The long-run price elasticity of electricity demand*

Tatyana Deryugina
University of Illinois
and NBER

Alex MacKay
Harvard University

Julian Reif
University of Illinois

December 16, 2016

PRELIMINARY AND INCOMPLETE.
NOT FOR GENERAL CIRCULATION.

Abstract

Predicting the effects and incidence of climate change policy requires understanding how consumers respond to electricity prices. To date, studies of long-run electricity demand have relied on price changes that are transient or endogenous, and none have utilized experimental or quasi-experimental variation. We estimate the short-run (one-year) and long-run (three-year) price elasticity of residential electricity demand by exploiting price variation arising from a natural experiment: an Illinois policy that enabled some communities to select new electricity suppliers on behalf of their residents. Employing a flexible matching approach, we find that participating communities experienced long-lasting average price decreases in excess of 10 percent in the three years following adoption of a new supplier. Our estimates imply a short-run average price elasticity of -0.14 and a long-run elasticity of -0.29. We also present evidence that consumers reacted in anticipation of these price changes: usage began changing after the policy announcement but prior to the price change, which is consistent with a durable or habit good model of consumption. Our results demonstrate the importance of accounting for long-run adjustments when projecting the effects of climate change policies.

JEL codes: Q41, Q48, D12

*We thank ComEd for generously sharing electricity usage data with us, and are particularly grateful to Renardo Wilson for many helpful discussions. We also thank Torsten Clausen and the Illinois Commerce Commission for helpful discussions and Erica Myers for excellent comments and suggestions. Noah Baird, Dylan Hoyer, and Chitra Jogani provided excellent research assistance. Views reflected here are solely those of the authors. Emails: deryugin@illinois.edu, amackay@hbs.edu, and jreif@illinois.edu.

1 Introduction

The threat of climate change has prompted many governments to work toward reducing greenhouse gas emissions. The electricity industry is a natural target because it accounts for a large share of emissions and has relatively few emitters. Electricity prices in some countries, such as Germany, have already increased substantially as a result of climate change policies (Karnitschnig, 2014). Evaluating the effects and incidence of these policies depends crucially on the price elasticity of electricity demand. For example, the magnitude of an emissions decrease resulting from a carbon tax is a function of this parameter. Likewise, the costs of an emissions cap or other quantity-based policies also depend on this elasticity. It is particularly important to obtain an estimate of the *long-run* price elasticity of demand, because most policies targeting the electricity sector are intended to be long-lasting.

Yet, as we discuss in more detail below, there is little consensus on the magnitude of the price elasticity of electricity demand either in the short- or the long-run, and virtually no work on long-run elasticities that exploits quasi-experimental variation. Two major challenges are identifying exogenous variation in prices and finding a suitable control group. Much of the price variation, such as an increase in the electricity price due to an unusually hot summer, is likely to be endogenous (because weather also affects demand directly) and temporary. Small price changes, such as those due to short-run changes in fuel costs, may not be salient to consumers. Moreover, a large fraction of electricity consumption is attributable to durable goods such as refrigerators and air conditioners, so the long-run response of consumers may differ significantly from the short-run response.

We overcome these challenges by exploiting plausibly exogenous variation in electricity prices attributable to Illinois' Municipal Electric Aggregation (MEA) program. In 2009, Illinois passed a law allowing municipalities to choose an electricity supplier on behalf of their residents. Participating municipalities were required to first pass a local referendum approving the policy. Individual residents could opt out of their municipality's aggregation if so desired, but the vast majority of residents were defaulted into their municipality's chosen supplier. Importantly, because the electricity *distributor* did not change, the switch affected only the price on the electricity bill. The billing format did not change, and consumers did not have to take any action to benefit from aggregation. The first referendum took place in November of 2010, and hundreds of others followed in subsequent years. As of February 2016, 741 of Illinois' 2,800 communities had approved aggregation programs, and the vast majority of consumers in these communities switched suppliers as a result.[1]

Our analysis employs monthly community-level usage data from ComEd, one of the two elec-

[1]Source: https://www.pluginillinois.org/MunicipalAggregationList.aspx

tricity distributors in Illinois. ComEd services 885 of Illinois' communities, including the city of Chicago. Our data span the years 2007-2014, which allows us to estimate trends in electricity over large periods of time before and after most communities' passage of MEA. The large number of ComEd communities that did not pass MEA (479) in combination with a lengthy pre-period makes our setting an ideal application for a matching estimator. Specifically, we combine a difference-in-differences methodology with the matching estimator developed by Abadie and Imbens (2006, 2011). Electricity usage is highly seasonal and varies drastically across different communities. High levels of idiosyncratic baseline variation (noise) can present a challenge for traditional linear estimators. A key advantage of our matching estimator, which we demonstrate in the main text, is that it significantly increases precision by selecting appropriate control communities.

We match each MEA community to five "nearest neighbors" (control communities that did not pass MEA) on the basis of their electricity usage in 2008 and 2009, which precedes the first implementation of MEA by a year and a half. The identifying assumption is that, conditional on a match, usage is independent of whether or not a community adopts MEA. In other words, the estimator assumes that communities with similar usage profiles prior to MEA do not select into the policy based on expected future usage. We show that treated and control communities have very similar usage patterns in 2010, after the matching period, but before any community implements MEA. These usage patterns diverge only after communities begin to implement MEA, which supports the plausibility of our identifying assumption. We also show that usage trends in MEA and non-MEA communities are very similar in the two years prior to the MEA referenda.[2]

We estimate that prices fell by 22 percent (0.25 log points) and that usage increased by 5.1 percent in months 7 through 12 following the MEA referendum, relative to control communities that did not adopt MEA. The usage response over this period implies an average price elasticity of -0.16. In the second and third year, the relative price differences shrank to 13 percent and 10 percent, respectively. This modest price convergence was a result of a long-term contract expiration that significantly lowered prices among control communities. We find a corresponding (relative) decrease in usage among MEA communities, as expected. Despite employing price variation from two different sources–a large initial price decrease following adoption of MEA, and a subsequent modest (relative) price increase following a long-term contract expiration–our estimates of the price elasticity smoothly fall from -0.14 in the first year to -0.27 in the second year and -0.29 in the third year, demonstrating the large impact of long-run dynamics in this setting. Our results show that consumers are more elastic in the long run than the short run. When evaluating climate policies like carbon taxes, employing short-run estimates to project the long-run effects would severely underestimate the degree of consumer response.

[2]This result is not mechanical: the vast majority of MEA referenda in our sample are held after February of 2012, more than three years after the end of the matching period.

We also find that usage in MEA communities began increasing shortly after passage of the referendum, but before the actual price decrease (the median time between MEA passage and a subsequent price decrease is four months). Because all residents were notified by mail of the exact month of the price change, we interpret this as evidence consistent with an economic model of forward-looking consumers who purchase durable, energy-intensive goods or have consumption habits. These results demonstrate the importance of properly accounting for anticipation when estimating responses to price changes or to policy more generally.

The electricity sector was responsible for 30% of US greenhouse gas emissions in 2014, more than any other sector (U.S. Environmental Protection Agency, 2016), and is a likely target for any carbon policy. Indeed, one of the first proposed greenhouse gas regulations in the U.S., the EPA's Clean Power Plan Rule, addresses only carbon emissions from electricity generation. The effectiveness and the social cost of such regulations depend on the price elasticity of demand, among other things. Changes in the equilibrium quantity of a good following the imposition of a tax depends on both supply and demand elasticities (Salanie, 2011).[3] Underestimating the demand elasticity will lead to an underestimate of how much a carbon tax would reduce emissions, while overestimating it would do the opposite. Under cap-and-trade or a similar policy, underestimating the demand elasticity will make any policy appear more costly than it actually is. Finally, the demand elasticity also affects which side of the electricity market bears more of the cost of carbon policy - producers or consumers. All else equal, a higher demand elasticity corresponds to a lower consumer burden (Salanie, 2011).

A number of papers have attempted to estimate the short- and long-run price elasticity of residential electricity demand. The estimated elasticities vary widely: from close to zero and insignificant to about -0.9 in the short run (one year or less) and from -0.3 to about -1.1 in the long run. Much of this literature relies on state-level data and dynamic panel models, which include current prices, lagged consumption, and lagged prices as independent variables (see Alberini and Filippini (2011) for a brief review). Typically, the lagged variables are instrumented for using deeper lags of the same variables. Short-run elasticities are inferred from the relationship between current electricity consumption and current prices, while longer-run elasticities are calculated from the relationship between current and past consumption. The consistency of estimates from such models requires strong assumptions about the form serial correlation takes, and Alberini and Filippini (2011) show that the models are particularly sensitive to the exact specification used. Conversely, our approach relies on quasi-experimental variation and makes relatively few assumptions.

Some previous literature has argued that a state's average price of electricity can be considered

[3] Because carbon emission rates depend on an electricity generator's type (coal, natural gas, wind, etc), translating a carbon tax into changes in electricity consumption is more complicated in practice. But the influence of the demand elasticity is similar.

exogenous because it is regulated (Paul et al., 2009) or because the unregulated component is driven by national trends (Bernstein and Griffin, 2005). However, these facts do not eliminate the possibility of endogeneity. For example, electricity rates may be set based on the anticipated cost of electricity to suppliers and that cost, in turn, may be based on anticipated demand. Moreover, without explicitly constructing fuel-cost-based instruments, it is not possible to separate variation driven by national changes in fuel prices from endogenous variation. Finally, Alberini and Filippini (2011) point out that there also appears to be non-trivial measurement error in state-level average prices, causing attenuation bias in the demand elasticity estimates. However, they instrument for contemporaneous price using lagged price and use a dynamic panel model, which leaves the other endogeneity issues unresolved. Conversely, in our setting the price change is much more likely to be exogenous.

There are a few papers that do not use dynamic panel models, but they typically estimate short-run rather than long-run elasticities. Using a structural model and exploiting the non-linearity of the electricity price schedule in California, Reiss and White (2005) estimate the average annual elasticity to be about -0.39. Ito (2014) uses quasi-experimental variation to estimate average price elasticities 1-4 months following a price change, finding that they range from -0.07 to -0.09. However, neither of these papers estimate the price elasticity over a longer time period.

There is also a substantial literature on the role of "nudges", social norms/peer comparisons, and information provision in consumer electricity use (e.g., Allcott, 2011b; Ayres et al., 2013; Costa and Kahn, 2013; Jessoe and Rapson, 2014; Gilbert and Graff Zivin, 2014; Allcott and Rogers, ming). Whether such mechanisms are efficient means of reducing electricity usage, however, depends on both how they affect utility and how sensitive consumers are to electricity prices. The latter question has proven difficult to answer. The majority of prior work has relied on small and/or potentially temporary price changes that may not have been anticipated or recognized by the consumer (e.g., Ito, 2014; Jessoe et al., 2014). Jessoe and Rapson (2014) show that informing consumers about price increases is crucial: households that are informed are significantly more responsive to short-run price increases. Our estimates also suggest that information is important for longer-run price changes and that studies relying on month-to-month changes in electricity may be underestimating the responsiveness of consumers to longer-run price changes. Moreover, Allcott (2015) has shown that the benefits of social norms estimated by researchers may be overstated, as early adopters may also be more responsive. Our analysis mitigate this concern by imposing a municipal-level policy on a heterogeneous set of customers. Additionally, the price structure in our data is simple: on top of a monthly fixed fee, customers pay a single rate for each kilowatt-hour of electricity they consume. Given the simplicity of the pricing scheme, consumers are more easily able to respond to the marginal price.

Finally, there is a growing literature on the impact of real-time pricing (e.g., Wolak, 2011; Allcott, 2011a; Jessoe and Rapson, 2014). The elasticity we identify here is fundamentally different from the elasticity estimated in the real-time pricing literature. The latter reflects intra-day substitution patterns as well as any overall reductions in electricity. In our case, we are able to pick up both short- and long-run changes in electricity usage.

The rest of this paper is organized as follows. Section 2 discusses the history of electricity market regulation and Municipal Electric Aggregation in Illinois. Sections 3 and 4 describes our data and empirical approach, respectively. Section 5 presents the results. Section 6 discusses their implications, and Section 7 concludes.

2 Electricity Market Regulation in Illinois

2.1 Background on the Illinois electricity market

For the majority of the 20th century, the supply of electricity in Illinois was controlled by two regional monopolies: Commonwealth Edison Co. ("ComEd") and Ameren Illinois Utilities ("Ameren").[4] These two utilities both generated electricity and delivered it to everybody in the state. Deregulation was introduced in 1997 with the passage of the Electric Service Customer Choice and Rate Relief Law ("Customer Choice Act"), which separated generation from delivery. ComEd and Ameren remained the sole distributors, but they were prohibited from participating in the generation of electricity and had to sell all assets related to electricity generation.

In August of 1998, the Customer Choice Act reduced the electricity rates of residential and small commercial customers by 15-20 percent and kept them at that level for a period of 5 years. The act was later amended to extend the rate freeze through the end of 2006.

In May of 2002, residential and small commercial customers gained the ability to buy their electricity from competing providers, called alternative retail electric suppliers (ARES).[5] However, because of the rate freeze and other barriers to competition in the residential market, ARES initially served commercial customers only. By October 2005, 22,000 commercial customers were purchasing their electricity from an ARES, but the residential market remained practically nonexistent. The state tried to encourage the ARES to serve the residential market by passing the 2006 Retail Electric Competition Act, which removed some barriers to competition and provided discount programs to customers, but this had little effect. By 2009, only 234 residential customers had switched electricity providers. By contrast, there were 71,000 small commercial, large commercial, and industrial customers who had switched (Spark Energy, 2011).

In response to the perceived failure of deregulation in the residential and small commercial

[4] Ameren was formerly known as Illinois Power Co.

[5] Large commercial and industrial customers gained this ability at the end of 1999.

markets, the state decided to hold a procurement auction in order to choose their electricity suppliers and avert a rate hike following the end of the rate subsidies in 2007. However, the auction did not result in low prices and was widely considered a failure. Electricity rates jumped by over 50 percent for most residential and small commercial customers beginning in January of 2007. Following a public outcry, Illinois passed the 2007 Power Agency Act, which provided new rate subsidies for residential and small commercial customers.

2.2 The introduction of Municipal Electric Aggregation

In 2009, the Power Agency Act was amended to allow Municipal Electric Aggregation (MEA), whereby municipalities or counties could negotiate the purchase of electricity on behalf of their residential and small commercial customers.[6] The amendment was motivated by the observation that few consumers switched away from the incumbent supplier on their own, even when the potential savings were large. In order to ensure that individual consumers retained the ability to choose their provider, municipalities had to allow individuals to easily opt out of aggregation.

In order to implement an opt-out MEA program, municipalities must first educate their communities about MEA using local media and community outreach meetings. After the proposed MEA program has been registered with the state, the municipality must hold a referendum.[7] If the referendum is approved, the municipality must develop a plan, hold public hearings on it, and then have it approved by the local city council. A typical plan consists of soliciting bids from several different suppliers and then selecting the one that submits the most attractive bid. The two main ways in which suppliers differentiate themselves are price and environmental friendliness (i.e., what percent of the generation is from "renewable" sources). Because of the second component, a community will not necessarily select a supplier with the lowest price (though, in practice, most do). Once a supplier is chosen, the price is guaranteed for the length of the contract, typically about 24 months.

Importantly, instead of having to "opt in" to an ARES, customers in a community that passed an MEA referendum are automatically switched to the electricity supplier chosen by their community unless they opt out by filling out and mailing a card, calling, or going online.[8] MEA officially begins at the conclusion of the opt-out process. When MEA starts, the only change to the consumer's electric bill is the electricity supply rate. The bill is still issued by the incumbent distributor, either

[6] The amendment went into effect on January 1, 2010. On July 18, 2012, the act was further amended to allow for township governments to pursue aggregation. Two or more communities can also join together to implement a single MEA plan.

[7] The wording of the referendum question is specified in the Act and given in the Appendix.

[8] The few residential customers who had already opted into an ARES or into real-time pricing are not switched over to the chosen supplier. While we do not have specific numbers, ComEd and several energy suppliers have told us that the opt-out rate is very low. The number of non-MEA customers does, however, grow slowly over time because new residents who move to an MEA community are not defaulted into the MEA program.

Ameren or ComEd. The bill includes additional charges for distribution and capacity, which are charged by the utility and are equal for all customers in the territory. Conveniently, this means that the price effects of MEA will not be confounded by billing confusion. The appendix to our paper includes a sample ComEd bill, a sample letter notifying households of the MEA program, and a sample opt-out card.

Municipal Electric Aggregation has proven very popular in Illinois. As of March 2016, 741 of Illinois' 2,800 communities voted to implement MEA. However, a number of Illinois communities voted on but did not pass MEA. There are several possible reasons for this.[9] If residents do not trust their local government to choose the cheapest supplier and they perceive the costs of opting out as nontrivial, it may be rational for them to vote "no". Some residents may have been concerned about the resulting electricity use increase for environmental reasons. Others may not have understood the opt-out provision (although it is mentioned in the referendum) or thought that choosing an electricity provider for residents was a waste of local resources.

2.3 Determinants of electricity prices

The price of electricity can be separated into a supply component and a distribution component.[10] The distribution component is common for all customers, MEA and non-MEA, in the ComEd territory. Consumers differ in supply prices.

ComEd Prices

The default (i.e., non-MEA) supply price for ComEd customers is, by law, equal to the procurement cost to ComEd.[11] The procurement cost is determined by an auction, and unanticipated changes to procurement costs are passed through to customers. As they cannot profit from supply, ComEd does not compete on the supply price. According to ComEd, they are indifferent to the prices offered by alternative suppliers.

MEA Prices

After a community elects to implement MEA, the municipal government arranges a contract with an electricity supplier. Usually, the government hires a consultant to solicit and negotiate terms with a number of suppliers. In the majority of cases, multiple suppliers submit (private) bids for

[9] These are based on authors' attendance of a public hearing in Champaign as well as discussions with ComEd and the Illinois Commerce Commission, which regulates Illinois electricity providers and distributors.

[10] See the next section for more details on these rates.

[11] ComEd cannot make profits from electricity generation and supply. Its profits stem from delivery fees, which are set by the Illinois Commerce Commission (ICC) (DeVirgilio, 2006).

predetermined contract lengths (e.g., one-, two- and three-year contracts). In other cases, the government negotiates directly with a supplier. When determining the bid or negotiating directly, each supplier obtains aggregate community-level usage data from ComEd. This usage data, along with electricity futures, are the main factors in each offered price. Importantly, our analysis employs the same community-level usage data used by the suppliers for all non-electric space heat customers, which are the majority of customers in the ComEd territory.

3 Data

We obtain electricity usage data directly from ComEd, one of the two electricity distributors in Illinois. ComEd serves the vast majority of communities in Northern Illinois, including the city of Chicago. The data contain monthly residential electricity usage at the municipality level for ComEd's 885 service territories for the time period February 2007 to June 2014. We drop 106 communities from our analysis that are missing data or that experience changes in their coverage territory during our sample period (see the appendix for further details). For our main analysis, we drop an additional 11 communities that pass a referendum approving MEA but never implement the program. We estimate our model using a balanced panel of monthly usage for the remaining 768 ComEd communities.

As shown in Table 1, 300 communities passed a referendum on MEA during our sample period, and 289 implemented MEA.[12] Data on the characteristics of these communities were obtained from the 2005-2009 American Community Survey (ACS) Summary File. We are able to match most of the communities that implemented MEA to their ACS data (286 out of 289).

Table 2 shows the mean and median characteristics of the communities in our sample for which ACS data are available. We display results separately for communities that (i) implemented MEA and (ii) did not pass MEA. Compared to non-MEA communities, communities that implemented MEA are significantly larger, younger, and more educated. They are also less white and have more expensive housing. However, with the exception of population and housing values, the differences are not large in magnitude. Moreover, the average per capita electricity use in 2010 is similar for MEA and non-MEA communities, while the median electricity use is actually lower in MEA communities. Finally, median incomes are very similar. Overall, this table suggests that while it may be inappropriate to compare communities that implemented MEA to the typical non-MEA community, the relatively small differences between the two groups should enable us to identify an appropriate control group.

The geographic locations of these communities are displayed in Figure 1. There is a dispro-

[12] This includes 5 communities that passed a referendum in November of 2014, five months after the end of our usage data.

portionate number of communities that implemented MEA in the suburbs of Chicago, but there are also many communities in the same area that did not implement MEA. Similarly, while the Southwest corner of the ComEd territory has many non-MEA communities, there are several communities that did implement MEA. In general, communities that implemented MEA at some point during our sample period are well-dispersed throughout the ComEd territory, adding credibility to our identification strategy.

Figure 2 displays the total monthly usage among the 779 communities in our sample (including communities that passed but never implemented MEA). Usage is highly seasonal, with peaks in winter and summer and troughs in spring and fall. Usage also varies across years: the largest peak occurs in July 2012, which corresponds to a record heat wave.[13] By contrast, summer peaks are much less pronounced in 2009 and 2013, when the summers were mild.

We constructed the time series of ComEd electricity rates using ComEd ratebooks, which were made available to us by the Illinois Commerce Commission.[14] Data on MEA referenda dates, MEA supply prices, and MEA implementation dates were obtained from a variety of sources, including PlugInIllinois, websites of electricity suppliers, and municipal officials. The median length of time between passing the referendum and the MEA contract start date is 4 months. As many of the initial contracts were in effect through the end of our usage data, our estimates will be based largely on the first MEA contract signed by the community.

A consumer's electricity bill is composed of usage prices, which are equal to a usage rate multiplied by the consumer's actual electricity usage, plus an assortment of fixed fees. Illinois does not employ "block pricing", so the marginal price of electricity is constant. Implementing MEA requires a community to sign a contract with an electricity supplier that specifies a particular supply rate, which is the largest component of the usage rates; non-MEA communities pay the default ComEd supply rate. All remaining usage rates, and all of the fixed fees, are otherwise nearly identical across the communities in our sample.[15] Municipal taxes vary across communities, but the variance is small relative to the total rate. For our analysis, we use the median tax across ComEd communities (0.557 cents/kWh).

The red and green lines in Figure 3 display ComEd's monthly supply rate and the total of all remaining usage rates, respectively, during our sample period. ComEd's supply rate dropped sig-

[13]July 2012 was the warmest July in Illinois since 1936 (https://climateillinois.wordpress.com/2013/07/30/july-2012-and-2013/).

[14]Prior to June of 2013, customers with electric space heating faced a lower rate than those with non-electric space heating. Because electric space heating is relatively rare and because we do not observe household-level usage, we assume that the incumbent rate is equal to the non-space heating rate, which will be true for the majority of non-MEA customers.

[15]The average fixed fee for customers residing in ComEd service territories during our sample period is $12.52. This fee does not vary across communities, and we ignore it in our analysis as we are concerned with the marginal price. There are a few instances where the MEA supplier charges an additional fixed fee, but theses are rare and short-lived.

nificantly in 2013, when its last remaining high-priced power contracts expired.[16] The blue line in Figure 3 shows the average monthly supply rate for communities that implemented MEA. This line begins on June, 2011, when the first community implemented MEA. The average MEA supply rate is always lower than the default ComEd supply rate. The price variable in our estimating equations is equal to the MEA supply rate plus all other usage rates if the community has implemented MEA; otherwise it is equal to the ComEd supply rate plus all other usage rates.

Figure 4 plots the mean, the 90th percentile, and the 10th percentile of the log difference between the MEA and ComEd supply rates as a function of the number of months since the MEA referendum. This figure shows heterogeneity in the MEA supply rates, and that the average difference between these rates and the ComEd supply rate changes over time. We use the referendum date as our base period to conservatively capture anticipation effects that might occur prior to the actual price change.[17]

4 Empirical Strategy

4.1 Difference-in-differences Matching Framework

We select control communities by matching on pre-MEA electricity usage. Our setting is an ideal application for a matching estimator. The large set of communities available in the control group makes it likely that the nearest-neighbor matching approach will successfully find suitable comparison groups. Moreover, we have enough data in the pre-MEA period to both match treated units to control units and internally validate the matching approach. As we show below, treated and control communities that are matched based on their 2008-2009 usage also have very similar usage patterns in 2010. The usage patterns diverge only after communities begin implementing MEA in June of 2011.

Specifically, we apply a difference-in-differences adjustment to the bias-corrected matching estimator developed by Abadie and Imbens (2006, 2011). For each of the 289 treatment communities (i.e., those that implemented MEA), we find the five nearest neighbors by matching on 2008 and 2009 usage from the pool of 479 control communities available in our sample. We use these nearest neighbors to construct counterfactual usage, and we employ the the standard difference-in-differences technique to adjust for pre-period differences. The identification assumption is that, conditional on 2008-2009 electricity usage, the passage of MEA is unrelated to anticipated electricity use. We can test for this assumption indirectly by considering differences between the control and treated groups after the matching period but before the passage of MEA.

[16]This drop was not a surprise. See, e.g., http://citizensutilityboard.org/pdfs/CUBVoice/SummerCUBVoice12.pdf.

[17]The graph indicates that at least 10 percent of MEA communities implemented the change within three months of the referendum, whereas 10 percent had not implemented a price change six months afterward.

A key advantage of the nearest-neighbor approach is that it eliminates comparison communities that are not observationally similar to treated communities and whose inclusion would add noise to the estimation. Electricity usage is highly seasonal, and the degree of seasonality varies widely across the different communities in our sample. Filtering out less relevant control communities can therefore greatly increase precision.

Figure 5 provides a demonstration of this benefit. Panel (a) displays seasonally-adjusted usage for treated (MEA) communities and control communities. The difference between these two time series, which roughly corresponds to a standard event-study regression plot, is displayed in panel (c). There is a visible increase in the difference beginning in late-2011, which can be attributed to the implementation of MEA, but this difference is quite noisy. The heterogeneity in seasonal patterns poses a challenge for a standard regression that compares treatment communities to all control communities in the sample. Statistically, it is difficult to estimate an effect when the baseline month-to-month divergence in usage is of the same order of magnitude as the effect.

Panels (b) and (d) of Figure 5 show analogous plots for the nearest-neighbor matching estimator that we employ. Panel (d) shows again that the difference in log usage between treatment and (matched) control communities increases beginning in late-2011. This difference exhibits far less noise, however, than the difference displayed in panel (c), because the matching estimator selects only those control communities that are similar to treated (MEA) communities. This allows the matching estimator to generate more precise estimates than the standard difference-in-differences estimator presented in the previous section.

4.2 Estimating the Effect on Usage

We estimate elasticities using a two-stage approach. First, we use matched control communities to estimate the effect of the policy change on usage for each MEA community. Second, we use the observed change in price and the estimated change in usage to estimate elasticities.

To select the five nearest neighbors, we match on both levels and seasonal (monthly) patterns from 2008 and 2009. We use annual log usage and monthly log deviations from annual usage to construct 13 match variables, taking the average for each measure across 2008 and 2009. We standardize the variables and use an equal-weight least squares metric to calculate distance, selecting the control communities with replacement.

Let Y_{it} denote log usage for community i in period t, where $t = 0$ corresponds to the referendum date for treatment communities. For control communities, $t = 0$ corresponds to the referendum date of the treated community to which they have been matched. Let the indicator variable D_i be equal to 1 if a community implements MEA and 0 otherwise. Y_{it} is a function of D_i, so that $Y_{it}(1)$ indicates usage when treated and $Y_{it}(0)$ indicates usage when not treated. $Y_{it}(1)$

is observed for MEA communities and $Y_{it}(0)$ is observed for non-MEA communities. To calculate the effect of MEA, we construct an estimate of untreated usage for MEA communities, $\hat{Y}_{it}(0)$, which we describe below. Finally, let N denote the total number of communities in the sample, and $N_1 < N$ denote the number of MEA (treated) communities in our sample.

We estimate the average treatment effect on the treated, calculated for each period t after the referendum was passed:

$$\hat{\tau}_t = \frac{1}{N_1} \sum_{i=1}^{N} D_i \left(Y_{it}(1) - \hat{Y}_{it}(0) \right) \forall t \geq 0$$

We observe the outcome $Y_{it}(1)$ in our data. The counterfactual outcome, $\hat{Y}_{it}(0)$, is unobserved and is calculated as follows. For each treatment community i, we select $M = 5$ nearest neighbors. Let $\mathcal{J}_M(i)$ denote the set of control communities for community i. The counterfactual outcome, $\hat{Y}_{it}(0)$, is then equal to

$$\begin{aligned} \hat{Y}_{it}(0) &= \hat{\mu}_i^{m(t)} + \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} \left(Y_{jt}(0) - \hat{\mu}_j^{m(t)} \right) \\ &= \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} Y_{jt}(0) + \left(\hat{\mu}_i^{m(t)} - \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} \hat{\mu}_j^{m(t)} \right) \end{aligned}$$

where

$$\hat{\mu}_i^{m(t)} = \frac{1}{2} \left(Y_{i,month(t)}^{2008} + Y_{i,month(t)}^{2009} \right)$$

represents the average log usage in the calendar month corresponding to t for the years 2008 and 2009. The parameter $\hat{\mu}_i^{m(t)}$ is a standard bias correction that accounts for the average month-by-month usage patterns of each community. For example, if $t = 25$ corresponds to January 2014, then $\hat{\mu}_i^{m(25)} = \frac{1}{2} \left(Y_{i,January}^{2008} + Y_{i,January}^{2009} \right)$ is equal to the average log usage in January 2008 and January 2009. Thus, the estimated counterfactual $\hat{Y}_{it}(0)$ is equal to the average usage for a community's nearest neighbors plus the average (seasonal) difference in usage between that community and its neighbors.

The difference in usage between treated and control communities in the periods $t < 0$ leading up to the referendum is observed and can be calculated as:

$$\hat{\tau}_t = \frac{1}{N_1} \sum_{i=1}^{N} \left(Y_{it} - \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} Y_{jt} \right) \forall t < 0$$

Finally, the difference-in-differences matching estimator is defined as

$$\hat{\tau}_t^{\ DID} = \hat{\tau}_t - \sum_{s \in N_s} \hat{\tau}_{-s} \tag{1}$$

where N_s indicates the number of periods in the year prior to the policy change (e.g., $N_s = 12$ for monthly data). Our difference-in-differences estimate thus reflects the change in usage between treated and control communities in period t relative to the average difference in the year leading up to the policy change.

4.3 Estimating Elasticities

To construct elasticities, we regress community-specific estimates of the change in usage on the observed community-specific price changes. The community-specific measure $\hat{\tau}_{it}^{DID}$ is the single-community analog of Equation (1). It serves as the outcome variable in the following regression:

$$\hat{\tau}_{it}^{DID} = \beta_g \cdot \Delta \ln p_{it} + \eta_{it}$$

This allows us to flexibly construct period-specific estimates for elasticities to show how the response changes over time. In our main results, we run separate regressions with g corresponding to half-year intervals. The parameter of interest, β_g, corresponds to the average elasticity over the time interval g.

4.4 Inference

We employ subsampling to construct confidence intervals for our matching estimates. Subsampling resembles the bootstrap in that a distribution of parameter estimates is obtained by sampling from the observed data, but there are key differences. The bootstrap has a smoothness condition that is not met by matching estimators. Subsampling does not require this condition and is appropriate for matching. On the other hand, subsampling generally requires a larger sample size. We are fortunate to have many treatment communities in our sample. The subsampling procedure is described below.

We subsample $B_1 = R \cdot \sqrt{N_1}$ treatment communities and $B_0 = R \cdot \frac{N_0}{\sqrt{N_1}}$ control communities, where R is a tuning parameter (Politis and Romano, 1994) and N_0 corresponds to the pool of control communities.[18] The matching estimator of the average treatment effect on the treated converges at rate $\sqrt{N_1}$ (Abadie and Imbens, 2006, 2011). The estimated CDF of $\hat{\tau}$ is given by:

[18] This maintains a stable ratio of treatment to control.

$$\hat{F}(\hat{\tau}) = \frac{1}{N_b}\sum_{b=1}^{N_b} \mathbf{1}\left\{\frac{\sqrt{B_1}}{\sqrt{N_1}}(\hat{\tau}_b - \hat{\tau}) + \hat{\tau} < x\right\}$$

The lower and upper bounds of the confidence intervals can then be estimated as $\hat{F}^{-1}(0.025)$ and $\hat{F}^{-1}(0.975)$. We employ $R = 3$ ($B_1 = 51$) for the confidence intervals reported in our main tables, and show that these confidence intervals are robust to different values of R in the appendix. Similarly, we calculate elasticity estimates of β_g for each subsample to generate confidence intervals for our elasticities.

5 Results

5.1 Main Results

Figure 6 displays the average change in log prices for the treatment communities in our sample, relative to their matched control communities. The price change is exactly equal to zero in the pre-period because both the treatment and the controls communities face ComEd supply prices during that time period. Within 12 months of passing the referendum, prices in MEA communities decrease by nearly 0.3 log points relative to control communities, although they rebound significantly a few months later. The rebound is attributable to a sharp decrease in ComEd's supply price in June of 2013 (see Figure 2).

Figure 7 displays the corresponding matching estimates for usage. Prior to the referendum, the difference in usage between treatment and control communities is relatively constant. Following the referendum, usage in MEA communities increases and eventually stabilizes at around 0.04 log points. The large increase in usage in the first year followed by a modest decrease mirrors the price patterns illustrated in Figure 6. This zig-zag effect, which demonstrates that customers respond to both price decreases and price increases, provides good evidence that we are capturing the causal effect of price changes on usage.

A key parameter of interest for policymakers is the price elasticity of electricity demand. Estimated elasticities are displayed in Figure 8. The estimates increase in magnitude from about -0.1 in the first 3-6 months following the referendum up to -0.3 after two years, indicating that consumers are more elastic in the long run than the short run. To generate more precise estimates, we also estimate a specification at the biannual, rather than monthly level. Those are displayed in Figure 9. Finally, we also estimate a specification, displayed in Figure 10, that models the elasticity as a quadratic function of the number of months since the referendum.

All three specifications demonstrate an increase in the magnitude of the elasticity from about -0.1 to nearly -0.3. Moreover, these results suggest that the estimate stabilizes around 24 months after the policy change, although the standard errors and data limitations preclude us from drawing a definitive conclusion.

Our main results from the difference-in-differences matching approach are summarized in Table 3. Table 4 reports the corresponding yearly estimates.

5.2 Anticipation Effects

Economic theory suggests that forward-looking individuals will respond to policies prior to their implementation if those policies can be anticipated and if there is a benefit to the individual. For example, prior studies have documented that expectations of future policies matter when purchasing durables such as cars or houses or when making human capital investments (e.g., Poterba, 1984; Ryoo and Rosen, 2004). The effect of MEA on electricity usage is an ideal setting for detecting anticipation effects: the implementation of MEA was widely announced months ahead of time thanks to the referendum, and electricity usage depends on durable goods like dishwashers and dryers.

Figure 11 displays changes in electricity prices and usage relative to the date of MEA implementation. Prices, by construction, do not change until MEA is implemented. However, usage begins increasing in the three months prior to the price change. Moreover, Figure 7 showed that this increase did not occur prior to the referendum. Together, these results suggest that the referendum served as an announcement to consumers that prices would soon decrease, and that at least some consumers were forward looking and reduced their usage immediately, perhaps by reducing their purchases of energy efficient appliances. It is also possible that some consumers were confused and thought their electricity prices had already decreased immediately following the referendum, although we believe this is unlikely because all residents were notified by mail of the exact month of the price change.

Pursuing this point further, Figure 12 displays results for communities that passed a referendum but never implemented MEA. Although the estimates are somewhat noisy, they suggest that there was no increase in usage following the referendum. This gives further confidence to our conclusion that, prior to implementation of MEA, consumers are not responding to the passage of the referendum but rather to the announcement of future price changes.

5.3 Elasticities by Demographic Characteristics

We now investigate how our elasticity estimates vary with demographic characteristics, which matters greatly for understanding the distributional effects of policies that affect electricity prices. For this exercise, we regress log usage on log price change and add interactions between the log price change and dummy variables indicating whether or not a community is in the top half of the distribution for that variable:

$$\hat{\tau}_{it}^{DID} = \beta_g \cdot \Delta \ln p_{it} + \sum_{j=1}^{n} (\beta_j \cdot \Delta \ln p_{it} \cdot \mathbf{1}[x_i^j > median(x^j))] + \eta_{it}$$

The indicator function $\mathbf{1}[x_i^j > median(x^j)]$ is equal to 1 if the the value of the (time-invariant) variable x^j for town i is above the median of the distribution and 0 otherwise. We estimate this regression using $n = 8$ different characteristics obtained from the ACS, and report our results in Figures 13 and 14. Figure 13 shows the variables relevant to the housing stock. Where the demographic variable is significant, it is indicated with the presence of a marker. Though few of these housing coefficients are significant, communities with newer homes have a more elastic demand response, conditional on the other characteristics. This is not surprising, as newer homes often have technology, such as better insulation, that puts greater control of electricity consumption in the hands of the consumer.

Figure 14 shows the results for socioeconomic characteristics. The aggregate characteristics regarding age and race are important predictors of the elasticity of response. Younger communities have a more elastic response, as do communities with a greater percentage of white people. The magnitudes of these differences are striking, especially in comparison to the results of no effect for important economic variables such as income and education.

5.4 Robustness of Results

One concern raised by our empirical approach is whether the magnitude of the price change that a community experiences is correlated with its demand elasticity. For example, suppliers with market power might offer lower rates to more inelastic customers, as more elastic customers would demand more at the same price and drive up supply costs. We check whether this might be the case by splitting our treatment communities into seven groups based on the price change in the first two years after the referendum. We then calculate elasticities separately for each group. Figure 15 plots these estimates. We find no evidence of a relationship between the price change and the estimated elasticity.

Our choice to match on January 2008 - January 2009 electricity usage is driven by the tradeoff between allowing for a long enough post-matching period to evaluate pre-trends and matching on recent enough electricity usage to maximize match quality. However, our results are very similar if we match on electricity usage between February 2007 and February 2008 (available upon request).

Finally, we have also estimated the impact of MEA using a standard differences-in-differences event study and the sample of communities that passed MEA at some point. These results, discussed in detail in the Online Appendix, are qualitatively similar. In particular, we also see no evidence of pre-trends, supporting the identification assumption that passage of MEA was not prompted by growth in electricity usage.

6 Incidence and Policy Implications

It is well known that the partial equilibrium incidence of a tax depends on the relative elasticity of supply and demand (Salanie, 2011). In a perfectly competitive market, the change in the tax-inclusive price for a small change in the tax rate is approximately

$$\frac{\partial P}{\partial t} = \frac{\epsilon_s}{\epsilon_s + \epsilon_d}, \tag{2}$$

where P is price, t is the tax, and ϵ_s and ϵ_d are the absolute values of supply and demand elasticities, respectively. The share of the tax that is passed through to consumers, $\frac{\partial P}{\partial t}$, is sometimes called the "pass-through" rate.[19] If it is close to 1, the majority of the tax is passed through to consumers, causing them to bear the incidence of the tax. Conversely, if $\frac{\epsilon_s}{\epsilon_s+\epsilon_d}$ is close to zero, then the tax is passed through to producers rather than consumers, and consumers do not bear the burden. Equation (2) demonstrates that, all else equal, a larger demand elasticity (in absolute value) implies a lower incidence of tax on consumers, and can be used to evaluate the significance of our findings.

Suppose that the elasticity of supply is equal to 1.5. Using the one-year estimate of 0.16 for the elasticity of demand implies a pass-through rate of 90 percent for consumers. Using the three-year estimate of 0.29, by contrast, implies a pass-through rate of 84 percent. The corresponding pass-through rates for suppliers are 10 percent and 16 percent. The difference in these pass-through estimates implies a reduction in the tax burden paid by consumers relative to producers, from a ratio of 9.0 down to 5.3.

An alternative to estimating the demand elasticity is estimating the pass-through rate directly. However, this is difficult to do in the electricity sector because consumer prices are typically controlled by utility regulators. Thus, any changes in generator costs (including a carbon tax or cap-and-trade) are passed onto consumers gradually, making it difficult to directly estimate the long-run pass-through rate.

Finally, we note that policies which employ a tax to target a specific level of emissions need to correctly quantify how equilibrium electricity consumption responds to changes in taxes. For a small tax increase, the fall in equilbrium quantity is equal to:

$$\frac{\partial Q}{\partial t} = -\frac{\epsilon_s \epsilon_d}{\epsilon_s + \epsilon_d}\frac{Q}{P} = -\frac{1}{1/\epsilon_s + 1/\epsilon_d}\frac{Q}{P}. \tag{3}$$

[19] Note that $\frac{\epsilon_s}{\epsilon_s+\epsilon_d}$ is necessarily between 0 and 1.

Thus, more elastic demand corresponds to a greater fall in equilibrium electricity consumption.

7 Conclusion

An accurate estimate of the price elasticity of electricity demand is essential for evaluating the effects of energy policies such as a carbon tax. Policies that address climate change can be expected to permanently affect the price of electricity, which in turn will affect emissions. However, few reliable estimates of the price elasticity exist, as price changes in this market are often endogenous, short-lived, small, or unnoticed.

We overcome these challenges by exploiting a policy change in Illinois that allowed municipalities to select electricity suppliers on behalf of their residents. We show that communities implementing Municipal Electric Aggregation experienced large and lasting price changes relative to communities that did not implement MEA. These price drops, in turn, led to increased electricity usage. Overall, we estimate that the price elasticity of electricity is twice as large in the long-run as in the short-run.

Our finding underscores the importance of identifying settings that accurately capture long-run elasticities, as short-run data may grossly understate total effects. We demonstrate that consumers need at least two years to full adjust to a new policy. Although our data suggest that the consumer response to elasticity prices stabilizes after a two-year period, it is possible that further adjustments may occur over a longer time window. That may be a good avenue for future research.

In their analysis of the Clean Power Plan Rule, Bushnell et al. (2015) set the median demand elasticity to -0.05, an extremely low value meant partly to reflect imperfect pass-through in the wholesale electricity market. However, in their study of the Spanish wholesale electricity market, Fabra and Reguant (2014) estimate the pass-through of carbon prices to be nearly full. Combined with this finding, our estimates suggest that the policy-relevant price elasticity should be substantially higher. The findings of Fabra and Reguant (2014) also suggest that the price elasticity of consumers and other energy users is of first-order importance for translating emissions prices into emissions reductions.

Finally, we note that the natural experiment created by MEA decreased electricity prices, whereas price-based climate policies would increase prices to reduce total carbon emissions. One consideration for policy-makers is whether the demand response is symmetric for price increases and price decreases. In our data, we find some evidence that consumers respond similarly to the relative price increase occurring over a year after MEA implementation. More direct evidence of the demand response to price increases is welcome.

References

Abadie, A. and G. W. Imbens (2006). Large sample properties of matching estimators for average treatment effects. *Econometrica 74*(1), 235–267.

Abadie, A. and G. W. Imbens (2011, January). Bias-Corrected Matching Estimators for Average Treatment Effects. *Journal of Business & Economic Statistics 29*(1), 1–11.

Alberini, A. and M. Filippini (2011). Response of residential electricity demand to price: The effect of measurement error. *Energy Economics 33*(5), 889–895.

Allcott, H. (2011a). Rethinking real-time electricity pricing. *Resource and Energy Economics 33*(4), 820–842.

Allcott, H. (2011b). Social norms and energy conservation. *Journal of Public Economics 95*(9), 1082–1095.

Allcott, H. (2015). Site selection bias in program evaluation. *The Quarterly Journal of Economics 130*(3), 1117–1165.

Allcott, H. and T. Rogers (forthcoming). The short-run and long-run effects of behavioral interventions: Experimental evidence from energy conservation. American Economic Review.

Ayres, I., S. Raseman, and A. Shih (2013). Evidence from two large field experiments that peer comparison feedback can reduce residential energy usage. *Journal of Law, Economics, and Organization 29*(5), 992–1022.

Bernstein, M. A. and J. Griffin (2005). Regional differences in the price-elasticity of demand for energy. Technical report. RAND Corporation Technical Report.

Bushnell, J. B., S. P. Holland, J. E. Hughes, and C. R. Knittel (2015). Strategic policy choice in state-level regulation: The epa's clean power plan. NBER Working Paper 21259.

Costa, D. L. and M. E. Kahn (2013). Energy conservation "nudges" and environmentalist ideology: Evidence from a randomized residential electricity field experiment. *Journal of the European Economic Association 11*(3), 680–702.

DeVirgilio, B. (2006). A discussion of the deregulation of the energy industry in illinois and its effects on consumers. *Loy. Consumer L. Rev. 19*, 256.

Fabra, N. and M. Reguant (2014). Pass-through of emissions costs in electricity markets. *American Economic Review*.

Gilbert, B. and J. Graff Zivin (2014). Dynamic salience with intermittent billing: Evidence from smart electricity meters. *Journal of Economic Behavior & Organization*.

Ito, K. (2014, February). Do Consumers Respond to Marginal or Average Price? Evidence from Nonlinear Electricity Pricing. *American Economic Review 104*(2), 537–63.

Jessoe, K. and D. Rapson (2014). Knowledge is (less) power: Experimental evidence from residential energy use. *The American Economic Review 104*(4), 1417–1438.

Jessoe, K., D. Rapson, and J. B. Smith (2014). Towards understanding the role of price in residential electricity choices: Evidence from a natural experiment. *Journal of Economic Behavior & Organization*.

Karnitschnig, M. (August 26, 2014). Germany's expensive gamble on renewable energy. *The Wall Street Journal*.

Paul, A. C., E. C. Myers, and K. L. Palmer (2009). A partial adjustment model of us electricity demand by region, season, and sector. RFF Discussion Paper 08-50.

Politis, D. N. and J. P. Romano (1994). Large sample confidence regions based on subsamples under minimal assumptions. *The Annals of Statistics*, 2031–2050.

Poterba, J. M. (1984). Tax subsidies to owner-occupied housing: An asset-market approach. *The Quarterly Journal of Economics 99*(4), 729–52.

Reiss, P. C. and M. W. White (2005). Household electricity demand, revisited. *The Review of Economic Studies 72*(3), 853–883.

Ryoo, J. and S. Rosen (2004). The engineering labor market. *Journal of Political Economy 112*(S1), S110–S140.

Salanie, B. (2011). *The economics of taxation*. MIT press.

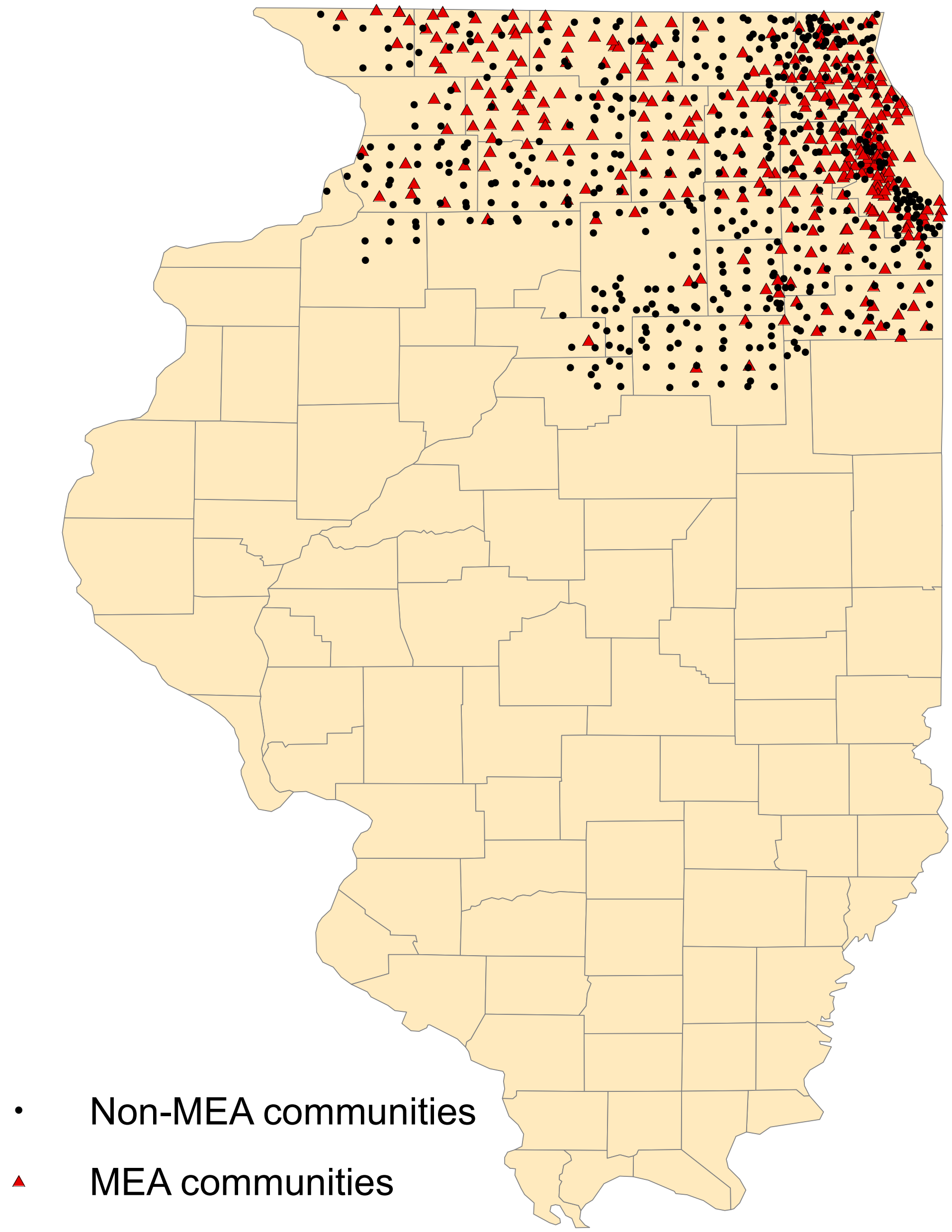
Spark Energy (2011). The history of electricity deregulation in illinois.

U.S. Environmental Protection Agency (2016). Inventory of u.s. greenhouse gas emissions and sinks: 1990-2014. Technical report. EPA 430-R-16-002.

Wolak, F. A. (2011). Do residential customers respond to hourly prices? evidence from a dynamic pricing experiment. *American Economic Review: Papers and Proceedings 101*(3), 83–87.

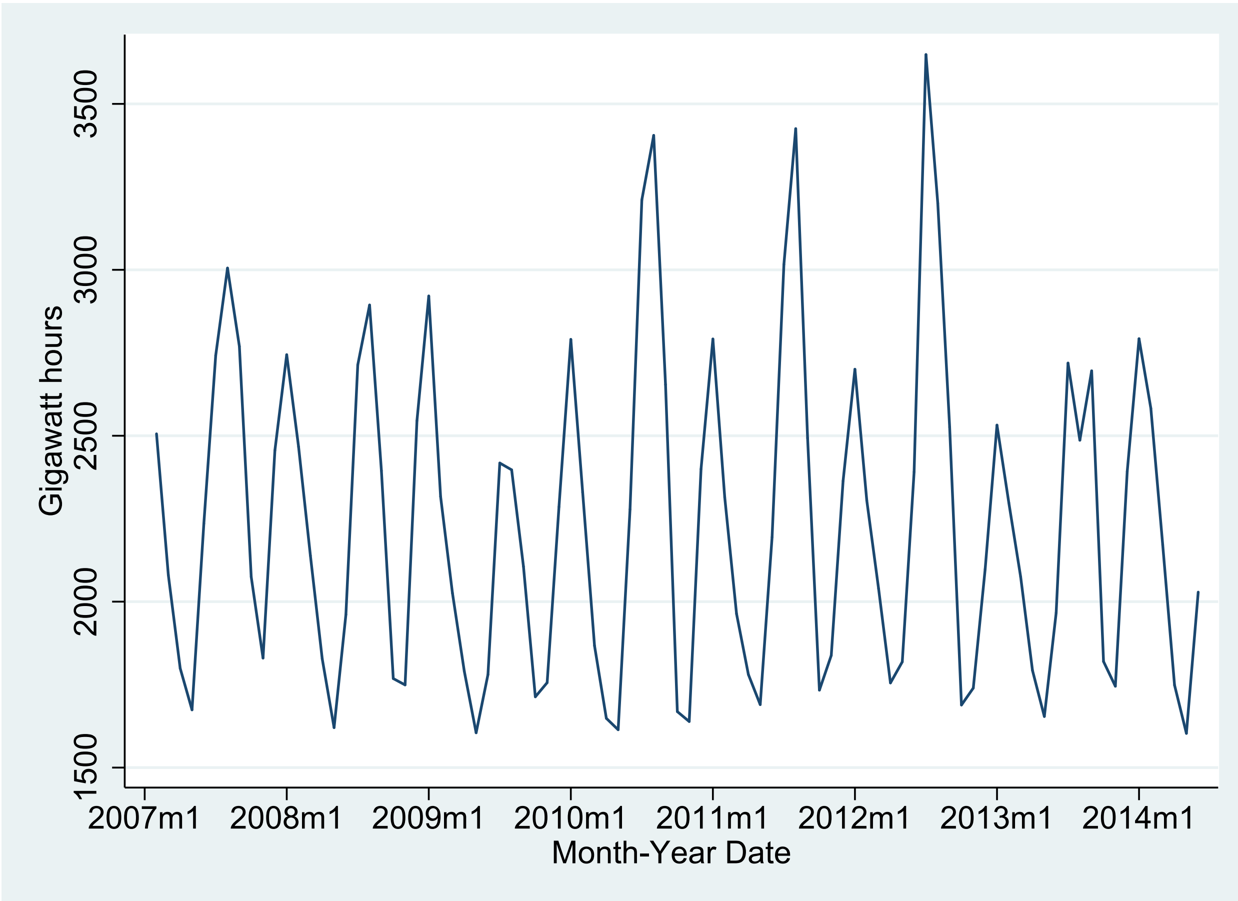
Figures

Figure 1: Locations of ComEd communities in our sample



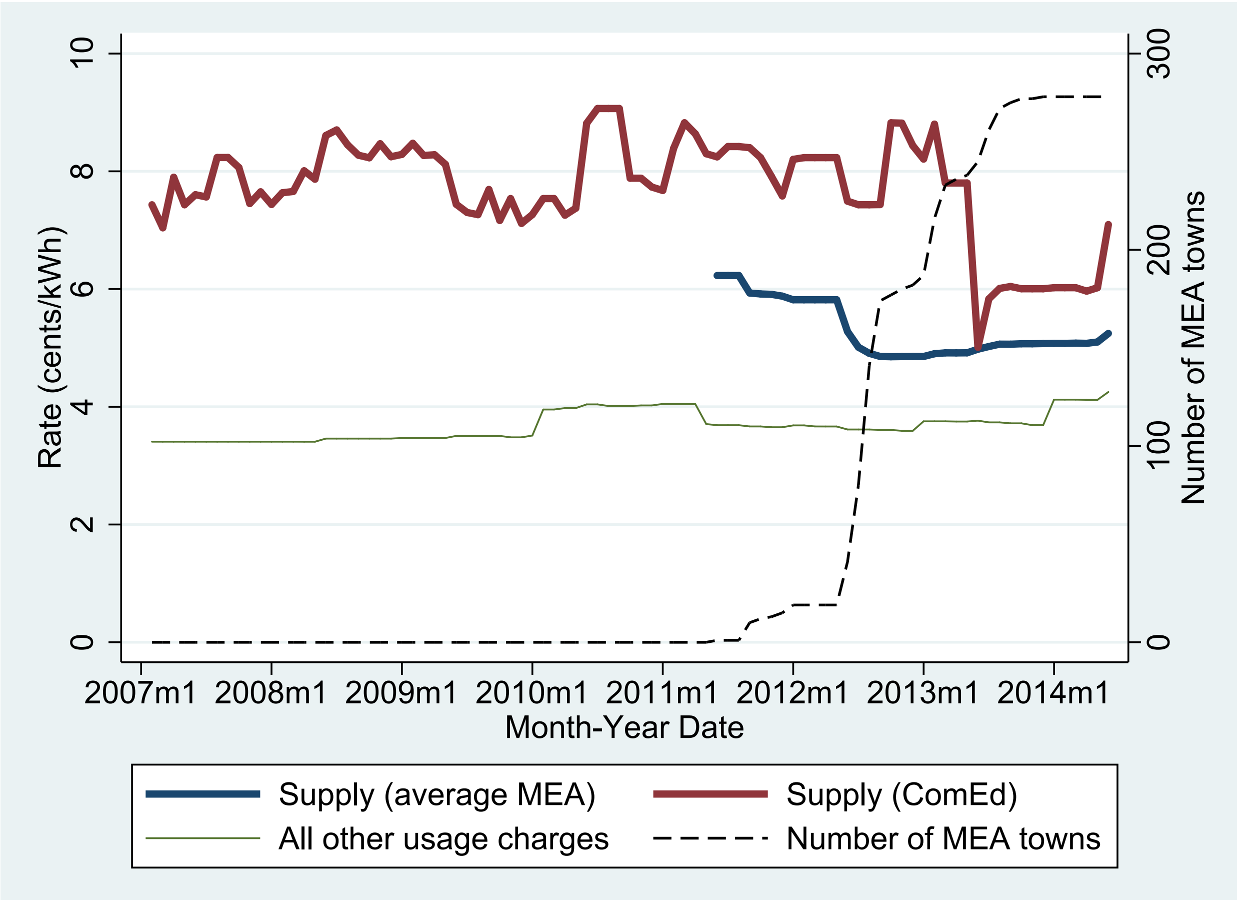
Notes: Figure displays total electricity usage across the 671 ComEd service territories in our estimation sample that we matched to observations in the 2005-2009 American Community Survey.

Figure 2: Monthly electricity usage in Illinois ComEd service territories, 2007-2014



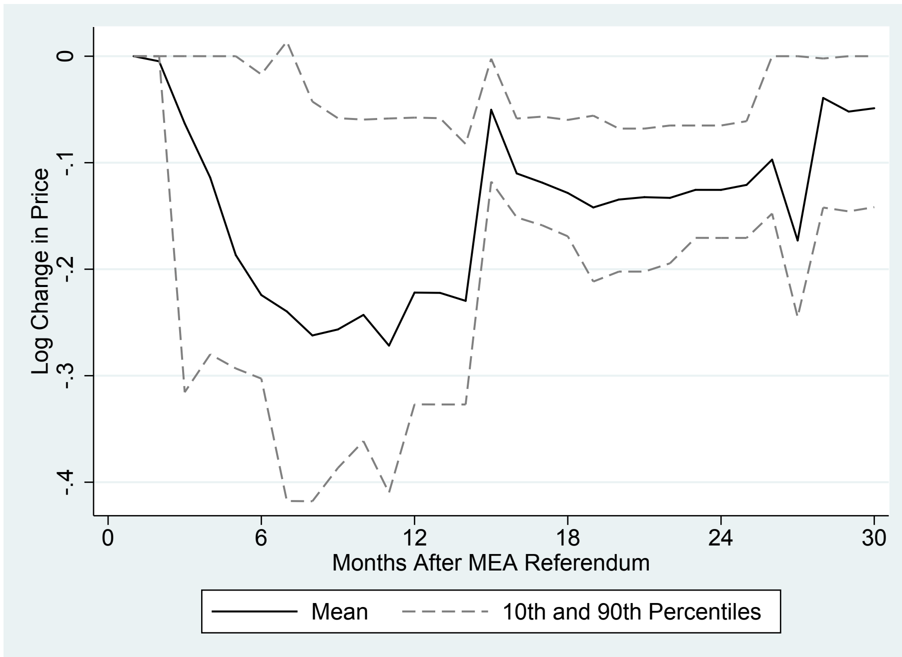
Notes: Figure displays total electricity usage across the 768 ComEd service territories in our estimation sample.

Figure 3: Monthly electricity rates in Illinois ComEd service territories, 2007-2014



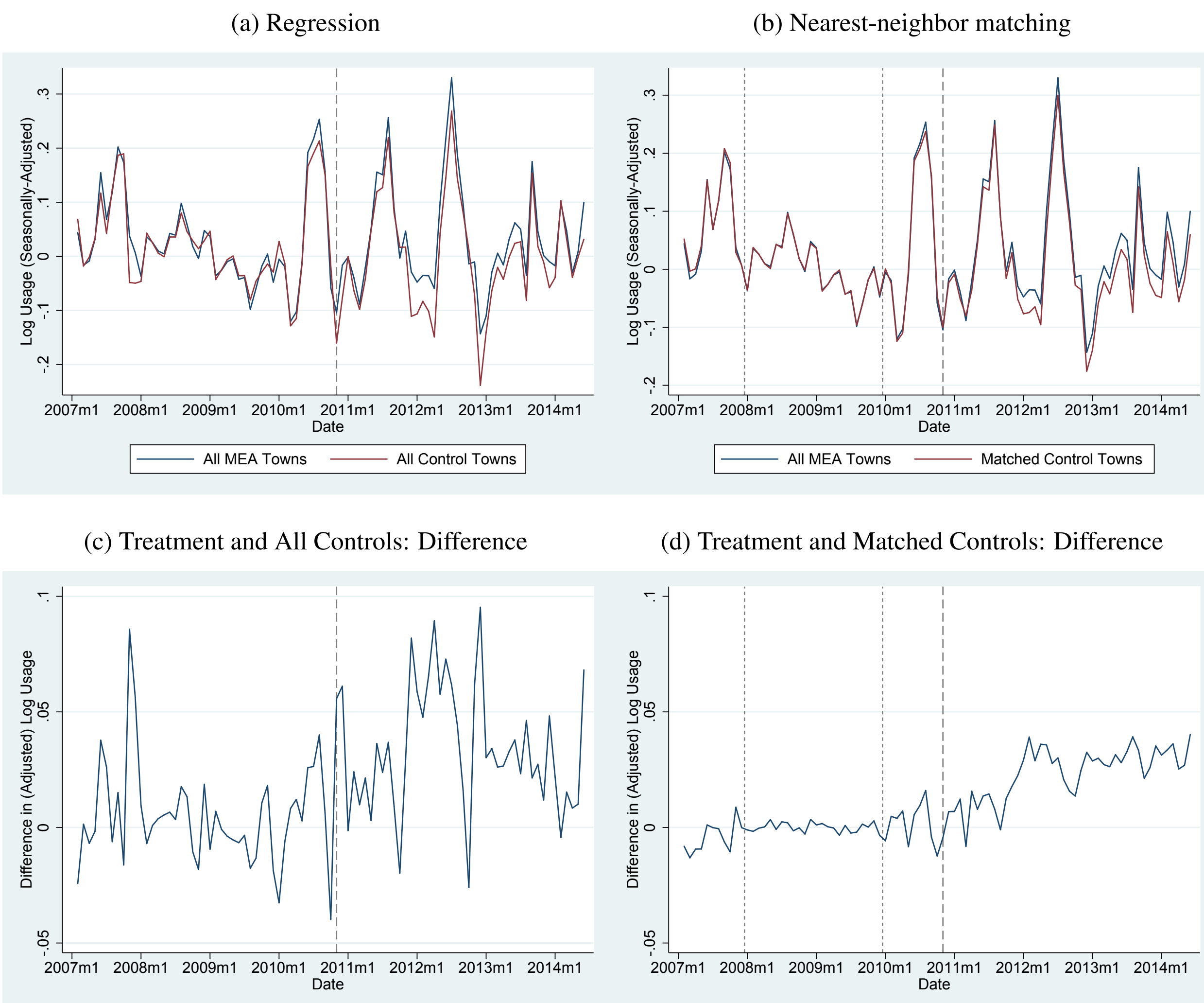
Notes: The blue line displays the average supply rate among all communities that adopted municipal electric aggregation (MEA). The first community adopted MEA in June of 2011. Non-MEA communities pay the supply rate charged by ComEd (red line). The green line displays the total of all other electricity rates on a consumer's residential bill, which do not depend on whether a community has adopted MEA. These displayed rates correspond to those for a single family residence with gas heating.

Figure 4: Log Change in Electricity Prices for MEA Communities



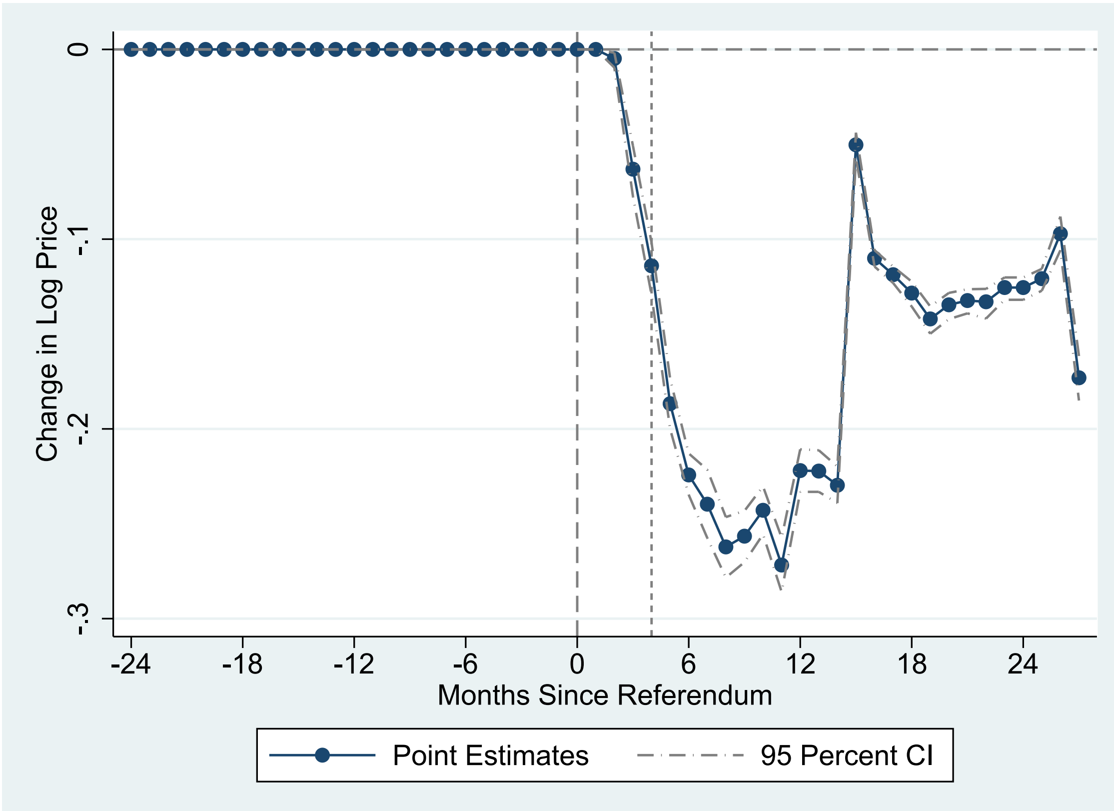
Notes: This figure illustrates the heterogeneity in price changes realized by MEA communities. Three points of the distribution are displayed: the mean, the 90th percentile, and the 10th percentile. The distribution is plotted as a function of the months after the MEA referendum was passed; the mean difference of zero in period one is explained by the fact that all communities took at least one month to implement a price change. A few communities realized supply prices higher than the ComEd rate.

Figure 5: Comparing regression to nearest-neighbor matching



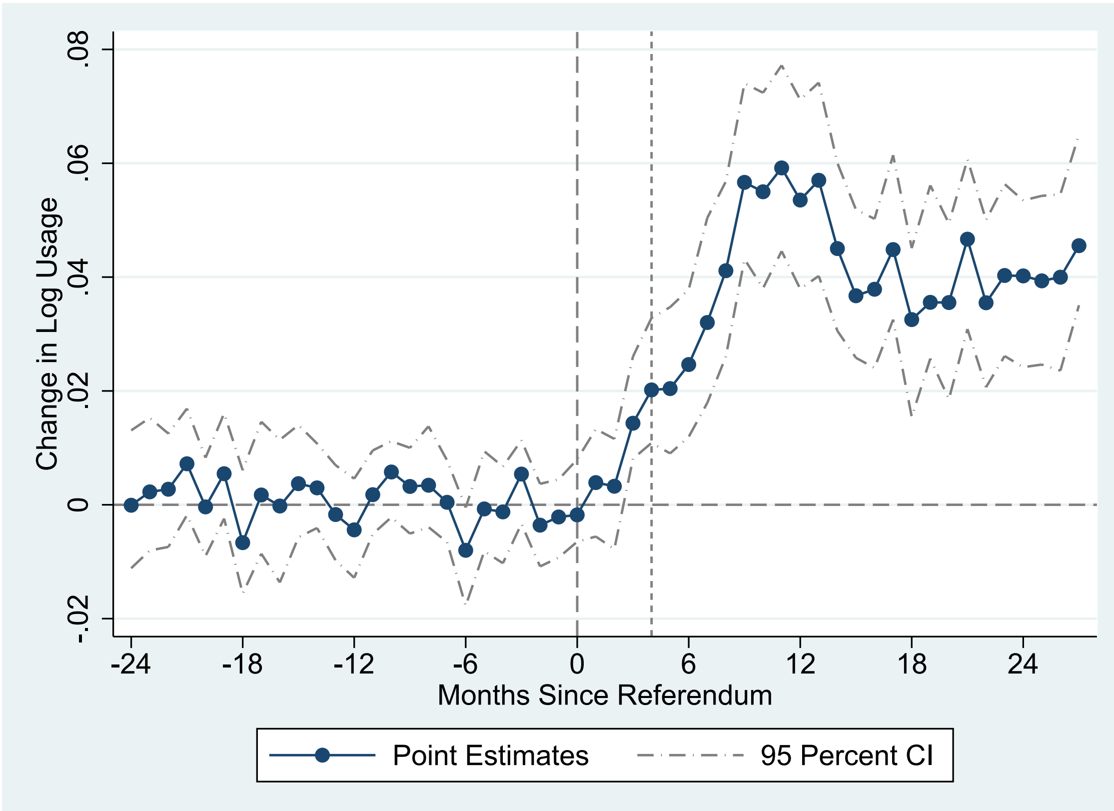
Notes: Panel (a) displays seasonally-adjusted usage for all MEA and non-MEA communities. The red line corresponds to the control group in a typical regression. Panel (b) employs the nearest-neighbor matching procedure, in which five communities are selected for each MEA community, and the control line is weighted by how often each control community is selected. Panels (c) and (d) plot the differences between the treatment and control lines in Panels (a) and (b), respectively. Panel (d) demonstrates that the matching procedure greatly reduces noise compared to a standard regression. The pre-period fit is much better, even for 2010 usage, which was not used in the matching procedure. The vertical dashed lines indicate the first referendum date. The vertical dotted lines in Panels (b) and (d) indicate the window used to match based on usage.

Figure 6: Effect of implementing MEA on log prices



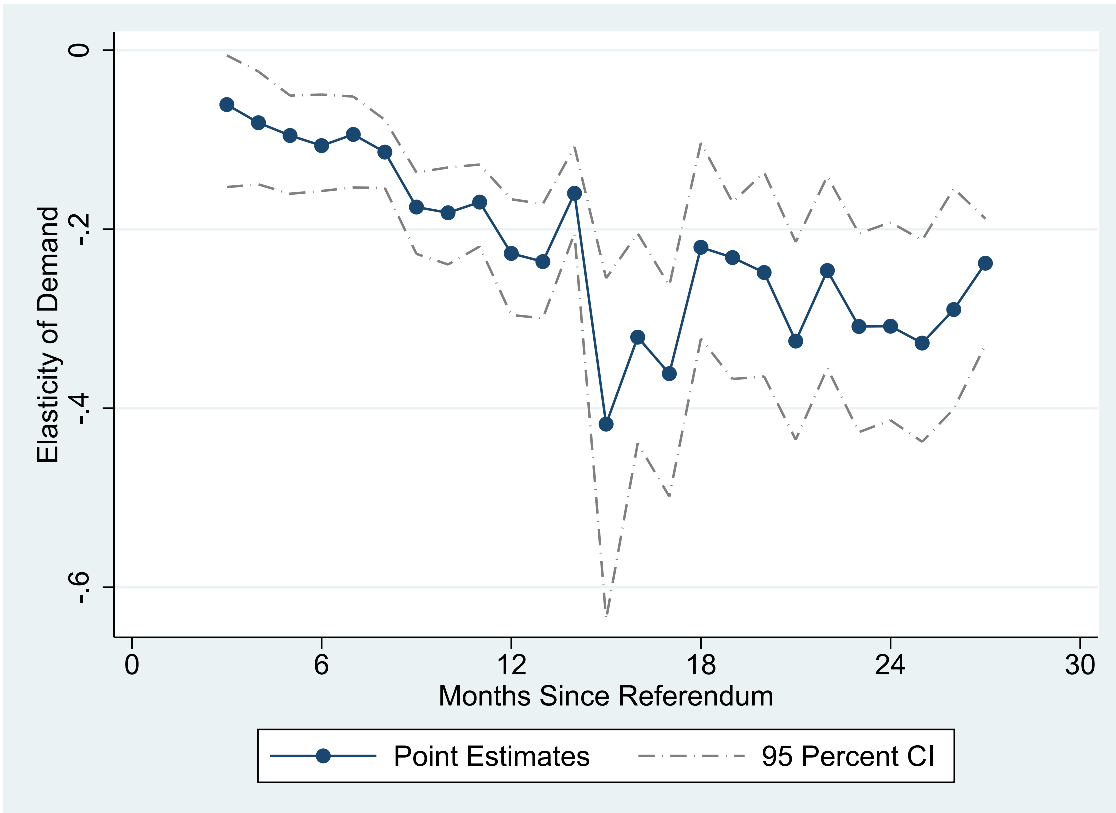
Notes: The figure displays estimates of the mean price effect of implementing MEA in a community relative to that community's five nearest-neighbors, as defined by the matching procedure outlined in the main text. Prices differences are calculated using the natural log of the marginal electricity supply rates. The pre-period difference is exactly zero because all communities faced the ComEd price during that period. The short dashed line indicates the median implementation date relative to when the referendum was passed. Confidence intervals are constructed via subsampling.

Figure 7: Effect of implementing MEA on log usage



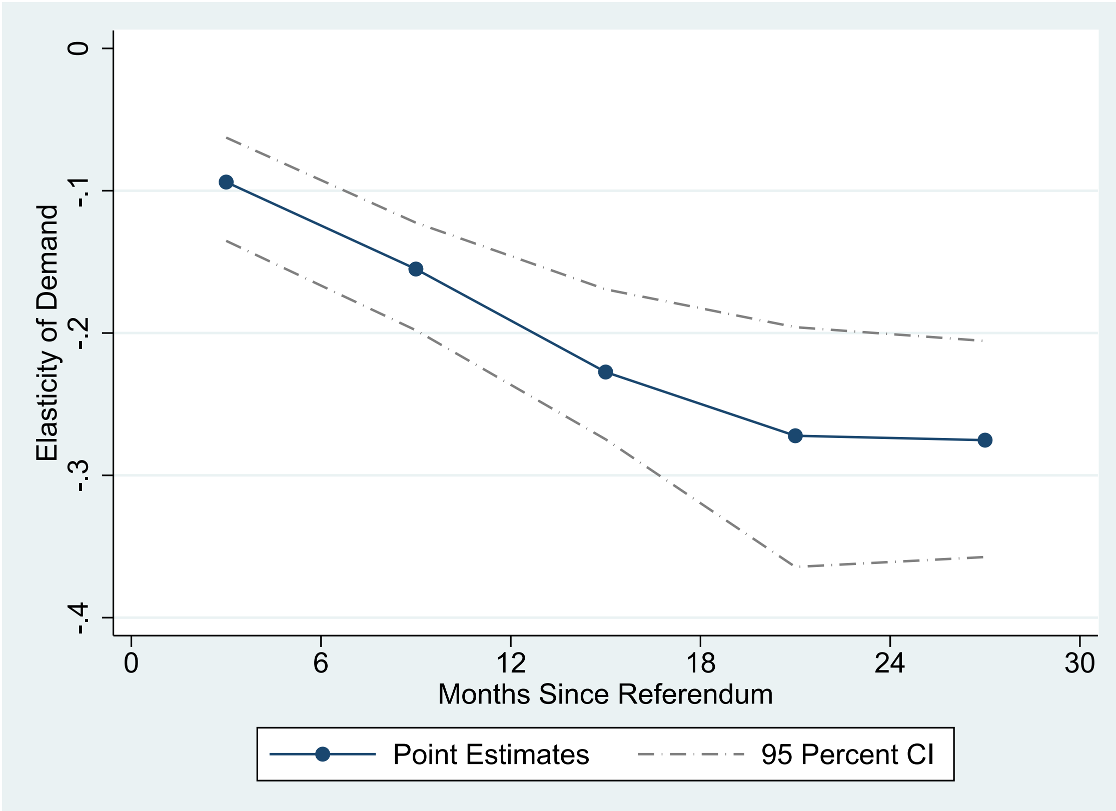
Notes: The figure displays estimates of the mean usage effect of implementing MEA in a community relative to that community's five nearest-neighbors, as defined by the difference-in-differences matching procedure outlined in the main text. The short dashed line indicates the median implementation date relative to when the referendum was passed. Confidence intervals are constructed via subsampling. The usage is normalized so that the average usage difference in the year prior to the referendum is zero. After this normalization, the difference in the month prior to the referendum is -0.002.

Figure 8: Estimated price elasticities, monthly



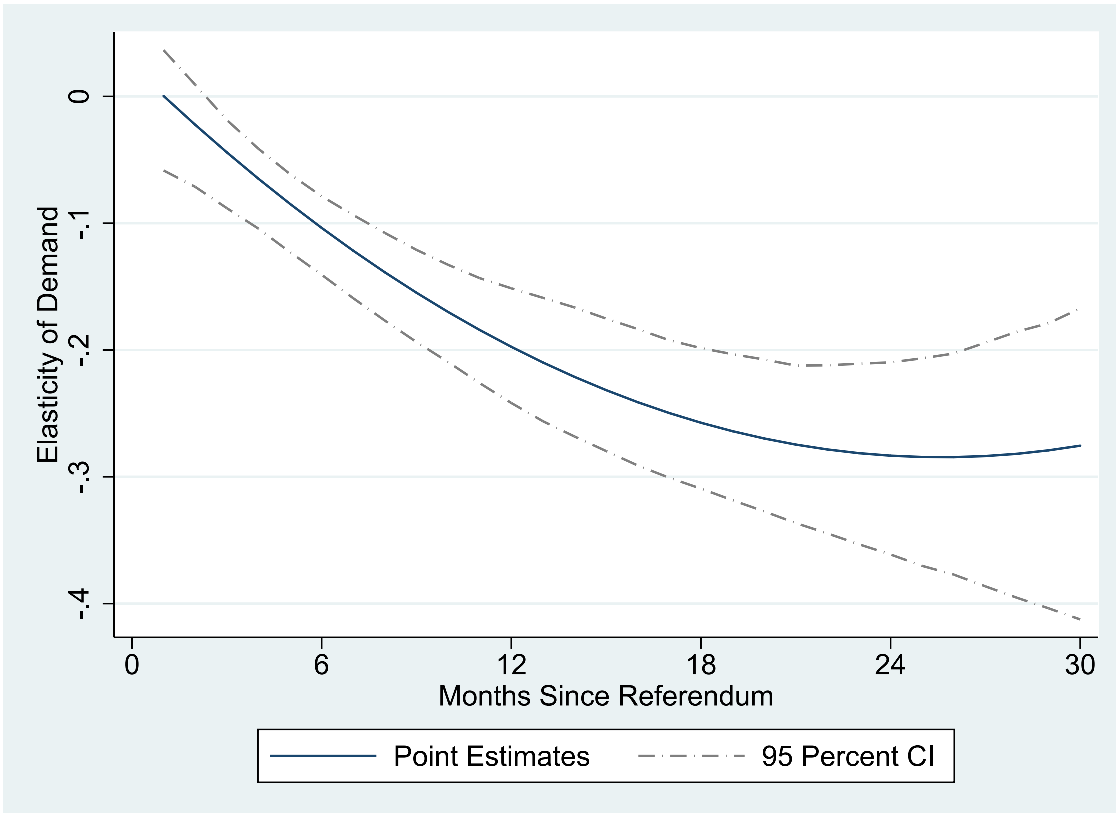
Notes: Elasticities are calculated for each month by regressing community-month changes in log usage on the observed change in log price. Confidence intervals are constructed via subsampling.

Figure 9: Estimated price elasticities, biannual



Notes: Elasticities are calculated for each six-month period by regressing community-month changes in log usage on the observed change in log price. The corresponding counts of observations for each six-month group are: 1685, 1656, 1504, 1144, and 589. Confidence intervals are constructed via subsampling.

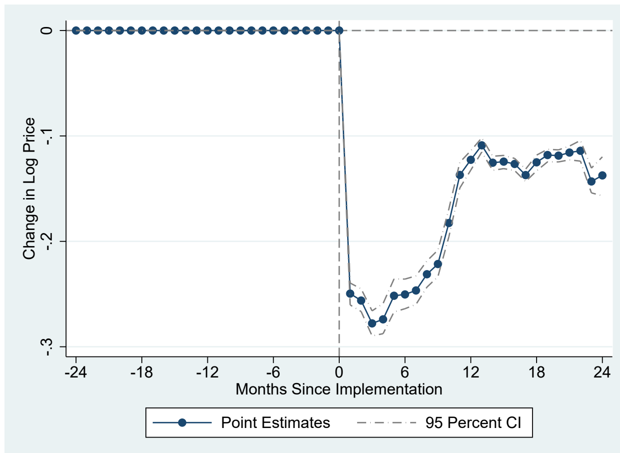
Figure 10: Estimated price elasticities, quadratic fit



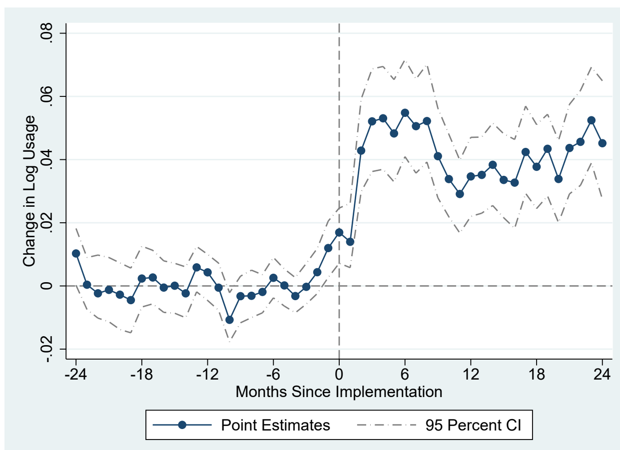
Notes: The time-dependent elasticity is estimated using a quadratic specification. Community-month changes in log usage are regressed on changes in log price, where the log price changes are also interacted with months since referendum and the square of months since referendum. These three parameters are used to construct the estimated elasticity response curve as a function of time. Confidence intervals are constructed via subsampling.

Figure 11: Anticipation effects of implementing MEA on log usage

(a) Log price

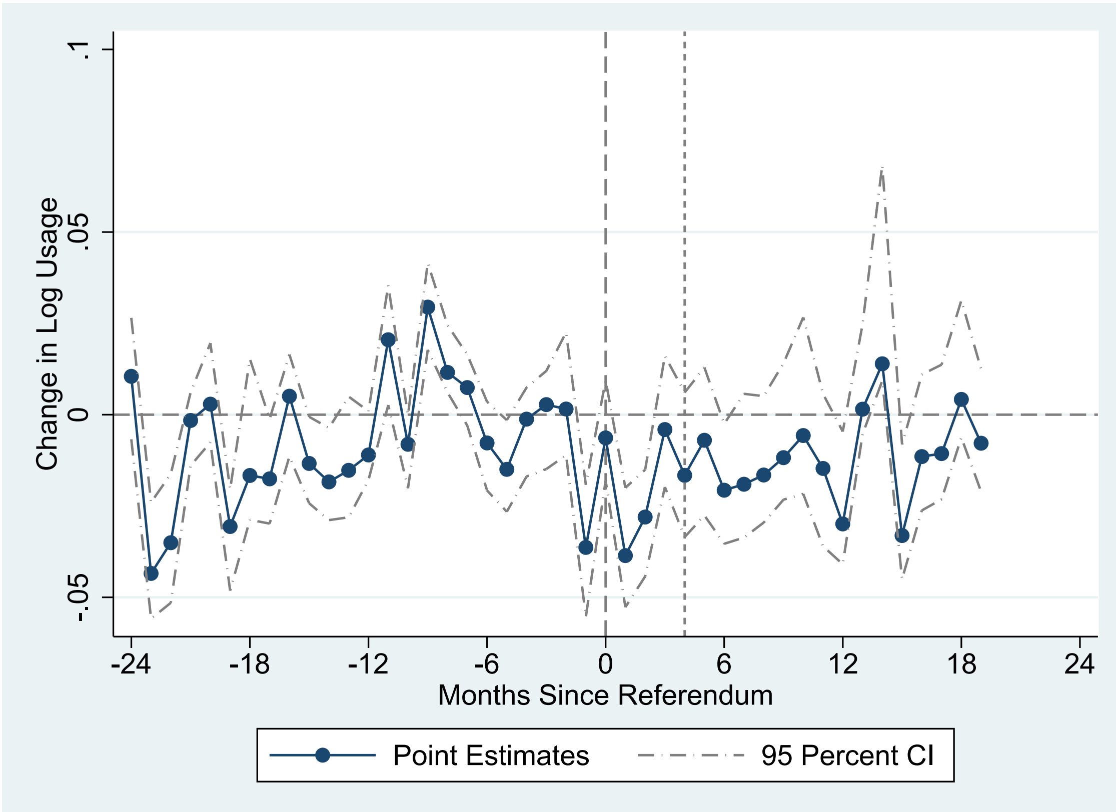


(b) Log usage



Notes: The figure displays estimates of the anticipation effect of implementing MEA in a community relative to that community's five nearest-neighbors, as defined by the difference-in-differences matching procedure outlined in the main text. The vertical dashed line corresponds to the month before MEA was implemented (i.e., the month before the price change). Confidence intervals are constructed via subsampling. The usage is normalized so that the average usage difference in the year prior to the referendum is zero.

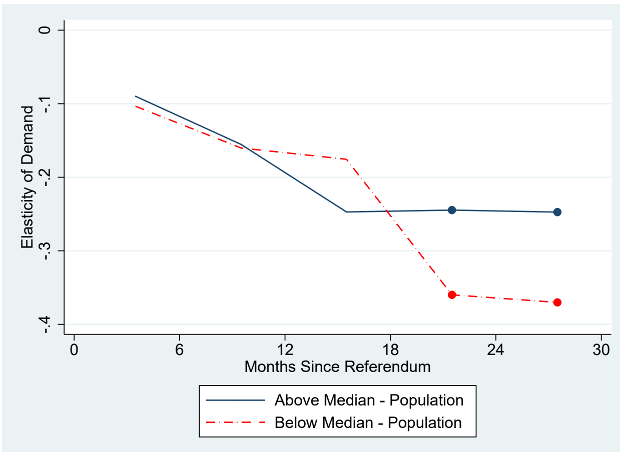
Figure 12: Effect on log usage for those that passed but did not implement MEA



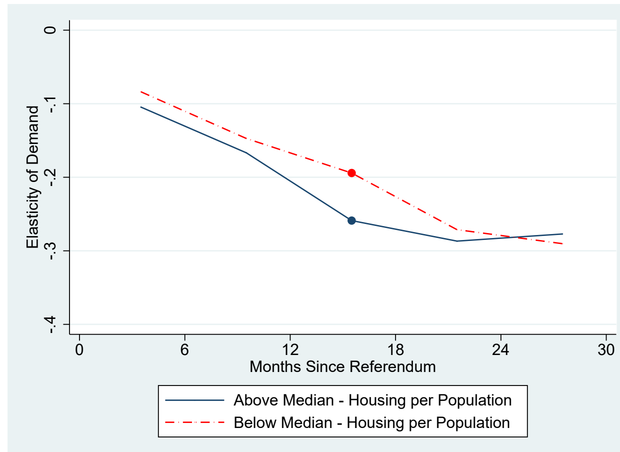
Notes: The figure displays estimates of the mean usage effect for the eleven communities that pass MEA but never implement it. The effect is estimated relative to that community's five nearest-neighbors, as defined by the difference-in-differences matching procedure outlined in the main text. The short dashed line indicates the median implementation date relative to when the referendum was passed. Confidence intervals are constructed via subsampling.

Figure 13: Elasticities by Housing Demographics

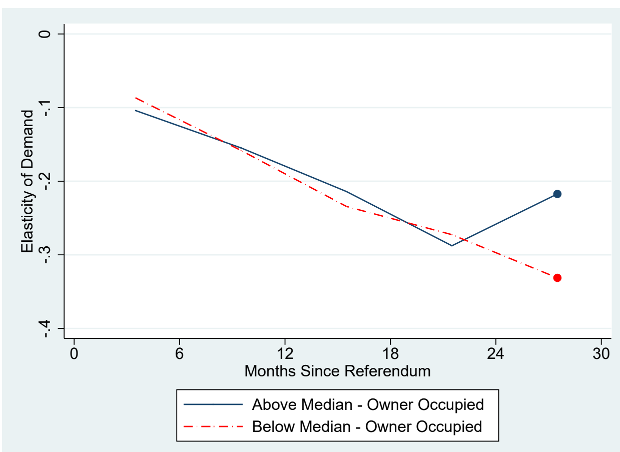
(a) Population



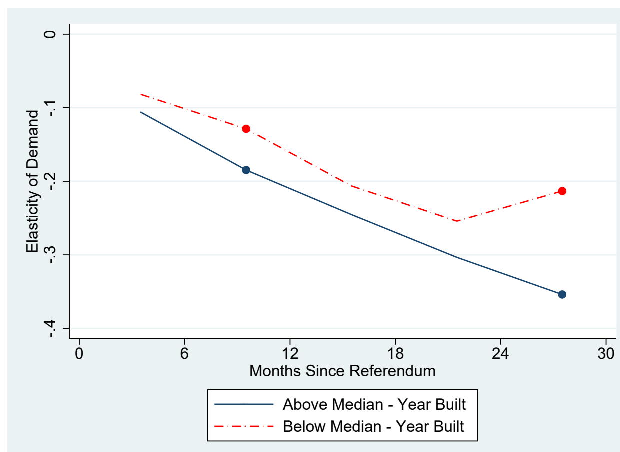
(b) Housing Units per Capita



(c) Percent Owner Occupied



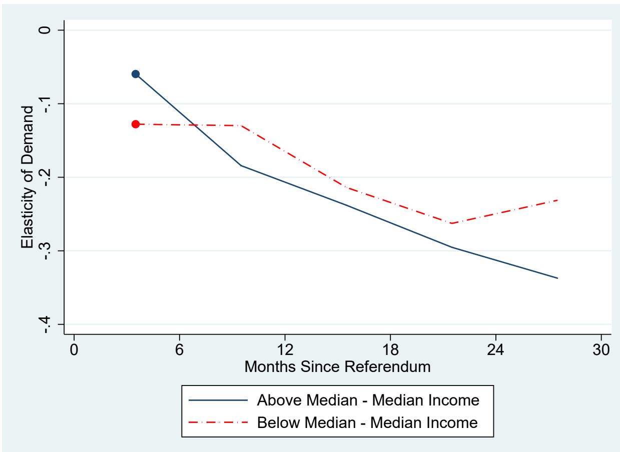
(d) Year Built



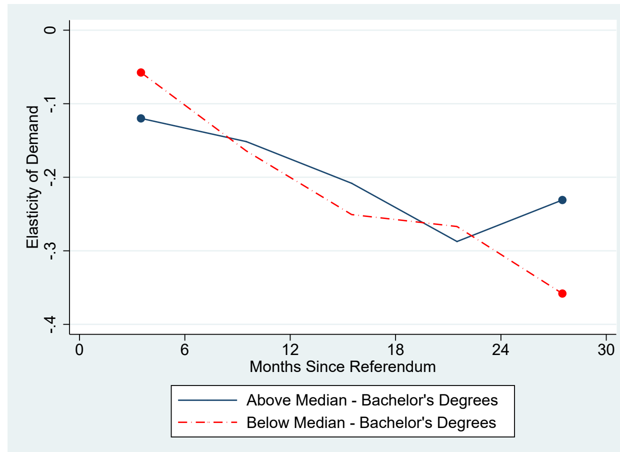
Notes: These panels display elasticity estimates for the upper half and lower half of demographic variables. The estimates are calculated by regressions of log usage on log price, where the price change is interacted with a dummy indicating whether or not the two is in the upper half of the distribution. The regressions control for eight interactions simultaneously - total population, housing units per capita, percent owner occupied, median year built, median income, percent with bachelor's degree, median age, and percent white. Significant coefficients ($\alpha = 0.10$) are indicated by the presence of a marker.

Figure 14: Elasticities by Socioeconomic Demographics

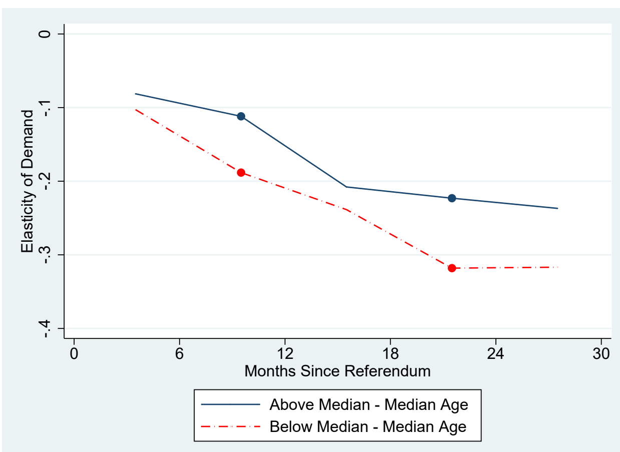
(a) Income



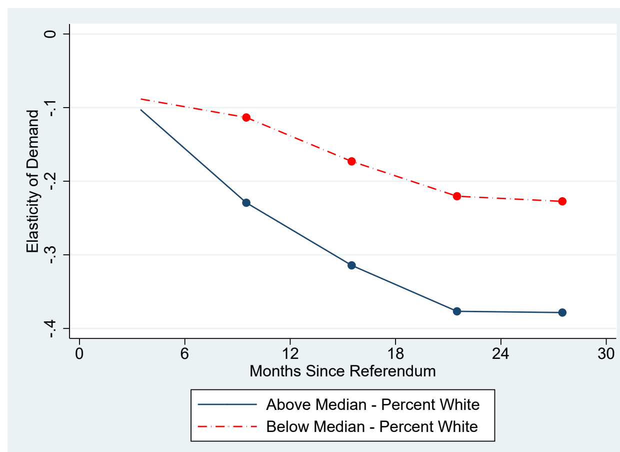
(b) Education



(c) Age

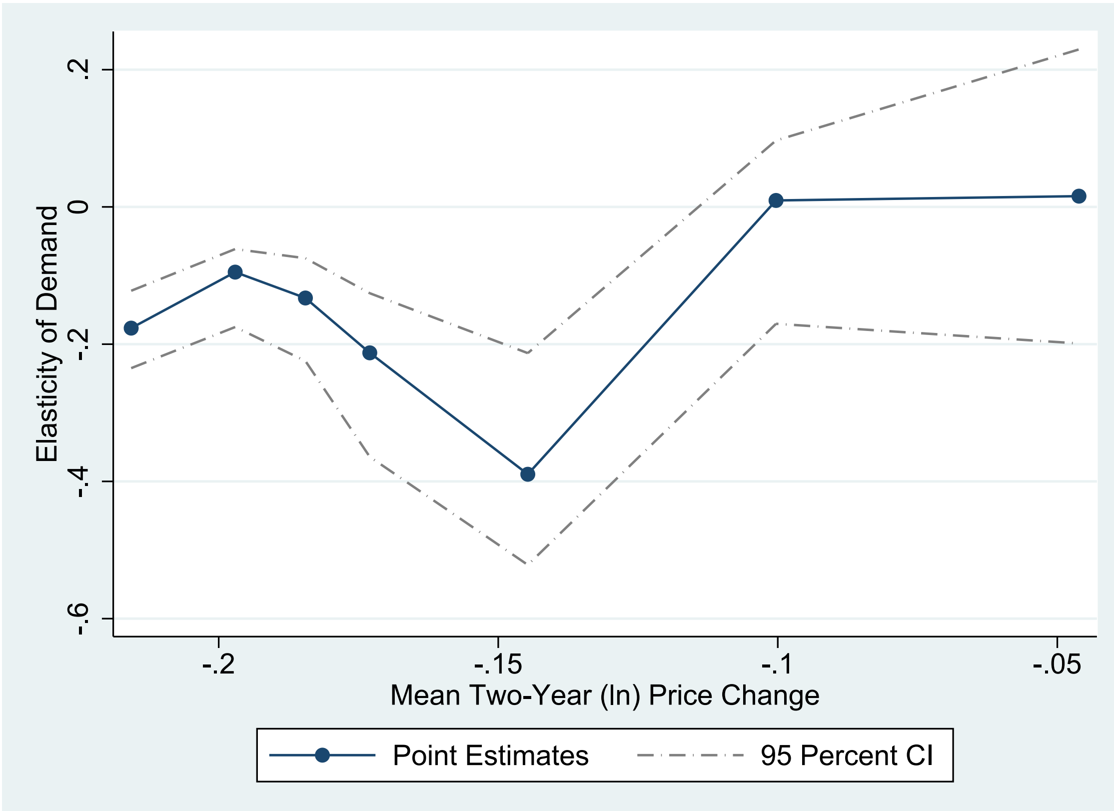


(d) Race



Notes: These panels display elasticity estimates for the upper half and lower half of demographic variables. The estimates are calculated by regressions of log usage on log price, where the price change is interacted with a dummy indicating whether or not the two is in the upper half of the distribution. The regressions control for eight interactions simultaneously - total population, housing units per capita, percent owner occupied, median year built, median income, percent with bachelor's degree, median age, and percent white. Significant coefficients ($\alpha = 0.10$) are indicated by the presence of a marker.

Figure 15: Estimated elasticities versus mean log price change



Notes: Communities are split into seven groups based on the average two-year price change. Elasticities are calculated separately for each group. The graph shows no relationship between the estimated group elasticity and the price change, mitigating some concerns about endogeneity. Confidence intervals are constructed via subsampling.

Tables

Table 1: Count of MEA Towns in Sample

Referendum Date	Implemented	Passed, Not Implemented	Voted, Not Passed
November 2010	1	0	0
April 2011	18	0	0
March 2012	164	0	28
November 2012	57	5	2
April 2013	38	3	6
March 2014	8	1	0
November 2014	3	2	0
Total	289	11	36

Table 2: Mean and median characteristics of MEA and non-MEA communities

	(1)	(2)	(3)	(4)
	Implemented MEA		Did not pass MEA	
	Mean	Median	Mean	Median
Per capita electricity usage in 2010, kWh	4,893	3,915***	5,078	4,614
Total population	23,856*	6,351***	4,926	1,043
Percent black	4.92	1.15***	5.41	0
Percent white	86.54*	90.95***	89.06	95.50
Median income	71,848	64,165	68,371	64,866
Median age	38.63***	38.10***	40.80	40.60
Total housing units	9,651*	2,449***	1,832	422
Median year built	1,969***	1,970	1,965	1,967
Median housing value	264,723***	230,500***	222,617	193,350
Percent with high school education	29.80***	31.04***	36.29	36.79
Percent with some college education	29.73***	29.49**	31.39	31.46
Percent with bachelor degree	18.32***	16.49***	14.31	12.51
Percent with graduate degree	11.22***	7.59***	7.43	5.88
Latitude	41.91***	41.93***	41.67	41.67
Longitude	-88.41**	-88.13***	-88.53	-88.39
Number of communities with non-missing ACS data	286	286	385	385

Stars denote significant differences from non-MEA mean or median, based on robust standard errors. Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Electricity usage data come from ComEd. All other characteristics are from the 2005-2009 American Community Survey. Number of MEA observations is smaller for median year built (285). Number of non-MEA observations is smaller for median housing value (383).

Table 3: Matching estimates of the effect of MEA on usage and prices

	Log Usage	Log Price	Elasticity	Usage Obs.	Price Obs.
1-6 months post-referendum	0.014***	-0.098***	-0.094***	1692	1692
	(0.003)	(0.003)	(0.019)		
7-12 months post-referendum	0.050***	-0.249***	-0.155***	1668	1668
	(0.007)	(0.007)	(0.020)		
13-18 months post-referendum	0.043***	-0.147***	-0.227***	1516	1514
	(0.005)	(0.002)	(0.027)		
19-24 months post-referendum	0.039***	-0.133***	-0.272***	1155	1154
	(0.006)	(0.003)	(0.043)		
25-30 months post-referendum	0.043***	-0.122***	-0.275***	606	600
	(0.007)	(0.004)	(0.039)		

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Estimates are constructed by a nearest-neighbor matching approach where each MEA town is matched to the five non-MEA towns with the most similar usage in 2008 and 2009. The number of price observations corresponds to the number of observations for each elasticity estimate, as we always observe usage where we observe a price change. Standard errors are in parentheses. Significance is determined by subsampling to construct confidence intervals.

Table 4: Matching estimates of the effect of MEA on usage and prices, yearly

	Log Usage	Log Price	Elasticity	Usage Obs.	Price Obs.
1-12 months post-referendum	0.032***	-0.173***	-0.140***	3360	3360
	(0.005)	(0.004)	(0.018)		
13-24 months post-referendum	0.041***	-0.141***	-0.242***	2671	2668
	(0.005)	(0.002)	(0.028)		
25-36 months post-referendum	0.046***	-0.109***	-0.285***	720	714
	(0.008)	(0.006)	(0.042)		

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Estimates are constructed by a nearest-neighbor matching approach where each MEA town is matched to the five non-MEA towns with the most similar usage in 2008 and 2009. The number of price observations corresponds to the number of observations for each elasticity estimate, as we always observe usage where we observe a price change. Standard errors are in parentheses. Significance is determined by subsampling to construct confidence intervals.

Appendix

Referendum wording

Excerpt from Sec. 1-92. A[20]
The election authority must submit the question in substantially the following form:
Shall the (municipality, township, or county in which the question is being voted upon) have the authority to arrange for the supply of electricity for its residential and small commercial retail customers who have not opted out of such program?
The election authority must record the votes as “Yes” or “No”.

[20]From 20 ILCS 3855/1-92, Text of Section from P.A. 98-404. Available from http://www.ilga.gov/legislation/ilcs/fulltext.asp?DocName=002038550K1-92.

Kane County
C/O Dynegy Energy Services
1500 Eastport Plaza Dr.
Collinsville, IL 62234

John A. Smith
123 Main St
Anytown, IL 65432

Kane County is pleased to announce that Dynegy Energy Services, LLC ("DES") has been selected as the Supplier for its Municipal Aggregation program. This includes a 24-month program with a fixed price of $**0.06533 per kilowatt hour (kWh)** for the first 12 months (August 2015 to August 2016) and steps down to **$0.06065 per kWh** for the last 12 months (August 2016 to August 2017). DES is an independent seller of power and energy service and is certified as an Alternative Retail Electricity Supplier by the Illinois Commerce Commission (ICC Docket No. 14-0336).

As an eligible residential or small business customer located in unincorporated portions of Kane County, you will be automatically enrolled unless you opt out.

HOW TO OPT-OUT
You need do nothing to receive this new fixed rate. However, if you choose not to participate, simply return the enclosed Opt-Out Card **or call DES at 844-351-7691** by **July 10, 2015.** For more information, visit www.DynegyEnergyServices.com or contact DES Customer Care at 866-694-1262 from 8:00am to 7:00pm Mon- Fri or via email at DESCustCare@Dynegy.com.

There is no enrollment fee, no switching fee, and no early termination fee. This is a firm, fixed all-inclusive rate guaranteed until **August 2017**. This program offers automatic enrollment in Traditionally-sourced Power, but you have an option of purchasing Renewable Power at a rate of $**0.06766 per kWh** for the first 12 months (August 2015 to August 2016) which steps down to $**0.06327 per kWh** for the last 12 months (August 2016 to August 2017).

ENROLLMENT PROCESS
Once your account is enrolled, you will receive a confirmation letter from ComEd confirming your switch to DES. A sample ComEd notice is attached. Approximately 30 to 45 days after enrollment you will receive your first bill with your new DES price. Please review the enclosed Terms and Conditions for additional information.

Please be advised you also have the option to purchase electricity supply from a Retail Electric Supplier (RES) or from ComEd pursuant to Section 16-103 of the Public Utilities Act. Information about your options can be found at the Illinois Commerce Commission website: www.pluginillinois.org and www.ComEd.com. You may request a list of all supply options available to you from the Illinois Power Agency.

See Reverse for Frequently Asked Questions…

Sincerely,

Christopher J. Lauzen
Board Chairman
Kane County

Kurt R. Kojzarek
Development Committee Chairman
Kane County

Electric Aggregation Program
Frequently Asked Questions

Overview of Municipal Aggregation

What is Municipal Aggregation?
Illinois law allows municipalities and counties to negotiate the purchase price of electricity on behalf of residential and small business utility customers living within their borders. While these governmental entities choosing community aggregation would be responsible for negotiating the price of power from a supplier other than the traditional utility, your utility would still be responsible for delivering that power to your home, and billing you for it.

How can I get more information about the municipality or county's aggregation program?
Contact your municipality or county for information related to the referendum and the aggregation program. Additional resources can be found at:
http://www.dynegyenergyservices.com/residential/municipal-aggregation/communities-we-serve.php

Eligibility and Enrollment

Who is eligible to participate?
Residential or small business customers located in the participating governmental entity boundaries may participate. Customers enrolled in real time pricing, Power Smart Pricing, space electric heat rate, or served by an alternative retail supplier may not be eligible.

How do I enroll?
It's simple. It's automatic. Unless you "opt-out" of the program, all eligible ComEd customer accounts within the boundaries will be enrolled in the program. You will receive a "switch" letter from your utility, ComEd, confirming your enrollment.

Do I have to participate in the municipal or county aggregation plan?
All eligible ComEd utility customers within the municipal or county boundaries will receive an opt-out notification letter via U.S. mail. You may "opt-out" by returning the Opt-Out card by the deadline date identified in your notification. If you choose to opt-out, your account remains with ComEd at the current utility rate.

What if I decide to opt-out after the opt-out deadlines have passed?
You may opt out at any time by calling our toll free number or sending us an email.

Rate and Term Information

What are the Rates and Terms for my Municipality or County?
A listing of communities served by DES can be found at www.DynegyEnergyServices.com. Select your municipality or county to find the applicable rates, contract length, and the terms and conditions for your particular governmental entity. You can expect to receive your first bill with the new DES rate in September 2015.

What if ComEd rates decrease?
If at any time during the term of this Agreement ComEd rates fall lower than the DES price, you will have the option to return to the utility without penalty.

Why does the price change in the second and third year?
DES is committed to offering the lowest possible price to participants in municipal aggregation programs. Cost factors in the power market will change during the second & third year. Specifically, the price DES is charged for capacity changes in year two and three and is reflected in the price.

What happens at the end of the Agreement term?
At the end of the Agreement term, as defined in the Terms and Conditions you have the option of staying with a new Municipal Aggregation program, returning to the utility, or signing with a new supplier independent of the Municipal Aggregation program.

Billing and Service Information

Who will bill me for electricity? Will I get two bills?
You will continue to receive one monthly bill from ComEd. The bill will include the charges for electricity supplied by us, as well as the delivery service charges from ComEd.

Can I still have my payment automatically deducted from my checking account?
Yes, how you pay your bill will not change.

Can I stay on budget billing?
Yes, your budget billing will not be affected by your participation in this program.

Who is responsible for the delivery of power to my home or business?
ComEd will continue to deliver your electricity and will be responsible for maintaining the system that delivers power into your home. As your energy delivery company, they will continue to respond around-the-clock to outages, service calls and emergencies regardless of your electric supplier.

Who do I call to report a power outage or problems with my electric service?
You will continue to call ComEd for power outages, problems with your service or questions regarding your monthly bill.

ComEd Residential Customers: 800.334.7661
ComEd Business Customers: 800.334.7661

Who do I call if I have questions regarding the Municipal or County Opt-Out Electricity Aggregation Program?
Questions should be referred to a member of our DES Customer Care team.
DES Customer Care: 844.351.7691
DESCustCare@Dynegy.com

A complete list of Frequently Asked Questions can be found at
http://www.dynegyenergyservices.com/residential/municipal-aggregation/faq-residential-municipal.php
or by calling DES at 844.351.7691

Example of an opt-out card

PLACE

STAMP

MC SQUARED ENERGY SERVICES, LLC
344 South Poplar Street
Hazleton, PA 18201

___ **Opt-Out by returning this form:** I wish to opt-out of the Village of South Barrington electricity aggregation program and remain with my current provider. By returning this signed form, I will be **excluded** from this opportunity to join with other residents in the electricity aggregation program.

You must mail this form by **June 15, 2012**

Name: ______________________________

Service Address: ______________________________

City, State, Zip: ______________________________

Phone: ______________________________

Account Holder's Signature: ____________________ Date: __________

Rev 1 – 5/17/12

ComEd
An Exelon Company

SERVICE FROM 1/11/16 THROUGH 2/11/16 (31 DAYS)
Residential - Single

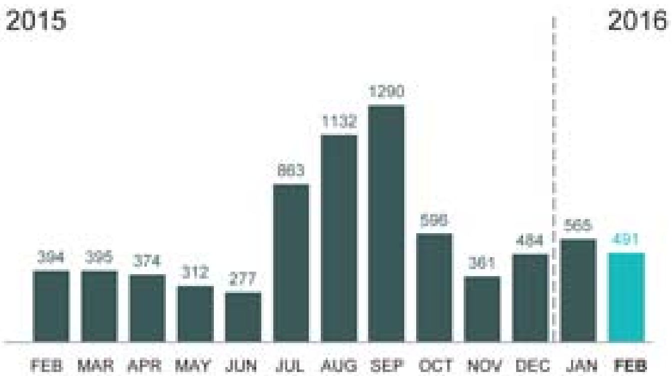
Customer Name
Service Address
City, ST ZIP
000.000.0000

Issued **2/11/16** Account # **0000000000**

Total Amount Due by 3/4/16 **$69.42**

Thank you for your payments totaling **$77.44.**

TOTAL USAGE (kWh)



Current month's reading is **actual.**

AVERAGE DAILY USE (monthly usage/days in period)

Current Month 25° avg. temp

15.8 kWh ↑ **24 %** from last year

Last Month 36° avg. temp	Last Year 25° avg. temp
17.1 kWh	**12.7** kWh

Ten 100W light bulbs for 1 hour = 1 kWh

CURRENT CHARGES SUMMARY

See reverse side for details

SUPPLY
$31.98

ComEd provides your energy.

ComEd.com
1.800.334.7661

DELIVERY
$30.96

ComEd delivers electricity to your home.

ComEd.com
1.800.334.7661

TAXES & FEES $6.48

For Electric Supply Choices visit pluginillinois.org

Return only this portion with your check made payable to ComEd. Please write your account number on your check.

ComEd
An Exelon Company

0100001 00 IV 0.000 6577 -C65-B3-P00000-I1 4 6 89A C

Pay your bill online, by phone or by mail.

See reverse side for more info

Account # **0000000000**

CUSTOMER NAME
ADDRESS 1
ADDRESS 2
CITY, ST ZIP

Total Amount Due by 3/4/16 **$69.42**

Payment Amount:

COMED
PO BOX 6111
CAROL STREAM, IL 60197-6111

000000000000000069426064006942 4

For Questions, Support, and Outages visit ComEd.com

English	1.800.EDISON1 (1.800.334.7661)
Español	1.800.95.LUCES (1.800.955.8237)
Hearing/Speech Impaired	1.800.572.5789 (TTY)
Federal Video Relay Services (VRS)	Fedvrs.us/session/new

Issued **2/11/16** Account # **0000000000**

Total Amount Due by 3/4/16 $69.42

METER INFORMATION

Read Dates	Meter Number	Load Type	Reading Type	Previous	Present	Difference	Multiplier	Usage
1/11-2/11	000000000	General Service	Total kWh	94278 **Actual**	94769 **Actual**	491	x 1	491

CHARGE DETAILS

Residential - Single 1/11/16 - 2/11/16 (31 Days)

SUPPLY — $31.98

Electricity Supply Charge	491 kWh X 0.05865	$28.80
Transmission Services Charge	491 kWh X 0.01122	$5.51
Purchased Electricity Adjustment		-$2.33

DELIVERY - ComEd — $30.96

Customer Charge		$10.53
Standard Metering Charge		$4.36
Distribution Facilities Charge	491 kWh X 0.03156	$15.50
IL Electricity Distribution Charge	491 kWh X 0.00116	$0.57

TAXES & FEES — $6.48

Environmental Cost Recovery Adj	491 kWh X 0.00038	$0.19
Energy Efficiency Programs	491 kWh X 0.00345	$1.69
Franchise Cost	$30.39 X 2.36300%	$0.72
State Tax		$1.62
Municipal Tax		$2.26

Service Period Total **$69.42**

Thank you for your payment of $77.44 on January 29, 2016

Total Amount Due $69.42

UPDATES

ComEd

- WAYS TO PAY: ComEd offers a variety of ways to Pay. Learn more at ComEd.com/pay
- WE ARE HERE FOR YOU: As of Feb. 1st, we'll be open 1 hour later. Call us from 7 am to 7pm Monday through Friday. 1-800-EDISON-1 ComEd.com/ContactUs
- APPLIANCE REBATES: Get Appliance Rebates from the ComEd Energy Efficiency Program to upgrade your appliances. ComEd.com/Rebates
- SCAM ALERT: ComEd will never call you to request cash or ask you to buy a prepaid credit card to pay a bill. ComEd.com/ScamAlert
- PART 280: View a copy of the ICC Commission 83 Ill. Adm. Code 280 rules at ComEd.com/Part280
- YOUR COMED BILL: Need help understanding your bill line item definitions? Please visit us at ComEd.com/UnderstandBill or call us at 1-800-334-7661
- ENVIRONMENTAL DISCLOSURE STATEMENT: ComEd's Environmental Disclosure Statement can now be found online at ComEd.com/EnvironmentalDisclosure
- Past due balances are subject to late charges.

6577-65-0100001-0001-0000001

OTHER WAYS TO PAY YOUR BILL

Visit ComEd.com/PAY for more information including applicable fees for some transactions.

Online

Set up an automatic payment, enroll in paperless billing, or make a convenience payment at ComEd.com/Pay.

Mobile App

Download the ComEd mobile app on your Apple® or Android™ device to view and pay your bill, or manage your account.

Phone

Call us to make a convenience payment with a credit card, ATM card, or your bank account: 1.800.588.9477. (Fee Applies)

In-Person

Pay your bill in-person at many ComEd authorized agents located throughout the region. Visit ComEd.com/Pay for details.

When you provide a check as payment, you authorize us to use information from your check either to make a one-time electronic fund transfer from your account or to process the payment as a check transaction.

Data Processing

In the usage data provided by ComEd, several communities change definitions over time, moving customers from one community to another or creating a new community. This appears as large, discrete changes in our community-level aggregate usage data. To eliminate this noise, we apply two filters to search for large structural breaks. For each community, we run 89 separate regressions of log usage on month dummies and a structural break indicator, where we start the structural break indicator at each month in the sample. We then compare the maximum R-squared to the minimum R-squared among a community's set of regressions. If this difference exceeds 0.5, then it is dropped from the sample.

For the second filter, we run a series of similar regressions with the addition of a linear time trend. For this filter, we drop any communities for which the explanatory power of the break increases the R-squared by more than 0.2.

One concern with this filter is that we may eliminate actual structural breaks arising from our policy of interest. The communities that are removed in this fashion are primarily small communities that did not implement MEA. Further, the coefficient on the structural break indicator implies an unrealistic response to the price change.

Event study difference-in-difference specification

In this section, we outline how we estimate the effect of implementing MEA on electricity prices and usage using a standard difference-in-differences model:

$$Y_{cmy} = \sum_{\tau=-24,\tau\neq-1}^{24} \beta_\tau MEA_{c\tau} + \beta_{25} MEA_{c,25} + \beta_{-25} MEA_{c,-25} + \alpha_{cm} + \alpha_{my} + \varepsilon_{cmy}, \qquad (4)$$

where Y_{cmy} is either the natural logarithm of the monthly price or the natural logarithm of total monthly electricity use in community c in calendar month m and year y. The main parameter of interest is β_τ. The variable $MEA_{c\tau}$ is an indicator equal to 1 if, as of month m and year y, community c implemented MEA τ months ago. The month before MEA implementation ($\tau = -1$) is the omitted category. To ensure that our estimated coefficients are relative to this category, we include indicators for MEA having been implemented 25 or more months ago ($MEA_{c,25}$) and for MEA being implemented 25 or more months in the future ($MEA_{c,-25}$). We include a full set of month-by-year (α_{my}) and community-by-month (α_{cm}) fixed effects and cluster standard errors at the community level. We discuss the robustness of our estimates to different sets of fixed effects in Section 5.

We also estimate a second, more parametric specification that assesses the effect by six-month periods and uses the entire two years prior to MEA as the reference period:

$$\begin{aligned}Y_{cmy} = \gamma_1 MEA_{c,0\text{ to }6} + \gamma_2 MEA_{c,7\text{ to }12} + \gamma_3 MEA_{c,13\text{ to }18} + \gamma_4 MEA_{c,19\text{ to }24}\\ +\beta_{25} MEA_{c,25} + \beta_{-25} MEA_{c,-25} + \alpha_{cm} + \alpha_{my} + \varepsilon_{cmy}.\end{aligned} \tag{5}$$

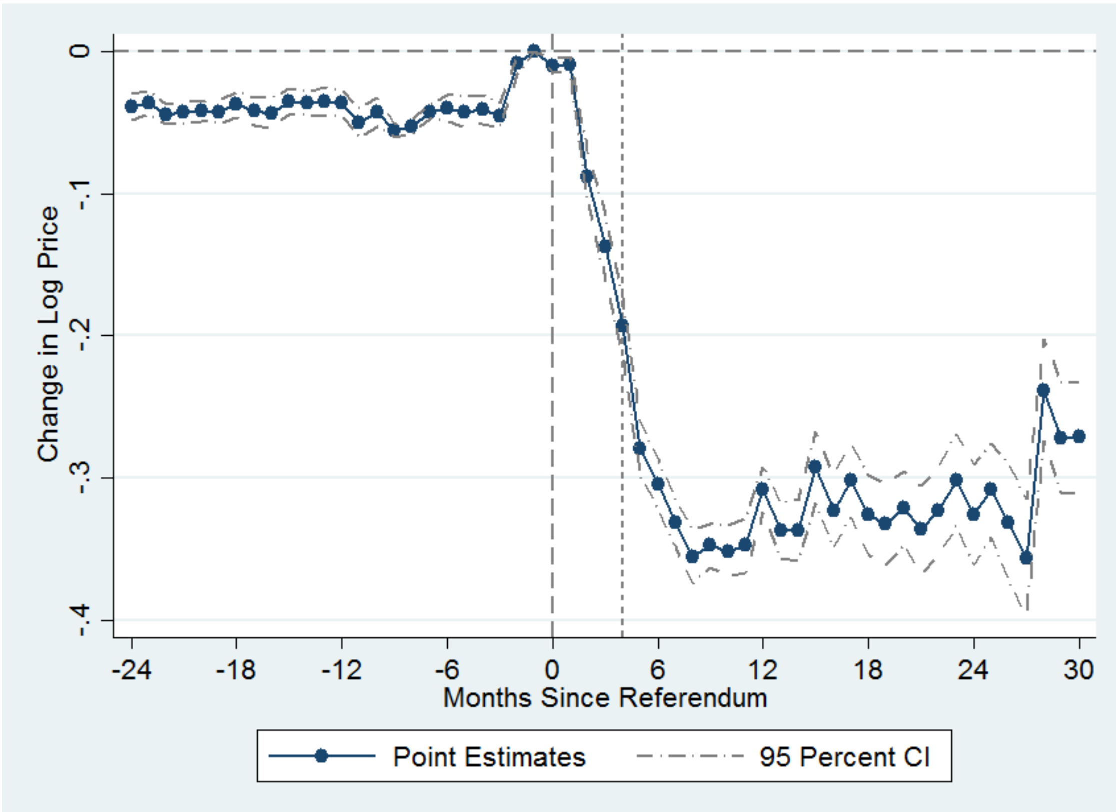
In this specification, $MEA_{c,0\text{ to }6}$ is an indicator variable equal to 1 if the community implemented MEA in the past 6 months and 0 otherwise. Similarly, $MEA_{c,7\text{ to }12}$ is an indicator equal to 1 if the community implemented MEA between 7 and 12 months ago, and so on. The other variables are defined as in equation (4).

One could use this framework to estimate the effect of implementing MEA by comparing communities that implemented MEA to those that did not implement MEA. However, this raises the concern that towns that did not adopt MEA may not serve as adequate counterfactual for towns that did adopt MEA. That is, the decision to adopt MEA may be correlated with future energy usage. We therefore restrict our estimation sample to towns that implemented MEA. Our main identifying assumption for these estimates is that, conditional on a host of fixed effects, the *timing* of MEA adoption is exogenous with respect to electricity use.

Event study difference-in-difference results

Figure A.1 presents the change in electricity prices following MEA, in logs, as estimated by equation (4). Similar to our matching results, prices do not drop immediately following the referendum because it takes time for communities to switch to a new supplier. Unlike the matching estimator, the pre-period change is not exactly equal to zero in the event-study difference-in-difference. Although treatment and control communities face identical prices in the pre-period in *calendar time*, they do not face identical prices in *event-study time* because ComEd's prices fluctuate month-to-month. This distinction does not matter for the matching estimator, which creates counterfactuals separately for each treatment community. The second vertical dashed line in Figure A.1 shows the point at which half of all communities have implemented MEA (4 months after passing the referendum). Prices continue to drop as more communities switch and then eventually stabilize. Within 8 months of passing the referendum, the average electricity price has decreased by more than 0.3 log points (26 percent) in MEA communities relative to the control group. There is an increase in the relative MEA price 28 months after passing MEA, which is due to the fact that electricity prices fell sharply for ComEd customers in June of 2013 (see Figure 2), the middle of our sample period. Despite this increase, prices in MEA communities remain significantly lower than those in the control group for the entire sample period.

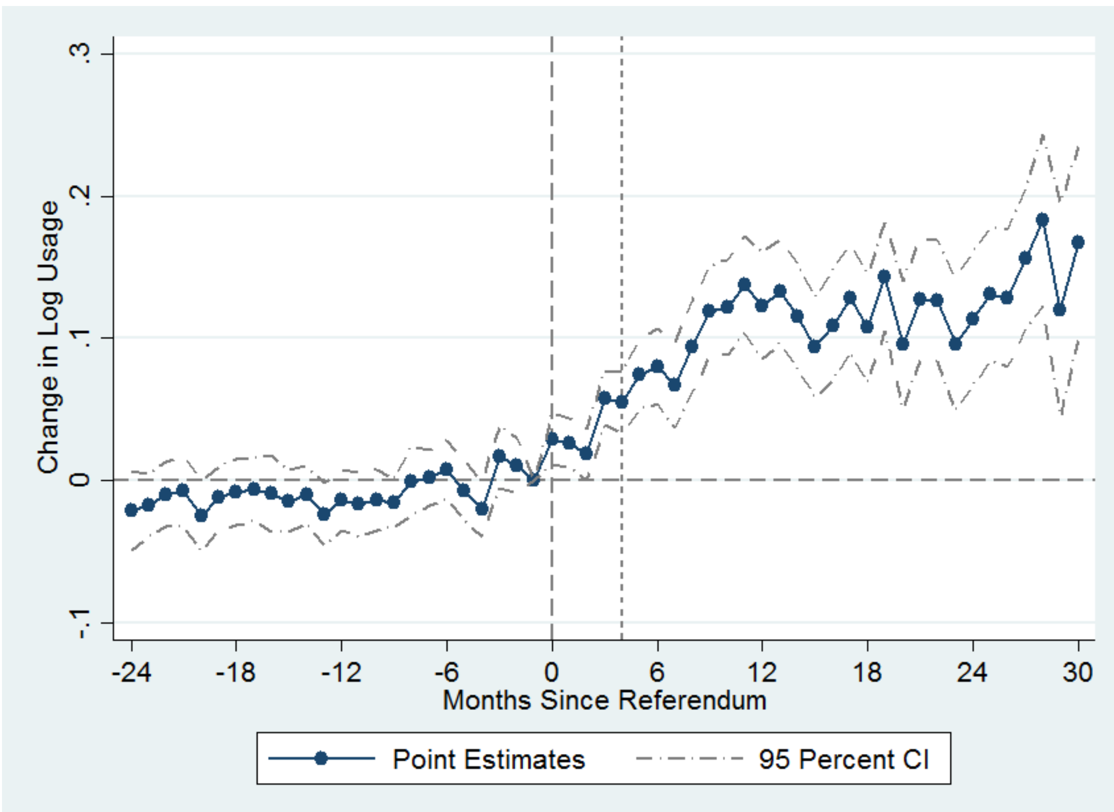
Figure A.1: Regression estimates of the effect of MEA on electricity prices, communities that passed MEA



Notes: Outcome is the natural log of the electricity price. The first vertical dashed line indicates the date of the MEA referendum. The second dashed line indicates the date of MEA implementation. Regressions include month-by-year and community-by-month fixed effects. Standard errors are clustered by community. Sample includes only communities that passed MEA at some point during our sample.

Figure A.2 shows the corresponding estimates for electricity usage. Prior the referendum, the difference in usage between MEA and the control communities is statistically indistinguishable from zero. Usage in MEA communities then begins to increase following the referendum. By the end of the first year, usage in MEA communities is about 0.1 log points (9.5 percent) higher relative to the counterfactual.

Figure A.2: Regression estimates of the effect of MEA on electricity usage, communities that passed MEA



Notes: Outcome is the natural log of total electricity use. The first vertical dashed line indicates the date of the MEA referendum. The second dashed line indicates the date of MEA implementation. Regressions include month-by-year and community-by-month fixed effects. Standard errors are clustered by community. Sample includes only communities that passed MEA at some point during our sample.

Table A.1 shows the estimated impact of MEA on the log of the electricity price in these communities 0-6, 7-12, 13-18, and 19-24 months after implementation, as estimated by equation (5). Overall, the results consistently show large and significant price drops. Our preferred specification is presented in Column 4 and includes community-by-month and month-by-year fixed effects. This specification estimates that electricity prices fell by 0.1 log points in the first six months, and eventually stabilizes at around 0.3 log points by the end of the first year. These estimates are robust to including different fixed effects.

Table A.1: Effect of MEA adoption on electricity prices, communities that passed MEA

	(1)	(2)	(3)	(4)
0-6 months post-MEA	-0.119***	-0.100***	-0.123***	-0.101***
	(0.005)	(0.005)	(0.005)	(0.005)
7-12 months post-MEA	-0.307***	-0.313***	-0.312***	-0.320***
	(0.007)	(0.007)	(0.007)	(0.007)
13-18 months post-MEA	-0.298***	-0.266***	-0.303***	-0.267***
	(0.008)	(0.009)	(0.008)	(0.010)
19-24 months post-MEA	-0.283***	-0.285***	-0.285***	-0.287***
	(0.010)	(0.013)	(0.010)	(0.013)
25-30 months post-MEA	-0.283***	-0.264***	-0.298***	-0.279***
	(0.013)	(0.017)	(0.014)	(0.018)
Community fixed effects	X	X		
Month and year fixed effects	X		X	
Month-by-year fixed effects		X		X
Community-by-month fixed effects			X	X
Dep. var. mean	2.202	2.202	2.202	2.202
Observations	25,710	25,710	25,710	25,710
Adjusted R-squared	0.794	0.899	0.804	0.908

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) clustered by community. Outcome variable is the log of the per-kWh electricity price.

Table A.2 shows the estimated change in usage as estimated by equation (5) for the sample of communities that implemented MEA. Our preferred specification, presented in Column 4, estimates that electricity usage is 4.8 log points higher in the first 6 months following the referendum, and this increases to 11.4 log points within one year.

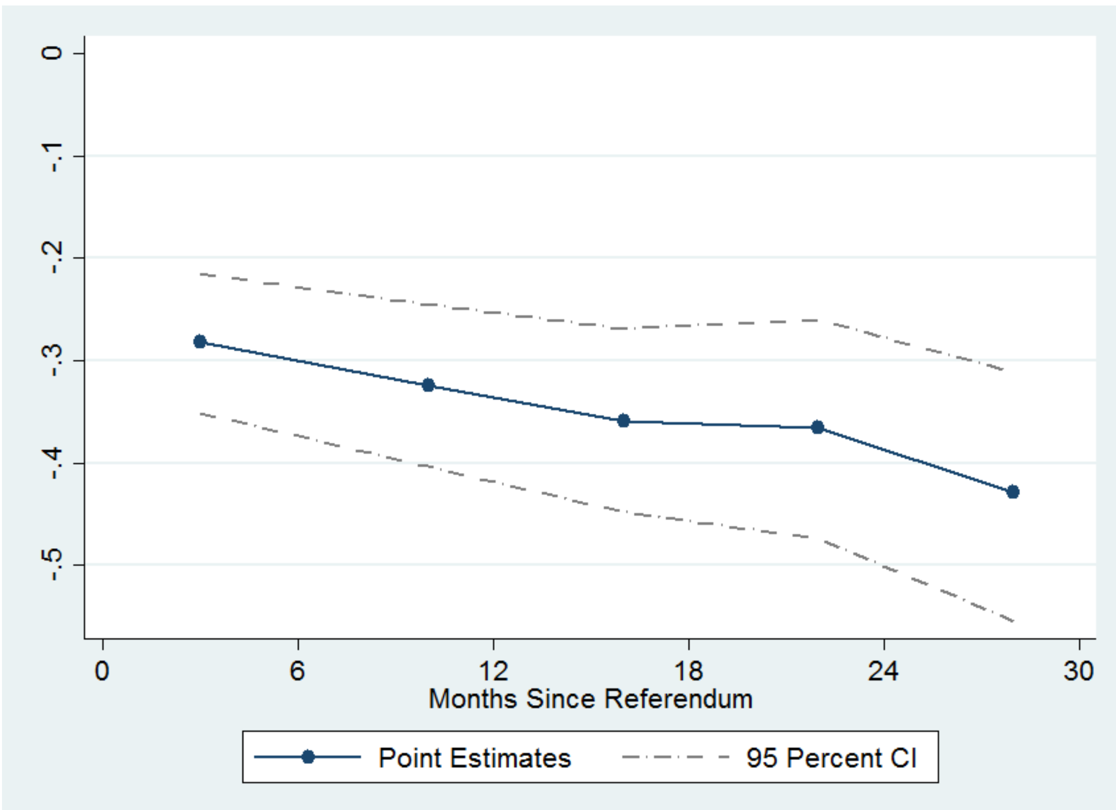
Table A.2: Effect of MEA adoption on electricity usage, communities that passed MEA

	(1)	(2)	(3)	(4)
0-6 months post-MEA	0.073***	0.059***	0.066***	0.048***
	(0.008)	(0.009)	(0.005)	(0.006)
7-12 months post-MEA	0.054***	0.095***	0.065***	0.114***
	(0.012)	(0.016)	(0.012)	(0.016)
13-18 months post-MEA	0.107***	0.140***	0.088***	0.114***
	(0.015)	(0.019)	(0.014)	(0.017)
19-24 months post-MEA	0.084***	0.073***	0.109***	0.113***
	(0.016)	(0.023)	(0.015)	(0.021)
25-30 months post-MEA	0.067***	0.139***	0.068***	0.134***
	(0.020)	(0.025)	(0.020)	(0.024)
Community fixed effects	X	X		
Month and year fixed effects	X		X	
Month-by-year fixed effects		X		X
Community-by-month fixed effects			X	X
Dep. var. mean	14.371	14.371	14.371	14.371
Observations	25,710	25,710	25,710	25,710
Adjusted R-squared	0.991	0.993	0.996	0.998

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) clustered by community. Outcome variable is the log of total electricity usage.

Finally, Figure A.3 shows the elasticities implied by the two preceding tables. Specifically, we show the ratio of coefficients from Tables A.2 and A.1, which estimate the MEA-induced change in electricity quantities and prices, respectively. Because the outcomes are in logs, their ratio will be approximately equal to the elasticity. The implied elasticity ranges from -0.33 7-12 months after passage of MEA to -0.45 in the first six months after passage.

Figure A.3: Estimated price elasticities, communities that passed MEA



Notes: Sample includes only communities that passed MEA at some point. Elasticities are calculated for each six-month period by regressing town-month changes in log usage on the observed change in log price. Confidence intervals are constructed by bootstrap.