Does Private Money Buy Public Policy? Campaign Contributions and Regulatory Outcomes in Telecommunications

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Abstract

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1. Introduction

Regulatory outcomes often vary substantially from one US state to the next. For example, at the end of 2002 regulated prices for access to the local loops of incumbent telephone networks varied from \$2.79 per month in downtown Chicago, IL to \$7.70 in Manhattan, NY to \$12.14 in Houston, TX. Regulatory outcomes can also vary substantially over time within a state. For example, the regulated price for local loops in downtown Little Rock, AR rose from \$14.00 in 1998 to \$18.75 in 2000 before falling to \$11.86 by the end of 2002. This raises an obvious question: what explains such variation in these policy outcomes?

As in most regulated utility industries, telecommunications regulators are required to set prices with reference to some measure of cost. In theory at least, technological and geographic cost considerations might therefore explain some of the observed variation. But can costs alone explain the level of variation described above, if at all?

We examine instead a *political* explanation for the variation, namely the influence of relative levels of campaign contributions to state legislators from competing interests in the industry. Using a new data set on state campaign contributions by telecommunications companies, we find that there is indeed a correlation between the relative levels of contributions by incumbents and entrants and the level of local loop prices which effectively transfer benefits from one group to the other; when pooled from 1997 to 2002, this correlation is 0.22.

In this paper, we subject this correlation to four potential sources by which the association could be spurious. First, we control for measurable sources of potential spuriousness, including other economic, political and institutional variables. Second, studies of the determinants of state level policy outcomes typically suffer from a lack of time-series variation: with cross-sectional variation alone it is difficult to control for unobserved state specific effects that might simultaneously influence both the dependent and modelled independent variables. Since our dataset contains a panel of contributions and access prices, we can rule out the effect of time- or state-invariant confounds through the use of fixed effects. Indeed, we find that included variables without fixed effects explain almost 50 percent of the variation in the dependent variable, while more than 80 percent of this variation is explained with the inclusion of fixed effects. A third potential source of confound is that causality may flow in the opposite direction – from regulated access prices to the level of contributions. In order to eliminate this threat to causality, we employ instrumental variables analysis to eliminate any correlation between the contribution mix and the residuals. Finally, given that there is still the potential for some unobserved selection bias – that is, factors correlated with contribution mix and that vary across state and time – we employ a method proposed by Altonji, Elder and Taber (2002) to assess the extent of omitted variable bias (OVB) that would have to exist to obviate the estimated result.

After conducting this analysis, our findings are stark. We find an extremely robust relationship between relative levels of contributions and regulated access prices. We estimate that, even after controlling for observable confounding variables, unobservable but time-invariant or case-invariant effects, and simultaneity, a 10 percentage point increase in the percentage of contributions by entrants reduces the transfer price by 4% from its mean. Further, we find that for this estimated relationship to be entirely explained by OVB, omitted variables would have to explain more than five times the variation in the mix of private money as is explained by the variables included in our analysis. We consider this to be extremely unlikely.

The strength of this result informs two literatures. On the one hand, it provides an important lesson for students of regulatory economics. By finding that campaign contributions influence policy outcomes, our results support a shift in the focus of discourse from "regulation for the public interest" to "regulation for political interests." On the other hand, our findings also inform the debate on whether campaign contributions matter. The broader literature on the relationship between campaign contributions and policy finds at best, mixed, and at worst, little effect of contributions on any political outcome, a result which defies the conventional wisdom. There are many reasons, however, that this non-result might occur. As we note below, by using a different setting for analyzing this relationship, we are able to address many of the potential limitations in the existing literature, and therefore our finding in distinction to this literature provides important counter-evidence to the finding that campaign contributions "do not matter."

In the remainder of this paper, we proceed as follows. Section 2 describes the empirical context we have chosen (regulated access to telecommunications networks) to evaluate our hypothesis that private money has a significant effect on regulatory policy decisions. In Section 3, we discuss potential determinants of access prices, with a focus on political and economic factors and the mechanisms by which they may operate. Section 4 presents our empirical methods and results, including discussion of the robustness of our main finding to a number of attempts to uncover potential spuriousness. Finally, section 5 concludes.

2. The regulatory setting for access to telecommunications networks

We examine a set of telecommunications wholesale price determinations by state regulatory commissions under the *Telecommunications Act of 1996* (TA96). TA96 empowers state commissions to set (through arbitration) prices for entrant firms to access certain elements of incumbent networks (called unbundled network elements, or UNEs). Perhaps the most important of these elements, and the focus of this research, are the pairs of twisted copper wire ("local loops") connecting switching offices in incumbent networks to customer premises. We shall refer to these in shorthand as "UNE loops". These loops are usually the most expensive element that entrant firms must purchase from incumbents, and the most difficult to duplicate.

In most cases, states have found it appropriate to set different prices for UNE loops in different customer density zones. The most attractive loops for entrant firms are in the densest zones – usually called "Zone 1" in each state. These zones contain lines in downtown areas where there are high proportions of business customers. The prices for access to loops in the densest zones are the major battlegrounds between incumbents and entrants to the local telephony industry, making them the natural focus for this research.

An important feature of this battle is the impact that lower Zone 1 UNE loop prices are likely to have on the systems of cross subsidies that have pervaded retail pricing in telecommunications for decades. Lower Zone 1 UNE loop prices place pressure on the margins that incumbents can earn from profitable business and metropolitan customers, compromising their ability to continue to provide cross subsidies to residential and non-metropolitan customers respectively.

The public interest rationale for UNE loop price regulation is reflected in the requirement in TA96 that prices for these loops be based on their cost.[1] A close correspondence between Zone 1 UNE loop prices and costs across the states and over time would support a hypothesis that prices are set by economic criteria alone. Table 1, however, provides circumstantial evidence that Zone 1 UNE loop prices reflect much more than cost considerations. For example, in January 2003 the Zone 1 UNE loop price was $2.59 per month in Illinois, $7.70 in New York and $12.14 in Texas. It is implausible that these differences can be fully or even substantially explained by

1 The normative tradition in the study of regulation proposes that allocative efficiency and overall social welfare are promoted by access prices set close to costs. Indeed, restraining the ability of natural monopoly utilities to extract prices substantially above cost is the most common policy justification for economic regulation.

technological or geographical cost variation. The question we are faced with is: what explains the substantial non-economic variation we observe in Zone 1 UNE loop prices?

3. The determinants of access prices

3.1 Private interests and private money

According to conventional wisdom in political economy, one of the main instruments by which private interests can achieve policy influence is by contributing to elected officials' campaigns (Baron 1996). However, demonstrating a link between contributions and policy outcomes has proven difficult, and the empirical literature to date is perhaps best characterized as inconsistent. Indeed, it seems the evidence is beginning to weigh against the common perception that private money buys policy outcomes. But if contributions do not buy policies, what do they buy and why do firms and other interest groups give to apparently unswerving politicians?

One plausible explanation (Ansolabehere, de Figueiredo and Snyder 2003) is that most giving to politicians is motivated by its consumption value rather than by an expectation of returns in terms of policy outcomes. This argument presents an important problem for any paper testing for a causal effect of contributions on policy outcomes. To the extent that policy outcomes affect the incomes of contributors, and consumption of political participation is a function of income, a regression of outcomes on contributions might be unable to distinguish between a hypothesis that contributions influence outcomes and an alternative hypothesis that prior outcomes determine contributions. We address this issue in our empirical analysis.

In order to advance the discussion, we take a new approach to examining the link between money and politics. Perhaps most importantly, almost all of the literature that attempts to analyse the link between contributions and policy actually analyses the effect of contributions on votes by legislators. This approach suffers from three shortcomings. First, while the literature establishes a relationship (or non-relationship) between contributions and votes, it never makes the link explicitly to the true dependent variable of interest: *policy outcomes*. This is problematic because legislators have many ways of influencing policy outcomes in addition to simply through votes. As Ansolabehere, de Figueiredo and Snyder (2003) note, campaign contributions may affect policies in ways other than through the roll call votes of legislators – for example through providing either *access* to the policy-making process or through the exertion of influence in oversight of regulatory bodies. Studies that simply examine occasional voting behaviour potentially ignore these mechanisms of influence. Thus, one advantage of our work is that by moving to a domain of observable and measurable policy outcomes, we overcome this limitation and more directly mirror the spirit of the claim that money influences policy.

Second, and relatedly, in focusing on legislative votes, contemporaneous causal linkages may be difficult to observe. As Snyder (1992) argues, in the *legislative* arena, the political climate is unfriendly to the blatant purchasing of favors, so "contributors must develop a relationship of mutual trust and respect with officeholders in order to receive tangible rewards for their contributions." Given this constraint, long-term giving without obvious connection to day-to-day activities by officeholders emerges as the best feasible strategy for influencing legislative outcomes. In the context of *regulatory* influence, however, connections between current contributions to legislators and coincident regulatory outcomes are indirect, and less publicly obvious, making contemporaneous *quid pro quo* giving more feasible. Thus, we might expect to find short-term contribution strategies targeted at delivering immediate regulatory outcomes rather than less certain longer-term strategies.

Finally, most of the extant literature suffers from a lack of variation: namely, in general, tests are performed using effects of contributions on individual or a series of votes at the federal level, which limits the available variation upon which to control for potential confounds between contributions and policy behavior. Indeed, there is no prior systematic analysis of the influence of campaign finance contributions in the US states on policy outcomes of *any kind*, despite the opportunities that exist for exploiting interesting state level variation.[2] To the best of our knowledge, only one study has previously tested for a relationship between contributions to legislators and decisions by regulatory bodies, and this test was performed at the federal rather than state level.[3] By employing state level data over a series of cycles, we are able to increase

[2] There appears to be a blind spot in prior research of political influence with regard to the possibility of a link between contributions to legislators and influence over *regulatory* decisions. This blindspot could have policy or even legal consequences, as demonstrated in a hearing before the District Court for the District of Columbia concerning the constitutionality of the Bipartisan Campaign Reform Act of 2002 (Pub. L. No. 107-155, 116 Stat. 81 (2002)). Among lengthy discussions in the record of the effects of contributions on public policy there is not a single reference to the potential for influence beyond the legislative arena (*Senator Mitch McConnell et. al. v Federal Election Commission et. al.*, Civ. No. 02-582).

[3] The one study is by Hansen and Park (1995), a study at the federal level concerning antidumping and countervailing duty decisions by the International Trade Administration (ITA). Among many

the amount of variation in both our dependent and independent variables of interest as well as potential sources of bias, allowing a deeper exploration of causality.

In order to examine a potential market for influence we begin with a maintained hypotheis; namely, consistent with an extensive literature which we will discuss later, we employ a maintained assumption that legislatures exercise some measure of control over regulatory agency policymaking and implementation. Given (perhaps imperfect) legislative control of regulatory agencies, if contributors believe contributions can buy policy influence they should be as ready to seek influence over regulatory outcomes as influence over legislative votes.[4]

To examine these linkages, we test a model of interest group competition by Baron (2001) (see also Bernheim and Whinston (1986) and Grossman and Helpman (1994)). Baron utilizes a common-agency framework in which influence over an executive institution by a recipient of contributions can simultaneously reflect contributions from both interests.[5] In this model, the decision maker chooses a policy that maximizes the sum of its utility and the utility of each of the interests. The preferences of all three players are therefore incorporated in the equilibrium outcome. The equilibrium policy favors the interest with the most extreme policy preference and the greatest willingness to contribute. Our analysis draws upon this model's predictions that, in equilibrium, both interests will contribute, and relativities in contributions matter more than absolute levels of contributions by one or other interest.

There are two main classes of interests in the outcomes of UNE loop price determinations. First, incumbent local telephony operators own the telecommunications networks to which wholesale access is being sought. Second, entrants comprising competitive local telephony operators and, particularly, traditional long-distance telephony companies such as AT&T and MCI (formerly Worldcom), seek access to incumbent networks in order to provide competitive local services. In

determinants of ITA decisions, this study found PAC contributions by concerned domestic industries to be predictive of these decisions.

4 For an interesting and apposite theoretical discussion, see Holburn and Vanden Bergh (2002).

5 For analysis of the influence of campaign contributions on regulatory outcomes, a common-agency model is appropriate and preferable to other extant models of simultaneous move games in which offers of more than one interest can be accepted (Baron 2001: 76).

general, and for obvious reasons, incumbents prefer high wholesale prices, while entrants prefer low wholesale prices.[6]

An important preliminary question is what causes variation in the relative contribution mix (state by state and over time). Baron's (2001) model predicts that the interest with the most extreme preference will contribute the most and succeed in shifting the regulatory outcome in the direction of its ideal point. Both the locations of preferences of the interests and the tightness of their budget constraints are likely to vary state by state and over time within states. The factors driving this variation are many and complex. Entrant preferences will obviously depend on the profitability of entering a particular state at a particular time, while incumbent preferences will depend on the impacts upon margins and sales if entrants enter. Network effects between and within states might also alter the value to each side of "winning" regulatory battles in particular states and at particular times. Further, ideological shifts in a state might not only directly impact on the regulatory outcome, but might also alter the "prices" (contribution levels) needed for the respective interests to achieve their ideal points. Finally, assuming a degree of capital market imperfection, cash on hand (a function of the gap between revenues and costs and thereby possibly a function of lagged UNE prices) might determine budget constraints for contributions.[7]

Data on campaign contributions from the telecommunications industry to candidates for state legislatures (lower and upper houses) from 1995 to 2002 were obtained from the Institute on Money in State Politics. Data were available for most states for each of the four electoral cycles in this period.[8] Contributions at the state level can come from individuals, firms or PACs.[9] In

6 Evidence in support of these alignments abounds. For instance, see Federal Communications Commission (1996), paragraphs 635-671.

7 In addition, when considering the influence of interest groups and their political strategies, the history of the industry should be considered. Major incumbents have had relationships with state regulatory commissions since the beginnings of state regulation of telephony in the first half of the 20th Century. The "price" of influence for entrants might well be prohibitive in states where incumbent interests have become firmly entrenched. Such historical peculiarities could be expected to affect both relativities in political activities and access price outcomes, and will be controlled for in the empirical analysis by the inclusion of state fixed effects.

8 The data was provided by the Institute on February 12, 2003 and is estimated to be nearly 100 percent complete for the 1996, 1998 and 2000 cycles and 85 percent complete for the 2002 cycle. For some state-cycles no data was available, either due to incomplete records or if there were simply no contributions from either side of the industry in that state-cycle. This reduced the number of available observations for our study from 144 to 133.

all, the dataset we use consists of more than 53,000 contributions from more than 1,000 different contributors, totalling more than $22 million.[10] As we will use electoral cycles as time units, sums of contributions by entrant and incumbent firms over each two-year electoral cycle were calculated. The measure used in all our regression models is the share of contributions attributable to entrants, recalling that relativities in contribution levels should matter more than levels. There is substantial variation in this measure across states and over time.

3.2 Controls

As a first step to address selection bias, we control for economic, political and institutional features that are likely to also influence regulated access prices.

Costs

As noted earlier, TA96 prescribes a cost basis for the pricing of UNE loops. We therefore expect that costs are important determinants of these prices. Measuring costs in local telecommunications is inherently difficult, and no set of cost estimates will ever be precise or free from controversy. Fortunately, it is not necessary for our study to accurately estimate absolute cost levels in each state. Rather, what is required is a set of cost estimates that reflects relativities in costs among the states. For this purpose, we use a measure of state average UNE loop costs derived in 2002 from the Federal Communications Commission's Hybrid Cost Proxy Model (HCPM).[11] While our dependent variable is the price of UNE loops in Zone 1 areas, we

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9 Contributions were attributed to entrant or incumbent firms using the FCC's *Telecommunications Provider Locator* (October 2000 and February 2003 editions), Hoovers Online, Internet searches and other industry resources. Where individuals are listed as contributors and their employer is also listed, these contributions have been attributed to the employer.

10 An ideal analysis would examine the effects of both lobbying activities and campaign finance at the state level. Unfortunately, only data on campaign finance is currently available on a reasonably comprehensive and systematic basis across the states. However, as we suspect corporate contributions are highly correlated with lobbying (Ansolabehere, Snyder and Tripathi 2002) contribution data should be a good proxy for all non-market activity by corporations.

Additionally, while the dataset includes contributions to both incumbent and non-incumbent politicians, contributions to non-incumbents are relatively trivial given the high rates of re-election (incumbency advantage) at the state level and the tendency for the overwhelming majority of contributions to flow to incumbent politicians.

11 The FCC relies on these data not as correct measures of absolute costs in the various states, but as useful measures of relative costs across states.

believe that any variation in *Zone 1* UNE loop costs is likely to be closely correlated with variation in state *average* UNE loop costs: rural states, such as North Dakota, with high average UNE loop costs, are likely to have Zone 1 areas that are also relatively high cost compared to largely metropolitan states such as New Jersey. Average UNE loop costs therefore provide a reasonable proxy for Zone 1 UNE loop costs, in the absence of a more direct measure of Zone 1 costs.

As costs estimates are only available for 2002, we are unable to construct a time series to include in panel data estimations. We therefore include these static cost estimates only in pooled regression models, and rely instead upon state fixed effects to control for state-by-state cost variation in panel data models. We consider this to be a reasonable approach as even if cost estimates had been produced year on year, these would be unlikely to predict different costs within any state over the time period of our study, as technology in UNE loops did not radically alter over this period. In any event, if there were any fundamental changes in cost conditions over time these would be likely to be nationwide, rather than state specific, and captured in our models by time fixed effects.

Ideologies

In principle, regulatory agencies are agents of the legislature, designed to enhance the resources and expertise devoted to regulatory policy without sacrificing the legislature's right to policy discretion. The theory of legislative control of regulatory policy (Fiorina 1979; Weingast and Moran 1983; McCubbins and Schwartz 1984; McCubbins 1985; Calvert, Moran and Weingast 1987; McCubbins, Noll and Weingast 1987, 1989; Calvert, McCubbins and Weingast 1989; cf Moe 1989, 1990) proposes that, even if less than perfect, legislatures control regulators using procedural requirements, oversight,[12] budgets and appointments.[13] The literature further suggests

[12] Oversight can be direct (for example, through committee hearings in which the regulator must demonstrate in a transparent manner that it has properly exercised its mandate) or indirect (for example, through interest group feedback to legislative committee members).

[13] See also Kaufman (1981) and Wilson (1989) for anecdotal descriptions of legislative influence over agencies. While Peltzman (1984) has argued that the ideologies of legislatures do not matter for their behaviour (all that matters are the demands of their constituency and specific interest groups) others would disagree (for example, see Kau and Rubin (1979)) and the matter is far from settled. By controlling for constituency and interest group effects the analysis in our current study should be able to identify any separate effect of legislative ideology, providing evidence to better inform the debate.

that the ideological orientation of governors (Eisner and Meier 1990; Moe 1982, 1985; Wood and Waterman 1991, 1993)[14] and the regulatory commissioners themselves (Ogul 1976; Weingast 1981: 150)[15] might also be significant determinants of state level regulatory decisions. As republican ideology tends to favor less regulation, we should expect greater republican influence in each branch of government to lead to higher Zone 1 UNE loop prices.[16]

Regulatory commission ideology enters the analysis as a categorical variable coded zero if the majority of commissioners were Democrat, one if Republican, and 0.5 if the commission was evenly divided or entirely composed of Independents.[17] The measure of legislative ideology is coded zero if both houses were Democrat controlled, one if Republicans controlled both houses, and 0.5 if the houses were divided, there was no clear majority in one house or the legislature was non-partisan (Nebraska).[18] Finally, the measure of Gubernatorial ideology is coded zero if a state governor was Democrat, one if Republican and 0.5 if Independent.[19]

Demographics

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14 With a few exceptions in the area of social regulation, studies that have tested for gubernatorial influence over state regulatory agencies have failed to find any, possibly as they have been limited to analysis of cross-sectional data without controls for state specific factors (Gerber and Teske 2000). This paper's analysis of panel data with state fixed effects should provide better evidence of whether gubernatorial ideologies influence regulators. This paper also improves on much of the prior empirical literature on political control of regulatory agencies by simultaneously testing for the influence of both the legislature and the executive on regulatory outcomes.

15 See also, Wilson (1975) and Dodd and Schott (1979). Further references to writings in the agency independence paradigm can be found in McCubbins and Schwartz (1984: footnote 1).

16 Teske (1991) similarly argues that republican ideology favors the interests of the regulated incumbent firms, while democratic ideology tends to favor consumers and the promotion of competitive entry.

17 This data was obtained from annual membership directories of the National Association of Regulatory Utility Commissioners (1996, 2000, 2001 and 2002).

18 This measure is similar to Teske (1991). Two alternative approaches to measuring legislative ideology were tested. First, two dummy variables were created – one for Democrat control of both houses and another for Republican control of both houses. Second, two categorical variables were created – one indicating the dominant ideology of the lower house and the other indicating the dominant ideology of the upper house. In each case, substituting these alternative measures made little difference to the regression results and contributed little additional insight.

19 Legislative and gubernatorial ideologies were obtained from the US Census Bureau's *Statistical Abstract of the United States* (1997-2002).

The outcomes of Zone 1 UNE loop price determinations by state regulatory commissions should also vary with voter characteristics. While all voters, as consumers, would surely prefer to pay less for high quality telephone service, there is heterogeneity in the preferences of different consumer types.[20] In general terms, those consumer classes enjoying the benefits of cross-subsidies from others have interests in preserving the incumbent's ability to continue these cross-subsidies. Specifically, reductions in Zone 1 UNE loop prices squeeze the margins incumbents can earn from Zone 1 consumers (usually comprising a high percentage of business consumers) placing pressure on the extent of cross subsidies that can continue to be provided to consumers in less profitable zones (predominantly residential consumers). On this basis, while business consumers should favor lower Zone 1 UNE loop prices, we expect that as a group, residential consumers favor higher prices for the densest zones. We therefore expect that the higher the percentage of business lines in a state, the lower will be the Zone 1 UNE loop price.

Moreover, residential consumers are not homogenous. Two classes of residential consumers can be distinguished – metropolitan and non-metropolitan consumers. Some metropolitan consumers will enjoy the benefits of lower Zone 1 UNE loop prices. Even if they do not, metropolitan consumers tend to be less subsidised than non-metropolitan consumers and have less to lose from lower Zone 1 UNE loop prices. We therefore expect that the higher the percentage of metropolitan residents in a state, the lower will be the Zone 1 UNE loop price. A summary of the preferences of the various interest groups regarding Zone 1 UNE loop prices is provided in Table 2.[21]

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20 As Gilligan, Marshall and Weingast (1989: 60) observe, consumers come in many varieties and "[r]egulation, in many cases, appears not to follow the stylized pattern of a concentrated producer group against an undifferentiated, diffuse set of consumers."

21 Telecommunications access price determinations (in contrast with retail price determinations) are often an issue of low saliency with the general constituency. We might expect that with low saliency of access price determinations, constituencies will have little effect on the determinations. However, low access prices should, in time, place pressure on regulatory commissions to accede to retail rate deaveraging. When making a determination on access prices, rational regulatory commissions should recognize the impact deaveraging will ultimately have on the state's constituency. Constituencies can therefore, indirectly, retain a significant effect.

Percentages of phone lines in each state that serve business customers have been collected from the FCC's annual *Statistics of Communications Common Carriers* (1997-2002).[22] Percentages of state populations living in metropolitan areas were collected from the *Statistical Abstract of the United States* (1997-2002). As these constituency measures do not vary substantially over time, neither is included in regressions with state fixed effects.

Institutional variables

It is also important to control for the structure and rules of the institutional environment surrounding regulation (Weingast and Moran 1983; McCubbins and Schwartz 1984; McCubbins, Noll and Weingast 1987, 1989; Moe 1989, 1990; Bawn 1995; de Figueiredo, Spiller and Urbiztondo 1999; Epstein and O'Halloran 1999; de Figueiredo 2002). We consider three institutional variables. First, whether a state's regulatory commission is elected or appointed might determine the commission's responsiveness to constituency interests, although the evidence to date is mixed (Hagerman and Ratchford 1978; Gormley 1983; Harris and Navarro 1983; Costello 1984; Primeaux and Mann 1985; Boyes and McDowell 1989; Besley and Coate 2002; Holburn and Spiller 2002; Ka and Teske 2002).[23] Second, we account for whether price cap regulation or rate of return regulation is applied by the state's regulatory commission in the determination of retail prices (Lehman and Weisman 2000: Ch. 7).[24] Third, we include a dummy

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[22] Alternative regression specifications including gross state product in the finance, insurance and real estate (FIRE) sector to proxy for the strength of high demand business customer interests consistently returned insignificant coefficients on this measure of business interests.

[23] Currently, eleven states elect their regulatory commissioners. Besley and Coate (2002) propose that direct election of regulators should lead to more consumer-oriented policies than appointment of regulators. When regulators are appointed, the pivotal appointing institution (whether this be the governor or the legislature) is elected by the constituency on a broad platform of policies encompassing many issues of concern to constituents and interest groups of which regulatory policy is typically just one, and one of low saliency. Where regulators are elected however, regulatory policy is the only salient issue in the election. As elected commissioners must seek votes to achieve election or re-election, we expect elected commissioners will be particularly sensitive to constituency majorities (residential consumers) and, all else equal, will set higher Zone 1 UNE loop prices than appointed commissioners.

[24] Using a single cross-section of data, Lehman and Weisman (2000: Ch. 7) find empirical support for the proposition that access prices are higher under rate of return regulation (RRR) than under price cap regulation (PCR). The theoretical explanation for this finding is that PCR "insures" the regulator against the flow-through of wholesale UNE prices to retail rates – in other words, in the immediate term the regulator need worry less about setting UNE prices below cost in terms of the impact on retail rates. Under RRR, on the other hand, low UNE prices will need to be compensated for almost

variable measuring whether an RBOC has successfully sought Section 271 approval to enter long distance markets originating from the state, as RBOCs who have been approved are likely to have lowered UNE loop prices on their own initiative.[25]

Data on whether regulatory commissions were elected or appointed were obtained from annual editions of The Council of State Governments' *The Book of the States* (1997-2002). A dummy variable was coded one if the regulatory commission was elected and zero otherwise. This variable varies mainly between states, not within. It was therefore included only in regressions without state fixed effects. Data on the form of retail rate regulation in each state-cycle were collected from various sources.[26] A categorical variable was created in which rate of return regulation (RRR) is coded zero, price cap regulation (PCR) is coded one, and an earnings sharing plan is coded 0.5.[27] Finally, data on the state-cycles in which RBOCs filed successfully for S 271

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immediately by an upward revision of retail rates if they would otherwise lead to an earnings deficiency. Low UNE prices are therefore less likely under RRR. Possible endogeneity of the choice of regulatory regime casts some doubt on this result – the decision whether to use PCR or RRR is made by the same entity (the regulatory commission) as decisions regarding UNE prices. These decisions might be simultaneously influenced by other variables discussed in this paper, not least the mix of campaign finance. By including many variables that are likely to simultaneously determine both the choice of retail rate regulatory regime and UNE prices, and by performing an analysis that includes state fixed effects, our results should better identify the effect of the choice of regulatory regime on UNE prices.

[25] In combination with its wholesale access regime "stick", TA96 also lays out an important carrot to encourage the largest incumbents – the Regional Bell Operating Companies (RBOCs) – to open their local markets to competition. Section 271 states that the RBOCs may begin to provide long distance service originating in a state if they can satisfy the Federal Communications Commission (FCC) that their own local market in that state is sufficiently competitive. In deciding whether to grant S 271 approval, the FCC gives consideration to prices for access to incumbent network elements in the state – lower access prices enhance the likelihood of approval to enter long-distance service.

A fourth institutional variable – whether consumer interests are enfranchised in the state's regulatory process (Holburn and Spiller 2002) – was included in robustness tests. Its coefficient was positive and very close to statistically significant at the 10 percent level in a regression with state and time fixed effects, suggesting that enfranchised consumer advocates might promote higher Zone 1 UNE loop rates in the interests of residential and rural consumers (against the interests of business consumers).

[26] Namely, Abel and Clements (1998), National Regulatory Research Institute (2000) and Kirchhoff (2002a, 2002b and 2002c).

[27] An alternative measurement approach involving separate dummies for PCR and RRR made little difference to the overall regression results and did not add further insight. Note that state-cycles with rate freezes (with or without price caps) were coded one, as rate freezes provide regulators with similar incentives to price caps (rate freezes provide insurance against erosion of subsidies in retail

approval were collected from an FCC document titled *RBOC Applications to Provide In-Region, InterLATA Services Under Section 271*, available from the FCC's website. A dummy variable was created, coded one if an RBOC had applied successfully for S 271 approval in the state-cycle or a prior one, and zero otherwise.[28]

4. Empirical methods and results

We utilize a panel data set of the contiguous US states over three electoral cycles (1997/1998, 1999/2000 and 2001/2002) so the unit of analysis is a state-cycle.[29] Descriptive statistics of the variables included in the analysis are provided in Table 3. Our dependent variable is the monthly price of a Zone 1 UNE loop purchased from the historical RBOC incumbent in a state at the end of an electoral cycle.[30]

This research employs variations on the following base model of Zone 1 UNE loop prices:

$$\begin{aligned} \textit{Zone 1 UNE Loop Price}_{i,t} = {} & \alpha + \beta \textit{Contributions}_{i,t} + \gamma_1 \textit{Costs}_i + \gamma_2 \textit{Ideologies}_{i,t} \\ & + \gamma_3 \textit{Demographics}_i + \gamma_4 \textit{Institutions}_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{1}$$

rates when UNE loop prices are lowered). State-cycles with no regulation of retail rates were coded zero, as not regulating rates in non-competitive areas is most analogous to rate of return regulation.

28 Robustness tests using an alternative dummy variable coded one in state-cycles in which an RBOC first applied or had previously applied for S 271 approval (whether or not the application was successful) returned coefficients of similar size but less significance than the "successful application" dummy described in the text.

29 We exclude Alaska and Hawaii due to missing data on many important variables of interest, reducing the number of observations available for the study from 150 to 144. Note that while five states – Kentucky, Louisiana, Mississippi, New Jersey and Virginia – have odd cycles (cycles that conclude in odd years) the results of the analysis are robust to exclusion of these states.

30 Data were collected for each state-cycle from separate sources. While it was not possible to find data sources that presented prices exactly at the end of each cycle, every effort was made to collate sets of prices reported as near as possible to the end of each cycle. Data for 1998 is from Lehman and Weisman (2000), Appendix 7-1, Table 6, "Final urban UNE", amended and supplemented by an ex parte document provided by AT&T in FCC Docket No. CC 96-98, March 2, 1999 and using data on states still with interim rates in 2000 from Regulatory Research Associates (2000). Data for 2000 is from Gregg (2001), Table 2. Data for 2002 is from Gregg (2003), Table 2. Crosschecks were conducted with other sources to identify errors, using the following additional sources: Regulatory Research Associates (2000); Commerce Capital Markets (2001), Table 2-5; UBS Warburg (2001); AT&T (2002); and Gregg (2002a and 2002b), Table 2.

where α is a constant, *Contributions*$_{i,t}$ represents the percentage of contributions attributable to entrants in state i in electoral cycle t, *Costs* represents FCC HCPM estimates of state average UNE loop costs, *Ideologies* represents a vector of ideological variables for commissioners, legislatures and governors, *Demographics* and *Institutions* represent vectors of variables capturing other relevant political and institutional controls, and ε is the error term.

Table 4 reports results from six alternative models of Zone 1 UNE loop prices. Model 1 presents the results of a simple pooled regression (using all states over the three electoral cycles) with cycle fixed effects to control for nationwide time trends in the data. In this model we find no effect of relative contributions on Zone 1 UNE loop prices. Nor do we find our measure of state average UNE loop costs to be a significant determinant of these wholesale prices. This very simple, preliminary regression nonetheless explains more than 50 percent of the variation in these prices. We find that regulatory commissions serving under republican legislatures tend to set higher Zone 1 UNE loop prices than regulatory commissions subject to democrat legislatures. Constituency demographics favoring lower Zone 1 UNE loop prices (in particular, higher percentages of the state population in metropolitan areas) also appear to affect these prices in the direction predicted, although it is possible that these variables are capturing some residual cost effects. States with elected commissions are associated with \$3 higher Zone 1 UNE loop prices than states with appointed commissions. This may be because elected commissions may be more responsive to majority voter interests in preserving cross-subsidies in telecommunications prices. Finally, this model confirms Lehman and Weisman's (2000) prediction of a statistically significant relationship between the form of retail rate regulation and Zone 1 UNE loop prices; we find that states applying price cap regulation of retail rates set lower Zone 1 UNE loop prices than states applying rate of return regulation.[31]

An obvious empirical issue with pooled analysis is whether coefficient estimates suffer from heterogeneity bias (i.e. whether it is reasonable to assume the included political and institutional variables are truly exogenous). For example, the shares of campaign contributions by entrants or the forms of retail rate regulation applied in the various states might not be determined

[31] Alternative regressions including additional controls measuring gross state product, gross state product in communications, gross state product in FIRE (finance, insurance and real estate), regulatory commission staff numbers, enfranchised consumer advocates and incumbent investment returned insignificant coefficients on each of these variables without substantially altering the size and significance of coefficients on the original set of regressors.

independently of unobserved heterogeneity among the states that also affects Zone 1 UNE loop prices.[32] A benefit of constructing a panel data set is the ability to include state fixed effects to control for time invariant state specific omitted variables. Model 2 is therefore a two-way fixed effects model controlling for time-invariant state-specific effects as well as nationwide time trends.[33]

When controlling for state fixed effects, it is difficult to get statistically significant estimates of the effects of variables that might vary between states but vary little over time within states. Furthermore, including such variables reduces the degrees of freedom available to estimate the effects of other variables while adding no explanatory power to the estimation. For these reasons, the measures of costs, state demographics (business line and metropolitan population percentages) and elected commissions are not included in our regressions with state fixed effects.

In contrast to Model 1, we now find that when controlling for time-invariant state-specific effects as well as nationwide time trends, the relative mix of contributions does have a significant impact upon regulated Zone 1 UNE loop prices. Higher shares of contributions by entrants over an electoral cycle lead to lower Zone 1 UNE loop prices set by the end of the same cycle. Quantifying the effect, a one standard deviation increase in the percentage of total contributions by entrants in a cycle (0.232 percentage points) is associated with a fall of around three-tenths of a standard deviation in Zone 1 UNE loop prices (around $1.40 per month) in that cycle.

Including state fixed effects and several time-varying state level variables that together account for more than 80 percent of the variation in Zone 1 UNE loop prices gives some confidence that omitted variable bias (OVB) is minimal (i.e. that we have controlled for otherwise unobserved

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[32] To elaborate, an endogeneity concern arises when we consider that entrant shares of contributions might be influenced by unobserved, time-invariant state-specific factors such as the degree to which an incumbent has become entrenched in the state's regulatory scene. Another endogeneity concern is that the choice of retail rate regulatory form (PCR or RRR) is made by the same entity that decides on UNE loop prices, and both decisions might conceivably be influenced by unmodeled time-invariant, state-specific factors. If this is the case, simple cross-section or pooled analyses are unable to properly identify the effect of the form of retail rate regulation on UNE loop prices and are likely to overestimate the effect – incumbents entrenched in a state's political process may have simultaneously persuaded the state's regulatory commission to maintain both the RRR method and high UNE loop prices.

[33] F tests support the hypotheses that state and time fixed effects are significant at the 5 percent level.

state level variables that independently determine both the contribution mix and Zone 1 UNE loop prices). Nonetheless, while the inclusion of state fixed effects in particular is an important identification strategy in this research, it cannot control for unobserved factors that vary over time within states. Possible endogeneity of the contribution mix is a particular concern. Returning to the earlier discussion of the drivers of variation in the mix of contributions, while many of these will be captured by the fixed effects or are otherwise controlled for in the model, there might be some time varying state level determinants of relative contributions that remain uncontrolled for.

Table 5 demonstrates that the significance of the contribution mix as an explanator of Zone 1 UNE loop prices is robust to numerous refinements of the data set, alternative estimation methods and alternative constructions of the contribution mix variable. The robustness of the estimated relationship between Zone 1 UNE loop prices and the contribution mix, both to the inclusion of a number of important controls, and to the numerous alternative specifications of the regression model presented in Table 5, gives some confidence that state specific time varying omitted variables are not causing bias.

Another approach to allay concerns of OVB is to instrument for the contribution mix. The advantage of this approach is that it also allows us to deal with potential biases introduced by simultaneity.[34] In our view, while this type of bias is not as much of a concern since the stakes for policy outcomes are large, it is worth examining here. The specific instrument we use follows the approach developed by Kroszner and Stratmann (1998) who develop a rank-based instrument (see also Evans and Kessides (1993)) in their analysis of contributions to committee members in the US House of Representatives. We construct this instrument by sorting the observations in our sample from lowest entrant contribution percent to highest, and assigning ranks (1, 2 and 3 respectively) to observations in the smallest, middle and largest thirds of the sample. By construction, this instrument is correlated with the contribution mix (the first stage F-test with the inclusion of the rank instrument is $F(1,77) = 66.97$, p-value < 0.0001). Under a reasonable set of

[34] Instrumentation offers a general control for simultaneity. We also attempted to assess the direction of causation more simplistically by performing regressions of contribution mixes on lagged values of regulatory outcomes; if the consumption theory of Ansolabehere, de Figueiredo and Snyder (2003) is correct, the mix of contributions in the current cycle should be influenced by Zone 1 UNE loop prices set by the end of the prior cycle. These regressions reported that lagged UNE loop prices are not significant determinants of campaign finance contribution mixes.

assumptions, this rank instrument is also orthogonal to the error in (1), as shown by Wald (1940) and Koenker and Bassett (1978).[35] Second stage results are presented in Model 3 of Table 4. These results are very similar to the results in Model 2, and the contribution mix in particular remains significant and of similar size.

As a last test that our results are not spurious, we implement a technique developed by Altonji, Elder and Taber (2002) that is specifically designed to evaluate causation in non-experimental data where available instruments are weak. This technique utilizes the idea that the amount of selection of the potentially endogenous variable on the other observed explanatory variables in a model provides a guide to the amount of selection on the unobserved variables and the extent of endogeneity bias. Applied to our model, the method compares the normalized shift in the unobservables conditional on high and low entrant shares of contributions (2) with the equivalent normalized shift in the observables (3):

$$\frac{E\,[\,\varepsilon \mid \text{high entrant share of contributions}\,] - E\,[\,\varepsilon \mid \text{low entrant share of contributions}\,]}{var\,[\,\varepsilon\,]} \tag{2}$$

and

$$\frac{E\,[\,X'\gamma \mid \text{high entrant share of contributions}\,] - E\,[X'\gamma \mid \text{low entrant share of contributions}\,]}{var\,[X'\gamma\,]} \tag{3}$$

where $X'\gamma$ are fitted values predicting Zone 1 UNE loop prices from regression model 3 but excluding information on contributions (i.e. the vector X contains all observed variables except the contribution mix), ε represents associated residuals, and high (low) entrant share of contributions indicates state-cycles in which the entrant share of contributions is above (below) the median.

Following Altonji et. al. (2002: 6 and 32-33), an assumption that the observables (X) were randomly chosen from the vector of all characteristics that determine the dependent variable (a conservative assumption given the care taken in the development of our empirical model) implies

35 If a change in entrant contribution percent does not alter the rank, then the rank is independent of the error term. This condition will be violated only for observations near the thresholds between the ranks, so we have chosen a small number of ranks to reduce the likelihood of changes in ranks.

that (2) will equal (3). We can then assess the strength of our estimate of the effect of the relative mix of contributions (our estimate of β) by asking how much of it would remain if this (conservative) assumption were true and how many times stronger the selection on the unobservables must be than the selection on the observables in order for the unobservables to explain the entire estimate of K. If it must be several times stronger, then the case for a causal effect of entrant shares of contributions on Zone 1 UNE loop prices is strengthened. Full details of the method and calculations are provided in the Appendix. We find that, under the assumption that (2) and (3) are equal, a significant effect would remain. Indeed, we find that the normalized shift in the unobservables (2) would have to be more than five times larger than the normalized shift in the observables (3) in order to explain away the entire estimated effect of contributions on Zone 1 UNE loop prices.[36] Given the likelihood that (2) is in fact much smaller than (3), owing to the care with which we have chosen our observables to minimize bias, we emerge from this analysis with enhanced confidence that we have correctly identified an important relationship between entrant shares of contributions and Zone 1 UNE loop prices.

To test whether short-term (*quid pro quo*) or long-term (investment) contribution strategies predominate in this context, models 4, 5 and 6 mirror models 1, 2 and 3 but include the lagged contribution mix (relative contributions in the prior cycle) as an additional regressor.[37] With the inclusion of lagged contributions, models 5 and 6 return larger and more significant coefficients on contributions in the current cycle while the coefficients on lagged contributions are, in each model, insignificant.[38] These results suggest that, for influence over these regulatory outcomes, the current cycle mix of contributions to legislatures is more important than the prior cycle

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[36] In the context of their analysis of the effectiveness of catholic schools, Altonji et. al. (2002) consider a ratio (of the normalized shift in the unobservables to the normalized shift in the observables) of 2.78 to be highly unlikely.

[37] Unfortunately, data limitations prevent the inclusion of lags two or more cycles prior.

[38] Alternative regression specifications (results available from the authors on request) using years as time units rather than electoral cycles and including one year and two year lagged contributions and state fixed effects report significant coefficients on current and one year prior contributions while contributions two years prior are insignificant. Restricting the sample to election years (1998, 2000 and 2002) only current year (election year) contributions remain significant.

Alternative regression specifications including only the lagged contribution mix (excluding the current cycle contribution mix) also returned insignificant coefficients on the lagged contribution mix.

contribution mix, although it is still possible the effects could be cumulative and over longer periods than it has been possible to test for with the available data.

5. Conclusion and discussion

This research provides a positive analysis of the determinants of regulatory outcomes in the telecommunications industry. Economic models based solely on cost considerations cannot explain all the variation we observe in regulated access prices state by state and over time within states. This research has used state level panel data and fixed effects analysis to identify several features of the political and institutional environment of regulation as significant determinants of regulated prices in the US telecommunications industry. There are a number of specific implications of this research, which we summarize below.

First, and perhaps most importantly, we provide evidence that private money in the form of campaign finance contributions can influence public policy outcomes – a missing link in prior research in political economy. While evidence from prior studies concerning the influence of private money on *legislative* outcomes is mixed at best, our study suggests substantial scope exists for interests to use private money directed towards legislators to influence *regulatory* outcomes.[39] This result is robust to various alternative model specifications and attempts to expose OVB, including instrumentation and an assessment of the extent of OVB that would have to exist to obviate the estimated result.

Our analysis also suggests the effects of campaign finance on regulatory outcomes can be rapid. Specifically, after controlling for state fixed effects, the mix of contributions to legislative candidates in an election cycle is highly correlated with the regulatory outcome set during that same cycle. Alternative specifications of the empirical model including lagged contributions report insignificant coefficients on the mix of contributions in prior periods. One possible interpretation of these results is that when interests seek to influence regulatory outcomes, long-term investment strategies might be unnecessary (or at least devalued) as short-term, *quid pro*

[39] Alternative specifications of the regression model including the mix of campaign finance contributions to gubernatorial candidates returned insignificant coefficients on this variable, suggesting that interest groups concerned about regulatory outcomes are better off targeting their contributions to legislators rather than governors. This conclusion must be treated with some caution however, as less campaign finance data is available for gubernatorial candidates and the alternative regression specification was therefore performed over fewer observations.

quo contracts work well. While "give early and give often" might be the best feasible strategy for influencing *legislative* outcomes when the direct and obvious links between giving to legislators and legislative outcomes preclude *quid pro quo* arrangements (Snyder 1992), the prescription for influencing *regulatory* outcomes might instead be "give in an issue timely fashion." Whether these results are generalizable to the federal arena is debateable. Some might argue that political outcomes are more easily manipulated at the state level where there is typically significantly less public oversight.

Second, the empirical model has identified an effect of legislative ideology even when controlling for constituency and interest group effects, providing some state level support for Kau and Rubin (1979) over Peltzman's (1984) argument that legislative ideology is unimportant. In addition, while legislative ideologies appear to matter for the determination of Zone 1 UNE loop prices, gubernatorial ideologies do not (even when controlling for state specific effects). This is somewhat surprising given most governors play a critical role in the appointment of regulatory commissioners. However, this is consistent with the proposition that legislatures are able to employ administrative procedures to constrain the exercise of gubernatorial influence over executive agencies (Epstein and O'Halloran 1994, 1995, 1996, 1999; Huber and Shipan 2001).

Third, there is some, albeit weak, evidence that constituency demographics also matter in the determination of Zone 1 UNE loop prices. States with greater percentages of metropolitan residents tend to set lower Zone 1 UNE loop prices. This provides some support for the intuition that the less the concern for the maintenance of cross-subsidies in a state, the lower will be the Zone 1 UNE loop price in that state.[40]

Finally, our study confirms that the institutional environment of regulation is important. Elected commissions set higher Zone 1 UNE loop prices than appointed commissions, apparently paying greater favour to the interests of residential consumers. This is consistent with recent findings by Besley and Coate (2002) and Holburn and Spiller (2002) in the context of electricity regulation. We also find that the negative effect of price cap retail rate regulation on Zone 1 UNE loop prices remains significant even after controlling for campaign finance, ideologies, demographics,

[40] While we did not find the percentage of business lines in a state to be a significant determinant of Zone 1 UNE loop prices, its coefficient was in the predicted direction.

and remaining unobserved state-specific variation. More trivially, as expected, Zone 1 UNE loop prices are significantly lower in cycles in which RBOCs have applied successfully for S 271 approval to enter long distance markets.

Overall, one could interpret our results as supporting the proposition that regulators are strongly influenced by the interests of the legislature. We have reported evidence that legislative ideologies, constituency demographics (a measure of voter interests) and flows of private money to legislators might each be important determinants of regulatory outcomes. Meanwhile, commissioners' own ideologies and the ideologies of the governors who typically appoint them appear to be insignificant.

While we conjecture that these results should readily generalize to regulatory outcomes in other industries, further evaluation in alternative industries and regulatory environments is certainly warranted. Generalizability from the state level to the federal level requires even greater caution. More broadly, beyond the regulatory domain, the empirical method and forms of data used in this study can be applied to questions concerning the influence of political activities and institutional arrangements on legislative and gubernatorial policy setting at the state level and should prove to be fruitful ground for future research on democratic systems in the United States.

Appendix: Evaluating causation using selection on the observables

Following Altonji, Elder and Taber (2002: 6 and 32-33) we begin by assuming that (2) is equal to (3), which would be the case if the observables were chosen randomly from the set of characteristics (observable and unobservable) that determine the dependent variable. In fact, in most studies, (2) is likely much smaller than (3) given the very non-random care with which observables are chosen to eliminate bias. This assumption can thus typically be regarded as conservative.

Recall the main regression of the form:

$$Z = \alpha + \beta C + X'\gamma + \varepsilon \qquad \text{(A.1)}$$

where Z is the Zone 1 UNE loop price, C is the entrant share of contributions and X is a vector of all other observed explanatory variables. Now let $X'\delta$ and μ represent the predicted value and residuals of a regression of C on X so that $C = X'\delta + \mu$. We can then rewrite (A.1) as:

$$Z = \alpha + X'[\gamma + \beta\delta] + \beta\mu + \varepsilon \qquad \text{(A.2)}$$

As μ is orthogonal to X, we can express the bias in the estimate of β as:

$$\text{plim } b \cong \beta + cov(\mu,\varepsilon)/var(\mu) = \beta + [var(C)/var(\mu)][E(\varepsilon \mid C \text{ high}) - E(\varepsilon \mid C \text{ low})] \qquad \text{(A.3)}$$

Assuming (2) = (3), we can estimate $[E(\varepsilon \mid C \text{ high}) - E(\varepsilon \mid C \text{ low})]$ and then estimate the magnitude of the bias. Our estimate of $[E(X'\gamma \mid C \text{ high}) - E(X'\gamma \mid C \text{ low})]/var(X'\gamma)$ is 0.051 and of $var[\varepsilon]$ is 2.855. The implied estimate of $[E(\varepsilon \mid C \text{ high}) - E(\varepsilon \mid C \text{ low})]$ is therefore 0.144. Multiplying by $var(C)/var(\mu)$ (0.054/0.007) gives a bias estimate of 1.108. This is clearly small relative to our estimate of β (–6.030) and does not explain away the estimated effect. Dividing the magnitude of our estimate of β by the estimate of the magnitude of the bias, we find that the normalized shift in the unobservables would have to be more than five times larger than the normalized shift in the observables in order to explain away the entire estimated effect of the contribution mix on Zone 1 UNE loop prices. We consider this to be extremely unlikely.

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Table 1: Zone 1 UNE Loop Prices (January 2003) ($/month)

State	Zone 1 Price	State	Zone 1 Price
Alabama	11.55	Montana	23.10
Alaska	14.92	Nebraska	12.14
Arizona	9.05	Nevada	11.75
Arkansas	11.86	New Hampshire	11.97
California	8.83	New Jersey	8.12
Colorado	5.91	New Mexico	16.04
Connecticut	8.95	New York	7.70
Delaware	10.07	North Carolina	10.75
Florida	9.97	North Dakota	13.53
Georgia	10.80	Ohio	5.93
Hawaii	10.44	Oklahoma	12.14
Idaho	15.65	Oregon	13.95
Illinois	2.59	Pennsylvania	10.25
Indiana	8.03	Rhode Island	11.19
Iowa	12.69	South Carolina	13.76
Kansas	11.86	South Dakota	17.01
Kentucky	9.64	Tennessee	11.11
Louisiana	11.77	Texas	12.14
Maine	11.44	Utah	11.41
Maryland	11.11	Vermont	7.72
Massachusetts	7.54	Virginia	10.74
Michigan	8.47	Washington	6.05
Minnesota	8.81	West Virginia	14.49
Mississippi	10.98	Wisconsin	10.90
Missouri	12.71	Wyoming	19.91

Source: Gregg (2003)

Table 2: Summary of Interests Regarding Zone 1 UNE Loop Prices

Interest Group	Preference for Zone 1 UNE loop prices
Incumbents	High
Entrants	Low
Business consumers	Low
Residential consumers	High (relative to business consumers)
Metropolitan consumers	Low (relative to non-metropolitan consumers)
Non-metropolitan consumers	High (relative to metropolitan consumers)

Table 3: Descriptive Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
Zone 1 UNE Loop Prices	13.389	4.878	2.59	27.41
Entrant Contribution %	.331	.232	0	1
Costs	20.618	14.900	13.91	35.4
Commission Ideology	.653	.455	0	1
Legislative Ideology	.500	.426	0	1
Gubernatorial Ideology	.642	.479	0	1
Business Lines %	.317	.036	.236	.405
Metropolitan Population %	.683	.206	.278	1
Elected Commission	.243	.430	0	1
Price Cap Regulation	.806	.388	0	1
271 Application Approved	.278	.449	0	1

Table 4: Regression Results for Zone 1 UNE Loop Prices

	Current Cycle Contributions Only			Current and Prior Cycle Contributions		
	Pooled with Cycle Fixed Effects	State and Cycle Fixed Effects	Instrumental Variables	Pooled with Cycle Fixed Effects	State and Cycle Fixed Effects	Instrumental Variables
	(1)	(2)	(3)	(4)	(5)	(6)
Contribution % in Current Cycle	-0.909 (1.925)	-6.030*** (2.109)	-5.486** (2.564)	-1.904 (1.776)	-8.425*** (2.918)	-7.462** (3.165)
Contribution % in Prior Cycle				2.138 (1.705)	-1.352 (1.958)	0.160 (1.840)
Costs	0.154 (0.134)			0.205 (0.145)		
Commission Ideology	-1.040 (0.792)	-0.812 (1.000)	-0.818 (0.745)	-1.090 (0.985)	0.109 (1.244)	-0.101 (0.918)
Legislative Ideology	2.647** (1.009)	3.746* (2.188)	3.875*** (1.433)	2.731** (1.047)	2.310 (2.589)	2.583 (1.575)
Gubernatorial Ideology	0.801 (0.768)	-0.903 (1.030)	-0.876 (0.688)	0.496 (0.755)	-0.259 (1.154)	-0.240 (0.839)
Business Lines %	-11.085 (13.394)			-6.525 (15.946)		
Metropolitan Population %	-5.368* (2.984)			-5.968* (3.141)		
Elected Commission	3.074*** (0.791)			2.982*** (0.874)		
Price Cap Regulation	-3.030** (1.239)	-2.629*** (0.848)	-2.658*** (0.808)	-3.417** (1.534)	-2.977** (1.234)	-2.980*** (0.999)
271 Application Approved	-0.507 (0.841)	-2.942*** (0.704)	-2.941*** (0.620)	-0.637 (0.804)	-3.113*** (0.790)	-3.134*** (0.610)
Constant	16.458** (7.056)	19.052*** (1.751)	21.057*** (1.318)	18.578** (7.502)	20.823*** (1.844)	20.956*** (1.334)
State Fixed Effects	No	Yes	Yes	No	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	133	133	133	109	109	109
Adjusted R-squared	0.53	0.81		0.53	0.83	

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Robustness Tests for the Coefficient on Contribution Mix (Based on Model 3)

Alternative Specification	Coefficient on Contribution Mix	Standard Error on Contribution Mix
Balanced Panels	-6.677***	2.113
Excluding States With Odd Cycles (KY, LA, MS, NJ, VA)	-6.086***	2.215
Excluding States With Outlier Zone 1 UNE Loop Prices (IL, OH, MT, WY)	-6.773***	2.141
GLS Regression Assuming AR(1) Autocorrelation Within Panels	-4.888***	1.639
Weighted Estimation Using Population as Weights	-4.883***	1.438
Weighted Estimation Using Total Lines as Weights	-4.484***	1.378
Weighted Estimation Using Total Contributions as Weights	-4.290***	1.434
One Year of Contributions (Contributions in Election Years Only)	-4.978**	1.926
Four Cumulative Years of Contributions (Summing Contributions Over Current and Prior Cycles)	-6.806*	3.623

* significant at 10%; ** significant at 5%; *** significant at 1%