

Institutions and Ephemera in Urban Reform: The Municipal Expenditure Effects of Council-Manager Government

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Abstract

Scholarly treatments of the relationship between urban reform and municipal expenditures have arrived at wildly different estimates of council-manager government's effects on spending behavior [23, 27]. While a failure to use proper econometric estimation techniques is partially responsible for these divergent findings [35], a more fundamental analytic oversight has confounded this research agenda. These treatments of council-manager government have ignored the commitment problem. I employ game-theoretic tools to illustrate how the strength of commitment technology mediates the relationship between the adoption of council-manager government and municipal expenditures. The predicted implications of the formal model are largely borne out in an empirical analysis of a new panel data set that covers 307 U.S. cities over the period 1946-1966. Cities where available commitment technology is strong experience significantly larger

*All statistical analyses were performed in STATA 9 and graphics were generated in R. Copies of both the code and original data are available for replication purposes by e-mailing zfp1@columbia.edu.

reductions in subsequent expenditures than cities with weaker commitment technology at their disposal.

1 Introduction

Since the onset of the urban reform movement, scholars have debated the sources of the reformers' success and the effects of the changes they unleashed. The reform agenda consisted of a nexus of initiatives — the short ballot, at-large council apportionment, nonpartisan elections, civil service reform, council-manager government, and the like — that systematically transferred formal political power away from immigrants, ethnics, and political machines toward the wealthier and more educated strata of the urban polity [38]. Why would the groups that lost political power acquiesce to the demands of the reformers? An equally vigorous literature has attempted to isolate the effects of these reforms. Did the implementation of municipal reforms affect public employee wages [12], government expenditures [10, 11], and long-run economic growth [33]? Scholars have offered an abundance of theoretical and empirical answers to these two puzzles, but the literature has yet to incorporate a commitment-based game theoretic approach to urban political institutions. This omission has retarded the development of the urban reform literature, which has only made fleeting advances since Banfield and Wilson's [8] identification of these puzzles.

Institutions are durable features of the political landscape that structure political conflict. When political actors cannot easily alter institutions they allow groups to commit to particular actions that would otherwise be non-credible. Game theory reveals that adoption of new institutions can induce equilibria that are Pareto improving and otherwise unattainable under the status quo. This approach to institutions has firm microfoundations in rational choice theory and has successfully explained numerous episodes of institutional change[30]. I contend that this commitment-based approach to urban reform can satisfactorily explain

both the impetus and the effects of urban reform. The majority acquiesced to the demands of the reformers in an attempt to discourage them from exiting the city limits by giving them formal control over the city's financial policies. Transferring power to the reformers prevented the dominant groups in the city from raising taxes so high that the reformers would migrate out to the emerging suburbs and shrink the city's tax base in the process.

Despite the intentions of urban electorates, council-manager government did not achieve a uniform effect in every city where it was adopted. Adopting council-manager government will only have a negative effect on municipal expenditures when actors expect that the council-manager government will continue operating into the future. When council-manager government is exposed to the risk of repeal via the referendum or ballot initiative, political actors will behave very differently than when there is negligible risk of council-manager abandonment. In the absence of strong commitment technology, the adoption of a formal rule will have little effect on the actions of rational forward-looking agents [4]. The failure to differentiate between durable institutions and ephemeral formal rules has led to mistaken inferences about the effects of institutions on municipal economic policy and growth.

While the informal logic of this argument is compelling, in the body of the paper I provide a simple formal model of commitment as a continuation game to illustrate the precise dynamic at play. The model illuminates the incentives of two groups, the Rich and the Poor, who collectively determine the amount of expenditures and redistribution in a city. The Rich have the option of leaving the city, but must endure a cost to do so, and when they leave the Poor receive no redistribution through municipal expenditures. The players can achieve a Pareto-superior outcome by transferring formal political power away from the Poor and to the Rich. As the extensive form game illustrates, municipal expenditures probabilistically decrease as the strength of the available commitment technology increases.

Advances in econometric theory have improved the methodological substance of studies of urban reform. The conclusions that Morgan and Pelissero [27] reach in their inquiry

into the municipal expenditure effects of council-manager government are difficult to justify in light of contemporary econometrics [35]. But even well-designed studies that employ appropriate econometric estimation techniques are hamstrung by the theoretical lacuna that I identify. I use the commitment approach to guide my empirical examination of urban reform. While there were many structural aspects of the urban reform agenda, I focus on council-manager government as my independent variable of interest. Council-manager government is a compelling operationalization of reform because it was at the vanguard of the reform movement and there is reliable data on its diffusion among U.S. cities over the period 1946-1966. *The Municipal Yearbook*, an annual publication that chronicles developments in urban politics and administration, included extensive reports on council-manager government over this period and I use it to create a new panel data set of 307 cities over the 1946-1966 period. *The Municipal Yearbook* also reports data on per capita municipal expenditures, which I employ as the dependent variable of the analysis. Unlike cross-sectional data, panel data allows us to observe precisely when council-manager government was adopted and the subsequent changes in municipal expenditure patterns. This is a tremendous advantage over cross-sectional data, but it does not completely eliminate the possibility that another factor is driving the results. An unobserved factor could be causing both the adoption of council-manager government and the reduction in expenditures.

The great difficulty of empirical assessments of institutional effects is the possibility of misattributing changes in municipal behavior to institutional change [34]. Cities did not randomly adopt council-manager government so it is possible that reverse causality or some type of endogeneity could be responsible for any observed differences in expenditure patterns after the adoption of council-manager government. The use of panel data provides a partial solution to the endogeneity and reverse causality problems. Panel data estimators, such as fixed effects models, account for individual-level idiosyncrasy by allowing the intercept of the regression equations to vary across individual units [18]. However, this is not a complete

solution because there is still the possibility of omitted variable bias in the regression models.

The estimation strategy I pursue attempts to solve the endogeneity and reverse causality problems more completely than panel data and fixed effects estimation. I focus on potentially exogenous variation in state-level attributes to differentiate between cities that were able to commit credibly to council-manager government and those that were not. The early 20th century United States offers a useful laboratory for assessing the importance of commitment technology. Some state legislatures adopted home-rule policies that enabled cities to play a large role shaping the rules of municipal governance. These home-rule institutions rendered council-manager government a less credible commitment because it allowed local electorates to undo the institution in the future. In contrast, electorates in cities that lacked state-granted home rule were not able to renounce them in the future. These restrictions made the adoption of council-manager government a much stronger commitment than it was in cities that possessed home rule. I find strong empirical evidence that the strength of commitment technology has a negative association with municipal expenditures after the adoption of council-manager government. To the extent that home-rule institutions are not the result of endogenous choice processes, this estimation strategy might provide evidence of a causal relationship between credible adoption and changes in municipal expenditure behavior. In the penultimate section I evaluate whether these coefficient estimates can be interpreted causally and find tentative evidence that a causal interpretation is appropriate.

Although the specifics of this historical episode may not be of pressing interest to contemporary observers, the underlying processes illuminate the logic of institutional change. The findings travel to a wide variety of settings and provide validation for commitment-based explanations of institutional change. In recent years, a rich and exciting literature examining the effects of political institutions on economic outcomes has blossomed. Much of this scholarship analyzes cross-national variations in institutional form [2]. Examining subnational institutions can allow scholars to reach stronger inferences as there are more observations

to analyze and the unmeasurable and unobservable variation between units of analysis is less problematic than in the case of cross-national comparisons [7]. Of course, there is an inevitable tradeoff in the generalizability of these findings because they are only from one country. The subnational approach is ultimately complementary with cross-national studies. They are each part of a research agenda that seeks to identify the effects of institutional structure on economic and political outcomes. These commitment-based explanations are prominent in the literature on political institutions, but they are far from the dominant analytic paradigm. Historical institutionalism, constructivist anti-rationalist approaches, and others are competing schools of thought. I believe that commitment is a more compelling model and that this study of the urban reform movement supports this contention.

2 Historical Overview of Urban Reform

Between the end of the Civil War and the World War I armistice, the United States experienced an urban transformation. In 1880 28.2 percent of U.S. citizens resided in cities and by 1920 this figure had nearly doubled to 51.2 percent [21]. The rise of political machines, large-scale municipal infrastructure projects, and a wave of foreign immigration from eastern and southern Europe were some of the most prominent novelties of urban life during this era. Each of these developments helped move cities from a peripheral to a central role in U.S. politics. Municipalities financed their growth through massive borrowing in the credit markets, allocated public works contracts through graft, and handed out government employment according to the logic of patronage [38].

The urban reform movement was created to do battle with this world. While control of city government may have seemed a trifling matter to earlier generations, the increased power of municipal government, particularly in terms of its taxing and spending authority, made the reins of power a valuable prize. This prize proved increasingly elusive to the Anglo-Saxon

elite as immigrants flooded the cities and political machines organized them into powerful voting blocs. Reformers found much of the new urban politics disreputable and set about to reverse what they saw to be its most egregious excesses. The disease they diagnosed consisted of many symptoms: “Wastefulness, a disregard of the public interest where private ones were concerned; inefficiency; frequent corruption and scandals; and an indifference on the part of the citizen, at once dangerous to the welfare of the community and subversive of popular government” [46].

Many of the reform battles took place along ethnic and class cleavages. Business groups, professionals, and educated individuals of Anglo-Saxon stock, and in some cities the assimilated Jewish population, were frequently pitted against poorer Irish, Italian, and eastern European immigrants. As Link argues, “[b]ecause they [businessmen] hated waste, mismanagement, and high taxes, they, together with their friends in the legal profession, often furnished the leadership of good government campaigns” [24]. Their origins in the business world had a prominent role in shaping the reformers’ rhetoric. The movement made appeals to efficiency, professional administration, and good government in face of the waste and corruption that they identified in the political machines and the new urban politics [43].

The reformers believed that the problems of cities could be best addressed using the management techniques of the private sector. The city was not a diverse group of preferences that the government ought to aggregate passively, but instead it was an entity that should be operated like a corporation. In their conceptualization, just as a corporation exists to maximize the profits of its shareholders, a city exists to maximize the wellbeing of its residents. The reformers envisioned cities with well-maintained infrastructure, efficient public services, disinterested managers, and professionalized bureaucracy. To this end, they organized advocacy groups — the National Municipal League, the National Short Ballot Organization, the International City Managers’ Association, and innumerable local organizations — with the intention of advancing the sundry components of the broader reform

agenda.

At its core, reform was more concerned with changing the structure of government than particular policies. As Finegold writes, the urban reform movement was an “attempt to change what is systematic about government, rather than, or in addition to, what is transitory” [13]. At one time or another, the reformers’ armamentarium of institutional adjustments consisted of non-partisan elections, at-large districts, commission government, civil service reform, and the council-manager plan. Reformers recognized that they and their sympathizers made up a distinct minority of municipal voters and their goals could not be achieved directly through the ballot box. Although the reformers cloaked their appeals in the language of the public good, the actual substance of their institutional changes systematically transferred power away from the poorer and less-assimilated segments of the population that made up a large portion of the urban electorate [37]. The reform movement was a major threat to the livelihoods of many recent immigrants and impoverished citizens. To take one example, civil service reform forced citizens to pass a series of tests before they could obtain a government position [33]. This institutional change had the obvious effect of preventing many of the patrons of political machines from holding a municipal job and the desirable wage that typically came with it. Other changes, such as non-partisan and at-large elections, made it harder for the political machines to maintain control over the city government and dispense favors to their supporters.

For much of the movement’s history, the central goal of the reformers was the implementation of council-manager government. The council-manager plan consisted of three interlocking components: the unification of political authority in the city council, a short ballot, and the appointment of a professional manager whose employment was at the discretion of the city council. The designers of council-manager government were concerned about the diffusion of political authority (and responsibility) among numerous government agencies and sought to centralize control within the city council. The short ballot intended to

prevent politicians from holding government positions, such as police and fire commissioner, that reformers believed should be occupied by experienced individuals. Finally, the most important feature of the plan was the appointment of a professional city administrator who had formal authority over city management.

The reformers believed that these three components were powerful tools for combating the political machines. By ceding control over government job allocation to the city manager, the reformers hoped to end the patronage they associated with the machine bosses' reign. The city manager did not derive her power from the electorate so there was no incentive to dole out favors and patronage to the city's residents. More importantly, the manager did not cut her teeth in local politics, but was instead professionally trained and often from a different city altogether. Reformers believed that this would limit opportunities for graft and quid pro quos in municipal affairs. Council-manager government would lead to both the more efficient allocation of city resources and a decrease in the level of municipal expenditures [10]. By shifting formal political authority away from a politicized and electorally-sensitive mayor and toward an insulated and professionalized administrator, council-manager government attempted to muzzle the city's urge to spend.

Despite their sizable disadvantages at the ballot box, the reformers were able to achieve many of the institutional changes that they sought. By the reform movement's last gasps in the 1970s, council-manager government was a reality in thousands of U.S. municipalities. Although larger cities proved more resistant to the council-manager plan and other reforms, these institutional changes did make significant inroads into these cities. Even cities where ethnics and labor were dominant, adopted council-manager government and other organs of the reform movement [38]. To understand the ultimate sources of the reformers' success requires us to leave behind the historical description of the reform movement and examine game-theoretic models of institutional change.

3 Credibility and Commitment

Commitment is a foundational problem in political economy [9, 29, 32, 41]. While private actors can write enforceable and legally-binding contracts in order to undertake transactions that would otherwise be impossible [45], the fundamental power asymmetry between the state and private citizens makes contracting a non-credible solution to the commitment problem. Who is to enforce the contract when the government is itself a party to it? Institutions can solve the commitment problem and enable political actors to achieve Pareto-improving gains by reallocating formal political authority. Independent central banks [22], parliamentary government [30], and legislative committees [42] are examples of institutions that transfer power away from political actors, but allow political actors to achieve other desired ends by committing themselves to a particular course of action. Just as Odysseus was able to hear the songs of the Sirens by tying himself to the mast, political institutions that strategically restrain actors can permit the actors to enjoy outcomes that would be impossible in the institutions' absence.

The concept of subgame perfect Nash equilibrium, as introduced by Selten [36], is essential to an understanding of the commitment problem. In extensive form games, a Nash equilibrium is only subgame perfect when each players' moves are credible in each of the subgames in the tree. The broader concept of Nash equilibria is less useful in the context of these sequential games because it generates equilibria that cannot be attained by rational egoistic actors. The narrower concept of subgame perfection is necessary to differentiate between equilibria that can be reasonably achieved and those that cannot.

For illustrative purposes, consider the following sequential game. There are two actors, the Rich and the Poor, who are endowed with income Y^R and Y^P respectively.¹ The Poor

¹One could object to the model on the grounds that it reifies groups and does not properly account for individual-level motivations. Collective action only emerges after individuals are adequately incentivized [31]. While this criticism is well-placed, I am making the strong, but historically accurate, assumption that the collective action problem had already been solved by the midpoint of the 20th century and that each of these groups were coherent and organized political actors. By making this assumption I am able to begin the

are a majority of voters and control the government so they determine the level of municipal expenditures. The expenditures are costlessly redistributed to the Poor and the Rich have to finance all of the expenditures. The Rich move first and determine whether they leave the city or stay within it. If the Rich leave they pay the cost of moving, $\mu > 0$, the game ends with no municipal expenditures, and the Poor receive no redistribution. If the Rich stay, the Poor move to determine whether government expenditures will be high, χ^H , or low, χ^L . The game tree with the players' strategies and payoffs is below. (Throughout this paper, the payoffs to the Rich are placed first and the Poor's payoffs second in every game tree).

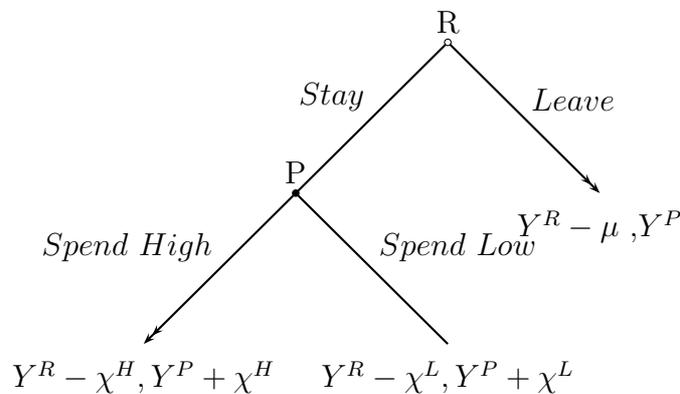


Figure 1: A commitment problem

When $\chi^H > \mu$ the unique subgame perfect Nash equilibrium is *(Leave, Spend High)*, the path of play is *(Leave)*, and the equilibrium payoffs are $Y^R - \mu, Y^P$. (The equilibrium strategies are drawn with arrows in the game tree). The Poor would obviously prefer for the Rich to stay in the city, but there is no way for them to commit to a low level of expenditures once the Rich have chosen to stay in the city. Once the Rich have made this move, the Poor will certainly choose the high level of expenditures because $Y^P + \chi^H > Y^P + \chi^L$. By backward induction [28], when $\chi^H > \mu$ the Rich will prefer to leave the city. This outcome is Pareto inferior to the equilibrium *(Stay, Spend Low)*.

Why can't the Poor simply promise to play *(Spend Low)*? By making a credible promise to this effect, the Poor would induce the Rich to stay in the city whenever $\mu > \chi^L$. However,

analysis later in the history of urban reform.

there is no way, within the constraints of the game, for the Poor to commit to following through with this promise once the Rich have made their move. After the Rich move, there is no external “check” on the actions of the poor. They are unconstrained by anything the Rich may do in the future so they simply choose to maximize their welfare by playing (*Spend High*). The Rich recognize the fundamental interests of the Poor and know that any promise by the Poor to play (*Spend Low*) is simply a promise, nothing more.

One could quibble with this logic by arguing that strategic interactions do not take the sequential form displayed here. The commitment problem ultimately springs from the precise temporal ordering displayed here. There are two primary objections to the way the model is formulated. First, it is possible that there are future ramifications if the Poor choose to play (*Spend High*). If the Rich had the opportunity to reverse their initial move after the Poor make their move the equilibrium path of play would be quite different. Knowing that the Rich have the option of leaving after the expenditure level is set would induce the Poor to play (*Spend Low*) and the Rich to play (*Stay*) in the initial node whenever $\mu > \chi^L$, as seen in Figure 2.

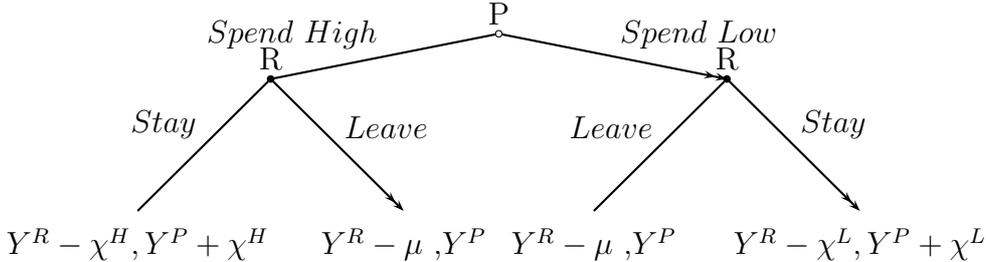


Figure 2: An external check on the Poor

A second more fundamental objection is that the game should not be represented in the extensive form of a game tree. Instead, it is possible to imagine that the interactions between Poor and Rich take the form of an infinite reciprocation game. If the actors value the future highly, cooperation could be sustained simply by the mutual understanding that strategic interaction between the two actors is to endure for a long period of time [6]. If both the Poor

and the Rich believed that the threat of the Rich leaving would last indefinitely then it is possible that the Pareto-optimal equilibrium could be achieved simply by this mutual belief.

Although both of these objections are based on solid game-theoretic logic, they are misplaced in this particular context. The reform movement was based on a transitory change in the de facto allocation of power. Advances in transportation technology, such as the invention of the electric streetcar in 1887, made residing outside of the city limits feasible for many wealthy citizens [40]. This previously nonexistent exit option increased the political power of those citizens who could leave the city. The municipality would now have to be more responsive to their desires [17]. They could channel this newfound power into demands for policies that were more to their liking. But, as argued in the previous section, the reformers devoted their energies to changing the structure of government Why?

The reformers were concerned that their power would soon weaken and needed to implement institutional changes to achieve their desired ends. They were wary of future political, economic, and technological changes that might reduce their de facto political power and wanted assurances that their political preferences would be influential not only now, but also into the future. The commitment problem is likely to be more salient when transactions have a temporal dimension. As numerous examples illustrate [3], political conflict is frequently concerned with future allocations of authority and power.

Although the Pareto-superior outcome of $(Y^R - \chi^L, Y^P + \chi^L)$ cannot be reached within the constraints of this game, there are other tools that the Poor could theoretically employ to achieve a more desired outcome. Institutional change is one potential solution to the commitment problem. Institutions transfer formal political power from the hands of one actor to another and can serve to solve the commitment problem [30]. As Acemoglu and Robinson argue in their commitment-based study of democracy, “democratization enables the citizens not only to be more powerful today but also in the future relative to an alternative regime that is nondemocratic” [4]. Democracy for the citizens in their analysis serves precisely the

same purpose that reform does for the Rich in this paper. When an actor temporarily holds de facto political power it will strive to secure this power into the future by changing the institutional rules that aggregate political preferences.

When $\chi^L \leq \mu < \chi^H$, an institutional rearrangement of power can solve the commitment problem. (For modeling convenience, I assume that when $\chi^L = \mu$ the Rich play (*Stay*) and when $\mu = \chi^H$ the Rich play (*Leave*)). Consider the possibility where the Poor are able to “reform” the government. By reforming government, authority over municipal finances is transferred from the Poor to the the Rich. In other words, the Rich are now able to determine the expenditure level as seen in the game tree below.

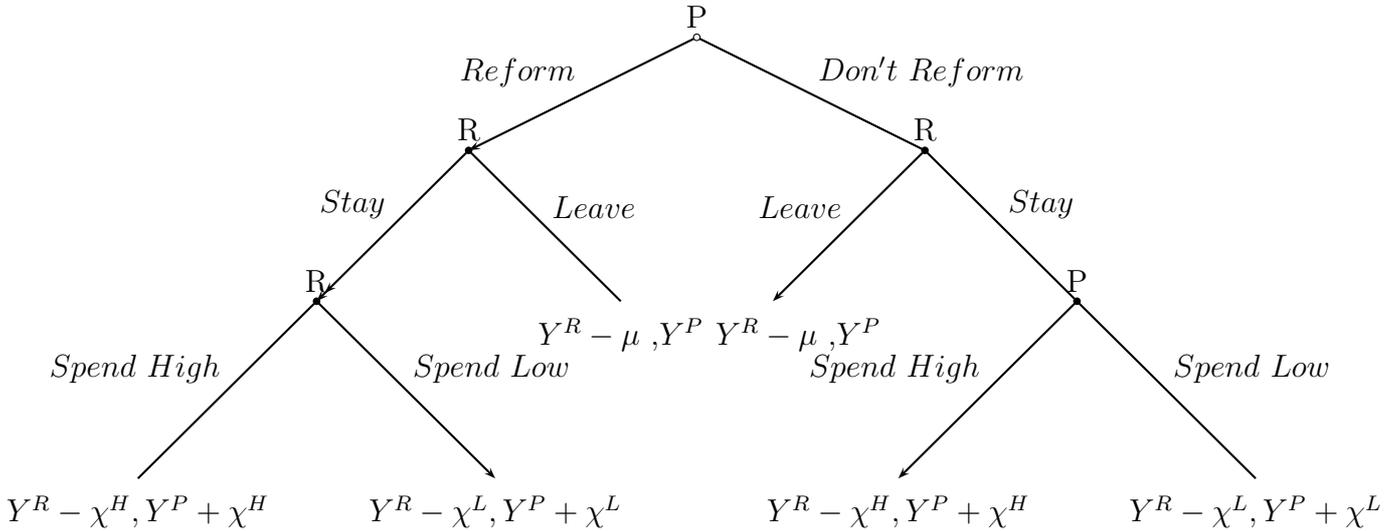


Figure 3: Commitment via institutional change

As this game reveals, by strategically transferring power the Poor are able to achieve a Pareto-superior equilibrium to the one they experience when they do not reform the government. However, the adoption of a new form of government is not a guaranteed solution to the commitment problem. A reformed government can only provide commitment when all relevant parties expect the institution to endure. If one player has the option of removing the institution in the future then the adoption of an institution is not likely to induce the desired changes in behavior that result in a Pareto-superior outcome. When an institutional adop-

tion is perceived as ephemeral, players will not believe that the transfer of power will endure and the commitment problem is not solved. As North and Weingast wisely observe: “Rules the sovereign can readily revise differ significantly in their implications for performance from exactly the same rules when not subject to revision” [30].

This distinction between institutions and ephemera is fundamental to the arguments in the remainder of this paper, especially in the empirical section, and it is worthwhile to explore it formally. Ephemera can be modeled as a continuation game where players have a probabilistic opportunity to undo the institution after its adoption [4]. The continuation game includes an additional player, Nature. In the event that the Poor choose to reform the municipal government, Nature randomly determines if the institution will endure. With probability p , reform continues and the Rich determine the expenditure level. With probability $1 - p$ reform is undone and the Poor get to determine the expenditure level, as seen in Figure 4. (From the previous analysis we already know that when the Poor choose the expenditure level in the terminal node they play (*Spend High*) and the Rich make the decision they play (*Spend Low*). To save space, I omit these subgames from the game tree in Figure 4 and simply write the payoffs after Nature moves).

When $p = 1$ all actors believe that the institution will endure and the Poor are able to commit to reform. This allows the adoption of reform institutions to achieve the Pareto-superior outcome that the Poor desire. However, when $p = 0$ the Rich know that they will not be able to choose their desired expenditure level, χ^L , and will leave the city. Given the model’s assumptions we can solve for the minimum value of p , which I will denote as p^* , that will induce the Rich to remain in the city after the implementation of reform. After the Poor choose to play (*Reform*) in the initial node, the Rich are forced to choose between the expected utility of remaining in the city, $p^*(Y^R - \chi^L) + (1 - p^*)(Y^R - \chi^H)$, and the payoff from leaving, $Y^R - \mu$. When these two values are equal, the adoption of reform will be sufficiently credible to induce the Rich to play (*Stay*).

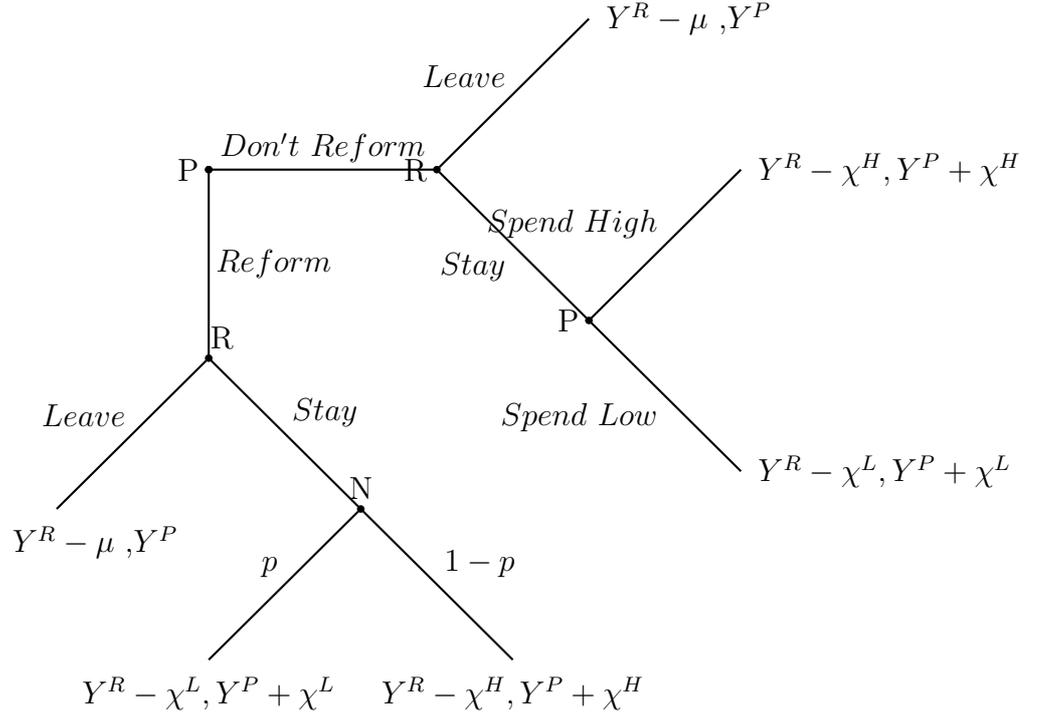


Figure 4: Institutions and ephemera

$$p^*(Y^R - \chi^L) + (1 - p^*)(Y^R - \chi^H) = Y^R - \mu \quad (1)$$

Solving for p^* in equation 1 yields:

$$p^* = \frac{\mu - \chi^H}{\chi^L - \chi^H} \quad (2)$$

For p^* to abide by the fundamental axioms of probability it must be the case that $0 \leq p^* \leq 1$. Above we made the assumption that $\chi^L \leq \mu < \chi^H$ implying that both the numerator, $\mu - \chi^H$, and the denominator, $\chi^L - \chi^H$, of equation 2 are negative so p^* will always be positive. Additionally, p^* will always be less than or equal to 1 because the numerator will always be less than or equal to the denominator in equation 2. So p^* will always be in the required interval when the assumption $\chi^L \leq \mu < \chi^H$ holds.

Now that we know equation 2 will always yield a meaningful value, we can turn to interpreting the implications of the model. When $p < \frac{\mu - \chi^H}{\chi^L - \chi^H}$ urban reform will not

be sufficiently credible to keep the Rich in the city. Over this interval, the Poor will be indifferent between playing (*Reform*) and (*Don't Reform*) and, for convenience, I assume that they play (*Don't Reform*). When $p \geq \frac{\mu - \chi^H}{\chi^L - \chi^H}$ reform is sufficiently credible and the Rich will choose to stay in the city. By backward induction, the Poor will play (*Reform*) and commitment is achieved through the institutional change. Although reform will take place under these circumstances this does not necessarily mean that municipal expenditures will decrease. There is still a $(1 - p)$ chance that reform will be overturned so with probability $(1 - p)$ municipal expenditures will equal χ^H and with probability p municipal expenditures will equal χ^L .

The major implication of this model is that council-manager government will not result in uniform reductions in expenditure. In expectation, cities with higher values of p will have greater reductions in municipal expenditures and cities with lower values of p will have smaller reductions in municipal expenditures. In contrast to the urban reform literature that treats all adoptions of the council-manager plan equally, this game-theoretic analysis reveals that, in expectation, the effects of council-manager government hinge on p . As the strength of the available commitment technology increases, municipal expenditures will decrease probabilistically. Given a large enough sample, this theoretical perspective leads us to expect a significant differential in the after adoption spending behavior of cities with high and low values of p .

What factors determine the value of p ? Theory suggests that p would vary based on other attributes of the local governmental landscape. If a city permitted changes to its governmental structure through the ballot initiative or referendum then this would tend to decrease p . As soon as the threat of the Rich leaving the city dissipated, the electorate would be able to reverse council-manager government. The adoption of reform would also be more credible if it was handed down from a higher level of government. In the context of urban reform, if a state legislature passed a regulation obligating cities to have council-manager

government then the value of p would be higher than if council-manager government was adopted via internal municipal deliberations. Even if the threat of the reformers exiting the city vanished, the municipal government would be stuck with council-manager government because the state legislature imposed it from above. This idea is closely related to the international relations literature that argues that international institutions are commitment devices for sovereign nation states [19, 26]. However, it is important to recognize that this device will prove ineffectual in the event that it is not in the interests of the higher level of government to preserve the institutional change. State legislatures might be fairly strong commitment devices because rural and wealthier interests that would look favorably upon reform are usually overrepresented within their chambers.

The above logic would lead us to suspect that home-rule institutions, like those that many state legislatures adopted in the early 20th century, would weaken the commitment technology available to cities. When cities have home rule, state governments are not a viable commitment device. The increased sovereignty that flows from home rule increases the capacity of local electorates to alter the structure of municipal government. The net effect of home rule is a diminished ability to commit to the urban reform institutions into the future. In the empirical section, I will use the presence of home rule to differentiate between cities with low and high values of p , but it is first necessary to derive the expected implications of council-manager government in each type of city.

Formally, I denote p^{HR} as the probability of council-manager government enduring in a city that has home-rule institutions and p^{NHR} as the probability of council-manager government enduring in a city that does not have home-rule institutions. By the above argument, $p^{HR} < p^{NHR}$ because the adoption of council-manager government is much harder to reverse in the absence of home-rule institutions.² Because council-manager government

²I am assuming $\frac{\mu - \chi^H}{\chi^L - \chi^H} < p^{HR} < p^{NHR}$ because otherwise p would be too low for council-manager government to be adopted in the first place.

is expected to persist with probability p , in which case $\chi = \chi^L$, and is expected to be reversed with probability $(1 - p)$, in which case $\chi = \chi^H$, we can derive the expected expenditure levels in cities with and without home rule. In expectation, cities with home rule will spend $E(\chi|p^{HR}) = p^{HR}\chi^L + (1 - p^{HR})\chi^H$ while cities without home rule will spend $E(\chi|p^{NHR}) = p^{NHR}\chi^L + (1 - p^{NHR})\chi^H$. As $p^{HR} < p^{NHR}$, it immediately follows that $E(\chi|p^{HR}) > E(\chi|p^{NHR})$. The model and qualitative logic suggest that, upon the adoption of council-manager government, cities with home rule will have much smaller reductions in their expenditures than cities that lack home rule. Although home rule is not a perfect operationalization of p it cleanly distinguishes between cities where council-manager government could be directly overturned by an electorate that, once the threat of the Rich's exit vanished, did not benefit from the continued presence of home rule.

Before proceeding to the empirical analysis, it is valuable to identify what the formal model does not predict and what it does predict. The model does not predict that council-manager government cities will have lower expenditure levels than their non-council manager brethren. Imagine a scenario where the Rich leave the city because they are concerned about high expenditure levels and the adoption of council-manager government is not credible, then the model predicts municipal expenditures of \$0 because there will be no one for the Poor to tax but themselves. Another possibility is that some cities are unable to resolve the collective action problem so there is no initial impetus for council-manager government. The model is not a particularly useful analytic tool for comparing the expenditure patterns of cities that have home rule and those that do not. The model's strengths instead lie in differentiating between cities that adopt council-manager government. If the model is useful³ then there will be a statistically significant difference in the spending patterns of cities that adopt council-manager and possess home rule and those that do not have home rule. Empirically, this can

³The criterion for judging a model is "usefulness", as opposed to "truthfulness." A deductive model, such as the one in this section, cannot be true or false because it is simply a representation of reality, not a statement of fact. In this respect a model is analogous to a map [16] and can only be judged on its relative usefulness.

be tested by employing an F test to see if the adoption coefficient estimates are statistically different in cities with and without home rule.

4 Data and Methods

From the first case of its adoption in Staunton, Va. in 1908, council-manager government spread rapidly after the first large city, Dayton, Ohio, implemented the plan in 1914. With Dayton as a showcase, advocates for council-manager government, most notably the International City Managers' Association, championed the efficiency and professionalism that it contended emanated from the plan. I employ council-manager government as the main institutional predictor of municipal expenditures. Although the reform movement advocated numerous institutional changes to limit the power of the political machines, council-manager government was the most prominent institutional change from the onset of World War II until the reform movement petered out in the 1970s. Additionally, the data available on council-manager government are more comprehensive and consistent than for any other municipal reform. These realities and practical considerations make council-manager government the best available operationalization of urban reform.

In a thoughtful piece of applied work, Ruhil [35] applies modern econometric estimation techniques to a panel data set of 222 cities over a 21-year period to examine the relationship between urban political reform and municipal expenditures. Earlier studies that used cross-sectional data or panel data for a small number of cities reached conflicting conclusions about the effects of urban reform. Ruhil illuminates the flaws in much of this earlier work and in a series of fixed effects models shows that the adoption of council-manager government is associated with a subsequent decrease in municipal expenditures. However, the decrease is fleeting and cities quickly return to pre-adoption levels.

Using the ICMA *Municipal Yearbook*, I collected a panel of 307 cities for this analysis.

The Municipal Yearbook is the only reliable source of information on cities' use of the council-manager governmental form and for many of the years 1946-1966 it only reports governmental form data for cities with population greater than 25,000. To minimize potential attrition in the panel, I restricted the scope of my analysis to cities with an enumerated population of 30,000 or more at the time of the 1930 U.S. Census. This decision potentially limits the generalizability of my empirical findings to medium and large cities. Additionally, employing the 1930 Census as the cutpoint for the sampling frame results in a geographic distribution of cities that is quite different from the one that prevailed during the last year of the panel, 1966. Many Western cities were sparsely populated in 1930, but grew tremendously in the decades following World War II. Conversely, Northeastern cities are overrepresented in the sample. The geographic distribution of the cities raises doubts about the representativeness of the sample. Despite these shortcomings, this purposive sample will have to suffice to reach empirical conclusions about the relationships between governmental form and municipal expenditures.

The Municipal Yearbook also reports data on the dependent variable of interest, municipal expenditures. These figures are broken up into several categories, educational spending, debt service, and the like, but I restrict my analysis to the most consistently available series, general expenditures. I make inflation adjustments to all of the municipal expenditure values using the Bureau of Labor Statistics' Consumer Price Index. Dollar values are standardized to \$100 in 1982 terms. To generate per capita figures I divide by municipal population. The population data are from the U.S. Census' Consolidated City and County Data Book as maintained by the Inter-University Consortium for Political and Social Research [39]. Unfortunately, population data is not available for every year between 1946-1966 so I linearly interpolate cities' population for all years that are unavailable.

In addition to the dependent variable and primary independent variable of interest, I include several covariates as control variables. Theory and Ruhil suggest that municipal ex-

penditures would tend to increase with the percentage of the population that is non-white, the median resident's age, and the amount of economic activity related to manufacturing. To the extent that these variables are associated with the adoption of council-manager government, omitting them from the regression models would lead to biased estimates for the coefficient on council-manager government. I collect all of these data from the Consolidated City and County Data Book [39] as well.

After plotting the data, I begin by fitting OLS models that pool the data across cities. Although this approach gives us a rough feel for the empirical relationship between council-manager government and municipal expenditures it is problematic. Simply pooling the data and fitting an OLS regression model is likely to generate biased coefficient estimates [20]. A pooled sample ignores the inherent variation across municipalities and makes the dubious assumption that each city-year is an independent observation. I proceed with panel data estimators, namely fixed effects models, that treat each city as an independent and idiosyncratic unit of analysis. Fixed effects models are justified in the context of this study on both theoretical grounds and by the Hausman specification test. Theory also suggests that heteroskedasticity is a concern in these data. The variance of per capita municipal expenditures should increase with the population of a city. One explanation for why this may be the case is that larger cities have the potential to perform more government services than smaller cities. Larger cities will display more variance in the types of services that they provide to their residents. Some will construct athletic stadiums, others will operate public health programs, and some will implement none of these, opting instead to restrict their expenditures. A higher proportion of small cities will choose to limit themselves to this set of core services, such as road maintenance, and police and fire departments.

In expectation, heteroskedasticity does not bias the point estimate of the $\hat{\beta}$ matrix, but it does introduce the possibility of mistaken statistical inferences about the sign of particular β_j 's. To test for the presence of heteroskedasticity, I employ the Breusch-Pagan test. This

diagnostic test's main virtue is that it relies on weak assumptions about the precise form of the heteroskedasticity. The test generates high test statistic values and leads us to reject the null hypothesis of homoskedasticity at the 99 percent significance level. To correct for heteroskedasticity I employ Huber-White robust standard errors in all model specifications [44]. White's proof is an asymptotic result so it is theoretically uncertain whether a particular sample size is sufficiently large to use robust standard errors, but with a sample size of 307 cities I am reasonably confident in the robust standard errors.

After fitting several models that treat all adoptions of council-manager government equivalently, I examine the effect of commitment technology, p in the terminology of the formal model, on municipal expenditures. As discussed at length above, cities where council-manager government had a greater probability of enduring should have larger reductions in municipal expenditures than those cities that had smaller probabilities. To operationalize p I rely on *Municipal Yearbook* data on home-rule legislation. By 1935, 19 states had adopted home rule and I use this list of states to differentiate between cities with high and low values of p . As discussed in the previous section, cities with p^{HR} are predicted to spend at higher rates than cities with p^{NHR} and to test this hypothesis I employ an F test. After creating an indicator variable for adoptions in cities with home rule and adoptions in cities without home rule, I fit fixed effects regression models that include indicator variables for both types of adoption and then use an F test to see if the coefficient estimates are systematically different between p^{HR} and p^{NHR} cities. I also test several model specifications that include lagged independent variables to probe further the temporal nature of the relationship between council-manager government adoption and municipal expenditures.

Although Ruhil's paper is the best-designed econometric investigation of council-manager government it suffers from a number of shortcomings. This study attempts to improve on Ruhil's in a number of dimensions. As discussed above, the panel data set for this analysis is larger than Ruhil's and potentially more representative. I also make heteroskedasticity cor-

reactions. Most importantly, my empirical analysis is motivated by theoretical considerations and this allows me to elucidate statistical regularities that Ruhil does not investigate. As shown below, p^{NHR} is strongly associated with statistically significant decreases in municipal expenditures and cities with p^{NHR} spend at significantly lower rates than cities with p^{HR} . The strength of commitment technology plays a major mediating role in the relationship between the adoption of council-manager government and city expenditure behavior.

5 Empirical Results

I began by calculating the average per capita expenditures value for all of the cities with council-manager government in a given year. After making this calculation for each of the 21 years in the sample I plotted the time-series expenditure graph for all of the cities with council-manager government. For comparison purposes, I repeated this procedure for cities that did not have council-manager government. Several trends are immediately apparent from the graph. Municipal expenditures, even after controlling for inflation, increased dramatically over the period under analysis and the general pattern of expenditures is quite similar in both types of cities. For most of the early years of the sample, average expenditures are actually higher in cities with council-manager government, but starting in 1954 council-manager cities spend at lower levels and this continues for the remainder of the sample.

While this graph reveals some relevant information, it does not offer strong evidence on the relationship between council-manager government and municipal expenditures. There are several limitations that prevent us from reaching any conclusions based on the evidence in this graph. Our true interest does not lie in those cities that possess council-manager government at a particular time, but the changes in expenditure behavior that occur when a city transitions from a mayor-council or commission government to the council-manager plan. We need to employ a measure of whether a city *adopts* council-manager government in

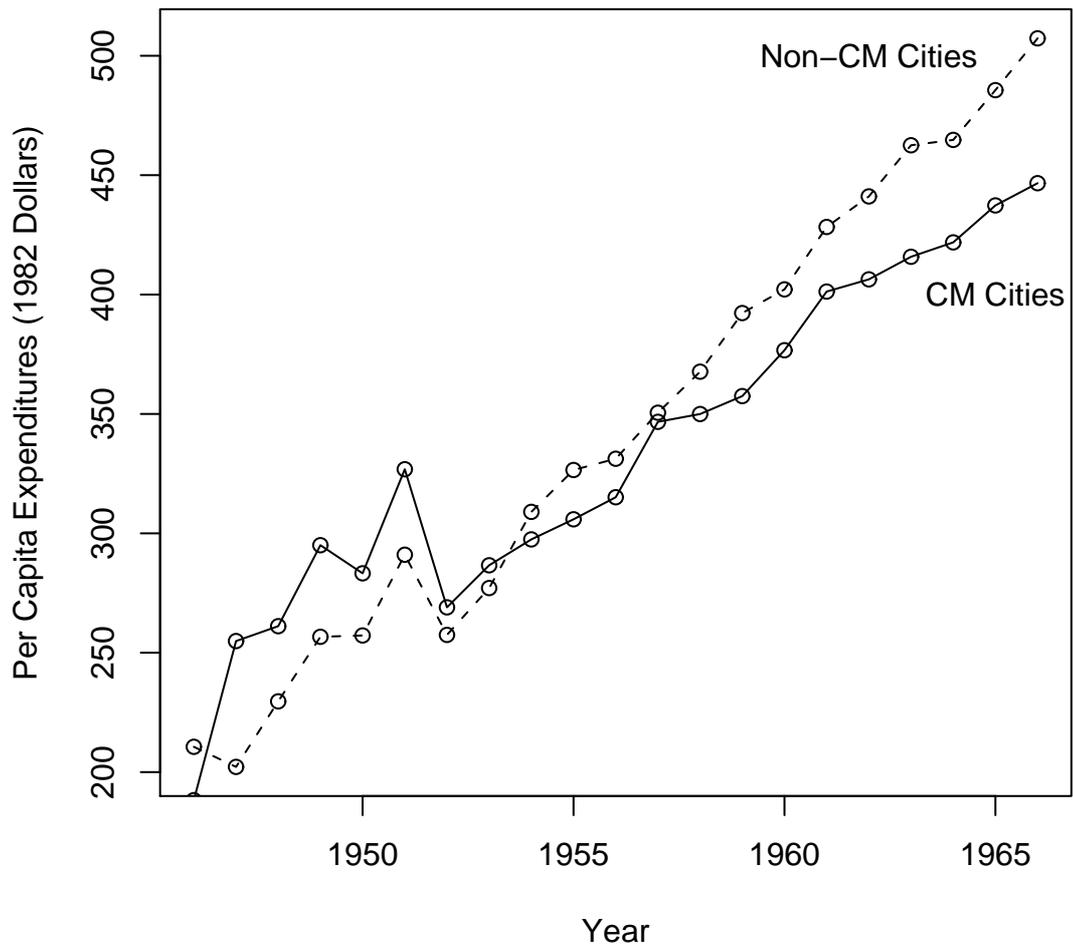


Figure 5: Time-series plots of mean municipal expenditures for cities with and without council-manager government.

a given year not simply whether it has the institution. It may be the case that cities possess council-manager government because they have unobserved impulses to spend at high levels. Thus, cities that have council-manager government may appear to have elevated municipal expenditures, but they would have spent even more in the absence of council-manager government. A related point is the possibility of omitted variables that are confounding the observed relationship. Council-manager government is not randomly assigned to cities so there may be other factors that affect both whether a city has council-manager government and its municipal expenditures. Finally, by aggregating the data into a mean measure for each year of the time series I have discarded very useful information on the individual-level variation in expenditure patterns. This information is quite valuable in making statistical inferences, as we will see in the fixed effects models below.

As evidence of the importance of individual-level data, consider the case of Richmond, Va. The city has dramatically higher expenditures than any other city in the sample. The extent of this idiosyncrasy can be seen directly in a time-series plot of the mean expenditures in the 306 cities and a separate plot for Richmond. Richmond expenditure levels are approximately 10 times higher than the average city in the sample and are the highest of all the 307 cities for all 21 years. While this graph does not reveal why Richmond displayed such extreme spending behavior, it does alert us to the presence of an observation that may exert high leverage over the statistical results. (In the regression models that follow I report results that include all of the cities in the sample. I separately fit models that omitted Richmond, but these results were qualitatively similar so I do not report them here).

To gain a better understanding of the relationship between adoption of council-manager government and municipal expenditures it is useful to fit multiple regression models. Multiple regression allows us to control for potential confounding covariates. Following Ruhil's [35] example, I include the median age, the percentage of the population that is non-white, and the percentage of the working population employed in manufacturing as control variables.

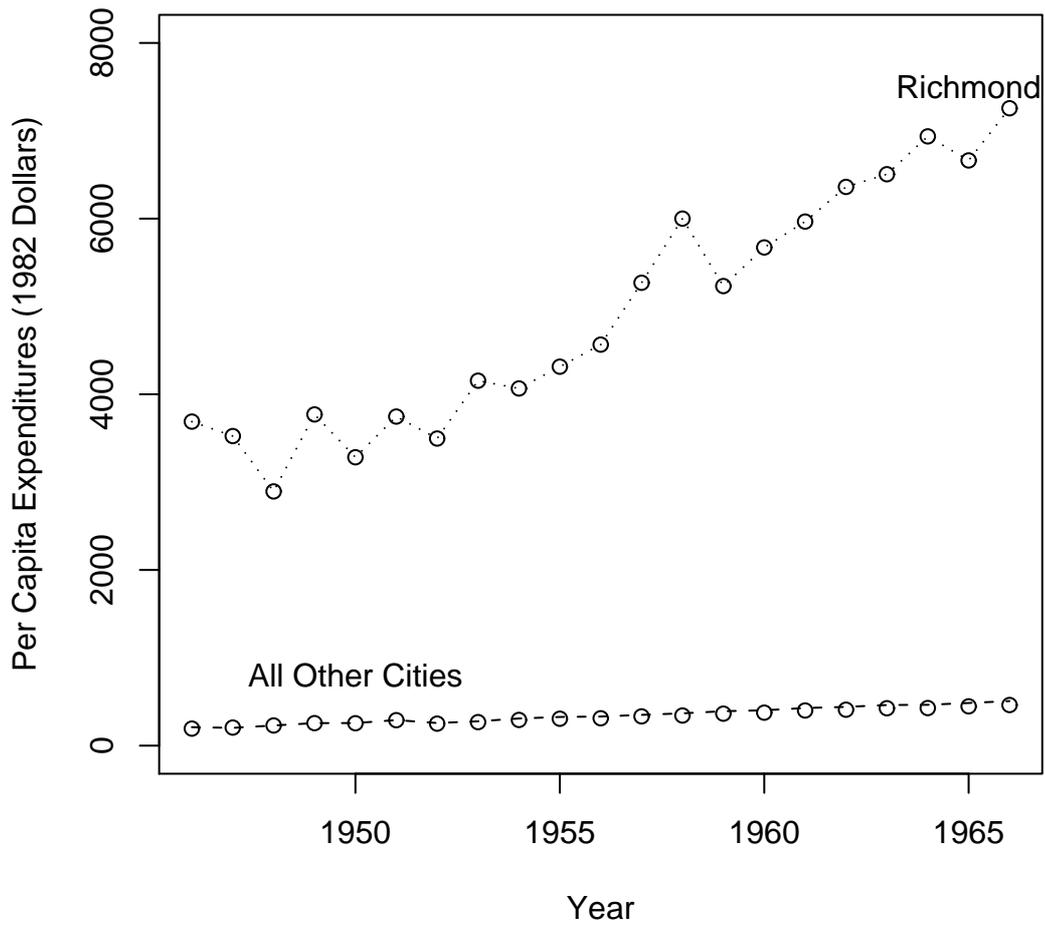


Figure 6: Time-series plot of mean municipal expenditures for 306 cities in the sample and a separate plot for Richmond, Va.

	Model 1	Model 2	Model 3
	(1)	(2)	(3)
Adopt	-23.294 (69.898)		-37.759 (70.291)
CM Govt		22.959 (13.233)*	23.780 (13.316)*
Manufacturing (%)	3.323 (.458)***	3.523 (.553)***	3.519 (.553)***
Non-White (%)	1.198 (.350)***	1.244 (.345)***	1.235 (.344)***
Median Age	-.518 (2.211)	-.147 (2.034)	-.131 (2.036)
Const.	241.009 (64.054)***	214.582 (53.307)***	214.294 (53.369)***
Obs.	6429	6429	6429
R^2	.015	.016	.016
F statistic	42.123	46.925	37.814

*: significant at 90 percent **: significant at 95 percent ***: significant at 99 percent

Table 1: OLS models with robust standard errors in parentheses.

As seen in Table 1, OLS regression models that include the control variables do not yield statistically significant coefficient estimates for the *Adopt* variable. The results in Table 1 are displayed with robust standard errors to take into consideration heteroskedasticity. As hypothesized, the *Adopt* coefficient estimate is negative in both of the models that include it. However, the council-manager government variable is positive and significant at the 95 percent level. As discussed earlier it is quite possible that cities have council-manager government because they possess underlying impulses to spend at high levels.

Econometric theory suggests that pooling the data is inappropriate. Instead, panel data models can better account for the idiosyncratic variation between units of analysis. Two commonly-used panel data estimators are the random effects and fixed effects models. When we are primarily concerned with individual-level variation, fixed effects is the more appropriate estimator. In the context of this paper, fixed effects appears to be the preferred estimation technique as city-level factors account for much more of the variation in expenditures than

other factors such as time trends or regional location. Fixed effects estimates are found by simply demeaning the data for each individual in the sample and then fitting an OLS regression. As this methodological process reveals, it is impossible to estimate coefficients for variables that are time invariant in the sample. Time invariant variables will be equal to their mean for every city in the sample so the demeaning transformation will set all of these variables equal to 0 for all observations and the rank condition for OLS will be violated. When we want to include time invariant variables in a panel data analysis random effects models may be a viable solution.

How do we determine whether fixed or random effects is more desirable in a particular data set? A Hausman specification test can provide empirical evidence about which model is more appropriate. The fixed effects estimator is asymptotically consistent, but is relatively inefficient in comparison to random effects. The random effects model, in contrast, is not consistent. When the Hausman specification test suggests coefficient estimates are systematically different from one another then the fixed effects estimator is more appropriate. In these data, the Hausman test provides strong support for using the fixed effects estimator.

The fixed effects models provide the first solid evidence that the adoption of council-manager government affects subsequent expenditure behavior. As seen in model 1 of Table 2, the coefficient estimate for the *Adopt* variable is negative and significant at the 95 percent level. Notice also that the estimated coefficient is approximately equal to 73.6. This model predicts that adopting council-manager government reduces expenditures by about \$73.6 per capita in 1982 terms. Although this reduction may appear trivial it is actually quite sizable when we consider that the mean level of per capita expenditures in this sample is about \$341 in 1982 terms, after controlling for the other independent variables. This regression model predicts that the adoption of council-manager government reduces spending by more than 20 percent. This is a reasonably large reduction so the coefficient estimate is both statistically and substantively significant [25].

	Model 1	Model 2	Model 3
	(1)	(2)	(3)
Adopt	-73.596 (33.295)**		-92.641 (34.399)***
CM Govt		36.734 (10.608)***	47.385 (11.480)***
Manufacturing (%)	-10.499 (1.062)***	-10.524 (1.062)***	-10.534 (1.056)***
Non-White (%)	16.560 (.857)***	16.504 (.855)***	16.478 (.851)***
Median Age	-12.611 (1.547)***	-12.078 (1.591)***	-11.693 (1.620)***
Const.	897.206 (74.675)***	868.813 (77.205)***	854.239 (78.303)***
Obs. (307 groups)	6429	6429	6429
R^2 within	.202	.202	.205
F statistic	402.545	410.842	334.338

*: significant at 90 percent **: significant at 95 percent ***: significant at 99 percent

Table 2: Fixed effects models with robust standard errors in parentheses.

I fit a separate fixed effects model that only includes the council-manager government indicator and the controls. As in the pooled OLS regressions, the coefficient estimate on the council-manager government variable is positive and statistically significant. Even after employing the fixed effects estimator, the presence of council-manager government appears to be positively associated with municipal expenditures. This empirical finding is more difficult to explain away than in the pooled OLS regression case. The fixed effects model is explicitly taking into account individual-level variation in political institutions and expenditure behavior so this finding appears to be at odds with the hypothesis that council-manager government induces reductions in municipal spending. However, this model is probably suffering from bias because the adoption indicator variable is omitted. The formal model in section 3 does not predict that council-manager cities will have lower expenditures than cities without council-manager government. As discussed above, cities that lack council-manager government may actually spend at lower level than cities that do have council-manager govern-

ment. The model is useful for analyzing changes in expenditure patterns after the adoption of council-manager government.

Despite these caveats, as a sensitivity check, I also fit models that include an indicator variable for the presence of council-manager government. If regression models that include the council-manager government indicator also support the hypothesis we can be more confident in usefulness of the game-theoretic model. In model 3, I include both of these variables and find the prediction that adoption of council-manager government decreases expenditures in the year of adoption by about \$45.2 and then is about \$47.4 higher in the following years. After an initial dropoff, council-manager government appears to be associated with subsequent rises in expenditures, but we must be concerned with the possibility of lurking variables that are associated with council-manager government. As discussed above, council-manager government may not be responsible for the increase, but city attributes that are systematically different between council-manager and non-council-manager cities may be responsible for the positive coefficient estimate.

With these findings in mind, we can now turn to the problem of credibility. As outlined above, I use state-level home-rule institutions to isolate the commitment technology available to cities that adopt council-manager government. In lieu of the *Adopt* variable, I generate two indicator variables *Adopt (Home Rule)*, which takes on a value of 1 when a city that has home rule adopts council-manager government and 0 otherwise, and *Adopt (No Home Rule)*, which takes on a value of 1 when a city that does not have home rule adopts council-manager government and 0 otherwise. While I am interested in changes in expenditure behavior that occur in the year that council-manager government is adopted, I would also like to examine the relationship in the years following adoption. Council-manager government's effect may not manifest itself immediately. For instance, if a city passed its budget in July and adopted council-manager government in December there is no reason to suspect that expenditures would be lower in the year of adoption. If there was a lag before council-manager government

	Model 1	Model 2	Model 3
	(1)	(2)	(3)
Adopt (No Home Rule)	-113.467 (57.290)**	-128.736 (59.985)**	-55.760 (14.591)***
Adopt _{t+1} (No Home Rule)		-155.495 (86.406)*	-165.879 (90.034)*
Adopt _{t+2} (No Home Rule)			-117.933 (55.272)**
Adopt (Home Rule)	-26.261 (22.422)	-8.877 (18.249)	-15.825 (19.685)
Adopt _{t+1} (Home Rule)		-37.248 (18.819)**	-23.602 (16.060)
Adopt _{t+2} (Home Rule)			-30.959 (20.226)
Manufacturing (%)	-10.486 (1.062)***	-11.000 (1.143)***	-11.650 (1.233)***
Non-White (%)	16.579 (.858)***	16.285 (.896)***	15.912 (.940)***
Median Age	-12.596 (1.549)***	-11.984 (1.663)***	-11.288 (1.758)***
Const.	896.109 (74.774)***	900.647 (80.141)***	907.192 (84.992)***
<i>F</i> test (HR β 's= NHR β 's)	2.01	4.65**	5.03**
Obs. (307 groups)	6429	6122	5815
R^2 within	.202	.203	.199
<i>F</i> statistic	322.148	216.41	153.118

*: significant at 90 percent **: significant at 95 percent ***: significant at 99 percent

Table 3: Fixed effects models of adoption and commitment technology. Robust standard errors in parentheses

affected expenditures, only including a variable for the year of adoption would prevent us from observing the true nature of the relationship between council-manager government and expenditure levels. To ameliorate this problem, I created lagged versions of the *Adopt (Home Rule)* and *Adopt (No Home Rule)* variables. In Table 3, I fit three fixed effects regression models. Model 1 includes the controls and the adoption variables, model 2 adds the one-year lagged version of the adoption variables, and model 3 adds the one- and two-year lagged versions of the adoption variables.

The results in Table 3 are quite revealing. In the fixed effects model specification with robust standard errors, the coefficient estimates of adoption of council-manager government in cities without home rule are negative and statistically significant. The point estimates are substantively large as seen in model 3 which predicts expenditure reductions of more than \$55 per capita in the year of adoption, more than \$165 per capita in the year following adoption, and almost \$118 per capita two years after adoption. The point estimates for adoption in cities that had home rule are much closer to \$0 and only one of the six coefficient estimates in the three different models is statistically significant the 90 percent level. However, this is not the most compelling finding of these models. More importantly, the F test statistic is significant in models 2 and 3 allowing us to reject the null hypothesis that post adoption of council-manager government expenditure patterns are identical in cities with and without home rule. Models 2 and 3, as suggested above, are more compelling than model 1 because there are reasons to believe that council-manager government will not manifest itself instantaneously. Table 3 provides ample support for the hypothesis that commitment technology plays a major mediating role in the relationship between council-manager government and municipal expenditures. Cities that have weaker commitment technology at their disposal experience expenditure reductions that are significantly lower than cities with superior commitment technology. These findings persist even after including control variables, employing a fixed effects estimator that accounts for city-level idiosyncrasy, and using robust standard errors in reaching inferences.

While Table 3 offers persuasive evidence, it is valuable to test the sensitivity of the findings to the inclusion of council-manager government indicator variables into the models. Although the primary interest of this analysis is the adoption of council-manager government, the presence of council-manager government may affect the results. Table 4 contains three models that are identical to those in Table 3, but each model includes two additional indicator variables. *CM Govt (Home Rule)*, which takes on the value 1 when a city has council-manager

	Model 1	Model 2	Model 3
	(1)	(2)	(3)
Adopt (No Home Rule)	-131.447 (58.957)**	-141.257 (62.245)**	-67.712 (17.851)***
Adopt _{t+1} (No Home Rule)		-167.080 (89.192)*	-175.237 (93.439)*
Adopt _{t+2} (No Home Rule)			-126.027 (56.791)**
Adopt (Home Rule)	-46.054 (22.774)**	-30.001 (18.744)	-38.592 (20.945)*
Adopt _{t+1} (Home Rule)		-57.824 (19.713)***	-45.943 (17.052)***
Adopt _{t+2} (Home Rule)			-52.403 (21.085)**
CM Govt (No Home Rule)	49.441 (19.690)**	35.929 (15.797)**	29.931 (16.942)*
CM Govt (Home Rule)	44.242 (10.493)***	47.440 (11.186)***	51.837 (12.076)***
Manufacturing (%)	-10.515 (1.064)***	-11.047 (1.147)***	-11.712 (1.236)***
Non-White (%)	16.503 (.858)***	16.203 (.897)***	15.813 (.941)***
Median Age	-11.681 (1.629)***	-11.185 (1.747)***	-10.525 (1.850)***
Const.	853.344 (78.764)***	863.354 (84.324)***	871.582 (89.521)***
<i>F</i> test (HR β 's = NHR β 's)	1.84	4.82**	4.54**
Obs. (307 groups)	6429	6122	5815
R^2 within	.205	.205	.201
<i>F</i> statistic	239.368	172.626	127.69

*: significant at 90 percent **: significant at 95 percent ***: significant at 99 percent

Table 4: Fixed effects models of adoption, commitment technology, and council-manager government. Robust standard errors in parentheses.

government and home rule and 0 otherwise, and *CM Govt (No Home Rule)* which takes on the value 1 when a city has council-manager government and no home rule and 0 otherwise.

The results are quite similar to those in Table 3. Both forms of the adoption variable tend to have higher coefficient estimates and more of the *Adopt (Home Rule)* estimates are significant. But the most important finding from Table 3 remains. In models 2 and 3 of Table 4, the F test statistics are sufficiently large to reject the null hypothesis of equal coefficients at the 95 percent significance level.⁴ The findings appear to be robust to model specifications that include the presence of council-manager government.

5.1 Causality?

Until this point, I have been careful to refrain from using any language that implies that these statistical results are indicative of a causal relationship. Although there are strong theoretical reasons for believing that the presence of home-rule institutions mitigates the expenditure effects of adopting council-manager government, the empirical findings that support these claims are still suspect. As I have stressed previously, political institutions are not randomly assigned. Political institutions are the outcomes of internal political and economic processes and we would naturally expect cities that differ in regard to these processes to differ in respect to other unobserved features. Cities may adopt council-manager government because of the ascent to power of pro-business interests on the city council. The newly enshrined interests may also favor reductions in expenditures. While a regression would generate a negative coefficient estimate for the relationship between council-manager government and expenditures, the true effect would be \$0. By omitting a variable that captures the presence of the pro-business interests, multiple regression generates biased coefficient estimates. The problem of non-random assignment is extremely difficult to resolve in observational studies

⁴In order to test the total estimated effect of council-manager government, the F tests in Table 4 include the *CM Govt(Home Rule)* and *CM Govt (No Home Rule)* coefficient estimates in addition to the adoption variables.

and thoughtful estimation strategies are required to make causal claims [5].

One way to make causal inferences is restricting our analysis to a subset of the data where we have reason to believe that predictor of interest was randomly assigned. When we make this type of restriction we also need to hedge any inferential claims and only apply them to this particular subset of the data. While there are many reasons to suspect that council-manager government is endogenous, there are fewer reasons to believe that cities play a role in determining whether or not the state legislature implements home rule. Home rule emerged from state legislatures and governors' offices, not city councils and mayoral offices. This does not prove that home rule did not result from internal choice processes, but it provides some suggestive evidence to that effect. If we restrict the regression analysis to the population of cities that adopted council-manager government and find that cities with stronger commitment technology have larger reductions in expenditures than cities that possess weaker commitment technology, we would have grounds to make a causal claim that, among cities that adopt council-manager government, the strength of commitment technology has a causal effect on expenditure reductions.

To that end, I fit a series of fixed effects models to the subset of cities that adopted council-manager government at some point over 1946-1966. The results are displayed in Table 5 and are even more striking than those presented in Tables 3 and 4. The coefficient estimates for the *Adopt (No Home Rule)* variables are comparable to those in Table 4, but the *Adopt (Home Rule)* estimates are much smaller. The F tests generate test statistics that are significant at the 90 percent level in model 1, the 95 percent level in model 2, and the 99 percent level in model 3.

The most serious shortcoming of these models is the relatively small sample size. Although more than 900 "observations" are analyzed in each of the models, these do not represent independent units of analysis. There are only 48 cities in the sample that adopt council-manager government during the period of analysis. While a sample size of 48 is still

large enough to reach meaningful inferences, it does call into question the use of Huber-White robust standard errors as a correction for the heteroskedasticity problem. These standard errors are mathematically justified on asymptotic grounds and it is unclear what their effect is in a small sample size. I refit the models in Table 5 with conventional standard errors and the results were qualitatively similar. In the conventional standard error analogue of model 1 the F statistic is not significant at the 90 percent level, but in models 2 and 3 the F statistic is significant at the 99 percent level. Among cities that adopt council-manager government, the strength of the available commitment technology appears to exert a causal effect on subsequent municipal expenditures.

But before reaching such a strong claim, it is necessary to evaluate further the possibility that home rule was endogenous to the municipal policymaking process. Although municipal governments did not directly pass home rule it is possible that they had an indirect effect on home rule by lobbying and pressuring state governments to implement home rule within the state. Based on the naive and potentially incorrect assumptions that cities desired home rule because of the additional power over policymaking that comes from it and that state governments were more responsive to these desires where urban populations made up a larger portion of the electorate, it is possible to investigate the endogeneity of home rule empirically. If these assumptions hold then there should be a strong statistical relationship between the percentage of state residents living in urban areas and the probability of home rule.

The dependent variable is a dichotomous indicator that takes on the value of 1 when a state had home rule according to the 1935 Municipal Yearbook. To measure urbanicity, I use data on the percent of state residents living in urban areas from the 1930 U.S. Census. Ideally, the independent variable would only operationalize the number of people living in municipalities and not outlying suburbs, but data of this form are unavailable. Equipped with these data, I fitted a logistic regression of the home-rule indicator on the urbanicity measure. Logistic regression is the preferred model form because the dependent variable

	Model 1	Model 2	Model 3
	(1)	(2)	(3)
Adopt (No Home Rule)	-118.771 (53.653)**	-136.113 (56.285)**	-75.313 (21.232)***
Adopt _{t+1} (No Home Rule)		-163.143 (82.866)**	-175.975 (86.732)**
Adopt _{t+2} (No Home Rule)			-127.289 (52.366)**
Adopt (Home Rule)	-17.661 (20.530)	-6.175 (17.815)	-15.342 (18.611)
Adopt _{t+1} (Home Rule)		-25.249 (18.201)	-16.676 (16.239)
Adopt _{t+2} (Home Rule)			-20.045 (19.930)
Manufacturing (%)	8.363 (4.130)**	8.249 (4.532)*	7.510 (5.061)
Non-White (%)	33.036 (4.855)***	33.060 (5.056)***	33.441 (5.371)***
Median Age	-5.279 (6.311)	-2.347 (7.084)	.478 (7.865)
Const.	56.206 (302.390)	-23.382 (333.829)	-88.767 (366.104)
<i>F</i> test (HR β 's = NHR β 's)	3.06*	6.16 **	7.04 ***
Obs. (48 Groups)	1005	957	909
R^2 within	.059	.073	.073
<i>F</i> statistic	33.768	21.793	17.387

*: significant at 90 percent **: significant at 95 percent ***: significant at 99 percent

Table 5: Fixed effects models of adoption and commitment technology in the subset of cities that adopted council-manager government at some point during 1946-1966. Robust standard errors in parentheses.

takes on dichotomous values. Robust standard errors are not appropriate because there is no theoretical reason to believe there is heteroskedasticity in the data so I use conventional standard errors for statistical inferences.

The results of this initial regression model suggest that urbanicity did not play a major role in the adoption of home rule. The estimated coefficient on the urbanicity variable is positive, but the standard error is so large that the coefficient estimate is not statistically significant and we cannot reject the null hypothesis that the coefficient is equal to zero. This result supports the hypothesis that home rule is not the result of the endogenous choice processes of municipal governments, but a more thorough investigation is needed to reach this conclusion.

A scatter plot of the data points reveals the presence of a relationship that might be better modeled with a quadratic term. At low and high levels of urbanicity few states have home rule. It is in the middle range of urbanicity that states are most likely to have home rule. The theory that I began with above posited a monotonic relationship between urbanicity and home rule — the higher the level of urbanicity the more likely a state adopts home rule. Instead, the data appear to follow a quite different pattern as seen in Figure 7. There may be an omitted variable that is correlated with urbanicity that is driving this relationship.

Another possibility is that the results are driven by the presence of Washington, D.C. in the data set. The data set only consists of 51 observations and it is possible that the idiosyncratic case of Washington, D.C. — the case with the highest urbanicity level, 100 percent, in the sample — could be affecting the results. It is far from clear that Washington, D.C. should be included in the statistical analysis on theoretical grounds. Unlike all of the other cases in the data set, Washington, D.C. is not a state and under the U.S. Constitution, the federal government is responsible for determining its political institutions. This information suggests that, for our purposes, Washington, D.C. is much closer to a political territory than a state and is not relevant to this analysis of state policymaking. Given these concerns, I fit

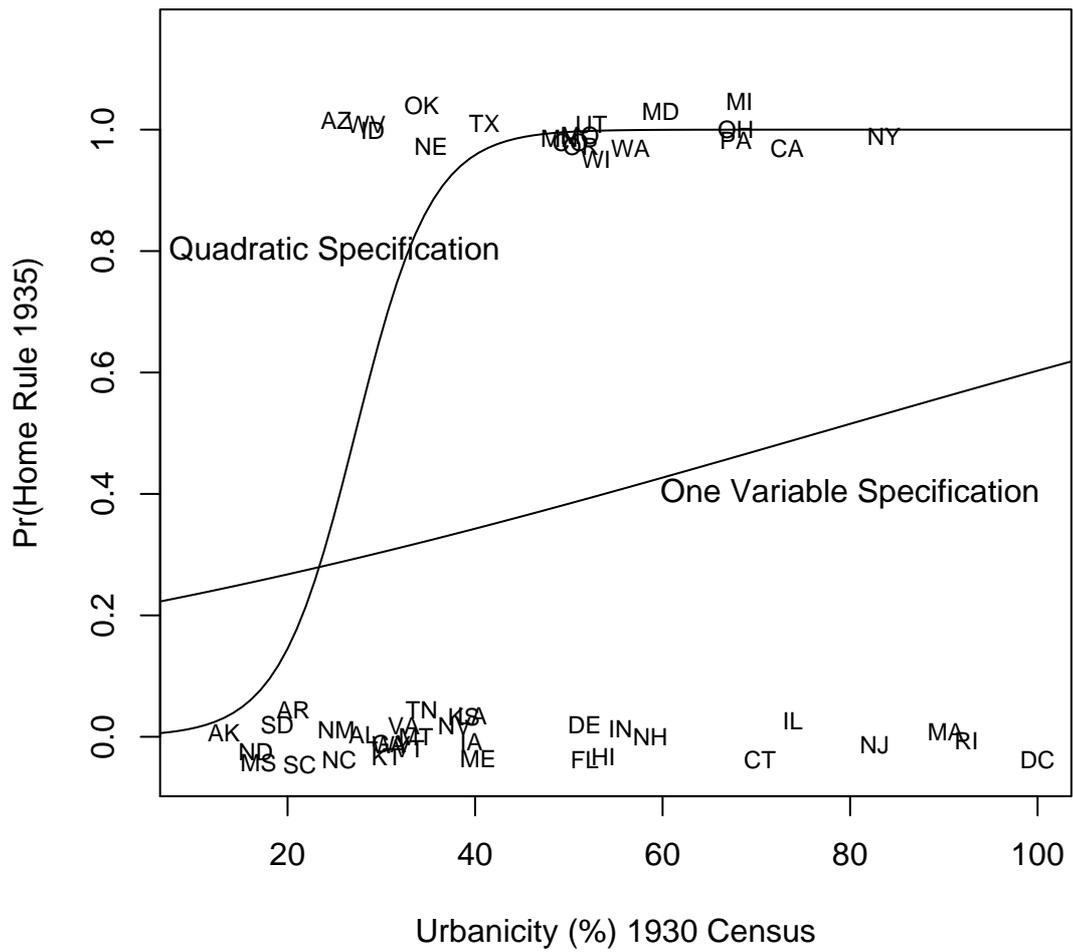


Figure 7: A logistic regression of home rule on urbanicity with jittered data points.

both of the logistic regression models with Washington, D.C. omitted from the sample. The models illustrate that Washington, D.C. does not have a major effect on the results. The statistical significance of the coefficient estimates remains the same for both models.

The question of whether the core results of this paper can be interpreted causally ultimately hinges on the endogeneity of home rule. This exploratory analysis provides some evidence to suggest that the probability of home rule is not related to the strength of urban interests. However, this evidence is far from conclusive. There is a strong quadratic relationship between urbanicity and home rule and the logistic regression models are probably suffering from omitted variable bias. Although the main results can be tentatively interpreted as causal, in the absence of a more thorough investigation of the factors that affected home rule adoption in the early 20th century United States a definitive interpretation is somewhat premature.

6 Conclusion

This paper applies a game-theoretic approach to explain a puzzling episode in U.S. politics, the 20th century urban reform movement. As this paper attempts to illustrate, game theory can generate insights that informal forms of reasoning overlook. The theoretical literature on the urban reform movement suffers from this dearth of formality, which has, in turn, retarded empirical attempts to discern the consequences of reform political institutions. The failure to recognize the importance of commitment technology as a determining factor in the municipal expenditure effects of council-manager government is a critical omission. Without properly accounting for commitment technology, econometric estimates of the relationship between council-manager government and municipal expenditures are likely to be biased.

Turning to empirics, this paper finds that the negative statistical relationship between council-manager government and municipal expenditures that so much of the empirical lit-

eratures has deemed illusory is actually quite real. Applying modern econometric techniques for panel data and correcting for heteroskedasticity with robust standard errors, yields statistically significant coefficient estimates. As predicted by the commitment approach, the strength of commitment technology plays a statistically significant role in mediating between the adoption of council-manager government and subsequent municipal expenditure behavior.

The commitment approach resolves several of the enduring puzzles of the urban reform movement. Specifically, it can explain both the institutional successes of the reformers and the divergent repercussions of council-manager government that emerge from differences in commitment technology. While this paper's main contribution is the application of a game theoretic approach to urban reform and the identification of statistically significant and, possibly, causal reductions in municipal expenditures following the adoption of council-manager government, it also provides additional validation for the commitment approach to the study of institutions.

Despite these findings, this paper does not offer the definitive word on the urban reform movement. Three areas are especially appealing for future research. First, the institutional relationships discussed in this paper might affect cities' economic growth. There is a robust [2, 1, 7] literature on the links between institutions and economic growth and urban reform cries out for a similar analysis. Council-manager government may have increased investment in productive infrastructure, encouraged business formation within the city limits, and fostered other changes that may have had a salubrious effect on economic growth. To make the empirical components of this project manageable, I have restricted my inquiry to the link between institutional structure and municipal expenditures, but a broader investigation with economic growth as the dependent variable would be valuable. Second, Bayesian empirical techniques [14], specifically multilevel modeling [15], could be productively applied to these data. The cities in this data set are embedded in states and a multilevel model might offer a more powerful tool for disentangling the relationships between council-manager government,

commitment technology, and municipal expenditures than the classical regression models used in the empirical section of this paper. Finally, the causality dimension should be investigated more thoroughly. While statistical correlations are suggestive, properly-designed causal inferences allow researchers to make much more compelling claims. In this context, a more complete investigation into the diffusion and adoption of home-rule institutions in the early 20th century could provide critical insights into whether home rule can legitimately be considered “exogenous.”

Institutions perform a central role in shaping political equilibria. In the presence of sufficient commitment technology, institutions have the capacity to shape the long-term allocation of political power and, in the process, achieve political outcomes that would be otherwise impossible. Like any powerful theory, the commitment approach is able to explain diverse historical and contemporary episodes and elucidate puzzling relationships. As this paper shows, an explanation of the urban reform movement should be added to the list of the commitment approach’s many successes.

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