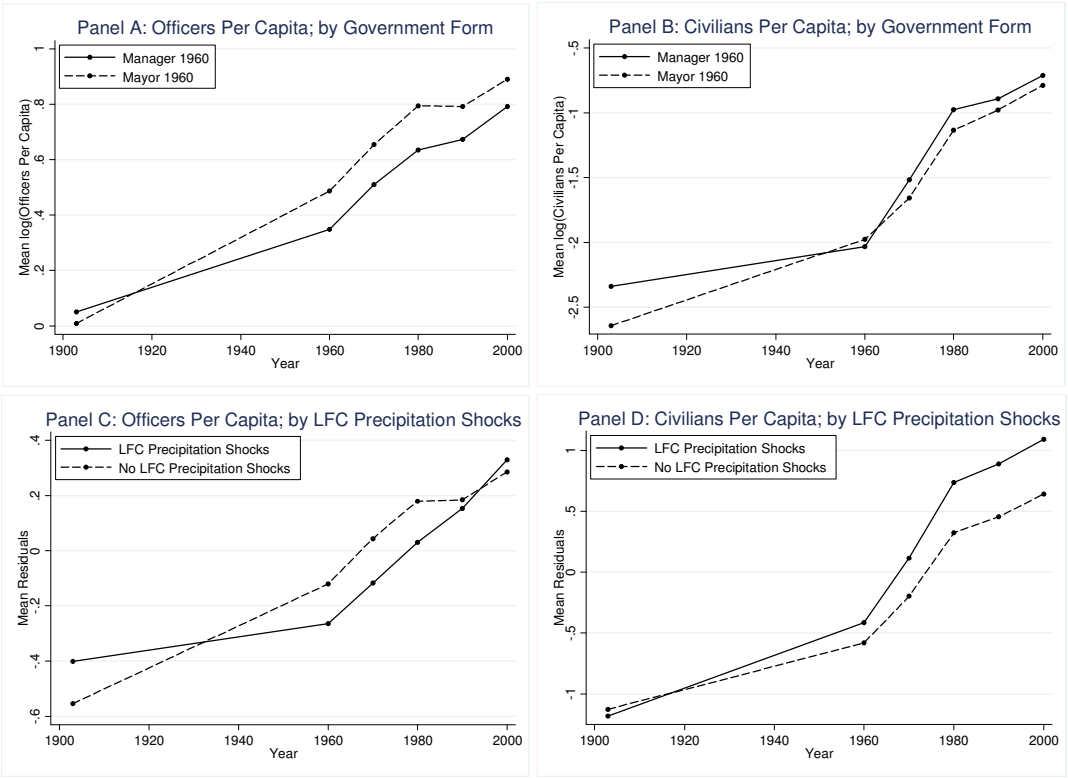
Notes: Authors' calculations for the sample of the 248 largest self-governing cities in 1900. Mayor government locations are plotted based on 1960 form of government. The map reflects state jurisdictional boundaries.

FIGURE A3: Timing of Manager Charter Adoptions



Notes: Authors' tabulations using date of adoption from the *Municipal Year Book*, newspapers, and city charters for the sample of the 248 largest self-governing cities in 1900. Year of manager government adoption is based on year of charter approval by voters.

FIGURE A4: Police Employment, by Indicated Cohort



Notes: Authors' calculations for the sample of the 248 largest self-governing cities in 1900. Panels C and D plot the mean residual from a regression of *log(Police Per Capita)* on *Century Precipitation Shocks* and *Median Precipitation 1900-2000*, where *Police* is either *Officers* or *Civilians*.

TABLE A1: Precipitation Shocks Timing and Soil Indicated Flood Frequency: OLS Estimates

Dependent Variable =	Soil Indicated Flood Frequency Index					
	(1)	(2)	(3)	(4)	(5)	(6)
LFC Precipitation Shocks:						
Fraction of Years in Top 1 Percent 1900-1936	31.79***			16.68**		
	(6.91)			(7.26)		
Fraction of Years in Top 5 Percent 1900-1936		8.67***			1.72	
		(2.08)			(2.63)	
Fraction of Years in Top 10 Percent 1900-1936			8.79***			3.09
			(1.48)			(1.99)
Century Precipitation Shocks:						
Fraction of Years in Top 1 Percent 1900-2000	-3.89			1.52		
	(5.09)			(5.83)		
Fraction of Years in Top 5 Percent 1900-2000		-3.35*			2.17	
		(1.79)			(2.42)	
Fraction of Years in Top 10 Percent 1900-2000			-4.85***			-0.38
			(1.36)			(2.06)
Median Precipitation 1900-2000	0.69	0.60	0.14	-2.55	-3.20*	-3.32
	(0.86)	(1.01)	(1.08)	(1.66)	(1.90)	(2.11)
Additional Controls:						
Geography & Division	No	No	No	Yes	Yes	Yes
Sample	Cross-Section	Cross-Section	Cross-Section	Cross-Section	Cross-Section	Cross-Section
Number of observations	248	248	248	248	248	248

Notes: Authors' calculations with the city data described in the Data Appendix. The unit of observation is a city for the sample of the largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. Standard errors reported in parentheses. The models in columns (1)-(3) include no additional controls. The models in columns (4)-(6) also include the following geographic controls: *elevation minimum, elevation maximum, latitude, longitude, a latitude-longitude interaction, distance to nearest river, presence of large river, presence of swamp, located on the coast, percentage of clay in the soil* and Census division fixed effects. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE A2: Early (1900) City Characteristics and Precipitation Shocks: OLS Estimates

Dependent Variable =	Log (Officers Per capita)	Log (Civilians Per capita)	Log (Arrests Per Capita)	Log (Miles of Paved Roads Per Sq. Mile)	Log (Miles of Sewers Per Sq. Mile)	Log (Population)	Distance to Nearest River
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LFC Precipitation Shocks	0.09	-3.73	7.11	-17.25	-12.23	-3.01	-185.85
	(2.60)	(7.20)	(6.26)	(10.73)	(9.44)	(5.69)	(276.73)
Century Precipitation	2.77	1.79	4.54	-11.22	-2.82	3.51	496.30*
Shocks	(1.92)	(5.04)	(4.85)	(7.54)	(5.93)	(4.13)	(192.60)
Median Precipitation 1900-	0.38	-0.35	-0.83	5.80***	1.38	-0.10	-102.85***
2000	(0.34)	(0.72)	(0.58)	(1.72)	(0.94)	(0.78)	(28.65)
<u>Additional Controls:</u>							
Geography & Division	No	No	No	No	No	No	No
Sample	Cross-Section	Cross-Section	Cross-Section	Cross-Section	Cross-Section	Cross-Section	Cross-Section
Number of Observations	248	172	246	246	244	248	248

Notes: Authors' calculations with the city data described in the Data Appendix. The unit of observation is a city for the sample of the largest 248 self-governing cities in the United States in 1900. Standard errors reported in parentheses. Each column reports the results from one regression with no additional controls. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE A3: Weather Shocks, City Government Form, and Police Employment: OLS Estimates

Dependent Variable =	Manager					Log (Officers Per Capita)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
LFC Precipitation Shocks	10.14***	10.18***				-4.91**	-4.62**			
	(3.04)	(3.03)				(2.11)	(2.01)			
FFC Precipitation Shocks		0.32					2.26			
		(2.39)					(1.68)			
Century Precipitation	-6.03***	-6.37**				2.68	0.26			
Shocks	(2.20)	(3.05)				(1.63)	(2.42)			
Median High Precipitation	-0.61	-0.61				0.75***	0.74***			
1900-2000	(0.46)	(0.46)				(0.24)	(0.24)			
LFC Drought Shocks			3.64*					0.01		
			(1.90)					(1.12)		
Century Drought Shocks			-4.46**					0.49		
			(2.14)					(1.28)		
Median Low Precipitation			-1.17***					1.09***		
1900-2000			(0.38)					(0.21)		
LFC Hot Shocks				-10.12**					2.91	
				(4.48)					(2.75)	
Century Hot Shocks				10.42					-2.87	
				(4.72)					(2.90)	
Median High Temperature				1.39					0.87***	
1900-2000				(0.51)					(0.27)	
LFC Cold Shocks					1.70					-0.19
					(1.03)					(0.36)
Century Cold Shocks					-1.95*					0.11
					(1.06)					(0.38)
Median Low Temperature					0.54					0.19
1900-2000					(0.58)					(0.34)

Additional Controls:										
Geography & Division	No	No	No	No	No	No	No	No	No	No
Number of Observations	248	248	248	248	248	243	243	243	243	243

Notes: Authors' calculations with the city data described in the Data Appendix. The unit of observation is a city for the sample of the largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. Standard errors reported in parentheses. The models include no additional controls. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE A4: Precipitation Shocks and Other City Institutions: OLS Estimates

Dependent Variable =	Manager 1960	Non-Partisan Elections 1960	Fraction Seats At-Large 1960	Early Civil Service
	(1)	(2)	(3)	(4)
LFC Precipitation Shocks	10.14***	-2.79	2.76	-3.88
	(3.04)	(2.73)	(2.83)	(3.68)
Century Precipitation Shocks	-6.03***	7.67***	-0.86	1.65
	(2.20)	(2.02)	(2.19)	(2.63)
Median Precipitation 1900-2000	-0.61	-1.02***	0.27	-0.83**
	(0.46)	(0.35)	(0.41)	(0.34)
Additional Controls:				
Geography & Division	No	No	No	No
Sample	Cross-Section	Cross-Section	Cross-Section	Cross-Section
Number of Observations	248	247	247	248

Notes: Authors' calculations with the city data described in the Data Appendix. The unit of observation is a city for the sample of the largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. Standard errors reported in parentheses. The models include no additional controls. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE A5: City Government Form and Police Employment: Alternative IV Estimates

Dependent Variable =	Log (Officers Per Capita)					Log (Civilians Per Capita)				
Alternative	Precipitation		Reform Era (RE)		Drought	Precipitation		Reform Era (RE)		Drought
Specification=	Shock Definition		Definition		IV	Shock Definition		Definition		IV
Definition=	Any Year Top 1 %	Any Year Top 5 %	RE 2: 1909-1936	RE 3: 1900-1929	Shock: Bottom 1 %	Any Year Top 1 %	Any Year Top 5 %	RE 2: 1909-1936	RE 3: 1900-1929	Shock: Bottom 1 %
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A: Second Stage: Dependent Variable = Log (Police Employment Per Capita)									
Manager	-0.33**	-0.44**	-0.27**	-0.30*	-0.61**	1.11**	-0.05	0.83**	0.74*	-0.74
	(0.14)	(0.20)	(0.13)	(0.16)	(0.27)	(0.44)	(0.35)	(0.37)	(0.39)	(0.54)
Century Weather Shocks	0.16**	0.15***	-0.26	-0.27	-0.09	0.16	0.30***	7.18***	7.13**	0.25
	(0.05)	(0.05)	(0.86)	(0.89)	(0.18)	(0.14)	(0.10)	(2.06)	(1.89)	(0.33)
Median Weather 1900-2000	-0.11	-0.23	0.56***	0.54**	0.00	-0.57	-2.30***	-1.28**	-1.37**	-1.79**
	(0.26)	(0.32)	(0.22)	(0.23)	(0.36)	(0.87)	(0.63)	(0.64)	(0.64)	(0.62)
	Panel B : First Stage: Dependent Variable = Manager									
RE Precipitation Shocks	0.50***	0.31***	12.04***	8.41***		0.50***	0.31***	12.03***	8.42***	
	(0.12)	(0.07)	(3.09)	(2.39)		(0.12)	(0.08)	(3.09)	(2.38)	
Century Precipitation Shocks	-0.10	-0.19**	-5.05***	-6.15***		-0.10	-0.19**	-5.08***	-6.20***	
	(0.08)	(0.08)	(1.78)	(2.15)		(0.08)	(0.08)	(1.78)	(2.15)	
Median High Precipitation 1900-2000	-0.98**	-1.28**	-0.74	-0.57		-0.98**	-1.28**	-0.74*	-0.57	
	(0.47)	(0.52)	(0.43)	(0.45)		(0.47)	(0.53)	(0.43)	(0.45)	
LFC Drought Shocks					4.54***					4.51***
					(1.65)					(1.65)
Century Drought Shocks					-5.40***					-5.35***
					(1.84)					(1.85)
Median Low Precipitation 1900-2000					-1.06***					-1.06***
					(0.37)					(0.37)

Excluded Instrument F-Statistic:	17.34 [0.0000]	17.61 [0.0000]	15.22 [0.0001]	12.44 [0.0005]	7.65 [0.0061]	17.17 [0.0000]	17.48 [0.0000]	15.15 [0.0001]	12.48 [0.0005]	7.46 [0.0068]
Additional Controls:										
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography & Division	No	No	No	No	No	No	No	No	No	No
Sample Years	Full	Full	Full	Full	Full	Full	Full	Full	Full	Full
Number of Observations	9,974	9,974	9,974	9,974	9,974	9,850	9,850	9,850	9,850	9,850
Number of Clusters	248	248	248	248	248	248	248	248	248	248

Notes: Authors' calculations with 1960-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of the largest 248 self-governing cities in the United States in 1900. Standard errors clustered at the city level reported in parentheses. The models in columns (1)-(4) and (6)-(9) only include year fixed effects as additional controls. The models in columns (5) and (10) also include indicator variables for *presence of very large river*, *presence of large river*, *presence of small river* and *located on the coast.* * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE A6: City Government Form and Crime Rates

Dependent Variable =	Log(Violent Crime Rate)	Log(Violent Crime Rate)	Log(Property Crime Rate)	Log(Property Crime Rate)
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Manager	0.08	0.26	0.14***	0.12
	(0.07)	(0.39)	(0.03)	(0.23)
Additional Controls:				
Year Fixed Effects	Yes	Yes	Yes	Yes
Geography & Division	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
Sample Years	1975-2000	1975-2000	1975-2000	1975-2000
Number of Observations	6,131	6,131	6,148	6,148
Number of Clusters	248	248	248	248

Notes: Authors' calculations with 1975-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of the largest 248 self-governing cities in the United States in 1900. Each column reports the results from one regression. Standard errors clustered at the city level reported in parentheses. The models include the following geographic controls: *elevation minimum, elevation maximum, latitude, longitude, a latitude-longitude interaction, distance to nearest river, presence of large river, presence of swamp, located on the coast, percentage of clay in the soil* and Census division fixed effects, as well as demographic controls: *population, fraction of population non-white, fraction of population college graduate*, and *median household income*. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.

TABLE A7: Crime Rates and Policymaker Salary: OLS Estimates

Dependent Variable =	Log(Policymaker Salary)			
	(1)	(2)	(3)	(4)
Manager	0.53*	0.55*	0.53	0.57*
	(0.31)	(0.30)	(0.34)	(0.32)
Log(Violent Crime Rate)	0.01	0.01		
	(0.04)	(0.03)		
Log(Violent Crime Rate) × Manager	0.02	0.01		
	(0.03)	(0.03)		
Log(Property Crime Rate)			0.01	0.01
			(0.03)	(0.02)
Log(Property Crime Rate) × Manager			0.00	0.00
			(0.03)	(0.03)
Additional Controls:				
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
Demographic	No	Yes	No	Yes
Sample Years	1992+	1992+	1992+	1992+
Number of Observations	1,052	1,052	1,055	1,055
Number of Clusters	233	233	233	233

Notes: Authors' calculations with 1992-1993, 1995-2000 city data as described in the Data Appendix. The unit of observation is a city-year for the sample of the largest 248 self-governing cities in the United States in 1900. Standard errors clustered at the city level reported in parentheses. The models include year and city fixed effects. The models in columns (2) and (4) also include the following demographic controls: *population, fraction of population non-white, fraction of population college graduate*, and *median household income*. * indicates significance at the 10 percent level, ** significance at the 5 percent level and *** significance at the 1 percent level.