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Do Consumers Distinguish Fixed Cost from Variable Cost? "Schmeduling" in Two-Part Tariffs in Energy†

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A central assumption in economics is that consumers properly distinguish fixed cost from variable cost. This assumption is fundamental to various economic theories, including optimal taxation, redistribution, and price discrimination. Using a quasi-experiment in heating price reform in China, we find empirical evidence that is inconsistent with this conventional assumption and more consistent with the "schmeduling" model in Liebman and Zeckhauser (2004). As we demonstrate its policy implications for two-part energy tariffs, this schmeduling behavior makes fixed costs directly relevant to the perceived relative prices of goods, and therefore alters the welfare implications of price, tax, and subsidy designs. (*JEL* D12, D91, H24, L94, O12, P28, P36)

A central assumption in economics is that individuals properly distinguish between fixed cost and variable cost. In public finance, a lump-sum tax or subsidy is considered to be nondistortionary because it does not distort the relative prices of goods as long as taxpayers distinguish variable cost from fixed cost (Stiglitz 1986). In industrial organization, a two-part tariff—a price schedule with a fixed charge and a variable charge—allows profit-maximizing firms to price-discriminate and natural monopolies to achieve allocative efficiency under the assumption that consumers distinguish variable cost from fixed cost (Tirole 1988).

In this paper, we provide empirical evidence that this assumption may not be consistent with data on consumer behavior. Despite the fact that this assumption is fundamental to many theoretical models and empirical studies in economics, there is limited direct empirical evidence on this question. The closest literature is studies on tiered marginal price schedules, in which individuals face multiple marginal prices or taxes for the same good. In this context, prior studies find evidence that consumers and taxpayers tend to respond to average price rather than marginal price (de Bartolome 1995; Borenstein 2009; Kahn and Wolak 2013; Ito 2014; Rees-Jones

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and Taubinsky 2020). However, in studying a two-part tariff, Borenstein and Davis (2012) clarify that the evidence from the literature on tiered pricing cannot demonstrate whether consumers distinguish variable cost from fixed cost. This is because differentiating between a fixed cost and a single variable cost can be much less complex than identifying a correct marginal price from tiered marginal pricing that involves multiple variable prices in a price schedule.

To empirically test this assumption, we use a quasi-experiment in a recent heating price reform in China. Until recently, most Chinese households paid only fixed charges for their heating consumption. That is, their heating expenses did not depend on their usage. Starting in 2005, in collaboration with the World Bank, the Chinese Ministry of Housing and Urban-Rural Development (MOHURD) introduced a new pricing system called consumption-based billing (CBB)—a two-part tariff with a much lower annual fixed charge and a price per unit of consumption.

We exploit three unique features of this reform to test our research question. First, the policy induced an increase in variable cost but a decrease in fixed cost. With this price variation, many consumers experienced an *increase* in marginal price but a *decrease* in average price. Standard theory predicts that these consumers would reduce their heat usage because the marginal price of heating had increased. However, an alternative theory, originating with the theory of "schmeduling" by Liebman and Zeckhauser (2004), contends that consumers may misperceive the average price as the true marginal price. In this case, consumers may *increase* their heating usage even though their marginal price has increased. We exploit this price variation to develop a simple nonparametric test of these competing theories of consumer behavior.

Second, in collaboration with the World Bank, MOHURD, and a regulated utility company, we obtained newly available administrative data on daily heating usage at the household level from 2007 to 2019 in Tianjin, a city in northeastern China. Our data address a key empirical challenge that is common in the literature. Usually, individually metered usage data are available only *after* the introduction of metered pricing because firms tend to install meters at the same time that they introduce metered pricing. This makes empirical analysis challenging because individual-level usage data are unobserved before the policy change. Our data overcome this challenge because regulators required household-level metered data to be collected for at least one year before the introduction of metered pricing. This allows us access to daily household-level usage data both *before* and *after* the reform.

Third, the CBB reform had a staggered rollout. Using this quasi-experimental variation in treatment timing, we estimate the causal effects of the reform using the staggered difference-in-differences (DID) method. Although the validity of the identification assumptions is untestable, we show that the timing of treatment is uncorrelated with observables and that our event-study figures support parallel trends between treated and untreated households in the pretreatment periods. Following the recent econometric literature on the estimation of staggered DID, we implement an estimation method that allows heterogeneous treatment effects across households using the estimation method developed by de Chaisemartin and D'Haultfœuille (2020).

We begin by estimating the reform's overall impact on heating usage. The intention-to-treat (ITT) estimates indicate that the reform decreased heating usage on average by 10.1 percent in the first year, 10.7 percent in the second year, and 8.7 percent in the third year. These impacts are economically substantial and long-lasting compared with a variety of policies on residential energy usage studied in the literature (Wolak 2011; Ito 2014, 2015; Ito, Ida, and Tanaka 2018; Deryugina, MacKay, and Reif 2020; Shaffer 2020).

We show that this overall reduction in usage can be interpreted as an improvement in social welfare if we consider the standard theoretical framework of two-part tariffs. Before the reform, the marginal price was zero. After the reform, it was set to the private marginal cost of heating production. Therefore, if we assume that consumers distinguish between fixed and variable costs, this usage reduction improves allocative efficiency. If we also consider the environmental externality from coal-based heat generation, the social marginal cost exceeds the private marginal cost. In this case, the overall welfare gain from the reform is even larger.

However, this standard framework is not applicable if consumers do not respond to the change in marginal price by properly distinguishing fixed and variable costs. We show that the welfare impact of the reform is ambiguous if consumers respond to the average price of the bill, rather than the marginal price. Both the gains from improving allocative efficiency and lowering environmental externalities are likely to be smaller than those calculated in the standard framework, and therefore, the overall social welfare impact from the reform could be ambiguous in theory.

To investigate the social welfare impact, we empirically test whether consumers distinguish fixed cost from variable cost. As described above, many consumers experienced an increase in marginal price but a decrease in average price. For those who had a policy-induced decrease in average price, we find that the reform caused a statistically and economically significant *increase* in heating usage—even though their marginal price increased. In addition, the reform made some consumers experience an increase in marginal price, with nearly zero change in average price. We find that these consumers had nearly no change in usage—even though the reform increased their marginal price.

The set of our empirical findings suggest that consumer behavior is more consistent with the schmeduling model than with standard theory. However, there are at least three alternative mechanisms that could explain this consumer behavior: income effects, category budgeting, and spurious correlations. We explore these possibilities in Section V. First, we show that the income effect in our setting is very small and unlikely to explain our empirical findings.[1] Second, we examine whether our empirical results could be explained by categorical budgeting. We exploit the fact that consumers could opt out from the new pricing and show that the opt-out decision is inconsistent with the prediction from the category budgeting

[1] The income effect of the CBB policy, if any, was likely to be very small. The CBB reduced the annual fixed charge by approximately \$208 per household. The average household income in Tianjin, China (the city featured in this study), in our sample period was \$15,041. Therefore, the change in fixed costs was approximately 1.38 percent of household income. In the literature on residential energy demand, short-run income elasticity is found to be fairly inelastic, with estimates averaging around 0.239 based on a recent meta analysis (Zhu and Yang 2018). This implies that the income effect of CBB on heating usage is 0.33 percent. See Section VA for a more detailed discussion.

model in Hastings and Shapiro (2013). We also examine whether the potential "within-category income effect" implied by a model in Farhi and Gabaix (2020) could explain our results. Our analyses suggest that it is challenging to explain our empirical results based on the within-category income effect unless there are large and particular patterns of unobserved heterogeneity in income and price elasticity among households. Third, we explore whether our findings are driven by spurious correlations between household characteristics—and our analysis suggests that this is unlikely.

Finally, we calculate the welfare impact of the reform based on our empirical findings. We obtain an estimate for the environmental externality by using the estimated willingness to pay for clean air in Ito and Zhang (2020) and ambient air pollution data. We first calculate welfare impacts based on the standard theoretical framework of two-part tariffs, which assumes that consumers distinguish fixed cost from variable cost. In this framework, the total social welfare gain is US$18.4 per year per household, which adds up to US$78.7 million per year for Tianjin. The one-time administrative cost of the reform—including installing metering—was US$99 per household. This implies that if we conduct a cost-benefit analysis following standard economic theory, the net present value of the policy's benefits would exceeded its cost within 6 years of the reform with a discount rate of 3 percent.

In contrast, we show that the benefit of the reform is much smaller when we incorporate schmeduling behavior. In our second welfare calculation, we incorporate the empirical finding that consumers may not properly distinguish between fixed and variable costs. In this case, the total social welfare gain is US$2.8 per year per household and US$12.2 million per year for Tianjin. This implies that the CBB reform is unlikely to be cost-effective for a reasonable range of discount rates, opposite to what one would expect with the conventionally assumed behavior in the standard framework. These results imply that consumers' schmeduling behavior could substantially alter the welfare implications of two-part tariffs.

Related Literature and Our Contributions.—This paper provides three primary contributions to the economics literature and the design of economic policy. First, our findings provide a new insight to the literature on consumer inattention under complex pricing (Busse, Silva-Risso, and Zettelmeyer 2006; Gabaix and Laibson 2006; Chetty, Looney, and Kroft 2009; Finkelstein 2009; Brown, Hossain, and Morgan 2010; Hastings and Shapiro 2013; Ito 2014; Feldman, Katuščák, and Kawano 2016; Rees-Jones and Taubinsky 2020). Our results suggest that a central assumption in economics—that consumers properly distinguish fixed cost from variable cost—may not be consistent with consumer behavior in reality. This finding is consistent with evidence in Feldman, Katuščák, and Kawano (2016), which finds that US taxpayers misinterpret at least part of a lump-sum tax liability change as an increase in their marginal tax rate. In the welfare analysis, we show how this consumer behavior critically changes the welfare implications of two-part tariffs. It also suggests that this behavior could alter key conclusions of many fundamental economic models, including those of optimal taxation, redistribution, natural monopolies, and price discrimination (Stiglitz 1986; Tirole 1988). This is because if consumers do not properly distinguish fixed cost from variable cost, a fixed payment or subsidy could affect the relative prices of goods, and therefore have a direct impact on the efficiency of price, tax, or subsidy design. Our welfare analysis shows that this implication is empirically substantial and policy-relevant.

Second, our results have important policy implications for energy and climate policy across the globe because many energy policies involve a combination of fixed and variable incentives in practice. As we show in Section VI, the introduction of metered energy pricing could have different welfare implications if consumers do not properly distinguish fixed cost from variable cost. Another relevant policy is the compensation scheme of carbon pricing in climate change policy. When considering introducing carbon pricing, many governments—including the US federal government—propose monetary compensation to citizens who would be negatively impacted by carbon pricing.[2] Policymakers usually propose a lump-sum credit on energy bills, hoping that a fixed credit would not distort the marginal incentive to conserve energy. However, if customers do not distinguish fixed cost from variable cost, a fixed credit on energy bills may still discourage conservation and defeat the purpose of carbon pricing.[3]

Finally, we provide one of the first pieces of empirical evidence on long-run responses to energy prices in developing countries. In the coming decades, most of the increase in global energy demand will come from developing countries (Wolfram, Shelef, and Gertler 2012). Understanding how to design energy pricing in these countries is therefore a first-order priority for addressing climate change and global scarcity in natural resources. However, the energy demand literature has focused on developed nations because of the availability of administrative billing data.[4] Moreover, nearly all existing studies focus on estimating short-run demand elasticity because long-run exogenous variation in energy prices is rarely available.[5] We use administrative billing data in China and a quasi-experimental design to estimate three-year responses to long-run price variation. Our findings suggest that the impact of inefficient energy pricing schemes is likely to be substantial in developing countries, and therefore it is important to conduct rigorous studies in these settings.[6]

[2] An example includes the compensation scheme proposed in the American Clean Energy and Security Act of 2009, described on page 901 of US Congress (2009).

[3] Rivers and Shaffer (2022) empirically investigate this question by studying carbon tax rebates in British Columbia in Canada, and find that consumers do not spend rebate income in the same way as "normal" income. Burtraw (2009) and Burtraw, Walls, and Blonz (2010) also note that distributing a fixed credit may not work as desired if residential customers do not pay attention to the difference between their marginal price of electricity and their total electricity bill.

[4] For example, see Borenstein (2012); Aroonruengsawat and Auffhammer (2011); Wolak (2011); Ito (2014); Jessoe and Rapson (2014); Ito (2015); Ito, Ida, and Tanaka (2018); Deryugina, MacKay, and Reif (2020); and Shaffer (2020) for studies based on administrative energy billing data in the United States, Japan, and Canada. Recently, researchers have started to collect such data in developing countries: Mexico (Davis, Fuchs, and Gertler, 2014), South Africa (Jack and Smith 2015, 2020), Colombia (McRae 2015, 2024), Brazil (Costa and Gerard 2021), and Kenya (Lee, Miguel, and Wolfram 2020).

[5] Deryugina, MacKay, and Reif (2020) emphasize this point and estimate two-year responses to electricity prices in Illinois. They find that Illinoisan households gradually respond to changes in electricity prices, which is consistent with our findings for Chinese households. Another related study is Costa and Gerard (2021), which focuses on persistent responses to a temporal policy shock and is therefore distinct from Deryugina, MacKay, and Reif (2020) and our study.

[6] For example, Wolak (2011) and Ito, Ida, and Tanaka (2018) find that the introduction of residential dynamic electricity pricing in the United States and Japan—which increased peak-hour prices by 100 percent to 300 percent—induced reductions in electricity usage of 10–15 percent. Another policy that has been extensively studied in many

I. Key Features of the Heating Price Reform

This section describes key features of the heating price reform in Tianjin that are relevant for our research design. First, the city of Tianjin required household-level metered usage data to be collected for at least one year before the introduction of CBB, allowing us access to daily household-level usage data both before and after the price reform. Second, the reform was introduced with staggered rollout, which created quasi-experimental variation in treatment. Third, the policy-induced price variation allows us to test whether consumers properly distinguished between fixed and variable costs.

A. *Metered Data*

Since 1958, the Chinese government has provided centralized, coal-fired heating to cities north of the Huai River. Urban heating accounts for approximately 25 percent of total commercial energy use north of the river. Heat in the form of steam is provided to these cities, which constitute roughly half of China's urban population. This coal-based heating system is inefficient for two major reasons. First, the heating facilities were mostly built in the 1950s and 1960s based on standards of Soviet technology. There were no heating controls in individual residents. It was common practice for households to regulate temperatures by opening their windows.[7] Second, there were no meters to record household-level usage. Without metered usage data, it has been practically impossible to provide incentives for households to respond to market-based energy costs. Billing was based on a flat price per square meter for an entire heating season, regardless of actual heating usage.

In 2005, in collaboration with the World Bank, China's Ministry of Housing and Urban-Rural Development (MOHURD) started a reform in seven cities to improve the efficiency of the heating sector. The reform created a market mechanism so that consumers would pay for their actual heating consumption. Individual heating controls were installed to enable households to control indoor temperatures. Specifically, the heating controls are thermostatic radiator heads with seven different temperature settings.[8] Household-level meters were installed at the same time as the controls. The meters measure household heating consumption by kilowatt-hour (kWh). Metered data have been collected ever since household-level meters and controls were installed. As described previously, the city of Tianjin required that the new pricing (the CBB) start no sooner than a full year after the meter installation.

developed countries is the provision of information on peer energy usage, which typically induces reductions in energy use of 1–2 percent (Allcott and Rogers 2014).

[7] Households could not turn the heating on or off either. If households planned to use no heating for the entire heating season, they could request that the utility company stop supplying heat to their residences.

[8] The seven temperature settings are: no heating, 6–8°C, 9–12°C, 13–16°C, 17–20°C, above 20°C, and maximum heating supply.

B. *Staggered Rollouts*

Consumption-based billing was introduced to households in Tianjin in a staggered rollout lasting from 2008 to 2016. The long period for the rollout allows us to estimate the policy's long-run effects using a staggered difference-in-differences (DID) design. The vast majority of residences in Tianjin are condominiums, and the rollout was done at the condominium building level.[9] By 2016, 429 multiunit condo buildings had introduced CBB, for a total of 16,425 units across these buildings, which constituted the households in our sample. Supplemental Appendix Figure A.1 shows the time-series variation in the number of households introduced to CBB each year.

The city's annual operating budget for the reform was constrained, which forced the rollout to span nine years. According to city officials, rollouts were done in an unsystematic order, though the timing was not randomly assigned. We test whether rollout timing was correlated with building characteristics. We do not find statistically significant relationships between this timing and the observable building characteristics, including the year it was built, number of square meters, and value of its condos (see Section IIIB for a detailed discussion). This provides supportive evidence for the standard identification assumptions for a staggered DID design, as we describe further in Section III.

Households were fully informed about the start of the new billing scheme. The homeowners association office sent every household a letter in October to announce the change in billing method. Along with the letter, every household also received a user handbook from the utility company. The handbook explains the new billing policy in detail, including how households can adjust indoor temperature, how household usage is metered, how metered heating is priced, etc. We include the handbook's section on pricing (translated from Chinese) in Supplemental Appendix A.

Once a building was assigned to start CBB, all of its households received CBB by default. However, households could opt out from CBB and keep the fixed payment scheme they had prior to the reform. To take this option, households had to opt out before the first winter of CBB. In our data, 68 percent of households complied with CBB and 32 percent opted out. For this reason, we estimate both the intention-to-treat (ITT) effect and the average treatment effect on the treated (ATET) in Section III.[10]

C. *The Price Variation Created by the Reform*

Before the policy change, households paid an annual fixed charge equal to US\$3.97 times their residence's square meters. For example, a household with 100 square meters of space paid \$397 every winter, regardless of heating usage.

After the policy change, a heating bill included a two parts: (i) an annual fixed charge of US\$1.895 per occupied square meter, and (ii) a variable charge of

[9] For this reason, we cluster standard errors at the building level in our estimation

[10] We observe daily metered heating usage for both CBB compliers and noncompliers.

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. All consumers had the same change in marginal price, but households with relatively low levels of usage experienced a decrease in average price, while households with relatively high levels of usage experienced an increase in average price.

1.4 cents per kWh of heating used.[11] This policy change provides useful variation for our empirical analysis, because many consumers experienced an increase in marginal price but a decrease in average price. For example, consider a household occupying 100 square meters of space whose typical usage is 10,000 kWh per winter, or a usage per square meter of 100 kWh. The household's pre-reform payment for the winter would have been \$397, with a marginal price of zero. With the same usage, its post-reform payment would be \$338.5 $(= 198.5 + 0.014 \cdot 10{,}000)$, with a marginal price of 1.4 cents. Thus, for the same usage, this household would experience an increase in marginal price but a decrease in average price after the reform.

Figure 1 visualizes how CBB changed the marginal and average price of heating for a given level of usage per square meter. The change in marginal price was common to all households—from 0 to 1.4 cents per kWh. However, the change in average price depended upon heating usage per square meter. Given the same usage level, households whose usage per square meter was less than 142 kWh experienced a decrease in average price, while all other households experienced an increase in average price. Together, this implies that after the reform, many consumers were likely to experience an increase in marginal price but a decrease in average price, which is the key variation we use in Section IV.

[11] The regulator set the marginal price equal to marginal cost based on information about heating production.

II. Data

A. *Household Heating Usage Data*

We obtained administrative data on daily heating usage at the household level for the city of Tianjin from a regulated utility company (Anonymous Firm 2019). The data include all of the company's residential customers from December 2007 to February 2019. Heating usage is automatically recorded once a day and uploaded to the company's database. With a confidentiality agreement, we obtained direct access to the database. To our knowledge, our study is among the first to use such high-frequency administrative data on energy usage in a developing country.

In Tianjin, the winter heating season starts in mid-November and ends in mid-March. The exact start and end dates depend on each year's temperatures. To make our analysis consistent across years, we focus on daily usage in three fully heated months of the heating season—December, January, and February. In these three months, heating is on every day of the month.

All households in our data have at least one year of metered heating data (that is, there are non-missing usage data for three winter months) prior to the start of CBB. For about 40 percent of households, we observe at least two years of metered heating data (i.e., six or more winter months of usage data) in the pre-reform period. For the post-reform period, all households have at least three years of metered heating data.

Table 1 reports summary statistics. We observe daily heating usage from 16,425 households in 429 buildings. The total number of observations is 278,041 household-months. For each household, we also observe the address, condo number, number of square meters, and condo value. The average heating usage is 98.3 kWh per day before CBB and 94.6 kWh per day after CBB. The average size of condos is 105 square meters with an average value of US$524,300. The take-up rate of CBB is 68 percent.

In columns 2 to 5, we show summary statistics by quartile of the policy-induced change in average price, which is the key variation we use in Section IV. As described in Section IC and Figure 1, the unique feature of the reform is that it created substantially different changes in average price across customers, even though they shared a common change in marginal price. To exploit this price variation, we construct the *predicted* change in average price for households by using their heating data for two years prior to the introduction of CBB. We consider these predicted changes to be the policy-induced variation in average price because they do not depend on concurrent heating usage. In the table, we show descriptive statistics for each quartile of this variable. Heating usage before the introduction of CBB is lower for lower quartiles and higher for higher quartiles. Home values and square meters of residence are similar across the four groups. As described in Section IC, the fixed charge under CBB is a function of square meters. Because the number of square meters is similar across the four groups, the change in fixed charge is also similar between them.

Note that the difference between heating usage before and after the introduction of CBB in Table 1 is informative but should not be interpreted as causal evidence. This statistic does not control for potential confounding factors such as weather

Table 1—Summary Statistics

	All sample	By the policy-induced change in average price			
		Quartile 1	Quartile 2	Quartile 3	Quartile 4
Heating usage (kWh/day) before CBB	98.3	74.1	93.1	104.7	136.2
	(52.7)	(34.7)	(48.2)	(40.8)	(58.0)
Heating usage (kWh/day) after CBB	94.6	81.0	91.4	95.7	110.4
	(49.2)	(37.3)	(48.9)	(41.3)	(54.3)
Square meters of residence	104.7	108.7	105.0	109.0	104.4
	(42.5)	(33.7)	(44.2)	(36.1)	(35.9)
Home value (1,000 US$)	524.3	571.8	501.4	552.1	521.1
	(273.7)	(220.0)	(270.0)	(217.5)	(208.8)
Take-up rate	0.68	0.53	0.66	0.66	0.73
	(0.47)	(0.50)	(0.47)	(0.47)	(0.45)
Change in fixed charge (US$/year)	207.7	215.8	208.5	216.3	207.3
	(84.3)	(66.9)	(87.7)	(71.6)	(71.2)
Change in marginal price (US$/kWh)	0.014	0.014	0.014	0.014	0.014

Notes: There are 16,425 households in the data and 278,041 observations by household and month. We show sample means and standard deviations in parentheses. The changes in fixed charge and marginal price are the changes in these variables before and after the CBB.

conditions and mean reversion. We use the staggered difference-in-differences method to estimate the causal effects of CBB in Sections III and IV.

B. *Air Pollution Data*

To examine the impact of the heating price reform on environmental externalities, we ask two questions following the change in household heating due to CBB: (i) How does this affect the emission of pollutants from the utility company's heating plant? (ii) How does this affect local ambient air quality?

Our research site, a district of Tianjin, provides an interesting setting to answer these questions. First of all, the heating plant is situated near a residential area encompassing most of the households in our data, and there is an ambient air pollution monitor nearby. The heating plant is a major local source of emissions in winter, and is located about 8 kilometers away from the pollution monitor. If changes in household heating affect the pollution emissions of this plant, we would also expect local ambient air quality to be affected. Second, the district of interest is located in a relatively isolated part of Tianjin, about 55 kilometers away from the Tianjin metro area. The remoteness of its location is useful for our analysis because other emitting sources in the metro area are less likely to affect local air quality in the district.

We obtained pollution data from two sources. To measure pollution emissions from the heating plant, we received hourly emission concentration data for SO_2, NO_x, and PM from a Continuous Emission Monitoring System (CEMS) monitor placed at the heating plant (Tianjin Environmental Monitoring Center, 2018). To measure local ambient air quality, we compiled daily readings of SO_2, PM_{10}, and $PM_{2.5}$ from the district's pollution monitor (China National Environmental Monitoring Center, 2019). The PM_{10} readings are particularly useful for the welfare analysis on externalities, because we can combine the district's changes in PM_{10} with the measure of marginal willingness-to-pay for PM_{10} reductions from Ito and Zhang (2020) to evaluate the changes in welfare due to reductions in environmental externalities.

III. The Impacts of CBB on Heating Usage

In this section, we estimate the causal effect of CBB on heating usage. As described in Section IB, CBB was implemented with the ability for households to opt out, and about 32 percent of households chose to do so. This created one-sided incomplete compliance, because all households in the control group were untreated, and there was imcomplete compliance in the treatment group. For this reason, we estimate both the intention-to-treat (ITT) effect and the average treatment effect on the treated (ATET).

A. *Overall Policy Impacts*

We begin by estimating the ITT. Our identification strategy is based on the staggered rollout of CBB as described in Section IB. Our estimation equation is

$$y_{it} = \alpha_i + \gamma_t + \sum_{k=a}^{b} \phi_k D_{it}^k + u_{it}, \tag{1}$$

where y_{it} is the natural log of average daily heating use by household i in year-month t, α_i indicates household-level fixed effects, and γ_t indicates year-month fixed effects. We use $k = [a, b]$ to denote the event-time relative to the first month of treatment (i.e., the time that CBB was introduced to household i). For example, $k = 0$ is the last month of the pretreatment period and $k = 1$ is the first month of treatment. Note that we use data from three winter months, a period lasting from the first day of December to the last day of February. Therefore, if we consider a household whose treatment started in December 2010, k equals 0 in February 2009, 1 in December 2010, 2 in January 2011, 3 in February 2011, 4 in December 2012, and so on. The dummy variable $D_{it}^k = 1$ if year-month t falls within the event-time k for household i.

Recent developments in the econometrics literature point out that using conventional OLS could produce biased estimates for a two-way fixed effects model, such as that in equation (1), if treatment effects are heterogeneous across households or time (de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2021). To address this problem, we use a method developed by de Chaisemartin and D'Haultfœuille (2020) to estimate equation (1) so that we do not impose the assumption of homogeneous treatment effects. For comparison, we also show results based on the conventional OLS method in Supplemental Appendix B. We find that our results indeed differ when we impose the assumption of homogeneous treatment effects. The reason why the conventional OLS method may not produce the correct average treatment effect, as explained in de Chaisemartin and D'Haultfœuille (2020), is because it produces an incorrectly weighted average of treatment effects across cohorts and time. Moreover, some of these incorrect weights could be negative in theory, which would cause the OLS estimate to significantly differ from the correctly weighted average of the cohort-by-time treatment effects. In Supplemental Appendix Figure A.4, we show that this is indeed the case in our data. We find that 46 percent of cohort-by-time weights are negative if we use the conventional OLS, which suggests that it is important to allow for heterogeneous treatment effects in our setting.

The primary variable of interest is ϕ_k. This coefficient provides an ITT estimate of the mean log average daily usage for event time k, controlling for household fixed effects and time fixed effects. The excluded group is $k = -1$, the last month of the pretreatment period. Thus, we interpret ϕ_k as the difference in mean log average daily usage between event month k and the last month of the pretreatment period. We need identification assumptions that are standard in the difference-in-differences method—in the absence of treatment, ϕ_k should be zero. The validity of this identification assumption is untestable, but we can assess whether our data are consistent with the parallel trends assumption in the pretreatment period.

In Figure 2, we show the estimates of ϕ_k for $k = [-5, 9]$, which comprises heat usage from two years before to three years after the reform. This figure provides three key results. First, there is no statistically significant difference in heat usage trends between the treatment and control groups before the event of treatment. Second, the ITT estimate for the first year is approximately a 10 percent reduction in heating usage. Third, the impact of CBB remains similar for the second and third year after implementation.

In Table 2, we provide the ITT estimates of CBB for each of the three post-reform years in column 1. These results suggest that CBB resulted in reductions in heating usage by 10.1 percent in the first year, 10.7 percent in the second year, and 8.7 percent in the third year (in log points, these are -0.107, -0.113, and -0.091, respectively).[12]

In column 2 of Table 2, we provide the ATET of CBB for each of the three post-reform years. We estimate equation (1) by replacing treatment assignment D_{it}^k with T_{it}^k, which is household i's actual treatment status at event time k. We use D_{it}^k as an instrument for T_{it}^k to obtain the IV estimate. With the standard assumptions for the local average treatment effect (Imbens and Angrist 1994), the IV estimates can be interpreted as the ATET because we have incomplete compliance only in the treatment group. The ATET of CBB is 14 percent in the first year, 16 percent in the second year, and 13 percent in the third year (in log points, -0.155, -0.167, and -0.138, respectively).

B. *Assessing the Validity of Identification Assumptions*

The validity of our estimation is subject to a standard set of identification assumptions for the staggered DID design. A key assumption is parallel trends in the counterfactual, untreated outcome: in the absence of the treatment, the trajectory of the outcome variable (in our context, this is heating usage) has to be parallel between the treatment and control groups. Although this is an empirically untestable assumption, we provide two pieces of supporting evidence. The first piece of evidence is the

[12] Since the outcome variable y_{it} is logged heating usage, the ITT estimate of ϕ_k is in log points. Converting these ITT estimates to their percentage change can be done by calculating $\exp(\phi_k) - 1$.

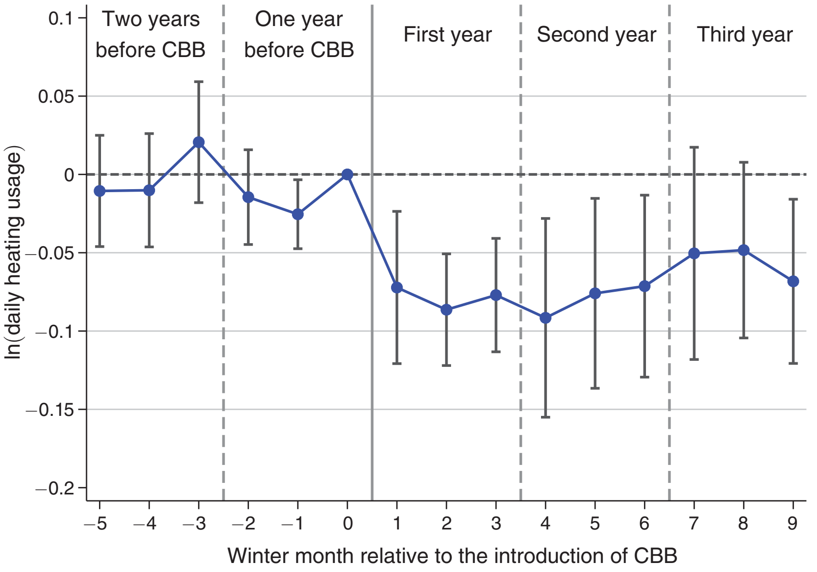

FIGURE 2. STAGGERED DIFFERENCE-IN-DIFFERENCES ANALYSIS: INTENTION-TO-TREAT (ITT)

Notes: This figure shows the ITT estimates of the staggered difference-in-differences analysis described in equation (1) based on the estimation method developed by de Chaisemartin and D'Haultfœuille (2020). There are three heating months in each year because the heating season is December, January, and February. For example, −2, −1, and 0 are the three winter months in the year prior to CBB. The bars indicate the 95 percent confidence intervals. Standard errors are clustered at the building level.

TABLE 2—IMPACTS OF CONSUMPTION-BASED BILLING ON HEATING USAGE DEPENDENT VARIABLE: LOG OF DAILY HEATING USAGE

	ITT	ATET
First year of CBB	−0.107	−0.155
	(0.014)	(0.025)
Second year of CBB	−0.113	−0.167
	(0.031)	(0.036)
Third year of CBB	−0.091	−0.138
	(0.030)	(0.044)
Observations	278,041	278,041

Notes: This table shows the estimation results of equation (1). The estimation includes household fixed effects and year-by-month fixed effects. Standard errors in parentheses are clustered at the building level.

absence of distinct pre-trends in Figure 2. The pattern of heating usage was not statistically different between the treatment and control groups prior to the event of treatment. Second, we test whether building characteristics are associated with the timing of policy implementation and report these results in Supplemental Appendix Table A.1. We do not find statistically significant relationships between the timing of CBB rollout and the building's age, square meters per unit, condo values, and annual heating usage prior to the introduction of CBB.

To interpret our IV estimates as the ATET, we also need to satisfy the standard set of assumptions for the local average treatment effect (Imbens and Angrist 1994). A potential concern is that the Stable Unit Treatment Value Assumption (SUTVA) could be violated if a household's usage were affected by other households' compliance decisions. To evaluate this possibility, we test whether the change in each household's heating usage is correlated with the compliance rate of its neighbors. As reported in Supplemental Appendix Table A.3, we do not find statistically significant correlation between a household's response to CBB and the compliance status of its neighbors living next door and on the floors above and below.

C. *Interpreting the Overall Impacts of CBB*

Overall, the findings in Table 2 indicate that CBB resulted in statistically and economically significant changes in heating usage. Reductions in residential energy usage by 10 percent for the ITT effect and 15 percent for the ATET are sizable relative to the estimates of other policies evaluated in the literature. For example, the effects of non-price energy conservation programs such as providing home energy reports with peer comparisons usually produce reductions in residential energy usage of 1–2 percent (Allcott 2011b). The short-run effects of dynamic electricity pricing on reductions are estimated to lie between 10 percent and 15 percent (Wolak 2010; Ito, Ida, and Tanaka 2018, 2023).

In addition, the staggered rollout of CBB allows us to estimate long-run effects, which most existing studies find challenging to estimate because it is difficult to obtain long-run exogenous variation in energy prices.[13] Our results suggest that CBB produces reductions in heating usage for at least three years after the introduction of the policy.

Hence, if a policymaker's objective is to reduce residential heating usage, our analysis indicates that CBB is an effective method that has long-lasting and sizable impacts. However, the overall welfare implications of CBB depend upon whether consumers distinguish between fixed and variable costs when faced with a two-part tariff. For this reason, we investigate whether consumers respond to this distinction in the next section before we discuss the overall welfare implications of CBB in Section VI.

IV. Do Consumers Distinguish between Fixed and Variable Cost?

In Section III, we find that CBB induces reductions in heating usage *on average*. However, this finding by itself does not reveal whether consumers properly distinguish between fixed and variable cost when responding to the two-part tariff. In this section, we exploit the price variation created by CBB to test this question. As we show in Section VI, testing this hypothesis is key to the welfare implications of CBB and two-part tariffs in general.

[13] Most studies in the literature on residential energy demand estimate short-run effects based on price variation that lasts for a few months to a year (Wolak 2010; Ito, Ida, and Tanaka 2018). Few papers are able to estimate long-run effects, but those that do include Allcott and Rogers (2014) and Deryugina, MacKay, and Reif (2020).

A. *Conceptual Framework*

Consider a utility maximization problem for heating demand y. A consumer has income I and faces a marginal price of heating p and a fixed charge f. We consider a quasi-linear utility function $u = v(y) - py - f + I$.[14] A standard utility maximization problem solves the first-order condition for the utility function with respect to y, yielding $v'(y^*) = p$. Therefore, the optimal usage y^* occurs when the marginal utility from consumption equals the marginal price. Therefore, the standard model predicts that an increase in marginal price will result in a decrease or no change in heating usage.

In contrast, Liebman and Zeckhauser (2004) suggest "schmeduling" as an alternative model of consumer behavior. A consumer who is faced with nonlinear pricing may misperceive a change in average price p_a as a change in marginal price p. Consumers facing two-part tariffs may be particularly susceptible to this misperception if they do not properly distinguish between changes in fixed costs and those in variable costs. In this case, the optimal usage y^{**} would be characterized by $v'(y^{**}) = p_a(y^{**})$.[15] Importantly, the schmeduling model allows for the possibility that an increase in the marginal price of heating could lead to an *increase* in heating usage when a consumer has an increase in marginal price but an overall *decrease* in average price.

B. *Empirical Tests for "Schmeduling"*

We propose a simple nonparametric test for the schmeduling model with a two-part tariff. Our approach exploits the unique price variation created by the introduction of CBB. As described in Figure 1, some consumers in our data experienced a policy-induced increase in marginal price and a decrease in average price. This is because CBB increased the marginal price while lowering the fixed charge. We use β to denote the impact of CBB on heating usage for these consumers. As described in the conceptual framework from Section IVA, the standard model predicts that $\beta \leq 0$, while the schmeduling model predicts that $\beta > 0$. Therefore, we can apply the estimation method described in Section IV to conduct a simple statistical test of these models for this subgroup of consumers. An advantage of this test is that we do not need to impose functional form assumptions on the demand curve. We simply test how the marginal change in price affects heating consumption.

A naïve way to identify this subset of consumers is to look at the actual average price paid by each consumer. However, this approach would create an endogeneity concern, because the actual average price is a function of contemporaneous heating usage. To address this issue, we follow the literature on nonlinear income taxation and pricing to identify the *policy-induced* change in average price (Saez, Slemrod, and Giertz 2012; Ito 2014). For each customer, we construct the predicted change in

[14] A quasi-linear utility function assumes there is no income effect. This assumption is likely to be valid in our empirical context, because the income effect of CBB is likely to be very small. We explore this point in Section VA.

[15] In general, utility bills are delivered to consumers after they consume utility services such as energy and water. In this context, consumers may instead respond to the lagged average price, based on their past bills. In this case, the equation for their optimal usage becomes $v'(y^{**}) = p_a(y^{\dagger})$, where $y^{\dagger}$ is their lagged usage level.

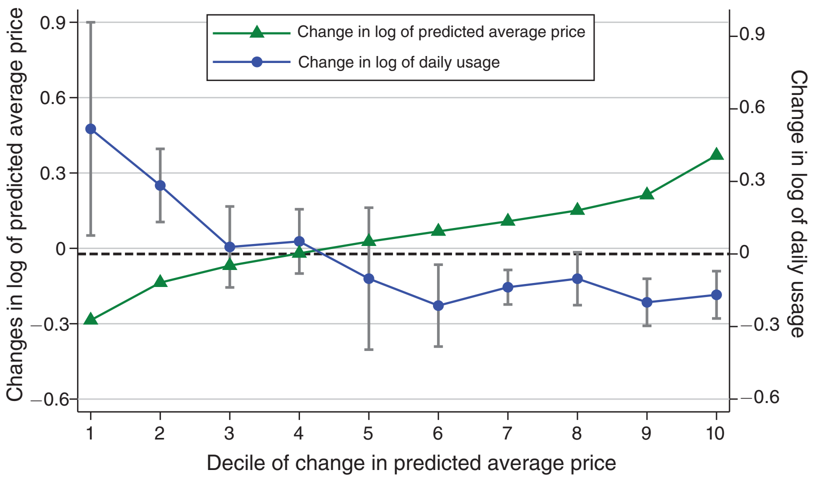

Figure 3. Policy-Induced Changes in Average Price and Usage

Notes: We divide customers by decile based on their policy-induced changes in average price. For each decile, we estimate the ITT of CBB on the log of heating usage based on the difference-in-differences estimation method developed by de Chaisemartin and D'Haultfœuille (2020). We also apply the same method to estimate the ITT on the log of the policy-induced change in average price. The bars indicate the 95 percent confidence intervals. Standard errors are clustered at the building level.

average price from their heating usage two years prior to their introduction to CBB. This predicted change in average price does not depend on each customer's heating decisions after the introduction of CBB, and therefore is driven by the changes in price schedule induced by the reform. Consistent with previous studies in this literature, we find that the predicted and actual change are highly correlated, as we show below.

We begin by testing for visual evidence in Figure 3. We divide households into deciles based on the predicted change in their average price.[16] Separately for each decile, we estimate the ITT effect of CBB using the estimation method described in Section IV. Recall that all consumers face the same change in marginal price—an increase of \$0.014 per kWh. However, the changes in *average* price are different across deciles. Households in the first to third deciles experience decreases, those in the fourth decile experience nearly no change, and remaining households (in the fifth to tenth deciles) experience increases in average price.

The standard model predicts that households across all deciles would reduce their usage, because all face an increase in marginal price. However, the observed changes in heating usage shown in Figure 3 are inconsistent with this prediction. Households in the first and second decile have increases, those in the third and fourth deciles have nearly no change, and remaining households (in the fifth through tenth deciles) have decreases in their heating usage. The relationship between the

[16] To construct the predicted change in average price from baseline heating usage, we exclude households who lack data in the baseline year. We also exclude outliers that report more days than a typical heating season.

Table 3—Impacts of CBB by Quartiles of the Predicted Changes in Average Price

Dependent variable: Log of daily heating usage				
	ITT			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
CBB	0.216	0.019	−0.159	−0.154
	(0.060)	(0.052)	(0.060)	(0.028)
Observations	44,384	57,106	31,602	44,362
Change in marginal price	0.014	0.014	0.014	0.014
ITT on ln(average price)	−0.221	0.044	0.190	0.270
ITT on ln(predicted average price)	−0.116	0.055	0.057	0.230

Notes: We divide customers by quartile based on their policy-induced changes in average price and estimate equation (1) for each quartile group separately. The estimation includes household fixed effects and year-by-month fixed effects. Standard errors in parentheses are clustered at the building level.

Table 4—Testing for Schmeduling

	ITT				
	Marginal price	ln(predicted average price)	ln(actual average price)	ln(usage)	$H_0: \beta \leq 0$ (p-value)
Full sample	0.014	0.216	−0.116	−0.221	0.000
		(0.060)	(0.005)	(0.047)	
Households with home value > median	0.014	−0.117	−0.166	0.170	0.038
		(0.009)	(0.065)	(0.096)	
Households with home value ≤ median	0.014	−0.113	−0.182	0.153	0.007
		(0.008)	(0.054)	(0.063)	
Households with home size > median	0.014	−0.115	−0.155	0.175	0.071
		(0.009)	(0.072)	(0.119)	
Households with home size ≤ median	0.014	−0.117	−0.229	0.195	0.002
		(0.008)	(0.056)	(0.068)	

Notes: As described in the text, we use households who are in the first quartile of the policy-induced change in average price to provide a test for schmeduling. In this table we report the ITT estimates for this group. Standard errors in parentheses are clustered at the building level.

changes in average price and the changes in usage that we observe empirically suggests that consumer behavior is more consistent with the schmeduling model.

With this insight from Figure 3, we conduct statistical tests for schmeduling in Table 3 and Table 4. Our procedure is similar to the approach used in Figure 3, except that we use quartiles rather than deciles to increase the precision of the tests. We sort households into quartiles based on the predicted change in their average price. Separately for each quartile, we estimate the ITT effect of CBB using the estimation method described in Section IV.

While all groups experienced the same increase in marginal price, the results reported in Table 3 indicate that for changes in average price, households in the first quartile experienced a decrease, those in the second quartile experienced nearly no change, and those in the third and fourth quartile experienced an increase. CBB induced an *increase* in usage by 0.216 log points (a 24.1 percent increase) for households in the first quartile.[17] For those in the second quartile, the impact of CBB on heating usage is statistically insignificant from zero. For those in the third and fourth quartiles, CBB induced decreases in heating usage.

In Table 4, we provide further analysis by focusing on households in the first quartile, which experienced a policy-induced increase in marginal price but a decrease in average price. The first row replicates the results shown in column 1 of Table 3. The standard economic theory predicts $H_0 : \beta \leq 0$, in which β is the ITT of CBB on heating usage. We provide the *p*-value from this test in the last column. We reject this null at the 1 percent statistical significance level for the households first quartile, indicating that consumer behavior is inconsistent with the standard model.

We also examine whether there are subgroups of these customers that behave more consistently with the standard model. We further divide the first quartile, by home value in rows 2 and 3, and by condo size in rows 4 and 5. Even though the magnitudes of the ITT effects are heterogeneous across these groups, we reject the null hypothesis (and therefore, the prediction from the standard model) for all of these subgroups.

In Supplemental Appendix Tables A.4 and A.5, we provide the same analyses based on the ATET instead of the ITT effect. Qualitatively, our findings do not change and we reject the null hypothesis for all subgroups based on the ATET as well. In fact, we reject the prediction from the standard theory even more strongly than with the ITT effect, because the induced change in consumption from the ATET is larger in absolute value than the ITT effect, due to one-sided incomplete compliance.[18]

C. *Mean Reversion and the Validity of Identification Assumptions*

The empirical evidence in the previous section suggests that consumer behavior is inconsistent with the standard economic model and more consistent with the schmeduling model. An important factor that we want to be careful about is mean reversion in the outcome data and its possible threat to identification when evaluating consumer behavior with nonlinear price schedules (Saez, Slemrod, and Giertz 2012; Ito 2014). Many types of economic panel data, including household-level heating consumption, tend to have mean reversion. For example, consider a household which has a negative (positive) transitory shock to its heating consumption in one period. Then, this household's consumption tends to be higher (lower) in other periods because of mean reversion. This natural mean reversion is important to consider when analyzing consumer behavior under nonlinear price schedules.

In our context, we calculate each household's policy-induced change in average price using their consumption in the baseline period prior to CBB. Consider a household with low baseline usage. Naively comparing its baseline usage against usage in later periods could yield a misleading conclusion if this household had a transitory

[17] As explained previously, the treatment effect in log points (β) can be converted to the percent change by the formula $\exp(\beta) - 1$.

[18] In our context, consumers could have opted out from CBB after being assigned to treatment, but those who were not assigned to treatment were not able to have CBB. Since the ITT effect equals the product of the ATET and the take-up rate, the ATET is larger than the ITT effect.

negative shock in the baseline period. Its usage will be higher in these other periods because of mean reversion, not necessarily because of the policy impact.

Our empirical analysis controls for mean reversion by using the staggered DID design presented in previous sections. Instead of naively comparing a treated household's usage before and after CBB, our staggered DID design uses data from untreated households to control for time-varying changes in usage, including mean reversion. To make this point clear, consider example households A and B, which have an identical level of consumption in the pre-CBB baseline period. Suppose that in the staggered rollout of CBB, household A starts CBB several years earlier than household B. In this example, our staggered DID design controls for the mean reversion for household A by using data from household B. A key assumption in this approach is the standard parallel trends assumption—time-varying unobserved factors that affect heating usage, including mean reversion, should not be systematically different between households with an earlier rollout of CBB and those with a later rollout.

This identification assumption is untestable, as is the case for any DID design with quasi-experimental data, but we can assess its validity by examining pre-trends in an event study plot in Figure 4. The pre-trends suggest that time-varying unobservable factors that affect heating usage are not systematically different between treated and untreated households in the pre-CBB period. In particular, if mean reversion is the reason for the increase in usage for households in the first quartile and the decrease in usage for those in the third and fourth quartiles, we should observe this usage pattern every year, including the period before the introduction of CBB.[19] However, Figure 4 suggests that we do not observe such changes until the beginning of CBB. This provides supporting evidence that time-varying unobservable factors, including mean reversion, are unlikely to be systematically different between households with different rollout timing.[20]

V. Alternative Explanations

In the previous sections, we find empirical evidence that is more consistent with the "schmeduling" model than with standard theory. In this section, we explore if there are alternative explanations that could be consistent with what we observe in the data.

A. *Income Effect*

In Section IVA, we consider a quasi-linear utility function, which abstracts from the income effect. If we consider a more general utility function, a change in fixed cost could create an income effect on heating usage.

[19] Recall that we define quartiles by predicting the change in average price from the first year a household appears in our data, before it faces CBB. Therefore, if mean reversion drives the increase or decrease in usage, we should observe it before the start of CBB. However, the pre-trends do not show such evidence.

[20] We provide further robustness checks in Supplemental Appendix Table A.2 and Figure A.3. In these analyses, we include further flexible controls for mean reversion by interacting the time fixed effects with the calendar year of the pre-CBB baseline period. This way, we allow mean reversion and other time-varying unobservable factors to differ between households whose pre-CBB baseline period fell in different calendar years. We find that results are robust to these controls.

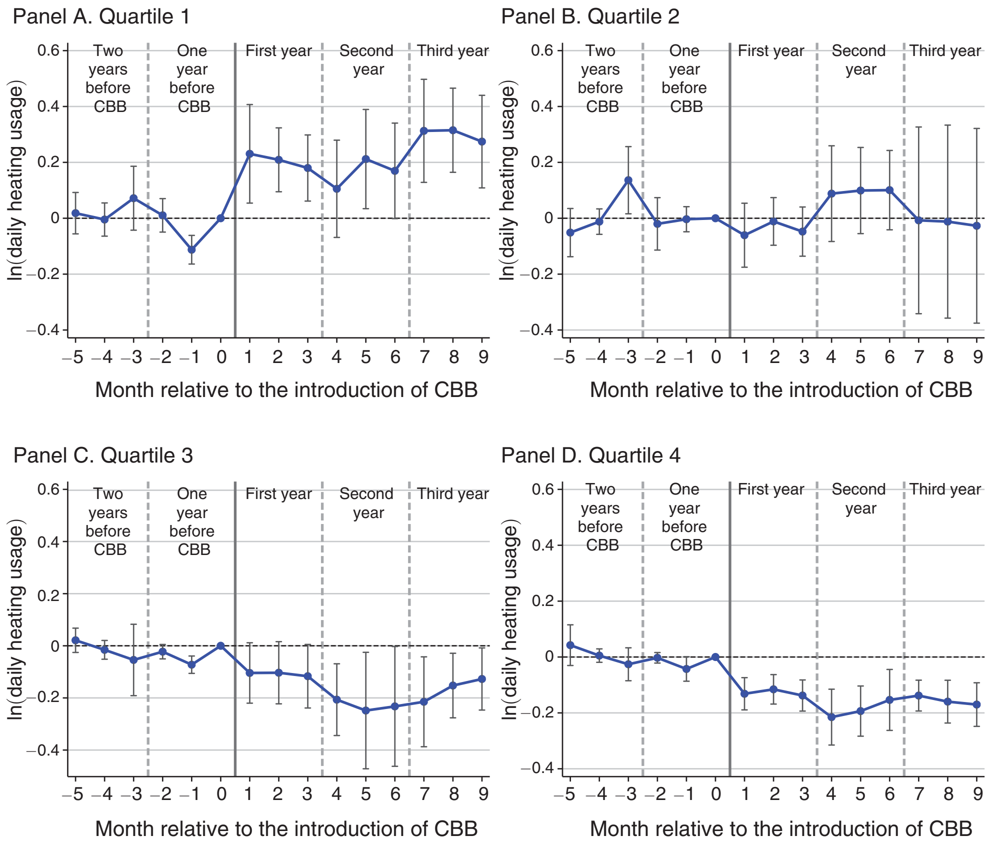

Figure 4. Staggered DID Analysis by Quartile of the Policy-Induced Change in Average Price

Notes: This figure shows the ITT estimates of the staggered difference-in-differences analysis described in equation (1) based on the estimation method developed by de Chaisemartin and D'Haultfœuille (2020). There are three heating months in each year because the heating season is December, January, and February. The bars indicate the 95 percent confidence intervals. Standard errors are clustered at the building level. We divide customers by quartile based on their policy-induced changes in average price. We then estimate equation (1) for each quartile group separately to make these event study figures.

In our empirical setting, however, the income effect of CBB is likely to be too small to explain our findings. On average, CBB reduced the fixed cost of heating by \$208 per year (based on an average home size of 105 square meters). Average household income in Tianjin in our sample period was \$15,041. Therefore, the change in fixed cost was about 1.38 percent of household income. In the literature on energy demand, estimates of short-run residential income elasticity are found to be inelastic at an average of 0.239, based on a recent meta analysis (Zhu and Yang 2018). This implies that the income effect of CBB would be an increase in heating usage of 0.33 percent.[21]

[21] In addition to calculating the income effect for the average household, we do the same for households in the first decile of the predicted change in average price. As discussed in Section IC and Figure 1, the predicted change in average price depends on usage per square meters rather than the number of square meters by itself. Therefore, the

This effect is too small to explain our findings in Table 4 and Supplemental Appendix Table A.5. Our estimated ATET result implies that customers who experienced an increase in marginal price and a decrease in average price *increase* their usage by 36.6 percent (a 0.312 log-point increase), which is substantially more than 0.33 percent. To explain this finding by an income effect, the short-run income elasticity of heating demand would have to be 25.2, which is far larger than typical empirical findings in the literature.

B. *Category Budgeting*

Category budgeting models suggest that individuals may consider within-category budgets as opposed to a standard budget constraint that assumes the fungibility of money (Heath and Soll 1996; Antonides and Raaij 2011; Hastings and Shapiro 2013, 2018; Farhi and Gabaix 2020).

We consider two existing category budgeting models that are relevant to our context. Hastings and Shapiro (2013) present a model showing that households may experience disutility from spending an atypical amount on certain goods. In our context, this model implies that consumers experience disutility from spending a different amount of money on heating in the new price schedule (the two-part tariff) compared to what they used to pay under the previous price schedule (the fixed payment).

We examine whether consumer behavior in our data can be explained by predictions from this model. To do so, we exploit the opt-out feature of the reform (recall that households could opt-out of CBB if they wanted to keep the former pricing scheme). If households wanted to maintain the same heating expenditures, the most reliable way to do so would be to opt out. Importantly, this category budgeting model implies that the incentives to opt out were equally large for those who expected a bill *increase* and those who expected a bill *decrease* under CBB. That is, the magnitude of this effect was symmetric for both an increase and a decrease in the predicted change in annual heating bill.

We test this prediction in Figure 5. For each household, we calculate the predicted change in annual bill based on their pre-CBB heating usage. That is, we calculate how much more or less a household would pay under CBB compared to the fixed charge system, if they were to use the same amount of heating. The horizontal axis in the figure is the decile of predicted change in annual bill. The dashed line shows the average change in annual billing for households in each decile, which ranges from approximately a decrease of \$120 to an increase of \$170. The solid line shows the opt-out rate relative to that for households in the first decile. We find that the opt-out rate is increasing in the expected increase in payment. That is, those who expected their bill to increase were more likely to opt out, and those who expected a decrease were less likely to opt out. Notably, the opt-out rate is *not*

first decile of households do not necessarily live in larger condos. In our data, we find that the average number of square meters for condominiums in this group is $109\,\mathrm{m}^2$, which is slightly larger than the average number for all households ($105\,\mathrm{m}^2$). We also find that the average condo value was US\$571,800 for this group and US\$524,300 for all households, which suggests that income levels are unlikely to widely differ between the two. With this information, the income effect of CBB for this group is calculated to be an increase in heating usage by 0.346 percent.

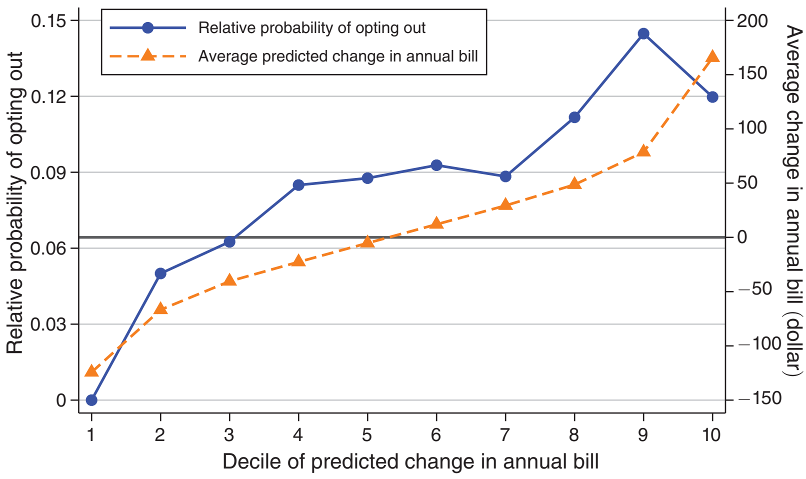

Figure 5. Household Opt-Out Decision

Notes: The consumption-based billing was introduced with an option to opt-out, and 32 percent of customers in our data opted out. In this figure, we examine if the opt-out decision was related to the predicted change in annual payment at each customer's average heating usage in the pre-reform period. The blue solid line shows opt-out rates relative to the first decile of predicted change in annual billing. The orange dashed line shows the average predicted change in the annual bill. This suggests that selection was positively related to the expected gain from the policy, which is consistent with selection on the level in Ito, Ida, and Tanaka (2023). That is, relatively heavy users who would have faced higher bills were more likely to opt out, whereas relatively light users who would have faced lower bills were less likely to opt out.

symmetric for those who expected increases and decreases in the predicted change in annual heating bill.

This empirical finding is inconsistent with the prediction from the category budgeting model because, as described above, the model predicts symmetric opt-out incentive magnitudes for households facing a predicted increase or decrease in their annual heating bill. Rather, the empirical evidence is more consistent with the schmeduling model, because the direction of the predicted bill change is the same as that for the predicted change in average price. Although this evidence does not conclusively rule out category budgeting as a possible mechanism behind our empirical findings, these opt-out decisions provide useful empirical evidence on this question.

Another relevant model is the mental accounting model developed by Farhi and Gabaix (2020). This model allows individuals to have within-category budget constraints, and, if their behavioral bias is extremely large (i.e., money in a category is considered to be completely nonfungible), there can be a within-category income effect from a lump-sum shock to a category. This within-category income effect may be much larger than the conventional income effect discussed in Section VA. Recall that CBB resulted in a reduction in the fixed payment equivalent to 50 percent of each household's pre-reform heating bill. That is, the within-category income shock was 50 percent. As noted previously, the average income elasticity of residential energy demand is 0.239 in the literature. This implies that the within-category income effect would be an 11.95 percent increase in heating usage, as opposed to a 0.33 percent increase in usage—the conventional income effect calculated in Section VA.

We examine whether this potential within-category income effect can explain our empirical results. Recall that the ATETs of CBB for households in the first through fourth quartiles of policy-induced changes in average price are a 36.6 percent, 2.2 percent, −20.0 percent, and a −20.0 percent change in heating usage, respectively.[22] Our analysis suggests that it would be challenging for the within-category income effect by itself to explain our empirical results, unless there are particular patterns of large heterogeneity in income and price elasticity.

First, the increase in heating usage for households in the first quartile (35.8 percent) cannot be fully explained by the potential within-category income effect (11.9 percent), unless income elasticity is three times larger than the typical elasticity estimates found in the literature.

Second, it is difficult to explain the pattern of empirical findings across the quartiles based on the within-category income effect, unless there are large and particular patterns of heterogeneity in the price elasticity and income elasticity of demand. To start, suppose that income and price elasticity are homogeneous between the four groups. In this case, all groups would have the same magnitude of substitution effect (a decrease in usage) and the same magnitude of within-category income effect (an increase in usage). Therefore, the changes in usage should not differ between four groups, which is inconsistent with our empirical results.[23]

We now consider the particular heterogeneity in price elasticity and income elasticity that would make the within-category income effect able to explain our empirical results. Suppose the income elasticity is three times larger than the typical income elasticity in the literature, so that the within-category income effect is a 35.8 percent increase in usage. Further, although CBB made the same change in variable price to all households, suppose that the substitution effects from this price change were heterogeneous for households in different quartiles, at 0 percent, −32.0 percent, −57.7 percent, and −55.3 percent, respectively. In this case, the within-category income effect, combined with this particular heterogeneity in the substitution effects, could explain our empirical results.[24]

As shown in Table 1, we find that condominium characteristics such as the number of square meters and condo values are similar across the four groups. In Table 3, we also find limited heterogeneity in the treatment effect by observables. Therefore, it would be difficult to expect that the differences in the price elasticity and income elasticity across the four groups have these large and particular heterogeneity that would explain the patterns of our empirical findings. However, it is still possible that there are particular patterns of unobserved heterogeneity, and therefore, our analysis

[22] In log points, the ATETs of CBB for households in the four quartiles are 0.312, 0.022, −0.221, and −0.224, respectively (Supplemental Appendix Table A.4). We use the standard formula for converting log points to percentages, that is, $\exp(\beta) - 1$.

[23] As described previously, CBB resulted in a 50 percent reduction in fixed cost for all households. The level of changes in the fixed cost are also similar between the four groups, as we show in Table 1.

[24] In addition to this example, one can consider different combinations of heterogeneity in the income elasticity and price elasticity to make them consistent with our empirical findings, although any combination requires substantial heterogeneity in either elasticity because the ATETs differ substantially between the four groups.

does not completely rule out that the within-category income effect may play an important role in our context.

C. *Spurious Correlation*

Recall that customers who experienced an increase in marginal price and a decrease in average price were those who had relatively low usage per square meter before the introduction of CBB (see Figure 1). One potential concern is that this customer type is correlated with other important household characteristics, and may increase usage in response to CBB for a reason unrelated to average price.

One example of a potential spurious correlation is the location of housing units. For instance, although we showed in Section IIIB that a household's response to CBB is unlikely to be affected by its neighbors, we could imagine the following scenario. Consider households located in the middle floors that are non-corner units, and suppose they receive heating spillovers from their neighbors prior to the reform. For this reason, their pre-reform usage per square meter would be lower than it otherwise would. After the reform, many households reduced usage, and therefore, these spillover-receiving households would need to increase their usage to maintain comfortable indoor temperatures.

We test this hypothesis in Table A.6 in the Supplemental Appendix. In panel A, we estimate the ITT effect of CBB separately for units on the middle floors versus the top and bottom floors. Similarly, in panel B, we estimate the ITT effect of CBB separately for households in corner versus non-corner units. In each panel, we find that both types of customers reduce usage under CBB. If anything, the reductions in usage were larger in point estimate for households in units on middle floors and for those in non-corner units. These findings are inconsistent with a hypothesis of spurious correlation by unit location.[25]

VI. Welfare Implications

In Figure 6, we describe the social welfare gains from CBB. Panel A considers consumer behavior under a two-part tariff in the standard theory. This model assumes that consumers distinguish between fixed and variable costs, and therefore, their consumption is determined by the intersection of their demand curve and the marginal price of heating. In this case, both types of consumers (labeled A and B in the figure) would reduce usage because CBB increases the marginal price of heating from 0 to $0.014 per kWh. Then, the social welfare gains from CBB would be the shaded areas under the social marginal cost curve.

In contrast, the welfare implications are different with the "schmeduling" model in panel B. In this model, consumers do not distinguish between fixed and variable costs, and they instead respond to average price. This implies that the reduction in

[25] To locate corner and non-corner units, we collected information on the building structure and unit locations. In buildings with three, four, or eight units per floor, one can clearly define corner units versus non-corner units. We therefore focus on this subsample in panel B.

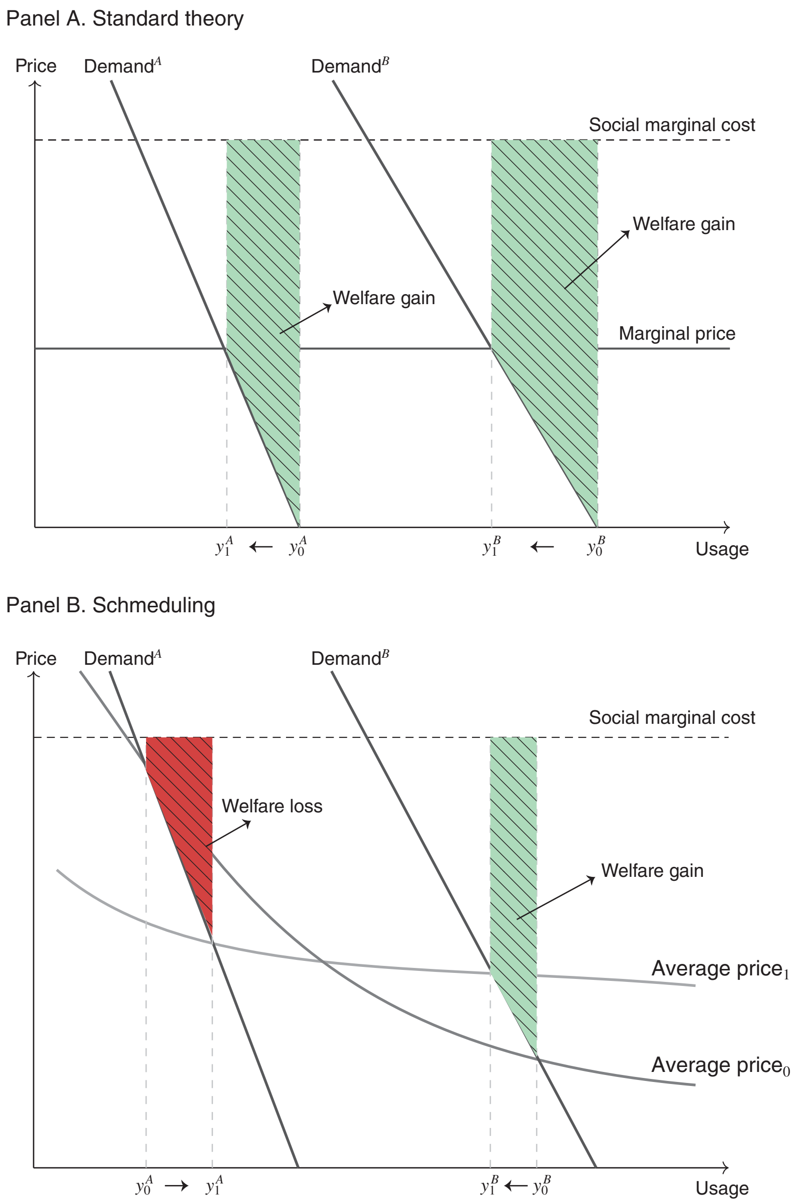

Figure 6. Welfare Implications

Notes: These figures show the social welfare gains from the CBB based on two alternative assumptions regarding consumer behavior. Panel A considers the standard theory of consumer behavior on a two-part tariff. Panel B considers the schmeduling model, in which consumers do not distinguish fixed cost from variable cost, and therefore respond to average price. These figures suggest that the social welfare gains depend on whether consumers respond to the two-part tariff as predicted by the standard model or the schmeduling model.

consumer B's usage is smaller in panel B than in panel A, because the change in average price is smaller than the change in marginal price. Moreover, consumer A

Table 5—Welfare Implications

	Welfare gain per household (US$/year)		Welfare gain for Tianjin (million US$/year)	
	Standard	Schmeduling	Standard	Schmeduling
Total social welfare gain	18.4	2.8	78.7	12.2

Notes: This table shows the social welfare gain from the consumption-based billing based on two different assumptions regarding consumer behavior. Columns 1 and 3 consider the standard theory of consumer behavior on a two-part tariff. Columns 2 and 4 consider the schmeduling model, in which consumers do not distinguish fixed cost from variable cost, and therefore respond to average price.

would increase usage because CBB lowers the average price. This implies that the change in social welfare is negative for consumer A in this framework.[26]

In Table 5, we calculate the social welfare gains from CBB using our data and empirical findings, and based on the two models shown in Figure 6.[27] We present the social welfare gain per household per year in the first two columns and wefare gains for Tianjin per year in the last two columns. Our estimates suggest that the standard approach overstates the welfare gain. If we use conventional cost-benefit calculation based on standard theory, we would estimate that the social welfare gain from CBB is US$18.4 per household per year, or US$78.7 million per year for the city of Tianjin. However, if we incorporate the shcmeduling behavior, we estimate that the social welfare gain from CBB is US$2.8 per household per year, or US$12.2 million per year for the city of Tianjin.

Our analysis suggests that incorporating schmeduling behavior substantially changes the welfare implications of the two-part tariff. In the implementation of CBB, the city of Tianjin reported that the one-time cost of introducing CBB—including the cost of installing meters—was about US$99 per household. With the standard discount rate in China in this time period (an annual 3 percent discount rate), the welfare gain based on standard theory suggests that the policy's net present value of the benefits would exceed its costs in about six years. However, once we incorporate the shcmeduling behavior, the CBB is unlikely to be cost-effective for a reasonable range of discount rates. This result suggests that a cost-benefit analysis would substantially overestimate the benefits if schmeduling behavior is not taken into account.

Finally, we also evaluate whether CBB increases or decreases consumer surplus. Consumer surplus is another important welfare measure, as it may be challenging to obtain political support for a policy if it would decrease consumer surplus for many constituents, even when it would result in an overall increase in social welfare. We find that CBB increases consumer surplus by US$172 per household per year on

[26] Note that if the social marginal cost is low enough, the social welfare gain for consumer A could theoretically be positive. This is because the socially optimal level of consumption could then be closer to y_1^A than y_0^A. However, we empirically find a high level of environmental externalities, as shown in Supplemental Appendix C. Thus, in the figure, we draw the social marginal cost curve consistent with our empirical setting.

[27] We calculate the level of negative environmental externalities using ambient air pollution data, reported in Supplemental Appendix C. We add these externalities and the private marginal cost of heating to compute the social marginal cost.

average. We also calculate the change in consumer surplus by home value, which is a proxy for wealth. We find that CBB increases the consumer surplus by US$119 per household per year for households in the first quartile of home values, US$202 for those in the second quartile, US$191 for those in the third quartile, and 249 for those in the fourth quartile. These findings suggest that CBB results in an increase in consumer surplus for a broad set of the population.

VII. Concluding Notes and Directions for Further Research

In this paper, we examine the long-run effects of consumption-based billing, a recent heating price reform in China that replaced a fixed annual payment with a two-part tariff. Using staggered timing in the policy rollout and administrative data on household-level daily heating consumption, we find that the reform induced significant and persistent reductions in heating usage. We find that consumer behavior in our data is inconsistent with the standard economic theory that assumes consumers properly distinguish fixed cost from variable cost and is more consistent with the "schmeduling" model in Liebman and Zeckhauser (2004).

The schmeduling behavior makes the fixed cost directly relevant to the perceived relative prices of goods, and therefore, alters the welfare implications of price, tax, and subsidy designs. For example, our analysis shows that incorporating schmeduling behavior substantially changes the welfare implications of the two-part tariff. Without considering schmeduling, we would conclude that the net present value of CBB's benefits would exceed its costs in about six years. However, once we incorporate schmeduling behavior, our estimates suggest that this policy is unlikely to be cost-effective for a reasonable range of discount rates. This result implies that a cost-benefit analysis would substantially overestimate the benefits if schmeduling behavior is not taken into account.

We describe a few key issues that were not fully addressed in our study and some potential directions for further research. First, our empirical results are based on an environment in which consumers were informed about the two-part tariff when it was introduced, but there was no further education about this pricing. This suggests that further information provision or education on the two -part tariff may help consumers better distinguish fixed cost from variable cost. For example, a typical feature of utility billing is that consumers pay their fixed and variable costs together on a single bill. In a setting with clear separation in payment for these two types of costs (e.g., if the fixed charge were paid in a separate bill—with different timing—from the payment for variable charges), consumers might be able to distinguish them more clearly.

Second, while our findings in Table 4 suggest that households with various observable characteristics all show schmeduling behavior on average, this does not necessary mean that all households are "schmedulers" and that no one behaves as a neoclassical consumer. Although our study does not have the statistical power to credibly estimate this possibility, there is a chance that consumers could be divided into two types: those who behave as "schmedulers" and those who behave as neoclassical consumers. If this were the case, the results of our welfare analysis are likely to understate the welfare effects of schmeduling, as our method uses the average estimates within each decile of the predicted change in average price. Further study on this point could be helpful for understanding more about the potential unobserved heterogeneity in the extent of schmeduling behavior.

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