Voter Turnout, Regulatory Commitment, and Capital Accumulation: Evidence from the US Telecommunications Sector*

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Abstract

Voter turnout is frequently cited as gauging a polity's health. The ease with which electoral members can produce political support, however, can retard an economy's productive capacity. For example, while mobile electorates might efficaciously monitor political agents, they may also lack credibility when committing to regulatory policies. Consequently, a "healthy" polity's economy may rest at an inferior discretionary equilibrium. I find evidence for this hypothesis by relating voter turnout to regulated telecommunications capital. Ceteris paribus, local exchange carriers employ relatively little capital in US states that house high turnout electorates. This evidence is remarkably difficult to dismiss as an artifact of endogeneity bias.

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"Let us never forget that government is ourselves and not an alien power over us" (Franklin D. Roosevelt).

1 Introduction

Evidence from the turnout and distribution literature suggests that "mobile electorates" (i.e., political principals who exhibit strong voting propensities) recognize favorable policy distributions. V. O. Key's (39) seminal inquiry, for example, produced evidence that constituents' policy treatment is sensitive to how electoral institutions influence voting costs. Extending this inquiry, Rob Fleck ((25), (26)) finds that depression-era distributive policy favored mobile electorates. Timothy Besley and Robin Burgess (10) develop related evidence in a comparative politics setting, while others uncover such evidence from industrial organization applications. James Hamilton (32), for example, reports that mobile North Carolinians faced a significantly reduced probability of realizing expanded hazardous waste facilities. And more immediately related to the present article's application, Dino Falaschetti (22) finds that end-user prices for local exchange telecommunications services are significantly lower in US states that house high-turnout electorates.

In this light, electoral mobility appears to significantly influence distributive policy.[1] This appearance, in turn, can provide motivation for democracy-advocates to argue (at least implicitly) that welfare increases monotonically with electoral participation.[2] While such participation might make representatives

[1] It is important to note that the post-Key evidence cited above comes to us from **pliable** policies - i.e., policies over which electoral constituents' preferences point in the same direction. Here, the capacity for electoral constituents as a whole to produce political support, not the mobilization of any particular constituency, appears influential. This empirical regularity is rationalizable via a menu-auction game in the spirit of Douglas Bernheim and Michael Whinston (9). Bidders in such a game (e.g., interested constituencies) compete over a fixed prize's distribution (e.g., an allocation of sunk costs for producing telecommunications' services) based on their capacity to remunerate an auctioneer (e.g., political agent).

[2] Prominent organizations such as the Institute for Democracy and Electoral Assistance (IDEA), for example, characterize strategies that would increase electoral participation as being dominant. A recent IDEA conference on "Building Electoral Participation" is illustrative. Associated details are available from http://www.idea.int/whatsnew/whatsnew.html (accessed July 31, 2003).

"better" political agents, however, its prospect can also check electoral members' capacity to commit to economic agents against pursuing expropriative polices. Empirical results reported in the present article suggest that this second effect can indeed overwhelm the first. In particular, they are consistent with electoral members' capacity to influence political agents checking an economy's productive capacity by weakening regulatory commitments that might otherwise encourage investment.

To be sure, my argument here is not that electoral participation necessarily reduces welfare, but rather that it can move economies towards inferior discretionary equilibria. We can see this distinction more clearly by considering a model of rent seeking through regulation (e.g., see Gary Becker (8); Sam Peltzman (54); and George Stigler (64)).[3] Left unopposed, firms pressure political agents to push prices to their monopoly levels.[4] Mobile electorates, on the other hand, can counter this force. If, for example, the weight with which political agents consider constituents' preferences increases with constituents' capacity to produce political support,[5] and if electoral members' unit of support is a vote, then mobility should drag prices away from their monopoly levels, increase output, and thus increase the economy's total surplus. Here, electoral mobility enhances welfare by essentially constraining producers' pursuit of surplus-reducing regulatory rents.

Making politicians "better" agents, however, also increases regulated subjects' exposure to electoral expropriation. The "institutions and commitment" literature already suggests that political agents' capacity for ex post re-distribution curbs productive activity.[6] So shouldn't that same capacity for

[3] Thomas Lyon (44) employs a related framework to distinguish whether electricity regulation's migration from municipalities to states reflects utilities' capacity to capture regulators or political agents' incentive to strengthen regulatory commitments.

[4] Opposing firms might, nevertheless, produce policy equilibria that are close to electoral constituents' ideals (e.g., see Arthur Denzau and Michael Munger (18)). I explicitly control for this potential in my empirical investigation.

[5] Bernheim and Whinston (9) show that just such a capacity influences distributions in menu-auction games, such as those that are played between utility regulators and interested support constituencies.

[6] See, for example, Daron Acemoglu, Simon Johnson, and James Robinson (2); Falaschetti (23); Witold Henisz (35); Brian Levy and Pablo Spiller (42); Douglass North (48), (49); North and Barry Weingast (50); Dani Rodrik, Arvind Subramanian, and Francesco Trebbi (56); and

electoral principals exert a related force? For example, if electoral principals' capacity to shift resources influences property right stability, then evidence on turnout's distributional implications may reflect electorates' ability to expropriate associated investors' product. Here, voting costs might enhance electoral constituents' capacity to commit against playing time inconsistent strategies, rather than simply reduce their capacity to counter organized interests' weight in a political agent's objective.

Whether electoral mobility enhances welfare in any particular application thus appears to be an important, open, and empirical question. I address this question here by evaluating how telecommunications capital varies with voter turnout across US states. The US telecom-sector offers an especially rich empirical setting in which to pursue this investigation.[7] Service-producers incur extensive sunk costs while facing (as well as generating) considerable political forces. These forces, in turn, vary across states. For example, a state's local exchange carriers (LECs) do not set prices alone, but rather do so in concert with associated public utility commissioners (PUCs). Electoral members, in turn, have available a number of channels through which to influence associated regulatory processes (e.g., offering contingent support to relevant political agents). To the extent that these members can jointly produce political support, say through turning out to vote, they can either (i) check service providers' political pressure or (ii) opportunistically decrease LECs' ability to recognize their investments' product.[8]

If electoral constituents simply weigh against organized pressure groups in a political agent's static objective, then capital should increase with turnout. Here, electoral mobility pushes regulated prices towards their competitive levels and thus increases output from its otherwise inferior monopoly level. On the

David Stasavage (63).

[7] Besley and Anne Case (11) review the more general benefits of employing a cross-state institutional analysis to understand related political economy questions. Spiller (62) highlights the value of studying commitment problems via applications to utilities.

[8] Throughout the paper, I refer to opportunistic actions as those that strategically exploit ex-post bargaining positions and thus curb rational agents' ex-ante incentives to enter otherwise mutually beneficial transactions.

other hand, if electoral mobility reflects political principals' capacity to expropriate sunk investments' product, then capital should decrease with turnout. Here, just as political agents' ability to strategically exploit ex-post bargaining positions appears to retard real activity, political principals' lack of commitment-capacity would also reduce welfare.

My present data offer robust evidence that forces associated with this second case overwhelm those associated with the first. In particular, they suggest that local exchange carriers employ significantly smaller capital stocks in US states that house high-turnout electorates. Since these capital stocks are subject to price cap regulation (as opposed to, say, rate of return regulation), this relationship is unlikely to evidence electoral mobility's capacity to enhance welfare by checking regulatory distortions. Moreover, this relationship is remarkably difficult to dismiss as an artifact of endogeneity bias. For example, a significant and negative relationship between turnout and capital persists when I instrument for turnout via an electoral institution proxy.[9] An overidentification test supports this strategy's merits by suggesting that this proxy can confidently be excluded from the regressor-set (i.e., it does not independently relate to telecommunications capital). Other considerations of omitted variables bias (OVB) corroborate this suggestion. Indeed, for capital's significant and negative relationship with turnout to be artifactual in this regard, unobservables would have to explain over three times the variation of interest as do my observables.[10] OVB also appears innocuous in light of the capital-turnout relationship's remarkable stability across numerous alternative specifications of the regressor matrix. This stability is unlikely to exist if unobserved variables truly account for turnout's relationship with capital in the present data. Finally, support for my identification strategy's validity comes from the bootstrapped distribution of my instrumental variable (IV) estimate, as well as a non-parametric matching estimator, both of which confidently suggest that my measured relationship between capital and turnout is not a sample-size artifact.

[9] This proxy is an indicator of whether states allow election day registration.

[10] This treatment of OVB draws on Joseph Altonji, Todd Elder, and Christopher Taber's (4) recent innovation for evaluating causation from non-experimental data.

While potentially new, though, this evidence may not be surprising. Indeed, if valid evidence exists that political agents' discretionary capacity can retard real activity, then it should also exist for those agents' principals. One should be careful, however, about interpreting the present research's normative implications. In particular, they do not say that restricting the franchise is a dominant strategy. Rather, they simply highlight the potential for political discretion, whether it is placed with electoral principals or their agents, to retard real activity.[11] Understanding this implication is nevertheless important since much (if not all) of the literature's attention aims at strategically organizing the institutional landscapes on which agents play political games. Evidence reported here suggests that electoral constituents' relative bargaining power is also a salient consideration.[12]

I develop this evidence more fully in the article's remainder. In the following section, I argue that the institutional landscape on which telecommunications policy games are played offers numerous channels through which electoral members (and other interested individuals) can influence relevant regulatory decisions. I also argue that, for electoral mobility to enhance welfare through such channels, it must create pressure for increased output (and thus capital levels). This observable implication exactly opposes that from an institutions and commitment setting. There, mobility reduces electoral constituents' capacity to commit against expropriating the product of telecoms' sunk investment. This discretion, in turn, decreases the equilibrium level of capital below that of the corresponding (and superior) commitment equilibrium. In Section 3, I evaluate these competing arguments' empirical relevance by examining how voter turnout relates to telecommunications capital across US states. The resulting evidence is robust and consistent with turnout reducing total surplus (at

[11] I thank Gary Miller for encouraging this interpretation.

[12] Philippe Aghion, Alberto Alesina, and Trebbi (3) also call attention to this consideration - e.g., see their section on "insulation, inequality, and voting rights." Nicolas Marceau and Michael Smart (45, p. 241) offer the related observation that "Democratic governments must be responsive to the desires of voters, and so they may find it difficult to commit to stable policies over time." Their investigation turns, however, to how corporate lobbying can work against electoral constituents' contribution to the "capital levy problem."

least on the margins considered here). I conclude in Section 4 by relating this evidence to that appearing in the institutions and commitment literature and considering opportunities for future research.

2 Theoretical Motivation

The US telecommunications sector offers a rich empirical setting in which to evaluate conventional wisdom as it relates to electoral participation's welfare consequences. Here, the institutional landscape from which telecom policy emerges offers interested individuals numerous channels through which to exert influence. Electoral constituents' capacity to vote, for example, can directly pressure elected regulators, or elected appointers and overseers who can pressure non-elected regulators. The manner in which such pressure influences an economy's total surplus, in turn, creates observable implications. In particular, if electoral mobility expands surplus, then output should increase with the capacity for turnout. The opposite relationship should be evident where mobility contracts surplus.

2.1 Electoral mobility and telecommunications policy

Producing access to telecom networks employs a (physical) capital intensive technology. In short, local exchange companies (LECs) connect end-users to switching plants via "loops." 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 (Steve Parsons (52)). If capital employment is sensitive to expectations about recovering these costs, then political forces that influence telecommunications prices in general, and end-user connection and line charges in particular, have a channel through which to exert real effects.

Telecommunications prices indeed appear subject to such forces. For example, while Public Utility Commissions (PUCs) ultimately set relevant prices,[13] interested groups in general, and electoral constituents in particular, can exert significant influence. Groups might offer to elected commissioners contributions or votes contingent on relevant prices. They might also offer contingent support to governors and legislators who, in turn, can influence prices via the appointment process.[14] Finally, whether commissioners are elected or appointed, interested groups might influence prices by contingently supporting governors and legislators who, in turn, can sway commissioners by altering a PUC's regulatory authority or budget.[15] These institutional features offer ample opportunity for interested players, "within a single political jurisdiction," to "adjust rates in order to achieve political goals" (Brock (14)).

Roger Noll (47) identifies an additional channel through which state-specific political forces can influence telecom prices. In particular, he argues that the

> "principal source of challengers to incumbent federal legislators is state and local government. Governors and big-city mayors run for the Senate, and state legislators and other local officials run for the House. To the extent that the basic exchange rate becomes a salient political issue at the state and local level, incumbent legislators could become vulnerable to challenges based in part on their association with the big increases in telephone prices" (47, emphasis added).

That political agents can reasonably expect this issue to become salient is evident, for example, in Representative Timothy Wirth's (D, CO) early 1980s election fortunes. While telecommunications policy is complex, residential users

[13] Regulatory jurisdiction over telecommunications policy is split 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. Jurisdiction over long-distance services is split, on the other hand, with the FCC regulating interstate service and state regulatory or public utilities commissions regulating intrastate service (Robert Harris and Jeffrey Kraft (33)).

[14] Nationwide, 12 states elect their public utility commissioners. Others employ an appointment process (Council of State Governments (17)).

[15] Since 1989, several states' legislatures have statutorily constrained utility commissions' authority over telecommunications rates and revenues (Nancy Zearfoss (69)). Gerald Brock (14) argues that such channels for "micromanagement" effectively transform elected legislators into "independent telecommunication policy makers" (independent, that is, of associated regulators).

can easily monitor its effects on associated prices. Consequently, potential challengers have a pliable and relatively transparent policy with which to mobilize otherwise "inattentive publics." Challengers apparently employed this issue against Wirth, a prominent advocate of telecommunications deregulation. Indeed, when deregulation increased local exchange prices, Wirth's winning margin slipped by almost 10%. Wirth's principal assistant attributes this drop to the price increase (Douglas Arnold (6)).

Other informal evidence also appears consistent with political agents being sensitive to residential users' preferences over local service policy. For example, as part of the telephone industry deregulation and breakup of AT&T, the Federal Communications Commission (FCC) attempted to increase a component of residential users' access fee by $6 per month. The House, however, voted overwhelmingly to bar this increase's implementation. The proposed increase

> "was a visible, immediate addition to every customer's telephone bill. . . It was thus relatively easy to create a 'consumer rip-off' issue alleging that this was a plan to help large businesses. . . at the expense of ordinary consumers" (Brock (14)).

The Senate was about to concur with the House when the FCC postponed the increase. Subsequently, the FCC implemented a $1.00 fee with increases phased in annually (Arnold (6)).

The theoretical motivation for these anecdotes is that potentially mobile electorates are unlikely to remain latent when policies on which their preferences agree (e.g., end-user prices) move against them. Falaschetti (22) supports this motivation with formal evidence that local exchange prices decrease significantly with increases in electoral constituencies' capacity to produce political support. In this light, whether PUCs receive pressure directly from electoral constituencies, or indirectly via federal and state legislators and executives, channels appear to exist through which electoral preferences can weigh on telecommunications prices. If, in turn, these prices influence LECs' asset returns, then relevant capital levels should vary across states according to electoral constituents' ability to act on those preferences (i.e., to produce political support).

2.2 Electoral mobility and rent seeking through regulation

Whether electoral mobility's influence in this regard enhances welfare creates distinct observable implications. For example, consider the following extension of Peltzman's (54) "majority generating function."

$$M(\theta p, (1-\theta)\pi) \tag{1}$$

Here, p denotes the "price" that electoral constituents face for connection services, π denotes an LEC's "profit," and θ denotes the capacity for electoral constituents to produce political support relative to that for organized interests (i.e., θ reflects electoral constituents' voting propensity).[16]

If electoral constituents exhibit zero capacity to produce support (i.e., $\theta = 0$), a political agent would maximize its support (1) by maximizing the LEC's profits - i.e., by setting price p to its monopoly-level p^M.[17] As electoral constituents' relative capacity to produce support increases (i.e., as $\theta \longrightarrow 1$), this agent's objective places increasing weight on the electoral constituency's welfare.[18] Given M's curvature properties, such movements will encourage agents to decrease price p below p^M.[19] Moreover, for $p \in \left[p^C, p^M\right]$ where p^C denotes the competitive price-level, these decreases will encourage profit maximizing LECs to continuously increase output. Over this range, price decreases will

[16] $M \subset C^2$, $M_1 < 0$, $M_{11} > 0$, $M_2 > 0$, $M_{22} < 0$, $\theta \in [0, 1]$, and $\pi = f(p)$.

[17] Given that $\frac{d\pi}{dp} < 0$ for $p > p^M$, and $M_1 < 0$, a rational agent will never set price above p^M.

[18] This evaluation implicitly assumes that turnout is exogenous. I formally address this assumption in my empirical investigation.

[19] Bernheim and Whinston (9) explain this phenomenon in a "menu auction game" where political agents fully allocate fixed costs between competing interests, and interests attempt to influence agents by offering political support "menus" (i.e., lists of support that groups supply as a function of agents' feasible actions). In this setting, interested constituencies influence agents' decisions according to their capacity to produce support. Hence, if a PUC's pricing decisions allocate LECs' capital expenditures between investors and end users, and if voter turnout reflects electoral constituencies' capacity to produce political support, then access prices should vary inversely with turnout.

cause total surplus **and** capital to increase. In this model, electoral mobility increases welfare by dragging prices away from their monopoly levels and thus exhibits a **positive** relationship with output.[20] For ease of reference, I restate this implication as Hypothesis 1.

> **Hypothesis 1**: If electorates' capacity to produce political support (i.e., votes) increases welfare, then telecommunications capital should **increase** with voter turnout.

2.3 Electoral mobility and regulatory commitment

In a Peltzman-type model, electoral members' capacity to produce political support enhances welfare by essentially checking producers' ability to seek regulatory rents. Whether mobility indeed enhances welfare, however, is sensitive to whether its weight against organized interests overwhelms its capacity to facilitate electoral opportunism. The "institutions and commitment" literature following North's (48) and North and Weingast's (50) seminal contributions suggests that capital-intensive production processes are especially sensitive to such opportunism. Given that producing local exchange services is capital intensive, and if electoral members' capacity to expropriate the product of sunk investment increases with their capacity to produce political support, turnout and telecommunications capital could thus exhibit a **negative** relationship.

Finn Kydland and Edward Prescott's (41) seminal model makes this implication more transparent. Adapted to the present application, this model says that electorates solve the following two-period problem.

$$\max_{p_1, p_2} \{S(k_1, k_2, p_1, p_2)\} \tag{2}$$

subject to

$$k_1 = K_1(p_1, p_2) \tag{3}$$

[20] If, as $\theta \longrightarrow 1$, the agent optimally chooses p such that $p < p^C$, then increased turnout would be associated with decreased capital levels and lower welfare.

$$k_2 = K_2 \ (k_1 \ , \ p_2) \tag{4}$$

Here, social welfare $S \subset C^2$ increases at a decreasing rate with telecommunications capital k_t and decreases at a decreasing rate with service prices p_t $(t = 1, 2)$, while service providers' induced capital choices $K_t \subset C^2$ increase in each of their arguments. Evaluated at welfare maximizing prices $(p_1^*, \ p_2^*)$, the following condition must hold.

$$\frac{\partial S}{\partial k_1}\frac{\partial K_1}{\partial p_2} + \frac{\partial S}{\partial k_2}\frac{\partial K_2}{\partial k_1}\frac{\partial K_1}{\partial p_2} + \frac{\partial S}{\partial k_2}\frac{\partial K_2}{\partial p_2} + \frac{\partial S}{\partial p_2} = 0 \tag{5}$$

To the extent that mobility expands electorates' capacity to strategically act on ex-post bargaining positions, however, voters will "replan" in period 2 by solving equation (2) subject to the following constraints.

$$p_1 = \overline{p}_1 \tag{6}$$

$$k_1 = \overline{k}_1 \tag{7}$$

$$k_2 = K_2 \left(\overline{k}_1 \ , \ p_2 \right) \tag{8}$$

For an interior solution, the price p_2^d (where d denotes "discretionary") that maximizes welfare (2) subject to constraints (6, 7, and 8) must fall short of that which maximizes welfare subject to constraints (3 and 4) - i.e., $p_2^d < p_2^*$. This relationship follows from p_2^d having to satisfy the following condition

$$\frac{\partial S}{\partial k_2}\frac{\partial K_2}{\partial p_2} + \frac{\partial S}{\partial p_2} = 0 \tag{9}$$

(rather than that of equation 5) and the curvature properties assumed for S and K_t.

Interpreted within this model, electoral mobility checks political principals' capacity to commit to the optimal plan $(p_1^*, \ p_2^*)$, thus leaving them at the inferior discretionary equilibrium where both capital and social welfare are relatively low (i.e., $p_2^d < p_2^* \longrightarrow k_t^d < k_t^* \longrightarrow S^d < S^*$). Considering pressure group competition within this dynamic framework creates the following observable implication.

> **Hypothesis 2**: If electorates' capacity to produce political support (i.e., votes) decreases their ability to commit to the optimal plan $(p_1^*,\ p_2^*)$, then telecommunications capital should decrease with voter turnout.

Here, just as in the static pressure group model, the "distance" between electoral constituents and their political agents decreases with electorates' capacity to produce votes. This model's dynamics highlight, however, that reducing agency costs increases regulated producers' exposure to re-contracting risk. If forces associated with this increased risk overwhelm those associated with decreased agency costs, then electoral mobility will reduce an economy's surplus.

3 Empirical Results

The institutional landscape on which interested individuals play telecom-policy games offers numerous channels through which electoral mobility can exert downward pressure on service prices. Whether this distributive force expands or contracts an economy's surplus, in turn, creates opposing observable implications. If turnout increases welfare in a competing pressure group model, and given the capital-intensive technology with which local exchange services are produced, turnout should exhibit a positive relationship with regulated capital. A negative relationship between turnout and capital, on the other hand, evidences inferior welfare levels in a model where mobility decreases electorates' capacity to commit against opportunistic expropriations. Whether turnout exhibits a positive or negative relationship with telecommunications capital can thus shed new light on the turnout and distribution literature's normative implications.

3.1 Data

To evaluate this relationship, I examine how telecommunications capital stocks vary with voter turnout across the contiguous US states.[21] While several measures exist for these stocks, I focus on the variable *Loops*, which equals the number of loops per 1,000 population that incumbent local exchange carriers (ILECs), subject to price cap regulation, maintained on December 31, 2000.[22] Table 1 summarizes this variable's distribution, as well as those for other variables introduced below.[23]

——Insert Table 1 Here——

Loops refines the dependent variable that Henisz and Bennet Zelner (36) use in relating political executives' discretionary capacity to telecommunications infrastructure penetration. These authors attribute evidence of this capacity's negative relationship with telecommunications penetration 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 intense incentives to efficiently monitor under "used and useful" rate of return regulation (Richard Gilbert and David Newbery (29)). Hence, while Henisz and Zelner's evidence appears consistent with Hypothesis 2 (but at the level of political agent rather than that of electoral principal), it may instead reflect a superior outcome where executive-monitoring checks service providers' incentive to overinvest under regulatory distortions.[24]

[21] Faced with significant adjustment costs, LECs' investment tends to be discrete (personal communication with Steve Parsons of Parsons Applied Economics). Electorates' capacity to produce political support, on the other hand, exhibits little in the way of time series variation (at least over the period for which telecommunications infrastructure data are readily accessible). I thus identify this capacity's real effects from its cross sectional variation with accumulated investment.

[22] Recall that loops are land-line connections between end users and switching facilities.

[23] Appendix A collects in one place each variable's description.

[24] I thank Jonah Gelbach and Roger Noll for highlighting the importance of making this distinction. Harvey Averch and Leland Johnson (7) offer a seminal and formal treatment of how regulation can influence firms' investment decisions.

By constraining itself to service providers who do not face such distortions (i.e., price capped ILECs), *Loops* diminishes this alternative interpretation. Indeed, removing the incentive for "over-capitalisation" is one of price cap (or "incentive") regulation's frequently purported benefits, and the manner in which US regulators have implemented incentive regulation appears to have, in large part, facilitated these benefits' realization (e.g., see David Sappington (58)). Moreover, even in repeated games, regulators who take consumer surplus as their objective face relatively intense pressure to act opportunistically under price cap regulation (Gilbert and Newbery (29); Levy and Spiller (42); Spiller (62)). Applied to the telecommunications sector, such actions can be played by re-setting 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" (Sappington (58, p. 285)). Paolo Panteghini and Carlo Scarpa (51) offer illustrative examples of this phenomenon where UK regulators, "subject to considerable political pressures," appear to have opportunistically decreased utilities' output prices in just such a manner. In this light, the present investigation appears capable of developing relatively clean inference about electoral mobility's relationship to regulatory commitments.[25]

The independent variable of interest is *Turnout*, which equals the average percentage of voting age individuals that cast ballots for the office of president in the 1992, 1996, and 2000 general elections.[26] Hypothesis 1 says that, if electoral mobility enhances welfare, *Loops* should increase (ceteris paribus) with *Turnout*. Hypothesis 2, on the other hand, suggests that a negative relation-

[25]In unreported regressions, I also evaluate electoral mobility's relationship to alternative measures of telecommunications capital - i.e., a broader measure of capital employed to produce local exchange services and a measure of high speed infrastructure. Unlike *Loops*, these substitute measures cannot distinguish whether the underlying assets are subject to price cap regulation. They do, however, reflect capital to which individuals' welfare may appear more immediately sensitive. Results from these regressions are largely consistent with those reported below.

[26]By employing an average level of turnout, I attempt to reduce the error in measuring each constituency's capacity to produce political support. Since my investigation is restricted to cross-sectional data, employing any particular year's turnout would increase exposure to potentially spurious year-specific shocks. Nevertheless, in unreported regressions, results reported here do not appear sensitive to employing any particular years' turnout as a regressor. In addition, they do not appear sensitive to employing a longer run average (i.e., from 1960 through 2000) of turnout.

ship between *Turnout* and *Loops* would reflect an inferior equilibrium. Figure 1 provides a coarse illustration of how the data are organized across these dimensions and preliminary evidence for Hypothesis 2.[27]

——Insert Figure 1 Here——

To evaluate this relationship more carefully, I control for demand and supply side forces, as well as the capacity for interested individuals other than electoral members to exert political influence.[28] In particular, I control for consumers' budget constraints and preferences by employing the variables *Population Density*, *Education*, *Income*, *Poverty*, and *Age* 65 as a set of demographic regressors. To address supply side forces, I control for the costs that local exchange carriers incur for maintaining loops via the regressor *Loop Cost*.[29] Finally, I employ the variables *LEC Concentration*, *LD Concentration*, and *Fortune* 500 to address the potential for LECs, long distance service providers, and businesses, respectively, to exert either political or market influence on relevant capital stocks.

3.2 Identification strategy

To identify electoral mobility's relationship with telecommunications capital, I employ the variable *Election Day Registration* (*EDR*) as an instrument and subject the consequent coefficient estimate to numerous robustness checks.[30] To facilitate valid inference, *EDR* must share a strong first stage or "reduced

[27] This figure also highlights potentially influential outliers, such as the graph's most "northwesterly" observation (i.e., Nevada). Regression results reported below are robust to omitting this and other potentially influential observations.

[28] Jaison Abel (1) formally develops a similar specification for a related application. U. Sankar (57, Equation (10)) does so for electric utilities' desired level of capital stock - i.e., for another sector where production technologies exhibit scale economies and limited scope for substituting labor and capital.

[29] Note that forces associated with the regressor *Population Density* (e.g., loop length) can also influence relevant costs.

[30] *EDR* equals 1 for states where prospective voters can register on election day. In the present sample, *EDR* states are Idaho, Maine, Minnesota, New Hampshire, Wisconsin, and Wyoming. Because North Dakota does not require registration, I also treat it as being a member of this group. In unreported regressions, I follow Benjamin Highton (37) by coding *EDR* to equal one only for states that are early adopters of this electoral institution (i.e., North Dakota, Maine, Minnesota, and Wisconsin). Inference from this alternative coding is qualitatively identical to that reported here.

form" relationship with *Turnout* (i.e., the potentially endogenous regressor), but maintain no independent variation with *Loops* (i.e., it must be redundant in the second stage or "structural" equation). Evidence appearing in the voter turnout literature suggests that *EDR* is a "good" instrument, at least with respect to this first criterion. For example, following Raymond Wolfinger and Steven Rosenstone's (66) seminal work, Besley and Case (11), Highton (37), and Samuel Patterson and Gregory Caldeira (53) report evidence that registration closing dates significantly influence electoral mobility. Parameter estimates disclosed in my Appendix B suggest that the present data exhibit a similar relationship.

In this light, *EDR* appears to satisfy at least a minimal requirement for being a valid instrument. However, for associated inference to be unbiased, *EDR* must also be "excludable" - i.e., *EDR* must relate to *Loops* only through *Turnout*. Besley and Case (11, Table 16) offer some support for this excludability via evidence that states may have randomly received the election day registration "treatment." Here, *EDR* appears unrelated to changes in either legislatures' political composition or states' demographic characteristics.

While this evidence is supportive, however, it does not alone warrant a strong maintenance of *EDR* satisfying the exclusion restriction. I thus further evaluate *EDR*'s potential redundancy by adding an indicator of whether states hold open primaries to the set of instruments (i.e., the variable *Open Primary*) and testing the consequent overidentification restriction (i.e., evaluating whether the over-identified estimate differs significantly from its just-identified counterpart). *Open Primary*'s "first stage validity" finds support in Besley and Case's (11, Table 5) evidence that general election turnout increases significantly with primary elections being open. Parameter estimates reported in my Appendix B suggest that the present data also exhibit this strong first stage relationship. Consequently, if both *EDR* and *Open Primary* satisfy the exclusion restriction, then IV estimates from associated overidentified specifications should not differ significantly from their just identified counterparts. Evidence that I report in Table 2 (and discuss further below) is consistent with this difference being negligible and thus furthers confidence that my IV estimates are valid.

I gain additional confidence that omitted variables are not problematic in this regard by employing Altonji et al.'s (4) recent innovation for evaluating causation from non-experimental data. In short, this method lets me measure how much of the coincident variation between *Loops* and *Turnout* would have to be attributable to unobservables for OVB to completely rationalize a relationship like that illustrated in Figure 1. To the extent that unobservables would have to explain "a lot" of this relationship (relative to observables), one can gain confidence that channels do not exist through which an instrument and dependent variable can vary independently. Pushing even further in this direction, I explicitly evaluate my IV (and OLS) estimates' robustness to "expanded" regressor sets - i.e., those that include numerous potentially influential, but otherwise "unobserved," variables. Robustness here would offer confidence that ignoring the still myriad variables that I do not (and indeed cannot) explicitly consider creates little potential for biasing my coefficient estimates. Evidence from each of these expansions strongly corroborates that developed elsewhere in this section - i.e., it suggests that *Turnout*'s significant and negative relationship with *Loops* evidences a real phenomenon rather than an artifact associated with endogeneity bias.

Finally, I consider the potential for turnout's significant and negative relationship with telecommunications capital to be a sample-size artifact. The strength of my first stage regressions (reported in Appendix B) offers some assurance that bias associated with the IV estimator's finite sample properties does not afflict inference from coefficient estimates reported below (Angrist and Krueger (5), Hahn and Hausman (31), and Jeffrey Wooldridge (67)). It does so because finite sample bias emerges from the "structural" and "reduced form" regressions' errors being correlated, and thus grows as the first-stage regressions' goodness of fit decreases.[31]

To gain additional confidence here, I "re-sample" the present data 1,000

[31] Since this same correlation also biases the OLS estimator, the 2SLS estimator's finite sample properties can bias it towards its OLS counterpart.

times to bootstrap my IV estimate's distribution. The resulting bias-corrected estimate and confidence intervals are similar to those associated with my OLS and IV estimates. Finally, I evaluate *Turnout*'s relationship with *Loops* via the "nearest neighbor" matching estimator. This nonparametric method is attractive here because its estimates do not rely on asymptotics for their validity.[32] Nevertheless, it too returns evidence consistent with that from my parametric estimates.

3.3 Reduced form evidence

My evaluation of Hypotheses 1 versus 2 begins with reduced form evidence of *Turnout*'s relationship to *Loops*. I develop this evidence by estimating parameters from the following model.

$$Loops_i = \alpha_0 + \alpha_1 Turnout_i + \sum_{k=2}^{K} \alpha_k Controls_{k,i} + u_{1i} \tag{10}$$

Results from this estimation, a representative sample of which are reported in Table 2, appear consistent with Hypothesis 2 - i.e., at least on the margins evaluated here, they evidence a realized potential for electoral mobility to weaken regulatory commitments and thus leave an economy (sector) at an inferior discretionary equilibrium.

——Insert Table 2 Here——

Regression (1) makes explicit the relationship that *Turnout* and *Loops* exhibit in Figure 1. Here, *Turnout*'s coefficient estimate (i.e., -5.96) says that a standard deviation increase in electoral mobility (i.e., *Turnout*'s approximate increase from Virginia to Michigan) is associated with just under a 1/3 stan-

[32] Its validity does rest, however, on only observables having selected states to receive the *EDR* "treatment." For the matching estimator to facilitate valid inference, selection into the set of *EDR*-states must be random in the sense that, conditioned on observables, outcomes for treated and non-treated states differ only from the electoral institution's influence (e.g., see Richard Blundell and Monica Costa-Dias (13) and James Heckman, Hidehiko Ichimura, and Petra Todd (34)). I implicitly address this dimension of the matching estimator's validity when evaluating *EDR*'s potential to violate the exclusion restriction - e.g., see my discussion below of Altonji et al.'s (4) method.

dard deviation decrease in telecommunications capital (i.e., *Loops*' approximate decrease from Virginia to Michigan).[33]

In regression (2), I begin to evaluate whether this relationship can be attributed to endogeneity bias. I do so by controlling for both supply and demand side forces, as well as influences from potentially competing interests. The coefficient estimate on $Turnout$ (i.e., -11.86), nevertheless, remains consistent with Hypothesis 2. In particular, it suggests that a standard deviation increase in electoral mobility is associated with nearly a 2/3 standard deviation decrease in telecommunications capital. I treat the potential for endogeneity bias more generally in regression (3) by employing EDR as an instrument for $Turnout$. The coefficient estimate on $Turnout$ (i.e., -11.05), however, changes only negligibly from its OLS counterpart - e.g., the *p*-value from Hausman's test is 0.84.

These results certainly support Hypothesis 2. But, they do not firmly establish the case against $Turnout$'s potential endogeneity. For example, Hausman's test might incorrectly dismiss this potential if EDR shares an independent relationship with *Loops* - i.e., if EDR is related to an influential but heretofore unobserved variable. I begin to address unobservables' potential influence in this regard by adding $Open\ Primary$ (i.e., an indicator of whether states maintain open primaries) to the set of instruments. $Open\ Primary$ appears to share a strong first stage relationship with electoral mobility. Besley and Case (11) report evidence of such a relationship (e.g., see their Table 5), as does my Appendix B for the present data. Consequently, if EDR violates the exclusion restriction but $Open\ Primary$ is truly redundant in equation (10) (or vice-versa), then the overidentified estimate of $Turnout$'s coefficient should dif-

[33]Michigan is associated with the 15^{th} highest level of $Turnout$ in the present data, while Virginia is associated with the 30^{th} highest. A qualitatively similar relationship persists throughout $Turnout$'s distribution. For example, $Turnout$ for Maine (i.e., the highest $Turnout$ state) is about one standard deviation above that for Vermont (i.e., the 5^{th} highest Turnout state), while its level of telecommunications capital (as measured by *Loops*) is almost a quarter of a standard deviation below Vermont's. On the distribution's opposite end, Nevada's $Turnout$ (i.e., the lowest $Turnout$ state) is about one standard deviation below that for Maryland (i.e., the 34^{th} highest $Turnout$ state), while its level of telecommunications capital (as measured by *Loops*) is over three-quarters of a standard deviation above Maryland's.

fer significantly from it's just identified counterpart in regression (3). Table 2's regression (4) reports an overidentified estimate with which to formally evaluate this difference. In light of results from Whitney Newey and Kenneth West's (46) overidentification test (i.e., $J = 1.01$, p-value $= 0.32$), strong evidence does not exist here for either instrument alone violating the exclusion restriction.[34]

While offering additional confidence that my identification strategy validly addresses *Turnout*'s potential endogeneity, this test cannot dismiss a case where both *EDR* and *Open Primary* share independent relationships with *Turnout*. Because received research offers little in the way of prior information with which to dismiss this case, I gain additional confidence that *Turnout* relates negligibly to influential but omitted variables via Altonji et al.'s (4) innovation for evaluating causality from non-experimental data. This innovation stems from formally comparing omitted variables' capacity to select the treatment under examination (e.g., whether a state receives high turnout) to that same capacity for observables. If observed variables' capacity is relatively large, then the econometrician can gain confidence that omitted variables did not select the treatment of interest. This inference comes from recognizing that, if OVB is truly innocuous, then an index of omitted variables should not vary systematically with the variable of interest - e.g., unobservables should not vary across "high" and "low" turnout states. To the extent, however, that OVB spuriously creates a relationship of interest, an index of omitted variables should vary with the variable of interest, and that variation should parallel selection based on observables. Here, unobservables should exhibit just as much capacity to select outcomes into high and low turnout states as do observables.

Applied to the question of present interest, Altonji et al.'s (4, equation (1.3)) method says to compare the following two normalized index-shifts:

$$\frac{E\ [u_1 \mid \text{"}high\text{"}\ Turnout] \ - \ E\ [u_1 \mid \text{"}low\text{"}\ Turnout]}{Var\ [u_1]} \tag{11}$$

[34] This test result also corroborates evidence reported below from the IV estimate's bootstrapped distribution and corresponding matching estimator that the problem of weak instruments does not afflict the present investigation (Hahn and Hausman (31)).

and

$$\frac{E\ [X'\gamma \mid \text{"}high\text{" } Turnout] \ - \ E\ [X'\gamma \mid \text{"}low\text{" } Turnout]}{Var\ [X'\gamma]} \tag{12}$$

where $X'\gamma$ is a series of fitted values that predict *Loops* without information about $Turnout$ (i.e., X contains all of regression (2)'s independent variables except $Turnout$), u_1 is a series of associated residuals, "$high$"$Turnout$ indicates states for which $Turnout$ exceeds its median, and "low"$Turnout$ indicates states for which $Turnout$ falls short of its median. In short, equations (11) and (12) measure the degrees to which unobservables and observables, respectively, vary with electoral mobility in the reduced form model (10). To the extent that my observed regressors account for selection into high and low turnout states, the normalized shift of unobservables (11) should thus equal zero and OVB should not complicate available inference. If, on the other hand, omitted variables completely rationalize this selection (i.e., my measured relationship between $Turnout$ and *Loops* is entirely attributable to OVB), then this normalized shift should move away from zero and indeed equal the normalized shift of observables (12). Here, observed and unobserved regressors would exhibit the same capacity to account for *Loops*' variation with $Turnout$, and suggest that available inference is peculiar to my regression specification.

Appendix C reports detailed data from which these shifts can be calculated. From these data, the normalized shift in observables' magnitude (12) (i.e., 0.0066) is over three times that of the normalized shift in unobservables (11) (i.e., -0.0021). This ratio suggests that selection on omitted variables would have to be considerable for inference that is available from Table 2 to be completely spurious. Altonji et al. (4, p. 6) concur by suggesting that the case for a causal effect is strengthened if selection on unobservables must be several times stronger than that on observables.

As a final push in this direction, I evaluate the capacity for a number of heretofore omitted, but potentially influential, variables to rationalize $Turnout$'s coefficient estimate.[35] If, contrary to evidence from my overidentification test and application of Altonji et al.'s (4) method, selection on omitted regressors is considerable, then *Turnout*'s coefficient estimates should be unstable. A representative set of results reported in Table 3 argues strongly against this sensitivity.

—–Insert Table 3 Here—–

Coefficient estimates on *Turnout* are remarkably stable across a large set of potentially influential, but heretofore omitted, variables. An important consideration in this regard is whether political forces that influence electoral rules also influence relevant regulatory institutions. If they do, then *EDR* may exhibit an independent relationship with *Loops*. I address this difficulty by controlling for whether PUC members are elected or appointed. Including this control by itself, or interacting it with *Turnout*, does not alter available inference (e.g., see Table 3's row A). Indeed, the correlation between *EDR* and a dummy variable that equals one for states that elect their PUCs is small and negative. This "non-result" adds confidence that correlation with an omitted measure of relevant regulatory structures does not compromise *EDR*'s validity as an instrument.[36]

EDR could also be problematic if my regressors do not control for variation in electorates' ideologies, and these ideologies influence both electoral rules and policy outcomes. Inter-state heterogeneity of ideologies indeed appears considerable (e.g., see Besley and Case (11)). To the extent, however, that all electoral members prefer lower prices, such variation may not be important for

[35] Besley and Case (11) offer an excellent discussion of how electoral institutions might relate to policies such as those investigated here through extra-turnout channels.

[36] Employing similar arguments to those that appear in the "turnout and distribution" literature, Besley and Stephen Coate (12) develop evidence that electoral constituencies recognize significantly lower electricity prices in US states that elect (rather than appoint) relevant regulators. Susan Smart (61) develops analogous evidence for the telecommunications sector. To the extent that my Hypothesis 2 characterizes our empirical reality, extending this received evidence to the present research suggests that telecommunications capital stocks should be relatively low, ceteris paribus, in states that elect their PUC members. In unreported regressions, coefficient estimates' signs are consistent with this implication, but the estimates themselves are not always significant.

the present application. In this light, telecommunications policies appear "pliable" in that they are available to all politicians as a tool for attracting swing voters.[37] I evaluate this conjecture by omitting observations on Idaho and North Dakota from the sample. In addition to being able to register on election day (or not having to register at all), prospective voters from these states maintain some of the most conservative measured opinions in the US (e.g., see Robert Erikson, Gerald Wright, and John McIver (19) and Wright et al. (68)).[38] Extending my analysis further in this direction, I also omit observations on Minnesota and Wisconsin. While these states' electorates maintain moderate policy preferences, their parties' leaders appear relatively liberal (Erikson et al. (19)). Constraining the sample in both cases, nevertheless, leaves *Turnout*'s coefficient estimate essentially unchanged (see Table 3's rows B and C).

All of the other estimates on *Turnout* reported in Table 3 (and many others that are not reported) exhibit a similar robustness. If *EDR* relates to *Loops* via any of the associated "unobserved" variables, then inference from my IV estimates could be biased. Coefficient estimates on *Turnout*, nevertheless, remain stable across each expansion of the regressor-set. This stability implies that *Turnout*'s coefficient estimate is unlikely to change, even upon introducing a yet unspecified variable (Altonji et al. (4), Jonah Gelbach (27)).

As a final check on my identification strategy's validity, I consider the potential for the IV estimator's finite sample properties to introduce bias. Performing this check is important because the two-stage least squares (2SLS) estimator can be biased in finite samples (even if the exclusion restriction is satisfied), and this bias is towards the corresponding OLS estimate.

I do so first by bootstrapping the distribution of *Turnout*'s 2SLS coefficient estimate, the result of which is reported in Figure 2.[39]

[37] Smart (61) offers evidence that telecom policies may indeed be pliable in this regard. She finds, for example, that basic service rates do not differ significantly across the parties of gubernatorial PUC-appointers.

[38] Other states for which *EDR* equals one do not vary considerably in their liberal-opinon measures and these measures are centrally located in the distribution of opinions across states (Erikson et al. (19), Wright et al. (68)).

[39] Joel Horowitz (38), amongst others, rationalizes this treatment of the potential for finite-sample bias.

——Insert Figure 2 Here——

This distribution's mean (i.e., -10.66) implies that the bias-corrected estimate of *Turnout*'s coefficient is essentially identical to the corresponding IV estimate reported in Table 2 (i.e., -11.05 in regression (3)). In this light, inference that can be drawn from Table 2's regressions does not appear to be a finite sample artifact.

Finally, I evaluate *Turnout*'s relationship with *Loops* via the nearest neighbor matching estimator - i.e., an estimator whose validity is unrelated to sample size considerations. I do so by interpreting states for which *EDR* equals one as receiving a "high electoral mobility" treatment and then estimate from a linear probability model each state's propensity to receive such treatment.[40] These "propensity scores" are conditioned on the above described controls for demand side forces, supply side forces, and other interests' capacity to exert political influence (i.e., control variables included in regressions (2) - (4)). In light of the South's historical aversion to institutions that would facilitate electoral mobility (e.g., see Besley and Case (11); Key (39)), I also condition this propensity on an indicator of the 11 states that attempted secession during the US's civil war.

For each treated state, I then measure the linear distance between the own-propensity score and that of each other state. The non-treatment state that minimizes this distance becomes the treatment state's "nearest neighbor." The nearest neighbor matching estimator measures the difference in outcomes (i.e., *Loops*) that is attributable to electoral mobility as the average difference in outcomes between treated observations and their nearest neighbors (e.g., see Blundell and Costa-Dias (13)).

For the specification described above, this difference implies that LECs in treated states maintain about 133 less loops per 1,000 population (i.e., about one standard deviation in *Loops*) than do LECs in untreated states. If unobservables negligibly influenced states' selection into "high turnout" electorates,[41]

[40] Recall from Appendix B's estimates that "treated" states exhibit, on average, just over a standard deviation increase in *Turnout*.

[41] Evidence reported below from Altonji et al.'s (4) method formally evaluates the extent to which the present specification satisfies this condition.

and each state has a non-zero probability of being treated, then this variation in outcomes between high and low turnout states is attributable to increased electoral mobility in treated states. Under these conditions, and if treatment responses are homogenous, a standard deviation increase in $Turnout$ would cause nearly a 7/8 standard deviation decrease in *Loops*. Again, evidence that emerges from my addressing $Turnout$'s potential endogeneity via the EDR instrument does not appear to be an artifact of my sample's relatively small size.

3.4 Structural evidence

Reduced form evidence of $Turnout$'s relationship with *Loops* is consistent with Hypothesis 2 and robust across several considerations of how endogeneity bias might render that evidence spurious. While persuasive in this regard, however, it does not explicitly evaluate my theoretical motivation where regulated prices channel electoral pressures to LEC's capital decisions. Received evidence in the turnout and distribution literature, and Falaschetti (22) in particular, fills part of this lacuna by supporting at least the first half of this structure's empirical relevance - i.e., electoral pressures act on regulated prices. I conclude the present section by attempting to close this gap more completely.

I do so by estimating parameters from the following recursive system where prices at which electoral constituents connect to the telecommunications network (i.e., $Connection\ Charge$) depend on $Turnout$ and, in turn, *Loops* depend on $Connection\ Charge$.

$$Connection\ Charge_i = \beta_0 + \beta_1 Turnout_i + \sum_{k=2}^{K} \beta_k Controls_{k,i} + u_2 \quad (13)$$

$$Loops_i = \gamma_0 + \gamma_1 Connection\ Charge_i + \sum_{k=2}^{K} \gamma_k Controls_{k,i} + u_3 \quad (14)$$

In principle, *Connection Charge* appears to be exactly the variable with which *Loops* shares a structural relationship (e.g., see my Section 2 and Parsons (52)). Doing so comes at a considerable degrees of freedom cost, however, since this variable is observable for only 38 states.[42] My estimated structural relationships thus exhibit some loss of efficiency vis-a-vis their reduced form counterparts. In addition, since *Connection Charge* is observable for only 3 of the 7 *EDR*-states, I do not report any IV-results in Table 4.[43]

——Insert Table 4 Here——

My specification of regression (5.1) follows that of Falaschetti (22) and produces qualitatively similar evidence - e.g., a standard deviation increase in *Turnout* is associated with just over a 1/3 standard deviation decrease in *Connection Charge*. In turn, the coefficient estimate on *Connection Charge* in regression (5.2) suggests that this increase in *Turnout* is associated with about a 1/8 standard deviation decrease in *Loops*. This relationship's qualitative nature appears robust to introducing demographic controls in regressions (6.1) and (6.2). Here, the first equation's coefficient estimate on *Turnout* remains negative, but becomes statistically insignificant (p-value = 0.22). The negative relationship between *Connection Charge* and *Loops*, on the other hand, maintains its statistical significance. Coupled with evidence from Falaschetti (22) that prices for local exchange services decrease significantly with increases in electoral mobility,[44] evidence reported here on *Turnout*'s relationship to

[42] *Connection Charge* equals the average price that end-users from 1996-1998 paid to connect the telecommunications network. While this price should be closely associated with the decision to supply loops, it can only be observed for states that contain at least one of the 95 urban areas surveyed for the Bureau of Labor Statistics (BLS) Consumer Price Index (CPI) in 1988 (Brown (15)).

[43] In light of the insignificance of Hausman's test statistic (see Table 2) and the IV estimates' robustness to considerations of both small and large sample bias (see regressions 4 and 5), the potential for *Turnout*'s endogeneity to bias inference here appears negligible. Indeed, unreported IV estimates, as well as estimates from samples that exclude states for which *EDR* equals one, are qualitatively similar to those reported here.

[44] Prices that Falaschetti (22) investigates are paid monthly by end-users to remunerate LECs for recurring service costs. This variable includes revenues from interstate and intrastate monthly service, local calling, connection charges, vertical features, inside wiring maintenance, and other local exchange services (source: email correspondence with Jim Eisner of the FCC). As such, it is a relatively noisy proxy of remuneration that LECs receive

Connection Charge and *Connection Charge*'s relationship to *Loops* further corroborates my reduced form evidence for Hypothesis 2.

4 Conclusion

Evidence from the "institutions and commitment" literature suggests that political agents' opportunistic potential can significantly reduce productive activity. North and Weingast (50) offer a seminal contribution in this regard. Left unconstrained, England's 17^{th} century monarchy could not credibly commit against reneging on debt obligations or confiscating capital investments' product. This condition encouraged a "Glorious Revolution" that saw political authority devolve to Parliament and an independent judiciary. By insulating investors' product from expropriation-risk, this devolution spurred a marked increase in England's real activity.

Others have formalized and extended North and Weingast's insight. Henisz (35), for example, formally measures political constraints by, in effect, carefully counting the number of "veto points" that characterize a country's polity. He also shows that this measure exhibits a significant and positive relationship with economic growth. Henisz rationalizes this evidence much like North and Weingast do theirs – i.e., increasing the number of veto players encourages productive economic activity by increasing the cost for political agents to coordinate opportunistic actions and thus insulating investors' product from expropriation.[45]

While the "institutions and commitment" literature suggests that political agents' opportunistic potential can retard real economic activity, however, it is silent about whether the analogous potential for these agents' principals exerts a similar influence. Interestingly, several scholars address how principals' expropriation threat constrains investment activity in the formally related setting

in return for supplying loops. Nevertheless, evidence that Falaschetti (22) produces remains relevant for my present application since the regulatory process through which regulators set local exchange and connection prices are similar.

[45] Acemoglu et al. (2), Falaschetti (21), (23), Henisz and Zelner (36), and Stasavage (63) offer related evidence. Aghion et al. (3) and Torsten Persson, Gerard Roland, and Guido Tabellini (55) offer corresponding theoretical treatments.

of shareholders and managers (e.g., see Mike Burkart, Denis Gromb, and Fausto Panunzi (16), Falaschetti (20), Charles Knoeber (40), and Andrei Shleifer and Lawrence Summers (60)). Electoral principals' capacity to act in this manner, nevertheless, appears relatively unexplored. I begin to address this gap in the present paper.

On its face, received evidence that distributive policy favors mobile electorates may suggest that electorates' welfare increases monotonically with voter turnout. Increasing electoral mobility need not induce superior outcomes, however, when the same evidence is evaluated within the "institutions and commitment" framework. Here, real activity can decrease when political agents face a relatively low cost of pursuing opportunistic incentives. One might thus expect similar decreases where electoral principals face analogously low mobilization costs.

By essentially synthesizing insights from the institutions-and-commitment and turnout-and-distribution literatures, I evaluate a policy-strategy that is now popularly, and often uncritically, characterized as dominant - i.e., increasing electoral mobility. Interestingly, non-contemporary social scientists since at least the Federalist Paper's authors (30) have appreciated majorities' potential to act tyrannically. Robust evidence that I develop here suggests that this frequently cited possibility may be easier to realize than popular accounts of mobility's normative properties suggest.

The present research thus encourages a more cautious normative reading of evidence that distributive policy favors high turnout electorates. More generally, it suggests that political opportunism's real effects may not have political agents as their only source. This suggestion is important because the institutions and commitment literature tends to draw reformers' attention to constraining political agents' opportunistic potential. Unbound by similar constraints, however, electoral principals might effectively control their political agents while significantly retarding their economic agents' productive incentives (especially those who operate in highly regulated sectors). An important objective for future research is to improve our understanding of this trade-off's welfare implications. Moving the present investigation to an international setting, where costs of electoral participation may lie outside the bounds of those observed across US states, could be instructive in this regard.

5 Appendix A - Data

—–Insert Table A Here—–

6 Appendix B - First Stage Results

——Insert Table B Here——

7 Appendix C - Evaluating OVB

—–Insert Table C Here—–

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Appendix A
Data Descriptions

Variable	Description
Loops	Number of loops per 1,000 population that incumbent local exchange carriers (ILECs), subject to price cap regulation, maintained on December 31, 2000
Turnout	Average percentage of voting age individuals that cast ballots for the office of president in the 1992, 1996, and 2000 general elections.
Election Day Registration (*EDR*)	Indicator of states in which prospective voters can register on election day.
Population Density	Population per square mile.
Education	Percent of 1990 population, 25 years and older, that graduated high school.
Income	1990 per capita personal income (in 000's).
Poverty	Percent of 1989 families with income below poverty line.
Age 65	Perecent of 1990 population aged 65 and over.
LEC Concentration	Percentage of zip codes without any competitive local exchange carriers (CLECs).
LD Concentration	Herfindahl-Hirschman index of long distance service providers' market shares.
Fortune 500	Number of Fortune 500 headquarters in 1999.
Loop Cost	Monthly cost of maintaining a loop in 1996.

Appendix B
First Stage Regression Results
Dependent Variable = *Turnout*

Variable	Coeff.	SE	Coeff.	SE
Constant	-27.5626	20.5994	-38.5157	21.0026*
EDR	8.2953	2.0311***	7.0877	1.8768***
Open Primary			3.5525	1.3885***
Population Density	0.0018	0.0049	-0.0018	0.0041
Education	0.8316	0.1787***	0.7985	0.1639***
Income	0.1813	0.4716	0.6472	0.4669
Poverty	0.7194	0.4343	0.7480	0.4042*
Age 65	1.0946	0.4925**	1.2800	0.4362***
LEC Concentration	0.0095	0.0300	0.0131	0.0259
LD Concentration	0.4770	8.5427	-1.6748	8.7031
Fortune 500	-0.1031	0.0785	-0.1078	0.0686
Loop Cost	-0.2745	0.2541	-0.2069	0.2153
N	48		48	
R^2	0.66		0.71	
Adj. R^2	0.57		0.62	
$\bar{y}$	54.69		54.69	
σ_y	6.52		6.52	
F-stat.	7.11		8.07	
Prob(F-stat.)	0.00		0.00	

Notes: ***, **, and * indicate confidence at the 99%, 95%, and 90% levels, respectively. Coefficients are estimated via OLS and standard errors are White-consistent.

Appendix C

Evaluating Omitted Variables Bias

Summary Statistics for High and Low Turnout States

High Turnout States

	Loops	Turnout	EDR	Pop. Density	Education	Income	Poverty	Age 65	LEC Concentrate	LD Concentrate	Fortune 500	Loop Cost
Sum	N/A	N/A	7.00	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Mean	616.96	60.18	0.29	153.74	79.34	17.76	8.92	12.78	45.17	0.24	6.29	22.88
Median	633.68	59.28	0.00	53.14	80.05	17.50	8.50	13.30	47.50	0.24	3.00	22.89
Max	829.04	70.41	1.00	958.22	85.10	25.40	19.40	15.40	96.00	0.38	28.00	36.30
Min	366.13	54.90	0.00	4.79	68.30	13.99	4.40	8.70	0.00	0.09	0.00	15.67
SD	110.07	3.81	0.46	259.95	4.00	2.51	2.89	1.67	29.33	0.07	7.45	4.77

Low Turnout States

	Loops	Turnout	EDR	Pop. Density	Education	Income	Poverty	Age 65	LEC Concentrate	LD Concentrate	Fortune 500	Loop Cost
Sum	N/A	N/A	0.00	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Mean	679.43	49.19	0.00	190.11	72.64	17.52	11.17	12.58	41.17	0.27	13.42	22.89
Median	649.43	49.56	0.00	120.78	74.65	16.82	10.70	12.60	39.50	0.27	6.50	23.61
Max	975.69	53.77	0.00	1,054.09	78.80	24.88	20.20	18.30	100.00	0.41	58.00	30.85
Min	446.58	44.03	0.00	12.17	64.30	12.83	5.60	10.10	2.00	0.15	0.00	15.40
SD	134.09	3.04	0.00	222.03	4.76	3.08	3.84	1.94	33.03	0.07	17.25	5.19

Appendix C (continued)
Evaluating Omitted Variables Bias
Selection on Observables vs. Unobservables

Rank	State	LOOPS	TURNOUT	u_1	X'γ	Rank	State	LOOPS	TURNOUT	u_1	X'γ
1	ME	586.1	70.4	91.44	1,324.0	25	DE	866.8	53.8	170.79	1,329.4
2	MN	624.2	68.2	48.13	1,378.8	26	IL	683.4	53.7	-2.50	1,318.0
3	MT	475.0	64.6	-18.19	1,253.5	27	NJ	884.4	52.8	24.21	1,481.6
4	WI	529.9	64.2	-16.71	1,302.6	28	OK	581.6	52.7	-14.18	1,217.0
5	VT	630.8	63.2	-1.43	1,376.6	29	PA	626.7	52.3	-32.04	1,274.6
6	SD	389.9	61.9	-115.38	1,234.6	30	VA	738.6	51.1	1.44	1,339.4
7	IA	528.1	61.2	-53.22	1,302.6	31	IN	626.1	51.0	-41.26	1,268.3
8	ND	366.1	61.2	-118.75	1,206.0	32	AL	563.1	51.0	9.49	1,153.9
9	OR	688.0	61.2	46.66	1,361.8	33	KY	539.7	50.9	32.54	1,106.8
10	NH	718.6	61.0	-55.91	1,492.7	34	MD	798.1	50.6	-71.48	1,465.4
11	WY	572.8	60.5	-52.33	1,337.5	35	FL	837.6	49.9	109.75	1,315.4
12	CT	770.8	59.7	-50.89	1,524.6	36	NY	714.5	49.6	-35.54	1,334.2
13	ID	676.4	58.9	114.55	1,255.5	37	AR	446.6	49.5	-47.43	1,077.4
14	NE	563.1	58.5	-87.78	1,340.4	38	TN	619.1	49.5	35.65	1,166.6
15	MI	664.2	57.9	8.70	1,337.4	39	MS	518.7	48.9	-23.78	1,118.7
16	MO	650.0	57.8	91.64	1,239.5	40	NC	652.3	48.6	-5.15	1,230.5
17	KS	636.5	57.7	-34.97	1,351.4	41	NM	609.5	48.2	-5.25	1,181.9
18	MA	755.5	57.6	-39.47	1,473.2	42	WV	565.9	47.1	38.64	1,082.2
19	WA	691.3	57.2	-40.18	1,405.4	43	AZ	809.7	46.6	61.24	1,297.4
20	LA	565.1	57.0	62.33	1,174.4	44	CA	762.8	45.8	-13.56	1,315.9
21	OH	586.2	56.9	14.07	1,242.5	45	TX	724.5	44.5	40.28	1,208.6
22	CO	829.0	56.8	77.24	1,420.9	46	SC	514.3	44.4	-131.26	1,168.3
23	UT	649.5	55.9	3.28	1,304.6	47	GA	646.5	44.1	-98.29	1,264.8
24	RI	659.8	54.9	24.62	1,281.8	48	NV	975.7	44.0	100.25	1,394.1

	LOOPS	TURNOUT	u_1	X'γ
Mean(All)	648.2	54.7	0.0	1,292.4
Mean(High)	617.0	60.2	-4.3	1,330.1
Mean(Low)	679.4	49.2	4.3	1,254.6
Variance	15,723	42.5	4,190	11,459
Normalized Shift			-0.0020	0.0066

Figure 1

Loops vs. *Turnout*

$\sigma_{\text{Loops, Turnout}} = -0.31$

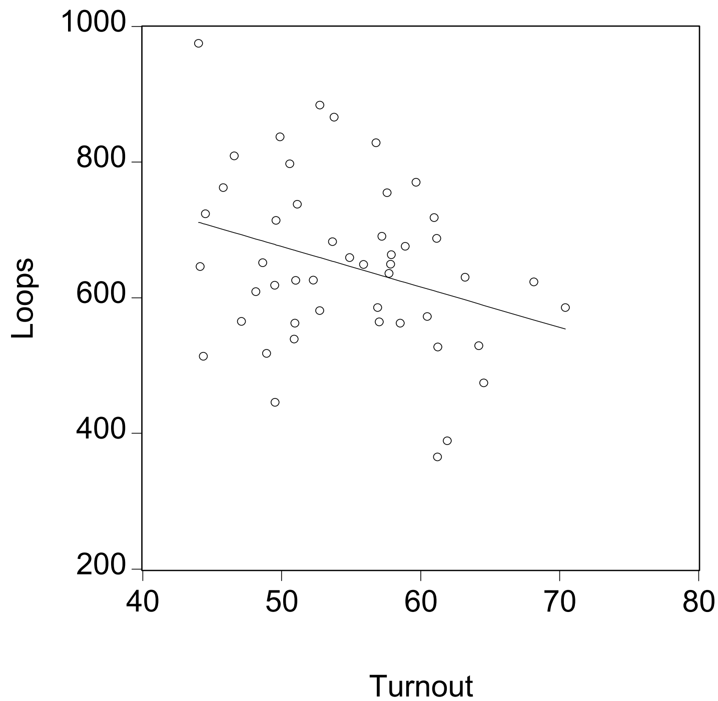
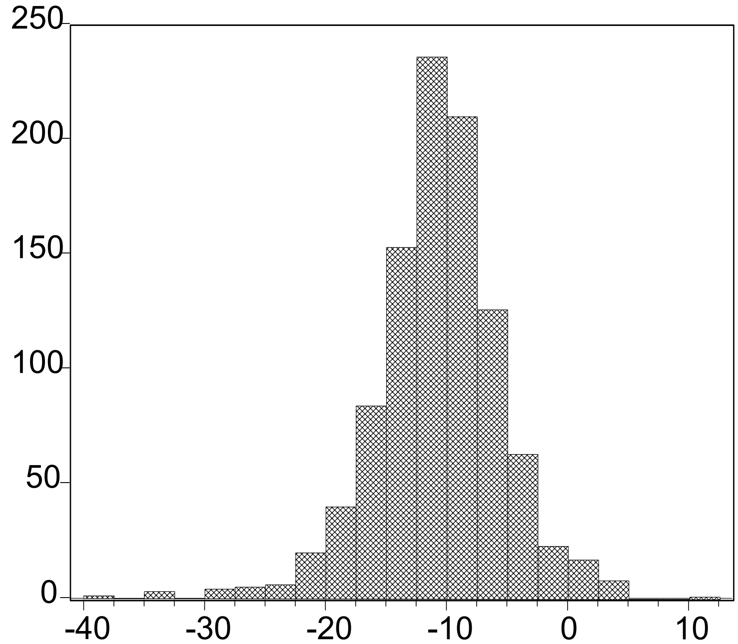


Figure 2
Bootstrapped Distribution of the 2SLS
Coefficient Estimate on *Turnout*



Re-Samples	1,000
Mean	-10.66
Std. Dev.	5.25
Upper-Bound 90% Confidence Interval	-2.59
Lower-Bound 90% Confidence Interval	-19.24

Notes: *Election Day Registration* (*EDR*) instruments for the potentially endogenous regressor, *Turnout*. Confidence intervals are calculated via the percentile method. The hypothesis that the mean equals zero can be rejected at any reasonable level of confidence (*t*-statistic = -146.44).

Table 1
Data
Summary Statistics

	Loops	Turnout	Election Day Registration	Population Density	Education	Income	Poverty	Age 65	LEC Concentration	LD Concentration	Fortune 500	Loop Cost
N	48	48	48	48	48	48.00	48	48	48	48	48	48
Sum	N/A	N/A	7.00	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Mean	648.19	54.69	0.15	171.92	75.99	17.64	10.0	12.7	43.2	0.26	9.85	22.88
Median	641.53	54.33	0.00	79.38	76.70	17.39	9.2	12.7	44.0	0.24	4.50	23.15
Max	975.69	70.41	1.00	1,054.09	85.10	25.40	20.2	18.3	100.0	0.41	58.00	36.30
Min	366.13	44.03	0.00	4.79	64.30	12.83	4.4	8.7	0.0	0.09	0.00	15.40
SD	125.39	6.52	0.36	239.85	5.51	2.78	3.6	1.8	31.0	0.07	13.63	4.93
Source	F	B	B	C	C	C	C	C	D	G	E	A

A. Falaschetti 2003(a)
B. Federal Election Commission 2003
C. Geospatial and Statistical Data Center (GEOSTAT)
D. *Local Telephone Competition* 2002 - Table 13
E. Savageau and D'Agostino 1999
F. *Trends in Telephone Service* 2002 – Table 8.2
G. Author calculated from *Trends in Telephone Service* 2002 – Table 10.11

Table 2
Reduced Form Evidence
Dependent Variable = *Loops*

Variable	(1) OLS Coeff.	SE	(2) OLS Coeff.	SE	(3) 2SLS Coeff.	SE	(4) 2SLS Coeff.	SE
Constant	974.34	154.27***	121.66	400.14	141.26	423.33	137.48	377.75
Turnout	-5.96	2.73**	-11.86	2.64***	-11.05	4.77**	-12.68	4.32***
Population Density			-0.05	0.06	-0.05	0.06	-0.05	0.06
Education			9.52	3.58***	8.82	4.71*	10.23	4.62**
Income			30.15	8.36***	30.19	8.31***	30.11	8.28***
Poverty			2.57	6.21	2.36	6.27	2.78	6.68
Age 65			-2.85	9.38	-3.74	10.40	-1.94	9.66
LEC Concentration			-0.73	0.58	-0.74	0.60	-0.71	0.59
LD Concentration			102.75	174.69	102.71	176.17	102.79	173.65
Fortune 500			-2.13	1.02**	-2.06	1.13*	-2.21	1.10**
Loop Cost			-1.51	3.03	-1.43	3.19	-1.59	3.01
N	48		48		48		48	
R^2	0.10		0.74		NA		NA	
Adj. R^2	0.08		0.66		NA		NA	
$\bar{y}$	648.19		648.19		648.19		648.19	
σ_y	125.39		125.39		125.39		125.39	
F-stat.	4.89		10.27		NA		NA	
Prob(*F*-stat.)	0.03		0.00		NA		NA	
Prob(Hausman)	NA		0.84		NA		NA	
Prob(*J*-stat)	NA		NA		NA		0.32	

***, **, and * indicate confidence at the 99%, 95%, and 90% levels, respectively.
Standard errors are White-consistent.

Table 3
Evaluating the Stability of *Turnout*'s Coefficient Estimate
Dependent Variable = *Loops*

	Specification Change	2SLS Coefficient Estimate on *Turnout*	White-Corrected Standard Error
A	Indicator of elected public utility commissioner (PUC)	-12.53	3.74 ***
B	Omit observations on ID and ND	-11.93	3.28 ***
C	Omit observations on MN and WI	-13.70	5.85 **
D	Index of state level campaign contribution limits	-12.58	4.49 ***
E	Indicator of direct electoral access to ballot initiative	-12.48	4.71 ***
F	Indicator of no gubernatorial term limits	-11.76	4.31 ***
G	Index of polity's competitiveness (outcomes)	-12.12	4.09 ***
H	Regulatory history (change to incentive regulation)	-12.89	4.27 ***
I	Average unified party control of state government	-12.67	4.36 ***
J	Electorate's revealed party preferences (%-Clinton 1992)	-12.56	4.37 ***
K	Regional dummy for western states	-11.98	5.41 **
L	Regional dummy for southern states	-12.82	4.40 ***
M	Population	-12.51	4.79 ***
N	Number of households	-12.43	4.83 ***
O	Percent of population that is white	-13.42	4.92 ***
P	Indicator of significant consumer advocacy presence	-12.72	4.37 ***
Q	Net universal service subsidies	-12.73	4.36 **

Notes: ***, **, and * indicate confidence at the 99%, 95%, and 90% levels, respectively. The regressor matrix includes all of the covariates reported in Table 2. Instruments for *Turnout* are *EDR* and *Open Primary*. Qualitatively similar results appear when *EDR* acts as a single instrument or if the coefficient estimate on *Turnout* is estimated via OLS.

Table 4.1
Structural Evidence
Dependent Variable = *Connection Charge*
Estimation Method = SUR

Variable	(5.1) Coeff.	SE		(6.1) Coeff.	SE	
Constant	69.37	24.26	***	57.86	45.16	
Turnout	-0.56	0.26	**	-0.39	0.31	
Population Density				-0.01	0.01	
Education				-0.66	0.46	
Income				2.56	1.25	**
Poverty				0.53	0.86	
Age 65				0.14	0.83	
LEC Concentration	-0.09	0.05	*	-0.05	0.06	
LD Concentration	5.22	24.00		14.88	24.02	
Fortune 500	0.15	0.14		0.01	0.17	
Loop Cost	0.04	0.43		-0.01	0.53	
N	38			38		
R^2	0.27			0.39		
Adj. R^2	0.15			0.17		
$\bar{y}$	39.85			39.85		
σ_y	10.55			10.55		

Note: ***, **, and * indicate confidence at the 99%, 95%, and 90% levels, respectively.

Table 4.2
Structural Evidence
Dependent Variable = *Loops*
Estimation Method = SUR

Variable	(5.2) Coeff.	SE		(6.2) Coeff.	SE	
Constant	633.80	103.84	***	666.58	104.33	***
Connection Charge	3.88	1.50	***	3.17	1.52	**
Loop Cost	-6.12	3.40	*	-6.31	3.40	*
N	38			38		
R^2	0.13			0.15		
Adj. R^2	0.08			0.10		
$\bar{y}$	649.95			649.95		
σ_y	106.70			106.70		

Note: ***, **, and * indicate confidence at the 99%, 95%, and 90% levels, respectively.