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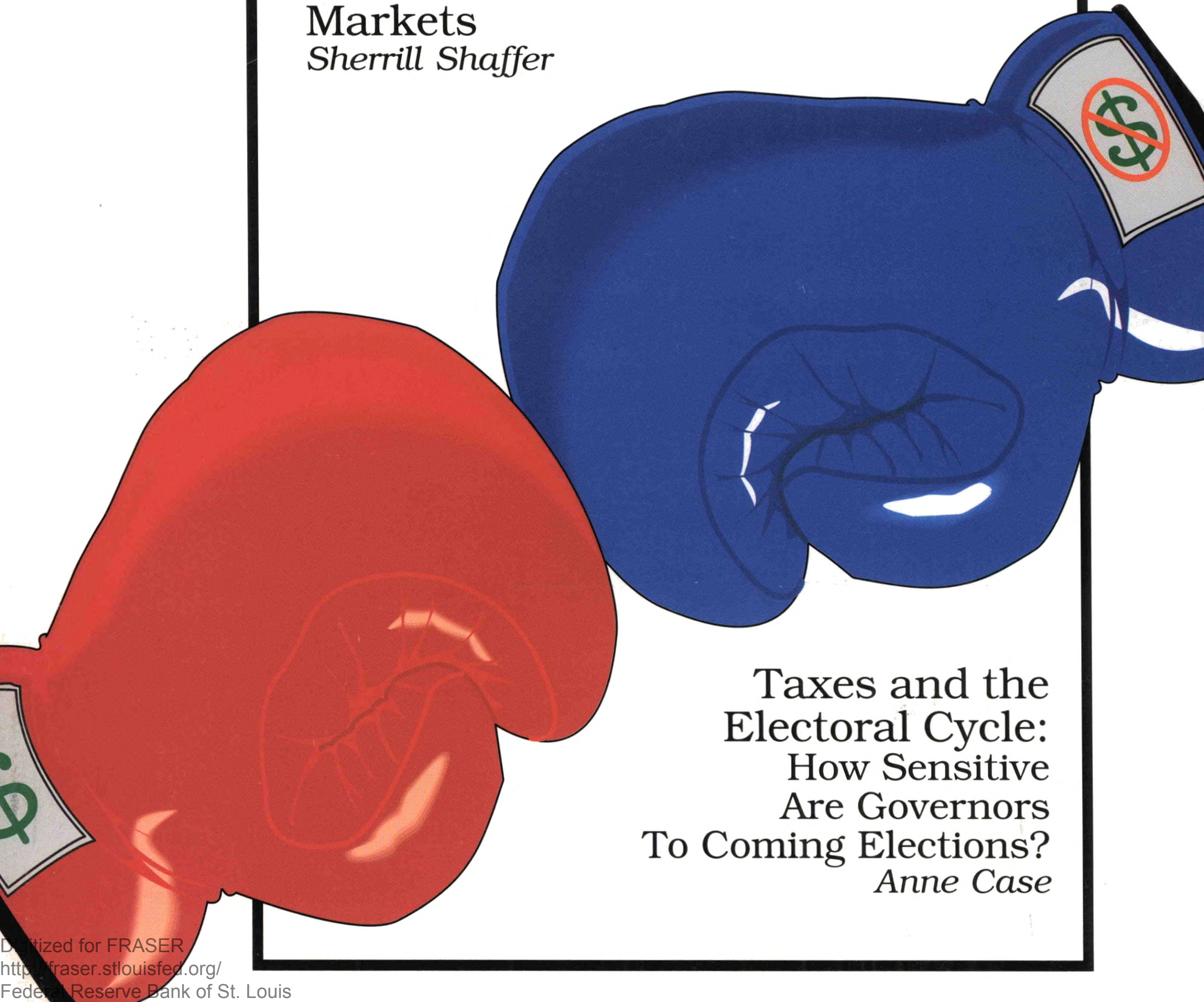

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Anne Case

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Bank Competition In Concentrated Markets

*Sherrill Shaffer**

Between 1985 and 1991, more than 4000 mergers occurred among U.S. commercial banks, a rate of consolidation more than four times greater than in previous decades. During the same period, consolidation transferred control of more than $350 billion in financial assets from smaller acquired banking institutions to the 100 largest U.S. depository institutions.[1] Deregulation of deposit interest rates in the early 1980s opened the door for intensified competition among banks while, at the same time, foreign banks and nonbanking financial firms began to compete more vigorously for traditional banking business. These forces prompted many banks to merge as a way of improving their diversification, efficiency, or possibly market power.[2] Over that period,

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[1]See Historical Statistics on Banking, 1934 to 1991 (Washington, D.C.: Federal Deposit Insurance Corporation), p. 6; and U.S. House of Representatives, Analysis of Banking Industry Consolidation Issues (Committee on Banking, Finance and Urban Affairs, Staff Report, March 2, 1992), p. 18.

[2]Dozens of studies have found evidence that banks smaller than some "minimum efficient scale" suffer intrinsically higher costs, while other studies have found evidence that some banks do not minimize their costs; mergers could potentially reduce the costs of either type of bank. Whether some banks merge in order to enhance their dominance of the market has not been directly studied.

historical legal restrictions on intrastate branching and interstate bank holding company affiliation have been progressively weakened, expanding the opportunities for consolidation. Most industry analysts and economists expect the trend of accelerated consolidation to continue unabated during the next several years, although many small and medium-size banks should remain.

This consolidation has renewed fears of market concentration and monopoly power in the banking industry. Policymakers are suspicious of concentration—a market structure in which only a few banks supply most of the deposit and loan services demanded by the market—and seek to limit it because they believe that it enables banks to exercise monopoly power, thereby harming depositors and borrowers. Such harm would theoretically take the form of less favorable prices (for example, higher interest rates on loans and lower interest rates on deposits) and a lower volume of services provided (including less available credit).

However, new theoretical and empirical evidence suggests that the link between concentration and monopoly power is not uniform. Depending on various factors, competitive outcomes might be observed in concentrated markets as well as unconcentrated ones while, under different conditions, monopoly power might be sustained in unconcentrated markets as well as concentrated ones. Therefore, public policy toward bank consolidation cannot rely solely on structural measures.

To review the evidence, let's start with the traditional structure-conduct-performance (SCP) hypothesis.

THEORETICAL BASIS OF SCP

The original formulation of the SCP hypothesis (Mason, 1939; Bain, 1951) simply asserted that fewer firms in a market (that is, a concentrated *structure*) will generally lead to less competitive *conduct* (in terms of higher prices and reduced output levels) and less competitive *performance* (higher ratios of price to cost and higher profits at the expense of lower consumer welfare). The U.S. Department of Justice has long adhered to this view by maintaining an explicit policy of challenging mergers between rival firms (whether between banks or in other industries) that result in concentration levels above certain thresholds. Federal bank regulators (the Comptroller of the Currency, the Federal Deposit Insurance Corporation, and the Federal Reserve) also apply these guidelines when reviewing applications for bank mergers, as described by Holder (1993).[3]

Beginning around 1970, a more rigorous theoretical basis for the SCP hypothesis was sought. Work by Cowling and Waterson (1976), Dansby and Willig (1979), Novshek (1980), and others demonstrated that there are some market conditions under which the hypothesis is valid. For example, if each firm chooses its output level as though its rivals will not vary their output levels in response, and if firms set a target output level rather than price per se, firms' profitability will depend on the sum of squared market shares of all firms in the market. This measure of market structure is known as the Herfindahl-Hirschman index of concentration (HHI).[4] Similarly, Saving (1970) has shown that a dominant cartel of (say) four firms, plus a competitive fringe of smaller firms, generates a fixed relationship between (in this case) the combined market share of the four largest firms in the market and firms' perfor-

[3]Supplementary nonstructural information, such as the likelihood of entry, may be taken into account in determining market power, and mitigating factors may also be considered where market power is found. Such factors could include any public benefits of a bank merger, such as serving the "convenience and needs" of the local community or providing services at lower cost. Willig (1991) and Holder (1993) discuss these supplementary factors.

[4]Stigler (1964) represents an earlier attempt to relate the HHI to performance measures.

mance. Under the conditions postulated by these studies, an antitrust policy could rely on an appropriate structural formula.

Counterexamples. However, both newer and older economic theory has challenged the realism of those specialized conditions and has also shown that the uniform linkage between structural concentration and market performance can disappear under alternative conditions. At one extreme, Baumol and others (1982) have shown that competitive pricing (that is, a price that just covers the costs of production plus a normal rate of return on capital) could result for any number of firms in a market if an entering firm can attract customers by charging a low price and could recover any cost of entry while abandoning the market if older firms retaliate by underpricing in turn. A similar outcome is predicted by the nineteenth-century analysis of Bertrand (1883), regardless of the ease of entry or exit, whenever firms produce identical products and try to maximize profits by setting their prices rather than by setting targets for how much they would like to sell; Tirole (1988, p. 210) summarizes this theory.

At the other extreme, Friedman (1971) and others have shown that even large numbers of firms in a market may tacitly collude to set high prices if they think ahead, since the temporary profits one firm could gain by underpricing its rivals today could be more than offset by subsequent losses if its rivals retaliate by cutting their prices in turn. Other recent models, such as those by Rotemberg and Saloner (1986) and Worthington (1990), predict patterns of conduct and pricing that are intermediate between perfectly competitive and monopolistic, depending on such factors as interest rates and the cost of adjusting size or capacity. Even when market structure can be shown to influence this sort of pricing, as in Worthington's analysis, the pattern of that linkage may vary according to some factors that are not easily measured, such as costs of adjustment. Theoretical and empirical work by Mester (1987, 1992) further suggests that, when banks compete against each other in more than one market, the actual pattern of conduct may be more competitive than the structure of the individual markets might indicate.

One implication of cost-plus pricing is that structure would have no predictable impact on the level of prices or profits.

To complicate the picture still more, surveys of firms' managers over several decades have found that most claim to set prices at some fixed percentage above cost, using a simple rule of thumb that falls outside the strategies typically analyzed within the SCP framework.[5] Of course, one must view such surveys with caution, since firms may be reluctant to reveal details of their actual pricing strategies; nevertheless, Naish (1990) has shown that such a simple pricing rule—often called "cost-plus" pricing—makes sense when firms find it costly to acquire market information or to adjust their plans. One implication of cost-plus pricing is that structure would have no predictable impact on the level of prices or profits.

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[5]See Hall and Hitch (1939), Skinner (1970), Shipley (1986), and Nagle (1987). Such a simple pricing rule would ignore many important factors, possibly including market structure.

These studies, together with several others not discussed here, demonstrate at a minimum that economic theory alone cannot determine whether the degree of firms' monopoly power is uniformly linked to market structure.[6] Rather, we must turn to empirical studies to address that question.

Two basic types of empirical studies are relevant here: one using an older method that measures statistical correlations between market concentration and measures of performance, and one using newer methods that attempt to estimate patterns of firm conduct directly. Both types of studies have been conducted across a variety of industries, and many focus on the banking industry in particular.

SCP-STYLE EMPIRICAL STUDIES

Economists and policymakers would like to be able to look at a single number (such as profitability, price level, bank size, or number of banks in the market) for a bank or for a market, compare it with the value that would occur in a perfectly competitive market, and conclude something about the degree of competition or monopoly power in the bank or market in question. Unfortunately, it is very difficult to establish a reliable index for this purpose. Even though market participants rely on measures of profits and prices in making business decisions, it is not an easy matter to use these numbers to assess competition in a market. Reported profit rates are influenced by accounting practices, tax rules, and other variables; price levels must be compared with costs to be meaningful; and, as indicated above, structural indices need not correspond to the degree of monopoly power.

To assess whether structural indices tend to be associated with monopoly power *in practice*, many previous studies have measured the historical relationship between profit rates and market concentration or between price levels and concentration. Such studies have a long tradition and continue to be undertaken.

Profit-Concentration Studies. Gilbert (1984) reviews several dozen profit-concentration studies, noting that they present a mixed set of results in aggregate and tend to suffer from various methodological flaws. A primary shortcoming of conventional profit-concentration studies is that they cannot distinguish between market power and efficiency as a source of concentration and profitability (Demsetz, 1973; Peltzman, 1977).[7] Economic theory tells us that a firm that can deliver a superior product or operate at a lower cost will drive its rivals out of a competitive market unless they are able to emulate the successful firm. Such superiority would therefore show up as a combination of high profitability and large market share for the leading bank(s)—producing a more concentrated market—precisely in those markets that are competitive.

Two possibilities then arise. Either other firms (perhaps new firms) can eventually imitate the efficient firm's success, driving the price down until all the efficient firms just break even and all inefficient firms have been forced out of the market. Or if the extra efficiency results from a unique factor that others cannot replicate, the cost and profit differentials may persist, but in a market where all firms set

[6]Hannan (1991b), in attempting to formalize a modern theoretical basis for structure-performance linkages specific to banking, assumes the crucial condition that "interdependence [among banks] is more easily recognized in more concentrated markets" (p. 72). This, of course, begs the question. Likewise, Willig's (1991, pp. 287f.) derivation of the link between welfare and concentration overlooks the fact that firms' behavior can realistically vary in ways that alter or even cancel that linkage.

[7]Indeed, Clarke and Davies (1982) note more generally that profitability and market structure are actually jointly determined by other factors involving the production technology and demand for the product.

prices in a competitive fashion, there would be no way of further improving the use of resources. In either case, traditional profit-concentration studies that fail to test for cost differences are incapable of determining whether society would benefit from a public policy of restricting market concentration.

More recent studies have tried to fix this flaw. Smirlock (1985), Berger (1991), and others attempt to control statistically for some aspects of efficiency. They find that the linkage between concentration and profitability largely disappears in the presence of this correction. Evanoff and Fortier (1988) find evidence that some profit-concentration linkage may persist in markets with substantial barriers to entry, after correcting for efficiency. In general, such studies do not entirely rule out structure as a contributing factor to monopoly power, but they do establish that its influence is at most very limited (Berger and Humphrey, 1992).

Common to both profit-concentration studies and price-concentration studies is the further difficulty of identifying the true geographic market...

Price-Concentration Studies. Other studies have focused instead on the correlation between concentration and price levels. Berger and Hannan (1989), Calem and Carlino (1991), and Hannan (1991a) all find some evidence that high market concentration is correlated with prices that are unfavorable to the consumer.[8] These studies are less likely to confuse market power with efficiency because any extra efficiency among leading firms in a competitive market would tend to show up as lower, not higher, prices. However, Calem and Carlino (1991) found evidence of some monopoly power in deposit rates even in unconcentrated markets, contrary to the predictions of the SCP paradigm. In addition, Berger (1991) examined the price-concentration relationship that remains after efficiency is taken into account, concluding that "SCP may have some validity in deposit markets, but not in loan markets" (p. 25). Moreover, price-concentration studies do not fully control for differences in costs across banks and therefore cannot prove the existence of market power.[9]

Market Definition. Common to both profit-concentration studies and price-concentration studies is the further difficulty of identifying the true geographic market, which determines the measured level of concentration (Whitehead, 1980; Jackson, 1992; Shaffer, 1992). This problem is especially severe in a multiproduct industry such as banking: the market for large commercial loans and large certificates of deposit is not geographically restricted, whereas small-business borrowers and most retail depositors are more locally limited (Elliehausen and Wolken, 1990; Jackson, 1992). And many banks operate in several geographic markets simultaneously,

[8]Weiss (1989) reviews additional price-concentration studies in banking that span 49 different data sets. A minority of these studies support the SCP hypothesis.

[9]Some studies, such as Berger and Hannan (1989) and Calem and Carlino (1991), include a bank wage rate as a partial measure of cost. However, any study that fully controls for costs (for example, by including an explicit cost function) would be implicitly measuring the profitability of banks and would therefore be subject to all the criticisms of profit-concentration studies discussed above.

making it difficult to identify prices or profitability for an individual market. Competition from nonbanking firms in some product lines further complicates the task of delineating the true market shares, as does the question of proper aggregation or disaggregation of the various products.

Thus, like modern economic theory, SCP-style empirical studies have had mixed results. The most sophisticated of these studies have tended to find little connection between concentration and monopoly power, and none of these studies may be regarded as definitive. Therefore, a different empirical approach is needed to assess the link between market structure and competition. A promising alternative is found in the so-called new industrial organization (IO) literature.

NEW INDUSTRIAL ORGANIZATION EMPIRICAL STUDIES

This new IO literature actually dates back nearly 20 years, though its application to the banking industry has evolved more recently. Several methods appear in these studies, most relying on some combination of the notions that banks seek profits or that banking markets are in equilibrium. Two primary techniques will be discussed here, the Rosse-Panzar test and the markup test.[10]

The Rosse-Panzar Test. This test relies on the fact that an individual bank will price differently in response to a change in its costs, in a way that depends on whether the bank enjoys some monopoly power or instead is operating in a competitive market. The various possible pricing strategies have definable implications for changes in the bank's gross revenue.

If a bank has monopoly power and sets prices so as to maximize profits, it will choose prices such that its gross revenue responds in the opposite direction as a change in unit costs. For example, consider a proportional increase in all input prices (and hence an increase in unit costs) that causes a bank to choose a smaller size. When it shrinks, it reduces its total costs. But the shrinkage must lead to reduced revenue as well. (Suppose, to the contrary, revenue increases or stays the same as output shrinks: then profits would increase also because total costs fall as output shrinks. But this means the bank could have earned higher profits by shrinking even without a change in input prices.) Therefore, the increase in unit costs leads to a decrease in revenue.

If a market is perfectly competitive, on the other hand, the industry's gross revenue could either rise or fall, depending on demand factors, but banks' entry or exit would eventually force each surviving *bank's* gross revenue to change in the same direction as its unit costs. For example, if unit costs rise, all banks would suffer losses at their original prices and must increase prices (or reduce their deposit interest rates) to survive; some banks may fail or be forced to merge in the process of this adjustment. Conversely, if unit costs fall, banks would earn excess profits at their original prices, so competition would force prices down until they merely cover the new costs. If we saw a

[10]Both tests are discussed in more detail by Bresnahan (1989). A third technique, Tobin's "q" test, measures the ratio of a firm's market value to its book value (Smirlock and others, 1984). If this ratio exceeds 1, the firm is worth more than its assets. A common assumption has been that market power would be the underlying cause of such an excess; however, Spiller (1985) shows that systematic risk and efficiency can theoretically affect this ratio, potentially confusing the measurement of market power. Likewise, Angbazo (1992) presents evidence that efficiency, rather than market power, may be the true cause of values of q greater than 1, at least in the banking industry. A fourth technique, measuring asymmetric price rigidity as a function of market concentration, postulates that market power would lead to slower or less frequent adjustments in favor of the consumer but faster or more frequent adjustments in the bank's favor. Hannan and Berger (1991) and Neumark and Sharpe (1992) find evidence of this aspect of consumer deposit pricing.

case where revenue changed in the same direction as unit costs, but not by the same proportion, the conclusion would be that the market is substantially competitive but not perfectly so. (See *The Rosse-Panzar Test for Competition*, at the end of this article, for more details.)

These theoretical properties allow us to draw inferences about the degree of competition or market power from historical data on individual banks' costs (or on input prices) and revenues. A major advantage of the technique is that no geographic market need be defined a priori; even data from a single bank can suffice for the test. This avoids much potential bias from misspecified market boundaries. If the bank operates in more than one market, the measured conduct will reflect an average of the bank's conduct in each of its markets—which may tell us less than we would like to know to evaluate a particular merger involving one market, but it's at least useful in studying the validity of structural indices or the overall degree of competition in the banking industry.

One drawback of the test is that it can give misleading results under a variety of circumstances, such as when the number of banks in the sample has not fully adjusted to market conditions; the direction of bias in this case is always toward a spurious appearance of market power (Shaffer, 1982b, 1983a).[11] But, in general, when the test indicates a competitive outcome, we can be relatively sure that monopoly power is not being exercised.[12]

Several studies to date have applied this technique to banks (Table 1). One striking feature of these studies is the preponderance of competitive findings—even though the technique itself is biased against such findings and even though some of the markets examined in these studies (such as Canadian banking, dominated by half a dozen nationwide banks; or Fulton County, Pennsylvania, which contains only two banks) are highly concentrated. Taken together, these results suggest that competitive performance at the bankwide level is attainable with relatively few banks, although one study by Hannan and Liang (1993) finds that local market power may exist in some individual product lines such as money market deposit accounts.

The Markup Test. A second technique involves using historical data to estimate market demand curves, which indicate the amount demanded at each price, and banks' marginal cost curves, which indicate the amount it costs to produce each additional unit of output. These estimates can be combined in a way that determines where along the range between the competitive and monopolistic extremes the actual markup of price over marginal cost lies. Markups near zero indicate active competition, whereas markups near the monopolistic value indicate substantial market power.[13]

One advantage of this test is that it can pinpoint the degree of competition in a more precise way than can the Rosse-Panzar test. For

[11]This anticompetitive bias means that, in the absence of a reliable test for market disequilibrium, the Rosse-Panzar test cannot be used to rule out competitive pricing, as some studies have claimed.

[12]The test is also unable to distinguish between competitive pricing and simple "cost-plus" pricing, discussed above; but since cost-plus pricing is not specifically associated with a particular degree of market power, the implications of this limitation for interpreting the Rosse-Panzar test are not clear.

[13]This relative markup is directly related to the so-called "conjectural variation," defined as a firm's expectation of how its rivals would respond to a change in its own output level (see Shaffer, 1983b, in conjunction with Bresnahan, 1982). In fact, some of the studies (including the pioneering study by Iwata, 1974) describe the method in those terms. However, although the conjectural variation concept has drawn fire from game theorists as a method of predicting firms' behavior theoretically, the relative markup—even when called a conjectural variation—is a valid empirical index of market power (see Bresnahan, 1982; Tirole, 1988, p. 245, footnote 12; and Worthington, 1990).

TABLE 1
Rosse-Panzar Tests of Banking Competition

Author	Sample	Findings
Shaffer (1982a)	(1) unit banks in NY outside NYC (2) rural Illinois market leaders (3) single-market NYC banks (4) largest 20 NY banks (5) largest 20 CT banks (all samples from 1979-80)	rejects market power rejects market power possible market power rejects market power possible market power
Nathan and Neave (1989)	(1) Canadian banks, 1982-84 (2) Canadian trust companies, 1982-84 (3) Canadian mortgage companies, 1982-84	rejects market power for all three samples (see Neave and Nathan,1991, for more complete interpretation)
Molyneux and others (1992)	Japanese banks, 1986 and 1988	possible market power in 1986 but not in 1988
Hannan and Liang (1993)[a]	U.S. deposit accounts, 1983-89	possible market power
Shaffer and DiSalvo (1994)	Fulton County duopoly, 1970-1986	possible market power over full period; market power rejected for 1976-1986

[a]Used a technique very similar to Rosse-Panzar.

example, it allows us to measure actual market behavior in terms of an index that ranges from 0 for perfect competition to 1 for monopoly pricing.

This test also is not subject to the same sort of anticompetitive bias that plagues the Rosse-Panzar test under certain conditions, though other conditions can cause a similar bias. For instance, the test would overstate the degree of monopoly power if applied to only a subset of banks in a market (Shaffer, 1993b). To avoid this possibility, the investigator must make sure that the sample of banks studied spans at least one full geographic market. However, the remedy is fairly simple, as there is no bias if the sample is defined so broadly as to include several markets: then the test would show the average degree of monopoly power across the markets—not enough to evaluate a single-market merger, but enough to assess the general validity of structural indices or the overall degree of competition in the banking industry. Therefore, if the sample is defined broadly enough, a finding of market power by this test is more likely to be genuine, rather than a mere reflection of some other condition, than when using the Rosse-Panzar test.[14]

Several studies have applied the markup test to banking (Table 2). The results support the Rosse-Panzar studies in suggesting that

TABLE 2
Markup Tests of Banking Competition

Author	Sample	Findings
Shaffer (1989)	U.S. aggregate, 1941-83	competitive
Shaffer (1993a)	Canadian aggregate, 1965-89	competitive
Shaffer (1993b)	15 developed countries, 1979-91	market power in five countries; competitive in the most concentrated countries
Shaffer and DiSalvo (1994)	Fulton County duopoly, 1970-86	nearly competitive

competitive performance can result on a bankwide level from relatively few banks. The two techniques together form a way of cross-checking the results from either test alone, as was done in Shaffer and DiSalvo (1994) or as can be done for Canadian banking by comparing the Rosse-Panzar study by Nathan and Neave (1989) with the markup test by Shaffer (1993a).

POLICY IMPLICATIONS

Modern economic theory does not support a uniformly restrictive public policy toward market structure, either in the banking industry or in general. Rather, it implies that policies regarding market structure need to be grounded in empirical research, recognizing that the degree of competition in a market may not be systematically linked to market structure.

SCP-style empirical studies have given mixed results as a whole for banking. Further, among those studies that appear to show a link between structure and conduct or performance, most have been recognized as methodologically flawed, rendering their findings unsuitable as a basis of public policy. Studies that addressed these methodological problems provide much more limited support for a link between structure and the degree of competition in banking markets.

The new industrial organization style of empirical studies has a stronger conceptual underpinning, offers a variety of techniques that allow results to be cross-checked, and has given more consistent results for banking. The results generally suggest that most U.S. and Canadian banking markets behave quite competitively at the bankwide level, even where highly concentrated; this means that regulatory constraints on mergers and acquisitions are not always *necessary* to sustain competitive outcomes. There is also some evidence that with respect to certain individual product lines, such as consumer deposit accounts, banks may exhibit a degree of monopoly power in

[14] Although the markup test cannot work properly with data from only one bank (unless it's the only bank in the market), it can be applied to aggregate market data when individual bank data are unavailable. In fact, when aggregate data are used, this method also gives a useful estimate of excess capacity—positive or negative—in the market, relative to the competitive norm (Shaffer, 1993a,b). The same need to define the market broadly enough, as discussed above, arises whether aggregate data or bank-specific data are used.

unconcentrated as well as concentrated markets; this means that regulatory constraints on mergers and acquisitions are not always *sufficient* to attain competitive outcomes. Thus, the weight of recent evidence suggests that the SCP hypothesis does not adequately describe the banking industry.

However, the new IO style of studies cannot answer the important question of whether competitive conduct (where it is observed) is being sustained by the threat of antitrust action against objectionable pricing or other behavior, rather than being an intrinsic property of the markets. Thus, their results should not be interpreted as supporting repeal of all antitrust provisions; rather, they call into question the *structure-based* subset of antitrust policy, at least for the banking industry. An important implication of the new IO studies is that the current wave of consolidation in the banking industry—although it will likely increase concentration in some banking markets—will not *necessarily* lead to less *competition* in banking markets.

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The Rosse-Panzar Test for Competition

The idea behind this test is to observe whether a bank's total revenue changes in the same or opposite direction as its input prices (such as wages, office rental rates, etc.). As the following example illustrates, changes in the same direction indicate a competitive market, whereas changes in the opposite direction tend to reflect some degree of market power. The test was developed by Rosse and Panzar (1977) (see also Panzar and Rosse, 1987) and can be shown to be much more general than the simple example might suggest.

In a competitive market, as banks vie for customers, the selling price will eventually be driven down to the minimum average cost of production, and each bank will produce the asset quantity that minimizes its average cost. The bank's total revenue is the competitive price times its quantity. In the table below, average cost is originally lowest for a bank that produces $20 million in assets, and total revenue is initially $4 million (= $20 million times an average cost of $0.20 per dollar of assets), as shown in the left-hand "original revenue" column. If the bank's input prices fall, the bank's average cost curve may shift down to resemble the right-hand average cost column; the efficient size remains at $20 million, but total revenue declines to $3.8 million (= $20 million times $0.19 per dollar of assets), since the price is driven down by competitive forces—perhaps involving the entry of additional banks into the market—to match the new lower average cost. Here, total revenue changes in the same direction as costs. (The same effect could also be illustrated by considering an increase in costs.)

Bank Assets ($Mil.)	Original Average Cost per $ of Assets	Original Revenue ($Mil.)	New Average Cost After Input Prices Fall	New Revenue ($Mil.)
$17M	$0.23	$3.91M	$0.22	$3.74M
18	0.22	3.96	0.21	3.78
19	0.21	3.99	0.20	3.80
20	**0.20**	**4.00**	**0.19**	**3.80**
21	0.21	4.41	0.20	4.20

If instead a bank facing these same original and new average cost figures has some market power—that is, if its market is not perfectly competitive—its selling price (the interest rate it charges on a loan) will vary with the amount it produces, and it may choose a smaller size to maximize its profits. The table below shows the price that such a bank can charge at different asset sizes, as well as the resulting profit levels (calculated by subtracting total costs, using the average cost figures shown in the table above, from total revenues).

Bank Assets ($M)	Orig. Avg. Cost	Price per $ of Assets	Total Revenue (=Col.1 x Col.3)	Total Cost (=Col.1 x Col.2)	Original Profit (=Col.4 - Col.5)	New Avg. Cost	New Total Cost ($M)	New Profit (Col.4 -Col.8)
$17M	$0.23	$0.325	$5.525M	$3.91M	$1.615M	$0.22	$3.74M	$1.785M
18	0.22	0.31	5.58	3.96	**1.62**	0.21	3.78	1.80
19	0.21	0.295	5.605	3.99	1.615	0.20	3.80	**1.805**
20	0.20	0.28	5.60	4.00	1.60	0.19	3.80	1.80
21	0.21	0.265	5.565	4.41	1.155	0.20	4.20	1.365

Given the original costs, the bank can earn maximum profits by operating at a level of $18 million in assets, yielding a total revenue of $5.58 million and net profits of $1.62 million; in this protected market, competition does not force the bank to expand to the cost-minimizing size, and the bank can earn a positive profit. After the reduction in costs, the bank can earn maximum profits by operating at a level of $19 million in assets, yielding a total revenue of $5.605 million and profits of $1.805 million; no entry occurs to challenge these profits or to force the bank to reach the cost-minimizing size. Here, even though the asset quantity that minimizes average costs has not changed (i.e., $20 million), the bank with market power responds to a downward cost shift by expanding its output. As a result, its total revenue increases even though its average costs have fallen. Again, the same effect (that revenue moves in the opposite direction as average costs) could also be shown by considering an increase in costs.

Taxes and the Electoral Cycle: How Sensitive Are Governors To Coming Elections?

*Anne Case**

Recent tax increases in several eastern states have received attention from both voters and the press. In 1991, New Jersey Republicans gained veto-proof majorities in both houses of the legislature for the first time in 20 years. Shortly after the election, the *New York Times* of November 13, 1991, attributed the outcome to "[Governor Florio's] unpopularity and the $2.8 billion tax package he pushed through the legislature." In the wake of Governor Weicker's income tax legislation, 40,000 Connecticut residents "carried signs that called for everything from impeachment to lynching for the Governor and his budget officers," according to the *New York Times* of October 7, 1991. History suggests these tax changes may have cost the governors their jobs. In New Jersey, Governor Florio was unsuccessful in his re-election bid in

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November 1993. In Connecticut, Governor Weicker has announced he will not stand for re-election. Thad Beyle documents that "tax loss" governors have been a common sight on the political landscape since the 1960s.

Political economists, pollsters, and the popular press have long understood the tension that taxes create between elected officials and their constituents. Such tension is to be expected: governors are at times called upon to introduce or increase taxes in order to carry out the wishes of the electorate. Voters understand that tax increases are sometimes unavoidable, but they have limited information with which to assess each call for higher taxes. The electorate also has few tools available with which to punish or reward officials for their performance, and this may add to the tension. Citizens may protest tax increases, as was seen in both Trenton and Hartford, or reduce their political donations in the face of unwelcome tax changes. The electorate may also vote with its feet, leaving the state for one more frugal. These strategies, however, are limited in the size of the punishment they can bring to bear on incumbents. Exit may impose a larger cost on those who choose to move than on the errant official.

A more effective strategy for disciplining elected officials is, often, the ballot box. Threatening to unseat an incumbent may provide the most powerful lever under the electorate's control. However, fiscal decisions are made by elected officials who understand that their re-election odds depend upon their tax policies. For this reason, tax decisions may be based not only on their economic merits but on their political merits as well. This gives way to two potentially important phenomena. Voters, with limited access to information on the need for new taxes, may evaluate and vote on their governor's performance by comparing his fiscal policies with those of governors in neighboring states. Governors who would like to be re-elected may, for this reason, time state tax changes to coincide with those in states nearby. This would lead to a correlation between tax changes in states in close geographic proximity. In addition, tax increases may be postponed until a governor no longer fears the ballot box: tax changes may be timed to correspond with term limits.

Conventional wisdom suggests that voters react to recent changes in taxes. This article quantifies that reaction and shows how it depends upon what neighboring states have done. It also examines the impact of voters' comparisons between tax changes at home and in nearby states on a governor's tax setting behavior. Overall, the results tend to support Ferejohn, who suggests "the key to the voting decision is found not in the earnest pledges of the contenders but, rather, in the infamous remark of a Kansas farmer: 'But what have you done for me lately?'"

EFFECTS OF TAX CHANGES AND ECONOMIC PERFORMANCE ON RE-ELECTION

Data on state tax changes and economic performance indicate some clear differences in states in which the governor was re-elected from those in which he or she was not. We present data on state economies for the two years leading up to each election from 1979 to 1988 (Table). We use two-year changes in state economic and fiscal performance because it sometimes takes governors a full fiscal year to implement their fiscal policies.[1] States in which the governor was re-elected and those in which he or she was not differ dramatically in their taxing behavior. Increases in state income tax liabilities, measured in constant dollars, were significantly lower in those states in which governors were re-elected compared with states in which governors were not. On average, the

[1]The results presented in this article are similar when three-year changes are used.

TABLE

A History of Gubernatorial Re-elections

1979 - 1988

	All States Holding Elections	Governor Re-elected	Governor Not Re-elected	Significance Level of Difference[c]
Number of observations	74	47	27	
Change in income tax liability[a]	34.03	10.81	74.44	0.033
Change in state unemployment rate[b]	0.49	0.24	0.92	0.178
Change in state income per capita[b]	353.46	464.14	160.81	0.075

All changes are two-year differences: that is, the change between the fiscal year ending just prior to the election and two years before the election.

[a]Change in income tax liability for joint filers with no dependents who earn $25,000 annually, calculated for filers taking average deductions for this income category. Sample here restricted to states with income taxes.

[b]Source: Statistical Abstract of the United States.

[c]Significance level of difference between the average among governors re-elected and those unseated.

Source: National Bureau of Economic Research, TAXSIM data.

tax liability of joint filers earning $25,000 increased by $10 in the two years leading up to the elections in which the incumbent was returned to office. This contrasts with the $74 increase on average in states in which the incumbent was not re-elected. This difference, of $64, amounts to roughly 10 percent of the income tax liability of filers earning $25,000.[2] Voters appear to take tax increases during an incumbent's term into account when standing in the voting booth.

The overall economic health of the state also seems to influence whether a governor is re-elected. Changes in state income per capita during a governor's watch significantly affects the probability of his or her re-election. States in which governors were re-elected had lower increases in state unemployment rates (a 0.24-percentage-point increase versus a 0.92-percentage-point increase, on average) and significantly larger increases in income per capita ($464 versus $160, on average) than did states in which governors were not re-elected.

Thus, tax changes and economic performance seem to influence election outcomes. Can we quantify the effects? We can estimate the impact of changes in taxes on a governor's re-election odds and also the effects of state unemployment and income by using a statistical technique known as probit analysis. From 1979 to 1988, incumbents were eligible to stand for re-election in 74 races under study and were returned to office roughly 60 percent of the time. Governors ineligible to stand for re-election due to a binding term limitation are not included in this part of the analysis. In addi-

[2]Throughout this article we use joint filers with $25,000 in income as the basis for our analysis, but the basic results hold for other income levels as well.

tion, we do not study races in which the governor was eligible to run again but chose instead to run for the United States Congress. Governors eligible for re-election who chose not to run again and who did not run for Congress are included as governors "not re-elected." Voluntary retirement from public office is often a masked defeat: some governors would rather retire than go down to defeat at the polls.[3]

The results of our probit analysis suggest that an increase in tax liability significantly increases the probability that a governor will not be re-elected. (See *Increasing Taxes Lowers the Probability of Re-election*, in which the effect of an increase in tax liability on the probability of re-election is estimated to be .146.) If a governor were to increase taxes by $34.03, which is the average tax change observed during this period, this would reduce the probability of re-election by 5.0 percent (.3403 x .146), holding all else equal.

Voters appear to hold the governor more accountable for the impact of tax policy on their disposable income than for the impact of overall economic conditions within the state. In the period from 1979 to 1988, increases in state unemployment also appear to reduce the odds of re-election, but its effect is not as clear as that of tax changes.[4] The probability of re-election does not depend significantly upon changes in state income per capita. Gubernatorial sensitivity toward tax changes may, for this reason alone, be well placed.[5]

TAX CHANGES IN NEIGHBORING STATES

Do tax increases always reduce re-election odds? Good governors must raise taxes or cut services, or both, when costs rise more quickly than revenues. How does the electorate decide whether a tax increase is "appropriate"? Evidence from a study by Besley and Case (1992) suggests voters may look to neighboring states when determining whether a given tax increase is out of line. For example, a recession-driven revenue shortfall may require that taxes be raised if the government is expected to provide a minimum level of services. Voters without access to perfect information about the magnitude of such a recession may find it difficult to assess the need for a tax increase. However, if the recession has a regional component, voters may be able to add to their information base by noting how neighboring states have responded. Voters in New Jersey, for example, may look to the tax changes occurring in Pennsylvania and New York to determine whether a tax increase is appropriate. Neighboring states may provide a benchmark against which a given state's

[3]Inclusion of governors who voluntarily retire improves the precision of (reduces the standard errors on) our estimates but does not otherwise affect the analysis. We could also model transitions in the governor's chair from one party to another and, in this way, include in the analysis decisions made by governors who cannot stand for re-election because of term limits. Party loyalty may force elected officials to behave prudently even though they personally will not benefit at the ballot box. However, Besley and Case (1993) find that party loyalty does not appear to play a role in the decisions made by governors facing term limits, and for this reason, governors facing term limits are excluded from the current analysis.

[4]The statistical significance level of the unemployment rate effect varies greatly depending on what other variables are entered into the equation. (See *Increasing Taxes Lowers the Probability of Re-election*.)

[5]For a detailed analysis of the relationship between tax changes and gubernatorial re-election, see Besley and Case (1992). It is possible that when states face fiscal crises they simultaneously raise taxes and reduce expenditures. The effect of tax changes on re-election odds may be in part proxying for the impact of reduced expenditures on re-election odds. To test for this, changes in state expenditures per capita were added to the re-election equation. We found that the probability of gubernatorial re-election in this period is insensitive to changes in total state expenditures per capita, whether or not change in taxes is used as an explanatory variable.

performance may be measured. Information on tax levels and changes within a region is available in local newspapers. For example, at the time income taxes were introduced in Connecticut, the *New York Times* ran a front page article under the headline "Neighbors Challenge New York's Tax Reputation." The article compared effective tax rates for filers in different income categories living in a variety of cities in New Jersey, New York, and Connecticut. Such articles appear at regular intervals and may provide information adequate to allow voters to evaluate their governor's relative performance.

Our data suggest that voters gather and use such information. (See *... But Neighbors' Tax Policy Matters, Too.*) Tax increases in neighboring states appear to offset the effect of home-state tax increases. Governors are not penalized for tax increases if neighbors are raising taxes simultaneously. If neighboring states increase their tax liability by $34, holding all other things equal, this will increase the probability of re-election for the home-state governor by 6.6 percent (.34 x .194), almost exactly offsetting the reduction in the likelihood of re-election that (as we showed earlier) results from the same-size tax increase in his own state. This may have implications for gubernatorial behavior. Governors, recognizing that voters are making comparisons between tax changes at home and in neighboring states, may wait until neighbors are raising taxes before calling for a tax increase at home. Therefore, governors may become responsive to what neighboring states are doing.

We find tax changes are positively and significantly correlated between neighboring states during the 10-year period 1979-88.[6] There are several possible explanations for this correlation. Neighboring states may face shocks to their economies that are regional in nature, as argued above. Furthermore, changes in the national economy may cause neighboring states' tax changes in a given year to appear significantly correlated. We do not want to attribute to re-election concerns a correlation that is actually due to regional or national economic conditions. One natural way around this is to look separately at governors who are eligible to stand for re-election and those who are not. If correlation between neighboring states' taxes is due primarily to political concerns, we should find a positive and significant relationship between changes in home-state taxes and changes in neighbors' taxes only in those states in which the governor can stand for re-election. This is indeed what we find: in states in which the governor is ineligible to stand for re-election, there is no correlation between tax changes in home and neighboring states; in states in which the governor is eligible to run again, there is positive and significant correlation between tax changes.[7]

These relationships are presented graphically (Figures 1 and 2) for two-year tax changes observed in 1983 for joint filers earning $25,000 in each state. Similar patterns are present in every year. In Figure 1, the tax change in a given state is marked on the vertical axis, and the average tax change in that state's neighbors is marked on the horizontal axis. For example, Michigan had a very high change in taxes from 1981 to 1983, and so did Michigan's neighbors. Fiscal year 1983 ends before the elections of 1983. Comparing states whose governors were eligible to stand in their states' next election,

[6]The correlation coefficient is 0.17.

[7]The correlation coefficient for these states is 0.19. We continue to find a positive and significant relationship between neighbors' tax changes in states where governors can run again, even when we control for state income and unemployment, state demographic variables (proportion elderly and young in the state population), year effects, and state-specific fixed effects. We continue to find no relationship between neighbors' tax changes in states governed by lame ducks. See Besley and Case (1992) for tests based on alternative econometric specifications.

Increasing Taxes Lowers the Probability of Re-election

To estimate the effect of tax increases net of changes in income and the unemployment rate, we include all three variables in a probit equation. The dependent variable equals 1 if the governor was defeated in the primary or election or was eligible to run but "retired" and did not run for Congress; it equals 0 if the governor was re-elected. Changes in tax liability is the change in the effective state income tax liability of joint filers earning $25,000, expressed in hundreds of 1982 dollars. Change in state income per capita is also expressed in hundreds of 1982 dollars. The coefficients reported here are changes in the probability of incumbent defeat, evaluated at sample means.

$$\text{Governor Defeat} = \underset{(t=1.94)}{0.146} \times \text{change in tax liability} - \underset{(t=0.47)}{0.005} \times \text{change in state inc./cap} + \underset{(t=0.73)}{0.026} \times \text{change in state unemp.}$$

$$\text{Governor Defeat} = \underset{(t=1.77)}{0.126} \times \text{change in tax liability} + \underset{(t=0.54)}{0.007} \times \text{change in state inc./cap} + \underset{(t=1.70)}{0.082} \times \text{change in state unemp.} +$$

$$\underset{(t=2.75)}{0.022} \times \text{gov.'s age} + \underset{(t=1.42)}{0.245} \times \text{pres. election yr.} - \underset{(t=0.82)}{0.139} \times \text{pres. coattails}$$

Number of observations = 74.

If state income tax liability for joint filers were to increase by $100, this would act to reduce the probability of an incumbent's re-election by almost 15 percent. When changes in taxes, income and unemployment are entered simultaneously, it appears that the change in taxes is the dominating force behind incumbent defeat.

In addition to changes in state economic variables, many political variables may influence election results. For example, incumbents who must run in presidential election years may find it relatively more difficult to win re-election, given the larger voter turnout from both parties that occurs in presidential election years. In addition, there may be presidential "coattails." That is, if an incumbent is of the same party as the winning contender in the presidential race, he may receive votes that reflect the popularity of a president-elect. While this is possible, we find no evidence for either effect in the period studied here.

held sometime between 1983 and 1986, we see that states with large tax increases have neighbors with large tax increases, and states with small tax increases have neighbors with similarly small tax increases. In contrast, among states run by governors who are ineligible to stand for re-election, there is no observable pattern between neighbors' tax changes.

Regional shocks could cause state tax changes to be correlated between states in a region, regardless of whether the state is run by a governor eligible to stand for re-election. The data suggest, however, that only states in which the governors can run again show a positive

... But Neighbors' Tax Policy Matters, Too

We define geographic neighbors as states that share a common boundary. "Neighbors' tax change" is the average tax change experienced in a given state's geographic neighbors. In results presented here, all neighbors are given equal weight. Changes in taxes and state income are in hundreds of 1982 dollars.

Governor Defeat =

0.110 (t=1.66)	x	tax change $25,000 filers	- 0.194 (t=1.74)	x	neighbors' tax change $25,000 filers	+ 0.004 (t=0.29)	x	change in state inc./cap	+ 0.090 (t=1.97)	x
change state unemp.	+ 0.025 (t=3.03)	x	gov.'s age	+ 0.269 (t=1.66)	x	pres. election yr	- 0.112 (t=0.72)	x	pres. coattails	

Governor Defeat =

0.017 (t=1.68)	x	tax change $100,000 filers	- 0.059 (t=2.69)	x	neighbors' tax change $100,000 filers	- 0.010 (t=0.79)	x	change in state inc./cap	+ 0.053 (t=1.29)	x
change state unemp.	+ 0.029 (t=3.42)	x	gov.'s age	+ 0.165 (t=1.21)	x	pres. election yr.	- 0.138 (t=0.91)	x	pres. coattails	

Number of observations = 74.

Increases in neighboring states' taxes offset the effect of tax changes at home on an incumbent's re-election odds; the absolute value of the coefficients on own tax changes and neighbors' tax changes are not statistically different from one another.

The effect of changes in income taxes on the probability of re-election is present in different parts of the income distribution. The increase in the probability of gubernatorial defeat when taxes are raised at home, and the offsetting effect of increases in neighbors' taxes, are seen here for both $25,000 joint filers and $100,000 joint filers.

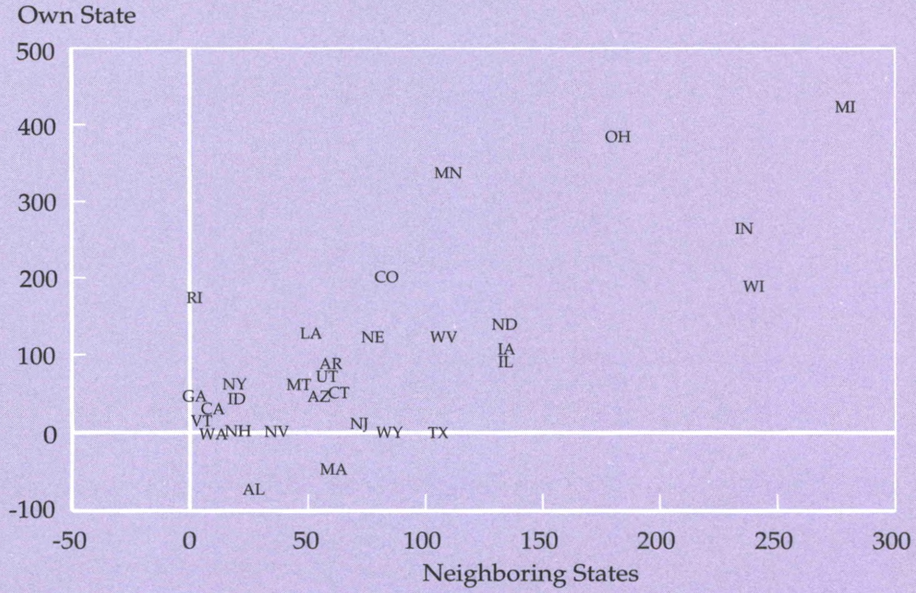
and significant correlation with neighboring states' tax changes. We take this behavioral difference as evidence that the sensitivity to neighbors' taxes is due to electoral effects.

Contrary to textbook public finance models in which state taxation decisions are based solely on economic criteria, our evidence suggests that the governors' political timetable and the behavior of neighboring states may influence state taxation decisions.

TERM LIMITS, ELECTORAL CYCLES, AND TAXATION

The timing of tax changes may be affected

FIGURE 1
The Relationship Between Own and Neighbors' Tax Changes (1981-83) When the Governor Is Eligible for Re-election[a]



[a]Increased tax liability in dollars for joint filers with incomes of $25,000

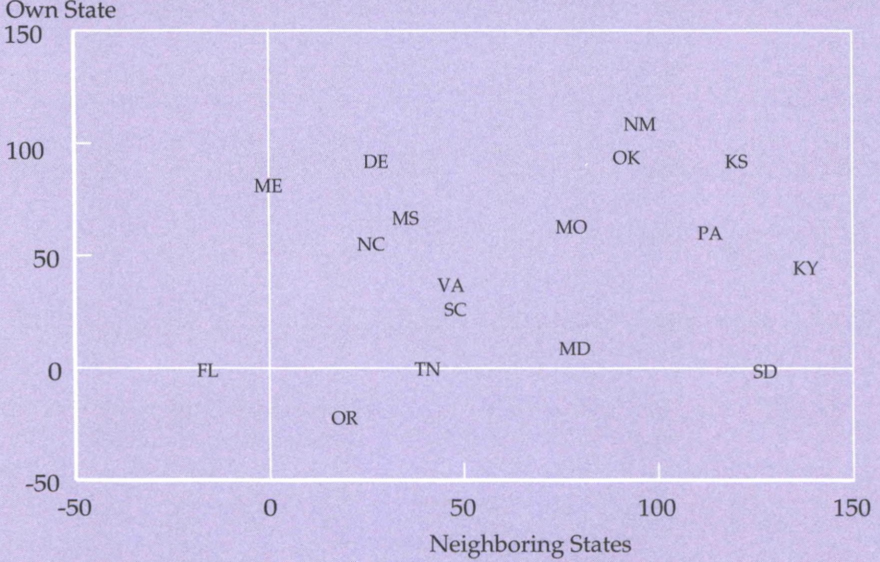
not only by changes in neighboring states but also by the presence of term limits. The political economy literature discusses the potentially offsetting effects of such limits. James Adams and Lawrence Kenny suggest that, in the absence of term limits, it may be relatively easy for one party to put a lock on the governor's office, especially in small states. Political capital may accrue to the party in office, acting to increase the odds of gubernatorial re-election. States may perceive term limits as a way to block the accrual of political capital and, thus, as a means to broader representation.

While term limitations provide a guarantee that a state will not be stuck with a bad incumbent indefinitely, this guarantee may come at a price. In addition to the costs associated with learning about candidates and voting, there is also the possibility that incumbents, as lame ducks, may change their behavior to better suit their own long-term goals. Some analysts do not believe such a change in behavior is likely. Given that parties live forever even when incumbents do not, Alberto Alesina and Stephen Spear suggest that the incumbent's political party could compensate the official to keep him in line and, in this way, protect others within the party from being punished in response to the lame duck's behavior. Lott (1990) provides some evidence of this among congressmen.

We find differences in many aspects of the taxing behavior of governors eligible to stand

FIGURE 2

The Relationship Between Own and Neighbors' Tax Changes (1981-83) When the Governor Is Ineligible for Re-election[a]



[a]Increased tax liability in dollars for joint filers with incomes of $25,000.

for re-election and those facing term limitations, in addition to the difference in sensitivity toward neighbors' tax changes discussed above. Our data suggest that governors who are hitting term limits increase taxes more than those who can stand for re-election. For example, controlling for state income per capita, state unemployment, and state-specific effects, we find that in each year of the term of an incumbent ineligible to stand for re-election, state income tax liability for $25,000 joint filers increases by $26 more per year than it does when that state is governed by an incumbent who can run for re-election.[8] Over a four-year term, this amounts to a tax increase of $106 (26.49 x 4), or roughly 15 percent of the tax liability of $25,000 filers. This is true even though, on average, states with term limits have lower income tax liability for $25,000 joint filers: $650 versus $783

[8]This result comes from the regression: Tax liability = 26.49 x an indicator that the incumbent is ineligible to run again (t = 2.33), controlling for state income per capita, state unemployment, and state- specific fixed effects. (Number of observations = 480: 48 states for the period 1977-86. Regression run with heteroskedasticity consistent standard errors.) The sample is larger than that for gubernatorial re-elections because we have tax data for every year of an incumbent's term, not just election year data. In addition, the sample covers both governors who can and cannot run for re-election. Indicator variables were used to see if taxes varied within the four years of a term. We found no evidence that they did. Taxes were higher by about the same amount in every year of a lame duck's term.

on average from 1977 to 1988. This lower tax is maintained through gubernatorial behavior in the years in which the governor is eligible to run for re-election. In many states with term limits, a governor can serve for two consecutive terms. In the first term, the governor holds tax changes below the state's average. If re-elected, the governor raises taxes more than the average for that state. In this way, term limits lead to electoral tax cycles.

CONCLUSION

Models of fiscal decision-making must take political variables into account if they are to adequately capture reality at the state level. The analysis of gubernatorial behavior suggests re-election looms large in choices made by incumbents. The common perception is that governors who raise taxes do not get re-elected, and therefore, governors are reluctant to propose tax increases (at least in the two years prior to an election). In fact, the situation is more complicated. The experience of neighboring states in raising taxes has a great deal of influence. Moreover, governors who are not eligible for re-election may raise taxes more than other governors, producing an electoral cycle in tax policy.

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