Does School Finance Litigation Cause Taxpayer Revolt? *Serrano* and Proposition 13

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An influential theory argues that court-ordered school finance equalization undermines support for public schools. Residents of wealthy school districts who cannot keep their tax revenues for their own school districts may vote to limit school funding altogether. Proponents of this theory point to *Serrano v. Priest,* a 1977 decision of the California Supreme Court that mandated equalization of school financing and was followed almost immediately by Proposition 13, a ballot initiative to limit the local property tax. I test the theory that these two events were causally related by using hierarchical models to analyze voters within school districts. I find no evidence that opposition to school finance equalization contributed to the tax revolt. Claims about the perverse consequences of school finance litigation should be greeted with skepticism.

Since the late 1960s, civil rights attorneys have challenged the unequal financing of public schools, arguing that the reliance on local property taxes to fund education violates the constitutional rights of children to equal educational opportunity. In response, several state courts have overturned established systems of school finance. These decisions have typically mandated remedies that redistribute tax revenues from wealthy districts to poor ones.

Most scholarship on the impact of school finance litigation investigates whether it has the effects intended by reformers. The question is important because it bears on the long-standing controversy over whether the judiciary can create social change. Critics argue that courts are relatively powerless to promote equality, because they depend on other institutions to implement their decrees (Horowitz 1977; Rosenberg 1991). Students of school finance litigation have generally taken a more optimistic view of the courts.

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Where judges have decided in favor of school finance equalization, their decisions have been implemented, if sometimes "grudgingly" (Canon & Johnson 1999:127; see Rebell & Block 1982; McUsic 1999), and the result in at least some states has been a substantial redistribution of resources among school districts (Bosworth 2001; Paris 2001; Reed 1998, 2001; Rebell & Block 1982; but see Horowitz 1977: Ch. 3).

This article addresses a different question: whether such litigation has effects *unintended* by reformers. It is a commonplace that judicial policies—and social policies in general—can have effects that their promoters did not foresee or intend (Bogart 2002; Canon & Johnson 1999; Horowitz 1977). This criticism is commonly leveled against courts when they attempt to equalize resources that the market has distributed unequally. According to the critics, egalitarian judicial policies may have a variety of perverse consequences that range from reducing the aggregate well-being of the public (see, e.g., Posner 1998) to undermining respect for the judiciary (Horowitz 1977) to provoking a political backlash by the privileged (Rosenberg 1991).

The last of these arguments has gained widespread currency among students of education finance. Proponents argue that court-ordered school finance equalization undermined popular support for schools and ultimately triggered a backlash that crippled public education. Voters who had happily paid heavy taxes to support their local schools were unwilling to pay the same taxes for schools outside of their own communities. When courts mandated equalization, voters responded by demanding laws to limit the property tax levy. An unnamed legislator interviewed by Kozol (1991) summarized the theory succinctly: "This [property tax limitation] is the revenge of wealth against the poor. 'If the schools must actually be equal,' they are saying, 'then we'll undercut them all.'" (1991:220).

This theory originated in California, which was home to both the most widely cited school finance equalization decision *(Serrano v. Priest ["Serrano I"]*, 5 Cal. 3d 584, 96 Cal. Rptr. 601, 487 P. 2d 1241 [1971]) and the most dramatic and well-known antitax backlash initiative, Proposition 13 (Cal. Const. Art. XIIIA, Sec. 1–6 [1978]). Some observers at the time suspected that these two events were related (Oakland 1979:388; Kuttner 1980:23). The economist William Fischel (1989) developed the underlying theory in an article entitled "Did *Serrano* Cause Proposition 13?" and a series of subsequent publications (Fischel 1992, 1996, 2001, 2002, 2004). Largely because of Fischel's efforts, the hypothesis that court-ordered school finance equalization caused the tax revolt has become, in his words, "part of the conventional wisdom among local public-finance scholars and students of Proposition 13" (2002:103). Stark and Zasloff (2003), who are critical of the Fischel hypothesis,

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acknowledge it as "a leading, perhaps *the* leading, explanation for the root cause of Proposition 13" (2003:815). They count more than 20 recent articles that make reference to the hypothesis in law journals alone (2003:805, note 19). Several economists have endorsed the hypothesis (see, e.g., O'Sullivan et al. 1995; Wassmer 1997; Downes & Schoeman 1998; Reschovsky 1999; Sonstelie et al. 2000). Kozol popularized a version of the hypothesis in his best-selling book, *Savage Inequalities* (Kozol 1991), and other writers have popularized it in mass-market books that describe tax revolts in California and New Jersey (Schrag 1998; Cohen 2003). It is no exaggeration to say that the Fischel hypothesis is among the most influential interpretations of the property tax revolt.

If the Fischel hypothesis is true, then court-ordered school finance equalization is a Pyrrhic victory for reformers. Property tax limitation has had dire consequences for public schools. Tax limitation laws have enforced budgetary restraint, with consequences that range from increased class sizes (Figlio 1997, 1998; Shadbegian 2003) and decreased spending per pupil (Shadbegian 2003) to diminished teacher qualifications (Figlio & Rueben 2001) and lower test scores (Figlio 1997; Downes et al. 1998; Downes & Figlio 1999).

But is the Fischel hypothesis true? In this article, I subject it to empirical scrutiny. A substantial literature on the causes of property tax limitation predates Fischel's work and does not address his hypothesis (Courant et al. 1980; Ladd & Wilson 1983; Lowery & Sigelman 1981; Sigelman et al. 1983; Hansen 1983; Stein et al. 1983; Sears & Citrin 1985; Neiman & Riposa 1986). A handful of recent publications attempts to test the hypothesis indirectly, with mixed results (Fischel 2001, 2004; Stark & Zasloff 2003). This article is the first to test the hypothesis directly with survey data on individuals' voting behavior. I draw on two archived data sets drawn from independent samples of Californians who voted in the 1978 primary election. I also exploit geographic information in the Master Area Reference Files (MARFs) from the 1980 census (U.S. Bureau of the Census 1983a, 1983b) to match a subset of individual survey respondents to institutional data on school districts. This novel method allows me to test whether voters defined their interests along school district lines, as the Fischel hypothesis would predict.

To preview my conclusions, I find no support for the Fischel hypothesis. I conclude that the popularity of the hypothesis derives from the unwarranted inference that two dramatic events that were roughly coincident in time and place (California in the late 1970s) were therefore also causally related. Future research on the impact of school finance litigation—and judicial policy in general—should

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subject claims about unintended outcomes to the same close scrutiny that greets claims about intended outcomes.

The remainder of this article has four sections. To explain why both advocates and critics of the Fischel hypothesis see California as a crucial test case, the first section offers a brief historical overview of school finance equalization and property tax limitation in California. The second section describes Fischel's theoretical argument and reviews previous attempts to test his hypothesis. The third section describes the data and methods I use to test the Fischel hypothesis, and the fourth discusses the results.

School Finances and Tax Revolt in California

In the early 1960s, civil rights advocates in California and throughout the United States began to question the constitutional basis for local financing of public schools (Elmore & McLaughlin 1982; Minorini & Sugarman 1999). The ideal of equal educational opportunity articulated in *Brown v. Board of Education* (347 U.S. 483 [1954]) and championed by the civil rights movement seemed to many advocates to imply more than desegregation. Even schools that were not legally segregated by race might nevertheless fail to provide equal opportunity if they allocated opportunity according to wealth. This critique implied that the local property tax was a poor basis for funding public schools, for as long as school revenues came from local property taxes, students who attended school in an area of high property values could expect to have greater access to educational resources than their peers who attended school in an area of low property values. By the mid-1960s, activists had articulated a number of legal theories under which such inequalities were unconstitutional. By the end of the decade, civil rights attorneys had initiated challenges to local property tax financing of public schools in Michigan, Illinois, Virginia, Texas, and California (Kirp 1973; Minorini & Sugarman 1999).

The California case was the first to succeed. In 1968, a group of Los Angeles legal aid attorneys sued the state on behalf of a plaintiff named John Serrano to overturn California's system of school financing. Serrano had recently moved with his wife and two sons from the barrio of East Los Angeles to the middle-class suburb of Whittier, because Whittier's bigger tax base supported better public schools (Kirp 1973). His legal team argued that the quality of public schooling should not depend on the taxable wealth of the community. In 1971, the California Supreme Court ruled in their favor (*Serrano I,* 96 Cal. Rptr. 601). Although the justices remanded the case to the lower courts, they affirmed that there was an issue to be tried. If the local property tax was as unequal as the plaintiffs

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alleged, then it was indeed an unconstitutional basis for school finance (*Serrano I* 1971; see Elmore & McLaughlin 1982).

The decision was a watershed for the national movement. It immediately drew the attention of national news media and civil rights advocates (Elmore & McLaughlin 1982:50). It only gained in importance two years later when the U.S. Supreme Court heard and rejected a similar suit brought by Texas plaintiffs in 1973 (*Rodriguez v. San Antonio Independent School District*, 411 U.S. 1). *Rodriguez* signaled that the federal courts would henceforth be inhospitable venues for school finance equalization cases. Advocates of equalization thus came to see state litigation modeled on *Serrano* as their most effective way forward (Tedin 1994; Reed 1998; Minorini & Sugarman 1999).

It nevertheless took a second lawsuit to force action by the state of California. In 1973, the state legislature rewrote its school aid formula and enacted revenue limits on high-spending districts in an attempt to comply with the *Serrano* court's ruling. The lawyers for Serrano argued that the new formulas still permitted vast inequalities (Elmore & McLaughlin 1982). In 1976, the California Supreme Court again decided for the plaintiffs (*Serrano v. Priest* ["*Serrano II*"], 135 Cal. Rptr. 345). The court ordered the legislature to come up with a new system of school finance and suggested that such a system would be constitutional if it equalized "wealth-related" expenditures across school districts to within $100 per pupil. Under pressure of this standard, the legislature agreed on a new school funding formula that combined state aid and revenue caps, and on September 17, 1977, Governor Jerry Brown signed it into law (Elmore & McLaughlin 1982; see also the historical overview in *Serrano v. Priest* ["*Serrano III*"], 226 Cal. Rptr. 584 [1977]).

The property tax revolt followed almost immediately. In December 1977, activists submitted a petition with 1.3 million signatures in favor of a constitutional amendment to limit local property tax levies throughout the state (Lo 1990; Allswang 2000). The proposed amendment would establish a 1% limit on the property tax rate and a 2% limit on the annual increase in the assessed taxable value of any individual property. This meant that property owners would be protected from tax increases in perpetuity. After a contentious campaign, voters approved the amendment as Proposition 13 on the primary election ballot of June 1978 (Schrag 1998).

The passage of Proposition 13, like the *Serrano* decision, was a watershed event that drew national media attention and inspired imitators in other states (Kuttner 1980). Indeed, to observers who were concerned with public school finance, the *Serrano* case and Proposition 13 were probably among the two most salient political events of the 1970s. Both events, moreover, occurred in close

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spatial and temporal proximity. It is therefore not surprising that observers should have seen them as causally related (see Fiske & Taylor 1991:58–9).

But what was the nature of the relationship between these two events? As a prominent California journalist later remarked, the legislature's bid to comply with *Serrano* "was hardly discussed at the time" (Schrag 1999:148). There is little qualitative evidence to suggest that voters saw any connection between *Serrano* and Proposition 13.

The campaign for Proposition 13 emphasized the threat of rising tax assessments rather than the distribution of school expenditures. Howard Jarvis, the coauthor of the initiative and its main spokesperson, pointed out that property tax bills had been rising for years, and he warned voters that their property tax bills would double or triple if they did not vote to limit taxes (Jarvis & Pack 1979). Promotional mailings for the campaign mimicked official assessors' notices (Kuttner 1980) or informed recipients of the windfall they would receive if Proposition 13 passed (Smith 1998). The proponents of tax limitation were generally silent about the distributional consequences of their measure for education and other spending programs.

Had opposition to court-ordered school finance equalization been the issue at the heart of the tax revolt, one might expect the proponents of Proposition 13 to have appealed directly to residents of wealthy school districts. They might have posed as defenders of local control against a redistributive mandate imposed by the state —as residents of wealthy communities often do when their privileges are threatened (Briffault 1990). They did not. Instead, they posed as defenders of the beleaguered taxpayer against state and local governments—*including* local school districts. When Wilson Riles, the state superintendent of schools, asserted that Proposition 13 would destroy public education in California, Jarvis replied, "The initiative is to cut property taxes in California and to save a couple of million people from losing their homes. They are a lot more important than twenty thousand schoolteachers" (Schrag 1999:146). When the Los Angeles County supervisors warned that Proposition 13 would cause most of the county libraries to close, Jarvis's response was even blunter: "That doesn't matter. Why do we need books? The schools aren't teaching kids to read anyway" (Kuttner 1980:78). This was not an appeal to defend local schools against *Serrano,* or indeed anything else.

Jarvis's dismissive attitude toward public schools did not mean that education was a major campaign theme. It was not. Proponents of Proposition 13 rarely mentioned schools or other public services at all, except when pressed to do so by their opponents. Instead, they found that the most productive appeal was the

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simplest: property taxes were just too high. "We learned the best approach to use on someone when you want to get their signature on a petition [was]: 'Sign this—it will help lower your taxes,'" Jarvis later recalled. "That usually worked" (Jarvis & Pack 1979:48). Likewise, he wrote, "[t]he best piece of mail we sent out in the entire sixteen years"—the years from 1962 to 1978, during which Jarvis campaigned tirelessly to limit the property tax—"was one during the [Proposition] 13 campaign that told every property owner how much his tax bill was, how much he would save on [Proposition] 13 in one year, and how much he'd save in five years" (Jarvis & Pack 1979:49). The campaign for Proposition 13 was a campaign for lower taxes. Jarvis did not mention the *Serrano* case or the issue of school finance equalization once in his 300-page memoir, most of which concerned the campaign for Proposition 13 (Jarvis & Pack 1979).

Nor did the text of the initiative itself address the sections of the state constitution that were at issue in *Serrano*. Instead, it amended the state constitution to limit the local property tax rate, to limit the annual increase in property assessments, and to limit the ability of the state legislature to raise new taxes (see Jarvis & Pack 1979:105). This would seem an indirect way to address grievances that concerned the distribution of school funding. The initiative could just as easily have amended the state constitution to permit local property tax financing of public schools. It did not.

In short, *Serrano* cannot have been the sole cause of voter dissatisfaction with the local property tax. Indeed, at the time of *Serrano II,* California activists had already tried many times to abolish the tax or limit its growth (Lo 1990). Proposals for property tax limits similar to Proposition 13 had appeared on the ballot in 1968 and 1972, and others had almost qualified for the ballot in 1976 and early 1977.

Nevertheless, something had changed by late 1977. For the first time, a majority of voters were willing to support such proposals. It is this newfound willingness, according to Fischel, that is attributable to *Serrano*.

Theoretical Foundations and Literature Review

The theoretical puzzle Fischel addresses is why voters withdrew their support for the property tax in the late 1970s. He begins with the assumption that people assess the fairness of their tax burden in relation to the value of public services they receive. This means that there is not necessarily a fixed threshold above which voters will refuse to tolerate taxation. Instead, the key to popular support is the balance between taxes and public services. In particular,

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Fischel argues, people willingly paid the property tax as long as they perceived it to be a fair price for local schools. When they no longer perceived it to be a fair price (or a "benefit tax"), they rejected it.

Government has a monopoly on the supply of many public goods that allows it to set the level of taxation with little regard to demand. According to Fischel, however, the property tax for public schools was different because it was local. Following Tiebout (1956), he argues that a simple market mechanism operated to bring the supply of schooling into equilibrium with demand. Public schools were governed by local districts that until *Serrano* had broad discretion to set property tax rates and make spending decisions. People were free to move to the school district that provided their preferred level of schooling given the tax price they could afford. Districts, in turn, had to compete to attract taxpayers. As in any competitive market, they did so by offering different products—in this case, different levels of schooling—at different prices. Because property taxes were capitalized in housing prices, high mortgage bills tended to offset low tax bills, with the result that the residents of the same school district all paid the same price for the same public schools.[1]

Until *Serrano II,* Fischel argues, the market for public schools was at equilibrium. California residents generally received the levels of public education they wanted from their local school districts at levels of taxation that they were willing to pay. The mandate to equalize school finances destroyed this equilibrium. Voters in high-demand districts—those who paid premium prices to live in districts with good public schools—were suddenly slated to receive less public schooling even as their taxes remained high. They were no longer getting what they paid for.

Had these voters been able to undo school finance equalization, they could have restored the former equilibrium without cutting taxes. But *Serrano* made this impossible. The legislature was constrained by the judiciary, and the California Supreme Court was immune to voter sanction. Unable to restore spending, voters instead chose to cut taxes. In short, Fischel argues, "Proposition 13 succeeded because voters in high-demand school districts no longer regarded the property tax as a benefit tax" (Fischel 1992:176).

There are two ways to test this hypothesis. One is to compare California after *Serrano* to the counterfactual scenario of California without *Serrano*. Empirically, one could approximate this

[1] Following Hamilton (1976), Fischel argues that when the housing market is at equilibrium, higher mortgage payments will offset lower tax payments, and the true cost of the local property tax to everyone in a jurisdiction will be equivalent to the average per-household price of supplying local public services.

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comparison by identifying similar states with and without *Serrano*-like decisions (King et al. 1994). In his early work, Fischel argued against this comparative strategy on the grounds that no other state court had imposed a remedy quite comparable to *Serrano* in the extremity of its egalitarianism (Fischel 1989:471). Although school finance equalization measures in other states since 1989 have obviated this objection (see Reed 1998), Fischel now offers a second argument against comparative tests, which is that the "penumbra" of school finance litigation may have caused even states without *Serrano*-like decisions of their own to change their tax policies (Fischel 2002:104–6).

In the interest of providing a generous test of the Fischel hypothesis, I pursue a second strategy: to compare individuals in California whose interests were affected differently by *Serrano*. I thus accept the decision of Fischel and at least some of his critics to eschew cross-state comparisons and to treat the California primary election of June 1978 as the principal test case of the argument. Note, however, that nothing in the logic of the argument is otherwise peculiar to California, and that Fischel believes the policy implications of California's experience apply quite generally to other states that may be considering legislation to equalize school finances (Fischel 2002). Like much of Fischel's work, then, this article treats California as merely a case study to test a more general hypothesis: to wit, that court-ordered school finance equalization causes property tax revolts.[2]

All previous attempts to test the Fischel hypothesis to date have relied on community-level voting returns from California to test whether people in school districts with higher property valuation per pupil were more likely to vote for Proposition 13. Districts with above-average property valuations per pupil were losers under the *Serrano* redistribution formula, and the Fischel hypothesis implies that voters in such districts should have been particularly inclined to favor tax limitation. Fischel (2001) tested this hypothesis by calculating the bivariate correlation between valuation per pupil and the aggregate vote. He controlled for other, non-*Serrano* influences on the vote by transforming the dependent variable: namely, by subtracting the percentage who had voted for a tax limitation in 1972 (the last property tax limitation initiative prior to *Serrano II*)

[2] Fischel's objections notwithstanding, a comparative test of his hypothesis seems reasonable, even if it is beyond the scope of this article. Fischel has asserted that *Serrano* was a *necessary* cause of Proposition 13 ("[I]f it had not been for *Serrano,* Proposition 13 would not have passed" [1996:608, note 9]) and has strongly suggested that it would also have been a *sufficient* cause of "something like Proposition 13" even in the absence of housing inflation (1996:636). Like any claim about necessary and sufficient conditions in history, this argument should be accessible to comparative historical evaluation (Ragin 1987; Mahoney & Rueschemeyer 2003). Fischel is, of course, correct to point out that the comparisons would be complicated by the possibility of cross-state influence (see Lieberson 1985).

from the percentage who voted for Proposition 13 (the first property tax limitation initiative after *Serrano II*), and then dividing the result by the former quantity. Subsequent analyses have followed the same procedure, although they have also introduced additional controls by means of multivariate OLS regression (Stark & Zasloff 2003; Fischel 2004).

The results of these studies are inconclusive for two reasons. First, they suffer from ecological inference problems. Both attempt to describe individual behavior with aggregate data. As Fischel correctly points out, using aggregate data does not entail an ecological fallacy as long as the hypothesis itself concerns only aggregate differences in behavior—in this case, district-level differences in voting (Fischel 2004; see King 1997). The difficulty arises because both Fischel and his critics attempt to use aggregate data to adjudicate between this hypothesis and alternative hypotheses that concern differences at the individual level. For example, Stark and Zasloff (2003) argue that any apparent effect of district property values on the Proposition 13 vote is spurious and arises in part because elderly individuals, who favored Proposition 13, tended also to live in property-rich school districts. They show that the percentage of the population that was elderly was correlated with the percentage voting for Proposition 13 in any given school district. They also demonstrate that controlling for the percentage of the population that was elderly reduced the effect of property values to insignificance. Fischel (2004) points out in response that Stark and Zasloff have not shown that it was the elderly *within* these school districts who voted for Proposition 13. He argues that the correlation is spurious: elderly people tended to reside in property-rich school districts, but people in property-rich school districts tended to vote for Proposition 13 regardless of their age. The question obviously cannot be resolved without individual-level data.

The second limitation of the existing studies is that they make untested assumptions about the geographic boundaries of school districts. In order to test whether voters in different school districts behave differently, Fischel and his critics require voting returns at the school district level. Such data are not available. Fischel (1996, 2001, 2004) and Stark and Zasloff (2003) therefore base their analyses on municipal voting returns and limit their samples to school districts whose names correspond to the names of municipalities. They assume in effect that correspondence of nomenclature indicates correspondence of boundaries. Although we know that this assumption is false in at least some cases (Fischel 2004), we do not know the direction or magnitude of the biases introduced thereby. Nor do we know the direction or magnitude of the biases introduced by restricting the analysis to a small and nonrandom

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sample of school districts. The problem here is not only that the sample is biased—most samples are—but also that these biases are unknown and unknowable without better data on the geographic distribution of individual voters.

I resolved these problems with a multilevel research design that relied on archived survey data and institutional data. First, I used geographic information to match individual survey respondents to the school districts in which they lived at the time of the survey, thus permitting a multilevel model of the net effect that a school district's financial position under *Serrano* had on the voting behavior of the individuals who lived in the district. Second, where possible, I used individual-level measures of individuals' attitudes toward the *Serrano* decision and their votes for Proposition 13.

Data and Methods

The research design for this study had two parts. In the first, I tested whether voters' positions on Proposition 13 corresponded to their school districts' situations under the *Serrano* ruling. Individuals in property-rich school districts, or districts with greater than average property valuations per pupil, stood to lose from the redistribution of property tax revenues. Fischel argues that individuals in such districts should be more likely than others to favor Proposition 13. Following Fischel (1996, 2001, 2004) and Stark and Zasloff (2003), I measured a voter's objective interests by the assessed property valuation per unit of "second period average daily attendance"—a measure of public school enrollment—in that voter's school district.[3]

I conducted hierarchical probit regressions of the vote for Proposition 13 on selected individual- and district-level covariates, using subsamples of survey respondents from two independent surveys who could be matched to school districts. These multilevel analyses corresponded in many respects to the district-level analyses conducted by Fischel (2001, 2004) and Stark and Zasloff (2003). They differed in two main respects. First, the dependent variable in the analyses presented here was the individual's vote for or against Proposition 13, rather than a "swing" in the aggregate vote between two propositions. Second, the models reported here included control variables for voter characteristics that are

[3] Fischel (2001, 2004) and Stark and Zasloff (2003) take the natural logarithm of this fraction. I prefer to work with valuation per pupil in $10,000s, but I have also tested the logarithmic specification in all of the models reported in Table 1. The choice does not affect the results. In order to facilitate comparison across high school districts and unified school districts, I also limit the denominator to high school enrollments only. The calculation of the variable is described further in the Appendix.

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measured at the individual level. The data on voter characteristics came from two independent samples. The first is the Field Institute's California Poll No. 7807 (FICP), which was conducted two months after the California primary election of 1978. The second is the California Tax Revolt Study (CTRS) (Sears & Citrin 1980), which sampled California adults in November 1979. In the first stage of the research, I pooled data from both samples.

To calculate the school district value per pupil for each voter, it was necessary to match individual survey respondents with published institutional data on property valuations and school enrollments for the fiscal year 1977–78 (California Department of Education 1979). The basis for the geographic match was zip code information that respondents provided to each poll. Using two MARFs for the 1980 census (U.S. Bureau of the Census 1983a, 1983b), I matched these zip codes to sets of census block groups and then matched census block groups in turn to California school districts. The correspondence between zip code area boundaries, census block group boundaries, and school district boundaries was imperfect, so geographical information was lost at each stage of the match. As a consequence of this information loss, unambiguous assignment of individuals to school districts was possible for only 346 of the 1,656 voters in the pooled sample. The Appendix describes the matching procedures in more detail and describes the biases in the resulting sample.

I analyzed the matched sample with hierarchical probit models that decomposed the error term into separate components representing the individual- and school-district levels of analysis. Such models are common in educational research where the purpose is to estimate the effects of district- or school-level variables on individual students' educational achievements. Because property value per capita is measured at the school district level, its values are not observed independently for every voter. Thus, an ordinary, nonhierarchical probit model might underestimate the standard errors associated with the effect of this variable.[4]

School districts with high valuations per pupil stood to lose from the *Serrano* decision. Thus, the Fischel hypothesis implies the following:

> *H_1: Individuals in districts with high valuations per pupil were more likely to vote for Proposition 13.*

[4] In practice, the estimated district-level error component was zero in all of the models reported here, so that the estimated coefficients and standard errors were equivalent to those estimated using ordinary (i.e., nonhierarchical) probit regression. See Snijders and Bosker 1999.

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I also tested whether this district-level effect was present only for homeowners. Fischel argues that all homeowners in wealthy districts tended to prefer the local funding of schools to statewide equalization, since better-funded local schools tended to increase their property values. Thus, we might expect that the *Serrano* decision undermined support for the property tax among homeowners in particular:

> *H_2: Homeowners in districts with high valuations per pupil were more likely to vote for Proposition 13.*

Fischel assumes that homeowners are generally interested in the value of their assets rather than in the quality of schooling per se. It is nevertheless reasonable to suppose that parents should be particularly sensitive to the link between property taxes and the real or perceived quality of their local schools (see Tedin 1994). Thus, even if *Serrano* did not affect the voting behavior of all Californians, it may have affected the voting behavior of parents:

> *H_3: Parents of schoolchildren in districts with high valuations per pupil were more likely to vote for Proposition 13.*

By necessity, I operationalized *parents of schoolchildren* differently for each sample. For respondents to the FICP, I constructed a dichotomous variable equal to one if the respondent reported having any children between ages six and 18 inclusive. For respondents to the CTRS, I constructed a dichotomous variable equal to one if the respondent reported having any children in the public schools.[5]

The tests of H_1–H_3 that I report below, like all previous tests of the Fischel hypothesis (Fischel 2001, 2004; Stark & Zasloff 2003), relied of necessity on a nonrandom subset of California school districts that could be matched to voting data. Because I drew on voting data from two representative samples of Californians, however, it was possible to quantify the resulting biases. The multilevel structure of the data set also permitted me to model sample selection explicitly (Heckman 1979). I report the results of probit models with explicit corrections for sample selection in the Appendix. These models suggest that sample selection bias does not affect the conclusions of the analysis.

In the second part of the study, I analyzed all voters in the FICP, regardless of whether they could be matched to school districts. The key to this part of the study was a pair of attitude

[5] In models not reported here, I examined whether this difference in measurement affected the conclusions of the analysis by testing for two-way interactions between sample and parents of schoolchildren, and then testing for three-way interactions among sample, parents of schoolchildren, and value per pupil. I also ran regressions separately in the CTRS and FICP samples. None of these alternative specifications changed the substantive conclusions reported here.

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questions in the FICP that permitted me to test whether the subjective interests that survey respondents expressed with respect to the *Serrano* decision were associated with their propensity to vote for Proposition 13. The survey recorded individuals' opinions about school finance equalization in general and the *Serrano* decision in particular. Fischel has argued explicitly that the average voter understood the distributive implications of *Serrano* (Fischel 1989), and that voter support for Proposition 13 reflected an underlying preference for unequal school finance (Fischel 2002). If he is correct, opposition to equalization and opposition to *Serrano* should both be positively associated with the vote for Proposition 13.

The survey included two relevant questions that could be used to test these implications. The first asked respondents for their opinion of school finance equalization in principle:

> How do you feel about the idea of making sure that [the] poorest school districts have the same amounts of money to spend per pupil for buildings, book [*sic*], salaries, and so on as the richest school districts have? Are you very favorable, moderately favorable, moderately unfavorable, or very unfavorable toward this idea?

A favorable response to this question could indicate positive feelings toward equality as an abstract ideal, without indicating a favorable attitude toward any particular redistributive means for achieving equality. The second question, however, asked respondents for their opinion about the *Serrano* remedy in particular, after specifically drawing their attention to the implications that redistribution would have for wealthier districts:

> As a result of recent court and legislative decisions all school districts in the state should have about equal amounts of money to spend on their local public schools. This means that some school districts which are spending more than others would have to reduce their spending and the money they save will go to other poorer school districts. What is your opinion of this? Are you very favorable, moderately favorable, moderately unfavorable, or very unfavorable toward this idea?

Both questions could be expected to have measured individuals' subjective interests with respect to the issue of school finance equalization. I used responses to both questions as separate independent variables. I coded the responses to each question as a dichotomous variable, equal to one if the respondent answered either "very unfavorable" or "moderately unfavorable." I called the two resulting variables "opposition to equalization" and "opposition to *Serrano,*" respectively. These variables allowed me to test two implications of the Fischel hypothesis:

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H_4: Individuals who opposed school finance equalization in general were more likely to vote for Proposition 13.
H_5: Individuals who opposed the Serrano decision in particular were more likely to vote for Proposition 13.

The tests of H_4 and H_5 reported below used the full FICP sample and were therefore not subject to the biases introduced into the previous analyses by the multilevel data merge.

Finally, I tested an important alternative to the Fischel hypothesis. Many scholars have argued that the tax revolt was a response to housing inflation (Kuttner 1980; Sears & Citrin 1985; Lo 1990). Property values in many California communities were increasing dramatically in the 1970s. As home values increased, so did homeowners' property tax bills. The rapid increase in property taxes may have led homeowners to vote for Proposition 13 even if they did not object to how those tax revenues were distributed among schools. Both Fischel and his critics have generally treated this hypothesis as a competitor, although it is not logically incompatible with the view that *Serrano* also had some effect (Fischel 1996, 2004; Stark & Zasloff 2003). I tested the inflation hypothesis:

H_6: Homeowners who anticipated large property tax increases were more likely to vote for Proposition 13.

I tested this hypothesis on the subsample of FICP respondents who were homeowners and who reported that they voted for or against Proposition 13. I measured anticipated property tax increases by responses to this question: "If Proposition 13 had failed to pass, by about how much did you expect your property taxes for this year to rise above last year's taxes?" The Field Institute coded responses on an ordinal scale, with values ranging from (1) "not at all" through (17) "$3,000 or more." I recoded the variable into $1,000s, with each category equated to the mean of the corresponding bounds. The bottom category was recoded to 0, and the top category was recoded to an imputed category mean of 5.6 on the assumption that expected property tax increases followed a Pareto distribution (see Parker & Fenwick 1983). I also included a quadratic term—the square of the expected property tax increase—to capture nonlinearities in the effect of property inflation.

The full regression models used to test hypotheses H_1 through H_6 included control variables for sex, age, income, education, race, and homeownership. I operationalized age as a continuous variable measured in years.[6] The income variable referred to family income in the FICP, and household income in the CTRS.

[6] In other analyses, I experimented with nonlinear specifications of age, including a quadratic term and a dummy variable for senior citizen status (age $\geq$ 65). These alternative specifications did not affect any of the conclusions reported in this article.

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Respondents gave their income in discrete categories; I recoded income in each case as a continuous variable, with values set at the mean of the category bounds, in $1,000s. If an income category was unbounded above (e.g., "$50,000 or more"), I imputed a mean value on the assumption that income is Pareto-distributed (see Parker & Fenwick 1983). I operationalized education with a pair of dichotomous variables, one indicating that the respondent had some college education but no four-year degree, and another indicating that the respondent had at least a bachelor's degree. In keeping with prior research (Sears & Citrin 1985), I treated race as a dichotomous variable equal to one if the respondent was black. Regression models on the pooled sample also included a dummy variable for the CTRS sample, to correct for differences in the regression intercept between the two samples. I dealt with missing values by listwise deletion.[7]

I limited all of the analyses to survey respondents who recalled and were willing to report that they had voted for or against Proposition 13. This procedure avoided confounding the causes of voting for Proposition 13 with the causes of voter turnout in general.

Results: Did *Serrano* Cause Proposition 13?

I begin with the multilevel analysis of voters in school districts. The results, reported in Table 1, failed to support the Fischel hypothesis. Individuals who lived in districts with high property valuations per pupil were no more likely than others to vote for Proposition 13. Regardless of whether we expected high valuation per pupil to have affected the behavior of all voters in the district, or only of homeowners or parents, the hypothesis found no support. The effect of valuation per pupil was not measurably different from zero in any of the models reported in the table.

This null finding could in principle be attributable to the small size of the merged sample (N = 314 voters, in 39 districts). One indication that the small sample size was not to blame is that the effect of valuation per capita does not appear to be measurably

[7] I chose listwise deletion in preference to several more technically demanding methods because estimates using this technique are reportedly more robust to the assumption that missingness —the absence of observed values of a particular variable for some cases— may depend on the unobserved value of the variable in question—a common and reasonable assumption for self-reported variables such as age, income, and race (Allison 2002). The cost of listwise deletion is the loss of data—in this case, the discarding of 32 cases. Because the analysis of the merged sample already involved a very small number of cases, I also replicated all of the analyses in Table 1 retaining these cases and imputing multiple values to each case with the information provided by the other variables in the model. I used the multiple imputation algorithm implemented in the program Amelia (Honaker et al. 2001; King et al. 2001). This alternative treatment of missing data did not affect the tests of hypotheses H_1–H_3 reported here.

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Table 1. Did Redistribution Cause Voters in Wealthy Districts to Vote for Proposition 13? Hierarchical Probit Regression of Vote on Individual- and District-Level Covariates, Pooled Sample

	(1) Coeff. (SE)	(2) Coeff. (SE)	(3) Coeff. (SE)	(4) Coeff. (SE)	(5) Coeff. (SE)
Intercept	0.50 (0.26)	−0.12 (0.37)	−0.16 (0.39)	−0.13 (0.38)	−0.10 (0.41)
CTRS sample	−0.01 (0.17)	−0.13 (0.17)	−0.13 (0.17)	−0.13 (0.17)	−0.13 (0.17)
Individual-level covariates					
Female	...	−0.13 (0.15)	−0.13 (0.16)	−0.13 (0.16)	−0.13 (0.16)
Age	...	0.009 (0.005)	0.009 (0.005)	0.009 (0.005)	0.009 (0.005)
Income	...	0.008 (0.005)	0.008 (0.005)	0.008 (0.005)	0.008 (0.005)
Some college	...	−0.24 (0.21)	−0.24 (0.21)	−0.24 (0.21)	−0.23 (0.21)
B.A. or higher degree	...	−0.52 (0.19)**	−0.52 (0.19)**	−0.52 (0.19)**	−0.52 (0.20)**
Black	...	−0.45 (0.26)	−0.45 (0.26)	−0.45 (0.26)	−0.45 (0.26)
Homeowner	...	0.59 (0.17)**	0.72 (0.38)	0.59 (0.17)**	0.59 (0.17)**
Parent of schoolchildren	...	...	...	0.02 (0.19)	−0.04 (0.38)
District-level covariates and interaction terms					
Value per pupil	−0.005 (0.025)	−0.003 (0.017)	0.001 (0.020)	−0.003 (0.017)	−0.006 (0.024)
Value × homeowner	...	...	−0.01 (0.04)	...	...
Value × parent	...	...	...	...	0.006 (0.033)
N individuals	314	314	314	314	314
n districts	39	39	39	39	39
Min. individuals per district	1	1	1	1	1
Max. individuals per district	114	114	114	114	114
Pseudo-R^2	0.00	0.09	0.09	0.09	0.09

*$p<0.05$, **$p<0.01$

Table 2. Vote for Proposition 13 by Opinion of School Finance Equalization

	All Voters N (%)	Voted against Proposition 13 N (%)	Voted for Proposition 13 N (%)
Opinion of equalization			
Very unfavorable	26 (100%)	10 (38%)	16 (62%)
Moderately unfavorable	46 (100%)	10 (22%)	36 (78%)
Moderately favorable	169 (100%)	45 (27%)	124 (73%)
Very favorable	353 (100%)	131 (37%)	222 (63%)
Don't know/No answer	22 (100%)	5 (23%)	17 (77%)
Total	616 (100%)	201 (33%)	415 (67%)
Opinion of *Serrano*			
Very unfavorable	48 (100%)	15 (31%)	33 (69%)
Moderately unfavorable	65 (100%)	18 (28%)	47 (72%)
Moderately favorable	198 (100%)	64 (32%)	134 (68%)
Very favorable	281 (100%)	94 (33%)	187 (67%)
Don't know/No answer	24 (100%)	10 (42%)	14 (58%)
Total	616 (100%)	201 (33%)	415 (67%)

different from zero even if we employ an unusually lax criterion for statistical significance ($p < 0.50$).

The null finding was also not attributable to multicollinearity. Although many of the models reported here included a large number of covariates relative to the sample size, valuation per pupil was not highly collinear with any combination of these covariates.[8] Moreover, *all* individual-level covariates other than valuation per pupil were excluded from the equations reported in column 1, and valuation per pupil still appeared to have no measurable effect.

Another possibility is that the null finding could be due to sample selection bias. To test whether this was the case, I reestimated the probit models from columns 1 through 3 on the full CTRS sample with an explicit correction for sample selection (Heckman 1979; Van de Ven and Van Praag 1981). These models are reported in Appendix Table A-4. They did not alter the negative conclusions reported here.[9]

I turn next to individual survey data without district-level controls. Table 2 includes all voters in the FICP, including those who could not be matched to school districts. It reports a simple crosstabulation of respondents' votes on Proposition 13 by their opinions toward school finance equalization. Two findings are especially noteworthy. First, large majorities of voters surveyed expressed favorable views of school finance equalization (85%) and also of the *Serrano* remedy (78%). The general finding that the public supported equalizing revenues between rich districts and poor districts

[8] An OLS regression of valuation per capita on all of the other covariates in model 2 had an unadjusted R^2 of 0.10.

[9] In another set of analyses, I also tested whether a large school district had an undue influence on the results, by excluding that district from the analysis. The results did not change appreciably.

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in the abstract is consistent with some other public opinion research, but this was an unusually large majority for a specific redistributive remedy (Reed 1998, 2001; Tedin 1994; Wassmer 1997). Fischel's argument that the California Supreme Court foisted redistribution on an unwilling electorate is difficult to reconcile with this evidence.

The second important finding is that there was no clear tendency for people who opposed *Serrano* to favor Proposition 13. The top panel of the table does show a weak but statistically significant association between opinion toward equalization and vote for Proposition 13 ($\chi^2 = 9.86$, d.f. = 4, $p < 0.05$). This bivariate association provides some support for H_4, the hypothesis that individuals who opposed school finance equalization in general were more likely to support Proposition 13. But as the bottom panel of Table 2 shows, the vote for Proposition 13 did *not* vary measurably by opinion toward *Serrano* ($\chi^2 = 1.75$, d.f. = 4). The evidence in this panel thus provides no support for H_5, the hypothesis that individuals who opposed *Serrano* in particular were more likely to support Proposition 13.

On balance, then, the public opinion data presented in this table provide little support for the Fischel hypothesis. The results differ between panels because opposition to equalization and opposition to *Serrano* were empirically distinct attitudes: many voters who opposed the redistributive remedy mandated by *Serrano* nonetheless said they supported equalization in principle. Only a handful of voters went further and said that they were opposed to equalization, even in principle. These anti-egalitarian voters were more likely than others to vote for Proposition 13, but their relatively extreme conservatism suggests that they might have been ideologically predisposed to vote for Proposition 13 regardless of the *Serrano* decision. I test this view below by controlling for other characteristics of voters that might have predisposed them to vote for Proposition 13.

Bivariate tests of H_4 and H_5 should not be regarded as definitive. It is conceivable that opposition to equalization simply acted as a proxy for some underlying predisposition that leads voters to favor conservative policies. Alternatively, it is conceivable that other characteristics of voters suppressed the real effect of school finance equalization on the Proposition 13 vote in Table 2. For this reason, I used probit regression to test whether opposition to school finance equalization had any effect on the Proposition 13 vote net of the effects of voters' sociodemographic characteristics. Table 3 reports the results of these analyses. The results were negative. When control variables were introduced, general opposition to school finance equalization had no measurable effect on the vote for Proposition 13. Nor did specific opposition to *Serrano*. The inclu-

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Table 3. Did Opposition to *Serrano* Cause People to Vote for Proposition 13? Probit Regression of Vote for Proposition 13 on Selected Covariates, FICP

	(1) Coeff. (SE)	(2) Coeff. (SE)	(3) Coeff. (SE)	(4) Coeff. (SE)	(5) Coeff. (SE)	(6) Coeff. (SE)
Intercept	0.38 (0.06)**	0.01 (0.26)	0.42 (0.06)**	0.04 (0.27)	0.27 (0.11)**	0.27 (0.44)
Female	...	− 0.07 (0.12)	...	− 0.08 (0.12)	...	− 0.26 (0.17)
Age	...	0.009 (0.004)*	...	0.009 (0.004)*	...	0.012 (0.006)
Income	...	0.001 (0.005)	...	0.0009 (0.005)	...	− 0.014 (0.007)
Some college	...	− 0.19 (0.14)	...	− 0.20 (0.14)	...	− 0.14 (0.21)
B.A. or higher degree	...	− 0.60 (0.16)**	...	− 0.61 (0.16)**	...	− 0.63 (0.22)**
Black	...	− 0.53 (0.27)	...	− 0.54* (0.27)	...	− 0.73 (0.40)
Homeowner	...	0.37 (0.14)*	...	0.36 (0.14)*	...	...
Parent of schoolchildren	...	0.01 (0.13)	...	0.02 (0.13)	...	0.12 (0.18)
Opposes equalization	0.31 (0.18)	0.24 (0.19)	...	...	...	...
Opposes *Serrano*	...	...	0.12 (0.14)	0.17 (0.15)	...	0.26 (0.21)
Expected tax increase	...	...	...	...	1.02 (0.24)**	1.26 (0.27)**
Expected tax increase, squared	...	...	...	...	− 0.19 (0.05)**	− 0.23 (0.05)**
N	548	548	543	543	335	335
Pseudo-R^2	0.00	0.06	0.00	0.06	0.05	0.12

Notes: Sample includes all voters in the FICP (cols. 1–4); voter-homeowners only (cols. 5–6).
*$p < 0.05$, **$p < 0.01$

sion of social and demographic covariates did not reveal any suppressed association between opinion and the Proposition 13 vote. The effects that are evident in these models are consistent with the literature on voter support for tax limitations: homeowners and older voters favored limiting the property tax, while black voters and highly educated voters did not (Courant et al. 1980; Lowery & Sigelman 1981; Ladd & Wilson 1983; Sears & Citrin 1985). Knowing whether voters were for or against school finance equalization adds nothing to our ability to explain the vote for Proposition 13.

To sum up briefly, I found no support for the Fischel hypothesis that court-ordered school finance equalization caused the property tax revolt. Instead I found a substantial body of evidence for the contrary conclusion: school finance equalization was *not* an important cause of voters' propensity to favor property tax limits.

By contrast, I found support for the inflation hypothesis. The greater the property tax increase that a homeowner expected, the more likely he or she was to vote for Proposition 13. The quadratic term indicates that the effect of property tax increases was nonlinear: property tax increases greater than approximately $2,700 had no further positive effect, because almost all homeowners who anticipated tax increases of this magnitude were already voting for Proposition 13. Below this level, the effect of rising property taxes appeared positive, substantial, and statistically significant. The effect of housing inflation was significant even though the analysis was restricted to a comparatively small sample of homeowners. Moreover, it appeared significant even when other influences on the vote were held constant, as in column 6 of Table 3. This finding supports Stark and Zasloff's (2003) argument that inflation-driven tax increases, rather than redistribution, were to blame for Proposition 13.

The models in Table 3 also identify several social and demographic influences on voters' choices. Older voters were likely to favor Proposition 13, controlling for the school district and the presence of children. This finding is also consistent with Stark and Zasloff's (2003) interpretation of their aggregate results and inconsistent with the interpretation of the same results put forward by Fischel (2004). College graduates were more likely than nongraduates to oppose Proposition 13. Homeowners were more likely than renters to favor it. As I note above, these findings are also consistent with prior research on Proposition 13 and analogous initiatives in other states. In general, the low values of McFadden's pseudo-R^2 for the models reported here indicate that none of them fit the data closely. This negative finding, too, is consistent with the literature, although the models that included opinion toward *Serrano* as the main explanatory variable appeared to do somewhat worse than models including a different battery of

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ideological and attitude measures.[10] In short, the Fischel hypothesis does not improve our ability to explain the tax revolt.

Does School Finance Litigation Undermine Support for Public Schools?

School finance litigation has continued since *Serrano*. According to one count, by 1998, 19 state courts had ruled against all or part of their states' school finance systems, and another 12 courts were considering challenges (Minorini & Sugarman 1999).[11] Will these decisions undermine support for public schools? The Fischel hypothesis would suggest a positive answer. Any fiscal equalization across districts will undermine support for school taxes, on this view, by interfering with the market that allows homeowners to choose the public education they want at the price—in property taxes and mortgage bills—that they can afford. Fischel (2002) offers this account of Proposition 13 as a cautionary tale. Prior to equalization, the story goes, California homeowners accepted their property taxes as a fair price for the public schools they had chosen; after equalization, they resented their property taxes as a burden that bore little relation to the service they received. They rebelled.

And yet, I have argued, this story is incorrect. The Fischel hypothesis fails to explain why voters rejected the property tax in California. First, Fischel's assertion that public education functioned as a market is probably wrong. In order to present public education as a market, Fischel must argue that the local property tax was capitalized in housing prices, so that people paid what was effectively a market price for public education when they bought into a school district. Although most economists now appear to think the property tax is at least partly capitalized in housing prices, however, Fischel's assumption of 100% capitalization is higher than most of the estimates reported in the empirical literature (see Palmon & Smith 1998). The conclusion that the property tax is perfectly capitalized depends on models that make psychologically unrealistic assumptions about the rationality of potential home buyers (see Shafir & LeBoeuf 2002) and sociologically unrealistic assumptions about the absence of barriers to residential mobility (Ladd & Yinger 1999). If these assumptions are not met, then the

[10] In their own analyses of postelection survey data on support for Proposition 13, including ideological and party identification variables not included in the present analysis, Sears and Citrin (1985) report values of R^2 averaging 0.14; with analyses limited to Proposition 13 voters, they averaged 0.19. See also Lowery and Sigelman 1981.

[11] With the 2005 decision of the Kansas Supreme Court in *Montoy v. State of Kansas* (278 Kan. 769), the number of state courts that have ruled against school finance systems is now at least 20. I thank an anonymous reviewer for drawing my attention to this case.

cost of public schooling is not distributed as a market price or benefit tax.

Second, Fischel's assumption that voters *perceived* the local property tax to be a fair price for local services is probably also wrong. It is certainly at odds with the available public opinion data. In national opinion polls conducted for the U.S. Advisory Commission on Intergovernmental Relations (USACIR) in 1972, 1973, 1975, and 1977, a plurality of respondents in each poll rated the property tax as "the worst tax, that is, the least fair" (USACIR 1991:4). And, as noted above, the Californians polled by the Field Institute in summer 1978 overwhelmingly supported the redistribution of tax revenues from rich to poor school districts. Fischel's strongest evidence that voters preferred the local property tax to other ways of financing public schools is simply the fact that the local property tax had survived for a long time (Fischel 1992:175). As many institutionalists in economics, sociology, and political science have argued, however, suboptimal institutional arrangements may persist despite individual preferences (Hall & Taylor 1996; Thelen 1999).

The criticism of the Fischel hypothesis presented here has broader implications for the study of school finance litigation. First, of course, if the Fischel hypothesis fails in California, it fails in general. The literature on the Fischel hypothesis treats California as a crucial case. Both *Serrano* and Proposition 13 were recognized by contemporaries as events of national significance. Both school finance reforms and tax legislation in other states may have been designed deliberately to preempt the kind of antitax backlash that California represented in the national imagination. Thus, as Fischel (2002) has argued, the long shadow cast by Proposition 13 may distort our view of the causal relationship between court-ordered school finance equalization and tax revolt in other states. The relationship should be visible in California if it is visible anywhere.

Second, claims about the unintended impact of judicial policymaking deserve closer scrutiny. Many studies address the question of whether judicial policy has its intended impact. Since Rosenberg (1991), the burden of proof has lain with those who claim that an activist judiciary can be effective, and much of the recent literature on school finance equalization shoulders this burden. But claims about the *unintended* impacts of judicial policy are often subjected to a less rigorous standard. As the career of the Fischel hypothesis shows, the scholarly community is sometimes quick to accept clever theoretical arguments about unintended consequences without adequate empirical testing.[12] The most

[12] Bogart's (2002) wide-ranging empirical study of the unintended consequences of law is an important and refreshing exception.

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general implication of this article is that in the case of unintended consequences, too, the burden of proof lies with those who see law as having a large impact. Claims about the unintended impacts of law should be neither more nor less empirically suspect than claims about the intended impacts of law.

Finally, of course, judicial policies are not the only events that transform the law with potentially perverse consequences. Consider Proposition 13. This constitutional amendment made it more difficult for state and local governments—including school districts—to raise revenue. Many observers argue that this tax limit thereby diminished the quality of public education in California. As the quality of the schools has declined, critics argue, the schools have become less popular with voters. In short, Fischel may have it backward. It may be Proposition 13, rather than *Serrano,* that undermined support for public education (Rubinfeld 1995; McUsic 1999).

Appendix

I calculated assessed valuation per high school pupil in each California school district from published institutional data on assessed valuation and high school attendance for the fiscal year 1977–78 (California State Department of Education 1979: Table IV-14).

In order to test hypotheses H_1 through H_3, which concern the effect of school district finances on individuals' propensity to vote for Proposition 13, it was necessary to match these institutional data to records of individual survey respondents. The most specific geographic information available for each individual survey respondent was a zip code. The key to the match was therefore a crosswalk file that would uniquely cross-reference zip codes with school districts. Because school district and zip code area boundaries

Table A-1. Characteristics of the Full and Merged Samples of California Voters from the FICP

	All Voters (N = 563)				Merged Sample (N = 99)			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Female	0.51	0.5	0	1	0.56	0.5	0	1
Age	45.8	16.4	18	89	46.2	17.2	20	88
Some college	0.35	0.48	0	1	0.38	0.49	0	1
B.A. or higher degree	0.28	0.45	0	1	0.26	0.44	0	1
Income	21.8	13.1	1.5	54.2	19.4	14	1.5	54.2
Homeowner	0.75	0.43	0	1	0.52	0.5	0	1
Black	0.04	0.2	0	1	0.11	0.32	0	1
Parent of schoolchildren	0.37	0.48	0	1	0.31	0.47	0	1
Voted for Prop. 13	0.67	0.47	0	1	0.63	0.48	0	1
Value per pupil (in $10,000s)	...	...	...	...	8.56	3.2	4.17	16.78

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Table A-2. Characteristics of the Full and Merged Samples of California Voters from the CTRS

	All Voters (N = 989)				Merged Sample (N = 215)			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Female	0.53	0.5	0	1	0.47	0.5	0	1
Age	46.5	16.7	18	92	47.7	17.9	20	88
Some college	0.21	0.41	0	1	25.9	19	0	1
B.A. or higher degree	0.31	0.46	0	1	0.18	0.38	0	1
Income	25	17.7	2.5	64.9	0.41	0.49	1.5	54.2
Homeowner	0.68	0.47	0	1	0.54	0.5	0	1
Black	0.04	0.21	0	1	0.07	0.26	0	1
Parent of schoolchildren	0.28	0.45	0	1	0.22	0.41	0	1
Voted for Prop. 13	0.7	0.46	0	1	0.61	0.49	0	1
Value per pupil (in $10,000s)	...	...	...	...	9.97	4.91	4.17	16.78

change frequently, the crosswalk file had to refer to these geographic areas as they were circa 1978—that is, approximately when the vote on Proposition 13 was taken and the surveys were done.

No such crosswalk existed. I therefore constructed one using the MARFs that were compiled for the 1980 census. These files were compiled to produce special tabulations of population by school district and by zip code. MARF 3 matches census block groups and enumeration districts to school districts (U.S. Bureau of the Census 1983a). MARF 5 matches census block groups and enumeration districts to zip codes (U.S. Bureau of the Census 1983b).

The match was complicated by the fact that any Californian in 1978 might live within the boundaries of two school districts: an elementary school district that governed the local elementary school or schools, and a larger high school district that governed the local high school or schools. High school districts did not overlap with one another, but each high school district encompassed at least one, and more typically several, elementary school districts. The typical elementary school district was fully contained within a single high school district, but a few overlapped with more than one. In addition to these two types of districts, there were so-called unified school districts that governed both elementary and high schools within their boundaries.

This complication might itself be regarded as evidence against Fischel's assumption that every voter could easily calculate his or her interest in the outcome of *Serrano*. In the interest of providing a generous test of the hypothesis, I nevertheless attempted to eliminate ambiguity by dropping all elementary school districts from MARF 3 and limiting my attention to high school districts and unified school districts. These districts were geographically larger than elementary school districts, and they did not overlap with one another. Limiting the file in this way therefore increased the prob-

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Table A-3. List of Included School Districts

District	County	CTRS	FICP
ABC Unified	Los Angeles	x	
Baker Valley Unified	San Bernardino	x	
Barstow Unified	San Bernardino	x	
Berkeley Unified	Alameda	x	x
Beverly Hills Unified	Los Angeles	x	
Carpinteria Unified	Santa Barbara		x
Chaffey Union High	San Bernardino	x	
El Dorado Union High	El Dorado		x
El Monte Union High	Los Angeles	x	x
Fremont Union High	Santa Clara	x	
Fresno Unified	Fresno	x	x
Fullerton Joint Union High	Orange	x	x
Garden Grove Unified	Orange	x	x
Glendale Unified	Los Angeles	x	x
Long Beach Unified	Los Angeles	x	x
Los Angeles Unified	Los Angeles	x	x
Lucia Mar Unified	San Luis Obispo	x	
Mountain View-Los Altos Union	Santa Clara		x
Mt. Diablo Unified	Contra Costa		x
Oakland Unified	Alameda	x	x
Orange Unified	Orange	x	
Oxnard Union High	Ventura	x	
Pajaro Valley Joint Unified	Santa Cruz	x	
Pasadena Unified	Los Angeles	x	
Richmond Unified	Contra Costa	x	x
Sacramento City Unified	Sacramento	x	x
San Diego City Unified	San Diego	x	x
San Francisco Unified	San Francisco	x	x
San Lorenzo Valley Unified	Santa Cruz	x	x
San Mateo Union High	San Mateo	x	x
Santa Barbara High	Santa Barbara	x	x
Santa Monica-Malibu Unified	Los Angeles	x	x
Sequoia Union High	San Mateo		x
Sierra-Plumas Joint Unified	Sierra	x	
Sonoma Valley Unified	Sonoma	x	
South Bay Union High	Los Angeles	x	
Stockton City Unified	San Joaquin	x	
Sweetwater Union High	San Diego	x	
Tamalpais Union High	Marin	x	
Torrance Unified	Los Angeles	x	x

ability that I could assign any individual survey respondent unambiguously to a single district.[13]

By joining the resulting file to MARF 5, I was able to match each zip code to each unified or high school district with which it shared one or more block groups. I dropped all zip codes that corresponded to more than one unified or high school district from the crosswalk file. The result was a crosswalk that identified the unique high school district or unified school district corresponding to each of a subset of California zip codes. The file re-

[13] I also dropped six small, rural districts whose MARF 3 identification codes I was unable to match to state data on assessed valuation per pupil. They were Ione Unified, Oro Madre Unified, Jackson Unified, Moorpark Memorial Union High, Kerman Union High, and Central Union High (in Fresno County).

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tained only zip codes whose areas corresponded exactly to, or were fully contained within, the boundaries of a school district. This rule ensured that knowledge of the zip code in which an individual resided was sufficient to identify the school district in which he or she resided. Unfortunately, this precision came at the price of discarding the majority of zip codes and the majority of individual survey respondents.[14]

With this crosswalk file, I matched data on assessed valuation per pupil to subsets of survey respondents in both the FICP and CTRS files. Tables A-1 and A-2 describe the characteristics of the corresponding merged samples, in comparison to the characteristics of *all* voters included in the FICP and CTRS samples, respectively. In both the FICP and the CTRS, the voters who could be matched to school districts were substantially more likely than the average voter to be renters, and less likely than the average voter to have school-age children. They were also more likely to be black, and less likely to vote for Proposition 13. All of these characteristics suggested that the merged samples were biased toward urban voters. This impression of urban bias was confirmed by inspection of the list of school districts represented in the merged samples (Table A-3). Large urban districts such as Los Angeles and San Diego were well represented; suburban and rural districts less so. Both merged samples nevertheless included districts with a wide range of property valuation per pupil. Assessed valuation per high school pupil in the districts in the FICP subsample ranged from $41,704 to $167,805, and in the CTRS subsample it ranged from $40,296 to $337,912.

To what degree did the biases in the sample affect the conclusions of the analysis? Formally, using a technique developed by Heckman (1979), I estimated maximum likelihood probit models with an explicit model of sample selection on the full sample of 1,003 Californians in the CTRS with complete data who reported casting a vote for or against Proposition 13. The selection equation included exogenous variables for age, homeownership, race, income, and years lived in California (coded as a pair of dummy variables, one for 5–19 years and one for 20 years or more). The results of the probit regression of the Proposition 13 vote corrected for sample selection are reported in Table A-4. They did not change the main conclusion of the analysis: namely, valuation per

[14] Data permitting, a less wasteful alternative would have been to assign a value per pupil to each zip code by taking a population-weighted sum over all the school districts with which that zip code overlapped. This was not possible because the MARFs do not contain sufficient information to calculate the appropriate weights. For example, while it was possible to conclude that 75% of the residents of a block group resided in school district X, and 75% of the residents of the same block group resided in zip code Y, it was not possible to discern whether it was the same 75% in each case.

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Table A-4. Results from Probit Regression with Sample Selection, CTRS

	Vote for Proposition 13			Selection Equation		
	(A1)	(A2)	(A3)	(A1)	(A2)	(A3)
Intercept	−0.13 (0.94)	−0.15 (0.94)	0.02 (1.00)	−0.49 (0.21)*	−0.49 (0.21)*	−0.49 (0.21)
Female	−0.24 (0.19)	−0.24 (0.19)	−0.24 (0.19)			
Age	0.005 (0.006)	0.005 (0.006)	0.005 (0.006)	0.009 (0.003)**	0.009 (0.003)**	0.009 (0.003)**
Income	0.01 (0.006)†	0.01 (0.006)†	0.01 (0.006)†	0.008 (0.003)**	0.008 (0.003)**	0.008 (0.003)**
Some college	−0.43 (0.27)	−0.43 (0.27)	−0.41 (0.27)			
B.A. or higher degree	−0.74 (0.25)**	−0.74 (0.25)**	−0.73 (0.24)**			
Black	−0.20 (0.37)	−0.20 (0.37)	−0.21 (0.37)	0.49 (0.21)*	0.49 (0.21)*	0.49 (0.21)*
Homeowner	0.41 (0.37)	0.47 (0.55)	0.43 (0.37)	−0.47 (0.10)**	−0.47 (0.10)**	−0.47 (0.10)**
Parent of schoolchildren	−0.16 (0.23)	−0.16 (0.24)	−0.36 (0.46)			
Residence in CA 5–19 years	...	...	...	−0.60 (0.19)**	−0.60 (0.19)**	−0.60 (0.19)**
>20 years	...	...	...	−0.13 (0.11)	−0.13 (0.11)	−0.13 (0.11)
Value per pupil	−0.008 (0.02)	−0.006 (0.022)	−0.018 (0.027)	...	...	...
Value × homeowner		−0.006 (0.039)		...	...	...
Value × parent			0.02 (0.04)	...	...	...
N individuals	215	215	215	989	989	989
n districts	34	34	34			
Min. individuals per district	1	1	1			
Max. individuals per district	72	72	72			
Pseudo-R^2	0.02	0.02	0.02			

†$p<0.10$, $p<0.05$, **$p<0.01$

pupil appeared to have had zero effect on the Proposition 13 vote. The main difference between Table A-4 and the results in Table 1 appears to be that the coefficient of homeownership was much less sharply estimated when sample selection was taken into account.

Heckman's (1979) correction for sample selection bias is asymptotically unbiased and efficient when the true selection process is correctly specified. In small samples such as the present one, however, Monte Carlo studies have shown that its performance is uneven (Stolzenberg & Relles 1990). The key point of this table, then, is not that the selection-adjusted estimates are superior to the estimates reported in Table 1, but that they do not provide any reason to question the negative conclusion reported there. Readers who are uncomfortable with this method should rely instead on the more direct tests of the Fischel hypothesis that are reported in Table 3. These tests use FICP data and are not subject to the same sample selection biases as the analysis reported in Table A-4.

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