Group Size, Measurement Precision, and Marginal Abatement Cost Curves: Evidence from Food Waste and Green House Gas Emissions

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Abstract

This paper evaluates two reforms in the pricing of a public environmental service across a metropolitan city, where the reforms were intended to reduce the city's greenhouse-gas (GHG) footprint and the public-finance burden of waste treatment. The first reform encouraged residential blocks to switch their assessment of each household's food-waste-emission-tax bill from a group-metered, equal-division regime to a household-metered regime. The second reform raised the posted unit-emission tax by 58 percent. We first find that the sub-metered billing induced a reduction of 32 percent, or 93 kg of food-waste emission per household-year, with the effect persisting until our data's end for 6.8 years. We next find that the subsequent posted-tax hike did not induce any emission reduction. We estimate that the internal rate of return for the sub-metered-billing reform is 72 percent, with public waste-treatment savings and life-cycle GHG-emission savings contributing similarly to returns, while the price elasticities of abatement are low: an arc elasticity of -0.18 and point elasticity of -0.06, converging to 0 over the domain of the second reform. It appears that the social return from the sub-metering of feedback combined with a small price incentive toward mitigation is high up to an extent, while the household abatement capacity is steeply constrained beyond the front-load return.

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1 Introduction

Urban public services deliver fundamental benefits to society but also facilitate significant environmental emissions.[1] Textbook economic proposals to regulate the externalities involve augmenting the service charge by a tax on emissions (Pigou 1920, Stavins 2011, IPCC 2014, Fang et al. 2015, Goldstein et al. 2020). Even when a public entity posts such a tax increase, however, the increase may not fully pass through to the marginal price experienced by end users if the pass-through process is hampered by market failures such as free riding. Indeed, across a number of urban-public-service-delivery settings, the usage is often metered at a group level rather than at the individual level, with the bill evenly split among individuals in the group.[2] In such cases, reducing the billing-group size, which could sharpen both feedback on service usage and the users' financial incentive to conserve, may confer potential benefits not limited to climate-change mitigation. Yet, such reforms may backfire if, for example, effective within-group regulation or high levels of pro-social motivations preexist to make the costs of the reforms outweigh any benefits (e.g. Andreoni 2007, Andreoni and Bernheim 2009, Barraque 2011). Investigations into the reforms' benefits or lack thereof in the field have been limited by a data challenge: a scarcity of opportunities to jointly observe a billing-group-size reform alongside a posted-tax-increase reform matched to real-world usage data (Olson 1965, Chamberlin 1974, Isaac and Walker 1988, Zhang and Zhu 2011, European Commission 2012).

In this paper, we study two pricing reforms in a public food-waste collection service provided to residential tower blocks (hereafter "towers") across a large metropolitan city. The reforms impacted approximately one half million, or eighty percent, of all households residing in the city. The data include monthly, tower-level billing-and-meter observations spanning seven years encompassing periods both before and after the implementation of the two reforms, allowing us to overcome data limitations faced by previous works. We first evaluate the causal effect of the adoption of a smartcard-based sub-metered billing system over the status quo of tower-metered building system.[3] We then evaluate the causal effect of a subsequent, government-induced, posted-tax hike on emissions from

[1] Electricity, heating, water, sewage and waste-management services, for example, are estimated to account for 21 percent of global greenhouse-gas (GHG) emissions (Ritchie and Roser 2020).

[2] For instance, cities in the US and Europe have historically employed one meter per building for water usage (Cowan 2010, Barraque 2011, OECD 2013). The same has also often been true for electricity and heating usage (Dewees and Tombe 2011, Elinder et al. 2017, Terés-Zubiaga et al. 2018, Jessoe et al. 2020).

[3] The system, technically known as the radio-frequency identification (RFID) system, relies on electromagnetic fields to automatically identify and track tags attached to objects and is used widely across manufacturing and transit transportation industries.

towers treated with sub-metered billing relative to emissions from towers remaining with group-metered billing. The two events generate two clusters of marginal price changes, which allow us to study price elasticities in the two separated areas of the households' marginal-abatement-cost curve. That is, we test to what extent the price signals reveal the households' capacities for abatement across the range of quantities observed in the data. We also trace out the group-time dynamics of the sub-metered billing treatment effect to (i) test against potentially confounding influences of endogenous adoption and (ii) examine the long-run effect up to seven years.

Our empirical setting is the Gwangju Metropolitan City—one of the largest cities in South Korea. Following the central government's mandate to expand a unit-based food-waste pricing policy in 2013, every tower in Gwangju has been subject to a variable food-waste-emission tax during our sample period between 2014 to 2020. At first, all but 18 towers were under the group-metered billing regime. Recognizing the potentially limited incentives in waste reduction, Gwangju has expanded a smartcard-based sub-metering system over multiple years. As for the second reform, the city increased the posted tax on March 1st, 2020. The initially posted emission tax was KRW 63 or 5.7 cents per kg, but this was raised to KRW 100 or 9.1 cents per kg via the tax-hike reform.[4]

The empirical analysis reveals three key results. First, we find that reducing the billing-group size from towers to households reduces per apartment food waste quantity by 32 percent. This result is robust to various sample cuts as well as estimation methods. In particular, we estimate the policy effect using both a conventional two-way-fixed-effects (TWFE) model and the group-time average-treatment-effects-on-treated (GTATT) models to cleanly study aggregate and dynamic effects (Goodman-Bacon 2018, Baker et al. 2021, Callaway and Sant'Anna 2021). We find that the estimated effects closely overlap, consistent with a sharp impulse response.

Second, the individual metering effect is persistent over time with relative emission reductions of 29.3 percent after three years, 26.4 percent after six years, and 28.7 percent after 6.8 years. If we restrict our purview to six years, we observe a moderate fade-out effect on the order of 2 percentage points per annum. The magnitude of this fade-out effect is smaller than those reported for behavioral nudges in a different resource consumption setting (Allcott and Rogers 2014). The finding is consistent with Ito et al. (2018), who find that economic incentives can have effects that are more persistent than purely behavioral suasion.

[4] The representative household in the sample produces approximately 300 kg of food-waste emission per year.

Third, we compare the effect of sub-metered billing to the effect of increasing the posted-emission tax on food waste emission. We reject that the sub-metered towers reduced waste emission by more than their group-metered peers, despite the fact that the magnitudes of the marginal price changes were similar across the sub-metering reform ($5.7/N \rightarrow 5.7$ cents/kg, where N is 60 or above) and the tax-hike reform ($5.7 \rightarrow 9.1$ cents/kg). The results together suggest (1) that sub-metering can pay an important role in the mitigation of environmental externalities; (2) that the capacity of household abatement may be steeply limited beyond a certain point; and (3) that increases in the marginal price of emission beyond this point would largely be a transfer between households and government.

Using empirical estimates, we conduct a simple welfare analysis. For the benefit of the sub-metering, we consider savings on government spending and GHG reductions throughout the life cycle of the wasted food. For the cost, we take into account the installation and operational costs of the smartcard system as well as household-abatement-effort costs. We find that the internal rate of return (IRR) of the sub-metering is 72% over five years.[5]

This paper contributes to three strands of the literature. First, it adds new evidence to research on the relationship between group size and public-good outcomes that has thus far seen a wealth of theoretical and lab-experimental results but relatively few empirical causal estimates from the field. Second, it contributes evidence to research on the effects of pricing reforms in the context of urban public service delivery. Third, this work complements research investigating the design and implications of ecological ("green") taxes including carbon taxes by contributing evidence on the extensive- and intensive-margin abatement responses to taxation.

First, the relationship between group size and public-good outcomes has been a fundamental question in the literature, and to this inquiry we contribute new evidence from the context of environmental emissions. A major focus of this literature has been the free-rider hypothesis: that public-good outcomes deteriorate as the group size grows (Olson 1965, Bliss and Nalebuff 1984, Fries et al. 1991, Bilodeau and Slivinski 1996). Empirical evidence on the group-size effect has mostly been based on lab-experimental data with overall ambiguous effects (Ledyard 1995). In laboratory settings, Sweeney (1973) and Chamberlin (1978) found that public-good outcomes deteriorated with group size; Marwell and Ames (1979) and Goeree et al. (2002) found weak and unclear effects; Isaac and Walker

[5] Five years is the recommended depreciation time frame of the smartcard machine. To the extent that that the machine can be re-installed and the benefits can accrue for many years, the IRR assessment based on five years is conservative.

(1988) found that public-good outcomes improved with group size when the marginal per capita return to collective action was high, with Isaac et al. (1994) finding a plateau between group sizes of 40 and 100. In two rare instances involving field data, Zhang and Zhu (2011) found that increasing group size increased public-good contributions based on a relationship between the Chinese government's access restrictions to Chinese Wikipedia and non-censored authors' contributions to Chinese Wikipedia; Yang et al. (2013) report an inverse-U-shaped relationship between group size and forest cover improvements when forestry management was decentralized to groups of 1 to 16 households in a nature reserve in Sichuan Province, China. To this body of literature, we contribute new evidence on households' emission-abatement responses to reductions in group size from between 60 and 2020 to 1 in a real-world, urban-public-service-delivery setting.

Second, we contribute to the literature on the effect of pricing reforms, in particular, reforms introducing household- or tenant-metered billing (Levinson and Niemann 2004, Gillingham et al. 2012, Elinder et al. 2017, Ito and Zhang 2020, Jessoe et al. 2020). While earlier works mostly focused on identifying the "introduction effect," our work exploits a subsequent, additional posted-tax variation to show that households exhibit little additional abatement responses to the price shock; we do so with the internal validity preserved within the same set of residences. Further, our data allow us to investigate long-run effects—to the best of our knowledge our horizon of seven years is at the tail end of the horizon lengths reported in the literature.

In terms of the study area, we present evidence from waste-management services, which have seen relatively few studies based on high-frequency, long-horizon, panel-time-fixed-effects evidence. In particular, this paper advances works investigating the price elasticity of unit-based waste-pricing policies. We use novel data with a high granularity of sample coverage. Further, our identification exploits not only the introduction of sub-metered pricing but also an exogenous price increase. These differences could explain why we discover an order of magnitude smaller price elasticity than many earlier works (for a review, see Bel and Gradus 2016).[6] We provide empirical evidence on welfare-relevant dimensions that complement conceptual frameworks investigated by authors such as Katare

[6] In our setting, waste is measured in weight as opposed to volume, and earlier works find that weight-based price elasticity could be much smaller because of the "Seattle stomp" effect (Fullerton and Kinnaman 1996). Our paper focuses on food waste while many other papers study municipal solid waste (MSW), which includes, but is not limited to, food waste. Food waste merits a focused attention for a number of reasons, one of which includes that it is the primary cause of landfill-based methane emission which is a function of the biodegradability of the landfill content. We also compare our results with Linderhof et al. (2001), who include a focused analysis on compostable waste.

et al. (2017), who point out that food insecurity is among the social reasons to be concerned about food waste and also that the human-capital dimension, encompassing educational and awareness-related aspects of the problem of food waste, is an important aspect to consider as well. In this work, we consider also the social-welfare implication of the greenhouse-gas aspect and the potentially important role that monitoring-and-feedback technologies may play in determining mitigation.

Third, our work contributes evidence to works investigating the designs and implications of ecological taxes (or "green taxes"), including carbon taxes. Existing empirical evidence on carbon taxes focused mostly on the extensive margin effect, but understanding the intensive margin effect may be just as important as research suggests that at present there remain a large gap between the actual and optimal levels of the carbon tax (Williams 2016, Andersson 2019, Pretis 2019, Carleton et al. 2020, Rafatya et al. 2021).[7] Evidence remains limited on how economic agents might respond along the trajectory between this gap as the posted tax and/or the marginal price moves closer to the social cost. Our finding suggests that the end-household-users could be highly price inelastic after a front-load response to a small marginal-price incentive.

In addition to making contributions to the academic literature, our findings have climate-change policy implications. Food waste is estimated to account for 8 percent of the total global GHGs throughout its life cycle (Scialabba 2015).[8] Thus, encouraging food-waste reduction through pricing reforms could generate benefits, which we quantify in this setting (Foley et al. 2011, IPCC 2018).

The paper proceeds as follows. Section 2 provides further descriptions over our empirical setting of the Gwangju City's residential towers and the city's unit-based food-waste pricing policies. Section 3 details the data sources and provides summary statistics. Section 4 lays out the empirical strategy. Section 5 presents the results. Section 6 concludes.

[7] A part of the reason is that the social cost of carbon remains a subject of active research, yet the current economic and scientific consensus seems to be that the prevailing posted-emission tax is too low let alone the household-facing marginal price—the average regulated carbon price in the 20 largest economies is approximately USD 4.0 a metric ton, whereas the IPCC recommends a carbon price between USD 135 and USD 5,500 by 2030.

[8] The figure is an estimate of the Food and Agriculture Organization of the United Nations (FAO). Landfills in particular are estimated to account for 2-5 percent of the global GHG emissions, while the World Bank estimates that 44 percent of all waste generated globally is food waste (IPCC 2013, Kaza et al. 2018, Zhang et al. 2019, Ritchie and Roser 2020).

2 Background

Gwangju is a metropolitan city with a population of 1.44 million. The city's food-waste quantity per capita tends to be the highest among the seven metropolitan cities of South Korea.[9] The city has considered cutting food waste as one of the most important GHG-mitigation policies (Gwangju Metropolitan City 2013, 2016). In addition to concerns over GHG emissions, food waste has created substantial financial and political burden for the metropolitan government. The level of the emission tax has never been high enough even to recover the overall waste-management cost. The city spends more than USD 20 million on food-waste services alone.[10] Moreover, the city's processing sites' combined capacity stands below the level required to handle the quantity generated, pushing the city to ship its food waste to neighboring municipalities.[11] Doing so is not only expensive but can invite political complications between the city and host municipalities (Gwangju Metropolitan City 2016).

As eighty percent of the population in the city reside in residential tower blocks producing over 60 percent of the entire food waste in the city, cutting food waste from towers was viewed as a priority by the city government.[12] The city decided that reducing food waste from the towers would remain challengnig so long as the financial incentives remained weak. Specifically, the tower residents paid a fixed monthly fee (KRW 1,200 or USD 1.1 per household) irrespective of the waste quantity generated until 2013.[13] After July 2013, following the central government's mandate to expand the unit-based food-waste pricing scheme to reduce food-waste emissions, the city government began charging a variable tax (KRW 63 or USD 0.057 per kg) on food-waste emissions (GGC 2010). However, given that food-waste emissions in towers were collected using a common dumpster, which does not allow for detecting the individual household's contributions, the variable pricing was administered at the tower level, and the fee was divided equally among the residents. Given that the number of apartments within a tower block can be in the hundreds or even in the thousands (figure 2.2 (a)), the marginal price faced by each individual household under the group-metered billing was low. Recognizing the potential limitation of the group-based metering, both the central and the city gov-

[9] The city's policy note speculates that this may be due to its unique culinary culture, well-known not only for its taste but also for its serving portions. It is unclear, however, how the city's restaurant culture may affect the city's household emission levels.

[10] The revenue has covered 42 percent of the operation cost in a typical year (Gwangju Metropolitan City 2016).

[11] Constructing a new food-waste processing site could be a solution, but given the political risk of the enterprise, the city does not consider it as a viable, short-term solution.

[12] The rest was coming from landed houses, restaurants and bulk generators.

[13] We use an exchange rate of 1 USD = 1,100 KRW throughout the paper.

(a) Group-Metered (1/N Pricing) (b) Household-Metered (Unit Pricing)

Figure 2.1: Two Different Types of Food Waste Emission Pricing Regimes

ernments pushed for instituting the smartcard-based food-waste-tax billing system across the towers, which allows for household-metered (or "sub-metered'') billing. The system consists of an ID card for each household, an electric weighing machine combined with a smartcard receptor and a central server that communicates with each electric weighing. The weighing equipment incubates a large waste bin and has a lid that opens if a resident tags a uniquely identified smartcard on the sensor (see figure 2.1). At the time of disposal, the machine weighs and informs the resident of the weight and the corresponding fee amount. During the majority of sample period (Feb 2014 - Feb 2020), the per KG fee was KRW 63 or 5.7 cents, but the fee was increased to KRW 100 or 9.1 cents in March 2020. The average fee across the sample period was 2.0 cents/kg. Five district governments in Gwangju collect a monthly fee from each tower's management office, and the management office collects the fee from its residents.

Despite its benefits, the smartcard system has taken many years to roll out. The system is somewhat costly, each machine allowed to serve between 60 and 100 households costing USD 1,818.[14] Some towers did not want to pay the relatively small operation cost of USD 10.91 per machine-month, even thought this fee would get divided over 60 households. Each year, the district offices solicit applications for the smartcard system, and the system gets installed in towers that manage to obtain enough consent from its residents. A conversation with a city official suggests that who gets elected as the president of the resident association is one of the most important factors in determining whether a tower applies for the smartcard system. Figure 2.2 panel (b) shows the number

[14] The metropolitan government amended its ordinance to mandate newly built towers to adopt the sub-metered billing system. We drop these from the analysis, because they are always-treated units.

of households using the smartcard system, namely the individual metering system, has increased over three fold between 2014 and 2020, covering approximately 50 percent of the entire towers by the data's end.

As the smartcard system has never been used for food-waste collection before, a typical tower has about 2-4 weeks of pilot period.[15] During the pilot period, residents are still charged as a group but are required to use the smartcard system for their waste disposal. It is useful to note that the equipment starts informing users how much waste they have emitted. That is, the residents start getting feedback even during the pilot period while the household-variable pricing kicks in only after the actual billing-regime switch date.

3 Data

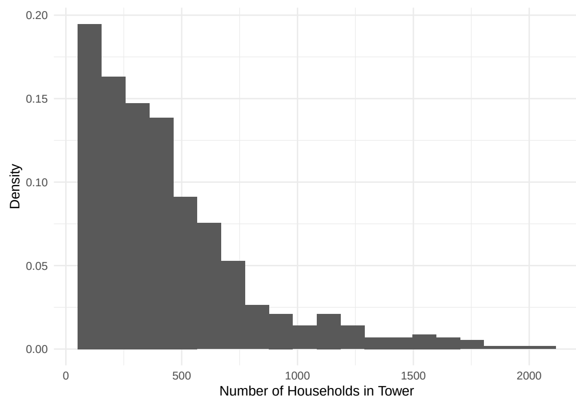
3.1 Data Description

We obtained administrative data on tower-level monthly food-waste quantities from five separate district offices in Gwangju Metropolitan City through several separate Official Information Disclosure Act requests (South Korean equivalent of FOIA). The present paper's analysis is based on the administrative dataset from four districts.[16] The waste data was collected for the emission-tax billing purposes and thus is highly credible. The data cover all towers in the city from January 2014 to December 2020. At the towers with sub-metered billing, the waste quantity is automatically recorded each time households dispose of their waste. At the towers with group-metered billing, the waste quantity is documented each time the waste truck retrieves the waste. In addition to the food-waste quantity, the data document each tower's number of units; per kg posted tax; the monthly emission tax bill; and the start date of the sub-metered billing. Because the data was populated for billing purposes, the weight information in the data is highly credible.[17] We apply a few sample restrictions.

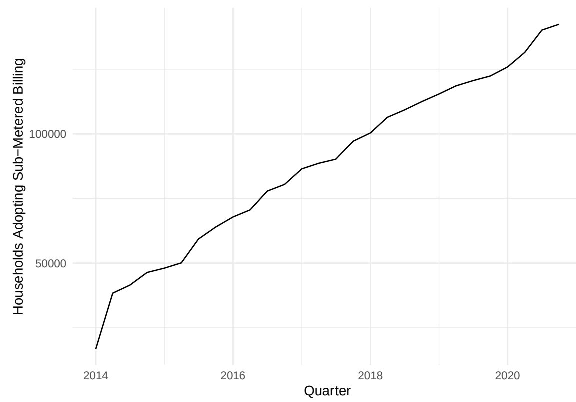
[15] A subset of district offices separately reported pilot and actual start dates; the average pilot period lasted for 18 days. Conversation with other district offices also confirmed that 2 to 4 weeks is a typical pilot period for a typical tower.

[16] We are still in the process of compiling information from the fifth (the smallest) district and other tower-level characteristics.

[17] An additional data work in progress is combining the food-waste data with two datasets: (1) apartment-transaction data from the Ministry of Land, Infrastructure, and Transportation, which documents every apartment transaction for our sample period. The collected information includes tower name, transaction date, price, unit size (square meter) and construction year with the exact address. The second dataset has tower-level electricity and water consumption records. We have acquired these data sets from Korea Electric Power Corporation and K-Water, respectively, which are public-utility companies.



(a) Distribution of Tower Block Size



(b) Number of Households Adopting Sub-Metered Billing over Time

Figure 2.2: Characteristics of Gwangju Tower Blocks. Panel (a) plots the distribution of residential tower block size in terms of the number of households. Panel (b) shows the number of households using the smartcard-based household-metered billing system plotted across quarters.

First, we remove towers with the number of apartments below 60. The city government considered these towers too small and allowed them to emit their food waste via sticker-priced bags instead of via the waste trucks, and hence these households can show zeros in the data. Also, the government stipulated that only for towers with 60 or more households were eligible for the sub-metering infrastructure. To obviate these comparability issues, we remove the smaller-sized towers. Second, we remove newly built towers (towers built after January, 2014) to limit composition effects. The data after these steps constitutes our "All" observations.

For our "Balanced" (preferred) sample, we apply a few additional restrictions. First, we keep a balanced panel of towers, namely those with 84 observations throughout the sample period. This is to minimize any composition effects in our estimation. Second, we remove towers with missing waste quantity information caused by arbitrary gaps in the district-office provided data. After applying these three sample restrictions, we end up with 47,880 observations or 570 towers, representing 255,143 households.

3.2 Summary Statistics

Table 3.1 presents summary statistics for key variables used in the analysis. To calculate these, we aggregate each tower's monthly observations up to the year 2014 level. We report the number of households and baseline food-waste quantity per tower. Per household food-waste quantity is calculated by dividing the tower-wise monthly food-waste quantity by the number of households.

In the first column, we present our balanced sample. In the second and third column, we split the sample into "Never-" and "Ever-Treated" towers. Note that about half of the sample towers get eventually treated by 2020. As some of the towers in the third column get treated in 2014, we additionally present baseline characteristics for Ever-Treated towers after 2015 in the fourth column. In the final column, we present the difference between this group and the Never-Treated towers.

Table 3.1: Characteristics of Gwangju Metropolitan City's Residential Tower Blocks in 2014

	Balanced	Balanced, Never Treated	Balanced, Ever Treated	Balanced, Ever Treated after 2015	Difference (Ever Treated after 2015 - Never Treated)
N of households ("HH") per tower	448	399	495	513	115
	(358)	(354)	(356)	(352)	(33.4)
Mean emission per HH (kg)	291	290	291	326	35.2
	(84)	(88.8)	(79.1)	(63.9)	(7.56)
Log emission per HH	5.61	5.59	5.63	5.76	0.175
	(0.419)	(0.489)	(0.335)	(0.22)	(0.038)
N of HH per tower, weighted by tower size	734	712	751	754	42.2
	(465)	(477)	(455)	(449)	(43.1)
Emission per HH, weighted by tower size	289	284	293	323	38.8
	(80)	(85.1)	(75.7)	(62.8)	(7.03)
Log emission per HH, weighted by tower size	5.61	5.58	5.64	5.76	0.177
	(0.372)	(0.436)	(0.312)	(0.211)	(0.033)
Number of towers	570	282	288	185	467
Number of HH in towers	255,143	112,451	142,692	94,994	207,445
Total emission across towers (Kg)	73,775,922	31,989,840	41,786,081	30,711,946	62,701,787

Note. Observations are Gwangju Metropolitan City's residential tower blocks ("towers") across all districts except Dong-gu. Columns 1 through 4 report means and standard deviations of characteristics for varying sets of towers. Column 5 reports the difference (and the standard errors) between the fourth column and the second column. Column 1 subsets the sample of all (570) towers with no characteristic missing in any of the 84 sample months between 2014 and 2020. Column 2 subsets the Never Treated towers, where "Treat" indicates sub-metered billing. Column 3 subsets the Ever Treated towers. Column 4 subsets Ever Treated towers after 2015. Rows 3 through 5 apply tower size analytic weights.

A few points are worth noting. First, the average tower has about 450 residences. These tower blocks can have multiple buildings within each block and represent one of the most common living arrangements in South Korea. When weighted using analytic weights, the difference between the Never- and Ever-Treated towers becomes statistically insignificant. Second, the average household produces roughly 290 kg of food waste per year, which is approximately 30 kg higher than the greater Seoul area and is comparable to the quantities emitted in the EU or North America (Gustavsson et al. 2011, Lee 2021). The difference column suggests that the Ever-Treated towers produce more waste than Never-Treated towers both in levels and logs (approximately 19 percent). We conduct multiple tests against potential biases induced by this endogeneity in section 5.

4 Empirical Strategy

We exploit a staggered adoption of sub-metered billing to estimate the causal effect of free riding on household food-waste emission quantity. We first estimate the treatment effect using a conventional two-way fixed effect (TWFE) approach as in equation (1).

$$\ln(Emission_{jt}) = \beta SubMeteredBilling_{jt} + \alpha_j + \theta_t + \epsilon_{jt} \tag{1}$$

Here, Y_{it} is per household food-waste emission quantity for tower j in year-month t. $SubMeteredBilling_{it}$ is a dummy variable takes 1 if tower j in year-month t is under individually-metered taxation and 0 otherwise. We include tower- and time-fixed effects to control for unobserved heterogeneity across different towers and also year-month-specific common shocks across towers in food waste emission. β estimates the effect of sub-metered taxation on per household food-waste emission quantity.
Further, to leverage the posted-tax increase in March, 2020, we interact $SubMeteredBilling_{it}$ with $PostedTaxHike_{it}$ as equation (2) where $PostedTaxHike_{it}$ is an indicator which takes 1 if the disposal date is after February, 2020.

$$\ln(Emission_{jt}) = \gamma_1 SubMeteredBilling_{jt} + \gamma_2 SubMeteredBilling_{jt} \times PostedTaxHike_{jt} + \alpha_j + \theta_t + \epsilon_{jt} \tag{2}$$

Note, γ_1 illustrates the treatment effect before the tax hike, while γ_2 is the differential treatment effect after the tax hike. For instance, if $\gamma_2 < 0$, it indicates that the individually-metered taxation's effect on food waste reduction is larger when the tax rate is higher.

While equation (1) represents a widely-used empirical method, recent advances in the econometrics literature have pointed out that β in equation (1) might not be interpreted as the estimand of interest such as the Average Treatment Effect (ATE) or Average Treatment Effect on Treated (ATT) especially when there is a dynamic treatment effect (Goodman-Bacon 2018, Callaway and Sant'Anna 2021).[18] Thus, we follow Callaway and Sant'Anna (2021) and estimate ATT using both the Never Treated and Not Yet Treated observations when applicable to complement the information from our TWFE estimates.

The estimation works in two separate steps: (i) identify policy-relevant disaggregated causal parameters and (ii) flexibly aggregate these parameters. In practice, the disaggregated causal parameters are "group-time ATTs": $ATT(g,t) = \mathrm{E}[Emission_t(g) - Emission_t(0)|G_g = 1]$. $ATT(g,t)$ is the ATT for units in group g at time period t where g is the time period of the initial treatment. The estimator is identified under the parallel-trends assumption that naturally extends the version from the canonical difference-in-difference framework (Callaway and Sant'Anna 2021). Namely, in the absence of the treatment, the average untreated potential outcomes of the units in group g and the "never treated" group should follow a parallel trend in all post-treatment periods $t \geq g$.[19]

We estimate $ATT(g,t)$ for each g for $t \geq g$ periods using the regression method as in equation (3). Observe that equation (3) shows a simple weighted average of the within-unit long differences in the outcome variable.

$$ATT(g,t) = \mathrm{E}[\frac{G_{jg}}{\mathrm{E}[G_{jg}]}(Emission_{jt} - Emission_{j,g-1} - \mathrm{E}[Emission_{jt} - Emission_{j,g-1}|C = 1])], \quad (3)$$

where $C = 1$ represents the Never-Treated units. We aggregate the estimated $ATT(g,t)$ in three different ways. First, we aggregate $ATT(g,t)$ for each g over t. This aggregation allows us to average the treatment effect over time period for groups treated at different time periods. These parameters are useful to test if unobservably different units selected into treatment timing at different times. Second, we further aggregate ATT for each g into a single number. This corresponds to β from equation (1) in a sense that this parameter summarizes the overall effect of treatment across all groups that

[18] Baker et al. (2021) shows that the equation (1) produces an unbiased true ATT when (1) there is single period of treatment or (2) when treatment effect is homogeneous over units or over time.

[19] An analogous assumption is needed when we use the Not-Yet-Treated units as well as the Never-Treated units as the control group. We present results from these two different approaches to constructing the control group.

are ever-treated. Finally, we aggregate $ATT(g,t)$ in event time. This allows us to understand how the treatment effect evolves over time and also compare pre-treatment trends between treated and control groups[20]

The source of variation comes from the individual metering expansion. We must note that the towers apply for the smartcard system installation and hence there could be a potential selection problem. In the next section, we examine any potential differences between the Never-Treated and Not-Yet-Treated towers, differences in the treatment effects across treatment timing, and evidence for any pre-trends.

5 Results

In this section, we report causal estimates of the impacts of (1) a switch from from group-level billing to household-level billing ("Sub-Metered Billing") and (2) a subsequent government-induced increase in the marginal price of emission ("Posted Tax Hike") on group-level food-waste emissions. Subsection 1 presents aggregate estimates. Subsection 2 presents group-time ATTs, investigating the long-run dynamics of the aggregate impact and the balance of the ATTs on treatment timing. Subsection 3 reports estimates disaggregated by contributions from the switch to Sub-Metered Billing and the government-induced Posted Tax Hike event.

5.1 Aggregate Estimates

In table 5.1, columns 1 through 4 show that the average emission-abatement effect of switching from group-metered billing to household-metered billing to be precisely between 32 and 33 percent across specifications.[21] Column 1 reports the result from estimating equation 1 on all towers with data recorded in January, 2014, the starting month of our sample period. The coefficient is -0.407 natural log points, suggesting that the adoption of household-metered billing reduced average food-waste emissions by about 33 percent.[22] In column 2, dropping towers with missing data to examine the sample balanced across 2014 to 2020 scarcely changes the estimate to -0.387 natural log points or 32.1 percent, suggesting that the data restriction minimally affects the representation of towers

[20] These correspond to $\theta_{sel}(\tilde{g})$, θ_{sel}^{O}, and $\theta_{es}(e)$ from Callaway and Sant'Anna (2021), respectively.

[21] Recall that these are estimates from regressing the outcome variable, $\ln(Emission_{jt})$, on $SubMeteredBilling_{jt}$.

[22] $1-\exp(-0.407) \approx 0.334$.

Table 5.1: Abatement Effects of Sub-Metered Billing and Pooled Emission Price Changes

	(1)	(2)	(3)	(4)	(5)
Sub-Metered Billing (Tower $\Rightarrow$ Household)	−0.407	−0.387	−0.385	−0.380	
	(0.020)	(0.014)	(0.018)	(0.018)	
ln(Marginal Price of Emission)					−0.059
					(0.002)
Model	TWFE	TWFE	CS	CS	TWFE
Sample	All	Balanced	Balanced	Ever-Treated	Balanced
Num. clus.	692	570	552	270	570
Num. obs.	57785	47880	46368	22140	47880

Note: Observations are residential tower-months in the Gwangju Metropolitan City. The dependent variable is ln(Emission per Household in kg). "Sub-Metered Billing (Tower → Household)" is an indicator variable that equals 1 when a tower switches from group- to household-metered billing and remains 1 thereafter. Columns (1), (2), and (5) are estimated using the two-way-fixed-effects model weighted by tower size. Columns (3) and (4) report the group-time-average-treatement effects estimated at the tower level without weighting (Callaway and Sant'Anna 2020), implemented using R's `did` package. Column (3) uses both the Never-Treated and Not-Yet-Treated towers as control observations. Standard errors are clustered at the tower level.

across the metropolitan.

Columns 3 reports the aggregated group-time ATTs estimated with the balanced sample using the Not-Yet-Treated and Never-Treated towers as the control group.[23] To reiterate, this analysis first estimates the disaggregated effects for each treatment-time group—that is, collection of towers receiving treatment in the same calendar month—and thereafter averages the effects across treatment-time groups. Column 4 repeats the same analysis using the Ever-Treated sample only. Estimates of columns 3 (-0.385 or 32.0 percent) and 4 (-0.380 or 31.6 percent) closely overlap, implying that the Never-Treated towers exhibit comparable time trends as the rest of the Not-Yet-Treated towers over the sample period, serving as practically valid control observations for increased precision. Moreover, the coefficients across columns 1 through 4 closely overlap, suggesting a sharp timing of the impulse response. Anticipating the discussion, we examine dynamic impacts and the balance of disaggreated group-time ATTs on treatment timing in the next subsection.

Column 5 reports the estimated aggregate impact of the marginal price changes—to reiterate, the result of regressing $\ln(Emission_{jt})$ on $\ln(MarginalEmissionPrice_{jt})$. Recall that panel variations in $MarginalEmissionPrice_{jt}$ result from two sources. First, the Sub-Metered Billing event, occurring in different months for different towers, changes the denominator of the marginal price per kg of emission faced by households and hence the marginal price from $\frac{PostedTax_t}{N}$ to $\frac{PostedTax_t}{1}$. Second, the Posted Tax Hike event changes the numerator, $PostedTax_t$, from 5.7 cents/kg to 9.1 cents/kg on March 1st, 2020. The coefficient on $\ln(MarginalEmissionPrice_{jt})$ can be taken as an estimate of the price elasticity of abatement based on this variation. We see that the effect is a precisely esti-

[23] Relative to column 2, column 3 mechanically drops 18 towers beginning the sample period treated in January, 2014.

mated -0.059. With this aggregated estimate alone, it is difficult to ascertain to what extent the first variation contributes to the estimate as opposed to the second variation. This motivates our analysis of decomposing this estimate by the source of the price variation further below.

5.2 Balance on Treatment Timing and Long-run Dynamics of Impacts

Figure 5.1 plots disaggregated effects for each treatment-time group—that is, collection of towers receiving treatment in the same calendar month. The treatment variable is the same as in columns 1-4 of table 5.1, "Sub-Metered Billing (Tower → Household)." The figure does not suggest a clear pattern of coefficient magnitudes across treatment timing, suggesting that (1) influences of each residential tower block's endogenous political economy determining the timing of the selection into treatment do not bias our estimates of the average impact in a substantive manner; (2) potential (out-of-sample) effects on Never-Treated towers may also be similar. If, for example, towers where residents are particularly sensitive to the inequity of the group-metered billing tend to adopt household-metered billing more quickly, then we may expect the treatment effects to be larger for the earlier-adopting towers. If, on the other hand, there are towers where a higher proportion of free-riders tend to delay the formation of a tower-wide consensus for adoption, and free-riders tend to respond more steeply to the adoption of household meters, then we may expect the treatment effects to be larger for the later-adopting towers. The evidence seems to suggest minimal impacts of such influences. Then, too, that that the later-adopting towers do not exhibit smaller treatment effects suggests that the treatment effects may also be similarly large in magnitude for many of the remaining Never-Treated towers.[24]

How dynamic and persistent are these effects? In figure 5.2, we plot dynamic group-time ATTs, the difference-in-difference of the treatment leads and lags, which can be interpreted as tracing the cumulative impulse response of the switch to Sub-Metered-Billing. We plot the maximum number of leads and lags that allowed by the data at 81 months each. While we must note that the estimates are supported by increasingly fewer towers further forward and backward in time, as indicated by the widening confidence intervals, the following two findings appear robust. First, the effects are shown to be persistent, with the differences remaining as large as -0.347 (29.3%) by the third year, -0.306

[24] Granted, the Never-Treated towers are smaller, and hence it could be the case that unobservables about these smaller towers may differently affect these towers than the Ever-Treated towers.

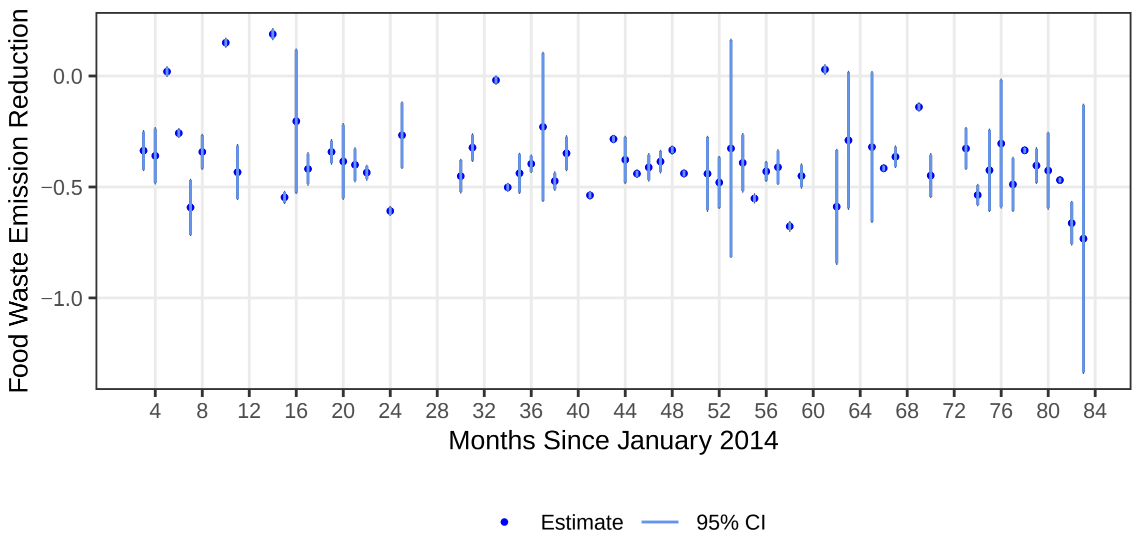


Figure 5.1: Group-time Average Treatment Effects by Treatment Timing. This figure plots disaggregated effects for each collection of towers receiving treatment in the same calendar month. The dependent variable is the natural logarithm of average kg of emissions per household at the tower level. The treatment variable is an indicator that equals 1 when a tower switches from group- to household-metered billing and remains 1 thereafter.

(26.4%) by the sixth year, and -0.339 (28.7%) by the 6.8th year (81 months).[25] Second, if we restrict our purview to within six years, there is evidence of a fade-out effect of between 0.024 and 0.021 log points (5.7% and 4.8%) per annum as calculated by the third and sixth years, respectively.[26] This suggests that while households are quick to begin to engage in conservation efforts in response to the billing-group-size reduction, the price sensitivity may fade over time.[27]

5.3 Impacts of Sub-Metered Billing vs. Posted Tax Hike Event

[25] For the effect by the third year, we take the average of the coefficients 35, 36 and 37 months out; for the effect by the sixth year, we take the average of the coefficients 71, 72 and 73 months out.

[26] For the fade-out effect in year 3 (6), we take the coefficients between months 3 and 36 (3 and 72) and run a linear regression. The t-statistic is 14.0 (29.0). By comparison, South Korea's inflation remained far lower at between 0.4 and 1.9 percent per annum over the sample years.

[27] It is interesting to note that the effects in the first two post periods (months 1 and 2) are shown to be unusually large relative to the trend that follows, while the effect upon impact (at month 0) is relatively muted, and there also seems to be a forward impact (at month -1). The "honeymoon" or "novelty" effect of the first two post periods may be consistent with a number of learning models. As for the earlier months, these may be due to a "trial-period'' effect in towers where the RFID meter was installed a month (or sometimes multiple months) prior to the beginning of sub-metered billing. These may also be due to structural noise in the data caused by differences in the variable definitions employed by different districts: (1) some districts may have reported the RFID-meter-installation month as opposed to the RFID-price-realization month, muting the estimates of the earlier months; (2) some districts may have reported the month of the date of billing as opposed to the month of the emission, shifting some estimated effects forward by a month. Works are in progress to understand these period 0 and period -1 effects in further detail; however, results do not substantively change when we shift the months forward or behind by a month.

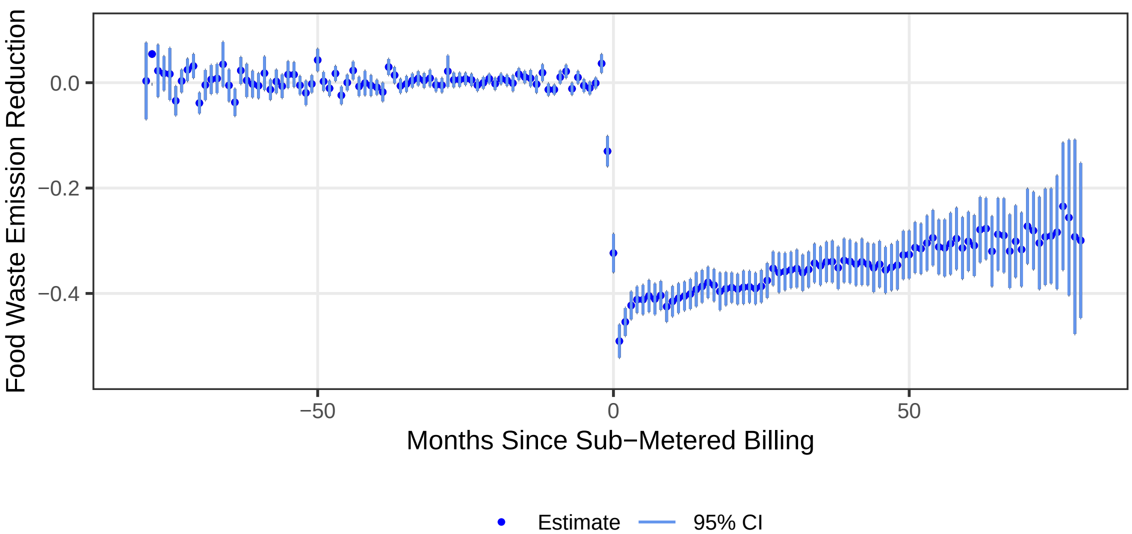
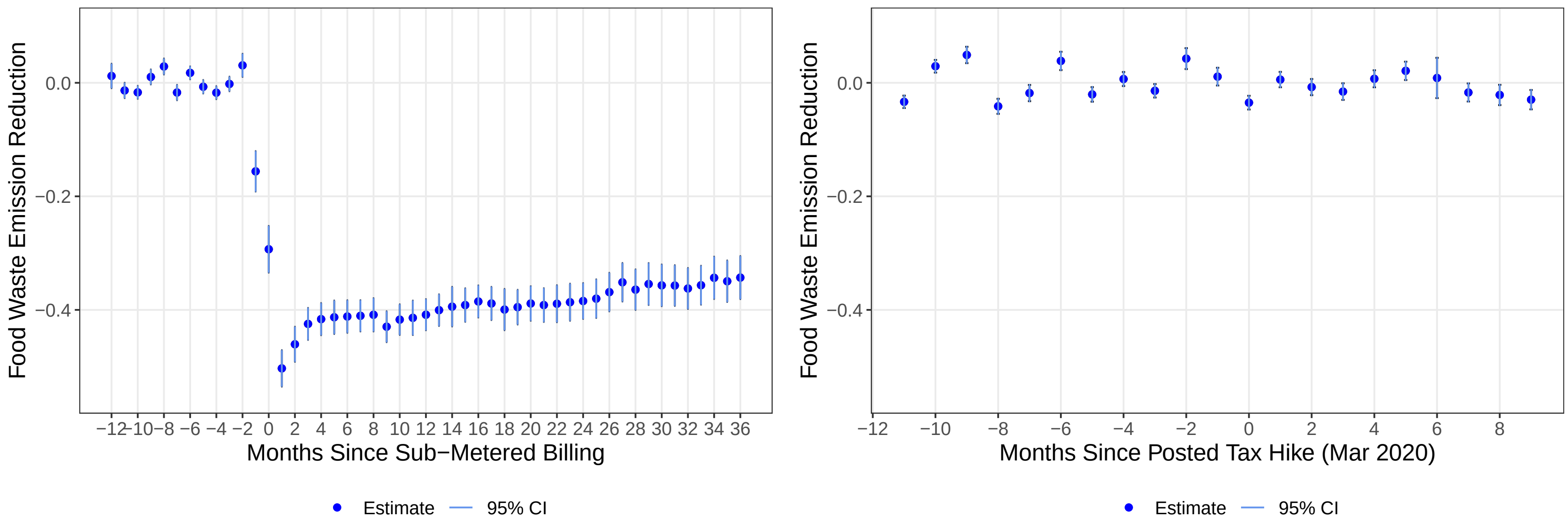


Figure 5.2: Dynamic Treatment Effects. This figure plots group-time difference-in-difference estimates for each treatment leads and lags at the monthly level. The dependent variable is the natural logarithm of average kg of emissions per household at the tower level. The treatment variable is an indicator that equals 1 when a tower switches from group- to household-metered billing and remains 1 thereafter.



(a) Sub-Metered Builling (Tower -> HH)

(b) Posted Tax Hike Event

Figure 5.3: Abatement Effects of Sub-Metered Billing vs. Posted Tax Hike Event. Panel (a) estimates differences over time between towers switching to billing by household meters and towers remaining with billing by group meters. Panel (b) shows the differential effects of the 58-percent posted-tax increase compared between towers switching to billing by household meters versus towers remaining with billing by group meters.

How does the effect of sub-metered billing compare with the effect of a hike in the posted tax of emission? Recall that the government price hike occurred in March, 2020, allowing us an attempt at distinguishing these effects. Figure 5.3 plots the estimated effects dynamically side by side, respectively. In panel (a), we first truncate the data at February, 2020; estimate the dynamic group-time average treatment effect; and then plot the coefficients. We also drop any towers switching to household-metered billing after February, 2020, to maintain sample comparability with panel (b). In panel (b), we redefine the treatment indicator to be the interaction of the Sub-Metered Billing indicator with the time indicator for March, 2020, or after. Thereafter, we estimate the group-time average treatment effects, using emissions data after February, 2020, and keeping the set of towers the same as in panel (a).

The results show that while the introduction of sub-metered billing shows large effects consistent with that seen in the previous section, the subsequent posted-tax hike shows a minimal impact on emissions. Removing the towers treated after February, 2019, minimally influences the pattern of treatment effects, as expected from the lack of treatment-timing effects seen in the previous sections. In contrast with the large sub-metered billing effects, the effects seen in panel (b) do not suggest a clear direction, positive or negative, but rather suggest a zero effect overall.

Table 5.2 formalizes this comparison by way of regression models. In column (1), we use the full balanced sample and include an indicator for the sub-metered billing treatment and an interaction with the posted-tax-hike indicator (which is equivalent to the post February-2020 indicator). If the posted-tax-hike event caused a significant additional abatement response, we would expect the coefficient on the interaction term to be significantly negative. Instead, the coefficient is significantly positive. We consider how this coefficient compares to the fade-out effect of between 0.024 and 0.021 log points (5.7% and 4.8%) per annum found earlier. The confidence interval of the coefficient includes the fade-out effect, although the magnitude of 0.053 log points (13%) still remains larger. In column (2), we run the same regression restricting the time period to post February, 2019, and dropping the towers treated post February, 2019, as in panel (b) of figure 5.3. The first indicator is now subsumed under tower-fixed effects and hence dropped. The interaction coefficient attenuates though remaining significantly positive.[28] Column (3) reports the analogous estimate using the group-time

[28] The attenuation could be due to several reasons. We consider two. (1) Towers treated in earlier years lose the earlier years where they had a lower mean, mechanically raising the pre-Tax-Hike baseline of these towers. (2) Towers treated after February, 2020, may contribute particularly large effects, which could be interesting to probe further in a future

Table 5.2: Effects of Pricing-Reform Events on Food Waste Emission

	(1)	(2)	(3)
Sub-Metered Billing (Tower $\Rightarrow$ Household)	-0.397		
	(0.012)		
Sub-Metered Billing $\times$ Posted Tax Hike Event	0.053	0.040	-0.008
	(0.022)	(0.015)	(0.006)
Model	TWFE	TWFE	CS
Sample	Balanced	Tax Hike	Tax Hike
Num. clus.	570	513	513
Num. obs.	47880	11286	11286

Note: Observations are residential towers. The natural logarithm of emission quantities are regressed on the sub-metered billing event and the interaction of the sub-metered billing event with the indicator for the posted-tax-hike event on March 1st, 2020. Column 1 uses the balanced towers. Column 2 only uses data from March, 2019, dropping towers treated with sub-metered billing after February, 2019. Column 3 uses the same data as in column 2 but instead of the two-way fixed effects model estimates the group-time avearage treatment effect on treated. Columns (1) and (2) are weighted by tower size.

ATT specification—the aggregation of the dynamic coefficients reported in figure 5.3 panel (b). The result is a precisely estimated 0. The magnitude of this coefficient may differ from that in column (2) because the group-time ATT specification aggregates across dynamic effects differently. Suffice it to note here that across the three specifications, we reject that the posted-tax-hike event had a negative impact on emission quantities in towers with sub-metered billing relative to towers with group-metered billing. That is, despite the posted-tax hike being passed through as an effective marginal-price hike for the treated towers relative to the control towers, the treated households did not respond by further reducing quantities relative to the control households.

Table 5.3 repeats the corresponding analysis using log-log specifications. Column (1) shows the estimate for the price elasticity of emissions before the posted-tax-hike event and how the elasticity changes after the posted-tax-hike event. The first estimate, remaining small, appears slightly attenuated from that reported in column (5) of table 5.1; the second estimate is positive and weakly estimated. The relative magnitudes are similar in proportions to those seen in column (1) of the previous column. In column (2), we apply the same restrictions seen in the previous analyses, where we restrict the months to post February, 2019, and drop from the regression the sample towers treated post February, 2019. The price elasticity estimated from the posted-tax-hike event is weakly positive and significant with a t-statistic of 2.29. The evidence rejects that the treated households responded to the effective marginal tax hike by reducing emission quantities more relative to the control households.

Our estimated elasticity is an order of magnitude smaller than the compostable-waste price elasticity

draft. Yet, the overall magnitude of the attenuation remains small.

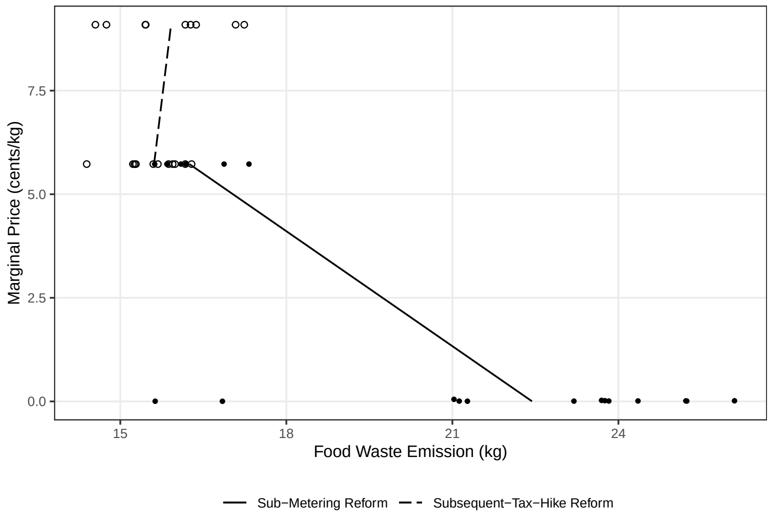
Table 5.3: Price Elasticities of Food Waste Emissions

	(1)	(2)
ln(Marginal Price of Emission)	−0.061	
	(0.002)	
ln(Marginal Price of Emission) x Posted Tax Hike Event	0.008	0.007
	(0.004)	(0.003)
Model	TWFE	TWFE
Sample	Balanced	Tax Hike
Num. clus.	570	513
Num. obs.	47880	11286

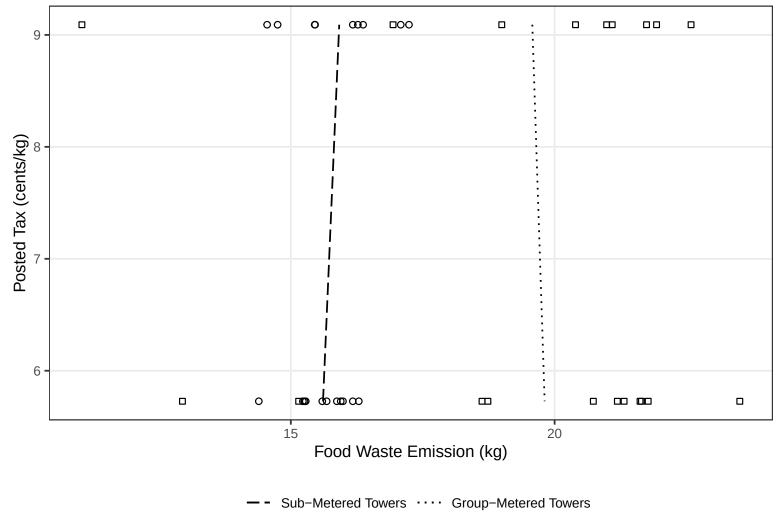
Note: Observations are residential towers. The natural logarithm of emission quantities are regressed on the natural logarithm of the effective marginal price faced by households and the price interacted with the indicator for the emission tax hike event on March 1st, 2020. Column 1 uses the balanced towers. Column 2 only uses data from March, 2019, dropping towers treated with sub-metered billing after February, 2019. Columns (1) and (2) are weighted by tower size.

found in an earlier work. For instance, Linderhof et al. (2001) found a short-term price elasticity of -1.1 for compostable waste in a small municipality of 3,000 residents in the Netherlands. In their work, the estimate was based on an "arc"-elasticity calculation depending on the average level of price in the data which stood at 34 cents per kg. In hindsight, some choice of a base was necessary in their setting because the price variation in the municipality data was from 0 to 43 cents per kg, which mathematically does not allow one to estimate a log-log specification (the logarithm of 0 is undefined). When we estimate the same "arc" elasticity using our estimated change (32 percent) with our estimated average price (2 cents per kg), we obtain -0.180, still far smaller than the earlier estimate. An intuitive way to rationalize this discrepancy seems to be that the previous estimate confounds the front-load "introduction" effect with the subsequent "price sensitivity" effect. The aggregate impacts implied by their data—on the order of 30 to 50 percent—is comparable to the effects we have found despite the posted-tax prices in our data being nearly an order of magnitude smaller. Conversely, with a marginal price hike in levels nearly an order of magnitude higher than that observed in our range of price hikes, they found an aggregate impact not far greater than ours, lending additional support to our conclusion that household abatement responses in these settings are characterized by a lumpy front-load return from small albeit sharp increases in household-level accountability, followed by a highly price-inelastic portion of the household abatement curve, suggested to resemble a vertical line.[29]

[29] There may be other differences in the characteristics of households also affecting the differences in the estimates, but we argue that these potential differences may also further accentuate, rather than detract from, the derived conclusions. In urban South Korea, home composting is not a scalable option compared to the Dutch municipality setting characterized by gardened suburban homes. The fact that the Gwangju tower residents also generated abatements nearly as large in magnitudes reinforces the weight of the evidence for large returns in low-cost, indoor, home-based abatement efforts and negligible capacity for abatement thereafter.



(a) Comparison of Abatement Responses: Sub-Metered-Billing Reform vs. Subsequent-Tax-Hike Reform



(b) Comparison of Abatement Responses to Posted Tax Hike: Sub-Metered Towers vs. Group-Metered Towers

Figure 5.4: Binned Plots of Emission Quantities against Posted Taxes and Marginal Prices. Panel (a) shows the emission quantities before (below) and after (above) the marginal-price hike induced by the sub-metered-billing reform against the quantities before and after the marginal-price hike induced by the posted-tax-hike event. Panel (b) shows emission quantities before (below) and after (above) the posted-tax hike event on March, 2020, across sub-metered-billing towers and group-metered billing towers. The lines show fitted slopes of the responses of the respective meter groups to the indicated events.

Figure 5.4 uses binned plots in original price-quantity (or "level-level") scales to recapitulate these relationships. Plotted on the y-axis are magnitudes of the per-unit emission charge in cents/kg, either as household-facing marginal price (panel a) or the government's posted emission tax (panel b). Recall that the household-facing marginal price is the posted emission tax divided by N in group-metered towers. Panel (a) compares abatement responses across the sub-metered-billing reform vs. subsequent-tax-hike reform events. The sub-metered-billing reform shows a high degree of food waste emission reduction mechanically against the household-facing marignal-price hike induced by sub-metered billing. In constrast, the posted-tax-hike reform shows no response—or even a borderline perversely sloped response—to the marginal-price hike induced by the subsequent tax-hike reform.[30] Panel (a) compares abatement responses to the government's posted-tax hike across the sub-metered towers and the group-metered towers. It can be seen that across both groups, the response slopes are approximately vertical, and the two clusters of points are distinguished by a horizontal shift. The two figures together reiterate (i) how a posted-tax change may fail to pass-through to marginal-price faced by agents in the presence of free riding; (ii) how a more granular allocation of feedback and incentives induces a large abatement response; (iii) how steeply sloped the abatement response is to any further increase in the emission tax.

5.4 Welfare impact estimates

In this section, we evaluate the cost effectiveness of the reforms. We combine the coefficients and quantities estimated above with off-the-shelf cost parameters referenced from the IPCC guidelines, the national GHG inventory of Korea. the Ministry of the Environment documentation as well as the broader scientific literature (IPCC 2006, Ministry of Environment 2010, GHGIRC 2014, Poore and Nemecek 2018, Lee 2021). We combine these figures to assess the annualualized welfare flows as well as the implied IRR. We also evaluate the minimum-threshold group size or "density" of the residential block at which a positive IRR may be delivered.

On the cost side of the sub-metered-billing reform, we have (i) the smartcard-machine installation cost, (ii) the machine's monthly management fee and (iii) the welfare costs of household effort.[31]

[30] We plot raw (weighted) bins quantiled by tower size and do not incorporate any time- or tower-fixed effects in order to present the axis magnitudes in the most intuitive manner. If we plot the residuals after taking out the fixed effects, neither the slopes nor the implications substantively change (omitted from presentation due to space and time). As an aside, this shows that disincluding fixed effects do not substantively bias the estimated relationships.

[31] We assume that any additional administrative costs get divided across a large number of households and tend to

In Gwangju, each machine was installed to serve between 60 to 100 households for a period of five years.[32] In the analysis going forward, we assume a block size of 80 households as the reference point. We assume an annual per-household reduction of 92.5 kg, which is 32 percent of the mean of 2014 annual household emission reported in table 3.1. The installation fee has been KRW 2,000,000 or USD 1818.18—borne by the city government—reducing to USD 22.73 per household. This we take to be the initial investment cost of the reform in year 0. The smartcard billing also incurs a monthly management fee of KRW 12,000 or USD 10.91, which when translated to household-year amounts to USD 1.64. Households also incur a monthly abatement-effort cost. We adopt the reasoning that this cost can be no larger than the amount of household tax savings from the food-purchase portion of the emission reduction; this reasoning is based on a conservative assumption (for the benefit side) that all of the rest of the reductions are leaked to landfills.[33] In his paper containing an analysis of upstream benefits from a different region in Korea, Lee (2021) estimated that 69 percent of food-waste reduction in fact stems from food-purchase reductions likely due to more careful meal planning.[34] Hence, we take 69 percent of 5.7 cents multiplied by the annual emission reduction 92.5 kg to bound the annual household effort cost at USD 3.67. In sum, on the cost side, we have the initial investment cost of 22.73 in year 0, and the total management fee plus the household-effort cost of 5.30 from year 1 onward.

On the benefit side of the sub-metered-billing reform, we have (i) methane (CH4) reduction; (ii) nitrogen oxide (NOx) reduction; (iii) upstream carbon-dioxide-equivalent (CO2eq) reduction; (iv) "negative benefit" from the leakage of some food waste to landfills; (v) cost savings from the government's reduced food-waste-treatment burden. For (i) and (ii), we takes estimates from the national GHG inventory (itself based on the IPCC guidelines) of CH4 4 g per kg of food-waste emission and NOx 0.3 g per kg. We assume global warming potentials (GWP) of 30 and 300 for CH4 and NOX, respectively, and the 2021 Environmental Protection Agency (EPA) estimate of the social cost of carbon at USD 51. Together, we arrive at annualized benefits of USD 0.57 and USD 0.42 for CH4 and

zero.

[32] Technically, the machine could serve a smaller number of people. Food waste is corrosive; therefore, the government has followed the manufacturer recommendation to replace the machines every five years.

[33] In reality, individuals may also care to eat more or find yet other means to abate. While these means should involve effort costs as well, reductions via such means would also generate much larger benefits in terms of environmental savings. See further below.

[34] Lee (2021) also found that the introduction of a bag-based, food-waste tax across different municipalities of Korea led to an increase of landfill disposals by a statistically insignificant 5 percent.

NOx, respectively. For (iii) and (iv), we use CO2eq 3.377 kg per kg of food-waste emission for the upstream savings and CO2eq 0.655 kg per kg of food-waste emission for the leaked GHG losses (GHGIRC 2014, Poore and Nemecek 2018, Lee 2021).[35] We again adopt the 69 percent estimate from Lee (2021) and conservatively assume that all of the rest are leaked to landfills.[36] This translates into USD 0.12 of upstream benefits and USD 0.01 of leaked losses per kg of food-waste emission, or USD 11.03 of upstream benefits and USD 0.95 of leaked losses per household-year. Finally, waste-treatment cost savings per kg of food-waste emission minus the waste-treatment cost of food waste leaked to landfills stand at USD 0.13, or when annualized to household-year at USD 11.77 (Ministry of Environment 2010). To recap, on the benefit side, we have USD 0.57 CH4 savings, 0.42 NOx savings, 11.03 upstream CO2eq savings, -0.95 leaked losses, and 11.77 waste-treatment cost savings, for a total benefit of 23.68 from year 1 onward.[37]

The costs and the benefits together imply an IRR of 72 percent, where we have conservatively assumed an investment period of five years.[38] Alternatively, the raw sum of the aforementioned costs amount to USD 49.25 over a period of five years, while the raw sum of the aforementioned benefits amount to USD 114.18. For every dollar invested into the sub-metered billing reform, these figures imply a social value of USD 2.32. We observe that 48 percent of the benefit (i-iv) comes from reductions in related greenhouse-gas emissions, while 52 percent comes from reductions in the public-waste-treatment burden.

Note that, according to the categories outlined above, the costs and benefits of the posted-tax-hike reform are zero—disregarding equity concerns or the government's marginal monetary welfare effect. For one, the tax hike does not involve any installation cost or management fee. For another, it does

[35] The GHG contribution of food waste diverted to landfill is larger than food waste properly segregated and treated.

[36] We argue that this is a conservative assumption. It is useful to note that earlier works on unit-based waste pricing policies have pointed out that illegal dumping or leakage could seriously undermine the benefit of the policy (Fullerton and Kinnaman 1996, Kinnaman 2006). While this is a valid concern, this problem is not likely to dominate the welfare gains in this setting, because a large majority of the estimated welfare gains comes from reducing food waste by purchasing less excessive food in the upstream of the consumption cycle (Lee 2021). Moreover, in South Korea, (1) non-food-waste MSW emissions are covered by a bag-(unit-)based tax system; (2) MSW-emission bags are checked by collectors who have the power to refuse to take the bags if contaminating food waste is found; (3) non-food-waste MSW emissions are also covered by additional, rigorous government audits whereby district government officials randomly open the bags for indications of violations and track down the violating households based on personally identifiable information found in the bags. These reasons suggest households in this setting may face substantive deterrents to leakage. We also conduct our welfare-impact analysis incorporating literature-estimated, upper bound of leakage, and explicitly show this is not a large concern in this setting.

[37] The public cost of treating non-food landfill waste is approximately 1/5 of the cost of treating food waste.

[38] Alternatively, the same welfare flows extended by ten years beget an IRR of 74 percent.

not involve any changes in effort costs or social benefits given that it does not affect emissions.[39]

Indeed, the suggested consequence of the posted-tax-hike reform involves a pure transfer from the household to the government. Such a transfer may make sense to the extent that the government has been operating its waste-management services at a loss (as is commonly the case across the world) and it might be more equitable to align the costs more closely with usage. It may also make sense if the revenues can be directed toward proper climate-adaptation investments and/or if the overall social return from a dollar to the government is high for another reason. Finally, given that the sub-metered billing reduced the scope for free riding in this setting, the reform generated transfers from households that were free riding to households that were not. To the extent that households are averse to the scope of free riding of the group-metered regime, the reduction in the scope itself generates welfare benefits. These aspects are beyond the scope of the current paper.

Lastly, we consider the minimum group size at which the social cost and the social return of the sub-metered-billing reform would be equalized, assuming that the effect estimated in this paper of 32 percent for groups sized 60 and above would also hold true for groups of smaller sizes. Alternatively, this inquiry explores at which minimum density of a residential block would implementing the reform still make a positive social return. The above numbers suggest that the group size at which the IRR would equal 0 is 26.[40] This analysis exemplifies the role that the per-capita cost of the monitoring technology plays in using the technology to reduce the scope of free riding, and how—other things equal—a higher density from urbanization may facilitate such a role. In the case of the Gwangju Metropolitan City, it may be socially beneficial to allow residential blocks with somewhat lower density than sixty households to adopt the reform as well.

6 Conclusion

In this paper, we study (1) to what extent reducing the group size of emission-tax billing from a large N to the level of the household generates abatement effects; and (2) to what extent abatement responds to marginal emission price changes thereafter. We do so by exploiting (1) a staggered ex-

[39] We do not have enough information to assess whether this would increase or decrease leakage, but circumstantial reasons suggest that the magnitude of any such change if at all positive would be small. One reason is that we do not detect any change in emission which serves as a potentially proportional proxy for leakage. Another reason is that the income effect from the small, additional tax burden would lead to a reduction in food purchases and the corresponding decrease in leakage, if at all.

[40] The group size at which the IRR would equal 2 percent, if we take this number to be the risk-free rate, is 27.

pansion of sub-metered billing via a metropolitan food-waste smartcard system; and (2) subsequent level increase in the posted emission tax on the order of 58 percent. We find that per household food-waste emission drops by 32 percent across the sample period, suggesting that households increase efforts to reduce their waste generation. We also find that the subsequent posted-tax increase does not elicit any additional emission from either the sub-metered households or the group-metered households. Put differently, the sub-metered billing effect seems to exhibit a degree of independence from the level of the marginal price beyond a point. This finding suggests there may only be available a limited set of low-cost abatement options for households, which is exhausted when the sub-metered billing is first introduced. This line of reasoning suggests that households' capacity to respond to a financial abatement incentive is front-loaded and limited thereafter.

The findings together suggest high returns from a potentially lumpy abatement effort that can be induced by accurately metered feedback and a small, accountability-aligned price incentive. The findings also suggest that, apart from this range of response, household abatement is price inelastic, evidenced by the treated towers' price elasticity relative to control tower's converging to zero over the posted-tax reform period. In this particular setting, the consequence of increasing ecological taxes beyond this point is likely to involve limited mitigation returns and lean more toward redistribution and adaptation; the desirability of such a policy would depend on whether the political economy of the redistribution process is functioning efficiently.

Limitations of this study are important to note. Our study examines the slopes of the abatement curves within the particular context of food waste emissions by households. While other urban-public-service settings may share similar features—for instance, the literature has reported low price elasticities of electricity use in several contexts—estimating these curves with the internal validity preserved for the same sample of economic agents across settings may be compelling questions for future research. Second, our study did not examine why and how households attained such a large abatement response. Especially puzzling is how the suggested savings from upstream food purchases could be so large in response to a relatively small disincentive levied on downstream emissions—and to what extent such a leverage over the supply chain may be found across contexts beyond this setting. Future studies could seek to understand the relative importance of different motivations and mechanisms of such abatement responses.

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