

Proposition 13 and The California Shell Game

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Proposition 13 and The California Fiscal Shell Game

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ABSTRACT

Here, we study the effects of California's Tax and Expenditure Limitations, especially Proposition 13. We find that Proposition 13 was indeed effective at reducing both ad valorem property taxes per capita and total state and local taxes per capita, at least in the short run. We further argue that there have been unintended secondary effects that have resulted in an increased tax burden, undermining the aims of Proposition 13. To circumvent the limits imposed by Proposition 13, the state has drastically increased nonguaranteed debt, has privatized the public fisc, and has devolved the authority to lay and collect taxes and to spend the proceeds so gained. The devolution of authority has been among the swiftest growing aspects of government finance in California, to a far greater extent than in other states. Lastly, we argue that the new tax and spending authorities that have been created to circumvent Proposition 13 have led to a reduction in government transparency and accountability and pose an increasing threat to our democracy. Our replication data set and STATA coding can be found at <http://mmccubbins.usc.edu>.

Proposition 13 and The California Fiscal Shell Game

Californians have long been displeased with their government. The presence of the state's largely unchecked initiative process, however, has allowed Californians to “[vent] their frustrations with government by drafting and passing *direct initiatives*” (Gerber et. al 2001). Only the citizens of Oregon passed more initiatives in the 20th century than did Californians.

Recent turns in the business cycle have accentuated Californians' frustrations with their government. Indeed, despite having enacted tax and expenditure limitations (TELs) in the form of Propositions 13 and 4 in 1978 and 1979 Californians have continued to tinker with their government's fiscal institutions. Voters chose to amend Proposition 4 (also known as the Gann Initiative) twice, through Propositions 98 and 111. They have additionally rejected a slew of other proposals. Proposition 13 has been similarly amended. Presently there are more than fifty initiatives circulating in California, with another dozen waiting for the Attorney General to approve them for circulation, and with four having qualified for the ballot. Of these proposals, four call for Constitutional Conventions, six deal with taxes, one seeks to repeal Proposition 13 and several others seek to establish a new TEL, and three seek to change the state's budget process, despite the passage in 2004 of a balanced budget initiative. Given this level of activity, and with several more proposals circulating in California, the question becomes how effective have these measures been at actually limiting taxes and expenditures?

Despite the faith voters seem to put in them, recent research on TELs suggests that, all else constant, they are generally ineffective in their primary goal of limiting spending and/or

revenues (Kousser et. al 2007, Kousser et. al 2008, McCubbins 2008)³. There are two principal reasons for their failure. First, as is true of any initiative (see Gerber et. al 2001, 2004), TELs need to be implemented and enforced by the same government officials who are the target of the limitation. This creates a classic agency problem and we should expect agency losses to the extent that voters cannot monitor the actions of the state government.⁴ Kiewiet and Szakaly (1996) call this the “circumvention hypothesis.” State governments often carry out such circumvention by burying spending within non-germane bills, by devolving fiscal responsibility to other agents who are not subject to the limitations, by inventing new debt mechanisms, and by privatizing public policy.

Second, although the voters use TELs to erect obstacles to taxation, those same voters continue to demand increased government spending, inducing legislators to bypass the voters’ obstacles. There are myriad possible explanations for the apparent contradiction in voters’ preferences: voters’ preferences may be changing; differential turnout may change the composition of the electorate in each election; the statewide initiative electorate may differ from the district-based constituency of the State Assembly and Senate; the existence of TELs may affect voters’ preferences; or voters may simply hold contradictory preferences. These are examples of a time-inconsistency problem facing voters who must consider choices offered to them on the ballot. Thus, there will be an incentive to find a way around a TEL. This, in turn, causes greater citizen frustration with the legislature and further calls for newer and better TELs.

In what follows, we show that California’s Proposition 13 was indeed effective at reducing both ad valorem property taxes and total state and local taxes, at least temporarily. We

³ Others, however, have argued that these limitations are indeed effective. See Elder (1992), Shadbegian (1998), Bails and Tieslau (2000), and New (2001).

⁴ For more on agency problems and agency loss see Kiewiet and McCubbins (1991] Epstein and O’Halloran (1999).

also argue that there have been insidious and unintended secondary effects that result in Proposition 13 being undercut. We also find that the state government's efforts to bypass Proposition 13 has been one cause of the increase in nonguaranteed debt, the privatization of the public fisc, and the devolution of democracy in the state. We will argue further that the new tax and spending authorities that have been created to circumvent the strictures of Proposition 13 have led to a reduction in government transparency and a loss of accountability. In our discussion, we will provide a brief history that shows how California's fiscal landscape has changed in response to the passage of Propositions 13 and 4.

The California Tax Revolt: Proposition 13 and Proposition 4

Californians passed Proposition 13 in June of 1978, limiting ad valorem property taxes to 1% of the property's assessed value, and limiting increases in assessed value to 2% per year. This measure, designed to prevent property taxes from pricing residents out of their homes, was passed by nearly a two-thirds margin and remains popular to this day. The years preceding the passage of Proposition 13 saw especially significant gains in property tax revenue collected, with the share of state and local revenues derived from property taxes increasing from 34% at the turn of the decade to 44% in 1978 (Schwartz 1998). California housing prices doubled while income remained flat in the five years leading up to passage of the initiative, so that many Californians, especially retirees, were at risk of being taxed out of their homes, This made Proposition 13 very popular.

In 1979, Proposition 4, known as the Gann Initiative, advanced the broader agenda set by Proposition 13. The Gann Initiative extended limitations across all taxes in an effort to prevent the rapid growth in government seen in the 1960s and early 1970s. The Gann Initiative pegged spending to the baseline year 1979, and limiting budget increases each year as a function of

inflation and population growth. The Gann Initiative required a tax refund to be paid out to taxpayers when revenues overran capped expenditures.

Other tax limitations, which have been proposed since, have not been nearly as popular and most have been rejected. Indeed, Californians regularly accept increases in sales taxes, charges and fees, and assessments, which are collected as part of a property owner's tax bill, but are not pegged to the value of the property. Further, Californians regularly pass bond measures and other spending requirements on the state government. For example, recent bond measures committed the state to support stem cell research and build high-speed trains.

Research Design and Data

The most common approach to testing the effect of TELs has been to assume that all states are the same and that all TELs are the same, and that the state finances, and the passage of TELs are independent.⁵ Of course, states are not all the same. Wyoming has little in common with California and Maine is not identical to Florida. Similarly, Proposition 2 in Massachusetts, which limited property taxes, is different from TABOR in Colorado, which attempted to limit general fiscal policy. Lastly, states' finances are often correlated, one to the other, and the

⁵Abrams and Dougan 1986; Elder 1992; Shadbegian 1996; Mullins and Joyce 1996; Shadbegian 1998; Bails and Tieslau 2000; New 2001; Mullins 2004; Mullins and Wallin 2004. These studies have had mixed results: some find that TELs have produced a significantly negative effect in government spending or revenue (Elder 1992, Bails and Tieslau 2000, New 2001), while some have found no statistically significant effects on either part of government fiscal behavior (Abrams and Dougan 1986, Mullins and Joyce 1996).

passage of a TEL in one state often leads to the introducing of TELs in other states (see Moule and Weller 2009), thus violating the stable unit treatment value assumption, or SUTVA (see Rubin 1978).

Existing panel studies almost always assume that a set of “control variables” or covariates make the subjects of these studies (states) otherwise identical. Furthermore, the “treatment” in these studies is supposed to be identical, applied in an identical manner to these identical subjects. Of course, none of the techniques used, from differences-in-differences (Wooldridge 2002) to a random coefficients model get around the fact that we do not have random assignment of subjects to treatment and control, and thus cannot overcome the fundamental problems of causal inference (Holland and Rubin 1988). The essence of the FPCI is that we cannot, for example, observe fiscal policy in California after 1979 for both the true case, in which Proposition 13 was passed, and the counterfactual case where it did not (Sekhon 2004).

There are problems that cannot be overcome, however. SUTVA demands that all subjects and treatments be identical and one cannot get around the fact that the subjects and treatments in these types of studies are not. Thus, there are large threats to conclusion validity. For example, we know from these studies that the Census sends out a survey to the states, that different states have different officers filling out the survey, and that the identification of these officers changes from state to state and in the same state over time. This is an instrumentation threat. Following the law of anticipated reactions (Almond and Verba 1963, Weingast 1979), state legislatures in one state may, after witnessing the passage of a TEL in another state, adapt their fiscal policy (or at least the reporting of their fiscal policy) in order to avoid a TEL of their own. This is a social-interaction threat. Furthermore, the state’s fiscal behavior and the passage of a TEL may be endogenous to political and economic conditions within the state. This is a selection-history

threat. Typical methods used to study policy effects such as differences-in-differences (Wooldridge 2002), time-series cross-sectional methods with panel-corrected standard errors (Beck and Katz 1995), while they have their merits, do nothing to mitigate these problems if and when they exist.

As such, in order to better study the effects of these measures, we will explore the “gold-standard” research design employed in Kousser et al. (2004, 2008) and suggested in the financial literature (for a survey see MacKinlay 1997), which is a single state event study with a benchmark comparison (McCubbins 2008, McCubbins and Moule 2010) or a comparable interrupted time-series design. Before doing this, it is useful to see the data.

Figure 1 displays California’s real property tax per capita. A reference line is added to represent the passage of Proposition 13. It is apparent in Figure 1 that there is, indeed, a large drop in the amount of per-capita property taxes collected. This is consistent with our results. Notice that, although property taxes have increased over time, they have never caught up to their original pre-1978 level. This is an early indication that Proposition 13 was successful in limiting taxes.

Figure 1: Real Property Tax Per Capita in California, 1971-2000

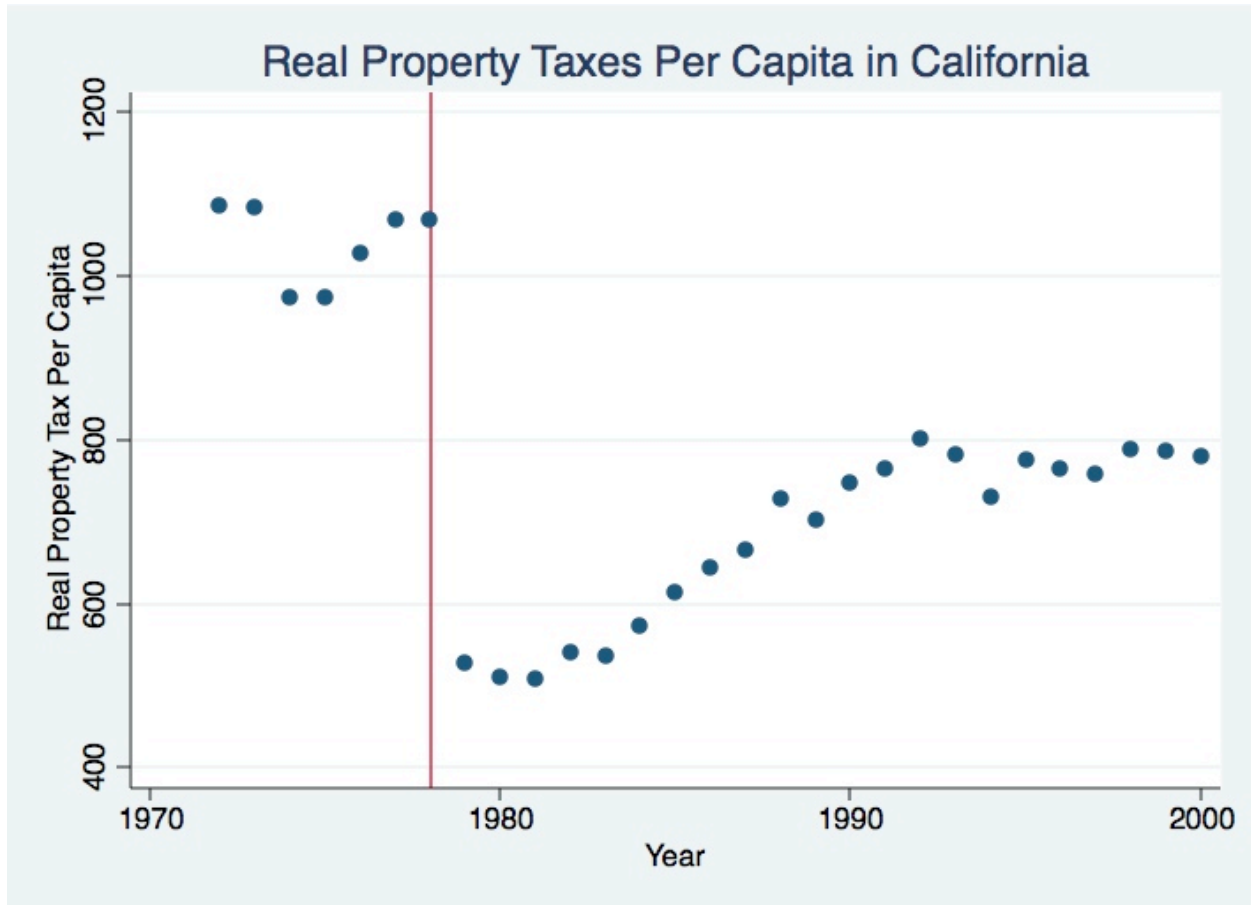


Figure 2, which displays the total tax revenue collected in California, shows a similar drop. However, unlike property taxes, that amount of total tax revenue catches and exceeds its pre-1978 level within the decade.

Figure 2: Real Total Taxes Per Capita in California, 1971-2000

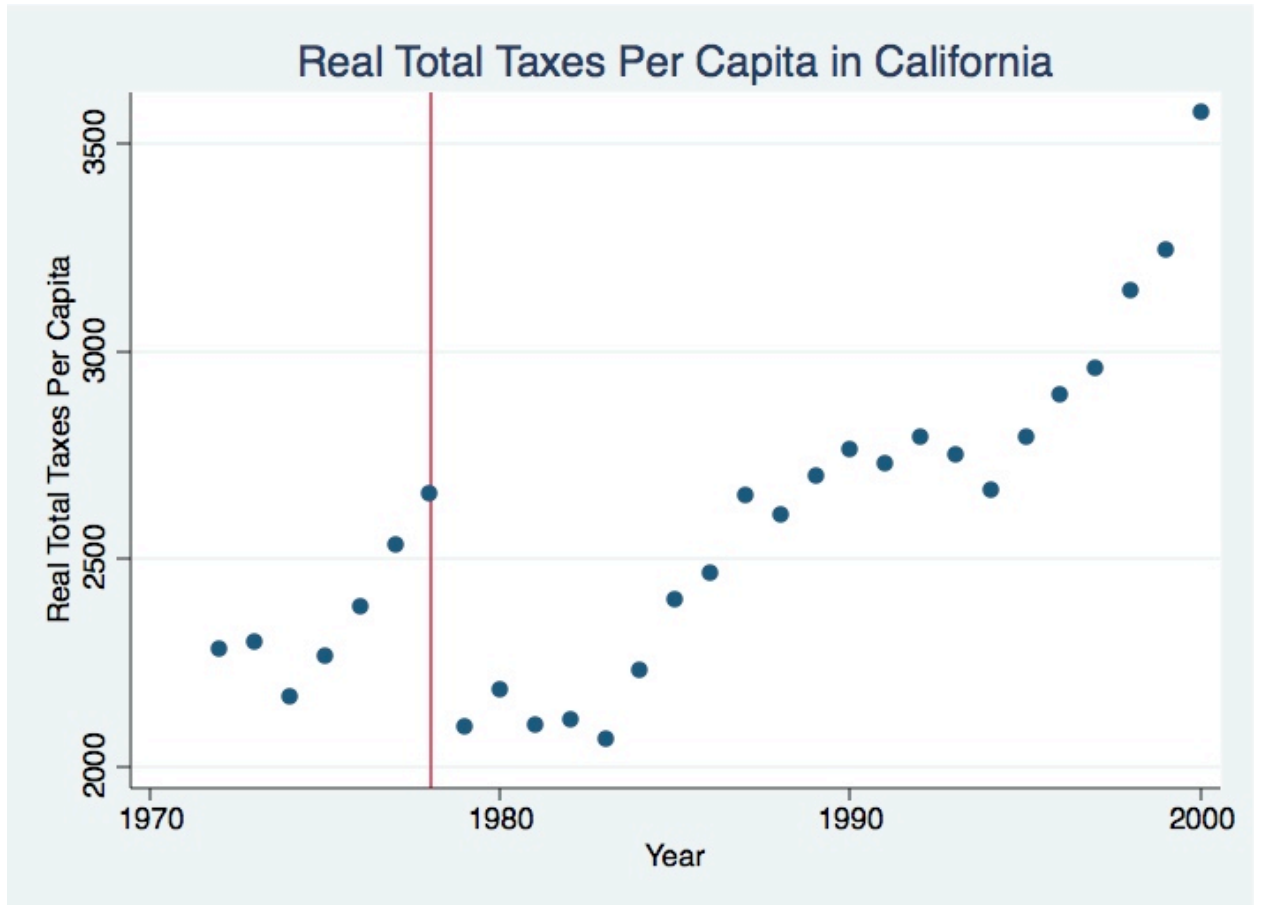


Figure 3 shows real per capita expenditures in California. Unlike the previous two graphs, there is no discernable drop. In fact, California's expenditure increased substantially approximately 6 years after the implementation of Proposition 13.

Figure 3: Real Expenditure Per Capita in California, 1971-2000

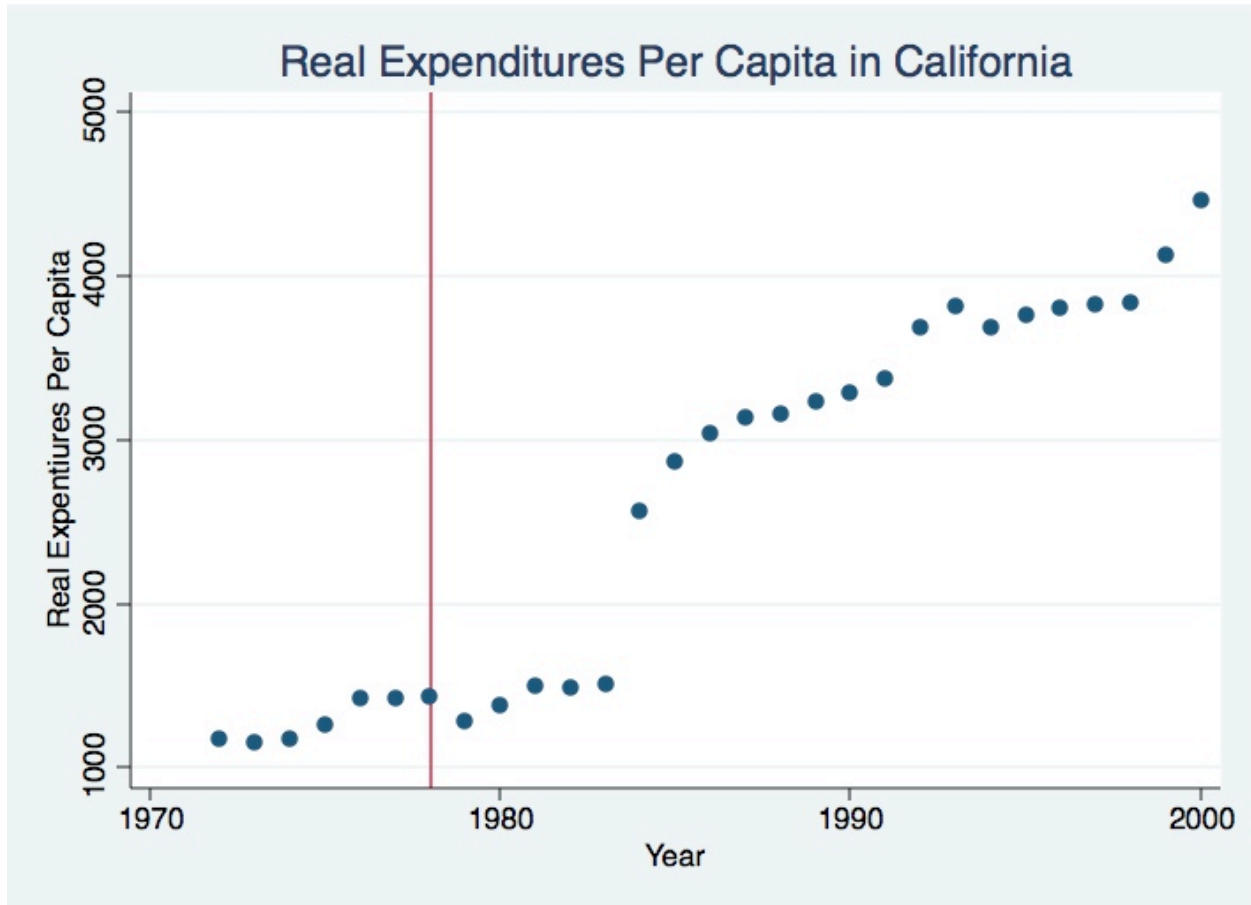
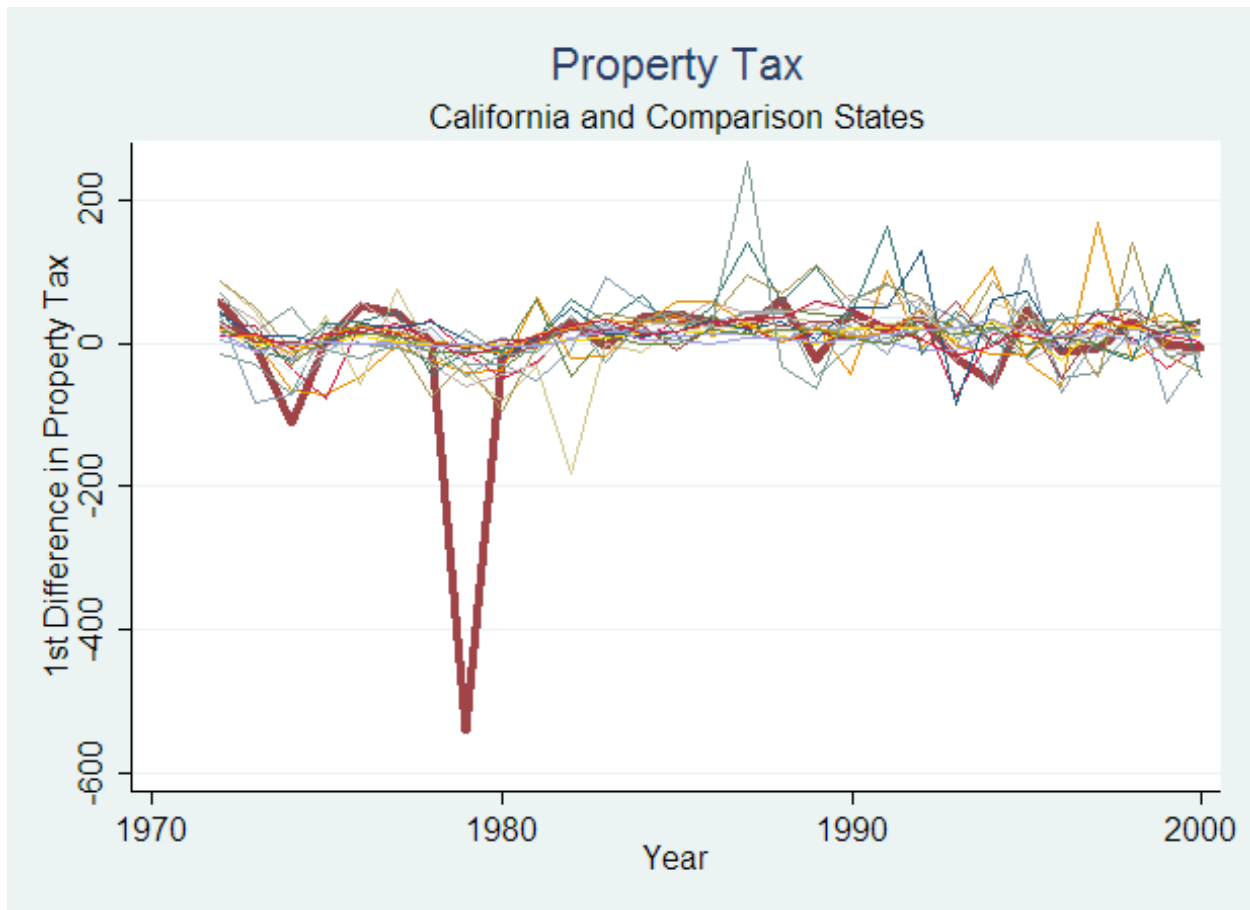


Figure 4 displays the first-differences in property taxes (that is the increases or decreases) per capita for California and a set of comparable states (labeled comparison states in Figure 4). This corroborates the evidence displayed in Figure 1 and accentuates the drop seen in California's property tax collections.

Figure 4: First Difference in Property Taxes Per Capita across a Panel of States, 1971-

2000

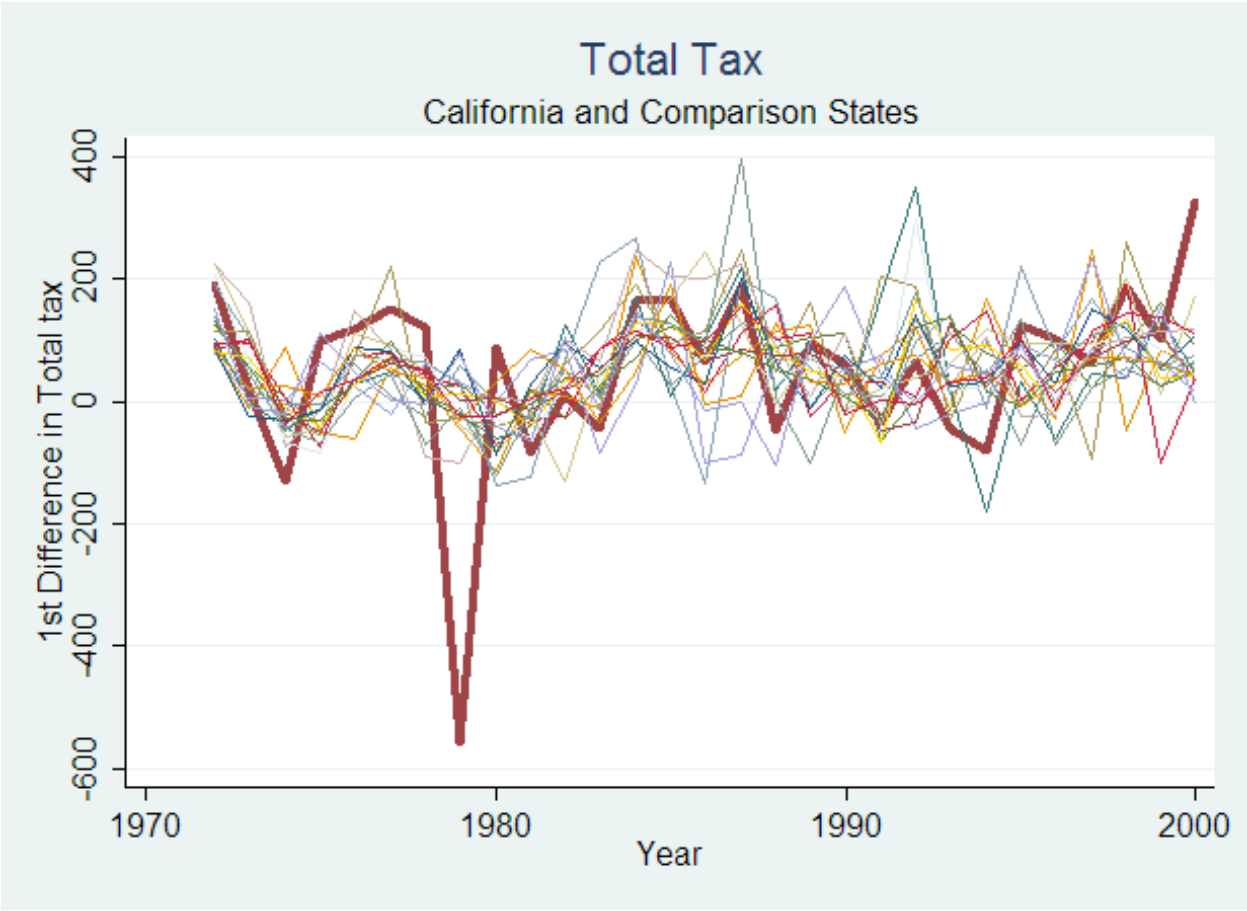


Note: The line for California is in bold. All numbers are multiplied by 1×10^8 for graphical purposes

By this measure, Jarvis, Gann, and company were successful, and it is this data that they can point to in support of their claim of effectiveness. One thing to note is that real property taxes per-capita have still been increasing (after, of course, the dramatically large drop seen in 1979) at a steady rate, at some points even peaking above most of the comparison states.

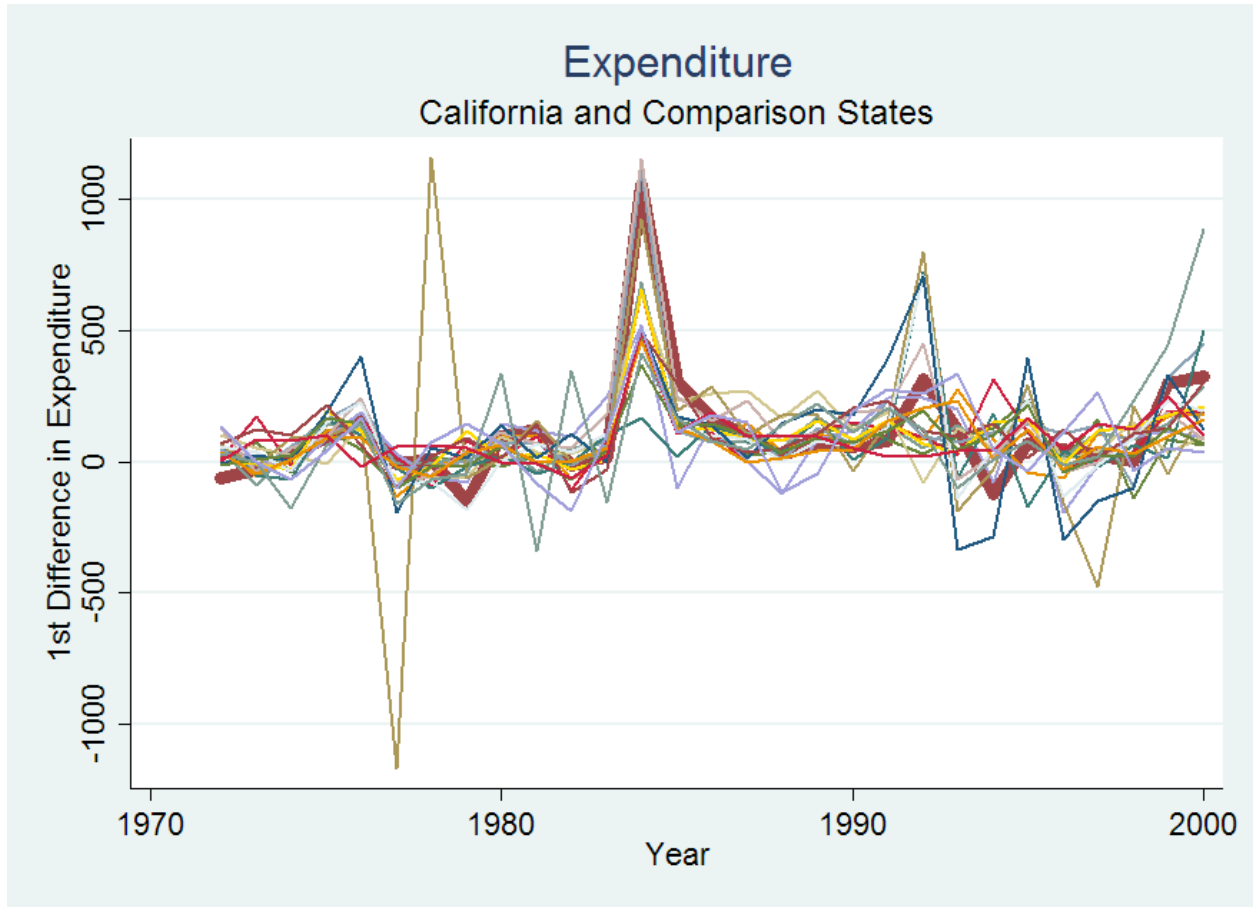
Similarly, we can look at the effect of Proposition 13 on total state and local tax revenues. Figure 5 displays first differences in total state and local taxes for California and a set of comparison states from 1971 to 2000. As was seen in Figure 4 with respect to property taxes, we can see a huge drop in total state and local tax revenues in the year following the enactment of Proposition 13. Figure 6 displays first differences in total state and local expenditures. There is not the same kind of change in this graph; indeed, it would seem that expenditures were unaffected by Proposition 13 (as well as Proposition 4, as there is no decline in 1980 or 1981). The line with the largest swing prior to 1980 is New York, which was suffering in the 1970s where the drop occurs. Also in evidence are the economic recovery in the mid-1980s and the recession and recovery in the early 1990s and late 1990s, respectively, although interestingly not every state seemed to share in the latter recovery.

Figure 5: First Difference in Total State and Local Tax Revenues Per Capita across Panel of States, 1971-2000



Note: The line for California is in bold. All numbers multiplied by 1×10^8 for graphical purposes.

Figure 6: First-Difference in Total State and Local Expenditures Per Capita across Panel of States



Note: The line for California is in bold. All numbers multiplied by 1×10^8 for graphical purposes.

To test the null hypothesis that Proposition 13 was ineffective, we ran the following regression:⁶

⁶ Due to the near simultaneous adoption of the two propositions, we cannot sort out the effect of Proposition 13 from Proposition 4 with the data and methods used here.

$$\Delta(\text{FB}_t) = \alpha_t + \beta(\text{Prop13}_t) + \gamma_1 \Delta(\text{PCI}_t) + \gamma_2 \Delta(\text{SAP}_t) + \gamma_3 \Delta(\text{EP}_t) + \gamma_4 \Delta(\text{HD}_t) + \gamma_5 \Delta(\text{SD}_t) + \gamma_6 \Delta(\text{GD}_t) + \varepsilon_t \quad (1)$$

Where t denotes the years 1971 to 2000 and Δ signifies the first difference (for all of the continuous variables) from t to $t-1$. FB is state fiscal behavior (i.e. we use three dependent variables here, property taxes, total state and local own source revenue, and total state and local expenditures). Prop13 is a dummy variable representing the effective range of Proposition 13. We test two forms of the effect of Proposition 13. We first test the immediate effects for the year following the implementation of the limit (thus Prop13 is zero for all years except the first one following the implementation of the Prop13 for which it takes on a value of one). Second, we test the long-run effect, allowing the dummy to take on a value of zero before the revenue initiative is implemented and a value of one for all years succeeding the event. PCI is real per capita income within the state. SAP is the fraction of school age population (ages 5-17) and EP is the fraction of the elderly population (ages 65+) within the total population. HD, SD, and GD are the dummy variables for political party control of various parts of the government. HD is coded 1 if the Democratic Party has control of the state's lower house in year t . SD is coded 1 if the Democratic Party has control of the state upper house. GD is coded 1 if there is a Democratic governor (cf. Alt and Lowry 1994, 2000, 2003).

Data was collected from the Census Bureau books "*Government Finances*" for the years 1971-1992 and from the Census Bureau website on Federal, State, and Local Finances (located at: <http://www.census.gov/govs/www/estimate.html>) for the years 1993-2000. Numbers given are divided by total population and deflated by the implicit price deflator (the implicit price

deflator is available at <http://research.stlouisfed.org/fred2/data/GDPDEF.txt>). Numbers are in millions of U.S. dollars. Values for income and sales taxes are not differentiated from total tax revenue until 1973, so there are fewer observations for these variables. Values for the year 1978 for these variables were unavailable so values are interpolated. In order to help correct for non-stationarity that is typical of economic data, we employ first differences on all budget variables (Pindyck and Rubinfeld 1998).

In our first cut at the data we estimate Equation (1) by ordinary least squares (OLS).⁷ Table 1 displays the estimated coefficient, associated p-values, and 95-percent confidence intervals for the effect of the Proposition 13 dummy variables, as well as the adjusted R-squared statistic for the regression. As can be seen in Table 1, Proposition 13 did lead to significant decline in property taxes and total state and local taxes for the year following its implementation. Since our fiscal and economic variables are all first-differences, we would only expect a change in these variables to be significant for the first year after the implementation of the policy, as after that, growth in income and population will lead to a linear growth in revenues again and thus the post-Proposition 13 mean (of the first differences) would be roughly equal to the pre-Proposition 13 mean, if taken over a long enough time period in which no other major shocks occurred. Expenditures were not cut, leading us to believe that the effect we see here is caused by Proposition 13 and not Proposition 4, since Proposition 4 also aimed to cut government spending and, if it was successful, we should also see an effect on expenditures, which we do not. The evidence for this can be seen in Figure 3.

⁷ See Kousser et. al. 2008 for several alternative means of estimating the impact of the Proposition 13 “event,” on California fiscal behavior.

Table 1: OLS Regression of Proposition 13 on Three Fiscal Behaviors (Property taxes, total state and local expenditures, and total state and local taxes).

Proposition 13 – Dummy Variable Denoting One Year Following the Tax Limitation			
Type	Effect (Test Statistic and P value)	95% Confidence Interval	R-Squared
Property Tax	-12.57 (0.000)	(-639.1534, -458.1001)	0.8990
Expenditure	-1.47 (0.155)	(-828.3228, 140.2498)	0.2761
Total Tax	-6.59 (0.000)	(-778.406, -405.8877)	0.7909
Proposition 13 – Dummy Variable for Fiscal Years 1979 to 2000			
Type	Effect (Test Statistic and P value)	95% Confidence Interval	R-Squared
Property Tax	1.57 (0.131)	(-23.69392, 170.1883)	0.2570
Expenditure	1.80 (0.085)	(-24.37188, 349.6884)	0.3071
Total Tax	0.71 (0.487)	(-82.6923, 168.154)	0.3401

The analysis in Table 1 is a single state event study, not a comparable time series. In what follows we will make use of data from comparison states that never enacted a TEL and could not enact a TEL through the popular initiative. We exclude states that have the popular initiative even if they have not enacted a TEL, following the logic of Gerber (1999) and Lupia and Matsusaka (2004), we believe there could be a possible intent to treat effect, as the threat of the initiative process could sway the legislatures to limit their fiscal policies even without a TEL. Our comparison group, then, includes Minnesota, Iowa, Kansas, Texas, Louisiana, Tennessee, Indiana, Georgia, South Carolina, North Carolina, West Virginia, Virginia, New Jersey, Pennsylvania, New York, Massachusetts, Rhode Island, Vermont, and New Hampshire. We drop Alabama and Michigan, as they have legislatively enacted TELs. Our selection of comparison states could be seen as analogous to Snow’s seminal study of cholera in England. In the study, he

found that there was a higher concentration of cholera cases in locations in London served by a company that drew their water from the Thames River downstream. Residents did not know what part of the river their water was drawn from, due to piping, so it was as if random. Our study is analogous to Snow's in the sense that each state is susceptible to having a TEL passed, similar to how each person is susceptible to contracting cholera. However, states with the initiative process are much more susceptible to contraction than non-initiative states. Thus, in order to make a control group, we focus on the group that is least susceptible.

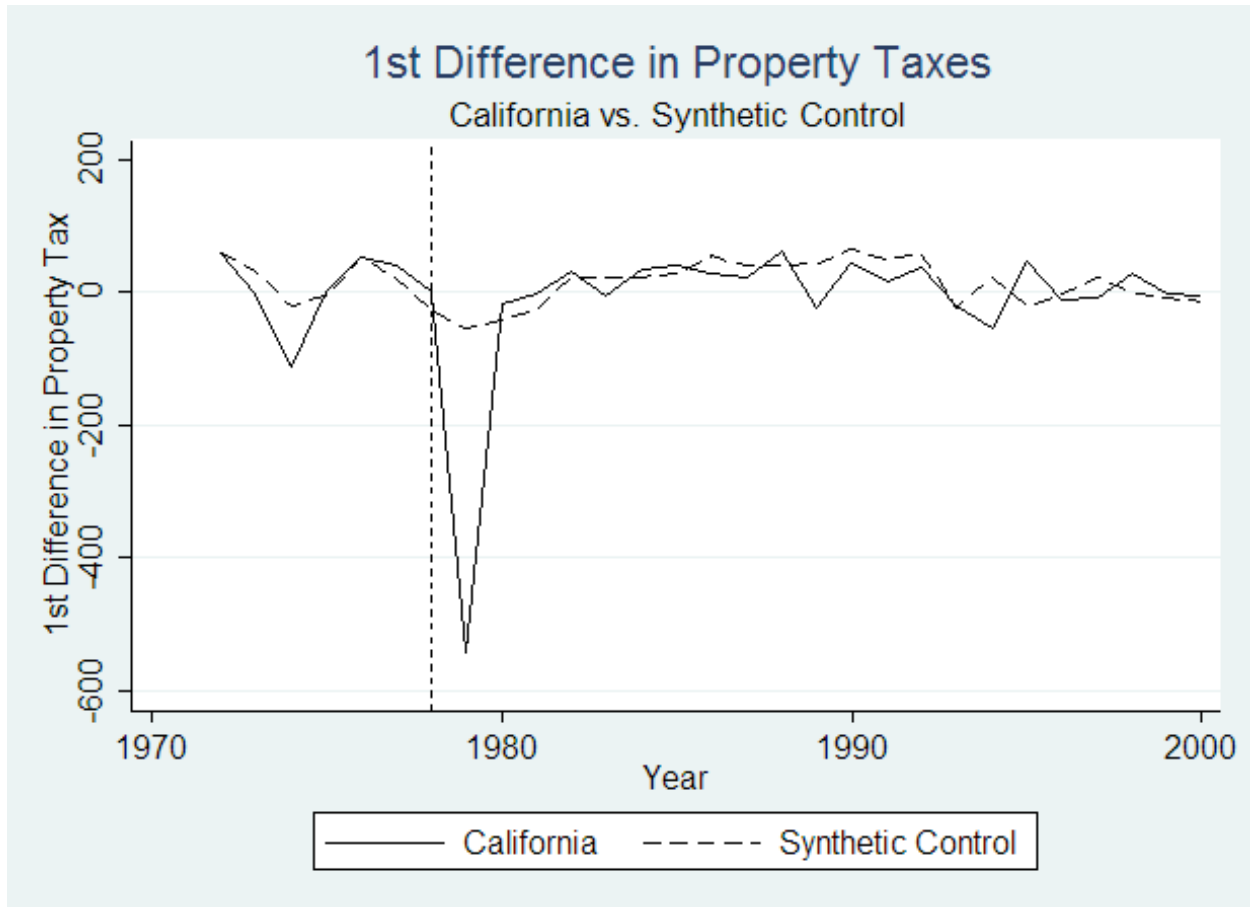
We use a subset of our observations on these nineteen states from fiscal year 1971 to fiscal year 2000 to construct a synthetic control for California. That is, we construct a counterfactual or synthetic estimation of California's fiscal behaviors that we hope matches closely the Golden State's fiscal behavior had it not enacted Proposition 13. We provide two robustness checks of the synthetic control data:⁸ first, we measure how closely the synthetic control comports with the actual fiscal behaviors of our treated case – California – prior to the treatment; and second, considering the covariates listed in Equation (1) above, we measure covariate balance between the synthetic control or estimated counterfactual and the factual data from California's real history. Our synthetic control weights each true observation among the comparison states so that they reconstruct the true observation in the pre-test treated case (e.g., California before Proposition 13). See the appendix for a discussion of the synthetic control method.

In Figure 7 we plot the actual changes in California's property taxes against the synthetic control. The algorithm used to construct the synthetic control is estimated from the pre-test

⁸ It is referred to as a synthetic control in that the estimation of the counterfactual data serves as a baseline, or control, that approximates the untreated case. In this analysis, the control is a mix of untreated states designed to approximate California without Proposition 13.

observations. Notice how closely the synthetic control matches the actual data prior to the passage of Proposition 13. The effect of Proposition 13 can be seen by the large difference between the synthetic control and the actual changes in property taxes in 1979. The error between the counterfactual synthetic control and the actual value for the change in property taxes in 1979 is much larger than any other error in the time series. The sharp decline for 1979 is, in fact, far larger than any year-state observation for any of the time series. Indeed, we do a very good job of estimating the fiscal behavior of any of the comparison states as well as the treated state, California.

Figure 7: Changes in California Property Taxes vs. Synthetic Control



Note: All numbers are multiplied by 1×10^8 for graphical purposes.

Table 2 provides some insight into the estimate of the synthetic control and how well it approximated California's property taxes. First, the Mean Squared Prediction Error (MSPE) of 40.447 between California and its comparison state appears to be driven mainly by the very steep drop in the changes in the property tax variable immediately post-treatment. The other thing to notice in Table 2 are that the covariate values for the treated (actual) and synthetic (estimated) cases are very nearly identical for the pre-test period, giving further evidence that the synthetic control is an appropriate case for comparison to California. Lastly, notice the unit weights given to the comparable series for changes in property taxes. Only North Carolina and New York have

non-zero weights and of these two, nearly 92 percent of the weight goes on the New York observations, which makes sense because California property taxes have little in common with Iowa's or Minnesota's.

Table 2: Synthetic Control Outputs – Property Taxes

RMPSE: 40.447

Predictor Balance

	Treated	Synthetic
School Age Population	-0.013	-0.008
Elderly Population	0.005	.004
Per Capita Income	104582.3	9437.126
Governor Dummy	1	.959
House Control	.5	.486
Senate Control	.5	.459

Unit Weights

Covariate	Unit Weight
Georgia	0
Indiana	0
Iowa	0
Kansas	0
Louisiana	0
Massachusetts	0
Minnesota	0
New Hampshire	0
New Jersey	0
New York	0.918
North Carolina	0.082
Pennsylvania	0
Rhode Island	0
South Carolina	0
Tennessee	0
Texas	0
Vermont	0
Virginia	0
West Virginia	0

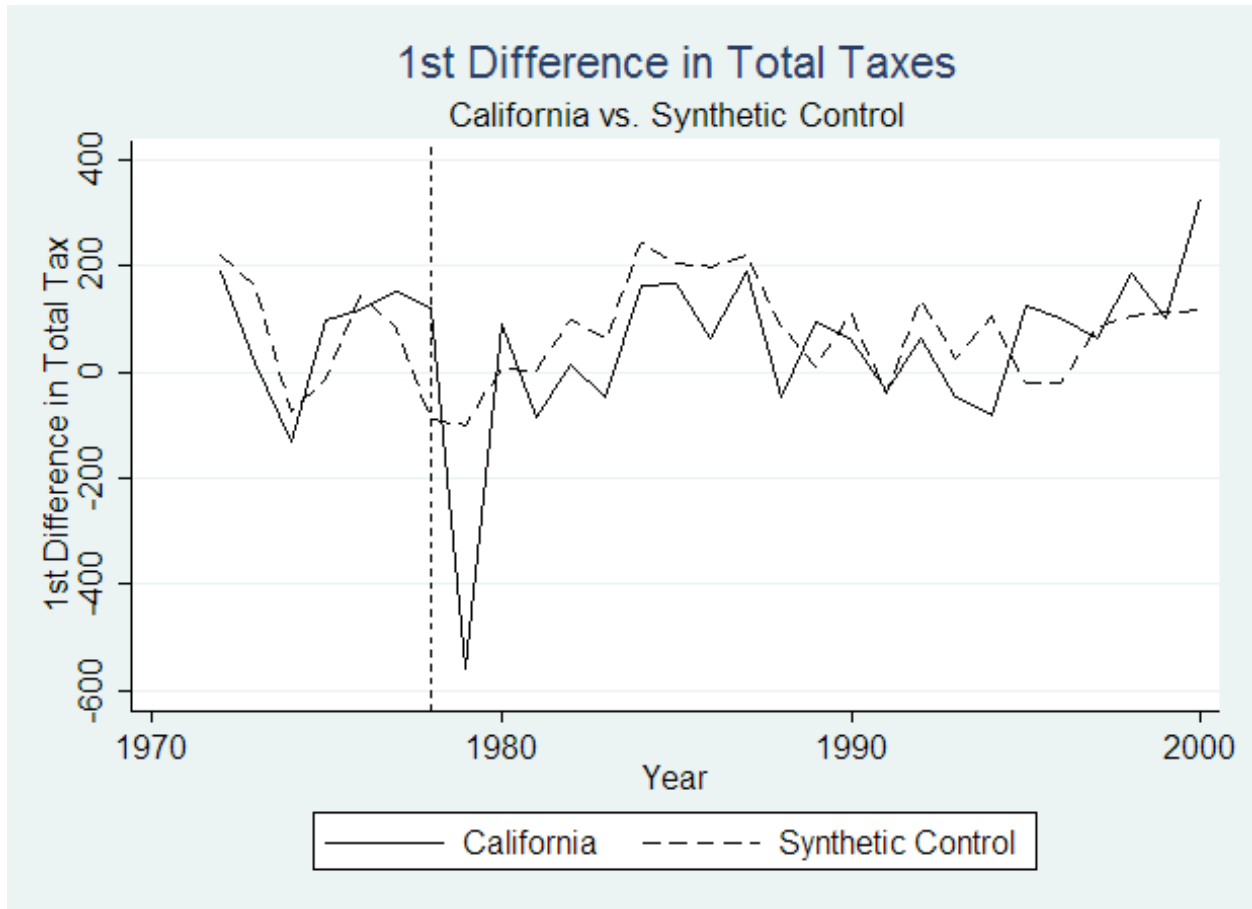
There could be several reasons for the pattern seen in Figure 7: First, there could have been a change in the manner of assessment. Property taxes in California are ad valorem, meaning that the amount taxed is based on the assessed value of the property upon which the tax is levied. One of the stipulations placed on the government by Proposition 13 was that these assessments were to occur only upon acquisition or transaction of property and then this assessed rate was to increase by either the inflation rate or 2%, whichever was lower. This turned California's property tax base from "assessed value" to "acquisition value" (even though property is still technically assessed upon acquisition). This, however, had several unintended consequences.

Second, Proposition 13 could have created a "lock-in effect" among California homeowners. Scholars have argued that the acquisition value system changes the incentives for homebuyers. Proposition 58, which allows children to inherit their parent's Proposition 13 assessments, and Proposition 60, which allows senior citizens to transfer their assessments to their new houses when they move (with certain restrictions), further changed incentives. While Stohls et al. (2001) attribute the lock-in effect to Proposition 13, Wasi and White (2005) attribute lock-in to the widespread adoption of rent control in California cities in the wake of Proposition 13. Another benefit to the acquisition value system in place under Proposition 13 has been the aforementioned revenue stability. Cal Tax Digest⁹ notes that this assessment system creates an environment under which property tax revenues are inherently stable, even in the face of economic downturn. This is because these assessments do not occur annually, or even regularly. Consequently, there is no way to capture the effect of a recession in housing prices, as they remain pegged to their baseline. There is, almost, no way to go but up.

⁹ <http://www.caltax.org/MEMBER/digest/May98/may98-4.htm>

Figure 8 shows changes in total state and local tax revenues in California. The figure seems similar to Figure 7, with a steep drop in 1979 followed by ups and downs since. Although the drop in total revenues in 1979 is not quite as dramatic as the drop in property tax revenues shown in Figure 7, there is a large effect of Proposition 13 in 1979 when actual changes in total taxes are compared to the counterfactual level derived from the comparison group. As shown in Table 3, the synthetic controls were, again, largely derived by reference to fiscal behavior in New York. The regressions show very strong covariate balance between California and the synthetic control.

Figure 8: California Total State and Local Taxes vs. Synthetic Control



Note: All numbers are multiplied by 1×10^8 for graphical purposes.

Table 3: Synthetic Control Outputs – Total Taxes

RMPSE: 83.655

Predictor Balance

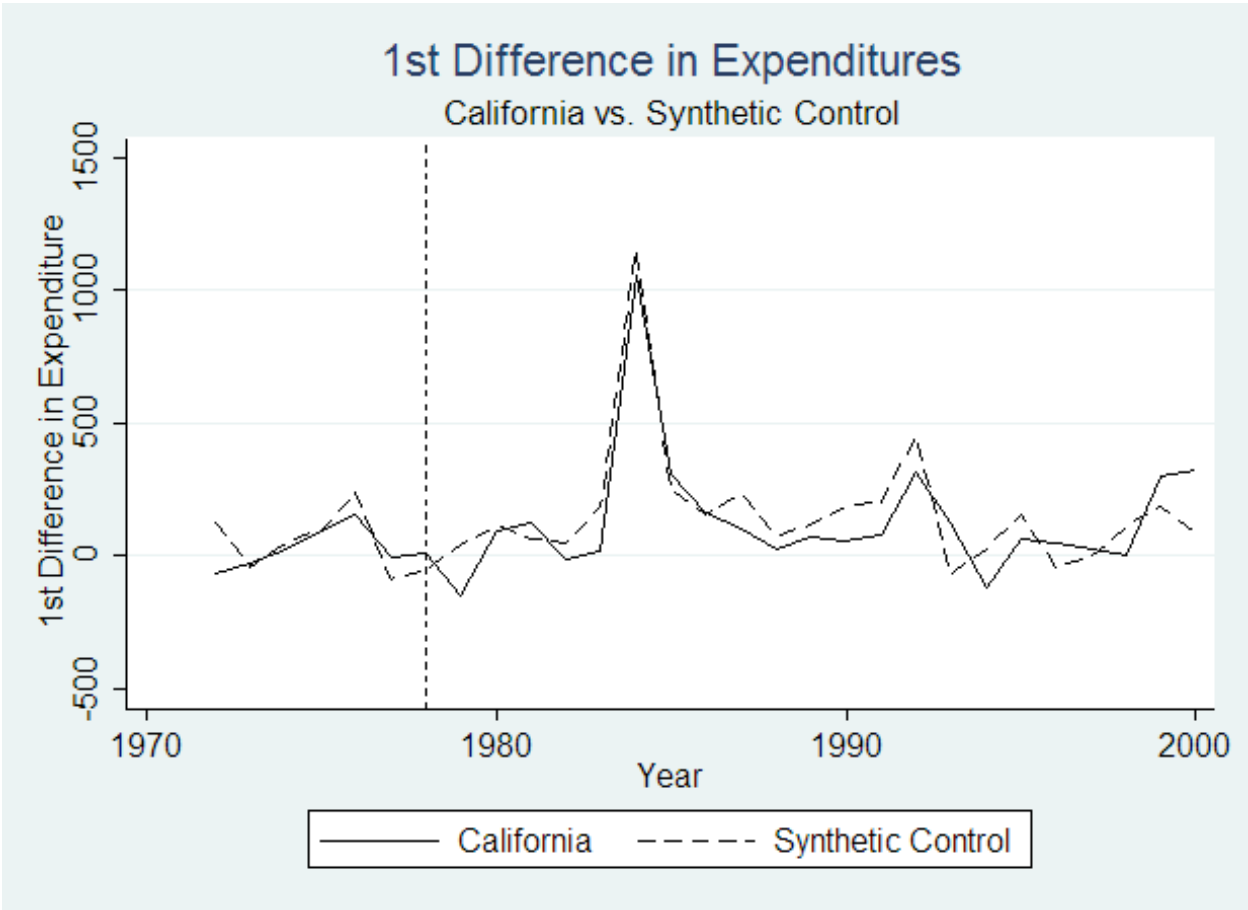
	Treated	Synthetic
School Age Population	-0.013	-0.008
Elderly Population	0.005	0.004
Per Capita Income	104582.3	8829.502
Governor Dummy	1	.988
House Control	.5	.496
Senate Control	.5	.488

Unit Weights

Covariate	Unit Weight
Georgia	0
Indiana	0
Iowa	0
Kansas	0
Louisiana	0
Massachusetts	0
Minnesota	0
New Hampshire	0
New Jersey	0
New York	.975
North Carolina	.025
Pennsylvania	0
Rhode Island	0
South Carolina	0
Tennessee	0
Texas	0
Vermont	0
Virginia	0
West Virginia	0

In contrast to the marked drop in revenues shown in Figures 5 and 6, Figure 9 suggests that there was no significant drop in state spending. Indeed, spending levels for the synthetic control almost exactly match those of California. The statistical results can be seen in Table 4. In this case we achieve near perfect covariate balance and our synthetic measures are based on only one state, New York.

Figure 9: California Total State and Local Expenditure vs. Synthetic Control



Note: All numbers are multiplied by 1×10^8 for graphical purposes.

Table 4: Synthetic Controls – Total State and Local Expenditures

RMPSE: 92.107

Predictor Balance

	Treated	Synthetic
School Age Population	-0.013	-0.008
Elderly Population	0.005	0.004
Per Capita Income	104582.3	8563
Governor Dummy	1	1
House Control	.5	.5
Senate Control	.5	.5

Unit Weights

Covariate	Unit Weight
Georgia	0
Indiana	0
Iowa	0
Kansas	0
Louisiana	0
Massachusetts	0
Minnesota	0
New Hampshire	0
New Jersey	0
New York	1
North Carolina	0
Pennsylvania	0
Rhode Island	0
South Carolina	0
Tennessee	0
Texas	0
Vermont	0
Virginia	0
West Virginia	0

The Fiscal Shell Game

The results in the previous section show a much greater effect for Proposition 13 than much of the previous literature has credited to it. Yet, given the dramatic decrease in property taxes and total state and local revenue, how did spending continue to increase? There are several ways that the state and local governments have changed the revenue landscape in order to accommodate the revenue losses caused by Proposition 13.

Since it is apparent that Proposition 13 was successful in its design, there might be a rush to extend the measure's capabilities at the least and, at the most, pass a whole new initiative to further limit the government's ability to raise revenue. After all, if it worked once, why wouldn't it work again? Today's recession together with California's budget crisis have made tax limitations all the more appealing. The question remains, thus, should the public vote to pass another one of these initiatives or should further measures be adopted in a constitutional convention? Our answer is no. First, there is substantial evidence similar to what we just presented that most TELs do not have the intended effects (Kousser et al. 2007, Kousser et al. 2008, McCubbins and Moule 2010). Second, even when those effects are achieved, there are other implementation problems. It is those that we will discuss here.

We mentioned previously that state voters face two problems related to implementing their enacted policies: time-inconsistency and agency loss. Despite their ability to recognize the evils of higher taxes, polls indicate that the public still prefers to increase government services. In polls conducted since the passage of Proposition 13, Californians have routinely expressed a desire for an ever-increasing level of government services.

Given the time-inconsistency with respect to voters fiscal demands, how can state legislators provide people with the services that they seem to want without simultaneously increasing taxes? It was obviously still politically favorable to provide these services.

Proposition 13 undercut the ability of local governments to set property tax rates (Shapiro and Sonstelie 1982). Grasping for revenues, municipalities turned to other sources of income. Many governments increased sales taxes to compensate for the lost property-tax revenue. The public outcry against sales taxes is not nearly as great as it is for property tax levies (Schwartz 1998) and it is relatively easy for increases in sales taxes to occur without being noticed, as sales taxes increase linearly with per capita income. By raising sales tax, the local governments were able keep their revenue high while upholding the letter of the law that limited property tax rates. This tax substitution allowed them to circumvent the obstacles erected by Proposition 13 (Kousser et al 2008). Notice in Figure 8 the first difference in total taxes appears to increase slightly over time, especially in the more recent years.

Municipalities also sought to increase their sales tax bases through the fiscalization of land use policy. In many cities, planning became a mad dash to attract automotive dealerships, shopping malls and strip malls by offering favorable tracts of land along main roads. Sales-tax farming has led to the erosion of family owned “Ma and Pa” shops that do not generate nearly the same sales-tax receipts as do department stores and auto malls. Thus the state and local governments are cultivating, planting, and harvesting their own growth in available revenue, just as they rode the bubble in real estate. Indeed, Schwartz (1998) notes that the sales tax as a portion of city general funds increased from 15.54% prior to the passage of Proposition 13 to

26.85% after.¹⁰ Redevelopment has made these land-grab policies even easier for cities. Under the pretext that certain neighborhoods or buildings are “blighted,” cities can trigger sweeping neighborhood reforms in which old, worn out buildings are demolished to make way for polished, new malls.

While sales taxes and land grabs have received much attention in the literature, a subtler substitute for property taxes has grown in popularity over the past three decades. Charges and fees have become an integral part of the California budgetary landscape, especially in the wake of the Gann limit. Kousser, McCubbins and Moule (2008) note that California’s proportion of fees with respect to total general revenue increased from 18.1% in 1969 (immediately before the passage of Proposition 13) to 31.6% by 1994. Within the group of comparison states average charges and fees started at 22.4%, well above California’s level, and ended at 30.9%, which is slightly below that of California (Kousser et al. 2008). Fees give the government a revenue stream that is not subject to limitation and hard for voters to track. Cities, counties, and even school districts are able to levy fees for everything from buildings, parks, and permits, to traffic. This not only keeps the revenue from being limited (since it is non-tax revenue), it also helps to “internalize” the costs of public services. It has been noted that these fees are much more numerous and cover a wider variety of infrastructure in more recently developed areas (Chapman 1998). Chapman notes that the share of service charges with respect to the total revenue collected increased from 6% to 11% of total revenues since the passage of Proposition 13.

Another means of circumventing Proposition 13 is the creation of assessments and assessment districts. While the practice of creating assessment districts was not initiated in

¹⁰ This in turn has led to the erosion of property rights, as cities have used their power of eminent domain to remove lower-tax property owners.

California, their use in California may be unique. Eisenhorn (1991) wrote that, traditionally, these districts were a rejection of resource redistribution by the local governments: establishing a “you get what you pay for” philosophy for governmental services. California’s situation is different.

Similar to the proliferation of charges and fees in government revenue structure, California has seen a rise in the amount of special assessment districts and the amount of money raised by them since the passage of Propositions 4 and 13. Researchers who have studied Proposition 13’s various effects on the state budget landscape have found that special assessments have, indeed, grown more prominent (Chapman 1998, Shires 1999, Kogan and McCubbins 2008), and these districts are often used as a means to subvert the limitations placed on the government. Special assessments often take the form of special charges on property that pay for construction, maintenance, or other public works and services. Ironically, property owners have to enact these assessments. Kogan and McCubbins point out that these are “often indistinguishable from traditional property taxes in the way they are collected and paid.” However, they further point out that they are legally distinct because of their enactment procedures. As determined by the California Court of Appeals in *County of Fresno v. Malmstrom* 1979, a “special assessment is charged to real property to pay for benefits that property has received from a local improvement and, strictly speaking, is not a tax at all”. Thus, the Census Bureau’s report of property taxes refers only to involuntary charges that are determined by the assessed value of land (i.e., ad valorem property taxes). These “special taxes” are similar, but are earmarked for a certain purpose, rather than for a variety of purposes or for the general activities of government.

The legal status of these assessment districts as a means of paying for public improvements was hotly debated until the California legislature passed the Mello-Roos Community Facilities District Act of 1982. Despite Republican worries about the subversion of Proposition 13 by special assessment districts, Community Facilities Districts (CFDs) actually expanded on the authorized mandates of previous special assessments, allowing them to pay for a variety of public services (such as police and fire) that had previously been covered by property taxes. In addition, CFDs no longer had to take into account the benefits garnered by particular properties.

Kogan and McCubbins (2008) note that these assessment districts have led to a “hyper-fragmenting” of California’s local governments with a steeply “layered democracy.” Kogan and McCubbins argue that this has had negative effects on the ability of citizens to monitor their agents, simply because there are so many more agents to look after and voters have so few tools with which to discipline their agents.

Proposition 13 supporters were not unaware of the potential problems created by CFDs and, as such, made a variety of attempts to expand the limitations of Proposition 13 to include these new forms of revenue. While voters rejected such a measure in 1984 (apparently not wanting to “save Proposition 13”), eventually, Proposition 218, passed in 1996, defined what qualified as an assessment and required a majority vote in an election before one of these assessments could be levied. While this may have slowed the growth of various assessment districts, it has not curtailed them completely.¹¹ It is difficult to know the extent of these special

¹¹ As mentioned above, often these CFDs or other assessment districts will levy their charges upon the public in terms of parcel taxes. As shown above, these taxes are based on acreage or a “parcel” rather than on assessed value of property and thus are not considered property taxes under the limitation imposed by Proposition 13. They are, however, very similar. They are, obviously, a tax levied on property, they are collected in a similar manner, and they

assessments; parcel taxes are not traditional taxes. They collected at a local level in California, so they are not aggregated in the data presented by the Census Bureau.

Yet another aspect of this circumvention is the underfunding of pensions. Take the City of San Diego, for example. Riding the stock market growth of the late 1990's, the city was able to "fund" their pension system without having to make the required payments. This freed up money for use elsewhere in the budget, which council members there did as politicians would do anywhere, they used to aid their reelection. This strategy, while good for council members in their campaigns, eventually led to massive debt problems. This story, however, is not unique to San Diego; it permeates all levels of government. Indeed, the problem is not even unique to public pensions. One, as yet unanswered question, is the extent to which the underfunding of pensions is a consequence of Proposition 13, although it has been quite conspicuous in California.

Conclusion

Proposition 13 appears to have had a strong initial effect: it severely limited property tax revenues in the years following implementation of the measure. Proposition 13 also limited total state and local revenue in the year following its implementation. In this sense, the initiative achieved its purpose. However, our survey of the literature has argued that these effects have been undercut over time, as legislators have come up with creative alternatives for government financing, substituting away from the traditional ad valorem property taxes towards fees, assessments, and income and sales taxes. This circumvention of Proposition 13 is authorized by various state laws but is implemented by municipal and county governments.

have turned to paying for the same things that property taxes in California traditionally paid for, such as school bonds (among other things). Even as such, they are not calculated into the state or local level "property tax".

One of the policy lessons to learn here is that, often, these limiting initiatives will have a reach that extends much farther than originally anticipated. This very problem shapes California's finances to this day. Voters are stuck playing in a fiscal shell game, where they must guess under which governmental shell the responsibility for policy lies. But governments demand high revenues because the people they serve demand a high level of government services. If voters cut their governments off from one source of revenue, governments will find another source to take its place. The net result of this, at least in California, is a towering pyramid of various levels of government each with a distinctive source of revenue, some very small and hard to trace, others large and visible. The multiplication and stratification of local governments leaves voters unable to track where their taxes go or how they are spent, exacerbating the agency problems inherent in political delegation.

With tax and expenditure limits, as with any citizen initiative, the voters need to consider the fact that their policy preferences may not line up exactly with the agents they are attempting to control. Enacting initiatives and then delegating implementation to these same agents could prove to be problematic.

Appendix: The Logic of Synthetic Controls

Our synthetic control weights each true observation among the comparison states so that they reconstruct the true observation in the pre-test treated case (e.g., California before Proposition 13). The observations for the pre-test treated case must be contained within the convex hull observations for the control cases. In other words, for the case at hand, California cannot be so different from the set of control cases that the observations on the control cases do not do a very good job of predicting the pre-Proposition 13 values for California. We then use the model generated by the estimating the weights for each state and how well it does in estimating the pretest values for California and use that to estimate the post-Proposition 13 values for California. In other words, we estimate what California would have looked like had Proposition 13 not passed.

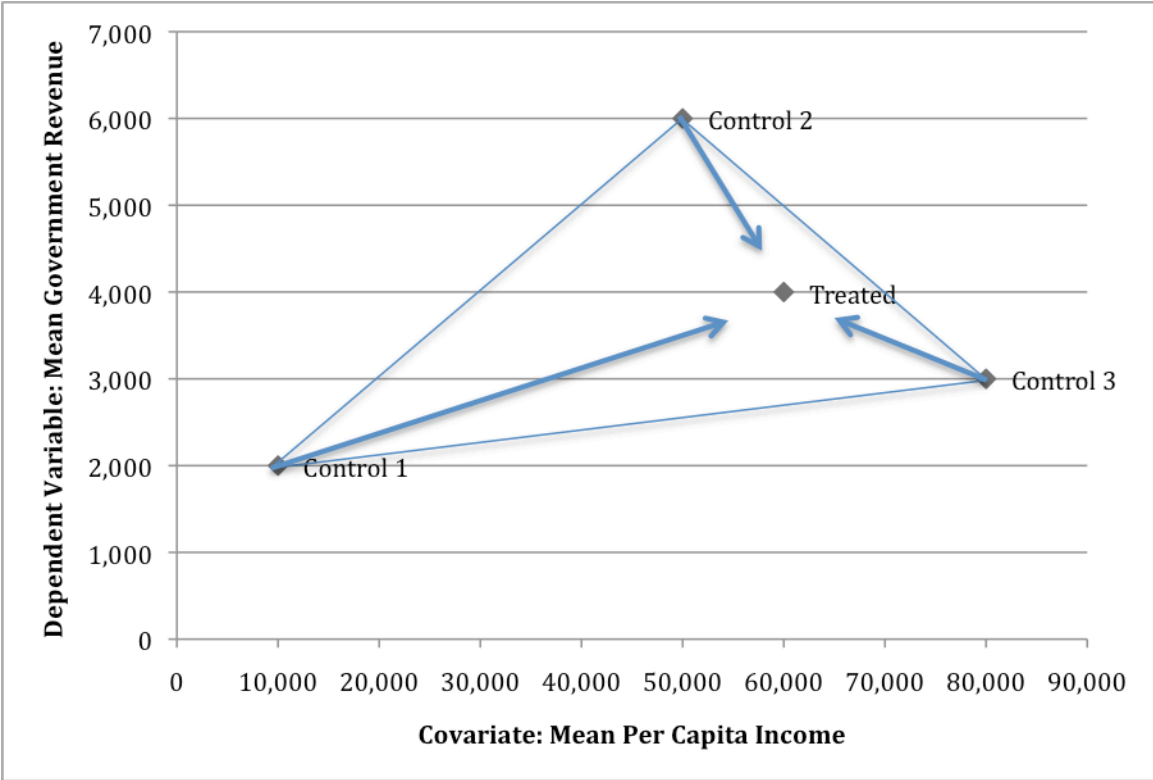
Take, for example the data presented in Table 2. The data in Table 2 is fabricated so as to present an exceedingly simple example¹². We present the mean values for the independent variable, per capita income, and the mean values for the independent variable, per capita state revenue. Figure 4 shows that the treated observation is contained within the convex hull of the control or comparable observations. The arrows show the vector weights for each control observation along the two dimensions for our example.

¹² We thank Brad LeVeck at UC San Diego for this simple rendition of Abadie et al.'s synthetic control method. Their paper outlining and applying this method to California's Proposition 99 can be found at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=958482.

Table 2: Synthetic Control Example Data: Mean Per Capita State Revenue and Mean Per Capita Income

	Mean Per Capita Income	Mean Per Capita State Revenue
Control 1	10,000	2,000
Control 2	50,000	6,000
Control 3	80,000	3,000
Treated	60,000	4,000

Figure 4: Synthetic Control Example Data: Mean Per Capita State Revenue and Mean Per Capita Income



So, in this overly simplistic example, the synthetic control observation(s) can be derived as the solution to a set of three equations, given by Equations (2), (3), and (4) below, with three unknowns, $W = [w_1, w_2, w_3]$. The solution, for this simple example, is given by W^* in Equation (s). The weights of the synthetic control will be derived by matching the pre-test values of

$$w_1 + w_2 + w_3 = 1 \tag{2}$$

$$80000w_1 + 50000w_2 + 10000w_3 = 6000 \tag{3}$$

$$300000w_1 + 600000w_2 + 200000w_3 = 400000 \tag{4}$$

$$W^* = [3/8, 1/2, 1/8] \tag{s}$$

The weights will be chosen so that the synthetic California's fiscal behavior most closely resembles the actual fiscal behavior of California pre-test, in other words, prior to Proposition 13. So, a little more formally, drawing on Abadie and Gardeazabal, 2003, let X_1 be a $(K \times 1)$ vector of pre-Proposition 13 values of the predictors (covariates) of California's fiscal behavior. Let X_0 be a $(K \times J)$ vector of values for the K predictor variables for the J possible comparable (control) states. Let V be a diagonal matrix with nonnegative components. The diagonal values of V reflect the relative importance of each predictor in predicting state fiscal behavior. The vector of weights, W^* , is chosen to minimize $(X_1 - X_0W)'V(X_1 - X_0W)$ subject to $w_1 + w_2 + \dots + w_J = 1$. The vector W^* defines the combination of comparable states that best resembles California's fiscal behavior prior to Proposition 13. W^* is itself a function of V . V is chosen so that the per capita fiscal behavior for California prior to Proposition 13 is best reproduced by the synthetic control defined by $W^*(V)$. Let Z_1 be a (8×1) vector containing the observations on fiscal behavior for California prior to Proposition 13. Let Z_0 be a $(8 \times J)$ matrix containing the values of the same variables for the J potential comparable states. Then:

$$V^* = \operatorname{argmin}_{V \in \mathcal{V}} (Z_1 - Z_0W^*(V))'(Z_1 - Z_0W^*(V)) \quad (5)$$

Where \mathcal{V} is the set of all nonnegative diagonal $(K \times K)$ matrices. The weights for the synthetic control are given by $W^* = W^*(V^*)$ (there are infinitely many equivalent solutions to this problem, each subject to a constant shift).

Let Y_1 be a (30×1) vector whose elements are the values of the fiscal behavior for California for the 30 years in our time series. Let Y_0 be a $(30 \times J)$ matrix that contains the values for the same variables for the comparable states. Our goal is to approximate the path of fiscal behavior that California would have experienced in the absence of Proposition 13. This counterfactual of the

path of fiscal behavior is calculated as $Y_1^* = Y_0W^*$. We will plot the counterfactual fiscal behavior against the true fiscal behavior for property taxes, total state and local taxes and total state and local spending. It is to this task that we now turn. Returning to our simple example, equation (2) would now yield $Y_1^* = 8000(3/8) + 5000(1/2) + 10000(1/8) = 5625$.

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