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Promoting clean energy investment: An empirical analysis of property assessed clean energy

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ABSTRACT

From 2008 to 2010 a handful of Property-Assessed Clean Energy (PACE) programs offered property-secured loans to homeowners for residential clean energy investments. This analysis uses difference-in-differences models and synthetic counterfactual models to estimate the effect of three California PACE programs on residential photovoltaic installations. While PACE programs do not offer superior terms to other solar financing options, we find that PACE financing increases solar installations by approximately 3.8 watts per owner-occupied household per quarter, a 108% increase over the mean watts per owner-occupied household.

Introduction

Promoting renewable or “clean” energy has been a focus of both Federal and State policies in the United States and a focus of national-level policies in many European countries. The goals of these policies are multifaceted and include reducing dependence on foreign energy sources, reducing pollution of both greenhouse gases and conventional pollutants, and increasing jobs in a new and growing field. Governments have adopted a variety of different policies to increase production of renewable energy. One such initiative is Property Assessed Clean Energy (PACE) financing. PACE is a novel form of financing for clean energy generation investments where a property owner obtains a loan from her local municipality and repays the loan through a property tax assessment rather than a traditional loan payment.

The PACE moniker covers a wide variety of programs encouraging energy generation through solar, wind, or geothermal at both the residential and commercial level. The identifying common threads are (1) capital is loaned by a municipal lender who secures the loan by creating a “special assessment district” which serves as a primary lien on the property, and (2) the assessment for repayment is attached to the property and transfers to a new owner along with other traditional property tax assessments. If a borrower fails to make payments on a property tax assessment, the municipality may seize and sell the property for the balance due. If a borrower chooses to sell their property, the PACE assessment, as well as the improvements, transfer to the new owner.[1]

PACE was first proposed in the Association of Monterey Bay Area Governments (AMBAG) Regional Energy Plan of 2006 as “On Tax-Bill Financing” (Association of Monterey Bay Area Government, 2006). The first fully-implemented PACE program in

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[1] On-bill financing (OBF), where repayment of a loan is linked to a property's power bill and transfers to the new owner or occupant functions similarly to a PACE program. The “Green Deal” in the UK, initiated in 2012, is an OBF program (United Nations Economic and Social Commission for Asia and the Pacific, 2002), as is the set of statewide programs spurred by the California Public Utilities Commission's 2012 proposal to implement a state-wide OBF program. This paper does not attempt to extend analysis to OBF programs.

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Table 1
PACE programs active as of 2010.
Sources: Long Island Green Homes, 2012; Boulder County ClimateSmart Loan Program, 2011, City of Palm Desert, 2011; Farrell, 2010; City of Yucaipa, 2009.

Municipality	Program name	Start date	Loan terms	Program requirements		
				Energy efficiency audit	**Energy efficiency investment**	**Credit-based**
Berkeley, CA	Berkeley FIRST	2008 (Pilot)	7.75 percent over 20 years	No	Voluntary	No
Palm Desert, CA	Palm Desert EIP	August 2008	7 percent over 20 years	No	Voluntary	No
Sonoma County, CA	Sonoma County EIP	March 2009	7 percent over 20 years	Yes after July 2011	10 percent minimum investment, after July 2011	No
Yucaipa, CA	Yucaipa EIP	July-December 2009	7 percent over 20 years	No	Voluntary	No
Placer County, CA	mPOWER Placer	May 2009	7 percent over 20 years	Yes	Voluntary	No
Boulder County, CO	Climate-Smart Loan Program	2009	Not to exceed 4.5 percent (income qualified) or 7.75 percent (open) over 15 years	Yes	No	Partial
Babylon, NY	Long Island Green Homes	2008	3 percent over 10 Years	Yes	Voluntary	No

the US was the Palm Desert, California "Energy Independence Program" (PDEIP), which was officially approved on August 28, 2008 (City of Palm Desert, 2008).[2] In January 2009, Sonoma County, California approved the Sonoma County Energy Independence Program (SCEIP), which began operations in March 2009 (Home Performance Resource Center, 2010). Yucaipa, California followed in August 2009 with the Yucaipa Energy Independence Program (YEIP) (City of Yucaipa, 2009). By 2010, seven cities had active programs (see Table 1) and five of these programs were in California.

Proponents of PACE claim that making it easier for homeowners or businesses to invest in efficiency or clean energy generation projects will decrease dependence on non-renewable forms of energy generation, and will keep a larger portion of energy spending in the local economy. Opponents of PACE programs point to the fiscal insecurity it places on mortgages – a PACE loan is primary to a mortgage, meaning that a mortgage holder may recover a lower percentage of a mortgage's value in the event of a default. In California, the average installation price, and upper-bound loss for a foreclosing mortgage holder, is approximately $37,899 (California Solar Initiative, 2012).

In July 2010, in response to concerns about mortgage security, the Federal Housing Finance Agency (FHFA), the federal agency tasked with monitoring and regulating the mortgage–buying Government Sponsored Entities (GSEs) Fannie Mae and Freddie Mac, issued a letter essentially forbidding the GSEs from purchasing mortgages that carry the senior liens and assessments instrumental to PACE programs (City of Palm Desert, 2010; Federal Housing Finance Agency, 2010). Because mortgages that are unable to be re-sold to Fannie Mae and Freddie Mac are less desirable to mortgage underwriters, new PACE borrowers face an effective penalty in their mortgage interest rates, rendering residential PACE programs undesirable by most loan seekers. The July 2010 letter essentially stalled all residential PACE programs. Subsequent to the letter, legislative efforts to reverse the FHFA decision were introduced with bipartisan support in the 112th Congress, and the FHFA has been ordered by a federal court to undergo the formal rulemaking process for the decision. While the FHFA has successfully appealed the decision, residential PACE programs across the US have begun to restart. Given PACE's "in-limbo" status, this study provides important information on the effect of PACE and may help guide further municipal and federal decisions.

In this paper, we assess the impacts of the FHFA decision by estimating the causal effect of the PACE financing programs on residential photovoltaic solar installations. We construct a city-level panel of data from the California Solar Initiative public database (California Solar Initiative, 2012), socio-economic data from the US Census' American Community Survey (ACS) 3-year datasets for 2005–2010 and the 2010 US Census, data on solar potential from the National Renewable Energy Laboratory, and data on party affiliation of representatives from California GIS shapefiles. This city-level panel is used to assess increases in watts per owner-occupied housing of solar energy from three California PACE programs – Palm Desert, Yucaipa and Sonoma County. We use both difference-in-differences and synthetic counterfactual methods to estimate the causal effect. We find that PACE financing increases solar energy installations by approximately 3.8 W per owner-occupied house, which is an 108% increase over the mean watts/owner occupied house for the state of California.

Estimating a **causal** effect using **panel data** with a **difference in differences** method.

[2] The idea was first implemented in Berkeley, California, in 2008 as the "Berkeley FIRST" program; however, this program was a limited trial run and full roll-out was not achieved.

The paper begins by describing the interaction of PACE programs with the market for residential solar installations, focusing on the mechanisms through which PACE programs might increase solar adoption. We then introduce the three residential PACE programs studied and detail the data used in the analysis. The next section provides our methods and results. The final section offers our conclusions and implications for policy.

PACE programs and the theory of clean energy investment

In a standard model of technology diffusion, new technology adoption is typically demonstrated to follow an s-shaped curve. The s-curve results from both heterogeneous preferences and heterogeneous net present values for a new technology (Griliches, 1957). The diffusion s-curve, then, may be thought of as a distribution of consumer hurdle rates as a function of both observed and unobserved characteristics of adopters (Jaffe et al., 2004).

In many ways, this traditional model is well-suited to explaining adoption of solar energy installations among residential users. There is heterogeneity in preferences for solar energy. Research has shown that residential solar energy adoption is frequently done both for energy and to make a statement about the "green" preferences of the household (Dastrup et al., 2012). Thus traditional sources of heterogeneity in preferences for a new technology are likely supplemented by heterogeneity in preferences for conspicuously green consumption.[3] There is also heterogeneity in net present values for the residential solar systems. For residential photo-voltaic installations (RPV), studies have indicated that the average net present value rises with increased expectations of energy prices (Fuller et al., 2009), is higher for homeowners facing top-tier energy rates and for homes with good afternoon sun exposure (Bernstein and Starry, 2009).

Some have questioned whether heterogeneity in preferences and net present value of investment are sufficient to explain patterns in energy adoptions. While there is little literature on behavioral drivers of, or market barriers to, solar energy adoption, there is a sizeable literature on market barriers to energy efficiency investments. Many of these investments share characteristics with solar energy investments and we might expect to see similar barriers arise. Some potential market barriers include high up-front costs which interact with behavioral deviations from the rational individual actor model due to cognitive biases (R. Thaler et al., 2010; R.H. Thaler and Sunstein, 2008; Allcott and Greenstone, 2012; Shogren and Taylor, 2008; Tversky and Kahneman, 1974; Kahneman and Tversky, 1979), poor use of choice architecture in structuring individual decision sets (R. Thaler et al., 2010), illiquidity of investment (Jaffe et al., 2004; Sanstad et al., 1995), uncertainty over payoff of the investment (Thompson, 1997; Jaffe and Stavins, 1994a; Jaffe and Stavins, 1994b; National Research Council, 1984), asymmetric information between lenders and homeowners (Golove and Eto, 1996), and asymmetric information on capitalization of an RPV system between homeowner and potential homebuyers (Dastrup et al., 2012). If any of these market barriers exist, they will be empirically observed as high hurdle rates for investment (Train, 1985; Hausman, 1979).

Furthermore, given the underlying uncertainty in future energy prices, option value theory suggests that the rate of investment in clean energy may be lower than what would be predicted (Hassett and Metcalf, 1993; Sutherland, 1991). Because installation costs for solar panels are largely irreversible, there is value in waiting for some of the uncertainty in energy prices to resolve before making these investments.

The aggregate effect of all of these factors on diffusion rates is unclear. Some factors, such as green consumerism, suggest that adoptions of solar energy should be higher than economically justifiable based on estimates of energy saving alone. Other factors, particularly the market barriers and option value, suggest that diffusion may be slower than economically justifiable. In order to know the overall affect on residential solar energy, one needs to have a good estimate of how many net present value-positive (hereafter, NPV+) projects exist relative to how many are undertaken. However, the degree of NPV+ residential solar projects in California is debatable.

An analysis of the distribution of energy use and solar potential performed by the United States Association for Energy Economics determined that 15 percent (1.8 million) of all homes in California would benefit from replacing some or all of their electricity consumption with RPV. These 15 percent of homes represent 30 percent of all residential energy use in California (Bernstein and Starry, 2009). Other studies have shown that solar installations can generate rates of return exceeding 11 percent over a large share of California residences (Black, 2004). However, Borenstein (Borenstein, 2008a, 2008b, 2012) argues that the assessment of the number of NPV+ residential solar projects in California is overstated because the engineering estimates fail to adequately account for the interest rate on money borrowed and the price of energy that the solar power replaces. Given the lack of a clear assessment of the number of NPV+ solar investments "there should be" in California, it is difficult to know whether policy should promote more investment or less.

Now let us introduce PACE financing into this already cloudy landscape. One thing that is quickly apparent is that it will be challenging to determine whether or not PACE programs enhance overall social welfare because it is not clear if more solar adoption is desirable or not. Instead, we focus on whether PACE financing increases adoptions and leave the social welfare determination aside. If policy makers desire an increase in solar adoptions, would PACE financing be a potentially successful policy option?

The other restriction on our analysis is that we will focus on testing for a causal effect and not on testing different causal mechanisms. While we cannot distinguish causal mechanisms with our data, we hypothesize briefly here are potential causal mechanisms that may be testable in future research.

[3] The solar panels are highly visible to others.

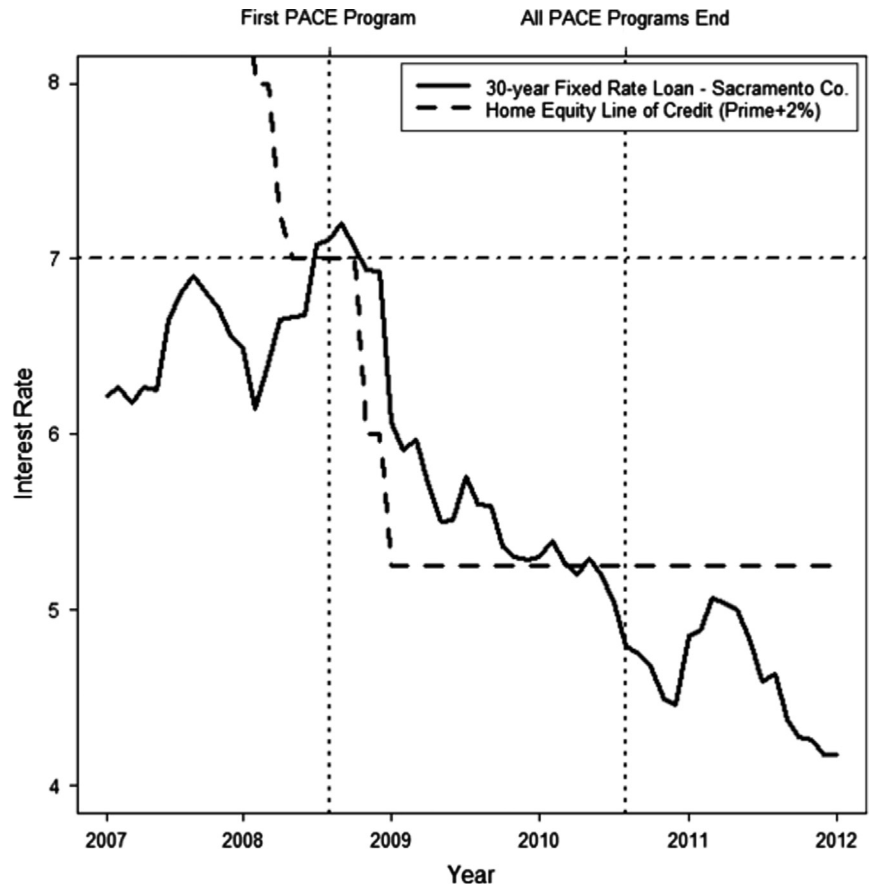

Fig. 1. Interest rates for alternative forms of residential solar energy financing relative to PACE.
Source: HSH.com, 2012

The most obvious way in which PACE financing would increase solar adoptions is by lowering the cost of financing. One might think that PACE programs increase installations by providing credit to address high up-front costs. However, many other financing options exist exclusive of PACE financing to address these costs, so PACE is not unique in providing a means of amortizing investment.

However, PACE might shift the s-curve if the program provided a lower cost of capital (i.e. lower interest rates) relative to alternative financing options. Strictly in terms of interest rates, there is little reason to believe that PACE programs lowered the cost of financing. Interest rates for PACE programs examined in this paper were 7% (Table 1). The most likely sources of alternative financing for solar installations are refinancing with a 30-year fixed rate mortgage, home equity lines of credit (HELOCs), or Energy Efficiency mortgages all of which had equal or lower interest rates.

The rates for 30-year fixed rate mortgages for comparable metropolitan areas are shown in Fig. 1. These rates were lower than 7% during all but 4 months of the study period (HSH.com, 2012).[4] HELOCs are variable interest rate and are tied to US prime interest rate. Common HELOC terms are a set percentage above prime, and many have overall caps on the total interest rate. During the study period, a 2 percent above prime HELOC would have had a rate at or less-than a PACE loan. In the Sacramento area, the Sacramento Metropolitan Utilities District (SMUD) offers a 10-year secured energy investment loan at 8.75 percent interest. Additional points and fees may apply for refinancing, and loans obtained through mortgage refinance require minimum credit ratings. Mortgage and HELOC interest payments as well as property tax payments enjoy the same tax treatment. When compared to existing financing methods that address up-front costs, none of the studied PACE programs exhibit superior financial terms.[5]

[4] Mortgage data for the San Bernardino/Riverside County Area was not available for 2007–2010. The nearest comparable area with available mortgage rate history, the Sacramento Metropolitan area, was substituted.

[5] Because PACE loans are secured by property taxes and do not require income verification, it is possible that homeowners who were unable to secure loans through regular markets were able to secure these loans through PACE programs. If this were the case, then PACE would effectively have a lower interest rate for these borrowers. However, during the time of this study, which pre-dates the housing crisis, many mortgages and HELOCs had limited income verification requirements. Defaults on PACE loans have been extremely low, less than 1% (California First). Defaults on prime rate mortgages in 2008

However, there are at least two possible ways in which PACE financing may have lower total costs than traditional financing for solar installations. First, PACE loans are transferable through property sales, which may provide a form of diversification against the risks of these loans. In general, the inability to diversify non-systemic risks combined with dual uncertainty over baseline energy costs and effectiveness of RPV installations causes individuals to inflate their hurdle rate for an investment. A traditional form of financing requires either that a full repayment of the system price be completed upon sale (regardless of the sign of the balance of equity in the home), or that an installing homeowner continues to pay for an installed system even once the home has been sold. Thus the homeowner may feel "locked in" to the investment. Though a PACE loan does not allow a RPV to be divested in the traditional market sense, the lack of acceleration terms on PACE loans (requirements to pay the loan in full upon sale of the property) may be perceived as lowering the risk of the investment. Even at similar interest rates, loans with lower risk are likely to be preferred and in the event of an investment which fails to reach break-even, potential buyers may not price this fact (if known) into their bid, providing a chance at divestment of a "losing" investment.

A second possible mechanism stems from informational asymmetry between lenders and borrowers. Traditional lenders do not generally account for the savings on electricity bills when calculating a borrower's income for loan amounts. An energy efficient investment may save a borrower $100 per month; however, their income considered "available" to repay a loan is not usually increased by the expected savings.[6] Further, communicating potential savings to lenders is a difficult process due to lack of trust in published estimates, uncertainty over effectiveness, etc. PACE loans avoid both problems and may lower the fixed-costs associated with securing financing for solar installations.

PACE loans are also thought to address information costs by increasing certainty about RPV installations. The perceived "endorsement" of RPV installations by a trusted source (the municipality) provides information at a cost lower than an individual's search cost. Finally, all of the PACE programs are accompanied by extensive marketing campaigns which lower information costs for customers.

Economic theory, however, argues against many of these quasi-price mechanisms. While the PACE loan may be transferable, selling a home with a future stream of tax liens would result in that difference being accounted for in the sale price. Informational asymmetry, it could be argued, is removed as banks search for profitable potential customers. Furthermore, endorsement of RPV installations by a municipality has likely already happened, and perhaps should carry little weight. By these arguments, PACE loans are little different from other forms of financing and therefore might have no additive impact on solar installations.

As previously mentioned, these causal mechanisms are intriguing, but ultimately untestable with our data. The remainder of the paper tests for whether PACE programs have a causal effect on solar installations. This is a critical first step in the welfare analysis of these programs. If there is no impact, no further research on causal mechanisms is required.

Sample and data

Sample of pace programs

This analysis is limited to California and focuses on estimating the impact of the PACE programs in Palm Desert, Sonoma County and Yucaipa. All three programs were reasonably mature prior to the FHFA decision to preclude Fannie Mae and Freddie Mac from purchasing mortgages with PACE assessments. Restricting the analysis to California also allows us to control for significant differences in statewide programs and incentives across states while losing very little information (at the time of the FHFA decision only 2 PACE programs were outside of CA). The analysis excludes both the BerkeleyFIRST and Placer County mPOWER programs. BerkeleyFIRST had caps on participation, so it cannot be used to estimate the uncapped impact of PACE on solar installation rates. The Placer County mPOWER program had little public information available on its implementation as of 2011. Both Berkeley and Placer County are omitted from both the "treatment" and "control" groups throughout this study. The remainder of this section provides more detail on the three PACE programs examined in our study.

Palm Desert, California "Energy Independence Program" (PDEIP)

The Palm Desert city council established the first residential PACE program in California in July 2008 (City of Palm Desert, 2008). Funding for PDEIP initially consisted of $2.5 million from the city's General Fund (City of Palm Desert, 2011) and an additional $2.5 million from the city's Redevelopment Agency (City of Palm Desert, 2012). The council also adopted a maximum interest rate of 7 percent for the initial $5 million. In February of 2010, an additional $6 million in funding was announced by the city (City of Palm Desert, 2012).

Palm Desert's "Directors Report" established the requirements for approval for a PACE loan. Under the PDEIP's "solar system" track, PV installations were required to be rated by the California Energy Commission and bids for installation must be reviewed by the program director. Installation prices that were higher than a normal rate (determined by the city) were

(*footnote continued*)
ranged from 1.2% to 1.7% in California while default rates for subprime mortgages in 2008 varied from 6.1% to 8.8% (Li and White 2009). These data do not suggest that PACE loans were extensively used as a form of financing for low credit-worthy consumers.

[6] The exception to this is the Federal Housing Authority's "Energy Efficient Mortgage Loan" program, which performs this exact function.

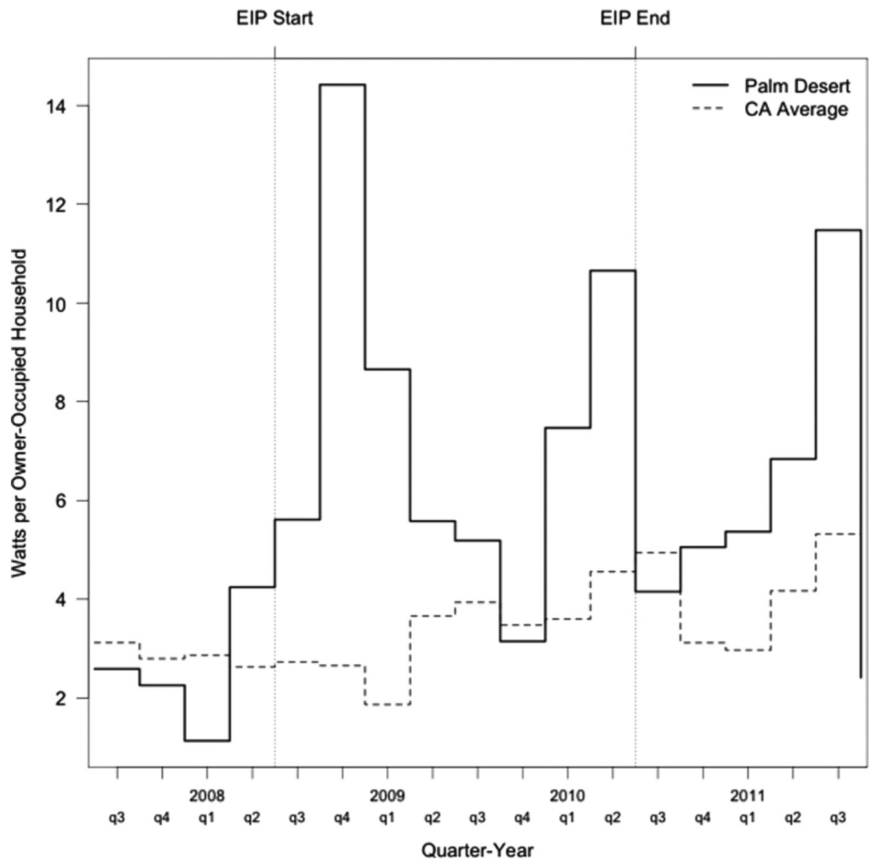

Fig. 2. Palm Desert watts installed per owner-occupied household.
Source: Authors' calculations from California Solar Initiative

required to obtain additional bids, eliminating the possibility of fraudulently inflated installations. Participation was limited to residents within Palm Desert city limits who were current on property tax assessments and did not have a history of delinquency on payments. A property's value-to-lien ratio, comprised of the property's assessed value against the total of existing special assessments on the property (i.e. assessments for streets, lighting, parks, schools, etc.) plus the EIP assessment, needed to be lower than 10–1 (City of Palm Desert, 2011). Applicants were not required to submit credit checks, mortgage balance reviews, or income verification.

Anecdotal reports from city employees indicate "lines around the block" for applicants wishing to participate in the PACE roll-out (Druyon, 2011). Fig. 2 shows the Palm Desert time trend for RPV installations in watts per owner-occupied household (W/OOH) in comparison to the statewide mean. Quarterly installations jumped from 1.13 W/OOH in the first quarter of 2008 to 5.61 W/OOH in the third quarter of 2008. A significant drop may be observed in the final quarter of 2009. This corresponds to the period between the initial funding and the February 2010 new funding. Though no applicants were turned away, a waiting list was created in this period anticipating additional funding; customers who did not wish to be on a "waiting list" may have delayed RPV installation.

The FHFA letter of July 2010 placed the PDEIP program on hiatus for 2 months. Reports from Palm Desert employees estimate a drop of about 75 percent in PACE applications following the program's FHFA-fomented hiatus (Druyon, 2011). Fig. 2 shows this drop in the 3rd quarter of 2010. From 2011 onward, the program continued to exist, but with new guidelines requiring that PACE loan applicants obtain a signed approval from their mortgage-holder prior to PACE loan approval. The effect of these requirements confounds treatment after the 3rd quarter of 2010.

Sonoma County, California "Energy Independence Program" (SCEIP)

SCEIP was initially proposed in August of 2008, and was approved by the county council in January 2009 (Quackenbush, 2008), with the first applications accepted in March 2009 (Home Performance Resource Center, 2010). The 60-day rollout from January to March of 2009 allowed local solar installation companies and industry groups to advertise the program to potential customers (Yager, 2011).

The maximum interest rate for PACE loans under SCEIP was 7 percent and participation was limited to county residents who would owe less than 110 percent of their property value inclusive of PACE loans and other loans or mortgages and are current on their property taxes and mortgage.[7] Loans were limited to 70 percent of property value for those who own their property outright (Home Performance Resource Center, 2010). No credit check or income minimums were required for participation. Effective July 1st, 2011, SCEIP participation required participants dedicate a minimum of 10 percent of the loan amount to energy efficiency improvements such as improved insulation or efficient windows unless the home rates extremely high on an energy assessment.

Proponents of SCEIP have released studies showing a significant correlation between green jobs and the SCEIP program. These reports provide anecdotal evidence from local solar installation firms and from customers. Prior to the PACE program, Sonoma County exhibited a higher-than-normal average W/OOH (Fig. 3). But installations did appear to increase significantly during the program.

After the FHFA letter of July 2010 the SCEIP required that PACE loan applicants obtain a signed approval from their mortgage-holder prior to PACE loan approval. This "partial" phase occasionally results in high, but confounded, installations after the 2nd quarter of 2010.

Yucaipa, California "Energy Independence Program" (YEIP)

Originally slated to begin in August of 2009 but delayed due to concerns over readiness, Yucaipa's Energy Independence Program (YEIP) began advertising in September of 2009 (Gray, 2012). YEIP's guidelines share structure and wording with PDEIP's, but include financing for water conservation efforts as well as electricity conservation and RPV. Fig. 4 shows the quarterly W/OOH installation amounts versus the state average. The raw data trends in W/OOH for solar installations in Yucaipa are the least compelling of any of the three programs examined in this study. There is significant volatility in W/OOH installations throughout the period and spikes in installation rates do not always align with program dates. However, there is a general upward trend in the data and most quarters following the implementation date have higher W/OOH installations relative to the state average.

Data

Descriptive statistics for all variables used in the analysis can be found in Table 2. Data on RPV installations were collected from the California Solar Initiative public database (California Solar Initiative, 2012). The California Solar Initiative (CSI) is responsible for disbursing RPV incentive payments mandated under Senate Bill 1, originally passed in 2006. Under SB1, all RPV installations in California qualify for a one-time direct payment that represents a significant portion of the RPV system's cost. Although the CSI and its database cover only RPV installations within investor owner utilities (IOUs)[8] service areas, the IOUs provide 75 percent of all of California's electricity. The incentives average $5526 and are obtained through a simple process: a customer first calls to "reserve" his incentive to lock in the current incentive rate. Once installation is completed, the customer submits a confirmation of installation, and a payment is sent by mail. Due to the significant size of incentives, we believe that the CSI database is the most accurate database of RPV installations available.

Here is the definition of time, t, which is "quarterly", and the definition of i, which is "city".

The CSI database contains individual installation-level data including installed wattage, date of first reservation, installation city, and current status. Though the database became active in January of 2007, the first 6 months of operation were a transition from the prior incentive system. Only data from July 2007 forward were included in this analysis. To avoid counting uninstalled systems, only "completed" incentives were used.

CSI data were aggregated by city and summed by quarter of reservation to smooth the volatility of solar installations, losing some information, but gaining precision. A monthly aggregation was also performed as a robustness check. The dependent (outcome) variable used in this analysis is W/OOH (watts per owner-occupied household), which is the total number of watts installed divided by the total number of owner-occupied households present in each quarter. Quantity of installations per owner-occupied household (QTY/OOH) is also used to check robustness of results. By measuring the dependent variable per owner-occupied household, we address the principal-agent or owner–renter problem.[9] Because homeowners make independent decisions on RPV installation regardless of city size, no issues arise from assuming that watts installed is linear in the number of owner-occupied households.

Here the outcome variable is also defined - W/OOH.

To control for economic and social characteristics that may drive RPV installation independent of a PACE program, yearly socio-economic city-level data was taken from the US Census' American Community Survey (ACS) 3-year datasets for 2005–2010 and the 2010 US Census. Although socio-economic city-level data varies only slightly over the study period, it is quite varied over the study cities. As such, it provides useful between-group information as well as some within-group information. Whenever possible, we focused on economic and social characteristics specific to owner-occupied housing. Owner-occupied household data is indicated with a "*" in Table 2.

And here, the exogenous covariates, the x's, are discussed.

(RPV is Residential Photovoltaics, or solar panels on houses).

[7] Some additional restrictions apply for homeowners who have filed for bankruptcy within the previous 3 years.

[8] IOUs are San Diego Gas & Electric (SDGE), Southern California Edison (SCE), and Pacific Gas & Electric (PG&E). Other areas are served by municipal, state, or federal utilities.

[9] Owners have little incentive to invest in projects that lower electricity bills when they do not pay those bills, while renters have little incentive to invest in costly infrastructure that they do not have ownership rights to.

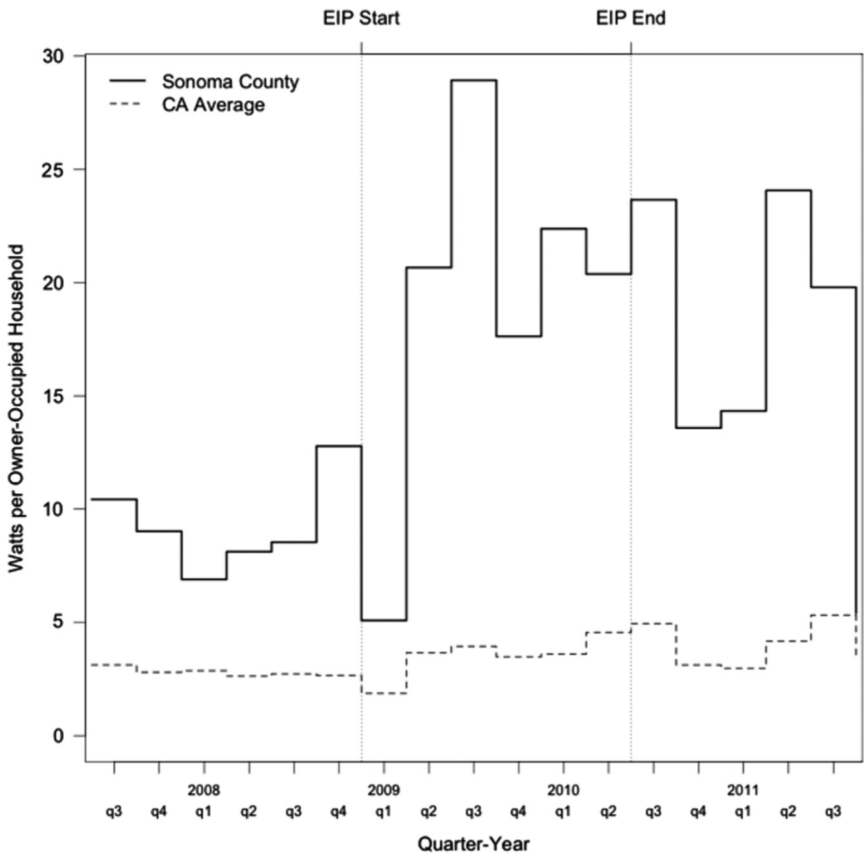

Fig. 3. Sonoma County watts installed per owner-occupied household.
Source: Authors' calculations from California Solar Initiative.

ACS 3-year datasets are available on a yearly basis, but are limited to cities and metropolitan areas greater than 20,000 in overall population.[10] Five of the nine treatment cities within Sonoma County have populations below 20,000 and are excluded when ACS socio-economic data are used.

Power prices and state incentive amounts are likely drivers of RPV installations as these are the primary economic variables facing a homeowner. Power prices vary by year, but are constant over each utility. Incentive amounts are designed by legislation to be declining – once a set amount of capacity for a utility has been installed at a given "step", the incentive rate decreases. In 2007, the incentive rate for all utilities was set at $2.50 per watt. By the end of 2011, the incentive rate for SCE customers had dropped to $0.60 per watt, while PG&E customers were offered $0.25 per watt due to higher uptake of RPV in that service area. We exploit the cross-utility variation in incentive prices to disentangle the general upward time trend in installations from the downward pressure of decreasing incentives. Real incentive rates are calculated by dividing total incentive amount by total watts installed over each utility and quarter. These rates accurately reflect the incentives faced by consumers, and may vary from the reported incentive rates.

Solar potential, a measure of the total annual gigawatt-hours absorbed in a given area, was extracted from GIS shapefiles created by the National Renewable Energy Laboratory (National Renewable Energy Laboratory, 2009). Legislative representation for each city was compiled from shapefiles maintained by the Statewide Database project at UC Berkeley (UC Berkeley, 2010). Neither solar potential nor legislative representation varies over the time period studied.

Cities implementing PACE programs display a range of incomes, education, and power prices. Palm Desert is characterized as more Caucasian and of average income. Residents are older, are more likely to be veterans, and household sizes tend to be smaller, consistent with the city's reputation as a suitable retirement location. Residents are more educated than the state average. Residents have access to average-priced electricity, have very high solar potential, and are represented in the State Assembly by a Republican.

[10] ACS 3-year datasets consist of a weighted average from a rotating subsample consisting of 1/3 of the sample population. Data is conceptually a "rolling average" over these three groups. For this analysis, this data condition is not obtrusive as socio-economic covariates are best used to illustrate trends in population. The likely lag in RPV installations resulting from a change in socio-economic conditions is sufficient to justify using "rolling average" data.

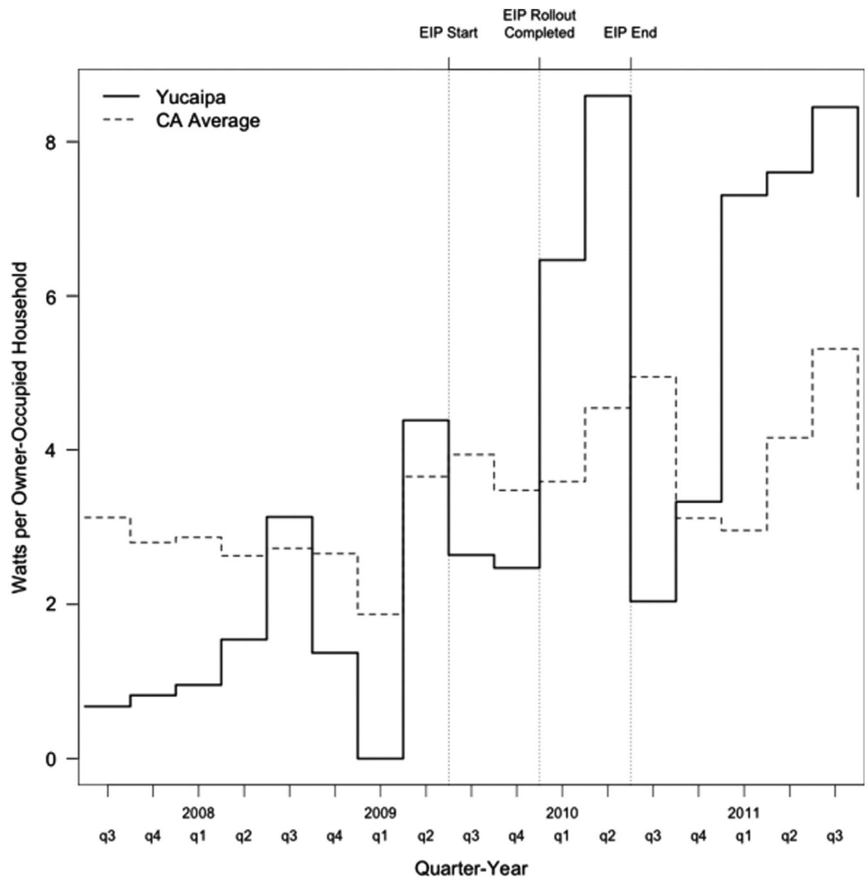

Fig. 4. Yucaipa watts installed per owner-occupied household.
Source: Authors' calculations from California Solar Initiative.

Yucaipa is comprised of a lower percentage of minorities than the state average, but residents have slightly lower income and are approximately as old as the state average, and are considerably younger than Palm Desert residents. Veteran service is higher than the state average, but lower than Palm Desert. Education is below the state average. Yucaipa is served by the same IOU as Palm Desert and thus has the same RPV incentive schedule and power pricing. The city enjoys higher-than-average solar potential, but lower than Palm Desert. Yucaipa, like Palm Desert, is represented in the State Assembly by a Republican.

Sonoma County consists of 9 cities – Cloverdale, Cotati, Healdsburg, Petaluma, Rohnert Park, Santa Rosa, Sebastopol, Sonoma, and Windsor. Countywide means are shown in Table 2. For cities with a population of less than 20,000, no data is available for fields marked with "*". These cities are omitted from the means shown here.

Sonoma County has a higher percentage of minorities, is much wealthier than the state average and both Palm Desert and Yucaipa, and is of average age, slightly younger than Yucaipa. Within the county, the percentage of owner-occupied households with income over $100,000 per year ranges from 25.59 percent to 44.83 percent. Residents are more likely to be veterans than the state average, but less likely than both Palm Desert and Yucaipa. The countywide average for undergraduate education is lower than the state average but higher than Yucaipa. Graduate degree education is lower than the state average. The average RPV incentive rate is lower than that enjoyed by Palm Desert and Yucaipa. Sonoma County residents have average power prices and, due to their coastal range location, have lower-than-average solar potential. Democrats represent all cities in Sonoma County in the State Assembly.

Methods and results

Any empirical test of the effect of PACE financing on solar installations depends, critically, on the ability to assess changes in solar installation capacity relative to what would have occurred in the absence of PACE financing. We approach this problem of causal inferences in two ways. The first approach uses difference-in-differences methods. These methods assume that in the absence of PACE financing, trends in solar installation rates among cities that adopted PACE programs and those that did not, would have been the same. Hence, observed differences in trends in installation after PACE adoption are reasonably attributable to the program. For this analysis, all three PACE program areas – Palm Desert, Yucaipa, and Sonoma Country are considered "treated" and all other cities with more than 20,000 residents constitute the set of controls.

There's the parallel trends assumption! Cities that enacted a PACE program are "treatment" and the others are "control"

Table 2
Descriptive statistics.

Variable name	Variable meaning	Units	Palm Desert mean	Yucaipa mean	Sonoma County mean	State mean	SD	Min	Max
CSI database									
W/OOH	Watts installed per owner-occupied household	Watts of RPV installed	5.776	2.700	14.964	3.542	7.833	0.000	127.970
QTY/OOH	Quantity of RPV installation per 1000 owner-occupied households	Quantity of installations	0.892	0.538	2.969	0.699	1.410	0.000	24.202
US Census / ACS									
PCTCAUC	Percentage of residential population of Caucasian descent	Percent	82.50	79.50	78.56	63.15	17.48	19.40	94.00
PCTWEALTHOOH *	Percent of owner-occupied households earning > $100,000 per year	Percent	35.07	31.85	37.26	38.31	15.08	5.30	74.26
MEDIANAGE	Median resident age	Years	53.00	37.80	39.89	37.06	7.05	23.50	77.00
PCTVET	Percentage of residential population with prior military service	Percent	14.66	12.59	8.63	7.78	3.36	1.38	25.80
HHS_OOH *	Average household size of owner-occupied households	Persons	2.00	2.79	2.64	2.90	0.42	1.42	4.18
PCTBS	Percentage of residents holding a bachelor's degree	Percent	32.06	21.03	27.91	30.12	17.58	3.00	80.70
PCTGRAD_FULL	Percentage of residents holding a graduate degree or higher	Percent	12.30	9.10	10.99	11.84	10.38	0.30	51.20
Other sources									
REALINCENTIVERATE	Real CSI incentive rate	$ per watt installed	2.07	2.07	1.57	1.80	0.52	0.65	2.50
PWRPRICE	Base tier power price	Cents per kilowatt-hour	0.153	0.153	0.157	0.156	0.007	0.148	0.181
CITYSOLARPOT	Annual average solar radiation	Gigawatt-hours per year	6.28	5.99	5.39	5.56	0.29	4.29	6.38
REP	Representation in State Assembly	0 if Democrat, 1 if Republican	1 (R)	1 (R)	0 (D)	0.40	0.49	0	1

* Indicates census variables specific to owner-occupied households.

There are some concerns about the DID estimation strategy in this context. First, it is evident from the raw data that there was an increase in applications for rebates immediately prior to the beginning of the PACE programs in Palm Desert and Sonoma County (Figs. 2 and 4), while there was a marked decrease in applications just prior to the beginning of the PACE program in Yucaipa (Fig. 3). These increases/decreases may invalidate the assumption of parallel trends underlying DID estimates. In the case of Palm Desert and Sonoma County, the pre-treatment increase in reservations would make it harder to reject the null hypothesis of no treatment effect. However, in Yucaipa, a pretreatment decrease in reservations would increase the probability of estimating a positive treatment effect even if the true effect was null. While we do test these identifying assumptions (see Section "Robustness checks" below) and find the identifying assumptions hold, statistically, some concern may remain.

This is the part where we have to **justify** the parallel trends assumption. Later, we show some evidence that supports the assumption.

To address this concern we use the synthetic counterfactual method to estimate the treatment effects. The synthetic counterfactual uses a weighted combination of cities without PACE programs to artificially construct a "without PACE" time trend for each treatment city. The effect of PACE programs is then the difference between the true trend in installations for those cities and the trends predicted by the synthetic control group. The specifics of both methods, along with the results, are described in more detail below.

Difference-in-differences

Methods

The difference-in-differences model controls for unobserved city-specific variables that may confound the effect of a PACE program. For instance, it is possible that the City of Palm Desert may have some unseen propensity to adopt new renewable energy technologies. The area is the former home to many defense and aerospace manufacturing facilities, and it is possible that the residents of Palm Desert are therefore more open to trying new technologies. With proper specification, the difference-in-differences model is able to control for these unobserved city-level effects that are time consistent. However, these models cannot control for any exogenous jumps or dips in RPV installations that occur only in a treatment city, and only at a specific quarter or quarters. If such an exogenous shock were to occur at the same time as a PACE program, the difference-in-differences model would mistakenly attribute it to the PACE policy. A media search of relevant newsletters and papers was performed to ensure no confounding events occurred, including on-bill financing programs. Further,

As we discussed in class, DID is not robust to time-variant, i-specific confounders .

Table 3
Difference-in-differences results – quarterly watts installed.

PACE is the variable of interest. D.

	W/OOH			
	(1) Fixed effects cities > 20,000 population	(2) Fixed effects all cities	(3) Fixed effects excluding top 1% of cities based on W/OOH	(4) Fixed effects cities > 20,000 population Without demographic controls
PACE	3.82**	8.52**	4.09**	3.81**
	(0.80)	(3.16)	(0.61)	(0.78)
PCTWEALTHOOH	0.09			
	(0.07)			
PCTWEALTHOOH2	−0.002			
	(0.001)			
PCTVET	−0.06			
	(0.09)			
HHS_OOH	0.81			
	(0.73)			
PCTBS	0.11**			
	(0.04)			
REALINCENTIVERATE	0.61	1.15	1.07*	0.75*
	(0.34)	(0.76)	(0.44)	(0.34)
PWRPRICE	70.03*	59.55	50.17	70.40*
	(29.84)	(47.63)	(30.00)	(29.98)
Constant	−15.97**	−8.52	−7.90	−10.74*
	(05.79)	(8.03)	(4.97)	(4.70)
Model statistics				
Observations	3396	5343	5031	3406
R-squared	0.186	0.047	0.078	0.179
Number of cities	262	411	387	262

The numbers in (parentheses) are SE(beta). Stars indicate significant p-values.

Note: All models include quarter-year dummy variables. Huber–White robust standard errors in parentheses.
** $p < 0.01$.
* $p < 0.05$.

because each observed program began in a different quarter ranging from July 2008 to September 2009, it is unlikely that an exogenous, city-level, time-variant shock could occur at the initial program start time for every program in a manner sufficient to bias estimates.

The difference-in-differences specification is given as

$$WPOHH_{it} = \alpha_0 + \alpha X_{it} + \beta D_{it} + \gamma_t + \theta_i + \varepsilon_{it}$$

Phi and Theta are the fixed effects. Here, beta is the coefficient of interest (the treatment effect)

where $WPOHH_{it}$ is the installed watts per owner-occupied household in time (quarter) t and city I; X_{it} is a vector of time-variant socio-economic variables; D_{it} is the binary variable for the presence of a PACE program at time t and in city I; γ_t are quarter dummy variables; θ_i are the city-level fixed effects, and ε_{it} are errors assumed to be independent but heteroskedastic.

This model is run on two different samples. The first uses only cities with a population greater than or equal to 20,000. For this sample we have more complete demographic data that vary over the time period of the study and the X_{it} matrix includes portion of owner-occupied households earning over $100,000 per year, bachelor's degree attainment, veteran status, owner-occupied household size, CSI incentive rate, and base tier power price as well as the square of income variables. The second sample uses data from all cities, but the demographic variables are excluded as those variables do not vary over the time period of the study. Use of the second sample also addresses concerns regarding the potentially problematic lack of time-variance of the yearly socio-economic variables as this sample assumes that socio-economic variables are constant and are absorbed by the city-level fixed-effects. Seven treatment cities in Sonoma County with populations below 20,000 are included only in the second sample.

Results

Estimates from difference-in-differences models are reported in Table 3. Table 3, column 1 provides the results on the sample of cities over 20,000 and includes the full set of demographic controls. Using this sample, PACE programs are estimated to increase solar installation capacity by 3.8 W per owner-occupied household and the impact is statistically significant at the 1% level. An increase of 3.8 W per owner-occupied house is substantial given that the average quarterly watts per owner-occupied house for California is 3.5 W. The availability of PACE financing is estimated to increase photovoltaic installations by 108%.

To gauge how important it might be to limit the sample to similar sized cities and to use the demographic controls we also ran the model on the full sample of cities without demographic controls (recall for cities smaller than 20,000 people annual demographic data were not available from the ACS). These results are in column 2. Here the estimated effect of the

PACE program is significantly higher, at 8.5 W per owner occupied house. However, this sizable result is not justifiable and is driven by inclusion of small treatment cities whose small numbers of households result in extremely large and volatile watts per owner-occupied households[11] and by exclusion of covariates. In column 3 we report the results of the specification for the full sample where we exclude cities with the top 1% of values for watts per owner-occupied households.[12] This sample restriction reduces the estimate of PACE to 4.1 W/OOH. Column 4 contains the results for the sample with cities greater than 20,000 but does not include the demographic covariates. The estimated effect of PACE is 3.8, almost identical to the estimate in column 1. Thus, our preferred estimate is 3.8 W/OOH from the restricted sample as this estimate seems stable across specifications and is more likely to satisfy the identifying assumptions of the difference-in-differences estimator.

The other covariates largely perform as expected. Most of the demographic variables are not statistically significant. This is primarily because the variation in these variables over the time frame of the analysis is relatively small. The real incentive rate is generally positive, although it varies in magnitude and statistical significance between specifications. Base tier power price had a positive effect for the restricted sample and was insignificant for the full sample.

Robustness checks

All difference-in-differences specifications were also tested using quantity of installations per owner-occupied household. Results were nearly identical in sign and significance (see Table 3b in online supplementary materials). Robustness to aggregation was tested by replicating all specifications using a monthly aggregation (see Table 3c in online supplementary materials). Treatment effects remain identical in sign and significance and show similar magnitude once rescaled from monthly results to quarterly.

To test the identifying assumption of the DID estimator, that in the absence of treatment trends in installations would have been parallel, we conduct a placebo test by keeping only the data that existed prior to the first PACE program (3rd Quarter 2008) and falsely designating 2nd Quarter 2008 as treatment for all treatment cities. Because no PACE programs were actually in place during this time, we would expect a DID estimate of zero if the identifying assumptions hold. Significant treatment effects in the pre-treatment period would signal violations of the underlying assumptions. The results of these tests can be found in Table 4. In our sample, the estimated impact of "placebo" treatment was positive, but not statistically significant. The remainder of the model performs as expected. In particular, the incentive rate remained highly significant (all $p < 0.005$) in all specifications.

One way to test for false positives is to falsely designate a treatment period and see if it's coefficient is significant. This is a placebo test.

Synthetic counterfactual

Methods

A concern with the difference-in-differences approach is that it relies heavily on the assumption that in the absence of PACE programs, trends in adoption rates in PACE cities would have been similar to those used as controls. The placebo test is suggestive that this identifying assumption is reasonable, but it is not conclusive. Hence we examine the treatment impacts using an alternative estimation strategy, the synthetic counterfactual process (herein "synthetic method"). The synthetic method is similar in spirit to matching methods where important covariates are used to find non-treatment cities of similar composition to treatment cities. However, instead of matching only on observable covariates, the synthetic method matches treated areas to control areas based on observable covariates *and observed outcomes over the pre-treatment period*. By minimizing the difference between the counterfactual and the actual city in the pre-treatment period, a reliable counterfactual is developed that behaves as the treatment city would have in absence of the treatment.

To create the synthetic counterfactual, city-level weights are established through a nested optimization function. The first level of weights is chosen to minimize differences between each treatment city (PACE city) and each city in the "control pool" (set of all non-PACE cities). A weights vector, W, is estimated with the intent of minimizing the difference between a $[(r+M)\times 1]$ vector containing the treatment city's r covariates (averaged over time) and M pre-treatment values of the treatment city's dependent variable (W/OOH) and a $[(r+M)\times J]$ matrix of similar vectors for each potential control city. In this stage of weighting, cities that are "more similar" to the treatment city along any of the covariate or pre-treatment outcome variables receive a higher weight. The use of M pre-treatment outcomes is not necessary in this stage and may lead to the "drowning out" of other covariates, a situation where all the weight is on pre-treatment outcomes and none (or very litte) on the r observable covariates. Although this "drowning out" is not *prima facia* invalid, we avoid this issue by specifying only observable covariates in the first level (i.e. $M=0$).

To evaluate the resulting difference vector in a single dimension, a second level of weighting is introduced. This matrix, V, is a $[(r+M)\times(r+M)]$ positive, definite and diagonal matrix chosen to minimize the difference between treatment city's outcome variable and the weighted pool (or "synthetic") outcome during the pre-treatment period. In this second stage weights correspond to the "importance" of each covariate and the observed outcome (Abadie et al., 2011, 2010, 2014; Abadie and Gardeazabal, 2003).

[11] For example, the City of Sebastopol (Sonoma County, only 1973 owner-occupied households) ranked two of the top-three quarterly installation rates in CA from 2008 to 2011. In each of these two quarters, only 40 installations citywide resulted in 110 W/OOH, 17 standard deviations from the statewide mean.

[12] The 99% percentile value for W/OOH is 42, which is 11 times the mean value. The cities excluded have average values of W/OOH of 60.8 with a maximum value of 128.

Table 4
Difference-in-differences placebo results.

	W/OOH			
	(1) Fixed effects cities > 20,000 population	(2) Fixed effects all cities	(3) Fixed effects excluding top 1% of cities based on W/OOH	(4) Fixed Effects Cities > 20,000 population Without demographic controls
PACE (PLACEBO)	0.69	0.20	1.05	0.63
	(0.50)	(1.47)	(1.29)	(0.48)
PCTWEALTHOOH	0.16			
	(0.09)			
PCTWEALTHOOH2	−0.002			
	(0.001)			
PCTVET	−0.05			
	(0.17)			
HHS_OOH	1.35			
	(0.98)			
PCTBS	−0.06			
	(0.06)			
REALINCENTIVERATE	4.63**	9.58**	6.44**	4.70**
	(1.18)	(3.18)	(1.60)	(1.17)
PWRPRICE	28.80	−219.91	−30.80	43.26
	(36.41)	(137.68)	(62.58)	(36.36)
Constant	−18.84*	12.99	−8.58	−16.38**
	(7.46)	(17.06)	(7.30)	(5.35)
Model statistics				
Observations	1044	1644	1596	1048
R-squared	0.082	0.011	0.094	0.07
Number of cities	261	411	399	262

Note: All models include quarter-year dummy variables. Huber–White robust standard errors in parentheses.
** $p < 0.01$.
* $p < 0.05$.

The advantage of the synthetic method over a difference-in-differences model is that it allows the difference between the counterfactual and the treatment to vary non-linearly over time based on the outcomes of the weighted control pool. In estimating V and W over both observable covariates *and* pre-treatment outcomes, the synthetic method establishes a counterfactual that minimizes the *unobserved* errors for the pre- and during-treatment periods. Although this method is unable to account for unobserved city- and time-specific exogenous shocks, no alternative model exists that does. Furthermore, the usefulness of the synthetic method lies in its ability to account for a non-linear, unobserved time trend, even if the trend would impact only a certain type of city, provided the identifying characteristics are included as covariates, or that the unobserved relationship existed during the pre-treatment period and thus contributed to selection of W, city weights. As a result, the synthetic control method is far less dependent on functional form specifications than difference-in-differences models.

We estimate the synthetic controls using code provided by Abadie et al. (2011). Because the number of possible control cities in the control pool is small and the time series is relatively short, we cannot rely on large-sample properties to accurately provide standard errors. Instead we estimate standard errors using a pseudo-bootstrapping method in which the initial subset of 411 cities is further limited to those cities that are relatively similar in important covariates (education and income) and a smaller subset is drawn for each iteration. 1100 Iterations were performed for each studied program and each iteration was performed with a random draw of 60 control cities from a sample universe of approximately 200 similar cities.

The synthetic method is applied to each single-city PACE program (PDEIP and YEIP). The countywide SCEIP would require aggregation of all six Sonoma County cities into one artificial city not observed in the real world. Therefore, we estimate the synthetic counterfactual for the largest city in Sonoma County, Santa Rosa.

Results

Results of the synthetic method for Palm Desert can be found in Fig. 5 which shows the actual trend in adoptions in Palm Desert against those predicted by the synthetic counterfactual and includes the 95% confidence bounds on the estimated counterfactual. The actual W/OOH installations for Palm Desert exceed the 95% bounds in five of eight quarters. Point estimates of the synthetic counterfactual for each quarter are provided in Table 5.[13]

[13] If the synthetic counterfactual is run including pre-treatment outcomes as possible covariates in the determination of the weighting matrix, V, (see section Methods) all of the weight in V is placed on the pretreatment outcomes. However, the pattern of impacts is largely similar. Particularly there is still

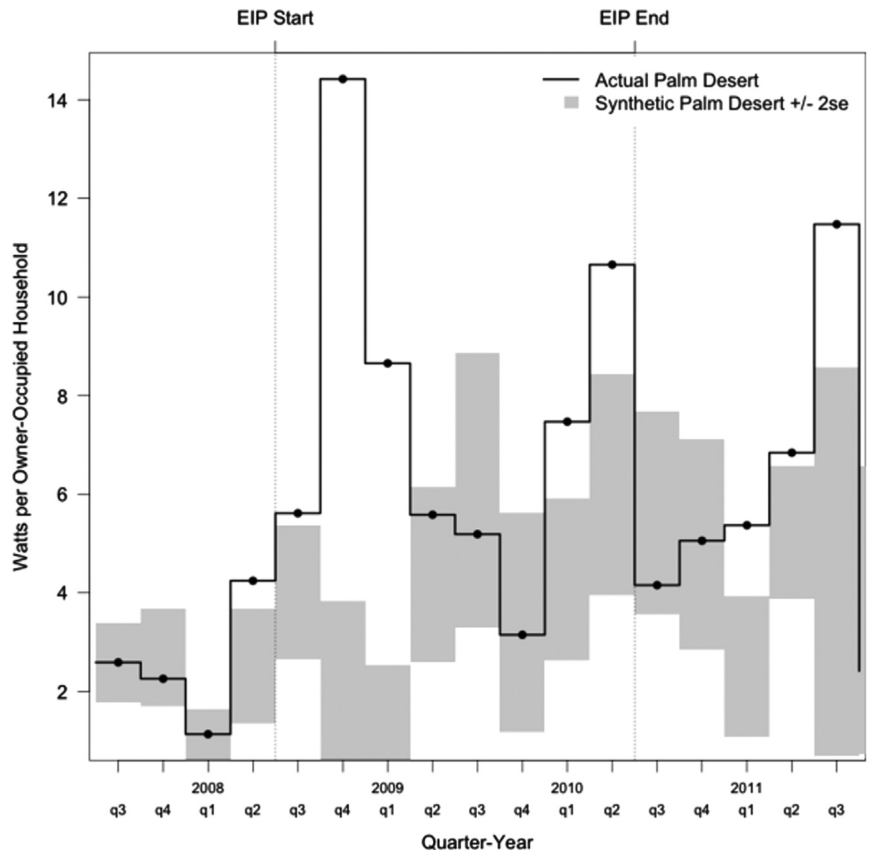

Fig. 5. Palm Desert actual vs. synthetic with 95% confidence bounds.
Source: Authors' calculations.

Table 5
Synthetic results – quarterly watts installed.

Quarter	**Palm Desert**				**Yucaipa**				**Santa Rosa (Sonoma County)**			
	Actual	Synthetic	Difference (pretreatment)	Difference (treatment)	Actual	Synthetic	Difference (pretreatment)	Difference (treatment)	Actual	Synthetic	Difference (pretreatment)	Difference (treatment)
2007q3	2.59	2.58	0.00		0.68	1.62	−0.94†		2.67	3.19	−0.53	
2007q4	2.25	2.69	−0.43		0.82	1.16	−0.34		5.25	3.01	2.24*	
2008q1	1.13	1.06	0.07		0.95	1.27	−0.32		3.04	3.45	−0.41	
2008q2	4.24	2.52	1.73*		1.54	1.33	0.22		1.99	1.65	0.34	
2008q3	5.61	4.00		1.61*	3.13	2.73	0.40		2.97	3.16	−0.19	
2008q4	14.42	1.65		12.77*	1.37	1.33	0.04		4.52	4.30	0.21	
2009q1	8.65	1.14		7.52*	0.00	1.09	−1.09†		2.61	1.83		0.78
2009q2	5.58	4.37		1.21	4.39	2.45	1.94*		9.00	3.60		5.41*
2009q3	5.19	6.07		−0.88	2.64	2.57		0.07	13.81	5.11		8.71*
2009q4	3.15	3.40		−0.25	2.47	2.73		−0.26	11.40	3.10		8.30*
2010q1	7.47	4.28		3.19*	6.46	1.75		4.71*	15.01	2.41		12.60*
2010q2	10.65	6.19		4.46*	8.59	4.33		4.26*	12.58	4.35		8.22*
		Mean	0.34	3.70		**Mean**	−0.01	2.20		**Mean**	0.28	7.34

* Indicates actual W/OOH > synthetic 95% confidence interval W/OOH.
† Indicates actual W/OOH < synthetic 95% confidence interval.

(*footnote continued*)
a large estimated effect of PACE treatment in the first quarters after implementation. The reported results, which exclude the pretreatment outcomes, are more conservative in that it is harder to reject the null hypothesis of no impact.

Relative to the synthetic Palm Desert, the pattern indicates a significant effect of the treatment. With the introduction of the PACE program, the actual Palm Desert W/OOH installed increases over the synthetic counterfactual, reaching a peak gap of 12.77 at the end of 2008. As the financing choke-point at the end of 2009 is approached, the actual Palm Desert approaches the behavior of synthetic Palm Desert. Following the additional tranche of funding, the gap increases to close to 4 W/OOH, but then returns to near-parity in the quarter containing the FHFA letter. Following the hiatus of the PDEIP program, the synthetic and the actual Palm Desert display a great deal of noise expected under the inconsistent nature of the EIP program after July of 2010. The cumulative effect of the PACE financing for Palm Desert was an increase in W/OOH of 29.60, or a quarterly average increase of 3.70.

The estimated impacts of PACE financing track nicely with funding availability. The spikes in 3rd and 4th quarters of 2008 are associated with the beginning of the PACE program while the spike in 2010 is associated with the second tranche of PACE financing issued in February 2010. The quarter containing the FHFA letter of July 2010 marks a significant drop in RPV installation.

Table 6
Installation reservations in the 2nd Quarter of 2008, Palm Desert.

Reservation date	Incentive claim review (installed)	Delay (days)
4/2/2008	6/3/2008	62
4/21/2008	10/21/2008	183
4/22/2008	7/29/2008	98
4/23/2008	7/30/2008	98
5/7/2008	1/27/2009	265
5/8/2008	10/7/2008	152
5/27/2008	6/30/2009	399
6/19/2008	1/13/2009	208
	State-wide mean delay	120

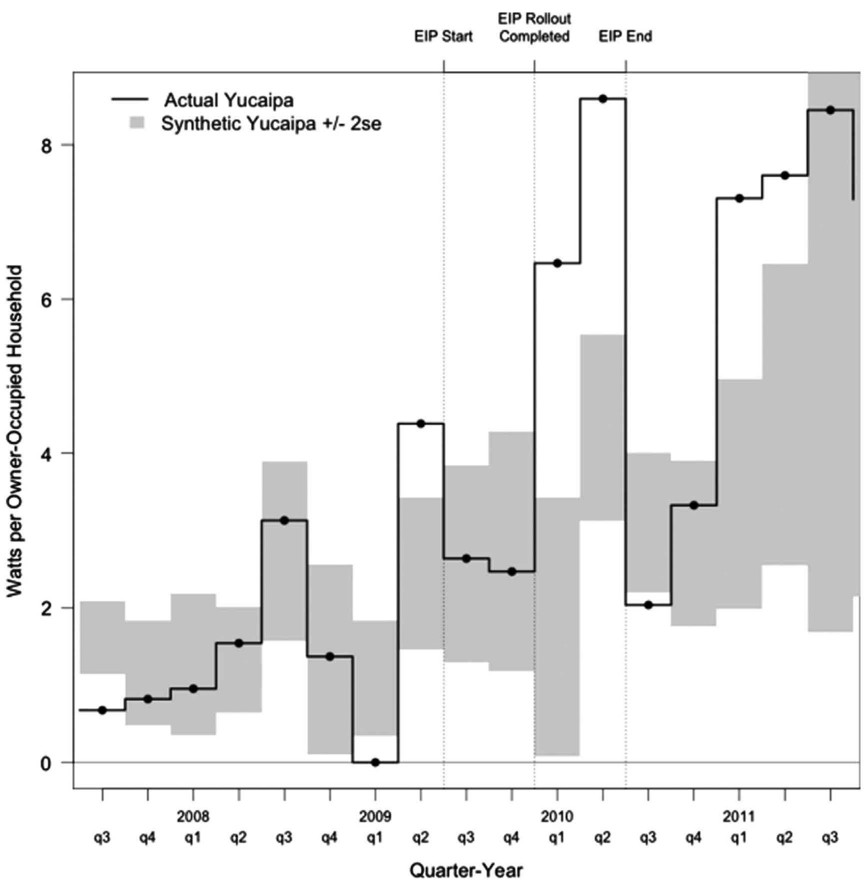

Fig. 6. Yucaipa actual vs. synthetic with 95% confidence bounds.
Source: Authors' calculations.

One anomaly in the treatment effect time series is worth further exploration. There is a sizable increase in solar installation in Palm Desert that is not predicted by the synthetic counterfactual during the 2nd Quarter of 2008. This is an untreated time period as PDEIP was authorized by state law in early July 2008 and was enacted in August 2008. As was discussed in Section Sample and data, a delay is common between the reservation of a CSI incentive and the actual installation. Residents anticipating the availability of a PACE loan may reserve their incentive early with actual installation occurring later, once a loan is completed and disbursed. In fact, residents would have a positive, risk-free incentive to call prior to a loan approval – the highest possible incentive is ensured when calling early, and no penalty is incurred for failing to complete a reserved incentive. This effect may explain the level of RPV shown in 2nd Quarter of 2008. Table 6 shows reservation and completion dates for this quarter. The average statewide delay between reservation and installation was 120 days, the average delay for reservations made in the first month was below average at 110 days, and the average delay for reservations made in the last month of the quarter was 256 days, indicating that anticipation of the program may have shifted reservations to the pre-treatment period. These installations would bias the treatment estimator downward.

The results from Yucaipa can be found in Fig. 6 and Table 5. Prior to the scheduled implementation of YEIP, the data shows a dip followed by a jump, possibly as homeowners anticipated the availability of PACE loans and reserved incentives. The dip in first quarter of 2009 is one of only two pre-treatment observations to be below the synthetic's 95% confidence interval. A dip prior to a PACE program may indicate a shifting of installations from one quarter to another, which would suggest an upward bias for the treatment estimator and the synthetic results. This dip is not observed in other cities, and the dip is far smaller than the increase occurring in the following quarter. The synthetic analysis reveals that W/OOH during the period between the scheduled rollout and the actual start (3rd and 4th Quarter 2009) were not different from the synthetic counterfactual. W/OOH in 2010 shows a marked increase over the counterfactual. In this case, the “fuzzy” nature of the initial implementation of the program likely made the program less effective. However, once implementation issues were resolved, we estimate a clear deviation from what is predicted by the synthetic counterfactual. The increase in solar installations due to PACE financing reaches a peak of 4.71 W per owner-occupied house in the first quarter of 2010 (see Table 5). The YEIP program was estimated to have lower cumulative and average impacts relative to PDEIP. The cumulative impact of YEIP was 8.80 W/OOH, which is an average quarterly impact of 2.20 W/OOH.

Results for Santa Rosa, the largest city in Sonoma, are shown in Fig. 7 and Table 5. The actual installations greatly exceed the synthetic counterfactual's upper confidence level bound in all but one treatment quarter. Prior to the initiation of SCEIP,

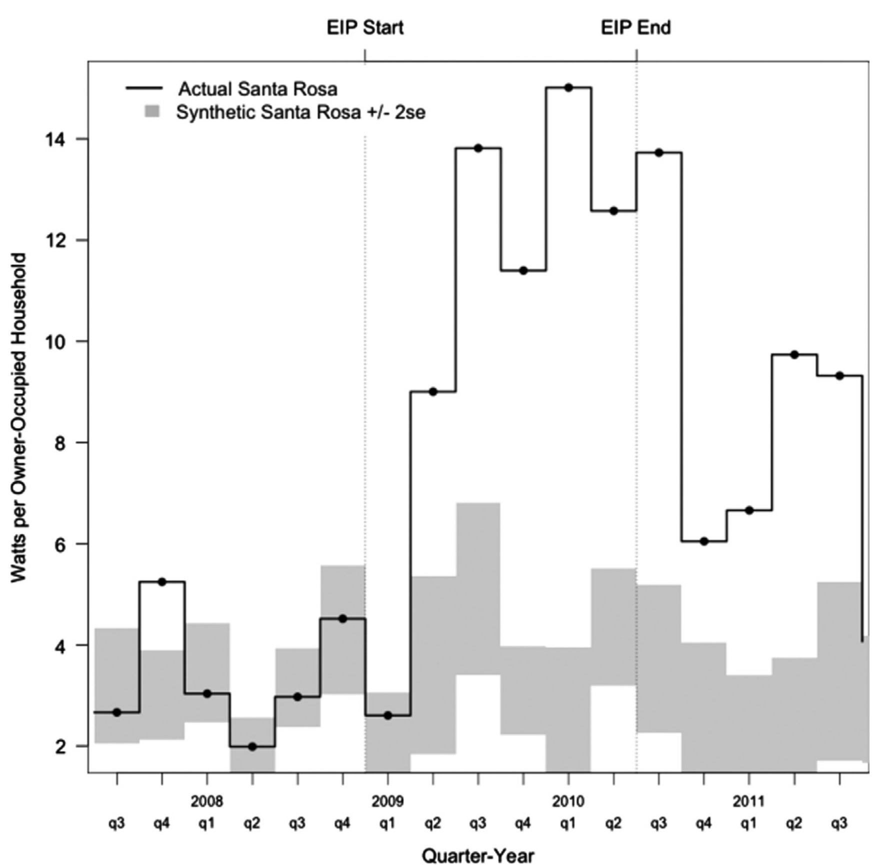

Fig. 7. Santa Rosa actual vs. synthetic with 95% confidence bounds.
Source: Authors' calculations.

Santa Rosa's installations track the counterfactual closely with one quarter outside of the synthetic counterfactual's confidence interval. No dip prior to treatment is observed. Following the July 2010 FHFA letter, Santa Rosa continues to outperform the synthetic counterfactual. Because all PACE programs existed in a state of legal uncertainty dissimilar to the pre-treatment period, some residual effect would likely remain, as observed in Fig. 7. The cumulative impact of SCEIP was 44.04 W/OOH, which is an average quarterly impact of 7.34 W/OOH.

Robustness checks

As suggested by Abadie et al. (2011), a permutation test is used to visualize the strength of the synthetic method. In a permutation test, individual synthetic counterfactuals are built for each of the untreated control pool cities. In this case, synthetic counterfactuals are matched through the same start date as the PDEIP synthetic counterfactual: 3rd Quarter of 2008. The assumption used in creating the treated city counterfactuals should hold here, namely, by matching during the pre-treated period, an accurate counterfactual is built that represents the city's RPV installation rate over the treatment period. However, in the case of the permutation tests, we actually observe the “without-treatment” outcomes during this period. Synthetic counterfactuals in the permutation tests should behave identical to the observed. Fig. 8 shows the difference between synthetic and actual for all cities in California (in gray), as well as the PDEIP difference identical to that shown in Fig. 5 (in black). Cities with a mean squared prediction error (MSPE) of greater than five times that of Palm Desert were omitted from this plot as they represent poorly-synthesized counterfactuals. The result shows that the difference in PDEIP is greater than most, but not all, other observed differences. This is particularly true of the spike in installations in 2009. However, it is clear from Fig. 8 that while the bulk of synthetic controls yielded differences from observed installation rates that bordered zero, the synthetic control method did produce some “counterfactuals” that deviated significantly from observed trends even in cities without PACE programs.

To further examine the validity of the synthetic counterfactual results, we compare these results to those obtained from the difference-in-differences model. The two models are both subject to bias if identifying assumptions are not met, yet the direction and magnitude of the bias is not necessarily related as the two methods take substantially different approaches. Thus, if the synthetic estimates are similar to those measured by the difference-in-differences models this provides more support for the validity of the findings.

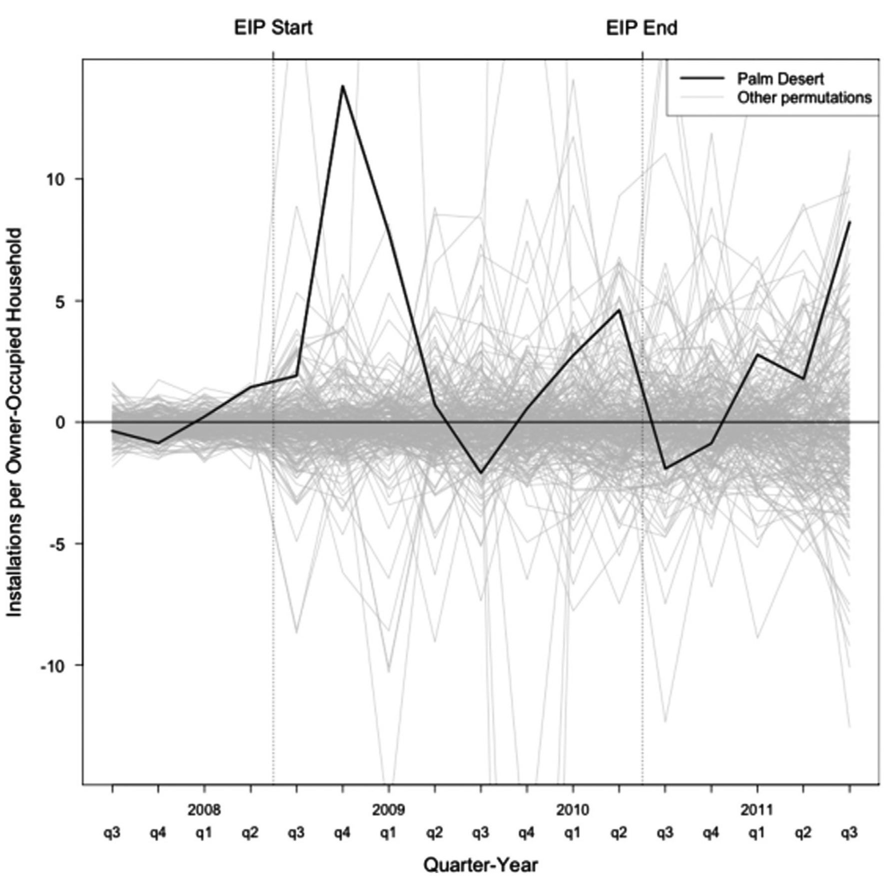

Fig. 8. Permutation test results – Palm Desert.
Source: Authors' calculations.

The 95% confidence intervals for the difference-in-differences models are 2.25–5.39 with a point estimate of 3.82. The synthetic-estimated average quarterly effect for the PDEIP program is 3.70, the estimated average quarterly impact for the YEIP is 2.20 W/OOH, and the estimated average quarterly impact for SCEIP in Santa Rosa is 7.34 W/OOH. The PDEIP estimate is well within the 95% confidence bounds for the difference-in-difference models while the estimate for YEIP is slightly below the 95% confidence interval and SCEIP in Santa Rosa is above the interval. However, the differences-in-differences estimated is a weighted average of the impacts from PDEIP, YEIP and Sonoma County. The average of synthetic-estimated quarterly impacts weighted by quarters of treatment is 4.41, which is close to the point estimates from the difference-in-differences models and well within the confidence interval.

Due to the truncated treatment periods for each program, conclusions cannot be drawn for the extended impact of a PACE program. Results do not suggest that a PACE program would produce similar results in every quarter with no diminishing return over time, nor do they indicate that PACE programs are a "one-time" impact. The longer-term impacts of PACE programs cannot be assessed due to the FHFA decision.

Conclusions and policy implications

Our analysis examines the causal effect of PACE financing in three California cities/counties using a differences-in-differences approach and a synthetic counterfactual method. Using both methods we find that PACE financing increases the adoption of residential photovoltaic solar energy systems by an average of roughly 3.8 W per owner-occupied household, which is an 108% increase over the state average. Because the PACE programs we examine were terminated after only 1 or 2 years by the FHSA, we cannot say whether these average impacts would be sustained over longer time periods.

While direct tests of causal mechanisms cannot be performed, arguably the appeal of PACE financing stems from some combination of altering the transferability of financing and capital, overcoming information asymmetries between households and lenders, reducing information costs through the accompanying marketing campaigns, and increasing certainty about expected benefits of solar installations through implicit endorsement by municipalities. Future research that could distinguish among these competing explanations would be particularly useful.

This analysis does not examine or measure the increase in risk to the national mortgage market. Default rates among PACE borrowers are less than 1% (California First) which is slightly lower than the default rate during this period among prime mortgages and substantially lower than the default rate among subprime mortgages (Li and White 2009). Because early adopters are likely to be distinctly different from the type of PACE borrowers who would participate in a large-scale PACE roll-out, policy makers must address concerns over the creditworthiness of participants and the parity between PACE payments and electricity generation or savings. This analysis may guide the benefits side of the equation; however, quantification of overall social costs must be examined in the same level of detail before public policy recommendations may be made.

Appendix A. Supporting information

Supporting information associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jeem.2014.05.001.

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