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WHAT ECONOMISTS SAY (AND DON'T SAY) ABOUT POLITICS

ABSTRACT: *Sam Peltzman has brought discipline and common sense to economic analyses of voting and representation. Yet his approach suffers, like that of other economists, from disciplinary provincialism and a singular devotion to econometrics as a research methodology. Political science offers alternative models and research methods that can enliven and deepen the political analyses of Peltzman and other economists.*

In *Political Participation and Government Regulation* (Chicago: University of Chicago Press, 1998), a collection of ten papers authored over 17 years, Sam Peltzman displays the power of economic inquiry at its best, sprinkled occasionally with inadvertent disclosures of its profound weaknesses. Peltzman, who followed George Stigler in pioneering the fields of regulation and modern political economy, offers formal theory and empirical evidence that seems to contradict both the belief that voters are largely ignorant and the simple "capture theory" of economic regulation. His research suggests that voters not only are well informed about the macroeconomy, but are able to discern the extent of legislators' responsibility, and are willing to reward or punish them based on that knowledge. Peltzman has also modified the growing economic theory of regulation (Becker 1983; Jordan 1972; Stigler 1971; Stigler and Friedland 1962) by illuminating the likely tendency of regulators to spread benefits among both producers and consumers. Peltzman has

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also been vigilant in focusing attention on weaknesses in the tradition he helped begin, including phenomena that his own research cannot satisfactorily explain.

In the realm of voter rationality and political representation, Peltzman stepped into a debate among economists that it would not be unfair to characterize as both methodologically flawed and inexcusably parochial. I will elaborate on the latter charge below. Methodologically, as Peltzman noted in "Economic Conditions and Gubernatorial Elections" (1987, ch. 3), for example, early research claiming that voters take only very recent economic conditions into account when evaluating U.S. presidents either suffered from too small a sample size, or included data from early years when government control over the macroeconomy was negligible (70–71). Research into political representation of constituents was worse; most economists proceeded as if all residents of a congressional district support their representative, so they used district-wide averages of income, education, and other demographic variables to represent constituent interests in their econometric models, failing to distinguish between a legislator's supporters and opponents. Recognizing the absurdity of this approach, Peltzman did research showing that constituents' economic interests were significantly less influential on congressional votes than had been thought.

The effort to introduce political realism into economic analyses of political relationships is characteristic of Peltzman's approach. He writes in the introduction:

> My interest in the working of the political process arose out of my earlier work on regulation. This had convinced me that the economic analysis of any government activity, whether it be regulation or something else, like the size of a budget, could not be separated from analysis of politics. So I felt the need to delve more deeply into how the primary participants in the political process—the voters and their representatives—made their decisions. (xv.)

In pursuit of political realism, Peltzman is just as reliant as his brethren on formal models and econometric analysis of data, but the papers in this volume evidence a theoretical (and hence methodological) precision that, he rightly suggests, distinguishes his work from predecessors who applied economic models to politics. A 1984 paper reprinted here, for example, entered a debate (largely among economists) about whether legislators are motivated more by constituent economic interests or by their own ideologies (i.e., whether they are "shirking" their responsibilities as representatives).[1] The conclusion reached by most, after econometric comparisons of variables taken to represent both influences, was that ideology matters more (Kalt 1981; Kalt and Zupan 1984; Kau and Rubin 1979; Mitchell 1979; Nelson and Silberman 1987).[2] Peltzman injected realism and common sense into this debate by developing a formal model of constituent representation that distinguished between the characteristics of average voters, supporters, and campaign contributors in a congressional district, and assessed the effects of each on a legislator's roll-call votes. Whereas earlier work had attributed a sizable impact on congressional voting to the ideology of individual members of Congress, Peltzman found that legislator ideology all but disappeared as a determinant of votes in every arena except domestic social policy, drowned out by variables purported to represent constituent economic interests.[3]

One criticism of this approach is that it uses at best a very crude measure of *how much* a given bill serves the interests of constituents. Imagine that we could rate bills on a 200-point scale in terms of how much each serves constituent interests, with -100 reflecting the greatest harm to constituent interests, and 100 reflecting the greatest benefit. Treating roll-call votes as dichotomous (i.e., either benefiting or not benefiting supporters) doesn't help us discern the relative importance of various votes. If the Congress routinely turns out bills that deserve a ranking of "1," for example, legislators will still be evaluated as serving the economic interests of their constituents so long as, when voting on a "1" bill, they vote "Yes." If a legislator votes for three "1's" and a "-100," he is coded no differently than the legislator who votes for three "100's" and a "-1." In other words, assessing whether a legislator's roll-call votes are congruent with the economic interests of his supporters may leave one vulnerable to mistaking crumbs for a full meal (on this point see also Stigler 1972).

A deeper investigation of the extent to which legislators serve constituent interests would probably involve some distinctly "non-economics" methods, but not necessarily. One could assess legislative proposals according to their advantage to constituents, and then correlate this rating with their observed likelihood of passage. Even this would depend on legislators introducing bills in the first place, but it would move one closer to a meaningful assessment of constituent service through legislation.

If one were willing to abandon econometric analysis for a moment, one could interview and observe legislators, their staff members, interest-group leaders, presidential aides, journalists, and other informed political elites in order to assess the extent to which legislators weigh the interests of their constituents when making decisions. This is what political scientists were doing, in fact, at least five years before the debate began among economists. While Peltzman rightly corrected economists who treated every district inhabitant as a constituent, his work was inaccurate as well; he identified only what appeared to be stable groups of supporters, based on economic demographics. The political scientist Morris Fiorina (1974) observed, on the other hand, that the collection of constituents to which a legislator must pay attention shifts from policy to policy. Salience varies across issues and constituent groups, meaning that legislators have the problem and opportunity of trying to please multiple, shifting constituencies (Fenno 1978; Kingdon 1977; Orfield 1975; Price 1978).

This observation by no means refutes Peltzman's finding that U.S. congressmen serve the economic interests of their constituents. But while Peltzman could say no more than that it appears that legislators vote according to the economic interests of a bloc of constituents, political scientists have painted a much clearer picture of precisely how legislators incorporate the conflicting interests of various constituent groups into their decisions, which include much more than roll-call votes. Their work illuminates a level of constituent-service skill among legislators that no econometric analysis like Peltzman's could ever reveal. The fact that this debate could go on among economists for ten years with virtually no reference to the work of people knowledgeable about the daily decision making of legislators reveals both disciplinary self-absorption and an excessive reliance on a single research methodology. These two tendencies have long threatened to make economics less a field of social inquiry than a coven of technical esoterica.

While the analysis in Peltzman's 1984 paper was as parochial as the economics literature in which it was embedded, his model of legislative representation proved more valuable in a second paper, "An Economic Interpretation of the History of Congressional Voting in the Twentieth Century" (1985) (ch. 2). In it he sought to explain changes in congressional voting patterns that appeared to be driven simply by regional ideology. He found that regional ideology indeed matters, but that while it was once constrained, it later became increasingly liberated by regional economic conditions. Peltzman concluded that while Southerners were more ideologically opposed to redistribution, they were also poorer than most Americans. According to Peltzman, this dampened the effect of ideology on congressional voting, as liberal Northerners faced a high price in voting for redistribution, while conservative Southerners faced a high opportunity cost for indulging their inclination to vote against it.[4] As the vast income gap between North and South at the end of the Civil War slowly declined, however, Northern voters (from whom relatively less was therefore taken for redistribution to the South) found that the price of electing the liberals they preferred also declined, while the price for Southerners (who were decreasingly recipients of redistribution) increased. Thus, while 100 years ago the liberal tendency of Northerners and the conservative tendency of Southerners were attenuated by their countervailing economic interests, changes in income increasingly reinforced their ideological differences, leading Peltzman to speculate that "regional political differences will *grow* in the future even as the economic element of these differences diminishes" (66, emph. original). Some evidence appears to favor this prediction: there are noticeable differences between white Northerners and Southerners on economic and social matters, while the percentage of Southern whites registered as Democrats has declined since the 1950s (National Election Studies 1995–98).[5] The latter datum has stabilized since Peltzman made his prediction, but so has the per-capita personal-income gap between Southern and New England states (U.S. Department of Commerce 2000).

Beyond the immediate question of Peltzman's prediction is his assumption, reflected here and in other papers, that ideology and perceived economic interest can be separated. I take this up below.

The Economics of Voter Rationality

Peltzman's recent volume contains three papers that address the question of voter rationality: namely, whether citizens properly reward or punish elected officials. "Properly" here means that they act as if they are aware of social conditions, legislators' actions, and the connections between them, and reward or punish legislators accordingly. (The phrase "as if," I will argue, is a powerful tool both for theorizing and for misunderstanding.) For economists, including Peltzman, the subject of this question is quickly narrowed from citizen to voter, and from all preferences to selfish economic preferences: does each voter accurately reward or punish elected officials for economic outcomes that benefit or harm him?

Peltzman overcame the small-sample problems of earlier works, in one case (1987) by examining data from large U.S. gubernatorial elections, and in others by examining gubernatorial and senatorial returns along with each party's share of votes in presidential races. He also distinguished between incumbents and challengers as a means of controlling for the political advantages held by the former. Unlike previous scholars, he found that voters seemed surprisingly knowledgeable, that they had longer memories than had been thought, and that they were capable of distinguishing between local and national policy effects, between policy-induced and exogenous income changes, and between expected and unexpected inflation. He noted that in the years 1964, 1976, and 1980, income and inflation appear especially significant as predictors of marginal voter behavior compared to other years, many of which reveal little correlation between economic variables and voting. In a 1992 paper Peltzman also determined that since 1950, U.S. voters had penalized federal and state spending growth, and that the type of spending (e.g., welfare versus highways) had affected the extent of electoral punishment.

Readers familiar with the topic of voter rationality will recognize the tangle of problems awaiting anyone attempting to define "rational," let alone trying to determine whether voters behave rationally. It is a credit to Peltzman's insight that he avoided many of these problems, apparently by reason alone, since they have largely been discussed in political science works that he does not cite. It is easiest to understand the traps awaiting research like Peltzman's by noting two essential assumptions about voter preferences underlying his model of voter behavior: that voters have the knowledge to act on their interests, and that voting is determined largely by economic self-interest. To these can be added the assumption that legislators seek to maximize their share of the electoral vote.

A large body of research undermines these assumptions, and before going forward, it is helpful to consider why this matters. The value of any empirically testable model as an explanatory tool[6] depends both on its conformity to observed outcomes and—if plausible competing hypotheses (i.e., hypotheses that predict a significant portion of observed outcomes) exist—on the tenability of its assumptions compared to those incorporated in the alternative hypotheses. If two hypotheses equally predict some set of outcomes, but the first is based on demonstrably untrue assumptions while the assumptions of the second are demonstrably true, it is logical to favor the second as the most correct explanation of reality on offer. As we shall see, there are plausible hypotheses that can explain voting and election outcomes just as well as Peltzman's research, yet they rest on very different assumptions about political actors' motivations and capabilities. Therefore it is helpful to consider the viability of Peltzman's assumptions before comparing them to the assumptions underlying alternative models of voter and legislative behavior.

There is considerable evidence that voters are largely ignorant of even basic information about political and economic conditions, let alone their legislative actions, contrary to Peltzman's first assumption. When only a minority of American citizens can name their congressman, and fewer than 40 percent can name both of their senators, the belief that voters are capable of assessing individual legislators' contributions to income gains and unexpected inflation appears farfetched (Erikson, Luttbeg, and Tedin 1988). One can also question whether voters have coherent belief systems (Converse 1964) that allow them to integrate the information they do have into a set of preferences (i.e., the relative value of inflation, job growth, crime, etc.) that can rationally and consistently inform their voting.

Peltzman's second assumption is that voters are rational, self-interested maximizers of personal resources who vote for the candidates they think likeliest to increase their economic well-being. In his 1985 paper Peltzman went so far as to write that "constituents from high-income, manufacturing-intensive, and urban areas have *generally* been asking for opposition by their congressmen to expansion of the federal budget for at least the last sixty years" (44, emph. original). He based the claim that they are *asking* for spending restraint not on survey evidence or interviews with congressional staff, but on a statistical analysis suggesting that voters with these characteristics are better off economically if they retain their tax dollars rather than seeing them go to federal spending.

For economists, it is a small step to assume that voter preferences can be deduced from a reading of narrow economic interests. Yet many political scientists have offered models, econometric analysis, and qualitative research that brings this assumption into question. To be sure, political scientists agree that economic conditions affect voter behavior. But their inquiries have been, in fact, much richer than the description provided by Peltzman in this book, where he sets it up primarily as a question about which macroeconomic variables matter and how long voter memories are. Political scientists have also studied, for example, the extent to which voters evaluate candidates based on their perceptions of national economic conditions ("sociotropic" voting—see Kinder and Kiewet 1979 and 1981; Markus 1987 and 1992; Meehl 1977) rather than their individual economic condition. They have found that other things matter as well. Voters have preferences for public goods (Hawthorne and Jackson 1987), for example, which are not entirely predictable by their income, and which affect legislative behavior (Jackson and King 1989). And at a root level, their ideologies and economic interests are so intertwined that attempting to isolate one from the other moves the theorist away from a realistic model of political decision making (Kingdon 1989) and introduces the likelihood of deeply flawed conclusions (Jackson and Kingdon 1990).

What is more, evidence from voter surveys suggests that beyond supporting legislators solely because of their policies, constituents may support them at least in part because legislators are skilled at manipulating symbols (Sears, et al. 1979 and 1980), or because they share the same party identification. In a rich treatment of voter behavior, Angus Campbell, et al. (1960, 67–76) demonstrated that political party identification among voters in the United States

> is a psychological identification . . . [that] raises a perceptual screen through which the individual tends to see what is favorable to his partisan orientation. The stronger the party bond, the more exaggerated the process of selection and perceptual distortion will be.

Campbell and his colleagues found that voter perceptions of economic conditions certainly matter, but not independently of other factors.

To see how the phenomenon of party identification—well known to political scientists but largely ignored by economists—can create problems when the latter seek to explain voter behavior, consider some of the variables Peltzman uses to represent constituent economic interests in his 1984 and 1985 papers: education levels, urban residence, and the percentage of the population that is black. Families are likely to be similar between one generation and the next on the first two variables, and are all but guaranteed to stay the same on the third. This is relevant because when children inherit their parents' characteristics on these dimensions, they are also likely to inherit party identification. In other words, just as education levels are positively correlated across family generations, so is party identification.[7] Thus, when we find that the aforementioned demographic variables in a Congressional district help predict a legislator's votes, a simple regression model will not tell us whether voters are acting on their economic interests as reflected in these variables, or whether they are simply acting on a party identification acquired from their parents. Therefore we can't know, without deeper investigation, if the legislator is representing supporters, or if both legislator and supporters share a common ideology that may or may not promote the supporters' interests.

Beyond the question of whether voters have the knowledge and inclination to act on economic self-interest is the question of whether legislators respond to this self-interest in an effort to maximize votes—the third of Peltzman's assumptions. Political scientists who have interviewed and closely observed legislators have found otherwise. David Mayhew (1974) discovered that congressmen seek to win re-election comfortably, in order to avoid showing weakness to potential competitors, but that they do *not* try to maximize their electoral vote; rather, they engage in "minimax" behavior by trying to avoid costly (from an electoral standpoint) voting decisions while sustaining the coalition that put them in office. Richard Fenno (1973) found that Congressmen have three goals: get re-elected, achieve influence within Congress, and make "good" public policy. The last two goals certainly create complications for the vote-maximization assumption, because they indicate that legislators may make decisions that are suboptimal in terms of potential vote gain, but optimal in pursuit of influence-building or doing "the right thing."

Cognizant of the inherent weaknesses of the first two assumptions, Peltzman wisely tailored his central questions accordingly. In his 1990 and 1992 papers he recognized the potential importance of party identification and focused on the marginal voter who is, "on other grounds, essentially indifferent between the two parties and uses macroeconomic information to choose between them" (81). Attempting to isolate marginal voters both narrowed and strengthened Peltzman's findings, muting criticisms like those leveled above but restricting his findings to at most 20 percent of the electorate.

Retrodiction vs. Political Analysis

Although there are reasons to question the assumptions underlying Peltzman's models, one might argue in defense of any model that it should be judged by its predictive (or retrodictive) ability. In other words, "the proof is in the pudding." Milton Friedman articulated the claim that the positive economist develops theories that are to be judged solely on their predictive accuracy, not the supposed realism of their "assumptions" (a term that, he argued, misleads us about the nature of hypotheses). If the economist's predictions hold true, then it is "as if" the hypotheses underlying the model are true. A critique of the model's assumptions, then, is beside the point:

> The relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic," for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. (Friedman 1953, 15.)

By this standard Peltzman is on solid ground, and he is well aware of the problems with assuming a real-world connection between a legislator's ideology and a voter's economic self-interest. For example, he notes (16), regarding the findings in his 1984 paper, that

> these results should not be interpreted to say that [supporter] interest rather than [the legislators'] political kinship really determines [legislative] voting patterns. There is much collinearity between party affiliation and the characteristics of supporters and contributors, perhaps too much for such a conclusion to be confidently drawn.

What matters, he argues, is the ability of the economic model to predict outcomes: does the world function as if the assumptions are in fact true? His econometric results led him to conclude that it does:

> The results do imply that economists unfamiliar with the workings of party loyalties can proceed *as if* such things did not matter and focus instead on who the constituents are and where the campaign funds come from. (Ibid., emph. original.)

This is a reasonable argument in context because the question addressed by Peltzman's 1984 paper is whether constituents' economic interests are being served by legislators' votes—regardless of whether legislators respond to constituents, or whether constituents respond to legislators, or whether constituents vote for other reasons and accidentlly have their economic interests served anyway. In answer to this question, his findings were powerful, especially in light of the argument accepted by other economists: that legislators were shirking their duties as representatives.

Neither Peltzman's 1984 paper, however, nor any of his other work discussed above, should be mistaken for the "analysis of politics" that he claims in his introduction to this volume to have undertaken. Political analysis implies an attempt to understand what drives political behavior, and by this measure the work of Peltzman and other economists is unsatisfying. Peltzman found that voters and legislators appear to behave as if certain assumptions fundamental to an economist's view of politics hold true. This is not so much a confirmation of this model of political behavior as a finding, based on an extremely limited research methodology (econometrics alone, without surveys or interviews), that the assumptions haven't been *disconfirmed*.

I claimed earlier that, if one's goal is not simply prediction of behavior but also explanation of its motivations, then competing hypotheses equally capable of explaining observed outcomes should be compared according to the tenability of their assumptions.[8] So far I have criticized Peltzman's assumptions without offering a plausible alternative theory. Economists are often unfairly treated in this manner by other social scientists, who assume that establishing the unrealistic nature of an assumption is equivalent to debunking the research on which it is based. Of course this is untrue: prediction has value, as does a clear model that provides understanding not directly, but by virtue of extreme assumptions that, when weakened, help one understand why actual events diverge from those predicted by the model.

Friedman rightly rebuked noneconomists who content themselves with attacking the unrealistic assumptions of economists without providing "evidence that a hypothesis differing in one or another of these respects from the theory being criticized yields better predictions for as wide a range of phenomena" (1953, 31). Friedman's wording, however, suggests that competing theories would not be judged by many economists as yielding "better predictions for as wide a range of phenomena" unless they were testable econometric models. The problem with this requirement is that it anchors one's field of belief and knowledge to the availability of large data sets that happen to be, not surprisingly, filled disproportionately with financial and demographic information. Thus the theorist who is partial to treating man as a rational economic maximizer, and who disregards any noneconometric evidence to the contrary as "unscientific," surrounds himself only with confirming evidence and rejects recalcitrant facts out of hand.

Fortunately there is a persuasive alternative theory of elections that was advanced by political scientists seven years before Peltzman's 1990 paper; that explains observed electoral outcomes just as well as, or better than, Peltzman's model; that is based on more realistic assumptions about human motivation; and that is empirically testable. Developed by Gary Jacobson and Samuel Kernell in 1983 and substantiated with additional data by Jacobson in 1997, the theory is that legislative votes and noncatastrophic economic conditions do not affect voter decisions so much as they affect decisions by potential candidates about whether to run for office—especially since the 1970s, with the decline of political parties and the rise of candidate-centered elections.[9]

While polls reveal that voters are pervasively ignorant about the economic issues that Peltzman believed they were incorporating (or acting as if they were incorporating) into their voting decisions, potential challengers and powerful supporters pay considerable attention to (political and) economic conditions. When an opponent appears weak, either due to political missteps, a voting record that can be easily attacked, or poor economic conditions, strong challengers (those who are relatively experienced or skilled and already possess high public visibility) are more likely to throw their hats into the ring. Likewise, influential supporters are more likely to invest heavily in helping such a challenger win.

This is significant when combined with survey evidence offered by Jacobson and Kernell suggesting that voters make decisions between pairs of competing candidates based more on their campaign abilities and personal attributes than on voters' evaluation of their ability to affect macroeconomic conditions. Not surprisingly, Jacobson and Kernell found that when economic conditions are poor, more experienced challengers with better financing emerge. Econometric analysis designed to assess the impact of economic conditions on voting, especially when controlling for the natural advantage of incumbents, will therefore suggest that voters are making decisions based on their evaluations of economic conditions, even if what is really happening is that economic conditions are driving the quality of challengers, which in turn drives voter decisions.

To test this hypothesis, Jacobson (1997, ch. 6) used regression analysis to distinguish the effects that presidential party and approval ratings, real per-capita income changes, and the electoral quality of a challenger have on changes in the political parties' share of seats in the

U.S. House won in nonpresidential elections. His measure of a challenger's quality was whether she had previously held elective office, under the assumption that this is a suitable proxy for campaign skills, connections to influential supporters, charisma and speaking ability, and so forth. Jacobson found that the difference attributable to income change is three times higher when challenger quality is excluded from the equation; when it is included, the effect of income change is indistinguishable from zero.[10] The more fully specified equation revealed that every percentage point of difference between Republicans and Democrats in terms of challenger quality yielded a shift of 1.8 House seats. In short, Jacobson's analysis indicates that challenger quality is a better predictor of election outcomes than changes in per-capita income. All the better is the fact that this model provides an explanation of its predictions that does not amount to the a-priori assertion that all people everywhere are maximizers of their self-interest.

Perhaps the most interesting observation that arises from a comparison of Peltzman with Jacobson and Kernell is their congruence at the macro level. Both models suggest that somehow, legislators are rewarded for good economic performance and punished for bad economic performance.[11] Peltzman believes that knowing that voters behave *as if* they are aware of legislators' impact on economic conditions helps close the theoretical gap created by the fact that "we do not know why [economic conditions] matter or how plausibly to characterize the process by which voters translate information about economic conditions into voting decisions" (78). At the same time, he acknowledges at the end of his 1990 paper that his analysis "deepens the mystery" surrounding the picture of voters who are instrumentally rational, yet who gather—in what can fairly be described as an irrational act, given the individual's odds of influencing an election outcome—extensive information about candidates.

If one wants to know how the electoral accountability Peltzman uncovered actually comes about, it appears that economic theory is not as helpful as an awareness of what voters, candidates, and supporters actually know, and how they factor their knowledge (or lack of it) into their decisions. In this case Peltzman's "as-if" answer is not as illuminating as Jacobson's empirical analysis:

> The choice between pairs of candidates across states and districts . . . varies systematically with the strategic decisions of potential candidates

> and associated activists. These decisions are . . . informed by perceptions of national political and economic conditions. Voters need only respond to the choice between candidates and campaigns at the local level to reflect, in their aggregate behavior, national political forces. Pervasive individual [voter] rationality . . . is not essential for the process to work. The intervening strategic decisions of congressional elites provide a mechanism sufficient to explain how national forces can come to be expressed in congressional election outcomes. (Jacobsen 1997, 135.)

The Monologue of Economics

Throughout this volume Peltzman's ingenuity in both model construction and data analysis is apparent, yet his improvements upon previous economic inquiries also serve to illuminate more glaringly what seem to be fundamental flaws in the entire enterprise of the economic analysis of politics. These flaws are symptoms of a disciplinary self-absorption among economists that is rightly perceived by outsiders as antithetical to intellectual inquiry and as baselessly arrogant. An excerpt from Peltzman's 1984 paper is illustrative:

> Suppose an economist initially seeks to explain auto purchases with two variables—price and party registration—and he finds that party is clearly the more important of the two variables. An economist, *unlike a sociologist or a political scientist*, would probably suspect that party is simply a proxy for income. (15, emph. added.)

Of course this example is loaded. The real question is how an economist will approach not a universally understood transaction, but instead an activity surrounded by multiple contexts and a variety of plausible actor motivations. Given the work of Peltzman and other economists in the area of political analysis, it appears that in the latter situation an economist, unlike a sociologist or political scientist, would: (a) consult little literature on this activity not written by other economists; (b) assume a priori that the actors are driven almost exclusively by narrowly defined economic self-interest; and (c) prefer econometric analysis over simpler but time-proven methods of investigation, like *asking the people involved why they do what they do.*[12]

The difficulties with (b) should be clear from the aforementioned discussion. Regarding (a), in a volume containing ten academic papers on political participation, government regulation, and public policy, I found hundreds of references to economists, 14 references to work by political scientists, and none to work by sociologists or historians. If the examples I have discussed are not enough, consider the fact that political scientists had identified the crucial need to distinguish supporters from general constituents at least six years before the debate among economists on legislator "shirking" began, and 11 years before Peltzman weighed in.

As for (c), it is telling that none of Peltzman's work, nor the work that he cites, refers to surveys or interviews with the people whose behavior it seeks to explain. Economists have pioneered extremely useful methods of statistical analysis from which other disciplines have benefited greatly, yet they seem reluctant to use methods that have generated a wealth of information in adjacent fields. To take just one example of how much could be gained, consider how political scientist Gregory Markus (1987 and 1992) was able to separate the effects of personal from national economic conditions on voting by using National Election Study surveys. His work exhibits the econometric rigor dear to economists, yet it is based on more reliable data: namely, the reports of voters about why they voted the way they did. This is not to say that surveys by themselves are always definitive, but there is no reason to ignore them given their potential to illuminate the "preferences" that are supposed to motivate *Homo economicus*.

In order to model purposive behavior, a theorist must make assumptions (based on theory, empirical observations, or some combination of these) about the actor's preferences. Economists tend to model voters, legislators, and regulators as resource maximizers (with resources defined broadly enough to include votes in the case of legislators, and political support in the case of regulators). While no doubt a safe assumption in many contexts—according to the criteria of plausibility as well as relative predictive power—eventually the selfish-maximization assumption must confront alternative hypotheses. (Geoffrey Brennan and Loren Lomasky [1997], to take one example, have argued that an indication of rationality in a voter is the fact that he does *not* consistently vote for outcomes that are in his best economic interest.) While acknowledging the possibility of alternative preferences, Peltzman and other economists often hew to the resource-maximizing assumption anyway, despite the plausibility of alternatives and the availability of methods that can reveal political actors' preferences more directly than goodness-of-fit statistics in regression models.

Even after noting in his 1990 paper, for example, that the *Homo eco-*

nomicus model of voters is paradoxical (79), given the cost to a voter of voting and of obtaining information on legislator actions, coupled with the minuscule likelihood that her vote will affect the outcome, Peltzman is undeterred. In a triumph of empiricism over theory, the consistency of his econometric model with the assumption of well-informed voters appears proof enough for Peltzman that they indeed are well informed, despite the internal incoherence of that assumption. It appears that when faced with evidence that a fundamental assumption of his discipline is invalid, an economist, unlike a sociologist or a political scientist, will make the assumption anyway, so long as the final regression results do not disagree.

The Economics of Regulation

One way to look at the two primary topics captured in the title of Peltzman's book—political participation and government regulation—is to observe that "political participation" refers to behavior that takes place in the political realm (although it can be shaped by economic interests), while the "government regulation" in question takes place in the economic realm. This may explain why, even though Peltzman's analysis of political participation at many points rings hollow to the ears of a political scientist, his work on economic regulation appears on much firmer ground; indeed, it is probably not controversial to assert that his is among the pre-eminent research and analysis in this area.

To understand the depth both of Peltzman's analytical capacity and his contribution to the economic theory of regulation, it is helpful to read his papers in the second half of *Political Participation and Government Regulation* out of order. In two papers, "Current Developments in the Economics of Regulation" (1981) (ch. 8) and "The Economic Theory of Regulation after a Decade of Deregulation" (1989) (ch. 9), Peltzman provided lucid surveys of the literature that were refreshing changes from much work on the topic. As Peltzman observed in 1981 regarding theories of market failure (more below): "vague beliefs are now enshrined in jargon and clothed in formal models which give them the correct ritual flavor and exclude the uninitiated" (277).

In his 1981 paper Peltzman described the evolution of perspectives on government economic regulation as beginning with an early public-interest view (traceable at least to Adam Smith, as Peltzman noted in 1989) that inferred from market failures in some industries a genuine need for regulation. Two papers dramatically shifted this perspective: one by George Stigler and Claire Friedland (1962), in which they showed that electrical-utility regulation had not produced lower electricity rates; and a second by Stigler (1971) that provided a model of regulators as rational self-interested actors. Peltzman provided an insightful summary and critique of both papers in "George Stigler's Contribution to the Economic Analysis of Regulation" (1993) (ch. 10), where he credited Stigler's papers with spawning "the 'capture' view of regulation, whereby compact interest groups, usually of producers rather than consumers, were held to dominate regulatory decisionmaking" (272). However, Peltzman (325) observed,

> the main problem with this professional consensus [regarding market failure as the source of regulation] was that it had never been subject to empirical verification prior to 1962. The tendency of economists to accept without examination the effects of a wide range of government regulation was pervasive.

Thus, the capture theory began to confront problems, described by Peltzman in 1981 as "creeping realism" (272), as economists began to realize that complicating factors like mixed goals and implicit bargaining on the part of regulators have the potential to produce outcomes not predicted by the simple capture view (even if regulators are assumed to be self-interested maximizers). Peltzman pointed to work by other economists illustrating how regulations that are inefficient, according to contemporary theory, were in fact a natural outgrowth of the desire by regulators to build supporting coalitions (Leone and Jackson 1981). Others found evidence that some regulations benefiting consumers may yield net positive economic gains, such that regulation can be viewed as more than the reallocation of profits (Munch and Smallwood 1981). Though more complex than their progenitor, these modifications were still rooted in Stigler's rational-choice approach, yielding a growing body of work that has properly come to be called the economic theory of regulation.

Peltzman displays not only a thorough command of this theory's strengths and weaknesses, but a wonderful ability to ask precise questions designed to challenge its very foundations. Consider his comments (277–78) on the state of thinking among economists about market failures:

> This degree of concern for empirical relevance does not, however, seem to carry over to our treatment of market-failure issues. If someone today asserted that any substantial reduction in pollution would have trivial benefits, or that the resources spent in the name of pollution control had trivial effects on pollution, there would be no substantial concrete basis for laughing him out of court. For all we know, this regulation may be only [a] disguised form of entry control . . . or a WPA project for the suppliers of control equipment, or something else that would call for a fundamentally different analytical framework than we have so far brought to bear.

Indeed, it is not an exaggeration to characterize Peltzman's remarks in these papers as a gold mine of potential research topics. Regarding safety, for example, he recommends determining the extent to which prices vary with product quality (or pay with job quality) as a means of assessing how much the market values regulations in these areas. To determine whether "deadweight losses" (negative effects to some groups that are not offset by positive gains to others) would indeed be large without regulation, Peltzman suggests doing a comparative study of the activity in question across international jurisdictions. If the vast majority of similarly developed jurisdictions regulate this activity, that may be an indication, reasoned Peltzman, that large deadweight losses would otherwise accrue. If the record is spotty, this variation is itself an opportunity to actually measure the extent of deadweight losses.

Perhaps Peltzman's deepest contribution to the economic theory of regulation came in "Toward a More General Theory of Regulation" (1976) (ch. 6), which he describes in the forward to this volume as one of his most influential works (ix). Relying on the rigorous mathematical expression of his basic assumptions about the preferences and behavior of regulators, regulated groups, and outsiders, Peltzman set forth convincing arguments that by necessity government regulation, even when the regulating agency is dominated by regulated interests, must distribute some benefits to nonregulated groups. What is more, the regulator has incentives to be an arbitrator between producers and consumers, making adjustments based on market changes (such as a decline in demand or in production cost) so that the equilibrium distribution of benefits between the two groups remains stable. Peltzman's analysis implies that the capture theory misses important nuances in the political incentives for regulators that work in favor of consumers.

Another source of "creeping realism" was the widespread deregulation in the United States during the late 1970s, a trend that created problems that the economic theory of regulation, as it stood, seemed ill equipped to handle. In his 1989 paper Peltzman provided a model of regulatory entry and exit to address this shortcoming, which, although highlighted by deregulation, had been implicit in the economic theory of regulation all along. According to Peltzman, the two competing explanations for the formation of regulatory bodies, the public-interest theory and the economic theory (as it stood at the time), poorly predicted both the emergence and the elimination of such bodies. On the one hand, numerous regulations—such as many professional licensing requirements, trucking regulation, and several banking and finance restrictions—are demonstrably not, according to most economists, in the public interest (302). Peltzman (301) quipped:

> To be sure, a good economist needs no more than fifteen minutes' notice to produce a market failure to "explain" any of these interventions. But credulity is strained when the list of market failures grows at roughly the same rate as the number of regulatory agencies.

On the other hand, the economic theory had little success explaining why more industries weren't regulated—let alone why many once-regulated industries were deregulated in the 1970s—given its assumption of power-seeking regulators (299–300). In short, Peltzman concluded, the public-interest theory underpredicts regulation, while the economic theory overpredicts it.

Peltzman extended the economic theory to regulatory entry and exit by arguing that the condition likely to yield the creation of a regulatory body, "a wide discrepancy between the political balance of pressures and the unregulated distribution of wealth" (320), would eventually deteriorate as a consequence of technological change, economic competition, and the regulation itself, leading in many cases to an eventual end to the regulation. By dispersing excessive producer profits and hamstringing innovation, then, regulations may well carry the seeds of their own demise. Peltzman concluded by noting that the economic theory could not explain some American regulatory experiences, such as those in trucking and telecommunications. More significant was the apparent inability of this theory to explain both why regulators didn't seize some available opportunities to preserve regulatory rents (as predicted by a theory that treats them as self-interested maximizers), and why deregulation was the dominant option chosen by others in response to the erosion of producer profits.

Reconsidering Assumptions

Even regarding regulatory theory, however, Peltzman is vulnerable to the criticism that there are more plausible alternative explanations of the phenomena he has sought to explain. In his defense, Peltzman writes (303):

> Here I ignore these political factors, partly because economists have so far had limited success in pinning them down, but mainly because the more familiar terrain of the economic factors is sufficiently fertile.

For someone interested in whether the economic theory of regulation affords a reliable explanation of real-world institutions and events, however, alternative hypotheses may well be worth consideration when compared to Peltzman's and other economists' assumptions: that regulators seek to maximize political returns, and that fragmented government institutions have a negligible effect on the very concept of a "regulator" as a uniform and predictable actor.

The first assumption, for example, comes into play when Peltzman (176) tries to explain the cross-subsidization of higher-cost customers by lower-cost customers through flat pricing structures, a common tendency in a variety of regulated industries. Peltzman theorizes that, assuming he is a self-interested maximizer, a regulator would want to minimize interest-group opposition to his regulatory scheme. Relying on mathematical modeling, Peltzman goes on to demonstrate that the regulator would benefit most from cross-subsidization. An alternative hypothesis that also explains cross-subsidization, however, is that regulators genuinely seek to ensure public safety, or to spread the benefits of an industry to users who otherwise would be unable to afford them. One or both of these arguments have been advanced to defend airline, telephone, electricity, and drug regulation, as well as to oppose privatizing the U.S. Postal Service.

The unitary-regulator assumption is crucial because, in the economics literature, regulation is effected by a tight linkage between legislators, who seek to maximize votes, and bureaucrats—who possess a deep knowledge of the economics and structure of an industry. It is presumed that the bureaucrats either obey the instructions of legislators, or are curbed by legislators from acting in ways that might anger their constituents. Peltzman described this relationship in his 1976 paper (158):

> Though appointment of a regulatory body may lie effectively with a legislature, a committee thereof, or an executive, the electorate's receptivity to these intermediaries ought to be affected by the performance of their appointees.

A brief consideration of the differences between this model of the regulatory process and one informed by political science and sociology may be informative.

The first clarification, of course, is that in reality there is no such thing as the "regulator" found in the economics literature. There are at least four institutions whose officials, acting under very different norms, procedures, knowledge, and training, profoundly influence the shape of regulatory policy. Legislators, regulatory-agency officials, executive-branch officials,[13] and judges influence regulations in ways that vary across time and subject matter. Given their fragmented and often conflicting modes of influence, it is reasonable to ask both whether the theoretical regulator can be considered rational enough to match the economic theory, and whether her preferences are well formed enough to make maximization a meaningful concept. In short, if the gaps in and between knowledge, decisions, implementation, and the monitoring of regulations are wide enough, we may have reason to question the viability of the economic theory as a source of understanding, prediction, or prescription.[14]

As in the case of legislative elections, it is not enough merely to question the assumptions made by Peltzman and other economists. But, as in that case, there is at least one widely regarded work that does not rest on implausible assumptions. John W. Kingdon's *Agendas, Alternatives, and Public Policies* (1984) combines rich contextual detail with a modified version of Michael Cohen, James March, and Johan Olsen's "Garbage Can Model of Organizational Choice" (1972) to explain both gradual and drastic policy change:

> Three process streams [flow] through the system—streams of problems, policies, and politics. They are largely independent of one another, and each develops according to its own dynamics and rules. But at some critical junctures the three streams are joined, and the greatest policy

> changes grow out of that coupling of problems, policy proposals, and politics. (Kingdon 1984, 19.)

In Kingdon's account, problems rise to the attention and agendas of decision makers through several avenues, prompting as solutions various policy proposals. Many of these solutions exist *before* the problems to which they are ultimately linked (Kingdon offers as one example the fact that waterway user charges originated from the use of such charges in other transportation modes), whether because they have dedicated advocates or simply because decision makers tend to fall back on what they know. These streams interact in an environment of elections, macroeconomic performance, and self-interested behavior among bureaucrats and politicians. Successfully coupling solutions to problems most often requires that "policy windows"—highly publicized disasters, a change in political leadership, other opportunities—open, enabling policy entrepreneurs to act. Based on his interviews with numerous players in the public-policy field, Kingdon concluded that problems and politics set the governmental agenda, while policy activists produce the alternatives that are—eventually—matched to items on the agenda (ibid., 194).

Although many who are knowledgeable about politics and policy find Kingdon's description accurate, it does not offer precise predictive power. He acknowledges that his model has outcomes that "can be quite unpredictable."

> An administration proposes a bill, then is unable to control subsequent happenings and predict the result. Solutions become attached to problems, even though the problems themselves did not necessarily dictate those particular solutions. Thus a mine disaster sparks legislation not only for mine safety, but also for black lung disease. . . . Once the agenda is set, control over the process is lost. (Kingdon 1984, 177–78.)

An economist might look at this example and argue that, while the black-lung disease legislation wasn't itself predictable from the economic model,[15] one can predict that regulators and politicians will act to establish rents that increase their economic or political payoff once a policy window opens. This may well be true, and Kingdon's model seems useful as a means of predicting how, given such motivations, policies tend to come in waves. Policy windows create entrepreneurial opportunities, one might say, by rapidly changing the subjective values of "customers" (citizens, interest groups, business firms) and the information available to decision makers, much the way such opportunities emerge (or are created) in the economic marketplace.[16]

But Kingdon's model possesses even greater macropredictive power (for lack of a better term) than does the economic theory of regulation. While it is no better at predicting precise policy outcomes, the model offers global predictions, such as the claim that policy solutions will tend to float around among interest groups and administrative agencies for years, sometimes decades, before finally attaching themselves to a problem. Likewise, a problem (e.g., the deadweight loss resulting from some form of regulation) may go unsolved for years until the right combination of political actors arises. For instance, Kingdon argues —quite sensibly, except in the world of economists—that the appointments of successive chairmen of the Civil Aeronautics Board partial to deregulation, along with the departure of several members who favored regulation, was essential to reform. Sen. Edward Kennedy's hearings on deregulation in a Judiciary subcommittee also served to spark reform by creating an intense competition with Sen. Howard Cannon's aviation subcommittee; Cannon, Kingdon maintains, held hearings in order to protect his turf from Kennedy. Kingdon (1984, 11) holds that this groundwork primed aviation deregulation to be one of the first items on newly elected President Carter's "get-government-off-your-back" agenda. This momentum then carried into other areas: "At that point, policy makers' attention turned with a vengeance to the other transportation modes." Kingdon also shows how deregulation mushroomed as an issue (and as a floating solution) among the network of policy elites he interviewed.

Kingdon implies that the "market" for policies is beset by both high transaction costs and dispersed information. Such an environment can be expected to produce waves of related policies sparked by attention-getting "crises," interspersed with periods of stasis. This is because the events that arouse public opinion serve, for a particular problem, to increase both the payoff for addressing it that accrues to political entrepreneurs, and the amount of information shared by relevant actors, even while it shapes the subjective values those actors use in selecting among policy options. If a policy is enacted, the resulting momentum reduces the transaction costs of taking actions in related areas (such as coal-mine legislation to address black-lung disease, or airline and trucking deregulation to address declining profits). At some point, however, the vein of public attention is tapped and action in the affected policy area ends, at least for a time, as policy entrepreneurs move on to more fruitful areas.

When added to Peltzman's argument that deregulation was a natural result of the interplay between political actors' preferences and economic forces, Kingdon's attribution of structural power to changes in public opinion produces a portrait of a U.S. trend toward deregulation that is as predictable (at least retrospectively) as it would have been using Peltzman's model, but with the origin of deregulation and its subsequent speed and vicissitudes better explained by Kingdon.

* * *

The case of deregulation illustrates the untapped potential of an interdisciplinary approach to institutions. Economics, with its focus on rules, individual actors' preferences, and their resulting interaction, can provide a rich understanding of political institutions, and quite possibly of mass political behavior. It is probably not sufficient, however, any more than traditional sociological models were. Increasingly it appears that the future of research into any level of human behavior, from the individual to the organization, will be most fruitful when it is organized around the unit (or units) of analysis, rather than the discipline from which one draws one's tools. Disciplinary insularity leads to hyperspecialized irrelevancy when compared to the proliferation of research tools and the continual alteration of "models" that can be expected once the models are directed toward the reality that forms the unit of analysis.

It is often said that to someone with a hammer, every problem seems to be a nail. Despite Peltzman's efforts at creative hammering, his research is still too bound by his discipline to offer us anything approaching a realistic picture of politics.

NOTES

1. Of course this assumes that constituents have lower preferences for ideological satisfaction than for economic satisfaction, and that their interests can be separated in such a fashion for the purposes of econometric analysis. I discuss these problems briefly below.
2. Most of the work by economists in this area (including Peltzman's), along with some by political scientists, measures ideology by using scores provided by interest groups such as Americans for Democratic Action. Since these scores are based on legislators' voting records, using them as an independent variable means that one is explaining votes with votes—so it should not be surprising when the score turns out to be a significant predictor of behavior. For a com-

plete explanation of the problems with this approach, along with recommendations for improvements, see Jackson and Kingdon 1992.

3. There is a critical flaw in Peltzman's 1984 paper—the use of stepwise regression. This involves inserting one variable after another into an equation, until adding additional variables explains little of the remaining variance in the dependent variable. This is problematic when the explanatory variables are collinear, because the order in which they are inserted affects whether some variables are attributed much explanatory power at all. More complete discussions are available in any introductory text on econometrics, such as Hanushek and Jackson 1977.
4. One could argue that in the long run, the opportunity cost to the poor of electing a liberal is greater because of reductions in economic growth that result from excessive redistribution. Peltzman's characterization assumes that the average poor voter either doesn't perceive the structure of "voting costs" this way, or else discounts future earnings to the point at which the payoff from greater long-term economic growth is less than the more immediate payoff of redistribution. Underlying these assumptions, of course, are the further assumptions that voters keep track of how their legislators vote, and that they make their electoral decisions accordingly.
5. It is necessary to distinguish whites from blacks when assessing the extent of Southern conservatism because conservatism has historically been largely reflected in white rather than black attitudes.
6. Contrasted with "predictive," on which more below.
7. To be sure, the beginning of this tradition may well have been based on rational economic considerations, but there is little evidence that this reasoning repeats itself in each generation; party identification tends to be absorbed by children in the absence of critical reflection.
8. In fairness to Friedman, he did allow for testing hypotheses based on the implications of their assumptions for other hypotheses. His main goal was to rebut the notion that proving assumptions unrealistic is, in itself, a sufficient argument against a hypothesis.
9. As opposed to party-centered elections, which, by their nature, focus more on such national issues as the macroeconomy.
10. In other words, the standard error was higher than the estimated coefficient, so one can have little confidence in the estimate.
11. Jacobson and Kernell's work is, however, more easily extended into other issue areas: strong potential challengers may, for example, decide to enter a race because of an incumbent's positions on civil liberties or his recently revealed adulterous affair. The end result would be that the incumbent is "held responsible" by voters for actions beyond his votes on economic issues.
12. Of course many economists are very interdisciplinary (Nobel laureate Douglass North is a prime example; see also Caplan 2000), while noneconomists can be among the most rigid of academic isolationsists.
13. Regulatory-agency and executive-branch officials are technically part of the same institution, but in terms of the variables that matter most in this discus-

sion—norms, incentives, knowledge, ability, and mental models—they are clearly distinguishable from one another.

14. A few exemplary works that illustrate the difficulties of governing bureaucracy and implementing policy are Light 1983; Pressman and Wildavksy 1984; Riley 1987; and Wildavsky 1987 and 1988.
15. At least not yet; there is always the faint hope among many economists that, with enough knowledge about preference functions, the modeler can make predictions at this level.
16. For an excellent discussion of the concept of political entrepreneurs, see Schneider, Teske, and Mintrom 1995.

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