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AGAINST INTEREST-GROUP THEORY: A COMMENT ON PELTZMAN, "THE POLITICAL ECONOMY OF THE DECLINE OF AMERICAN PUBLIC EDUCATION"*

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MANY people think that political outcomes can be "explained" as products of the influence of self-interested, well-organized groups with high stakes in the outcome. Undoubtedly this is often true. But causation in politics is a complex matter. If we find that a group has benefited from a public policy, we know something important, but one thing we do not know is that the policy exists *because* it benefits that group. If controls on automobile emissions are enacted in a period in which the labor movement is weakening, we know something, but we do not know that the controls are *caused* by the weakening of the labor movement. These are prosaic claims, but I think they have large implications for many of the arguments of interest-group theory.

Peltzman's intriguing article[1] is generally cautious. But in several places, it becomes rather bold. In explaining the decline of American education from 1960 to 1980, Peltzman claims, "[T]he plausible rule of politics is substantial."[2] Later in the article, he says, "Industry pressure does have a substantial effect on performance."[3] These claims are reasonable, but we do not know that they are true. Both of them seem to me inadequately supported. I conclude that Peltzman has generated extremely interesting data and shown some suggestive correlations. But he has not yet made any real demonstration of causation.

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[1] Sam Peltzman, The Political Economy of the Decline of American Public Education, in this issue.

[2] *Id.* at 322.

[3] *Id.* at 359.

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I want to support this general point with six particular questions.

1. *Did School Performance Really Deteriorate?* Peltzman makes strong arguments for the view that school performance deteriorated from 1960 to 1980, but the proposition remains a bit blurred.[4] We do know that Scholastic Aptitude Test (SAT) scores went down, but the SAT is taken by a college-bound elite. Decreases in SAT scores do not necessarily mean decreases in overall school performance. And there is indeed some contrary evidence for high school students as a whole. For example, national norm studies—taken from 1955 to 1983 and including a nationally representative sample of high school juniors—suggest a much steadier set of scores. These studies do not reveal the same kind of decline as is shown by the SAT trends. Other tests suggest similar results. Indeed, a federal study, Project TALENT, actually found a slight gain in scores by 1970 eleventh graders over those by 1960 eleventh graders.

None of this demonstrates that Peltzman is wrong in claiming that school performance deteriorated. But it does suggest the possibility that the SAT decline shows a decline in performance for elites, not for high schoolers as a whole.[5] If this is so, the causal picture gets more complicated. It may also be true that in 1960–80 the schools emphasized skills not picked up by the SAT, a possibility that would introduce even greater difficulties into the picture.

2. *What about Factors Independent of Schooling?* School performance is almost certainly a function of family structure and family background.[6] Children in two-parent households appear to do better than children with only one parent in the home.[7] We can speculate that school children do less well if their parents are in the midst of a divorce. It is thus possible that the decline in SAT scores was attributable to factors unrelated to the educational environment, narrowly defined.

Peltzman would do well to attempt to control for this possibility, measuring his educational factors against noneducational causes. The absence of such an effort makes it especially difficult to make confident claims about causation.

3. *Timing Problems and the Aggregate Data.* Peltzman explores and sometimes seems to endorse the view that schools deteriorated in part because of (*a*) the growth in labor unions and (*b*) shifts in financial

[4] See Charles Murray & R. J. Herrnstein, What's Really behind the SAT-Score Decline, 106 Pub. Interest 32 (1992), on which I draw here.

[5] *Id.*

[6] See John Chubb & Terry Moe, Politics, Markets, and America's Schools, at ch. 4 (1990).

[7] *Id.*

responsibility from local to state government. But if we look at the aggregate data, we will find some general oddities. Between 1946 and 1972, the financing ratios between local and state government remained about the same. From 1946 to 1950, the state funded about 39 percent, whereas local government funded about 58 percent. By 1972, the state was still funding 39 percent, and local government was funding over 52 percent—hardly a significant shift. And yet it was in the period from 1961 to 1972 that SAT scores were significantly declining. The aggregate data thus suggest a puzzle. The supposedly causal shift in financial responsibility did not occur at all during a significant part of the crucial period.

The data for labor unions raise a similar issue. From 1961 to 1962, there was a huge jump in National Education Association (NEA) membership—from less than 4 percent to 54 percent. But from 1962 to 1971, teacher union density remained roughly constant, rising to a statistically insignificantly increased level of 56.9 percent in 1971. Note again that it was in this very period—from the early 1960s to 1971—that SAT scores were declining. The labor union data pose an additional puzzle for Peltzman since they suggest that teacher union density stayed roughly constant while educational "output" declined.

Perhaps the disaggregated data take care of the problem. Perhaps we can think about differences in the nature of the NEA over time or about the rise of the "bad union," the American Federation of Teachers (AFT). But even if this is so, Peltzman's own, more particular findings undermine the view that the data show a causal effect from unionization and shifts in financing.

4. *Labor Unions?* Peltzman notes that the evidence on labor unions is mixed, because of matters of timing and detail.[8] The disclaimer seems necessary. School performance actually seems to have improved with the success of the NEA in the 1960s and 1970s. There is therefore no evidence that the NEA contributed to a decline in education. There appears to be some correlation between such decline and success by the AFT, but the data here are merely fragmentary. The AFT has never had a large percentage of teachers—somewhere between a tenth and a fifth of teachers during the period of national SAT decline. Moreover, we appear to have incomplete information on AFT membership between 1960 and 1971. Surely there is too little evidence to claim causation as a result of AFT membership, let alone union membership. At the very best, I think that we have a mildly suggestive correlation.

5. *Shifts in Financing?* There is apparently a correlation between the early shifts to state funding and early declines in SAT performance.

[8] Peltzman, *supra* note 1, at 352–56.

But there is no correlation at all between the later shifts to state funding—those in the 1980s—and SAT performance. For this reason Peltzman concludes, consistently with the data, that the shift "is a symptom rather than a cause."[9] I am not certain what he means by this. But at least we can agree that the data do not establish, or even strongly suggest, that the shift in financing produced a decline in performance. Thus far, then, the claim of causation seems unproved with respect to both union density and financing practices.

6. *Businesses?* Peltzman's most striking finding is that school performance declined in areas in which the use of educated labor by politically important employees grew the most, and in which newly important industries used the most educated labor. As Peltzman emphasizes, the finding is strongly counterintuitive. If industry pressure was to have any effect, one would expect school performance to increase in precisely those areas.

Without more, we might think that this counterintuitive correlation says absolutely nothing about causation. We might see this finding as resembling a demonstration that the performance of a sports team, or the economy of a foreign country, was correlated with shifts in SAT scores. (Both of these are likely true.) Now it is possible to come up with a plausible story to explain the anomaly—as Peltzman has in fact done.[10] But it is almost always possible for inventive people to come up with a plausible story. The question is whether with respect to the causal role of industry pressure, we have anything more than that. I think that on the present evidence, we do not.

I conclude with a more general remark, one that extends well beyond Peltzman's article to theories of regulation in general. "Functional analysis" involves the attribution of causality to a practice after a showing of its effects.[11] A key part of pseudoscience, it is prominent in Marxism (*X* is in the interest of capitalists; therefore, it exists *because* it is in the interest of capitalist) and feminism (*X* is in the interest of men, therefore it exists *because* it is in the interest of men). It is also prominent in economics, including interest-group theory. Having shown that a practice is efficient, some people think that they have shown that it exists because it is efficient; having shown that a practice serves identifiable interests, some people think they have shown that it exists for that very reason.

[9] *Id.* at 366.

[10] *Id.* at 367–68.

[11] It is well discussed in Jon Elster, Ulysses and the Sirens: Studies in Rationality and Irrationality 28–35 (1979); Jon Elster, Explaining Technical Change, at ch. 2 (1983): Jon Elster, Sour Grapes 101–8 (1983).

Economists and economically inclined lawyers have become extremely sophisticated in revealing the effects of legal regulation. We might be able to demonstrate, for example, that occupational safety and health regulation is in the interest of large firms, or that regulation of new risks (and exemption of old ones) helps to cartelize existing industry. But from this it is often said to follow that regulation exists *because* of its beneficial effects on large firms or existing industry. I hope that I have said enough to show that this does not follow. Often it is more than enough to be able to demonstrate the social consequences of legal rules, without also claiming to have shown causation.[12]

[12] Of course, a demonstration of effects may be relevant to the issue of causation; from effects we can generate plausible stories about contributing factors. But there is a large difference between plausibility and truth. To assess the matter of truth about the causes of regulation, we need to know a good deal more than interest-group and public choice theory typically provide.

A related point: Sometimes economic theories of regulation appear insufficiently to investigate the possibility that the system preceding the specific regulation at issue is itself the product of self-interested action and that that specific regulation is in some sense a counter or a corrective to an interest-driven status quo. The lack of investigation here has led to a significant gap in the literature. It would be exceptionally interesting to see whether failure to regulate is sometimes attributable to interest-group power, or (less ambitiously) to show that the absence of regulation produces predictable winners and losers. See Einer Elhague, Does Interest Group Theory Justify More Intrusive Judicial Review? 101 Yale L. J. 31 (1991); Cass Sunstein, After the Rights Revolution 2–3 (1990). The issue is not raised by Peltzman's article here, however, and hence I do not discuss it.