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Energy Tax Credits and Residential Conservation Investment: Evidence from Panel Data (with Kevin Hassett)

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Energy tax credits and residential conservation investment: Evidence from panel data

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Abstract

Using panel data on individual tax returns and variation in state tax policy, we measure the impact of government tax policies to encourage residential conservation investment on the probability of making these investments. Unlike previous work, we account for unobserved heterogeneity in tastes for energy-saving activities and its possible correlation with tax policy at the state level. We find that controlling for unobserved heterogeneity is very important. Based on our preferred point estimate of the tax price coefficient, a 10 percentage point change in the tax price for energy investment would lead to a 24 percent increase in the probability of making an investment.

Key words: Investment; Energy taxation; Subsidies

JEL classification: E2; H2

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1. Introduction

Investment tax credits have been shown in a variety of contexts to be powerful tools for inducing investment. However, the residential energy-conservation credit, one of the more important credits available to individuals, has been perceived as relatively ineffective at inducing investment. Any number of models can be constructed with the prediction that greater amounts of tax incentives should lead to higher probabilities of investment. Empirical work to date has not been very successful at finding that result. Either tax incentives appear to decrease investment (Walsh, 1987, 1989) or their coefficient estimates are statistically insignificant (Dubin and Henson, 1988). This perception of ineffectiveness of tax policy has contributed to a belief in what we call the 'energy paradox' (EP), the seeming anomaly that very attractive investment opportunities in energy-efficient capital, opportunities with high ex ante rates of return, are routinely passed up by investors. The existence of this 'paradox' has led some to conclude that consumers are irrational or myopic.[1] It has also had a strong impact on US policy-makers, who appear to have abandoned tax credits altogether.[2]

We believe that there are two major reasons why studies of tax incentive programs for energy conservation have been unable to find a statistically and economically significant relationship between the tax incentive programs and investment. First, many of the state tax incentive programs are deduction programs. A dollar of deduction reduces a taxpayer's taxable income by 1 and tax liability by τ where τ is the marginal tax rate. Therefore, the tax price of one dollar's worth of investment is $1-\tau$. Measuring the tax price accurately for residents of states with deduction incentives is important. Below, we use the NBER TAXSIM state tax calculator to measure the tax price at the individual level.[3]

Second, and more important, there are individual specific effects that are likely to be correlated with the explanatory variables. These individual effects include conservation 'taste' factors as well as attributes of the housing stock in which individuals choose to live. If a state contains a large fraction of citizens with a higher than average propensity to invest because of a taste for conservation, then there is less reason for this state to introduce a tax

[1] See, for example, Gates (1983), Cavanaugh (1983) and Carlsmith et al. (1990).

[2] The most recent US energy bill, passed in early 1993, earmarks monies for 'education' of consumers about energy-conservation's cost-effectiveness.

[3] TAXSIM is a set of Fortran routines for computing taxes from IRS data on individual tax returns. For a description of the calculator and its use in constructing tax prices, see Feldstein and Metcalf (1987).

incentive program to encourage conservation. Failing to control for this factor imparts a positive bias to the tax price variable in a regression of the decision to invest on various factors. In the empirical work reported below, controlling for individual effects has an important effect on the estimated coefficients.[4]

In brief, we find the following. When we do not control for fixed effects, we find, as did previous researchers, that energy tax credit programs actually *decrease* investment. Highlighting the importance of controlling for endogenous policy, after introducing correlated individual effects into the discrete choice regressions, we find the opposite: tax incentives are statistically significant and increase the probability of investing in energy-efficient capital. This finding suggests that consumers do respond in a rational way to energy-conservation incentives, and that explanations for the energy paradox based upon optimizing models without myopia may yet bear fruit.[5]

Our results also resolve a somewhat puzzling paradox in the literature. While previous researchers found that changes in energy prices affect conservation investment rates, they were unable to find statistically significant effects of changes in tax incentives on investment rates in a direction that theory would predict.[6] This has led policy-makers to believe there is 'something different' about tax policy variables that make them especially ineffective. Allowing for unobservable correlated individual effects resolves the paradox with both energy prices and tax incentives shifting the probability of investment in theoretically plausible directions. Our paper is structured as follows. The next section provides some background and a model from which the empirical work follows. Section 3 presents the empirical work using data from the University of Michigan panel of individual tax returns for the years 1979–1981. We conclude in Section 4.

2. Background

Tax incentives to stimulate conservation investment existed during the 1970s–1980s at both the federal and state level. In addition to a federal credit, nine states offered a conservation incentive (either a deduction or a

[4] Alternatively, one might argue that states with a large fraction of individuals with a taste for conservation might be *more* likely to implement a conservation incentive program. This would lead to a negative bias for the tax price variable. Ultimately, it is an empirical matter as to which bias is relevant.

[5] In a companion paper, we derive a theoretical diffusion model that shows that if the home improvement is irreversible, then the hurdle rates consumers use will be high, but the tax program will nonetheless have effects similar to those estimated here.

[6] See Hassett and Metcalf (1993) for a survey of the relevant literature.

credit) over the period 1979–1985 when the federal credit expired.[7] These state programs are important in the econometric work below as they provide variation in the tax price of conservation investment, which allows us to identify the importance of the programs in stimulating investment. As most state programs 'piggy-backed' on the federal system, we discuss the latter program in greater detail.

The federal Energy Tax Act of 1978 (ETA78) provided homeowners with tax credits to encourage conservation investment activities such as insulating walls and ceilings, replacing furnace burners and ignition systems, storm or thermal windows and doors, installing clock thermostats, and weatherstripping. These investments received a credit of 15 percent, with a credit ceiling set at $300 and could only be taken on houses that were constructed prior to 1977.[8]

We test the effectiveness of tax incentives directly by estimating the effect of state tax incentive programs on the decision to invest in a discrete choice framework. To do this, we exploit the information filed on federal returns by tax payers claiming the federal residential energy conservation tax credit along with variations in state level tax incentives.[9] We use variation in state tax incentive programs because there is no variation in the federal tax credit in our sample. All variation in the federal program comes from the program beginning or ending. However, we have no tax data on investments in the absence of the federal program.

Table 1 presents information from the *Statistics of Income* on the fraction of federal returns that claimed the credit for either conservation or renewable energy activities. The credit is most heavily taken in 1978 where 6.5 per cent of the returns claimed a credit. Note that the energy tax credit was retroactively applied beginning 20 April 1977. Credits for investments made in 1977 could be taken in 1978; hence the data for 1978 cover roughly 20 months. The fraction of returns filing the credit drops from 6.5 per cent in 1978 to roughly 3 percent by 1985, the last year in which the credit could

[7] Arizona, California, Colorado, Hawaii, Montana, and Oregon offered credits of some form while Arkansas, Idaho, and Indiana offered deductions. Information on these programs comes from Walsh (1987) and tax forms.

[8] ETA78 also encouraged investment in solar, wind and geothermal energy equipment used to heat, cool, and supply hot water or electricity to the principle residence. These investments received a higher credit, with 30 percent of the first $2000 and 20 percent of the next $10,000 qualifying for the credit, with a maximum credit of $2600. ETA78 was amended by the Crude Oil Windfall Profits Tax Act of 1980, which increased the tax credits available for renewable systems to 40 per cent of up to $10,000 in expenditure. The credit for these investments was available to all principle residences regardless of when built.

[9] Ideally, we would also look at the effect of tax incentives on the *level* of investment. Unfortunately, we have no data on house size or other housing characteristics that will affect the size of the expenditure. We are currently investigating this question with another data set.

Table 1
Fraction of returns taking residential energy credit

Year	Number of returns (×1,000)	Returns w/credit (×1,000)	%	Sample	
				Frequency	%
1978	89,772	5,843	6.51	–	–
1979	92,694	4,775	5.15	2,053	5.17
1980	93,902	4,670	4.97	2,032	4.94
1981	95,396	3,870	4.06	2,044	3.90
1982	95,337	3,136	3.29	10,323	3.28
1983	96,321	NA	NA	5,038	2.37
1984	99,439	NA	NA	10,186	2.41
1985	101,660	2,979	2.93	5,032	2.72

Source: The first three columns come from *Statistics of Income*, various years. Column 3 shows the fraction of returns each year claiming the federal residential energy credit. The next two columns are computed by the authors. Column 4 shows the sampling frequency for the University of Michigan/Ernst and Young Tax Panel; for example, the value 2,053 in 1979 means that one return was sampled for every 2,053 returns in the population. Column 5 shows the fraction of returns each year claiming the federal residential energy credit in the panel.

be taken. One might think that conservation credits might be fraudulently claimed, or that, unaware of the possibility of taking them, taxpayers might fail to claim a credit for which they were eligible. Fortunately, the TCMP audit data allow us to analyze the extent to which mistakes or frauds occur. The 1986 TCMP data indicate that of the 560 million dollars of tax credits claimed in 1985, 531 million were legitimate, and an additional 28 million allowable claims that were not originally reported were discovered. These numbers are typical of those for most items covered by the audit, and indicate that fraud or mistakes will not be an important source of measurement error in the empirical work we present below.[10]

Table 2 reports the fraction of federal credit-takers and mean conservation expenditures by state for 1979 along with the average credit for those who took the credit in each state. The geographic distribution of the propensity to take the federal credit for the most part is not surprising. However, certain states stand out, California most prominently. With the exception of Hawaii, California had the lowest fraction of credit-takers of all the states. California's state conservation incentive program is unique in offering a very generous credit for conservation activity (40 percent of costs). However, the credit is *net* of the federal credit. For most households, it is simply easier to claim the entire 40 percent on the state return than claim 15 percent on the federal return and the remaining 25 percent on the

[10] We thank Joel Slemrod for providing this information.

Table 2
Conservation expenditures and credit by state

State	Percentage	Expenditures	Credit
Alabama	0.060	331	49
Alaska	0.052	257	38
Arizona	0.035	384	57
Arkansas	0.048	780	96
California	0.021	762	101
Colorado	0.064	730	104
Connecticut	0.098	813	98
Delaware	0.080	359	53
District of Columbia	0.061	659	87
Florida	0.022	723	91
Georgia	0.039	428	58
Hawaii	0.0	–	–
Idaho	0.052	266	40
Illinois	0.062	706	95
Indiana	0.048	641	91
Iowa	0.114	598	82
Kansas	0.055	449	58
Kentucky	0.067	747	87
Louisiana	0.029	677	89
Maine	0.095	342	51
Maryland	0.082	803	100
Massachusetts	0.098	695	93
Michigan	0.077	826	106
Minnesota	0.112	625	84
Mississippi	0.029	610	91
Missouri	0.071	527	69
Montana	0.098	587	88
Nebraska	0.051	708	105
Nevada	0.041	828	105
New Hampshire	0.039	299	45
New Jersey	0.086	695	94
New Mexico	0.058	899	98
New York	0.088	840	107
North Carolina	0.048	543	79
North Dakota	0.089	334	50
Ohio	0.060	652	88
Oklahoma	0.072	401	60
Oregon	0.063	878	127
Pennsylvania	0.071	698	95
Rhode Island	0.084	509	63
South Carolina	0.043	570	82
South Dakota	0.066	620	93
Tennessee	0.052	712	88
Texas	0.035	760	91
Utah	0.057	385	156
Vermont	0.049	1,202	156
Virginia	0.060	606	85

Table 2. *Continued*

State	Percentage	Expenditures	Credit
Washington	0.067	662	94
West Virginia	0.034	1,104	154
Wisconsin	0.093	453	62
Wyoming	0.046	586	88

Notes: This table reports the fraction of conservation credit-takers and average expenditures and credit for credit-takers for tax payers in the data set for 1979. *Source:* Authors' calculations.

state return. Thus, the low participation rate in the federal program for California reflects a measurement problem, a problem we address in the estimation below.

To motivate the regressions that follow, we sketch out the simplest of investment models in which energy prices rise exponentially. Consider an individual wishing to minimize the lifetime cost of energy expenditures (inclusive of capital costs) for a given level of heating comfort. She is deciding when to invest (if ever) in some energy-saving capital that will result in percentage energy savings of amount δ. We take the capital cost to be the numeraire and fix the level of investment at amount K.[11] Her problem then is to choose an optimal time (T) to purchase the capital to minimize:

$$V = \int_0^T P_t \, e^{-\gamma t} \, dt + \int_T^\infty (1 - \eta\delta) P_t \, e^{-\gamma t} \, dt + (1 - \pi) K_T \, e^{-\gamma T} , \tag{1}$$

where γ is the discount rate, η a variable that determines whether the energy savings accrue to the investor or not, and π the value of any tax incentive that may be available to the investor. Energy prices are assumed to rise exponentially at rate α:

$$\frac{dP}{dt} = \alpha P_t . \tag{2}$$

First-order conditions for the optimal investment time imply that she will make the investment when

$$\eta\delta P_T - \gamma(1 - \pi)K \geq 0 . \tag{3}$$

If we allow for optimizing error, we can recast this equation as

$$y^* = \eta\delta P - \gamma(1 - \pi)K + \varepsilon , \tag{4}$$

[11] Allowing the investment amount to be endogenous does not change our argument. The hurdle rate rule we derive is unchanged in its essential form.

where ε is a random variable reflecting errors in optimization and construct a discrete choice model.[12] The individual invests ($I = 1$) or does not invest ($I = 0$) according to

$$I = \begin{cases} 1\,, & \text{if } y^* \geq 0\,, \\ 0\,, & \text{otherwise}\,. \end{cases} \tag{5}$$

Assuming the distribution of ε is symmetric, the probability that $I = 1$ is given by

$$\Pr(I = 1) = F(\eta\delta P - \gamma(1 - \pi)K)\,, \tag{6}$$

where F is the cumulative distribution function of ε. Assuming the extreme value distribution for ε we obtain

$$\log\left(\frac{\Pr(I = 1)}{1 - \Pr(I = 1)}\right) = \eta\delta P - \gamma(1 - \pi)K\,. \tag{7}$$

We estimate a reduced form version of Eq. (7) as we do not have measures of η, δ, or γ, each of which is likely to be a function of underlying characteristics of the household.[13] Eq. (7) gives us guidance, however, as to what variables should be included in the logit regression. Permanently higher energy prices should increase the probability of making an investment. Similarly, being a homeowner increases η and thus should increase the investment probability.[14] We postulate that energy savings (δ) should be greater in colder climates and add heating degree days (HDD) as a regressor with the expectation that higher amounts of HDDs increase the probability of investment.[15] Higher discount rates (γ) should decrease investment. Hausman (1979) and others have found an inverse relationship between discount rates and income. Hence, we add income to the regression and expect higher levels of income to increase the probability of investment.

Finally, increases in tax incentives for investment (π) will increase the probability of investment. As noted above, we concentrate on the value of tax incentives at the state level as the federal credit was in effect over the

[12] We can also motivate ε as an unobservable flow benefit from the investment (measured in dollars) reflecting satisfaction from saving energy or increased living comfort.

[13] This is the basic model that underlies the empirical work of Cameron, Walsh, and Dubin and Henson.

[14] This model setup assumes that homeowners appropriate the entire future stream of returns from the investment whether they continue to live in the house or not (i.e. that the returns are capitalized into housing prices). Previous research indicates that the future stream of energy savings are capitalized into the price of residential property (e.g. Johnson and Kasserman, 1983, and Dinan and Miranowski, 1989).

[15] Data on heating degree days by state and year are from the US National Oceanic and Atmospheric Administration.

entire time of our sample. For those states that offer a credit, π is the value of the state credit. For states that offer a deduction, π equals τ, the individual's marginal tax rate. We use the NBER TAXSIM State Tax Calculator to compute τ for individuals in states with tax deductions. We first compute each taxpayer's tax liability, then recompute after adding $100 of wage income. The difference in tax liability divided by 100 gives a measure of the marginal tax rate on wage income.[16] In the regressions that follow, we drop observations on individuals with AGI less than $10,000. Many individuals in this group are not 'life-cycle' low income people but rather 'transitory' low income due to large business losses. It is difficult to measure their tax price accurately; moreover, this group may be most likely to take advantage of carry-forward provisions in the federal code, which confounds our measurement of the appropriate tax price driving their investment.

In addition, we include individual specific effects that are likely to be correlated with the tax incentive variable. Individual effects control for a 'taste' for conservation as well as unobservable attributes of the housing stock in which individuals choose to live. As noted about, a higher propensity for individuals to invest in energy-saving capital reduces the need for the state to implement programs to encourage conservation. Failing to control for these individual effects imparts an omitted variable bias to the tax incentive coefficient, biasing the coefficient in an upward direction.[17] Finally, we include the change in the unemployment rate in the state in which households reside to control for shocks to the state economy and a trend variable to capture secular changes in the rate of conservation investment. We expect that in states in which the unemployment rate is rising, people might be reluctant to tie up savings in an illiquid conservation investment given the possibility of unemployment.

Eq. (7) then becomes

$$\log\left(\frac{\Pr(I_{it}=1)}{1-\Pr(I_{it}=1)}\right)=\alpha_i+\beta x_{it}\,, \tag{8}$$

where we have now subscripted by individuals (i) and time (t), x is a vector

[16] We also zero out the federal deduction for the residential energy tax credit when computing τ with TAXSIM. This ensures that there is no endogeneity between the computed tax rate and the error term in the logit regression.

[17] The endogeneity of the state tax incentive distinguishes this analysis from empirical work that uses state programs to create 'natural experiments' with which they can identify the parameters. Unlike those studies we can not assume that the state programs are exogenous. Also, as noted in footnote 4, one can think of arguments that would imply that the bias goes in the other direction. The empirical results reported below, however, are consistent with our interpretation.

of the explanatory variables discussed above and β is the coefficient vector. Eq. (8) can be rewritten as

$$P(I_{it} = 1) = \frac{e^{\alpha_i + \beta x_{it}}}{1 + e^{\alpha_i + \beta x_{it}}} . \tag{9}$$

The presence of individual effects in the discrete choice model complicates the estimation somewhat. Unlike linear models, we cannot use a fixed effects (deviation from means) estimator nor can we simply difference the data to sweep out the individual effect. Chamberlain (1980) has suggested a logit analogue to the first-differences estimator which follows from maximizing a conditional likelihood function (conditional on making an investment at least once, but not in all years). Conditioning in this fashion has the effect of differencing out the individual effect.[18] Under a null hypothesis of homogeneity ($\alpha_i = \alpha$ for all i), both the unconditional logit and the conditional logit yield consistent estimates of β. However, the conditional logit is inefficient both because it fails to use the information that the α's are all the same and because it fails to use all the data. Under the alternative hypothesis, however, the unconditional logit yields inconsistent estimates of β, while the conditional logit continues to yield consistent estimates.[19]

Unlike a fixed effects regression model with a continuous dependent variable, we cannot 'back out' estimates of the fixed effects and forecast the dependent variable. Thus, we are unable to answer the question, "How does increasing x change the probability of making a conservation investment?". However, we can compute the log odds as

$$\ln\left[\frac{P(1|x = x'')}{P(0|x = x'')}\Big/\frac{P(1|x = x')}{P(0|x = x')}\right] = \beta(x'' - x') , \tag{10}$$

which is of interest, and does not depend on α because the odds format makes the α's cancel. Thus we can answer the question, "By what proportion will the probability of an agent taking the action increase?".[20]

[18] The approach used by Chamberlain originates with Neyman and Scott (1948) who suggested a general strategy of constructing functions of the data that are independent of the nuisance parameters (in our case the fixed effects). As they and others (e.g. Anderson, 1970, and Wright and Douglas, 1976) point out, this approach yields consistent estimates of the structural parameters under mild regularity conditions.

[19] This suggests a Hausman (1978) type specification test for heterogeneity in the intercepts. See Green (1993) for some discussion on this point.

[20] This assumes that our sub-sample is representative of the entire sample. We address this issue in the empirical work.

3. Empirical results

We utilize data from tax results for households follows over the three-year period from 1979–1981. The tax data are drawn form the Ernst and Young/University of Michigan Tax Research Database, which consists of a simple random sample of returns drawn by Social Security Number for the tax years 1979–1986. The number of returns each year varies from 9235 to 46,670.[21] From these returns we are able to construct a three-year panel that follows 37,658 individuals. We have information on each individual's state of residence, income, number of dependents, and home mortgage and property taxes (from which we can infer homeownership status). There is also information on whether they filed an energy tax credit form, how large a credit they received and their expenditures on the conservation portion. In addition, there is detailed information on expenditures by sub-categories (e.g. storm windows, insulation). Finally, as noted above, we can compute a measure of the tax price for conservation investment using TAXSIM.

We merged tax data on energy prices from the Department of Energy State Energy Price and Expenditure Data System (SEPEDS). This data set has detailed price and expenditure information by state and year on the residential sector for various energy sources. In the regression results reported below, we use the price for petroleum.[22] We divide this price by a price index for insulation to normalize energy costs in terms of capital costs. Note that the index varies across states but not individuals within the state.[23]

Sample statistics for the data set are provided in Table 3. As noted above, roughly 6 percent of the sample take a credit for residential energy conservation. Conservation expenditures range from zero to $16,970 and the credit ranges from zero to $301. Our procedure for identifying homeowners (presence of a mortgage interest deduction or property taxes) is imperfect as we only identify 32 percent of the households as homeowning. Finally, we note that there is reasonable variation in the tax price variable with which we identify the effectiveness of state tax incentive programs.

Our first regression result is for the pooled sample, and is reported in

[21] Column 5 of Table 1 provides information on the sampling frequency, while column 2 shows how many returns were filed that year.

[22] The return to conservation depends on the path of *future* prices. Under a number of plausible assumptions about the price path (e.g. random walk in levels or logs, mean reversion), current price is a sufficient statistic for the present discounted value of the future stream of prices. We have also experimented with using alternative price series. Results are not in any way significantly altered by which set of prices we use.

[23] We would prefer to use a measure of price for the actual conservation measure undertaken. Unfortunately, data do not exist for this purpose.

Table 3
Summary statistics

Variable	Mean	Standard deviation	Minimum	Maximum
Credit taken (dummy variable)	0.057	0.233	0	1.000
Conservation expenditures	39.130	261.590	0	16,970
Credit (dollars)	5.040	28.710	0	301.000
AGI (× 1,000)	22.660	19.860	−278.990	271.150
Homeowner (dummy variable)	0.315	0.464	0	1.000
Heating degree days	4.849	2.035	0.783	10.420
Change in unemployment rate (%)	0.540	1.040	−2.000	4.600
Price	0.026	0.074	0.019	0.048
Tax price	0.974	0.074	0.730	1.000

Notes: Summary statistics are for the 112,974 observations over the three-year period 1979–1981.

Table 4.[24] The dependent variable is the dummy variable indicating the presence of a credit with conservation expenditures. We first discuss the non-price variables as their effect is relatively stable across regressions. The probability of investing goes up with income. Homeowners are more likely to take a credit as are residents of states with colder climates (more heating degree days). Each of these variables is statistically significant with p-values less than 0.01. These results are consistent with results in earlier studies (e.g. Dubin and Henson, 1988, and Walsh, 1989). In addition, we find that as the unemployment rate increases, there is a lower probability of making an

[24] For the first regression, we also include a dummy variable for California in the regression. As noted above, California offers a generous credit for conservation activity (40 percent of costs net of the federal credit). This fact suggests that California residents should be less likely to claim the federal credit.

Table 4
Regression results

Variable	(1)	(2)	(3)	(4)	(5)
Tax price	0.978[b]	−2.428[b]	−2.271	−2.081[a]	−2.552[b]
	(0.350)	(1.183)	(1.489)	(1.063)	(1.195)
Price	−5.111	11.541	26.434[b]	0.255	10.454
	(6.354)	(10.170)	(13.26)	(0.258)	(10.290)
AGI (×$1,000)	0.0006[b]	0.0110[b]	0.0109[b]	0.792	0.0111[b]
	(0.00008)	(0.0024)	(0.0027)	(0.111)	(0.0024)
Homeowner*	1.508[b]	0.948[b]	–	0.917[b]	0.947[b]
	(0.035)	(0.089)		(0.089)	(0.089)
Heating degree days	0.144[b]	0.201[b]	0.079	0.200[b]	0.204[b]
	(0.0009)	(0.051)	(0.098)	(0.052)	(0.052)
California*	−0.456[b]	−0.774	–	−0.785	−0.792
	(0.103)	(0.626)		(0.632)	(0.625)
Change in unemployment rate (%)	−0.037[b]	−0.017	0.026	−0.016	−0.016
	(0.014)	(0.021)	(0.030)	(0.021)	(0.021)
Trend	−0.162[b]	−0.186[b]	−0.291[b]	−0.170[b]	−0.182[b]
	(0.027)	(0.041)	(0.058)	(0.042)	(0.042)
Lead tax*	–	–	–	–	0.070
					(0.095)
Constant	−4.764[b]	–	–	–	–
	(0.378)				
Fixed effects	no	yes	yes	yes	yes
Sample size	74,792	12,915	8,496	12,915	12,915

[a] Significant at the 90 percent level.
[b] Significant at the 95 percent level.
Note: Regression results are for individuals in the 50 states plus the District of Columbia followed over the three-year period 1979–1981. Standard errors are reported in parentheses. An asterisk on a variable indicates a dummy variable. Tax price, price, and AGI are in logs in column 4.

investment. Finally, there is a trend toward less conservation investment over time in the sample.

Turning to the price and the tax price variables, we note that both have the wrong sign in the levels regression. One reason that the price variables may not be explaining investment very well is the presence of correlated individual effects in the error term. The conditional logit fixed effects regression allows us to estimate the price effects consistently in the presence of correlated fixed effects.

We now turn to these estimates (column 2). As noted above, the regression is conditional on individuals making investments in at least one year.[25] Thus, the sample size drops from 74,792 to 12,915. The tax price variable now has the correct sign and is significant at the 5 percent level with a coefficient estimate of −2.43. A decline in the tax price leads to an increase in the probability of investment.[26] Below, we discuss how one should interpret the economic importance of this coefficient estimate. The price coefficient now has the correct sign, albeit the t statistic is only slightly larger than 1.

The dramatic change in the price and tax price coefficients suggests the importance of correlated fixed effects. We can formally test the null hypothesis of no correlated fixed effects by constructing a Hausman specification test comparing the vector of coefficient estimates from the first and second regressions. For this model, the specification test statistic has 8 degrees of freedom and equals 77.7. The p-value under the null hypothesis of no correlated fixed effects is essentially zero and we reject this hypothesis in favor of correlated fixed effects.[27]

We next present some alternative specifications to check the robustness of our results. The third regression in Table 4 restricts the sample to homeowners who never move. One might argue that homeowners are more likely to be responsive to tax incentive programs than renters. Restricting the sample to homeowners only reduces the sample from 12,915 to 8,496.[28] The tax price coefficient is essentially unchanged from the conditional logit estimate

[25] We are also conditioning on their not taking the credit in all three years. We can only use households that exhibit some variation in the dependent variable.

[26] In footnote 4, we noted that the bias could go in either direction based on your assumption about what drives the decision to implement state tax incentive programs. The change in estimates suggests that states implement programs on average in cases where the taste for investment is low.

[27] The dramatic change in price and tax price coefficient estimates could also result if this sub-sample is different from the remainder of the sample. To check for this possibility, we ran a simple logit on the reduced sample. Differences in coefficient estimates between this regression and our first regression should measure the effects of conditioning on households that take a credit at least once. The results suggest that sample selection is not driving our results: the tax price coefficient continues to have the wrong sign with a value of 0.57 and is statistically insignificant (t-statistic equals 1.25).

[28] Homeowners are only 30 percent of our sample (based on the presence of mortgage and property tax deductions). However, they comprise a large fraction of the federal credit filers.

and has a two-sided p-value of 0.12. The price coefficient is slightly more than double and is now statistically significant at the 95 percent level.[29]

Column 4 presents a regression in which the tax price, energy price, and income are in logs. The variables continue to have the expected sign. The log regression allows us to compare the relative effectiveness of the tax incentive program and the energy price effect. A permanent 10 percent increase in energy prices should have (roughly) the same effect as a 10 percent cut in the cost of conservation capital. Thus, if changes in energy prices are perceived as permanent, then the coefficients on the tax price and the price variables should be equal in absolute value but of opposite signs. On the other hand, if energy price movements are perceived as temporary, then the price coefficient should be smaller than the tax price coefficient.[30] The coefficient on the tax price variable is roughly eight times the size of the price coefficient. This would be consistent with households acting as if a roughly 12 percent of change in energy prices is a permanent change.[31]

Finally, we test for the possibility that the tax price effect is a spurious result. One explanation for the negative relationship between the tax price and the probability of investment is that people anticipate the beginning of a new program and delay investment until the program goes into effect. While the net change in investment would be zero with the implementation of a program, the estimated coefficient on the tax price variable would be negative. To test for this possibility, we constructed a dummy variable equalling one if a state had a program in effect the *following* year and zero otherwise. If investment-shifting were occurring, we should anticipate a negative coefficient on this variable. The last regression includes this variable. The estimated coefficient on the future tax variable is positive (although statistically insignificant) indicating that tax timing is not driving out result.[32]

How do we interpret the coefficient estimate on the tax price variable?

[29] We obtained very similar results when we restricted the sample to homeowners in the states in which a tax incentive program was in effect at some point during the three years.

[30] If the tax incentive is viewed as temporary, this will increase the tax price effect as households invest during a window of opportunity.

[31] Another reason that the tax incentive coefficient may be larger than the price coefficient follows from the publicity effect of the tax incentive program itself. We have no way of distinguishing the two interpretations.

[32] We considered other specifications not reported here. For example, a referee speculated that unemployment shocks would affect the probability of investing differentially across income classes. There is weak evidence for this; we interacted dummy variables for three income classes with the unemployment variable and found stronger effects for lower income groups. However, one could not reject the hypothesis that the effect was the same across all income groups. The tax price coefficients were not materially affected in any of these specifications.

Consider a plan to provide an additional 10 percentage point decrease in the tax price of conservation capital. What effect will this decrease in the tax price have on the probability of investment? Based on the coefficient estimate of −2.43 (column 2 of Table 4) and a probability of investing equal to the mean of the data set (0.057), the probability of investing rises by 1.4 percentage points to 0.071.[33] Across a wide variety of specifications, the percentage change in probability is fairly large, indicating that energy tax credits were indeed effective at inducing investment, at the margin.[34]

4. Conclusion

In this paper, we have argued that variation in state tax laws can allow us to identify the effect of tax incentives on energy conservation investment. Using a data set on individuals followed over a three-year period, we find that the conservation incentive programs offered by state governments in addition to the federal program have a statistically significant effect on investment once we control for individual (fixed) effects.

This result contributes to the existing empirical literature by shedding new light on the question of whether consumers irrationally ignore tax incentives for home improvements. Based on our preferred estimate of the tax price coefficient, a 10 percentage point change in the tax price for energy investment would lead to a 24 percent increase in the probability of energy conservation investment. Whether it is a good idea for the government to be in the business of providing tax incentives for conservation investments is another matter and cannot be resolved in this paper. For one, any subsidy to conservation investment is earned by households that were planning to make conservation investments in the absence of the credit. For these households, the credit is a windfall. Moreover, one must ask whether energy consumption falls after investment (assuming reduced energy consumption is the policy goal) and why it is in the public interest to promote energy conservation. However the policy debate unfolds, an important piece of information will be whether tax incentives can increase the probability at the margin of making conservation investments. The evidence reported here suggests they can.

[33] The log specification in column 4 yields the same predicted change in the probability of investment.

[34] This conclusions assumes that our conditional logit results are applicable to the entire population. We believe this to be plausible given the discussion in footnote 27.

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