

## Chapter 2

# Natural Experiments

### State Telecom Sectors Offer Attractive Labs for Studying Politics, Law, and Economics

We saw in Chapter 1 that by strengthening the principal–agent relationship between electorates and politicians, democratic governance can protect against collective choices that overly serve concentrated economic interests and thus improve the welfare of consumer electorates while expanding society’s economic opportunities more generally. But we also raised the principled concern that electorates can pursue their own concentrated interests, even at the expense of efficiency, and showed that this concern gives rise to a theoretically robust and observable implication – when democratic governance goes too far, firms curb their productive activity and market output decreases as a result.

The US telecommunications sector offers an attractive quasi-experimental setting in which to empirically evaluate this relationship. Importantly, institutions that influence the strength of democratic governance (e.g., campaign finance laws, election and appointment processes, voter registration rules), as well as of corresponding economic activity, vary in a comparable manner across state telecommunication sectors, and the potential for confounding variables to bias statistical inference can be readily addressed. Citing features like these, Timothy Besley and Anne Case (2003) characterized cross-state investigations as being able to yield more confident conclusions about causal relationships than might, say, cross-country studies where unobserved differences between regulatory jurisdictions and hard-to-translate institutional measures can be more difficult to address.

Exploiting this research design’s strengths, we will see in Chapter 3 that proxies for stronger democratic institutions (i.e., restrictions on campaign contributions, the selection of regulators through elections (rather than appointments), and voter registration rules that increase turnout) share a statistically significant, economically large, and negative relationship with output. Interpreted within the robust theoretical framework of Chapter 1, this evidence supports the conclusion that democratic governance not only risks giving too much weight to consumer electorates but also has likely weakened economic performance in a sector whose salient features are broadly shared.<sup>1</sup>

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<sup>1</sup> Although they may not offer the same quasi-experimental advantages as does the local exchange sector, any sector where institutions expose producers to non-market distributional influences faces

In addition, we will see that this evidence is difficult to dismiss as a statistical artifact. For example, to increase confidence that states received a “random treatment” of contribution restrictions, we will employ the innovative method of Joseph Altonji et al. (2005) for gauging “selection on unobservables” when otherwise attractive data lack interesting time series variation or when theory is relatively silent about what constitutes a good instrument.<sup>2</sup> We will also evaluate the theory using alternative proxies for the strength of democratic governance—proxies that, by construction, exhibit considerable independence from confounding variables that might bias inference from measures of campaign finance restrictions. In doing so, we will find that even the lower (absolute) bound of our estimated relationship between democratic governance and economic performance is considerable; that is, an alternative rationalization would have to explain an implausibly large share of this relationship to wholly dismiss it as an artifact.

## 2.1 General Requirements for a Natural Experiment

An attractive setting for estimating the relationship between democratic governance and economic performance would be one where democratic governance randomly varies in its strength, and the response of associated quantities to this variation can readily be observed. The local exchange sector approaches this ideal. Institutions like campaign finance laws, methods for selecting public utility regulators, and rules that govern voter registration exhibit considerable variation across states, and various statistical methods can be used 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 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 plausibly random variation in accountability.

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this type of risk (e.g., insurance, which we will investigate in Chapter 5). Fred McChesney (1987) carefully anticipated this important possibility.

<sup>2</sup>Altonji, Elder, and Taber’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 our proxy for output (i.e., land-line connections to telecommunications networks) likely exhibits noisy time series variation (e.g., 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 our regressors of interest are institutional proxies and theories of endogenous institutions are not very well-developed, good instruments can be difficult to find for the present application.

## 2.2 Experimental Conditions in the Telecommunications Sector

Producing access to telecommunications networks employs an irreversible, capital-intensive technology where local exchange companies (LECs) connect end-users to switching plants via “loops” (e.g., see Hausman and Sidak 1999, Pindyck 2003). A loop generally consists of a pair of twisted copper wires and the portion of associated infrastructure-capacity that these wires consume (e.g., trench and telephone pole space). LECs incur both initial and recurring costs to build and maintain loops and recover some of these costs via connection and line charges (Parsons 1996). If capital employment is sensitive to expectations about this cost recovery, then political forces that influence various regulated prices have a channel through which to exert real economic effects.

While Public Utility Commissions (PUCs) proximately set a number of potentially relevant prices,<sup>3</sup> interested groups in general, and LECs in particular, can exert significant influence.<sup>4</sup> 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 via the appointment process.<sup>5</sup> 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.<sup>6</sup> These institutional features offer ample opportunity for interested players to “adjust rates in order to achieve political goals” (Brock 1994).

The legal setting in which LECs attempt to influence prices also varies considerably across relevant jurisdictions. For example, the data that we will evaluate in Chapter 3 offer information on 19 (of 48 contiguous) US 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 and other measures, local exchange service producers exhibit considerable variation in their capacity to lobby relevant policy makers.

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<sup>3</sup>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 (Harris and Kraft 1997).

<sup>4</sup>Moreover, because “incumbent” LECs (not “competitive” LECs) tend to maintain sunk investments, they enjoy a comparative advantage in lobbying in models like those of Michelle Garfinkel and Jaewoo Lee (2000) and Nicolas Marceau and Michael Smart (2003).

<sup>5</sup>Nationwide, 12 states elect their public utility commissioners. Others employ an appointment process (Council of State Governments 1999).

<sup>6</sup>Since 1989, several states’ legislatures have statutorily constrained utility commissions’ authority over telecommunications rates and revenues (Zearfoss 1998). Gerald Brock (1994) argued that such channels for “micromanagement” effectively transform elected legislators into “independent telecommunication policy makers” (independent, that is, of associated regulators).

Finally, the technology for producing local exchange services constrains suppliers from offering services in jurisdictions other than those in which they confront democratic institutions of interest (e.g., lobbying restrictions). The “institutional elasticity” of supply for local exchange carriers is thus likely to be higher than for producers in other networked sectors (e.g., 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, available measures of quantity supplied in high research and development sectors like pharmaceuticals may not strongly respond to our modeled regulatory forces, since 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).

### 2.3 What Should We See if Democratic Governance Goes Too Far in This Application?

While attractive, this quasi-experimental setting leaves open issues that might weaken confidence in the inference that Chapter 3’s results make available. Perhaps the most important empirical limit comes from the difficulty of producing evidence on intermediating channels, that is, policies that result from democratically governed public choices and, in turn, influence the quantity of output that firms are willing to supply. For example, our theoretical framework from Chapter 1 suggests that we evaluate a channel like the following:

$$\text{Campaign Finance Law} \rightarrow \text{Contributions} \rightarrow \text{Regulated Price} \rightarrow \text{Output} \quad (2.1)$$

But considering this channel’s first relationship (i.e., *Campaign Finance Law*  $\rightarrow$  *Contributions*) is unlikely to produce insights that are important for our application. Yeon-Koo Che and Ian Gale (1998) showed that by strengthening the incentive for smaller players to enter the game, constraining campaign contributions can reduce, increase, or leave unchanged aggregate contributions.<sup>7</sup> Consequently, even if campaign finance laws truly influence final allocations according to the Chapter 1 theory, an empirical relationship between campaign finance laws and contributions, or contributions and prices, need not exist.

In this light, ignoring intermediating relationships, like that between campaign finance laws and campaign contributions, may not overly weaken our empirical

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<sup>7</sup>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.

research design. The design's cornerstone remains isolating plausibly random variation in the institution of interest (e.g., campaign finance law).

We may still be interested, however, in whether we can safely ignore *Regulated Price* as an intermediating channel. In other words, we may wonder whether the following structure still deserves our attention.

$$\text{Campaign Finance Law} \rightarrow \text{Regulated Price} \rightarrow \text{Output} \quad (2.2)$$

By formally reducing the channel through which democratic institutions can influence economic performance to a single regulated price, our theoretical motivation from Chapter 1 assumes that we can practically make such an evaluation.<sup>8</sup> However, PUCs regulate *numerous* prices, any combination of which might represent the true channel through which lobbying-rules influence real activity in local exchange markets.<sup>9</sup> 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 while ILECs frequently cite such pricing (and its interaction with associated retail pricing) as curbing their incentive to invest (e.g., see Jorde et al. 2000, MacAvoy and Sidak 2000, Dreazen and Young 2003, and Pociask 2003), associated regulatory decrees tend to be complex (e.g., see Squeo and Young 2004) and have therefore lacked systematic documentation (e.g., see Abel 2002). Confronted with this complexity, for example, Robert Crandall et al. (2003) excluded from their formal empirical analysis 14 states for which unbundled network element (UNE) prices are not reliable.

Rather than have our inference rely on a price index that has created difficulty with past investigations, we will empirically evaluate the following reduced form of relationship and put our research efforts instead into assessing how confidently the data speak to the *ultimate* effect of democratic institutions on economic performance.

$$\text{Campaign Finance Law} \rightarrow \text{Output} \quad (2.3)$$

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<sup>8</sup>Rui de Figueiredo and Geoff Edwards (2007) found evidence that, on its face, appears to support this channel – that is, actual contributions influence prices in the hypothesized direction. As modeled in our Chapter 1, however, capital-accumulation decisions ultimately rest on campaign contribution laws (i.e., the *potential* for consumer or producer influence). In addition, de Figueiredo and Edwards gained 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.

<sup>9</sup>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 Chapter 1, even if those relationships are empirically important.

Considering the reduced-form relationship between output and democratic institutions like campaign finance restrictions arguably addresses the law and economics question of present interest. Identifying relevant channels is important, but perhaps more so for readers who are interested in the telecommunications sector than in the general insights that this research develops about how the political setting in which laws are created ultimately affects economic performance. And while such channels plausibly exist,<sup>10</sup> establishing confidence in any *one* of them encounters considerable difficulty.

## 2.4 Conclusion

A productive research design for our purposes may thus be one that exploits a considerable richness in institutional variation to carefully measure the *gross* relationship between real activity and producers' formal capacity to pressure politicians. To be sure, 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—isolating exogenous variation in monetary policy so that causal inference about economic performance can be drawn from non-experimental data.

Finally, a less important (though perhaps not obviously so) limit of working in this sector is one of controlling for unobserved heterogeneity, that is, forces that “truly” influence economic performance but are hard to measure and happen to vary with our institutional proxies. The paucity of time series variation in our proxies for democratic institutions and output, for example, technically discourages us from drawing interesting inference from panel data. While 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 across time (e.g., campaign finance restrictions).

In addition, while states offer an attractive quasi-experimental setting on several dimensions, they do not always 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 our application. When an independent variable of interest (i.e., an indicator of contribution limits) is dichotomous, for example, instrumenting with the initial year of such limits would trivially confirm our OLS results.

More generally, lagged endogenous regressors can be “bad” instruments because, while they can share a strong correlation with the endogenous regressor, they may not be “excludable;” that is, rather than isolating the exogenous variation in a largely

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<sup>10</sup>See, for example, Smart (1994), Besley and Coate (2003), and Falaschetti (2003), each of which developed evidence that regulated prices decrease as  $\alpha$  increases (i.e., as consumer interests weigh more heavily on regulatory objectives in equation (1.1)).

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 confidently establish a restriction's validity.<sup>11</sup>

Difficulties like these are common in politics, law, and economics literatures, but stronger substitutes for addressing omitted variables bias (OV) are becoming available. Altonji et al. (2005), for example, recently developed a method for drawing causal inference from non-experimental data without relying on a priori exclusion restrictions. Coupled with other robustness checks (e.g., employing independent proxies for consumer pressure and examining alternative specifications), Altonji et al.'s method will let us 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.

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<sup>11</sup>To be sure, theory is making progress, but does little to help the present identification problem. Besley and Case (2003) and Thomas Stratmann and Francisco Aparicio-Castillo (2006), for example, used party preference and educational attainment as instruments for campaign finance laws. In our 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. For example, in unreported 2SLS regressions where measures of education and party preference act as instruments (as opposed to controls), output and consumer pressure continue to share a significant and negative relationship (as they do in corresponding OLS regressions). The coefficient estimates' magnitude, however, is implausibly large.

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