Do Consumers Distinguish Fixed Costs from Variable Costs? Evidence from Heating Price Reform in China

Koichiro Ito
University of Chicago and NBER

Shuang Zhang
University of Colorado Boulder*

This version:

Abstract

A central assumption in economics is that consumers distinguish fixed costs from variable costs. This paper empirically tests this assumption by using a quasi-experiment in heating price reform in China. The introduction of a two-part tariff created policy-induced variation in variable costs and fixed charges, and staggered policy implementation allows us to develop an event-study research design. Using administrative records on daily heating usage at the household level, we find strong evidence that consumers distinguish fixed costs from variable costs in a way that is consistent with standard economic theory. Consequently, the policy provided intended social welfare gains from allocative efficiency and environmental externalities. Our findings provide important implications for energy policy because a growing number of developing countries including China are in the process of implementing consumption-based energy billing in lieu of pre-existing inefficient fixed-charge billing.

*Ito: Harris School of Public Policy, University of Chicago, and NBER (e-mail: ito@uchicago.edu). Zhang: Department of Economics, University of Colorado Boulder (e-mail: shuang.zhang@colorado.edu). For helpful comments, we thank Douglas Almond, Severin Borenstein, Lucas Davis, Michael Greenstone, Kelsey Jack, Ryan Kellogg, Catherine Wolfram, and seminar participants at Colorado Boulder and UC Berkeley Energy Camp. We thank Theodor Kulczycki and Chenyu Qiu for excellent research assistance.

1 Introduction

A central assumption in economics is that consumers distinguish fixed costs from variable costs. In public finance, a lump-sum tax or redistribution is considered to be non-distortionary because it does not distort the relative prices of goods given the assumption that taxpayers understand the difference between fixed costs and variable costs (Stiglitz, 1988). In Industrial organization, a two-part tariff—a service fee that consists of a fixed charge and a variable charge—allows profit-maximizing firms to price-discriminate and natural monopolists to achieve allocative efficiency, under the assumption that consumers distinguish fixed costs from variable costs (Tirole, 1988).

However, many recent studies find that standard economic theory often fails to explain how consumers respond to prices in reality. For example, the salience of price information changes how consumers react to prices (Busse et al., 2006; Gabaix and Laibson, 2006; Chetty et al., 2009; Finkelstein, 2009; Brown et al., 2010; Jessoe and Rapson, 2014). Category budgeting is often consistent with data on consumer behavior in the field (Hastings and Shapiro, 2013). Consumers respond to average price rather than marginal price when they are faced with a multi-tiered marginal tax or price schedule (de Bartolome, 1995; Borenstein, 2009; Ito, 2014; Kahn and Wolak, 2013). These findings suggest that whether consumers *actually* distinguish fixed costs from variable costs is ambiguous and ultimately an empirical question. Although the answer to this question is important for many fields of economics, the literature is largely silent on the empirical evidence.

In this paper, we empirically test two competing hypotheses on the effects of fixed costs and variable costs on consumer behavior. Standard economic theory predicts that substitution effects are induced by marginal price, and fixed costs affect consumption only through a potential income effect. An alternative hypothesis—originates to the theory of "schmeduling" by Liebman and Zeckhauser (2004)—suggests that when a price schedule is nonlinear, consumers may think of average price as "true marginal price." In this case, a change in a fixed cost does induce substitution effects because a different fixed cost results in a different average price.

We develop our empirical strategy based on a quasi-experiment created by heating price reform in China. In 2005, the Ministry of Housing and Urban-Rural Development (MOHURD)—a ministry of the central Chinese government—introduced a new residential heating price schedule, called "Consumption-Based Billing (CBB)." Before the reform, households paid only a fixed charge, which

was independent of their consumption—that is, the marginal price was zero. The reform introduced a two-part tariff, which consisted of non-zero marginal price and a new fixed charge.

This quasi-experiment provides a nearly ideal research environment to test our question. First, the reform created an increase in marginal price for all consumers but a *decrease* in average price for many consumers. That is, many consumers found that their total payment and average price were reduced by the reform, although their marginal price was increased. This price variation is rarely available in the literature and allows us to conduct a simple statistical test using the fact that standard theory predicts these consumers to reduce usage, while the alternative theory expects them to increase usage. Second, the introduction of the reform was staggered over several years, which allows us to have control groups in an event study research design. Third, the research question is directly relevant to the policymaker's objective. The policy would improve allocative efficiency, reduce environmental externalities, and promote energy conservation as long as consumers behave in a way that standard theory predicts. However, if the alternative theory is more consistent with empirical evidence, the policy could create a perverse effect on these policy goals.

In collaboration with a utility company in the city of Tianjin, we obtained newly available administrative records on daily heating usage at the household level. The records include 16,133 households from December 2007 to January 2017. An empirical challenge in the literature is that individually-metered data are usually available only after the introduction of usage-based billing in developing countries (McRae, 2015). Our research design addresses this challenge because individually-metered data are available both *before and after* the introduction of the consumption-based billing in Tianjin.

A set of our empirical findings provide strong evidence that consumers distinguish fixed costs from variable costs in a way that is consistent with the prediction from standard economic theory. We first show that the reform induced a reduction in heating usage for all parts of the consumption distribution, including consumers who experienced an increase in marginal price but a decrease in average price and total payment. Second, we use the encompassing test (Davidson and MacKinnon, 1993) to examine whether consumers respond to marginal price or average price when they are faced with the two-part tariff. We find that marginal price has a significant effect on consumption, while the effects of average price become near zero and statistically insignificant from zero once we control for the effect of marginal price.

Our results suggest that the reform provided intended social welfare gains, which come from

improved allocative efficiency and reduced environmental externalities. We use our empirical findings to calculate the welfare impacts of the policy. The social welfare was improved by 2.89 million dollars for the city of Tianjin per year and 25.55 dollars per household per year, which was about 6.5 percent of their heating expenditure. In terms of energy conservation, the reform provided about a 10 percent reduction in heating usage based on our ITT estimate and a 16 percent reduction in usage based on our ATET estimate. We also calculate the arc price elasticity of heating demand with respect to marginal price based on our empirical evidence. The medium-long run price elasticity estimates are between -0.059 and -0.118, which are close to the price elasticity estimates in the recent literature on residential energy demand in the U.S. and other countries (Wolak, 2011; Ito, 2014; Ito, Ida, and Tanaka, 2018).

Another important welfare question is the redistributional impacts of the policy. To investigate this question, we collected each household's housing price data as a proxy for their wealth. We then evaluated the policy impact by housing price. We find that this reform itself was regressive—the consumer surplus was increased for wealthier households but decreased for less wealthy households. This regressivity comes from the fact that the pre-reform policy was very progressive because the fixed charge was proportional to the size of a house, resulted in high payments for wealthier households regardless of their heating usage.

This paper provides two main contributions to the economics literature and the design of economic policy. First, our findings provide empirical justification for the assumption that consumers distinguish fixed costs from variable costs. For example, Davis and Muehlegger (2010) and Borenstein and W. Davis (2012) use this standard assumption to analyze the welfare implications of two-part tariffs for natural gas and electricity pricing in the United States. In addition, this assumption is ubiquitous in the design of public policies. For example, a climate change bill proposed during the Obama administration—the American Clean Energy and Security Act of 2009 (Congress, 2009)—included a proposal of a lamp-sum redistribution for electricity consumers to compensate for an expected increase in electricity price. This policy design would not distort electricity usage as long as consumers correctly distinguish fixed costs from variable costs in their payment for electricity.

Second, our findings suggest that reforming inefficient energy pricing can be an effective policy tool to address allocative inefficiency and environmental externalities in developing countries, where

subsidized fixed energy charges are still common. Such fixed charges not only create allocative inefficiency but also provide little incentive for energy conservation, resulting in high levels of air pollution and subsequent health and economic burdens for economic growth. Recent papers show the magnitude of the welfare loss due to such air pollution (Almond et al., 2009; Jayachandran, 2009; Chen et al., 2013; Greenstone and Hanna, 2014; Hanna and Oliva, 2015) and the willingness to pay for addressing the problem (Ito and Zhang, 2016). To our knowledge, our paper is among the first studies to investigate a solution to this problem and evaluate an actual ongoing policy in China.

2 The Heating Policy Reform

2.1 Introduction of the consumption-based billing

The Chinese government has provided centralized, coal-fired heating to cities north to the Huai River, covering half of China's urban population, since 1958. The urban heating sector accounts for about 25 percent of total commercial energy use north to the river. The centralized heating, predominantly coal-fired systems, provides no incentives for consumers to respond to market-based energy costs: the systems are based on standards of Soviet technology that do not allow consumers to control their heating. There are practically no meter-based systems and billing is based on a flat per square meter price for an entire heating season.

In seven cities in 2005, China's Ministry of Housing and Urban-Rural Development (MOHURD), in collaboration with the World Bank, started a pilot reform to improve energy efficiency in the heating sector. The reform created a market mechanism so that consumers pay for their actual heating consumption. Individual manual or thermostatic valves are installed to enable households to control indoor temperature, and household meters are installed to establish metering consumption and then introduce consumption-based billing.

Our research site Tianjin is among the most polluted cities in China, with average $PM_{2.5}$ in winter over 100 ug/m^3. In Tianjin, a heating season runs from mid-November to mid-March. Tianjin's reform has two key features for our research design. First, the city government requires that the consumption-based billing should be introduced at least one year *after* household controls and meters are installed. Household heating usage under the flat-rate billing is metered for at least

one heating season. Second, the consumption-based billing was introduced by apartment building on a phase-in basis between 2008 and 2016, allowing for a quasi-experimental design to evaluate the reform effects.

Households are fully informed about the start of the new billing scheme. Before heating is turned on in the first season of consumption-based billing, the HOA office sends every household a letter in October to announce the change in billing method. At the same time, every household also receives a user handbook from utility companies. The handbook explains the new policy in detail, including how households can adjust indoor temperature, how household usage is metered, how a consumption-based bill is calculated, etc.

Finally, according to MOHURD and the city government of Tianjin, all households in apartment buildings that implement the consumption-based billing are obligated to participate in the new pricing scheme, and they should sign a contract of consumption-based billing with utility companies. In practice, households can file an application to opt out and stay in the flat-rate billing scheme. Households make their decisions on whether to opt out before the first heating season of consumption-based billing started. In data provided by the utility company in Tianjian that we collaborate with, around 31 percent of households opt out of consumption-based billing.

2.2 How prices change after the reform

Household heating bills before and after the introduction of consumption-based billing were calculated as follows. Before the policy change, households paid an annual fixed charge, which was equal to 3.79 dollars times square meters. For example, for a household whose condo has 100 square meters, the payment was 379 dollars for every heating season, regardless of how much heating this household used.

After the policy change, a heating bill was a sum of two parts: 1) a new annual fixed charge that was a half of the pre-reform fixed charge—that is, 1.895 dollars times square meters; and 2) 1.4 cents per kWh of heating consumption. For example, consider a household whose house size is 100 square meters and who uses 10,000 kWh in a heating season. The pre-reform payment would be 379 dollars. On the other hand, the post-reform payment would be 329.5 dollars ($= 189.5 + 0.014 \cdot 10,000$) dollars. In this example, this household would experience an increase in the marginal price but an decrease in the average price for the same level of usage.

Figure 1 visualizes how the CBB policy changed the marginal price and average price of heating for a given level of usage per square meter. The change in marginal price was common to all households. However, the change in average price depended on usage per square meter. Consider a household whose square meter size is s, heating usage is y, and the pre- and post-reform heating bills are B_0 and B_1. For a given level of usage y, the household would experience a decrease in the total bill and the average price if the following inequality holds:

$$B_1 - B_0 \equiv (1.895 \cdot s + 0.014 \cdot y) - (3.79 \cdot s) < 0$$
$$\frac{y}{s} < 136$$

where *size* denotes the square meters of the residence, and *usage* is heating consumption for a heating season.

3 Data

The major utility company in the Binhai district of Tianjin provides us two datasets: building-level rollout of the consumption-based billing and household-level daily heating usage data.

3.1 Rollout of the consumption-based billing by complex

There are 245 apartment buildings that have installed meters and introduced consumption-based billing by 2016, and there are 16,133 households in these buildings. Because the policy change we evaluate is the introduction of consumption-based billing, we consider the year when individual meters were installed as the first year of available household-level consumption data. Figure 2a shows substantial variation in the rollout by the number of buildings between 2008 and 2016. The rollout peaked in 2014. Figure 2b shows similar patterns in the reform rollout by the number of households. In our analysis below, we consider December of the first year that implements consumption-based billing to trigger the beginning of the policy treatment. There are 21 buildings where household usage is recorded from the first month of the reform and thus no pre-reform data are observed.

We examine the correlation between the timing of introducing consumption-based billing and complex characteristics in Appendix Table A.1. We find that in more expensive apartment buildings,

the reform started later. The average condo size and year of the building are not significantly correlated with reform timing. We also analyze the pre-reform trends between early reformers and later reformers below.

3.2 Household-level daily heating usage

After household meters are installed, accumulated heating usage by household is automatically recorded once a day and uploaded to the utility company's database. The household-level usage data covers from December 2007 to January 2017. To our knowledge, our study is among the first to use such high-frequent administrative data on energy usage in developing countries. Most previous studies in developing countries rely on survey data on energy usage, which could suffer from issues in self-selection and measurement errors.

Because a heating season starts in mid-November and ends in mid-March, we use data in three months: December, January and February. We focus on the last heating season before the consumption-based billing started and three seasons after. For the pre-reform period, data show that in most apartment buildings, the consumption based billing was implemented one year after individual meters are installed. For around 86% of households, we observe their daily heating usage for three months in the heating season before the consumption based billing started. The number drops sharply to less than half beyond one heating season and even further for more seasons. For the post-reform period, because the majority of buildings had started the reform by 2014, and the last month in the data is January 2017, we observe eight months in heating seasons after the reform for most households.

We construct two samples of household daily usage data as follows. Because the reform was rolled out by building, the sample is unbalanced in event time. For our analysis below, we require that households have monthly panel data within the event study window. In the first sample, to estimate the change in heating usage one heating season after the reform, we require that households have balanced data in three months before and three months after reform. There are 8,916 households in 148 buildings in this sample. In the second sample, for reform effect in three post-reform heating seasons, we focus on households who have balanced data in three months before and eight months after reform. We observe data from 5,895 households in 126 buildings in this sample.

Table 1 reports summary statistics of the two samples described above. In the sample for one

post-reform heating season (column 1), household heating usage is on average 94.75 kWh per day. Average daily usage before the reform is 100 kWh, and it changes to 92.32 kWh after the reform. An average size condo is about 103.8 square meters, and it costs on average 484,942 dollars. The take-up rate of the consumption based billing is 69.4%. In the sample for three post-reform heating seasons (column 2), households have slightly higher usage, live in larger and more expensive condos, and have a slightly lower take-up rate.

4 Treatment Effect of the CBB Policy on Heating Usage

This section investigates the causal treatment effect of the CBB policy on heating usage. As we described in Section 2.1, the take-up rate of the policy was high—69 percent on average—but incomplete. We therefore begin with an intention-to-treat (ITT) analysis and proceed with an analysis for the average treatment effect on the treated (ATET).

4.1 Intention-to-Treat Analysis

An ITT analysis provides the effect of treatment *assignment*—this may not be equivalent to actual treatment status under incomplete compliance—on an outcome. In our context, we estimate the effect of the CBB policy on household heating usage, regardless of whether the household actually complied with the policy or not.

Our empirical strategy exploits the staggered timings of treatment assignment. We therefore begin with a standard event study analysis, which provides visual investigation of treatment effects in the presence of staggered treatment assignment (McCrary, 2007; Kline, 2012). Note that we have household-level daily usage data, but the treatment timing differs by building. For this reason, our treatment dummy variables are defined by the building level, and we also cluster our standard errors at the building level. We estimate the following equation by OLS:

$$y_{it} = \alpha_j + \gamma_t + \sum_{c=\underline{c}}^{\bar{c}} \phi_c D_{jt}^c + \eta D_{jt}^{\underline{c}} + \rho D_{jt}^{\bar{c}} + u_{it}, \tag{1}$$

where y_{it} is the natural log of daily heating usage for household i in day t. We include building-level fixed effects (α_j) to absorb time-invariant variation at the building level, and day fixed effects (γ_t)

to capture common time shocks such as weather.

We use $c = \{\underline{c}, ..., -1, 0, 1, .., \bar{c}\}$ to denote the *event study months* relative to the first treatment month for each household. For example, $c = -1$ is the last month of the pre-treatment regime, $c = 0$ is the first month of treatment, and $c = 1$ is the second month of treatment. Because we use data from three winter months (between the first day of December and the last day of February), it is helpful to consider the following example. Consider a household whose building was introduced the CBB policy in the beginning of the winter 2010. For this household, c equals -1 in February 2009, 0 in December 2010, 1 in January 2011, 2 in February 2011, and 3 in December 2012. For each c, we define a dummy variable D_{jt}^{c}, which equals one if day t falls within the event study month c for household i's building j.

McCrary (2007) and Kline (2012) emphasize that there are at least two technical but important issues to be addressed in an event-study regression. First, we need to define the end points of $\underline{c}$ and $\bar{c}$ and make sure that all of the households included in the regression have data for each of $c = \{\underline{c}, ..., -1, 0, 1, .., \bar{c}\}$. Without this restriction, the coefficients β_c cannot be comparable between different values of c. Second, because of the staggered introduction of the policy, usage data for earlier t or later t for some households have to be outside the range of $c = [\underline{c}, \bar{c}]$. Failing to control for the event months outside the range of $c = [\underline{c}, \bar{c}]$ would result in biased estimates for β_c. To address this issue, we follow the approach by McCrary (2007) and include two dummy variables, $D_{jt}^{\underline{c}}$ and $D_{jt}^{\bar{c}}$, where $D_{jt}^{\underline{c}} = 1$ if $c < \underline{c}$ and $D_{jt}^{\bar{c}} = 1$ if $c > \bar{c}$. Finally, u_{it} is the error term, and we cluster the standard error at the building level to account both for within-building correlation and serial correlation over time.

The primary variables of interest are ϕ_c. These coefficients provide the ITT estimates of mean log daily usage for event month c, controlling for building fixed effects and day fixed effects. The excluded group is $c = -1$ (the last month of the pre-treatment regime) so that we can interpret β_c. as the difference in mean log daily usage between event month c and the last month of the pre-treatment regime. An event-study design requires the identification assumption that is standard for the difference-in-differences approach. In the absence of treatment, there would be no difference in mean log usage between the event months—that is, ϕ_c would be zero. The validity of this identification assumption is untestable, but we are able to test if this assumption holds for the pre-trend by estimating β_c in the pre-treatment period.

In Figure 3, we show the estimates of ϕ_c. In Panel A, we estimate equation (1) using households whose data are available for each of event month c in $c = [-3, 2]$—between a year before and a year after the policy assignment. This method allows us to include 8,916 households. In Panel B, we include households whose data are available for each of c in $c = [-3, 7]$—between a year before and three years after the policy assignment. This method reduces the sample size to 5,895 households because we do not observe data for three years after the policy introduction for late adopters, as usually the case in an event study design.

The figures provide three key results. First, there is no statistically significant pre-trend before the policy introduction. The flat pre-trend of the outcome variable provides strong evidence that the identification assumption for the event study design is likely to be valid. Second, the ITT estimate for the first year is between 4 and 8 percent reductions in heating usage. Third, the policy impact seems to persist and become larger in the second and third years after the policy implementation.

To provide statistical evidence for the second and third points, we estimate the following equation by OLS:

$$y_{it} = \alpha_j + \gamma_t + \phi D_{jt} + \eta D_{jt}^{c} + \rho D_{jt}^{\bar{c}} + u_{it}, \tag{2}$$

where D_{jt} is the treatment assignment dummy variable for the policy, which equals one if building j was under the CBB policy in t.

Table 2 shows the results. Column 1 presents the ITT estimate corresponding to Figure 3a. The point estimate (-0.054) implies that the policy caused a reduction in heating usage by 0.054 log points (5.3 percent). Column 2 shows the ITT estimate corresponding to Figure 3b. For the households who experienced the policy for three years or more, the ITT estimate in the first year after reform is a reduction in usage by 6 percent, in the second and third years by about 11 percent.

4.2 Average Treatment Effect on the Treated

In our research design, nobody in the control status was treated, while we have incomplete compliance for households in the treated status. Therefore, under the standard assumptions for the local average treatment effect (LATE), we can use two-stage least squares (2SLS) to estimate the average treatment effect on the treated (ATET). In our context, the ATET shows the average treatment effect for households who complied with the new policy.

We define T_{it}^{c} as a dummy variable for the treatment *status* for event month c for household i. $T_{it}^{c} = 1$ if the household's building j falls in event month c in time t (i.e. $D_{jt}^{c} = 1$) and household i was a complier for the CBB policy. To create the event study figure for the ATET estimates, we estimate equation (3) by 2SLS:

$$y_{it} = \alpha_j + \gamma_t + \sum_{c=\underline{c}}^{\bar{c}} \psi_c T_{it}^{c} + \eta D_{jt}^{\underline{c}} + \rho D_{jt}^{\bar{c}} + u_{it}, \tag{3}$$

using the treatment assignment dummy variables D_{jt}^{c} from equation (1) as instruments. Except for the instruments, this estimation strategy is similar to what we described for equation (1). The primary variables of interest are ψ_c. These coefficients provide the ATET estimates of mean log daily usage for event month c, controlling for building fixed effects and day fixed effects.

Similarly, we estimate the ATET of the CBB policy on heating usage by 2SLS for equation (4), which is analogous to equation (2),

$$y_{it} = \alpha_j + \gamma_t + \psi T_{it} + \eta D_{jt}^{\underline{c}} + \rho D_{jt}^{\bar{c}} + u_{it}, \tag{4}$$

where T_{it} is the treatment status dummy variable for the policy, which equals one if household i was treated in t, and we use D_{jt} as an instrument for T_{it}.

Figure 4 and Column 3 and 4 in Table 2 show the ATET estimates. Similar to the evidence for the ITT estimates, there is no statistically significant pre-trend before the policy introduction. Column 3 in Table 2 indicates that the ATET for the first year is a reduction in heating usage by 8.2 percent. Column 4 implies that for households who experienced the policy for three years, the ATET is a reduction in usage by 9.4 percent in the first year, and about 17 percent in the second and third years.

Note that the ATET estimates are larger than the ITT estimates in absolute value because of the one-sided incomplete compliance for the treatment status. In general, we cannot tell much about the potential treatment effects for non-compliers. In case we can assume that the ATE does not differ between the compliers and non-compliers, we can use our ATET estimates for all households.

In sum, the ITT and ATET estimates indicate that the CBB policy incentivized households to reduce heating usage significantly. This finding itself is important for the policy. However, the

simple analysis in this section does not uncover a few more questions we want to answer. First, we want to know if the CBB policy induced a reduction in usage for everyone, including those who experienced an increase in marginal price but a decrease in average price and total payment. The answer to this question provides evidence for whether consumers distinguished fixed costs from variable costs. Second, we want to use the quasi-experiment provided by the CBB policy to estimate the price elasticity for heating demand, which is a useful parameter for the optimal rate design. We investigate these questions in the next section.

5 Demand Estimation Based on Policy-Induced Price Changes

5.1 Conceptual Framework

Before we present our empirical strategy to estimate heating demand, it is useful to provide a brief conceptual framework. Consider a utility maximization problem for heating demand y. A consumer has income I, the marginal price of heating is mp, and the fixed charge is f. We consider a quasi-linear utility function $u = v(y) - mp \cdot y - f + I$.[1]

A standard utility maximization problem simply solves the first order condition for the utility function, which leads to a condition: $v'(y^*) = mp$. Therefore, the optimal usage (y^*) can be obtained when the marginal utility from heating usage equals to the marginal price. On the other hand, if the consumer is a "schmeduler" (Liebman and Zeckhauser, 2004), the consumer can be confused between changes in variable costs and fixed costs, and misperceive a change in average price (ap) as a change in marginal price. Then, the optimal usage under this condition can be characterized by $v'(y^{**}) = ap$, which means that the marginal utility from heating usage equals to the average price.

In Section 2.2, we described that the CBB policy produced a common policy-induced change in marginal price for all customers but different policy-induced changes in average price. Figure 1 suggests that customers with smaller usage per square meter were more likely to experience a decrease in average price, while those with larger usage per square meter were more likely to find

[1] A quasi-utility function assumes that there is no income effect. This assumption is likely to be a valid in our empirical context because the income effect of the CBB policy was likely to be very small. The CBB policy reduced the annual fixed charge by about \$192 per household. The average household income in Tianjin in our sample period was \$15,041. Therefore, \$200 was about 1.2 percent of household income. In the literature on residential energy demand, the income elasticity is found quite small, around 0.01. It implies that the income effect of the CBB policy on usage would be a change in usage by about 0.012 percent.

an increase in average price. We exploit this policy-induced variation in marginal price and average price to test whether consumers respond to marginal price or average price under a two-part tariff.

5.2 Policy-Induced Changes in Marginal Price, Average Price, and Usage

Consider the distribution of usage per square meter for building j and time t and the deciles ($k = 1, ..., 10$) of the distribution. To estimate how the CBB policy affected different parts of the distribution, we conduct the ITT analysis in equation (2) and the ATET analysis (4) for each decile separately. For each decile, we need the same identification assumption as the one in the previous section. The changes in mean log usage would have to be the same between households who were assigned to the CBB policy and other households in the absence of the CBB policy.

Table 3 shows the ITT estimates, along with the CBB sign-up rate for each decile. The ITT estimates are statistically significant in all deciles, and the point estimates in absolute value are larger in lower deciles. This can be partially because the CBB sign-up rate was higher in lower deciles. Recall that customers in the lower deciles were more likely to be the "structural winners" of the policy—those who gained most from the policy—because their average price and total bills were more likely to be reduced. The difference in the CBB sign-up rate between the deciles suggests that at least to some degree, households were self-selected in response to their expected gains from the policy. This finding is consistent with evidence in previous studies.[2]

Table 4 shows the ATET estimates. In terms of point estimates, the lower deciles still exhibit stronger responses to the policy than the higher deciles. However, these estimates are not statistically different from each other. These estimates indicate that the ATET was negative and statistically significant in all deciles, ranging from -0.058 log points (a 5.6 percent reduction in usage) and -0.107 log points (a 10.1 percent reduction in usage).

This empirical evidence is consistent with standard economic theory and inconsistent with the alternative prediction based on "shmeduling." To visualize this point, we plot the estimated coefficients in Table 3 in Figure 5. It shows the policy-induced changes in the log of daily usage with the 95 percent confidence intervals. The figure also shows the policy-induced changes in marginal price and average price by the decile of usage per square meter. As we expected, consumers in lower

[2]For example, Ito, Ida, and Tanaka (2017) find in their randomized controlled trial for residential electricity plan choice that structural winners were more likely to switch to a new tariff particularly when the expected gain from switching was explicitly shown to consumers.

deciles experienced an increase in marginal price but a decrease in average price. Therefore, we expect an *increase* in usage for these customers if they cannot distinguish fixed costs and variable costs and therefore respond to average price. However, our empirical finding is a statistically significant *decrease* in usage at the 1 percent significance levels for the lower deciles. We strongly reject the null hypothesis that the change in usage is positive for these consumers because this statistical test is equivalent to one-sided t-test for the coefficient in Table 3. The p-value for this test is therefore a half of the p-value in the two-sided t-test.

5.3 Encompassing Test

As a second statistical approach, we use the encompassing test (Davidson and MacKinnon, 1993) to test the demand responses to marginal price and average price. Consider the following log-linear demand function for y_{it}, which is log daily heating usage for household i on day t:

$$y_{it} = \alpha_j + \gamma_t + \beta_1 MP_{it} + \beta_2 AP_{it} + \eta D^{c}_{jt} + \rho D^{\bar{c}}_{jt} + u_{it}, \tag{5}$$

where MP_{it} and AP_{it} are the marginal and average prices of heating. In the case of log-linear specification, β_1 and β_2 are the semi-elasticities of demand, which approximates the percentage change in usage with respect to one unit change in price. For the average price, we also use the log-log specification to examine the robustness of our analysis.[3]

In the encompassing test, we examine how the inclusion of one variable or another (MP_{it} and AP_{it}) affects the estimates of β_1 and β_2. For example, if consumer behavior is more consistent with standard economic theory, we expect that the inclusion of AP_{it} does not affect the estimates of β_1 and that the estimate of β_2 is near zero after we control for the effect of MP_{it}.

To estimate equation (5), we need to address the endogeneity of the price variables. For the marginal price, households in our sample have a constant marginal price for given t. This situation differs from the case of a tiered multiple marginal price schedule, which is common in electricity, natural gas, and water pricing in the U.S (Olmstead et al., 2007; Borenstein, 2009; Ito, 2014). The constant marginal price makes the empirical strategy simpler than those in tiered price schedules because the marginal price does not depend on usage. However, we still need to address another

[3]We cannot use log-log specification for marginal price because the pre-CBB marginal price was zero.

endogeneity, which comes from the incomplete compliance of the policy. A household's self-selection in the take-up of the CBB policy can be correlated with the error term in equation (5). To address this concern, we use the treatment assignment status D_{jt} as an instrument for the marginal price. The identification assumption required for this method is equivalent to the assumption required in the event study regression in equation (3).

For the average price, we construct instruments D_{jkt} for $k = 1, ..., 10$, which equals one if D_{jt} equals one and household i's usage on day t falls in decile k. As we showed in the previous subsection, households in different parts of the distribution of usage per square meter were likely to experience different policy-induced changes in average price. These instruments capture such variation. A threat to identification is that consumers in different parts of the distribution of usage per square might also have different unobservable shocks in u_{it} in equation (3). However, we can control for such confounding factors by including decile-specific building fixed effects (α_{kj}) and decile-specific day fixed effects (γ_{kt}), as long as the identification assumption for our event study research design holds.

To get intuition behind this instrumental variable approach, consider a hypothetical simple randomized controlled trial for a cross-sectional data for two groups. Suppose that the CBB policy is randomly assigned to a treatment group, and a control group is assigned to the pre-CBB pricing. In this case, we can define D_k, which equals one for households who are in the treatment group and in decile k in the distribution of usage per square meter. We can use D_k as an instrument for average price and include decile-specific fixed effects (α_k). The identification assumption for this hypothetical case is that D_k is orthogonal to the error term after controlling for α_k. That is, the expected value of the error term for households in decile $k$is the same between the treatment and control groups, which holds by construction in this RCT.

In our context, conditional on our identification assumptions, the event study design provided exogenous variation analogous to the random variation in the hypothetical RCT. As an example, consider that in a certain time period t, some buildings were in the treatment status (group T) and other buildings were in the control status (group C). Our identification assumption is that in the absence of the CBB policy, the changes in the mean log usage in decile k would be the same between groups A and B. This assumption assures the orthogonality between the instruments and the error term.

We estimate the following equation by 2SLS:

$$y_{it} = \alpha_{kj} + \gamma_{kt} + \beta_1 MP_{it} + \beta_2 AP_{it} + \eta D^{c}_{jt} + \rho D^{\bar{c}}_{jt} + u_{it}, \tag{6}$$

using instruments D_{jkt} $(k = 1, ..., 10)$ for MP_{it} and AP_{it} and decile-specific building fixed effects (α_{kj}) and decile-specific day fixed effects (γ_{kt}).

Panel A of Table 5 shows the results for households who experienced the CBB at least one year. Column 1 includes only the marginal price. The estimate implies that the semi-elasticity with respect to marginal price is -5.89. Because the unit of the price is in dollars, the coefficient implies that an increase in the marginal price by one cent would produce a reduction in usage by 5.89 percent. Note that the change in marginal price in the CBB was 1.4 cents. If we multiply 5.89 and 1.4, it will be 8.2 percent, which is by construction equal to the ATET estimate in Column 3 in Table 2.

Likewise, Column 2 includes only the average price. We expect this coefficient to be negative and statistically significant if consumers are confused between changes in fixed costs and variable costs. However, the estimate is near zero and statistically insignificant. In column 3, we include both of the marginal price and the average price. Including the average price in this regression merely changes the coefficient for the marginal price, suggesting that the variation in average price does not explain much about the variation in usage.

For a robustness check, we repeat this exercise with log average price. The results in columns 4 and 5 imply that there is a spurious correlation between usage and log average price. Without controlling for the effect of marginal price (column 4), we might mistakenly conclude that there is a statistically significant relationship between usage and log average price. However, once we control for the effect of marginal price (column 5), the coefficient for the log average price becomes near zero and statistically insignificant.

Panel B includes households who experienced the CBB at least three years. Consistent with our findings in the ATET estimation, the semi-elasticity estimates are larger (in absolute value) than those in Panel B. Aside from the quantitative differences in the coefficients, the conclusion is the same as Panel B. Once we control for the effect of the marginal price, we do not find statistically significant relationship between usage and average price.

In sum, both of the encompassing test results and the results form the previous subsection are consistent with the prediction from standard economic theory. Our empirical evidence suggests that consumers are not confused between changes in fixed costs and variable costs.

5.4 Price Elasticity of Residential Heating Demand

In addition to the semi-elasticity estimates, we provide estimates for the arc price elasticity of heating demand. The arc elasticity provides a point elasticity at the middle point of the price change. In our case, consumers experienced a change in marginal price from 0 to 0.014 dollars per kWh of heating. Therefore, the arch elasticity is a point elasticity at $p = 0.007$.

We show the estimated arc price elasticities with respect to marginal price at the bottom of Table 5. The short-run elasticity, which is calculated based on households who experienced the CBB for a year—is -0.04. The medium-long-run elasticity, which is calculated based on households who experienced the CBB for three years—is -0.059. Note that these elasticities would be doubled if we calculate a point elasticity at the post-policy marginal price $p = 0.014$. At this price level, the short-run and medium-long-run point elasticities are -0.08 and -0.118. These price elasticity estimates—ranging between -0.04 to -0.118—are quite similar to the price elasticity estimates in the recent literature for residential electricity consumers in the U.S. and other countries (Wolak, 2011; Ito, 2014; Ito, Ida, and Tanaka, 2018).

6 Welfare Analysis

Our analysis in the previous sections suggests that consumers in our sample distinguished fixed costs from variable costs in a way that is consistent with the prediction from standard economic theory. It implies that the CBB policy was likely to provide its intended policy impacts. In this section, we quantify the welfare implications of the CBB policy based on our demand estimation results. This welfare analysis is policy-relevant because many developing countries and international organizations such as the World Bank are investing substantial money on the introduction of metered pricing, as a replacement of conventional fixed charges for utility services, including energy, water, and telecommunications.

6.1 Allocative Efficiency, Environmental Externalities, and Consumer Surplus

Figure 6 summarizes the welfare implications of the CBB policy. Consider a consumer who has a quasi-utility function for heating, and therefore, her Hicksian demand is equivalent to her Marshallian Demand. Before the CBB policy, her marginal price was zero, and her heating usage was y_0. The CBB policy introduced a new marginal price, which approximates the marginal cost of heating, according to the utility company. The new marginal price incentivized her to consume heating at y_1.

The CBB policy induced two forms of social welfare gains. The first gain comes from the improvement of allocative efficiency independent from environmental externalities. With zero marginal price, the consumer consumed too much heating (y_0) than the efficient level (y_1). This welfare gain—we call it "social welfare gain from allocative efficiency"—equals to the triangle C in the figure.

The second welfare gain comes from a reduction in environmental externalities. In many developing countries, energy services such as heating and electricity produce high environmental costs due to dirtier fossil fuel and lower-quality abatement technologies than other countries. These costs are usually not internalized in the private marginal cost. Then, the reduction in usage also reduces a deadweight loss associated with such environmental externalities. This welfare gain—we call it "social welfare gain from lowered environmental externalities"—equals to the rectangle D in the figure.

Finally, although the policy is likely to increase the social welfare, it could increase or decrease consumer surplus. Before the CBB, consumers paid a fixed change $f \cdot s$, in which s is the square meters of the consumer's residence. After the CBB, they paid a new fixed charge $\frac{1}{2} \cdot f \cdot s$ and $p \cdot y$, where p is the marginal price for heating usage y. Therefore, the change in consumer surplus due to the CBB policy was $\frac{1}{2} \cdot f \cdot s - (A + B)$ in the figure. Thus, the change in consumer surplus depends on the consumer's square meters as well as her demand curve. We quantify the change in consumer surplus at the aggregate level and also at the different wealth levels to assess whether the policy change was regressive or progressive.

6.2 The Effects of the CBB Policy on Social Welfare and Consumer Surplus

Table 6 shows the welfare analysis results. Column 1 provides annual welfare estimates in dollar per household based on our ATET estimates. In Column 2, we take a more conservative approach by using our ITT estimate. Finally, in column 3, we provide the city-wide welfare estimates for Tianjin using our ITT estimates.

The social welfare gain from allocative efficiency (triangle C in Figure 6) is 11.51 dollars per household with the ATET estimate and 7.41 dollars with the ITT estimate.

We also conduct a back-of-the-envelop calculation on the social welfare gain from lowered environmental externalities (rectangle D in Figure 6) as follows. First, we use the formula that the MOHURD uses to convert the amount of coal usage saved from the amount of reduced heating usage. Second, we use two measures on the external costs from coal from previous studies: a lower estimate of 52 dollars per ton in the US in Muller et al. (2011), and a higher estimate of 62 dollars per ton in China in Greenpeace (2010). We then calculate that the social welfare gain ranges from 14.04 to 16.74 based on the ATET estimates, and 9.04 to 10.78 based on the ITT estimates, depending on the estimates for the external cost of coal.

A lower bould of the total social welfare gain is 25.55 dollars per household with the ATET estimate and 16.45 dollars with the ITT estimate. These numbers are 6.5 or 4.2 percent of the expenditure for heating. Using the ITT estimate and the total number of households that started consumption-based billing in Tianjin, we also calculate the aggregated social welfare gain at the city level, which equals to 2.89 million dollars per year.

Finally, we show the mean change in consumer surplus. On average, the consumer surplus was increased by 13.11 dollars (with the ATET) or 9.01 dollars (with the ITT estimate). Therefore, the policy not only increased the social welfare but also increased consumer surplus. In addition to the mean change in consumer surplus, we are also interested in the redistributional impacts of the policy. Figure 7 shows the change in consumer surplus by quantile of household wealth, proxied by their housing prices. The figure suggests that this reform itself was regressive—the consumer surplus was increased for wealthier households but decreased for less wealthy households. This regressivity comes from the fact that the pre-reform policy was very progressive because the fixed charge was proportional to the size of a house, resulted in high payments for wealthier households regardless of

their heating usage.

7 Conclusion

This paper uses a quasi-experiment in heating price reform in China to empirically test whether consumers distinguish fixed costs. The introduction of a two-part tariff created policy-induced variation in variable costs and fixed charges, and staggered policy implementation allows us to develop an event-study research design.

Using administrative records on daily heating usage at the household level, we find strong evidence that consumers distinguish fixed costs from variable costs in a way that is consistent with standard economic theory. Consequently, the policy provided intended social welfare gains from allocative efficiency and environmental externalities. Our findings provide important implications for energy policy because a growing number of developing countries including China are in the process of implementing consumption-based energy billing in lieu of pre-existing inefficient fixed-charge billing.

References

Almond, D., Y. Chen, M. Greenstone, and H. Li (2009). Winter heating or clean air? unintended impacts of china's huai river policy. *The American Economic Review: Papers and Proceedings 99*(2), 184–190.

Borenstein, S. (2009). To what electricity price do consumers respond? residential demand elasticity under increasing-block pricing. *Working Paper*.

Borenstein, S. and L. W. Davis (2012). The equity and efficiency of two-part tariffs in US natural gas markets. *The Journal of Law and Economics 55*(1), 75–128.

Brown, J., T. Hossain, and J. Morgan (2010). Shrouded attributes and information suppression: Evidence from the field. *Quarterly Journal of Economics 125*(2), 859–876.

Busse, M., J. Silva-Risso, and F. Zettelmeyer (2006). 1,000 cash back: The pass-through of auto manufacturer promotions. *The American Economic Review 96*(4), 1253–1270.

Chen, Y., A. Ebenstein, M. Greenstone, and H. Li (2013). Evidence on the impact of sustained exposure to air pollution on life expectancy from china's huai river policy. *Proceedings of the National Academy of Sciences 110*(32), 12936–12941.

Chetty, R., A. Looney, and K. Kroft (2009). Salience and taxation: Theory and evidence. *The American Economic Review 99*(4), 1145–1177.

Congress (2009). The american clean energy and security act of 2009 (HR 2454).

Davidson, R. and J. G. MacKinnon (1993). Estimation and inference in econometrics. OUP catalogue, Oxford University Press.

Davis, L. W. and E. Muehlegger (2010). Do americans consume too little natural gas? an empirical test of marginal cost pricing. *RAND Journal of Economics 41*(4), 791–810.

de Bartolome, C. A. M. (1995, January). Which tax rate do people use: Average or marginal? *Journal of Public Economics 56*(1), 79–96.

Finkelstein, A. (2009). E-ZTAX: tax salience and tax rates. *The Quarterly Journal of Economics 124*(3), 969–1010.

Gabaix, X. and D. Laibson (2006, May). Shrouded attributes, consumer myopia, and information suppression in competitive markets. *The Quarterly Journal of Economics 121*(2), 505–540.

Greenpeace (2010). The true cost of coal in china. Technical report, Greenpeace East Asia.

Greenstone, M. and R. Hanna (2014). Environmental regulations, air and water pollution, and infant mortality in india. *American Economic Review 104*(10), 3038–3072.

Hanna, R. and P. Oliva (2015). The effect of pollution on labor supply: Evidence from a natural experiment in mexico city. *Journal of Public Economics 122*, 68–79.

Hastings, J. S. and J. M. Shapiro (2013, November). Fungibility and Consumer Choice: Evidence from Commodity Price Shocks*. *The Quarterly Journal of Economics 128*(4), 1449–1498.

Ito, K. (2014). Do consumers respond to marginal or average price? evidence from nonlinear electricity pricing. *American Economic Review 104*(2), 537–63.

Ito, K., T. Ida, and M. Tanaka (2017). Information frictions, inertia, and selection on elasticity: A field experiment on electricity tariff choice. *Working Paper*.

Ito, K., T. Ida, and M. Tanaka (2018). Moral suasion and economic incentives: Field experimental evidence from energy demand. *American Economic Journal: Economic Policy*.

Ito, K. and S. Zhang (2016). Willingness to Pay for Clean Air: Evidence from Air Purifier Markets in China. *NBER Working Paper* (w22367).

Jayachandran, S. (2009). Air quality and early-life mortality: Evidence from indonesia's wildfires. *Journal of Human Resources 44*(4), 916–954.

Jessoe, K. and D. Rapson (2014). Knowledge is (less) power: Experimental evidence from residential energy use. *American Economic Review 104*(4), 1417–38.

Kahn, M. E. and F. A. Wolak (2013). Using Information to Improve the Effectiveness of Nonlinear Pricing: Evidence from a Field Experiment : Final Report. Working paper, Stanford University.

Kline, P. (2012). The impact of juvenile curfew laws on arrests of youth and adults. *American Law and Economics Review 14*(1), 44–67.

Liebman, J. and R. Zeckhauser (2004). Schmeduling. *www.hks.harvard.edu/jeffreyliebman/schmeduling.pdf*.

McCrary, J. (2007). The effect of court-ordered hiring quotas on the composition and quality of police. *American Economic Review 97*(1), 318–353.

McRae, S. (2015). Efficiency and Equity Effects of Electricity Metering: Evidence from Colombia. *Working Paper*.

Muller, N., R. Mendelsohn, and W. Nordhaus (2011). Environmental accounting for pollution in the united states economy. *American Economic Review 101 101*, 1649–1675.

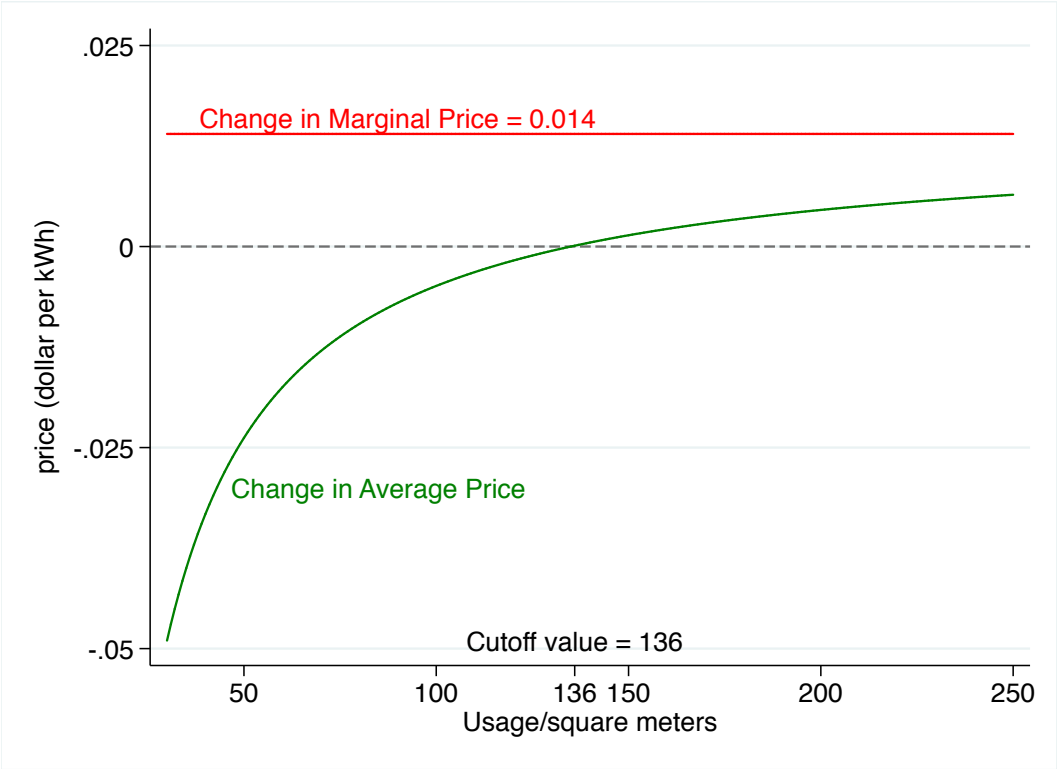
Olmstead, S. M., W. M. Hanemann, and R. N. Stavins (2007). Water demand under alternative price structures. *Journal of Environmental Economics and Management 54*(2), 181–198.

Stiglitz, J. E. (1988). *Economics of the public sector*. WW Norton.

Tirole, J. (1988). *The theory of industrial organization*. MIT press.

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

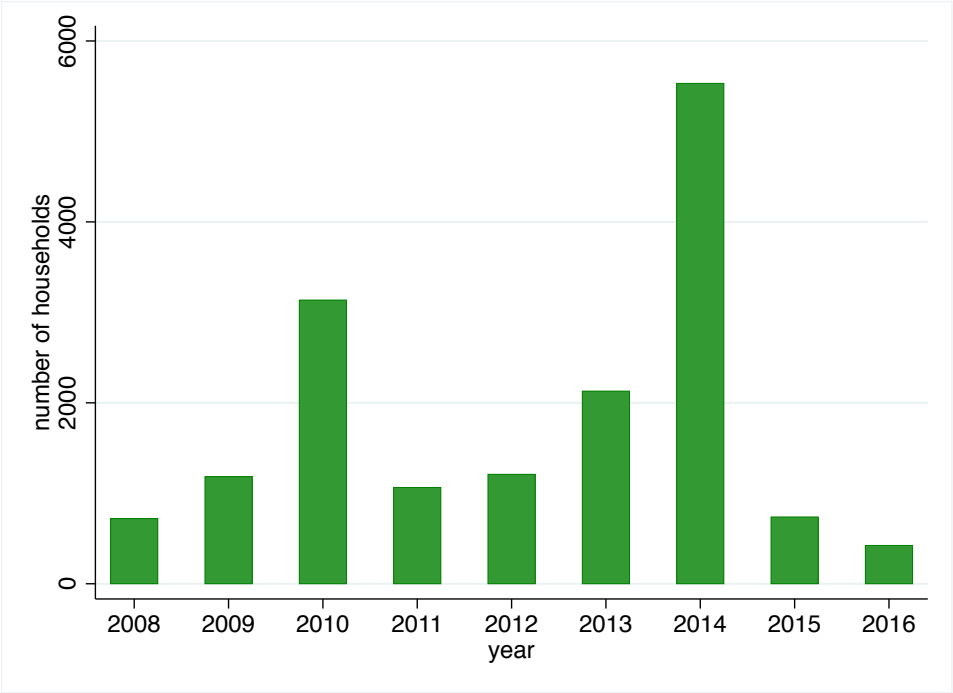
Figure 1: Policy-Induced Changes in Marginal and Average Prices



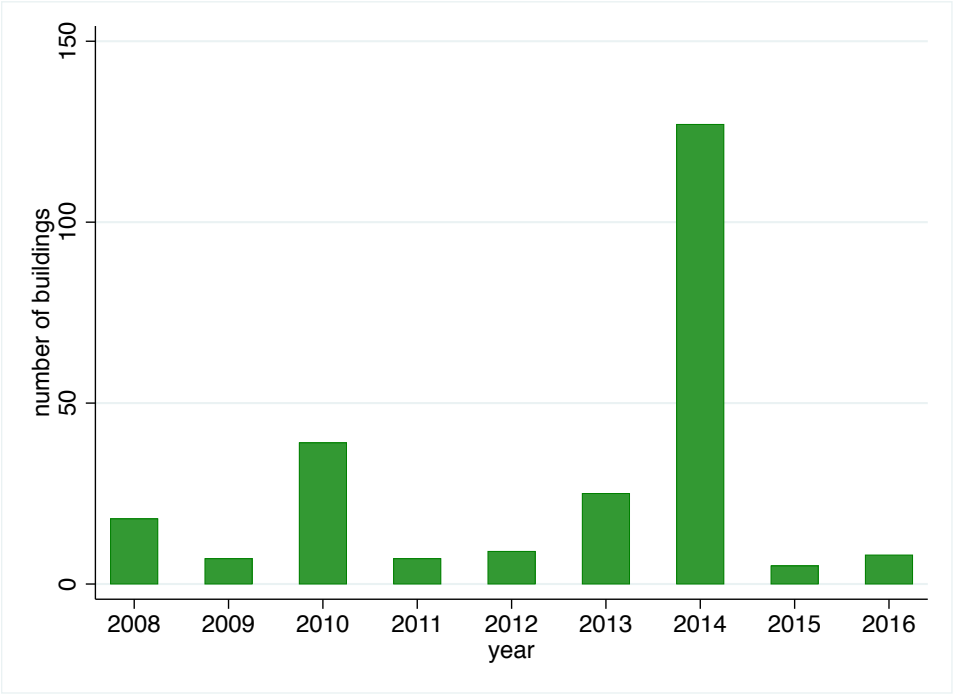
Notes: This figure shows the changes in marginal price and average price induced by the introduction of the consumption-based billing policy.

Figure 2: Rollout Timings of the Consumption-Based Billing Policy

(a) Number of households



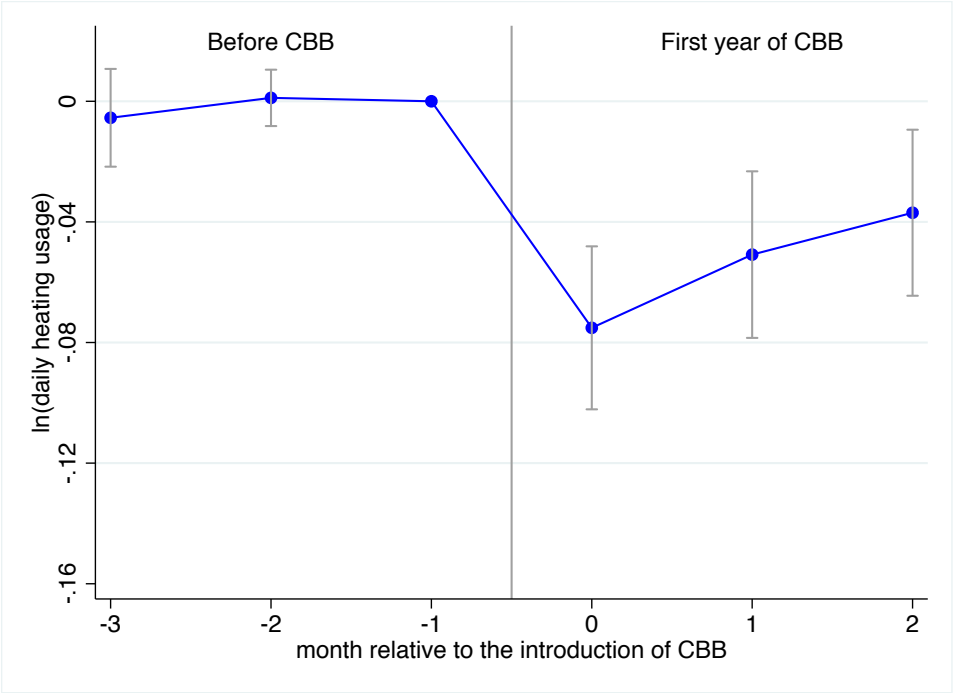
(b) Number of buildings



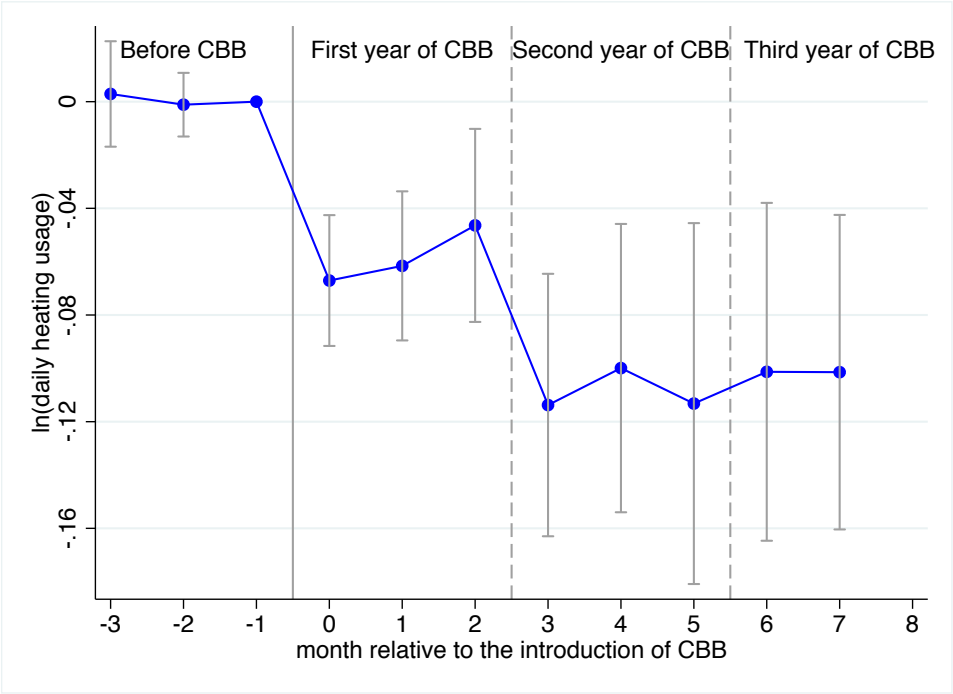
Notes: This figure shows the rollout of the consumption-based billing policy.

Figure 3: Event-Study Analysis: Intention-to-Treat (ITT)–

(a) One year after the policy implementation



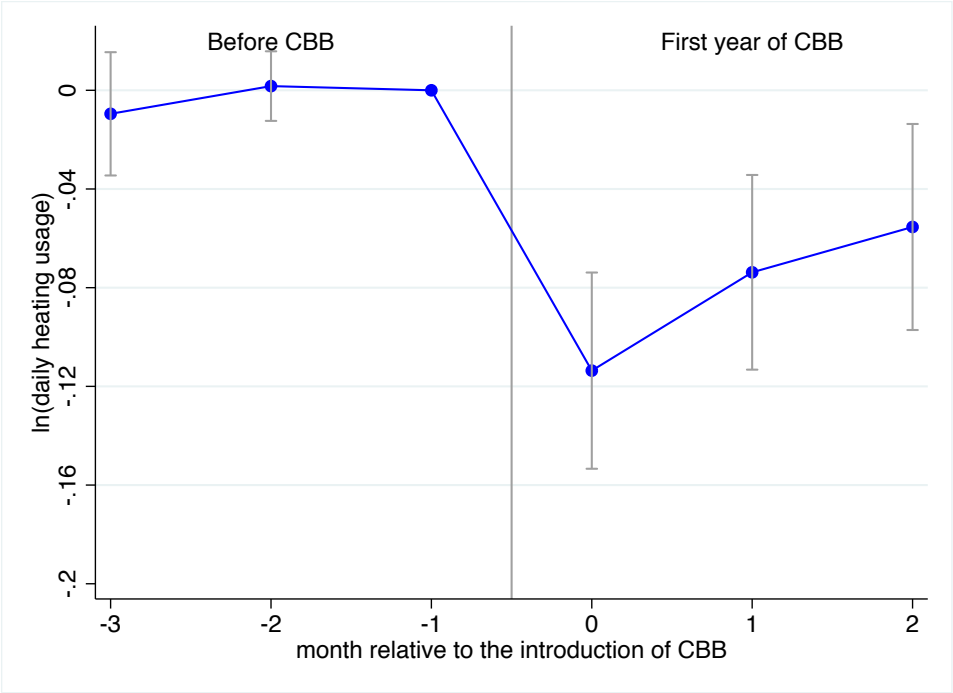
(b) Three years after the policy implementation



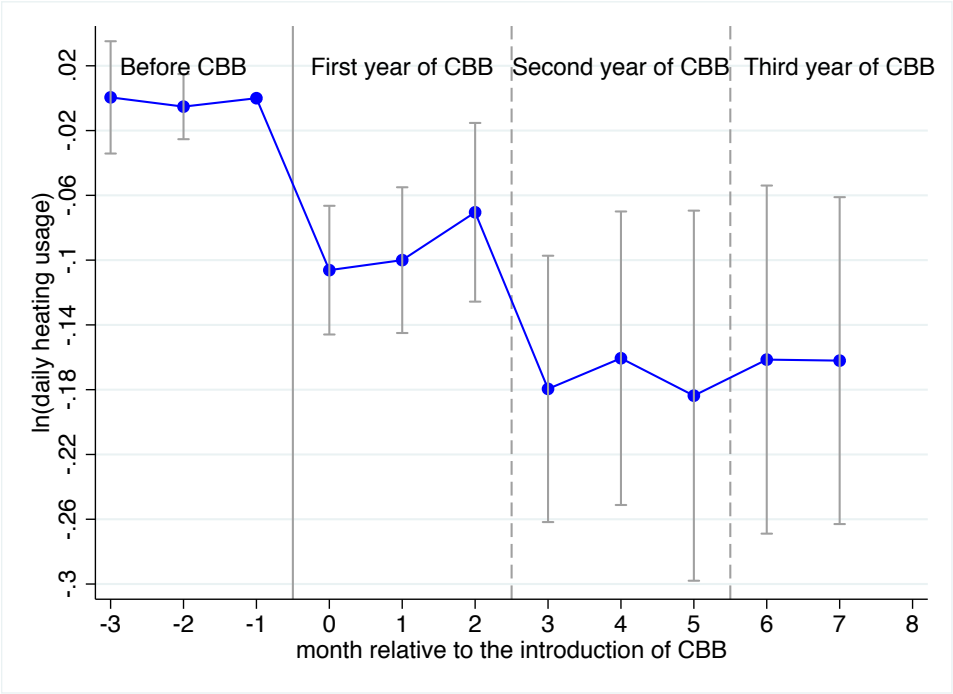
Notes: This figure shows the ITT estimates of the CBB policy on log daily heating usage. The bars indicate the 95 percent confidence intervals.

Figure 4: Event-Study Analysis: Average Treatment Effect on the Treated (ATET)

(a) One year after the policy implementation

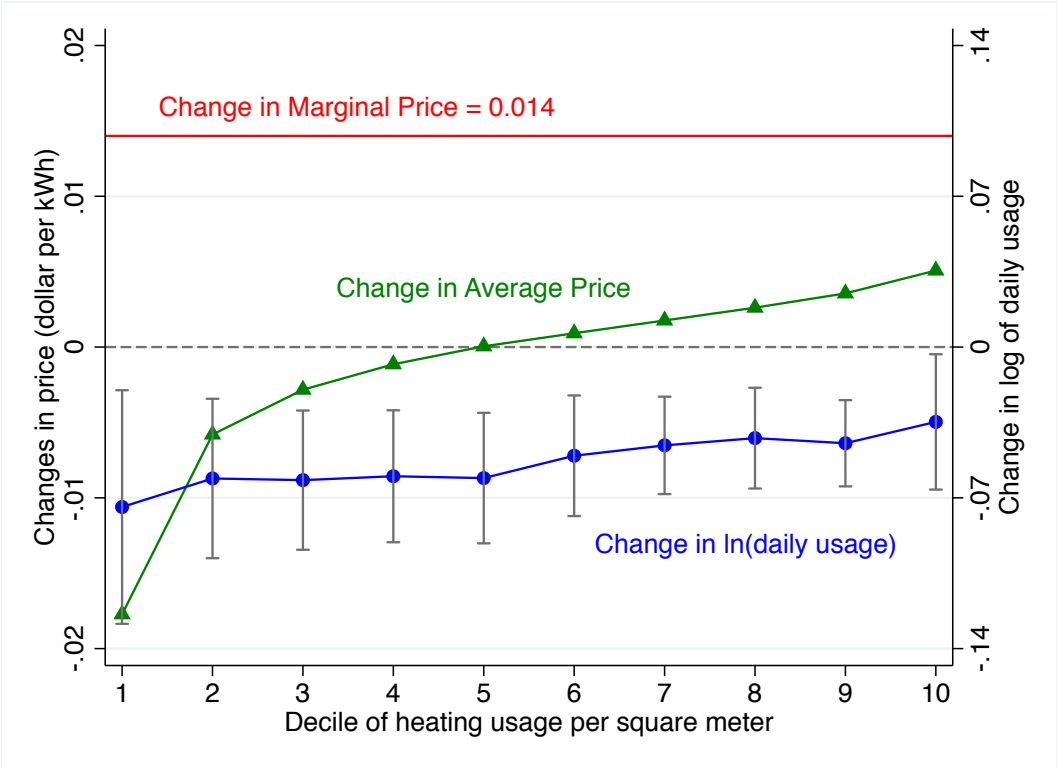


(b) Three years after the policy implementation



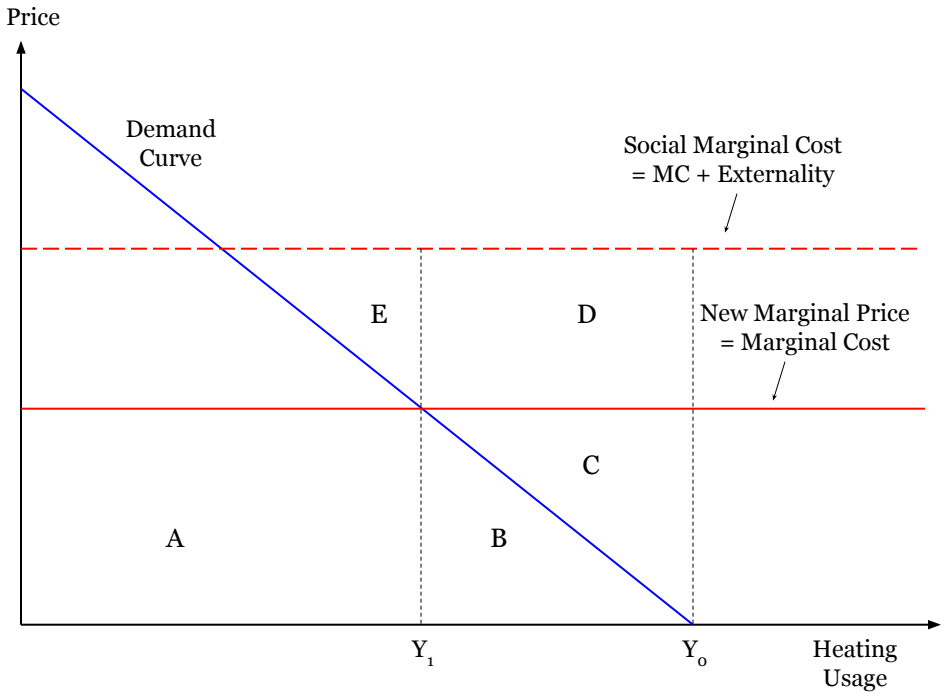
Notes: This figure shows the ATET estimates of the CBB policy on log daily heating usage. The bars indicate the 95 percent confidence intervals.

Figure 5: Policy-Induced Changes in Marginal Price, Average Price, and Usage



Notes: This figure shows the effects of the CBB policy on marginal price, average price, and usage by the deciles of usage per square meter.

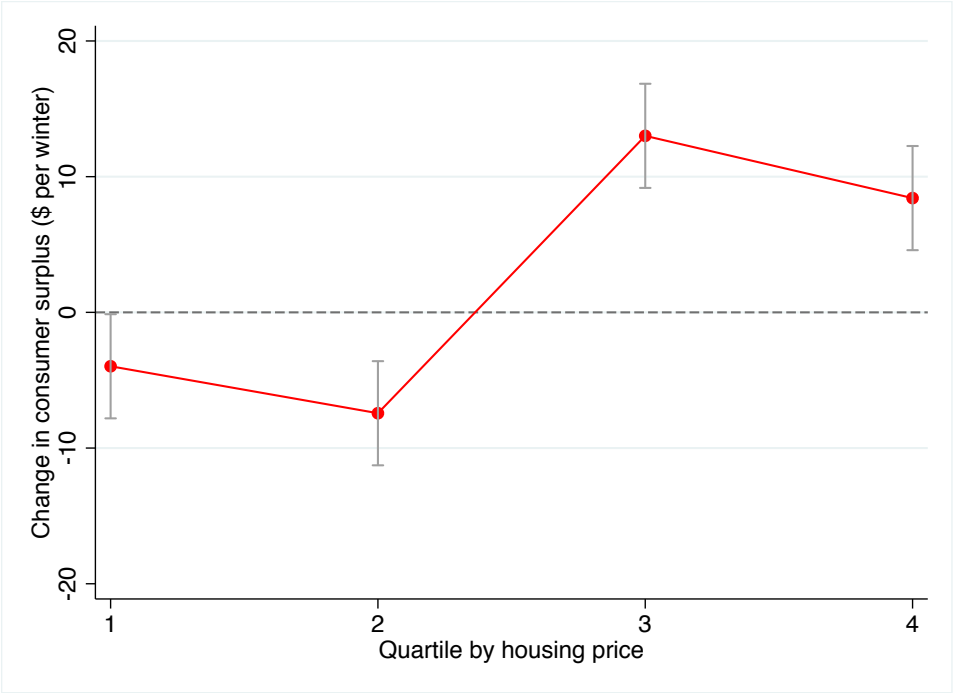
Figure 6: Welfare Implications of the CBB policy



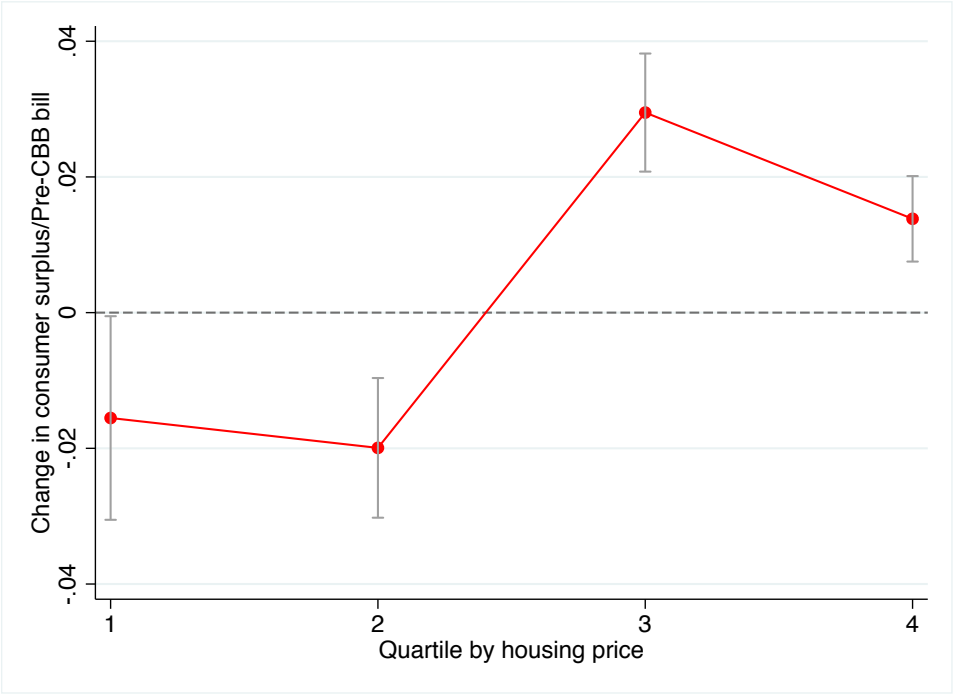
Notes: This figure shows the conceptual framework for the welfare analysis of the CBB policy. Note that the pre-CBB marginal price was zero. Triangle C indicates the change in allocative efficiency. Rectangle D is the change in environmental externalities.

Figure 7: Change in Consumer Surplus by Quartile of Housing Price

(a) Change in consumer surplus (USD per winter)



(b) Change in consumer surplus divided by pre-CBB bill (ratio)



Notes: This figure shows the effect of the CBB policy on consumer surplus by the quartiles of housing price. The bars indicate the 95 percent confidence intervals.

Table 1: Summary Statistics

	(1) 1 year post-CBB sample	(2) 3 years post-CBB sample
Daily heating usage (kWh)	94.75	97.23
	(51.98)	(51.31)
Daily usage before CBB (kWh)	100.4	105.9
	(54.97)	(55.85)
Daily usage after CBB (kWh)	92.32	94.01
	(50.45)	(49.13)
Square meter of the residence	103.8	109.2
	(41.84)	(41.42)
Housing price (dollar)	484,942	512,929
	(244,979)	(246,762)
Take-up rate of the CBB policy	69.4	67.2
(percentage)	(46.1)	(47.0)
Number of households	8,916	5,895
Observations	3,647,663	2,636,400

Table 2: Treatment Effect of the CBB Policy

	ln(daily heating usage)			
	(1)	(2)	(3)	(4)
	ITT	ITT	ATET	ATET
	1 year post	3 years post	1 year post	3 years post
First year of CBB	-0.054***	-0.060***	-0.082***	-0.093***
	(0.012)	(0.015)	(0.017)	(0.023)
Second year of CBB		-0.110***		-0.172***
		(0.030)		(0.050)
Third year of CBB		-0.102***		-0.160***
		(0.033)		(0.055)
Observations	3,647,663	2,636,400	3,647,663	2,636,400
R^2	0.38	0.39		
Day FE	Y	Y	Y	Y
Building FE	Y	Y	Y	Y

Notes: Standard errors in parentheses are clustered at the building level. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 3: ITT Estimates the CBB Policy by the Decile of Usage per Square Meter

	ln(daily heating usage)				
	1st decile	2nd decile	3rd decile	4th decile	5th decile
CBB	-0.077***	-0.061***	-0.062***	-0.060***	-0.061***
	(0.027)	(0.019)	(0.016)	(0.016)	(0.015)
Observations	398,584	363,204	368,883	361,815	354,936
R^2	0.34	0.66	0.69	0.70	0.71
CBB take-up rate	0.749	0.726	0.710	0.704	0.697
	6th decile	7th decile	8th decile	9th decile	10th decile
CBB	-0.050***	-0.046***	-0.042***	-0.045***	-0.035**
	(0.014)	(0.012)	(0.012)	(0.010)	(0.016)
Observations	375,841	366,815	360,850	365,875	330,789
R^2	0.72	0.73	0.74	0.75	0.71
CBB take-up rate	0.690	0.683	0.677	0.659	0.630
Day FE	Y	Y	Y	Y	Y
Building FE	Y	Y	Y	Y	Y

Notes: Standard errors in parentheses are clustered at the building level. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 4: ATET Estimates the CBB Policy by the Decile of Usage per Square Meter

	ln(daily heating usage)				
	1st decile	2nd decile	3rd decile	4th decile	5th decile
CBB	-0.107***	-0.086***	-0.090***	-0.090***	-0.092***
	(0.037)	(0.025)	(0.022)	(0.021)	(0.022)
Observations	398,584	363,204	368,883	361,815	354,936
	6th decile	7th decile	8th decile	9th decile	10th decile
CBB_treat	-0.079***	-0.071***	-0.066***	-0.072***	-0.058**
	(0.021)	(0.017)	(0.017)	(0.016)	(0.026)
Observations	375,841	366,815	360,850	365,875	330,789
Day FE	Y	Y	Y	Y	Y
Building FE	Y	Y	Y	Y	Y

Notes: Standard errors in parentheses are clustered at the building level. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 5: Demand Estimation and Encompassing Tests

	ln(daily heating usage)				
Panel A: 1 year after CBB					
	(1)	(2)	(3)	(4)	(5)
Marginal price	-5.892***		-5.521***		-5.462***
	(1.260)		(0.964)		(1.255)
Average price		-0.107	-0.122		
		(0.320)	(0.327)		
ln(Average price)				-0.197***	-0.008
				(0.054)	(0.077)
Arc price elasticity	-0.040		-0.038		-0.038
w.r.t. marginal price	(0.009)		(0.007)		(0.009)
Observations	3,646,965	3,646,894	3,646,894	3,646,894	3,646,894
First-Stage F-Stat	387.05	10.86	10.01	24.05	21.63
Panel B: 3 years after CBB					
	(6)	(7)	(8)	(9)	(10)
Marginal price	-8.581***		-7.772***		-7.562***
	(2.066)		(1.494)		(1.657)
Average price		0.395	-0.033		
		(1.134)	(1.035)		
ln(Average price)				-0.260***	-0.025
				(0.055)	(0.078)
Arc price elasticity	-0.059		-0.053		-0.052
w.r.t. marginal price	. (0.014)		.(0.010)		(0.012)
Observations	2,635,631	2,635,549	2,635,549	2,635,549	2,635,549
First-Stage F-Stat	326.46	8.88	7.78	18.25	18.40

Notes: Standard errors in parentheses are clustered at the building level. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 6: Welfare Implications

	Welfare gain per household per year (in dollars)		Welfare gain for Tianjin city per year (in million dollars)
	(1) based on ATET	(2) based on ITT	(3) based on ITT
Social welfare gain from allocative efficiency	15.21	9.75	1.71
Social welfare gain from externalities (lower bound)	18.56	11.90	2.09
Social welfare gain from externalities (higher bound)	22.14	14.19	2.49
Total social welfare gain (lower bound)	37.35	23.94	4.20
Percentage of total social welfare gain relative to pre-CBB payment	10.1%	6.5%	
Change in consumer surplus	16.81	11.35	1.99
Percentage change in consumer surplus relative to pre-CBB payment	4.3%	2.9%	

Notes: This table shows the welfare effects of the CBB policy in dollar per household (columns 1 and 2) and in million dollars for the city of Tianjin (column 3) For environmental externalities, we use two estimates of the external cost of coal (row 1 and 2).

Online Appendices Not For Publication

A Additional Tables

Table A.1: Rollout timing and building characteristics

	Year of the CBB start
Average condo size (square meter)	-0.008
	(0.005)
Average housing price per square meter (1,000 dollars)	1.326***
	(0.447)
Year of the building	-0.013
	(0.073)
Observations	245
R^2	0.18

Notes: Standard errors in parentheses are clustered at the complex level. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

B Additional Figures

Figure A.1: Event study in early vs late adopters

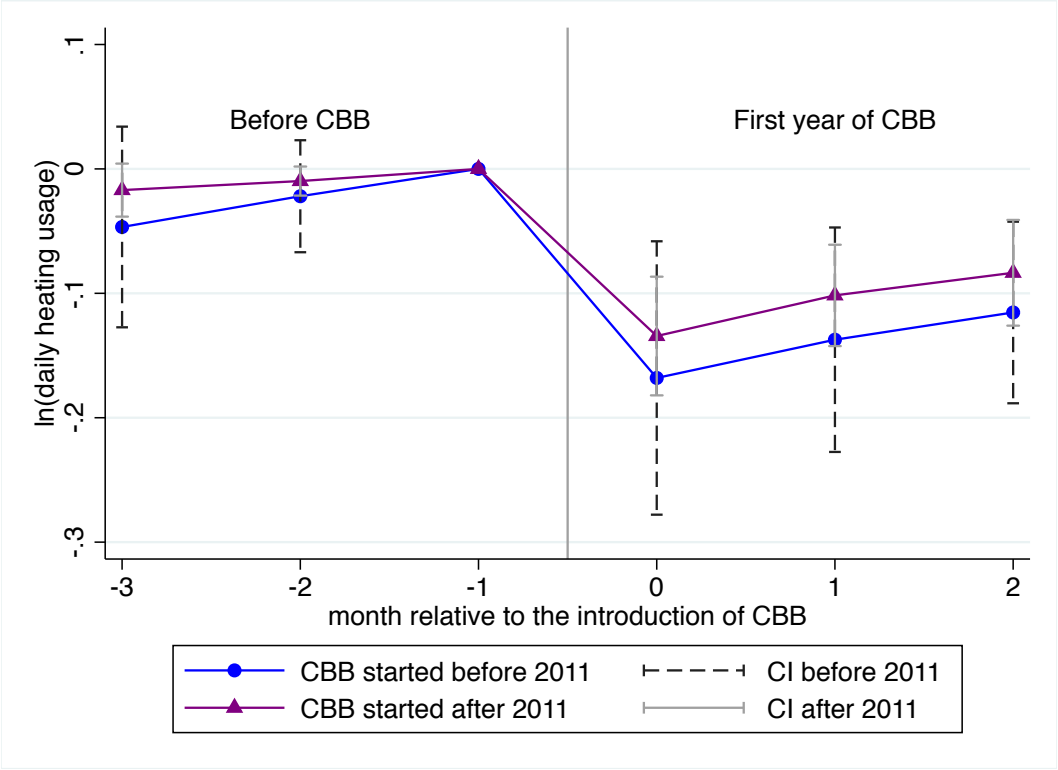
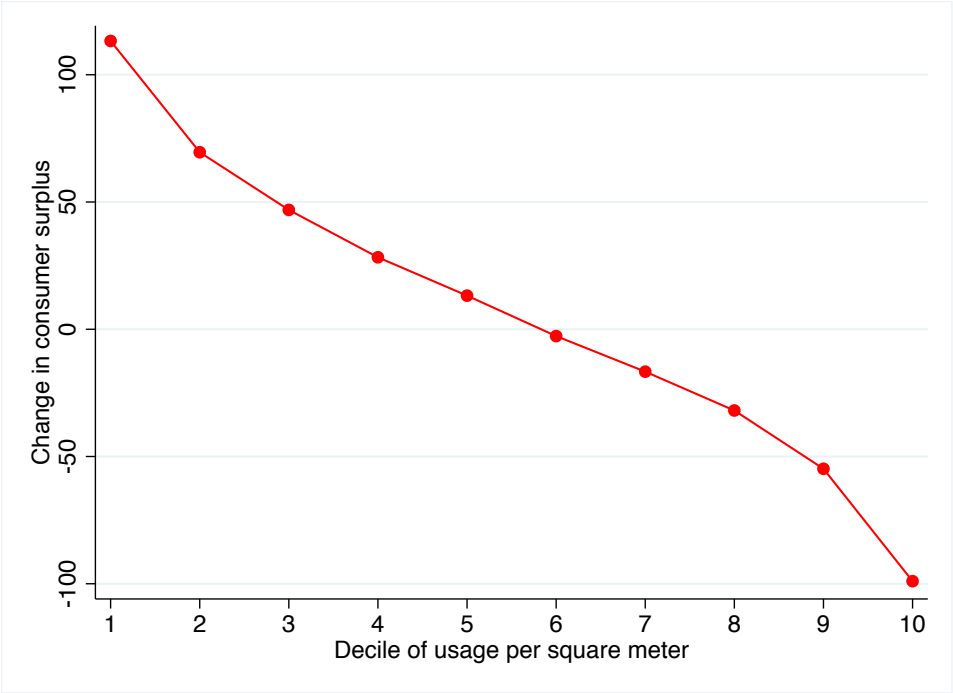


Figure A.2: Change in consumer surplus by decile of usage per square meter

(a) Change in consumer surplus



(b) Change in consumer surplus/Pre-CBB bill

