THREE ESSAYS IN PUBLIC FINANCE AND REGULATION

by

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DEDICATION

To all who inspired in me a love for learning

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Chapter 1

Introduction

This dissertation explores three empirical questions in public finance and regulation. Although it is difficult to find a single topical theme that unifies these essays, my interest in political economy played a direct or indirect role in the genesis of each one of them.

The first essay in my dissertation, entitled "The Effects of Political Competition on the Funding and Generosity of Public-Sector Pension Plans" undertakes an analysis of the variation in fiscal health of public-sector pension plans. The paper is motivated by the fact that when we look around, instances of underfunded public pensions abound. In spite of its wide prevalence, I find the existing theoretical and empirical explanations for the prevalence of underfunding to be less than satisfactory. I propose that political competition, defined as the lack of a systematic electoral advantage by either political party in a two-party system plays a key role in generating variation in the level of funding and generosity of public-sector pensions.

Consider the incentives faced by politicians when in office. On the one hand, politicians would like to offer generous retirement benefits to public sector workers to gain their electoral support and also to attract and retain skilled employees. On the other hand, as these retirement benefits are supposed to be pre-funded, providing generous benefits to public sector workers requires a higher level of taxes on workers in the private sector today. High taxes are however likely to cost the government electoral support. Facing this trade-off in terms of balancing electoral support from different groups of voters, politicians may decide to promise generous benefits for public-sector workers and simultaneously, to not make the actuarially required contributions to the pension fund whenever possible. Although such incentives are likely to be present at all points of time, my work focuses on the role that political competition

plays in exacerbating such incentives for incumbents. My central hypothesis is that as political competition decreases, the incentives to fund the pension plan fully increase. The intuition is that, as the dominant political party becomes more certain of being in power over successive election cycles, it concludes that although contributing fully to the pension plan may require a higher level of taxes today, higher taxes by itself would not substantially reduce the party's chances of re-election.

To examine this hypothesis, I use data on municipal pension plans from the state of Pennsylvania over a 25-year period from 1985 to 2009. Pennsylvania is chosen because its local governments offer over 2,000 local government pension plans and these account for over a quarter of all distinct public sector retirement systems in the U.S. I estimate the effects of political competition on three key dependent variables of interest: (a) the funding status, (b) generosity of pension plans, and (c) the interest rate chosen by these plans to discount actuarial liabilities. Controlling for other potentially confounding factors, I find robust evidence that in municipalities that are more politically competitive, defined benefit pension plans are less funded, more generous, and plan liabilities are discounted at a higher interest rate. In contrast to the results obtained with defined benefit pension plans, I find that political competition has no effect on the generosity of defined contribution plans, consistent with the view that these plans are less susceptible to political influence.

In my second essay, I examine the role that policy makers can play in influencing economic activity through the budgetary process. The genesis of this project lies in the observation that in the context of divided government, getting a law enacted through the traditional legislative process may prove to be a considerable barrier to overcome. Therefore, if there are alternatives to traditional legislation which can help political actors realize their goals, then they are likely to use such alternatives. I explore this hypothesis in the context of the administration and enforcement of tax laws by the Internal Revenue Service (IRS). If, we were to believe that Republican administrations want to reduce effective corporate tax rates and also accept the premise that it is more difficult to change the tax code (e.g. the statutory rates) than to reduce the probability of audits, then we arrive at a simple conclusion: Republican presidents would try to reduce effective tax rates by reducing the probability of audits – potentially through starving the IRS of resources.

The paper investigates whether that is indeed the case. It examines whether differences between the Republican and the Democratic parties induce different administrations to fund

the IRS differently with higher or lower budgetary allocations levels and in how these resources are allocated across various activities. It finds that although there is no evidence that the overall IRS budget or workforce is larger during Democratic administrations, party affiliation of the President makes a difference to the share of those resources that are allocated towards enforcement. In particular, the number of criminal investigators and revenue officers as a share of the IRS workforce are significantly higher during a Democratic Presidency. I also find that over the period 1978–2010, audit probabilities for corporate income tax returns, individual income tax returns, estate tax returns, and returns filed by trusts and fiduciaries are lower on average under Republican administrations than under Democratic administrations. Examining corporate audits in greater detail, I find that the lower audit probability is also reflected in a lower effective corporate tax rate. The overall take away from this essay is that that although the President has limited or no influence on budgetary levels and resources which require the approval of Congress, he is able to exert an influence on the allocation of these resources among various activities.

The third and final essay of my dissertation, co-authored with Jagadeesh Sivadasan, examines the deregulation of cable television franchising that took place between 2005–2008 in nineteen states of the U.S. and quantifies the effects of those reforms on prices of the two most common tiers of service. This work originated from an observation that there is very limited competition at the local level among cable providers and the additional insight that although there are economies of scale in the provision of cable service, those are not so large to rule out the possibility of competition as a disciplining force in the marketplace. Exploring the reasons for such limited competition at the local level, we observed that local "franchising" regulations which deter market entry play a significant role in the persistence of local monopolies. Given the reality of such limited competition in the cable industry, nineteen states reformed their franchising process between 2005–2008 to allow for statewide franchises. These reforms limited the ability of local governments to regulate cable operators and allowed for the issuance of a single statewide cable franchise to any company interested in providing cable service in the state.

Using an econometric approach that exploits the staggered introduction of these reforms, we find that prices for the lowest tier of service ("Basic") declined systematically by about 5.5 to 6.8 percent following the reforms. However, we find no statistically significant effect of these reforms on the average price for the more popular tier of service, "Expanded Basic".

Further analysis shows that although there is additional entry following the enactment of these reforms, the decline in price for "Basic" service also occurs in markets that did not experience actual entry, arguably because of an increase in the threat of entry as perceived by incumbents. In order to control for potential state-level shocks correlated with the reforms, we undertake a sample-split test examining changes in local markets which faced a greater threat of entry (because they were close to a prominent second entrant); we find larger decline in prices, for both "Basic" and "Expanded Basic" services, in areas likely to be more keenly contested. Our results are consistent with limit pricing models that predict incumbents respond to increased threat of entry (without actual entry taking place), and suggest that the reforms modestly benefited consumers in reformed states.

Chapter 2

The Effects of Political Competition on the Funding and Generosity of Public-Sector Pension Plans

2.1 Introduction

The bankruptcy of Stockton and San Bernardino has brought the issue of public-sector pension plans into focus. Unfunded pension and health care obligations have been key drivers behind the decisions of these municipalities to file for bankruptcy. The size and funding of public-sector pension plans are, however, issues affecting states and municipalities throughout the country.

Under defined benefit pension plans, the dominant type of pension in the public sector, state and local governments promise pension benefits that are typically a specific fraction of an employee's last drawn salary. Sponsoring employers are expected to put money into a retirement trust fund so that, when combined with employees' contributions, the fund will grow sufficiently to provide the promised retirement benefits when the employee retires. In practice, however, often large gaps exist between the assets available in the retirement trust fund and the promises made to employees. Some estimates put the magnitude of unfunded liabilities – the excess of promises over assets – at approximately \$4.4 trillion at the end of 2011.[1] This amounts to about 340% of the \$1.3 trillion raised in taxes in 2011 by these same state and local governments.[2]

The aggregate level of unfunded pension liabilities, however, conceals considerable heterogeneity across plans. The theoretical explanations that have been proposed in the liter-

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[1]Rauh, J. (Oct. 5, 2011). "Shortfall for State and Local Pension Systems Today: Over \$4 Trillion": http://kelloggfinance.wordpress.com/2011/10/06/shortfall-for-stateand-local-pension-systems-today-over-4-trillion – Accessed 09/30/2013.

[2]http://www2.census.gov/govs/local/summary_report.pdf–Accessed 09/30/2013.

ature for the existence of pension plan underfunding focus on the differences in borrowing costs of citizens from the pension fund relative to the costs of borrowing in the private market (Mumy, 1978), the desire to smooth taxes across periods to minimize deadweight losses (Epple and Schipper, 1981), and the desire of current residents to move out of a given jurisdiction and pass on the costs of these pensions to future residents (Inman, 1982). These explanations appear inadequate to explain the variation in the extent of funding found across retirement plans in practice.

This paper offers an alternative explanation for the existence of the underfunding that can also account for the wide variation in the funding levels and generosity of public-sector pensions. It proposes that political competition, defined as the lack of a systematic electoral advantage by either political party, plays a key role in the underfunding of pension plans. To the extent that public-sector workers are better informed than workers in the private-sector, competition for votes creates incentives for politicians from both parties to offer generous retirement benefits to workers in the public sector and simultaneously, to not fund them fully, in order to avoid raising taxes on workers in the private sector. A higher degree of political competition will therefore be associated with a decline in the funding status of pension plans, an increase in the generosity of retirement benefits, and a choice of higher interest rates to discount the actuarial value of future liabilities.

To test the effects of political competition on funding and generosity, I examine local pension plans in the state of Pennsylvania. Pennsylvania provides a rich setting to investigate these issues because its local governments offer over 1,400 retirement systems, more than three times the number offered by any other state in the U.S.[3] Panel data for all local pension plans from Pennsylvania are constructed using biennial reports of the Pennsylvania Public Employee Retirement Commission (PERC) available from 1985–2009. Using these data, I find that as the level of political competition in a municipality increases, pension plans become less funded and the level of unfunded liability per covered employee goes up. These results are obtained after controlling for municipality and decade fixed effects, suggesting that unobserved time-invariant heterogeneity across municipalities or aggregate time trends are not driving the results. The effects of political competition are economically large and statistically significant. Using the point estimates across various specifications, a one standard

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[3]Source: http://www.census.gov/govs/retire/- Accessed 09/30/2013. Of the 3,418 public-employee retirement systems in the United States, 1,422 (or, 41.6%) are local retirement systems from Pennsylvania.

deviation increase in the level of political competition is associated with a decrease in pension plan funding levels of approximately 7–10 percent and an increase in unfunded liabilities per active member of approximately \$2,300–3,200.[4]

In a different set of analyses using data from 2003 to 2009, I find that municipalities in Pennsylvania that are politically competitive offer more generous benefits and use a higher interest rate at which to discount future actuarial liabilities. An increase in the level of political competition by one standard deviation is associated with an increase in annual average retirement benefits of about \$470–620 per retiree, or about 3.4% relative to their mean value. The interest rate used to discount future actuarial liabilities is also higher by about 5 basis points on average for the same increase in the level of political competition.

To account for the possible endogeneity of political competition, I use demographic characteristics of the population as instruments to predict variation in the intensity of political competition at the municipal level. I find that variation in the ancestral origins and ethnicity of the population predicts a considerable part of the variation in the Democratic vote share across municipalities in Pennsylvania. In particular, municipalities where a large fraction of the population is of German descent have a higher Republican vote share, whereas municipalities with large Irish-American, Italian-American, and black populations, have a higher Democratic vote share. Instrumental Variable (IV) estimates of the effect of political competition using variation in these demographic characteristics corroborate the earlier findings. A one standard deviation increase in the intensity of political competition results in a 14–32 percent decline in the funded ratio and a corresponding increase of \$7,620–13,610 in the size of unfunded liabilities per active member, an increase in the generosity of the annual pension of about \$880–1,380 per retiree, and an increase in the interest rate used for discounting future liabilities of about 17–24 basis points.

Given that defined contribution pension plans are seen as less susceptible to political influence, I also examine the effects of political competition on the generosity of such plans.[5] In contrast to the results obtained earlier with defined benefit pension plans, which suggest that an increase in political competition is associated with a decline in the funding level and

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[4]A one standard deviation increase in the level of political competition, using the measure defined in Besley, Persson, and Sturm (2010), would result if the Democratic vote share were to go down from 57.2 percent (leaning Democratic) to 50 percent (most competitive), or conversely, go up from 42.8 percent (leaning Republican) to 50 percent (most competitive).

[5]"Defined contribution plans are retirement plans that specify the level of employer contributions, if any, and place those contributions in individual accounts. The value of an individual account is determined by the amount of money contributed and the rate of return on the money invested over time." (BLS, 2010)

an increase in the generosity and interest rate, I find that political competition has no effect on the generosity of defined contribution plans. These results are obtained, both in an OLS and in an IV set-up, in which I instrument for the level of political competition exploiting the variation in demographic characteristics described above.

The paper proceeds in six sections. The next section outlines a theoretical model and derives two comparative statics results that inform the empirical analyses. Section 2.3 describes the data sources and provides the empirical specifications. In Section 2.4, I present results, focusing on three key elements for defined benefit plans: the level of funding, the generosity of benefits, and the choice of interest rate for discounting actuarial liabilities. I also present the null results on defined contribution plans. Section 2.5 offers some discussion regarding the theoretical model and the internal and external validity of the results. Section 2.6 concludes.

2.2   Theoretical Model

This paper focuses on the role of political competition in explaining the variation in funding status of public-sector pension plans. It builds on an idea presented in Epple and Schipper (1981) who conjecture that increased political competition may pressure politicians to underfund pension liabilities, so as to be able to reduce taxes in the short-run; this behavior is rewarded by those voters who are unaware of deferred pension obligations. In this section, I analytically investigate the validity and significance of this conjecture in a stylized model that includes two groups of voters – workers in the public sector and workers in the private sector. A higher intensity of political competition exacerbates the incentives of politicians to not fund the retirement benefits that have been promised to public-sector workers fully, in order to avoid raising taxes on workers in the private sector. The two key assumptions made for obtaining predictions of the effects of political competition on the funding status of pension plans are that (i) politicians care about voter welfare in addition to winning elections (e.g. Ujhelyi, 2013), and (ii) as conjectured by Epple and Schipper (1981), private sector voters have imperfect information about the funding and generosity of public-sector pension plans (Glaeser and Ponzetto, 2013). The model then predicts that an increase in political competition will be associated with a decline in the funding status of pension plans.[6]

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[6]Without assumption (i), politicians would be tempted to minimize taxes by providing no funding for pension liabilities (which is inconsistent with the data), and without assumption (ii), private sector workers would be able to perfectly foresee and offset potential future tax hikes by appropriately adjusting savings level (a Ricardian

The two groups of voters are denoted as P (private-sector workers) and G (public-sector workers). Two parties, denoted as L and R compete by choosing electoral platforms simultaneously.[7] To keep the model tractable, I focus on a single policy decision by the government and a single economic decision by public-and private-sector workers. The government decides on the extent to which it funds the pension plan in the first period of a two-period model, taking as exogenous the level of wages in both sectors and the level of retirement benefits offered in the public sector. Workers decide on how much to save in the first period of the model, taking into account their wages, benefits (if any), consumption preferences, and projected path of taxes. The model is driven by the imperfect information problem private-sector workers face as they decide how much to save. Unable to anticipate the increase in taxes in the second period to make up for the shortfall that the government runs in the pension fund, private-sector workers face a decrease in utility caused by their sub-optimal savings decision and hence, sub-optimal intertemporal allocation of consumption. The parties, in turn, decide on their electoral platform involving a choice of the lump-sum tax for the first period of the model based on weighing the ego rents they receive from coming to office and the decrease in utility faced by private-sector workers as a result of the government running a deficit in the pension fund.

In the first stage of the model, both parties announce platforms simultaneously under uncertainty about an aggregate popularity shock. Second, the aggregate popularity shock is realized as voters consider the platforms announced by the two parties and cast their votes.[8] Finally, the party winning the election implements its announced platform and voters make private economic choices in the light of the policy chosen.[9] The next sub-sections deal with these choices in reverse order.

equivalence type result).

[7]I do not allow for free entry by other political parties. This keeps the model tractable and also reflects party competition in the United States. Local elections within Pennsylvania are held on partisan lines and an overwhelming proportion of candidates and an even larger proportion of winners are drawn from either of the two national parties.

[8]I assume full turnout in the model and do not consider abstentions.

[9]Although the model has two periods, I consider only one election, which is held prior to the first period of the model. Nothing hinges on this modeling choice. All that I require is that the government in office in period 2 is constrained to honor the pension obligations that were made by the government in period 1, irrespective of the identity of the party in power in that period.

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2.2.1 The economic model

There are two time periods in the model. Workers in both sectors work in period 1 and retire in period 2. Thus, in each time period, there are two groups of workers in each sector - currently active workers and retired workers. The only choice for workers who are currently active is a decision of how much to save in the first period of the model.

I assume that the government is constrained to running a balanced budget as far as the current compensation of public-sector workers is concerned and that the only tax instrument available to the government is a lump-sum tax.[10] It inherits a pension fund at the start of period 1 that is balanced, i.e. has enough assets to cover all liabilities and is constrained to leaving a pension fund that is balanced at the end of period 2. The only decision it makes is the extent, if any, to which it runs a deficit in the pension fund in period 1 of the model.

2.2.1.1 Optimization by private-sector workers

A critical assumption of the model is that private-sector workers are not fully informed of the retirement benefits that have been promised to public-sector workers. This is in line with the intuition in Epple and Schipper (1981) and the assumption made in Glaeser and Ponzetto (2013), who argue that pension obligations are shrouded because of lower availability of information about pensions than wages and because of the greater difficulty of understanding the accrual of pension obligations, in contrast to current compensation. In support of their claim, they mention that state employee salaries are publicly disclosed every year whereas no such database exists for the accruing pensions of currently employed civil servants.[11] The implication of this assumption is that private-sector workers cannot figure out whether the level of taxes announced by the parties for period 1 corresponds to funding the pension plan fully or underfunding it. In what follows I model how, in a competitive political environment, politicians can use this lack of information on the part of private-sector workers to announce a lower level of taxes in period 1 than required for funding the pension plan fully and thereby

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[10]Although the latter seems like a restrictive assumption, the model does not hinge on that. Assuming a lump-sum tax is equivalent to allowing a proportional income tax with an inelastic labor supply and earnings that are given exogenously. Allowing for the labor supply to be elastic would offer another reason for politicians to fund pensions fully and balance taxes over time as deadweight losses would be higher with a tax rate that fluctuates significantly between the two periods than an alternative regime that is level over time but raises the same amount of revenue.

[11]For example, salary data for state employees from the state of Pennsylvania are available at: http://www.pennlive.com/midstate/index.ssf/2013/03/search_pennsylvania_state_empl.html. Similar data from Georgia are available at: www.open.georgia.gov.

increase their probability of election.

I model the savings decision for the representative private-sector worker. With c_1^P and c_2^P as consumption in periods 1 and 2, her utility across both periods can be expressed as:

$$U^P(c_1^P, c_2^P) = u(c_1^P) + \beta u(c_2^P), \tag{2.1}$$

where β is a discount factor reflecting the weight placed on future consumption. If her earnings in period 1 are W^P (given exogenously), savings in period 1 are s^P, and period 1 and period 2 taxes are T_1 and T_2 respectively, then the problem for the private-sector worker is:

$$Max_{\{s^P\}} U_{per}^P(c_1^P, c_2^P) \equiv Max_{\{s^P\}}[u(W^P - s^P - T_1) + \beta u(s^P * (1+r) - E(T_2|T_1))] \tag{2.2}$$

where the "per" subscript denotes perceived (rather than realized) utility and $E(\mathrm{T}_2|\mathrm{T}_1)$ reflects the expected value of period 2 taxes given the level of period 1 taxes. To focus attention on the political economy of pensions, I set

$$\beta = 1/(1+r) \tag{2.3}$$

Although the specific functional form assumed here is not subsequently used in the derivation of the political equilibrium or the comparative statics, assuming a logarithmic utility function enables me to obtain a closed-form solution for the level of savings and offers additional insights into the problem. With that assumption, the solution to this optimization problem is for the private-sector worker to save an amount s^P from period 1 wages, W^P given by:

$$s^P = (W^P - T_1 + E(T_2|T_1))/(2+r). \tag{2.4}$$

Based on the assumption that private-sector workers do not know the retirement benefits offered in the public sector, they are unaware of whether taxes announced for period 1 are adequate to fund the pension plan fully or whether they fall short of full funding. As a result, private-sector workers cannot correctly anticipate what the level of taxes in period 2 would be in order to have the pension plan be fully balanced at the end of that period. I make the assumption that the private-sector worker naively sets $E(T_2|T_1) = T_1$. With that assumption, (2.4) simplifies to:

$$s^P = W^P/(2+r). \tag{2.5}$$

With that, I can express the utility perceived by private-sector workers, U_{per}^P, following the announcement of period 1 taxes, T_1 as:

$$U_{per}^P = u(W^P - s^P - T_1) + \frac{1}{(1+r)} * u(s^P * (1+r) - T_1) = \frac{(2+r)}{(1+r)} * u(W^P * \frac{(1+r)}{(2+r)} - T_1). \tag{2.6}$$

Based on the fact that period 1 taxes, T_1, must be adequate to support current compensation and partly fund the pension plan, T_1 is given by the following expression:

$$T_1 = N^G * (W^G + a * \frac{B^G}{(1+r)}) \tag{2.7}$$

with a, the level of pension plan funding chosen in period 1 of the model $\epsilon[0,1]$ and where N^G, W^G, and B^G denote the number of employees in the public sector, and wages and pensions offered to employees in that sector.[12] Substituting in (2.6), we see that:

$$U_{per}^P = \frac{(2+r)}{(1+r)} * u(W^P * \frac{(1+r)}{(2+r)} - N^G(W^G + a * \frac{B^G}{(1+r)})). \tag{2.8}$$

As the above expression suggests, the *perceived* utility of private-sector workers depends (negatively) on a, with no bound except at $a = 0$.

2.2.1.2 Optimization by public-sector workers

The optimization problem for public-sector workers is similar in spirit to the optimization problem for private-sector workers, with two important differences. First, public-sector workers have access to a second source of income during retirement, besides their own personal savings, namely a government-provided pension. Second, unlike workers in the private sector, who are uninformed of the level of benefits in the public sector, public-sector workers are aware of the level of benefits that they have been promised. As a result, the decision by either political party to set a low level of taxes in period 1 (and hence underfund the pension plan)

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[12]Recall that in any given period, there are two generations of workers in each sector of the economy, of which one generation is working and the other is retired. Thus, the total population at any given point of time is: $2 * N^P + 2 * N^G$. The normalization I choose is: $(2 * N^P + 2 * N^G) = 1$.

does not influence their voting behavior. A formal derivation for this intuition is provided in the Appendix.

2.2.2 The political model[13]

2.2.2.1 Parties

The two parties in the model differ on a dimension that is unrelated to their stance on economic issues, that I label as "ideology." Ideology is not amenable to change at will during an election campaign and is assumed to be invariant over time.[14] Parties care about winning elections but they also care about voter well-being. This latter assumption, although non-standard, is not without precedence. Wittman (1977, 1983) argues that politicians care about policies and makes the point that in standard voting models voters are assumed to vote for the candidate whose policies will yield them the highest expected utility and are hence interested in policy themselves. It seems strange then to assume that, unlike voters, politicians do not care about substantive policy, even though the effects of government policies are experienced by all, including the politicians themselves (Wittman, 1983). Along related lines, we have the following from Ansolabehere (2008):

> The labor market for politicians may sustain the expression of candidate preferences in electoral competition. ...Local posts are usually part-time or volunteer jobs. The appeal of such posts is the ability to make a difference in the community, rather than the pay. Those who get involved in local government, then, are motivated at first by ideological or "consumption" benefits, rather than by the value of office.

Arulampalam et al. (2009) in studying transfers from the federal government to state governments in India posit that the federal government is interested in maximizing total welfare accruing from grants in addition to reaping electoral gains from targeted transfers to aligned and swing state governments. In a similar vein, Ujhelyi (2013) in studying the policy impact of civil service regulations implemented at the state level in the U.S. during the 20th century, also assumes that politicians care about social welfare in addition to private benefits.

For these reasons, I let the utility of politicians be a function of both the ego rents obtained from winning office and the realized utility of all voters, which depends on policies chosen by the incumbent government. I allow for the possibility of a systematic difference in the ex-ante

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[13]The political model draws on the model of electoral competition laid out in Persson and Tabellini (2002).

[14]In the U.S. context, one might think of cultural issues such as abortion or gun control or gay marriage as non-economic issues along which voters sort across party lines.

utility as perceived by voters when the policies are announced (and on the basis of which they vote in the elections) from the ex-post utility as realized by them after policies in both periods have actually been implemented. This allows for the possibility that although some policies may appear to be favorable to voters at first sight (and hence popular with them), those policies may harm them in the long run. For example, taking on very high levels of debt in one period to have them be repaid in a subsequent period may be an example of one such policy as long as voters misperceive the true costs of the debt and do not make fully offsetting adjustments by saving in a period of high debt accumulation and dis-saving in the subsequent period. In such a setting, the Ricardian equivalence would fail to hold.

2.2.2.2   Voters

All voters, irrespective of the sector they are employed in, vote based on a combination of economic and ideological considerations. U_{per}^{ij} and U_{act}^{ij} capture the economic well-being of voter i belonging to group j as perceived by the voter prior to voting (and prior to the actual implementation of policies) and the utility as realized by the voter after the actual implementation of policies respectively, $j\epsilon\{P,G\}$. I let U_{per}^{iP} and U_{act}^{iP} differ for private-sector workers because although economic well-being for private-sector workers depends on the level of taxes chosen in periods 1 *and* 2, under the assumptions made in 2.2.1.1, decisions regarding savings for period 2 are made before the level of taxes in period 2 is known. This same argument does not apply for public-sector workers however, who correctly anticipate the level of taxes in period 2.

I let v_{per}^{ij} and v_{act}^{ij} denote the utility of voter i belonging to group j as perceived by the voters prior to voting and as realized after the actual implementation of policies respectively, reflecting the combination of economic and ideological considerations that voters value. v_{per}^{iP} and v_{act}^{iP} are likely different from each other, whereas v_{per}^{iG} and v_{act}^{iG} are the same. Based on the timing of the model, voting behavior for both group of workers depends on v_{per}^{ij}, whereas actual realized voter well-being depends on v_{act}^{ij}.

Finally, let p^k denote the probability that politician from party k wins when the level of period 1 taxes announced are T_1^k and T_1^{-k} respectively, $k\ \epsilon\{L,R\}$.[15] I express the probability of winning in terms of the level of funding implicitly chosen for the pension plan as there is a one-to-one mapping between the level of period 1 taxes announced, T_1^k and the level of funding

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[15]-k denotes the other party. For example, $p^R = p^R(T_1^R, T_1^{-R})$. Alternatively, $p^R = p^R(T_1^R, T_1^L)$.

chosen, a^k. Let E denote the ego rents for politicians from coming to office.[16] Thus, I express V^k, the utility of politician from party k, $k\epsilon\{L,R\}$ as:

$$V^k = p^k(a^k, a^{-k}) * [E + \sum_j N^j U^j_{act}(a^k)] + (1 - p^k(a^k, a^{-k})) * \sum_j N^j U^j_{act}(a^{-k}) \qquad (2.9)$$

where $U^j_{act}(a^k)$ ($U^j_{act}(a^{-k})$) is the ex-post economic utility realized for voters belonging to group j when a^k (a^{-k}) is the level of pension plan funding that is chosen. More simply, V^k is given by:[17]

$$V^k = p^k(a^k, a^{-k}) * [E + \sum_j N^j (U^j_{act}(a^k) - U^j_{act}(a^{-k}))] + \sum_j N^j U^j_{act}(a^{-k}) \qquad (2.10)$$

I express the perceived utility of voters based on the policy chosen as: $v^{ij}_{per}(a^k) = \kappa^j U^j_{per}(a^k) + (\sigma^{ij} + \theta) * D^L$, where D^L takes a value of unity if party L wins the election and zero otherwise. Here σ^{ij} is an individual-specific parameter and θ is a random variable capturing the preferences of the whole population. Individuals with $\sigma^{ij} > 0$ (< 0) have a bias in favor of (or against) party L, which is stronger the greater σ^{ij} is (in absolute value). I assume that $\sigma^{ij} \sim U[-\frac{1}{2m^j}, \frac{1}{2m^j}]$. This suggests that each group has members inherently biased towards each of the parties, even though the distribution of party bias may differ across groups. Groups may also differ in the extent to which they care about ideology. A higher κ^j reflects a higher weight placed by members of group j on economic well-being relative to ideology. Lastly, θ captures the average popularity of party L in the overall electorate. I also assume that $\theta \sim U[-\frac{1}{2h}, \frac{1}{2h}]$. The specific realization of θ is unknown to the parties when they announce their platforms, making the election outcome uncertain. We might think of θ as a piece of news (say, a scandal) which comes out shortly before an election, but after policies have already been announced by the parties, that affects the probability of voting for a particular candidate from a party for all voters equally.

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[16]For simplicity, I let the ego rents, E be the same for politicians of both parties.

[17]In order to rule out the possibility that the candidate wants to lose the election in order to maximize his utility, I impose the constraint that E, the ego rents from office are large enough such that $[E + \sum_j N^j (U^j_{act}(a^k) - U^j_{act}(a^{-k}))] > 0$ for all possible choices of a^k and a^{-k}, $k \epsilon\{L,R\}$.

2.2.3 Solution of the game

2.2.3.1 Equilibrium

I use backward induction to solve for the equilibrium of the game. The equilibrium concept used is that of sub-game perfect Nash equilibrium and, to solve for the Nash equilibrium of the game, I derive the best response function for the candidate of each party. Following a set of steps outlined in the Appendix, I show that the best response function is symmetric for both parties, L and R and does not involve any variables which are party-specific. Thus, in Nash equilibrium, the parties announce identical policy platforms, i.e. the same level of taxes for period 1 of the model, which, in turn, correspond to identical pension plan funding levels, i.e. in equilibrium, $a^L = a^R$.[18]

2.2.3.2 Comparative Statics

The goal of this sub-section is to consider the effects of an increase in the level of political competition on the equilibrium pension plan funding levels, a^L and a^R. I derive two results, each of which reflects alternative ways of thinking about the effect of an increase in the level of political competition. All proofs are provided in the Appendix.

(1) Result 1 – An increase in the weight voters place on economic well-being relative to ideology: The parameter in this model that captures the weight placed by voters on economic well-being is κ^j. Groups with a higher value of κ^j are more mobile and politicians may weigh their welfare more (and announce policies accordingly) because doing so increases the politicians' chances of winning the election. Thus, the interest is in $\frac{\partial a^k}{\partial \kappa^j}$, $j \, \epsilon \{P, G\}$ and $k \, \epsilon \{L, R\}$. I use the implicit function theorem to show that $\frac{\partial a^R}{\partial \kappa^P} < 0$ with $\frac{\partial a^R}{\partial \kappa^G} = 0$. Thus, as the weight placed by private-sector workers on (misperceived) economic well-being goes up (relative to the weight that they place on ideology), the policies announced by the politicians correspond to a lower level of funding of the pension plan in period 1. Interpreted more broadly, with an increase in the weight placed by individuals on economic well-being, politicians are less willing to announce policies that result in immediate economic pain at the cost of long-term gains because of the misperception by voters about the true costs of such policies. Thus the prediction that an increase in political competition would be associated with a decline in pension

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[18]Furthermore, under the set of conditions laid out in the Appendix in (2.6), I can show that both parties choose to fund the pension plan less than fully, i.e. $0 < a^L = a^R < 1$.

plan funding levels.

(2) Result 2 – Moderation of party preferences: The second comparative statics I consider is the effect of a change in the density m^j on the policy choice that is made, $j \ \epsilon \{P, G\}$. An increase in the density $m^P (m^G)$ corresponds to a higher fraction of workers from the private (public) sector having preferences that are "moderate."[19] This can be seen more readily in case σ^{ij} had a smooth unimodal distribution.[20] In that case, a shift of the mass in the distribution towards the middle would raise the probability distribution function, g_σ in that range. An increase in the density m^j of the assumed uniform distribution could be thought of as approximating such a shift towards a more ideologically neutral electorate. As before, I use the implicit function theorem to show that $\frac{\partial a^R}{\partial m^P} < 0$ and $\frac{\partial a^R}{\partial m^G} = 0$. Thus, if private-sector workers become more ideologically neutral, as proxied by an increase in the density m^P, we would anticipate a decline in equilibrium pension plan funding level.

In summary, an increase in the level of political competition, whether proxied by an increase in the weight private-sector workers place on economic well-being relative to ideology or by a moderation in their party preferences, is associated with a decline in the equilibrium pension plan funding level. Although the model does not directly predict that a higher level of political competition will be associated with a more generous benefit (recall that benefit levels were taken as exogenous), we can see that if the pension plan is not funded fully, then politicians would prefer compensating their employees in the form of benefits, which needs to be paid several years in the future, rather than in the form of wages. Thus, in practice, we would expect to see more politically competitive jurisdictions also offering more generous benefits. A similar logic would apply to the interest rate used for discounting actuarial liabilities as the effect of choosing a higher interest rate is to make liabilities appear smaller than what they are and to reduce the annual required contribution to the pension fund.

2.3 Empirical analysis

2.3.1 Data Sources

I turn now to empirically examine the key hypothesis that a higher level of political competition is associated with a lower actuarial funded ratio and more generous benefits. In order to do so, I examine local pension plans from the state of Pennsylvania. Pennsylvania provides a

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[19]Recall that voter bias towards party L is given by: $\sigma^{ij} \sim U[-\frac{1}{2m^j}, \frac{1}{2m^j}]$.

[20]The assumption that σ^{ij} is distributed uniformly is made for analytical convenience.

rich setting for the empirical analysis as it accounts for two-fifths of the nation's distinct public sector retirement systems and offers more than thrice the number of retirement systems as that of any other state. The existence of such a large number of retirement systems in Pennsylvania can be attributed to its complex system of local government. General purpose local governments, cities, boroughs, and townships, total approximately 2,600 units. Most general purpose local governments in Pennsylvania establish separate pension plans for their police and non-uniformed employees.[21] The advantages of using municipal data to test the hypothesis are the large number of comparable cases that share the same national and state-level political context (e.g. state income tax rates) at the same time they exhibit wide variation on the variables of interest, viz. political competition and funding status of pensions. In addition, the availability of rich municipal-level data from the Decennial Censuses and the American Community Surveys (ACS) enables me to control for many potentially important municipal characteristics.

Data regarding municipal pension plans offered by the various local governments within Pennsylvania are available from 1985 through 2009 in the form of biennial status reports prepared by the Pennsylvania Public Employee Retirement Commission (PERC). Status reports include the name of the municipal entity offering the plan, the employee group covered, the actuarial liabilities, actuarial assets, and number of active members in the plan. Using these reports, I construct two variables that are used in the empirical analysis: the actuarial funded percentage, defined as the ratio of actuarial assets to actuarial liabilities multiplied by 100, and unfunded liabilities per active member, defined as (Actuarial Liabilities - Actuarial Assets)/ (Number of active members). To take a numerical example, consider a plan with 100 active members whose actuarial liabilities and actuarial assets are valued at $4 million and $3 million respectively. For such a plan, the actuarial funded ratio is 3 / 4, or 75 percent, and unfunded liabilities per active member are ($4 million - $3 million)/100 or $10,000.

Constructing measures of political competition at the local level is challenging as there is no central repository for data on municipal elections at either the federal or the state level. I construct proxy measures for political competition at the local level by looking at the vote shares for the two parties for all races held between 1980 and 2009 to any of the six offices

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[21]Larger municipalities can offer multiple plans for the same class of employees (e.g. non-uniformed personnel). They will also often have a separate pension plan for their firefighters. Teachers are covered under a separate state-wide system, the Pennsylvania Public School Employees Retirement System (PSERS) that is not a part of this analysis.

for which elections are held on a state-wide basis, namely, U.S. President, U.S. Senator, Governor,[22] Attorney General, Auditor General, and Treasurer.[23] Data on votes cast for each of these offices for candidates from both the Republican and Democratic parties (and any other parties that may have contested) are available at the level of each individual municipality in successive issues of the Pennsylvania Manual. Because the results for a particular candidate in any one election cycle may have a large idiosyncratic component to it, I average the Democratic vote share[24] across all elections held within a given time period (either decade or year) to any of the six offices in constructing the average Democratic vote share for that time period. For example, in constructing a measure of political competition for a municipality for the 1990s decade, I examine all state-wide races held between 1990 and 1999 to any of the six offices for that municipality. The key measure of political competition I use in the paper is that used in Besley, Persson, and Sturm (2010) (henceforth BPS); political competition for municipality m in decade d is defined as: $PC_{md} = -|D_{md}-0.5|$ where D_{md} is the average Democratic vote share in municipality m in decade d.[25]

The use of data on races for national and state offices to generate measures of competitiveness at the local level is driven primarily by data limitations. However, an advantage to using this data is that the endogeneity bias inherent in using measures of competitiveness based on votes cast in local elections is reduced. Had I used measures of political competitiveness that are based on votes cast in local elections and introduced them in the regressions, the implicit assumption would have been that the intensity of political competition is uncorrelated with other factors which might affect the fiscal health or generosity of the pension plan. This may be untrue. For example, consider a city that is hit hard by an economic crisis. In its attempt to balance the budget, the incumbent government may end up raising taxes or cutting public services, as well as skipping the actuarially required contribution to the city's pension plan(s). The consequence of unpopular tax hikes and/or service cuts made might be that the incumbent officials lose in the next election cycle. In such a case, the city would have

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[22]Election for the office of Lieutenant Governor is held separately in the primary election; for the general election each party's ticket for Governor and Lt. Governor is made up of the highest vote getters in the separate primary elections.

[23]As Besley, Persson, and Sturm (2010) note, name recognition of candidates for down-ballot offices is typically very low among voters, making it likely that measures of political competition based on races for these offices is driven largely by party attachment of voters rather than the popularity of individual politicians.

[24]Defined as Votes cast for Democrats/ (Votes cast for Democrats + Votes cast for Republicans).

[25]Following BPS (2010), note that I can include both the Democratic vote share (D_{md}) and the measure of political competition (PC_{md}) in the same regression because of the kink in the measure of competition when the Democratic vote share reaches 50 percent.

experienced a change in its level of political competition, as per the measure of competition I construct and one would see a concomitant change in the level of political competition and a decline in the funding status of the pension plan(s).[26] However, it would be wrong to conclude on this basis alone that the two are causally related. The underlying factor driving the change in political competition and the decline in funding status of the pension plan(s) would be the city's poor fiscal condition. By constructing measures of competition at the local level that are based on races to national and state-level offices, it may be possible to reduce the importance of this potential omitted variable bias.

In terms of the control variables, I start off with a parsimonious specification that includes only the measure of political competition and average Democratic vote share along with municipality and decade fixed effects and employee-group dummies. In the second specification, I include time-varying controls at the municipal level that might affect pension plan funding level – the percentage of households that are owner-occupied (versus renter-occupied), the percentage of population aged 75 or older, and lastly, the unemployment rate. The first control is included because owners may have a longer time horizon than renters, who are more transient, and we may therefore expect municipal pension plans to have a higher funded ratio in jurisdictions where a larger fraction of households are owners. I include the percentage of population aged 75 or older as a control for the age structure of the population to allow for the possibility that municipalities with a larger fraction of older voters may be more willing to simply pass on these obligations to future generations. Lastly, the local unemployment rate proxies for local economic conditions as municipalities experiencing high levels of fiscal stress may find it hard to fund their pension plans. All of these variables are drawn from the 1980, 1990, and 2000 Decennial Censuses.

In subsequent specifications, I also include two additional controls: the share of tax revenues spent on debt servicing and the percent of pension costs paid by the state. The first of the two controls is constructed using municipal financial reports prepared on an annual basis by the Department of Community and Economic Development (DCED) of Pennsylvania and offers a snapshot of municipal fiscal health. For the second control, I note that the state of Pennsylvania distributes about $200 million each year as aid to municipalities in meeting their pension costs. This state aid is linked to the amount of tax collected on insurance

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[26]Whether political competition goes down or goes up depends on the initial starting level of political competition and the extent of the swing.

premiums from all out-of-state insurance companies and can only be used to subsidize the pension costs of municipalities. State aid is capped and limited to the entirety of the annual pension costs payable by the municipality. For other municipalities, where pensions costs are high enough that the state aid available does not defray the full pension costs, the amount of aid distributed per eligible employee is based on the total amount of money available for distribution and the total number of employee units across all municipalities.[27] The relative generosity of the aid available only for defraying pension costs (but not wages) has made municipal officials more willing to grant increased pension benefits and enhance them in lieu of other forms of compensation (PERC Actuarial Report, 2011). I control for the percentage of pension costs borne by the state as one might expect unfunded liabilities to be be smaller in municipalities where a greater share of the pension costs are borne by the state.

Although the variation in the amount available as state aid from year to year is exogenous to pension plan characteristics for a given municipality or a municipality's own fiscal conditions, it is likely that the degree to which municipalities use up the amount of state aid available in any given year depends on their intensity of political competition. Municipalities that are more politically competitive may be more likely to set their compensation package such that they use up the entire state aid that is available. Thus, introducing the percentage of pension costs borne by the state directly as a control could be problematic because of endogeneity issues. In order to alleviate this concern, I instrument the percentage of pension costs reimbursed by the state with the weighted average percentage of pension costs borne by the state in all *other* municipalities located within the same county as the municipality in question.[28] This data on pension aid received by each municipality over the period 1985 and 1990–2011 has been obtained through filing a Right-to-Know request with the Auditor General's office which is responsible for disbursal of pension aid to municipalities.

Summary statistics for all variables are presented in Table 2.1 below. The table indicates the large amount of variation in each of the dependent variables of interest: the actuarial funded ratio, the annual average pension benefit, and the interest rate used for discounting actuarial liabilities.[29] I also note the considerable variation in the level of political competition

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[27]The only exception to these general rules is a specific cap for the city of Philadelphia which is limited to receiving a maximum of 25 percent of the total money available under this scheme.

[28]Thus, for example, for State College Borough in Centre County for the 1990s, I instrument the percentage of pension costs paid for that municipality by the weighted average percentage of pension costs paid for in the remaining 35 municipalities within Centre County in that decade.

[29]I note that the summary statistics for the actuarial funded ratio and unfunded liabilities per active member are based on numbers as reported by the municipalities themselves and are based on discounting future actuarial

observed from -0.401 (corresponding to a Democratic vote share of 0.901 in Yeadon Borough, Delaware County for the 2000s – least competitive) to -0.000 (corresponding to a Democratic vote share of 0.500 in Highspire Borough, Dauphin County for the 2000s – most competitive). The difference in the level of political competition between Yeadon and Highspire Boroughs is the maximum variation in the independent variable of interest observed in the data.

[Table 2.1 about here.]

2.3.2 Empirical Specification

When considering the funded ratio and the size of unfunded liabilities for which data is available on a biennial basis over a 25-year period from 1985 to 2009, the empirical specification used is:

$$F_{imd} = \alpha + \beta_1 * PC_{md} + \beta_2 * D_{md} + \beta_3 * C_{id} + \beta_4 * X_{md} + \lambda_m + \gamma_d + \varepsilon_{imd} \qquad (2.11)$$

where:

- F_{imd} is the dependent variable: either the average funded ratio (defined as the ratio of actuarial assets to actuarial liabilities multiplied by 100) for plan i in municipality m averaged over decade d or the level of unfunded liabilities per active member in the plan, averaged over the same time period.

- PC_{md} is a measure of political competition in the municipality m averaged over decade d;

- D_{md} is the average Democratic vote share for all state-wide races in municipality m as of that same period;

- C_{id} are a set of dummy variables indicating which group of employees are covered by the plan (e.g. policemen or non-uniformed personnel, etc.);

- X_{md} are time-variant controls at the municipal level. These include the percentage of households that are owner-occupied, the percentage of population aged 75 or older, the

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liabilities at an interest rate ranging between 5.5–8.0%. If an interest rate of 3.5% were used instead reflecting the nominal yield on long-term Treasury bonds (as of September 2013), then using a back-of-the-envelope calculation, the median actuarial funded ratio would decline from 103.05 to 64.69. Assuming an interest rate of 5% for discounting actuarial liabilities, reflecting yields on high-grade corporate bonds, would cause the median actuarial funded ratio to decline from 103.05 to 79.13.

unemployment rate, the fraction of tax revenues spent on debt service, and the percentage of pension costs borne by the state, instrumented as described above.

- Lastly, λ_m are municipal fixed effects and γ_d are decade fixed effects.

The choice of control variables is influenced by the prior literature (Eaton and Nofsinger, 2008; Coggburn and Kearney, 2010; Munnell et al., 2010) and the availability of data. I include municipal fixed effects across all specifications and all municipal and plan-level factors that are invariant over time (such as age of the pension plan) would be absorbed in these fixed effects. I cluster standard errors at the county level all throughout the paper to account for inter-temporal correlation in the error terms (Bertrand, Duflo, and Mullainathan, 2004).

When considering the generosity of the pension plan, and the interest rate used for discounting future actuarial liabilities, for which data is available on a biennial basis for a 7-year period from 2003 to 2009, the empirical specification needs to be modified. As it becomes less plausible for a change in the level of political competition within a municipality to have an effect on the features of the pension plan within this short time period, I dispense with the use of municipal fixed effects. Instead, I use county fixed effects, λ_c in the following specification. I also replace decade fixed effects with year fixed effects, γ_t. Thus, the specification used is modified as:

$$F_{imt} = \alpha + \beta_1 * PC_{m(t-1)} + \beta_2 * D_{m(t-1)} + \beta_3 * Z_{it} + \beta_4 * X_{mt} + \lambda_c + \gamma_t + \varepsilon_{imt}. \qquad (2.12)$$

In addition to the differences noted above between (2.11) and (2.12), I lag the political variables by an year because elections to national and state offices are held in even-numbered years whereas the data on pensions is for the odd-numbered years. I also introduce data on three additional control variables in these specifications – the fraction of employees that are organized under collective bargaining, a dummy variable that captures the coverage of local employees under Social Security, and the class to which a municipality belongs, available from reports of the Pennsylvania DCED (for example, Township versus Borough versus City).[30] Municipalities of different classes differ in the set-up of their local governments, which might also influence the characteristics of their pension plans. Lastly, given that the 2010 Census does not have data on the same set of demographic and socioeconomic characteristics as were

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[30]About 28 percent of state and local government employees in the U.S. were not covered by Social Security in 2008 (Nuschler, Shelton, and Topoleski, 2011). I find that extent of coverage of local employees within Pennsylvania under Social Security is similar to the national average, with about 26 percent of local employees in the sample not covered by Social Security in 2009.

available for prior Censuses, I use the 2007–2011 5-year American Community Survey (ACS) to obtain necessary data on the control variables.[31]

2.4 Results

In this section, I present the results of analyzing the fiscal health of pension plans operated by the various municipalities within Pennsylvania as judged on a number of dimensions. I present my results in six sub-sections: the first deals with the actuarial funded ratio and the level of unfunded liabilities, the second deals with the generosity of benefits, the third with the interest rates used for discounting future actuarial liabilities, and the fourth goes back and takes another look at the actuarial funded ratio. All of these sub-sections focus on defined benefit plans, in contrast to the fifth sub-section which considers the effects of political competition on the generosity of defined contribution plans. The final sub-section summarizes the results of the prior sub-sections.

2.4.1 Results on funded ratio and unfunded liabilities

The data available from the biennial status reports of the Pennsylvania PERC from 1985 through 2009 make it possible to analyze the actuarially funded ratio and the level of unfunded liabilities per active member. Before presenting the evidence from the regressions, I present a graphical representation of the data. Consistent with the spirit of the subsequent regressions that involve municipal fixed effects, I calculate the *change* in funded ratio for all defined benefit plans between the first decade of the sample (including the years 1985–1989) and the third (and last) decade of the sample (including the years 2001–2009). I also calculate the change in the intensity of political competition over these two different decades and split municipalities into terciles based on the *change* in the intensity of political competition. Finally, I plot the median change in actuarial funded ratio for municipalities in each of the three terciles. Results are presented in Figure 2.1 below and suggest that, as the level of political competition goes up for a given municipality, the funding status of its pension plans deteriorates.

[Figure 2.1 about here.]

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[31]I use a linear interpolation using data from the 2000 Census and the 2007–2011 5-year ACS for estimates of the municipal demographic controls for each year between 2003 and 2009.

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2.4.1.1 OLS Estimates on the Effects of Political Competition on Funded Ratio and Size of Unfunded Liabilities

I present the results of estimating specification (2.11) with the actuarial funded ratio as the dependent variable in the first three columns of Table 2.2, and with unfunded liabilities per active member as the dependent variable in the last three columns of the same table. Column (1) corresponds to the most parsimonious specification and includes only controls for the average Democratic vote share, dummy variables for the various employee groups covered by the pension plans, and municipal and decade fixed effects. Column (2) introduces the time-variant controls from the Census that control for tenure structure, the age structure of the population, and the local unemployment rate. Finally, column (3) is the most complete specification and includes all municipal-level controls including the fraction of tax revenues spent on debt service and percentage of pension costs paid by the state (instrumented as described earlier). Columns (4) through (6) follow the same pattern as columns (1) through (3).

[Table 2.2 about here.]

The estimated coefficients on the political competition variable suggest that a higher level of political competition is associated with a statistically significant decline in the actuarial funded ratio. To provide a sense of magnitude of these effects, note that if the level of political competition were to increase by one standard deviation,[32] the funded ratio for the average pension plan would decline by about 7–8 percent. To take a more extreme example, an increase in political competition from the lowest level observed among all municipalities within Pennsylvania to the highest level observed would translate to a decrease in the funded ratio of about 40–43%, depending on the specification used.[33]

The conclusions from columns (1) through (3) of Table 2.2 are mirrored in columns (4) through (6) with unfunded liabilities per active member as the dependent variable. They suggest that an increase in the level of political competition is associated with an increase in the level of unfunded liabilities per active plan member. In terms of magnitude, a one standard

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[32]A one standard deviation increase in the level of political competition, using the measure defined in BPS (2010), would result if the Democratic vote share were to go down from 57.2 percent (leaning Democratic) to 50 percent (most competitive), or conversely, go up from 42.8 percent (leaning Republican) to 50 percent (most competitive).

[33]The level of political competition observed varies from -0.40066 (Yeadon Borough, Delaware County) to -0.00019 (Highspire Borough, Dauphin County). The difference is 0.40047, and 0.40047 * 98.9 (from column (3)) = 39.6 and 0.40047 * 108.1 (from column (1)) = 43.3. Those two numbers establish the possible range of variation in the actuarially funded ratio.

deviation increase in the level of political competition corresponds to an increase in the level of unfunded liabilities per active member of about \$2,300–2,600. Expressed differently, an increase in political competition from the lowest level observed in the data to the highest level observed would be associated with an increase in the level of unfunded liabilities per active member of approximately \$13,100–14,700. To put those numbers in context, the average level of unfunded liabilities per active member for plans which are less than fully funded is about \$11,000.

In addition to using the above measure of political competition, I can also operationalize political competition differently. As pointed out by Boyne (1998), one ought to take the volatility of party strength into account when constructing a measure of political competition. In the same vein, Riley (1971) states:

> The fact that the winning candidates in state X usually get 55% of the vote could mean that the state has a more or less permanent minority of 45% of the electorate or that the state's party identifiers are rather evenly split and there is a highly volatile set of 'independent voters' swinging from one side to the other.

Motivated by these considerations, an alternative measure of political competition I construct is the standard deviation of Democratic vote share across all elections over a decade. Results with political competition, thus defined, as the independent variable are presented in Table 2.3 following the same pattern as used in Table 2.2.

[Table 2.3 about here.]

As we can see, political competition continues to have a negative and statistically significant effect on the actuarial funded ratio and a positive and statistically significant effect on the size of unfunded liabilities per active member. A one standard deviation increase in the level of political competition leads to a decline in the actuarial funded ratio of about 10 percent and an increase in the unfunded liabilities per active member of about \$3,200.

2.4.1.2   IV Estimates on the Effects of Political Competition on Funded Ratio and Size of Unfunded Liabilities

Although the use of national and state election results to generate measures of political competition at the local level reduces the endogeneity concerns that would have applied had I used data on local elections in constructing these measures, there may be unobserved factors

(e.g. unobserved fiscal stress) that influence both local political competition and pension plan funding levels simultaneously and result in the negative relationship that has been captured above. In order to deal with such concerns, I adopt an Instrumental Variables (IV) approach and look for instruments which can predict variation in the intensity of political competition at the municipal level. In constructing these estimates, I draw on the literature referenced in Beyond the Melting Pot (Glazer and Moynihan, 1963) and subsequent discussions that emphasize the role of ethnicity and its influence on political behavior. In the context of Pennsylvania politics, ethnicity appears to play a significant role, akin to the role it has played in New York politics that motivated the original observations of the authors:

> It is striking that in 1963, almost forty years after mass immigration from Europe to this country ended, the ethnic pattern is still so strong in New York City. It is true we can point to specific causes that have served to maintain the pattern. But we know it was not created by the great new migrations of Southern Negroes and Puerto Ricans into the city; nor by the "new" immigration, which added the great new communities of East European Jews and Italians to the city; it was not even created by the great migration of Irish and Germans in the 1840's. Even in the 1830's, while the migration from Europe was still mild, and still consisted for the most part of English-speaking groups, one still finds in the politics of New York State, and of the city, the strong impress of group differentiation. In a fascinating study of the politics of the Jacksonian period in New York State, Lee Benson concludes: "At least since the 1820's, when manhood suffrage became widespread, ethnic and religious differences have tended to be relatively the most widespread sources of political difference." [Lee Benson, The Concept of Jacksonian Democracy, Princeton, New Jersey, 1961, p. 165]

> There were ways of making distinctions among Welshmen and Englishmen, Yorkers and New Englanders, long before people speaking strange tongues and practicing strange religions came upon the scene. The group-forming characteristics of American social life – more concretely, the general expectation among those of new and old groups that group membership is significant and formative for opinion and behavior – are as old as the city. The tendency is fixed deep in American life generally... (Glazer and Moynihan, 1963).

In the spirit of the above discussion, I explore variation in the ancestral origins and ethnic composition of the Pennsylvania population to predict variation in the intensity of political competition at the local level. Although the nature in which ancestry data gets reported has changed somewhat over time, it is possible to construct estimates of the percentage of people that belong to any one of the six largest ancestries - English, French, German, Irish, Italian, and Polish for the entire sample period using data from the 1980, 1990, and 2000 Censuses.[34] Of these six different groups, I find that for any given decade, (1) municipalities where a higher percentage of the population is of German ancestry have a lower Democratic vote share in national and state-level races, whereas municipalities where a higher percentage of

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[34]The 1980 Census was the first census in which individuals were asked to report their ancestry.

the population is (2) of Irish ancestry or (3) of Italian ancestry have a higher Democratic vote share. (4) In addition, municipalities where a higher fraction of households are headed by blacks, also have a higher Democratic vote share. I therefore include these four variables as instruments for Democratic party support. A graphical representation illustrating the pattern of correlation of these instruments with Democratic vote share is presented in Figure 2.2 for the 2000s decade.

[Figure 2.2 about here.]

The instruments, however, perform poorly when it comes to predicting variation in the intensity of political competition at the municipal level. In order to improve the predictive-fit of the first stage regression, I introduce meaningful interactions between the instruments. The two interactions introduced are the product of (5) percentage ancestry German and percentage ancestry Irish and (6) percentage ancestry German and percentage ancestry Italian. The intuition for introducing either of the interaction terms is the same: as both terms that constitute the interaction go up, the municipality becomes more politically competitive. For example, a municipality where the population is roughly evenly divided between those of German descent and those of Italian descent is likely to be highly competitive because both parties have a constituency that is naturally pre-disposed in their favor.

Results using these 6 different IVs (the four straight terms and the two interactions) as instruments for the two potentially endogenous variables, political competition and average Democratic vote share, are presented in Table 2.4. In Panel A of Table 2.4, the measure of political competition used is the one proposed by BPS (2010), whereas in Panel B of Table 2.4, the alternative measure of political competition, viz. the standard deviation of vote share is used. As in earlier tables, columns (1) through (3) of both panels consider the effect of variation in political competition on the actuarial funded ratio, whereas columns (4) through (6) examine the effect of this variation on the size of unfunded liabilities per active member. In the interest of brevity, only the coefficients on political competition and average Democratic vote share are included in the table. Full results are available from the author.

[Table 2.4 about here.]

As we can see, political competition continues to have a negative and statistically significant effect on the actuarial funded ratio, whereas it continues to have a positive and statistically significant effect on the size of unfunded liabilities per active member, irrespective of

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which measure of political competition is used. Comparing the coefficients in the first row of Table 2.2 with those in the first row of Panel A of Table 2.4, we see that the IV estimates of the effects of political competition, using the measure defined by BPS (2010), are considerably larger than the OLS estimates. We arrive at the same conclusion when comparing the coefficients in the first row of Table 2.3 with those in the first row of Panel B of Table 2.4 when the standard deviation of Democratic vote share is used as the measure. A one standard deviation increase in the level of political competition is now associated with a larger decline in the actuarial funded ratio of about 14–16 percent using the first measure, and about 31–32 percent when using the second measure. The corresponding increase in the size of unfunded liabilities per active member is about \$7,600–8,600 and \$12,000–13,600 respectively using the two measures.

2.4.1.3 Robustness Checks on the Effects of Political Competition on Funded Ratio and Size of Unfunded Liabilities

In order to examine the robustness of the results that political competition is associated with a decline in actuarial funded ratio and an increase in the size of unfunded liabilities, I conduct a number of robustness checks in Table 2.5. Panel A of Table 2.5 presents the robustness checks with actuarial funded ratio as the dependent variable, while Panel B presents the checks with unfunded liabilities per member as the dependent variable. OLS estimates are presented in columns (1) through (3) and IV estimates are presented in columns (4) through (6) of both panels. In the interest of brevity, I report the robustness checks using the first measure of political competition, viz. the absolute difference of the Democratic vote share from 50% and present only the coefficient on the political competition variable, omitting coefficients on all controls. Results using the standard deviation of Democratic vote share as the measure of political competition are similar in statistical and economic significance and are available from the author.

(a) *Robustness Check (RC) 1: Long differences:* The data used thus far encompasses three decades. In order to examine if long-run shifts in the intensity of political competition have the same effect as those found using data over all three decades, I estimate regressions using data from only the first decade (including the years 1985–1989) and the last decade of the sample (including the years 2001–2009). The negative relationship

between the intensity of political competition and actuarial funded ratio holds with this approach as well, and the coefficients are similar in economic and statistical significance to the base specifications.

(b) *RC2: Weighting the regressions by the number of members:* The regressions reported above are unweighted, thereby according equal importance to a plan with a single member and a plan with several hundred members. To explore whether the results hold if I were to assign different weights to plans based on their size, I re-estimate the regressions with weights assigned to each observation based on the number of active members in the plan.[35] The results are similar to what I had before suggesting that the effects of political competition are not driven by or limited to small plans but are present across plans of varying sizes.

(c) *RC3: Not controlling for average Democratic vote share:* In the regressions estimated thus far, I have included the average vote share for Democrats as a control variable. This lets us separately identify the effect of an increase in Democratic support from an increase in the level of political competition. In order to explore the robustness of my findings to excluding this variable, I estimate the regressions with just the intensity of political competition and find that an increase in political competition continues to be associated with a decline in the funded ratio and an increase in the size of unfunded liabilities per active member.

(d) *RC4: Using average vote share based on Presidential elections:* The approach of using votes cast for elections held to national and state offices to construct measures of political competition, while motivated by data limitations, is also less likely to be contaminated with reverse causality. Nevertheless, it could still be argued that voters consider the performance of their local government officials in casting their votes for officials elected to state-level offices, such as the Governor or the Auditor General.[36] Voters are, however, least likely to consider the performance of their local government in deciding on

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[35]To prevent some very large plans like those for Philadelphia and Pittsburgh from driving the results, I estimate the regression only using observations which have a leverage of less than 1. Leverage captures the deviation of an independent variable from its mean. As high leverage points can have a considerable effect on the estimate of regression coefficients, it is prudent to only include observations with leverage less than a pre-set threshold (in this case 1).

[36]The importance of local politicians in influencing the electoral success of politicians at higher levels has been recognized in a variety of contexts including India (Bohlken, 2012), sub-Saharan Africa (Kasara, 2007 and Baldwin, 2013), and Latin America (Ames, 1994 and Samuels, 2000).

their votes for the office of U.S. President. Thus, using vote share based on presidential elections is a way of minimizing the possibility of reverse causality and endogeneity that might be associated with using data on elections to all national and state-level offices. Therefore in this robustness check, I construct a measure of political competition based solely on votes cast in the Presidential elections and introduce that in the regressions to find that the coefficients on political competition are similar to their previous values.

(e) *RC5: Using a different operationalization of the measure of political competition:* The primary measure of political competition used in the paper uses the definition laid out by BPS (2010) as $PC_{md} = -|D_{md} - 0.5|$. In addition to using the standard deviation of Democratic vote share as a measure of political competition in Tables 2.3 and 2.4, an alternative approach in terms of operationalizing political competition is to introduce the average Democratic vote share and the average Democratic vote share squared in the same specification. If political competition tends to decrease (increase) the funded ratio (level of unfunded liabilities), then I would expect to see a negative (positive) coefficient on the linear term and a positive (negative) coefficient on the squared term. This is, in fact, what I find with this alternative operationalization of political competition.

[Table 2.5 about here.]

Overall the results presented in Tables 2.2 through 2.5 offer robust evidence that political competition plays a significant role in influencing the health of public-sector pension plans. In particular, an increase in the level of political competition is associated with a decline in the funding status of these plans and an increase in the level of unfunded liabilities per active member.

2.4.2 Results on plan generosity

Following the examination of the effects of political competition on the funding status of public-sector pension plans, I now turn to an analysis of the effects of political competition on the generosity of these plans. When defining the generosity of a plan, I include the pension offered to employees who retire from service in the normal course of events or employees who are enrolled in the Deferred Retirement Option Plans (DROP) but exclude the pension

received by disabled employees or recipients of surviving spousal or surviving child benefits.[37]

Before moving to the regressions, I first present a graphical representation of the data. In order to construct the figure, I split municipalities into terciles based on their level of political competition for the year 2009. I plot the median annual pension received by retirees for the three terciles into which municipalities have been split along with the median level of political competition in each of these three terciles.

[Figure 2.3 about here.]

The figure suggests that as the level of political competition in a municipality goes up, the plans it offers tend to be more generous in their average retirement benefits.

2.4.2.1 OLS Estimates on the Effects of Political Competition on the Generosity of Benefits

The pattern in which the regression results are presented in the next five tables (Table 2.6–Table 2.10) is similar to that of the prior tables with the exception that, for these tables, I include county fixed effects rather than municipal fixed effects and year fixed effects rather than decade fixed effects.[38] I am also able to include additional control variables, the fraction of employees covered by collective bargaining, Social Security coverage under the pension plan, and dummy variables for the class of municipality.

Columns (1) through (4) of the following table consider the variation in the level of benefits in absolute terms and do not control for the level of wages. Columns (5) through (8) introduce the log of benefits as the dependent variable and also control for the log of wages to allow for the possibility that wages may be lower to offset the increased generosity of pensions and in that case, looking at retirement benefits alone may offer a misleading picture of plan generosity.

[Table 2.6 about here.]

As the coefficients on political competition from the first row of Table 2.6 suggest, an increase in the level of political competition is associated with an increase in the average pension received by retirees. Based on the coefficients in columns (1) through (4), a one standard

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[37]Under DROP, employees accumulate their monthly service retirement benefit in an interest-bearing account while continuing to be employed by their employer.

[38]Additional regressions that involve municipal fixed effects, in a long difference setting, support the conclusions of this section and are available from the author on request.

deviation increase in the level of political competition is associated with an increase in the average pension received by retirees of about \$470–620 per retiree. Given that the average annual pension received by retirees is about \$15,360, this translates to an increase in the generosity of the pension of about 3.0–4.0%. An increase in the level of political competition from the lowest to the highest level observed in the data would be associated with an increase in the average annual pension received of about \$2,800–3,700 per retiree or 18–24%.[39]

The estimates in columns (5) through (8) support the conclusions reached on the basis of columns (1) through (4). The coefficients in the first row suggest that, controlling for the log of wages, a one standard deviation increase in the intensity of political competition is associated with an increase in the generosity of the pension benefit of about 3.2–3.8%. Contrary to the theory of compensating differentials (but consistent with much of the empirical literature), the coefficient on wages in these benefit regressions is positive and statistically significant across specifications.[40]

A number of the other coefficients also have expected signs, although only a few are statistically different from zero. The coefficient on the fraction of employees represented by collective bargaining is positive and statistically significant at the 1% level in each of the 8 specifications in which it is introduced, suggesting that unionization is strongly associated with an increase in the generosity of these benefits. The estimated effects of being organized under collective bargaining are large, ranging from 27–32% when I do not control for wages and 20–27% when I do. These estimates are in the same spirit as those which suggest that cities with collective bargaining spend about 20 percent more per capita on health benefits for policemen and firefighters compared to cities where such departments are not similarly organized (Anzia and Moe, 2012).

Among other findings, the negative coefficient on the unemployment rate in columns (3) and (4) suggests that municipalities that are experiencing fiscal stress are less likely to offer generous retirement benefits to their public-sector workers. A one standard deviation increase

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[39]For the years 2003–2009, the level of political competition observed varies from -0.4426 (Yeadon Borough, Delaware County – least competitive) to -0.0000 (Rockledge Borough, Montgomery County – most competitive). The difference is 0.4426. 0.4426 * 6.282 (from column (3)) = \$2,780 and 0.4426 * 8.289 (from column (1)) = \$3,668. Those two numbers establish the possible range of variation in the increase in annual pension received by retirees. Given that the mean annual retirement benefit available to retirees is \$15,360, the corresponding range in percent terms is 18–24%.

[40]However, this does not suggest that the higher retirement benefits simply result from higher wages. When I examine the effects of political competition on either the ratio of benefits to wages or on the difference between the log of benefits and the log of wages, I continue to find economically and statistically significant effects of political competition.

in the local municipality-specific unemployment rate of 2.84% is associated with a decrease in the average annual pension of $540 per retiree, roughly the same order of magnitude as a one standard deviation increase in the level of political competition. The coefficient on Social Security coverage is also negative, although statistically insignificant. This finding is consistent with pension plans that are not covered by Social Security providing somewhat more generous pensions to compensate their employees for the lack of coverage under the Social Security system.

2.4.2.2 IV Estimates on the Effects of Political Competition on the Generosity of Benefits

In addition to the OLS estimates presented above, I also obtain estimates using the IVs that had been introduced earlier: (1) percentage ancestry German, (2) percentage ancestry Irish, (3) percentage ancestry Italian, (4) percentage households headed by a black, and the two interaction terms, (5) the product of percentage ancestry German and percentage ancestry Irish and (6) the product of percentage ancestry German and percentage ancestry Italian. The IV estimates are presented in Table 2.7 with the absolute level of benefits as the dependent variable in columns (1) through (4) without controlling for wages and the log of benefits as the dependent variable in columns (5) through (8) controlling for the log of wages.

[Table 2.7 about here.]

In columns (1) through (4) when I consider variation in the absolute level of benefits, the effects of political competition continue to be statistically significant and estimates are about twice as large as those obtained earlier using OLS. A one standard deviation increase in the intensity of political competition is associated with an increase in the average annual pension received of about $880–1,380 per retiree, or 5.8–9.0% relative to its mean value. In columns (5) through (8) in which I introduce the log of benefits as the dependent variable and control for the log of wages, the effects of political competition fall short of statistical significance though, in this case as well, the IV estimates are somewhat larger than the corresponding OLS estimates and range from 5.1–5.9%.

The effects of the other control variables, including unionization are generally similar to those that were reported in the OLS specifications. Being represented in collective bargaining is associated with an increase in benefits of about $3,900–4,600 per retiree or 26–29%. The

effect of Social Security coverage is now statistically significant in some specifications and the coefficients suggest that employees who are covered by Social Security receive about $1,000 less in benefits annually.

2.4.2.3 Robustness Checks on the Effects of Political Competition on the Generosity of Benefits

To examine the robustness of the findings with regards to the generosity of the plans, I present a set of robustness checks, similar to those presented previously in Table 2.5. The only difference between the robustness checks undertaken in Table 2.5 versus those undertaken in Table 2.8 is that in the first robustness check of Table 2.8, instead of estimating long differences (based on considering data from the first and the last decades as in Table 2.5), I estimate regressions using only one year of data (2009). The rationale for this robustness check is as follows: including observations over the entire sample period from 2003 to 2009 results in the inclusion of multiple observations for the same plan even though these observations are likely to exhibit strong serial correlation. On the other hand, by choosing a single year of observation, I include each plan once in the estimation and exploit only the cross-sectional variation in the sample.[41]

[Table 2.8 about here.]

The robustness checks support the conclusions shown in Tables 2.6 and 2.7. The results are robust to concerns regarding sampling, concerns regarding the disproportionate influence of small plans on the estimates, minor changes in specification, endogeneity concerns regarding the use of national and state-level races to construct measures of local political competition, and alternative operationalizations of political competition.

2.4.3 Results on interest rates

Finally, I examine variation in the interest rate chosen to discount future actuarial liabilities. The choice of interest rate is crucial in arriving at an estimate of the level of liabilities for a pension plan as choosing a higher interest rate makes the liabilities appear smaller. Variation

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[41]Although the robustness check is presented using 2009 as the year of choice, the results are generally not sensitive to which year is chosen. Results using other years (2003, 2005, and 2007) are available from the author on request.

in the interest rates used to discount future liabilities contributes to the wide variation in the estimates of unfunded liabilities at the state level (Healey, Hess, and Nicholson, 2012).

The Government Accounting Standards Board (GASB)'s current standards on Accounting for Pensions by State and Local Governmental Employers recommend that "the appropriate interest rate is the rate of return on plan investments that was assumed in determining the annual required contribution for the current year" (GASB27, pp. 97). Employers have used this guideline to discount their future liabilities at a rate similar to the expected rate of return on pension fund assets. Currently, the interest rate assumed by state defined benefit public pension plans countrywide average around 8 percent[42] whereas the average for the municipal pension plans considered in this paper is about 7 percent. However, given the minimal risk involved in pension obligations, assuming a discount rate of 7 or 8 percent seems out of line with the professional judgment of economists who would recommend a considerably lower discount rate (see for example, Brown and Wilcox, 2009 and Novy-Marx and Rauh, 2011).

In any event, the choice of interest rate is a crucial assumption made by the plan sponsor. Prior research (Chaney, Copley, and Stone, 2002 and Giertz and Papke, 2007) finds evidence that, in the case of state pension plans, states in fiscal stress are strategic in selecting higher interest rates to obscure underfunding and reduce their plan contributions. Given the importance of interest rates in arriving at a measure of actuarial liabilities, I examine whether politically competitive municipalities are more likely to use a higher interest rate in order to mask the true magnitude of the promises that have been made from the general public and/or financial market participants involved in dealing in municipal securities. Figure 2.4 offers a glimpse of the data and foreshadows the regressions that follow.

[Figure 2.4 about here.]

2.4.3.1 OLS & IV Estimates on the Effects of Political Competition on Interest Rates

Results with the interest rate used to discount future liabilities as the dependent variable are presented in Table 2.9. For conciseness, both OLS and IV estimates are included in the same table, with OLS estimates in the first four columns and IV estimates in the last four.

[Table 2.9 about here.]

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[42]Congressional Budget Office (2011) and author calculations from the Pension Plan Database.

The results in Table 2.9 suggest that politically competitive municipalities are more likely to choose a higher interest rate for discounting their actuarial liabilities. Using the point estimates of the coefficients in columns (1) through (4), the effect of a one standard deviation increase in the level of political competition is to increase the rate used for discounting actuarial liabilities by about 5–6 basis points. IV estimates are considerably larger than the OLS estimates and suggest that a one standard deviation increase in the level of political competition is associated with an increase in the interest rate of about 17–24 basis points. I also find that plans with a higher fraction of employees covered under collective bargaining are associated with a higher interest rate as are plans in which employees are not covered by Social Security.

2.4.3.2   Robustness Checks on the Effects of Political Competition on Interest Rates

To examine the robustness of the findings with regards to the generosity of the plans, I present a set of robustness checks, identical to those presented previously in Table 2.8.

[Table 2.10 about here.]

The results of the robustness checks generally support the conclusions reached at in Table 2.9. The notable differences pertain to RC2, when I use a weighted regression with the number of active members as weights and RC3, when the average Democratic vote share is excluded. The coefficients are statistically insignificant for RC2 in the OLS specification and for RC3 in the IV specification suggesting that small plans may have a disproportionate influence in the unweighted regressions and that the effects of political competition may be sensitive to the inclusion of controls for partisan tendencies of the population. Beyond that, the results are robust to concerns regarding sampling, endogeneity concerns regarding the use of national and state-level races to construct measures of local political competition, and alternative operationalizations of political competition.

2.4.4   Calibrating all reported funded ratios to a common interest rate

Thus far in this paper, I have used the actuarial funded ratios as provided by municipalities themselves in calculating the effects of political competition. The implicit assumption I make in these estimations is that the interest rate used by municipalities in discounting their actuarial liabilities are uncorrelated to their underlying levels of political competition.

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Tables 2.9 and 2.10 suggest that this is not the case; instead, it appears that municipalities that are more competitive choose higher interest rates. That finding further strengthens the conclusions arrived at previously with respect to the effects of political competition on actuarial funded ratios and suggests that the estimates presented thus far may be lower-bound estimates of the true effects of political competition on the funding level of municipal pension plans.

With this in mind, I attempt to recalculate the funded ratios on the basis of a common interest rate. The task is challenging because pension plans rarely disclose the stream of cash flows that are discounted to arrive at an estimate of the actuarial liabilities. One has to go through an elaborate series of calculations to "reverse-engineer" the underlying cash flows before discounting them back and arriving at estimates of the liabilities for various different interest rates (Novy-Marx and Rauh, 2011). In this case, however, not all of the data that are necessary for undertaking the series of steps are available making it impossible to replicate that process.[43] Beyond that, data regarding the interest rates chosen by the various plans is not available in the biennial reports that are available from 1985–2009 but are only available over the period from 2003–2009.

I deal with these data limitations by, first, noting that estimates of the effective average duration of pension liabilities range from 13 years (Novy-Marx and Rauh, 2011) to 15 years (Waring 2004a, 2004b). Second, lacking data on interest rates for each year for which the data on funded ratios is available, I assume that the interest rate used by the plans for the period 2003–2009 is what was used over the entire sample period from 1985–2009. I discount all liabilities with respect to two different choices of interest rates. The first interest rate chosen is 7 percent, corresponding to the median across all municipal pension plans in the sample. However, as the 7 percent rate is likely too high, I also discount them back to an interest rate corresponding to the nominal yield on zero-coupon Treasury bonds of similar duration. Based on current market conditions (as of September 2013) and expectations of market participants about future economic conditions, I use 1.5 percent for the real yield on long-term zero-coupon Treasury bonds and add in 2 percent to reflect inflation expectations, for a nominal yield of 3.5 percent. Thus, Table 2.11 presents the results for two choices of interest rates – 7 percent and 3.5 percent and two choices of the weighted average duration of liabilities – 13 years and

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[43]For example, data on the benefit factor which captures the added benefit available for an additional year of service or data on the nature of COLA adjustments in these plans are unavailable in these datasets.

15 years. For brevity, only the coefficients on political competition are included in the table. Full results are available from the author.

[Table 2.11 about here.]

As we can see, the coefficients on political competition continue to be negative and statistically significant in each of the panels, under both OLS and IV estimation techniques. The smaller absolute magnitude of the coefficient on political competition when I use Treasury yields can be reconciled with the fact that the range of variation in the dependent variable is reduced when liabilities are re-calculated using an interest rate of 3.5%. For example, when a weighted average duration of liabilities of 13 years is used, the inter-quartile range of the dependent variable, actuarial funded ratio, re-calibrated with a 3.5 percent interest rate and winsorized at the 2.5% and 97.5% levels, is 35 percent, whereas the inter-quartile range for the actuarial funded ratio, as reported by the plans themselves and winsorized similarly, is 52 percent or about 50 percent larger.

2.4.5   Placebo regressions estimated using Defined Contribution Plans

As a final check on the results obtained thus far, I use data on all defined contribution (DC) plans, available for 2003–2009 and examine the effects of political competition on the generosity of these plans.[44] Anecdotal evidence suggests that political influences are less influential in affecting the parameters for a defined contribution plan compared to a defined benefit plan.[45] For example, a report prepared by the Florida TaxWatch in the context of reform of Florida's Retirement System (FRS) (Florida TaxWatch Report, 2013) suggests that,

> Another important benefit of the DC Investment Plan is that it is insulated from political temptations....Any benefit given under a DC plan must be paid for in that same year because it cannot be legally underfunded. This improves the financial health and security of the FRS because retirement assets belong to the individual state employees and are therefore not susceptible to the whims of the state.

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[44]The model does not have any predictions on the effects of political competition on the generosity of defined contribution plans. Workers from the public and private sectors would have opposing preferences over it and the final outcome would depend on the relative sizes of the two sectors and turnout (which, for simplicity, I have assumed to be 1), in addition to the other primitives of the model.

[45]The decision of whether to offer a DB or a DC plan is, in itself, endogenous. In a set of regressions (not reported), using both OLS and probit estimation approaches, I find that an increase in political competition makes it more likely that a municipality offers a DB plan over a DC plan. That finding is consistent with the explanation that politicians in politically competitive jurisdictions are able to pass on the costs of pensions to future generations in the case of a DB plan but not in the case of a DC plan.

In Table 2.12, I therefore examine the variation in the employer contribution rate for all defined contribution plans from Pennsylvania for the period 2003–2009. I choose to focus on the employer contribution rate because for defined contribution plans, it is not meaningful to talk of funded ratios or unfunded liabilities (as the plans are fully funded by design) or the average pension benefit received by a retiree on retirement (as that depends on the pattern of withdrawal from one's retirement account). The employer contribution rate to the defined contribution plan is, however, a meaningful plan parameter for such plans as it reflects the extent to which an employer puts aside money each year and comes closest to our conception of generosity of a retirement plan.

In the interest of brevity, I only present the coefficients on the variable representing the intensity of political competition and omit coefficients on the control variables. I first estimate the effects of political competition on the employer contribution rate using data for all years and for all plans, thereby hewing exactly to specification (2.12). Subsequent rows replicate the robustness checks that were conducted earlier on defined benefit plans with each row of the table corresponding to a different robustness check. OLS estimates are presented in columns (1) through (4) and IV estimates are presented in columns (5) through (8).

[Table 2.12 about here.]

As the coefficients on political competition suggest, defined contribution plans appear less susceptible to political influence than defined benefit plans. The coefficient on political competition is statistically insignificant in each of the 48 specifications presented in the table, in contrast to our previous set of findings on defined benefit plans.[46] This null result likely follows from the fact that with defined contribution plans, it is hard for politicians to pass on the costs of a more generous plan onto future generations of taxpayers; a more generous DC plan requires a higher level of contributions today that have to be met from current tax revenues and politicians are therefore less willing to make the plans more generous and risk alienating voters in the private sector.

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[46]A sample split, based on whether employees are represented by collective bargaining or not, suggests that the lack of effect of political competition on the generosity of these plans holds across both unionized and non-unionized samples.

2.4.6 Summary

I summarize the results of the previous analyses using data from Pennsylvania's municipal pension plans in Table 2.13. Along with the sign of the coefficients on political competition, I note the impact of a one standard deviation increase in the level of political competition on each variable.

[Table 2.13 about here.]

As we can see, the hypothesized relationship between the intensity of political competition and funding status of defined benefit pension plans is borne out in the data. In addition, the evidence suggests that political competition affects the generosity of the defined benefit pension plan and the interest rate used for discounting future actuarial liabilities. These results are consistent with the predictions of the model. In contrast to defined benefit pension plans, political competition has no effect on the generosity of defined contribution plans.

2.5 Discussion

2.5.1 Discussion regarding the theoretical model

My model captures a very simple mechanism through which political competition may harm economic well-being. To the extent that politicians care about voter welfare, a high level of political competition may stand in the way of the government implementing policies that have long-run payoffs but involve short-term sacrifices. A government that operates in an environment of significant electoral competition is less likely to make decisions that involve short-term sacrifices at the cost of long-term gains.[47] Jean-Claude Junker, Prime Minister of Luxembourg captured this sentiment for his party when he said in 2005: "We all know what to do, but we don't know how to get reelected once we have done it."

The benign motives ascribed to politicians in the model is not necessary for the predictions of the model to hold. Politicians may want to minimize the extent of unfunded liabilities motivated entirely by considerations of self-interest rather than by altruism. Politicians

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[47]I note that this intuition is similar (but not identical) to the model in Acemoglu and Robinson (2006), in which more political competition intensifies political instability and diminishes the incentives to implement growth-enhancing reforms rather than seeking short-term rents.

generally own residences in the jurisdictions that they govern[48] and may therefore desire to reduce the level of unfunded liabilities simply to avoid unfunded liabilities from being capitalized into the value of their housing stock. Politicians might also care about pension funding because of concerns about employment opportunities after their tenure in office. A politician may have fewer opportunities, either at other levels of government or in the private-sector, to the extent that voters associate his tenure in office with an underfunded pension plan and subsequent tax hikes to pay for those shortfalls. Regardless of politicians' motives, it seems plausible that politicians would like to fund the pension plan when in office, in addition to maximizing their chances of winning.

Another key assumption of the model is that workers in the private sector are unaware of benefits that are offered in the public sector. One could alternatively assume that only a fraction of voters in the private sector are unaware of the level of benefits offered in the public sector, and the qualitative predictions of the model would still hold. If I were to remove this friction in the model and assume full information on the part of both public-sector and private-sector workers, then it would be harder to argue that politicians would want to underfund pensions in order to reap immediate electoral benefits as voters would not care about the level of funding chosen but only about the absolute present value of wages and benefits.[49]

The assumption that private-sector workers misperceive the level of taxes in period 2 is inextricably linked with the previous assumption of lack of information for private-sector workers regarding the level of benefits in the public sector. As long as that assumption holds, it seems likely that private-sector workers would erroneously estimate the level of taxes that they would be subject to in the second period of the model.

Besides the friction introduced in terms of private-sector workers lacking information on the level of public-sector retirement benefits, I have not introduced any additional frictions. In the presence of additional frictions, a politician would have a stronger incentive to fully

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[48]A residency requirement is common within local governments in Pennsylvania. For example, the Second Class Township code specifies: "Supervisors shall reside in the township from which elected and shall have resided in that township continuously for at least one year before their election." (http://www.psats.org/subpage.php?pageid=secondclasstownshipcode–Accessed 04/04/2013.) The Borough code for Pennsylvania also has a similar provision for local council members. (http://boroughs.org/jcp/program_guide_to_borough_government.pdf–Accessed 04/04/2013.)

[49]Workers from the public and private sectors would have opposing preferences on those and there would not be any theoretical predictions about whether the level of wages and benefits would be higher (or lower) in politically competitive jurisdictions. In this scenario of full information, the final outcome would depend on the relative sizes of the two sectors and turnout (which, for simplicity, I have assumed to be 1), in addition to the other primitives of the model.

fund the pension plan in period 1 even if he attaches no weight to voter welfare, but is purely guided by self-interest.

2.5.2 Discussion regarding internal and external validity of the results

Because of data limitations, I have used data on national and state elections to construct measures of political competition at the local level. A sense for how reasonable that assumption is can be gauged by investigating the correlations between the very limited data available for local races and races to national and state-level offices held during the same approximate period of time. Local election data, obtained through filing Right-to-Know requests with various County Boards of Elections, are available for a total of 190 municipalities across five counties of Pennsylvania for the 1980s.[50] The correlation coefficient for the Democratic vote shares in the 1980s for local and all concurrent national and state races is 0.7011, ($p < 0.001$) suggesting that the measures of Democratic support across the two different data sources are strongly correlated with each other. A similar calculation using composition of municipal councils reveals a correlation coefficient between the average Democratic vote share for all national and state races held in 2008 and the share of council seats held by Democrats in 2009 (as a fraction of the seats held by either Democrats or Republicans), of 0.6525 ($p < 0.001$). Both of these patterns suggest that the average Democratic vote share for national and state races offers a reasonable picture of the dynamics of local municipal elections within Pennsylvania for the period 1985–2009.

In addition to examining the effects of political competition on the funding status of municipal pension plans, I investigate the effects of such competition on a number of municipal fiscal characteristics. Although I do not find a statistically significant relationship between the intensity of political competition and the absolute tax burden per capita, I find a relationship between political competition and the extent to which various revenue sources are used. Politically competitive municipalities appear to raise a lower share of their revenues in the form of taxes and a higher share from non-tax sources such as transfers, charges, and miscellaneous items. In addition, political competition also affects the mix of taxes used by municipalities. About 90 percent of all tax revenue raised by municipal governments in Penn-

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[50]The five counties are: Bucks, Chester, Dauphin, Lancaster, and Lehigh. These counties were not chosen at random among the 67 counties in Pennsylvania. Instead a series of Right-to-Know requests for providing local election data were made to the 18 counties with the largest number of pension plans. The five chosen here were among the most responsive in terms of providing the election data, going back to the 1980s, a period during which this data was not recorded or archived electronically.

sylvania comes from one of two sources: the property tax and an "earned income tax," which is generally limited to a rate of 1.5 percent and excludes capital income from the tax base. I find that the share of taxes that comes from property taxes is lower in politically competitive municipalities and correspondingly, the share of taxes that comes from the earned income taxes is higher in such municipalities suggesting that politically competitive municipalities use sources of revenue that are less salient to taxpayers.[51] These findings are consistent with those reported for Italian municipalities by Bordignon and Piazza (2010) and Bracco, Porcelli, and Redoano (2013).

With respect to the validity of these findings beyond Pennsylvania, I note that the variation in funding levels of local pension plans is not unique to Pennsylvania. For Florida, a state with over 300 retirement systems, the 25th and 75th percentile of the actuarial funded ratio were 65 percent and 89 percent respectively in 2011, illustrating a considerable degree of variation in the funded ratio of those plans as well.[52] In addition, preliminary work conducted using data on state defined benefit pension plans support the conclusions reached in this paper. Using data on 85 defined benefit public-sector pension plans from the Wisconsin Legislative Council for 1989 to 2009 and a measure of political competition, I find that as the level of political competition in a state goes up, the actuarial funded ratio of plans offered by that state declines.[53] The coefficients capturing the effect of political competition on the actuarial funded ratio of these plans are similar in magnitude to those reported here.[54]

2.6   Conclusions

This paper suggests that political competition plays a key role in influencing the level of funding of defined benefit pension plans offered to public-sector employees. In their desire to win re-election, politicians in jurisdictions that are politically competitive may end up promising generous benefits to public-sector workers and then fail to make the actuarial contribution necessary to fund them fully in order to avoid having to raise taxes too high. The results

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[51]Discussions regarding the high salience of the property tax relative to other taxes is offered in Cabral and Hoxby (2013). In addition to the reasons proposed therein, the lower salience of the earned income tax relative to the property tax in this context stems from the fact that the state of Pennsylvania limits how high the earned income tax rate can be for all municipalities (barring home-rule municipalities), whereas the property tax rate is determined exclusively by local officials.

[52]Author calculations using the 2011 Annual Report of the Florida Local Government Retirement Systems.

[53]This measure of political competition is constructed using the closeness of all state-wide races. I am grateful to Jim Snyder for sharing this dataset, which is an update of the data used in Ansolabehere and Snyder (2002).

[54]All of these additional results are available on request.

presented support this hypothesis and indicate that an increase in political competition is associated with a decline in the funding level of pension plans. The results are robust to controlling for municipal and decade fixed effects, suggesting that unobserved time-invariant heterogeneity across municipalities or aggregate time trends are not driving the results. In addition, politically competitive municipalities are also likely to offer more generous retirement benefits and to strategically use higher interest rates for discounting these future liabilities, arguably to make them appear smaller than they are in reality. Each of these findings is robust to instrumenting for the level of political competition using demographic characteristics of the population as instruments.

Given the magnitude of unfunded liabilities and their trajectory, reforming their pension plans has become a matter of first-order importance for policy makers in state and local governments. Reforms are currently on the table in state and local governments across the country of various political proclivities to reduce their liabilities, acknowledging that the costs they face for these benefits exceed what they are willing or able to pay. An understanding of the complex issues around public-sector pensions, towards which this paper has taken a step, can contribute to the development of such reforms and constrain the ability of politicians to pass on the costs of current labor services to future taxpayers. Moving from defined benefit plans that are susceptible to political influence to defined contribution plans (or cash balance plans) that the paper finds as less susceptible to such influence may be one step in that direction.

Beyond its policy implications, the paper contributes to the broader field of political economy by demonstrating that political competition systematically alters the behavior of politicians when in office and induces them to make decisions that are sub-optimal for society in the long run. Although this idea was developed in the specific context of public-sector pension plans, the notion that political competition promotes behavior oriented towards the short-term, at the cost of issues that are less salient to voters, may be of much broader relevance than simply the context examined here. Researchers should explore the role of political competition in other settings and see if introducing it in their models can help them make better sense of their phenomena of interest.

Table 2.1: Summary Statistics

Variable	Units	Mean	Median	Standard deviation	Minimum	Maximum
Pension plan characteristics						
Actuarial funded ratio	In percent terms	133.46	103.05	103.57	26.63	522.79
Unfunded liabilities per active member	In dollars	(11,049)	(1,004)	42,160	(126,390)	68,054
Average annual pension	In dollars	15,362	13,885	9,378	1,672	36,943
Interest rates used for discounting actuarial liabilities	In percent terms	6.98	7.00	0.77	5.50	8.00
Plan-level controls						
Employees covered by collective bargaining	In percent terms	35.27	35.29	32.17	0	100
Coverage in Social Security	0 = No, 1 = Yes	0.742	1	0.437	0	1
Controls at the municipal level						
Households that are owner-occupied	In percent terms	72.19	73.80	13.40	9.16	98.36
Population aged 75 or older	In percent terms	6.98	6.58	3.14	0.18	27.18
Unemployment rate	In percent terms	5.80	5.10	3.26	0.00	38.90
Taxes spent on debt servicing	In percent terms	11.97	4.66	21.54	0	360.78
Pension costs paid by state as aid	In percent terms	63.76	62.95	19.88	17.63	100
Political variables						
Average Democratic vote share	As a fraction	0.476	0.460	0.126	0.108	0.901
Political Competition	As defined in BPS (2010)	-0.106	-0.097	0.072	-0.401	-0.000
Political Competition	Defined as the standard deviation of Democratic vote share	0.087	0.085	0.026	0.026	0.211

Summary statistics for the first two dependent variables, actuarial funded ratio and unfunded liabilities per active member are based on biennial data from 1985–2009. Those for the next two dependent variables, average annual pension and interest rates used for discounting actuarial liabilities are based on biennial data from 2003–2009. These four variables were available from the Pennsylvania PERC. Data on Social Security coverage summarized are for the year 2009 and are also from the Pennsylvania PERC. The percentage of employees organized under collective bargaining is for 1982 from the Employment Summary Statistics of Census of Governments. Percentage of households that are owner-occupied, percentage of the population aged 75 or older, and unemployment rate are for years 1980, 1990, and 2000 and are from the Decennial Censuses. Taxes spent on debt servicing is based on annual data from 1985–2009 from the Pennsylvania DCED. Pension costs paid by the state of Pennsylvania in the form of state aid is based on annual data for 1985 and for years 1990–2009 from the Office of the Auditor General and has been instrumented as described in the text. All of these variables have been winsorized at the 2.5% and 97.5% levels. Lastly, the political variables, average Democratic vote share and measures of political competition are based on all elections to national and state-level offices held in even-numbered years between 1980–2009 and are constructed using successive issues of the Pennsylvania Manual.

Table 2.2: Effect of Political Competition on Actuarial Funded Ratio and Unfunded Liabilities per Active Member using the Absolute Difference of the Democratic Vote Share from 50% as the Measure of Political Competition

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Actuarial Funded Ratio			Dependent variable: Unfunded liabilities per active member		
Political Competition	-108.1***	-100.0***	-98.90***	37.04***	32.84**	32.75**
	(36.95)	(34.73)	(34.42)	(13.38)	(12.65)	(12.86)
Average vote share	105.6***	100.1**	103.2**	18.67	26.81*	25.49
of Democrats	(38.97)	(39.06)	(40.12)	(12.69)	(14.56)	(15.57)
Employee-group dummies:						
Plan for non-uniformed	-33.19***	-33.03***	-33.01***	2.996	2.983	2.984
personnel	(5.564)	(5.562)	(5.559)	(3.551)	(3.562)	(3.562)
Plan for policemen	34.94***	35.02***	35.00***	-26.05***	-26.05***	-26.05***
	(7.322)	(7.294)	(7.297)	(5.283)	(5.283)	(5.281)
Municipality-level controls:						
Percentage of households		-0.884	-0.873		0.295	0.296
that are owner-occupied		(0.751)	(0.750)		(0.207)	(0.208)
Percentage of the population		1.018	0.988		0.552	0.556
aged 75 or older		(1.240)	(1.260)		(0.526)	(0.523)
Unemployment rate		1.544**	1.539**		-0.445	-0.444
		(0.767)	(0.765)		(0.321)	(0.322)
Fraction of tax revenues			14.74			0.444
spent on debt service			(10.97)			(4.816)
Percentage of pension			0.0846			-0.0265
costs paid by the state			(0.270)			(0.0849)
Constant	91.58***	144.0***	136.9**	-3.279	-28.80*	-26.89
	(17.94)	(53.41)	(63.98)	(8.652)	(16.44)	(19.84)
Observations	5130	5130	5130	5130	5130	5130
R²	0.21	0.21	0.21	0.19	0.19	0.19

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 1985–2009. The dependent variable in columns (1) through (3), the actuarial funded ratio, is defined as the percent of pension liabilities funded. The dependent variable in columns (4) through (6), the unfunded liabilities per active member is defined as (Actuarial Liabilities - Actuarial Assets)/ Number of active members in the plan. It has been rescaled by dividing by \$1,000. The measure of political competition used is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. The fiscal controls introduced in columns (3) and (6) are the fraction of tax revenues spent on debt service, defined as the ratio of debt service to all taxes collected by the municipality and percentage of pension costs paid by the state. This percentage has been instrumented by the weighted average percentage of pension costs paid by the state in all other municipalities within that same county in that decade. The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. Municipality and decade fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: Effect of Political Competition on Actuarial Funded Ratio and Unfunded Liabilities per Active Member using the Standard Deviation of Democratic Vote Share as the Measure of Political Competition

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Actuarial Funded Ratio			Dependent variable: Unfunded liabilities per active member		
Political Competition	-375.6***	-368.3***	-368.3**	126.2***	121.8***	121.2***
	(130.3)	(131.7)	(139.0)	(35.28)	(35.71)	(36.51)
Average vote share	152.4***	147.4***	149.1***	3.066	11.13	10.44
of Democrats	(43.72)	(47.59)	(45.30)	(13.13)	(16.27)	(16.52)
Employee-group dummies:						
Plan for non-uniformed	-32.98***	-32.87***	-32.85***	2.924	2.930	2.930
personnel	(5.589)	(5.589)	(5.586)	(3.532)	(3.547)	(3.547)
Plan for policemen	35.10***	35.14***	35.12***	-26.11***	-26.09***	-26.09***
	(7.352)	(7.318)	(7.320)	(5.277)	(5.277)	(5.276)
Municipality-level controls:						
Percentage of households		-1.119	-1.103		0.372*	0.372*
that are owner-occupied		(0.752)	(0.750)		(0.213)	(0.213)
Percentage of the population		0.926	0.903		0.582	0.585
aged 75 or older		(1.282)	(1.301)		(0.534)	(0.533)
Unemployment rate		1.440*	1.437*		-0.411	-0.410
		(0.778)	(0.774)		(0.320)	(0.320)
Fraction of tax revenues			16.71			-0.207
spent on debt service			(11.59)			(4.851)
Percentage of pension			0.0537			-0.0163
costs paid by the state			(0.253)			(0.0813)
Constant	111.3***	180.2***	174.9***	-10.03	-40.69**	-39.46*
	(16.75)	(54.43)	(64.45)	(7.043)	(16.65)	(19.95)
Observations	5130	5130	5130	5130	5130	5130
R^2	0.21	0.22	0.22	0.19	0.20	0.20

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 1985–2009. The dependent variable in columns (1) through (3), the actuarial funded ratio, is defined as the percent of pension liabilities funded. The dependent variable in columns (4) through (6), the unfunded liabilities per active member is defined as (Actuarial Liabilities - Actuarial Assets)/ Number of active members in the plan. It has been rescaled by dividing by $1,000. The measure of political competition used is the standard deviation of Democratic vote share. The fiscal controls introduced are the fraction of tax revenues spent on debt service, defined as the ratio of debt service to all taxes collected by the municipality and percentage of pension costs paid by the state. This percentage has been instrumented by the weighted average percentage of pension costs paid by the state in all other municipalities within that same county in that decade. The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. Municipality and decade fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: Effect of Political Competition on Actuarial Funded Ratio and Unfunded Liabilities per Active Member (IV estimates)

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Actuarial Funded Ratio			Dependent variable: Unfunded liabilities per active member		
Panel A: Measure of political competition: Absolute difference of Democratic vote share from 50%						
Political Competition	-226.0***	-197.1***	-204.7***	119.8***	106.4***	106.3***
	(76.51)	(73.34)	(73.22)	(27.47)	(23.94)	(23.48)
Average vote share	63.53	62.05	70.71	44.32	58.38**	58.47*
of Democrats	(46.71)	(46.21)	(51.00)	(27.91)	(28.54)	(30.17)
Observations	5043	5043	5043	4999	4999	4999
R^2	0.21	0.21	0.21	0.18	0.18	0.18
First-stage F-stat of excluded	44.55 &	43.00 &	44.56 &	44.40 &	43.51 &	44.65 &
instruments	18.93	22.82	20.25	17.65	21.60	19.10
Hansen-J statistic	5.610	4.623	4.718	7.868	7.855	7.872
Associated p-value	0.2302	0.3282	0.3175	0.0965	0.0970	0.0964
Panel B: Measure of political competition: Standard deviation of Democratic vote share						
Political Competition	-1176.8***	-1180.7***	-1204.7***	520.2***	458.9***	457.0***
	(258.3)	(272.3)	(279.6)	(114.6)	(96.95)	(95.44)
Average vote share	358.6***	358.4***	360.8***	-25.63	-11.41	-10.15
of Democrats	(61.60)	(66.73)	(65.56)	(21.37)	(23.07)	(25.61)
Observations	5043	5043	5043	4999	4999	4999
R^2	0.19	0.19	0.19	0.16	0.17	0.17
First-stage F-stat of excluded	10.85 &	13.79 &	13.81 &	11.38 &	14.53 &	14.08 &
instruments	18.93	22.82	20.25	17.65	21.60	19.10
Hansen-J statistic	1.422	1.514	1.399	5.423	4.927	5.311
Associated p-value	0.8403	0.8242	0.8444	0.2466	0.2948	0.2568
Employee-group dummies	✔	✔	✔	✔	✔	✔
Municipal demographic controls		✔	✔		✔	✔
Municipal fiscal controls			✔			✔

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 1985–2009. The dependent variable in columns (1) through (3), the actuarial funded ratio is defined as the percent of pension liabilities funded. The dependent variable in columns (4) through (6), the unfunded liabilities per active member is defined as (Actuarial Liabilities - Actuarial Assets)/ Number of active members in the plan. It has been rescaled by dividing by \$1,000. The measure of political competition used in panel A is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. The measure of political competition in Panel B is the standard deviation of Democratic vote share. Municipal demographic controls included are the percentage of households that are owner-occupied, the percentage of population aged 75 or older, and the local unemployment rate. Municipal fiscal controls included are the fraction of tax revenues spent on debt service and percentage of pension costs paid by the state, instrumented by the weighted average percentage of pension costs paid by the state in all other municipalities within that same county in that decade. The IVs included are: the percentage of the population (i) of German ancestry, (ii) of Irish ancestry, (iii) of Italian ancestry, (iv) percentage of households headed by blacks and the product of (v) percentage ancestry German and percentage ancestry Irish and (vi) percentage ancestry German and percentage ancestry Italian. The first number for the first-stage F-stat corresponds to the first-stage for the level of political competition (variously defined) and the second number corresponds to the first-stage for average Democratic vote share. The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. Municipality and decade fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.5: Robustness Checks for the Effect of Political Competition on Actuarial Funded Ratio and Unfunded Liabilities per Active Employee

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Robustness check for the actuarial funded ratio						
	OLS			IV		
Base Specification	-108.1***	-100.0***	-98.90***	-226.0***	-197.1***	-204.7***
	(36.95)	(34.73)	(34.42)	(76.51)	(73.34)	(73.22)
RC1: Long differences	-108.7***	-94.85**	-90.80**	-161.7*	-120.9	-122.4
	(40.38)	(36.61)	(35.38)	(95.80)	(95.70)	(95.50)
RC2: Weighting regression by	-159.1***	-126.0***	-124.0***	-306.5***	-236.3***	-201.5***
number of active members	(42.07)	(41.63)	(42.74)	(37.16)	(38.86)	(42.81)
RC3: Not controlling for average	-87.65**	-80.61**	-80.21**	-283.0***	-238.6***	-246.3***
Democratic vote share	(35.67)	(34.01)	(33.39)	(45.29)	(45.51)	(47.17)
RC4: Using average vote share	-98.41***	-93.02***	-93.45***	-184.3***	-164.2***	-167.2***
based on Presidential elections	(25.16)	(24.51)	(24.18)	(57.55)	(56.28)	(57.80)
RC5: Including average vote share for Democrats and average vote share squared						
(i) Coefficient on the linear term	-393.1**	-380.1**	-375.9**	-830.1***	-714.9**	-737.6**
	(187.0)	(180.9)	(187.8)	(313.5)	(300.9)	(305.6)
(ii) Coefficient on the squared	493.0***	477.3**	476.6**	868.4***	756.0***	783.2***
term	(184.8)	(180.0)	(180.6)	(281.1)	(269.9)	(268.8)
Panel B: Robustness check for the unfunded liabilities per active member						
Base Specification	37.04***	32.84**	32.75**	119.8***	106.4***	106.3***
	(13.38)	(12.65)	(12.86)	(27.47)	(23.94)	(23.48)
RC1: Long differences	46.32***	39.91***	39.82***	110.0***	99.89***	100.4***
	(14.57)	(13.49)	(13.90)	(30.92)	(26.77)	(25.06)
RC2: Weighting regression by	93.06*	69.14	76.65*	115.1*	90.22	123.3**
number of active members	(51.44)	(49.77)	(45.75)	(67.15)	(74.16)	(54.05)
RC3: Not controlling for average	39.77***	37.01***	36.44***	115.9***	108.8***	109.2***
Democratic vote share	(12.85)	(12.28)	(12.70)	(27.68)	(24.79)	(24.33)
RC4: Using average vote share	25.19**	22.05**	22.24**	94.08***	84.00***	85.17***
based on Presidential elections	(10.28)	(9.742)	(9.866)	(21.35)	(18.76)	(17.96)
RC5: Including average vote share for Democrats and average vote share squared						
(i) Coefficient on the linear term	190.8***	184.5***	183.1***	471.4***	434.7***	435.0***
	(55.56)	(53.43)	(55.29)	(111.3)	(89.30)	(86.51)
(ii) Coefficient on the squared	-170.0***	-156.2***	-156.2***	-411.6***	-365.9***	-365.3***
term	(59.64)	(57.44)	(58.28)	(96.52)	(79.06)	(77.20)
Employee group dummies	✔	✔	✔	✔	✔	✔
Municipal demographic controls		✔	✔		✔	✔
Municipal fiscal controls			✔			✔

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 1985–2009. The dependent variable in Panel A, the actuarial funded ratio is defined as the percent of pension liabilities funded. The dependent variable in Panel B, the unfunded liabilities per active member is defined as (Actuarial Liabilities - Actuarial Assets)/ Number of active members in the plan. It has been rescaled by dividing by \$1,000. The measure of political competition used is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. For complete notes regarding municipal demographic and fiscal controls included, along with the list of IVs, please refer to notes following Table 2.4. The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. Municipality and decade fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: Effect of Political Competition on the Average Pension Benefit Received by Retirees

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: Annual pension per retiree				Dependent variable: Log of annual pension per retiree			
Political Competition	8.289***	6.778**	6.282**	6.325**	0.513***	0.474**	0.423**	0.424**
	(2.645)	(2.625)	(2.539)	(2.551)	(0.182)	(0.186)	(0.180)	(0.181)
Average vote share of Democrats	3.787	-1.659	0.195	0.159	0.777***	0.482***	0.281	0.283
	(3.170)	(2.952)	(2.565)	(2.566)	(0.187)	(0.168)	(0.190)	(0.192)
Log of wages					1.116***	0.966***	0.934***	0.932***
					(0.0971)	(0.102)	(0.105)	(0.106)
Employee-group dummies:								
Plan for non-uniformed personnel	-11.46***	-10.16***	-9.565***	-9.546***	-0.566***	-0.554***	-0.496***	-0.497***
	(0.755)	(0.835)	(0.832)	(0.833)	(0.0475)	(0.0507)	(0.0493)	(0.0497)
Plan for policemen	0.972	1.854***	2.541***	2.572***	-0.0339	0.0174	0.0951**	0.0952**
	(0.698)	(0.664)	(0.669)	(0.666)	(0.0400)	(0.0409)	(0.0433)	(0.0433)
Plan-specific controls:								
Fraction of employees organized under collective bargaining		4.911***	4.190***	4.138***		0.268***	0.201***	0.199***
		(0.554)	(0.674)	(0.682)		(0.0419)	(0.0448)	(0.0448)
Coverage in Social Security (0 = No, 1 = Yes)		-0.761	-0.760	-0.754		-0.0252	-0.00752	-0.00735
		(0.510)	(0.543)	(0.544)		(0.0321)	(0.0361)	(0.0362)
Municipal-level controls								
Percentage of households that are owner-occupied			-0.0338	-0.0322			-0.00477*	-0.00471*
			(0.0342)	(0.0337)			(0.00243)	(0.00243)
Percentage of the population aged 75 or older			-0.0101	-0.00755			0.00589	0.00586
			(0.0692)	(0.0679)			(0.00649)	(0.00647)
Unemployment rate			-0.191**	-0.190**			-0.00714	-0.00722
			(0.0774)	(0.0772)			(0.00595)	(0.00590)
Fraction of tax revenues spent on debt service				0.903*				0.0408
				(0.486)				(0.0460)
Percentage of pension costs paid by the state				0.0212				-0.000
				(0.0160)				(0.0012)
Constant	17.68***	18.04***	21.40***	19.58***	-2.602**	-0.979	-0.196	-0.151
	(2.092)	(1.934)	(2.477)	(2.967)	(1.027)	(1.067)	(1.161)	(1.182)
Observations	5157	5157	5157	5157	5157	5157	5157	5157
R^2	0.52	0.55	0.55	0.56	0.52	0.53	0.53	0.53

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 2003–2009. The dependent variable in columns (1)–(4) is the absolute level of average benefit received by all retirees (rescaled by $1,000 and including Deferred Retirement Option Plan (DROP) beneficiaries but excluding disability, surviving spousal, and surviving child beneficiaries) while the dependent variable in columns (5)–(8) is the log of annual average benefit. Controls for class of municipality are included in all columns but (1) and (5). Fraction of tax revenues spent on debt service is defined as the ratio of debt service to all taxes collected by the municipality and the percentage of pension costs paid by the state has been instrumented by the weighted average percentage of pension costs paid by the state in all other municipalities within that same county in that year. The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. County and year fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.7: Effect of Political Competition on the Average Pension Benefit Received by Retirees (IV Estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: Annual pension per retiree				Dependent variable: Log of annual pension per retiree			
Political Competition	18.46**	13.07	12.12*	11.85*	0.793	0.686	0.786	0.759
	(8.954)	(8.173)	(7.283)	(7.201)	(0.626)	(0.607)	(0.589)	(0.581)
Average vote share of Democrats	13.17**	4.068	4.661	4.533	1.058**	0.652	0.466	0.451
	(6.529)	(6.224)	(6.927)	(6.850)	(0.448)	(0.477)	(0.554)	(0.552)
Log of wages					1.105***	0.962***	0.903***	0.904***
					(0.0987)	(0.0956)	(0.0998)	(0.101)
Employee-group dummies:								
Plan for non-uniformed personnel	-10.64***	-9.666***	-9.312***	-9.291***	-0.541***	-0.536***	-0.500***	-0.500***
	(0.782)	(0.743)	(0.764)	(0.764)	(0.0452)	(0.0413)	(0.0421)	(0.0425)
Plan for policemen	1.598**	2.214***	2.558***	2.589***	-0.0200	0.0214	0.0874**	0.0867**
	(0.660)	(0.578)	(0.612)	(0.610)	(0.0392)	(0.0350)	(0.0359)	(0.0359)
Plan-specific controls:								
Fraction of employees organized under collective bargaining		4.570***	3.933***	3.910***		0.257***	0.199***	0.198***
		(0.595)	(0.528)	(0.537)		(0.0593)	(0.0505)	(0.0506)
Coverage in Social Security (0 = No, 1 = Yes)		-0.753	-0.943**	-0.928*		-0.0282	-0.0187	-0.0183
		(0.515)	(0.473)	(0.475)		(0.0332)	(0.0315)	(0.0317)
Municipal-level controls								
Percentage of households that are owner-occupied			-0.0483	-0.0469			-0.00542**	-0.00540**
			(0.0319)	(0.0317)			(0.00257)	(0.00258)
Percentage of the population aged 75 or older			-0.00556	-0.00565			0.00421	0.00406
			(0.0577)	(0.0575)			(0.00599)	(0.00597)
Unemployment rate			-0.187**	-0.188**			-0.0100*	-0.0102*
			(0.0934)	(0.0938)			(0.00576)	(0.00579)
Fraction of tax revenues spent on debt service				0.887*				0.0394
				(0.459)				(0.0457)
Percentage of pension costs paid by the state				0.0180				-0.000537
				(0.0142)				(0.00111)
Observations	5157	5157	5157	5157	5157	5157	5157	5157
R^2	0.46	0.49	0.51	0.51	0.47	0.48	0.49	0.49
First-stage F-statistic of excluded instruments	197.48 &	180.10 &	71.44 &	71.01 &	181.00 &	164.91 &	68.49 &	68.45 &
	59.29	77.29	66.51	70.17	56.51	73.45	60.01	63.55
Hansen-J statistic	3.115	2.040	1.369	1.470	2.246	1.836	1.381	1.460
Associated p-value	0.5388	0.7284	0.8495	0.8320	0.6907	0.7659	0.8475	0.8337

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 2003–2009. The dependent variable in columns (1)–(4) is the absolute level of average benefit received by all retirees (rescaled by \$1,000 and including Deferred Retirement Option Plan (DROP) beneficiaries but excluding disability, surviving spousal, and surviving child beneficiaries) while the dependent variable in columns (5)–(8) is the log of annual average benefit. Controls for class of municipality are included in all columns but (1) and (5). The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. For the definition of control variables and a list of the IVs used, please refer to notes following Table 2.4. County and year fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.8: Robustness Checks for the Effect of Political Competition on Annual Pension Received by Retirees

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			OLS				IV	
Dependent variable: Annual pension received per retiree (in $1,000 dollars)								
Base Specification	8.289***	6.778***	6.282**	6.325**	18.46**	13.07	12.12*	11.85*
	(2.645)	(2.625)	(2.539)	(2.551)	(8.954)	(8.173)	(7.283)	(7.201)
RC1: Using data for 2009 only	10.76***	7.901***	8.956***	9.136***	17.83**	12.72*	8.875	7.863
	(2.635)	(2.904)	(2.750)	(2.879)	(8.037)	(7.593)	(9.381)	(9.764)
RC2: Weighting regression by	8.213***	6.594**	7.252*	7.085*	17.32*	10.36	9.731	9.782
number of retirees	(2.986)	(3.019)	(3.731)	(3.565)	(9.236)	(7.640)	(7.234)	(6.833)
RC3: Not controlling for average	6.162	7.664**	6.206**	6.264**	2.276	8.074***	7.801***	7.656***
Democratic vote share	(3.695)	(3.566)	(2.872)	(2.878)	(3.131)	(2.289)	(2.566)	(2.567)
RC4: Using average vote share	5.549***	3.942**	4.001**	4.031**	15.10*	9.797	9.982	9.778
based on Presidential elections	(1.849)	(1.897)	(1.953)	(1.968)	(7.886)	(7.153)	(6.555)	(6.482)
RC5: Including average vote share for Democrats and average vote share squared								
(i) Coefficient on the linear term	33.36***	21.78**	23.34***	22.92***	62.69**	38.37	41.01	40.23
	(7.497)	(8.212)	(8.479)	(8.522)	(31.78)	(28.34)	(28.28)	(28.17)
(ii) Coefficient on the squared	-29.19***	-23.18***	-22.71***	-22.37***	-50.13**	-35.00	-35.91*	-35.22*
term	(6.924)	(7.599)	(7.638)	(7.648)	(25.51)	(22.57)	(21.34)	(21.26)
Employee group dummies	✓	✓	✓	✓	✓	✓	✓	✓
Plan-specific controls		✓	✓	✓		✓	✓	✓
Municipal demographic controls			✓	✓			✓	✓
Municipal fiscal controls				✓				✓

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 2003–2009. The dependent variable is the annual average benefit received by all retirees (including Deferred Retirement Option Plan (DROP) beneficiaries but excluding disability, surviving spousal, and surviving child beneficiaries). It has been rescaled by dividing by $1,000. The measure of political competition used is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. Plan-specific controls included are the fraction of employees organized under collective bargaining and coverage under Social Security. Municipal demographic controls included are the percentage of households that are owner-occupied, percentage of the population aged 75 or older, and local unemployment rate. Municipal fiscal controls included are the fraction of tax revenues spent on debt service and percentage of pension costs paid by the state. For complete notes regarding municipal fiscal controls included, along with the list of IVs, please refer to notes following Table 2.4. County and year fixed effects are included in all specifications, except in RC1 where the estimation involves only one year of data. The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.9: Effect of Political Competition on the Interest Rate chosen to Discount Actuarial Liabilities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Interest rate used by plan sponsors for discounting actuarial liabilities (in percent)								
	OLS				IV			
Political Competition	0.875***	0.753***	0.664***	0.665***	3.276***	2.768***	2.401***	2.358***
	(0.248)	(0.241)	(0.247)	(0.248)	(0.846)	(0.827)	(0.806)	(0.811)
Average vote share of Democrats	1.772***	1.145***	0.816***	0.813***	3.708***	2.926***	2.973***	2.946***
	(0.292)	(0.260)	(0.301)	(0.301)	(0.612)	(0.618)	(0.720)	(0.721)
Employee-group dummies:								
Plan for non-uniformed personnel	-0.456***	-0.298***	-0.183***	-0.183***	-0.271***	-0.182***	-0.0911	-0.0914
	(0.0745)	(0.0702)	(0.0642)	(0.0638)	(0.0768)	(0.0693)	(0.0574)	(0.0573)
Plan for policemen	-0.163**	-0.0493	0.0754	0.0753	-0.0152	0.0447	0.151***	0.151***
	(0.0762)	(0.0706)	(0.0624)	(0.0621)	(0.0730)	(0.0657)	(0.0521)	(0.0521)
Plan-specific controls:								
Fraction of employees organized under collective bargaining		0.549***	0.421***	0.416***		0.383***	0.306***	0.304***
		(0.0691)	(0.0724)	(0.0732)		(0.0683)	(0.0642)	(0.0662)
Coverage in Social Security (0 = No, 1 = Yes)		-0.128***	-0.0817**	-0.0807**		-0.0999**	-0.0821**	-0.0800**
		(0.0437)	(0.0395)	(0.0394)		(0.0431)	(0.0384)	(0.0382)
Municipal-level controls:								
Percentage of households that are owner-occupied			-0.00440	-0.00427			0.0000787	0.000134
			(0.00284)	(0.00286)			(0.00330)	(0.00337)
Percentage of the population aged 75 or older			0.00326	0.00328			0.00171	0.00206
			(0.00711)	(0.00706)			(0.00656)	(0.00644)
Unemployment rate			-0.0257***	-0.0257***			-0.0365***	-0.0360***
			(0.00649)	(0.00658)			(0.00868)	(0.00883)
Fraction of tax revenues spent on debt service				0.0869				0.0993
				(0.0707)				(0.0652)
Percentage of pension costs paid by the state				0.00186				0.00131
				(0.00143)				(0.00173)
Constant	6.573***	6.678***	7.260***	7.102***				
	(0.162)	(0.156)	(0.329)	(0.328)				
Observations	6882	6882	6882	6882	6882	6882	6882	6882
R^2	0.27	0.31	0.33	0.33	0.035	0.11	0.14	0.14
First-stage F-stat of excluded instruments					164.14 &	160.01 &	68.41 &	68.94 &
					55.37	61.10	45.62	47.38
Hansen J-statistic					3.005	2.638	4.023	4.287
Associated p-value					0.5569	0.6201	0.4029	0.3685

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 2003–2009. The dependent variable is the interest rate used for discounting long-term actuarial liabilities. Controls for class of municipality are included in all columns but (1) and (5). The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. For the definition of control variables and a list of the IVs used, please refer to notes following Table 2.4. County and year fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.10: Robustness Checks for the Effect of Political Competition on Interest Rate chosen to Discount Actuarial Liabilities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: Interest rate assumed for discounting actuarial liabilities							
	OLS				IV			
Base Specification	0.875***	0.753***	0.664***	0.665***	3.283***	2.693***	2.662***	2.653***
	(0.248)	(0.241)	(0.247)	(0.248)	(0.583)	(0.524)	(0.517)	(0.518)
RC1: Using data for 2009 only	0.992***	0.806**	0.750**	0.751*	2.663***	2.295***	3.225**	3.252**
	(0.355)	(0.348)	(0.371)	(0.380)	(0.808)	(0.795)	(1.281)	(1.300)
RC2: Weighting regression by number of active members	0.135	-0.00260	-0.0388	-0.0469	3.545**	2.675*	2.141*	2.037
	(0.696)	(0.652)	(0.516)	(0.500)	(1.542)	(1.463)	(1.267)	(1.274)
RC3: Not controlling for average Democratic vote share	0.0375	0.241	0.396*	0.399*	-1.055**	-0.511	-0.00331	-0.00966
	(0.319)	(0.244)	(0.233)	(0.235)	(0.507)	(0.398)	(0.457)	(0.470)
RC4: Using average vote share based on Presidential elections	0.948***	0.820***	0.710**	0.713**	2.872***	2.392***	2.055***	2.004***
	(0.270)	(0.254)	(0.269)	(0.270)	(0.694)	(0.704)	(0.679)	(0.683)
RC5: Including average vote share for Democrats and average vote share squared								
(i) Coefficient on the linear term	5.293***	4.110***	3.405***	3.367***	13.76***	11.12***	9.698***	9.536***
	(1.223)	(1.187)	(1.243)	(1.257)	(3.431)	(3.329)	(3.201)	(3.203)
(ii) Coefficient on the squared term	-3.455***	-2.911***	-2.540**	-2.508**	-9.820***	-8.020***	-6.596***	-6.470**
	(0.963)	(0.944)	(0.999)	(1.016)	(2.785)	(2.676)	(2.536)	(2.542)
Employee group dummies	✓	✓	✓	✓	✓	✓	✓	✓
Plan-specific controls		✓	✓	✓		✓	✓	✓
Municipal demographic controls			✓	✓			✓	✓
Municipal fiscal controls				✓				✓

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 2003–2009. The dependent variable is the interest rate used for discounting long-term actuarial liabilities. The measure of political competition used is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. Plan-specific controls included are the fraction of employees organized under collective bargaining and coverage under Social Security. Municipal demographic controls included are the percentage of households that are owner-occupied, percentage of the population aged 75 or older, and local unemployment rate. Municipal fiscal controls included are the fraction of tax revenues spent on debt service and percentage of pension costs paid by the state, instrumented by the weighted average percentage of pension costs paid by the state in all other municipalities within that same county in that year. The IVs included are: the percentage of the population (i) of German ancestry, (ii) of Irish ancestry, (iii) of Italian ancestry, (iv) percentage of households headed by blacks and the product of (v) percentage ancestry German and percentage ancestry Irish and (vi) percentage ancestry German and percentage ancestry Italian. The dependent variables and all control variables have been winsorized at the 2.5% and 97.5% levels. County and year fixed effects are included in all specifications, except in RC1 where the estimation involves only one year of data (2009). Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.11: Effect of Political Competition on Actuarial Funded Ratio, Normalized to Common Interest Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS			IV		
Panel A: Measure of political competition: Absolute difference of Democratic vote share from 50%						
Panel A1: Normalizing all liabilities using a 7% interest rate						
Duration of liabilities assumed = 13 yrs	-101.0**	-92.00**	-91.04**	-253.4***	-212.2**	-215.9**
	(39.12)	(36.63)	(36.84)	(94.25)	(88.39)	(88.27)
Duration of liabilities assumed = 15 yrs	-100.3***	-91.39**	-90.42**	-253.3***	-212.5**	-216.0**
	(39.03)	(36.56)	(36.78)	(94.86)	(88.89)	(88.78)
Panel A2: Normalizing all liabilities using a 3.5% interest rate						
Duration of liabilities assumed = 13 yrs	-65.53**	-59.71**	-59.08***	-164.5***	-137.7**	-140.1**
	(25.39)	(23.77)	(23.91)	(61.17)	(57.36)	(57.29)
Duration of liabilities assumed = 15 yrs	-60.87**	-55.49**	-54.91**	-153.8***	-129.0**	-131.1**
	(23.70)	(22.20)	(22.33)	(57.60)	(53.97)	(53.91)
Panel B: Measure of political competition: Standard deviation of Democratic vote share						
Panel B1: Normalizing all liabilities using a 7% interest rate						
Duration of liabilities assumed = 13 yrs	-394.2***	-388.3***	-385.6***	-1346.9***	-1340.0***	-1364.3***
	(130.4)	(132.8)	(140.3)	(321.6)	(339.8)	(344.6)
Duration of liabilities assumed = 15 yrs	-393.4***	-387.5***	-384.8***	-1352.8***	-1346.5***	-1371.0***
	(130.6)	(133.0)	(140.5)	(325.5)	(343.5)	(348.3)
Panel B2: Normalizing all liabilities using a 3.5% interest rate						
Duration of liabilities assumed = 13 yrs	-255.8***	-252.0***	-250.3***	-874.1***	-869.7***	-885.4***
	(84.60)	(86.17)	(91.04)	(208.7)	(220.5)	(223.6)
Duration of liabilities assumed = 15 yrs	-238.9***	-235.3***	-233.7***	-821.5***	-817.6***	-832.5***
	(79.31)	(80.78)	(85.32)	(197.6)	(208.6)	(211.5)
Employee group dummies	✓	✓	✓	✓	✓	✓
Municipal demographic controls		✓	✓		✓	✓
Municipal fiscal controls			✓			✓

Regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 1985–2009. The dependent variable, the actuarial funded ratio is defined as the percent of pension liabilities funded. The measure of political competition used in Panel A is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. The measure of political competition used in Panel B is the standard deviation of Democratic vote share. The 7% discount rate used in Panels A1 and B1 corresponds to the median interest rate across all plans. The 3.5% discount rate used in Panels A2 and B2 corresponds to the nominal yield on long-term Treasury bonds of 3.5% (as of September 2013). Municipal demographic controls included are the percentage of the population aged 75 or older, and percentage of households that are owner-occupied, percentage of the population aged 75 or older, and local unemployment rate. Municipal fiscal controls included are the fraction of tax revenues spent on debt service and percentage of pension costs paid by the state. For complete notes regarding municipal fiscal controls included, along with the list of IVs, please refer to notes following Table 2.4. The dependent variable and all control variables have been winsorized at the 2.5% and 97.5% levels. Municipality and decade fixed effects are included in all specifications. Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.12: Effect of Political Competition on Employer Contribution Rate of Defined Contribution (DC) plans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			OLS				IV	
Dependent variable: Employer Contribution Rate to the DC plan (As a percentage of payroll)								
Base Specification	1.018	0.987	1.302	1.272	4.733	4.718	6.846	6.749
	(2.592)	(2.593)	(2.518)	(2.506)	(9.449)	(9.572)	(10.49)	(10.47)
RC1: Using data for 2009 only	-1.470	-1.437	-1.177	-1.133	3.500	3.769	6.191	6.478
	(3.438)	(3.489)	(3.633)	(3.606)	(9.194)	(9.289)	(10.47)	(10.57)
RC2: Weighting regression by number of active members	3.762	3.735	3.346	3.344	6.816	10.78	14.06	14.28
	(2.624)	(2.683)	(2.802)	(2.812)	(10.47)	(9.679)	(10.66)	(10.66)
RC3: Not controlling for average Democratic vote share	1.227	1.178	1.436	1.403	1.131	-0.0798	0.548	0.659
	(2.580)	(2.569)	(2.580)	(2.564)	(6.348)	(6.358)	(6.448)	(6.459)
RC4: Using average vote share based on Presidential elections	-0.938	-0.978	-0.184	-0.245	3.147	3.113	5.076	4.973
	(3.076)	(3.065)	(3.056)	(3.057)	(7.476)	(7.537)	(8.398)	(8.398)
RC5: Including average vote share for Democrats and average vote share squared								
(i) Coefficient on the linear term	-2.236	-2.260	-2.518	-2.611	18.15	18.03	29.65	29.01
	(9.654)	(9.785)	(9.551)	(9.527)	(27.76)	(28.13)	(32.29)	(32.47)
(ii) Coefficient on the squared term	-1.372	-1.264	-2.295	-2.183	-16.02	-15.23	-24.67	-24.15
	(9.409)	(9.391)	(9.187)	(9.162)	(24.98)	(25.21)	(27.95)	(28.08)
Employee group dummies	✓	✓	✓	✓	✓	✓	✓	✓
Plan-specific controls		✓	✓	✓		✓	✓	✓
Municipal demographic controls			✓	✓			✓	✓
Municipal fiscal controls				✓				✓

Regressions estimated on all municipal defined contribution pension plans from Pennsylvania for the period 2003–2009. The dependent variable is the employer contribution rate to the defined contribution plan, expressed as a percentage of payroll. The measure of political competition used is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. Plan-specific controls included are the fraction of employees organized under collective bargaining and coverage under Social Security. Municipal demographic controls included are the percentage of households that are owner-occupied, percentage of the population aged 75 or older, and local unemployment rate. Municipal fiscal controls included are the fraction of tax revenues spent on debt service and percentage of pension costs paid by the state. For complete notes regarding municipal fiscal controls included, along with the list of IVs, please refer to notes following Table 2.4. The dependent variable and all control variables have been winsorized at the 2.5% and 97.5% levels. County and year fixed effects are included in all specifications, except in RC1 where the estimation involves only one year of data (2009). Robust standard errors are clustered at the county level and are in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 2.13: Summary of all Results

	Mean of dependent variable	Units	Direction of change	1 standard deviation increase	
				OLS estimates	IV estimates
Panel A: Effect of political competition using data for the period 1985–2009					
Panel A1: Defining political competition as the absolute deviation of Democratic vote share from 50%					
Actuarial funded ratio	133.46	Percent	Decrease	7.1–7.8	14.1–16.2
Unfunded liabilities per active member	(11,038)	Dollars	Increase	2,349–2,656	7,623–8,592
Panel A2: Defining political competition as the standard deviation of Democratic vote share					
Actuarial funded ratio	133.46	Percent	Decrease	9.6–9.8	30.8–31.5
Unfunded liabilities per active member	(11,038)	Dollars	Increase	3,172–3,302	11,959–13,612
Panel B: Effect of political competition using data for the period 2003–2009					
Defining political competition as the absolute deviation of Democratic vote share from 50%					
Annual pension benefit per retiree	15,362	Dollars	Increase	468–618	884–1,376
		Percent	Increase	3.0–4.0	5.1–5.9
Interest rate for discounting actuarial liabilities	698	Basis points	Increase	4.9–6.4	17.4–24.1
Employer contribution rate of defined contribution plans	5.69	Percent	No effect	–	–

Results in Panel A are based on regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 1985–2009. Actuarial funded ratio is defined as the percent of pension liabilities funded. Unfunded liabilities per active member is defined as (Actuarial Liabilities - Actuarial Assets)/ Number of active members in the plan. The measure of political competition used in Panel A1 is that defined by BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$. The measure of political competition used in Panel A2 is the standard deviation of Democratic vote share.

Results involving annual pension benefit per retiree and interest rate for discounting actuarial liabilities in the first three rows of Panel B are based on regressions estimated on all municipal defined benefit pension plans from Pennsylvania for the period 2003–2009. Annual pension benefit per retiree is is the average annual benefit received by all retirees (including Deferred Retirement Option Plan (DROP) beneficiaries but excluding disability, surviving spousal, and surviving child beneficiaries). Results in the last row of Panel B involving employer contribution rate of defined contribution plans are based on regressions estimated for all municipal defined contribution pension plans from Pennsylvania for the period 2003–2009. The dependent variable is defined as the employer's contribution to the plan, expressed as a percentage of payroll. The measure of political competition used in Panel B is that defined in BPS (2010), viz. $PC_{md} = -|0.5 - D_{md}|$.

Figure 2.1: Change in the Median Funded Ratio of Defined Benefit Pension Plans between the First (1980s) and Third (2000s) Decade, Split by Terciles in the Change of Political Competition
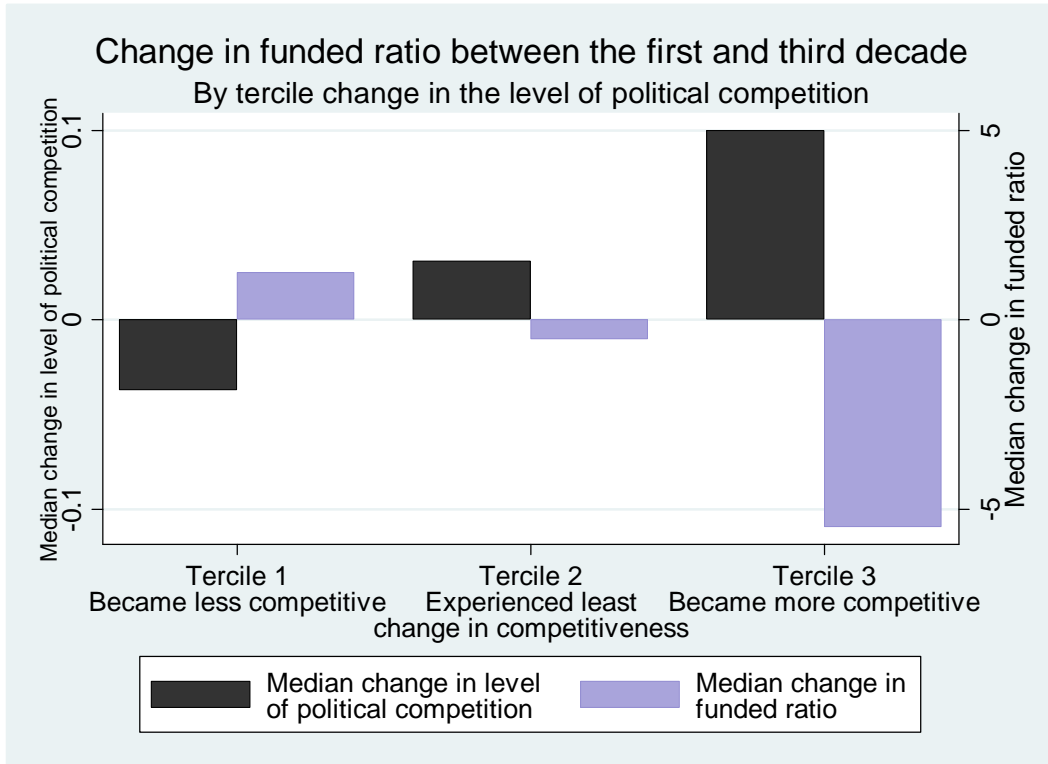
Data on actuarial funded ratio of municipal defined benefit pension plans are available from biennial reports of the Pennsylvania Public Employee Retirement Commission (PERC). This variable has been winsorized at the 2.5 and 97.5% levels. Data on Democratic vote share has been constructed using results of all national and state-level elections held in Pennsylvania during the period 1980–1989 for the first (1980s) decade and 2000–2009 for the last (2000s) decade. Election results are available from successive issues of the Pennsylvania Manual. Political competition is defined as the absolute difference of the Democratic vote share from 50% following Besley, Persson, and Sturm (2010). All municipalities are split into terciles based on their level of *change* in political competition between the first (1980s) and third (2000s) decade. The median change in the level of political competition for municipalities in each of these three terciles is plotted in the figure, along with the median change in funded ratio for municipalities in these three terciles.

Figure 2.2: Patterns of correlation between average Democratic vote share and IVs (percentage ancestry German, percentage ancestry Irish, percentage ancestry Italian, and percentage households headed by blacks) for the 2000s decade
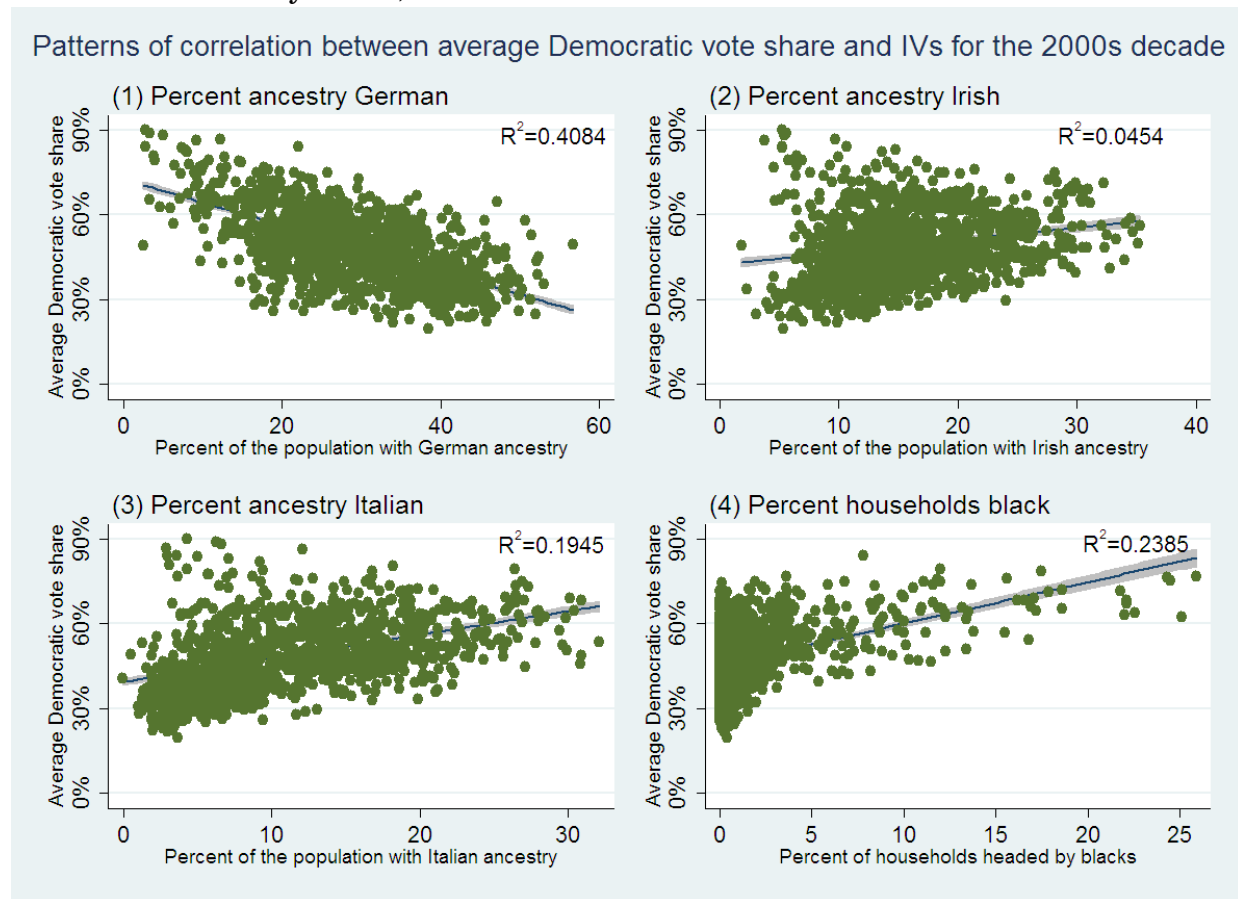
All variables were constructed using the 2000 Census. The dependent variable, average Democratic vote share, was constructed using results of all national and state-level elections held in Pennsylvania in even-numbered years between 2000–2009 and were available from successive issues of the Pennsylvania Manual. For each of the variables: percent ancestry German, percent ancestry Irish, percent ancestry Italian, and percent households headed by blacks, observations more than 3 std. deviations were not included in constructing the graphs. Predicted values along with 95 percent confidence intervals and with the R-squared from a linear fit are included for each plot.

Figure 2.3: Variation in the Median Annual Pension Received by Retirees, Split by Terciles in the Level of Political Competition
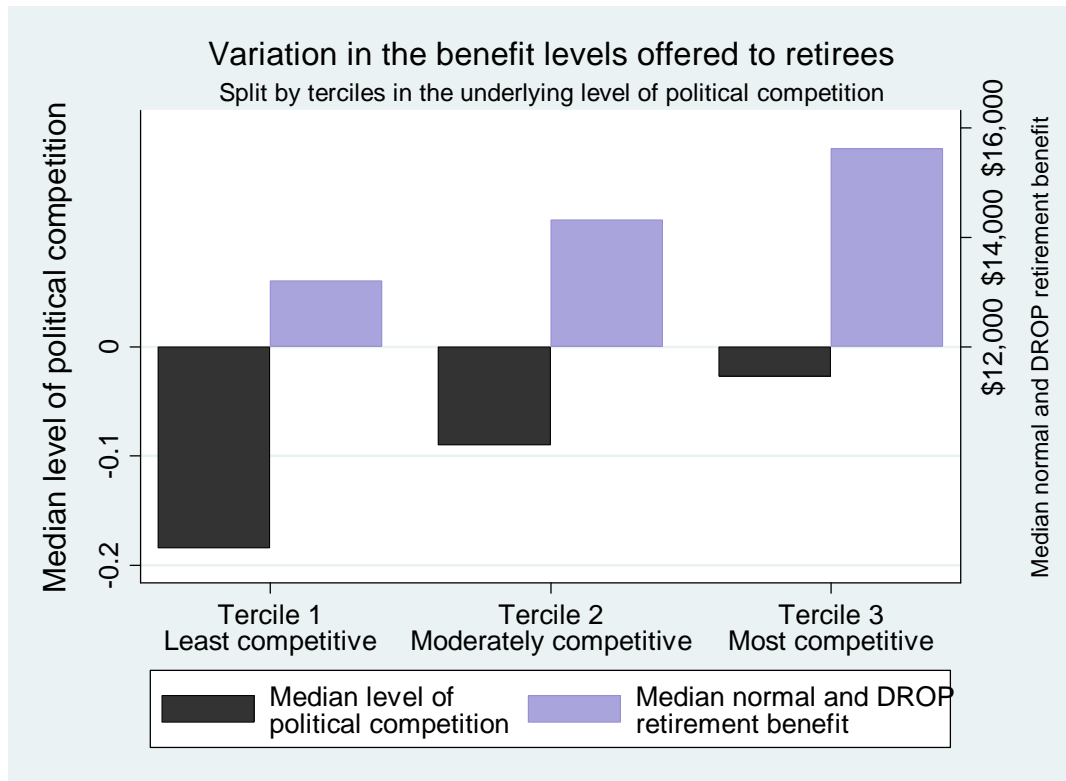
The data on retirement benefits were provided by the Pennsylvania Public Employee Retirement Commission (PERC). The annual retirement benefit used in constructing the graph is a weighted average of the normal and DROP retirement benefits available to retirees (DROP – Deferred Retirement Option Plan). The data has been winsorized at the 2.5 and 97.5% levels. The graph uses data for the year 2009 only. Data on average Democratic vote share has been constructed using results of all national and state-level elections held in Pennsylvania in the prior year, 2008 and were available from Volume 119 of the Pennsylvania Manual. Political competition is defined as the absolute deviation of the Democratic vote share from 50 percent, following Besley, Persson, and Sturm (2010). Municipalities are split into terciles based on their level of political competition in 2008. The median level of political competition for municipalities in each of these three terciles is plotted in the figure, along with the median pension benefit for municipalities in these three terciles.

Figure 2.4: Variation in the Mean Interest Rate used for Discounting Actuarial Liabilities, Split by Terciles in the Level of Political Competition
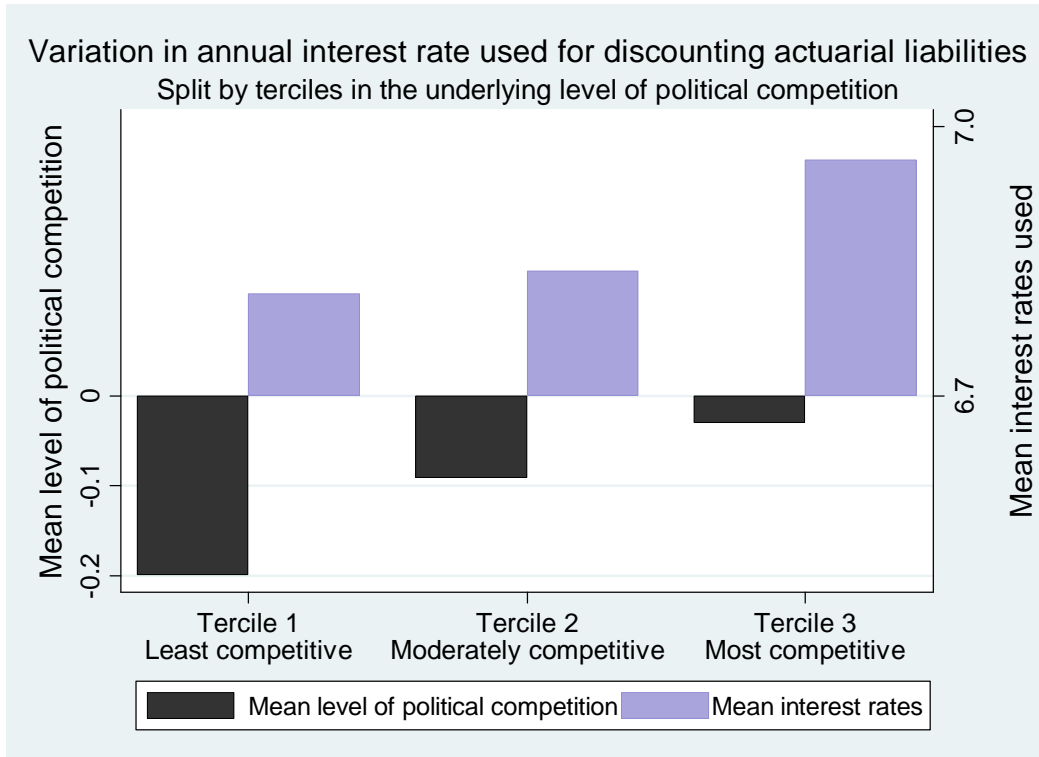
The data on interest rates used for discounting actuarial liabilities were provided by the Pennsylvania Public Employee Retirement Commission (PERC). The data has been winsorized at the 2.5 and 97.5% levels. The graph uses data for the year 2009 only. Data on Democratic vote share has been constructed using results of all national and state-level elections held in Pennsylvania in the prior year, 2008 and were available from Volume 119 of the Pennsylvania Manual. Political competition is defined as the absolute deviation of the Democratic vote share from 50 percent, following Besley, Persson, and Sturm (2010). Municipalities are split into terciles based on their level of political competition in 2008. The mean level of political competition for municipalities in each of these three terciles is plotted in the figure, along with the mean interest rate for municipalities in these three terciles.

Appendix A: Proofs of results stated in the theoretical model

Optimization by public-sector workers

Proof that utility of public-sector workers does not depend on the level of funding for the pension plan chosen in period 1 of the model:

The optimization problem for the representative public-sector worker is:

$$Max_{\{s^G\}}U(c_1^G, c_2^G) = Max_{\{s^G\}}[u(W^G - s^G - T_1) + \frac{1}{(1+r)}u(s^G * (1+r) + B^G - T_2)], \quad (2.13)$$

where T_1 is given by

$$T_1 = N^G * (W^G + a * \frac{B^G}{(1+r)}). \quad (2.14)$$

Public-sector workers know each of the three elements that go into determining the government's revenue requirements: N^G, W^G, and B^G and they also learn of the platforms announced by the parties.[55] Thus, using the above equation, they would be able to correctly infer the level of funding being chosen for the pension plan in period 1. But under the requirement that pension obligations must be honored in full and the constraint that the pension plan must be balanced at the end of period 2, they would also be able to exactly predict the level of taxes that would be imposed in period 2 using the budget balance equation for period 2:

$$T_2 = N^G * (W^G + (1 - a) * B^G + \frac{B^G}{(1+r)}). \quad (2.15)$$

Thus, if $a < 1$ (corresponding to less than full funding of pension benefits earned in period 1 in period 1 itself), then a public-sector worker can correctly anticipate the extent to which taxes would have to go up in period 2 to meet the shortfall in the pension fund. As a result, a public-sector worker can adjust her savings behavior accordingly to account for the increase in the level of taxes in period 2.

With the assumption of a logarithmic utility function, the solution to the optimization problem is:

$$s^G = (W^G - B^G - T_1 + T_2)/(2 + r). \quad (2.16)$$

For a public-sector worker as there is no difference between the utility that she perceives

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[55]They also know the discount rate, r as I assume that $\beta = 1/(1+r)$.

after she has made her savings decision but before time has elapsed and the utility actually realized by her after time has elapsed and tax levels in both periods have been fixed, her utility can be expressed as:

$$U_{per}^G = U_{act}^G = \frac{(2+r)}{(1+r)} * [u(\frac{(1+r)}{(2+r)}W^G + \frac{1}{(2+r)}B^G - \frac{(1+r)}{(2+r)}T_1 - \frac{1}{(2+r)}T_2)]. \qquad (2.17)$$

I note from the budget balance equations (2.14) and (2.15) that:

$$\frac{(1+r)}{(2+r)}T_1 + \frac{1}{(2+r)}T_2 = N^G * (W^G + \frac{B^G}{(1+r)}). \qquad (2.18)$$

As we can see, the overall present value of the tax burden when considered across two periods does *not* depend on a, the level of funding of the pension plan that is chosen in period 1. It simply depends on the size of the public sector workforce, and the net present value of their compensation. Substituting in (2.17), we can see that the well-being (either perceived or actual) of public-sector workers does not depend on the level of funding chosen in the pension plan, a and therefore, public-sector workers do not condition their voting behavior based on a.

Nash Equilibrium of the game

In terms of voting behavior, an individual from group j will vote for party R if:
$\kappa^j U_{per}^j(a^L) + (\sigma^{ij} + \theta) < \kappa^j U_{per}^j(a^R)$.
⇒She will vote for party R iff: $\sigma^{ij} < \kappa^j(U_{per}^j(a^R) - U_{per}^j(a^L)) - \theta$.
I identify the "swing voter" in group j as the individual who, given the parties' platforms, is indifferent between the two parties. I denote these voters' party bias as:

$$\sigma^j \equiv \kappa^j(U_{per}^j(a^R) - U_{per}^j(a^L)) - \theta. \qquad (2.19)$$

Swing voters toss a fair coin when deciding how to vote. In terms of the political equilibrium, we need to consider the first stage of the game when parties are choosing which platforms to announce. Based on the distributional assumptions for σ^{ij}, I can write the vote share for party R as:

$$\pi^R = \sum_j \frac{N^j}{N} m^j[\sigma^j(a^R, a^L, \theta) + \frac{1}{2m^j}].$$

By definition of σ^j in equation (2.19) and the assumption that $\theta \sim U[-\frac{1}{2h}, \frac{1}{2h}]$, p^R is given by:

$$p^R = \frac{1}{2} + h[\sum_j j \frac{N^j}{N} \frac{m^j}{m} \kappa^j (U_{per}^j(a^R) - U_{per}^j(a^L))] \qquad (2.20)$$

where $m \equiv \sum_j \frac{N^j}{N} m^j$ denotes the average density of party bias across groups.

Thus, party R chooses its platform, a^R to maximize V^R which is given by:

$$V^R = p^R(a^R, a^L) * [E + \sum_j N^j (U_{act}^j(a^R) - U_{act}^j(a^L))] + \sum_j N^j U_{act}^j(a^L) \qquad (2.21)$$

This involves setting $\frac{\partial V^R}{\partial a^R} = 0$ which results in:

$$\frac{\partial p^R}{\partial a^R} * [E + \sum_j N^j (U_{act}^j(a^R) - U_{act}^j(a^L))] + p^R * \sum_j N^j U_{act}'^j(a^R) = 0. \qquad (2.22)$$

This is the best response function for party R in response to the choice of funding level, a^L by party L where p^R is given by equation (2.20) and $\frac{\partial p^R}{\partial a^R} = h \sum_j \frac{N^j}{N} \frac{m^j}{m} \kappa^j (U_{per}'^j(a^R))$.[56] Thus, in deciding on an optimal policy, the politician from party R would not only consider the responsiveness of the probability that he wins to the policy chosen, but would also consider the sensitivity of voter well-being to that policy. Unlike candidates who only care about winning elections as in the standard Downsian models, a politician in this case may sacrifice a marginally higher probability of winning if that comes at the cost of a significant reduction in voter well-being. The optimal policy level of a^R involves considering these tradeoffs. I note that the best response function is symmetric for both parties L and R and does not involve any variables which are party-specific.[57] Thus, in Nash equilibrium, the parties set identical policy platforms:

$$a^L = a^R.$$

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[56]Prime is used to denote a derivative.

[57]The problem would not be symmetric if I let the ego rents be different for the two parties or if I let the two parties attach different weights to the ego rents from office vis-a-vis voter well-being. Assuming that these are the same for both parties, enables us to simplify the problem. However, the essential idea that in the presence of two competing forces that push the politicians in different directions on pension plan funding, politicians strive for a balance between them, holds regardless of whether the ego rents are the same for politicians of both parties or whether they attach the same weight to ego rents vis-a-vis voter well-being.

Conditions for existence of an interior solution and corresponding second-order condition

For an interior solution to exist, $\frac{\partial V^R}{\partial a^R} = 0$ for $a^R \epsilon$ $(0, 1)$. This can be guaranteed if $\frac{\partial V^R}{\partial a^R} > 0$ for $a^R = 0$ and $\frac{\partial V^R}{\partial a^R} < 0$ for $a^R = 1$, given that V^R is continuous with respect to the argument, a^R. The first condition requires that:

$$\frac{\partial p^R(0, a^L)}{\partial a^R} * [E + N^P(U^P_{act}(0) - U^P_{act}(a^L))] + p^R(0, a^L) * N^P U'^P_{act}(0) > 0 \qquad (2.23)$$

and the second condition requires that:

$$\frac{\partial p^R(1, a^L)}{\partial a^R} * [E + N^P(U^P_{act}(1) - U^P_{act}(a^L))] + p^R(1, a^L) * N^P U'^P_{act}(1) < 0. \qquad (2.24)$$

Intuitively, if we were to think of the first term as representing the marginal change in utility for the politician from party R resulting from a change in the funded ratio and the second term as representing the marginal change in utility for private-sector workers resulting from such a change of policy, then condition (2.23) requires that the marginal benefit to private-sector workers of increasing the pension plan funding levels at the point of maximum distortion, $a^R = 0$ be large enough to overwhelm the marginal cost to the politician of increasing funding levels at that point. Condition (2.24) requires that the marginal cost to private-sector workers from a decrease in pension plan funding levels at the point of least distortion, $a^R = 1$ be small enough that they are overwhelmed by the marginal benefit to the politician resulting from a decrease in pension plan funding levels at that point.

Finally note that if $\frac{\partial V^R}{\partial a^R} > 0$ for $a^R = 0$ and $\frac{\partial V^R}{\partial a^R} < 0$ for $a^R = 1$, given that V^R is continuous with respect to a^R, $\frac{\partial^2 V^R}{\partial a^{R2}} < 0$ at the point where $\frac{\partial V^R}{\partial a^R} = 0$.[58] Thus, whenever it is the case that $\frac{\partial V^R}{\partial a^R} > 0$ for $a^R = 0$ and $\frac{\partial V^R}{\partial a^R} < 0$ for $a^R = 1$, we are guaranteed an interior solution for a^R and furthermore, that interior solution will correspond to a maximum for V^R. In that case, politicians from both parties would choose to partially fund the pension plan.

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[58]This can also be proven by showing that each of the four terms in the expansion of $\frac{\partial^2 V^R}{\partial a^{R2}}$ is negative.

Proof of Comparative Statics Results

Let

$$F \equiv \frac{\partial p^R}{\partial a^R} * [E + \sum_j N^j (U^j_{act}(a^R) - U^j_{act}(a^L))] + p^R * \sum_j N^j U^{\prime j}_{act}(a^R) \qquad (2.25)$$

Or,

$$F \equiv h * [\sum_j \frac{N^j}{N} \frac{m^j}{m} \kappa^j (U^{\prime j}_{per}(a^R))] * [E + \sum_j N^j (U^j_{act}(a^R) - U^j_{act}(a^L))] +$$

$$(\frac{1}{2} + h[\sum_j \frac{N^j}{N} \frac{m^j}{m} \kappa^j (U^j_{per}(a^R) - U^j_{per}(a^L))]) * \sum_j N^j U^{\prime j}_{act}(a^R) \quad (2.26)$$

Proof of Result 1

Using the implicit function theorem,

$$\frac{\partial a^k}{\partial \kappa^j} = - \frac{\partial F / \partial \kappa^j}{\partial F / \partial a^k} \qquad (2.27)$$

Without loss of generality, consider $\frac{\partial a^R}{\partial \kappa^j}$. First,

$$\partial F / \partial \kappa^j = h * [\frac{N^j}{N} \frac{m^j}{m} (U^{\prime j}_{per}(a^R))] * [E + \sum_j N^j (U^j_{act}(a^R) - U^j_{act}(a^L))] +$$

$$h[\frac{N^j}{N} \frac{m^j}{m} (U^j_{per}(a^R) - U^j_{per}(a^L))] * \sum_j N^j U^{\prime j}_{act}(a^R)$$

At the Nash equilibrium, when $a^R = a^L$, this simplifies to:

$$\partial F / \partial \kappa^j = h * [\frac{N^j}{N} \frac{m^j}{m} (U^{\prime j}_{per}(a^R))] * E \qquad (2.28)$$

Given that $U^{\prime P}_{per}(a^R) < 0$, $\partial F / \partial \kappa^P < 0$. For obtaining $\partial F / \partial a^R$, I use the form of F suggested by (2.25).

$$\partial F / \partial a^R = \partial^2 p^R / \partial a^{R2} * [E + \sum_j N^j (U^j_{act}(a^R) - U^j_{act}(a^L))] + 2 * \partial p^R / \partial a^R * \sum_j N^j U^{\prime j}_{act}(a^R) + p^R * \sum_j N^j U^{\prime\prime j}_{act}(a^R)$$

In equilibrium, $a^L = a^R$ and hence the above expression simplifies to:

$$\partial F / \partial a^R = \partial^2 p^R / \partial a^{R2} * E + 2 * \partial p^R / \partial a^R * \sum_j N^j U_{act}^{'j}(a^R) + p^R * \sum_j N^j U_{act}^{''j}(a^R) \qquad (2.29)$$

Substituting for p^R, $\partial p^R / \partial a^R$, and $\partial^2 p^R / \partial a^{R2}$, I get:

$$\partial F / \partial a^R = h \sum_j \frac{N^j}{N} \frac{m^j}{m} \kappa^j (U_{per}^{''j}(a^R)) * E + 2 * h \sum_j \frac{N^j}{N} \frac{m^j}{m} \kappa^j (U_{per}^{'j}(a^R)) * \sum_j N^j U_{act}^{'j}(a^R) + p^R * \sum_j N^j U_{act}^{''j}(a^R)$$
$$(2.30)$$

Given the concavity of U, both the first and the last terms are negative. In the second term, $U_{per}^{'P}(a^R)$ and $U_{act}^{'P}(a^R)$ are of opposite signs with $U_{per}^{'P}(a^R) < 0$ and $U_{act}^{'P}(a^R) > 0$. Thus, all three terms in the expression for $\partial F / \partial a^R$ are negative. Thus, $\partial F / \partial a^R < 0$ and hence using (2.27), $\partial a^R / \partial \kappa^P < 0$. **QED.**

As $U_{per}^{'G}(a^R) = 0$, using (2.27) and (2.28), $\partial a^R / \partial \kappa^G = 0$. **QED.**

Proof of Result 2

Using the implicit function theorem,

$$\frac{\partial a^k}{\partial m^j} = -\frac{\partial F / \partial m^j}{\partial F / \partial a^k} \qquad (2.31)$$

Without loss of generality, consider $\frac{\partial a^R}{\partial m^j}$. First,

$$\frac{\partial F}{\partial m^j} = [h\kappa^j \frac{\partial U_{per}^j(a^R)}{\partial a^R} * \{E + \sum_j N^j (U_{act}^j(a^R) - U_{act}^j(a^L))\} + h\kappa^j (U_{per}^j(a^R) - U_{per}^j(a^L)) * \sum N^j \frac{\partial U_{act}^j(a^R)}{\partial a^R}] * \frac{\partial}{\partial m^j}(\frac{N^j m^j}{Nm})$$

At equilibrium, $a^R = a^L$ and the above expression simplifies to:

$$\frac{\partial F}{\partial m^j} = h\kappa^j \frac{\partial U_{per}^j(a^R)}{\partial a^R} * E * \frac{(Nm * N^j - N^j m^j * N^j)}{(Nm)^2} = h\kappa^j \frac{\partial U_{per}^j(a^R)}{\partial a^R} * E * \frac{N^P N^G m^{-j}}{(Nm)^2} \qquad (2.32)$$

Given that $\frac{\partial U_{per}^P(a^R)}{\partial a^R} < 0$, $\frac{\partial F}{\partial m^P} < 0$. As before, $\partial F / \partial a^R < 0$. Thus, using (2.31), $\frac{\partial a^R}{\partial m^P} < 0$. **QED.**

As $U_{per}^{'G}(a^R) = 0$, using (2.31) and (2.32), $\partial a^R / \partial m^G = 0$. **QED.**

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Chapter 3

The Political Economy of Tax Enforcement: A Look at the IRS from 1978–2010

3.1   Introduction

The gross tax gap is defined as the amount of true tax liability faced by taxpayers that is not paid on time. The Internal Revenue Service (henceforth, IRS)'s most recent estimates of the gross tax gap for the year 2006 are $450 billion or 16 percent of the true tax liability.[1] One potential avenue for reducing the tax gap is through increased spending on enforcement. However, in the budget agreement reached between the Republican-controlled House and the Democratic-controlled Senate in April 2011, funding on enforcement-related activities by the IRS was reduced by over $700 million relative to the $6 billion requested by the agency for FY 2012. At the time of the bipartisan agreement reached between both chambers of Congress, Speaker John Boehner's office released a statement on the issue: "The Obama administration has sought increased federal funding for the [IRS].... This increased funding is *denied* in the agreement."[2] Episodes like these have caught the attention of pundits. In a post in The New York Times Economix blog, Bruce Bartlett, who held senior policy positions in the Reagan and George H.W. Bush administrations wrote:

> "Unfortunately, Republicans have been treating the I.R.S. like a political punching bag for years, cutting its personnel and restricting its ability to do its job. The number of I.R.S. employees fell to 84,711 in 2010 from 116,673 in 1992 despite an increase in the population of the United States of 53 million over that period" (Bartlett, 2012).

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[1]http://www.irs.gov/newsroom/article/0,,id=252038,00.html, Accessed 06/12/2012

[2]http://www.speaker.gov/Blog/?postid=235069, Accessed 02/23/2012

The action by the Speaker of the House alluded to in the previous paragraph would not be an isolated example of political actors attempting to influence the course of direction at the agency. In 1997 during the presidency of Bill Clinton, the IRS came under scrutiny when a series of conservative nonprofit organizations like the Heritage Foundation and Citizens Against Government Waste were the targets of audits, prompting a bipartisan effort in the Senate to investigate the accusation that a political motivation was behind such audits (Mattos, 1997). More recently, the revelation by IRS exempt organizations division chief, Lois G. Lerner, that IRS employees singled out applications from "Tea Party" groups for 501c(4) tax-exempt status for additional scrutiny, has raised questions about the possibility that the President or Congress may try to alter the priorities of the agency in order to have it serve his (or their) political goals.

In spite of the significant tax gap of $450 billion and the anecdotal evidence presented above that political ideology impacts the operation of the IRS, there has been no systematic examination of the extent to which partisan control influences the resources allocated to the IRS. The lack of empirical literature on the topic motivates this paper. I seek to answer a number of questions: Do party ideologies matter for tax administration? Does Democratic control of the Presidency and Congress induce the allocation of more resources on tax administration and enforcement relative to Republican control? Furthermore, does the allocation of greater resources to the IRS translate to outcomes of interest such as audits and then eventually to revenue collections by the Treasury?

Before seeking to answer these questions, it is worth taking a step back and asking a more fundamental question of why political actors would choose to achieve their policy goals through a change in enforcement resources rather than through an explicit change in the tax code. As Kopczuk (2006) points out, changing the tax code typically requires politically costly tax reform, and "tax avoidance – letting well enough alone" may be all that is possible. Kopczuk's insights suggest that it may be politically infeasible for policy makers to explicitly reduce tax rates even if they would like to reduce the tax burden on their constituents.

This insight echoes the findings of Mahoney and Thelen (2010) who argue that in the face of the high veto probabilities that results in political gridlock, whole-scale change of existing rules (or *displacement*) may not be possible. Instead, such a political environment may be conducive to *conversion* when rules formally remain the same but are interpreted and enacted in new ways. In the context of enforcement of the nation's tax laws, politicians

desiring a lower tax burden for their constituents may achieve those goals not by reducing statutory tax rates, but by reducing enforcement and increasing opportunities for avoidance. Although reduced enforcement leading to greater opportunities for avoidance may not be the most efficient way of reducing the tax burden, that might be all that is possible given political gridlock and intense scrutiny from the media and the public on statutory tax rates.

This is the basic intuition for the current paper: When faced with political gridlock and scrutiny on statutory tax rates, Presidents and/ or Congress may attempt to affect the level of tax collection by varying the budget of the IRS, the administrative agency in charge of tax collection and enforcement, and by altering how its resources are spread across various activities. In particular, if we assume that Republicans want to reduce effective tax rates for individuals and corporations (either because of innate preferences or because that represents the views of their constituents) and if we accept the premise that it is more difficult to change the tax code (e.g. the statutory rates) than to reduce the likelihood of audits, then we arrive at a simple conclusion: Republican Presidents and Republican Congresses may try to reduce effective tax rates by reducing the likelihood of audits – potentially through starving the IRS of resources.

The paper examines whether the political affiliation of the party controlling the White House, the Senate, and the House affects the size of the overall IRS budget and workforce and its allocation among various activities. It finds that over the period 1978–2010 although there is a modest effect of the party affiliation of the President on the overall IRS budget and workforce, party affiliation of the President makes a difference to the share of those resources that are allocated towards enforcement. Enforcement-related activity, in particular, the number of criminal investigators and revenue officers is significantly higher during a Democratic Presidency. In contrast to the modest influence of the President on budgets, it appears that Democratic control of the chambers of Congress matters for the overall level of resources available to the IRS.

Beyond the analysis of IRS resources and personnel, in an examination of audit activity, the paper also finds that over the period 1978–2010, the likelihood of corporate audits are lower on average under Republican administrations than under Democratic administrations. The higher intensity of audit activity extends beyond corporate income tax returns to returns of individual income tax, estate tax, and trusts (fiduciaries). The results in this paper, taken as a whole, suggest that although the President has a modest influence on overall budgetary

levels and resources, he does have a more pronounced influence on the allocation of these resources to enforcement-related activity. In so demonstrating, this paper contributes to a literature in political science that has focused on gradual change and has suggested that changes to public policy need not result from major exogenous events alone, but can also arise from small shifts that are often not easily discernible (see for example, Howell, 1992; Mahoney and Thelen, 2010; Baumgartner, 2013; and Rocco and Thurston, 2014). Where it goes beyond the existing literature is in taking a quantitative lens to the study of American tax policy and in making the case that oscillating enforcement strategies between administrations can contribute to differences in tax policy even absent any formal changes to the tax code.

The paper is laid out in the following sections. A brief literature review section offers a preview of the papers which document the effect of politics on fiscal policymaking and the responsiveness of bureaucratic agency behavior to political actors. The next section uses data from the Policy Agendas Project to show that changes to the tax code are on the agenda of Republican Presidents and offers anecdotal evidence that the President can actually exert control over the operations of the IRS. The following section describes the data and the empirical specifications used for testing the hypotheses of partisan influence. Having presented the data and the empirical specifications, I turn to the results on IRS budgets, personnel resources, and the likelihood of an audit for various types of returns. I conclude in the last section and draw out implications for public policy more generally.

3.2 Literature Review

This paper relates to a broad literature that documents the effects of politics on decision-making in the economic realm. For example, Poterba (1994) finds that in the late 1980s when regional economic downturns and increased expenditure demands led to substantial state budget deficits, political factors played an important role in the adjustment process. Deficit adjustment was much faster in states where the state house and governorship were controlled by the same party than when control was divided (see also Alt and Lowry (1994)). Furthermore, tax increases and spending cuts were both significantly smaller in gubernatorial election years than at other times in the face of substantial state budget deficits. In addition to these political factors, state fiscal institutions also appeared to have real effects on the speed and nature of fiscal adjustment to unexpected deficits (Poterba, 1994). Beyond

noting the role of fiscal institutions and divided government in affecting fiscal policymaking, Reed (2006) finds partisan differences on taxes at the state level using data from 1960–2000. Tax burdens are higher when Democrats control the state legislature as compared to when Republicans are in control with the political party of the governor having little effect after controlling for partisan influences in the state legislature.

There is also a large body of work that examines the responsiveness of bureaucratic agency behavior to political actors at different levels of the government and to various economic and social conditions. The literature mainly finds that bureaucratic agencies are responsive to the preferences of Presidents (Olson, 1995; Scholz and Wood, 1998; Wood, 1990; Wood and Waterman, 1991, 1993) and / or Congress (Olson, 1995, 1996; Weingast and Moran, 1983; Wood, 1992).

Focusing more narrowly on the issue of tax administration, few papers have looked at the question of whether political ideology matters for enforcement of the nation's tax laws by the IRS. In an early examination of the issue, Scholz and Wood (1998) look at the ratio of corporate to individual audits at the state level over the period 1974–1992 and find that the odds of corporate versus individual audits change with different presidential administrations and increase with increased Democratic control over Congress.

Another paper that has examined the effects of political influences on tax administration is Young, Reksulak, and Shughart (2001). The authors use panel data for 33 IRS districts over a 6-year period from 1992–1997 and find evidence that "the fraction of individual tax returns audited is significantly lower in districts that are important to the President electorally and that have representation on key congressional committees" (Young, Reksulak, and Shughart, 2001, p. 201). Although the paper is relevant for our analysis, their conclusions must be tempered by a number of considerations. First, the study looks at a relatively short period from 1992 to 1997 and during five of these six years, President Bill Clinton was in the White House. Thus the evidence of executive branch pressure on the IRS could be unique to his administration. Second, during these six years, one party controlled both chambers. During the first three years, 1992 through 1994, Democrats controlled both the Senate and the House, whereas in the second half, from 1995 through 1997, control of Congress switched to Republican hands. Thus, using this limited period of time from 1992–1997 does not let us identify the effects that stem from controlling only one chamber of Congress independently of the effects of unified congressional control by a single party.

Compared to the literature cited above, the present work offers a number of advantages. First, it analyzes changes in tax administration and enforcement over a significantly longer period of time than has been considered in any previous study. The period spanning fiscal years 1978–2010 analyzed in the paper spans three Democratic (Carter FY 1976–1980, Clinton FY 1993–2000, and Obama FY 2009–2010) and three Republican administrations (Reagan FY 1981–1988, Bush I FY 1989–1992, and Bush II FY 2001–2008). It also encompasses a number of significant political movements; for example, the Reagan revolution of the 1980s, the Republican control of the House after a gap of forty years in 1994 following the "Contract with America", the closely divided electoral landscape of the early 2000s, and the Democratic triumph of the 2006 and 2008 electoral cycles. In addition, barring Scholz and Wood (1998) which looks at the ratio of corporate to individual audits, no previous study has investigated the effect of political ideology on audit rates of corporations. Even though corporate income tax revenue averaged only around 23 percent of personal income tax revenue over the sample period 1978–2010, the amount of revenue obtained through audits of corporate income tax returns was *over half the amount* generated directly through all audits. For example, in FY 2010, of the total recommended additional taxes and penalties of $44.8 billion, $26.2 billion or a full 58 percent came from audits of corporate income tax returns.[3] The dominance of taxes and penalties from audits of corporations holds for the entire sample period and is not unique to 2010. For example for 1978, the starting year of the sample period, recommended taxes and penalties from audits of corporations amounted to 53 percent of the $6.3 billion recommended on the basis of all audits. Thus, if one is to examine enforcement activity at the IRS and see whether political influences are operative on audit activity, audit rates of corporate income tax returns need to be at the front and center of that analysis.

This paper addresses that need by focusing on the variation in the likelihood of audits for corporations of different sizes over time for the period 1978–2010. It also takes a further step and demonstrates that the variation in audit probability for corporations affects the effective tax rate paid by corporations. Notwithstanding the focus on corporate tax returns, this paper analyzes (and finds similar effects) in terms of partisan influence on audits of individual income tax returns, estate tax returns, and returns filed by trusts (fiduciaries).

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[3]In 2010, audits of individual income tax returns yielded only $15.1 billion or 34% in additional recommended taxes, with the balance 8% coming predominantly from audits of estate and trust income tax returns.

3.3   Anecdotal evidence of Presidential influence on tax policy

The Introduction presents the claim that sub rosa form of tax enforcement (or retrenchment) may be used by politicians as an alternative to changing the tax code. Using the terminology of Mahoney and Thelen (2010), actors forced by the gridlocked institutional environment engage in conversion through budgetary and enforcement changes rather than engage in displacement through changes to the tax code. However, this suggests that the actors are making a conscious choice to engage in institutional conversion. In order to convince the reader that this is plausible, I turn to the Policy Agendas Project and offer evidence on the basis of analyzing the same.[4]

The Policy Agendas Project State of the Union Address dataset tabulates information on each quasi-statement in Presidential State of the Union (SOTU) Speeches.[5] Given the time period of analysis in the paper, I focus on all such speeches between 1978–2010 and count the number of quasi-statements made in total as well as those statements which pertain to tax policy.[6] Although the total number of quasi-statements made by Democratic Presidents during this period (6,740) is larger than the total number of quasi-statements made by Republican Presidents (4,970), Republican Presidents made 213 quasi-statements on the topic of tax policy in contrast to Democratic Presidents who made only 154 quasi-statements on this topic suggesting that Republican Presidents emphasize issues of tax policy more compared to Democratic Presidents. The difference is accentuated when expressed in relative terms: approximately 4.3% of all quasi-statements made by Republican Presidents were on the topic of tax policy, whereas only 2.3% of all quasi-statements made by Democratic Presidents were on this topic.

Furthermore, reading each quasi-statement made on the topic of tax policy by Presidents during this period and placing it in the context of the larger SOTU address, I classify quasi-statements into three groupings: those that were in favor of tax cuts, those opposed to tax

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[4]The data used here were originally collected by Frank R. Baumgartner and Bryan D. Jones, with the support of National Science Foundation grant numbers SBR 9320922 and 0111611, and were distributed through the Department of Government at the University of Texas at Austin. Neither NSF nor the original collectors of the data bear any responsibility for the analysis reported here. (Policy Agendas Project)

[5]A quasi-statement simply refers to text between periods and semi-colons. For example, the following text: "During the past year, we have also made a good start in providing housing for low-income groups; we have raised minimum wages; we have gone forward with the development of our natural resources; ..." is divided into three quasi-statements (and thus three rows) in this dataset.

[6]This is operationalized by focusing on all quasi-statements classified under topic 107 (Taxation, Tax Policy, and Tax Reform) and sub-topic 2009 (IRS administration).

cuts, and those that were neutrally disposed with respect to tax cuts. Of the quasi-statements made by Republican Presidents on tax policy, I classify 81.2% of those as being unambiguously in favor of a tax cut whereas only 44.8% of quasi-statements made by Democratic Presidents express a similar sentiment. In contrast, 25.3% of the quasi-statements made by Democratic Presidents argue in favor of tax increases whereas only 1.4% of all quasi-statements made by Republican Presidents call for a tax increase. These data are presented in Table 3.1 with notes following the table offering more details of the classification methodology adopted as well as one example of each type of statement.

[Table 3.1 about here.]

Although evidence of greater Republican interest in tax policy (and specifically in reducing taxes) is a useful first step in supporting the assertions of the paper, it is not sufficient. It would be also helpful to offer evidence that supports the paper's assertion that the President can actually exert control over the operations of the IRS. While a formal empirical analysis is deferred until later, I offer the following anecdotes which suggest that the President does have an influence on the administration of the nation's tax laws. I also note that the Commissioner of the IRS and its general counsel are political appointees who are appointed by the President, confirmed by the Senate, and can be dismissed without cause by the President, unlike Commissioners who serve on independent agencies such as the FCC or the SEC.

1. The Justice Department was due to file by Jan. 11 [1982] its Supreme Court brief in two cases challenging the denial of tax exemptions to schools that discriminate on racial grounds. On Jan. 8 the brief was ready to go to the printer, approved by Justice's tax division, the Solicitor General's office and the Internal Revenue Service. But that day President Reagan reversed the Government's policy on racist schools. The brief was not filed. "Since 1970," the brief said, "the I.R.S. has uniformly" denied tax exemptions to schools that discriminate.... From the beginning of tax exemptions for charity, the brief said, Congress intended to limit exemption to purposes that are "charitable" under the law of trusts - that is, "not illegal or contrary to public policy." . . . And the brief argued forcefully that Congress since 1970 had acquiesced in the ruling against exemptions for racist schools. . . . All this history - recited in much greater detail in the brief than a newspaper column can even suggest - was ignored by President Reagan and his advisers in reversing the policy. The President said he had ordered the I.R.S. to give exemptions to racist schools because there was no legal basis to deny them. (Lewis, 1982)

2. A quarter century ago, President Nixon tried unsuccessfully to force the IRS to "go after our enemies and not go after our friends." Today, the practice is more subtle. Members of Congress or the White House usually attach to their referral a letter from a like-minded constituent or a news article alleging wrongdoing. The Clinton White House once referred a conservative organization that relentlessly pursued the claim that [Vincent] Foster had not committed suicide, as ruled by authorities, but was murdered. Presidential aides also forwarded a complaint faxed

to President Clinton from a supporter in Beverly Hills, California, that the Western Journalism Center was engaged in a "vicious media campaign to hurt you." The fax didn't allege any specific tax violations. It simply noted the center was tax-exempt and "needs investigation." The IRS audited the group, but eventually upheld its tax-exempt status. (Margasak and Solomon, 1999)

3. The Times's David Cay Johnston reported yesterday that on Oct. 10, the I.R.S. commissioner Mark Everson told his troops to delay tax enforcement in areas affected by Hurricane Katrina – until after the midterm elections and the holiday season. Until after the elections? Mr. Everson also said that in his mind the elections were part of a continuum that ran through the holidays. That would make him the only person in the country who envisions Congressional campaigns as the start of Christmas shopping season. Mr. Everson does his agency and law-abiding taxpayers a disservice. In delaying the enforcement actions, there's no avoiding the appearance of a political motivation. Many voters in the devastated areas are bound to be angry at President Bush and, by extension, Republicans. By easing up now, the I.R.S. avoids stoking that anger. The possibility that Mr. Everson is wielding power in ways to please his boss, President Bush, is especially disturbing given that he has courted that suspicion before. **After the administration failed repeatedly this year to achieve its goal of repealing the estate tax, the I.R.S. moved to eliminate the jobs of nearly half of the agency's lawyers who audit estate tax returns.** (Emphasis added) Mr. Everson's explanation that the employees were no longer needed was unconvincing because the agency would not release enough data for researchers to independently verify his claim. Mr. Everson needs to admit his mistakes, rather than trying to say they were not mistakes at all. And to make the I.R.S. more transparent. And to stay out of politics. (New York Times Editorial Board, 2006)

3.4   Empirical Methodology and Data

Having offered anecdotal evidence suggesting that questions of tax policy are on the minds of Republican Presidents and that the President can and does have an influence on the operations of the IRS, I now turn to describing the data and laying out the empirical methodology used in the paper that attempts to document the nature of partisan influence. As I use data on IRS budgets to examine the nature of partisan influence on tax administration and enforcement, it is worth pointing out that budgets can make a difference in the ability of the IRS to enforce the nation's tax laws, given an earlier finding that, in the case of the FDA and the FCC, budgets appeared be too blunt a tool for change (Carpenter, 1996). The following anecdotes offer support for the claim that budgets do matter in the case of the IRS.

1. Increasing pursuit of offshore tax havens and the country's wealthiest individuals and corporations is not just smart politics. History suggests it is also good policy. Thus, while the Internal Revenue Service may be unpopular with the public at large, the maxim "you need to spend money to make money" has added significance in the area of tax enforcement. (Temkin, 2010)

2. U.S. tax collections may decline by $4 billion annually if the U.S. Congress cuts the budget of the tax-collecting Internal Revenue Service, according to a letter from the IRS commissioner on Monday. In a letter to congressional tax-writing committees, IRS Commissioner Douglas Shulman said steep IRS budget cuts would sap revenue collections and hamper the agency's ability to pursue identity theft, offshore tax evasion and other fraud. (Temple-West, 2011)

3. Some folks cheer when the Internal Revenue Service's budget shrinks. ...But before you join the starve-the-IRS-beast cause, keep this in mind: while recent budget cuts may have chipped away at the IRS' collection and enforcement activities, they have also hastened a dramatic decline in taxpayer service. On Wednesday, the IRS released annual tables showing it audited just 0.96% of individual tax returns in fiscal 2013 ended Sept. 30, the lowest since 2005, and that the audit rate for those earning $1 million plus, (a particular focus in recent years), fell from 12.48% in 2011 to 10.85% in 2013. Buried on the last of nine pages of numbers was the change most likely to affect the average law-abiding Jane Taxpayer: just 60.5% of taxpayers who called the IRS' toll-free assistance line got through to a human being last year, down from 74% in 2010 and 87% in 2004. In her 2013 Annual Report to Congress released today, National Taxpayer Advocate Nina E. Olson offered additional indicators of collapsing service and named **IRS budget cuts the second biggest problem facing taxpayers** (emphasis added), both because of substandard service and because, she contends (and studies she has commissioned seem to support), rotten service may lead to more tax noncompliance. (Novack, 2014)

3.4.1 Description of Data

The primary source of data for this paper is the annual IRS Data Books which offer a detailed picture of the IRS' operations. I obtain data on IRS operating costs and number of IRS personnel (Full Time Equivalents, or FTEs) from the data books. These books also provide data on the personnel resources devoted by the IRS to enforcement-related activity. However, the IRS does not break down these overall numbers down to the level of individuals vis-a-vis corporations and hence the numbers pertain to resources geared towards all entities served by the IRS: Individuals, corporations, estates, trusts, and others.

In addition to obtaining data on IRS' budgetary and personnel resources, I also obtain data on the number of tax returns audited and the number of tax returns filed in any given year for (1) corporations, (2) individuals, (3) estates, and (4) trusts (fiduciaries) from these data books. A note on the availability of corporate audit data: Although aggregate data regarding audit rates of corporations is available over a longer period of time, data disaggregated based on the size of the corporation (assets held), is only available for the period from fiscal year 1978 onwards; hence the choice of 1978 as the starting point for all our analysis. Having a breakdown of the likelihood of audit by asset class is useful because an aggregate number of audits does not distinguish between whether those audits are of the largest corporations or of relatively small corporations. The presence of disaggregated data also makes it possible to control for the fact that in any given year the largest corporations are more likely to be audited than corporations of a smaller size.

The number of asset classes in which the IRS reports information changes from year to year. However, it is possible to construct an integrated time series for the percent of returns

audited for corporations in four asset classes for the entire period from 1978–2010: Those with assets less than \$1 million, assets between \$1–10 million, assets between \$10–100 million, and assets in excess of \$100 million. In addition, we have data on all corporations for which the size of their assets is not known.[7]

The political variables included in the analysis pertain to the partisan control of the White House and the two chambers of Congress. Given the fact that budgets are set by the President in conjunction with Congress, I introduce interaction variables between party of the President and control of Congress. Given multicollinearity, I am left with two dummy variables as a result of the introduction of this interaction: the first dummy variable assumes a value of 1 when a Democratic President faces a Congress that is not controlled by Democrats and 0 otherwise. The second dummy variable assumes a value of 1 when a Republican President faces a Congress that is controlled by Democrats. Details regarding the data sources are provided in Appendix A.

One important aspect is the choice of the appropriate lag structure in the specification. Budgets and priorities for a given fiscal year are generally set in the prior fiscal year. Thus audit rates, IRS budgets and personnel, and their allocation to various activities in fiscal year t can only be ascribed to decisions reached in the previous fiscal year (t-1) which are a function of the political environment at that point of time. The first step in setting the federal budget involves the President presenting a budget proposal for the coming Fiscal Year to Congress on or before the first Monday in February. This gets debated in Congress which is expected to complete action on a budget resolution by April 15 (Keith, 2008) followed by the preparation and passage of appropriations bills and other spending and receipts legislation prior to the start of the fiscal year. Disagreements between the two chambers of Congress are resolved in a conference committee comprised of some members from the House and the Senate.[8] Although the April 15 deadline for passage of the budget resolution is frequently not met, it generally remains the case that the decisions regarding priorities for a given fiscal year are taken in the prior fiscal year. Hence, I consider the first lags of all exogenous variables. I provide summary statistics in Table 3.2 below.

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[7]This generally occurs when the corporate tax return does not include a balance sheet. As per current IRS guidelines, corporations with total receipts and total assets at the end of the tax year less than \$250,000 are not required to file a balance sheet with their tax return. (Source: http://www.irs.gov/pub/irs-pdf/i1120.pdf; Accessed 03/28/2014)

[8]A more complete description of the federal budgeting process is offered in a section of Analytical Perspectives, "Budget Concepts and Budget Process," available at http://www.whitehouse.gov/omb/budget/analytical_perspectives (Accessed 04/01/2014).

3.4.2 Empirical Approach

3.4.2.1 Effect of political ideology on IRS resources and allocation to enforcement-related activity

In analyzing the resources available to the IRS, I start off by examining the overall IRS budget for the reasons offered earlier regarding the importance of the budget in conducting the IRS' operation. As the IRS budget has grown over time in nominal terms simply as a result of inflation, it needs to be normalized. I use different alternative approaches for normalization. The first normalization scales it down by a deflator that converts current dollar outlays to constant dollars. The second approach scales the IRS budget by all non-defense related federal outlays as that ratio might better reflect the extent to which different administrations prioritize enforcement of the nation's tax laws. Federal outlays on non-defense items are also susceptible to partisan influence similar to those on the IRS budget whereas the size of the national economy is less likely to be affected by the political orientation of the actors involved. Thus, in the third approach, I scale the IRS budget by GDP as that gives a sense of the resources available to the IRS relative to the size of the national economy.

Given that the analysis spans a 33-year period from 1978–2010, it is appropriate to control for many economic and policy factors that have changed over this window. As a number of these factors are likely to be correlated with the party make-up of the Presidency and Congress, omitting these may result in overstating the identification of the effect of party affiliation on tax administration practices. To guard against that possibility, in all of the analysis that follows, I control for four factors that may independently influence IRS resources (and allocation of resources) in addition to the political party affiliations of the actors involved.

1. Tax rates: One consideration that may affect the allocation of resources within the IRS are tax rates. For an IRS that is simply concerned with collecting higher revenues for the federal government, the marginal gains from auditing tax returns would be higher in the presence of a higher tax rate. In addition, individuals and corporations may also have varying inclinations to under-report income depending on the tax rate they are subjected to.

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2. Size of the federal deficit: The Tax Reform Act of 1986 (TRA '86) under President Reagan involved sizable reductions in the statutory tax rates along with a broadening of the tax base designed such that it would be revenue-neutral. In the aftermath of the passage of TRA'86, the IRS was also provided with additional resources to close and/or detect tax loopholes as "the upshot of a concerted effort to avoid other legislated increases in taxes to meet budget deficits." (Steuerle, 2008, p. 90). Thus, it is possible that a higher federal deficit results in the allocation of more resources on enforcement in order to improve revenue collections by the Treasury.

3. Growth rate of the national economy: Tax collections are highly responsive to the growth of the national economy and enforcement could possibly take a back-seat in a situation where growth in the national economy results in buoyant revenue collections.

4. Changes in information reporting. Changes in the amount and types of information reporting is likely to have independent, apolitical effects on tax administration practices. As information reporting becomes more and more prevalent, the need for explicit audits could reduce as the federal government acquires the ability to automatically generate notices to taxpayers in case of any discrepancies between the information provided on these reports and those in the returns filed by taxpayers. This automatic matching process has become easier for the IRS with advances in information technology and as more and more information returns are received by it in electronic rather than in paper form (IRS Data books).

In order to deal with these potentially important influences on the resources available to the IRS, I introduce the following control variables: the size of the federal deficit (expressed as a percentage of GDP), the growth rate in real GDP, the number of information reports received by the government, and the individual / corporate income tax rate (depending on the analysis at hand). In addition to these control variables, I also introduce a linear time trend to control for any secular changes over time between 1978–2010. Finally, given the fact that the IRS is a cumbersome bureaucracy and resources and administrative practices are "sticky", I introduce a lagged term that accounts for the persistence in resources from one year to the next.[9] The

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[9]In each case, I conduct a Durbin-Watson test to examine whether autocorrelation is present in the data. In general, I find the test statistic is in the range where the Durbin-Watson test is inconclusive. However, given the possibility of positive autocorrelation in the data, I introduce a lagged term in the regressions. Results of the Durbin-Watson test are available on request from the author.

specification used is:

$$IRS\ budget_t = \beta_0 + \beta_1 * Party\ President_{t-1} + \beta_2 * Party\ Senate_{t-1} + \beta_3 * Party\ House_{t-1} +$$

$$\beta_4 * Party\ President_{t-1} * Congress\ with\ Democrats_{t-1} + \beta_5 * IRS\ budget_{t-1} + \beta_6 * X_t + \mu_t + \varepsilon_t$$

$$(3.1)$$

In the above specification, "Party President" is a dummy variable coded 1 when a Democrat is in the White House and 0 otherwise. Likewise, "Party Senate" and "Party House" are also dummy variables, coded 1 when Democrats are in charge of the Senate and the House respectively. For the term, "Party President" interacted with "Congress with Democrats," there are four possible combinations. Two of those combinations are omitted because of multicollinearity. I am left with two dummy variables, of which the first dummy variable assumes the value of 1 when a Democratic President faces a Congress that is not controlled by Democrats and 0 otherwise. The second dummy variable assumes the value of 1 when a Republican President faces a Congress that is controlled by Democrats. μ_t represents a linear time trend and ε_t represents the error term, which includes all influences that are not being captured in the regression specification. It is worth pointing out here that I do not observe all of the 8 configurations possible in the dataset that spans the period from 1978–2010. Instead only 6 of the 8 possible configurations appear in the data. I do not find any instance in which Democrats controlled the Presidency and control of Congress was split between the two parties. Democratic Presidents either faced a Congress that was controlled by Democrats or a Congress that was controlled by Republicans. Below is a frequency distribution of the different configurations that show up in the data along with the years for which they appear.

Presidency	Senate	House	No. of years observed	Years
Democratic	Democratic-controlled	Democratic-controlled	7	1978–1981, 1994–1995, 2010
Democratic	Republican-controlled	Republican-controlled	6	1996–2001
Republican	Democratic-controlled	Democratic-controlled	8	1988–1993, 2008–2009
Republican	Democratic-controlled	Republican-controlled	2	2002–2003
Republican	Republican-controlled	Democratic-controlled	6	1982–1987
Republican	Republican-controlled	Republican-controlled	4	2004–2007
Total			33	

I use specifications similar to (3.1) when I analyze the nature of partisan influence on

(a) the number of all IRS personnel or (b) the number of enforcement personnel dedicated to criminal investigation and revenue collection. In all specifications, I control for the control variables laid out earlier and also introduce a lagged term to account for the persistence in the levels of the dependent variables.

3.4.2.2 Effect of political ideology on likelihood of corporate audits

Using data from the IRS data books, I examine the hypothesis of whether Democratic administrations and Democratic Congresses audit more corporate returns over the period 1978–2010. The specification used is:

$$Log\ number\ of\ returns\ audited_{i,t} = \beta_0 + \beta_1 * Party\ President_{t-1} + \beta_2 * Party\ Senate_{t-1} + \beta_3 * Party\ House_{t-1} +$$

$$\beta_4 * Party\ President_{t-1} * Congress\ with\ Democrats_{t-1} + \beta_5 * Log\ number\ of\ returns\ filed_{i,t-1} + \beta_6 * X_t +$$

$$\alpha_i + \mu_t + \varepsilon_t \quad (3.2)$$

where i indexes asset class. As before, in order to account for changing macroeconomic conditions and tax laws, I include controls on the federal deficit, GDP growth rate, number of information reports received, and statutory tax rate along with linear time trends. I also control for the log of number of returns filed in the prior calendar year in that asset class as the number of returns audited may go up with an increase in the number of returns filed. Finally, I control for the fact that corporations with more assets (e.g. with assets more than $100 million) are audited at rates higher than those for corporations that have fewer assets (e.g. assets less than $1 million) through the use of asset-class specific effects in the regression.[10]

Given the possibility that patterns of audit activity are persistent over time, in an alternate specification, I introduce a lagged term for the probability of an audit for that asset class in the prior fiscal year. In both specifications, I introduce the dependent variable, number of returns audited, in logarithmic form to reduce heteroskedasticity (Wooldridge, 2008, p. 274).

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[10]Audit coverage ratios for a given asset class are a measure of enforcement and they may respond to non-compliance by corporations in that asset class, raising the issue of endogeneity between enforcement activity and non-compliance. I explored the use of a 3 SLS framework in which audit probabilities are influenced by various measures of non-compliance such as the level of recommended taxes and penalties per audit and the percent of audits which did not result in a change. Those regressions indicated that audit rates for a given asset class were *not* impacted by past levels of non-compliance for that asset class as best as I could tell from the measures of non-compliance at hand. Hence the decision to use a more parsimonious specification and proceed with an OLS framework as opposed to a more involved 3SLS framework.

3.4.2.3 Effect of audits on effective tax rate and voluntary compliance

One implication from a model of a corporation trying to maximize its expected post-tax profits is that following an increase in the likelihood of an audit, it should reduce the extent of underreporting of income. As true corporate profits can be assumed exogenous to the tax instruments such as the audit probability, a reduction in the extent of underreporting of corporate income should result in an increase in the net revenue collected by the Treasury. In order to examine this hypothesis, I analyze variations in the effective tax rate paid by corporations. The effective tax rate is defined as the ratio of the net corporate tax collections, as reported by the IRS to a measure of corporate profits from the Bureau of Economic Analysis (BEA).[11] Having constructed a measure of the effective tax rate, I subsequently regress it on the percent of returns audited to examine whether the effective tax rate is responsive to variations in the audit probability. I control for the federal deficit (relative to GDP), growth rate of real GDP, the number of information reports received, and the highest statutory corporate tax rate as each of those could have an independent effect on the effective tax rate.[12]

In addition to examining the effective tax rate, I construct a measure of voluntary compliance, which I define as the ratio of average net revenue collected per return filed to the average recommended taxes and penalties per audit. As the likelihood of an audit goes up, if compliance does improve, the net revenue collected per return should go up and recommended taxes and penalties per audit should come down.[13] Thus, an increase in the likelihood of an audit should result in an increase in voluntary compliance as per this measure.

A question that may arise in this context has to do with the use of contemporaneous audit probability on the right hand side and whether using a lagged measure of audit probability would be a better approach. One can think of this question in a variety of ways. Although it is true that audits are initiated during a given year, the priorities of the IRS are set in

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[11]The best measure to be used in the denominator in the calculation of the effective tax rate would be the income of corporations that is actually subject to income tax per Statistics of Income (SOI), IRS. Such data is available from the IRS for the period 1960-2007 but not beyond 2007. Since I would like a series which runs for the entire period from 1978–2010, I choose to go with the numbers for profits earned by all domestic corporations made available by the BEA. (http://www.bea.gov/iTable/index_nipa.cfm) The correlation coefficient between the two different measures of corporate profits, that from the IRS and from the BEA is 0.9821. Given this high correlation coefficient between the two measures of profits, I use the numbers from BEA because it is available over the entire period of time from 1978–2010.

[12]I include the top statutory marginal tax rate as a proxy for the statutory tax rate faced by corporations since the vast majority of corporate profits in any given year is earned by corporations that are subject to the top marginal corporate tax rate.

[13]Looking at the level of taxes and penalties, I indeed find consistent evidence that taxes and penalties are lower, both in absolute terms and on a per audit basis, when we have a Democratic administration in place.

advance in the prior fiscal year and informed agents such as corporations may very well be aware of what areas the IRS will focus on in the upcoming year. Thus if corporations learn about the fact that audits will be more numerous and stricter in the upcoming year, then they can plan and react to that information well before filing their returns at the end of their fiscal year. In addition, corporations rely on the advice of professionals such as accountants and lawyers who are particularly likely to have an incentive to keep themselves abreast of upcoming changes in priorities for the IRS. They can then share such information with their clients and influence the extent to which corporations are aggressive on their tax planning. As a result, even though an increased intensity of audits may not be visible for sometime, corporations can plan their tax strategies in anticipation of such increased enforcement.

Second, a view of the world in which corporations file returns once at the end of a year and then the IRS decides which of those returns to audit is not well-rooted in reality. In practice, as noted by the then IRS enforcement director Steven Miller, "the agency spends more time communicating with firms before they file tax returns" (McCoy, 2010). In addition, the IRS has pioneered a number of initiatives over the years which have tried to formalize such communication. The following excerpt details one such initiative by the IRS:

> The IRS is testing a program that may represent the future of corporate audits. Since 2005, the IRS has piloted the Compliance Assurance Process (CAP) program in its Large and Mid-Size Business Division. The CAP program is a simultaneous auditing process that aims to significantly reduce, or even eliminate, audit procedures after the corporate tax filing date. ... Currently, the IRS invites individual corporations to participate in the CAP program. If a corporation accepts, it signs a memorandum of understanding (MOU) that establishes the materiality thresholds and methods of communication to be used during the program. ...Once an MOU has been established, the IRS assigns an account coordinator to the corporation who serves thereafter as the corporation's primary IRS contact and oversees the CAP cycle...Throughout the tax year, the corporation and the account coordinator work toward an agreement regarding the tax treatment of material issues such as transfer pricing, foreign earnings repatriation, research and development tax credits, and acquisitions. At the conclusion of the tax year, the account coordinator completes Form 906, Closing Agreement on Final Determination Covering Specific Matters, which details the material issues and their resolved tax treatments. (Cleaveland, Epps, and Bradley, 2010)

Thus, to the extent that the IRS decides to be lenient or aggressive in its dealings, such a stance will be visible to its clients well before they file their end-of-year tax return. Therefore, it ought not to be surprising if the effects of an IRS interested in enforcement are visible within the course of the same year itself, especially when it pertains to compliance with the corporate income tax.

Finally, it turns out that for the period 1978–2010, the audit probability and its lagged measure are correlated with a correlation coefficient of 0.9690 ($p < 0.001$). As a result of this

high degree of correlation, the results obtained by using contemporaneous audit probability are likely to be similar to those obtained using their lagged values.

3.5 Results

3.5.1 Results on IRS budget and personnel

Figures 3.1 through 3.3 and Tables 3.3 through 3.5 provide the results of the analyses in which I examine variations in the IRS budget, its workforce, and the emphasis on enforcement over the period 1978–2010. As was noted before, the IRS data on its overall resources or their allocation to enforcement-related activity is not broken out by individuals and corporations but includes all resources that are geared towards serving all classes of taxpayers. The dependent variables have been rescaled in Tables 3.3 through 3.5 so as to avoid having unnecessary zeros in the estimated coefficients. The economic and statistical significance of the results is unchanged if we avoid rescaling the dependent variable in these tables.

3.5.1.1 Overall IRS resources

Figure 3.1 presents the underlying data while Table 3.3 presents the results of estimating specification (3.1) with the IRS budget as the dependent variable of interest. The IRS budget has been scaled by a deflator in column (1) that converts current dollar outlays to constant dollars, by non-defense outlays in column (2), and by GDP in column (3).

[Figure 3.1 about here.]

[Table 3.3 about here.]

In order to clearly understand the effect that a change in control of the White House, the Senate, and the House has, I list the various configurations possible and calculate the difference that a change in control of each institution makes to the IRS budget. For example, using the coefficients in column (1) of Table 3.3, the following table enables estimation of the difference that a change in control of the White House makes to the IRS budget.[14]

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[14]In all of such tables included in the paper, the values are normalized such that the level corresponding to unified control by Republicans (Republican President, and Republican-controlled Congress) is set to zero. The p-value provided corresponds to the test of whether a change in control of one of the three institutions of power matters, given the orientation of the other two institutions.

IRS Budget with the following orientation of political actors:

Orientation of Congress	Republican President	Democratic President	Delta	Associated p-value
Democratic Senate, Democratic House	32.73	39.77	7.04	0.8022
Democratic Senate, Republican House	24.57	36.57	12.00	0.6498
Republican Senate, Democratic House	14.29	26.28	11.99	0.6498
Republican Senate, Republican House	0	11.99	11.99	0.6498
"Average" effect[15]			10.75	

As the results in the above table suggest, party affiliation of the President does not make a statistically significant impact on the size of the overall IRS budget for any of the four possible configurations of Congress. In contrast, a table similar to the one above focusing on the role played by the Senate suggests that Democratic control of the Senate matters more in terms of raising the IRS budget.

IRS Budget with the following orientation of political actors:

Orientation of Presidency and House	Republican Senate	Democratic Senate	Delta	Associated p-value
Democratic President, Democratic House	26.28	39.77	13.49	0.7109
Democratic President, Republican House	11.99	36.57	24.58	0.0384
Republican President, Democratic House	14.29	32.73	18.44	0.4233
Republican President, Republican House	0	24.57	24.57	0.0384
"Average" effect			20.27	

The above table suggests that when the control of the Senate switches from Republicans to Democrats, it makes a statistically significant difference to the IRS budget in two of the four possible configurations. The average value of IRS budget when deflated (and rescaled) is 831 and hence an increase of 24.57 resulting in the second last row from a change in control of the Senate when Republicans control the Presidency and the House translates to an increase of about three percent in the IRS budget.

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[15]Instead of using a simple average of the four rows, if we were to take a weighted average, then the average effect of a change in control of the Presidency would be an increase in the IRS budget by 9.74 units given that the relative frequencies of the four configurations of Congress corresponding to the four rows of the table above are 15, 2, 6, and 10 respectively (the mean value of the dependent variable after rescaling is 830.66 units). Using the margins command in Stata11, I get the identical answer.

Although the specific numbers vary based on the specification, an analysis similar to the one above using coefficients from columns (2) and (3) of Table 3.3 suggests that in each of those cases, control of the Presidency has a modest influence on size of the IRS budget relative to the control of the chambers of Congress.

A story similar to the one outlined for the IRS budget emerges from considering the size of the IRS workforce. Two approaches of looking at the size of the IRS workforce are adopted: First, I consider the raw number of personnel without any normalization and second, I normalize the number of personnel by the size of the civilian labor force. Figure 3.2 presents the secular variation in the absolute number of FTEs while Table 3.4 analyzes the extent of partisan influence on the absolute number of FTEs and the number of FTEs normalized by the civilian labor force.

[Figure 3.2 about here.]

[Table 3.4 about here.]

In order to understand the effect that a change in control of each institution plays in influencing the number of IRS personnel, I look at all possible configurations and calculate the difference that a change in control of each institution makes to the number of personnel. For example, using the coefficients in column (1) of Table 3.4, the following table enables me to compute the difference that a change in control of the White House makes to the number of IRS personnel.

IRS personnel with the following orientation of political actors:

Orientation of Congress	Republican President	Democratic President	Delta	Associated p-value
Democratic Senate, Democratic House	1,999	3,990	1,991	0.2926
Democratic Senate, Republican House	3,702	2,585	(1,117)	0.6160
Republican Senate, Democratic House	706	(411)	(1,117)	0.6160
Republican Senate, Republican House	0	(1,117)	(1,117)	0.6160
"Average" effect			(340)	

As the results in the above table suggest, party affiliation of the President appears to not make a statistically significant impact on the size of the overall IRS work force for any of the four possible configurations of Congress. In contrast, a table similar to the above focusing on

the role played by a switch in control of the Senate suggests that Democratic control of the Senate matters substantively in terms of raising the number of IRS personnel.

IRS personnel with the following orientation of political actors:

Orientation of Presidency and House	Republican Senate	Democratic Senate	Delta	Associated p-value
Democratic President, Democratic House	(411)	3,990	4,401	0.1903
Democratic President, Republican House	(1,117)	2,585	3,702	0.0625
Republican President, Democratic House	706	1,999	1,293	0.7050
Republican President, Republican House	0	3,702	3,702	0.0625
"Average" effect			3,275	

Based on the above table, it would appear that a change in control of the Senate from Republican to Democratic hands results in an increase in the number of IRS personnel, with the increase being statistically significant in two of the four possible configurations. The average number of IRS personnel over the period 1978–2010 is 99,105 and hence an increase of 3,702 resulting in the second last row from a change in control of the Senate when we have a Republican President and a Republican-controlled House translates to an increase in the size of the workforce by about four percent. The results are very similar using the coefficients in column (2) of Table 3.4 when the number of FTEs is normalized by the size of the civilian labor force and suggests no Presidential influence but a considerable influence of the Senate.

Thus, the picture that emerges from the results in Tables 3.3 and 3.4 and the subsequent analysis is that the party of the President has only a modest influence on the overall resources available to the IRS. Congress, on the other hand, has a more significant role to play in influencing the size of the budget. In particular, there is suggestive evidence that Democratic control of the Senate raises the resources available to the IRS by about three percent, depending on whether we consider the IRS budget or the size of the IRS workforce and on how these variables are normalized.

3.5.1.2 Allocation of IRS' resources toward enforcement

The theoretical framework suggested in the Introduction indicates that the President would choose the least cost instrument available to him to achieve his political goals. If, as our discussions suggest, Republican Presidents would like to minimize the tax burden for indi-

viduals and corporations, then one possible way of achieving that would be to cut the overall IRS budget or workforce. However, the analysis based on the coefficients in Tables 3.3 and 3.4 suggests that Republican Presidents are not able to significantly reduce the funding level of the IRS and conversely, Democratic control of the Presidency appears not to raise the level of resources available to the IRS either.

Although the focus thus far has been on overall budgetary and personnel resources available to the IRS, examining budgets for indications of institutional change can obscure changes in the content of policy administration. A stable budget for the IRS can co-exist with significant shifts in how resources are allocated among various activities. In that case, it may not be very meaningful to look at overall resources but to look at disaggregated data to see if IRS employees are shifted from one function to another to better align with the administration's priorities. Moreover, if administrations hire more IRS employees in order to provide high levels of customer-service and be responsive to the needs of taxpayers, then the total number of FTEs may not be the appropriate measure to look at in any case. In either scenario, there will not be a perceptible change in the total number of employees but in the tasks they are assigned to and the number of returns they can audit. Since my focus is on the extent to which different administrations decide to crack down on tax evasion and avoidance, in the remainder of the analysis in this sub-section, I examine whether a change in administration from Republicans to Democrats increases the resources dedicated to reducing tax evasion and improving revenue collection.

One way of looking at this question is to look at the number of criminal investigators and revenue officers that the IRS hires. Criminal investigators look into two broad categories of cases: tax violations and money laundering violations (Dubin, 2004). The 2000 data book of the IRS also suggests that if we are to focus on resources devoted to ensuring tax compliance, it may be worthwhile to focus on Criminal Investigation:

> "IRS Criminal Investigation's primary resource commitment is to develop and investigate Legal Source tax investigations. Legal Source tax investigations involve legal industries and legal occupations and more specifically, legally earned income, in which the primary motive or purpose is the violation of tax statutes: Title 26 (tax violations) and Title 18 (tax related) of the U.S. Code.... **The prosecution of Legal Source Tax Crimes cases is key to promoting voluntary compliance with the tax laws.** (Emphasis added)"

In addition to criminal investigators, I include revenue officers within the IRS to the category of personnel dedicated to enforcement activity. The role of the Revenue Officer is to

collect taxes that are delinquent and have not been paid to the IRS and to secure tax returns that are overdue from taxpayers.[16]

Therefore, the next figure, Figure 3.3 and Table 3.5 examine partisan influences on the number of IRS employees that are dedicated to enforcement-related activity. As with the earlier analysis regarding IRS FTEs, I adopt a number of alternative normalizations to assure the reader of the robustness of the results. Column (1) simply looks at the raw number of enforcement personnel, column (2) normalizes the number of enforcement personnel by the total number of IRS FTEs, and finally column (3) normalizes the number of enforcement personnel by the size of the civilian labor force.

[Figure 3.3 about here.]

[Table 3.5 about here.]

As before, I look at all possible configurations and calculate the difference that a change in control of each institution makes to the number of enforcement personnel. For example, using the coefficients in column (1) of Table 3.5, I construct the following table which enables me to compute the difference that a change in control of the White House makes to the number of enforcement personnel.

Enforcement personnel with the following orientation of political actors:

Orientation of Congress	Republican President	Democratic President	Delta	Associated p-value
Democratic Senate, Democratic House	(97)	560	657	0.0256
Democratic Senate, Republican House	(416)	(203)	213	0.5909
Republican Senate, Democratic House	164	376	212	0.5909
Republican Senate, Republican House	0	212	212	0.5909
"Average" effect			324	

As results in the above table suggest, party affiliation of the President results in a statistically significant impact on the number of enforcement personnel when Congress is controlled

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[16]A description of this role from the IRS web page, reveals "Revenue Officers conduct face-to-face interviews with taxpayers (and/ or their representatives) at the taxpayer's place of business or residence or, on rare occasions, at the Revenue Officer's office. These interviews may be scheduled or unscheduled (cold calls), depending upon the case. This is done as part of the investigative process of collecting delinquent taxes and securing delinquent tax returns." (Source: http://jobs.irs.gov/midcareer/business-tax.html; Accessed 03/27/2014)

by Democrats. When averaged across all four configurations of Congress, a switch in control of the Presidency from Republicans to Democrats translates to an increase of about 324 enforcement personnel. Given that the average number of enforcement personnel over the period 1978–2010 was 9,390, this represents an increase of about three percent in the number of such personnel. In contrast, a table similar to the above focusing on the role played by a switch in control of the Senate suggests that Democratic control of the Senate does not matter in terms of raising the number of enforcement personnel. The role played by the House in terms of the number of enforcement personnel is similar to the role played by the President: a change in control of the House from Republican-control to Democratic-control is also associated with an increase in the number of such personnel.

The picture that emerges from Figure 3.3 and the above analysis is that administrations of different political dispositions are perhaps able to influence the allocation of personnel to tax enforcement. These results are consistent with the framework outlined in the Introduction which suggests that administrations prefer to choose the least cost instrument available to them to influence policy.

3.5.2 Audits of Corporations

Having examined the resources available to the IRS, I present the results from the analysis of partisan influence on the likelihood of audits for all corporations for the period 1978–2010 using specification (3.2) in Table 3.6. The dependent variable in this analysis is the log of number of returns audited controlling for the log of number of returns filed in the prior calendar year. Column (1) does not introduce any lags for audit probability in the prior fiscal year whereas column (2) introduces a lagged term for the audit probability in the prior fiscal year.

[Table 3.6 about here.]

As before, I look at all possible configurations and calculate the difference that a change in control of each institution makes to the likelihood of an audit. Using the coefficients in column (1) of Table 3.6, the following table helps me compute the difference that a change in control of the White House makes to the number of corporate income returns audited.

Log corporate income returns audited under the following orientation of political actors:

Orientation of Congress	Republican President	Democratic President	Delta	Associated p-value
Democratic Senate, Democratic House	0.102	0.368	0.265	0.0276
Democratic Senate, Republican House	(0.204)	0.133	0.337	0.0364
Republican Senate, Democratic House	(0.036)	0.301	0.337	0.0364
Republican Senate, Republican House	0	0.337	0.337	0.0364
"Average" effect			0.319	

As results in the above table suggest, party affiliation of the President makes a statistically significant impact on the number of returns audited for all four possible configurations of Congress. Expressed in terms of the mean of the dependent variable, the increase of 0.319 when averaged across the four configurations of Congress translates to an increase of about four percent in the number of returns audited. The following table highlights the role played by a change in control of the Senate.

Log corporate income returns audited under the following orientation of political actors:

Orientation of Presidency and House	Republican Senate	Democratic Senate	Delta	Associated p-value
Democratic President, Democratic House	0.301	0.368	0.067	0.6861
Democratic President, Republican House	0.337	0.133	(0.204)	0.1866
Republican President, Democratic House	(0.036)	0.102	0.139	0.4267
Republican President, Republican House	0	(0.204)	(0.204)	0.1866
"Average" effect			(0.05)	

The above table suggests that although the role of the Senate in affecting the number of audits of corporate income tax returns varies based on the orientation of the Presidency and the House, it is not statistically significant for any of the four possible configurations. The final table hones in on the role played by the House in influencing the number of corporate income returns audited.

Log corporate income returns audited under the following orientation of political actors:

Orientation of Presidency and Senate	Republican House	Democratic House	Delta	Associated p-value
Democratic President, Democratic Senate	0.133	0.368	0.235	0.2871
Democratic President, Republican Senate	0.337	0.301	(0.036)	0.8579
Republican President, Democratic Senate	(0.204)	0.102	0.336	0.1115
Republican President, Republican Senate	0	(0.036)	(0.036)	0.8579
"Average" effect			0.117	

This table suggests that the House has, at best, a modest influence on the number of audits of corporate income tax returns. A switch in control of the House from Republican to Democratic hands does not make a statistically significant difference for any of the four possible configurations. This pattern of results is replicated if we consider the coefficients in column (2) in which we introduce a lagged term for the percent of returns audited in the prior fiscal year to account for the persistence in IRS administrative practices.

Overall, the tentative take-away from these tables is that the partisan affiliation of the President matters more in influencing the intensity of corporate audit activity, whereas control of the Senate and the House seems to matter less. The fact that a change in control of the Presidency matters for audits of corporate income tax returns is consistent with the earlier analysis which suggests that such a change also makes a difference to the number of enforcement personnel.

3.5.3 Effect of audits on effective tax rate and voluntary compliance[17]

The evidence presented thus far suggests the possibility of partisan influences on the allocation of IRS resources to enforcement-related activity and on the number of corporate income tax returns audited in any given year. It would be interesting to note whether this variation in enforcement-related resources also manifests itself in the form of a variation in the effective tax rate paid by corporations. This sub-section takes steps in that direction. I analyze the effect of the variation in audit probability on the effective tax rate and a measure of voluntary compliance and present the results in Table 3.7. The measure of voluntary compliance used is

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[17]I note that there exists a broad literature on how audit rates affect voluntary compliance and that these papers face many empirical issues with regards to the (mis)measurement of dependent and explanatory variables and the lack of plausibly exogenous variation in audit rates. These issues are relevant for this paper as well and hence the results of this sub-section should be viewed with caution.

the ratio of average net revenue collected per return filed to the average recommended taxes and penalties per audit. Panel A of Table 3.7 omits the variables representing partisan control of the Presidency and Congress simply focusing on the economic variables, whereas Panel B adds the political variables to the specification as well. Both panels include the control variables introduced in earlier analysis, viz. the federal deficit as a percentage of GDP, the growth rate of real GDP, the number of information reports received, and the top statutory corporate tax rate.

[Table 3.7 about here.]

The results in both panels of Table 3.7 suggest that an increase in audit probability has the intended effect of increasing the effective tax rate and improving voluntary compliance. [18] To provide a sense of magnitude of these effects, the coefficient on percent of returns audited in column (1) of Panel A of 1.493 implies that raising the audit probability from 1.33 percent (25th percentile in the time series data from 1978–2010) to 2.67 percent (75th percentile in the data) increases the effective tax rate, on average, by two percent, which amounts to an eight percent increase in the effective tax rate. Taking this a step further, I note that the average audit probability for all corporations over this period is 2.03 percent under Republican administrations and 3.34 percent under Democratic administrations.[19] This increase of 1.31 percent in the audit probability as we move from a Republican to a Democratic administration translates to an increase in the effective tax rate of just under two percent. Given that corporate profits were $1.418 trillion in 2010, an increase of two percent in the effective tax rate translates to an annual increase in corporate tax collections of about $28 billion. Lastly, comparing the coefficients on percent of returns audited in Panels A and B, we find that the increase in effective tax rate and voluntary compliance largely stems from the increase in audit

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[18]To address concerns that a more appropriate specification ought to include lagged audit probability on the right hand side rather than contemporaneous audit probability, I have re-estimated these regressions with lagged audit probability as the independent variable of interest. In each case by comparing the coefficients obtained with contemporaneous audit probability and lagged audit probability and formally testing for whether the coefficients are of similar magnitude or not, I find that the null hypothesis that the coefficients are the same cannot be rejected at the 5% level of significance. This suggests that it does not make a material difference whether we introduce the contemporaneous audit probability or a lagged measure of the same on the right hand side when considering the effective tax rate or the measure of voluntary compliance as the dependent variable of interest. These additional results are available on request from the author.

[19]This is the average audit probability for all corporations. If I were to average the audit probability across the five asset classes, then I get an average of 15.5 percent as has been reported in the summary statistics in Table 3.2. The difference can be explained by the fact that the first measure considers each individual corporation as an individual observation, whereas the second measure considers each asset class as an individual observation. Given that smaller corporations are far more numerous, the audit probability averaged across all corporations is significantly less than the audit probability averaged across the five asset classes.

probability under Democratic administrations, rather than being influenced by the political identity of the actors in the White House and in Congress.

3.5.4 Audits of Individuals, Estates, and Trusts

The reason for focus on corporate audits in this paper has been two-fold: first, as mentioned earlier, for the entire period from 1978–2010, audits of corporations have yielded over half of the revenue generated from audits of all types of returns and hence to analyze whether political influences influence IRS operations, audit rates of corporate income tax returns need to be at the front and center. Second, other than an earlier contribution by Scholz and Wood (1998) who look at the ratio of corporate to individual audits at the state level over the period 1974–1992, there has been no examination of whether differences in partisan control of Congress makes a difference to the intensity of audit activity for corporations.

Notwithstanding this paper's focus on corporate audits, in the interests of completeness, a brief examination of audit activity for other types of returns is also included. Table 3.8 examines variation in the intensity of audit activity for three different types of returns separately: (1) individual income tax returns; (2) estate tax returns; and (3) fiduciary returns (returns filed by trusts) using a specification very similar to the one adopted earlier.[20] Data regarding the number of returns filed and the number of returns audited is available from the IRS data books for the entire period from 1978–2010.

[Table 3.8 about here.]

As before, I look at the various configurations and calculate the difference that a change in control of each institution makes to the likelihood of an audit. Using the coefficients in column (1) of Table 3.8, the following table helps us estimate the difference that a change in control of the White House makes to the number of individual income tax returns audited.

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[20]Audits of corporate income tax and individual income tax returns together accounted for over 92% of the $44.8 billion collected in recommended taxes and penalties. Once audits of estate tax returns and fiduciary returns are also included, audits of the types of returns analyzed in this paper (corporate income, individual income, estate, and fiduciary returns) account for over 95% of the total amount the IRS collected in recommended taxes and penalties from all audits in FY 2010.

Log individual income returns audited under the following orientation of political actors:

Orientation of Congress	Republican President	Democratic President	Delta	Associated p-value
Democratic Senate, Democratic House	0.169	0.438	0.269	0.0381
Democratic Senate, Republican House	(0.236)	0.200	0.435	0.0715
Republican Senate, Democratic House	(0.015)	0.421	0.435	0.0715
Republican Senate, Republican House	0	0.435	0.435	0.0715
"Average" effect			0.394	

As results in the above table suggest, party affiliation of the President makes a statistically significant impact on the number of returns audited for all four possible configurations of Congress. Given that the mean of the dependent variable is 14.02, the average increase of 0.394 in the number of individual income tax returns audited attributed to the President would correspond to an increase of about three percent in the number of audits. The following table highlights the role played by a change in control of the Senate.

Log individual income returns audited under the following orientation of political actors:

Orientation of Presidency and House	Republican Senate	Democratic Senate	Delta	Associated p-value
Democratic President, Democratic House	0.421	0.438	0.017	0.9327
Democratic President, Republican House	0.435	0.200	(0.236)	0.1781
Republican President, Democratic House	(0.015)	0.169	0.184	0.1010
Republican President, Republican House	0	(0.236)	(0.236)	0.1781
"Average" effect			(0.068)	

It is hard to draw definitive conclusions from the above table regarding the influence of the Senate on the number of audits of individual income tax returns. In general though, a switch in the Senate from Republican to Democratic hands does not appear to make a statistically significant difference for any of the four possible configurations. The final table hones in on the role played by the House in influencing the number of individual income tax returns audited.

Log individual income returns audited under the following orientation of political actors:

Orientation of Presidency and Senate	Republican House	Democratic House	Delta	Associated p-value
Democratic President, Democratic Senate	0.200	0.438	0.238	0.3842
Democratic President, Republican Senate	0.435	0.421	(0.015)	0.9370
Republican President, Democratic Senate	(0.236)	0.169	0.405	0.0021
Republican President, Republican Senate	0	(0.015)	(0.015)	0.9370
"Average" effect			0.153	

This table suggests that the House has a modest influence on the number of audits of corporate income tax returns. A switch in control of the House from Republican to Democratic hands makes a statistically significant difference only for the case that corresponds to Republican control of the Presidency and Democratic control of the Senate. For the remaining three configurations of the Presidency and the Senate, a switch in the control of the House does not make a statistically significant difference to the number of individual income tax returns audited.

The tentative take-away from this analysis is that the partisan affiliation of the President matters most in influencing the intensity of audit activity for individual income tax returns, whereas control of the Senate and the House seems to matter less. The pattern of results is identical if we consider the coefficients in column (2) in which we introduce a lagged term for the percent of individual income tax returns audited or if we consider the coefficients in columns (3) and (4) which examine the variation in number of estate tax returns or columns (5) and (6) which examine the variation in the number of fiduciary (trust) tax returns. In each case, Democratic administrations are significantly more likely to audit tax returns compared to Republican administrations. The additional analysis is available on request from the author. I also note that based on the coefficients in columns (1) through (4), audit activity increases with an increase in the federal deficit but decreases with an increase in the number of information reports received, consistent with a priori expectations of these control variables.

3.5.5 Summary of results

The tentative conclusion that emerges from the empirical analysis conducted thus far is that the President has a modest influence on the overall budgetary and personnel resources commanded by the IRS. In contrast, however, the President is able to exert more subtle influences

on the direction of the agency through changes in the share of IRS employees who are employed as criminal investigators and revenue officers. These subtle influences also manifest themselves in the lower frequency of audits of corporations, individuals, estates, and trusts under Republican administrations.

Perhaps unsurprisingly, Congress emerges as a powerful actor in the budget-setting process. The evidence presented suggests that Democratic control of Congress, in particular, the Senate increases the level of budgetary and personnel resources available to the IRS. In contrast to its effects on the budget, the Senate and the House have relatively little influence on the intensity of audit activity.

3.6 Conclusions

This paper provides a framework for understanding why political actors might care about the implementation of laws beyond the content of the laws themselves. It offers evidence that the likelihood of corporate audits are higher under Democratic administrations and that corporations respond to this increased likelihood of an audit by reporting their tax liability more accurately in the presence of higher audit rates. The effective tax rate borne by corporations is higher by about two percent on account of the higher audit activity which is observed when a Democratic administration is in place. It is worth noting that this increase in the effective tax rate comes about with no change in the tax code, in particular, without any change in the statutory tax rate but simply from the greater emphasis on detecting corporate tax avoidance under Democratic administrations. In addition to the analysis of corporate audits, the paper also finds similar influence of partisanship on the audits of individual income tax returns, estate tax returns, and fiduciary returns which suggests that Republican administrations audit fewer returns. The overall evidence presented in the paper suggests that the IRS is an agency which is not immune to the kinds of political influences that have been documented elsewhere for other federal agencies.

Taking a step back, the larger contribution of the paper stems from its use of hitherto unexplored quantitative data to identify American tax policy as highly mutable and subject to institutional conversion. In other words, the paper suggests that even in the presence of rules that do not formally change, political actors can influence the operation of those rules by altering the resources made available for their enforcement and by subtle cues they send

through their political appointees. It contributes to a very limited literature that has explored the interaction between politics and tax administration and illustrates that the relentless focus by politicians and the media on statutory tax rates may miss changes that occur at a level deeper but are nevertheless material in affecting revenue collections by the Treasury.

Although the examination of variation in IRS resources and intensity of audit activity was motivated by the goal of better understanding the effects of political ideology on tax administration and enforcement, the underlying intuition that motivated the analysis is much more broadly applicable. It could be easily extended to analyzing activities of other federal agencies such as the Federal Trade Commission or the Environmental Protection Agency where Republicans and Democrats have historically held different viewpoints about the missions of these agencies. It is likely that when faced with political gridlock and scrutiny by the media and the public, administrations will make use of the least-cost instrument available at their disposal to realize their desired policy goals. As the cost of using a given policy instrument increases, politicians may attempt to circumvent that instrument by using other instruments at their disposal which do not involve the active cooperation and support of other stakeholders. The increasing use of executive orders to accomplish policy goals is perhaps another illustration of these tendencies at work (see, for example, Mayer, 2001; Cooper, 2002; and Howell, 2003). Future research should explore the use of such less obvious instruments in other contexts besides the setting examined here of administration and enforcement of the nation's tax laws.

Table 3.1: Quasi-statements made by Democratic and Republican Presidents, classified based on whether they were in favor of or opposed to or neutral with respect to tax cuts

	Panel A: In absolute terms			
	Favor	Neutral	Opposed	Total
Democratic Presidents	69	46	39	154
Republican Presidents	173	37	3	213
	Panel B: In relative terms			
	Favor	Neutral	Opposed	Total
Democratic Presidents	44.8%	29.9%	25.3%	100%
Republican Presidents	81.2%	17.4%	1.4%	100%

Source: Author analysis based on Policy Agendas Project. Only quasi-statements classified under topic 107 (Taxation, Tax policy, and Tax Reform) and sub-topic 2009 (IRS Administration) are included in the analysis above. Quasi-statements were classified as being in favor of tax cuts if the President's position was unequivocally in favor of cutting taxes or tax rates for all groups of taxpayers. They were classified as being opposed to tax cuts if there was an explicit mention of increasing taxes or tax rates for at least some taxpayers. Finally, they were classified as being neutral with respect to tax cuts if there was either (1) no reference to cutting or increasing taxes or (2) if within that single quasi-statement, there was a reference to both increasing taxes for some taxpayers *and* decreasing taxes for others or (3) if the President simply stated facts without drawing any normative implications from that statement. One example of each type of statement is given below:

- Favor: I propose that we cut the maximum tax rate on capital gains to increase long-term investment. (Quasi-statement ID 13330 by President George H.W.Bush in his 1989 SOTU address)

- Opposed: And we will ensure that, through effective tax enforcement, foreign corporations who make money in America pay the taxes they owe to America. (Quasi-statement ID 14495 by President Bill Clinton in his 1993 SOTU address)

- Neutral: I've appointed a bipartisan panel to examine the tax code from top to bottom. (Quasi-statement ID 18654 by President George W. Bush in his 2005 SOTU address)

Table 3.2: Summary statistics for the period 1978–2010

	Units	Mean	Standard Deviation	Minimum	Maximum
Data on IRS budgets & headcount:					
IRS budget	In Millions of dollars	6,736	3,110	1,962	12,353
Non-defense outlays	In Billions of dollars	1,270	678	354	2,857
IRS budget / Federal expenses	In percent	0.54	0.056	0.41	0.65
IRS Budget / GDP	In percent	0.0892	0.00837	0.0775	0.108
IRS FTEs		99,105	10,565	83,756	117,945
Total civilian labor force	In millions	129.2	16.6	99	154.3
IRS FTEs / Civilian labor force	In percent	0.0777	0.0113	0.0592	0.0964
Enforcement personnel (Criminal Investigators & Revenue Officers)		9,391	1,205	7,745	11,335
Enforcement Personnel / IRS FTEs	In percent	9.49	0.92	7.84	11.71
Enforcement Personnel / Civilian labor force	In percent	0.00742	0.00147	0.00522	0.00928
Data on corporate audits:					
Audit probability (averaged across all five asset classes)	In percent	15.45	20.08	0.239	86.26
Revenue per corporate tax return filed (net of refunds)	In dollars	58,410	37,205	15,130	163,299
Taxes and penalties per audit [21]	In dollars	402,317	338,085	22,391	1,111,757
Effective Tax Rate	In percent	25.0	6.1	12.7	38.9
Data on political variables:					
Party President	0 = Republican; 1 = Democratic	0.394	0.496	0	1
Party in charge of Senate	0 = Republican; 1 = Democratic	0.515	0.508	0	1
Party in charge of House	0 = Republican; 1 = Democratic	0.636	0.489	0	1
Data on control variables:					
Top individual income tax rate	As a percent	42.06	12.13	28.00	70.00
Top corporate income tax rate	As a percent	38.45	5.43	34.00	48.00
Federal deficit	As a percentage of GDP	2.88	2.53	(2.30)	9.80
Growth rate in real GDP	As a percent	2.81	2.05	(3.35)	7.75
Number of information reports received	In billions	1.170	0.568	0.327	3.024

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[21]The average level of recommended taxes and penalties per audit is much higher than the average net revenue collected per return filed because audits rates are significantly higher for the largest corporations and hence the "average" corporation audited is significantly larger than the "average" corporation that files a tax return.

Table 3.3: Partisan influences on IRS budget over the period 1978–2010

IRS Budget: Normalized by	(1) Deflator for all federal outlays	(2) Non-defense outlays	(3) GDP
Political variables:			
Party President	0.910	-0.594	-17.60
(0 = Republican; 1 = Democratic)	(0.030)	(-0.26)	(-0.49)
Party in charge of Senate	24.57**	1.958**	22.76*
(0 = Republican; 1 = Democratic)	(2.21)	(2.45)	(1.98)
Party in charge of House	14.29	3.159	45.17
(0 = Republican; 1 = Democratic)	(0.40)	(0.97)	(1.15)
Republican President X	-6.129	-2.431	-18.30
Congress with Democrats	(-0.26)	(-0.95)	(-0.64)
Democratic President X	11.08	1.370	41.23
Congress not with Democrats	(0.30)	(0.48)	(1.10)
Control variables:			
Top individual income tax rate	-1.969	-0.263**	-2.221
	(-1.50)	(-2.09)	(-1.21)
Federal deficit	-0.468	-0.411	2.491
(as percentage of GDP)	(-0.094)	(-0.99)	(0.45)
Growth rate of real GDP	341.6	58.05**	-240.6
	(1.12)	(2.47)	(-0.62)
Number of information	-1.89e-09	-2.27e-09	-2.16e-08
reports received	(-0.042)	(-0.63)	(-0.41)
Lagged IRS Budget	0.653***	0.493***	0.734***
	(3.42)	(3.64)	(4.44)
Year (Demeaned)	5.542	-0.0732	-0.409
	(1.08)	(-0.23)	(-0.10)
Constant	265.2	39.32***	324.1
	(1.67)	(2.83)	(1.38)
Number of observations	33	33	33
R^2	0.98	0.87	0.90

Robust t statistics, reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Partisan influences on IRS workforce over the period 1978–2010

IRS FTEs	(1) Absolute number	(2) Normalized by size of civilian labor force
Political variables:		
Party President	-419.2	0.657
(0 = Republican; 1 = Democratic)	(-0.10)	(0.19)
Party in charge of Senate	3702.8*	2.655*
(0 = Republican; 1 = Democratic)	(1.97)	(2.01)
Party in charge of House	706.4	0.0261
(0 = Republican; 1 = Democratic)	(0.19)	(0.0088)
Republican President X	-2410.0	-0.980
Congress with Democrats	(-0.64)	(-0.30)
Democratic President X	-698.4	-1.408
Congress not with Democrats	(-0.18)	(-0.44)
Control variables:		
Top individual income tax rate	-399.3**	-0.331**
	(-2.23)	(-2.47)
Federal deficit	-434.4	-0.183
(as percentage of GDP)	(-0.99)	(-0.55)
Growth rate of real GDP	39229.4	40.57
	(0.86)	(1.17)
Number of information	4.93e-06	3.29e-09
reports received	(1.09)	(1.00)
Lagged IRS FTEs	0.716***	0.647***
	(5.19)	(4.21)
Year (Demeaned)	-558.6*	-0.728**
	(-1.86)	(-2.57)
Constant	47003.9*	46.82**
	(2.00)	(2.17)
Number of observations	33	33
R^2	0.95	0.97

Robust t statistics, reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: Partisan influences on enforcement personnel over the period 1978–2010

	(1)	(2)	(3)
Enforcement personnel	Absolute number	Normalized by IRS FTEs	Normalized by labor force
Political variables:			
Party President	812.1	7.098	0.547
(0 = Republican; 1 = Democratic)	(1.45)	(1.25)	(1.25)
Party in charge of Senate	-415.7	-7.841*	-0.300
(0 = Republican; 1 = Democratic)	(-0.84)	(-1.78)	(-0.79)
Party in charge of House	164.1	1.456	0.200
(0 = Republican; 1 = Democratic)	(0.40)	(0.27)	(0.59)
Republican President X	154.9	2.676	0.0912
Congress with Democrats	(0.27)	(0.45)	(0.20)
Democratic President X	-599.7	-4.217	-0.322
Congress not with Democrats	(-0.86)	(-0.56)	(-0.58)
Control variables:			
Top individual income tax rate	-50.23***	-0.0806	-0.0385***
	(-3.24)	(-0.74)	(-3.39)
Federal deficit	129.6	1.725*	0.118*
(as percentage of GDP)	(1.68)	(1.96)	(1.77)
Growth rate of real GDP	1260.6	-27.62	2.704
	(0.36)	(-0.52)	(0.83)
Number of information	-6.40e-07	-9.97e-09*	-5.25e-10
reports received	(-1.15)	(-1.84)	(-1.14)
Lagged number of enforcement	0.652***	0.635***	0.581***
personnel	(5.43)	(3.57)	(4.23)
Year (Demeaned)	-16.01	0.308	-0.0473
	(-0.44)	(1.00)	(-1.41)
Constant	5843.0***	40.90**	5.470***
	(3.32)	(2.39)	(3.43)
Number of observations	33	33	33
R^2	0.93	0.85	0.97

Robust t statistics, reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Partisan influences on audits of corporate income tax returns over the period 1978–2010

	(1)	(2)
	Log number of returns audited	
Political variables:		
Party President	0.608***	0.560***
(0 = Republican; 1 = Democratic)	(2.99)	(2.77)
Party in charge of Senate	-0.204	-0.221
(0 = Republican; 1 = Democratic)	(-1.32)	(-1.46)
Party in charge of House	-0.0363	0.0779
(0 = Republican; 1 = Democratic)	(-0.18)	(0.37)
Republican President X	0.343	0.296
Congress with Democrats	(1.48)	(1.29)
Democratic President X	-0.271	-0.193
Congress not with Democrats	(-1.05)	(-0.75)
Control variables:		
Top corporate income tax rate	-0.00208	-0.00492
	(-0.12)	(-0.28)
Federal deficit	0.0540*	0.0535*
(as percentage of GDP)	(1.81)	(1.84)
Growth rate of real GDP	-2.826	-3.808*
	(-1.47)	(-1.94)
Number of information	-3.89e-10	-4.74e-10*
reports received	(-1.44)	(-1.76)
Log number of	0.640***	0.511***
returns filed	(5.74)	(3.83)
Lagged audit probability	-	-0.000615
		(-1.38)
Year (Demeaned)	-0.0303	-0.0255
	(-1.52)	(-1.28)
Constant	1.704	3.506**
	(1.10)	(1.96)
Number of observations	165	160

t statistics, reported in parentheses * p < 0.10, ** p < 0.05, *** p <0.01. I use the command "xtreg" with random effects in Stata 11 for the estimation. The choice between a fixed effects and a random effects specification is made on the basis of a Hausman test. The p-value that the random effects specification is inconsistent is 0.9604 (col.(1)) and 0.6318 (col.(2)) indicating the appropriateness of a random effects specification.

Table 3.7: Effect of audit probability on effective tax rate and a measure of voluntary compliance over the period 1978–2010

	(1)	(2)
	Dependent variable: Effective Tax Rate	Dependent variable: Voluntary compliance
Panel A: Without controlling for party affiliation of the President and Congress		
Percent of returns audited	1.493**	192.1***
	(2.78)	(8.44)
Top statutory corporate tax rate	-0.0463	5.242
	(-0.21)	(1.26)
Constant	27.02***	-619.9***
	(4.34)	(-3.26)
R^2	0.59	0.93
Panel B: Controlling for party affiliation of the President and Congress		
Percent of returns audited	1.297**	177.6***
	(2.35)	(6.07)
Top statutory corporate tax rate	0.503*	13.56*
	(1.86)	(1.87)
Political variables:		
Party President	5.876	-6.016
(0 = Republican; 1 = Democratic)	(1.68)	(-0.041)
Party in charge of Senate	-5.328***	95.46
(0 = Republican; 1 = Democratic)	(-3.89)	(1.72)
Party in charge of House	5.537	-127.2
(0 = Republican; 1 = Democratic)	(1.66)	(-1.11)
Republican President X Congress with Democrats	9.688***	52.92
	(3.03)	(0.44)
Democratic President X Congress not with	-10.92***	-67.81
Democrats	(-3.00)	(-0.47)
Constant	11.65	-949.3***
	(1.47)	(-4.31)
Linear Time Trends	Y	Y
Number of observations	33	33
R^2	0.89	0.95

Robust t statistics reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Additional controls included in the regressions are the federal deficit as a percentage of GDP, the real growth rate of GDP, and the number of information reports received. Full results are available on request from the author.

Table 3.8: Partisan influences on audits of individual income tax, estate tax, and fiduciary returns over the period 1978–2010

Type of returns	(1)	(2)	(3)	(4)	(5)	(6)
			D.v.: Log number of returns audited			
	Individual Income		Estate Income		Fiduciaries (Trusts)	
Political variables:						
Party President	0.688***	0.456***	0.111*	0.111**	-0.817**	-0.755*
(0 = Republican;	(3.70)	(3.89)	(1.86)	(2.19)	(-2.13)	(-1.96)
1 = Democratic)						
Party in charge of Senate	-0.236	-0.122	-0.0158	-0.00161	0.394**	0.320*
(0 = Republican;	(-1.39)	(-1.41)	(-0.42)	(-0.05)	(2.40)	(1.83)
1 = Democratic)						
Party in charge of House	-0.0149	-0.00401	-0.124*	-0.145**	0.346	0.339
(0 = Republican;	(-0.08)	(-0.03)	(-1.91)	(-2.46)	(1.12)	(1.01)
1 = Democratic)						
Republican President X	0.419**	0.175	-0.0285	0.00679	-0.717*	-0.595
Congress with Democrats	(2.30)	(1.47)	(-0.53)	(0.12)	(-1.93)	(-1.60)
Democratic President X	-0.253	-0.294*	0.0570	0.0344	0.964**	0.897**
Congress not with Democrats	(-0.77)	(-1.78)	(0.86)	(0.50)	(2.54)	(2.19)
Control variables:						
Top individual income tax rate	-0.00356	-0.00689	0.00358	0.00295	0.0332**	0.0321**
	(-0.63)	(-1.52)	(1.21)	(1.27)	(2.63)	(2.55)
Federal deficit	0.0894**	0.0593*	0.0350***	0.0371***	-0.0259	-0.0170
(as percentage of GDP)	(2.19)	(2.09)	(4.20)	(4.02)	(-0.66)	(-0.52)
Growth rate of real GDP	-5.871***	-4.875***	-0.714	-0.556	2.732	2.347
	(-3.80)	(-3.05)	(-0.84)	(-0.59)	(0.90)	(0.71)
Number of information	-7.55e-10**	-6.18e-10***	-1.22e-10*	-1.56e-10*	2.48e-10	1.97e-10
reports received	(-2.58)	(-2.92)	(-1.78)	(-1.75)	(0.99)	(0.70)
Constant	220.7***	117.0***	8.343***	5.867***	10.48	8.700
	(4.33)	(2.98)	(12.18)	(4.03)	(0.43)	(0.37)
Linear Time Trends	Y	Y	Y	Y	Y	Y
Log number of returns filed	Y	Y	Y	Y	Y	Y
Lag for audit probability	N	Y	N	Y	N	Y
Number of observations	33	32	33	32	33	32
R2	0.77	0.89	0.99	0.99	0.86	0.85

Robust t statistics, reported in parentheses * p < 0.10, ** p < 0.05, *** p <0.01. Cols. (3)–(6) also include a control for the top corporate income tax rate.

Figure 3.1: IRS budget normalized by non-defense federal expenses (1978 - 2010)
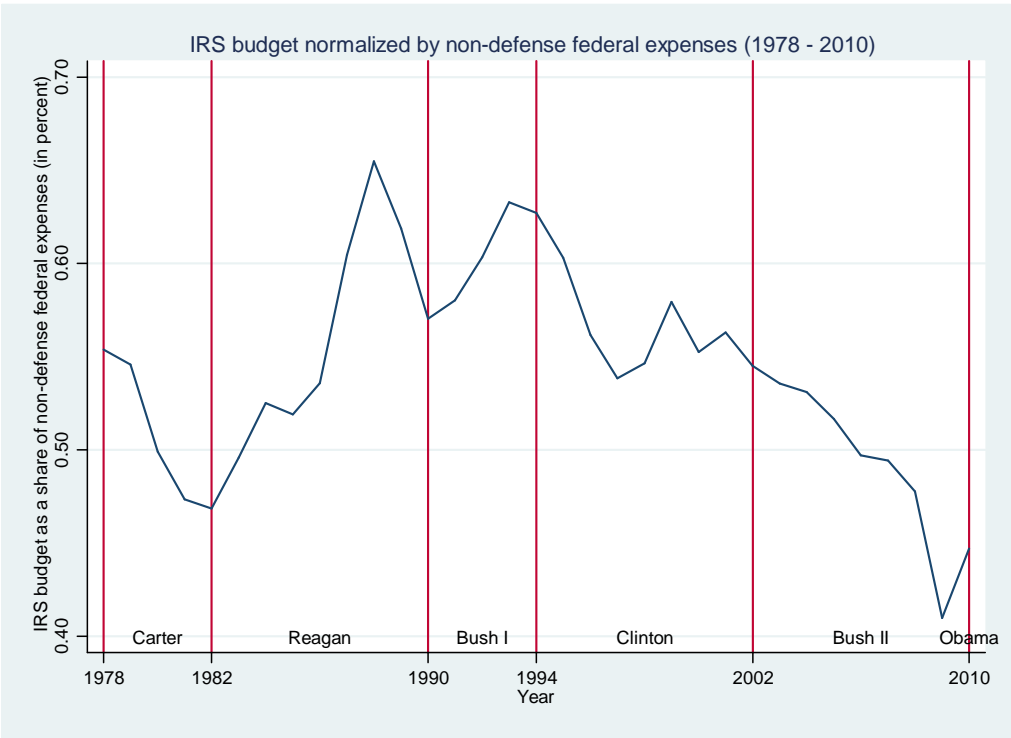
Figure 3.2: IRS workforce (1978 - 2010)

Figure 3.3: Criminal investigators and revenue officers normalized by number of IRS person-nel (1978 - 2010)

IRS criminal investigators and revenue officers as a percent of IRS FTEs (1978 - 2010)
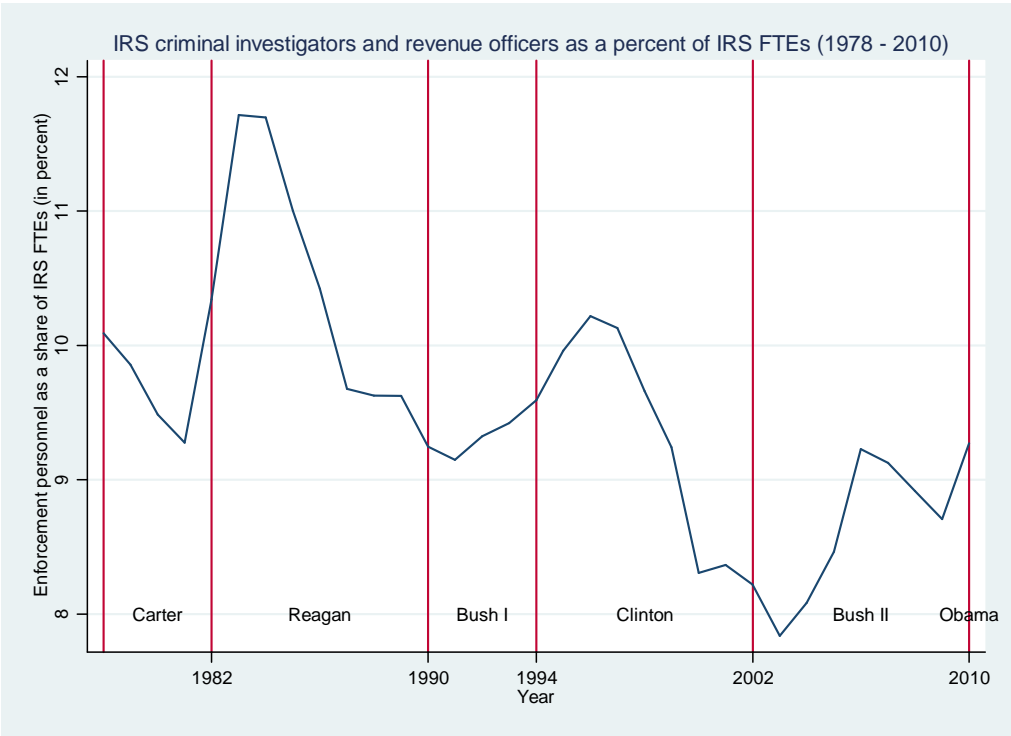
Appendix A: Data sources

- Data on the size of the IRS budget and personnel, its allocation across functions, the number of information reports received, and the number of returns filed and audited is obtained from the IRS data books available on the IRS website at:

  http://www.irs.gov/taxstats/article/0„id=226923,00.html (Accessed 11th March, 2014).

- The numbers for non-defense federal outlays, deflator, the size of the federal deficit, and growth rate of real GDP are from the Office of Management and Budget at:

  http://www.whitehouse.gov/omb/budget/Historicals (Accessed 11th March, 2014). I use Table 10.1 to obtain the deflator and construct the growth rate of real GDP. I use Table 15.6 to construct the size of the federal deficit as a percentage of GDP.

- The corporate income tax rates are available at:

  http://www.jct.gov/publications.html?func=startdown&id=3719 &

  http://www.irs.gov/pub/irs-soi/02corate.pdf (Accessed 14th December, 2011).

- The top individual income tax rate is extracted from

  http://www.irs.gov/uac/SOI-Tax-Stats-Historical-Table-23 (Accessed 11th March 2014).

- Data on corporate profits are available at: http://www.bea.gov/iTable/index_nipa.cfm (Accessed 29th December, 2011). I use Tables 6.16 B, 6.16 C, and 6.16 D to construct a comprehensive time series of corporate profits over the period 1978–2010.

Appendix B: Select robustness checks

I conduct a number of robustness checks to explore the sensitivity of the result that Democratic administrations are more likely to audit corporate income tax returns. In each of these robustness checks that were conducted, the substantive finding that the frequency of audits for corporations with a Democrat in the White House is higher is unchanged. Full results are available on request from the author.

1. Robustness check 1 (RC 1) – Alternative specifications:

(a) Fixed Effects versus Random Effects: The choice of a random effects specification versus a fixed effects specification is made on the basis of a Hausman test. However, I also examine the robustness of the results to Random Effects specification since Griliches and Hausman (1986) stress that observing consistent estimates across alternative panel data estimation techniques supports the absence of serious errors in variables problems. The results reported in Table 3.6 are robust to a Fixed Effects specification.

(b) Introducing additional interactions between the party of President and term of the President: Different parties may need to cater to different political bases and Republican Presidents may be especially reluctant to audit corporations in their first term in office. To allow for this possibility, I introduce an interaction term between party of the President and the term of the President. There appears no support for this hypothesis that the effect of having a Republican in the White House is different between the two terms; in both terms, Republican administrations audit significantly fewer corporate returns than Democratic administrations.

2. RC 2: Additional lags for number of returns filed: I explore alternate specifications in which I control for the log of the number of returns filed in all three calendar years prior to the fiscal year for which data is being reported. This is done because "audits completed in the current year include a mix of returns filed during the previous three years" (Scholz and Wood, 1998, p. 152). The results thus obtained are very similar to those in Table 3.6.

3. RC 3: Dropping corporations which belong to the largest asset class: One concern with the results above might be that for the largest firms, audit probabilities are close to 1 and there is limited variation in the percent of returns audited over time. That is not entirely true since in this sample, for the asset class that includes the largest firms, viz. those with assets in excess of $100 million, audit probabilities average 0.47 and vary between a low of 0.21 in 2009 to a high of 0.86 in 1985. In any case however, the results are robust to the exclusion of firms that belong to this asset class.

4. RC 4: Issues with drift across nominal asset classes: The figures on the likelihood of audits is constructed using the IRS data books is in terms of nominal asset classes, e.g. corporations with assets between $1 and $10 million, corporations with assets between

$10 and $100 million, etc. where the thresholds are not adjusted for inflation. One possible issue with the use of nominal thresholds is that over time, as the average asset size of corporations increases because of inflation, more and more corporations will fall in asset classes that correspond to (nominally) larger thresholds. However, given constraints on the IRS budget, fewer and fewer of such corporations that belong to (nominally) larger asset classes would be audited resulting in a general decline in the likelihood of audits over time for those asset classes. This decline in likelihood of an audit could then be attributed to a change in partisan control of the Presidency and the Congress possibly biasing us in favor of a spurious positive finding between political ideology and audit frequency. To rule out this alternative explanation, I introduce an additional control variable, viz. the fraction of returns that are filed by small firms in any given year. In doing so, I define small firms as all corporations that have assets less than $10 million, the same definition as is used by the IRS. The results obtained in Table 3.6 are robust to the inclusion of this additional control variable.

Alternatively, it is possible to re-define the threshold as either $1 Million or $100 Million and characterize all firms with assets less than those set thresholds as small. I have estimated these regressions and compared the results obtained with those in the base specification as well as the results obtained when the threshold is set at $10 million. Although the specific numbers obtained vary based on the threshold set, I observe that in each case, a change in control of the Presidency from Republican to Democratic hands is associated with an increase in the frequency of audits. The take-away from these additional regressions is similar to that reached earlier: partisan affiliation of the President matters more in influencing the intensity of corporate audit activity, whereas control of the Senate and the House seems to matter less. In that respect, our conclusions are robust with respect to alternative choices of the threshold.

Appendix C: Discussing the equality of effects

If one were to look at the table which illustrates the effect of the Presidency on any dependent variable given the four possible configurations of Congress, it turns out that three of the four numbers are exactly equal. A similar pattern emerges when we examine the effect of the Senate (House) for the four possible configurations of the Presidency and House (Senate). In

the latter cases, two of the four numbers are exactly equal. This section discusses why we observe these patterns.

The specification used allows for interactions between control of the Presidency and control of Congress by introducing dummy variables which correspond to Democratic control of the Presidency (or not) and Democratic control of Congress (or not). Theoretically, there could be as many as three dummy variables for these interaction terms given that each of the individual terms, control of the Presidency and control of Congress could have two states: control by Democrats or not. In practice, as we never observe a situation in which Democrats face a split Congress, it turns out that we have only two dummy variables instead of the maximum possible three. These correspond to (1) a Republican President facing a Democratic-controlled Congress and (2) a Democratic President facing a Congress that is *not* controlled by Democrats (i.e. at least one of the two chambers is under Republican control). Given that the specification is:

$$Dependent\ variable_t = \beta_0 + \beta_1 * Party\ President_{t-1} + \beta_2 * Party\ Senate_{t-1} + \beta_3 * Party\ House_{t-1} +$$

$$\beta_4 * Party\ President_{t-1} * Congress\ with\ Democrats_{t-1} + \beta_5 * Dependent\ variable_{t-1} + \beta_6 * X_t + \mu_t + \varepsilon_t$$

let $\hat{\beta_{41}}$ be the estimated coefficient for the first dummy variable, i.e. a Republican President faces a Democratic-controlled Congress and $\hat{\beta_{42}}$ be the estimated coefficient for the second dummy variable, i.e. a Democratic President faces a Congress that is *not* controlled by Democrats. In that case, the following table represents the level of the dependent variable for the four possible configurations of Congress:

Orientation of Congress	Level of dependent variable under		
	Republican President	Democratic President	Delta
Democratic Senate, Democratic House	$\hat{\beta_2} + \hat{\beta_3} + \hat{\beta_{41}}$	$\hat{\beta_1} + \hat{\beta_2} + \hat{\beta_3}$	$\hat{\beta_1} - \hat{\beta_{41}}$
Democratic Senate, Republican House	$\hat{\beta_2}$	$\hat{\beta_1} + \hat{\beta_2} + \hat{\beta_{42}}$	$\hat{\beta_1} + \hat{\beta_{42}}$
Republican Senate, Democratic House	$\hat{\beta_3}$	$\hat{\beta_1} + \hat{\beta_3} + \hat{\beta_{42}}$	$\hat{\beta_1} + \hat{\beta_{42}}$
Republican Senate, Republican House	0	$\hat{\beta_1} + \hat{\beta_{42}}$	$\hat{\beta_1} + \hat{\beta_{42}}$

One can readily see from this table as to why three of the four differences I calculate are exactly equal to each other.

For completeness, I also illustrate how the dependent variable changes as we move from a Republican-controlled Senate to a Democratic-controlled Senate for each of the four possible configurations of the Presidency and the House.

Orientation of Presidency and House	Level of dependent variable under		Delta
	Republican Senate	Democratic Senate	
Democratic President, Democratic House	$\hat{\beta}_1 + \hat{\beta}_3 + \hat{\beta}_{42}$	$\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3$	$\hat{\beta}_2 - \hat{\beta}_{42}$
Democratic President, Republican House	$\hat{\beta}_1 + \hat{\beta}_{42}$	$\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_{42}$	$\hat{\beta}_2$
Republican President, Democratic House	$\hat{\beta}_3$	$\hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_{41}$	$\hat{\beta}_2 + \hat{\beta}_{41}$
Republican President, Republican House	0	$\hat{\beta}_2$	$\hat{\beta}_2$

The table shows why two of the four differences I calculate (corresponding to a Republican-controlled House) are exactly equal to each other.

Finally, the following table illustrates how the dependent variable changes as we move from a Republican-controlled House to a Democratic-controlled House.

Orientation of Presidency and Senate	Level of dependent variable under		Delta
	Republican House	Democratic House	
Democratic President, Democratic Senate	$\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_{42}$	$\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3$	$\hat{\beta}_3 - \hat{\beta}_{42}$
Democratic President, Republican Senate	$\hat{\beta}_1 + \hat{\beta}_{42}$	$\hat{\beta}_1 + \hat{\beta}_3 + \hat{\beta}_{42}$	$\hat{\beta}_3$
Republican President, Democratic Senate	$\hat{\beta}_2$	$\hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_{41}$	$\hat{\beta}_3 + \hat{\beta}_{41}$
Republican President, Republican Senate	0	$\hat{\beta}_3$	$\hat{\beta}_3$

The table shows why two of the four differences I calculate (corresponding to a Republican-controlled Senate) are exactly equal to each other.

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Chapter 4

Barriers to Entry and Competitive Behavior: Evidence from Reforms of Cable Franchising Regulations

4.1 Introduction

Watching TV is the predominant leisure activity in the U.S., accounting for about half of leisure time on average for American adults (BLS 2012). Cable television plays a very significant role in this activity, with a share of over 60% of U.S. households in 2010 (Nielsen 2011). For an industry with such a significant presence in households across the nation, cable television continues to see little competition in many local markets, despite federal reforms such as the Cable Act of 1992 and the Telecommunications Act of 1996 that aimed specifically to enhance competition. In particular, based on a 2009 survey, the FCC documented effective competition (from either satellite TV (DBS) or a second wireline operator) for only 31.65% of subscribers (FCC 2011, p. 18), implying that 68.35% of subscribers did not have competitive offerings to choose from.

Policy measures to boost competition in the cable industry continue to be debated at the state and federal levels. In this paper, we examine the impact of one such reform – the deregulation of the franchising process for cable television, which was adopted in nineteen states of the U.S. between 2005 and 2008. Prior to 2005, in all but four states, prospective entrants to any local market (termed as "community") had to negotiate with local municipalities on a case-by-case basis. Because the local authorities often imposed significant restrictions on the new entrants, the need to negotiate individually with local municipalities posed a challenge to potential entrants, with the FCC concluding that "local franchising process in many jurisdictions constitute an unreasonable barrier to entry that impedes...cable competition" (FCC

2006).

Following deregulation, the reformed states have allowed for cable companies to receive a *single state-wide* franchise for providing service to communities anywhere within the state. By standardizing the terms and conditions that apply to both existing providers and new entrants into this industry, the reforms effectively liberalized entry by reducing the restrictions typically imposed on new entrants by local city councils (FCC 2006, FCC 2007, Bolema 2008). The significance of the reforms is reflected in the intense lobbying and legal efforts undertaken by incumbents to delay the reforms and by telecom companies ('Telcos') in support of the reforms.[1]

Although these reforms were the subject of strong debate and lobbying efforts, there has been surprisingly little systematic evaluation of the effect of the reforms,[2] and anecdotal evidence on the effect of the reforms is mixed.[3] In addition to being of direct policy interest, studying these reforms provides a unique opportunity to examine the effects of a reduction in barriers to entry on incumbent behavior. As pointed out by Goolsbee and Syverson (2008), the effect of an increased *threat* of entry has been a topic of interest in the theoretical literature, but has been much less investigated empirically, as it is difficult to find empirical instances where the threat of entry went up without necessarily inducing entry itself. More broadly, the reforms provide a setting to study the effect of competition on firm behavior; changes in regulatory barriers to entry helps overcome the difficult challenge of finding exogenous sources of variation in the competitive environment (Holmes and Schmitz, 2010).

Because deregulation was introduced across the nineteen states in a staggered manner, we are able to adopt a standard difference-in-differences (DID) approach exploiting differences in timing to control for common shocks (e.g., as in Card 1992). Using a rich micro data set which includes data on prices of cable television in every local community across the United States over a seven-year period from 2004 to 2010, we examine whether the price of subscribing to the two primary service tiers – "Basic" and "Expanded Basic" – declined in response to these reforms.[4] Complementing this dataset with hand collected data on entry by the two

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[1]For anecdotal evidence on the lobbying efforts see news articles by e.g., Reardon 2005a, Haugsted 2006, Sura 2006, and Sher 2008.

[2]One exception is a study by Bohanon and Hicks (2010), who examine the effect of franchising reforms on number of broadband connections using FCC data; they find an increase in broadband subscriptions following the reforms.

[3]Articles about specific states suggesting consumers benefitted include Spiwak 2006 (Texas), Schneider 2007 (Wisconsin) and Rogers 2008 (Michigan), while Kreucher 2008 (Michigan), and Barrett 2008 (Wisconsin) argue otherwise.

[4]"Basic" cable service, also referred to as the basic service tier ("BST"), is the minimum level of cable television

major Telcos, Verizon and AT&T, we also examine how entry by cable operators and Telcos was affected by the reforms.

We find that prices for "Basic" service are lower by about 5.5 to 6.8% in DID terms in states which have reformed their franchising process for cable television. We find no effect on prices of the more popular "Expanded Basic" service tier. We confirm that the DID effects were not impacted by pre-existing trends – the trends in prices for both "Basic" and "Expanded Basic" service in the reformed states were not different from that in the non-reformed states. We check and find these results robust to a range of robustness checks. In particular, we find the results robust to including additional controls for quality of channels offered (in addition to the control for number of channels), including amortized costs of installation in the price, examining long differences, restricting analysis to a balanced panel of communities, using alternative fixed effects, including only principal communities, and excluding some sub-sets of states. As a falsification test, we check and find no effects using residential, commercial and industrial electricity prices. Finally, we find that quantity (measured as number of subscribers) went up in response to the price decline, though the estimated effects are noisy.

We then examine entry, and find evidence for significant additional entry in reformed states, particularly by Telcos. While raw summary statistics suggest about 13.3% more communities in reformed states experienced entry, controlling for demographic and market characteristics in a linear propensity model, we find a DID excess entry rate between 7.95% and 13.8% in the reformed states.

Our findings carry implications for related theoretical literature. As we discuss in Section 4.3, the theoretical predictions about the effect of a reduction in costs of entry vary across models, as discussed in detail in Sutton (1991). E.g., in a model where potential entrants expect Bertrand competition in the second stage after entry, they would not enter and the incumbent would not change behavior, so long as sunk costs of entry are greater than zero (which is likely the case in our context, as entry requires considerable capital investment, even if the reforms reduce some costs associated with negotiating local franchises). In other models (e.g. a model where the post entry behavior is expected to be Cournot competition) there is indeed entry, and post-entry prices are lower than under monopoly. The baseline

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service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. "Expanded Basic" cable service, offers additional video channels on one or more service tiers (FCC 2009, p. 12).

results on price, and more importantly on entry, suggest evidence against the Bertrand model and potentially in favor of the other models.

In this context, an interesting question is whether we should expect any price declines without actual entry occurring. The models in Sutton (1991), do not explicitly address this question, but do predict declines only post-entry. Milgrom and Roberts (1982a) show that in a world with perfect information, cutting prices prior to entry does not deter entry, and hence such "limit pricing" behavior would not be a rational equilibrium. However, in a world with asymmetric information (Milgrom and Roberts, 1982b) or with switching costs (Klemperer, 1987), incumbents may lower prices before entry to signal lower costs or to lock up consumers, and hence discourage entry.

We examine whether there is evidence for ex-ante price declines, by excluding communities which experienced actual entry from the sample. We find that indeed there is evidence for incumbents cutting price in response to just the enhanced *threat* of entry: communities without entry have a greater price decline of 6.57% for "Basic" service, in reformed states relative to non-reformed states.[5]

Building on this finding that it was the *threat* of entry that lead to price declines, we devise a sample-split (triple difference) test that controls for potential state-level correlated shocks. In particular, we examine communities in counties where a major overbuilder is present.[6] Incumbents here are likely to face a greater threat of entry, as cable overbuilders are more inclined to enter into neighboring communities (RCN 2005 Annual Report, Seamans 2013). We find that there is indeed a greater DID decline in price in communities close to a major overbuilder; this decline is about 10.6% and statistically significant for "Basic" service, and equally large (10.2%) but statistically less significant for "Expanded Basic" service. Interestingly, we find very large and statistically significant increases in number of subscribers for both "Basic" and "Expanded Basic" services consistent with the observed price cuts, in communities close to the major overbuilder. To the extent that potential omitted shocks, due unobserved demand (e.g., due to differential entry by Satellite TV) or cost (e.g., due to local

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[5]Interestingly, comparing communities where there was actual entry, we see no significant differences between reformed and non-reformed states. This is unsurprising as we expect no differences between reformed and non-reformed states conditional on entry. In fact this result is reassuring, as it suggests omitted variables were not driving general price declines in reformed states.

[6]As discussed in FCC (2009, p. 15, footnote 97), the term "overbuild" describes the situation in which a second cable operator enters a local market in direct competition with an incumbent cable operator. In these markets, the second operator, or "overbuilder," lays wires in the same area as the incumbent, "overbuilding" the incumbent's plant, thereby giving consumers a choice between cable service providers.

cost inflation not captured by local income or wages included in baseline controls) shifters do not vary across locations within a state, this sample-split result suggests that reforms did indeed cause changes in incumbent behavior, consistent with the models that predict ex-ante price responses (such as Milgrom and Roberts 1982b or Klemperer 1987).

Our study contributes to the small literature discussing the effect of cable franchising deregulation (Spiwak 2006, Schneider 2007, Bohanon and Hicks 2010). To the best of our knowledge, our study is the first to systematically examine the effects of cable franchise deregulation on prices and entry using comprehensive national microdata, and hence the first to document systematic declines in prices and increased entry in reformed states. The cable industry provides an interesting setting for empirical work because there are numerous geographically separated markets. Our work also relates to the broader literature in Industrial Organization that has exploited this setting to examine various aspects of the cable market including effect of vertical integration between programming and distribution (Chipty 2001), welfare effects of bundling (Crawford 2008 and Crawford and Yurukoglu 2012), and welfare effects of new products (Goolsbee and Petrin 2004). In a related policy evaluation paper, Crawford (2000) examined the effects of the 1992 Cable Act, which was aimed at protecting consumers from price increases. Crawford found that intended price declines failed to materialize and observed system responses yielded no change in consumer welfare. In contrast, our results suggest some welfare gains, albeit modest on average, for consumers of the "Basic" service tier.

Because we find ex-ante price responses by incumbents to the increased *threat* of entry, this study also relates to the empirical literature on ex-ante reductions or limit pricing. Although a number of studies have examined ex-post responses by incumbents to entry (Yamawaki 2002, Simon 2005, McCann and Vroom 2010), relatively few have examined ex-ante responses. In recent work, Goolsbee and Syverson (2008) find that incumbents respond to the threat of entry by a low-cost competitor (Southwest) by preemptively reducing prices. In work related to ours, Seamans (2013) examines limit pricing behavior in cable TV markets. Seamans notes that limit pricing in the Milgrom and Roberts (1982b) model hinges on the presence of asymmetric information; with perfect information, ex-ante price reductions are not optimal. Using a novel measure that captures variation in asymmetric information between incumbents and new entrants across markets, he finds that limit pricing behavior is indeed more strongly prevalent in markets with greater degree of asymmetric information.

Our work differs in that we focus on inter-temporal variations in barriers to entry introduced by reform of local franchising regulations. In addition to testing for the presence of limit pricing, our objective is also to undertake an evaluation of this important policy reform.

The paper proceeds in eight sections. Section 4.2 provides background on the industry, regulations, and the state-level reforms that are the focus of this study. Section 4.3 lays out the theoretical underpinnings for our empirical analysis, while Section 4.4 introduces the data. Section 4.5 presents our analysis of the effect of reforms on prices. Section 4.6 examines actual entry following the enactment of these reforms, and examines whether incumbents responded to increased threat of entry. Section 4.7 presents the sample-split test examining communities adjacent to major overbuilders. We discuss results and conclude in Section 4.8.

4.2   Industry Background, Regulatory Framework, and Reforms

4.2.1   Industry background

For studying the effect of changes in entry barriers, an attractive feature of cable TV markets is their localized nature, which allows for considerable variation in the competitive settings across geographic markets. These local markets are defined by the "franchise" boundary of the incumbent system; typically the boundary is the geographical boundary of the city in which the incumbent system is based. This effectively segments the US into thousands of non-overlapping geographic markets.

Historically, a single incumbent cable TV system served each local market. In their 2009 annual report, the Federal Communications Commission notes that: "Relatively few consumers have a second wireline alternative, such as an overbuild cable system" (FCC 2009, p. 5). An earlier FCC Report on Cable Industry Prices (FCC 2005) found that cable television service was provided in 32,510 "non-competitive" communities while there were only approximately 400 communities with competitive wire line "overbuilds" (i.e., with more than one operator in the market) (Braunstein, 2008).

Although in theory competition from Direct Broadcast Satellite (DBS) could have limited the amount of pricing power enjoyed by cable TV incumbents, this appears not to have been borne out in practice (for the period of our study). An assessment by the FCC in 2009 noted that "... in the large number of communities in which there has been a finding that the statutory test for effective competition has been met due to the presence of DBS service, com-

petition does not appear to be restraining price as it does in the small number of communities with a second cable operator".[7, 8]

Traditionally, one reason proposed for why most markets remain monopolies (in terms of wired cable service) is the potentially significant economies of scale in providing cable service and the large investments required for laying coaxial cable to the homes of consumers. However, Owen and Greenhalgh (1986) argue that economies of scale are not so large as to rule out the possibility that competition, either direct or potential, can serve as a significant disciplinary force in the marketplace. More recently, Kelly and Ying find that "average cost savings with respect to a monopoly were fairly small, ranging from 1.37% with a 10% market overlap to 5.05% with a complete overbuild" (Kelly and Ying, 2003, p. 962). Another significant factor contributing to the persistence of local monopolies has been local "franchising" regulations (FCC 2006, FCC 2009), which have acted as a significant barrier to market entry. We discuss these local franchising regulations in the next section.

4.2.2 Franchising regulation of cable service providers

Cable companies have been traditionally regulated at the local level. To obtain permission from local government authorities to operate in a market, firms are required to agree to a number of terms as part of a "franchise" agreement. Local governments use these agreements to achieve multiple goals including raising revenue, providing access to cable TV equitably in the community, and minimizing disruptions from laying of cable. In particular, some of the key terms relate to:

- Franchise fees: Franchise fees to be paid annually typically amount to 5% of the revenue received by cable companies. These can be important sources of revenue for local governments.[9]

- Build-out provisions: The desire of local governments to provide access to consumers

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[7]Source: http://www.fcc.gov/Daily_Releases/Daily_Business/2009/db0116/DA-09-53A1.txt. Accessed on 01/12/2012

[8]Because barriers to DBS entry were related to geographical factors (Goolsbee and Petrin 2004), we do not expect entry rates to be correlated with reforms in a way that would bias our results. Nevertheless in Section 4.7 below, we discuss a sample-split (triple difference) approach that could control for shocks correlated with the reforms such as higher DBS entry into reformed states.

[9]A government committee report (Joint Study Committee, 1998) in Georgia documented that for the 525 governmental units studied, cable franchise fees represented an average of 6.66% of total tax revenues. The most recent estimate for franchise fees paid by cable operators is $3.2 billion per year, per the Cable & Telecommunications Association. (http://www.ncta.com/Statistics.aspx Accessed on 10/12/2012)

everywhere within their service jurisdiction generally leads them to prescribe build-out requirements where entrants are obligated under the franchise agreement to cover the entire service area within a prescribed span of time.

- Public, Education, and Government (PEG) channels to be carried on the service.

- Service networks to government and educational buildings (I-Net).

- Local agency control over public rights-of-way permits: These terms aim to minimize disruptions that would result from uncoordinated digging up of roads and walkways by cable companies.

- Consumer protection through customer service standards and enforcement.

Per the FCC (FCC 2004 - 2005, FCC 2009), the local franchising process imposes significant barriers for potential wireline entrants. In his study, Hazlett (2007) concludes that the local permitting process delays competitive entry, and depending on the nature of the administrative process and the terms and conditions imposed on the potential entrant, franchising may deter entry altogether.

Federal policy makers sought to outlaw anti-competitive build-out requirements and prevent discriminatory pricing that limit competition, through the Telecommunications Act of 1996. However, because municipal franchising agents were granted immunity from damage awards, there is no effective remedy if regulators impose unreasonable build-out requirements, or fail to enforce uniform pricing rules. An FCC (2007) report found that the franchise process was being manipulated so that "new entrants eager to provide video service are often delayed, and in some cases derailed, by the unreasonable demands made by local franchising authorities." This report reiterated the findings in an earlier FCC (2006) order, where the Commission concluded that: "the operation of the local franchising process in many jurisdictions constituted an unreasonable barrier to entry..." The FCC (2006) order discusses several ways by which local franchising authorities were unreasonably refusing to award competitive franchises. These include drawn-out local negotiations with no time limits; unreasonable build-out requirements; unreasonable requests for "in-kind" payments that attempt to subvert the five percent cap on franchise fees; and unreasonable demands with respect to public, educational and government access (or "PEG") channels.

4.2.3 Reforms of cable franchising regulation

Given the reality of limited competition in the cable industry, there have been a number of attempts at both the state and federal level to enhance competition (Spurgin 2008).[10] These have often taken the form of limiting local government's ability to regulate competitive cable operators and allow for the possibility of issuing statewide or nationwide cable franchises to companies interested in providing cable service, whether they be cable companies or telephone companies.

Although no federal action for nationwide franchising has been taken to date, twenty three states have reformed their franchising process to allow for statewide franchises. Such laws pre-empt the need for new entrants to negotiate individual franchise agreements with local municipal authorities, and give them the authorization to launch services anywhere in the state following approval of the state-wide franchise application.

Table 4.1 shows the status of franchise reform in all 50 states, along with the year of passage for all states that have reformed their franchising process. Four states, Alaska, Hawaii, Rhode Island and Vermont, had state franchising laws in place prior to 2005; nineteen states passed similar laws between 2005 and 2008.

[Table 4.1 about here.]

For the states that have passed state-wide cable television/video franchise laws, Spurgin (2008) provides a general comparison of key provisions (see Table A1 in Appendix) including variations in the franchise fees, requirements on provision of Public, Educational, and Government Access (PEG) channels, controls on Right of Way, and build-out requirements by state. We supplemented Spurgin (2008) by a review of the legislative bills; based on our review, we re-classified Virginia as "not reformed", as the legislation for the state did not include a provision for a single state-wide franchise. Also, we confirmed passage of the law for Louisiana, where the legislation was pending per Spurgin (2008).

A number of states set up separate commissions specifically charged with overseeing the new state franchises. Franchise fees for new service providers with a state-issued franchise are set by most states to be equal to incumbent fees, with a maximum amount typically being five percent of gross revenues. Build-out requirements under state franchise laws are gener-

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[10]This section draws from Spurgin (2008).

ally less stringent than under local franchise agreements. Very few bills to date have included build-out provisions, and those that do are heavily qualified.[11]

Given that the critical provisions (relating to granting a state-wide franchise to the applicant, equating of franchisee fees between entrants and incumbents, and relaxation of build-out requirements) are similar across the different reformed states, in our analysis we treat the reforms as a dichotomous variable. As noted before (in footnote 4.1), incumbent cable companies lobbied strongly against franchising reforms, while telecom companies, specifically Verizon and AT&T, worked to support these legislations.

As discussed earlier, there is little work systematically examining the effect of the reforms on prices and entry across all reformed states. Bohanon and Hicks (2010) examine the effect of franchising reforms on number of broadband connections using FCC data, and they find an increase in broadband subscriptions following the reforms. Anecdotal evidence on the effect of reforms in particular states has been mixed with some reports of reduced prices after entry (e.g., Spiwak 2006 (Texas), Schneider 2007 (Wisconsin) and Rogers 2008 (Michigan)), and other reports of no decline in prices (e.g., Kreucher 2008 (Michigan) and Barrett 2008 (Wisconsin)).

4.3   Theoretical Background

We view the enactment of reform that allows for the award of a single state-wide franchise as reducing the barriers to entry in the market for cable TV services, by reducing the exogenous, sunk costs associated with entering each local market.

In a large class of models, reduced (sunk) entry costs could lead to more entry in equilibrium which then puts downward pressure due to post-entry competition (e.g., in the Cournot or Monopoly model in Chapter 2 of Sutton 1991). However, as Sutton (1991) discusses in Chapter 2, this result hinges on assumptions about the nature of price competition in the post-entry stage. He shows for example that assuming homogenous products and Bertrand competition in the second stage, a reduction in entry costs does not necessarily attract new entry, as entrants anticipate price to equal marginal cost post-entry in the second stage, and this completely deters entry. In fact in the Bertrand model, changes in exogenous sunk entry

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[11]Most state laws that provide for a state-wide franchising authority allow existing local franchise agreements to remain in effect until a new service provider with a state-issued franchise begins offering service in a community. Many states allowing existing franchises the *option* to terminate the local franchise and replace it with a state franchise; some restrict this option to when wireline competition is present.

costs have no effect on the pricing behavior of the incumbent, as they charge the monopoly price as long as entry costs are non-zero.[12]

In contrast to the models in Sutton (1991), in the presence of asymmetric information about incumbent's costs (Milgrom and Roberts, 1982b) or switching costs (Klemperer 1987), the theoretical literature suggests incumbents may change pricing behavior in response to an increased *threat* of entry, even before actual entry occurs, as a means to try and deter entry.[13] In Milgrom and Roberts (1982b), incumbents may lower prices to signal lower costs; because lowering prices is a costly signal, it constitutes a credible signal to potential entrants. It should be noted that, as Milgrom and Roberts (1982a) show, cutting prices prior to actual entry is *not* a rational strategy to deter entry (or drive out rivals) in a world of complete information (and no switching costs). Potential entrants would realize that prices are being held artificially low in order to ward off entry but then once entry does take place, it is sub-game perfect for the incumbent to accommodate rather than to predate. In other words, as long as the established firm's pre-entry price does not affect post-entry demand or cost (and hence profit), limit pricing would not influence the potential entrant's decision to compete in the market. Thus the asymmetric information assumption in Milgrom and Roberts (1982b) is crucial to get rational ex-ante price cuts to deter entry. Asymmetric information could be relevant in our context due to cross-industry differences between Telco entrants and cable incumbents, or participation in industry R&D consortia (as discussed in Seamans 2013).[14]

Klemperer (1987) provides another justification for ex-ante price cuts – in his model lower prices helps to "lock-in" more consumers, who are harder for the entrant to attract because of switching costs. In our context, there may be switching costs for a few reasons. One, it was not unusual for companies to offer one and even two-year contracts, with an early termination fee. Two, even without early termination, switching could involve upfront installation fees. Also,

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[12]Also with product-differentiation, different models yield different predictions about the equilibrium number of firms in the market after reduction in entry costs. In the Hopenhayn (1992) class of industry equilibrium models, the effect of reduced sunk costs of entry on the mass of survivors is ambiguous (Balasubramanian and Sivadasan, 2009), but the cutoff productivity level increases and consequently, the equilibrium rate of entry (and exit) goes up.

[13]The idea of *ex-ante* limit pricing goes back to Bain (1956), who suggested that there may be a positive relationship between the initial price and the degree of entry - an incumbent firm may select lower than the profit maximizing price in order to deter entry by competitors.

[14]Seamans (2013) argues that variation in membership in CableLabs, an industry R&D consortia restricted to cable companies, is an important source of asymmetric information, and exploits that in his study. We checked and found that all three of the biggest overbuilders, and nine of the top 10 overbuilders in our data are *not* members of CableLabs. In fact, per its website, CableLabs has a fairly exclusive membership list of just 28 cable companies in the U.S., whereas our data includes about 1,200 cable operators.

lack of experience with the new service provider may yield a psychic switching cost due to potential concerns about service quality and customer support. In both Milgrom and Roberts (1982b) and Klemperer (1987), reducing prices is costly for the incumbent and so it follows that incumbents may be more likely to reduce prices the stronger the threat of entry.

To summarize, the effect of reduction in entry barriers on actual entry as well as pricing behavior of the incumbent is theoretically ambiguous with at least one model (Bertrand competition model in Sutton 1991) suggesting possibility of no effect on prices or actual entry. Other models suggest actual entry and consequent decline in prices (Cournot competition model in Sutton 1991) while others predict price declines without actual entry (Milgrom and Roberts 1982b, or Klemperer 1987). Given the varying predictions across different models, the cable reforms provide an interesting context to empirically investigate the effects of a reduction in entry barriers, specifically whether: (i) prices were affected, (ii) whether actual entry occurred, and (iii) whether there was evidence for ex-ante price reductions (as predicted by Milgrom and Roberts (1982b) or Klemperer (1987)).

4.4   Data and Summary Statistics

The data on cable television service offerings comes from seven years (2004 - 2010) of the Warren Publishing's Television and Cable Factbook. The Factbook data is the main source of cable TV system level characteristics used in most empirical studies of the industry (e.g. Rubinovitz 1993; Goolsbee and Petrin 2004; Della Vigna and Kaplan 2007; Seamans, 2013).[15] This rich data set includes information on monthly prices and installation fees, the number of consumers subscribing to the various tiers of service and a listing of the specific channels that are available on each tier of service for each individual cable system. It also classifies each system as either "Operating" or "Overbuild" where the latter term refers to a competing cable operator building a cable network system in an area already serviced by an existing cable operator. Finally, it also provides us the names of the communities which are served by each cable system.[16]

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[15]It is also used extensively by the FCC. Justifying the use of data from this source, an FCC report says: "Warren collects its data directly from cable television operators or individual cable systems to create a large database of cable industry information. Warren states that it is the only research entity that directly surveys every cable system at least once every year, providing the most complete source of cable data. In fact, the cable systems represented in Warren's database serve 96% of all subscribers nationwide." (FCC 2009, p. 198).

[16]For example, the cable system in Kalamazoo, Michigan serves not just the city of Kalamazoo but also towns in the adjoining area such as Alamo Township, Pavilion Township and Comstock Township. The Warren's data in-

In addition to data regarding the service offerings, we obtain controls for demographic variables that might affect either the demand for cable television service or the cost of providing cable television service or the likelihood of market entry by a competing service provider. We draw on Savage and Wirth (2005) to select relevant control variables; in particular, the demographic controls include per capita income (and its square), population density (and its square), local wage in the "Information" sector (NAICS code: 51)), age profile of the population, and growth in number of housing units. Data on these demographic variables is available at the county level. Additional information regarding data sources is provided in the Data Appendix.

Tables 4.2 and 4.3 present summary statistics. Table 4.2 gives a snapshot of the number of communities served for each year splitting the sample into the three different categories based on whether the state enacted franchising reforms, and when those reforms were enacted. Generally the boundaries of a cable community correspond to that of a municipality, though there may be multiple cable communities within a single municipality and vice versa. As Table 4.2 indicates, there are approximately 30,000 cable communities in the U.S. each year.[17]

[Table 4.2 about here.]

Summary statistics at a similar level of disaggregation on price of "Basic" and "Expanded Basic" tiers of service is presented in Table 4.3. As the fourth row of Table 4.3 suggests, the average price for "Basic" and "Expanded Basic" services stand at $18.98 and $42.02/month for the entire sample period, when averaged across all states. Also, a simple difference in means shows that the increase in average price for "Basic" service was 8% lower in the states that reformed between 2005 and 2008; these states move from an average price 46 cents above, to an average price 26 cents below, that for the non-reformed states. The average price for "Expanded Basic" service moves in tandem in both the states that underwent reform between 2005 and 2008 and the non-reformed states. These aggregate differences in means presage the results we find with more careful regression estimates below.

cludes the names of the principal community (in this case, Kalamazoo) as also the names of adjoining communities served by the cable system (in this case, Alamo Township, Pavilion Township, and Comstock Township).

[17]The number of communities in the Warren's data change from year to year, with a broad trend of decline in the number of communities. Warren's informed us that this was due to consolidation of communities. As part of our robustness checks (in section 4.5.4), we verify that baseline results hold for a balanced panel sub-sample of communities present throughout the seven year sample period.

To examine entry by Telcos, we also hand collected data on locations served by Verizon and AT&T; this data is described in more detail in Section 4.6 below.

4.5 Effect of Franchising Reforms on Prices

4.5.1 Empirical methodology

The staggered introduction of the reforms across different states allows us to adopt a difference-in-difference approach to estimating the effect of the franchising deregulation. As is standard, we compare the difference in outcomes after and before the intervention for communities affected by the deregulation (the "treated" group) to the same difference for unaffected communities (the "control" group) (Bertrand, Duflo, and Mullainathan, 2004). We look at the longest period possible from 2004 through 2010, based on availability of data. However, this period is also convenient for us since 2004 corresponds to a date when none of the states, barring four (Alaska, Hawaii, Rhode Island, and Vermont) had passed such state-wide legislation and by end-2008, an additional nineteen states had reformed their franchising process giving us at least two years after the enactment of reform to study its effects.

In order for us to make an apples-to-apples comparison across different tiers of service (and as is standard, e.g. in FCC reports on the industry), we look at two tiers of service individually - the first tier, "Basic" and the second tier, "Expanded Basic" - prices for which are reported separately in the Warren's data. Subscribers must purchase "Basic" service in order to subscribe to "Expanded Basic" service or to any other tier, or to buy premium programming such as HBO. To be clear, we define price of "Expanded Basic" as the total price charged to consumers who subscribe to "Expanded Basic" service, so this incorporates the price for the included "Basic" tier as well. In our data about 77 percent of cable subscribers take both "Basic" and "Expanded Basic" services; the remaining 23 percent take "Basic" service only. Because these two tiers of service make up well over two-thirds of the revenue derived from all tiers of TV programming (FCC 2006 p. 19, FCC 2009 p. 23), we focus on just these tiers for our analysis.

We use the following standard difference-in-differences specification (see e.g., Angrist and Pischke, 2009, Chapter 5), allowing for a different effect in the year of the reform:

$$p_{ijst} = \alpha + \beta_1.R^0_{st} + \beta_2.R_{st} + \beta_3.\mathbf{X_{it}} + \beta_4.\mathbf{Y_{jt}} + f_s + f_t + \epsilon_{ijst} \tag{4.1}$$

where p_{ijst} is the log of price for community i in state s at time t for the service offered by company j for a particular service tier. Price varies by service tier and is logged, following other research on pricing (e.g., Yamawaki, 2002; Seamans, 2013). R^0_{st} is a dummy for year-of-reform which is set to 1 if state s introduced reforms in year t. R_{st} is a post-reform dummy which is set to 1 if state s had reformed in a year prior to year t. f_s and f_t are state and year fixed effects.[18] The most parsimonious specification includes only these covariates in the analysis.

We then enrich our analysis by introducing more controls. We first introduce time-varying demographic controls for community i at time t in $\mathbf{X_{it}}$. These include variables that can affect the prices of cable service either by altering the demand from consumers (such as per capita income) or the costs to provide cable service (such as population density) or by altering the likelihood of market entry by competitors in that community (such as the growth rate of households) and local wage (for employees in the "Information" industry (NAICS code: 51)) which controls for shifts in cost due to wage inflation (Savage and Wirth, 2005). Thereafter, in $\mathbf{Y_{jt}}$, we also introduce variables corresponding to the characteristics of the cable company providing service in that community, viz. the number of subscribers it has at the national level, the share of subscribers it has at the state level, and its vertical affiliation with a content service provider. These variables are aimed at controlling for economies of scale, market power, and economies of scope in the provision of cable TV service.

Finally, in our strictest specification, to control for changes in composition or quality of service we introduce the log of number of channels offered on that tier of service as an additional control variable. In particular, this control ensures that any observed change in prices are not offset by changes in the number of channels included in the service.[19] ϵ_{ist} is the residual error term which accounts for all unobserved cost/demand shifters affecting prices. The identification assumption in the DID approach is that shifts in the unobserved variables is similar across states (so picked up by year effects) or fixed within states (so picked up by state fixed effects). We explore robustness to introducing more detailed fixed effects in Section 4.5.4

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[18]State level fixed effects generally yielded more conservative estimates and we use this as the baseline specification. Results using alternative levels of fixed effects, such as county fixed effects, or community fixed effects are presented in the robustness checks (See Section 4.5.4).

[19]We explore robustness to additional controls for quality of service in Section 4.5.4 below.

below.

Because the reforms are introduced at the state level, we cluster standard errors at the state level to account for inter-temporal correlation in the error terms (Bertrand, Duflo, and Mullainathan, 2004).[20]

4.5.2 Baseline Price Effects

Raw average price trends by reform status   Figures 4.2a and 4.2b present the annual average by reform status for the prices of "Basic" and "Expanded Basic" services over the period 2004 - 2010 respectively. Figure 4.2a shows that "Basic" prices in states that reformed between 2005 and 2008 shows a significantly flatter trend relative to the states that did not reform, so that the relative price levels change from a premium in reformed states (prior to the reform) to a discount (after the reform) relative to the non-reformed states, consistent with the mean price changes documented in Table 4.3. Also consistent with Table 4.3, Figure 4.2b shows no change in relative prices for "Expanded Basic" service from 2004 to 2010 between reformed and non-reformed states.[21] The results from the regression analysis discussed below allows us to control for a number of factors that could impact the simple means plotted in Figures 4.2a and 4.2b.

[Figure 4.2a about here.]

[Figure 4.2b about here.]

Regression results   Panel A of Table 4.4 presents the results from the regression runs quantifying the effect of reform on the monthly price of "Basic Service" and Panel B of Table 4.4 presents the same for "Expanded Basic". In column (1), there are no controls other than state and year fixed effects. Column (2) adds in the demographic controls, namely personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18 (as a control for the age structure of the population) and the local wage for NAICS code, 51 (Information). All of these controls are available at the county level and are introduced in log form. In addition, we also

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[20]We also clustered in both the state and time dimensions for our preferred specifications, following Petersen (2009). We found standard errors are *smaller* than what we obtain by clustering only at the state level. Given that, to be conservative, we report results clustering on just the cross-sectional (state) dimension.

[21]There is a somewhat surprising increase in prices in reformed states for one year in 2008, but this reverses in 2009, so by 2010 the prices are back at same levels.

include two additional controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the Designated Market Area (DMA) rank. DMA rank measures the strength of the local television market and affects demand by proxying for alternative sources of entertainment in the local system area (Crawford, 2000). Column (3) adds controls for the market structure, viz. the total number of subscribers the company has nationally, the share of state subscribers for the company providing service in that community and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Finally, column (4) adds controls for the quality of service, measured in terms of the log of number of channels. For brevity, Table 4.4 (and all subsequent tables) include only the coefficients of interest corresponding to price effects in the year of reform and in the years following the enactment of reform for the states that reformed between 2005 and 2008. Full results with coefficients on the control variables are available on request from the authors. The specifications used in Table 4.4 are used through the rest of the paper for all regressions which involve prices of either "Basic" or "Expanded Basic" service.

[Table 4.4 about here.]

Depending on which specification is used, we see that the monthly price of "Basic" service is lower by 5.5 to 6.8% in states which have reformed their franchising process as compared to states which have not. Given that the average price of "Basic" service in 2010 is about $20/ month (see Table 4.3), the percentage decline translates to a drop of $1.10 to $1.36 per month per subscriber or $13.20 to $16.32 per year per subscriber for "Basic" service.

In contrast to the robust evidence for decline in price for "Basic" service, we do not observe any statistically significant change in the price of "Expanded Basic" service in any specification in Panel B of Table 4.4. In columns (1) and (2), the coefficient on the post reform dummy is close to zero; in columns (3) and (4) after addition of demographic controls and log number of channels, the estimated coefficient turns positive but is not significant in any of the specifications.[22]

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[22]It could be noted in Table 4.4 that the number of observations for "Expanded Basic" prices is lower, reflecting availability of data in the Warren's database. We checked to see if sample differences were salient in explaining different results for "Expanded Basic", by estimating results for "Basic" service on the "Expanded Basic" sample. We found the results qualitatively similar, with coefficient magnitudes only slightly lower - a 4.8% decline in the column (4) specification compared to a 5.8% decline in the full sample. Thus differences in the sample do not appear to explain the differences in results for "Expanded Basic". Also, we checked and verified that the proportion of observations for "Expanded Basic" was very similar (66% and 63% respectively) for states that reformed between 2005 and 2008, and non-reformed states.

In the sections below, we check robustness of the finding of a post-reform decline in price of "Basic" service to a number of different concerns.

4.5.3   Checking for pre-existing trends

A fundamental concern in any DID analysis is the possibility that the observed mean effects are driven by differences in pre-existing trends. In particular, a declining trend in prices for "Basic" service in those states that reformed their franchising process between 2005 and 2008 could lead to the observed mean decline documented in Table 4.4. We address this concern in two ways.

First, in Table 4.5, we specifically examine the trend in price prior to the reforms. The regressions include only observations prior to the reforms, and we include dummies for two years and one year before the reform – all other pre-reform years are absorbed into the constant. The results reveal that, once we add in demographic controls (column (2)), there is no significant declining trend (as the coefficients on Reform year -1 and Reform year -2 are both relatively small and statistically insignificant). Once we add in additional controls for market structure and number of channels (column (4)), the magnitude of the coefficients Reform Year -2 and Reform year -1 are both very small and statistically insignificant. Thus relative to the period prior to two years before the reform, the price in the two years just before the reforms is within 0.2% in Reform year -2 and within 0.7% in reform year -1, which suggests no prior declining trend in prices of "Basic" service.

[Table 4.5 about here.]

We expand on this to estimate and plot coefficients on an index relative to year of reform, as suggested in Angrist and Pischke (2009, Chapter 5). In particular, we estimate the following regression:

$$p_{ijst} = \alpha + \sum_{k=-4}^{5} \lambda^k . R_{st}^k + \beta_3 . \mathbf{X_{it}} + \beta_4 . \mathbf{Y_{jt}} + f_s + f_t + \epsilon_{ist} \tag{4.2}$$

where all the variables are as defined in (4.1), and index R_{st}^k equals one if state s in year t is k years from the reform for states that reformed between 2005 and 2008; for example, for California which reformed in 2007, R_{st}^{-2} is equal to one for year 2005 and zero otherwise.[23]

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[23]For the states that reformed prior to 2005 we set the index to 20 so that they are effectively excluded from the

In Figure 4.3, we plot the coefficients λ^k for $k = -3$ to $k = +4$, to observe trends before and after the year of reform, conditioning on demographic controls, market structure variables, and the number of channels. As the figure suggests, there were no marked pre-existing trends prior to the enactment of these reforms; in fact the trend over the period -3 to -1 remains remarkably flat. Figure 4.3 also shows there is beginning of a decline in prices in the year of the reform, and then within 3 years of the passage of these reforms, the price of "Basic" service is significantly less than their levels prior to the enactment of the reforms and it continues to be lower subsequently.

[Figure 4.3 about here.]

To see if prior trends could explain the lack of results for "Expanded Basic" service tier, we undertook the same test as in Table 4.5 for this tier. The results, presented in Appendix Table A4, show that there were no differential trends in "Expanded Basic" price in the pre-reform period in reformed states relative to non-reformed state. In fact, the differential effects are very small, less than 1.6% across all the different specifications.

4.5.4 Decline in prices for "Basic" service: Robustness checks

In this section, we check robustness of the finding that prices declined for "Basic" service to a number of alternative tests. Results for tests RC1 to RC7 are summarized in Table 4.6; results for the remaining tests are presented in Appendix tables.

RC1: Alternative controls for service quality The observed reduction in price is economically meaningful only if it was not offset by reduction in the quality of the service offering. In column (4) of Table 4.4, our specification includes the log of number of channels, which controls for adjustments in terms of addition or deletion of channels. However, cable operators could respond by dropping more popular channels and adding less popular ones, so that the quality of offering declines while the total number of channels stays the same (e.g., Crawford (2000) finds that cable operators changed composition of offerings in response to the 1992 Cable Act). To address this concern, in rows 2, 3 and 4 of Table 4.6, we include alternative measures to control for changes in composition of "Basic" service. In row 2, we include log of the number of channels that are distributed nationally to systems via satellite (also known

range of indices plotted in the figure.

as satellite channels), in row 3 we include log of the number of channels that are within the top 10 most popular channels, and finally in row 4 we include log of the number of channels that are within the top 20 most popular channels.[24] We find the baseline results to be robust; in fact in row 3 and 4, including controls for the top 10 and top 20 channels yields stronger results, suggesting that the composition changes in programming quality in reformed states may have reinforced, rather than offset, the decline in prices.

RC2: Including Installation Charges The economic significance of the observed decline in prices, particularly for new consumers switching into cable, could be affected by concurrent changes to installation fees. To check whether changes to installation fee offset the decline in monthly price, we look at the *net price* of receiving "Basic" service in which we amortize the installation fees over a 12-month period and add that to the monthly price. Results for net price of receiving service are presented in row 5 of Table 4.6. While the price drop reduces slightly, it remains statistically significant and ranges from 4.6 to 5.6%. It appears that installation fees increased slightly in reformed states, but note that because the average cable consumer is likely to have her installation for a period that is longer than 12 months, amortizing installation fees over a 12-month period is likely to understate the decline in prices brought about by the reforms in these specifications.

RC3: Long Differences If there is some lag in the response to the reforms, or if incumbents changed behavior prior to the reforms once passage of the law became more certain, then the DID regressions could give smaller estimates than the true long-run effects of the reforms. To get at the more long-run effects, we use a long difference approach (similar to Donohue and Levitt, 2001). In particular, we rerun baseline regressions including only data for the starting and ending years of the sample, i.e. for 2004 and 2010. As expected (and consistent with the pattern in Figure 4.3), the estimates in row 6 of Table 4.6 are uniformly larger, ranging between 7.5 - 9.8% suggesting that the longer run impact of these reforms may be larger than the about 6% estimate we find in our baseline analysis.

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[24]Satellite channels include some of the most easily recognizable names in cable, such as MTV, CNN, and ESPN. The list of most popular channels is drawn from Crawford and Yurukoglu (2012) which in turn, is based on ratings from Nielsen Media. For Nielsen Media, the ranking of channels is based on the national average cumulative rating for that channel during the fourth quarter of 2006; given that we have data from 2004 to 2010, this corresponds to roughly the mid-point of our sample period.

RC4: Balanced panel The results thus far have been estimated on all available data. Because of consolidation of cable systems over the years, there are fewer cable systems in the later years of the sample, so that the panel in the baseline analysis is unbalanced. To ensure results are not driven by compositional effects, we examine robustness to using a balanced panel of communities that were present in all seven years of the data. The estimates (in row 7 of Table 4.6) yield somewhat stronger results (a decline of about 7% in column (4)) relative to the baseline.

RC5: Alternative fixed effects The baseline regression include state fixed effects in every regression, as the effects of state-level reforms are identified off cross-state and time variations, and hence the main omitted variables that concern us are state-level variables. Nevertheless, we explore alternative specifications in which we introduce county fixed effects (in row 8) or cable system fixed effects (identical to principal community fixed effects, in row 9) or community level fixed effects (in row 10 of Table 4.6). Across all these alternative specifications, we continue to see a drop in the price of "Basic" service by approximately 6%.

RC6: Principal communities only The Warren's data includes information on all communities served by a cable head-end, with the location of the cable head-end designated as the principal community. Because in non-reformed states and in reformed states prior to the reform the local franchising regulations operate at the community level, our baseline analysis uses observations on all communities. In this robustness check, we exclude all these communities that are served by the same cable system and limit our analysis only to the principal communities where the cable head-ends are located. Even though this causes our sample size to drop to about 20% of the original sample size, our results hold with somewhat smaller magnitudes with this sub-sample as well (in row 11 of Table 4.6).

RC7: Alternative control groups In the baseline analysis, we have included the four states which reformed prior to 2005 (Alaska, Hawaii, Rhode Island, and Vermont). Although we allow the effects for these states to be different in the post reform period in the baseline analysis, we explore the robustness of our results to excluding these four states altogether (in row 12 of Table 4.6), and find the estimates similar to the baseline.

Further, in the baseline, the control group of non-reformed states includes the states of Alabama, Utah, and Virginia which have laws on their books prohibiting municipal electric

utilities (MEUs) from cross-subsidizing their entry into the cable TV business. As Seamans (2012) shows, incumbent cable systems located in such states are less likely to upgrade equipment even though they may face entry from a municipal entrant because the latter are legally prevented from cross-subsidizing entry into the cable TV business. In order to preclude the results from being affected by these states, we exclude these three states from the analysis (in row 13 of Table 4.6), and find that the results are largely unchanged.

Finally, the control group of non-reformed states includes the states of Alabama, Kentucky, Minnesota, New Hampshire, and Oklahoma which have "level playing field" laws on their books. As Hazlett(2007) argues, these level playing field laws impose far harsher costs on competitors than the incumbent and have the effect of forestalling competitive entry into the local cable market. Because we expect the existence of such laws on the books to impact the price of cable service in such states, we examine the robustness of the results to the exclusion of these states from the control group (in row 14 of Table 4.6) and find the results to be similar to baseline.

RC8: Falsification test using Electricity prices It could be the case that the price declines for "Basic" service is driven by relatively larger price declines in these states due to other reasons, or because of increases in prices in the non-reformed states (driven by say increases in income or changes in business climate in ways not captured by our demographic controls). To address this concern, we conduct a falsification test in which we consider the change in the average electricity prices paid by residential, commercial, and industrial consumers over the same period of time from 2004 to 2010 across all 50 states. If other shocks were negatively impacting prices in reformed states, or positively impacting prices in non-reformed states, then that should also be reflected in the electricity prices paid by residential, commercial, and industrial consumers. As the results in Appendix Table A2 show, we find no evidence for a relative decline for either residential, commercial or industrial electricity prices. In fact, the coefficient on the Post Reform dummy is *positive*, very small (less than 1%) and statistically indistinguishable from zero in all the four specifications of this table.

RC9: Effect on number of subscribers In Appendix Table A3, we check if observed price declines led to increases in the number of subscribers for "Basic" service. The results suggest an increase in the number of consumers who subscribe to just the "Basic" tier which is

consistent with the observed price decline. However, the effect is not statistically significant. There appears to be a greater magnitude of increase (7.4%) in column (3), but this appears to be explained partially by changes to number of channels, as the magnitude declines to 2.8% in column (4). Also, it appears that during the same time, despite no evidence of a decline in price, there is a statistically insignificant increase in subscribers to "Expanded Basic" service as well.

Robustness checks for price of "Expanded Basic" service   To see if the finding of a null effect on the price of "Expanded Basic" was robust, we also undertake all of the checks RC1 to RC7 for "Expanded Basic". Results are presented in Appendix Table A5; we find the null effect result for "Expanded Basic" remarkably robust across all the different checks.

[Table 4.6 about here.]

4.6   Role of Entry

The stated objective of the reforms allowing for franchising at the state level was to increase the incidence of competition in the localized cable television markets and bring lower prices to consumers.[25] However, as discussed in section 4.3, the theoretical effect of lowering of entry barriers in this context is ambiguous, given that entry still required significant upfront (sunk) investments, especially for the "last-mile" connections to residences (e.g., see Wagter (2010)). In particular, in a model with Bertrand price competition in the second stage (Sutton, 1991, Chapter 2), rational firms would not enter as long as upfront costs are greater than zero, as they anticipate zero profits in the post-entry stage. Thus, whether actual entry occurred is an interesting empirical question.

In this section we examine two questions. First, in Section 4.6.1, we examine whether the reforms spurred greater entry, especially by Telcos, who lobbied for the passage of the reforms in many states. (e.g., see news articles by Sher (2008) and Haugsted (2006)). Second, in section 4.6.2, we examine whether observed baseline price results were driven by post-entry price declines, or whether there is evidence for ex-ante price declines even without entry (as predicted by Milgrom and Roberts (1982b) or Klemperer (1987)).

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[25]E.g., Governor Jon Corzine, the then Governor of New Jersey's statement when signing the relevant bill stated: "The power of competition can improve quality and lower prices. Under the legislation signed today authorizing new cable franchises, New Jersey cable television customers will soon see the benefits of competition." (U.S. Fed News, August 2006)

One hurdle to studying entry is that data for Telcos offering TV services is not included in the Warren's database. Therefore, data on these companies had to be hand-collected separately. We focused on the two major players – AT&T and Verizon – as these two together accounted for over 90% of the marketshare of Telcos in the Cable TV market 2010 according to the Frost & Sullivan online database.[26] There are two limitations of this data. First, we are able to reliably assess the presence of AT&T and Verizon only at the county level and not at the individual community level. Thus, to the extent that some communities within a given county were not served by these Telcos, our data overstates entry at the community level. However, because this measurement error is unlikely to be correlated with reform status, we do not expect this to bias our estimates of the effect of reforms on entry. Second, we were only able to obtain data on presence of AT&T and Verizon in 2010. Because we know that TV service was launched by Verizon in late 2005 (Reardon 2005b), and AT&T in early 2006 (Reardon 2006), we can bound the entry dates as being between 2005 and 2010. In our analyses, we check robustness to assuming alternative entry dates for these Telcos.

4.6.1 Reforms' effect on entry

To find out whether the reforms lead to greater entry by cable companies or Telcos, in Table 4.7a we examine the fraction of communities with either form of entry in 2004 and 2010. As the last column indicates, there was significantly more entry in the states that reformed between 2005 and 2008 (39.1%) relative to the non-reformed states (25.8%). Columns (7) and (8) show that bulk of the entry, as well as the source of the difference between reformed and non-reformed states come from Telcos. Per column (7), there was only modest new entry by cable operators (2.3% overall) between 2004 and 2010, and the difference between recently reformed states and non-reformed states while positive was only modest (2.4% versus 2.1%). Thus, consistent with the anecdotal evidence on lobbying behavior of Telcos (e.g., Reardon 2005a, Haugsted 2006, Sura 2006, Sher 2008), the reforms appear to have facilitated greater entry by Telcos in reformed states.

[Table 4.7a about here.]

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[26]Specifically, at the end of 2010 Verizon FiOS had 3.203 million customers, AT&T U-verse had 2.504 million customers and all other Telcos combined had 0.611 million customers. That gives Verizon and AT&T a combined market share in 2010 among Telcos of 90.3%.

Although these summary differences are strongly suggestive, differential entry rates could be due to differences in trends for demographic or market structure characteristics. To control for these factors, in Table 4.7b we examine a linear propensity model of entry by either a cable overbuilder or by a Telco. As noted above, we do not have data on precisely when either AT&T or Verizon entered a given market. In the absence of such information, we assume in the baseline case that all of the Telco presence in 2010 occurred in 2008. Subsequently, we also consider alternative scenarios assuming all entry occurred in 2006 (Alternative 1), 2007 (Alternative 2), or 2009 (Alternative 3). Across all scenarios, we find significantly higher difference-in-differences entry rate in reformed states relative to non-reformed states. The magnitude of the effect ranges from 7.95% (under alternative 3) to 13.8% (in alternative 2).

[Table 4.7b about here.]

Thus, based on the evidence in Tables 4.7a and 4.7b, we conclude that there was indeed more entry in reformed states following the reforms.

4.6.2 Were "Basic" service price declines due to *actual* or *increased threat* of entry?

The results in Section 4.6.1 confirm that the reforms resulted in significantly greater amount of entry in reformed states, with about 40% of communities experiencing entry by 2010 compared to 26.5% for the non-reformed states (Column (6) of Table 4.7a).

As discussed in Section 4.3, price declines following reductions in entry barriers could arise in theory for two reasons. One, in the monopoly or Cournot models discussed in Sutton (1991, Ch. 2), there would be price declines due to increased competition in the post-entry stage. On the other hand, in models by Milgrom and Roberts (1982b) or Klemperer (1987), the reduction in entry barriers could lead to ex-ante price cuts by incumbents, as a means to deter entry (by signaling lower costs in the former model and by locking in potential customers in the presence of switching costs in the latter).

As has been well documented, prices are indeed lower by 17% in communities which have a competitive wireline overbuild (FCC 2009, p. 22), consistent with greater price competition post-entry. In this context, it is interesting to examine whether our baseline results of price decline for "Basic" service are driven by larger average prices declines in reformed states

caused by greater entry, or because of ex-ante price reductions by incumbents in response to a greater threat of entry.

We address this question in Table 4.8. In columns (1) and (2), we drop all observations on cable overbuilds; thus if these new entrants were particularly aggressive in terms of price cuts, the exclusion of this sub-sample helps isolate the behavior of incumbent cable operators.[27] We find the results in columns (1) and (2) similar to the baseline results. Although this is not surprising given that cable overbuilds constitute only 3.5% of the sample in 2010 (see Column (4) in Table 4.7a), nevertheless it suggests that *incumbent* pricing behavior was indeed affected by the reforms.

[Table 4.8 about here.]

We examine the more interesting question of whether there was price reduction by incumbents prior to/without actual entry in Columns (3), (4), (5) and (6). In columns (3) and (4), we exclude all communities that had more than one cable (i.e., wireline) service provider. In columns (5) and (6) we exclude communities with more than one service provider, whether those were cable (i.e., wireline) companies or either of the two major Telcos.[28] We find that effects are in fact stronger when we focus only on communities without entry; the coefficient estimate suggests a decline of 6.57% in column (6) relative to the baseline effect of 5.78% (in column (4) of Table 4.4). Note that we exclude communities with entry from both reformed and non-reformed states; thus the results in columns (3) to (6) suggest that incumbents in reformed states reduced prices more than incumbents in non-reformed states following a decline in barriers to entry.

Finally in columns (7) and (8) we restrict the sample to communities that experienced actual entry; the results here suggest greater price declines in reformed states, but the magnitudes are smaller and statistically insignificant. This is as could be expected – there is no reason to expect that the effect of actual entry would be different in the two regimes. In fact, this result is reassuring, in the sense that if omitted shocks were causing the strong declines in reformed states, we could have expected to see equal sized effects even conditional on entry.

Overall, the evidence in Table 4.8 suggests strong ex-ante responses by incumbents for "Basic" service, consistent with the models of Milgrom and Roberts (1982b) and Klemperer (1987), and contradicting the perfect information model of Milgrom and Roberts (1982a).

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[27]Note that our data do not include price observations for Telco entrants.

[28]We exclude from the analysis all communities where there was a Telco entry by 2010.

4.7 Controlling for Correlated Shocks: A Sample-Split (Triple-Difference) Test

Although the standard DID approach we use controls for state-level fixed omitted variables and our tests in Section 4.5.3 suggest no bias from pre-existing trends, the DID results could still be affected by unobserved *time varying* state level shocks correlated with reform status. For example, if some unobserved demographic or market trend impacted cable pricing and was different between the reformed and non-reformed states, this could potentially bias our analysis.

One potential source of bias is entry by Direct Broadcast Service ("DBS") providers. While we do not have direct information on DBS providers (they are not covered in the Warren's data), other secondary sources suggest no differential trend in DBS entry across states. As Crawford (2008) notes, by 2003, before the start of our sample in 2004, DBS availability was pervasive and prices were generally similar across markets. The Annual Reports of DISH Network Corporation and the DIRECTV Group confirms their availability across the entire continental U.S. For example, the Annual Report for 2000 for DISH Network states that: "As of December 31, 2000, approximately 5.26 million households subscribed to DISH Network programming services. We now have six DBS satellites in orbit which enable us to offer over 500 video and audio channels, together with data services and high definition and interactive TV services, to consumers across the continental United States through the use of a small satellite dish."(p. 1) Likewise the 2001 Annual Report for DISH Network states that: "DIRECTV has launched six high powered DBS satellites and has 46 DBS frequencies that are capable of full coverage of the continental United States."(p. 14) Given the pervasive availability of DBS over our sample period, 2004–2010, we expect included time dummies to control for potential effects of increased DBS penetration in our analysis, as we do not expect systematic differences in the spread of DBS between reformed and non-reformed states.

Nevertheless, we cannot completely rule out incidental differences in trends of DBS penetration across states, or differential trends in other relevant variables unobserved by us. To try to address this concern, we consider exploiting within-state differences in the strength of entry threats.[29] The Milgrom-Roberts/Klemperer models would suggest that costly entry de-

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[29]We note that our approach, though developed independently, is similar to that used in contemporaneous work by Seamans(2013).

terrence strategies are more valuable in locations where the threat of entry is the strongest, so that incumbent cable companies are more likely to respond by cutting prices in such communities.

Specifically, we examine responses for incumbents operating in a county where one of the largest overbuilders is already in operation. We expect the largest price drops to occur in those communities that are geographically "close" to a community already served by one of the top overbuilders. This approach rests on the intuitive assumption that it is easiest for cable overbuilders to expand into geographically proximate locations (Seamans 2013). Building off of an existing footprint allows the overbuilder to spread the fixed costs of building a video delivery platform and take advantage of economies of scale in customer service, maintenance, and repair. There is also anecdotal evidence supporting this assumption. For example, in its 2005 Annual Report, RCN, one of the top three overbuilders in the country, describes its strategy as: "RCN will continue to seek opportunities to increase its network footprint within and adjacent to its existing market clusters." We can thus say that communities located near an aggressive overbuilder are more likely to experience entry, and cable franchising deregulations that allow for a state-wide franchise make such entry even more likely to occur.

To operationalize this idea of largest or aggressive overbuilders, we look at the top 10 companies with the highest number of subscribers belonging to overbuilt communities. In classifying companies as overbuilders or not, we set a threshold that at least 30% of a company's subscribers must reside in overbuilt communities. The threshold is chosen in order to exclude large incumbent service providers such as Comcast Communications Inc. and Time Warner Cable for whom less than 3% of their subscribers reside in overbuilt communities. The final list of companies including the top 10 overbuilders is comprised of the following companies: WideOpenWest (WOW) LLC, RCN Corp., Knology Inc., Block Communications Inc., Armstrong Group of Companies, WaveDivision Holdings LLC, Tacoma Public Utilities, Millennium Digital Media LLC, Broadstripe, and Qwest. Of these, the first three and Armstrong Group of Companies belong to the list of top 25 cable companies in the U.S.. Cumulatively these top ten overbuilders account for 44% of all subscribers who reside in overbuilt communities in our data.

We label any county where one of the top 10 overbuilders is in operation as "County has a top 10 overbuilder." Note that not all of the cable systems operated by these so-called overbuilders are overbuilds; indeed in many cases, the cable systems run by these companies

are the only cable system in operation in a given community. However because at least 30% of their subscribers reside in overbuilt communities, these companies are likely to have a reputation within the industry as overbuilders whether or not a particular cable system is an overbuild.

A pictorial representation of our approach towards analyzing heterogeneity of impact across communities is provided in Appendix Figure A1. Our approach examines the sample of communities Y_1 and Y_2 in the reformed and non-reformed states before and after reform, which yields a difference-in-differences (DID) estimate for communities neighboring a top 10 overbuilder (communities in counties of type A). If incumbents are indeed responding to the greater threat of entry, we expect the DID estimate for this sample to be greater than the DID effect for the sample of communities in counties of type B where the threat of entry is lower.

The sample-split regression results for "Basic" service are presented in columns (1) through (3) of Table 4.9 and those for "Expanded Basic" service are presented in columns (4) through (6). To conserve space, the regression results for only the most complete specification that includes controls for demographics, market structure, and the number of channels are reported.

[Table 4.9 about here.]

Comparing results in columns (1) and (2), we find that the magnitude of the price decline is larger by about 11.85% in counties where a top 10 overbuilder is present. To focus exclusively on incumbent responses to a *threat* of entry (rather than actual entry), in Column (3) we exclude communities where there is an actual overbuild. This yields lower estimates, but the decline is still about *twice* as large as for communities in counties where a top 10 overbuilder is absent.

We repeat the analysis for "Expanded Basic" service in Columns (4) to (6). These results also consistent with what we find for "Basic" service, again reaffirming the importance of a higher threat of entry to the reaction by incumbents. While individual estimates are not statistically significant, we find that there is a notable difference in the magnitude and signs of the coefficients. In particular, there is a 9.2% decline in price (in Column (5)) for high threat counties, while there is a 3.6% increase (in Column (4)) for counties in which top 10 overbuilders are absent. Excluding those communities which have actually experienced entry does not change the results significantly, as we find a 10.2% decline in this sample as well.

To the extent that within state shocks correlated with reforms do not vary across counties

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with and without a top 10 overbuilder, these results confirm that there was indeed a significant effect of the reform on incumbent pricing behavior. In particular, because DBS entry is only limited by geographic factors such as terrain and elevation (Goolsbee and Petrin 2004) that is unlikely to vary systematically between top 10 overbuilder counties and other counties, the results here suggest that incumbents responded specifically to the greater threat of entry in reformed states.

As a check on the economic meaningfulness of these differential declines in price for communities with specifically higher threat of entry, we also undertook a similar sample-split test for number of consumers subscribing to the two tiers of service. Results presented in Appendix Table A6 show substantial responses to the higher price declines in high-threat counties. For "Basic" service, quantity responded by about 101 log points (in column (3) of Table A6) to the 10.6% decline in prices (in Column (3) of Table 4.9). Number of consumers subscribing to "Expanded Basic" also increased by 77 log points (in column (6) of Table A6) to a 10.2% decline in prices (in Column (6) of Table 4.9).

Taken together, the results from Table 4.9 and Table A6 suggest that incumbents cut prices significantly (and signed up more consumers) in counties where the threat of entry was relatively higher, consistent with the reforms increasing the threat of entry, and consistent with ex-ante price cutting predicted by models of the type in Milgrom and Roberts (1982b) and Klemperer (1987).

4.8   Discussion of results and conclusion

We investigate the effect of state-level cable franchising reforms. We find difference-in-differences decline in the price of "Basic" service of approximately 5.5 to 6.8% following the reforms. We also find evidence for significantly more entry, particularly by telecom companies, following the reforms. We find that DID price declines occurred even in counties which did not experience entry. Further, we find that the magnitude of decline in prices was highest in counties with a greater threat of entry (identified using presence of a top 10 overbuilder in the county). The price reduction could be thought of as a direct 5.5% to 6.8% gain in consumer surplus relative to expenditure for "Basic" only customers; a simple back of the envelope estimation yields implied aggregate dollar gains in consumer welfare for "Basic" only consumers of about

$5.72 million per month.[30]

While the magnitude of the average effect of the reforms is modest, our findings are note-worthy for a number of reasons. One, our robust finding of decline in price for "Basic" service contrasts to the results in Crawford (2000), who finds no effect on price (or consumer welfare through improved quality) of the federal Cable Act of 1992 (which had the stated objective of controlling cable price increases). Thus, these state-level reforms intended to reduce entry barriers appear to have been more effective in controlling prices than the direct price regu-lation of the Cable Act of 1992. Two, the decline in price is notably larger in communities facing a higher threat of entry (where the upfront sunk costs for potential entrants is lower). This suggests that the impact of the reforms, which reduced the hassle and costs associated with negotiating local franchises, may have been moderated by the need for significant up-front sunk costs for entrants. Finally, we provide evidence for the effect of a *threat* of entry on incumbent behavior, which as Goolsbee and Syverson (2008) note has received much less attention in the empirical literature, relative to theoretical and policy debate on the topic. Specifically, our findings are consistent with models of limit pricing by incumbents (e.g., Mil-grom and Roberts (1982b) and Klemperer (1987)), and contrary to models that predict no change in incumbent behavior absent actual entry (e.g., Milgrom and Roberts 1982a, or the Bertrand Competition model in Sutton, 1991).

Our finding that there was no DID decline in the price of "Expanded Basic" service on average is somewhat puzzling, especially given that subscription levels for this tier is sig-nificantly higher than "Basic".[31] We offer two speculative explanations based on alternative theories of ex-ante price reductions. One, as in the Milgrom and Roberts (1982b) model, price reductions serve as signals of underlying marginal costs. It could be the case that "Basic" ca-ble prices provided a sufficiently credible signal of true costs for incumbents, and so they only responded to the increased threat of entry following reforms by cutting prices for the "Basic" tier. Two, it is possible that the goal of cutting prices was to lock-in consumers as in Klem-perer (1987). As discussed earlier, it is plausible that the price declines were accompanied by fixed-term contracts with early termination penalties discouraging customers from switching.

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[30]Average monthly price for "Basic" service across all states for 2010 is $19.95 (from the summary statistics in Table 4.3). The number of consumers who subscribe only to "Basic" service in 2010 in the nineteen states that have reformed is 4,959,681. Therefore, neglecting any change in the number of subscribers following the enactment of reform and using the 5.78% price decline in column (4) of Table 4.4, the approximate aggregate welfare gain = $\Delta P * Q = (0.0578 * \$19.95) * 4,959,681 = \$5.72 \ million/month$.

[31]On average across communities, "Basic" only consumers constitute about 23% of the subscriber base in our data.

Because customers of the cheaper "Basic" service tier may be more likely to be price sensitive, they may also be the ones more likely to switch if new entrants were to offer lower prices. Thus, it could be rational for incumbent cable companies to offer lower prices for the product segment with more price sensitive customers.[32] Data limitations prevent us from further analysis that could have shed more light on this finding. For example, information on contract terms could have allowed us to explore whether incumbents tried to lock-in consumers using longer contract periods and/or larger early termination fees.

Cable systems are among one of relatively few services where U.S. consumers have very limited choices. Our finding that there is significantly more entry in the reformed states suggests that local franchising process which vests authority in the local franchising authorities does indeed play a role in limiting competition. Our results suggest that adoption of the franchising deregulation could boost entry and improve welfare in the remaining 27 states that are yet to adopt these reforms.

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[32]Comparing results for "Basic" and "Expanded Basic" prices and quantities in Table 4.9 and Table A6 does provide suggestive evidence that "Basic" service has more elastic demand. In particular, in column (3) of Table 4.9 a 10.6% decline in price yields a quantity response of 101 log points (in column (3) of Table A6), while for "Expanded Basic" in column (6) of Table 4.9, a similar price decline of 10.2% yields a lower 77 log points increase in quantity (in column (6) of Table A6).

Figure 4.1: Progression of reforms across the 50 states
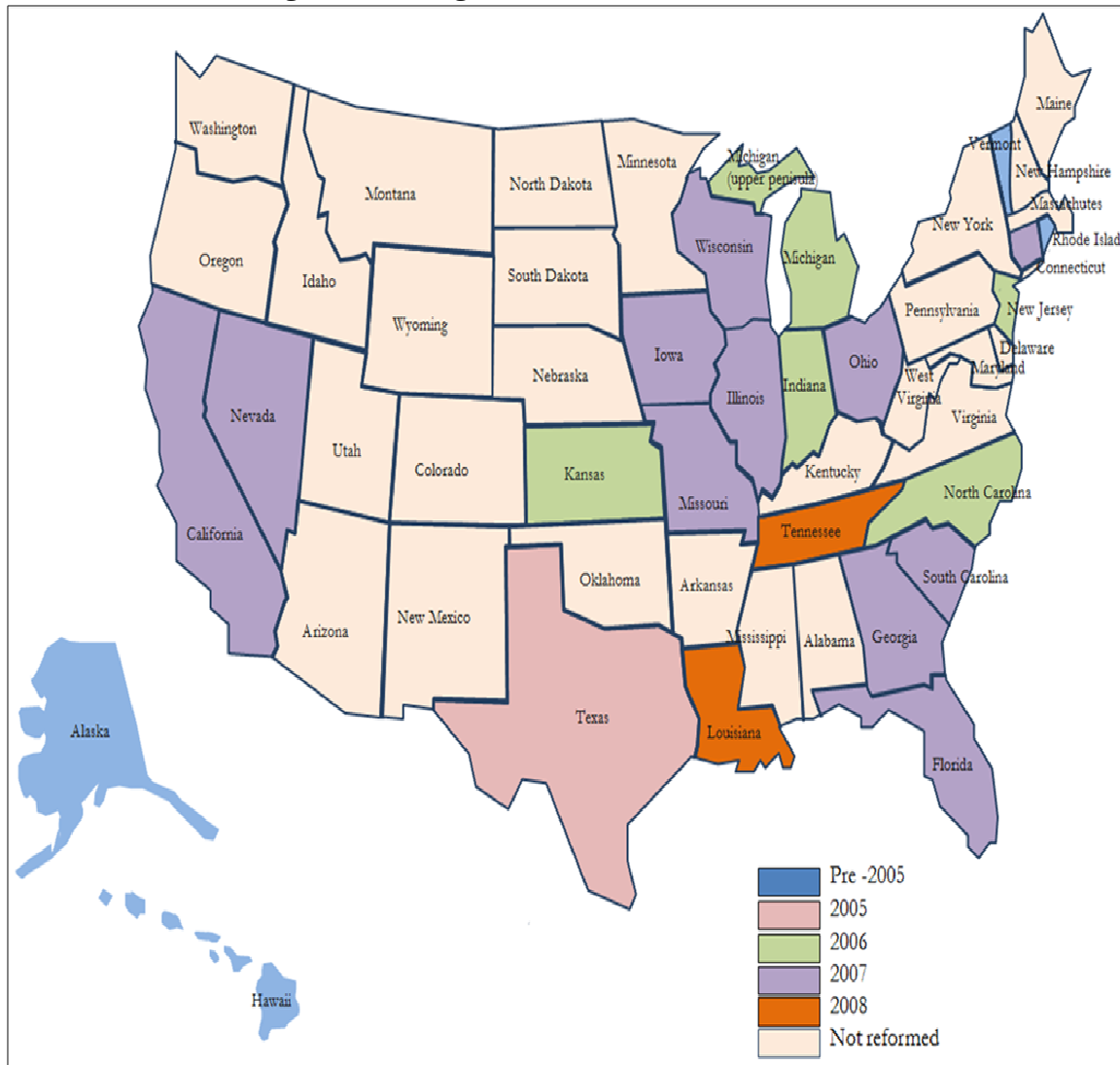
Notes: Based on Spurgin (2008). We recoded Virginia (which had been listed in the report as reformed) as not reformed because our research shows that the state did not pass legislation allowing for a state-wide franchise. Also, we confirmed passage of the law for Louisiana, where the legislation was pending per Spurgin (2008).

Figure 4.2a: Trend in price of "Basic" service
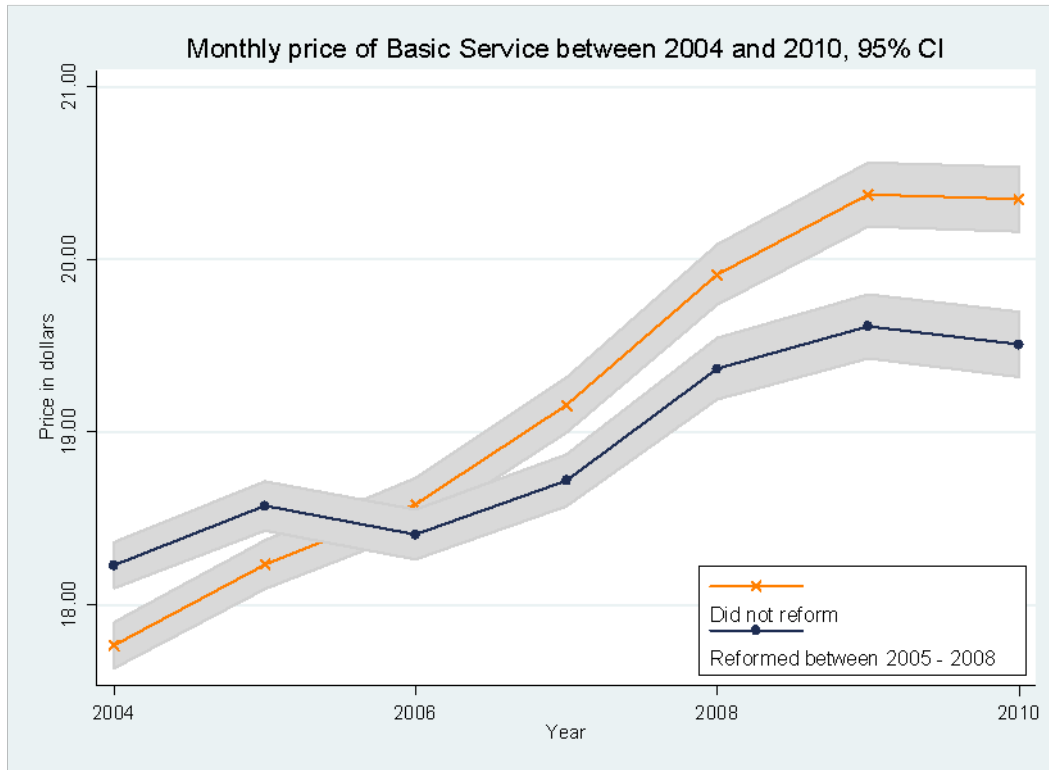
Notes: Data on cable prices is taken from successive issues of Warren's TV Factbook (2004 – 2010). Price data is available at the level of each individual community. Generally the boundaries of a cable community correspond to that of a municipality, though there may be multiple cable communities within a single municipality and vice versa. There are approximately 30,000 cable communities in the U.S. The monthly price data used in this graph pertains to that of "Basic" service. This price excludes all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. "Basic" service is the level of cable television service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. The list of states which reformed their franchising process between 2005 and 2008 along with the list of states which did not reform is provided in Table 4.1, and shown graphically in Figure 4.1.

Figure 4.2b: Trend in price of "Expanded Basic" service
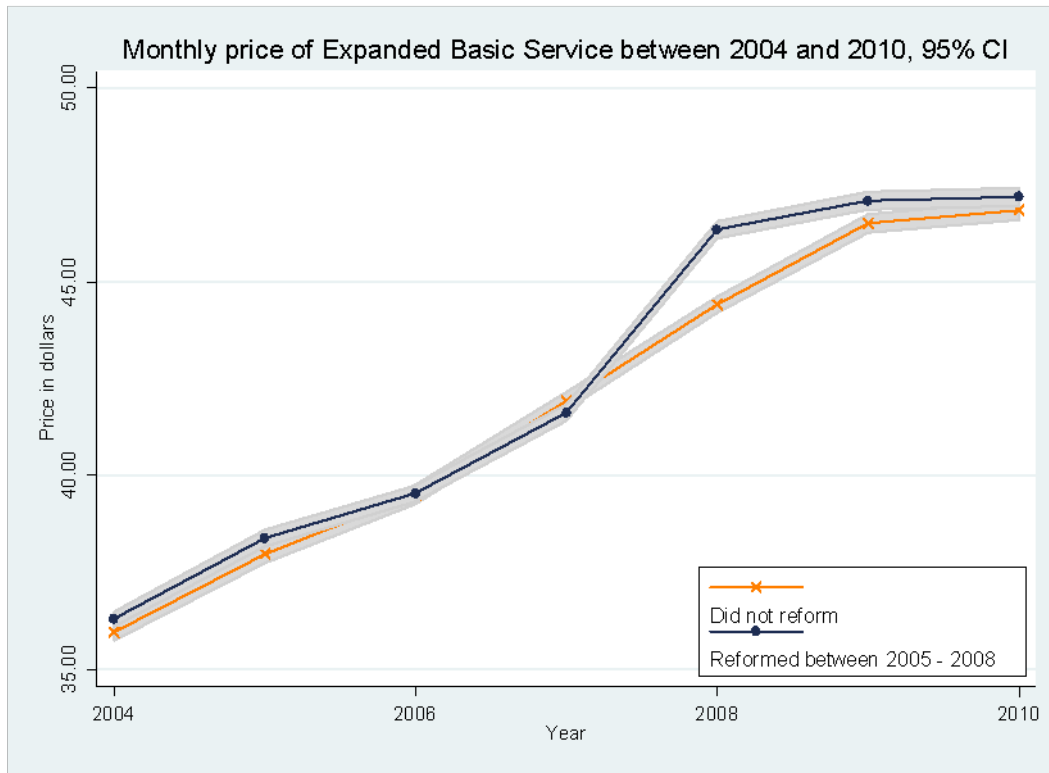
Notes: Data on cable prices is taken from successive issues of Warren's TV Factbook (2004 – 2010). Price data is available at the level of each individual community. Generally the boundaries of a cable community correspond to that of a municipality, though there may be multiple cable communities within a single municipality and vice versa. There are approximately 30,000 cable communities in the U.S. The monthly price data used in this graph pertains to that of "Expanded Basic" service. This price excludes all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. "Expanded Basic" service offers additional video channels on one or more service tiers and includes most of the better-known national cable television networks. The list of states which reformed their franchising process between 2005 and 2008 along with the list of states which did not reform is provided in Table 4.1, and shown graphically in Figure 4.1.

Figure 4.3: Trend in price of "Basic" service in reformed states, around reform year
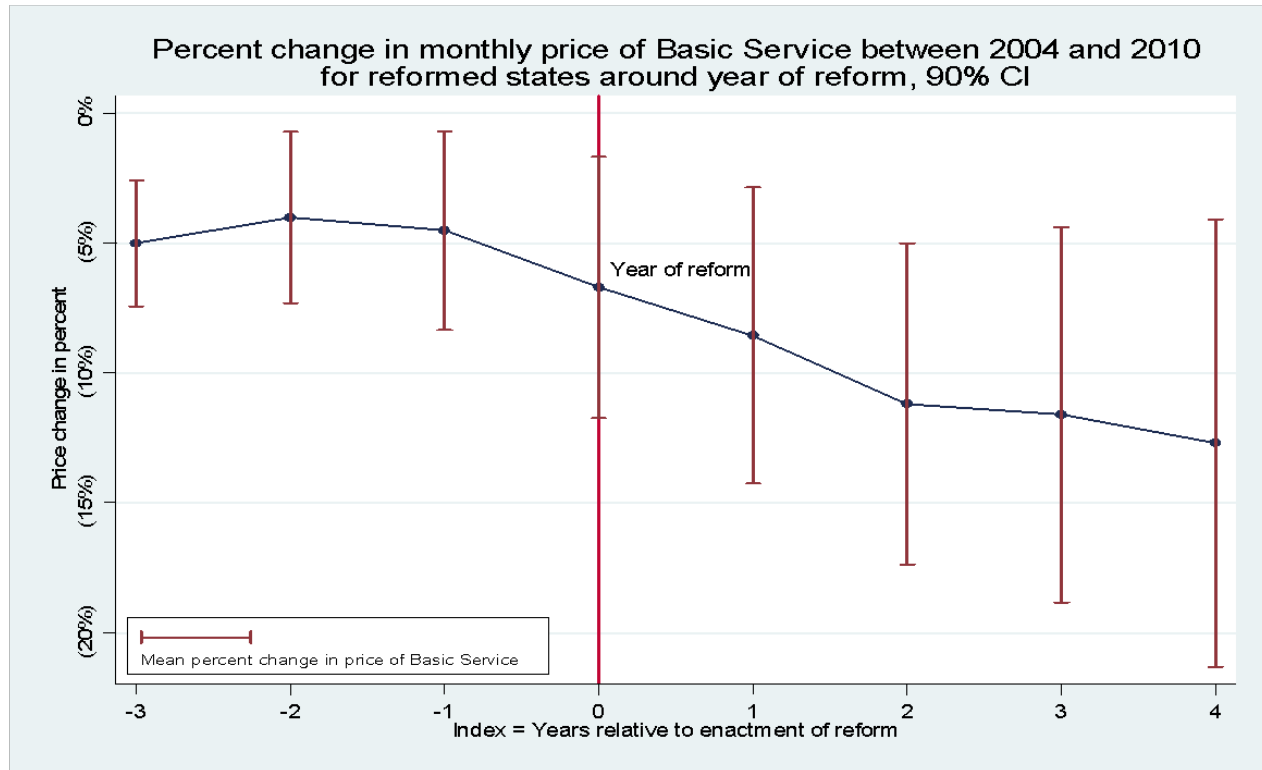
Notes: The percent numbers on the vertical axis are log points relative to prices for the year - 4 (i.e., four years prior to reform). The underlying regression includes all controls included in column (4) of Table 4.4 including state and year fixed effects, controls for market structure, demographic controls and control for number of channels. See notes to Table 4.4 for details on individual control variables. Data on cable prices is taken from successive issues of Warren's TV Factbook (2004 – 2010). Data plotted are for 3 years prior to the enactment of reform and for 4 years after the enactment of reform. This graph is based on all communities, including states which reformed between 2005 and 2008, states which did not reform, and states which reformed prior to 2005.

Table 4.1: Status of cable franchise reform legislation in all fifty states

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4 States with laws prior to 2005:

> Alaska, Hawaii, Rhode Island, and Vermont

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19 States that enacted laws in 2005, 2006, 2007, or 2008:

> 2005: Texas
>
> 2006: Indiana, Kansas, Michigan, New Jersey, and North Carolina
>
> 2007: California, Connecticut, Florida, Georgia, Illinois, Iowa, Missouri,
>
> Nevada, Ohio, South Carolina, and Wisconsin
>
> 2008: Louisiana and Tennessee

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27 States that have not enacted laws (as of May 2013)

Alabama, Arizona, Arkansas, Colorado, Delaware, Idaho, Kentucky, Maine, Maryland,

Massachusetts, Minnesota, Mississippi, Montana, Nebraska, New Hampshire, New

Mexico, New York, North Dakota, Oklahoma, Oregon, Pennsylvania, South Dakota,

Utah, Virginia, Washington, West Virginia, and Wyoming

Source: "State Video Franchise Law: State of Art or State of War?" by Jay T. Spurgin (2008) supplemented by authors' research.

Table 4.2: Summary statistics -- Number of communities

Reform Status	2004	2005	2006	2007	2008	2009	2010	Total
No reform	16,320	16,191	15,622	15,077	14,172	14,221	14,012	105,615
Reform before 2005	512	489	487	470	464	463	466	3,351
Reform between 2005 - 2008	18,805	18,391	17,818	17,379	15,981	16,018	15,723	120,115
Total	35,637	35,071	33,927	32,926	30,617	30,702	30,201	229,081

Notes: The lowest level of disaggregation at which data is available from the Warren's TV Factbook (2004 – 2010) is the individual community. Generally the boundaries of a cable community correspond to that of a municipality, though there may be multiple cable communities within a single municipality and vice versa. The status of reforms by state is summarized in Table 4.1.

Table 4.3: Summary statistics on monthly price of "Basic" and "Expanded Basic" service

Reform Status	"Basic" service				"Expanded Basic" service			
	2004	2010	Average across the sample	% change between 2004 -2010	2004	2010	Average across the sample	% change between 2004 -2010
No Reform	$17.77	$20.35	$19.11	15%	$35.96	$46.85	$41.75	30%
Reform before 2005	$17.05	$22.94	$19.39	35%	$39.80	$52.79	$46.21	33%
Reform between 2005 -2008	$18.23	$19.51	$18.85	7%	$36.31	$47.20	$42.11	30%
Overall	$18.00	$19.95	$18.98	-	$36.23	$47.11	$42.02	-
Change in reformed states relative to non-reformed states				= 7% - 15% = - 8%				= 30%-30% = 0%

Notes: Data on cable prices is taken from successive issues of Warren's TV Factbook (2004 – 2010). Price data is available at the level of each individual community. Generally the boundaries of a cable community correspond to that of a municipality, though there may be multiple cable communities within a single municipality and vice versa. There are approximately 30,000 cable communities in the U.S. "Basic" service is the level of cable television service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. "Expanded Basic" service offers additional video channels on one or more service tiers and includes most of the better-known national cable television networks. The price data provided in this table exclude all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. The status of reforms by state is summarized in Table 4.1.

Table 4.4: DID effect of reform on price of "Basic" and "Expanded Basic" tiers of service

	(1)	(2)	(3)	(4)
Panel A: "Basic" service				
Year of reform	-0.0229**	-0.0237*	-0.0264+	-0.0241
	(-2.06)	(-1.95)	(-1.48)	(-1.39)
Post-reform	-0.0553***	-0.0603***	-0.0684***	-0.0578**
	(-2.95)	(-2.85)	(-2.94)	(-2.26)
Number of observations	211,500	183,253	181,704	181,637
R-squared	0.12	0.14	0.27	0.44
Panel B: "Expanded Basic" service				
Year of reform	-0.00435	-0.00776	-0.00236	0.00144
	(-0.21)	(-0.35)	(-0.10)	(0.066)
Post-reform	0.00134	0.00358	0.0171	0.0212
	(0.042)	(0.11)	(0.50)	(0.63)
Number of observations	137,180	121,369	120,653	120,637
R-squared	0.18	0.20	0.23	0.27
Demographic controls	N	Y	Y	Y
Controls related to market structure	N	N	Y	Y
Control for number of channels	N	N	N	Y

Notes: Data on cable prices is taken from successive issues of Warren's TV Factbook (2004 – 2010). Price data is available at the level of each individual community. Generally the boundaries of a cable community correspond to that of a municipality, though there may be multiple cable communities within a single municipality and vice versa. There are approximately 30,000 cable communities in the U.S. All regressions include state and year fixed effects. Demographic controls: Personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18, and the local wage. All of these controls are available at the county level and are introduced in log form. They also include controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national subscribers, the share of state subscribers for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Control for number of channels include log of number of channels provided on that tier of service. ``Year of reform'' is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. Thus, for example, for California which reformed in 2007, this dummy variable assumes the value 1 for that year and that year alone. ``Post-reform'' is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform. Thus, for example, for California, this variable assumes the value 1 for years 2008 through 2010. "Basic" service is the level of cable television service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. "Expanded Basic" service offers additional video channels on one or more service tiers and includes most of the better-known national cable television networks. The price data provided in this table exclude all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.5: DID effect of reform on price of "Basic" service – Test for prior trends

	(1)	(2)	(3)	(4)
Reform year – 2	-0.0102	-0.00106	-0.00633	-0.00186
	(-0.84)	(-0.086)	(-0.47)	(-0.15)
Reform year -1	-0.0299*	-0.0186	-0.0197	-0.00661
	(-1.72)	(-1.00)	(-0.82)	(-0.30)
Demographic controls	N	Y	Y	Y
Controls related to market structure	N	N	Y	Y
Control for number of channels	N	N	N	Y
Number of observations	144,790	128,402	127,522	127,478
R-squared	0.14	0.16	0.28	0.45

Notes: The dependent variable is the monthly price data for accessing`` Basic" service. This price excludes all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. All regressions include state and year fixed effects. All years on or after the actual enactment of reforms are dropped. Demographic controls: Personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18, and the local wage. All of these controls are available at the county level and are introduced in log form. They also include controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national subscribers, the share of state subscribers for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Control for number of channels include log of number of channels provided on that tier of service. Year of reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. Post-reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform. Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.6: Robustness checks of effect of reform on price of "Basic" service

	(1)	(2)	(3)	(4)
Base Specification	-0.0553***	-0.0603***	-0.0684***	-0.0578**
	(-2.95)	(-2.85)	(-2.94)	(-2.26)
Introducing alternative controls for quality of service (RC1)				
• Log of number of satellite channels	-0.0444**	-0.0461*	-0.0558**	-0.0572**
	(-2.17)	(-1.99)	(-2.20)	(-2.19)
• Log of number of channels included in the top 10	-0.0673***	-0.0685***	-0.0856***	-0.0865***
	(-3.20)	(-3.13)	(-3.75)	(-3.52)
• Log of number of channels included in the top 20	-0.0690***	-0.0713***	-0.0865***	-0.0843***
	(-3.22)	(-3.11)	(-3.57)	(-3.25)
Net price of service with the installation charges included (RC2)	-0.0479***	-0.0498**	-0.0562***	-0.0463**
	(-2.68)	(-2.66)	(-2.80)	(-2.08)
Long difference estimate using data only from 2004 and 2010 (RC3)	-0.0790***	-0.0982***	-0.0936***	-0.0747**
	(-3.12)	(-3.14)	(-2.92)	(-2.05)
Balanced panel (No. of years for each cable system = 7) (RC4)	-0.0806***	-0.0789***	-0.0738***	-0.0705***
	(-4.11)	(-3.71)	(-3.00)	(-2.84)
Alternative Fixed Effects (RC5) at the				
• County level	-0.0617***	-0.0465**	-0.0643***	-0.0621***
	(-3.51)	(-2.43)	(-2.97)	(-2.85)
• Cable system level	-0.0618***	-0.0625***	-0.0651***	-0.0635***
	(-3.14)	(-3.20)	(-3.21)	(-3.32)
• Community level	-0.0581***	-0.0515**	-0.0534**	-0.0533***
	(-2.78)	(-2.59)	(-2.61)	(-2.89)
Including only principal communities (RC6)	-0.0422**	-0.0392+	-0.0477**	-0.0371*
	(-2.11)	(-1.60)	(-2.23)	(-2.01)
Varying the states included in the control group (RC7)				
• Excluding states which reformed prior to 2005	-0.0490***	-0.0544**	-0.0605**	-0.0515**
	(-2.75)	(-2.68)	(-2.68)	(-2.03)
• Excluding non-reformed states which prohibit MEUs from cross-subsidizing entry into cable TV	-0.0568***	-0.0643***	-0.0735***	-0.0612**
	(-3.04)	(-3.03)	(-3.12)	(-2.31)
• Excluding non-reformed states with level playing field laws	-0.0493**	-0.0551**	-0.0734***	-0.0668**
	(-2.44)	(-2.42)	(-2.95)	(-2.50)
Demographic controls	N	Y	Y	Y
Controls related to market structure	N	N	Y	Y
Control for number of channels	N	N	N	Y

Notes: Coefficients presented in columns (1) through (4) correspond to various combinations of control variables. The dependent variable is the monthly price data for accessing ``Basic'' service unless otherwise specified. This price excludes all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. However, the regression results reported for net price of service include monthly subscription charges and installation charges, amortized over 12 months. All regressions include year fixed effects and state fixed effects unless mentioned to the contrary. Demographic and market structure controls are as in Table 4.4. Reported estimates are coefficients on the Post-reform dummy, which is a variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform (see Table 4.1 for a full list). States which reformed prior to 2005 are Alaska, Hawaii, Rhode Island, and Vermont. States which prohibit municipal electric utilities (MEUs) from cross-subsidizing their entry into the cable TV business include Alabama, Utah, and Virginia. States which have level playing field laws on their books are Alabama, Kentucky, Minnesota, New Hampshire, and Oklahoma. The ranking of channels used to define top 10 and top 20 is based on the national average cumulative rating for that channel during the fourth quarter of 2006 (roughly midpoint of our sample period). Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.7a: Reforms' effect on entry – Summary statistics

Reform Status	2004			2010			Change (entry) between 2004 and 2010		
	Fraction overbuilds	Fraction Telcos	Fraction overbuilds or Telcos	Fraction overbuilds	Fraction Telcos	Fraction overbuilds or Telcos	Fraction of overbuilds	Fraction of Telcos	Fraction of overbuilds or Telcos
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
No Reform	0.70%	0.00%	0.70%	2.80%	24.60%	26.50%	2.10%	24.60%	25.80%
Reform before 2005	0.00%	0.00%	0.00%	7.90%	11.20%	11.20%	7.90%	11.20%	11.20%
Reform between 2005–2008	1.60%	0.00%	1.60%	4.00%	39.60%	40.70%	2.40%	39.60%	39.10%
Overall	1.20%	0.00%	1.20%	3.50%	32.90%	33.70%	2.30%	32.90%	32.50%

Notes: Table presents statistics on fraction of communities that had entry by one or more cable operators (``overbuilds") or Telcos (either AT&T or Verizon). Because the Telco data was collected at the county level, penetration rate reported here are likely to be upward biased, but this measurement error is likely to impact reformed and non-reformed states in a similar manner. The status of reforms by state is summarized in Table 4.1.

Table 4.7b: Reforms' effect on entry – Linear propensity model

	(1)	(2)	(3)
Baseline: Assuming 2008 year of entry for Telcos			
Year of reform	0.0202	0.0247[+]	0.0277[+]
	(1.21)	(1.48)	(1.67)
Post-reform	0.0824*	0.112**	0.116**
	(1.77)	(2.37)	(2.48)
Alternative 1: Assuming 2006 year of entry for Telcos			
Year of reform	0.0531[+]	0.0678**	0.0703**
	(1.61)	(2.09)	(2.17)
Post-reform	0.0767**	0.123***	0.125***
	(2.29)	(3.57)	(3.66)
Alternative 2: Assuming 2007 year of entry for Telcos			
Year of reform	0.0540	0.0628	0.0665
	(1.16)	(1.37)	(1.45)
Post-reform	0.0938*	0.134***	0.138***
	(1.95)	(2.72)	(2.81)
Alternative 3: Assuming 2009 year of entry for Telcos			
Year of reform	0.00991	0.0135	0.0167*
	(1.01)	(1.43)	(1.82)
Post-reform	0.0560*	0.0761**	0.0795**
	(1.71)	(2.34)	(2.53)
Demographic controls	N	Y	Y
Controls for market structure	N	N	Y
Number of observations	48,280	39,067	37,745
R-squared	0.17	0.27	0.27

Notes: The dependent variable in this analysis is a dummy variable set to 1 if there is an overbuild in that community or if either AT&T or Verizon provides cable service to any community within that county. In the absence of precise information regarding the year of entry by AT&T or Verizon in these communities, we assume that all entry by Telcos occurred in 2008 for the baseline. The results if we assume that all entry took place either in 2006 or in 2007 or in 2009 is presented as alternatives 1, 2, and 3 respectively. The regressions are estimated on cable head-ends, as entry is observed at that level. There are approximately 6,000 cable head-ends in the U.S. All regressions include state and year fixed effects. Demographic controls: Personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18, and the local wage. All of these controls are available at the county level and are introduced in log form. The list of demographic controls also includes controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national subscribers, the share of state subscribers for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.8: Monthly price of "Basic" service: Isolating effect of increased *threat* of entry (i.e., excluding effect of actual entry)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Excluding all observations on cable overbuilds		Excluding all communities with entry by cable overbuilders		Excluding all communities with entry by cable overbuilders or Telcos		Including only communities with actual entry by overbuilders or Telcos	
Year of Reform	-0.0209	-0.0220	-0.0306*	-0.0263+	-0.0338*	-0.0274	-0.00394	-0.0226
	(-1.26)	(-1.26)	(-1.71)	(-1.49)	(-1.69)	(-1.39)	(-0.14)	(-0.81)
Post Reform	-0.0605**	-0.0525*	-0.0799***	-0.0676**	-0.0758**	-0.0657**	-0.0172	-0.0271
	(-2.31)	(-1.81)	(-3.28)	(-2.58)	(-2.41)	(-2.04)	(-0.31)	(-0.47)
Demographic controls & controls related to market structure	Y	Y	Y	Y	Y	Y	Y	Y
Control for number of channels	N	Y	N	Y	N	Y	N	Y
Number of observations	177,292	177,225	172,385	172,318	148,282	148,215	33,422	33,422
R-squared	0.28	0.44	0.27	0.44	0.27	0.44	0.25	0.40

Notes: "Overbuilder(s)" refer to second or subsequent cable entrants and are so called because these networks are built in the same area as that already serviced by the incumbent. Columns (1) and (2) exclude all price observations relating to cable overbuilds (but include observations by incumbents in communities where the overbuilder is present), from both reformed and non-reformed states. Columns (3) and (4) exclude all communities which have experienced entry by an overbuilder (from the cable industry), from both reformed and non-reformed states. Columns (5) and (6) exclude all communities which have experienced entry, whether from an overbuilder (from the cable industry) or from the two main Telecom firms AT&T or Verizon, from both reformed and non-reformed states. Columns (7) and (8) include only those communities which have experienced entry, either from an overbuilder (from the cable industry) or from either AT&T or from Verizon, for both reformed and non-reformed states. The dependent variable in all cases is the Monthly price of "Basic" service, which excludes installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. All regressions include state and year fixed effects. Demographic controls: Personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18, and the local wage. All of these controls are available at the county level and are introduced in log form. They also include controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national systems, the share of state systems for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Control for number of channels include log of number of channels provided on that tier of service. Year of reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. Post-reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform. "Basic" service is the level of cable television service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.9: Price effect of threat of entry -- Communities neighboring a "top 10 overbuilder"

	"Basic" service			"Expanded Basic" service		
	Top 10 overbuilder absent from county	Top 10 overbuilder present in county	Same as Col (2) but excluding actual overbuild	Top 10 overbuilder absent from county	Top 10 overbuilder present in county	Same as Col (5) but excluding actual overbuild
	(1)	(2)	(3)	(4)	(5)	(6)
Year of reform	-0.0201	-0.0574	-0.0606*	0.00288	0.00300	-0.0221
	(-1.05)	(-1.41)	(-1.96)	(0.14)	(0.048)	(-0.32)
Post-reform	-0.0535*	-0.172**	-0.106**	0.0360	-0.0923	-0.102
	(-1.70)	(-2.60)	(-2.25)	(1.13)	(-1.31)	(-1.35)
Number of observations	166,841	14,796	12,410	109,955	10,682	9,654
R-squared	0.43	0.62	0.63	0.27	0.36	0.44
Demographic controls	Y	Y	Y	Y	Y	Y
Controls related to market structure	Y	Y	Y	Y	Y	Y
Control for number of channels	Y	Y	Y	Y	Y	Y

Notes: "Overbuilder(s)" refer to second or subsequent cable entrants and are so called because these networks are built in the same area as that already serviced by the incumbent. Top 10 overbuilders are defined as the largest 10 companies with at least 30% of subscribers residing in overbuilt communities. Columns (1) and (4) include only counties where no top 10 overbuilder was present during the year. Columns (2) and (5) include only counties where a top 10 overbuilder was present during the year. Columns (3) and (6) are same as Columns (2) and (5) respectively, but exclude overbuild observations with the actual overbuilder so only communities which have don't have an overbuild but have a top 10 overbuilder located in that county are included. Data on cable prices is taken from successive issues of Warren's TV Factbook (2004 – 2010). Price data is available at the level of each individual community. Generally the boundaries of a cable community correspond to that of a municipality, though there may be multiple cable communities within a single municipality and vice versa. There are approximately 30,000 cable communities in the U.S. All regressions include state and year fixed effects. Demographic controls: Personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18, and the local wage. All of these controls are available at the county level and are introduced in log form. They also include controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national subscribers, the share of state subscribers for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Control for number of channels include log of number of channels provided on that tier of service. "Year of reform" is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. "Post-reform" is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform.

"Basic" service is the level of cable television service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. "Expanded Basic" service offers additional video channels on one or more service tiers and includes most of the better-known national cable television networks. The price data provided in this table exclude all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. Robust t statistics, clustered by state, in parentheses + p < 0.15, * p < 0.10, ** p < 0.05, *** p < 0.01.

Figure A1: Schematic of sample split test -- Comparing communities neighboring a top 10 overbuilder

X: Cable system operated by a top 10 overbuilder
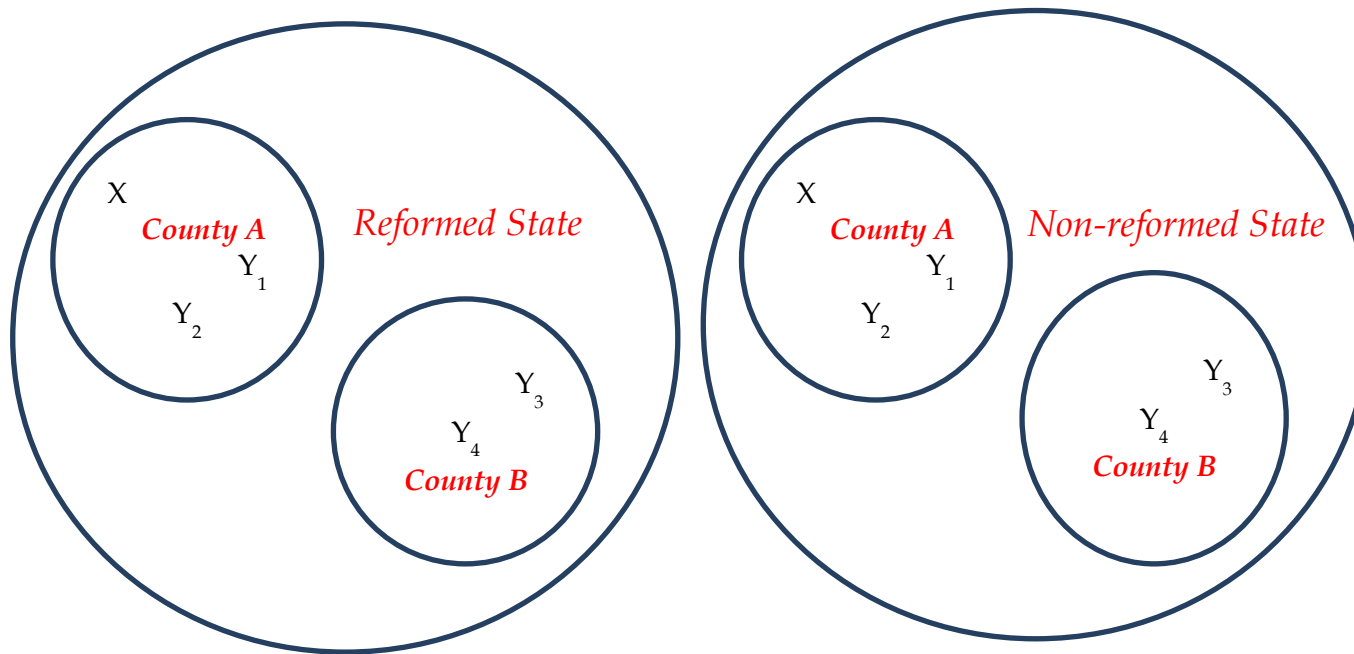Y_i: Cable system operated by a non-top 10 overbuilder/ non-overbuilder

Table A1: State cable/video franchise law summary

State	Bill Number	Date	Franchise Fees	PEG Channels	I-Net Service	Right of Way Control	Customer Service	Build-out requirements
California	AB 1715 AB 2987	7/20/07 9/29/06	Match incumbent; 5% maximum	Match incumbent; 3 minimum	Not required	Local encroachment permit required	State sets stds.; local enforcement	Phasing allowed
Connecticut	HB 7182	6/28/07	State distributes tax revenue per # of subscribers	Match incumbent	Required for libraries, schools	Not addressed	Not addressed	No specific requirements
Florida	HB 529	7/1/07	Local franchise fee replaced w/ Community Services Tax	Match incumbent; 2 minimum	Not required	Local control maintained; permit fees limited	FCC stds.; state enforces	Cannot deny service based on income level
Georgia	HB 227	7/1/07	Match incumbent; 5% maximum	Match incumbent; 3 for population >50k, 2 for population <50k	1 connection required	Local control maintained; permit fees limited	Not addressed	Cannot deny service based on income level
Illinois	SB 678 HB 1500	6/30/07	Match incumbent; 5% maximum	Match incumbent	Not addressed	Local control maintained	FCC stds. defined	Phasing allowed
Indiana	HR 1279	3/9/06	Match incumbent; 5% maximum	Utility commission provides guidance	A fee applies after 2009	Local control maintained	Not addressed	Not addressed
Iowa	SF 554	5/29/07	Match incumbent; 5% maximum	3 for population >50k, 2 for population <50k	Not required after initial agreement	Local control maintained	Not addressed	Not required
Kansas	SB 449	7/1/06	Set by cities; 5% maximum	2 channels maximum	Not addressed	Local control maintained; R/W defined	Implement system to handle inquiries	Cannot deny service based on income level
Louisiana	SB 807	6/12/08	5% maximum	3 for population > 50k, 2 for population < 50k	Not addressed	Local control maintained	FCC Stds.	Not required; Cannot deny service based on race or income level
Michigan	HB 6456	12/12/06	Match incumbent; 5% maximum	Match incumbent	Not required	Local control maintained	State enforces stds.	Phasing allowed

State	Bill Number	Date	Franchise Fees	PEG Channels	I-Net Service	Right of Way Control	Customer Service	Build-out requirements
Missouri	SB 284	8/28/07	Match incumbent; 5% maximum	3 for population >50k, 2 for population <50k	Not required	Local control maintained	1-800 customer service number must be in place	Phasing allowed
Nevada	AB 518 AB 526	5/31/07 6/4/07	Match incumbent; 5% maximum	Match incumbent	Not addressed	Local control maintained	State sets stds.	Not required
New Jersey	ACS 804	6/20/06	3.5% (1.5% previously)	2 minimum	Required for government buildings	Permit fees set by state	State sets & enforces stds.	Within 3 years
North Carolina	HB 2047	7/20/06	7% sales tax collected by state, remitted to cities	3 for population >50k, 2 for population <50k	Request of service required	Local control maintained	Monitored by state AG	Cannot deny service based on income level
Ohio	SB 117	7/17/07	Match incumbent; 5% maximum	Match incumbent; 2 minimum	Not required	Local control maintained	FCC stds.; local enforcement	Phasing allowed
South Carolina	HB 3396 H 4428	3/30/07 5/23/06	Match incumbent; 5% maximum	Match incumbent; 3 minimum	Not addressed	Local control maintained	Not addressed	Cannot deny service based on income level
Tennessee	HB 1421		Match incumbent; 5% maximum	Match incumbent; 3 for population >50k, 2 for population <50k	Not addressed	Local control maintained; bond required	FCC stds.; mediation to resolve issues	Cannot deny service based on income level
Texas	SB 5	8/9/05	Up to 5% based on # of subscribers	3 for population >50k, 2 for population <50k	Match incumbent	Local control maintained	Not addressed	Cannot deny service based on income level
Wisconsin	AB 207	4/1/07	Match incumbent; 5% maximum	Match incumbent; 3 for population >50k, 2 for population <50k	Not required	Permit fees not allowed	FCC stds.	Phasing allowed

Notes: Based on Spurgin (2008). We recoded Virginia (which had been listed in the report as reformed) as not reformed because our research shows that the state did not pass legislation allowing for a state-wide franchise. Also, we confirmed passage of the law for Louisiana, where the legislation was pending per Spurgin (2008). Details for Louisiana's cable franchise reform was sourced from: http://www.legis.la.gov/legis/Laws_Toc.aspx?folder=75&level=Parent. The details are available under RS 45:1363–1378.

Table A2: Log of average electricity prices paid by different categories of consumers, residential, commercial, and industrial between 2004 and 2010

	(1)	(2)	(3)	(4)
Year of reform	0.0133	0.101	0.0447	0.0530
	(0.77)	(1.14)	(0.41)	(1.02)
Post-reform	0.00185	0.000851	0.0181	0.00693
	(0.13)	(0.012)	(0.20)	(0.16)
Type of consumer	Residential	Commercial	Industrial	All
Number of observations	350	350	350	1050
R-squared	0.97	0.16	0.13	0.36

Notes: Data on electricity prices is drawn from: http://www.eia.gov/electricity/sales_revenue_price/. We look at average prices paid for electricity by Residential, Commercial, and Industrial categories of consumers over the period 2004–2010. Columns (1) through (3) are estimated separately for each category of consumers, and column (4) pools the data on all categories of consumers, but includes dummies for the industrial and residential sectors. All regressions include state and year fixed effects. "Year of reform" is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. "Post-reform" is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform. Robust t statistics, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: DID effect of reform on number of subscribers of "Basic" and "Expanded Basic" services

	(1)	(2)	(3)	(4)
Panel A: "Basic" service				
Year of reform	-0.0697	-0.0338	-0.0374	-0.0612
	(-0.92)	(-0.61)	(-0.59)	(-1.03)
Post-reform	-0.0324	0.0323	0.0739	0.0280
	(-0.25)	(0.39)	(0.84)	(0.30)
Number of observations	10,283	10,207	10,207	10,203
R-squared	0.12	0.31	0.34	0.36
Demographic controls	N	Y	Y	Y
Controls for market structure	N	N	Y	Y
Control for number of channels	N	N	N	Y
Panel B: "Expanded Basic" service				
Year of reform	-0.0521	0.0195	0.0172	0.0687*
	(-0.90)	(0.54)	(0.42)	(1.68)
Post-reform	-0.159+	-0.00721	0.0381	0.0983
	(-1.66)	(-0.12)	(0.59)	(1.44)
Number of observations	11,498	11,418	11,418	11,414
R-squared	0.12	0.40	0.45	0.56
Demographic controls	N	Y	Y	Y
Controls for market structure	N	N	Y	Y
Control for number of channels	N	N	N	Y

Notes: The dependent variable is the log number of subscribers for "Basic" (Panel A) and "Expanded Basic" (Panel B). Data on number of subscribers is taken from successive issues of Warren's TV Factbook (2004 – 2010). The regressions are estimated only on the cable head-ends as data on the number of subscribers is not available at the community level. There are approximately 6,000 cable head-ends in the U.S. All regressions include state and year fixed effects. Demographic controls: Personal per capita income (and its square), population density (and its square), and the fraction of the population aged between 5 and 18. All of these controls are available at the county level and are introduced in log form. They also include controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national systems, the share of state systems for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Control for number of channels include log of number of channels on that tier of service. Year of reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. Post-reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform. "Basic" service is the level of cable television service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. "Expanded Basic" service offers additional video channels on one or more service tiers and includes most of the better-known national cable television networks. The price data provided in this table exclude all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: DID effect of reform on price of "Expanded Basic" service – Test for prior trends

	(1)	(2)	(3)	(4)
Reform year – 2	-0.0153	-0.00887	-0.00428	0.00400
	(-0.96)	(-0.58)	(-0.26)	(0.23)
Reform year -1	-0.0115	-0.00664	0.000661	0.0109
	(-0.47)	(-0.28)	(0.027)	(0.43)
Demographic controls	N	Y	Y	Y
Controls related to market structure	N	N	Y	Y
Control for number of channels	N	N	N	Y
Number of observations	90,683	82,189	81,933	81,923
R-squared	0.18	0.21	0.24	0.30

Notes: The dependent variable is the monthly price data for accessing`` Expanded Basic" service. This price excludes all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. All regressions include state and year fixed effects. All years on or after the actual enactment of reforms are dropped. Demographic controls: Personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18, and the local wage. All of these controls are available at the county level and are introduced in log form. They also include controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national subscribers, the share of state subscribers for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Control for number of channels include log of number of channels provided on that tier of service. Year of reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. Post-reform: This is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform. Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Robustness checks of effect of reform on price of "Expanded Basic" service

	(1)	(2)	(3)	(4)
Base Specification	0.00134	0.00358	0.0171	0.0212
	(0.042)	(0.11)	(0.50)	(0.63)
Introducing alternative controls for quality of service (RC1)				
• Log of number of satellite channels	0.00875	0.00828	0.0204	0.0211
	(0.28)	(0.25)	(0.60)	(0.62)
• Log of number of channels included in the top 10	0.00572	0.00647	0.0183	0.0203
	(0.18)	(0.20)	(0.54)	(0.60)
• Log of number of channels included in the top 20	0.00642	0.00671	0.0185	0.0204
	(0.20)	(0.20)	(0.55)	(0.60)
Net price of service with the installation charges included (RC2)	0.00411	0.00728	0.0167	0.0194
	(0.13)	(0.22)	(0.49)	(0.57)
Long difference estimate using data only from 2004 and 2010 (RC3)	-0.0134	-0.00320	0.0114	0.0134
	(-0.33)	(-0.076)	(0.26)	(0.30)
Balanced panel (No. of years for each cable system = 7) (RC4)	0.000997	0.00536	0.0110	0.0117
	(0.027)	(0.16)	(0.32)	(0.34)
Alternative Fixed Effects (RC5) at the				
• County level	0.00132	-0.00100	0.00921	0.00860
	(0.041)	(-0.032)	(0.28)	(0.28)
• Cable system level	-0.00149	0.000352	-0.000153	-0.000792
	(-0.045)	(0.013)	(-0.0055)	(-0.029)
• Community level	0.00280	0.00678	0.00650	0.00409
	(0.078)	(0.21)	(0.20)	(0.13)
Including only principal communities (RC6)	0.00104	0.00638	0.0232	0.0334
	(0.042)	(0.27)	(0.91)	(1.40)
Varying the states included in the control group (RC7)				
• Excluding states which reformed prior to 2005	0.00155	0.00320	0.0154	0.0200
	(0.047)	(0.095)	(0.44)	(0.58)
• Excluding non-reformed states which prohibit MEUs from cross-subsidizing entry into cable TV	-0.000391	0.000946	0.0138	0.0180
	(-0.012)	(0.028)	(0.40)	(0.53)
• Excluding non-reformed states with level playing field laws	0.00257	0.00520	0.0185	0.0213
	(0.072)	(0.15)	(0.51)	(0.60)
Demographic controls	N	Y	Y	Y
Controls related to market structure	N	N	Y	Y
Control for number of channels	N	N	N	Y

Notes: Coefficients presented in columns (1) through (4) correspond to various combinations of control variables. The dependent variable is the monthly price data for accessing "Expanded Basic" service unless otherwise specified. This price excludes all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. However, the regression results reported for net price of service include monthly

subscription charges and installation charges, amortized over 12 months. All regressions include year fixed effects and state fixed effects unless mentioned to the contrary. Demographic and market structure controls are as in Table 4.4. Reported estimates are coefficients on the Post-reform dummy, which is a variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform (see Table 4.1 for a full list). States which reformed prior to 2005 are Alaska, Hawaii, Rhode Island, and Vermont. States which prohibit municipal electric utilities (MEUs) from cross-subsidizing their entry into the cable TV business include Alabama, Utah, and Virginia. States which have level playing field laws on their books are Alabama, Kentucky, Minnesota, New Hampshire, and Oklahoma. The ranking of channels used to define top 10 and top 20 is based on the national average cumulative rating for that channel during the fourth quarter of 2006 (roughly midpoint of our sample period). Robust t statistics, clustered by state, in parentheses + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Quantity effect of threat of entry -- Communities neighboring a "top 10 overbuilder"

	"Basic" service			"Expanded Basic" service		
	Top 10 overbuilder absent from county	Top 10 overbuilder present in county	Top 10 overbuilder present in county but excluding actual overbuild	Top 10 overbuilder absent from county	Top 10 overbuilder present in county	Top 10 overbuilder present in county but excluding actual overbuild
	(1)	(2)	(3)	(4)	(5)	(6)
Year of reform	-0.0837	0.442	0.312	0.0612	0.546***	0.516**
	(-1.30)	(0.96)	(0.63)	(1.44)	(3.32)	(2.29)
Post-reform	-0.0110	1.084**	1.019**	0.0748	0.818***	0.777**
	(-0.11)	(2.51)	(2.48)	(1.03)	(3.40)	(2.38)
Number of observations	8,622	399	376	9,623	452	428
R-squared	0.39	0.51	0.50	0.58	0.64	0.64
Demographic controls	Y	Y	Y	Y	Y	Y
Controls related to market structure	Y	Y	Y	Y	Y	Y
Control for number of channels	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is the log number of subscribers for "Basic" (Columns (1) to (3)) and "Expanded Basic" (Columns (4) to (6)). "Overbuilder(s)" refer to second or subsequent cable entrants and are so called because these networks are built in the same area as that already serviced by the incumbent. Top 10 overbuilders are defined as the largest 10 companies with at least 30% of subscribers residing in overbuilt communities. Columns (1) and (4) include only counties where no top 10 overbuilder was present during the year. Columns (2) and (5) include only counties where a top 10 overbuilder was present during the year. Columns (3) and (6) are same as Columns (2) and (5) respectively, but exclude overbuild observations with the actual overbuilder so only communities which have don't have an overbuild but have a top 10 overbuilder located in that county are included. Data on cable prices is taken from successive issues of Warren's TV Factbook (2004 – 2010). The regressions are estimated only on the cable head-ends as data on the number of subscribers is available only at this level of disaggregation. There are approximately 6,000 cable head-ends in the U.S.. All regressions include state and year fixed effects. Demographic controls: Personal per capita income (and its square), population density (and its square), the rate of household growth, the fraction of the population aged between 5 and 18, and the local wage. All of these controls are available at the county level and are introduced in log form. They also include controls for the size of the average cable system (measured in terms of number of subscribers per cable system in the state) and the DMA rank. Controls related to market structure: Number of national subscribers, the share of state subscribers for the company providing service in that community, and a dummy that is set to 1 whenever the local company is vertically affiliated with a content service provider. Control for number of channels include log of number of channels provided on that tier of service. "Year of reform" is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in their year of reform. "Post-reform" is a dummy variable that equals 1 for states which reformed between 2005 and 2008 in the years following the reform. "Basic" service is the level of cable television service that must be taken by all cable television subscribers. The content of basic cable service varies among cable systems but, pursuant to the Communications Act, must include all local television signals and public, educational, and governmental access channels and, at the discretion of the cable operator, may include other video services. "Expanded Basic" service offers additional video channels on one or more service tiers and includes most of the better-known national cable television networks. The price data provided in this table exclude all installation charges and any charges associated with equipment rental and reflect the monthly subscription charges for an individual consumer. Robust t statistics, clustered by state, in parentheses + p < 0.15, * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix A: Data Appendix

The variables that we introduce in the various specifications along with the respective sources are given below.

1. Population density and population growth: Data on population density is available only in the censal years. For the intervening years, population density estimates can be obtained using the estimates of population that are available. The data set is constructed using the following links: http://www.census.gov/popest/counties/CO-EST2009-03.html and http://www.census.gov/popest/archives/2000s/

2. The growth in the number of housing units comes from:

   http://www.census.gov/popest/housing/HU-EST2008-CO.html

3. Population profile comes from:

   http://www.census.gov/popest/counties/asrh/CC-EST2008-agesex.html

   Following Crawford (2000), we look at the fraction of the population that is aged between 5 and 18.

4. Per capita income comes from:

   http://www.bea.gov/regional/reis/default.cfm?selTable=Single%20Line

5. Wages and salaries are drawn from the BLS website. We look at wages for NAICS code, 51 (Information) since data is generally missing when we explore a finer level of disaggregation and look at either NAICS 515 Broadcasting, (except Internet) or at NAICS 5152 Cable and other subscription programming.

6. All of these data mentioned above in 1. through 5. are available at the county level. In addition, to control for whether local cable companies enjoy economies of scale and scope, we look at the three variables which quantify the market structure. National subscribers is simply the sum of all subscribers for a given cable company, across all communities. The share of state subscribers is the fraction of subscribers within a particular state that belong to the company operating the cable system in question. Lastly, we also examine whether these companies are affiliated with providers of content programming. Data on vertical affiliation between local cable companies and content providers comes from

Appendix C of FCC's 13th Annual Report to Congress on "The Status of Competition in the Market for the Delivery of Video Programming."

7. Data for the falsification test using electricity prices is drawn from: http://www.eia.gov/electricity/sales_revenue_price/. We look at the prices for the three categories of consumers: Residential, Commercial, and Industrial.

Further notes on methodology used for the demographic variables:

1. Population density: Obtaining population density for counties in the inter-censal years: Population density in year x in county y = Population density in year 2000 in county y (from the Census) * Population estimate in year x in county y (from the BEA series) / Population estimate in year 2000 in county y (from the BEA series) This technique works fine up to 2008. The BEA series runs out in 2008, hence for the 2009 numbers, we have to go back to using the Census data. It should be realized that by use of this approximation, we are implicitly using the land area that is used in the 2000 estimates of population density all throughout the intervening years as well. The only exception to this is Boulder County, CO and Broomfield County, CO.

2. Per capita income: Numbers are obtained directly from the BEA figures without any adjustments in general. However as for population, here too, two of the counties - Maui + Kalawao are grouped together with their own FIPS code - 15901 which is different from the FIPS codes of either Maui (15009) or Kalawao (15005). Hence we have to impute the per capita income for Maui + Kalawao to both Maui and Kalawao separately. Likewise for all jurisdictions in Virginia which are also combined similarly. E.g. Albemarle + Charlottesville, VA (FIPS: 51901) which includes two jurisdictions - Albemarle (FIPS: 51003) and Charlottesville (FIPS: 51540).

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