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CAN LOBBYING PREVENT ANTICOMPETITIVE OUTCOMES? EVIDENCE ON CONSUMER MONOPSONY IN TELECOMMUNICATIONS

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ABSTRACT

When basic competition rules cannot stop market power abuses, industry-specific regulations can improve economic performance. But regulations are also more immediately exposed to political pressures than are judicially administered antitrust laws, and this exposure can cause regulations to serve distributional rather than efficiency goals. Instead of supporting a Chicago School hypothesis where distributional forces tend to favor producers, however, I find evidence that regulations can inefficiently expand consumer surplus when producers lack a political voice. In particular, local exchange carriers maintain significantly smaller capital stocks in states that restrict campaign contributions from regulated utilities. This relationship is difficult to rationalize as either a statistical artifact or evidence that campaign finance laws discourage producers from restraining trade. Indeed, rather than endowing producers with political currency to capture regulators, allowances for campaign contributions appear to have strengthened competition by discouraging regulatory takings and balancing monopsonistic pressures from consumer-voters. These results highlight an empirically important potential for regulations to overly favor consumers, and strengthen arguments against consumer surplus as an objective for competition policies.

JEL: D72; E61; H11; K23; L43; L51; L96; M38

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> I think we should ban contributions from those people (John Edwards, referring to corporate lobbyists during his primary-campaign for U.S. President, January 7, 2004).
>
> Local telephone companies ... are stagnating under rules written for a bygone era where any investment made is subject to economic seizure by regulators and competitors. These rules assume a monopoly on access that no longer exists (Scott Ford, President and Chief Executive Officer of ALLTEL Corporation, testifying to the U.S. Senate Committee on Commerce, Science, and Transportation, May 12, 2004).

I. INTRODUCTION

When basic competition rules cannot stop market power abuses, industry-specific regulations can improve economic performance. But regulations are also more immediately exposed to political pressures than are judicially administered antitrust laws, and this exposure can cause regulation to serve distributional rather than efficiency goals. Chicago School scholars, for example, persuasively demonstrated that regulation can cartelize producers and thus create the very concentration of power that competition policy aims to diffuse.

The present paper argues that regulation can also work against antitrust ideals by overly favoring consumer interests. Here, the possibility for policy to restrain (rather than expand) trade mirrors that which concerns Chicago School critics—that is, it enforces consumer monopsonies rather than producer monopolies. Moreover, evidence from the U.S. local exchange sector (an attractive quasi-experimental setting and important economic sector) suggests that, rather than improving on antitrust law by weakening residual anticompetitive restraints, regulation encouraged inferior monopsonistic outcomes, at least in jurisdictions that maintain consumer-friendly political institutions.

These results suggest that, instead of endowing producers with political currency to capture public utility regulators, allowances for campaign contributions can promote competition by discouraging regulatory takings and balancing the monopsonistic influence of consumer-voters.[1] When campaign finance laws loosely restrict contributions, for example, producers can influence regulators in a manner that creates or protects monopoly positions. But as restrictions tighten, consumer welfare increasingly weighs on regulatory objectives. And although this increase can productively check monopolistic ambitions, it can also push economic activity below its competitive level.

[1] The channels through which lobbying relates to economic performance in the present paper are distinct from the informational and vote buying channels that contributors to the hefty lobbying literature frequently consider. Bailey (2004) reviews this literature and shows that its frequent characterization of lobbying as a welfare-reducing institution lacks robustness. Results that I develop corroborate those of Bailey (2004) by supporting qualitatively similar inference with evidence from a very different research design—that is, one that looks at distributions between consumer electorates and producer lobbyists, rather than between asymmetrically informed electoral groups.

In the latter case, restrictions cartelize consumers, inducing inefficient outcomes where regulation expands consumer surplus not from checking supracompetitive returns, but rather from enforcing infracompetitive returns.

As Figure 1 illustrates, this model implies that whether restrictions encourage economies to approach or overshoot efficient outcomes can be observed in how *quantities* relate to restrictions.[2] The U.S. telecommunications sector offers an attractive quasi-experimental setting for empirically evaluating this relationship. Importantly, campaign finance laws and relevant quantities are comparable across states, and the potential for confounding variables to bias statistical inference can be readily addressed. Citing features like these, Timothy Besley and Anne Case (2003) characterize cross-state investigations as being able to yield more confident conclusions about causal relationships in political-economic settings than might, say, cross-country studies where unobserved differences between regulatory jurisdictions and hard-to-translate institutional measures can be harder to address.

Exploiting this research design's strengths, I find that campaign contribution restrictions share a statistically significant, economically large, and negative relationship with output. Interpreted within a robust theoretical framework, this evidence supports the conclusion that "institutions for electoral accountability" (for example, campaign finance laws) not only risk an overweighting of consumer surplus in regulatory deliberations, they have weakened economic performance in a sector whose salient features are broadly shared.[3]

In addition, this evidence is difficult to dismiss as a statistical artifact. For example, to increase confidence that states received a "random treatment" of contribution restrictions, I employ Altonji et al.'s (2005) method for gauging "selection on unobservables" when otherwise attractive data lack interesting time series variation, or when theory is relatively silent about

[2] This relationship appears rather insensitive to my pressure group model's assumptions. If, for example, marginal cost is constant (rather than increasing per Figure 1), then a model of dynamic consistency might instead be salient. But normative inferences from how quantities respond to restrictions in models like Garfinkel and Lee (2000) and Marceau and Smart (2003) mimic those from static pressure group models. Indeed, an allowance for lobbying in dynamic settings facilitates the punishment of regulators who would opportunistically "re-plan," and can thus mitigate the potential for commitment failures when (as in my empirical application) investments are sunk. It can also strengthen the protection of "property interests," as in Sidak's (2001) argument, by discouraging political redistributions between shareholding and nonshareholding electoral members. In cases like these, producers who are subject to contribution restrictions can *again* maintain sub-optimal capital stocks. Models that account for real options yield qualitatively similar inference. Section II elaborates on these dimensions of robustness.

[3] Although they may not offer the same quasi-experimental advantages as does the local exchange sector, any sector where institutions expose producers to nonmarket distributional influences faces this type of risk (for example, electricity, catastrophe insurance, pharmaceutical). McChesney (1987) offers an early and careful anticipation of this potential.

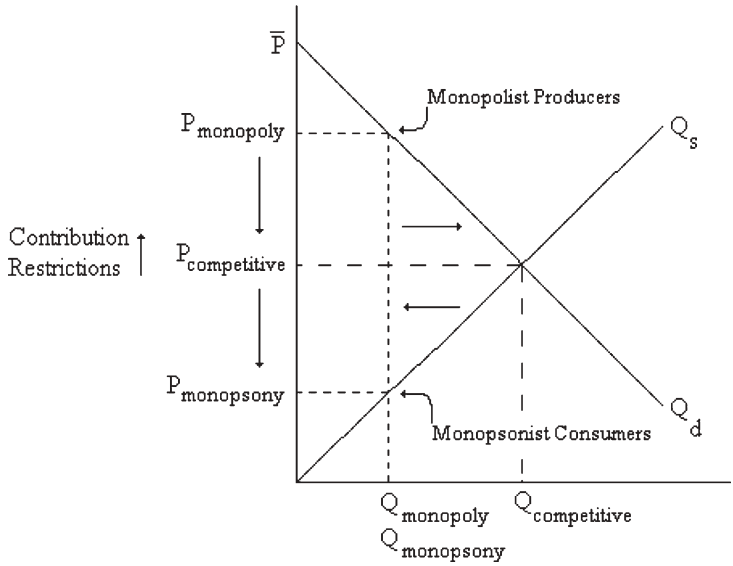

Figure 1. Tightening contribution restrictions *increases* total surplus when increasing relative consumer pressure checks regulatory capture—that is, when ("contribution restrictions" ↑) ⇒ ($P_{monopoly} \rightarrow P_{competitive}$). In this case, tightening restrictions *increases* equilibrium quantities—that is, ($P_{monopoly} \rightarrow P_{competitive}$) ⇒ ($Q_{monopoly} \rightarrow Q_{competitive}$). A further tightening of restrictions, however, facilitates consumer monopsonies. In this case, tightening restrictions pushes price below its competitive level and thus causes equilibrium quantities (and total surplus) to *shrink*—that is, ("contribution restrictions" ↑) ⇒ ($P_{competitive} \rightarrow P_{monopsony}$) ⇒ ($Q_{competitive} \rightarrow Q_{monopsony}$).

what constitutes a good instrument.[4] In doing so, I find that even the lower (absolute) bound of my estimated relationship between contribution restrictions and a proxy for total surplus is considerable—that is, unobservables would have to rationalize an implausibly large share of this relationship to dismiss it wholly as an artifact.

This research extends the literature on how regulation, through its more immediate exposure to political pressures, can either discourage or advance

[4] Altonji et al.'s (2005) method appears well-suited to aiding identification in the present application. First, although campaign finance restrictions exhibit considerable cross-state variation, they appear more stable when evaluated within states across time. In addition, the considerable cost of adjusting sunk telecom investments means that my proxy for output (that is, land-line connections to telecommunications networks) likely exhibits noisy time series variation (for example, variable lags in responding to stimuli) that can cloud evidence of causal relationships (even if they truly exist). Insight to whether campaign finance restrictions can strengthen economic performance thus appears unlikely to come from the time series dimension of relevant variables. Finally, because my regressors of interest are institutional proxies, and theories of endogenous institutions are not very well developed, good instruments are difficult to find for the present application.

the competitive goals of antitrust law.[5] For example, Peltzman (1976) and Denzau and Munger (1986) develop models where pressure from electorates and other interests can mitigate the negative consequences for competition of "Stiglerian" regulatory capture.[6] The pressure group model that I consider is both simpler, and in some ways more general, than those of Peltzman (1976) and Denzau and Munger (1986). In short, my model not only lets consumer pressure *mitigate* capture, it highlights the potential for such pressure to *reverse* capture theory's conventional implication—that is, it shows that onsumers, as well as producers, can overly influence regulators.[7] More importantly, it yields competing normative implications that are readily observable and thus lets me draw empirical inferences about economic performance (as opposed to distributions *per se*). This distinction is important because related empirical work[8] offers evidence that consumer pressure can reduce producer surplus, but does not address how this reduction affects total surplus.

This research also informs an ongoing debate about the proper goal of competition policy[9]—that is, should policy maintain a total or consumer

[5] This research may also interest readers of the institutions and commitment literature. Following North and Weingast (1989), scholars who study institutions' capacity to strengthen regulatory commitments have focused on the potential for political *agents* to expropriate the product of constituents' investment (*see, e.g.*, Henisz and Zelner, 2001). There, constraining the discretion of agents receives considerable credit for pushing economies toward superior outcomes. Interpreted within a dynamic consistency model like that of Garfinkel and Lee (2000) or Marceau and Smart (2003), my evidence suggests that campaign finance laws (that is, institutions that constrain the opportunistic objectives of agents' *principals*) may also be an important component of such commitment devices.

[6] G.J. Stigler, *The Theory of Economic Regulation*, 2(1) BELL J. ECON. & MGMT. SCI. 3 (1971).

[7] Lyon (2003) develops a similar implication to distinguish whether the historical migration of electricity regulation from municipalities to states reflects a growing ability of utilities to "capture" regulatory processes (and thus transfer surplus from consumers to producers while increasing deadweight losses) or a strengthening of regulatory commitments against opportunistically expropriating sunk investments (and thus an expansion of total surplus). In addition, David Card and Alan Krueger may have had this type of model in mind for rationalizing their provocative finding that raising the minimum wage can have a small, and sometimes positive, impact on employment. *See* D. Card & A. Krueger, *Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania*, 84(4) AM. ECON. REV. 772 (1994); D. Clement, Interview with David Card, The Region: Banking and Policy Issues Magazine (2006), *available at* http://www.minneapolisfed.org/pubs/region/06-12/card.cfm (accessed Dec. 20, 2006). There, if employers are monopsonists in the labor market, then raising the minimum wage will increase the quantity of labor supplied and expand total surplus. Finally, this model can rationalize other price ceilings that may have been set too low. *See, e.g.,* S. Tennyson, *Efficiency Consequences of Rate Regulation in Insurance Markets*, Networks Financial Institute Policy Brief, 2007-PB-03 (2007) (reviewing insurance price regulation).

[8] *See, e.g.*, W.G. Schwert, *Public Regulation of National Securities Exchanges: A Test of the Capture Hypothesis*, 8(1) BELL J. ECON. 128 (1977); G. Becker, *The Public Interest Hypothesis Revisited: A New Test of Peltzman's Theory of Regulation*, 49(3) PUB. CHOICE 223 (1986); D. Falaschetti, *Can Latent Groups Influence Policy Decisions? The Case of Telecommunications Policy*, 19(1) J.L. ECON. & ORG. 83 (2003a).

[9] *See, e.g.*, D. Carlton, *Does Antitrust Need to be Modernized?*, 21(3) J. ECON. PERSP. 155 (2007); P. Buccirossi, *Introduction*, *in* HANDBOOK OF ANTITRUST (P. Buccirossi ed., Cambridge MA: MIT Press 2008).

surplus standard? It does so by showing that consumer surplus standards not only risk inferior outcomes in principle, but may have done so in an important economic sector whose exposure to populist political pressures is unexceptional. In this light, problems of collective action or risks of producer capture that might have been productively addressed by consumer standards[10] appear to have instead received politically attractive but economically burdensome institutional remedies.

Moreover, this type of normative inference exhibits considerable robustness to modeling assumptions. Indeed, economic performance as evidenced in this paper appears inferior when evaluated not only in terms of deadweight losses, but also when compared with optimal outcomes in dynamic consistency and real option models. This agreement across both "Chicago School" and "New IO" approaches to antitrust suggests that something more than structural concerns for static regressive distributions may be necessary to defend consumer surplus as a standard for competition policy.

I develop these contributions more fully in the remainder of the article. In the following section, I build a robust model to distinguish the competing normative implications that campaign finance restrictions create empirically. I empirically evaluate these implications in Section III, and conclude in Section IV by considering how this evaluation can inform policy deliberations and future research.

II. THEORETICAL MOTIVATION

By following a consumer surplus standard, competition policy can discourage producer abuses of market power. Political influences can work against realizing such ideals, however. It is well known, for example, that regulatory processes can be captured to create excess producer rents. In cases like this, competition policy may not go far enough to improve economic performance.

But competition policy can also go too far. By overly weighting consumer surplus, for example, policy can cartelize consumers and thus give rise to inefficient monopsonistic outcomes. Here, political conditions favor consumers instead of producers, but sacrifice total surplus all the same.

These cases yield distinct observable implications, and offer a robust rationalization of the statistical results developed in this article—that is, *competition policy overly favored consumers in states where campaign finance laws restrict producer contributions*. The present section builds toward this rationalization by formally confirming Figure 1's intuition that, when a tightening of

[10] *See, e.g.*, D. Neven & L.-H. Röller, *Consumer Surplus vs. Welfare Standard in a Political Economy Model of Merger Control*, 23(9-10) Int'l J. Indus. Org. 665 (2005); J. Baker, *Competition Policy as a Political Bargain*, 73(2) Antitrust L.J. 483 (2006).

campaign finance restrictions reduces output, those restrictions diminish economic performance. It then considers this inference's sensitivity to the pressure group model's structure by reviewing models that focus on different aspects of my empirical application, but support the same inference—that is, a negative relationship between output and contribution restrictions is consistent with campaign finance laws having weakened economic performance. This robustness offers confidence that conclusions from the evidence developed in Section III are insensitive to modeling assumptions.

A. Restrictions and Welfare in a Pressure Group Model

Let a political agent take as its objective the maximization of total surplus as follows:

$$\max_{P}\{\alpha \times \textit{consumer surplus} + [1-\alpha] \times \textit{producer surplus}\} \tag{1}$$

where

$$\textit{consumer surplus} = \int_0^{Q(P)} Q_d(x)\mathrm{d}x - P \cdot Q(P) \tag{2}$$

$$\textit{producer surplus} = P \cdot Q(P) - \int_0^{Q(P)} Q_s(x)\mathrm{d}x \tag{3}$$

$$Q_d(P) = \overline{P} - P \tag{4}$$

$$Q_s(P) = P \tag{5}$$

$$Q(P) = \min\{Q_d(P), Q_s(P)\} \tag{6}$$

and $\alpha \in [0, 1]$ measures the intensity of contribution restrictions (for example, $\alpha = 1$ implies that restrictions eliminate producer pressure). This problem essentially pits consumers against producers in a "menu auction" game like that of Bernheim and Whinston (1986). In such games, political agents fully allocate a fixed "prize" (for example, P) between competing interests, and interests attempt to influence this allocation by credibly presenting to agents "political support menus" (that is, lists of support that groups supply as a function of agents' feasible actions). Distribution of the "prize" thus depends on bidders' relative capacity to produce support, represented here by the parameter α.[11]

[11] This dependence is also evident in Becker's (1983) model of pressure group competition.

Whether increasing consumer-accountability improves economic performance can be seen in how it relates to equilibrium quantities.[12] If the supply curve constrains equilibrium quantity, for example, then regulators choose prices according to the following rule:

$$P(\alpha) = \left(\frac{\alpha}{4\alpha - 1}\right)\overline{P} \tag{7}$$

Consequently, as consumer-accountability increases from $\alpha = 1/2$ to $\alpha = 1$,[13] equilibrium quantity decreases from $\overline{P}/2$ to $\overline{P}/3$, and total surplus shrinks from its maximum competitive level of $\overline{P}^2/4$ to its inferior consumer-monopsonist level of $\overline{P}^2/6$.

This relationship makes observable an implication of Schmalensee's (2004) and Armstrong and Sappington's (2006) hypothesis that regulation's objective is "consumers' surplus, not overall economic welfare."[14] But because producers and consumers symmetrically enter this model, increasing consumer pressure can also increase total surplus, and this influence makes itself observable through an increase in equilibrium quantity. *Whether increasing consumer pressure expands total surplus thus depends in this model on whether it discourages regulatory capture on behalf of producers or facilitates that on behalf of consumers*.

B. Robustness of Normative Inference

The pressure group model offers clear observable implications for how campaign contribution restrictions can influence economic performance. Because conclusions from my empirical results build on this relationship, considering its sensitivity to modeling assumptions is important.[15] This section's remainder therefore examines how restrictions relate to performance in models that focus on other salient features of my empirical setting.

Moving from a static to dynamic setting, for example, contributions give "voice" to interested producers.[16] If campaign finance laws silence this voice, then producers maintain only the action of "exit" to protest

[12] Peltzman (1976) argues that the cost of transacting in political markets limits the gains of "dominant groups." Applied to my framework, this limit implies that the parameter α will not *rest* at either of its extreme values (that is, $\alpha \neq 0$ or 1, although Peltzman's reference to the competitive outcome as a "benchmark," and corresponding reference to equilibrium [regulated] prices and quantities being read off of demand curves, imply that he considers $\alpha = 0.5$ as an effective maximum). My objective in examining the related problem (1) is to facilitate a more general normative investigation of "electoral accountability" by making transparent the observable implications of *changing* α.

[13] The constraint $Q(P) = Q_s(P)$ defines rule (7)'s domain as the interval $\alpha \in (1/2, 1]$.

[14] R. Schmalensee, *Sunk Costs and Antitrust Barriers to Entry* (MIT Sloan School of Management, Working Paper 4457-04, Jan. 2004); M. Armstrong & D.E.M. Sappington, *Regulation, Competition, and Liberalization*, XLIV(2) J. Econ. Literature 325 (2006).

[15] Leamer (1985) calls attention to this importance.

[16] *See, e.g.*, A.O. Hirschman, Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States (Cambridge, MA: Harvard University Press 1970).

undesirable political outcomes. But exit opportunities for those who made irreversible investments are (by definition) unattractive. Anticipating such a weak ex-post bargaining position, investors will shy away from sinking resources into production processes in the first place.

In this manner, restricting campaign contributions can exacerbate the capital levy problem. Following North and Weingast (1989), contributors to the "institutions and commitment" literature characterize this problem as a fundamental political obstacle to productive economic activity.[17] The inelasticity of supply from sunk investments makes capital levies an "optimal taxation" mechanism. But this feature also weakens commitments against expropriating the product of sunk investments, and thus discourages the productive employment of immobile resources in the first place (for example, land-line connections to telecommunications networks). Absent institutions that facilitate commitment, even surplus-maximizing political agents will thus follow strategies that induce inferior equilibria.

An allowance for campaign contributions can strengthen such commitments and thus weaken an economy's attraction to inferior, discretionary equilibria. Marceau and Smart (2003), for example, develop a model where capital levies are less problematic when producers can financially support (that is, "lobby") political agents. Garfinkel and Lee (2000) offer a similar insight, arguing that "reforms to limit [lobbying] may aggravate the credibility problem."[18]

This implication emerges from owners of sunk capital maintaining a relatively high willingness to pay for favorable policy. Understanding that rents from mobile capital are ephemeral, associated investors will rationally exert little in the way of lobbying effort, because that effort's product would be nonexcludable. But benefits from policies that favor immobile capital are relatively durable, and thus endow sunk-capitalists with a superior lobbying technology. This superiority, in turn, discourages political agents from setting taxes in accord with adjustment costs. By weakening this incentive against the pursuit of capital levies, tightening campaign finance restrictions can diminish economic performance.

Familiar models of both pressure group competition and dynamic consistency focus on different salient features of my empirical setting (that is, the potential for regulatory capture and the problem of credible commitment),

[17] *See, e.g.*, B. Levy & P.T. Spiller, *The Institutional Foundations of Regulatory Commitment: A Comparative Analysis of Telecommunications Regulation*, 10(2) J.L. ECON. & ORG. 201 (1994); D. Acemoglu, S. Johnson, & J.A. Robinson, *The Colonial Origins of Comparative Development: An Empirical Investigation*, 91(5) AM. ECON. REV. 1369 (2001); D. Rodrik, A. Subramanian, & F. Trebbi, *Institutions Rule: The Primacy of Institutions Over Geography and Integration in Economic Development* (NBER Working Paper No. 9305 2002); D. Stasavage, *Private Investment and Political Institutions*, 14(1) ECON. & POL. 41 (2002); D. Falaschetti, *Can Latent Groups Influence Policy Decisions? The Case of Telecommunications Policy*, 19(1) J.L. ECON. & ORG. 83 (2003a).

[18] M.R. Garfinkel & J. Lee, *Political Influence and the Dynamic Consistency of Policy*, 90(3) AM. ECON. REV. 649, 650 (2000).

but agree that a negative relationship between campaign finance restrictions and relevant quantities reflects a weakening of economic performance. This inference appears even more insensitive to assumptions when evaluated in the light of other setups. For example, Dobbs (2004) and Earle et al. (2007) show that price-capped monopolists can implicitly exercise an option by letting demand-uncertainty resolve itself before sinking resources into network development.[19] Commitments are feasible in these dynamic settings, but producers maintain an increasingly inferior capital stock as price caps become more binding. If, as in Smart (1994), Holburn and Spiller (2002), Besley and Coate (2003), and Falaschetti (2003a), caps tighten with increases in the relative weight that regulators place on consumers' surplus, then the expropriation of "real options" constitutes another channel through which a negative relationship between contribution-restrictions and equilibrium output can evidence a realized potential for restrictions to diminish economic performance.

III. EMPIRICAL RESULTS

An attractive setting for empirically considering this relationship would be one where campaign finance laws randomly vary, and the manner in which associated quantities respond to this variation can readily be observed. The local exchange sector approaches this ideal on both dimensions. Campaign finance laws exhibit considerable variation across states, and various statistical methods are readily available to isolate the portion of this variation that can confidently be treated as random. In addition, the nature of the local exchange technology precludes output from being distributed outside of the jurisdiction in which the institutions of interest are located, and the Federal Communications Commission (FCC) reports measures of output that are comparable across those jurisdictions. Features like these make the local exchange sector an attractive "laboratory" for examining how output responds to "random" variation in accountability.

Producing access to telecommunications networks employs an irreversible, capital-intensive technology where local exchange companies (LECs) connect end users to switching plants through "loops."[20] A loop generally consists of a pair of twisted copper wires and the portion of associated infrastructure-capacity that these wires consume (for example, trench and telephone pole space). LECs incur both initial and recurring costs to build and maintain loops, and recover some of these costs through connection and

[19] Hausman (1997) and Hausman and Sidak (1999) highlight the importance of accounting for such options when regulating the price at which incumbent local exchange companies sell unbundled network elements to competitors.

[20] *See, e.g.*, J. Hausman & J.G. Sidak, *A Consumer-Welfare Approach to the Mandatory Unbundling of Telecommunications Networks*, 190(3) YALE L.J. 417 (1999); R.S. Pindyck, *Mandatory Unbundling and Irreversible Investment in Telecom Networks* (MIT Sloan School of Management, Working Paper 4452-03, Dec. 2003).

line charges.[21] If capital employment is sensitive to expectations about this cost recovery, then political forces that influence telecommunications prices have a channel through which to exert real effects.

Although Public Utility Commissions (PUCs) proximately set prices,[22] interested groups in general, and LECs in particular, can exert significant influence.[23] LECs might lobby elected commissioners with contributions that are (perhaps implicitly) contingent on relevant prices. They might also offer contingent support to governors and legislators who, in turn, can influence prices through the appointment process.[24] Finally, whether commissioners are elected or appointed, LECs might influence prices by contingently supporting governors and legislators who, in turn, can sway commissioners by altering a PUC's regulatory authority or budget.[25] These institutional features offer ample opportunity for interested players to "adjust rates in order to achieve political goals".[26]

The legal setting in which LECs attempt to influence prices also varies considerably across relevant jurisdictions. My data offer information on 19 (of 48 contiguous) U.S. states where election authorities *prohibited* contributions from regulated utilities. They also offer information on 23 states that formally *restricted* contributions, with restriction-levels varying from $25 to $150,000 per election cycle. By this measure, local exchange service producers exhibit considerable variation in their capacity to lobby relevant policy makers.

Finally, the technology for producing local exchange services constrains suppliers from offering those services in jurisdictions other than those in which they confront lobbying restrictions. The "institutional elasticity" of

[21] S.G. Parsons, *The Economic Necessity of an Increased Subscriber Line Charge (SLC) in Telecommunications*, 48 ADMIN. L. REV. 227 (1996).

[22] Regulatory jurisdiction over telecommunications policy divides itself between the Federal Communications Commission (FCC) and state public utility or public corporation commissions. States maintain authority over most rates charged to customers for local exchange services. For long-distance services, the FCC regulates interstate service and state regulatory or public utilities commissions regulate intrastate service. R.G. Harris & C.J. Kraft, *Meddling Through: Regulating Local Telephone Competition in the United States*, 11 J. ECON. PERSP. 93 (1997).

[23] Moreover, because "incumbent" LECs (not "competitive" LECs) tend to maintain sunk investments, they enjoy a comparative lobbying-advantage in models like those of Garfinkel and Lee (2000) and Marceau and Smart (2003).

[24] Nationwide, 12 states elect their public utility commissioners. Others employ an appointment process. Council of State Governments, The Book of the States (1998–99 Ed., Vol. 32, Lexington, KY, 1999).

[25] Since 1989, several states' legislatures have statutorily constrained utility commissions' authority over telecommunications rates and revenues. N. Zearfoss, *The Structure of State Utility Commissions and Protection of the Captive Ratepayer: Is there a Connection?* (National Regulatory Institute Working Paper 98-14, 1998). Brock (1994) argues that such channels for "micromanagement" effectively transform elected legislators into "independent telecommunication policy makers" (independent, that is, of associated regulators).

[26] G.W. BROCK, TELECOMMUNICATIONS POLICY FOR THE INFORMATION AGE (Cambridge, MA: Harvard University Press 1994).

supply for local exchange carriers is thus likely to be higher than for producers in other networked sectors (for example, electricity) where output might be transmitted to more favorable regulatory jurisdictions. Likewise, this elasticity may be higher for telecoms than for other producers that also appear sensitive to the capital levy problem. For example, quantity supplied in high R&D sectors such as pharmaceuticals may not strongly respond to my modeled regulatory forces because that industry's production technology does not constrain output from migrating to markets where those forces are less powerful (efficiency consequences can, nevertheless, remain considerable).

Although attractive, this quasi-experimental setting leaves open issues that, left untouched, can weaken confidence in the inference that my results make available. Perhaps the most important empirical limit comes from the difficulty of producing structural evidence. For example, my theoretical motivation suggests that I evaluate a channel like the following:

$$\textit{campaign finance law} \rightarrow \textit{contributions} \rightarrow \textit{regulated price} \rightarrow \textit{output} \qquad (8)$$

But considering this channel's first relationship (that is, campaign finance law → contributions) is unlikely to produce insights that are important for my application. Che and Gale (1998) prove that constraining campaign contributions can reduce, increase, or leave unchanged aggregate contributions.[27] Consequently, even if campaign finance laws truly influence final allocations in the manner that my Section II describes, an empirical relationship between campaign finance laws and contributions, or contributions and prices, need not exist.

Although ignoring the laws–contributions relationship appears appropriate, the following structure may still deserve attention:

$$\textit{campaign finance law} \rightarrow \textit{regulated price} \rightarrow \textit{output} \qquad (9)$$

By formally reducing the channel through which lobbying can influence welfare to a single regulated price, my theoretical motivation makes such an evaluation appear feasible.[28] However, public utility commissions (PUCs)

[27] To see how caps can expand campaign finance activities, consider an all-pay auction where high-valuation players confront a binding cap. By formally reducing feasible bids for "high-valuation" players, such a constraint might be expected to reduce aggregate bidding. But capping high-types can also encourage low-valuation players to "enter the game." Indeed, absent a constraint, low-valuation players can find their equilibrium probability of winning so low that submitting a bid of "zero" becomes optimal. By encouraging low-valuation players to submit strictly positive bids in equilibrium, caps can thus increase the level of bidding from all players.

[28] De Figueiredo and Edwards (2007) find evidence that, on its face, appears to support this channel—that is, actual contributions influence prices in the hypothesized direction. As modeled in my Section II, however, capital accumulation decisions ultimately rest on campaign contribution laws (that is, the *potential* for consumer or producer influence). In

regulate numerous prices, any combination of which might represent the "true" channel through which lobbying-rules influence real activity in local exchange markets.[29] For example, in addition to pricing the various components of an end-user's services, PUCs can influence the price at which incumbent local exchange carriers (ILECs) must "unbundle" their network components for competitive local exchange carriers (CLECs). But because ILECs frequently cite such pricing (and its interaction with associated retail pricing) as curbing their incentive to invest,[30] associated regulatory decrees tend to be complex[31] and therefore lack systematic documentation.[32] Confronted with this complexity, for example, Crandall et al. (2003) exclude from their formal empirical analysis 14 states for which unbundled network element (UNE) prices are not published.

I thus treat the indexing of relevant prices as beyond the present article's objective. Instead, I empirically evaluate the following reduced form relationship:

$$campaign\ finance\ law \rightarrow output \tag{10}$$

Considering the reduced form relationship between output and campaign finance laws arguably addresses the law and economics question of present interest. Identifying relevant channels is important, but perhaps more so for

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addition, de Figueiredo and Edwards gain identification from time series variation in prices. The technology that exposes local exchange carriers to the capital levy problem, however, also creates considerable adjustment costs, and thus diminishes the responsiveness of investment to high-frequency price changes.

[29] Contributors (reviewed above) to the "public choice" literature found a negative relationship between retail price *indexes* and consumers' potential to pressure regulators. If an increase in such pressure decreases an investment-relevant price, then received indexes would have indeed exhibited a negative relationship with electoral accountability. However, such indexes would also be noisy proxies for investigations (like the present one) that focus on how laws influence economic performance (rather than distributions). In this plausible case, measuring variables with error could mask the theoretically robust relationships outlined in Section II, even if those relationships are empirically important.

[30] *See, e.g.* T.M. Jorde, J.G. Sidak, & D.J. Teece, *Innovation, Investment and Unbundling*, 17(1) YALE J. ON REG. 1 (2000); P.W. MacAvoy & J.G. Sidak, *What is Wrong with American Telecommunications?*, *in* COMPETITION AND REGULATION IN TELECOMMUNICATIONS: EXAMINING GERMANY AND AMERICA (J.G. Sidak, C. Engel, & G. Knieps eds., Dordrecht: Kluwer Academic 2000); Y.J. Dreazen & S. Young, *FCC Plans to Erase a Key Rule Aiding Local Phone Competition*, WALL ST. J., Jan. 6, 2003, at A1, A4; S.B. Pociask, *The Effect of Bargain Wholesale Prices on Local Telephone Competition: Does Helping Competitors Help Consumers?* (Sponsored Working Paper, TeleNomic Research, LLC, 2003), *available at* http://www.telenomic.com (accessed Mar. 8, 2005); Opening quote from Scott Ford testimony.

[31] *See, e.g.*, A.M. Squeo & S. Young, *Court Overturns Rules Governing Local Telephone Competition*, WALL S. J., Mar. 3, 2004, at A1, A12.

[32] J.R. Abel, *Entry into Regulated Monopoly Markets: The Development of a Competitive Fringe in the Local Telephone Industry*, 45 J.L. & ECON. 289 (2002).

readers who are interested in the telecommunications sector than the general insights that this research develops about how the institutional setting in which competition policy is decided affects economic performance. And although such channels plausibly exist,[33] establishing confidence in any *one* of them encounters considerable difficulty. A productive research design may thus be one that exploits an otherwise considerable richness in institutional variation to measure the *gross* relationship between real activity and producers' formal capacity to pressure politicians carefully.[34]

Finally, a less important (though perhaps not obviously so) limit of working in this sector is one of controlling for unobserved heterogeneity. The paucity of time series variation in my proxies for campaign finance laws and output, for example, technically discourages me from drawing interesting inference from panel data. Although controlling for fixed effects might appear to be attractive for addressing unobserved cross-sectional heterogeneity, doing so in the present application would also cloud inference from coefficient estimates on variables that vary more across space than time (for example, campaign finance restrictions).

In addition, although states offer an attractive quasi-experimental setting on several dimensions, they do not confidently admit an instrumental variable to the present analysis. The popular method of treating lagged endogenous regressors as instruments, for example, runs into difficulties that are both general and particular to my application. Because the independent variable of interest (that is, an indicator of contribution limits) is dichotomous, for example, instrumenting with the initial year of such limits trivially confirms my OLS results.

More generally, lagged endogenous regressors can be "bad" instruments because, although they can share a strong correlation with the endogenous regressor, they may not be "excludable." Rather than isolating the exogenous variation in a largely endogenous regressor, the "lagged-instrument" may correlate with the *same* endogenous variation that biases the OLS estimates. This potential may be especially concerning for cases where endogenous regressors proxy for political institutions. Here, a general theory of how institutions evolve does not appear readily available. But because exclusion restrictions cannot be tested (at least in the just identified case), such a theory is necessary to establish a restriction's validity confidently.[35]

[33] See, for example, Smart (1994), Besley and Coate (2003), and Falaschetti (2003a), each of which develops evidence that regulated prices decrease as α increases (that is, as consumer interests weigh more heavily on regulatory objectives in equation [1]).

[34] Other literatures have successfully taken such a "reduced form" approach. For example, macroeconomists have given theoretical consideration to monetary policy channels while focusing their empirical investigations on what is arguably a more pressing problem—that is, isolating exogenous variation in monetary policy so that causal inference about economic performance can be drawn from nonexperimental data.

[35] To be sure, theory is making progress, but does little to help the present identification problem. Besley and Case (2003) and Stratmann and Aparicio-Castillo (2006), for example, use party preference and educational attainment as instruments for campaign finance laws.

Difficulties like these are common in the politics, law, and economics literature, but stronger substitutes for addressing omitted variables bias (OVB) are becoming available. Altonji et al. (2005), for example, recently developed a method for drawing causal inference from nonexperimental data without relying on *a priori* exclusion restrictions. Coupled with other robustness checks (for example, employing independent proxies for consumer pressure and examining alternative specifications), Altonji et al.'s method lets me exploit the benefits of working with the present cross section while carefully addressing difficulties whose mitigation is sometimes thought to require access to panel data or good instruments.

A. Data

The variables of central interest to my investigation are measures of output and campaign finance institutions. To produce my benchmark result, I measure output through the variable *loops*, which equals the number of land-line connections between end users and switching facilities that incumbent local exchange carriers (ILECs), subject to price-cap regulation, maintained per 1000 households on December 31, 2000.[36] Given the capital-intensive technology with which local exchange services (that is, options to connect to a land-line network) are produced,[37] *loops* appears to be a reasonable proxy for output in this sector.[38] I measure associated

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In my application, however, education can independently relate to equilibrium output through its effect on consumer demand, whereas party preference can do so through unobserved policy channels. Channels like these for independent influence can create considerable bias. In unreported 2SLS regressions, for example, I employ measures of education and party preference as instruments (as opposed to controls), and continue to find that output and consumer pressure share a significant and negative relationship (as they do in corresponding OLS regressions). The coefficient estimates' magnitude, however, is implausibly large.

[36] These data predate the relaxing of unbundling requirements in 2003, which may have subsequently narrowed the channels through which electoral pressure can influence local exchange quantities. Such channels may still be "policy relevant," however, as consumer groups continue to lobby for the mandatory sharing of broadband lines, *see, e.g.*, R.W. Crandall & H.J. Singer, *Telecom Time Warp,* WALL ST. J., Jul. 11, 2007, at A15; S. Wallsten, *Towards Effective US Broadband Policies* (Progress & Freedom Foundation Progress on Point Paper No. 14.7, 2007), and the European Commission encourages incumbent operators to grant smaller competitors network access. *See, e.g.*, A. Jolis, *EU Telecom Regulation May Hurt Invest In New Tech–Report*, DOW JONES NEWSWIRE, Sept. 14, 2007, *available at* online.wsj.com.

[37] *See, e.g.*, Hausman & Sidak, *supra* note 20; Pindyck, *supra* note 20.

[38] Each of the models that guide my empirical investigation (*see* Section II) addresses a different feature of the local exchange sector, but agrees that data on accumulated capital (rather than, say, calling volume) contain information about economic performance. Consider, for example, a pressure group model with an upward-sloping supply curve. Here, a negative relationship between consumer-influence and "quantity" evidences a realized potential for consumers to weaken economic performance by creating or enforcing monopsony prices. If the marginal cost of constructing local exchange lines increases (for

Table 1. Summary Statistics, and Correlations

	Loops	*Contribution limit*	*Education*	*Income*	*Age 65*	*Clinton*	*Population density*
N	48	48	48	48	48	48	48
Sum	NA	19	NA	NA	NA	NA	NA
Mean	1,790.0	0.4	76.0	33,892	12.7	41.2	171.9
Median	1,734.1	0.0	76.7	32,694	12.7	42.4	79.4
Maximum	2,795.5	1.0	85.1	49,199	18.3	53.2	1,054.1
Minimum	963.7	0.0	64.3	24,448	8.7	24.7	4.8
SD	369.5	0.49	5.51	5,758	1.79	5.85	239.9
Source	A	B	C	C	C	D	C
Loops	1.00						
Contribution limit	−0.29	1.00					
Education	0.30	−0.19	1.00				
Income	0.63	−0.08	0.45	1.00			
Age 65	−0.39	0.18	−0.17	−0.14	1.00		
Clinton	0.02	−0.03	−0.42	0.22	0.15	1.00	
Population density	0.37	0.12	−0.03	0.73	0.20	0.38	1.00

A, *Trends in Telephone Service* (2002, Table 8.2). B, Feigenbaum and Palmer (2000). C, Geospatial and Statistical Data Center (GEOSTAT). D, Federal Election Commission (2003).

institutions through the variable *contribution limit*, which equals one for states that precluded regulated firms from contributing to campaigns during

example, if costs increase with the distance between marginal customers and local exchange switches), then data on those lines reasonably proxy for "quantity." In this case, it is the physical possibility of making a network connection, not the number of connections actually made (for example, calling volume), that exhibits an increasing marginal cost and thus measures quantity supplied. If we instead focus on the low marginal cost of producing local exchange services (instead of the increasing marginal cost of constructing lines), *loops* continues to inform us about economic performance. In this case, models of the capital levy problem become salient and imply that a negative relationship between proxies for consumer-influence (for example, restrictions on campaign contributions) and accumulated capital (for example, *loops*) evidences a realized potential for electoral accountability to become so strong that it encourages governments to expropriate sunk investments opportunistically (for example, local exchange lines). This channel for weakening economic performance creates no such observable implication for calling volumes. Finally, "real option" models focus on the value of letting demand uncertainty resolve itself before sinking resources into network connections. By limiting the returns on successful investments, consumer pressure for regulatory unbundling can diminish this option's value (transferring it instead to competitors and, ultimately, consumers), thereby increasing the risk of associated returns and discouraging the accumulation of sunk-cost investments (for example, residential loops). Here, again, a channel exists through which consumer-influence can weaken economic performance, and evidence of that effect appears in the capital with which incumbent suppliers produce end-user accessibility, not the intensity with which callers actually exercise a pre-determined capacity for access.

the election cycle ending in 2000. Table 1 summarizes these variables' distributions, as well as those for others introduced below.[39]

Loops refines the dependent variable that Henisz and Zelner (2001) use to relate political executives' discretionary capacity to telecommunications infrastructure penetration. Henisz and Zelner (2001) find a negative relationship between proxies for discretion and penetration, and attribute this relationship to a lack of formal constraints on laggard-governments' "arbitrary behavior."

An increased capacity for such behavior, however, can coincide with a superior technology for monitoring investment. Regulators whose objective is consumer surplus, for example, face relatively strong incentives to efficiently monitor under "used and useful" rate of return regulation.[40] Hence, although Henisz and Zelner's (2001) evidence supports the capital levy portion of my theoretical motivation, it may also be consistent with a *superior* outcome where executive-monitoring checks service providers' incentive to overinvest under regulatory distortions.[41]

By drawing on service providers that do not face such distortions (that is, price-capped ILECs), *loops* diminishes this alternative interpretation.[42] Indeed, removing the incentive for "over-capitalization" is one of price-cap (or "incentive") regulation's frequently purported benefits, and the manner in which U.S. regulators have implemented incentive regulation appears to have, in large part, facilitated these benefits' realization.[43]

[39] Appendix A describes each variable.

[40] R.J. Gilbert & D.M. Newbery, *The Dynamic Efficiency of Regulatory Constitutions*, 25(4) RAND J. ECON. 538 (1994).

[41] I thank Jonah Gelbach and Roger Noll for highlighting the importance of making this distinction. Averch and Johnson (1962) offer a seminal treatment of how regulation can influence firms' investment decisions.

[42] Directly addressing this important possibility precludes me from separating residential and business loops. Instead, I control for the intensity of business usage by holding constant the number of Fortune 500 headquarters and the number of "business access multilines for reporting ILECs as of December 31, 2000" (FCC, 2000, *Statistics of Communications Common Carriers*, Table 2.4). Results from this and other checks are reported in Appendix C, and are consistent with the evidence reported in this section's main tables. This robustness adds confidence to my application of Altonji et al.'s (2005) method (reported below), which implies that selection into "restricted states" largely occurs on observables. It also suggests that the interesting variation in *loops* comes from additional residential lines, a suggestion that is furthered in unreported regressions where nonlifeline residential access lines (FCC, 2000, *Statistics of Communications Common Carriers*, Table 2.19) and additional residential lines (FCC, 2002, *Monitoring Report*, Table 1.21) per household are one-quarter to one-third of a standard deviation less in states that restrict campaign finance contributions. Interpreted within the above theoretical framework, these supplementary results evidence an opportunity for LECs to restrict quantities supplied despite universal service commitments. I thank Robert Crandall and a referee for highlighting the importance of making this disclosure.

[43] *See, e.g.*, D.E.M. Sappington, *Price Regulation*, *in* HANDBOOK OF TELECOMMUNICATIONS ECONOMICS (M.E. Cave, S.K. Majumdar, I. Vogelsang eds., Amsterdam: Elsevier Science BV, vol. 1, 2002).

Even in repeated games, however, regulators who take consumer surplus as their objective continue to face considerable pressure to act opportunistically under price-cap regulation.[44] Applied to the telecommunications sector, such actions can be played by resetting prices "at the end of each price cap period to eliminate any extra-normal profit," and thus induce relevant firms to "rationally choose not to operate at peak efficiency."[45] Panteghini and Scarpa (2003) offer illustrative examples where U.K. regulators, "subject to considerable political pressures," arguably decreased utilities' output prices in just such a manner.

These pressures can increase with my independent variable of interest, *contribution limit*, which equals one for states that prohibited regulated utilities from contributing to campaigns in 2000. My pressure group and other relevant models imply that, if restricting this lobbying capacity strengthens economic performance, *loops* should increase (ceteris paribus) with *contribution limit*. A coarse evaluation of these variables produces preliminary evidence *against* this hypothesis. Here, *loops* and *contribution limit* share a considerable and negative correlation (that is, -0.35).

B. Evidence

1. OLS Estimates

To evaluate this correlation more carefully, I estimate parameters from the following model.

$$loops_i = \beta \cdot contribution\,limit_i + \sum_{k=1}^{K} \gamma_k \cdot controls_{k,i} + u_i \qquad (11)$$

Results from this estimation, a representative sample of which appear in Table 2, are consistent with a realized potential for contribution limits to facilitate consumer monopsonies, weaken regulatory commitments, or ease the expropriation of real options.

Regression (1) makes more precise the relationship that *contribution limit* and *loops* share through their simple correlation. Here, *contribution limit*'s coefficient estimate (that is, -220.28) says that LECs in states that permit contributions from regulated industries supply over one-half a standard deviation more local exchange services (as measured by *loops*) than do those in states that prohibit contributions.

In regression (2), I begin to evaluate the potential for omitted variables to bias this measured relationship. I do so by following my theoretical

[44] Gilbert & Newbery, *supra* note 40; Levy & Spiller, *supra* note 17.
[45] Sappington, *supra* note 43, at 285.

Table 2. Dependent Variable = *Loops*, Estimation Method = OLS, White Heteroskedasticity—Consistent Errors

Variable	(1) Coefficient	SE	(2) Coefficient	SE	(3) Coefficient	SE
Constant	1877.18	64.81***	2414.91	736.63***	2179.41	721.06***
Contribution limit	−220.28	106.53**	−174.55	89.43*	−147.92	85.08*
Interaction term					0.09	0.05*
Education			−12.38	10.59	−7.40	10.52
Income			44.78	13.36	37.55	13.14***
Age 65			−51.88	37.36***	−36.78	42.53
Clinton			−11.52	8.71	−13.79	10.09
Population density			0.00	0.26	0.09	0.23
N	48		48		43	
R^2	0.09		0.55		0.55	
Adjusted R^2	0.07		0.48		0.46	
$\bar{y}$	1789.98		1789.98		1788.73	
σ_y	369.45		369.45		374.56	
F-statistic	4.37		8.26		6.13	
Probability (*F*-statistic)	0.04		0.00		0.00	

***, **, and *, respectively indicate confidence at the 99 percent, 95 percent, and 90 percent levels.

motivation to control for "tastes and technology." For example, I control for consumers' budget constraints and preferences (both economic and political) by employing the variables *education*, *income*, *age 65*, and *Clinton*. To address supply-side forces, I control for the costs that local exchange carriers incur for maintaining loops through the regressor *population density*.[46] The coefficient estimate on *contribution limit* (that is, −174.55) implies that "prohibited" LECs supply almost one-half a standard deviation less services than do those in states that formally permit such contributions.

In regression (3), I introduce a finer measure of campaign finance laws by distinguishing between states that (i) *prohibit* contributions from regulated sectors, (ii) *restrict* contributions, and (iii) *do not restrict* contributions. In particular, I let *contribution limit* equal one for states that either prohibit or restrict contributions, and introduce an *interaction term* (that is, *contribution limit* multiplied by the lowest level at which states formally cap relevant contributions) to facilitate inference on how output reacts to relaxing campaign finance constraints. The results corroborate those from my more coarse measure of campaign finance laws—for example, "prohibited" LECs continue to supply almost one-half a standard deviation less services than do those who can make contributions, whereas those who operate in a more relaxed setting

[46] Sankar (1972, equation [10]) and Abel (2002) formally develop similar specifications for related applications.

(that is, states that permit an "average" level of contributions, or about $580) supply approximately one-quarter of a standard deviation less services.[47]

2. *Robustness of Positive Result*

a. Selection on Observables Versus Unobservables

Interpreted within salient models of pressure groups, capital levies, or real options, these results weigh against the popular characterization that campaign finance limits necessarily enhance economic performance. Instead, they empirically support the hypothesis that restrictions can retard performance by facilitating consumers' monopsonistic ambitions, weakening regulatory commitments, or easing the expropriation of real options.

That the large and negative correlation between *loops* and *contribution limit* appears robust to adding a preliminary set of covariates and considering an alternative proxy for campaign finance constraints (see Table 2) begins to offer confidence that this relationship is not a statistical artifact. Because institutional theory offers little "prior" information for achieving identification, I further evaluate the potential for such bias by employing Altonji et al.'s (2005) method for exploiting information about selection on observables to estimate the *maximum* bias from selection on unobservables.

This bound builds from a comparison of the following two normalized index-shifts:

$$\frac{E[u|\,contribution\,limit = 0] - E[u|\,contribution\,limit = 1]}{Var[u]} \tag{12}$$

and

$$\frac{E[X'\gamma|\,contribution\,limit = 0] - E[X'\gamma|\,contribution\,limit = 1]}{Var[X'\gamma]} \tag{13}$$

where $X'\gamma$ is a series of fitted values that predicts *loops* (without information about *contribution limit*) and u is a series of associated residuals.[48] In short, equations (12) and (13) measure the degrees to which relevant variation in unobservables and observables, respectively, intersect the relationship between campaign finance laws and local exchange loops in the reduced form model (11).

[47] A lack of data on restriction *levels* does not let me employ the entire set of observations for this part of my examination. Limiting the dataset in regressions (1) and (2) to only those observations for which restriction levels are observable does not change reported inferences.

[48] Interpreted within equation (11), $X_i'\gamma = \sum \gamma_k \cdot controls_{k,i}$. In what follows, I thus refer to a specification where X contains all of regression (2)'s independent variables (except *contribution limit*). This relatively sparse specification maintains the *greatest* (nontrivial) potential of those reported in Table 2 to evidence bias if omitted variables are truly influential.

To the extent that my observed regressors account for selection into "prohibited" states, the normalized shift of unobservables (12) equals zero, and omitted variables cannot bias available inference. For my OLS estimates to be unbiased, my regressions must be *perfectly* specified in this regard.

To evaluate the extent to which inference rests on such strong assumptions, Altonji et al. (2005) show that selection on observables can proxy for the *maximum* selection on unobservables, and thus develop a bound on how much of the OLS estimate can be attributed to endogeneity bias. Note that, if my regressions are randomly specified (as opposed to perfectly specified, which is necessary for an unbiased OLS estimate), then the normalized shifts in observables and unobservables will converge. Under this *worse-case* assumption, readily available information about selection on observables contains otherwise unavailable information about selection on unobservables.[49] Following Altonji et al. (2005) to estimate the maximum selection on unobservables in this manner,[50] I find that the "lower bound" of *contribution limit*'s coefficient equals −124.80—that is, *loops* decreases by at least one-third of a standard deviation when moving from states that allow contributions to those that prohibit contributions.[51]

b. Alternative Specifications

As a complement to this evaluation of omitted variables bias, I consider my estimates' robustness to alternative specifications. If contrary to evidence from my application of Altonji et al.'s (2005) method, selection on omitted regressors is large, then coefficient estimates on *contribution limit* should appear unstable. On the other hand, a robust estimate would further confidence from my "Altonji et al. results" that ignoring the still myriad variables that I do not (and indeed cannot) evaluate creates little potential for biasing my coefficient estimates.[52] A representative set of results reported in Tables 3 and 4 offers confidence in this regard.[53]

[49] To be sure, inference from Altonji et al.'s (2005) method does not *require* regressions to be randomly specified. Rather, it assumes a random specification to bound the OLS estimate's bias. This assumption, like that for an unbiased OLS estimate, is strong. The true coefficient, therefore, likely lies between the OLS estimate and the lower (absolute) bound from Altonji et al.'s method. To the extent that this interval contains economically large coefficient values, confidence can be gained that causal inference is not a statistical artifact.

[50] Appendix B reports detailed data from which the shift-indexes can be estimated.

[51] The corresponding upper (absolute) bound can be calculated by assuming that selection occurs only on observables, and thus equals the OLS estimate. J.G. Altonji, T.E. Elder, & C.R. Taber, *Selection on Observed Variables: Assessing the Effectiveness of Catholic Schools*, 113(1) J. POL. ECON. 151 (2005).

[52] *See, e.g.*, J. Gelbach, *When do Covariates Matter? And by How Much?* (University of Maryland, Working Paper 2004); Altonji, *supra* note 50.

[53] Other robustness checks include an evaluation of whether the coefficient estimate on *contribution limit* is an artifact of influential observations. For example, I deleted from the dataset those observations that most influence the regression line's slope (that is, the three

Table 3. Dependent variable = *loops*, estimation method = OLS, white heteroskedasticity—consistent errors

Variable	(4) Coefficient	SE	(5) Coefficient	SE	(6) Coefficient	SE
Constant	2458.39	1058.05**	1890.13	1188.02	1632.57	1355.49
Contribution limit	−165.22	90.84*	−148.22	82.26*	−145.39	77.33*
Education	−11.19	14.48	−18.26	14.23	0.14	16.62
Income	40.58	17.93**	58.27	17.34***	37.33	13.80***
Age 65	−53.20	42.38	−33.39	40.69	−63.14	31.62**
Clinton	−11.76	8.51	−7.18	8.10	−8.98	8.25
Population density	0.07	0.31	−0.09	0.28	0.19	0.28
Congealed preferences?	Yes		No		No	
Region effects?	No		Yes		No	
Firm effects?	No		No		Yes	
N	48		48		48	
R^2	0.56		0.59		0.63	
Adjusted R^2	0.45		0.50		0.51	
$\bar{y}$	1789.98		1789.98		1789.98	
$\sigma_{\bar{y}}$	369.45		369.45		369.45	
F-statistic	5.31		6.96		5.48	
Probability (F-statistic)	0.00		0.00		0.00	

***, **, and *, respectively indicate confidence at the 99 percent, 95 percent, and 90 percent levels.

Inference that can be drawn from my coefficient estimates on *contribution limit* exhibits considerable robustness as to how I specify relevant regressions. For example, I control in regression (4) for an expanded set of demographic characteristics (that is, measures of total population and the percentages of population that are black and white). In doing so, I can further address

largest positive- and negative-residual observations from regression (2) (*see* Table 2), which are Arizona, Florida, Nevada, North Dakota, Ohio, and South Carolina). As can be expected, deleting such "outliers" increases explanatory power (as measured by R^2). But, as evidenced in Appendix C (*see* regression [C]), this increase comes from residual variation in *loops* that is unrelated to *contribution limit*. Treating this issue more generally, I randomly chose states (with replacement) to construct a new set of observations and estimate the coefficient on *contribution limit* (using the specification from regression [2]), then repeated this process 1000 times. Because each of these 1000 datasets randomly excludes observations, any sensitivity of the coefficient estimate to particular states should cause these "bootstrapped" estimates to differ from those reported in my main tables. The mean of the bootstrapped estimates (that is, −290.5), however, exhibits the same magnitude as those reported above and, also like estimates reported above, the 90 percent confidence interval of [−555.5, −26.0] does not include zero. Appendix C details the frequency distribution of these coefficient estimates.

Table 4. Dependent variable = *loops*, estimation method = OLS, white heteroskedasticity—consistent errors

Variable	(7) Coefficient	SE	(8) Coefficient	SE	(9) Coefficient	SE
Constant	2838.34	736.65***	997.84	705.04	1826.57	706.04***
Contribution limit					−69.85	70.61
Elected PUC	−197.92	120.93*			−147.69	90.87
Turnout			−35.89	8.82***	−33.69	8.94***
Education	−14.29	10.66	28.90	9.72***	21.60	10.90**
Income	41.43	15.17***	28.71	9.61***	25.49	9.46***
Age 65	−58.78	38.68	−26.57	31.88	−27.10	28.69
Clinton	−13.64	8.29	−2.14	7.01	−7.53	7.00
Population density	−0.06	0.28	0.06	0.19	0.11	0.20
N	48		48		48	
R^2	0.54		0.70		0.74	
Adjusted R^2	0.47		0.66		0.68	
$\bar{y}$	1789.98		1789.98		1789.98	
$\sigma_{\bar{y}}$	369.45		369.45		369.45	
F-statistic	7.88		16.10		13.66	
Probability (F-statistic)	0.00		0.00		0.00	

***, **, and *, respectively indicate confidence at the 99 percent, 95 percent, and 90 percent levels.

contribution limit's potential to proxy for "congealed preferences" (rather than relative pressure).[54] For regressions (5) and (6), I add a set of indicators that respectively control for regional (that is, South and West) and firm-level (that is, ILEC) effects. Here, I attempt to hold constant potentially relevant, but heretofore unobserved, cross-sectional heterogeneity (for example, the capacity for campaign finance institutions to reflect associated firms' attributes). In each case, however, the coefficient estimate on *contribution limit* remains negative and statistically significant, while insignificantly differing from its counterpart in regression (2)'s more parsimonious specification (that is, −174.55).

In regressions (7)—(9), I further address the potential for *contribution limit*'s coefficient estimate to be an artifact of unobserved state characteristics by employing alternative proxies for customers' relative pressure (that is, the variable α from my theoretical motivation). To the extent that these alternatives are *not* collinear with *contribution limit*, they too should exhibit a negative relationship with *loops*, and their addition to the set of regressors should leave the estimated coefficient on *contribution limit* unchanged. For a large and negative relationship to be spurious here, the "independent" measures of relative pressure have to be artifacts of *different* sets of unobservables.

[54] *See* W. Riker, *Implications from the Disequilibrium of Majority Rule for the Study of Institutions*, 74 Am. Pol. Sci. Rev. 432 (1980).

In regression (7), I follow Smart's (1994) and Besley and Coate's (2003) evidence that the capacity for regulated utilities to pressure PUCs is relatively small in states that *elect* their utility commissioners.[55] Here, I employ the indicator *elected PUC* as an alternative proxy for the relative pressure that producers exert on regulators. In regression (8), I follow Arnold's (1991) and Falaschetti's (2003a) evidence that producers' relative pressure decreases with increased electoral mobility. In particular, I employ the variable *turnout* as another proxy for relative producer pressure.

In both cases, the negative and significant relationships between *loops* and alternative proxies for producer pressure support the same inference as does my estimated relationship between *loops* and *contribution limit*—that is, local exchange sectors rest at significantly smaller quantities (both statistically and economically) in states where producer pressure is relatively small.

Moreover, results from regression (9) offer additional confidence that my estimated relationship between *loops* and *contribution limit* is not an artifact of unobservables. Here, the coefficient estimates on *contribution limit* and *elected PUC* remain negative, but become statistically insignificant. They jointly remain significant, however, which is expected if a weakening of producer pressure truly associates itself with a decrease in quantities, but the regressors that measure this relative weakness are collinear. Consistent with this condition, *contribution limit* and *elected PUC* exhibit considerable collinearity (for example, $\rho_{contribution\ limit,\ elected\ PUC} = 0.32$). The coefficient estimate on *turnout*, on the other hand, remains negative and retains its significance. This result is similarly expected if the regressors that measure this relative weakness are instead largely independent. Consistent with this condition, *contribution limit* and *turnout* exhibit considerably less collinearity (for example, $\rho_{contribution\ limit,\ turnout} = 0.07$).

3. Robustness of Normative Result

If selection into states that prohibit campaign contributions varies only with observables, then the OLS coefficient estimate on *contribution limit* in specification (2) is unbiased. But how much confidence can we place in the assumption that selection takes place only on observables? That this estimate appears robust to reasonable specification changes, and the magnitude of its lower bound is economically meaningful, offers confidence that unobservables do not compromise the positive inference that my results make available.

In this light, decreasing PUCs' exposure to producer pressure appears capable of significantly reducing the level of capital that ILECs maintain.

[55] Besley and Coate (2003) develop evidence that electoral constituencies recognize significantly lower electricity prices in U.S. states that elect (rather than appoint) relevant regulators. Smart (1994) develops analogous evidence for the telecommunications sector. To the extent that consumer monopsonies or regulatory commitments are empirically relevant, extending this evidence to the present research suggests that telecommunications capital stocks should be relatively low, ceteris paribus, in states that elect their PUC members.

But what *normative* inference does this otherwise positive relationship support? Interpreted within standard models of related phenomena (that is, pressure group competition, the capital levy problem, and regulatory treatment of real options), it evidences a realized capacity for decreased producer pressure to diminish economic performance. In addition, a model where electoral accountability checks regulatory biases for overinvestment appears less relevant because my measured outcome is subject to price-cap (as opposed to ROR) regulation. Although my evidence takes a reduced form, then, it finds a robust rationalization as having come through a channel where diminished producer pressure can weaken economic performance.

I close the present section by considering this robustness a bit further. Here, I exploit the recognition that my theoretical motivation not only facilitates inference from how equilibrium *quantities* relate to producer pressure, but also from whether that pressure coincides with "surpluses" or "shortages."[56] If results like those reported in Table 2 indeed evidence an inferior equilibrium, then decreases in producer pressure should also be associated with "shortages" of local exchange services.

Although the local exchange sector offers a rich quasi-experimental setting on a number of important dimensions, the measures that it yields of "excess demand" are less precise than are those of equilibrium quantities (for example, *loops*). Nevertheless, such a proxy does exist and yields corroborating (though necessarily inefficient) evidence. In particular, for each large price-capped ILEC, the Federal Communications Commission reports the percentage of installations with which residential customers were "dissatisfied."[57] To the extent that these reports reflect ILECs' attempts to ration loops through nonprice mechanisms, they can proxy for relevant shortages.

With this interpretation in mind, I find that consumers who reside in "prohibited" states face relatively large shortages.[58] For example, the percentage of dissatisfied customers and the variable *contribution limit* share a positive correlation (that is, $\rho = 0.18$). In addition, when I employ regression (2)'s specification (see Table 2) to explain cross-state variation in dissatisfied customers, the OLS coefficient estimate on *contribution limit* is large and positive. Here, holding other covariates constant, dissatisfaction *increases* by about one-third of a standard deviation when moving to states that prohibit contributions (p-value $=$ 0.28).[59] Finally, employing regression (3)'s specification (see Table 2), I find that dissatisfaction significantly *decreases* when contribution restrictions are relaxed. Here, holding other covariates constant, increasing the lower limit on

[56] I thank Andy Hanssen for highlighting the importance of making this disclosure.

[57] *See* Chart 3 of the FCC's (2004) "Quality of Service of Incumbent Local Exchange Carriers" (published in Washington, DC by the Industry Analysis and Technology Division of the Wireline Competition Bureau).

[58] ILEC jurisdictions vary by state.

[59] The mean percentage of "dissatisfied residential installations" for sampled states in 2000 is 8.7 (SD$=$ 2.8).

campaign contributions by a standard deviation is associated with almost one-quarter of a standard deviation decrease in dissatisfaction (p-value = 0.05).

IV. CONCLUSION

Popular assessments of campaign finance laws frequently characterize contribution restrictions as being "dominant" (that is, superior under *any* condition). In doing so, they ignore the potential for lobbying to improve economic performance by checking consumers' monopsonistic ambitions, strengthening regulatory commitments, or protecting real options. Evidence reported in the present paper suggests that this potential is more than a theoretical curiosity. Indeed, it confidently rejects the proposition that restricting producer-contributions necessarily promotes competition, while supporting the alternative that constraints can weaken economic performance through several plausible channels.

To be sure, this evidence does not imply that restricting campaign contributions is "dominated" either. Models like those of Garfinkel and Lee (2000)[60] and Marceau and Smart (2003), for example, show that relaxing restrictions can facilitate a second-best equilibrium, where a first-best would have obtained except for the capital levy problem's distortions. This insight points to potentially fruitful research in both the areas of "regulatory capture" and "institutions and commitment." As my introductory comments highlight, received work on regulatory capture tends to focus on producers' potential to move economies away from surplus-maximizing competitive equilibria. The present results may thus encourage a more careful evaluation of how this potential interacts with that of consumers.

Interpreted within a "capital levy problem," my results also suggest that lobbying can check the opportunistic potential of what might otherwise be discretionary political agents. This insight offers a new direction in which "institutions and commitment" scholars might look for how public governance mechanisms relate to an economy's productivity. Heretofore, scholars have found an interesting relationship between macroeconomic activity and a polity's history of expropriation.[61] Rather than continuing to focus on political agents' histories, future research might dig deeper by considering how laws that influence competing *principals*' (for example, producers' and consumers') capacity to exert pressure affect economic possibilities. The institutional settings in which antitrust laws and regulations are administered offer a rich comparative setting in which to advance in this direction.

[60] Garfinkel & Lee, *supra* note 18, at 650.
[61] Acemoglu, *supra* note 17.

APPENDIX A

Table A1. Data Descriptions

Variable	Description	Source
Loops	Loops per 1000 households that incumbent local exchange carriers (ILECs), subject to price-cap regulation, maintained on December 31, 2000	*Trends in Telephone Service* (2002, Table 8.2)
Contribution limit	Equals 1 if 2000 state campaign finance law prohibited regulated utilities from contributing to state level candidates	Feigenbaum and Palmer (2000)
Education	Percent of 1990 population, 25 years and older, that graduated high school	Geospatial and Statistical Data Center (GEOSTAT)
Income	1990 median household income (in thousands)	Geospatial and Statistical Data Center (GEOSTAT)
Age 65	Percentage of 1990 population aged 65 and over	Geospatial and Statistical Data Center (GEOSTAT)
Clinton	Percentage of 1992 general election ballots for the Democrat presidential nominee, Bill Clinton	Federal Election Commission (2003)
Population Density	Population per square mile	Geospatial and Statistical Data Center (GEOSTAT)

APPENDIX B

Table B1. Evaluating Omitted Variables Bias, Selection on Observables Versus Unobservables

State	*Loops*	*Contribution limit*	*Education*	*Income*	*Age 65*	*Clinton*	*Population density*
Alabama	1546	0	66.9	28.7	13.0	40.9	81.5
Arkansas	1200	0	66.3	25.4	14.9	53.2	46.0
California	2270	0	76.2	40.6	10.5	46.0	198.1
Colorado	2240	0	84.4	35.9	10.0	40.1	33.4
Delaware	2420	0	77.5	40.3	12.1	43.5	353.4
Florida	2199	0	74.4	32.2	18.3	39.0	250.0
Idaho	1999	0	79.7	29.5	12.0	28.4	12.9
Illinois	1889	0	76.2	38.7	12.6	48.6	208.9
Indiana	1715	0	75.6	34.1	12.6	36.8	157.7

Continued

Table B1. *Continued*

State	*Loops*	*Contribution limit*	*Education*	*Income*	*Age 65*	*Clinton*	*Population density*
Iowa	1391	0	80.1	31.7	15.4	43.3	50.2
Kansas	1695	0	81.3	33.0	13.9	33.7	30.7
Louisiana	1613	0	68.3	26.3	11.1	45.6	98.2
Maine	1557	0	78.8	32.4	13.3	38.8	40.1
Maryland	2244	0	78.4	45.0	10.8	49.8	503.0
Massachusetts	2015	0	80.0	44.4	13.6	47.5	764.6
Michigan	1832	0	76.8	36.7	12.0	43.8	166.1
Missouri	1720	0	73.9	31.8	14.0	44.1	75.3
Nebraska	1496	0	81.8	31.6	14.2	29.4	20.8
Nevada	2795	0	78.8	35.8	10.6	37.4	12.2
New Mexico	1777	0	75.1	27.6	10.7	45.9	13.0
New York	1949	0	74.8	39.7	13.1	49.7	383.5
Ohio	1581	0	75.7	34.4	12.9	40.2	269.1
Oregon	1853	0	81.5	32.3	13.8	42.5	31.0
South Carolina	1473	0	68.3	30.8	11.4	39.9	119.7
Texas	2110	0	72.1	31.6	10.1	37.1	67.5
Utah	2190	0	85.1	33.2	8.7	24.7	22.0
Vermont	1711	0	80.8	34.8	11.8	46.1	61.8
Virginia	2061	0	75.2	38.2	10.8	40.6	161.5
Washington	1899	0	83.8	36.8	11.8	43.4	77.2
Arizona	2267	1	78.7	32.2	13.0	36.5	33.7
Connecticut	2054	1	79.2	49.2	13.6	42.2	676.8
Georgia	1850	1	70.9	33.5	10.1	43.5	116.9
Kentucky	1468	1	64.6	27.0	12.7	44.6	94.5
Minnesota	1692	1	82.4	36.9	12.5	43.5	56.1
Mississippi	1488	1	64.3	24.4	12.5	40.8	55.7
Montana	1276	1	81.0	28.0	13.3	37.6	5.6
New Hampshire	1949	1	82.2	41.6	11.2	38.9	124.3
New Jersey	2475	1	76.7	47.6	13.4	43.0	1,054.1
North Carolina	1772	1	70.0	31.5	12.1	42.7	140.3
North Dakota	964	1	76.7	28.7	14.2	32.2	9.2
Oklahoma	1546	1	74.6	28.6	13.5	34.0	46.7
Pennsylvania	1672	1	74.7	34.9	15.4	45.1	267.6
Rhode Island	1748	1	72.0	39.2	15.0	47.0	958.2
South Dakota	1066	1	77.1	27.6	14.7	37.1	9.3
Tennessee	1678	1	67.1	29.5	12.7	47.1	121.9
West Virginia	1487	1	66.0	25.6	15.0	48.4	75.1
Wisconsin	1452	1	78.6	35.1	13.3	41.1	91.9
Wyoming	1577	1	83.0	32.2	10.4	34.0	4.8
Mean (All)	1790	0.4	76.0	33.9	12.7	41.2	171.9
Limit = 0	1877	0.0	76.8	34.3	12.4	41.4	148.6
Limit = 1	1657	1.0	74.7	33.3	13.1	41.0	207.5
Sum	NA	19	NA	NA	NA	NA	NA

APPENDIX C

Table C1. Dependent Variable = *Loops*; Estimation Method = OLS; White Heteroskedasticity—Consistent Errors

Variable	(A) Coefficient	SE	(B) Coefficient	SE	(C) Coefficient	SE
Constant	2416.40	759.89***	2297.95	735.58***	2938.64	495.35***
Contribution limit	−175.23	94.70*	−152.23	88.53*	−155.40	49.93***
Education	−12.53	12.41	−7.72	11.34	−9.66	6.92
Income	45.07	17.28**	36.75	15.49**	30.74	8.09***
Age 65	−51.77	37.83	−53.97	33.23	−90.18	16.10***
Clinton	−11.49	8.71	−11.61	8.31	−7.95	5.08
Population density	0.00	0.29	0.09	0.27	0.30	0.18*
Fortune 500	−0.15	2.80				
Business lines			0.04	0.04		
N	48		48		42	
R^2	0.55		0.56		0.81	
Adjusted R^2	0.47		0.48		0.77	
$\bar{y}$	1789.98		1789.98		1777.15	
σ_y	369.45		369.45		319.11	
F-statistic	6.90		7.32		24.10	
Probability (F-statistic)	0.00		0.00		0.00	

***, **, and *, respectively indicate confidence at the 99 percent, 95 percent, and 90 percent levels.

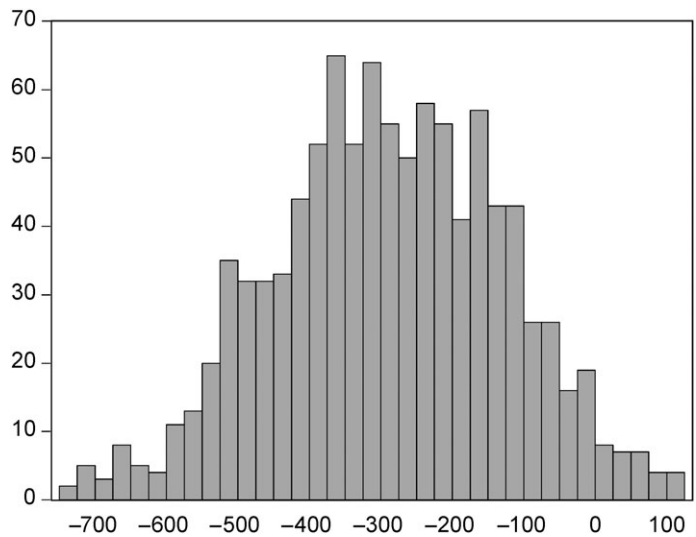

Figure C1. Frequency of OLS coefficient estimates on *Contribution Limit* when potentially influential observations are deleted from the dataset (Dependent variable = *Loops*).

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