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Economists and political scientists have long debated the nature of the process that determines government taxation and service levels in a democracy. During the 1980s, the role of referenda in determining city and town property taxes, and hence local spending, increased dramatically in Massachusetts. This article uses recent Massachusetts experience to examine the degree to which citizens “get what they want” from the local public sector and what it is they seem to want.

The passage of Proposition 2½ in November 1980 signalled both a shift in statewide voter sentiment against local officials’ previously “unfettered” decision-making and a change in rules, making it more difficult for localities to raise property taxes, their only major local revenue source. The impact has been uneven across communities. Nonetheless, until late in the decade, both sizable additions to local aid and a booming real estate market allowed many communities’ expenditures to grow moderately without bumping against their Proposition 2½ limits. 3

This article summarizes the Bank’s economic conference held in June 1990. The conference aimed to determine the extent to which the United States may be underinvesting in public infrastructure, explain the potential economic consequences, and suggest mechanisms to help alleviate any adverse trends. It focused on public investment in physical capital only to make the topic manageable, and should not be interpreted to mean that investment in human capital is in any way less important.

Two quite different perspectives on the need for more infrastructure investment emerge from the discussion. On one side are those who see a strong link between public capital investment and economic and social well-being; they view the current stock of public capital as inadequate and believe that additional investment is required. On the other side are those who are primarily concerned with the efficient use of existing infrastructure; they basically oppose increasing investment until the engineering, pricing, and financing of infrastructure are closer to the optimum. 23

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Recent experience with exchange rate management has rekindled interest in the efficacy of foreign exchange intervention. While there is broad evidence that sterilized intervention has no effect on the exchange rate through a portfolio balance channel, less evidence exists on the signalling role of intervention. This article considers the signalling role of intervention for the United States and West Germany between the 1985 Plaza Accord and the October 1987 stock market crash.

An examination of the data shows that intervention observed by the foreign exchange market did not precede changes in monetary policy in a proximate or consistent fashion. Thus the study concludes that, after the fact, intervention was not a signal of subsequent monetary policy. The study also explores the possibility that during this period participants in the foreign exchange market viewed intervention as a signal. While the daily response of the change in the deutsche mark/dollar exchange rate showed a significant effect of intervention in the early part of this sample period, the effect eroded over time as monetary authorities failed to back up intervention with monetary policy. 39

In the present climate of intense debate over deposit insurance reform, the nature and limits of market discipline become especially important. The widely accepted argument for greater reliance on market discipline is that it will restrain managerial risk-taking and reduce potential losses to the deposit insurance fund. Opponents of this view favor the traditional reliance on supervision by the bank regulatory agencies as the primary method to maintain the safety and soundness of the banking system and the integrity of the deposit insurance fund.

This article attempts to shed some empirical light on the issue by studying the effectiveness of market discipline as it is exercised by bank stockholders. Residual analysis is used to test whether the market anticipates the bank's downgrade to a problem bank status. The results show that shareholder returns fail to anticipate bank downgrades by examiners. These results cast serious doubt on the supposed advantages investors, and particularly uninsured depositors, would have over bank regulators in restraining risk-taking by banks and in monitoring their management. 51

Can Local Governments Give Citizens What They Want? Referendum Outcomes in Massachusetts

Katharine L. Bradbury

Assistant Vice President and Economist, Federal Reserve Bank of Boston. The author is grateful to Andrew Evans for excellent research assistance; to Roger Hatch, John Sanguinet, and Julie Slavet at the Massachusetts Department of Revenue's Division of Local Services for providing and explaining data from the Municipal Data Bank; and to Andrew Reschovsky, Peter Fortune, Martin Linsky, and colleagues at the Boston Fed for helpful comments on earlier drafts.

Economists and political scientists have long debated the nature of the process that determines government taxation and service levels in a democracy. The two basic questions are the degree to which citizens "get what they want" from the public sector and what it is they want. What citizens want from the public sector may vary with both individual and governmental characteristics, as well as with political currents, such as the "tax revolt" that swept the nation beginning in the late 1970s. Residents' desires may not be directly reflected in government outcomes when elected officials or even the voters themselves (through referenda) are in control of decisions about funding levels, and government agencies "produce" the public goods and services provided.

Some analysts view referendum outcomes as a direct expression of residents' preferences; after all, residents may go to the polls and approve or disapprove a specific project. Others, however, argue that disagreements among the citizens of a jurisdiction, lobbying by interested parties, or variations in voter turnout may skew the vote outcome. Furthermore, they point out that public officials, not residents directly, decide what will appear on the referendum ballot, limiting the choices facing voters. And local administrators decide on how the budget is converted into local public services. That is, even with referenda determining the total budget, voters may not trust local officials to spend the money as they wish.

During the 1980s, cities and towns in Massachusetts experienced a fundamental change in the ground rules for local revenue-raising that increased the importance of local referenda. Localities in Massachusetts have only one significant revenue source of their own—the property tax. In November 1980, the Commonwealth's voters enacted Proposition 2½, bringing down property tax rates and limiting the year-to-year rate of growth of property tax revenues. Under Proposition 2½, a community's property tax rate had to be reduced to 2.5 percent and thereafter

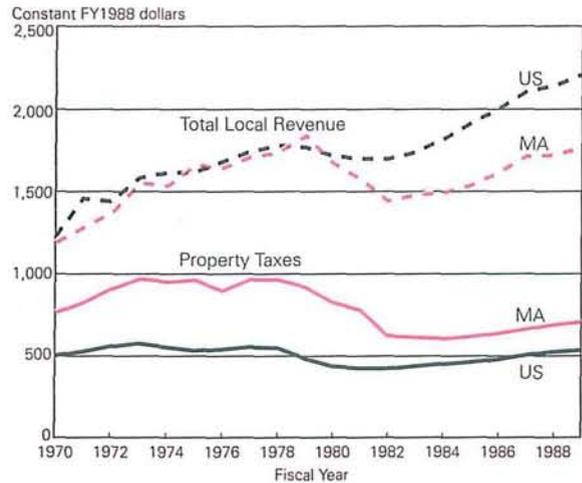
the community's cap on property tax revenues (the "levy limit") rises by 2.5 percent per year (plus an allowance for new growth) unless voters approve a local referendum to raise property taxes by more.

Because property values rose faster than 2.5 percent annually, property tax rates generally declined in the 1980s. But because general inflation and local costs also rose faster than 2.5 percent per year, an increasing number of communities became constrained by Prop 2½'s limit on levy growth as the decade progressed. As a result, an increasing number of localities proposed and approved referenda to

By FY1990, almost 300 of the Commonwealth's 351 cities and towns were taxing at 99 percent or more of their levy limits, up from fewer than 130 as recently as FY1987.

Figure 1

Local Government Revenues per Capita



Source: U.S. Bureau of the Census, *Government Finances*.

increase their Prop 2½ "levy limits."

This article uses the Massachusetts experience in the last decade to shed light on the general issue of whether and how resident preferences find expression in local public sector outcomes as well as on what those preferences appear to have been in this particular time and place. After reviewing recent patterns and trends of property taxation and referendum outcomes in the Commonwealth in Part I, the article focuses on votes that communities took to increase levy limits applying to fiscal year 1991. As background, some of the economics and political science literature on local voting is reviewed in Part II. Because so many communities have never once held a vote to raise the levy limit, the discussion of referenda distinguishes between the determinants of calling for a vote at all (in Part III) and the vote outcome (in Part IV). The conclusion speculates about possible future referendum patterns in Massachusetts.

I. Putting on a Lid: Property Taxation in Massachusetts in the 1970s and 1980s

Property tax revenues in the Commonwealth rose almost 1 percent per year in real terms in the 1970s and declined almost 1.5 percent per year in the 1980s. Nationally, by contrast, property taxes de-

clined slightly in real terms in the 1970s and grew over 3 percent annually in the 1980s.¹ This radical downshift in property taxation in Massachusetts was attributable to Proposition 2½, which began restricting local property tax levies in Massachusetts in fiscal year 1982 (FY1982).

Total property tax revenues actually declined in nominal terms in FY1982, as all communities with effective property tax rates in excess of 2.5 percent (about half of Massachusetts' 351 cities and towns) were required to cut revenues 15 percent (and in each year after FY1982) until the rate fell to 2.5 percent, while the other communities' property tax revenues could rise only 2.5 percent annually. (See the box for definitions of key terms and a summary of how Proposition 2½'s limits are calculated.) These declines caused the property tax gap between Massachusetts and other states to narrow noticeably (see Figure 1), but after most of the reductions were complete in FY1984, real per capita local property tax revenues rose at about the same rate in Massachusetts as in the nation as a whole.²

After the early years of revenue reductions in a number of communities, statewide property tax rev-

How Proposition 2½ Works

The levy limit is the maximum amount of property tax revenue a community is allowed to raise in a given fiscal year under the restrictions of Proposition 2½. Each community's initial limit was set at 2.5 percent of the market value of taxable property. The levy limit rises by 2.5 percent per year plus an allowance for new growth; local residents can also vote to raise the levy limit in a given year by enacting an override or exclusion. Even if a community's property tax revenue (its levy) is not at its levy limit in a given year, the limit rises by 2.5 percent per year plus new growth (unless this increase would cause the effective tax rate to exceed 2.5 percent). Thus a community's levy limit is not affected by its actual levy in earlier years. Furthermore, the levy can rise by more than 2.5 percent in a year if it is below the levy limit the previous year.

The allowance for new growth is calculated as the previous year's tax rate multiplied by the amount of new growth. New growth includes the value of all new or substantially renovated property on the tax roll (properties whose values rose by more than 50 percent in a year's time not simply because of revaluation, or exempt property returned to the tax roll, or the added value of subdivision parcels and condominium conversions). Each year's new growth goes into the levy limit which then automatically grows by 2.5 percent to form the basis for calculating the next year's

limit. (The legislature recently broadened the definition of new growth, as recommended by the Hamill Commission, to include all increases in value except those attributable to simple appreciation or revaluation, beginning in FY1992.)

An override is a permanent increase in a community's levy limit. Voters enact an increase in the levy limit for a specific fiscal year; the increase is permanent in the sense that the new levy limit then becomes the base for calculating future years' levy limits. An exclusion, by contrast, is a temporary increase in the levy limit for a specific capital expenditure or debt service. The levy limit is increased by the amount of funds needed for the capital expenditure (one year) or to pay debt service on specific debt issues (for the life of the debt issue).

Except in the case of exclusions, the levy limit can never exceed 2.5 percent of the market value of the property tax base (the "levy ceiling"). That is, the effective property tax rate at the levy limit cannot exceed 2.5 percent except when voters have enacted exclusions that temporarily raise the levy limit above this levy ceiling.

Note: For careful and more complete explanations of Proposition 2½'s workings, see *Everything You Always Wanted to Know About Levy Limits . . . But Were Afraid to Ask: A Primer on Proposition 2½*, prepared by the Division of Local Services, Massachusetts Department of Revenue.

enue growth was accelerating by the mid-1980s, and levy limits were growing about as fast, as allowances for new growth gave most communities considerably more than the 2.5 percent automatic annual additions to the levy limit. (Appendix Table A.1 reports the year-to-year changes in property tax revenues and levy limits on a statewide basis in the years after Proposition 2½.) But aid from the state, which increased substantially in the early years of Prop 2½, began to grow more slowly and then declined at the end of the decade, increasing pressure to raise property taxes and propelling more and more communities close to their limits. By FY1990, almost 300 of the Commonwealth's 351 cities and towns were taxing at 99 percent or more of their levy limits, up from fewer than 130 as recently as FY1987.

Meanwhile, property values skyrocketed, leading to declines in effective property tax rates and a growing difference between the levy limit and the levy ceiling (2.5 percent of property values). This meant that the "override capacity" that could be tapped by voting overrides or exclusions was also growing by leaps and bounds, at least through FY1990. The pressure on levy limits as more and more communities approached them, in conjunction with this growing "capacity" as property values rose and property tax rates fell, led to increased use of overrides and exclusions to raise the levy limit.

The number of communities attempting to raise their levy limits rose fairly steadily year by year through FY1990, and then jumped for FY1991. Even with a sizable number of first attempts each year, the

bulk of overrides and exclusions were enacted in communities that had passed them before. (Appendix Table A.2 reports the number of communities attempting and passing overrides and exclusions in FY1983 through FY1991.) The success rate of override and exclusion attempts also rose and then dived in FY1991, although the number of communities passing overrides and exclusions was higher for FY1991 than for FY1990. Even so, many communities still have not put any override or exclusion ballots before their voters. Almost one-sixth of the 351 cities and towns had proposed neither overrides nor exclusions to their voters through FY1991 (and another one-sixth had not passed any that were proposed).

The contribution of overrides and exclusions to the levy limit accelerated as the decade progressed. Table 1 reports statewide trends in levy limit growth, breaking out the contributions made by overrides, exclusions, and new growth; the limit also rises by an automatic 2.5 percent per year. While new growth accounts for over one-half the levy limit increase each year, voted increases jumped into double digits in FY1989 and continued to rise thereafter.

Even the sizable amounts of new growth that occurred in the late 1980s did not raise levy limits enough to keep many communities from bumping into them, and the prognosis is for much more constraint in the near future. The contribution of new growth declined in FY1990 and can be expected to

decline further in FY1991 and FY1992, because of the drastic falloff in new construction statewide as the economy has weakened. Among the 247 communities for which complete FY1991 data are available from the Department of Revenue, current new growth accounted for only 37 percent of the increase in the levy limit, while overrides and exclusions jumped to 29 percent of the total increase. The pickup in voted increases kept the overall rate of increase in the levy limit comparable to the previous year's for these communities.

Underlying these statewide trends were widely different experiences for cities and towns in the Commonwealth of varying population size. The key difference is that the smallest municipalities enacted more overrides and exclusions, and as a result, they experienced the most rapid increase in property tax revenues over the decade. The smaller places had both a greater incidence of override and exclusion attempts and a higher rate of success in passing them. (See Appendix Tables A.3 and A.4.)

Despite much faster growth in property tax revenues, the small and middle-sized towns enjoyed lower property tax rates and higher levels of local public services at the end of the decade than the biggest communities. Tax rates could be similar or lower even while financing above-average service levels where property value per capita (the underlying property tax base) was high. The per capita tax

Table 1
Sources of Growth in the Levy Limit Statewide

Fiscal Year	Dollar Increase in the Levy Limit (millions)	Percentage of Statewide Total Increase from:			
		Overrides ^a	Exclusions ^b	New Growth ^c	Automatic 2.5% ^d
1986	196.0	.4	3.4	60.4	35.7
1987	244.5	1.0	4.9	56.1	38.1
1988	263.2	2.4	6.2	56.3	35.1
1989	303.6	7.9	7.4	56.4	28.2
1990	297.3	8.8	9.5	52.7	29.0
1991 (est.) ^e	320.8	22.2	6.6	37.0	34.2

^aIncludes 2.5 percent compounded annual growth of override amounts approved in previous years.

^bNet change in exclusions affecting levy limit in fiscal year, including those voted in earlier years.

^cIncludes 2.5 percent compounded annual growth of "new growth" amounts certified in previous years.

^dThis column reports the (residual) difference between 100 percent and the sum of the three columns to the left; it may not exactly equal the actual automatic 2.5 percent increase from each community's base year levy limit because levy limits do not reflect retroactively reported new growth until it is reported.

^eEstimates for FY1991 based on data available for 247 communities.

Source: Massachusetts Department of Revenue, Division of Local Services, Municipal Data Bank, machine readable data files and author's estimates.

base in communities with population over 5,000 was only 60 percent of that in communities with fewer than 5,000 residents, on average.

II. Referenda on Property Tax Revenues: Hypotheses Regarding the Expression of Residents' Preferences

The changes over time and variation among communities in override experience in the 1980s might directly reflect differences in residents' demands for local public services or it might indicate differences in the ability of voters and/or officials to make the override process work for them. In studying the budget outcomes that result from democratic political processes, researchers have focused on referenda at the local level as being a closer indication of "what the voters want" than are decisions of elected officials that need no direct voter approval. The body of research on referenda has offered a variety of hypotheses about how local residents' preferences find expression in local tax rates and services.

Many observers would argue that Proposition 2½ has done exactly what it was intended to do—put control of the budget into the hands of community residents. Formerly, residents controlled the budget only through participation in Town Meetings or by electing local public officials they believed would do their bidding to ensure reelection. But the conventional wisdom says that the Commonwealth's voters approved Proposition 2½ because they no longer trusted local officials to serve the best interests of residents. Under Proposition 2½, community officials could increase property taxes only 2.5 percent per year (plus an allowance for new growth), unless they gained voter approval for an override. Especially as more and more communities bump up against their levy limits, the override process lets voters decide on service levels. In this view, the communities that do not have overrides on the ballot are communities in which an override would fail, anyway; local officials can read local voter sentiment reasonably well and do not bother the voters with obviously doomed proposals.

Who Controls the Agenda?

But other analysts argue that, even though residents can vote on overrides, their choices are limited to what is on the ballot. In particular, the failure of an override indicates only what the voters reject, not what they want. In this view, the critical question is

what group controls the agenda and what it is that they want. In Massachusetts, elected officials are the ones who decide what overrides or exclusions will be put to a vote: a majority vote of a community's Selectmen, or Town or City Council, with the Mayor's approval in some cases, can put an override question on the ballot. A two-thirds vote is required to put a capital or debt exclusion on the ballot.

If the process is to be successful in representing voters' desires, the "agenda setter" should propose overrides that have a reasonable chance of attracting at least 50 percent of the voters. But the agenda-setters may misread voter preferences in deciding what to offer the voters, or may even act strategically to influence the outcome.³ Different types of government (cities vs. towns, representative vs. open town meetings), or simply variation in the degree of diversity within a community, may affect the ability of the agenda setters to read voter preferences. Furthermore, local officials may not aim for 50 percent voter approval. Fearing voter rejection more than the consequences of underestimating voters' desires, officials may propose only overrides they view as certain to gain approval (Peterson 1991).

The basic issue is the all-or-nothing nature of an override vote. Residents can approve (or disapprove) only what is on the ballot and they influence what appears on the ballot only by convincing their elected officials to offer it to them. As a way around this problem, an increasing number of communities attempting overrides and exclusions in recent years have used a "menu" approach, putting multiple proposals on each ballot, thereby giving the voters more choice about which projects to fund and how much to spend. Also, some Town Meetings have recently voted "contingent appropriations" to pressure their Selectmen to put override questions on the ballot.⁴

Who Controls Production?

A second basis for doubting that Proposition 2½ really gives voters control over local services is the inescapable fact that local public employees control the actual production of public services, given the (voter-approved) budget. Residents may mistrust these employees, believing that it is possible for the Assessor, for example, or the Police Chief or Superintendent of Schools to produce current services at lower cost by operating more efficiently.⁵ But department heads and other administrators do not offer this as an alternative, either because it is not, in fact,

feasible or because it is in their self-interest to continue business as usual.

When this mistrust is widespread, as it appears to have been leading up to the passage of Proposition 2½, voters control what they can—the budget—in hopes that employees will economize. But administrators faced with a tighter budget may not make the choices that voters want them to make, perhaps because the voters are wrong about more efficient operations being possible. And one voter's "waste"

As middle-class taxpayers have shifted their view of government from concern about programs to a focus on the taxes they must pay, the link between the two is sometimes blurred.

may be another's most valued program.⁶

Thus, even under Proposition 2½, voters may have control over some decisions about property taxes and hence the budget, but not over how the money is spent. The agenda-setters still decide the purposes and amounts of overrides that will be put on the ballot and what will be cut from the budget if the override fails. Local legislators (who could be the attendees of an open Town Meeting) and administrators also make the decisions, as they did before Proposition 2½, about how to spend the funds raised within the Proposition 2½ limit (whether or not an override passes). Residents continue to have the power to vote their local officials out of office (or attempt to influence open Town Meeting outcomes), but this translates into budget control only indirectly and with substantial lags.

Do Voters Control Even the Vote?

Proposition 2½ override and exclusion vote outcomes might not reflect residents' wishes for a third set of reasons as well. Holding an election is a cumbersome and time-consuming undertaking. Voter turnout is never particularly high, and those who turn out may not be representative of all eligible

voters or residents. It is difficult for all voters to be well-informed. Various interest groups, or simply the more vocal segments of the population, may wield considerable power in shaping public opinion regarding the issues on the ballot. And given the generally negative view of taxation that has evolved in Massachusetts (and nationwide) in the last fifteen years, even voters who are well-informed regarding the specific issues they face on the ballot may not be sure how those issues relate to what they want from their local government.

As middle-class taxpayers have shifted their view of government from concern about programs to a focus on the taxes they must pay, the link between the two is sometimes blurred. For example, Massachusetts residents talk about moving to neighboring low-tax New Hampshire, "if it weren't for Massachusetts' good public schools." An override vote is explicitly aimed at raising taxes, which no voters will favor independent of a clear perception of individual or community benefit from the services those taxes will buy.

Findings/Hypotheses

Several things are clear from this discussion. First, Proposition 2½ made it more difficult for local governments to raise taxes than was the case with the old "rules." While it is impossible to separate the effects of the change in rules from the change in political consciousness that voted in the new rules, Proposition 2½'s rules undoubtedly had (and continue to have) a constraining effect on property taxes at the margin. In a sense, Proposition 2½ itself can be seen as a shift of "agenda control" toward those in favor of tax minimization, forcing a harder sell on program advocates. Some would say, indeed, the shift of power was too great, to the detriment of local public services, but others argue that voter control is crucial if "unwanted" spending is to be avoided.

Second, many of the concerns that are raised about how well referenda can represent residents' desires are alleviated by the "menu" approach to overrides, wherein voters are offered a number of proposals that can be separately approved or not. The menu approach can greatly reduce the all-or-nothing character of the override choice and provide an outlet for anti-tax sentiment and mistrust of local officials by giving voters some items to vote against even as they approve others. The menu approach may make it even more difficult, however, for voters to become informed, since more items appear on the ballot.

The "Menu" Approach: Multiple Override Proposals on One Ballot

In July 1987, the Legislature amended Proposition 2½ to allow any override to pass with a majority vote (overrides raising the levy limit more than 2.5 percent previously required a two-thirds vote) and to require that an override ballot proposal state the purpose of the override. As a result of these changes, many override ballots now contain a number of separate override proposals, each with a specific dollar amount and stated purpose, which can be voted up or down individually. Some communities present "pyramid" overrides to their voters, which allow voters to choose among two or more funding levels for a specific purpose; the highest dollar amount that gains approval governs.

Approaches such as these allow voters much more direct control over the local budget than do all-or-nothing votes on a sizable percentage increase in the levy limit. While local officials still control the proposals that appear on the ballot, they (obviously) cannot control which ones the voters approve or vote down, and as a result they have less discretion in making spending decisions after the vote is taken, no matter what the voters enact. Thus offering the voters more choice shifts some power from local officials to the voters. (Certainly the presence of a range of proposals on the ballot weakens an "agenda-setter's" ability to

gain voter approval of higher spending through contrast with a weak fallback. The possibility of voter choice among several proposals allows more of a continuum of possible outcomes.)

By the same token, however, officials who want override proposals to pass may increase their probability of success by offering some choices to the voters. One view of the process is that voters, mistrusting their local officials' judgment as to the urgency of various local needs, want to be able to express disapproval of some proposals even as they approve those they consider important.

An analysis of ballot outcomes for FY1991 confirms the view that giving voters some choice increases the likelihood of approval. The 598 override proposals for FY1991 appeared on 225 ballots; the number of override proposals on a single ballot ranged from 1 to 28. Slightly less than 42 percent of the single-question ballots passed, while voters approved at least one item on ballots with more than one proposal 46 percent of the time. Indeed, on ballots with more than 5 questions, something passed 57 percent of the time.

Note: For a detailed description of override procedures and requirements, see "Proposition 2½ Referenda Questions: Requirements and Procedures" (October 1990), prepared by the Division of Local Services, Massachusetts Department of Revenue.

Some observers argue that overrides cannot successfully replace the local budget process—they claim that in putting a "menu" before their voters, local officials have abdicated their responsibility to lead. Others, however, point out that broad slogans and single-issue campaigns have less power in the "menu" context, allowing (forcing?) voters to think through their programmatic priorities. (See the box.)

Third, the variety of individual communities' experiences to date undoubtedly reflects variation along all the dimensions just discussed. For many towns, the restrictions of Proposition 2½ combined with the override process probably work well in translating voters' desires into budgetary decisions; they either have no need for overrides or routinely pass them as needed. In others, the agenda setters may not have successfully tuned in to the alternatives

residents would like to see on the ballot. Yet other cities and towns seem stalled in a standoff between program advocates, concerned with maintaining the quality of local services, and a large bloc of voters, unconvinced that additional tax revenues are needed to obtain or maintain those services. And some municipalities may be unable to pass overrides even to maintain services because they lack the local resources to support increased taxes.

III. Getting on the Ballot: Who Tries?

About one-sixth of the Commonwealth's 351 cities and towns (containing over 40 percent of the state's population) had not attempted any override or exclusion votes through fiscal year 1991. Two types of

communities might be expected to "abstain" from attempts to raise the levy limit. Towns not taxing close to their levy limits or towns with considerable "new growth" could increase property tax revenues faster than 2.5 percent per year without a vote, and hence would not be likely to need to put any override proposals on the ballot. By contrast, a second group of communities with little excess capacity and low incomes, low property values, and high property tax rates might even face significant service cuts without an override, but their officials believe that voters are unwilling to approve still higher rates or they dare not risk the negative reaction that proposing higher rates might bring.

If the process of getting proposals on the ballot "works" in the sense that residents end up with what they want whether they have voted or not, then any communities that have never passed an override or exclusion, whether or not it has been attempted, should be fairly similar along critical voter preference dimensions, and differ from those that have passed overrides. But if the very process of getting an override on the ballot is a significant hurdle, then communities voting down override proposals might resemble communities successfully enacting increases in their levy limits as much as they resemble communities never making the attempt.

Attempts to Raise Levy Limits through FY1990: Patterns

Table 2 reports average values of some indicators of the cost of and need for (and presumably voter preferences regarding) overrides or exclusions for the three groups of cities and towns just discussed. The conventional economic wisdom says that residents with higher incomes would be more likely to enact an increase in the property tax levy because they generally demand a higher level of local public services and can afford to pay for it. The opposite would be true of residents facing high property tax rates that reduce their effective after-tax incomes. Significant excess capacity (room between the levy and the levy limit), whether from substantial rates of new growth, little need to raise taxes in the past, or other sources, would obviously reduce the need for an override or exclusion. The table also reports the "price" of local government services financed via the property tax, an indicator of what it costs a community's average single-family homeowner when community-wide property taxes rise by one dollar per capita. Where residents face a bigger increase in their tax bill to

Table 2
Characteristics of Massachusetts Communities Grouped by Pre-FY1991 Attempts to Raise the Levy Limit

	All Cities and Towns	Overrides and Exclusions through FY1990		
		No Attempts	Some Tries, No Passes	Some Passes
Number of Communities	351	106	42	203
1988 Income Per Capita (\$000)	14.7	13.5	14.1	15.4
Property Value Per Capita (\$000)	98.5	69.6	63.5	120.9
Property Tax Rate, FY1990 (%) ^a	1.20	1.21	1.23	1.19
Property Tax Rate, FY1981 (%) ^a	2.93	3.57	3.41	2.50
"Price" of Public Spending Financed by Property Tax, FY1989 (\$) ^b	2.2	2.4	2.5	2.1
"New Growth" FY1983-90 (%) ^c	18.7	19.7	19.4	18.1
Excess Capacity FY1990 (%) ^c	1.0	1.4	1.0	.8
Increase in Levy, FY1981-90 (%)	63.6	38.6	37.0	82.2
Cost-Adjusted Expenditures, FY1989 (\$) ^d	1,322	1,217	1,173	1,407
Average Population 1988	16,780	31,291	22,460	8,027

^aEqualized tax rate, calculated as property tax levy divided by the state's estimate of market value of taxable property in the community.

^bPrice of public spending indicates how much it costs an average single-family homeowner when the community raises property taxes by \$1 per capita; data for 45 communities are missing.

^cNew Growth and Excess Capacity are expressed as a percentage of the FY1990 levy limit.

^dEstimate of cost-adjusted per capita local public service level; calculated as expenditures per capita divided by the local cost index in additional assistance aid formula; spending reflects all local aid, including regional school aid attributed to member communities, for consistency.

Source: Massachusetts Department of Revenue, Division of Local Services, Municipal Data Bank, machine-readable data files; Massachusetts Department of Revenue, Division of Local Services, "A Report on Proposition 2½ Referenda Questions," May 1989, "Update: Proposition 2½ Referenda Questions," May 1990, and "FY91 Referenda Question Summary," printout January 1991; and author's calculations.

finance a given increase in services, overrides are likely to be less popular.

As expected, communities that passed at least one override or exclusion before FY1991 had higher

incomes and property wealth as well as slightly lower property tax rates and "prices" of public spending than those not attempting overrides. Those passing overrides or exclusions also had lower excess capacity than communities with no attempts; having voted increases in their levy limits, they were more likely to be taxing close to them (or conversely, communities with plenty of excess capacity had no need to vote increases in levy limits).

Communities that had passed one or more overrides or exclusions also had less new growth than those not attempting. One explanation for an effect of new growth independent of excess capacity is that the voters might believe that new growth augments the tax base and the levy limit enough to finance the services required by the new (or redeveloped) property and then some, reducing the pressure on the levy limit created by existing as well as new-growth-related service needs. In addition, perhaps the new voters associated with residential new growth were more financially stretched than long-time residents and were therefore more likely to vote against proposals to raise property taxes.⁷ Note, however, that voted increases in the levy limit (and the fact that these communities faced less serious revenue reductions in the first years of Prop 2½) more than made up for the new growth shortfall: after all was said and done, the actual levies of towns passing overrides or exclusions rose considerably more than those of communities with no attempts.

A key difference is that communities passing overrides and exclusions had much lower tax rates in FY1981 (before Proposition 2½ took effect), on average, than cities and towns with no attempts. Even after raising their levies much faster, the communities that approved overrides and exclusions still had slightly lower tax rates in FY1990 than those with no attempts because their property values per capita were so much higher. Their residents had also succeeded in obtaining more local services than communities not attempting to raise their levy limits (assuming that service levels are captured in the measure of cost-adjusted per capita spending).

Communities passing overrides and exclusions were considerably smaller than communities with no attempts. Various explanations are possible, centering on better communication and less mistrust between voters and officials in smaller places. Form of government may contribute to the directness of communication, both in terms of voters' beliefs about what officials are "up to" and officials' ability to predict voter wishes and to "educate" voters in

advance of a vote: The smallest towns generally have open town meetings as their legislative bodies, the bigger towns rely on (often large) representative town meetings, and city residents elect members of a (relatively small) city council. Furthermore, residents of cities may have more difficulty than do residents of more homogeneous small towns perceiving the benefits of the local spending that would be financed through an override. Each subgroup of a city's diverse population may be inclined to vote no because they believe that "too much" of the increase in public spending will benefit competing subgroups.⁸ Similarly, local officials may want to avoid the risk of appearing to favor one subgroup over another in proposing an override.

Communities that tried but failed to pass overrides and exclusions generally look more like the "no attempts" group than the "some passes" group, with the exception of excess capacity. This difference reflects the fact that communities with no "need" for an override (because they had plenty of leeway to raise taxes without getting voter approval) were not likely to have reason to attempt it. The broader similarities can be taken as evidence that getting something on the ballot is not a major obstacle to residents "getting

Residents of cities may have more difficulty than do residents of more homogeneous small towns perceiving the benefits of the local spending that would be financed through an override.

what they want"; proponents of the process would say that officials in the "no attempts" communities rationally put no overrides on the ballot, just as the voters in the "some attempts, no passes" group rationally turned down "unnecessary" or "unwanted" override proposals. As a result, communities without overrides, whether proposed or not, had similar outcomes, including the rate of increase in the levy over the FY1981-90 period, FY1990 effective property tax rates, and cost-adjusted expenditures.

Data such as those reported in Table 2, however, are not the best way to sort out relationships among community characteristics and override activity. Sim-

ple averages obscure variation within groups and cannot control for differences between groups in other variables that may also influence the outcomes.⁹ Multiple regression analysis of override attempts provides a better chance of sorting out these relationships. Multiple regression is a statistical technique that quantifies the relationships between each of a set of explanatory variables and another key variable of interest (such as whether or not a community puts an FY1991 override to a vote), controlling for the key variable's relationships with the other explanatory variables.

Override Attempts for FY1991

Unlike the comparisons reported in Table 2, the multiple regression analysis examines override votes alone, not proposals to raise the levy limit for debt service or a one-time capital expenditure. While funds in the local treasury are somewhat fungible, expenditures for capital (whether one-year outlays or debt-financed) are much more easily postponed than operating expenditures. Thus, if an exclusion vote fails, the capital expenditure is likely to be canceled or put off; funds within the Proposition 2½ limit would not be used for those purposes. Furthermore, the increase in the levy limit engendered by an exclusion is temporary, in contrast with the permanent nature of an override.¹⁰

This multiple regression analysis of the probability of a community holding a vote on an FY1991 override is consistent with the general patterns shown in Table 2 regarding override and exclusion attempts in earlier years. The regression was estimated to "explain" which among all the Commonwealth's cities and towns had one or more FY1991 override votes on the ballot. (See Table 3 for regression estimates.) Of the 351 cities and towns, 181 voted on override proposals to raise their FY1991 levy limits. Communities with lower per capita incomes, higher FY1990 property tax rates, more new growth, and more excess capacity were less likely to hold votes to raise the FY1991 levy limit than those with higher incomes, lower taxes, and so on.

Some of these effects were quite large: While a community with \$11,000 per capita income had only a 44 percent probability of making an override attempt, other things equal, a community with \$19,000 per capita income had a 58 percent probability.¹¹ Thus, communities with higher effective demand for local public services and less ability to meet that demand without an increase in the levy limit were

Table 3
Probability of Override Attempts and Passes in FY1991

Regression Results for All Communities
Coefficient Estimates (and Standard Errors)

Explanatory Variables	Dependent Variable		
	Override Attempted	Override Passed	Override Passed (Conditional) ^a
Constant	.60** (.17)	.33** (.14)	.90** (.25)
Per Capita Income (\$000) in 1988	.018** (.0066)	.031** (.0055)	.030** (.0077)
Residential Property Tax Rate, FY1990 (%)	-.20* (.11)	-.25** (.092)	-.37** (.16)
"Price" of Public Spending via Property Tax, FY1989	.030 (.041)	-.041* (.034)	-.11* (.061)
New Growth, FY1983-90, as % of FY1990 Levy Limit	-.0068** (.0033)	-.0069** (.0028)	-.010** (.0052)
Excess Capacity, FY1990, as % of FY1990 Levy Limit	-.040** (.011)	-.021** (.0092)	-.063** (.028)
Number of Override Attempts Pre-FY1991	.020** (.0090)	.026** (.0075)	.011 (.011)
Population Greater than 5,000 in 1980	-.11* (.067)	-.078 (.055)	-.075 (.086)
City Government	-.26** (.087)	-.15* (.073)	-.42* (.19)
R-squared	.19	.28	.28
Adjusted R-squared	.17	.26	.24
Number of Observations	306	306	155

Note: 45 of the state's 351 cities and towns are excluded from the first two regressions because of missing data; 26 are missing from the third regression. See Appendix Table A.5 for definition of variables.

^a These results are "conditional" on an override attempt; that is, the equation is estimated including only those cities and towns with at least one FY1991 attempt on the ballot.

* Estimated coefficient is significantly different from zero with 90 percent confidence.

** Estimated coefficient is significantly different from zero with 95 percent confidence.

Source: Author's estimates based on data provided by Massachusetts Department of Revenue's Division of Local Services.

more likely to attempt an override or overrides.

The estimates also indicate that the greater the number of previous override proposals on which a community had voted, the higher the probability that one or more proposals to raise the FY1991 levy limit

would be placed before the voters. Thus residents of an otherwise average community with no previous tries had a 47 percent probability of voting on an override in FY1991, while even five previous tries raised that probability to 57 percent. And cities were much less likely (by 26 percentage points) to attempt overrides than towns, even controlling for the economic differences between cities and towns captured by the other included variables.

Communities more likely to have an override on the ballot were also more likely to pass an override. The estimated results of a second regression examining which communities passed at least one override affecting the FY1991 levy limit look similar to those for override attempts, but some important differences emerge. (These estimates are also reported in Table 3.) The similarity presumably derives from two sources: First, to the degree that officials are successful in putting on the ballot only proposals with a reasonable chance of passing, they will make the two equations similar. That is, local officials attempt to judge "demand" and "need" for an override that the actual vote later reveals. Second, at a more mundane level, no override can pass if none are on the ballot, and the sample of observations for the "pass" equation, like the "attempt" equation, includes all communities, even those not attempting an override. (A third regression is also reported in Table 3, which examines which communities, *among those attempting*, actually passed an override.) Ninety-five communities passed at least one override for FY1991, out of the 181 communities with one or more override proposals on one or more ballots.

The interesting comparison between the "attempt" and "pass" equations is the size of the effects of various variables on the relative probabilities of attempting versus passing an override. (And the analysis of passage conditional on an attempt has similar implications.) At the margin, the indicators of effective voter demand for local public services—income, property tax rates, and especially prices—have bigger estimated effects on the probability of a community passing an override than on the probability of putting an override to a vote in the first place. At the same time, the indicators of the need for an override—new growth and excess capacity—raise the probability of having a vote the same or more than the probability of winning the vote.

The number of override attempts in prior years is about equally associated with passing an override and with attempting one, suggesting that both residents and officials learn by doing, or that officials,

having surmounted the obstacle once, find it easier to try again, while voters become convinced of the lack of other alternatives (such as greater efficiency) when their local officials make repeated proposals. It is worth emphasizing that earlier override attempts increase the probability of attempts and passes for FY1991, controlling for measures of voter demand and "need" for an override. Thus the process itself

Local officials have a better reading of their community's need for an increase in the levy limit than of their residents' desire for such an increase. Residents' views are made clearer as they vote.

(of voter education, perhaps, or officials confronting their fear of rejection) adds another dimension to the probability of success.

Finally, controlling for all these other influences, cities are much less likely to propose (by 26 percentage points) or pass (13 percentage points) overrides than communities with town government. Other things equal, one might interpret this fact as evidence that city officials' reluctance to propose increases in the levy limit is a more important factor than voters' disfavor.

While some of these differences between the two equations' estimated coefficients are small, they are consistent with the idea that local officials, in deciding whether to offer override proposals to the voters, have a better reading of their community's need for an increase in the levy limit (the community's lack of other options to raise revenue) than of their residents' desire for such an increase. Then residents' views are made clearer as they vote. These results leave open the possibility, therefore, that officials who decide not to propose any overrides similarly misread potential voter sentiment and fail to offer some overrides that their voters might pass. This possibility is reinforced by the lower explanatory power of the "attempts" equation.

IV. Taking a Vote: Who (What) Wins?

Once an individual override proposal is scheduled to appear on a local ballot, its passage or failure should depend on whether the community's residents favor the increase in services that will be financed with the proposed increase in the property tax levy, or prefer the fallback outcome (continuing to operate within the existing Proposition 2½ levy limit). Thus, many of the determinants are community characteristics like those important in getting an override proposal on the ballot.

Of course, the nature of any specific proposal would also be expected to affect its attractiveness: the size of the tax increase implied by the override, the purpose for which the funds are intended, and voting conditions such as whether other override or exclusion proposals share the ballot. A regression equation was estimated to "explain" the percent of voters in favor of 528 of the 598 override proposals that appeared on ballots for FY1991. (The other 70 proposals had some missing data. See Table 4 for coefficient estimates.)

The percentage of voters in favor of the 528 override proposals studied ranged widely, from 9 percent to 90 percent, but the middle one-half of the cases had vote outcomes in the smaller range of 34 to 51 percent in favor. The average vote came in with only 42 percent approving, and only 144 (one-quarter) of the override proposals passed. The wide range of outcomes and low success rate indicate clearly that community agenda setters do not propose only "sure things" and similarly that they do not propose the maximum override that will be approved by a bare majority—or, if they are attempting to do either of these things, the range of outcomes indicates that they are not very successful at predicting their voters' behavior!

"Purpose" of Override

If the voters place special priority on certain types of local spending, overrides designated for those purposes would have a higher rate of passage, other things equal. Among the FY1991 override proposals, those designated for school-related purposes (such as paying a town's share of regional school district expenses or removing asbestos from a school), public safety (police and fire), and public works (trash collection, roads, sewers, water) received about 4 percentage points more favorable votes than multi-purpose proposals or overrides des-

Table 4
Fraction of Voters in Favor of Individual Override Proposals in FY1991
Regression Estimates

Explanatory Variables	Estimated Coefficient (Standard Error)
Constant	.58** (.046)
Override Purpose is Schools, Public Safety or Public Works	.038** (.0099)
Override Purpose is Health and Welfare	.051** (.022)
Proposed Override Amount Relative to Levy Limit	-.19** (.082)
Other Override or Exclusion Proposals Share Ballot	-.059** (.015)
Second or Later Override Attempt for FY1991	.012 (.014)
Number of Years Since Community's First Override or Exclusion Attempt	-.011** (.0028)
Community Made No Override or Exclusion Attempts Before FY1991	-.039* (.021)
Per Capita Income (\$000) in 1988	.010** (.0013)
Residential Property Tax Rate, FY1990 (%)	-.095** (.026)
"Price" of Public Spending via Property Tax, FY1989	-.0012 (.0090)
New Growth, FY1983-90, as Percent of FY1990 Levy Limit	-.0021** (.00076)
Excess Capacity as a Percent of FY1990 Levy Limit	-.035** (.011)
Percentage Increase in Equalized Property Tax Rate, FY1983-90	.0011** (.00030)
Community Population Greater Than 5,000 in 1980	-.067** (.012)
City Government	-.095** (.028)
R-squared	.30
Adjusted R-squared	.28
Number of Observations	528

Note: 70 votes are excluded from the regression because of missing data. See Appendix Table A.5 for definitions of variables.

*Estimated coefficient is significantly different from zero with 90 percent confidence.

** Estimated coefficient is significantly different from zero with 95 percent confidence.

Source: Author's estimates based on data provided by Massachusetts Department of Revenue's Division of Local Services.

igned for general government, controlling for other differences.¹² Health and welfare proposals gained 5 percentage points over general government.

These "purpose" results should be interpreted with caution. Voters probably do place a high value on schools, public safety, and public works, and over half of the override questions were designated for these "basic" areas of local government spending. But from one point of view, the designation of an override's purpose is arbitrary in the sense that money is fungible once in the public coffers, and the voters do not directly approve the purposes to which funds raised within the Proposition 2½ limit are put. While community officials have generally used the "purpose" designation in a nonarbitrary way to indicate what projects will not be funded if the proposal fails, stories are often told about "purpose" designations chosen solely to maximize chances of the override passing, with the money freed up within the remainder of the budget being used to accomplish a different purpose.¹³ Furthermore, designated spending proposals in some communities may elicit efforts from specific lobbying groups that affect turnout and the composition of those who turn out, tilting the vote in favor of passage.

Override Size and Ballot Characteristics

Override proposals that will raise the levy limit by a large percentage are less likely to pass than small ones. Furthermore, the presence of other override proposals on the ballot reduces the favorable vote on any one override by 6 percentage points. It is worth noting, however, that when several overrides share a ballot, the probability of something passing is higher than when only one override is offered (as noted in the page 9 box on the "menu" approach). Previous attempts to raise the levy limit had a nonlinear relationship with the favorable vote in FY1991. The longer ago the first override or exclusion attempt was made, the less likely were voters to approve an individual override in FY1991, but having zero previous attempts also had a negative effect on the outcome.

Demand and "Need" Variables

As in the earlier analysis of override proposals across all communities, residents of higher-income communities were considerably more likely to vote in favor of individual FY1991 overrides. And high property tax rates, which reduce post-tax income, also reduce taxpayers' willingness to raise property taxes. But the percentage change in a community's property tax rate from FY1983 to FY1990 is positively associ-

ated with the yes vote: where tax rates fell the least, residents were most likely to approve an FY1991 override. (This could reflect reverse causation—communities most able or willing to pass overrides in previous years may have had the smallest declines in effective property tax rates and may have still been more able and willing to pass overrides for FY1991.) Furthermore, just as in the earlier community analysis, both new growth and excess capacity apparently reduce the need for overrides.

Unexplained Differences in Vote Outcome

Finally, even after controlling for all these economic determinants and ballot characteristics, override proposals were less likely to pass in large cities and towns (about half the communities had populations over 5,000 in 1980), and especially cities, than in smaller places with town government. Indeed, a large city with average resident characteristics and override "needs" would definitively vote down (31 percent in favor) a "typical" override, while an otherwise similar small town would almost pass it (47 percent). Given that the regression technique controls for a variety of other differences between large cities and small towns, this finding reinforces the notion that city voters have more difficulty with overrides. In addition to the consensus and trust explanations discussed earlier, this result may occur because the income and tax rate variables do not fully

Certain types of communities are much less likely to pass overrides than others, and they are precisely the places with which the state must concern itself—low-income, larger towns and cities with higher tax rates.

capture the dearth of resources that constrains the choices of bigger towns, and especially cities.

In sum, the determinants of both demand for local public services and the likely "need" for an override in a community had the expected effects on

the probability of passage for specific override proposals affecting the FY1991 levy limit. On the one hand, this suggests that the override process works well—voters' likely preferences are indeed reflected in the outcome. On the other hand, however, the findings confirm a fundamental problem with the override process.

Certain types of communities are much less likely to pass overrides than others, and they are precisely the places with which the state must concern itself—low-income, larger towns and cities with higher tax rates. While their voters can hardly be expected to want overrides (which would raise tax rates even higher), some of these communities are in need of additional revenues from some source. During the 1980s, the Commonwealth increased its aid to local governments, targeting some of the increment on these "needier" communities.¹⁴ But those aid funds are now scheduled for steady decline, and certainly cannot be counted on to offset the particular strictures the override process places on these communities' ability to raise revenue.

V. Conclusions

A majority of voters in many communities do appear to "get what they want" from the override process, given the constraints of Proposition 2½. Both the pattern of override attempts in FY1991 and the probability of passing individual override proposals reflect in a reasonably robust fashion the resident and community characteristics that relate to the demand for local public services and the need for an override (as distinct from other means) to meet that demand. But these relationships explain only a modest fraction of the intercommunity variation and leave open such questions as whether overrides that might pass are not proposed to the voters. And the analyses indicate that cities and/or larger towns systematically have fewer overrides proposed and a lower probability of passage (for those that do make it onto the ballot) than would otherwise be expected. Their voters are therefore less likely to be satisfied with outcomes than voters with similar characteristics in smaller places (unless some other unobserved difference systematically reduces voter desire for overrides in larger places).

What the Commonwealth's residents appear to have wanted (and obtained) in the 1980s was more modest growth in property taxes than in the prior decade. This was possible without significant service

disruptions and quality deterioration, in large part because the state increased its aid to localities. Some communities, however, raised their property tax levies quite substantially. Communities not proposing and not passing overrides generally had lower incomes and property values and higher tax rates than those passing overrides. Indeed, smaller towns ended the decade with higher service levels but not higher tax rates than bigger places. These patterns suggest that Proposition 2½ and its override process (even though combined with substantial growth in "equalizing" state aid to cities and towns in the 1980s) have not helped to undo the basic difficulty with property tax financing of local services—that disparities in tax bases translate fairly directly into disparities in tax rates and service levels. Just as in the absence of Proposition 2½ and its override process, the public sector outcomes in poorer communities are constrained by lack of local resources.

Over the next few years local aid is scheduled to decline, reducing the cushion that softened the impact of real declines in property taxes during the 1980s. As was the case in FY1990 and FY1991, these cuts will undoubtedly lead to increased pressure for overrides, and a greater number being proposed and passed, particularly as new growth has also slackened. For communities with the resources and political will to make up the losses by passing overrides, local service levels may be maintained, but more of the bill will be paid by local residents through the property tax. Even these communities, however, may find themselves with lower vote margins when the needed overrides involve much larger dollar amounts than in previous years.

But other communities, specifically those least able to raise sizable amounts of revenue through the property tax, will find it much more difficult to make up for the aid losses (and declines in new growth) through overrides. Their voters may feel unable to afford the tax increases required to maintain service levels. Also, given the importance of earlier tries in attempting and passing FY1991 overrides, other communities that have not needed to resort to overrides in the past may be at a disadvantage when they find themselves at their levy limits for the first time and in need of more revenues. Recognizing some of these potential difficulties, several proposals have been made to loosen Proposition 2½ (see the box) in order to minimize possible service disruptions and give local governments more flexibility to respond to the planned reductions in local aid.

Whether through more overrides or a looser

Proposals to Loosen Proposition 2½: Lessons from History

Several studies (notably the "Report of the Governor's Task Force on Local Finance" chaired by John Hamill) have suggested that the "automatic" annual increase in the levy limit reflect the inflation rate rather than being a constant 2.5 percent per year. If the levy limit automatically rose by the inflation rate, real service levels could be maintained without overrides (if other revenue sources were also growing at about the rate of inflation). But voters would retain a tight grip, through the override process, on any attempts to increase services or to respond to significant cuts in aid with a local tax increase. The calls to loosen Prop 2½ have gained urgency as the magnitude of aid cuts looms large. Even residents in favor of keeping local revenues growing slowly recognize the need for more short-term local flexibility to respond to large local aid losses.

The average annual inflation rate for state and local governments nationwide (the GNP price deflator for state-local government purchases) for the 1987-89 period was 4.5 percent. Thus, any communities passing overrides that represented an increase in the levy limit in excess of 2.0 percent (equals 4.5 minus 2.5) would still have needed to vote an override to obtain the same revenues. But communities passing smaller overrides could have increased their levies by that much without voting an increase in the levy limit if the levy limit had automatically risen by the inflation rate.

Of the 75 communities that passed overrides in FY1990, all but two enacted increases in the levy limit of more than 2.0 percent (to be added to the automatic 2.5 percent increase). Indeed, 48 voted levy limit increases of more than 5.0 percent. For FY1991, only 11 of the 95 communities passing overrides raised their levy limits less than 2.0 percent; and 65 raised them more than 5.0 percent. Thus it would appear that the major effect of loosening Prop 2½ in this way, if any, would be felt by communities not currently passing overrides.

The 296 communities that were taxing at 99 percent or more of their levy limits in FY1990 would be likely to increase their property tax revenues faster than 2.5 percent if Proposition 2½ were loosened; one-third had not passed an override or exclusion through FY1991. Whether all communities currently near their limits would raise taxes by the maximum amount (whatever local officials could "get away with" under the looser limit), is not the foregone conclusion that many taxpayers presumably fear. Until new growth and local aid began shrinking in the late 1980s, most communities were not so close to their levy limits (see Table A.1). Thus local officials might again exercise restraint beyond what Proposition 2½ requires, if the size of the aid cuts and slowdown in new growth did not overwhelm the "looser" limit.

Of course, if the override process worked perfectly, such a loosening would not be necessary, since overrides could handle the necessary adjustments to declining aid dollars. But the analysis in this paper suggests that the process may not serve all communities well, especially larger towns and cities. It is also worth noting that reductions in aid proposed for FY1992 amount to at least \$110 million and may go higher. To cover these aid losses with property tax revenues would require statewide property tax increases of more than 2 percent, with bigger increases in the communities most dependent on aid and facing the biggest cuts.

Because of the way the Administration has proposed making the cuts (as of April 1991), some communities' aid will rise, while others, notably the largest cities and towns, face sizable reductions. Replacing aid with property tax revenues would require a property tax increase of more than 3.5 percent, on average, in the largest communities (population over 50,000), or more than 6.5 percent if a projected \$75 million increase in "lottery" aid does not materialize.

Proposition 2½, the remaining years of the 1990s are likely to bring increases in property taxes in Massachusetts and a widening of the property tax gap between Massachusetts and other states that narrowed so noticeably in the 1980s. Even as average

property taxes rise, interlocal disparities in spending are likely to increase, unless the local aid distribution is changed to concentrate the shrinking resources on those places least able to raise revenues locally through the property tax.

Table A.1
Statewide Property Tax Trends

Fiscal Year	Property Tax Revenue	Levy Limit	Excess Capacity (Limit minus Levy)	Number of Communities Close to Levy Limit ^a	Ceiling (2.5% of Value)	Override Capacity ^b	Local Aid ^c	Total Local Revenues
Millions of Current Dollars, except Number of Communities								
1983	2,959	n.a.	n.a.	n.a.	n.a.	n.a.	1,726	5,807
1984	2,995	n.a.	n.a.	n.a.	n.a.	n.a.	1,860	6,012
1985	3,126	3,198	72	135	3,787	598	2,072	6,456
1986	3,309	3,394	84	143	4,621	1,242	2,245	6,950
1987	3,536	3,638	102	123	5,369	2,325	2,625	7,658
1988	3,805	3,902	97	167	7,050	3,193	2,836	8,269
1989	4,122	4,205	83	202	8,965	4,826	2,967	8,930
1990	4,465	4,502	38	296	10,592	6,184	2,745	9,339
Percentage Change from Previous Year								
1984	1.2	n.a.	n.a.	n.a.	n.a.	n.a.	7.8	3.5
1985	4.4	n.a.	n.a.	n.a.	n.a.	n.a.	11.4	7.4
1986	5.9	6.1	17.7	5.9	22.0	107.9	8.3	7.6
1987	6.9	7.2	20.8	-14.0	28.5	87.2	16.9	10.2
1988	7.6	7.2	-5.2	35.8	18.8	37.3	8.1	8.0
1989	8.3	7.8	-14.2	21.0	27.2	51.2	4.6	8.0
1990	8.3	7.1	-54.5	46.5	18.1	28.1	-7.5	4.6
1991 (est.) ^d	7.5	7.1	-32.4	1.7	-4.8	-12.7	-5.3	3.3

n.a. = not available.

^aTax levy equal to 99 percent or more of levy limit.

^bOverride capacity is defined as the ceiling minus the levy limit that would apply in the absence of capital and debt exclusions.

^cLocal aid includes direct aid from the state to cities, towns, and regional school districts.

^dEstimated changes for FY1991 based on data available for 272 communities.

Source: Massachusetts Department of Revenue, Division of Local Services, Municipal Data Bank, machine readable data files.

Table A.2
Override and Exclusion Attempts by Massachusetts Cities and Towns

Fiscal Year ^a	Number of Communities Attempting				Number of Communities Passing				Percentage of Communities Attempting That Passed at Least One ^b		Number of Communities Never Having Attempted		
	Overrides		Exclusions		Overrides		Exclusions		Override	Exclusion	Override	Exclusion	Either
	First Attempt	Total Attempts	First Attempt	Total Attempts	First Win	Total Wins	First Win	Total Wins					
1983	50	50	34	34	21	21	17	17	42	50	301	317	275
1984	30	47	31	38	9	16	27	30	34	79	271	286	229
1985	6	24	32	52	3	14	27	44	58	85	265	254	208
1986	5	23	18	48	4	13	16	40	57	83	260	236	197
1987	24	58	30	79	14	34	25	63	59	80	236	206	168
1988	14	63	25	99	14	41	28	86	65	87	222	181	143
1989	20	74	18	80	28	65	20	75	88	94	202	163	124
1990 ^c	31	87	13	71	40	80	14	66	92	93	171	150	106
1991	67	181	35	132	34	95	18	74	52	56	104	115	58

^a This table assumes that exclusion votes taken in a given calendar year (1985, for example) first affect the levy limit in the following fiscal year (1986).

^b Communities often vote on several override or exclusion proposals. The passage rates of individual proposals are considerably lower than those reported here, which indicate the fraction of communities voting on one or more proposals that passed at least one.

^c Unsuccessful override and exclusion attempts may have been underreported in FY1990.

Source: Massachusetts Department of Revenue, Division of Local Services, "A Report on Proposition 2½ Referenda Questions," May 1989, "Update: Proposition 2½ Referenda Questions FY90," May 1990, and "FY91 Referenda Question Summary," printout January 1991; and author's calculations.

Table A.3

Average Community Characteristics by Community Size

	Number of Communities	Increase in Levy FY1981–FY1990 (percent)	Percent of FY1990 Levy Limit Attributable to		Property Tax Rate, FY1990(%) ^a	Cost-Adjusted Per Capita Spending, FY1989(\$) ^b	Property Value Per Capita, FY1990(\$) ^c
			Overrides and Exclusions	New Growth			
All Cities and Towns	351	63.6	7.9	18.7	1.20	1,322	98,536
Population Size in 1980:							
Under 2,000	72	86.4	16.2	16.7	1.26	1,472	156,653
2,000–4,999	53	80.2	11.3	19.7	1.18	1,358	100,226
5,000–9,999	75	72.8	8.5	20.4	1.16	1,288	87,903
10,000–19,999	72	49.8	3.8	18.8	1.19	1,240	76,975
20,000–49,999	58	44.0	1.7	18.7	1.19	1,287	79,595
50,000 and over	21	12.9	0	16.9	1.30	1,206	59,217

^aEqualized tax rate.^bPer capita spending in FY1989 divided by cost index used in additional assistance aid formula; spending reflects all local aid, including regional school aid attributed to member communities, for consistency.^cEqualized property value in FY1990 divided by 1988 population.

Source: See Appendix Tables A.1 and A.2.

Table A.4

Override Patterns by Community Size

	Number of Communities	Excess Capacity as % of Levy Limit, FY1990	Percent of Communities Not Having Attempted Through FY1990		Percent of Communities Attempting in FY1991		Percent of Communities Attempting that Passed One or More in FY1991	
			Overrides	Exclusions	Overrides	Exclusions	Overrides	Exclusions
All Cities and Towns	351	1.0	48	43	52	38	52	56
Population Size in 1980:								
Under 2,000	72	2.4	21	42	63	49	67	71
2,000–4,999	53	.7	23	26	62	45	48	58
5,000–9,999	75	.4	40	30	55	47	51	51
10,000–19,999	72	.4	68	42	50	32	50	52
20,000–49,999	58	.9	78	60	36	24	38	29
50,000 and over	21	1.3	90	90	24	5	40	100

Source: See Appendix Tables A.1 and A.2.

Table A.5
Variable Definitions and Means

Variable	Definition	Average Value in	
		Attempt/ Pass Analysis (N = 306)	Favorable Vote Analysis (N = 528)
Favorable Vote Fraction	Ratio of yes votes to total votes for each override proposal	n.a.	.423
Override Attempted	Dummy = 1 if community attempted override for FY1991	.507	n.a.
Override Passed	Dummy = 1 if community passed override for FY1991	.26	n.a.
Per Capita Income, 1988	In thousands of dollars	14.8	14.8
Residential Property Tax Rate, FY1990	Tax rate on residential property, %	1.01	.908
"Price" of Public Spending via Property Tax, FY1989	Dollar cost to average single family homeowner of raising community property taxes \$1 per capita. Equals average single family tax bill divided by tax levy, multiplied by population	2.21	2.03
New Growth, FY1983-90, as % of FY1990 Levy Limit	Certified new growth FY1983-90 compounded at 2.5% annually to FY1990 divided by FY1990 levy limit, expressed as %	18.9	17.4
Excess Capacity as % of FY1990 Levy Limit	One hundred minus property tax levy as % of levy limit	.863	.179
Number of Override Attempts pre-FY1991	Number of override proposals put before community voters, 1983-90	1.9	3.50
Population Greater than 5,000 in 1980	Dummy = 1 if community population exceeded 5,000 in 1980	.654	.619
City Government	Dummy = 1 if community has city form of government	.118	.038
Override Purpose Is Schools, Public Safety, or Public Works	Dummy = 1 if declared purpose is one of these "basic" local services	n.a.	.525
Override Purpose Is Health or Welfare	Dummy = 1 if declared purpose relates to health and welfare	n.a.	.055
Proposed Override Amount Relative to Levy Limit	Dollar amount of override divided by FY1990 levy limit	n.a.	.0467
Other Override or Exclusion Proposals Share Ballot	Dummy = 1 if override ballot contains additional override or exclusion proposals	n.a.	.848
Second or Later Override Attempt for FY1991	Dummy = 1 if this override is not on first FY1991 ballot	n.a.	.176
Number of Years since Community's First Override or Exclusion Attempt	1991 minus fiscal year of first attempt to raise levy limit	n.a.	4.89
Community Made No Override or Exclusion Attempts before FY1991	Dummy = 1 if no attempts made to raise levy limit before FY1991	n.a.	.309
Increase in Equalized Property Tax Rate, FY1983-90	Expressed as %	n.a.	-34.2

n.a. = not available.

¹ Census of Governments data; 1980s refers to fiscal years 1980 through 1989.

² These data, which allow comparison with other states, are published by the U.S. Bureau of Census in the Census of Governments and are not available after FY1989. The Massachusetts Department of Revenue's Division of Local Services provides the more current and disaggregated information used in the analysis that follows.

³ Researchers Romer and Rosenthal (1978, 1982) have found that a budget-maximizing agenda-setter can obtain majority voter approval for higher spending than voters actually prefer when the fallback option (what occurs if the referendum does not pass) is very unattractive. The agenda-setter chooses the highest spending package that can gain approval compared with the fallback, and any vote outcome that is not a bare majority passage indicates that the agenda-setter has made a mistake. But given the repeated and incremental nature of many communities' override attempts and passes from year to year, this budget-maximizing strategic behavior is not likely to be key in Massachusetts cities and towns.

⁴ Town Meetings are the legislative arm of town government in Massachusetts, while Selectmen are the administrative arm. Town Meetings in some communities are open to all residents and hence quite directly reflect the views of a majority of those who attend. Residents of other (usually larger) towns elect representatives to Town Meeting. Of the 351 communities in Massachusetts, 39 are cities, 46 have representative town meetings, 263 have open town meetings and the remainder are towns with other legislative arms, such as a Town Council.

⁵ Ladd and Wilson (1981) found that 65 percent of respondents to their survey regarding possible effects of Proposition 2½ expected it to "make local governments more efficient," and 85 percent of those voting in favor of Proposition 2½ had those expectations.

⁶ One observer of the national political scene argues that "government by initiative" in California and elsewhere reflects exactly this distrust between voters and local officials—voters may not be against public spending, but rather are opposed to letting their elected representatives decide how public funds should be spent (Schneider 1991).

⁷ This explanation was suggested by Peter Fortune. It is certainly the case that the owners of *new* residential property in most Massachusetts communities during and soon after the 1984–87 real estate boom faced much higher housing costs than long-standing residents. Communities with considerable new

growth, if it were residential, would have a higher fraction of such potential voters, more inclined to vote against any additional increase in costs, such as property taxes.

⁸ Andrew Reschovsky suggested this explanation based on the diversity of population in bigger communities.

⁹ For example, the group of communities that have never voted on an override or exclusion includes subsets with high and low excess capacities, while excess capacity is less spread out within the groups of communities that attempted to pass overrides; such differences in range or dispersion make interpretation of group averages difficult. Furthermore, the FY1990 excess capacity figures shown in Table 2, for example, are actually the consequence of votes (or decisions not to vote) during the 1980s as well as the recent counterpart of measures that served as inputs into the officials' and voters' decisions during the decade.

¹⁰ In addition to these conceptual reasons for analyzing override and exclusion referenda separately, several practical considerations arose: (1) exclusion votes are recorded according to the calendar year in which they occur because the fiscal year in which they will have their first effect may not be known at the time of the vote; (2) the size of the impact of an exclusion proposal on the levy limit is not known at the time of the vote because it depends on the actual interest rate and amount borrowed (or spent in the case of capital expenditure exclusions).

¹¹ This calculation uses the range from about one standard deviation below the mean to about one standard deviation above; community income per capita ranged from about \$8,700 to \$41,500 in the 306 cities and towns included in the regression.

¹² These three categories of spending—education, public works and public safety—were combined in the regression because earlier versions indicated very similar coefficients for the three purpose variables when included separately.

¹³ The only requirement is that "the appropriation for the purpose of the override is at least the amount stated in the question"; such earmarking applies only in the first year. See Massachusetts Department of Revenue, Division of Local Services, "Proposition 2½ Referenda Questions: Requirements and Procedures."

¹⁴ See Bradbury and Browne (1990). Interestingly, a measure of state aid funds per capita, which might be expected to have effects similar to private income in raising the probability of passage, had virtually no effect on vote outcomes in an analysis not reported here.

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Is There a Shortfall in Public Capital Investment? An Overview

Anation can use its current output to provide for the future in numerous ways: it can undertake private capital investment, add to the stock of public capital, enhance income-producing assets abroad, invest in human capital through education and health programs, conserve natural resources and the environment, and invest in science and technology. During the 1980s none of these approaches were pursued vigorously and most of the country's increase in output went for consumption rather than the enhancement of future production; the adverse effects of debt-financed consumption on private investment, net foreign investment, and human capital have been well documented.

In the past few years, however, academic work, commission reports, and natural disasters have highlighted the fact that the nation has also been neglecting its stock of public capital. Stories abound of deteriorating roads, bridges, and sewer systems, which have often led to serious collapses or other disasters. Almost everyone has experienced the frustration and delay of congestion on overburdened roads and airports.

Political developments have also raised the importance of public capital investment on the national agenda. At the federal level, dissolution of Cold War tensions has spurred debate on the reallocation of spending from military to other uses, although this has been mitigated somewhat by recent developments in the Persian Gulf. The impending re-authorization of the federal highway bill also has sparked a great deal of interest. Fiscal problems at all levels of government have led policymakers and citizens to rethink spending priorities.

This conference aimed to determine the extent to which the United States may be underinvesting in public infrastructure, explain the potential economic consequences, and suggest mechanisms to help alleviate any adverse trends. The conference focused on public investment in physical capital only to make the topic manageable, and should

Alicia H. Munnell

Senior Vice President and Director of Research, Federal Reserve Bank of Boston. This article summarizes the Bank's economic conference held in June 1990. Information on how to obtain the conference proceedings appears on p. 37.

not be interpreted to mean that investment in human capital is in any way less important.

The conference consists of six sessions: The first three sessions discuss various topics related to the importance of infrastructure, while the last three tackle some practical policy issues in this area. The first session addresses the broad question of why infrastructure is important by discussing the impact of public capital on quality of life, the environment, and output. The second introduces a new data set on state-level public and private capital stocks to examine the impact of public capital on output, invest-

All conference participants agreed that public capital investment plays an important role in enhancing both the quality of life and private economic activity.

ment, and employment growth at the state level. The third session explores directly the question of whether public infrastructure is undersupplied.

In the second, policy-oriented set of papers, the first explores the extent to which the private sector can compensate for the lack of public investment. The next two papers focus on incentives. One addresses the issue of the efficiency of current infrastructure investment and pricing, specifically as related to highways and airports. The other analyzes the optimal financing of public infrastructure and investigates the incentives imbedded in existing federal programs for public capital investment.

All conference participants agreed that public capital investment plays an important role in enhancing both the quality of life and private economic activity. All concurred that public capital, like private capital, belongs in an economic production function, and that the decline in public capital investment may have played some role in the productivity downturn. A sharp disagreement arose over the estimated economic importance of public infrastructure. The great majority of participants rejected the estimates of the marginal productivity of public capital in the range of 50 percent to 60 percent that emerge from the time series analysis.

Despite the general acceptance of the economic and social importance of public capital investment,

two quite different perspectives on the need for more infrastructure investment emerge from the discussion. On one side are those who see a strong link between public capital investment and economic and social well-being; they view the current stock of public capital as inadequate and believe that additional investment is required. On the other side are those who are primarily concerned with the efficient use of existing infrastructure; they basically oppose increasing investment until the engineering, pricing, and financing of infrastructure are closer to the optimum.

Why Is Infrastructure Important?

David Aschauer sets the stage for subsequent discussion and much controversy by laying out the case for the importance of infrastructure to the quality of life, the environment, and private economic activity.

In the first part of his paper, Aschauer presents an informal discussion of the linkages between public capital investment and various aspects of well-being, such as the human habitat, economic opportunity, and leisure time. The major point of this section is that many observers question the ability of existing and projected infrastructure facilities to adequately support quality-of-life requirements; their apprehensions are most pronounced in the areas of the environment and transportation.

As evidence on the environmental front, Aschauer notes that, despite large-scale expenditure following the passage of the Clean Water Act in 1972, many streams and lakes in the United States remain incapable of supporting their designated commercial or recreational uses. The problem rests, in large part, with municipal wastewater treatment facilities, which account for about one-third of the use impairment of the waters. These treatment facilities also raise the toxicity levels of lakes and rivers. The Environmental Protection Agency (EPA) says that many municipalities have yet to construct sewage treatment facilities to meet permanent requirements.

A second area where inadequate infrastructure has an adverse impact on both health and aesthetics is the treatment of solid waste. Garbage is being generated at unprecedented rates, while the number of facilities to handle the waste is shrinking. Between 1978 and 1986, the number of operating landfills declined from 20,000 to 6,000. Forecasts predict that by 1993 more than 2,000 of the remaining landfills will be closed due to inadequate safety and environ-

mental practices or capacity constraints. These trends suggest increased health risks to residents and damage to the environment.

In the area of transportation, inadequate public transportation poses a serious barrier to employment for those without cars. Aschauer notes that disabled citizens cite a lack of transportation as the primary obstacle to obtaining jobs and being fully productive members of society. Moreover, in many cities job opportunities in the suburbs remain unfilled because of the lack of transportation from the urban core.

Increased congestion in the ground and air transportation networks both impairs people's leisure and raises business costs. The Federal Highway Administration forecasts a 436 percent increase in urban freeway congestion by the year 2005 if improvements to the interstate system are not forthcoming. Similarly, the Federal Aviation Administration forecasts a significant increase in the number of airports suffering serious delays during the next decade. In short, transportation is another area requiring additional investment, or else inadequate infrastructure likely will continue to detract from the quality of life.

In the second part of the paper, Aschauer shifts from quality-of-life issues to the impact of infrastructure on economic activity. He cites previous studies demonstrating the positive effect of public capital stock on output, both within this country and across countries. He further notes that public capital increases the rate of return to private capital, thus stimulating private investment; at the same time it substitutes for private investment, thus discouraging private initiatives.

Aschauer assembles these various forces into a simple model to simulate the effect of higher public investment on the aggregate economy. Specifically, he assumes that public investment during the period 1970 to 1988 remained near the average for 1953 to 1969, thereby eliminating most of the actual decline. The results suggest that the increased public investment would have raised the rate of return to private capital from 7.9 percent to 9.6 percent and the rate of productivity growth from 1.4 percent to 2.1 percent for the 1970–88 period. The impact on private investment is more complicated; initially higher public investment crowds out private investment, but eventually the higher rate of return dominates and simulated private investment exceeds actual levels. Aschauer emphasizes the tentative nature of these results and goes on to address criticisms that have been raised about his empirical work: that public investment is endogenous, that the estimated coeffi-

cient on public capital is too large to be reasonable, and that the model is too simple.

Aschauer then attempts to provide new evidence showing how public sector capital affects private sector productivity. This time he explores the relationship between private productivity and public capital investment across states, by including government capital as an intermediate input in a generalized Cobb-Douglas production function. To work around the lack of state capital stocks, Aschauer rewrites the production function so that the estimate of the relationship requires data on only the capital-output ratio, rather than the level of capital stocks. He then assumes, based on cross-country comparisons, that the capital-output ratio is constant over time. As a result, individual state capital-output ratios can be expressed as the ratio of investment to output times the rate of growth of output plus the depreciation rate, which Aschauer sets at 5 percent.

Aschauer estimates the production function using data averaged over the period from 1965 to 1983. His results show that state output per worker is positively and significantly related to public investment in core infrastructure, although the coefficient on the public investment variable (representing the marginal product) is extraordinarily high. More precisely, while the marginal product of private capital in his equations ranges between 9 and 12 percent, the

Aschauer's results suggest that increased public investment would raise the rate of return to private capital, the rate of productivity growth, and even the return to private investment.

marginal product of public capital exceeds 200 percent. Again, Aschauer addresses likely criticisms of this empirical exercise and attempts to demonstrate the robustness of his results by varying the assumed depreciation rate and using instrumental variables.

Aschauer concludes that given the importance of infrastructure, both for quality of life and economic competitiveness, and the dissolution of Cold War tensions, the time seems ripe for a reorientation of government spending priorities.

Henry Aaron, in commenting on Aschauer's work, notes that although Aschauer has made an important contribution to the productivity slowdown debate by including public capital as an explanatory factor, several serious questions surround his empirical work. Aaron cautions that if a result fits with our hopes and appears too good to be true, it probably is, and should be subjected to careful scrutiny.

Most fundamentally, Aaron rejects the estimates of the productivity of public capital in both Aschauer's earlier work and the paper presented at this conference. In the case of the earlier results, which show a productivity of public capital around 60 percent, Aaron attributes the implausible estimates to the pitfalls of time series analysis. Aggregate time series analysis based on variables expressed in levels is dominated by trend, and produces marvelous fits that do not really explain much of the relevant variance. Thus, unless the results are robust to estimation using other functional forms, the hypothesis should not be considered to have been proven. Another problem is that the production function model assumes competitive factor markets. Public capital, however, does not pass any market test in which productivity is balanced against a cost measure.

In terms of the current paper, Aaron attributes the startling results to an incredible list of assumptions required to estimate the model, and argues that more tests should have been run to assess the sensitivity of the results to other assumptions. He also raises another oft-cited criticism—reverse causation, whereby rapid output growth and high productivity lead to greater public investment, rather than public capital investment causing greater output per hour. While Aschauer attempts to treat this issue with instrumental variables, Aaron notes that he should have examined it through direct modeling and testing.

In a different vein, Aaron also questions much of the informal reasoning in Aschauer's argument about quality-of-life effects. He sees much of the advocacy for more infrastructure as a reflection of the vested interests of those agencies and organizations that gain from greater capital spending. Furthermore, while Aaron believes that government spending can improve the quality of life, this claim does nothing to support the thesis that infrastructure contributes to national output as conventionally measured.

Richard Musgrave also questions Aschauer's high estimated coefficient on public capital and wonders about reverse causality, but focuses his efforts on trying to identify the unique characteristics of infra-

structure and other issues. He concludes that infrastructure as an intermediate good is distinguished by its joint and cross-industry use, and then speculates whether these characteristics could lead to high productivity.

Musgrave also argues that much could be learned about the benefits of public capital through cost-benefit analysis. While this approach has its problems, it can, and should, be applied to estimate cost savings in production where public capital is an intermediate good. Musgrave also recommends that researchers attempt to quantify currently unrecorded pieces of GNP, such as quality of life indicators, and apply cost-benefit analysis to estimate the impact of infrastructure investment on these unrecorded aspects of national output.

Musgrave concludes with the thought that although it was appropriate to limit the conference to the subject of physical infrastructure, one must not forget that physical assets are only one part of the issue. Public investment in health and education is no less important and should be included in any more comprehensive analysis.

How Does Public Infrastructure Affect Regional Economic Performance?

Alicia Munnell's paper explores the impact of infrastructure investment on three measures of state-level economic performance. Since no comprehensive measures of public or private capital stocks are available at the state level, these data are constructed and used to estimate state production functions, to explore the relationship between public and private investment, and to analyze employment growth within a business location model.

The first step is to construct estimates of the public and private capital stocks by state. For public capital stocks, the perpetual inventory method is employed to generate an estimate of the net value of state and local government capital investments, which is then used to apportion Bureau of Economic Analysis (BEA) national stock estimates among the states. In the case of private capital, BEA stock estimates are distributed among states based on measures of each state's activity in various sectors of the economy. The observations show significant variation and appear to contain real information.

Munnell then introduces these stock estimates as inputs in a pooled cross-section production function based on data for 1970 to 1986. The results indicate

that public capital has a significant, positive impact on output at the state level. The regression coefficients also show rough equivalence between the marginal products of private and public capital; specifically, the coefficients imply a marginal productivity of 35 percent for both private and public capital. They also suggest slightly increasing returns to scale across the three inputs. When public capital was

Munnell concludes that more spending on public investment, which clearly would remedy serious safety hazards and improve the quality of life, may also induce greater productivity and growth.

disaggregated, water and sewer systems had the largest impact on output, followed by highways, with other public capital exhibiting a very small impact.

The next section examines the relationship between public and private investment in which two opposing forces are at work. On one hand, public capital enhances the productivity of private capital, raising the rate of return and encouraging more private investment. On the other hand, public capital serves as a substitute for private capital. An attempt is made to combine these opposing influences in a stock-adjustment model, where the desired stock of private capital is related to the level of output, the stock of labor, and the stock of public capital, and also to the marginal productivity of private capital. The results, while not robust, indicate that, on balance, public capital investment stimulates private investment. Munnell notes that these results should be interpreted only as an additional bit of evidence supporting public capital's economic importance and as an invitation to future researchers.

Finally, a business location model that includes a measure of public capital stock is used to analyze employment growth. This type of model assumes that firms strive to maximize profits and will choose a location based on their profitability at alternative sites. Any characteristics of the location that affect production costs or sales will influence this decision.

The specification used by Munnell analyzes the average annual percent change in private employment in the state as a function of variables reflecting the labor market, energy costs, cost of land, market size, tax burden, and public capital stock. Munnell notes that the results are generally in line with what one would expect, with public capital having a positive influence on employment growth, all else equal.

Taken together, the results of these three exercises indicate that public capital has a positive impact on private sector output, investment, and employment. Some areas need significantly more research and refinement, but these results are another piece in the emerging picture of public capital's economic importance. Munnell concludes that more spending on public investment, which clearly would remedy serious safety hazards and improve the quality of life, may also induce greater productivity and growth.

In his comments, Charles Hulten, while finding the coefficient on public capital in the production function quite plausible, and substantially more so than the results of aggregate time series estimates, notes several problems. First, since the nation's infrastructure networks are largely complete, the estimated coefficient on public capital may overstate the benefits from *additional* public investment. Second, without resource costs one cannot discern whether the allocation of public capital is efficient. Third, only a state's own public capital stock enters into the production function, which ignores the benefits that a state may derive from the public capital stocks in neighboring jurisdictions. Fourth, the equations include no adjustment for congestion. Finally, the production function is only one equation within a simultaneous system, and thus the correlation between public capital and private output might come from other parts of the economic system, which brings up the perennial issue of the direction of causation.

Ann Friedlaender sketches out an alternative framework that could be used in this type of research, a framework that would address the problem of resource costs. She advocates estimating a cost rather than a production function. This model would incorporate input price effects into the analysis, as well as allowing analysis of the efficiency of capital allocation. While admitting that the data requirements of this approach are substantial, she offers reasonable guidelines for estimating certain data, such as the cost of private and public capital by state. Friedlaender also proposes that one could add demand effects into the analysis through the use of a benefit

function. She concludes that such an approach is feasible and could yield interesting results to supplement the existing evidence on the importance of infrastructure to regional output, investment, and employment.

Is Public Infrastructure Undersupplied?

George Peterson addresses directly the question of whether public infrastructure is undersupplied. He begins by tracing the historical pattern of infrastructure spending over a longer period than previous studies. While public capital spending has indeed declined from its peak in the 1960s, this decline is only one downturn in a longer history of cyclical behavior. Moreover, the fact that infrastructure investment has declined does not in itself indicate that it is undersupplied. Thus, more information is required to determine whether there is a shortfall in public capital.

As one piece of evidence, Peterson basically accepts the Aschauer argument that the marginal productivity of public capital is extremely high compared to private capital. This suggests an undersupply even if the infrastructure has no value in providing services directly to the consumer. Peterson then looks to the taxpayer-voter for further evidence that infrastructure may be undersupplied.

Peterson obtains a partial answer through voters' revealed preferences as expressed in bond elections and other referenda. The answer is partial because only 25 percent of infrastructure spending passes through this process. Nevertheless, if public officials were trying to satisfy the median voter, as theory suggests, they would submit frequent bond proposals for consideration in order to assess voter demand. As a result, bond elections should be closely contested with bond approval rates and margins close to 50 percent. Instead, he finds that 80 percent of infrastructure bond proposals were approved between 1984 and 1989, and that the margin of approval exceeded 66 percent on average. This experience suggests an undersupply. But why? What forces could frustrate the demands of both business, which can gain as much from public capital investment as from its own investment, and the electorate, which appears disposed to approve higher levels of public capital outlays?

Peterson suggests three possible explanations. The first emphasizes spillover effects. As long as some of the benefits from public capital investment

spill over to users outside the local taxing district, and these users do not contribute to the costs of the projects, local taxpayers, who consider only their own benefit-cost trade-off, will choose to provide a suboptimal level of infrastructure capital. This problem could be solved through a user fee system, where all users, regardless of where they live, pay a fee to cover the marginal costs they impose on the network. In those instances when user fees are impractical, an alternative solution is intergovernmental matching grants.

A more innovative explanation is Peterson's notion that the undersupply might be traced to the "fear of rejection" on the part of public officials. Since the taxpayer revolts of the 1970s and early 1980s, the very act of referendum voting—and the possibility it brings of public repudiation—appears to intimidate officials. Rather than designing proposals to satisfy the median voter, they aim at garnering as large a majority as possible in order to minimize the chance of rejection. As a result, public capital spending proposals are simply not brought to the attention of voters.

Peterson's third explanation suggests that the political process systematically underweights the benefits from infrastructure that accrue to businesses.

Peterson addresses directly the question of whether public infrastructure is undersupplied, examining voting patterns on capital investment referenda.

He contends that the principle of "one person, one vote" provides no mechanism for aggregating the interests of both business and taxpayers.

Peterson concludes that infrastructure undersupply is as much a problem of politics as of economics. He argues that traditional decision-making processes are badly designed to handle joint consumer and producer demand for public goods. He also rejects the trend toward creating authorities and other institutions that can invest in infrastructure without submitting to the referendum process. Rather, he advocates the formation of business and consumer

alliances that together take the case for infrastructure spending directly to the public.

Alan Blinder, while agreeing that infrastructure is undersupplied, and that the causes include public officials' fear of rejection and externalities, questions the argument that business needs are not well represented in the political process. Each of us is both a producer and a consumer, and there is no evidence that people vote only their consumer interests. Furthermore, in an age when business has successfully lobbied to further its interests on regulatory, anti-trust, and trade protection issues, why should one believe that it is completely mute on the infrastructure front?

Because of the growth in both the economy and population that has occurred during this century, Blinder considers it inappropriate to compare only the absolute levels of capital spending across time. He notes that Peterson's median voter model implicitly assumes that the number of bond referenda proposed derives from previous approval rates. That may be a "good" model, but it does not embody rational expectations. Furthermore, while Blinder agrees that user fees are an appropriate way to deal with externalities, he cautions that user fees may not do the job if a free rider problem exists within a jurisdiction.

Joel Tarr focuses on the cyclical nature of infrastructure spending in an attempt to place the current developments in a historical context. He explains that both public and private capital spending have exhibited irregular cycles of spending bursts followed by periods of retrenchment and stability. Further, spending has shifted over time among levels of government and between private and public providers.

State governments were especially active from the 1820s through the 1840s, but curtailed their activities after depressions. Cities then assumed the role of primary infrastructure provider during the 1860s and early 1870s, after states suffered from over-investment, high taxes, corruption, and subsequent borrowing limitations.

At this point, private provision again became important, especially in water supply, as many municipal governments experienced defaults on their obligations and were hampered by spending limitations imposed by state governments. By the 1890s, however, municipalities regained their position as primary provider, which they held until World War I; after the war the states resumed the dominant role with heavy involvement in transportation investment.

The federal government was not deeply involved

in providing capital investment until the 1930s. It dominated through World War II. Since then, federal financing of capital spending has exhibited the familiar cycles of boom and bust.

Tarr then discusses the common characteristics of previous infrastructure spending bursts. Concerns over deterioration of facilities and adequacy of services have generally not been sufficient to spur investment. Earlier periods of rapid investment were characterized by a variety of demand- and supply-side conditions: major urbanization; critical technological developments, such as the automobile, the airplane, or advances in bacterial science; and new funding mechanisms, such as the gas tax.

Tarr concludes that current social, political, fiscal, and technological forces are unlike any previous period of growth in infrastructure investment, and thus suggests that those interested in expanding investment should investigate a variety of flexible approaches to achieve this goal.

What Are the Prospects for Privatizing Infrastructure?

Jose Gomez-Ibanez, John Meyer, and David Luberoff explore one alternative by investigating the prospects for privatizing infrastructure investment. Specifically, they analyze whether the private sector can do a more effective job of investing in and pricing infrastructure services. They focus on highways and wastewater treatment facilities as two areas where private participation appears most promising.

They make clear at the beginning that they would expect privatization to have little impact on the total quantity of infrastructure. In fact, they contend that the nation would probably end up with more infrastructure under public provision than under private. Their argument is that private infrastructure investment is likely to displace some other capital project, since it is financed from a limited pool of private savings. Public provision, in contrast, has some possibility of increasing total investment to the extent that the project is funded by user charges or taxes that are paid from a reduction in current consumption rather than from saving.

Rather than altering the quantity of infrastructure, privatization affects the distribution of burden between users, taxpayers, and wage earners. The conventional argument in favor of privatization is that the private sector is inherently more efficient and thus could build and operate facilities at a lower cost

than the public sector. This argument has been augmented in recent years by the concern that the public sector may be unable to finance facilities because of taxpayer resistance.

The commonly cited cost advantages of privatization are not entirely clear, the authors argue. Some of the reduction in cost reflects transfers among groups rather than real savings for society as a whole. For example, landowners may be more likely to donate rights-of-way to private road projects, but this is merely a transfer from landowners to road builders and does not change the amount of land needed for the project or the resource costs to society as a whole. On the other hand, private firms do have some real cost advantages: they have a stronger incentive and more flexibility to use resources productively, they can often build facilities more quickly, and they may be better able to exploit economies of scale, scope, and experience.

Proponents of privatization bemoan provisions of the Tax Reform Act of 1986 that restrict the use of tax-exempt financing for private projects; they claim that the higher financial costs for private providers makes it difficult for them to compete fairly with the public sector. Gomez-Ibanez, Meyer, and Luberoff argue, however, that even without tax exemption the costs of private and public providers do not differ markedly, since private providers can deduct interest payments as a business expense.

Cost, however, is often neither the only, nor the most important factor in the decision whether a particular project should be provided privately or publicly. Siting is often a major problem for highways as well as solid waste disposal facilities. Private providers may have some advantages in siting by allaying concerns of local residents and forming alliances with them before the project falls under the public spotlight, while public agencies are generally required to conduct site searches openly from the start. The private sector may also be more skilled in public relations—better able to market the benefits and minimize the risks of a project. Private involvement, however, does not eliminate the pressures or opportunities for government oversight or public involvement in siting decisions, since private facilities still require zoning permits and environmental approvals. Moreover, the public may be concerned that private firms may not take their environmental and other community responsibilities seriously. Public agencies may have an advantage simply because they have more established institutions and procedures for dealing with these issues. On balance, the authors

do not find that the private sector offers any major advantages in siting.

Other important issues are those of pricing and rate regulation. User charges seem to be appropriate financing mechanisms for both solid waste disposal and highways. While the choice of provider need not

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dictate the type of financing, the question arises whether a private firm or a public agency is more likely to charge the appropriate or socially desirable price. An argument in favor of private firms is that they are more likely to price services at marginal cost and to adjust charges to reflect the costs imposed by different types of users. The most important disadvantage of a private provider is that it may be tempted to exploit any monopoly power it might enjoy. Some states have turned to regulation to mitigate this problem; this strategy, however, may be inefficient because it could stifle market signals to increase capacity. In other words, the regulatory process, while necessary, could undermine many of the advantages of private involvement in infrastructure provision.

The authors then try to make some overall assessments about the winners and losers from privatization, with the caveat that the incidence of gains and losses depends in large part on the individual project. Organized labor and landowners are the most likely losers in private provision, due to the private firm's greater incentives to capture economic rents. The clearest winners are federal and state taxpayers. Investors might gain if they can hold onto economic rents or efficiency gains rather than passing them on to facility users; the outcome will depend on the competitiveness of the market for the particular service. Thus, privatization is a more attractive policy

for the public where the potential efficiency gains are great and the private operator faces effective competition.

The discussants find little with which to disagree. Sir Alan Walters adds that another argument for private provision is reducing the power of unions, thereby not only lowering wages but also reforming what he views as deleterious work practices. He also points out that the authors focus only on new construction and do not consider privatization of existing assets; this is probably a sensible tack since the likelihood of privatizing the Interstate Highway System is minimal. Nevertheless, an analysis of the efficacy of a completely privatized road system would have been interesting.

Walters does question the authors' argument that while privately provided infrastructure is likely to displace other private investment, publicly provided infrastructure, if funded by user charges or tax revenues rather than debt, is likely to generate additional investment. Walters believes that while the form of finance will affect the timing of savings, total investment will remain unchanged.

Gail Fosler states that the authors provide a useful discussion of the advantages and limitations of privatization; this effort adds an important perspective to the work of those advocating privatization as the solution to America's infrastructure problem. She notes the fact, implicit in their selection of highways and solid waste disposal facilities as examples, that privatization of infrastructure investment and public services generally has not progressed very far.

This raises the question: If private provision of infrastructure is such a good idea, why is it not done more frequently in the United States? Fosler concludes that the incentives required for private participation are extremely high. History shows that infrastructure activities are provided privately only when they are very profitable, and that they are often profitable when they enjoy significant noncompetitive market advantages. As a result, the efficiency gains from private provision are limited.

Fosler also reaffirms the authors' point that siting is a critical issue, and speculates that even if funding were available for all infrastructure spending it would probably not all be spent because of the politics of development. Fosler closes with the point that beyond providing infrastructure, the private sector has an important role in helping to shape the political process, so that the required levels of public spending and taxation are forthcoming from the government with as little economic distortion as possible.

How Efficient Is Current Infrastructure Spending and Pricing?

Clifford Winston argues that the focus of the current policy debate should be shifted from the question of how much to increase infrastructure spending—be it public or private—to a discussion of efficient pricing and investment guidelines. He believes the nation does not need to increase public capital outlays as much as it needs to price and spend more effectively. Users of infrastructure impose costs on themselves and others by increasing congestion and by wearing out the infrastructure. Thus, an efficient infrastructure policy will maximize the gap between social benefits and costs, including the costs that users impose on others, through pricing specifications that regulate demand and investment guidelines that specify design.

Winston lays out an efficient spending policy for both highways and airports. Current policy finances highway construction and repair through the fuel tax; this levy does not accurately reflect the pavement damage and congestion caused by different types of vehicles. Pavement damage varies with weight per

Winston believes the nation does not need to increase public capital outlays as much as it needs to price and spend more effectively.

axle, and thus users should be charged according to this measure. The current fuel tax provides the opposite incentive, because it encourages the use of small, fuel-efficient engines. Smaller engines, however, cannot pull as many axles as their larger counterparts. Thus, the fuel tax indirectly encourages shippers to use the least number of axles, and the most weight per axle, to transport a given load, thereby creating the most pavement damage per haul.

Pavement damage also depends on the thickness of the pavement. Previous analysis conducted by Winston found that optimal thicknesses are significantly higher than current thicknesses. Increasing pavement thickness would reduce annual mainte-

nance expenditures and, by lowering the marginal cost of a standard axle load, would soften the impact of taxes promoting efficient pavement wear.

Winston also examines the problem of congestion and finds that while congestion pricing has been advocated by economists for many years, it has been ignored or dismissed by policymakers. He addresses critics of congestion pricing by arguing that equity objections can be overcome if revenues are used properly and by citing existing systems that implement congestion pricing without disrupting travelers.

Winston then turns to airports and discusses the need for efficient pricing and investment in this area. Many observers argue that airport congestion and flight delays stem from capacity constraints. If increasing capacity through construction is the only method used in addressing the congestion problem, Winston claims that society will face a difficult and expensive task. Building new airports involves enormous costs and long lead times, and the predicted growth of air traffic volume is tremendous. He argues that efficient pricing and investment can provide immediate, low-cost relief.

Currently the most common method of assessing landing fees is by aircraft weight. This fee is inefficient, since the principal cost imposed by an aircraft takeoff or landing is the delay it causes other aircraft. Instead, Winston argues, congestion pricing should be implemented and runway capacity of existing airports should be expanded to the point where the marginal cost of an additional runway is equated with the marginal benefit of reduced delay. While less empirical work has been done on the effects of efficient policies on other infrastructure areas, the available information suggests that significant benefits could be derived.

In the final section of the paper Winston addresses common criticisms of efficient pricing and investment—technological infeasibility and the political difficulties of implementation. He also assesses the alternatives to efficient infrastructure policy—traditional approaches, privatization, and significantly increasing infrastructure spending. He cites evidence that efficient policies can be implemented with existing, proven technologies and believes that political hurdles could be overcome. In comparing efficient policies with the alternatives he finds efficient pricing and investment clearly preferable.

Alan Altshuler responds that despite the merits of the efficient pricing and investment argument, he does question the political feasibility of implementing this kind of policy. Winston's evidence in support of

his claims is only mildly suggestive, he says. Moreover, Winston does not carefully weigh the evidence contrary to his premise.

Altshuler judges that congestion pricing of roadways is still a political nightmare, and he will continue to view it as such until toll-road authorities have replaced commuter discounts with peak-period surcharges. Business, labor, and civic groups have consistently been hostile and quite vocal about proposed policies of this nature, and very successful in fighting their implementation. Altshuler also disputes Winston's claim that user fee systems can be structured to avoid regressivity, and to calm the ruffled feathers of vested interests.

He believes, however, that a shift in truck taxation from number of axles to axle weight is quite plausible, since it would entail only a minor revision of a long-standing arrangement. Airport congestion pricing policies are increasingly being implemented, according to Altshuler, but he doubts that they will be sufficient to alleviate airport congestion in the face of rapid predicted traffic growth, even if used in conjunction with runway expansion and air traffic control improvements. In sum, although specific initiatives may be feasible, Altshuler sees little reason to believe that economic efficiency will triumph in infrastructure policy; the values on which our political system is grounded routinely conflict with efficiency.

Michael Bell's comments begin by highlighting what he sees as the value in Winston's approach. Bell believes Winston takes an important step by considering not only the condition of the infrastructure but also its performance, since it is the services rendered by the facility that are important, and not the facility itself. Winston also explicitly links spending on new construction with operation and maintenance requirements, a very important, but often neglected, approach. Finally, Bell says that Winston raises legitimate questions about privatization, which is often seen as a panacea.

Bell believes that Winston's analytic approach could be extended in the following ways: expanding the definition of the output or product of public infrastructure spending, and including environmental costs as part of the social costs and thus incorporating these costs into the efficient pricing scheme.

Bell ends his discussion by raising two concerns about efficient pricing strategy. One is the same point made by Altshuler—however theoretically reasonable or technically feasible an idea may be, the public may not accept it. This applies especially to congestion pricing. Second, even if technically feasible

means of pricing were accepted and implemented for roads and airports, the task still remains of adapting these types of fees to environmental projects. This could be difficult because of distributional issues, the costs of administering such policies, and the weakening of economic tools as they are implemented through the political process.

How Should Public Infrastructure Be Financed?

Edward Gramlich further pursues the issue of getting the incentives right by evaluating the various mechanisms for funding public investment. He concentrates on state and local government spending, since the federal government undertakes little direct capital investment. The federal role in providing grants to states and localities for capital investment is central to the discussion, however.

Gramlich discusses three types of public capital investment and the appropriate funding schemes for each category. He begins with public capital investments that serve local needs with minimal spillovers to other communities and have no distributional implications. Here he argues that services should be financed by user fees; these fees apportion payment in accordance with benefits received and ensure efficient use. Some exceptions to this rule may arise in cases where, on equity grounds, officials want even those unable to pay to have access to, say, a park; the guiding principle, however, is that services that are enjoyed locally should be paid from a local revenue source.

Gramlich then discusses the second category of government investment, the case where spillovers occur, such as in national roads, wastewater treatment, or air pollution control. If feasible, the user fee is again the preferred funding mechanism. If user fees are costly to assess or inequitable, other options include the creation of a regional authority or the introduction of matching grants from the federal government. In the case of federal grants, the federal matching rates should correspond to the share of benefits accruing to out-of-jurisdiction users.

While many federal grant programs were designed with this principle in mind, their matching rates are much higher than appropriate, with the consequence that they must be capped to limit use. Gramlich proposes revamping the programs by reducing the matching rates significantly, while at the same time removing the caps. Changing the structure

of these programs would go a long way toward providing proper subnational government spending incentives and reducing federal grant spending.

The final category of investments entails both spillovers and long-run distributional considerations; the primary examples are public schools and higher education systems. These types of investments require different funding mechanisms. User fees are not appropriate for local schools, since education is a fundamental right of citizenship. Moreover, states have frequently been instructed by the courts to offset variations in the revenue-raising capability of communities in order to ensure that children in low-income communities are not educationally disadvantaged. The federal government currently has a limited grant program to assist poor school districts, again characterized by a cap and a high federal matching share. Gramlich notes that the problem

Gramlich suggests that uncapping federal grant programs and reducing matching rates would go a long way toward providing proper state and local spending incentives.

created by variations in community wealth is exacerbated by the federal deductibility of local property taxes. Thus, to improve schooling for children in underprivileged areas requires strengthening state equalization grants for education, reforming federal grants to poor school districts by removing existing caps and lowering the matching rate, and eliminating the federal tax deduction for property taxes.

Higher education is another area where long-run distributional implications come into play. In this case it is possible to impose user fees—tuitions—to cover the full cost of the service. This happens in some states, but typically only out-of-state students are charged the appropriate fee. Whether or not user fees cover the full costs, higher education has become very expensive, thus altering the issue somewhat: if fees do not cover the full cost, how can states afford

the programs, or if fees are full cost, how can families afford it?

After examining the issue of who should pay for which facilities, Gramlich then addresses timing questions. He emphasizes that in financing any project the cohort that reaps the benefits should pay the costs. Thus, capital expenditures should be financed by long-term bonds with maturities close to the life of the asset purchased. User fees or taxes should then pay annual depreciation plus interest and principal on the bonds. The good news is that, for the most part, this is already happening.

At both the federal and state levels much infrastructure investment is financed through dedicated trust funds. Trust funds are a useful way to link marginal benefits and costs when dedicated taxes or user fees can be assessed and when no externalities are present. Gramlich offers some suggestions for reform of the trust funds to best meet their intended purposes.

Gramlich's first discussant, Rudolph Penner, finds little with which to disagree and expands on the problem of capped grants. Many federal grants provide large windfalls to someone who would have engaged in the same activity regardless of the subsidy, rather than affecting the individual's marginal decision. This action, while irrational by textbook standards, is quite pervasive and thus deserves some attention. If the design of grant systems is fundamentally flawed, it severely limits the ability of higher-level governments to induce lower-level governments to provide optimal levels of public capital investment.

Penner has found that many phenomena that appear perverse to economists are often quite understandable and reasonable to legislators and others. He offers as an explanation of the popularity of these capped grants the fact that they convey a great deal of power to the bureaucracy and to the appropriate subcommittees. They also reduce the uncertainty facing politicians about the total amount required to fund a grant program. While the current situation is far from perfect, Penner believes it can improve. In large part improvement requires educating non-economists to the principles of economics (such as marginal decisions and horizontal equity). These issues are not intuitive to many people, but they need to be understood since they form the theoretical underpinnings of the proposed changes.

James Poterba, while generally agreeing with Gramlich's position, believes that some of his recommendations are open to debate. He begins by noting

that reforms of infrastructure finance are not merely accounting conventions; changes in financing mechanisms will also directly affect the level of spending. For example, one study showed that transit workers in urban mass transit systems with earmarked taxes received higher wage increases than those in systems without earmarked taxes. Similarly, Poterba's own work revealed that states with capital budgets spent 15 percent more on capital investment than states where capital and operating outlays were combined.

Poterba makes the same point as Penner: something must be going on to explain the pervasiveness of capped grants in the face of all the evidence of their inefficiency. He agrees with Penner that political factors are at work, but believes that the most important of these is the perceived need for equitable treatment of different jurisdictions. With open-ended grants, rich areas may contribute several times as much as their poorer neighbors to matching programs; the result is that absolute transfers from the federal government to the richer areas will be larger than those to poorer areas, thereby widening the inequities.

Poterba argues that capped grants may actually be efficient, citing literature from regulatory economics as evidence. For example, if federal grant-givers envision a minimum threshold of highway spending in each jurisdiction, then high subsidy rates on expenditures up to some level will ensure that most areas will take advantage of the program up to that point. Even if closed-ended grants are an efficient way to achieve an objective, Poterba emphasizes that this does not automatically imply that existing grant programs are well designed.

Poterba also raises a point about the applicability of user fees in certain situations. Regarding Gramlich's recommendation of user fees for solid waste disposal, for example, Poterba notes that user charges are generally more successful when levied at the time a consumer purchases a good than when charged to someone disposing of it. Finally, Poterba believes that calls for more efficient infrastructure financing will receive serious attention, especially given the current climate of fiscal austerity at both the federal and state levels.

Conclusion

Infrastructure is important for the environment, the quality of life, and economic performance. The United States has cut back sharply on infrastructure

investment in recent years. At the same time, few of the incentives that affect the decision to invest in new public capital or to use infrastructure services appear consistent with those advocated by economists. The

Resolving the infrastructure debate will be essential in order to determine the manner and appropriate level of capital spending in the 1990s.

question is what government officials should do now. Here opinion is sharply divided.

Those worried about the incentives to spend, the efficiency of design, and the appropriateness of the prices charged, want all efforts focused on elim-

inating current distortions and inefficiencies. They tend to believe that once the perversities in the existing system are removed, the present stock of infrastructure may meet most of the nation's needs. Additional investment at this time will divert attention and alleviate pressure to make the needed reforms.

While acknowledging the inadequacies in current funding, pricing, and design, other observers still see a need for more immediate investment. Dilapidated bridges and roads, large wastewater treatment requirements, and other needs make additional public capital investment essential. The positive impact of infrastructure on output and economic growth provides a further spur. Moreover, many question the likelihood that efficient pricing mechanisms will be adopted in the near future.

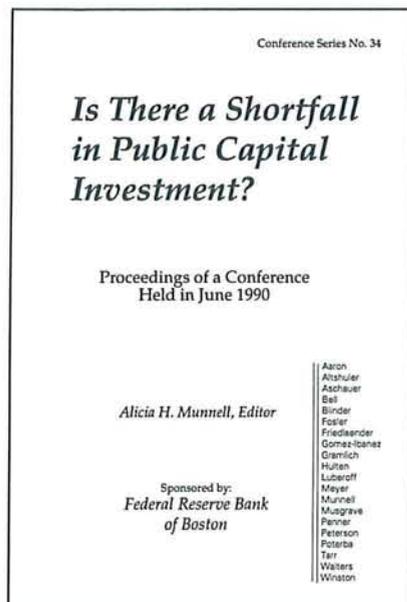
Resolving this infrastructure debate will be essential in order to determine the manner and appropriate level of highway and other capital spending during the 1990s.

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Foreign Exchange Intervention as a Signal of Monetary Policy

Midway through the 1980s, the position of the United States government toward international economic policy coordination shifted significantly. A noninterventionist stance in the foreign exchange market characterized the first half of the decade. This period saw a dramatic appreciation of the dollar, which rose over 50 percent in value against a weighted average of other major currencies between the beginning of 1980 and the beginning of 1985.¹ This large currency movement was accompanied by a large and growing deficit in the U.S. trade account and increasingly strident calls for protectionist legislation.

In the face of mounting concern regarding currency movements, policy shifted in the autumn of 1985 toward an attempt to manage the dollar. The watershed event was a meeting held at New York's Plaza Hotel on September 23, 1985. This meeting brought together central bankers and finance and treasury officials from the five largest industrial countries, the so-called Group of Five (G-5).² Over the next two years, the policy coordination initiated at the Plaza meeting continued with an economic summit meeting in Tokyo (May 5, 1986) and a meeting at the Louvre (February 22, 1987). Overall, the two-year period beginning with the Plaza meeting and ending with the worldwide stock market crash on October 19, 1987 marked the highest degree of international economic policy coordination between the United States and other major industrial countries since the advent of floating exchange rates in 1973.

In the wake of the Plaza meeting, the dollar depreciation that had begun in early 1985 but had stalled by late summer resumed apace.³ This path was consistent with policy goals. The communique issued after the Plaza meeting called for "some further appreciation of the main non-dollar currencies against the dollar" and stated that the G-5 governments would "stand ready to cooperate more closely to encourage this when to do so would be helpful." The dollar had depreciated 15 percent by the time of the Tokyo summit and another 8 percent by the time of

Michael W. Klein and
Eric S. Rosengren

Klein was Assistant Professor of Economics at Clark University when this article was written, and is now an Associate Professor at the Fletcher School of Law and Diplomacy, Tufts University. Rosengren is Assistant Vice President and Economist, Federal Reserve Bank of Boston. Karen Trenholme provided valuable research assistance.

the Louvre meeting. At the Louvre meeting, the policy goal shifted from dollar depreciation to currency stabilization. The eight-month period until the October 1987 stock market crash was the most stable for the major foreign exchange markets since the beginning of floating exchange rates, fourteen years earlier.

The apparent responsiveness of currencies to policy goals during the two-year period following the Plaza meeting has renewed interest in the efficacy of foreign exchange intervention. Most empirical studies of the effectiveness of intervention have concluded that intervention that leaves monetary policy unchanged has no lasting effect on the exchange rate. Despite this empirical finding, central banks may still choose to intervene to stabilize exchange markets or to signal a willingness to alter monetary policy. If central banks intervened to stabilize foreign exchange markets, exchange rate variance would be reduced but the level of exchange rates would remain unchanged. Alternatively, the goals of the central bank or economic circumstances may occasionally require greater emphasis on exchange rate management. Intervention may emphasize to market participants the importance of the exchange rate in policy determination and signal possible future changes in domestic monetary policy. This latter justification for intervention is explored in this article.

While intervention as a signal of monetary policy has been discussed in general terms in the interna-

Intervention may emphasize the importance of the exchange rate in policy determination and signal possible future changes in domestic monetary policy.

tional literature, it has not received a direct empirical testing. This article will examine the possibility that intervention by the United States and West Germany served as a signal of future monetary policy during the period between the Plaza Meeting and the October 1987 stock market crash. The first section discusses the nature of the intervention and the monetary policy changes undertaken by the United States and West Germany during this period. Section II

describes the properties of a signal and considers whether intervention served as an effective signal of changes in monetary policy. Examination of the data leads to the conclusion that intervention did not precede monetary policy changes in any regular or predictable manner during this period.

This finding is not necessarily inconsistent with intervention being perceived as a signal. Participants in the foreign exchange market may have thought at the time (incorrectly, as it turns out) that intervention was providing information about future monetary policy. Had market participants viewed intervention in this manner, a significant correlation would be expected between intervention and the exchange rate. This hypothesis is examined in Section III. Evidence is presented showing that intervention significantly affected the daily change in the exchange rate in the wake of the Plaza meeting. By the time of the Tokyo meeting and for the rest of the sample period, however, no evidence is found that the exchange rate responded to intervention in a significant way. The study concludes that, over time, foreign exchange market participants learned that intervention was not serving as a signal of changes in monetary policy.

I. Intervention and Monetary Policy

Official intervention in foreign exchange markets that leaves the money supply unchanged is called sterilized intervention. Sterilized intervention has no effect on the reserves of the banking system and thus does not alter monetary policy, because it involves a trade by a central bank of securities denominated in one currency for securities denominated in another currency.⁴ For example, the Federal Reserve may sterilize a purchase of German securities by concurrently selling U.S. securities. This transaction alters the composition of securities held by the public, but not monetary policy. As a result of the transaction, the public holds more U.S. securities and fewer German securities.

Sterilized intervention may affect the exchange rate through two channels: by changing the composition of the denomination of assets held by the public (the portfolio-balance channel), and by signaling central bank intentions on future monetary policy (the signalling channel). The portfolio-balance channel depends upon securities denominated in different currencies being imperfect substitutes. When securities are imperfectly substitutable, the

exchange rates and rates of returns on the securities must change in order to induce the public to hold the new portfolio. This portfolio-balance effect on exchange rates, while theoretically plausible, has not been found to be empirically significant.⁵ The lack of a portfolio-balance effect may be due to either the close substitutability of differently denominated securities or the small amount of intervention relative to the outstanding stock of securities.

An alternative rationale for sterilized intervention is that the central bank is signalling a willingness to alter monetary policy.⁶ The period examined here, from the September 1985 Plaza Accord to the October 1987 stock market crash, provides an important source of data for testing the role of intervention as a signal. This period stands in marked contrast to the first half of the decade, when the United States engaged in essentially no foreign exchange intervention. In the wake of the Plaza meeting, considerable consultation and coordination occurred among central banks. The importance of international considerations in setting monetary policy during this period is apparent from minutes of Federal Open Market Committee (FOMC) meetings and from descriptions of the Plaza and Louvre meetings.

Monetary Policy in the United States

The Federal Reserve implements monetary policy by altering the availability and cost of bank reserves. The supply of bank reserves can be changed by open market buying or selling of government securities, or through Federal Reserve discount window loans of reserves to banks. The demand for bank reserves depends on the reserve requirements and the amounts of funds held at banks in reservable accounts.

Monetary policy targeted the reserves borrowed by the private banking system from the discount window during the period that included the two years between the Plaza Accord and the 1987 stock market crash. This procedure affected short-term interest rates (in particular, the federal funds rate, which is the rate banks charge for lending bank reserves to other banks), as banks attempted to meet legal reserve requirements. The least costly way for banks to meet their reserve requirements is to borrow directly from the Federal Reserve, since the discount rate is generally below the federal funds rate. Although the Federal Reserve discourages using the discount window as a source of low-cost funds, bank borrowing from the Federal Reserve increases as the

Table 1
Changes in U.S. Monetary Policy, Plaza Accord to Stock Market Crash

Time Period	Borrowings Target (\$ million)	Discount Rate (Percent)	Daily Effective Federal Funds Rate (Percent)
9/23/85 – 11/ 5/85	500	7.5	8.18
11/ 6/85 – 12/17/85	450	7.5	8.17
12/18/85 – 2/12/86	350	7.5	8.26
2/13/86 – 3/ 6/86	300	7.5	7.97
3/ 7/86 – 4/17/86	300	7.0	7.40
4/18/86 – 7/ 9/86	300	6.5	7.02
7/10/86 – 8/19/86	300	6.0	6.47
8/20/86 – 4/29/87	300	5.5	6.32
4/30/87 – 5/20/87	400	5.5	6.98
5/21/87 – 9/ 2/87	500	5.5	6.79
9/ 3/87 – 10/16/87	600	6.0	7.51

Source: Federal Reserve Bank of New York.

spread between the federal funds rate and the discount rate increases. It is this relationship, between bank borrowing and the spread between the federal funds rate and the discount rate, that translates a borrowings target into an expected trading range for the federal funds rate, the short-term interest rate most influenced by monetary policy.

For each borrowings target an expected spread exists between the discount rate and the federal funds rate. If the borrowings target is unchanged and the discount rate is increased, the spread will be maintained by an increase in the federal funds rate of approximately the same amount. When the discount rate is raised initially, the spread is not wide enough to encourage discount borrowing, so banks bid more aggressively for federal funds. The federal funds rate then rises until the spread is restored to approximately the same level as before the discount rate change. Alternatively, if the borrowings target is raised with no change in the discount rate, the Federal Reserve pushes more banks to the discount window by selling bonds to absorb reserves. The diminished supply of reserves causes banks to bid up the rate on federal funds until enough banks borrow from the discount window to restore equilibrium.

Table 1 shows the prevailing borrowings target, the discount rate, and the federal funds rate at the time of changes in U.S. monetary policy between the Plaza Accord and the October 1987 crash. These data

show that monetary policy was easing, with drops in both the borrowings target and the discount rate, from September 1985 until April 1987. Monetary policy tightened from April 1987 until October 1987, with the average federal funds rate increasing almost 120 basis points over the final six months of the sample. The data in the table also demonstrate that reductions in the discount rate when the borrowings target was constant resulted in drops in the average federal funds rate of approximately the same magnitude. The relationship between changes in the borrowings target and the federal funds rate is less clear, in part because of technical problems during this period. The decrease in the borrowings target on February 13, 1986 and the increases on April 30, 1987 and September 3, 1987 all resulted in the average federal funds rate moving in the expected direction. Two decreases in borrowings at the end of 1985 did not result in a significant drop in the rate. Other factors at that time such as Hurricane Gloria, debt ceiling restrictions, and a major clearing house disruption caused the average federal funds rate to trade higher than anticipated. The failure of the average federal funds rate to increase after the May 21, 1987 policy change, however, cannot be attributed to any specific factors.

Monetary Policy in West Germany

The direction of German monetary policy can be ascertained from three interest rates: the discount rate, the Lombard rate, and the repurchase rate. Repurchase agreements are the primary instrument for implementing monetary policy in Germany. Repurchase agreements involve a transaction by the German central bank (the Bundesbank) that is reversed in the near future; for example, the Bundesbank may buy securities and agree to resell them in a month. In general, the initial transaction of a repurchase agreement is reversed after approximately one month, though longer repurchase agreements are made at slightly higher rates.

The discount rate is the rate at which universal banks sell securities, such as bills of exchange and treasury bills, to the Bundesbank.⁷ It differs from the U.S. discount rate in three ways. First, the discount loan in the United States is a collateralized loan set at a subsidized rate while the German discounting involves the sale of securities. German universal banks profit on the spread between the rate paid on the securities and the lower discount rate paid to the Bundesbank. Second, while U.S. discount borrowing

Table 2
*Changes in German Monetary Policy,
Plaza Accord to Stock Market Crash*

Time Period	Discount Rate (Percent)	Lombard Rate (Percent)	Repurchase Rate (Percent)
9/23/85 – 9/24/85	4.0	5.5	4.60
9/25/85 – 12/ 3/85	4.0	5.5	4.55
12/ 4/85 – 1/ 7/85	4.0	5.5	4.60
1/ 8/86 – 1/21/86	4.0	5.5	4.55
1/22/86 – 3/ 4/86	4.0	5.5	4.50
3/ 5/86 – 3/ 6/86	4.0	5.5	4.30
3/ 7/86 – 4/ 2/86	3.5	5.5	4.30
4/ 3/86 – 11/11/86	3.5	5.5	4.35
11/12/86 – 12/ 2/86	3.5	5.5	4.40
12/ 3/86 – 12/ 9/86	3.5	5.5	4.60
12/10/86 – 1/ 6/87	3.5	5.5	4.65
1/ 7/87 – 1/22/87	3.5	5.5	4.60
1/23/87 – 2/ 3/87	3.0	5.0	4.60
2/ 4/87 – 5/12/87	3.0	5.0	3.80
5/13/87 – 7/21/87	3.0	5.0	3.55
7/22/87 – 9/22/87	3.0	5.0	3.60
9/23/87 – 10/ 6/87	3.0	5.0	3.65
10/ 7/87 – 10/13/87	3.0	5.0	3.75
10/14/87 – 10/20/87	3.0	5.0	3.85

Source: Data Resources, Inc., and *Report of the Deutsche Bundesbank*.

by banks is expected to occur infrequently, to meet reserve needs of the financial institution, the German universal banks are expected to fully utilize their discount quotas. Third, while U.S. monetary policy incorporates expected discount borrowing into the system as a whole, no attempt is made to encourage particular institutions to borrow from the discount window. German discount policy sets quotas on discount loans for the system and for each individual bank according to a complicated formula that considers factors such as individual bank capitalization and loan structure. The German discount rate is adjusted relatively infrequently, and was changed only twice in our two-year sample.

The Lombard rate is the rate the Bundesbank offers on loans with collateral of qualified securities. Lombard loans are more analogous to the Federal Reserve discount window loans. They are both collateralized loans intended to meet the liquidity needs of the financial system, though the Lombard rate is a penalty rate rather than a subsidized rate. The Lombard loan has three major differences from German discount lending: the rate is set above market rates,

financial intermediaries besides universal banks have access to Lombard lending, and the Lombard loan provides short-term financing on an occasional basis. The Lombard rate moved only once during our sample and this was in conjunction with one of the two changes in the discount rate.

Table 2 shows the timing and magnitude of changes in indicators of German monetary policy. The first of the two discount rate reductions during our sample occurred immediately following a drop in the discount rates in the United States. The second discount rate reduction occurred at the same time as the reduction in the Lombard rate. The repurchase rate changed sixteen times, but only six of these represented a change of more than 5 basis points. The repurchase rates were generally falling through most of the sample period, with the largest drops occurring around the time of the drops in the Lombard and discount rates, although repurchase rates were trending upward in the five months prior to the October 1987 stock market crash.

II. Intervention as a Signal

Central to a discussion of intervention as a signal of monetary policy are its effectiveness and its relevance. For intervention to serve as an effective signal of monetary policy, it should precede future actions in a proximate and consistent manner. The effectiveness of intervention as a signal will be eroded by signalling failures, that is, interventions that are not followed by changes in monetary policy and policy changes that are not preceded by intervention. In order to be a relevant signal, intervention should disclose information that the market would otherwise not have. If the information provided by intervention were redundant (if, for example, interventions were always fully anticipated), then it would serve no role as a signal.

This section will assess the ex post effectiveness of intervention as a signal of monetary policy by examining the temporal relationship between intervention and monetary policy changes.⁸ The intervention data used are reports of intervention by the Federal Reserve or the Bundesbank that appeared in *The Wall Street Journal*, *The New York Times*, and the *Financial Times* of London. These intervention data serve the purpose of this study better than actual intervention data (which are confidential and difficult to obtain), since intervention serves as a signal only when it is widely observed by the market.

Table 3 summarizes the number of interventions reportedly undertaken by the Federal Reserve and the Bundesbank for the entire sample period as well as for three subperiods. The Bundesbank was much more active in the foreign exchange market than the Federal Reserve. The Bundesbank was reported to have intervened twice as many times as the Federal Reserve, with more than two-thirds of its actions occurring unilaterally. The Federal Reserve was reported to have intervened on 34 trading days out of the 532 trading days in the sample. Of those 34 interventions, nearly two-thirds were conducted in conjunction with the Bundesbank. Most joint interventions occurred immediately after the Plaza Accord

Table 3
Number of Days of Reported Dollar Interventions, Plaza Accord to Stock Market Crash

Time Period	Dollar Interventions	
	United States	Germany
Plaza—9/23/85–5/3/86		
Alone	2	19
Together	9	9
Tokyo—5/6/86–2/20/87		
Alone	2	18
Together	0	0
Louvre—2/23/87–10/16/87		
Alone	8	12
Together	13	13
Total	34	71
Alone	12	49
Together	22	22

Source: Intervention reports in *The Wall Street Journal*, *The New York Times*, and *Financial Times* of London.

and in the period following the Louvre meeting. No joint interventions were reported during the period from the Tokyo meeting to the Louvre meeting. While the Bundesbank intervened unilaterally throughout the period, the Federal Reserve rarely intervened without the Bundesbank, particularly in the period from the Plaza Accord to the Louvre meeting.

If intervention signalled changes in monetary policy, then a clustering of interventions would be expected to occur immediately prior to policy changes. Table 4 provides the cumulative number of days that interventions were reported to have oc-

Table 4
Interventions as Signals of Changes in U.S. Monetary Policy

Date of Monetary Policy Change	Type of Change ^a	Number of Federal Reserve Interventions ^b in Previous Trading			Number of Bundesbank Interventions ^b in Previous Trading		
		5 Days	10 Days	15 Days	5 Days	10 Days	15 Days
11/ 6/85	BR d 50	0	1	2	0	4	8
12/ 8/85	BR d 100	0	0	0	0	2,-1	2,-1
2/13/86	BR d 50	0	0	0	0	0	0
3/ 7/86	DR d .50	0	0	0	0	0	0
4/18/86	DR d .50	0	0	0	0	0	0
7/10/86	DR d .50	0	0	0	0	0	0
8/20/86	DR d .50	0	0	0	0	0	0
4/30/87	BR u 100	0	0	-2	-3	-4	-6
5/21/87	BR u 100	0	0	-2	0	0	-2
9/ 3/87	BR u 100 and DR u .50	-3	-3	-4	-3	-3	-3

^aBR = Borrowed reserves target (\$millions), DR = Discount rate (percentage point changes), u = up, d = down.

^bPositive numbers are \$ sales, negative numbers are \$ purchases.

Source: See Tables 1 and 3.

curred in the five, ten, and fifteen trading days prior to each change in U.S. monetary policy. Given the number of reported interventions by the Federal Reserve that appear in this sample, the probability of intervention on any particular day is 6 percent. The probability of observing at least one intervention in five trading days is 27 percent, in ten trading days is 48 percent, and in fifteen trading days is 60 percent. The observed numbers of days of interventions that appear in Table 4 are fewer than would be expected if interventions occurred randomly, except for the September 3, 1987 change in policy. Only 10 of the 34 Federal Reserve interventions occurred within 15 trading days of a policy change. Therefore, even if investors initially thought that intervention was signalling future policy changes after the Plaza Accord, the large number of interventions not followed by policy changes and the large number of policy changes not preceded by interventions would have quickly reduced the value of interventions as a signal.

Since two-thirds of the Federal Reserve interventions occurred jointly with the Bundesbank, it is possible that both interventions signalled a change in German rather than American monetary policy. Table 5 shows the number of days the Federal Reserve and the Bundesbank intervened in the 15 trading days prior to German policy changes. Since the sample includes 71 German interventions, if interventions

occurred randomly there would be a 13 percent chance of an intervention on any given day. The probability of observing at least one intervention in five trading days is 50 percent, in ten trading days the probability is 75 percent, and in fifteen trading days 88 percent. Again, the number of interventions prior to policy changes is lower than might be expected had intervention occurred randomly. Of the 71 days when German intervention occurred, 53 were not followed by a policy change in the subsequent three weeks. Thus, most interventions were not followed by policy changes and most of the policy changes were not preceded by a significant increase in interventions.

III. Exchange Rates and Intervention: The Role of 'News'

The data presented in the previous section indicate that no consistent and proximate link existed between foreign exchange intervention by the central banks of the United States and West Germany and changes in their respective monetary policies during the period between the September 1985 Plaza meeting and the October 1987 stock market crash. With the benefit of hindsight, one can conclude from these data that intervention did not signal future monetary

policy. The data failed to answer, however, the question of whether the foreign exchange market viewed intervention as a signal at the time. This section will address the issue by examining the effect of intervention and monetary policy changes on the daily deutsche mark/dollar exchange rates.

Modern international finance theory provides a framework for inferring whether market participants viewed intervention as a signal. The framework draws from models of exchange rate determination that focus on the role of the exchange rate as the relative price of assets denominated in different currencies. As with other asset prices, the exchange rate is forward-looking and a function of its own expected future value. This forward-looking characteristic ensures that the exchange rate responds to news about future events. Thus, information about future changes in monetary policy affects the exchange rate immediately. A significant link between exchange rates and interventions is consistent with intervention being viewed during the sample period as a

signal of future monetary policy. A failure to find such a link would suggest that the market did not view intervention as a signal, either because the information had already been revealed prior to the intervention or because the intervention was perceived as devoid of information.

Asset-Market Models of the Exchange Rate and the Effect of News

The high degree of integration in international capital markets and the vast daily volume of foreign exchange transactions underscores the importance of viewing the exchange rate as a measure of the relative price of assets denominated in different currencies. Central to this asset-market-based approach to exchange rate determination is the arbitrage relationship of interest parity. This relationship states that assets sharing common liquidity and political risk characteristics but denominated in different currencies have equal expected returns when these returns

Table 5
Interventions as Signals of Changes in German Monetary Policy

Date of Monetary Policy Change	Type of Change ^a	Number of Federal Reserve Interventions ^b in Previous Trading			Number of Bundesbank Interventions ^b in Previous Trading		
		5 Days	10 Days	15 Days	5 Days	10 Days	15 Days
9/25/85	RR d .05	2			2		
12/ 4/85	RR d .05	0	0	0	0	0	0
1/ 8/86	RR d .05	0	0	0	0	-1	-2
1/22/86	RR d .05	0	0	0	0	0	0
3/ 5/86	RR d .20	0	0	0	0	0	0
3/ 7/86	DR d .50	0	0	0	0	0	0
4/ 3/86	RR u .05	0	0	0	0	0	0
11/12/86	RR u .05	0	0	0	0	0	0
12/ 3/86	RR u .20	0	0	0	0	0	0
12/10/86	RR u .05	0	0	0	0	0	0
1/ 7/87	RR d .05	0	0	0	-1	-1	-1
1/23/87	DR d .50						
	and						
	LR d .50	0	0	0	0	-2	-4
2/ 4/87	RR d .80	0	-1	-1	0	-2	-2
5/13/87	RR d .25	0	-2	-2	0	-2	-5
7/22/87	RR u .05	-1	-1	-1	0	1	2
9/23/87	RR u .05	0	-1	-2	0	0	-2
10/ 7/87	RR u .10	0	0	0	0	0	0
10/14/87	RR u .10	0	0	0	0	0	0

^aDR = discount rate, LR = Lombard rate, RR = repurchase rate (percentage point changes). u = up, d = down.

^bPositive numbers are \$ sales, negative numbers are \$ purchases. Study begins 9/23/85.

Source: See Tables 2 and 3.

are expressed in a common currency. For example, the return on a security denominated in dollars and a security with similar liquidity and political risk attributes denominated in deutsche marks will provide the same expected return when the return on the deutsche mark security is expressed in dollars or when the return on the dollar security is expressed in deutsche marks. This relationship is shown in equation 1.

$$(1) \quad (1 + R_t) = (1 + R_t^*) \cdot (E_t S_{t+1}/S_t)$$

where R_t is the domestic interest rate on a security that comes due in period $t + 1$, R_t^* is the interest rate on a foreign security with similar risk and liquidity attributes, S_t is the exchange rate at time t (domestic currency per unit of foreign currency) and $E_t S_{t+1}$ is the expectation at time t of the value of the exchange rate at time $t + 1$.⁹ This equation can be rewritten as follows:¹⁰

$$(2) \quad R_t = R_t^* + [(E_t S_{t+1}/S_t) - 1]$$

where the term in square brackets represents the expected rate of change of the domestic currency.

The expected change in the exchange rate figures into the interest parity relationship because the domestic-currency-denominated return on the foreign currency security depends upon two factors, the interest paid by the foreign currency bond and the change in the exchange rate over the time the security is held. The expected change in the exchange rate is an important determinant of relative returns because it affects the amount of domestic currency the bearer of the foreign-currency security can purchase when the security matures. For example, a person who purchases a deutsche-mark-denominated security with a relatively strong dollar and later receives coupon payments in relatively strong deutsche marks enjoys the greater purchasing power of the coupon payments due to the depreciation of the dollar.

The interest parity relationship in equation 1 illustrates how sterilized intervention may affect the exchange rate. A sterilized intervention has no effect on the domestic or foreign money supply and thus it has no effect on either R_t or R_t^* . If intervention is a signal of future monetary policy it will affect the expected future value of the exchange rate, $E_t S_{t+1}$. Equation 1 demonstrates that, given foreign and domestic interest rates, interest parity will continue to hold, with a change in the expected future exchange rate only if today's spot exchange rate also changes in the same direction and by the same magnitude. Thus, news about future policy affects

the exchange rate today, through its effect on the expected future exchange rate.

This heuristic explanation of the effects of intervention on the current spot exchange rate through its role as a signal of future monetary policy obviously glosses over some important complications. Nevertheless, the basic intuition provided by this example continues to hold in a fully specified model.¹¹ One result apparent from a fully specified model that is

For intervention to serve as an effective signal of monetary policy, it should precede future actions in a proximate and consistent manner.

not immediately obvious from inspection of equations 1 and 2 is that an anticipated change in monetary policy can occur at any point in the future and still affect the exchange rate as soon as the information is revealed. A caveat to this point is that the effect of a given change in monetary policy is larger, the closer to the present it occurs. Finally, it is important to mention that news about monetary policy in either country will affect the exchange rate. If the expected change in monetary policy is the same in both countries the exchange rate may not be affected.

An Empirical Test of the Effects of Intervention on the Exchange Rate

This section will investigate the effect of intervention by the Federal Reserve and the Bundesbank on the daily deutsche mark/dollar exchange rate. The tests cover three subsamples of the period between the September 1985 Plaza meeting and the October 1987 stock market crash: the period between the Plaza meeting and the May 1986 Tokyo summit meeting, the period between the Tokyo summit and the February 1987 Louvre meeting, and the period following the Louvre meeting until the stock market crash.

The sample is divided into subperiods in order to discern whether the credibility of intervention as a signal evolved over time in the way suggested by the previous discussion. The commitment to manage the exchange rate, mentioned in the communique from the Plaza meeting, may have initially given credibility

to the use of intervention as a signal. In this atmosphere one would expect to find that intervention has a significant effect on the exchange rate. As participants in the foreign exchange market learned of the lack of a proximate and consistent link between intervention and monetary policy, however, the effect of intervention on the exchange rate would erode.¹²

The discussion in the previous section suggested a framework for specifying an empirical test for the effects of intervention on the exchange rate. If intervention were perceived to be a signal of future monetary policy change, then it would have an immediate impact on the exchange rate. If, on the other hand, intervention were not perceived as a signal, then it would not significantly alter the exchange rate. A regression was estimated to test this hypothesis.

The dependent variable in the regression equation tested is the logarithm of the change in the deutsche mark/dollar exchange rate in the New York market between 9:00 a.m. one day and 9:00 a.m. the following day. The intervention series are dummy variables that take the value 1 if intervention occurs between the initial and subsequent measurements of the exchange rate. These data were collected from newspaper accounts of intervention reported by *The Wall Street Journal*, *The New York Times*, or the *Financial Times* of London. Four separate intervention dummy variables represent intervention by either the Federal Reserve or the Bundesbank to increase or to depress the exchange value of the dollar. Two additional dummy variables represent coordinated intervention to increase or decrease the value of the dollar.¹³ The regressions also include variables to capture monetary policy changes: the German repurchase rate as well as a dummy variable to represent days when a change occurred in the U.S. discount rate, a dummy variable to represent a day when a change occurred in the borrowed reserves target by the Federal Reserve, and a dummy variable representing days with changes in the German discount rate. Estimation is by ordinary least squares using the White (1980) correction for heteroskedasticity.¹⁴

The results are presented in Table 6. Interventions by the Federal Reserve have the prefix FED and interventions by the Bundesbank have the prefix BB. The intervention dummy representing days when both central banks intervene has the prefix COMB. Intervention by either central bank to weaken the dollar has the suffix W while intervention by either central bank to strengthen the dollar has the suffix S. The expected coefficient on intervention to decrease

the value of the dollar by either the Federal Reserve (FED.W_t) or the Bundesbank (BB.W_t) is negative, and the expected coefficients on intervention to increase the value of the dollar (FED.S_t and BB.S_t) are positive. These coefficients (on FED.W_t, FED.S_t, BB.W_t and BB.S_t) represent the effect of intervention undertaken alone by either the Federal Reserve or the Bundesbank. The coefficient on either combined intervention dummy demonstrates that combined interventions provide a significantly more resolute signal than interventions undertaken alone. The regression controls for changes in monetary policy discussed above with the following variables: the U.S. discount rate (FED DR), the German discount rate (BB DR), the U.S. borrowings assumption (FED BR), and the German repurchase rate (RepoG).

The results support the hypothesis that intervention was perceived by the market as a signal in the first subperiod.

The results presented in Table 6 support the hypothesis that intervention was perceived by the market as a signal in the first subperiod, from the September 1985 Plaza meeting to the May 1986 Tokyo summit. No instances of combined intervention to increase the value of the dollar occurred during this period. Intervention undertaken by either central bank in isolation either had no significant effect on the deutsche mark/dollar exchange rate, or had a significant effect of the incorrect sign, as occurred in the only two cases of intervention by the Federal Reserve during this period. Intervention to decrease the value of the dollar undertaken jointly by the Federal Reserve and the Bundesbank, however, had a significant effect of the expected sign.

The second subperiod runs from the May 1986 Tokyo summit to the February 1987 Louvre meeting. During this time the Federal Reserve did not intervene jointly with the Bundesbank and intervened only twice in isolation. The estimates from this period demonstrate a significant and correctly signed effect of intervention to increase the value of the dollar by the Federal Reserve and a significant but incorrectly signed effect of intervention to decrease the value of the dollar by the Bundesbank.

Table 6
The Effects of Intervention on the Change in the DM/\$ Exchange Rate
 Dependent Variable: $\ln(s_{t+1}) - \ln(s_t)$ where $s = \text{DM}/\text{\$}$

Variable	Plaza to Tokyo 9/23/85–5/3/86	Tokyo to Louvre 5/6/86–2/20/87	Louvre to Crash 2/23/87–10/16/87
CONSTANT	.031 (.032)	-.0039 (.014)	.012 (.014)
FED.W _t	.0070* (.0007)	No observations	-.0051 (.0036)
FED.S _t	-.0068* (.0014)	.0048* (.0011)	-.0037 (.0026)
BB.W _t	.0013 (.0013)	.0063* (.0006)	-.0032 (.0028)
BB.S _t	.0004 (.0025)	-.0019 (.0019)	-.0024 (.0028)
COMB.W _t	-.014* (.003)	No observations	.0095* (.0046)
COMB.S _t	No observations	No observations	.0048 (.0044)
RepoG	-.0072 (.0073)	.0071 (.0032)	-.0032 (.0039)
BB DR	-.0027 (.0018)	-.0032 (.0019)	No observations
FED DR	.0037 (.0040)	.0008 (.0041)	-.0009 (.0005)
FED BR	-.0021 (.0024)	No observations	-.0007 (.0015)
R ²	.047	.013	.056
Durbin-Watson	1.90	2.01	2.11
Observations	154	203	166

*Significant at 95 percent level. Standard errors in parentheses.
 Estimation by ordinary least squares with White (1980) correction for heteroskedasticity.

The final period begins just after the Louvre meeting in late February 1987 and ends just before the October 1987 stock market crash. A full complement of the different types of intervention occurred during this time. Not one of the coefficients is both of the right sign and statistically significant.¹⁵

A test of the linear combination of each central bank's intervention dummy and the combined dummy demonstrates the overall effect of each central bank's intervention on days when the other central bank also intervenes.¹⁶ Table 7 provides summary statistics of the effect of coordinated interventions. Between the Plaza Accord and the Tokyo summit, interventions to decrease the value of the dollar by both the Federal Reserve and the Bundesbank were significant and of the right sign. In the

period between the Tokyo summit and the Louvre meeting, no coordinated interventions occurred, so no observations are available. After the Louvre meeting, efforts by the Federal Reserve and the Bundesbank to increase or decrease the value of the dollar had no effect.

The results in Tables 6 and 7 tend to support the hypothesis that, at least by the end of the sample period, intervention was not perceived by market participants to be a trustworthy signal of monetary policy. The results from the first subperiod, and to a lesser extent from the second subperiod, suggest that intervention policy may have enjoyed some success initially.¹⁷ The subsequent lack of efficacy of this policy may have been a consequence of a failure of monetary authorities to use intervention as a proxi-

mate and consistent signal of future policy. The impact of intervention on the exchange rate eroded as participants in the foreign exchange market learned of the lack of a nexus between intervention and monetary policy.

IV. Conclusions

Most empirical studies have found no evidence that sterilized intervention alone has an effect on the long-run value of the exchange rate; however, these studies frequently discuss the possibility that sterilized intervention may have significant short-run effects if it signals future monetary policy changes. This study found no evidence that reported interventions by the United States and Germany were used to signal monetary policy. Interventions did not precede monetary policy changes and periods of active intervention were not followed by monetary policy changes. Given the lack of correspondence between interventions and monetary policy, intervention could not have been an effective signal of monetary policy during the period between the Plaza Accord and the October 1987 stock market crash.

While in hindsight the data provide no evidence that interventions in foreign exchange markets were used to signal policy changes, it is possible that, at the time, market participants interpreted interventions as signals of future policy. If so, significant movements in the exchange rate would be expected

at the time of interventions. Central banks actively intervened in foreign exchange markets after the Plaza Accord. Evidence suggests that combined interventions to increase the value of the dollar during this period did result in a significant decline in the deutsche mark/dollar exchange rate. As it became apparent that intervention was not signalling monetary policy changes, market participants apparently stopped interpreting intervention as a signal. Consistent with that hypothesis, no evidence was found that announcements of intervention had a significant effect after the Tokyo or Louvre meetings.

The results reported in this article do not preclude intervention as a signal of monetary policy; however, to be an effective signal requires a greater nexus between intervention and monetary policy than occurred between the Plaza meeting and the stock market crash. When joint intervention was perceived as a signal immediately after the Plaza Accord, intervention caused the dollar to depreciate, but this effect attenuated as investors perceived no change in monetary policy in the United States and Germany. Intervention could have a significant signalling effect in the future if it indicated a willingness of central banks to alter domestic monetary policy to achieve exchange rate goals. During the period under consideration, however, the evidence on portfolio effects examined by other researchers, and the evidence on signalling examined here, seem to suggest that motives of central banks may have been more political than economic in nature.

Table 7
Test of the Effects of Intervention on the DM/\$ Exchange Rate
Dependent Variable: $\ln(s_{t+1}) - \ln(s_t)$ where $s = \text{DM}/\text{\$}$

Variable	Plaza to Tokyo 9/23/85–5/3/86	Tokyo to Louvre 5/6/86–2/20/87	Louvre to Crash 2/23/87–10/16/87
FED.W _t + COMB.W _t	-.0071* (.0031)	No observations	.0044 (.0030)
FED.S _t + COMB.S _t	No observations	No observations	.0011 (.0035)
BB.W _t + COMB.W _t	-.013* (.003)	No observations	.0063 (.0063)
BB.S _t + COMB.S _t	No observations	No observations	.0025 (.0033)

*Significant at 95 percent level. Standard errors in parentheses.

¹ The nominal effective exchange rates referred to here are taken from *World Financial Markets*, published by Morgan Guaranty Trust Company.

² The G-5 consists of the United States, West Germany, and Japan, which constitute the G-3, along with the United Kingdom and France.

³ Some controversy exists over whether the path taken by the dollar after the Plaza Accord was merely an extension of its depreciation during the first part of 1985.

⁴ In contrast, a change in monetary policy through an open market operation would involve trades of securities for money denominated in the same currency.

⁵ Some of the research that demonstrates the lack of a portfolio-balance effect includes Frankel (1982), Rogoff (1984), Lewis (1988), Engel and Rodrigues (1989), and Dominguez and Frankel (1990).

⁶ U. S. interventions can be conducted by the Federal Reserve as an agent for the Treasury, or for the Federal Reserve's own account. For expositional ease we will refer to all U.S. interventions as being Federal Reserve interventions.

⁷ Universal banks in Germany have much broader powers than American banks. Universal banks include private commercial banks, savings banks, and credit associations and all have access to discount loans. The specialized banks such as mortgage banks and investment companies cannot borrow from the discount window but do have access to Lombard lending, described later in this section.

⁸ While the relevance of intervention cannot be addressed with the data presented here, the regression results in the next section provide insight to the combined effectiveness and relevance of intervention.

⁹ A more general form of the interest parity relationship would allow for the existence of a risk premium term. Empirical attempts to model the risk premium, however, have been largely unsuccessful. This lack of success is closely tied to the inability to find a significant portfolio-balance channel for intervention, since if sterilized intervention operates through this channel it

must be through a predictable effect on the risk premium term.

¹⁰ The expression in equation 2 is an approximation of the expression in equation 1 that holds closely when interest rates or expected rates of depreciation are not too large.

¹¹ A fully specified model of the effect of "news" on the exchange rate has been developed by Engel and Frankel (1984), who used it to explore the effect of announcements of the money supply on interest rates and exchange rates.

¹² If intervention worked through a portfolio balance channel, and if the size of interventions did not decline significantly over time, one would not expect to see any differences in the effect of intervention on the exchange rate across subperiods.

¹³ There are no instances of days when the Federal Reserve was intervening in one direction and the Bundesbank was intervening in the other direction.

¹⁴ The assumption behind the regression is that in the absence of intervention exchange rates follow a random walk. Despite differences in stated objectives after the international summits, the intercepts which represent the time trend of the exchange rate are not significantly different across time periods.

¹⁵ None of the monetary variables enter the regression with the right sign and statistically differ from zero in any of the time periods. Models that control more completely for expectation effects do find significant effects of monetary policy changes on exchange rates. For example, see Brown (1981) and Batten and Thornton (1984) for studies that find significant effects of discount rate changes when controlling for expectations.

¹⁶ As indicated in Table 6, there are no observations of dollar-weakening intervention by the Federal Reserve or combined dollar-strengthening intervention during the second subperiod, or of combined dollar-strengthening intervention during the first subperiod.

¹⁷ Similar results are reported by Dominguez (1990), who studied the effect of intervention on excess returns. In particular, she found significant effects immediately following the Plaza meeting but an erosion of the effect subsequently. She also found different effects for unilateral and coordinated intervention.

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Do Capital Markets Predict Problems in Large Commercial Banks?

In the present climate of intense debate over deposit insurance reform, the nature and limits of market discipline become especially important. The widely accepted argument for greater reliance on market discipline is that it will restrain managerial risk-taking and reduce potential losses to the deposit insurance fund.

Opponents of this view favor the traditional reliance on supervision by the bank regulatory agencies as the primary method to maintain the safety and soundness of the banking system and the integrity of the deposit insurance fund. They question the ability of outsiders, in particular uninsured depositors, to evaluate the credit quality of commercial bank portfolios and thus to assess their risk without the more detailed inside information available to bank examiners.

This article attempts to shed some empirical light on this issue by studying the effectiveness of market discipline as it is exercised by bank stockholders. The interesting question to ask is whether the market may have recognized problems in a bank's loan portfolio before the regulators became aware of them. If that is in fact the case, then monitoring returns to bank shareholders can help bank regulators identify a problem bank earlier and target bank examinations where they are most needed.

*Katerina Simons
and Stephen Cross*

Economist, Federal Reserve Bank of Boston, and Director, Division of Banking Research and Statistics, Office of the Comptroller of the Currency. The authors thank Richard Kopcke and Eric Rosengren for helpful suggestions. Lawrence D. Herman provided able research assistance.

Bank Examinations

Traditionally, bank supervisors have relied extensively on on-site bank examinations to identify problems in individual commercial banks and to ensure their compliance with existing laws and regulations. During on-site examinations, examiners assess five dimensions of a bank's operations, rating them on a 1 to 5 scale, with 1 being the best rating. These five dimensions are the bank's Capital, Assets, Management, Earnings, and Liquidity. A composite rating, which combines the

above dimensions and is known by their acronym CAMEL, is also assigned. As a rule of thumb, banks with a CAMEL rating of 4 or 5 are considered to be problem banks.

In recent years, the deteriorating credit quality of bank loans in many regions of the country has placed an increasing strain on the limited resources available to bank regulators. An FDIC study of the Texas banking crisis (O'Keefe 1990) placed part of the blame for the severity of the crisis on the infrequency of bank examinations in the preceding years. The study found that the frequency of examinations for failed banks in the Southwest had been lowest in the nation for most of the previous decade. The study also found that the frequency of bank examinations declined significantly in 1984 and 1985 for the nation as a whole and for Texas in particular. This decline in supervision was due to a reduction in examination staff, caused by a hiring freeze precisely at the time when more supervisory resources were necessary.

Given that bank regulators have limited manpower to respond to bank problems, it is especially important that their resources be deployed in an optimal manner. It would be useful if deteriorating banks could be identified prior to scheduled examinations, so that bank examiners could concentrate their efforts on those banks most in need of supervision.

The Stock Market as an Early Warning Device

One method of identifying problem banks early that has been suggested in the academic literature is monitoring stock market returns of publicly traded bank holding companies. If, prior to a bank's classification as a problem bank, returns to its shareholders fall significantly below levels implied by previous results, it may be possible to use changes in the bank's stock price as an early warning signal for changes in its condition. Such a fall in stock returns would also imply that market discipline can, indeed, be effective in augmenting and even supplanting traditional periodic bank examinations.

Pettway (1980), Pettway and Sinkey (1980), and Shick and Sherman (1980) test whether bank shareholder returns are below estimated levels prior to the examination in which the bank's CAMEL rating is downgraded to problem level. These articles find some evidence suggesting that unexpectedly low stock market returns may precede a bank's inclusion on the problem bank list and thus have the potential

to be used as early warning signals.

This article follows the literature in examining stock returns for a sample of problem banks for a time period before their downgrade. This study also looked beyond the pattern of stock returns by inves-

This study shows that, in the aggregate, shareholder returns fail to anticipate bank downgrades by examiners.

tigating what information stockholders may have possessed before the banks were downgraded to problem-bank status. This was done by examining the news items about the banks reported in the financial press before the downgrade and by examining the pattern of insider transactions. The results show that, in the aggregate, shareholder returns fail to anticipate bank downgrades by examiners. In addition, examination of individual problem banks fails to reveal convincing instances of specific information that had been known to investors before the downgrade.

Sample and Methodology

Selected for the analysis was a sample of publicly traded bank holding companies in which the lead bank was a national bank that had its composite CAMEL rating downgraded to either 4 or 5 between 1981 and 1987. Twenty-two bank holding companies fitted this criterion. For each of these holding companies, weekly market returns were calculated for two years preceding downgrading.

In order to test whether the stock market anticipated the bank's downgrade to problem-bank status, the study employed the residual analysis technique first popularized by Fama, Fisher, Jensen, and Roll (1969) and now standard in event studies. The market-model residuals for each of the 22 bank holding companies were calculated using weekly market returns:

$$(1) \quad e_{jt} = R_{jt} - (a_j + b_j(R_{mt}))$$

where R_{jt} is the return to holders of security j at time

t , a_j and b_j are parameters of the one-factor market model, R_{mt} is the return on the market portfolio at time t (defined as the Standard & Poor's 500 stock average), and e_{jt} is the residual. Parameter estimates were generated using data from weeks -103 to -52 prior to the examination in which the bank was downgraded to problem status, and residuals were computed by comparing actual to forecast returns in weeks -51 to 0 prior to the downgrade.

The residuals were then cumulated through time to form cumulative residuals, CR_{jt}

$$(2) \quad CR_{jt} = \sum_{t=-51}^0 e_{jt}$$

The cumulative residuals were then averaged over the sample of 22 banks to arrive at cumulative average residuals, CAR_{jt}

$$(3) \quad CAR_t = \sum_{j=1}^{22} CR_{jt}/22.$$

Each cumulative residual, as well as the cumulative average residual, was then tested to determine whether the return for that week was of unusual size. The t-test used is described in the Appendix, which also points out some methodological differences between our approach and the previous literature.

If the model adequately captures the determination of returns to the holders of a bank holding company's common stock¹ and if capital markets anticipate a downgrading of a bank's CAMEL rating, then cumulative residuals for these problem banks should become negative and remain negative prior to their examination dates, period 0. They should become negative at the time at which new, unfavorable, information about a bank's future earnings becomes known.

Results

The cumulative average residuals for the group of problem banks, and their respective t-statistics, are presented in Table 1. The table shows that the residuals are consistently negative throughout all but one week of the 52-week forecast period. They are, however, too small to be statistically significant. The analysis of the residuals on a company-by-company basis revealed that in the group of 22 problem banks,

Table 1
Cumulative Average Residuals and T-Statistics

Weeks Prior to Exam	22 Problem Banks	
	Cumulative Average Residuals	t-statistics
51	-.00735	-.18547
50	-.01251	-.22320
49	-.00492	-.07163
48	.00128	.01612
47	-.02684	-.30285
46	-.02787	-.28713
45	-.03384	-.32277
44	-.05264	-.46965
43	-.05733	-.48223
42	-.05451	-.43497
41	-.06055	-.46070
40	-.04877	-.35530
39	-.03747	-.26226
38	-.03761	-.25364
37	-.04655	-.30327
36	-.04096	-.25841
35	-.04732	-.28959
34	-.03925	-.23347
33	-.04152	-.24037
32	-.03668	-.20697
31	-.03936	-.21676
30	-.03212	-.17278
29	-.01526	-.08031
28	-.01340	-.06902
27	-.01965	-.09918
26	-.02852	-.14114
25	-.03173	-.15409
24	-.05288	-.25217
23	-.06013	-.28176
22	-.05378	-.24777
21	-.04837	-.21921
20	-.05479	-.24443
19	-.05564	-.24440
18	-.06268	-.27128
17	-.06000	-.25591
16	-.07406	-.31149
15	-.06235	-.25865
14	-.06185	-.25318
13	-.07050	-.28485
12	-.05699	-.22738
11	-.06810	-.26837
10	-.08850	-.34458
9	-.09579	-.36863
8	-.09138	-.34763
7	-.07809	-.29375
6	-.09328	-.34704
5	-.08409	-.30951
4	-.07653	-.27875
3	-.08006	-.28862
2	-.06485	-.23143
1	-.07412	-.26191
0	-.08789	-.30756

the cumulative residuals were consistently negative in only 12 cases. They were generally positive in the other 10 cases, suggesting no systematic capability of detecting problem banks prior to an examination using only security returns.²

To determine if the 12 banks where the market appeared to have anticipated the downgrade had any special characteristics, the study looked at their geographic location and the timing of their downgrade, as well as the specific events surrounding their deterioration. The 12 banks that appeared to have been singled out by the market were not concentrated in any particular location or time period.³

Further, *The Wall Street Journal* Index was searched for any mention of these 12 banks during the time period when they had significant negative residuals. Presumably, the market needs specific events reported in the media in order to react. The results of this exercise were surprisingly unrevealing. Two banks did not rate any mention in the Index at all. Two other banks had downgrades of commercial paper and subordinated debt, in one case because of a "reduction in flexibility due to an acquisition and troubled energy loans." Five banks reported either a loss or an expected loss for the quarter or year in question, in one case because of bad real-estate loans. Two banks revised earnings downward to include a charge-off, but were still profitable. Finally, one bank was put on Standard & Poor's Credit Watch, two weeks before its examination.

In view of subsequent problems due to the poor credit quality of energy and real-estate loans and, of

Little news of lending problems appeared before the banks were downgraded by examiners.

course, with the benefit of hindsight, the paucity of reported news of these problems is rather striking. There appeared to be little appreciation of the importance of these factors in the news coverage before the banks were downgraded by examiners.

Next, to determine if the managers themselves were aware of the deterioration in their banks' condition before the examination, we studied the pattern of insider transactions in the six months before the downgrade, as reported on the Security and Ex-

change Commission's "Official Summary of Security Transactions and Holdings." If the managers were aware of the problem, we would expect to find a pattern of insider stock selling during the time of deterioration of the loan portfolio.

In fact, the opposite seemed to be the case. Of the 12 banks with the negative residuals, only three cases showed a clear selling pattern. Even in these three cases, moreover, only one was by an officer, while the other two were by outside directors, who are not true insiders with access to detailed relevant information.

Of the other nine banks, six had a clear pattern of purchases of stock, while three more had a mixed pattern of both buying and selling. It appears that even the managements were not aware of the magnitude of the deterioration of their loan portfolios until the downgrade during the bank examination. In view of this, it is hardly surprising that the stock market was not able to predict bank problems.

Control Sample

Residual analysis was also performed on a control sample of non-problem banks. The sample consisted of 15 bank holding companies that are members of the Keefe-Bruyette Bank Stock Index. These companies, for which stock price and examination data were available, have lead banks that maintained composite CAMEL ratings of 1 and 2 between 1981 and 1987.

For these banks the study followed an estimation procedure similar to the one used for problem banks, with one difference. This group had no "event"; they had not suffered a decline in their CAMEL ratings. Therefore, the reference date selected (period 0) for the control banks was the date at which an examination began, one in which no change was made in the banks' composite CAMEL ratings of 1 or 2. As for the problem bank sample, parameter estimates were generated using data from periods -103 to -52, and residuals were computed by comparing actual to forecast returns in periods -51 to 0. We found that cumulative residuals were negative for about one-half of the individual banks, and were positive for the other half, about the same split as among the problem-bank group.

The cumulative average residuals and their respective t-statistics for the control sample are presented in Table 2. The cumulative average residuals for the control group are positive, but not statistically

Table 2
*Cumulative Average Residuals and
 T-Statistics*

Weeks Prior to Exam	Control Group	
	Cumulative Average Residuals	t-statistics
51	.01297	.40828
50	.02357	.52485
49	.02161	.39288
48	.02916	.45913
47	.03269	.46044
46	.04719	.60672
45	.05598	.66633
44	.05164	.57493
43	.06255	.65663
42	.07230	.71997
41	.06972	.66197
40	.07090	.64451
39	.07670	.66990
38	.07682	.64652
37	.09224	.74998
36	.08108	.63832
35	.06595	.50373
34	.05592	.41503
33	.03736	.26991
32	.04654	.32770
31	.04146	.28492
30	.06642	.44594
29	.07505	.49280
28	.08200	.52712
27	.08377	.52760
26	.07224	.44613
25	.06732	.40797
24	.07590	.45170
23	.09125	.53360
22	.07988	.45926
21	.08535	.48274
20	.09553	.53179
19	.09078	.49763
18	.08129	.43901
17	.07121	.37904
16	.06199	.32535
15	.07241	.37486
14	.07922	.40468
13	.06740	.33986
12	.07694	.38309
11	.07769	.38210
10	.08058	.39155
9	.07448	.35768
8	.08615	.40899
7	.09695	.45512
6	.08935	.41485
5	.09456	.43436
4	.08966	.40751
3	.09650	.43413
2	.09727	.43318
1	.09441	.41633
0	.08234	.35959

significant. A consistent trend of positive residuals is somewhat puzzling because these residuals should cluster around zero if the model is representative of the return process.

Conclusion

The results of this study offer no reason to believe that the prices of bank holding company stocks can be monitored to improve the supervision of commercial banks. This is true both for the sample as a whole and for individual banks. In the sample, only 12 out of 22 problem banks had negative cumulative residuals. Moreover, neither the market nor the management of these banks seemed to be aware of the impending problems before the examinations took place. These results cast serious doubt on the supposed advantages investors, and particularly uninsured depositors, would have over bank regulators in restraining risk-taking by banks and in monitoring their management.

Appendix

T-statistics

The t-statistics for the individual cumulative residuals and for the cumulative average residual in each week of the prediction period (weeks -51 to 0) are calculated as:

$$(1A) \quad T_{cr} = CR_{jt} / \sigma_j(e_{jt}) \nu \tau$$

and

$$(2A) \quad T_{car} = CAR_t / \sigma(e_{jt}) \nu \tau$$

where $\tau = t + 52$, $\sigma_j(e_{jt})$ is the standard deviation of the individual (non-cumulative) residuals for bank j over the estimation period (weeks -103 to -52), and $\sigma(e_{jt})$ is the standard deviation calculated globally over individual residuals of all banks over the estimation period.

The Market Model

The one-factor market model is usually estimated for residual analysis in the form

$$(3A) \quad R_{jt} = a_j + b_j R_{mt} + u_{jt}$$

where R_{jt} is the return to holders of security j at time t , a_j and b_j are parameters of the one-factor market model (the latter representing the security's beta, or systematic risk coefficient), R_{mt} is the return on the market portfolio at time t , and u_{jt} is the error term.

It has been argued in the literature that the market model estimated for individual securities has low predictive power and that its parameter estimates are unstable. The estimates may also be biased by industry effects, in addition

to reflecting the financial conditions of individual firms. These problems may be overcome by estimating the market model for a portfolio of securities in the same industry in the form

$$(4A) \quad R_{pt} = a_j + b_j R_{mt} + u_{jt}$$

where R_{pt} is the return on the industry portfolio.

Pettway (1980) and Pettway and Sinkey (1980) estimate the market model for a portfolio of banks constituting the Keefe-Bruyette Bank Stock Index. This created another problem, however, in that returns for failed banks are forecast using parameters estimated for non-problem banks making up the Index. This assumes that the systematic risk associated with owning a problem bank stock is the same as with owning a portfolio of non-problem banks. If this assumption is not valid, then significant negative residuals would reflect a systematic market bias against the sample banks, rather than a change in market perceptions anticipating a rating downgrade.

This problem is somewhat less severe if one uses a portfolio of problem banks in the sample in place of the healthy-bank portfolio. This still assumes, however, that the risk associated with a portfolio of problem banks is the same as that of owning a stock of an individual problem bank. If the risk of owning an individual bank is, in fact, greater, this would overstate the ability of the market to anticipate a rating downgrade.

We have made portfolio estimates for our sample of problem banks, as well as individual estimates. The results are essentially the same, and the main conclusions do not change with the estimation method. Table 1A presents the comparison of the cumulative residuals for the 22 problem banks and their t-statistics, both for the individual banks and for the portfolio of problem banks.

Table 1A
Cumulative Residuals and T-Statistics

Bank	Individual Estimation		Portfolio Estimation	
	Residual	t-statistic	Residual	t-statistic
1	.13275	.81472	.18576	1.12400
2	-.40778	-2.13564	.05900	.32210
3	-.07459	-.35588	-.01384	-.08410
4	-.50603	-1.52030	-.15765	-.88430
5	-.10740	-.29630	-.16974	-.83160
6	-.53804	-1.48552	-.23951	-1.26910
7	-.39697	-1.14377	-.26617	-.95360
8	.55838	2.30513	.13587	.74237
9	.10273	.34828	.21180	1.33431
10	-.79960	-1.49212	-.53860	-2.94090
11	.62207	2.34198	.43046	2.29336
12	-.21925	-.71756	-.45996	-2.75560
13	.04934	.15597	-.08600	-.46990
14	-.27526	-.99084	.03078	.16358
15	.13350	.35946	-.11782	-.64080
16	.40647	.80114	.23294	1.26960
17	.17811	.71027	.25905	1.41528
18	-.07478	-.22097	-.13167	-.71940
19	-.33833	-1.29929	-.21583	-1.09580
20	.21493	.47393	.03061	.16060
21	.21785	.71595	-.33628	-1.86630
22	-.81171	-2.00411	-.28258	-1.20250

¹ The power of this model is somewhat weakened in the case of bank holding companies, where the put option value of deposit insurance might mitigate the effect of a downgrade on bank stock prices, because the insurer shares the losses associated with a worsening of a bank's portfolio.

² The choice of the forecast period, or "event window," is necessarily arbitrary, since banks could have realized negative returns at different times before their examinations. If the event window is too short, and the banks realized negative returns more than a year before the downgrade, our results would fail to show it.

On the other hand, making the event window longer increases the standard error of the cumulative abnormal returns and dilutes the power of the test. If the one-year event window is too long, and the banks' financial condition deteriorated at a time closer to the examination, then the large standard errors may be responsible for the lack of significance of the results. To check against this possibility, 26-week and 13-week forecast periods have been tried. The results remained essentially unchanged, with only one-half of the problem banks having consistently negative cumulative residuals.

³ Nine of the 22 problem banks in the sample were located in the Southwest (seven in Texas and two in Oklahoma), reflecting the fact that many problem banks during the period in question were suffering the consequences the oil slump brought to their borrowers. The market did not appear to be capable of anticipating problems at the Southwestern banks any better than in other regions of the country—six of the 12 banks with negative residuals were located in the Southwest. Nor did the market seem to become

aware of developing problems at a particular time—five of the 12 banks with negative residuals were downgraded from 1981 through 1983, and seven from 1984 to 1986. Of the 10 banks without negative residuals, five were downgraded before 1984, and five between 1984 and 1986.

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