Do Consumers Distinguish Marginal Cost from Fixed Cost? Evidence from Heating Price Reform in China

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This version: February 21, 2018

Abstract

A central assumption in economics is that consumers distinguish marginal cost from fixed cost. 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 the marginal cost and the fixed cost, and staggered policy implementation by building allows us to develop an event-study research design. Using administrative records on daily household-level heating usage over ten years, we find strong evidence that consumers distinguish marginal cost from fixed cost 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.

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1 Introduction

A central assumption in economics is that consumers distinguish marginal cost from fixed cost. 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 marginal cost and fixed cost (Stiglitz, 1988). In Industrial organization, a two-part tariff—a service fee that consists of a fixed charge and a marginal charge—allows profit-maximizing firms to price-discriminate and natural monopolists to achieve allocative efficiency, under the assumption that consumers distinguish marginal cost from fixed cost (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 marginal cost from fixed cost 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 marginal cost and fixed cost 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 marginal cost from fixed cost 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 8.6 million dollars for the city of Tianjin per year and 48.8 dollars per household per year, which was about 13.2 percent of their pre-reform heating expenditure. In terms of energy conservation, three years after reform, the reform provided about a 35 percent reduction in heating usage based on our ITT estimate and a 52 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 -0.075 and -0.15, 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 marginal cost from fixed cost. 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 marginal cost from fixed cost 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 38.5 percent of households opt out of the 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 3.97 dollars times square meters. For example, for a household whose condo has 100 square meters, the payment was 397 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 397 dollars. On the other hand, the post-reform payment would be 338.5 dollars ($= 198.5 + 0.014 \cdot 10,000$) 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:

$$B_1 - B_0 \equiv (1.985 \cdot s + 0.014 \cdot y) - (3.97 \cdot s) < 0$$
$$\frac{y}{s} < 142.$$

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 the CBB introduction and observed building characteristics in Appendix Table A.1. We estimate OLS, in which the dependent variable is the year of CBB introduction and the independent variables are building characteristics. Controlling for fixed effects of the timing of meter installation, we find that year of the building, average condo size and average price per square meter are not significantly correlated with reform timing. Therefore, our event study analysis below controls for these fixed effects—more specifically, our panel data regression includes day fixed effects interacted with the timing of meter installation to allow day fixed effects to be different among customers whose meter installation timings were different.

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, for this draft, we focus on three full months: December, January and February. To construct an event study design, we focus on households who have panel data between one year before the CBB introduction and threee years after the CBB introduction. For the pre-reform period, data show that in most buildings, the consumption based billing was implemented one year after individual meters were 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 about 40% 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.

For our analysis below, we require that households have monthly panel data within the event study window. We construct two samples of household daily usage data as follows. The first sample is our main sample of analysis for reform effect in three post-reform heating seasons. We require that households have balanced data in three months before and eight months after reform. We observe data from 5,039 households in 171 buildings in this sample. In addition, to examine longer pre-reform trends in usage, we use a second, smaller sample that has balanced household-by-month data six months before and eight months after reform. We observe data from 1,958 households in 59 buildings in this sample.

Table 1 reports summary statistics of the main sample of analysis. Household heating usage is on average 100.2 kWh per day, and 12,136.2 kWh for an entire heating season. Their heating bill per heating season is on average 452.5 dollars before the reform, and 390.4 dollars after the reform. An average size condo is about 114 square meters, and it costs on average 581,632 dollars. The take-up rate of the consumption-based billing is 61.5%.

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 incomplete (the average take-up rate was 61.5 percent in our main sample). This is one-sided incomplete compliance because all households in the control group were not treated. We 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:

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

where y_{it} is the natural log of daily heating usage for household i in day t. We include household-level fixed effects (α_i) to absorb time-invariant variation at the household level. Because the year when meters are installed and data start to be observed also differs by building, we include first meter data year (s) by day (t) fixed effects (γ_{st}) to capture time shocks that potentially differ by the first meter data year.

We use $c = \{\underline{c}, ..., -1, 0, 1, .., \overline{c}\}$ 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, 3 in December 2012, and etc. For each c, we define a dummy variable D^c_{jt}, 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 $\overline{c}$ and make sure that all of the households included in the regression have data for each of $c = \{\underline{c}, ..., -1, 0, 1, .., \overline{c}\}$. 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 $c = [\underline{c}, \overline{c}]$. Failing to control for the event months outside the range of $c = [\underline{c}, \overline{c}]$ would result in biased estimates for β_c. To address this issue, we follow the approach by McCrary (2007) and include two dummy variables, $D^{\underline{c}}_{jt}$ and $D^{\overline{c}}_{jt}$, where $D^{\underline{c}}_{jt} = 1$ if $c < \underline{c}$ and $D^{\overline{c}}_{jt} = 1$ if $c > \overline{c}$. 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 our main sample that has household-by-month balanced panel data for each of c in $c = [-3, 7]$—between a year before and three years after the policy assignment. This figure provides three key results. First, there is no statistically significant pre-trend before the policy introduction. Second, the estimate for the first year is between 10 and 15 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 further examine the pre-reform trend of usage in a longer period, we use a second, smaller sample of households that have balanced panel data from two years before and three years after the policy. Appendix Figure A.1 shows event study estimates for each of c in $c = [-6, 7]$. The absence of pre-trend in the outcome variable two years before the reform provides strong evidence that the identification assumption for the event study design is likely to be valid.

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

$$y_{it} = \alpha_i + \gamma_{st} + \phi D_{jt} + \eta D^{\underline{c}}_{jt} + \rho D^{\bar{c}}_{jt} + u_{it}, \tag{2}$$

where D_{jt} is the treatment assignment dummy variable for the policy, which equals one if household i's building j was under the CBB policy in t.

In Table 2, column 1 shows the ITT results corresponding to Figure 3. The estimates show that household daily heating usage decreases by 11.7 percent in the first post-reform year, 21.3 percent in the second year, and 34.6 percent in the third year.

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^c_{it} as a dummy variable for the treatment *status* for event month c for household i. $T^c_{it} = 1$ if the household's building j falls in event month c in time t (i.e. $D^c_{jt} = 1$) 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:

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

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),

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

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}.

Column 2 in Table 2 show the ATET estimates. There is a reduction in usage by 20.7 percent in the first year, 33.3 percent in the second year, and 52.3 percent in the third year. 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 marginal cost from fixed cost. 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 marginal cost and fixed cost, 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 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 Empirical Strategy

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 $\ln y_{it}$, which is log daily heating usage for household i on day t:

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

[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 \$226 per household. The average household income in Tianjin in our sample period was \$15,041. Therefore, \$200 was about 1.5 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 0.015 percent.

where MP_{it} and AP_{it} are the marginal and average prices of heating as USD per kWh, α_i is household fixed effects, and γ_{st} is the first meter data year by day fixed effects. With the 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.

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}. If consumer behavior is more consistent with Shumeduling (Liebman and Zeckhauser, 2004), then we expect that the inclusion of MP_{it} does not affect the estimates of β_2 and that the estimate of β_1 is near zero after we control for the effect of AP_{it}.

To estimate equation (5), we need to address the endogeneity of the price variables. For the marginal price, households in our sample had a constant marginal price for given t. This situation differs from the case of a tiered marginal price schedule that 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 potential endogeneity, which comes from the incomplete compliance of the policy. If the self-selection into the compliance is correlated with the error term in equation (5), the OLS estimates would be inconsistent.

To address this concern, we use the policy-induced variation in marginal price (MP_{it}^{PI}) as an instrument for the marginal price. MP_{it}^{PI} takes zero for the pre-CBB period and 0.014 USD for the post-CBB period regardless of customer i's compliance status. Therefore, this instrument is not subject to the potential endogeneity due to the incomplete compliance. The identification assumption is equivalent to the one required for the event study analysis in the previous section—given the control variables in the regression, the roll-out timings of the CBB policy had to be exogenous to customers.

We make a similar instrument for the average price, although a more detailed discussion is required for the construction of the instrument for average price. As Figure 1 suggests, the policy-induced variation in average price is likely to be different among customers who have different levels of usage. Households with lower annual usage per square meter were likely to experience a decrease in average price while those with higher annual usage per square meter were likely to have an increase in average price. To capture this price variation, we construct an instrument that is commonly used in the literature of nonlinear income taxation and nonlinear pricing in electricity (Saez, 2003; Saez et al., 2012; Borenstein, 2009; Ito, 2014). We denote customer i's average daily usage per square meter in the first month of the dataset by $\widetilde{y_i}$. We calculate the policy-induced, predicted average price AP_{it}^{PI} using household i's $\widetilde{y_i}$ with the price schedule at time t (i.e. marginal price and fixed costs at t).

This instrument is called a simulated instrument or policy-induced variation in price in the nonlinear taxation/pricing literature (Chetty et al., 2011; Saez et al., 2012). The advantage of this instrument is that it provides a strong first stage because a consumer's past usage is a strong predictor for the customer's future usage. If the price schedule itself is exogenous to the consumer (either because of a random assignment or quasi-random assignment of the price schedule), this instrument could work well to address the endogeneity between usage and price under nonlinear pricing.

However, Saez et al. (2012) and Ito (2014) emphasize that researchers have to be careful to address the mean reversion problem of usage data because it is likely to result in a violation of the instrument's exclusion restriction assumption. In general, economic data—such as energy usage and income earnings—show strong mean reversion. That is, customers with low $\widetilde{y_i}$ are likely to increase their usage in other periods, while whose with high $\widetilde{y_i}$ are likely to decrease their usage. This phenomenon is unrelated to their responses to prices. However, if we construct an instrument based on $\widetilde{y_i}$, this mean reversion—which is implicitly in the error term of the demand estimation—and the instrument are likely to have systematic correlation, leading to the violation of the exclusion restriction assumption.

To address this concern, we take a nonparametric control approach similar to the one in Ito (2014). Essentially, if there is a valid control group in a research design, one can include flexible controls for mean reversion, as long as the mean reversion is not systematically different between treatment and control groups. We divide customers into percentile groups ($k = 1, ..., 100$)) based on their average daily usage per square meter in the baseline month ($\widetilde{y_i}$). Then, we allow the day fixed effects (γ_t) to be different among the percentile groups. These flexible day fixed effects (γ_{kt})

controls for the mean reversion of usage in a nonparametric way. Usually, such flexible control variables destroy identification since they fully absorb the variation of typical instruments (Saez et al., 2012). However, in our research design, these controls do not fully absorb the variation in the instruments. This is because there are both treatment and control households in k group at time t in our event study research design.

We estimate the following equation by 2SLS:

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

using instruments MP^{PI}_{it} and AP^{PI}_{it} for MP_{it} and AP_{it}. For our main results, we estimate the most restrictive specification to control for the correlation between the instruments and the mean reversion of usage, by including the baseline percentile (k) by first meter data year (s) by day (t) fixed effects (γ_{kst}). These fixed effects allow the day fixed effects to be different among s groups as well as k groups.

5.3 Graphical Illustration of the Encompassing Test

Before we show the estimation results for the 2SLS, we use graphical illustration to explain the intuition behind the empirical strategy. Consider that we divide households to quartiles based on their first month average usage per square meter ($\widetilde{y}_i$). The lowest quartile group is a set of consumers whose average daily usage per square meter in the first month was low. Figure 1 suggests that these customers were likely to experience a *decrease* in average price when the CBB policy was introduced. Likewise, customers in the highest quartile were likely to see an *increase* in average price due to the CBB policy.

In Figure 4, we show that this is indeed the case with our data. For each quartile group, we use the event study regression presented in the previous section to estimate the ITT of the CBB policy on various outcome variables such as the average price, the policy-induced variation in average price ("Average Price (IV)" in the figure), and the log of daily heating usage. The estimates for the average price indicates that customers in the lower quartile group experienced a decrease in average price, and those in the higher quartile groups experienced an increase in average price. Note that this "actual average price" can be affected by the two potential endogeneity problems—1) the incomplete compliance of the policy and 2) the simultaneity between usage and average price—as we discussed in the previous section. We therefore use the instrument AP_{it}^{PI}. Similar to the previous studies that use this instrument (Chetty et al., 2011; Saez et al., 2012; Ito, 2014), the instrument has a strong first stage to predict the actual average price, which makes the two lines in the figure ("Average Price" and "Average Price (IV)" quite similar.

The policy-induced variation in average price in the figure indicates that a simple statistical test can be used to test if the response to the CBB policy is more consistent with standard economic theory or the alternative prediction based on "shmeduling." The figure shows that the first and second quartile groups experienced a decrease in average price and an increase in marginal price. The third and fourth quartile groups had little change in average price and an increase in marginal price. Therefore, the shmeduling model predicts an increase in usage for the first and second quartile groups and little change in usage for the third and fourth quartile groups.

Therefore, a simple statistical test would be to investigate the effect of the CBB on usage for each quartile group by running the event study regression for each group separately. Panel A of Figure 3 shows the ITT estimates of the event study regressions by quartile group. We find that all of the four groups reduced usage similarly in response to the CBB policy. In Panel B of Figure 3, we show the regression results for the ATET. Estimated ATET effects are larger in higher quartile groups, which could be explained by higher take-up rates in higher quartile groups. The table indicates that the CBB induced a long-run reduction in usage for all groups. We strongly reject the null hypothesis that the change in usage is positive because this statistical test is equivalent to one-sided t-test for the coefficient in the table—the p-value for this test is therefore a half of the p-value in the two-sided t-test.

Our findings are inconsistent with the prediction by the shmeduling model and more consistent with the prediction by an assumption behind standard economic theory. Although many consumers experienced a decrease in average price, they reduced usage in response to the increase in marginal price. To make this point clear, we also plot the ITT estimate of the usage response by quartile of baseline usage per square meter with the 95 percent confidence intervals in Figure 5. The reduction in usage in the first and second quartiles cannot be explained by the change in average price unless consumers had a positive price elasticity of demand with respect to average price.

5.4 Encompassing Test Results

Table 4 shows results of the encompassing test. Column 1 includes only the marginal price. The estimate implies that the semi-elasticity with respect to marginal price is -10.72. 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 10.72 percent. In column 2, 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.

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 marginal cost and fixed cost.

5.5 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 4. The medium-long-run elasticity, which is calculated based on households who experienced the CBB for three years—is -0.075. Note that the elasticity would be doubled if we calculate a point elasticity at the post-policy marginal price $p = 0.014$. At this price level, the medium-long-run point elasticity is -0.15. These price elasticity estimates—ranging between -0.075 to -0.15—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 marginal cost from fixed cost 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 5 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 34.55 dollars per household with the ATET estimate and 21.97 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 42.15 to 50.28 based on the ATET estimates, and 26.80 to 31.97 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 76.7 dollars per household with the ATET estimate and 48.77 dollars with the ITT estimate. These numbers are 20.7 or 13.2 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 8.55 million dollars per year.

Finally, we show the mean change in consumer surplus. On average, the consumer surplus was increased by 36.15 dollars (with the ATET) or 23.57 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 the marginal cost and the fixed cost, 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 marginal cost from fixed cost 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.

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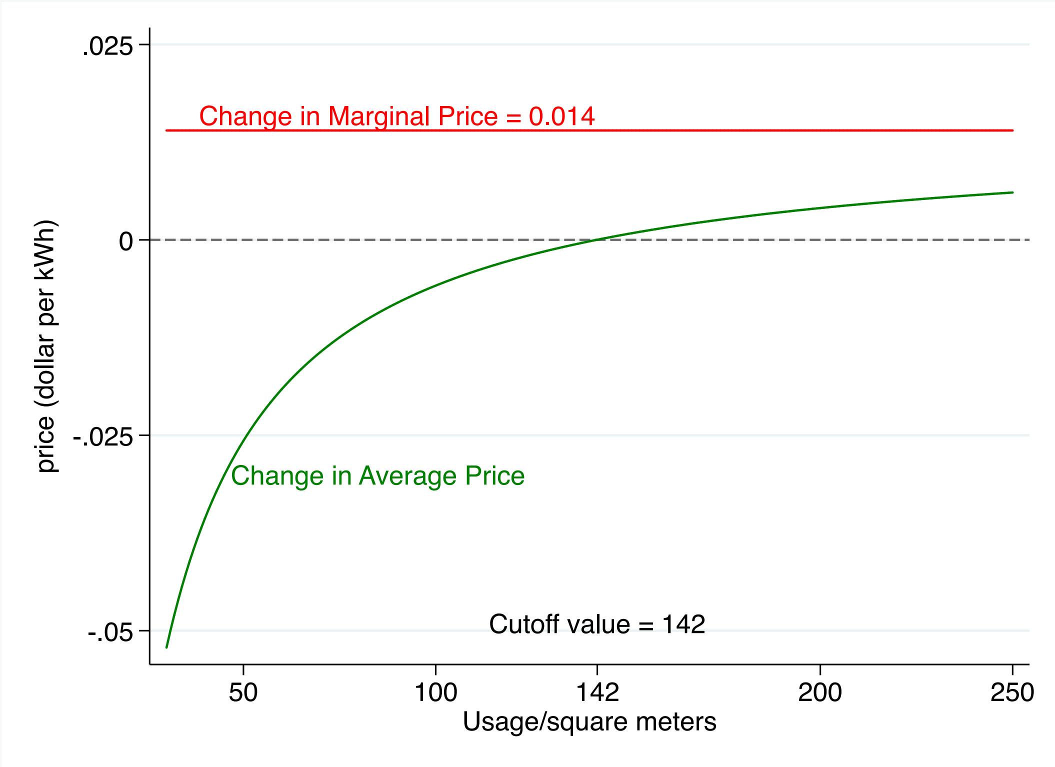
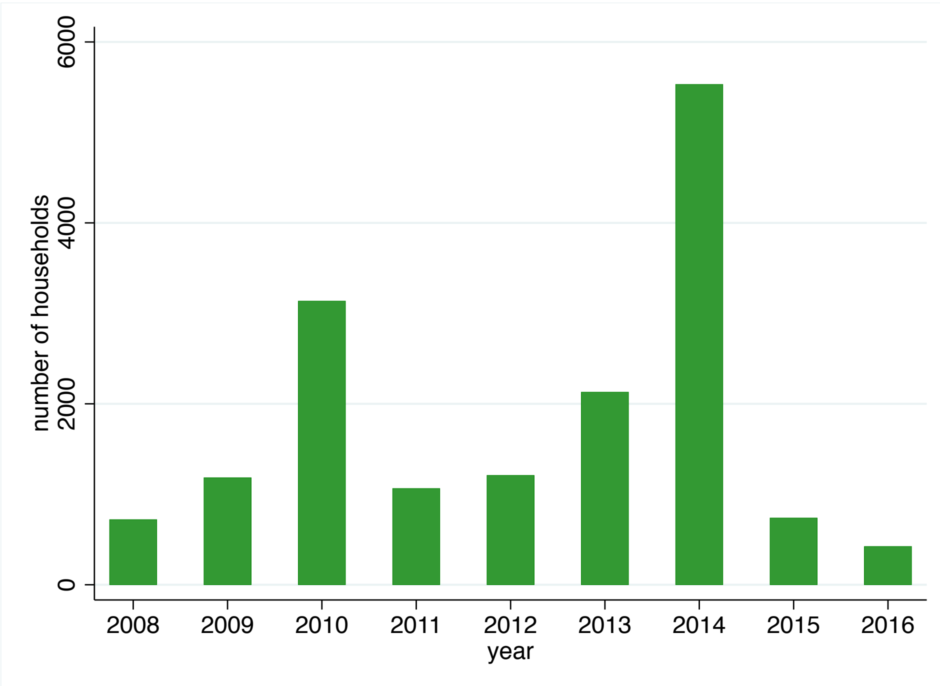
Figure 1: Policy-Induced Changes in Marginal and Average Prices



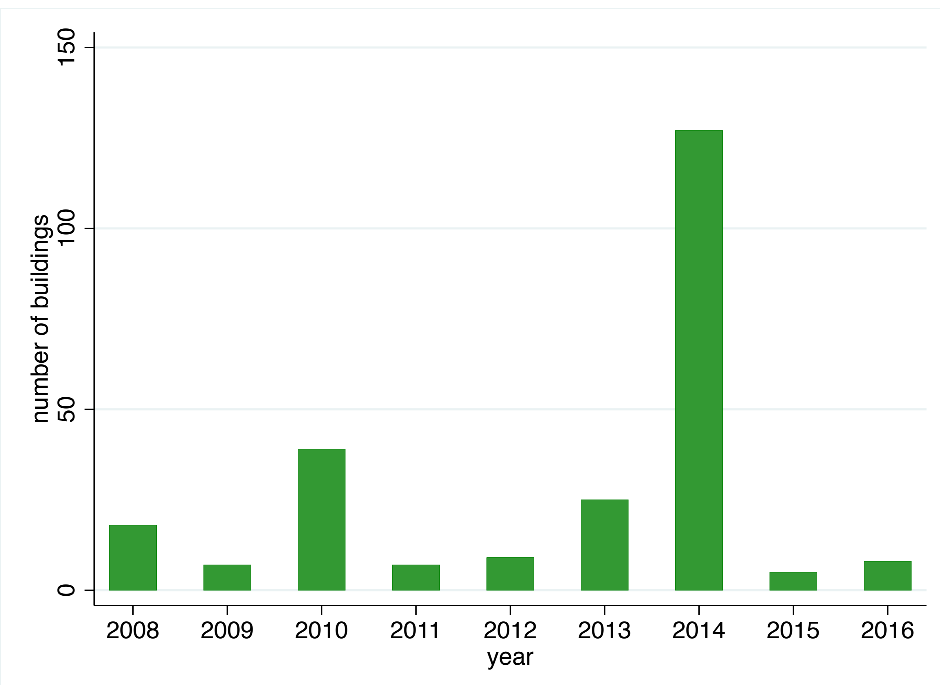
Notes: This figure shows the changes in marginal price and average price induced by the introduction of the consumption-based billing policy.

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

(a) Number of households

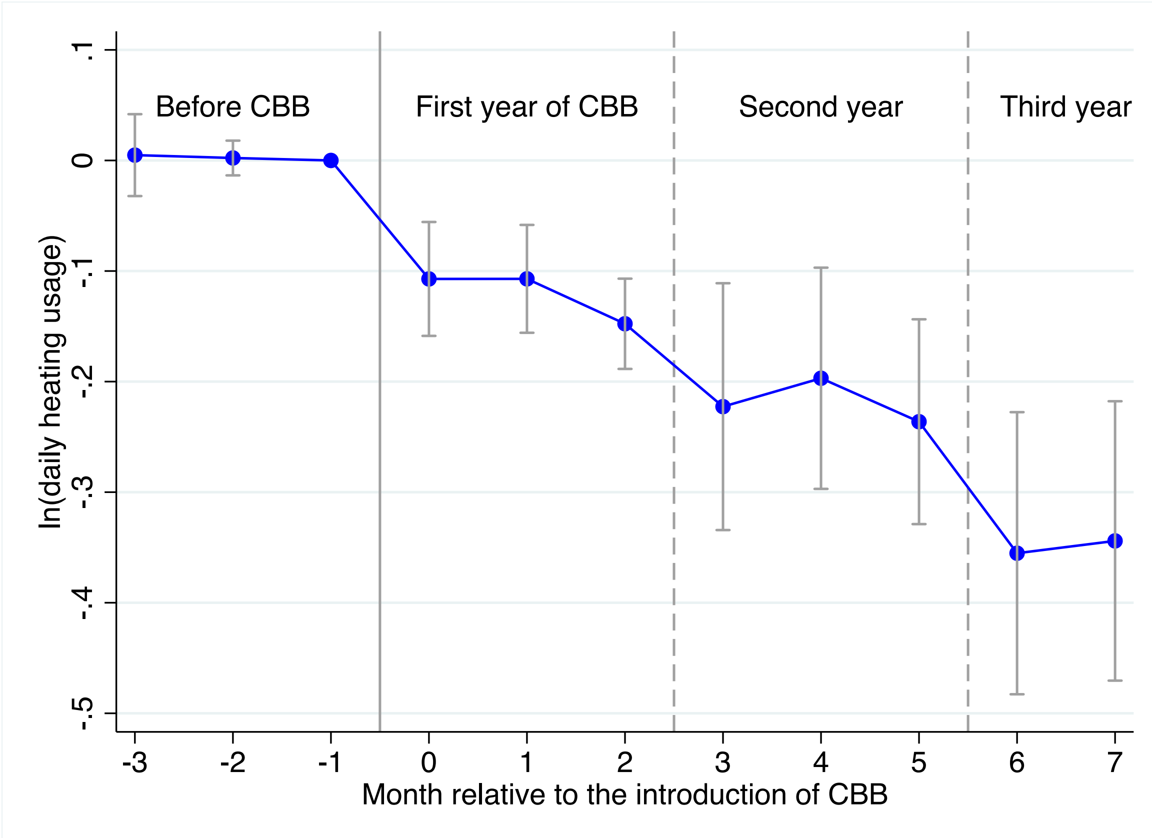


(b) Number of buildings



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

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



Notes: The bars indicate the 95 percent confidence intervals.

Figure 4: Policy-Induced Changes in Marginal Price, Average Price, and Usage

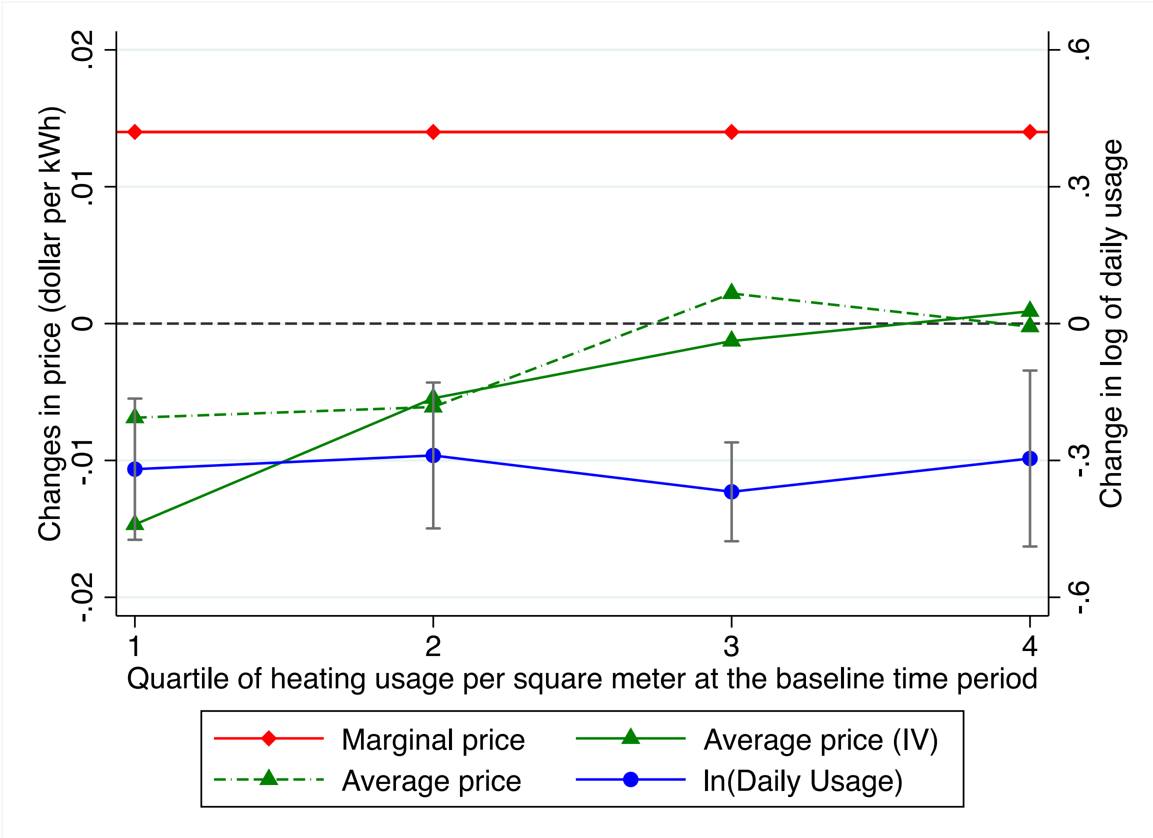
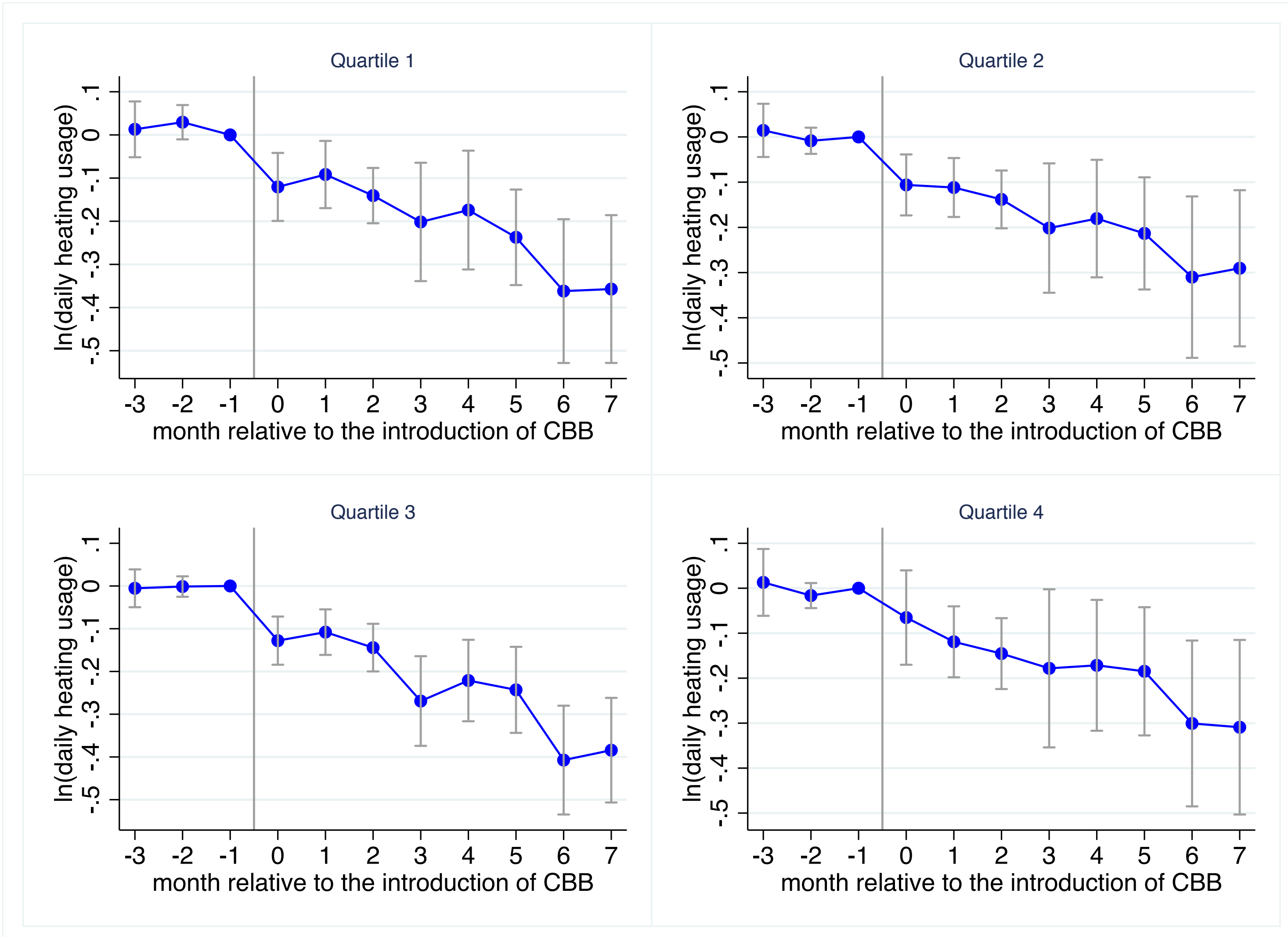
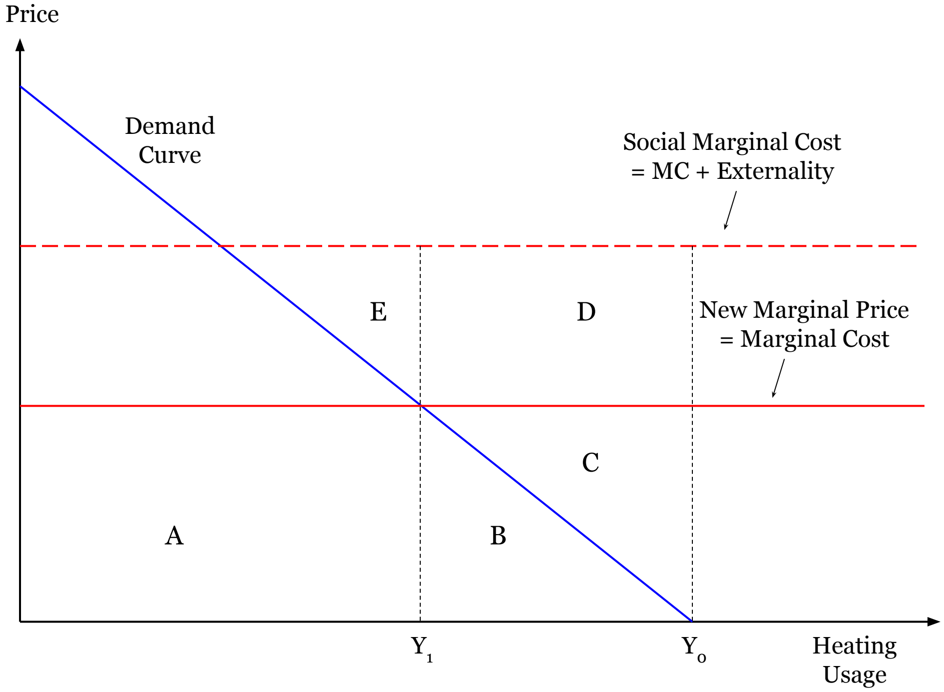


Figure 5: Event-Study Analysis (ITT) by the Quartile of Usage per Square Meter in the Baseline Year



Notes: We use the usage per square meter in the baseline year (first heating season when data are observed) to make quartile groups.

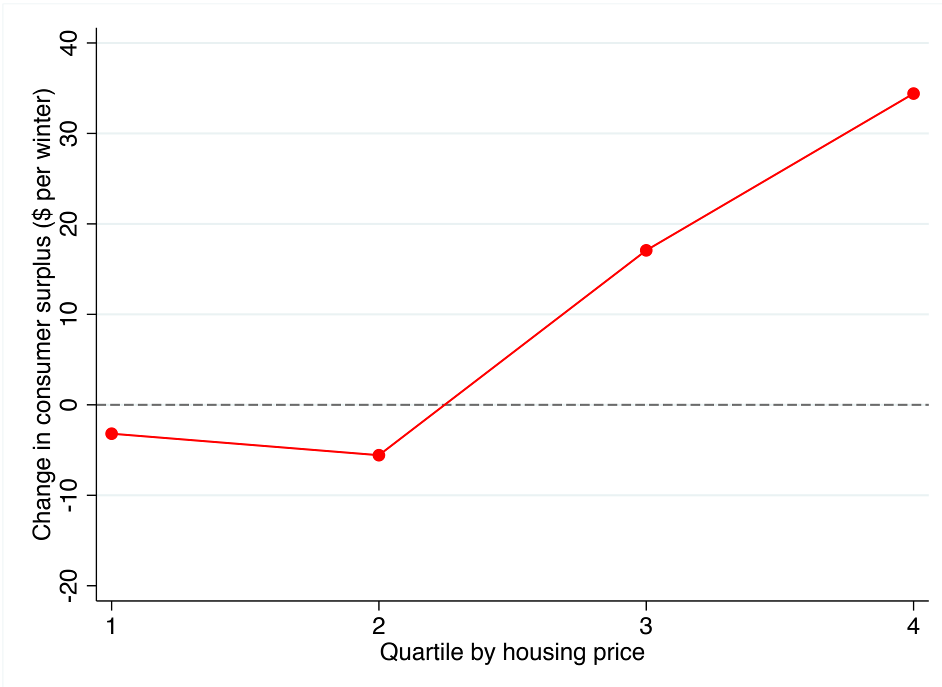
Figure 6: Welfare Implications of the CBB policy



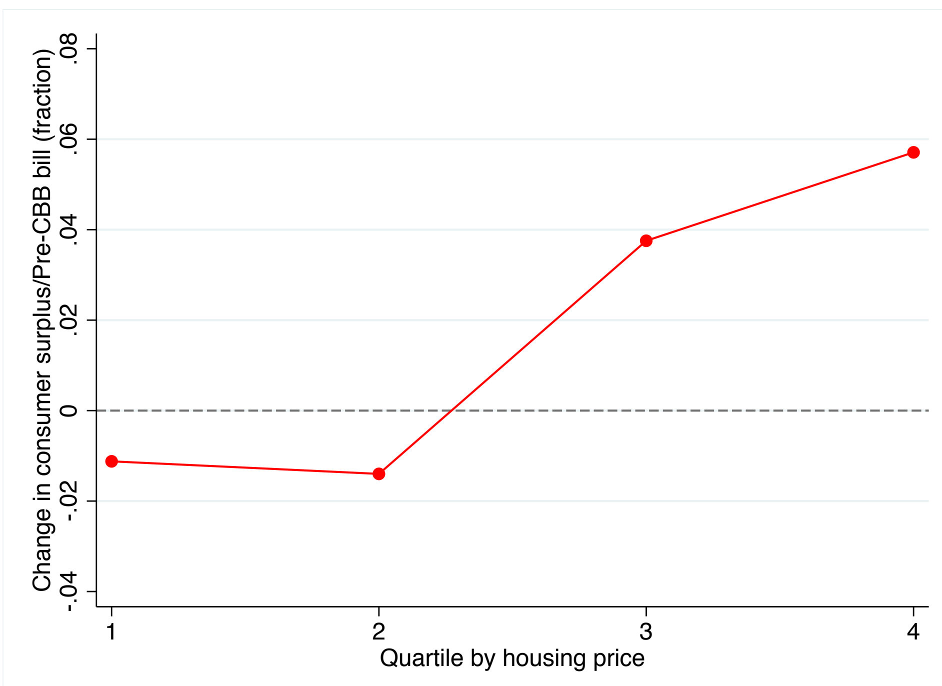
Notes: This figure shows the conceptual framework for the welfare analysis of the CBB policy. Note that the pre-CBB marginal price was zero. Triangle C indicates the change in allocative efficiency. Rectangle D is the change in environmental externalities. Note that another welfare change that is not included in the figure is a change in fixed cost, which we include in our welfare calculation in Figure 7 and Table 5.

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

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



(b) Change in consumer surplus as a fraction of pre-CBB bill per winter



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

Table 1: Summary Statistics

Daily heating usage (kWh)	100.2
	(50.81)
Total heating usage per heating season (kWh)	12,136.2
	(5,984.3)
Heating bill per heating season before reform (dollar)	452.5
	(154.6)
Heating bill per heating season after reform (dollar)	390.4
	(132.2)
Square meter of the residence	114.0
	(38.94)
Housing price (dollar)	581,632
	(258,824)
Take-up rate of the CBB policy	0.615
	(0.487)
Number of households	5,039
Observations	2,345,573

Table 2: Treatment Effect of the CBB Policy

	ln(daily heating usage)	
	(1)	(2)
	ITT	ATET
First year of CBB	-0.117***	-0.207***
	(0.022)	(0.035)
Second year of CBB	-0.213***	-0.333***
	(0.053)	(0.081)
Third year of CBB	-0.346***	-0.523***
	(0.066)	(0.101)
Observations	2,345,573	2,345,573
R^2	0.65	
First-Stage F-Stat		103.69
Day*First data year FE	Y	Y
Household FE	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: Effect of the CBB Policy by the Quartile of Usage per Square Meter

	ln(daily heating usage)			
	1st quartile	2nd quartile	3rd quartile	4th quartile
Panel A: ITT				
First year of CBB	-0.107***	-0.103***	-0.116***	-0.094**
	(0.031)	(0.028)	(0.023)	(0.040)
Second year of CBB	-0.166***	-0.190***	-0.229***	-0.165**
	(0.060)	(0.061)	(0.047)	(0.077)
Third year of CBB	-0.319***	-0.289***	-0.369***	-0.296***
	(0.079)	(0.082)	(0.055)	(0.098)
Observations	509,008	564,974	625,099	602,494
R^2	0.65	0.69	0.71	0.71
Panel B: ATET				
First year of CBB	-0.181***	-0.166***	-0.208***	-0.205**
	(0.049)	(0.046)	(0.047)	(0.080)
Second year of CBB	-0.238***	-0.259***	-0.387***	-0.313**
	(0.084)	(0.087)	(0.081)	(0.129)
Third year of CBB	-0.444***	-0.367***	-0.623***	-0.538***
	(0.104)	(0.106)	(0.095)	(0.151)
Observations	509,008	564,974	625,099	602,494
First-Stage F-Stat	70.43	62.76	36.87	28.32
CBB take-up rate	0.486	0.548	0.680	0.743
Day*First data year FE	Y	Y	Y	Y
Household FE	Y	Y	Y	Y

Notes: We use the usage per square meter in the baseline year (first heating season when data are observed) to make quartile groups. Standard errors in parentheses are clustered at the building level. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 4: Demand Estimation and Encompassing Tests

	ln(daily heating usage)	
	(1)	(2)
Marginal price	-10.724***	-10.330***
	(1.853)	(1.907)
Average price		0.946
		(0.626)
Arc price elasticity	-0.075	-0.072
w.r.t. marginal price	(0.013)	(0.013)
Observations	2,301,575	2,301,575
First-Stage F-Stat	1199.92	69.32
Day*First data year*Pct FE	Y	Y
Household FE	Y	Y

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

Table 5: 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 (C in Figure 6)	34.55	21.97	3.85
Social welfare gain from externalities (D in Figure 6): lower bound	42.15	26.80	4.70
Social welfare gain from externalities (D in Figure 6): higher bound	50.28	31.97	5.61
Total social welfare gain (C+D in Figure 6): lower bound	76.7	48.77	8.55
Percentage of total social welfare gain relative to pre-CBB payment	+20.7%	+13.2%	
Change in consumer surplus (A+B in Figure 6 + change in fixed cost)	36.15	23.57	4.13
Percentage change in consumer surplus relative to pre-CBB payment	+9.2%	+6.0%	

Notes: This table shows the total welfare effects of the CBB policy after reform 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

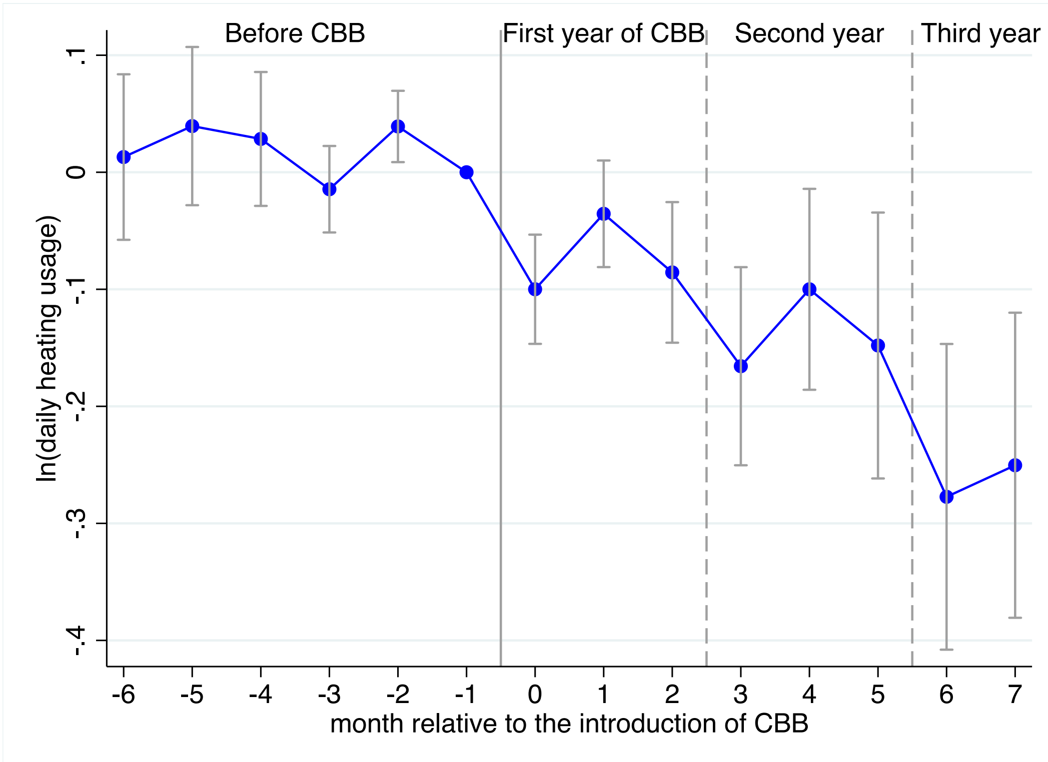
Table A.1: Rollout timing and building characteristics

	Year of the CBB start
Year of the building	0.008
	(0.029)
Average condo size (square meter)	-0.001
	(0.002)
Average price per square meter (1,000 dollars)	0.028
	(0.036)
First meter data year FE	Y
Observations	171
R^2	0.84

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 the subsample with two year pre-reform data



Notes: This subsample has 1958 households in 59 buildings. The bars indicate the 95 percent confidence intervals.