Heterogeneous effects of waste pricing policies

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Abstract Using machine learning methods in a quasi-experimental setting, I study the heterogeneous effects of introducing waste prices—unit prices on household unsorted waste disposal—on waste demands and social welfare. First, using a unique panel of Italian municipalities with large variation in prices and observables, I show that waste demands are nonlinear. I find evidence of nudge effects at low prices, and increasing elasticities at high prices driven by income effects and waste habits before policy. Second, I estimate policy impacts on pollution and municipal management costs, and compute the overall social cost savings for each municipality. Social welfare effects become positive for most municipalities after three years of adoption, when waste prices cause significant waste avoidance.

Keywords: Waste pricing, Causal effect heterogeneity, Welfare analysis, Machine learning

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Introduction

Waste management generates pollution externalities that are not internalized by households under traditional flat pricing schemes (Fullerton & Kinnaman, 1996). To correct this inefficiency, a growing number of municipalities have implemented Pigou prices (1932) known as Pay-As-You-Throw (PAYT) programs that require households to pay for each unit (per bag, can, or weight) of unsorted waste presented for collection. Theory predicts households will substitute unsorted waste with increased recycling and/or waste avoidance (Smith, 1972).[1] However, empirical estimates of average price effects provide mixed evidence on the magnitude of this reduction as well as the behavioral mechanisms behind it (Bueno & Valente, 2019). Moreover, policy impacts may vary across municipalities depending, e.g., on the adopted price level and household characteristics (Kinnaman, 2006). Disentangling sources of effect heterogeneity is important to tailor effective policies and deliver insights on why some municipalities refrain from PAYT adoption.[2]

In this paper, I examine heterogeneous demand responses to waste prices, and their impact on social welfare. The main challenge in the analysis is that determinants of waste generation and policy adoption are possibly many, and may confound the estimation of causal effects.[3] This motivates the collection of a unique panel of municipalities with a large variation in prices and observables, and the estimation of municipal level causal effects of prices (continuous treatment) via machine learning methods. These techniques, in fact, allow to control for a high-dimensional set of covariates.

I estimate social welfare effects for each municipality by combining price effects on unsorted and recycling waste with their impacts on pollution and municipal costs. First, I find that price levels matter. Higher prices cause larger unsorted waste reductions and recycling increases. Lower prices cause relatively more waste avoidance. Second, municipal characteristics also matter. While elasticities are rather constant at low prices, suggesting nudge effects, elasticities increase at high prices especially for municipalities with low income and little recycling before policy. Third, waste avoidance matters for welfare, and drives benefits for most municipalities after three years of adoption.

This paper contributes to three distinct literatures. First, my work connects to a large and growing literature on waste prices. One set of papers estimates the price elasticity of waste demands or the causal effect of PAYT (binary treatment) on waste amounts.[4]

[1]Waste avoidance is defined as using household effort to reduce total waste (via e.g. product reuse).

[2]In Italy, less than 8% of municipalities adopt PAYT (ISPRA, 2019). Policymakers fear cost increases due to, e.g., higher recycling (Facchini, 2020). For anecdotal evidence, see e.g. Gilli *et al.* (2018b), Allers & Hoeben (2010), Callan & Thomas (1999).

[3] Previous studies suggest that waste generation determinants may also drive policy adoption decisions. These are, e.g., households' income, education and pre-policy waste levels (see e.g. Gradus *et al.*, 2019).

[4]This includes, e.g., Bueno & Valente 2019; Carattini *et al.* 2018; Bucciol *et al.* 2015; Huang *et al.*

Compared to my study, these papers require more restrictive identifying assumptions, e.g., constant price effects across units (effect homogeneity) or deliver less precise estimates, e.g., because they exploit only one specific price change. In Bueno & Valente (2019), we have shown that time-varying effects of unobservables on waste generation lead to bias of the difference-in-differences estimator. The main difference between this paper and past studies is the analysis of heterogeneous policy effects and social welfare, while accounting for a wide range of prices, covariates, and lagged waste outcomes. Moreover, the welfare effects of PAYT policies are largely unexplored. Most relatedly, Dijkgraaf & Gradus (2015) show that waste prices are more cost-effective than other institutional modes of collection.

Second, this paper contributes to studies in behavioral environmental economics, analyzing drivers of waste behaviors.[5] Building on this literature, I rationalize welfare effect heterogeneity by showing that household reactions on recycling and waste avoidance are ambiguous from a theoretical viewpoint. My work also relates to the literature on nudging. Nudges, also in the form of low prices, can correct externalities by reducing, e.g., plastic bag consumption (Rivers, 2017). To my knowledge, my results provide the first empirical evidence of agents' nudgeability to waste prices, and of its social welfare impacts.

Third, to my knowledge, this is the first empirical application of machine learning for causal inference combining continuous treatment, staggered adoption, and self-selection. In addition, while random forests have been successfully applied in labor economics,[6] no study applied this algorithm to high-dimensional problems in environmental economics.

I study waste generation behaviors of about 3,600 Italian municipalities over 2010-2015. Italy provides an ideal setting to study heterogeneous effects of waste prices because both price levels and socio-economic characteristics largely vary across municipalities. I use web scraped and administrative data to construct a new and rich dataset on waste generation and price adoption at the municipal level. The final dataset includes 45 different price levels ranging from 1 to 18 euro (€) cents per liter of unsorted waste, and 90 municipal characteristics that may explain price adoption and waste generation.

To consistently estimate municipal level parameters with high-dimensional data, I use machine learning-inspired matching estimators called generalized Random Forests (RFs) (Athey *et al.*, 2019).[7] Intuitively, RFs partition the large covariate space into small neighborhoods of municipalities mostly similar in those characteristics that drive parameter heterogeneity. Within neighborhoods, I estimate constant treatment effects by the residual-on-residual regression estimator, or R-learner (Nie & Wager, 2019).[8]

2011; Allers & Hoeben 2010; Fullerton & Kinnaman 2000, 1996. See Kinnaman 2014 for a review.

[5]See, e.g., D'Amato *et al.* 2016, Bowles & Polania-Reyes 2012, and Gilli *et al.* 2018b for a review.

[6]See, e.g., Gulyas & Pytka 2019; Athey & Wager 2019; Davis & Heller 2017.

[7]RFs build upon Athey & Wager (2018), Athey & Imbens (2016) and, originally, Breiman (2001).

[8]R-learners are robust to confounding affecting outcome and treatment (Chernozhukov *et al.*, 2017).

The advantage of using RFs is to relax the assumption of constant price effects across municipalities and estimate the full effect distribution with pointwise-consistent confidence intervals. Estimation avoids ad hoc modeling choices, and flexibly accounts for parameter heterogeneity in the large set of (often correlated) covariates.[9] The improvement of RFs vis-à-vis, for instance, theory-informed heterogeneity analysis is to provide with a data-driven documentation of heterogeneous causal effects, as opposed to specification search.

Using these machine learning methods, I estimate municipal level price elasticities of demands for unsorted, recycling, and total waste per capita. The hypothesis of no heterogeneity is rejected for all outcomes. Waste prices cause large unsorted waste reductions driven by increased recycling and, to a smaller extent, waste avoidance. To disentangle sources of heterogeneity, I regress these elasticities on price levels and a parsimonious set of relevant regressors capturing household costs of waste disposal: income, education, and pre-policy waste levels.[10] I find a nonlinear relationship between elasticities and price levels: while at high prices (above 9 cents) elasticities are increasing, elasticities are rather constant at low prices. The estimated variation at low prices is 4% of the variation at high prices. In this range, a one cent price increase reduces unsorted waste by 5 to 10%, increases recycling by 2 to 6%, and reduces total waste by 0.1 to 0.7%. Low prices reduce total waste by more (0.6-0.8%) and increase recycling by less (2.5-3.2%).

I find no evidence of income effects at low prices. This finding suggests that low prices work as nudges, i.e. instruments that influence behavior without budgetary incidence (Farhi & Gabaix, 2020). To provide an intuition, households reducing unsorted waste by one standard trash bag (30 liters) save about 90 cents in the first price quartile (3 cents), while 4 € in the third price quartile (13 cents). As shown for plastic bag consumption (see, e.g., Rivers, 2017), low prices are symbolic but serve to remind households of the costs of waste as well as to promote recycling and avoidance behaviors.

Moreover, I find no evidence of waste prices being regressive at high prices: I estimate that high-income municipalities are less elastic, and pay more under PAYT.[11] This implies that municipalities could increase waste prices without distributional concerns.

Having established heterogeneity of price effects, the second part of the empirical analysis focuses on municipal waste management costs. I estimate that PAYT leaves unit costs of waste mostly unaffected, suggesting constant returns to scale.[12] Next, I simulate the impact of PAYT adoption on social welfare using prior estimates for the relative

[9] Differently, standard regression methods are justified if treatment effects are constant, observables have linear or pre-specified effects, and unobservables are time-invariant (Wager, 2020).

[10] Their relevance is discussed in, e.g., Bueno & Valente (2019). See Gilli *et al.* (2018b) for a review.

[11] Policy costs for rich vs. poor municipalities amount to on average €105 vs. €87 per capita/year. Rich municipalities are defined by having an annual per capita income above the third quartile of €16k.

[12] This is consistent with results on Italian municipalities by, e.g., Abrate *et al.* (2014).

environmental costs of unsorted versus recycling waste (Kinnaman *et al.*, 2014). I find that waste prices can raise social costs, especially when households respond by increasing recycling only. However, after three years of adoption, I predict welfare benefits for most municipalities of on average €30 up to €170 per person. As unit costs of unsorted waste are higher than those of recycling, waste avoidance triggers large welfare benefits. This implies that low prices may be preferable from a social cost perspective although they cause comparatively smaller unsorted waste reductions.

I present additional analyses in support of the identifying assumptions of unconfoundedness and no spillover effects (no waste tourism). Results are also robust to confounding from adoption of weight versus volume systems (see Bel & Gradus, 2016, for a review of past findings). Finally, I contrast my average estimates to the binary treatment case, event-study-like difference-in-differences, and R-learning LASSO regression. Results highlight the importance to account for continuous rather than binary treatment, the bias of difference-in-differences due to violation of its identifying parallel trend assumption, and robustness to the specific choice of R-learning estimator.

The remainder of the paper is structured as follows. Section 1 describes policy background and data. Section 2 discusses the theoretical framework and empirical methodology. Section 3 presents the main results and their policy implications. Section 4 concludes.

1 Background and Data

1.1 PAYT policies

PAYT policies in Italy, as in many municipalities worldwide, require households to pay a price per unit of unsorted waste according to either its volume (per bag or bin) or weight (per kilogram). PAYT fulfills the equivalence principle for which waste service consumers pay for its consumption (as, e.g., for energy and water), and the polluter-pays principle for which households pay according to their unsorted waste.

The baseline policy in both PAYT and non-PAYT municipalities is a flat fee independent of waste quantities, namely, the unit price is zero. Flat fees depend on house (m^2) and household size (number of inhabitants). PAYT municipalities reduce the flat fee to cover only fixed costs of waste management, and implement waste prices to cover variable costs.

Municipalities can decide whether and when to implement PAYT, as well as the price level and collection system. Policy adoption decisions are based on, for instance, goals of waste pollution and management cost reduction. Price levels are set based on lagged and expected levels of waste generation and management costs. Yet, political and socio-economic factors may also matter (see Gradus *et al.* 2019 and Section 1.4 for details).

System choices are also made at the municipal level, and depend on, e.g., demographic, geographic, and cost factors (see Appendix D.4.1 for details). Households can pay either ex ante via prepaid bags or ex post via identification (tag on bags, chip on bins, electronic keys). Generally, municipalities prefer volume over weight systems because this requires cheaper technology (Kinnaman, 2006). Yet, systems may create asymmetric incentives.[13] In sum, drivers of price adoption, waste generation and collection mode often overlap, and are possibly many. Their relevance is, therefore, an empirical question.

Credible enforcement and monitoring systems are crucial for policy success.[14] Illegal dumping is one of the main possible adverse effects. However, after about fifty years of PAYT experiences worldwide, adverse effects seem a bigger fear than reality.[15] In Italy, anecdotal evidence suggests that (i) waste tourism in surrounding municipalities is a rare and short-lived phenomenon, (ii) enforcement and monitoring systems allow to actually decrease illegal dumping episodes, and (iii) waste haulers encourage adoption and deem waste prices as successful (Legambiente, 2017).

1.2 Data

Using web scraped and administrative data, I construct a new municipal level dataset with information on waste prices, waste amounts (main outcomes) and management costs, as well as socio-economic, geographic, and political determinants of waste generation and price adoption. The final database is a panel of Northern and Central Italian municipalities over the sample period 2010-2015. I exclude municipalities with missing values as well as South and Insular Italy because arguably not suitable for being in the control group.[16] Treated municipalities for which second-order lags are missing are also excluded.[17]

The resulting municipalities are 3,574. Waste prices, the treatment, cover 1.7 million people living in 194 municipalities, of which 106 in the North-West, 82 in the North-East, and six in the Center. Most of the treated municipalities implement PAYT for the first time in 2013 (77), while the others in 2012 (48), 2014 (36), and 2015 (33). Figure 1 shows the distribution of PAYT and non-PAYT municipalities in the sample.

[13] For instance, volume systems can encourage waste compacting and be, therefore, less effective.

[14] E.g., trash bins are locked, drones and photo traps track illegal dumping, and reciprocal monitoring is enforced by charging all households in a building for lack of policy compliance (CONSEA, 2019).

[15] See, e.g., Bueno & Valente (2019) for Italy and Skumatz (2008) for the US. The latter found illegal dumping to be a short-term issue which lasts three months or less and involves 3% of municipalities.

[16] Missing values are due to merging administrations and data errors. See Appendix B for details. In South and Insular Italy, as defined by the NUTS 1 classification, there are no treated municipalities over the sample period, and data is incline to significant mismeasurement due to illegal disposal (ISTAT, 2018).

[17] Policy adoption decisions correlate with second- or earlier-order outcome lags. First-order lags are biased predictors due to anticipation effects. See Section 3.4.1 for details and estimates.

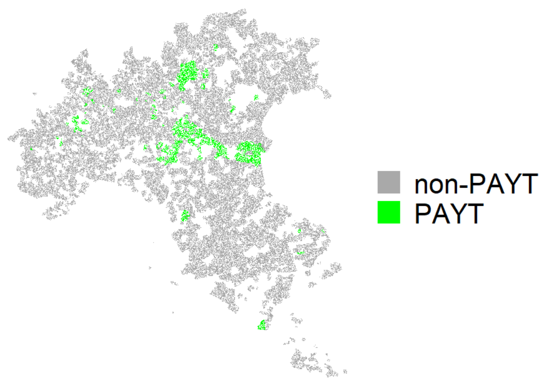

Figure 1: Map of PAYT and non-PAYT municipalities in the sample. White areas show the excluded South and Islands, municipalities with missing values, and those adopting PAYT before 2012.

This data allow for (counterfactual) predictions of policy and welfare effects for 26 million people (45% of the population) living in 3,380 municipalities without PAYT.

Variables come from a variety of sources. Waste generation and management costs are from the National Environmental Protection Agency (ISPRA). Municipal socio-economic attributes are from the National Institute of Statistics (ISTAT) and web scraping the online database of comuni-italiani.it. Political variables on, e.g., municipal elections and mayors' characteristics are obtained from the Ministry of the Interior upon request. Geographic variables measuring, e.g., the distance to waste treatment sites are geocoded using the software R and data of the European Pollution Release and Transfer Register (E-PRTR).

At the time of writing, there is no open-source database on PAYT prices in Italy. I acquired the list of PAYT municipalities from the National Environmental Protection Agency (ISPRA), the National Association of Italian Municipalities (ANCI), and from waste hauling companies.[18] I collected a cross-section of price data directly from municipalities and companies upon request, and indirectly from municipal balance sheets. I created a database containing average or median prices over the analyzed time period depending on the acquired data. Detailed information on data management, variable denomination and descriptive statistics is presented in the Appendix B.

1.3 Summary Statistics

Table 1 compares key attributes of never-treated (non-PAYT) and treated (PAYT) units before policy. The main outcomes are kilograms of per capita unsorted (UW), recycling waste (RW), and total (TW) which is the sum of the previous two. Full summary statis-

[18] E.g., Aimag, Ascit, Clara, Dolomiti Energia, Hera, Iren, SEAB, Soraris, etc.

tics of the included attributes are presented in Tables 8 and 9 in Appendix B. Relevant predictors are discussed in the next section.

Table 1: Summary statistics for key attributes of never-treated vs. treated municipalities before policy. Waste amounts are measured in kilograms (kg) per capita (p.c.).

		Never-treated		Treated (before)	
Obs. 19,448		Mean	Sd	Mean	Sd
Outcomes:	Recycling Waste (RW) kg p.c.	233.9	82.84	296.8	89.9
	Unsorted Waste (UW) kg p.c.	223.5	120.5	216.0	115.1
	Total Waste (TW=RW+UW)	457.4	127.0	512.7	142.9
Costs €:	RW unit costs per kg	0.18	0.12	0.16	0.08
	UW unit costs per kg	0.28	0.19	0.29	0.16
	RW management costs p.c.	40.34	22.48	47.73	24.31
	UW management costs p.c.	53.83	34.26	50.01	23.88
Covariates:	Income p.c. (x €1,000)	13.9	2.3	14.2	2.0
	Pop. share with college deg.	0.09	0.03	0.09	0.03
	Distance to PAYT city (km)	50.0	63.2	33.8	38.0
	(... up to 90 variables)	(...)	(...)	(...)	(...)

Households generate relatively more RW than UW on average. Municipal unit costs are largely heterogeneous, and managing one unit of UW costs on average twice as much as RW. Costs for UW are slightly higher also in per capita terms. Per capita costs indicate that one individual on average spends about €100 per year for waste services, and pays the most for unsorted waste.[19] This implies that UW reductions that do not translate into RW increases will potentially drive cost savings for both households and municipalities.

Comparing never-treated and treated units before adoption reveals that the latter recycle more on average. This is not surprising as adopting PAYT where households are used to recycle helps preventing policy adverse effects (Bueno & Valente, 2019). PAYT is implemented in municipalities with slightly higher income and comparable education levels. Units introducing PAYT, the treatment, in a certain year are much closer to units which already had the treatment in previous years, suggesting, e.g., information dissemination.

Focusing on treated municipalities, Table 2 compares per capita waste amounts and municipal waste management costs before and after policy.

[19] Municipal p.c. costs approximate household expenditures as, in Italy, municipalities are cost minimizers such that fee revenues finance total waste management costs, a principle known as budget balance.

Table 2: Before-and-after comparison of waste outcomes and management costs of treated units.

		Before		After	
Obs. 1,164		Mean	Sd	Mean	Sd
Outcomes:	Recycling Waste (RW) kg p.c.	296.8	89.90	337.9	92.10
	Unsorted Waste (UW) kg p.c.	216.0	115.1	115.9	80.90
	Total Waste (TW=RW+UW)	512.7	142.9	453.8	127.2
Costs €:	RW management costs p.c.	47.73	24.31	54.29	20.20
	UW management costs p.c.	50.01	23.88	40.31	18.11
	RW unit costs per kg	0.16	0.08	0.17	0.07
	UW unit costs per kg	0.29	0.16	0.43	0.26

As expected, UW and TW decrease, and RW increases after PAYT. This leads to higher costs of RW and lower costs of UW per capita. Unit costs of UW increase after policy suggesting possible economies of scale. Yet, large standard deviations indicate that average effects mask important heterogeneities across municipalities.

Table 3 reports price summary statistics by policy year. Prices range between 1 cent and 18 euro cents per liter of UW, for a total of 45 price levels. The average price is 8 cents, and the standard deviation is 0.05. Performing Wilcoxon and Kolmogorov-Smirnov tests (1945; 1971) shows that price distributions do not statistically differ across policy years.

Table 3: Price summary statistics for PAYT municipalities by policy year.

Treatment	Year	Mean	Sd	Min	P50	P75	Max
PAYT price € per liter	1 (obs. 194)	0.073	0.046	0.01	0.066	0.13	0.18
	2 (obs. 161)	0.080	0.046	0.01	0.079	0.13	0.18
	3 (obs. 125)	0.088	0.044	0.01	0.090	0.13	0.18

Using prices and UW post-policy, I calculate household variable costs in each municipality.[20] In the third policy year, households in high-price municipalities (>13 cents) pay on average €176 per capita. In low-price municipalities (<3 cents), they pay on average €21 per capita. The magnitude of this gap raises several questions. After partialling out confounding factors, do reactions in municipalities setting high versus low prices differ? What role play income effects in driving these differences? More broadly, how do behaviors toward recycling and waste avoidance adjust to a decrease in unsorted waste?

[20] I use waste haulers' data for liter-to-kg conversion. If missing, I use the median value.

1.4 Predictors

Accounting for observable determinants of price adoption and waste generation requires adjustments for many sorts of covariates. I include waste generation and policy adoption determinants for a total of 90 municipal attributes which can be grouped into six categories: socio-economic, geographic, and political variables, neighborhood effects, pre-policy waste amounts and management costs.

Socio-economic characteristics.–This group of covariates proxies for heterogeneous household opportunity costs of time and space. Above all, the literature highlights income and education levels.[21] The latter is measured by the population share with graduate degree or higher, and elementary degree or none. Income effects are indeterminate a priori (Callan & Thomas, 2006). Higher-income households may be less elastic due to, e.g., lower budget constraints and higher time opportunity costs. Both education and income may proxy for citizens' demands for environmental quality (Dunlap *et al.*, 2000). These variables, therefore, may drive both policy adoption and effects. Further, I control for demographics as, e.g., average house and household size, home ownership, age structure, and tourism intensity. Labor market attributes proxy for, e.g., time spent at home. I include the share of unemployed and out-of-the-labor force population, and indeces of labor market activity, commuting intensity, and social deprivation.

Geographic characteristics.–I control for the distance of each municipality to waste incinerators, landfills, and hazardous waste treatment facilities. Proximity to waste sites may induce, for instance, lower waste generation through households' pollution awareness. In addition, distant waste sites may cause high transportation costs and, therefore, correlate with price adoption decisions. I further distinguished communities by urbanization levels, and regional or provincial seats. These variables proxy for differences in administrative capacity, recycling infrastructure, and PAYT system (Gradus *et al.*, 2019).

Neighborhood effects.–Vicinity to other PAYT municipalities likely matters for two reasons. First, information dissemination: municipalities may adopt PAYT to mimic successful neighbors' policies (Allers & Hoeben, 2010). Second, consortia effects: nearby municipalities often share the same waste hauling company to save, e.g., collection costs. As a proxy, I control for the distance to the closest municipality that implemented PAYT in prior years. Hence, this variable takes similar values for neighboring municipalities that adopt PAYT and belong to the same consortium.

[21] See, e.g., Callan & Thomas (2006); Kinnaman (2006); Jenkins *et al.* (2003); Miranda & Bauer (1996); Van Houtven & Morris (1999); Richardson & Havlicek (1978); Grossmann *et al.* (1974); Wertz (1976).

Political variables.– Policymakers may adopt PAYT in response to citizens' demand for, e.g., better service quality, lower waste charges, and fairer waste pricing (Dijkgraaf & Gradus, 2009; Batllevell & Hanf, 2008). Public engagement may, in turn, impact policy acceptance and reactions. To proxy for political participation, I include municipal level voter turnouts in the 2013 Italian general election. Political polarization and lack of social cohesion may also impact policy adoption and effectiveness. Protest votes and extreme ideology are proxied by vote shares for big tent parties, and for extreme left- and right-wing parties. Mayor characteristics can matter as well. Newly elected, young mayors may be willing to invest in waste technology; citizens may elect green mayors putting PAYT in their agenda; locally born mayors of regional parties may rely on large consensus and push reforms. To control for this, I add mayor's age, term length, party, and place of birth.

Pre-policy waste generation.–Lagged waste generation may drive policy adoption as well as effect heterogeneity (Kinnaman, 2006). In fact, lagged waste outcomes reflect initial opportunity costs of household recycling and waste avoidance, and account for existing differences in the recycling infrastructure, e.g., at the curb. The waste generation history also contains information about unobservables such as motivation and experience in waste reduction, and pro-environmental attitudes (Bueno & Valente, 2019). Including lagged waste amounts, therefore, accounts for possible time-varying effects of fixed unobservables.

Pre-policy waste management costs.–Price adoption decisions largely rely on municipal cost levels, both in per capita and per kg terms. If salient to the household, lagged per capita costs may also impact household waste generation. Cost variables include budgetary costs for waste collection (e.g., labor), disposal (e.g., machinery and land), transportation (e.g., trucks), treatment (e.g., store and transform), and administrative services, net of recycling revenues from selling products and energy recovery.

2 Theory and Empirical Framework

2.1 Theoretical Predictions

I am interested in how a price on household unsorted waste disposal affects waste generation and social welfare. I show that household responses on recycling and waste avoidance behaviors are ambiguous from a theoretical viewpoint, and depend on the household cost structure as well as on whether these behaviors are substitutes or complements in household preferences. I provide simple theoretical predictions that account for effect heterogeneity in a possibly large number of household characteristics. Empirical analysis is, therefore, required to gauge the magnitude and the direction of price elasticities, as well as the impact of relevant socio-economic determinants. For the sake of simplicity, the theoretical model falsely assumes linear demands. In the empirical part, however, I will control for any potential nonlinear effect of prices. Finally, in line with the empirical setting, my discussion here will show that understanding policy effects on recycling and waste avoidance is crucial for welfare analysis.

Heterogeneous demand responses to PAYT.–By pricing unsorted waste generation, PAYT provides monetary incentives that decrease the relative price (opportunity cost) of two other waste disposal behaviors: waste avoidance and recycling. In this way, prices alter households' optimization problem regarding waste generation. To formalize these arguments, I derive a utility maximizing model of household consumption that highlights heterogeneous opportunity costs of recycling and waste avoidance behaviors. Based on comparative statistics, my simple formulas show that reactions to prices crucially depend on household marginal costs of these behaviors, as well as behaviors' substitutability or complementarity. All derivations are reported in Appendix A.

What makes recycling and waste avoidance more or less costly for households? The behavioral economic literature offers at least three (non mutually-exclusive) theories. Cecere *et al.* (2014) provides a first testable theory: recycling is more convenient than waste avoidance because the latter requires learning new techniques and relies on intrinsic motives. Taken to an extreme, households may find recycling so convenient that total waste increases, causing rebound effects (see, e.g., Hong & Adams, 1999, for evidence on Korea.)

Kinnaman (2006) provides a second testable theory: households recycling the most before policy would find it difficult to reduce waste substantially further after policy. Thus, waste habits pre-policy matter. Time opportunity costs and income levels can matter as well, since busy households may not take the time to engage in additional recycling.

Lastly, analyzing how behaviors affect each other, D'Amato *et al.* (2016) provide a third testable theory: recycling and avoidance behaviors may be negatively correlated due to multi-tasking effects. This refers to households being induced to focus on one behavior and reallocate some attention away from the other (Holmstrom & Milgrom, 1991). Alternatively, behaviors could be complements if waste prices crowd in pro-environmental preferences by, e.g., raising awareness of the waste pollution problem.

PAYT and social welfare.– I define policy social welfare effects as changes in social costs of waste management caused by PAYT. These are measured as the sum of municipal (private) plus environmental (external) costs of unsorted and recycling waste caused by PAYT. How do PAYT-induced changes in household waste generation affect social welfare?

This depends on the relative size of social benefits from unsorted waste reductions versus social costs of recycling increases.

The welfare analysis presents at least three challenges. First, municipal level estimates of the environmental impacts of unsorted and recycling waste are typically not available. Second, available estimates are generally not monetized, and the monetizing literature is scarce. Third, while it seems reasonable to assume that PAYT has no impact on external costs, private costs may instead be affected through, e.g., (dis)economies of scale. I overcome these issues by relying on prior estimates of environmental impacts, and assessing PAYT effects on municipal unit costs of waste management directly from the data.

Unsorted waste disposal causes external costs associated to landfill and incineration.[22] A review of the life-cycle literature shows that external costs are constant and rather small. Based on prior estimates, Kinnaman *et al.* (2014) assume a baseline external marginal cost of \$15/ton for landfill disposal and \$30/ton for incineration. Relying on these estimates, I assume an average external costs of unsorted waste of €20/ton for unsorted waste.[23]

Recycling waste causes, to a large extent, external benefits.[24] These are due to saved pollution from the avoided extraction of virgin materials. Based on prior estimates (\$200/ton), I assume a marginal external benefit of €180/ton for recycling. This is further reduced to €120/ton to account for actual recovery rates of 66% in Italy (Ronchi, 2016).

In my sample, private unit costs of recycling are on average higher, though comparable to its external benefits. Differently, private unit costs of unsorted waste are on average twice as high as its external costs. This implies that unsorted waste reductions that do not translate into an increase in recycling are the driving source of social cost savings. In other words, household reactions towards waste avoidance matter for welfare.

2.2 Identification and Empirical Specification

As the previous section shows, a large number of variables come into play in the estimation of heterogeneous causal effects of PAYT policies. Therefore, I have to include a large number of municipal characteristics possibly explaining price adoption and effectiveness.

I follow the potential outcome approach (Rubin, 1974). Let (X_{it}, Y_{it}, P_{it}) be the available data for municipality $i = 1, ..., n$ at time $t = 2010, ..., 2015$, where $X_{it} \in \mathbb{R}^d$ is a vector of d covariates, Y_{it} is the waste outcome for either unsorted (UW), recycling (RW), or total waste (TW), and $P_{it} \in \mathcal{P} = [0; p_{max}]$ is the price (treatment) variable in year t. Henceforth, I omit the true subscript t for simplicity whenever possible. Note that

[22]External costs include pollution from climate change emissions and waste transportation as well as local disamenity externalities such as nuisance effects to neighboring properties (Kinnaman *et al.*, 2014).

[23]Data on amounts sent to landfills and incinerators are unavailable. Thus, I consider the average cost.

[24]The treatment process of recycling also causes external costs. Yet, benefits overcompensate costs.

prices range between zero (for all untreated years) and the maximum price set in treated years (p_{max}). For every unit i, there is a set of potential waste outcomes $Y_i(p)$, $p \in \mathcal{P}$, each being a random variable mapping a particular potential treatment, p, to a potential outcome such that $Y_i = Y_i(p)$. This is also referred to as the unit level dose-response function. For any municipality defined by a vector of characteristics $X_i = x$, I wish to estimate the individual treatment effect of unit i, which is defined as $Y_i(p) - Y_i(0)$ and is, however, unobserved for any unit. Therefore, I will estimate the Conditional Average Treatment Effect (CATE) function $\Delta(x) = \mathbb{E}\left[Y_i(p) - Y_i(0)|X_i = x\right]$ and the Conditional Average Price Effect (CAPE) function $\delta(x) = \frac{\partial \mathbb{E}[Y_i(p)|X_i=x]}{\partial p}$ under the "canonical" assumptions of unconfoundedness and no spillovers, aka Stable Unit Treatment Value Assumption (SUTVA). I refer to Hirano and Imbens (2004) and Imai and van Dyk (2004) for a description of these assumptions. In essence, unconfoundedness requires to have enough controls - usually pre-treatment covariates and outcomes - so that, conditional on those controls, treatment assignment is as good as randomized. In the case of multivalued treatment, this assumption writes $Y_i(p) \perp\!\!\!\perp P_i|X_i \; \forall p \in [0; p_{max}]$, i.e., requires conditional independence to hold for each value of the treatment. Imbens (2000) referred to this as weak unconfoundedness, since it does not require joint independence of all potential outcomes.[25] While this assumption is not directly testable, I make it plausible by controlling for a large set of covariates and pre-policy outcomes. I further assess unconfoundedness in Section 3.4.1.

SUTVA excludes the possibility of interference between units and, given the observed covariates, allows to consider the potential outcomes of one unit to be independent of another unit's treatment status. To fulfill this assumption, I control for neighborhood effects, and I assess robustness to spillovers in untreated units (see Sections 1.4 and 3.4.1).

In order to fulfill unconfoundedness in the case of non-random price adoption, one needs to control for the sources of self-selection, i.e., capture the effect of X_i on P_i. Consider the partially linear waste outcome model:

$$Y_i = \mathbb{E}\left[Y_i(0)|X_i\right] + P_i\delta(X_i) + \epsilon_i. \tag{1}$$

Under unconfoudedness, $\mathbb{E}\left[\epsilon_i|X_i, P_i\right] = 0$; further, $\mathbb{E}\left[Y_i(0)|X_i\right]$ represents the possibly nonlinear direct effect of covariates on untreated outcomes. Note that X_i is of potentially very high dimension. Let $\mathbb{E}\left[Y_i|X_i\right]$ and $\mathbb{E}\left[P_i|X_i\right]$ be the conditional outcome and price mean, respectively. By means of few algebraic transformations, model (1) can be rewritten as:

$$Y_i - \mathbb{E}\left[Y_i|X_i\right] = (P_i - \mathbb{E}\left[P_i|X_i\right])\delta(X_i) + \epsilon_i, \tag{2}$$

[25] Yet, it is difficult to think of applications where the weaker form would be plausible but the stronger form would not be. Differences between the two are rather conceptual (see Imbens, 2000, for details).

where $\delta(X_i)$ identifies the CAPE as the effect of the leftover price variation $P_i - \mathbb{E}[P_i|X_i]$ on the leftover outcome variation $Y_i - \mathbb{E}[Y_i|X_i]$ not explained by the observed covariates. The standard partially linear model considers solely the case of constant treatment effects (Robinson, 1988). Yet, Nie & Wager (2019) study identification of model (2) for flexible heterogeneous treatment effect estimation via machine learning approaches. In particular, we can estimate $\delta(x)$ in two steps: First, we separately estimate the nuisance components $\mathbb{E}[Y_i|X_i = x]$ and $\mathbb{E}[P_i|X_i = x]$. Second, we plug in their fitted values to obtain $\hat{\delta}(x)$ by regressing residualized outcomes on residualized prices. In this step, we do not consider all residuals as equally important, and we estimate a local version of model (2) that gives more weight to those residuals in the neighborhood of x (see the next subsection for details).

Residual-on-residual regression methods, also known as R(esidualized)-learning in high-dimensional settings, make the parameter estimate insensitive to small errors in the nuisance components, thus improving its robustness. Note that, as an alternative approach, one could directly estimate model (1) by including the high-dimensional covariate set. However, regularization of $\mathbb{E}[Y_i(0)|X_i]$ would cause an especially large bias when covariates are correlated with prices (Athey *et al.*, 2017; Chernozhukov *et al.*, 2017).[26] Conversely, residualization removes the correlation of covariates with both prices and outcomes, rendering the estimator robust to the parametric form in which covariates are included. In particular, residualization makes the estimator "doubly robust", i.e., as long as either the estimator for either propensity scores or conditional outcome expectation is consistent, the resulting estimator for the treatment effect is consistent (Athey & Imbens, 2017a).

Estimation of the unit level causal effect of continuous treatment.–I now discuss how to estimate the unit level predictions for the conditional expectations of interest: as mentioned above, I estimate first the generalized propensity score $s(x) := \mathbb{E}[P_i|X_i = x]$ and the expected outcome marginalizing over treatment $y(x) := \mathbb{E}[Y_i|X_i = x]$, and finally the causal effect of continuous treatment $\delta(x)$ or CAPE. I propose to use nonparametric machine learning methods that explicitly account for heterogeneity in the parameter estimation procedure. This allows to overcome three econometric issues. First, standard k-nearest neighbor/kernel matching methods are bound to fail, as the concept of neighbor vanishes in high-dimensions (Abadie & Imbens, 2016; Giraud, 2015): conversely, machine learning allows to handle the large covariate dimension. Second, nonparametric estimation allows to capture possibly complex interactions in a data-driven model specification which, therefore, allows to reduce the risk of model misspecification and ad-hoc model selection. Third, and most importantly, machine learning allows to consistently estimate the full

[26] Without regularization, the inclusion of a large set of partly correlated predictors may lead to, e.g., variance inflation and incorrect signs.

CAPE mapping; in turn, this allows to construct a policy targeting function mapping observed covariates to unit level causal effects. Among the class of adaptive (data-driven) k-nearest neighbor matching estimators, I implement the Random Forest (RF) method developed in Athey *et al.* (2019) that generalizes the original algorithm of Breiman (2001) by adapting to the problem of both prediction and heterogeneous treatment effect estimation.

Breiman's RFs make predictions as an average of trees, as follows (see also, for a more comprehensive treatment Scornet & Biau, 2016): (1) For each tree $b = 1, \ldots, B$, draw a random subsample of training data $s_b^{tr} \subseteq \{1, \ldots, n\}$; (2) Estimate a tree via recursive partitioning on each such subsample of the data:[27] the resulting tree is a sequence of binary regions partitioning the covariate space and grouping observations in the bottom regions called leaves L_b; (3) Make predictions of $y(x)$ in the leaf containing the set of units $\{i : X_i \in L_b(x), i \in s_b^{tr}\}$, and average predictions made by individual trees:

$$\hat{y}(x) = \frac{1}{B} \sum_{b=1}^{B} \sum_{n=1}^{N} \frac{Y_i \mathbb{1}(X_i \in L_b(x), i \in s_b^{tr})}{|i : X_i \in L_b(x), i \in s_b^{tr}|} \tag{3}$$

where $L_b(x)$ denotes the leaf of the b^{th} tree containing the training sample x. In the case of out-of-sample prediction, I estimate $y(x)$ using $\hat{y}^{-i}(X_i)$, based on considering only those trees b for which $i \notin s_b^{tr}$.[28]

Breiman's RF is understood as an ensemble method averaging predictions made by individual trees. However, as shown in Athey *et al.* (2019), we can equivalently think of RF as an adaptive kernel method. For instance, we can re-write the forest prediction (3) as $\hat{y}(x) = \sum_{i=1}^{n} w_i(x) Y_i$ where $w_i(x)$ is a data-adaptive kernel that measures how often the i^{th} training unit falls into the same leaf as x. Weights sum up to 1, and define the forest-based adaptive neighborhood of x (see Athey *et al.*, 2019, for a formal definition). This kernel-based perspective of RFs allows to cast this method as an adaptive locally weighted estimator that first uses a forest to calculate a weighted set of neighbors for each test point x, and then solves a weighted version of the residual-on-residual regression (2). The resulting CAPE estimator writes:

$$\hat{\delta}(x) = \frac{\sum_{i=1}^{n} w_i(x)(Y_i - \hat{y}^{-i}(X_i))(P_i - \hat{s}^{-i}(X_i))}{\sum_{i=1}^{n} w_i(x)(P_i - \hat{s}^{-i}(X_i))^2}, \tag{4}$$

where $\hat{s}^{-i}(X_i)$ are out-of-sample predictions of the propensity scores $s(x)$. Equation (4) de-

[27] The process is termed recursive because each subsample of the data can in turn be split an indefinite number of times until the splitting process terminates after a particular stopping criterion is reached.

[28] Without out-of-sample prediction, random forest estimators are asymptotically normal, since each estimate is derived by averaging estimates from many trees, but they overfit and do not converge at the square-root-n rate, thus, are bias-dominated (Athey & Imbens, 2016; Mentch & Hooker, 2016).

fines the CAPE as a weighted residual-on-residual regression estimator with forest weights $w(x)$ defining the nearest neighbors of x produced by different trees.[29] This distinguishes the CAPE from the APE estimator which is the unweighted version of (4).[30] Variance of $\hat{\delta}(x)$ is estimated by evaluating the estimator on bootstrapped half-samples of the training data, also called bootstrap of little bags (Athey *et al.*, 2019; Sexton & Laake, 2009).

As for Breiman's forests, estimation of a tree proceeds greedily, namely, the partitioning is done continuously by choosing the split-point and the covariate that attain the best fit (maximize or reduce some metric in that specific partition). In the case of generalized random forest, such criterion is chosen to maximize heterogeneity in the quantity of interest (outcomes, propensity scores, treatment effects) between partitions. For instance, the metric chosen for estimation of the CAPE function maximizes $n_L n_R(\hat{\delta}_L - \hat{\delta}_R)^2$ where $\hat{\delta}_{L,R}$ is the CAPE estimated in each binary (Left; Right) partition including a fraction of training units equal to $n_{L,R}$. Athey *et al.* (2019) discuss the design of computationally efficient splitting rule for generalized forests in more detail.

Estimation of policy social welfare effects.–For each municipality $i = 1, ..., n$ with attributes $X_i = x$, I define the policy Social Welfare Effect ($SWE^{P>0}(x)$) in per capita euros as the sum of municipal (private) plus environmental (external) costs of unsorted and recycling waste caused by PAYT ($P > 0$). The $SWE^{P>0}(x)$ writes:

$$- \left[CATE_{UW}(x)(PC_{UW}^{P>0}(x) + EC_{UW}) + CATE_{RW}(x)(PC_{RW}^{P>0}(x) + EC_{RW}). \right] \tag{5}$$

$CATE_{UW}(x)$ and $CATE_{RW}(x)$ are municipal level causal effects of waste prices on unsorted and recycling waste, respectively (in kg per capita). $PC_{UW}^{P>0}(x)$ and $PC_{RW}^{P>0}(x)$ are municipal level unit costs of, respectively, unsorted and recycling waste management under PAYT (in €/kg). EC_{UW} and EC_{RW} are the external marginal costs of unsorted waste and recycling, respectively, where the first is positive and the second is negative (in €/kg). As a result, a negative $CATE_{UW}(x)$ (UW reductions) brings both private and external benefits, while a positive $CATE_{RW}(x)$ (RW increases) brings private costs and external benefits. The net welfare effect is ex-ante unclear, and remains an empirical question.

While EC are assumed to be constant across units and rely on prior estimates, $CATE(x)$ and $PC(x)$ are estimated for all municipalities in the sample, treated and untreated. $CATE(x)$ is estimated for each waste type (here, UW and RW) as $p\hat{\delta}(x)$ with $\hat{\delta}(x)$ being

[29] RFs have tuning parameters such as minimum leaf-size and penalties for imbalanced partitions. These are obtained via cross-validation as in Tibshirani *et al.* (2018), i.e., choosing the ones that make out-of-sample estimates of the regression errors minimized in the R-learning objective as small as possible.

[30] Motivated by the R-learning equation (2), I estimate the APE as $\frac{\sum_{i=1}^{n}(Y_i - \hat{y}^{-i}(X_i))(P_i - \hat{s}^{-i}(X_i))}{\sum_{i=1}^{n}(P_i - \hat{s}^{-i}(X_i))^2}$.

statistically significant CAPE estimates for the respective waste type.[31] For untreated units, the price p is assigned based on closest generalized propensity score prediction.

$PC^{P>0}(x)$ are observed for treated units (under treatment) but unobserved for untreated units. In order to predict counterfactual values for untreated units, I use the R-learning RF method described in Section 2.2. I regress all the covariates - including municipal attributes, lagged waste and cost values - and a policy indicator on municipal unit costs, running separate forests for UW and RW. I obtain a kernel function that maps municipal observables to policy causal effects on unit costs. I next use this function to predict unit costs for untreated units under PAYT. For each municipality defined by $X_i = x$, I assess if PAYT affects unit costs significantly. If this is the case, I replace $PC^{P>0}(x)$ for untreated units with $\hat{PC}^{P>0}(x)$, their predicted counterpart under PAYT.

3 Main results

I have explored from 500 to 10,000 trees in the RF, and treatment effect estimates become stable after 1,000 trees, thus, results are obtained using this value. Further, each tree is built with data for the same year to account for common shocks to all units. All trees are grown with cross-validated values for the number of randomly subsampled covariates, minimum leaf size, and penalty for imbalanced splits, namely, splits in which the size of parent and child node are very different are penalized. Additionally, since the treatment group is substantially smaller than the control group, each node is required to include a minimum number of both treated and control units, i.e., enough information about both factual and counterfactual to estimate the treatment effect reliably. For this reason, a penalization is imposed also to nodes including an unbalanced number of treated and control units. Following Athey *et al.* (2019), values for such parameters are obtained via cross-validation.[32] In order to credibly estimate policy causal effects, the common support condition for treated and untreated units needs to hold. Figure 10 in Appendix C provides evidence of such support.

3.1 Behavioral responses

I begin my empirical analysis by estimating unit level causal effects of prices on demands for unsorted, recycling, and total waste per capita (UW, RW, TW, respectively). In this section, I focus the attention on elasticities in the third policy year in order to have a

[31] As CAPE is the first derivative of the CATE, then an estimate of the CATE is just the integral of the estimated CAPE, i.e. of $\hat{\delta}(x)$ (Torricelli-Barrow theorem).

[32] I use the software R-3.4.2 and the grf package version 0.10.0 (Tibshirani *et al.*, 2018).

closer estimate to the long-term effects of PAYT.[33] I run separate random forests for each waste outcome: UW, RW, and TW (for robustness). Figure 2 presents the distribution of the estimated semi-elasticities of waste demands for all municipalities in the sample. All estimates are statistically different from zero (p-values < 0.01). The hypothesis of effect homogeneity is rejected for all outcomes and policy years.[34]

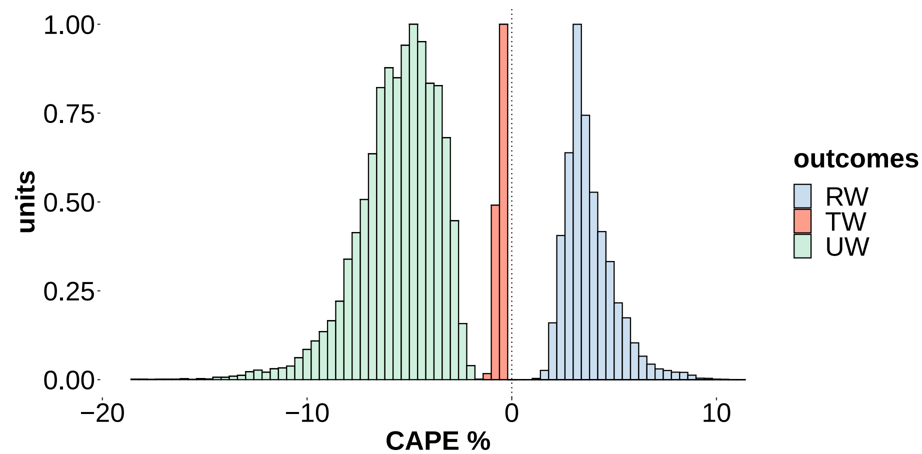

Figure 2: Unit level estimates of price semi-elasticities (CAPE) of waste demands. CAPE are measured as percent changes of waste amounts for a one cent price increase.

The estimated price effects on waste demands for UW and RW are large in magnitude. Using the estimates to compute factual and counterfactual waste amounts implies that prices cause an annual UW decline of 50% on average. This reduction is mainly driven by an average RW increase of 32% and, to a lesser extent, by an average TW reduction of 5%.[35] The large substitution effect of UW with RW caused by the policy, therefore, represents a strong change in household waste behavior.

I proceed with the analysis of the sources of effect heterogeneity. In order to summarize the CAPE function, I linearly project the CAPE estimates onto price levels, a set of features, and their interactions (Tibshirani *et al.*, 2019). The literature on waste prices is especially interested in learning the elasticity of demand as a function of a few variables such as income or education. Thereby, the set of features includes relevant regressors capturing household opportunity costs of waste disposal: income, education, and pre-policy waste levels. Figure 3 plots fitted semi-elasticities (CAPE) across prices, ceteris

[33] I analyze the dynamics of price effects in Section 3.2. One caveat is that these elasticities cannot be estimated for late adopters. Reassuringly, there are no statistically significant differences in the distribution of price effects between early and late adopters in the first two policy years.

[34] See Levene's tests (1960) and heuristics (Athey *et al.*, 2017) in Appendix D.2 and Table 11 therein.

[35] This corresponds to -110 kg (UW), +80 kg (RW) and -25 kg (TW) per capita. Thereby, estimates from separate forests are consistent with each other ($CATE_{TW} \approx CATE_{UW} + CATE_{RW}$).

paribus.[36] Pointwise estimates are obtained by fitting a smooth regression line. Shaded regions are the 95% confidence intervals.

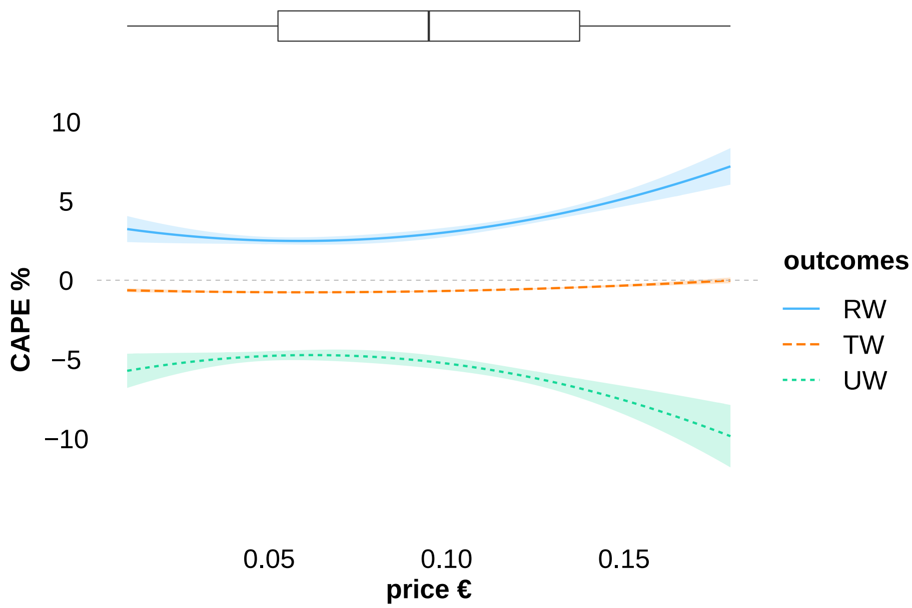

Figure 3: Fitted price semi-elasticities (CAPE) of waste demands at each price level.

Slopes of waste demands in high and low price regions differ. I use the median value of 9 cents to define these regions, indicated by the vertical line in the boxplot above the image. While at high prices elasticities are increasing, elasticities are rather constant at low prices. The estimated elasticity variation at low prices is 4% of the variation at high prices. All other things equal, a one cent price increase reduces unsorted waste by 5 to 10% (vs. 4.7-5.7% at low prices), increases recycling by 2 to 6% (2.5-3.2%) and reduces total waste by 0.1 to 0.7% (0.6-0.8%). Further, household behavior at high prices sheds new light on the mechanism behind UW reductions: increasing UW reductions reveal a negative correlation between RW and TW reactions, suggesting that recycling and waste avoidance are substitutes in individual preferences.[37] Thus, higher prices do not further reduce total waste, instead they simply reallocate waste to the recycling pile.

Having established effect heterogeneity across prices, I analyze possible income effects. Figure 4 plots fitted semi-elasticities by income level, ceteris paribus.

[36]Data inspection suggests quadratic demand curves. Thus, I fit a polynomial regression of order 2. Results for CAPE in kg are presented in Figure 11 in Appendix D.3.

[37]This result confirms case *(i)* among the theoretical predictions reported in Appendix A. Estimates of the elasticity of substitution between recycling and avoidance are provided in Section 3.3.

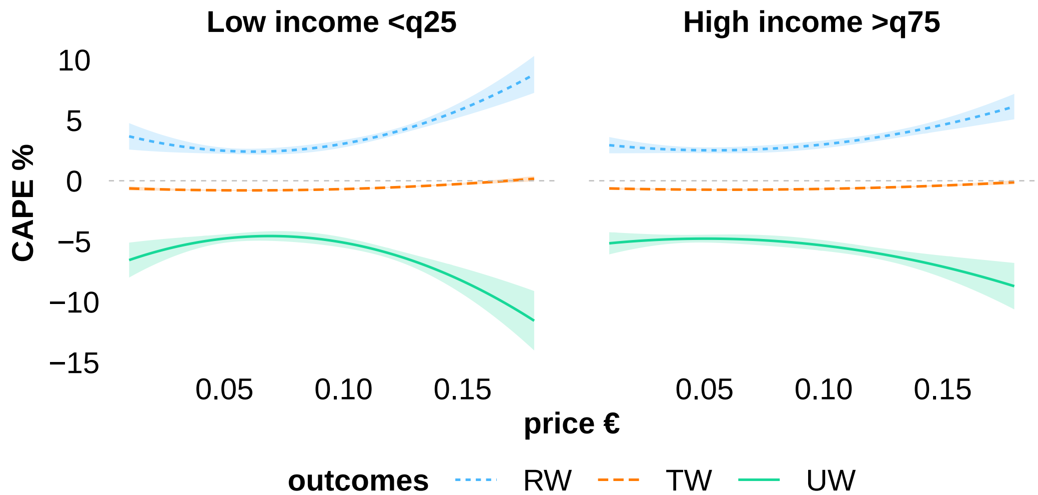

Figure 4: Fitted price semi-elasticities (CAPE) of waste demands by income levels. Thresholds q25 and q75 indicate first (€13k) and third (€16k) quartiles of annual income per capita.

Low-income municipalities are on average one percent point more elastic than high-income municipalities.[38] Thus, high prices enter budget constraints. However, income effects are not statistically significant at low prices. This means that high- and low-income households are similarly elastic, and possible differences in time opportunity costs do not trigger heterogeneous reactions.

Findings can be interpreted as follows: on the one side, no income effects and constant elasticities at low prices are consistent with nudge theories where small incentives have substantial behavioral impacts without budgetary incidence (Farhi & Gabaix, 2020). This interpretation is supported by the low monetary benefits of reducing UW at low prices: households reduce UW by 13.6% in the first price quartile (3 cents) and save less than one euro. On the other side, income effects at high prices provide evidence against possible regressive effects of waste prices. This result is new to the literature as previous studies typically consider the effects of low prices.[39]

Looking at heterogeneity across education levels, I estimate no significant effects, suggesting that education plays, if any, a relatively minor role. Conversely, recycling habits before policy matter, as predicted by Kinnaman (2006): households recycling little before policy are more elastic.[40]

[38] In particular, at high prices, semi-elasticities for low-income municipalities are on average -7.5% for UW, 5.2% for RW, and -0.5% for TW, ceteris paribus.

[39] The highest price in, e.g., Fullerton & Kinnaman (2000) is $2.18 per 32-gallon bag (120L) or $0.02/L. This data include prices up to €0.18/L. See Bel & Gradus (2016) for a review of previous studies.

[40] See Figure 12 in the Appendix D.3.

The literature typically estimates average point-elasticities smaller than one. For comparison, I compute point-elasticities at mean values and find similar values (see Table 10 in Appendix D.1). However, converting slope estimates into point-elasticity estimates may not be useful since price elasticity varies along the demand curve.

3.2 Welfare analysis

Welfare effect heterogeneity is driven by both demand and cost effects. In order to understand why municipalities may refrain from policy adoption, this section analyzes welfare effects in the short- versus longer-run. Demand effects (CATE) on UW are lowest in the first policy year. This is consistent with economic theory showing that elasticities are typically lower in the short-run because fewer alternatives are available (Usui, 2009). UW reductions are largely driven by increased recycling and, to a smaller extent, by waste avoidance. Further, behavior changes toward waste avoidance are relatively smaller and slower. This is consistent with behavioral economic predictions by e.g. Cecere *et al.* (2014) who argues that waste avoidance requires more effort and learning than recycling.[41] Figure 5 plots the distribution of statistically significant demand effects on TW by policy year.

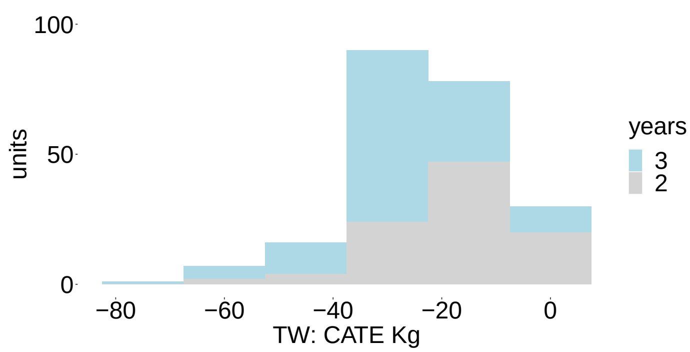

Figure 5: Unit level estimates of causal effects (CATE) on total waste demands in the second and third policy year (p-values < 0.05). The y-axis refers to the number of treated municipalities.

In year 1, there are no statistically significant effects on waste avoidance (total waste reduction). In year 2 (grey), waste avoidance grows in a fraction of municipalities, and increases significantly for all municipalities in year 3 (blue).

Next, I flexibly estimate policy causal effects on unit management costs for UW and RW via random forests: overall, I find no economies of scale, confirming results on Italian municipalities by e.g. Abrate *et al.* (2014). In a fraction of municipalities, estimates suggest economies of scale for UW and RW because their unit cost increases (decreases)

[41]For instance, households need to learn how to reuse and buy products with less packaging.

when UW (RW) reduces (increases).

Finally, I compute social welfare effects by combining CATE with management and pollution cost estimates. Table 4 shows social welfare effects (€ p.c.) by policy year.

Table 4: Summary statistics for Social Welfare Effects (SWE). Estimated private (municipal), external (environmental), and social cost effects per capita (p.c.) by policy year.

Year	SWE € p.c.	Mean	Sd
1	Private	0.13	10.16
	External	6.87	5.33
	Social	7.01	10.35
2	Private	3.14	7.10
	External	5.96	4.75
	Social	9.11	9.08
3	Private	20.17	16.28
	External	4.45	3.52
	Social	24.62	19.71

Especially in year 1 and 2, welfare effects can be negative. As feared, higher recycling can indeed increase costs for society. In year 3, however, PAYT generates welfare benefits in most municipalities. Average benefits are €25 per capita, which is about one fourth of what municipalities spend for waste management per person on average. These benefits are mainly due to private costs savings. As management unit costs are largely unaffected, savings come from UW reductions that do not translate into higher RW. In other words, welfare benefits are largely driven by waste avoidance.

As higher prices promote increased recycling rather than waste avoidance, the largest welfare benefits occur in municipalities setting low prices after three years of adoption. Figure 6 shows social welfare effects at different quartiles of the price distribution in the third policy year, when waste avoidance is highest.

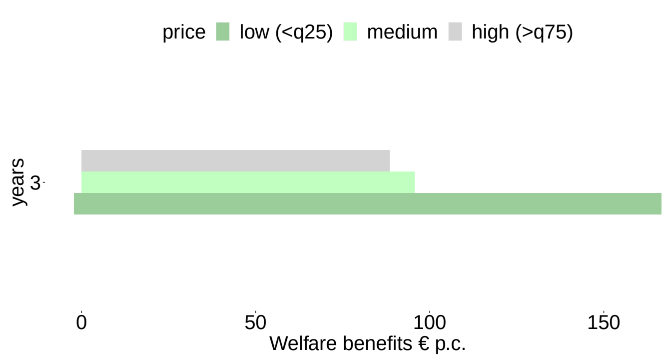

Figure 6: Social welfare effects (largely, welfare benefits) at different price levels after three years of adoption. First and third quartiles correspond to 3 and 13 cents, respectively.

This implies that (i) higher prices may be less desirable from a social cost perspective, although they cause larger unsorted waste reductions, and (ii) targeting waste avoidance in high-price municipalities could improve welfare benefits substantially.

3.3 Welfare simulations

Using random forests, I estimate a kernel function that maps municipal observables to policy causal effects. I next use this function to estimate overall causal effects on waste demands and social welfare if all municipalities in the sample were to implement PAYT.

Figure 7 plots the predicted price semi-elasticities of RW and TW demands after three years of adoption. As expected, color differences in the same area indicate that higher recycling is associated to lower avoidance and vice versa.

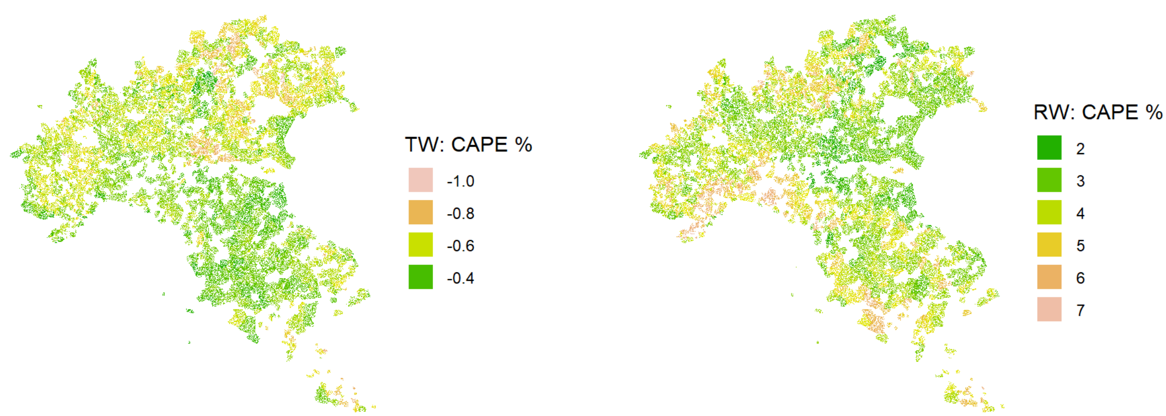

Figure 7: Predicted semi-elasticities (CAPE) on total waste and recycling demands for all municipalities three years after policy. Higher elasticities on waste avoidance and recycling indicated by warmer colors.

Figure 8 shows the negative correlation between price elasticities of recycling and waste avoidance aggregated by percentiles.

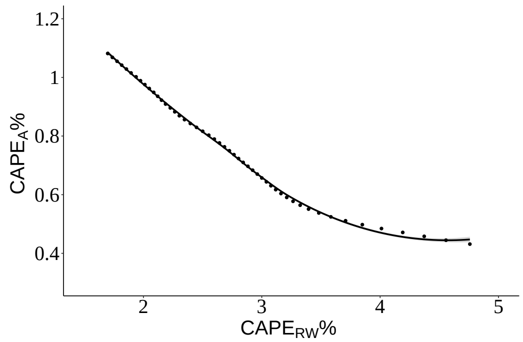

Figure 8: Semi-elasticities (CAPE) on recycling (x-axis) plotted against semi-elasticities on avoidance (y-axis). Note that $-\text{CAPE}_{TW}$=CAPE_A. A smooth line is fitted through percentile values.

The elasticity of substitution between behaviors is not constant and does not lead to rebound effects on total waste (i.e., a negative $CAPE_A$). Using local linear regression (Li & Racine, 2004), I estimate that one percent point increase in the price semi-elasticity of recycling reduces that of waste avoidance between 0.03 and 0.4 percent points. This result is consistent with theories predicting multi-tasking effects between recycling and waste avoidance behaviors (e.g. D'Amato *et al.*, 2016), and provides new evidence on the magnitude of these effects.

Despite substitutabilities between recycling and avoidance behaviors, welfare simulations show that most municipalities would benefit from PAYT adoption. Table 5 shows predicted welfare effects by policy year if all municipalities were to implement PAYT.

Table 5: Summary statistics for predicted Social Welfare Effects (SWE). Estimated private (management), external (environmental), and social cost effects per capita (p.c.) by policy year.

Year	SWE € p.c.	Mean	Sd
1	Private	1.45	10.90
	External	7.05	4.64
	Social	8.50	10.51
2	Private	3.95	9.33
	External	6.90	4.90
	Social	10.86	10.54
3	Private	23.85	18.33
	External	5.44	4.20
	Social	29.29	21.97

Compared to counterfactual municipal and pollution costs, UW reductions lead to benefits for both waste management (-24%) and the environment (-2.3%) on average over the policy years. Further, increased RW causes higher costs for waste managements (+21%) but also pollution reductions for the environment (-15%). In year 3, average welfare benefits per capita amount to €30, however, there is large variation. Figure 9 maps this variation: despite effect heterogeneity, waste prices cause welfare benefits for most municipalities.

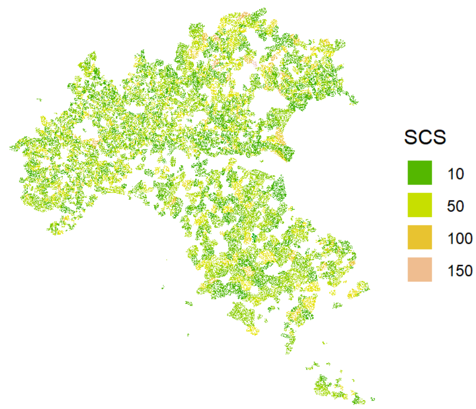

Figure 9: Distribution of predicted welfare benefits in the third policy year in € per capita.

3.4 Robustness and sensitivity

3.4.1 Assessing assumptions: Unconfoundedness and SUTVA

Anticipation effects.–By conditioning on a large number of observables, this analysis aims to make the impact of leftover unobserved factors negligible and, thereby, unconfoundedness a plausible assumption. Although being per se untestable, unconfoundedness can be assessed by testing for significant effects on outcomes pre-policy (Athey & Imbens, 2017b). I run a RF including a binary treatment indicator, all covariates and available lags (up to order 2). Table 6 shows average price effects (APE) for three years pre-policy estimated as average treatment effects at the mean price level ($ATE/\bar{P}$).

Table 6: Placebo APE at the mean price (ATE/$\bar{P}$) from log-level (%) and level-level (kg) residual-on-residual regression (***p-value < 0.01). Standard errors are clustered by year.

Year	UW %	kg	s.e.	RW %	kg	s.e.	TW %	kg	s.e.
-3	-5.63	-2.90	9.81	6.07	12.5	10.54	-2.34	-11.3	21.33
-2	-6.07	-16.8	17.98	5.16	12.6	8.42	2.68	15.5	22.27
-1	-8.29	-16.9	13.26	21.59***	46.4***	9.53	9.60***	28.8***	10.27

Results suggest that unconfoundedness holds. Though, as expected, there is evidence of anticipation effects in the first pre-policy year. Before the official start, municipalities typically implement pilot programs where the new system is in place but households still pay according to the old regime of flat fees. This serves to promote recycling habits and minimize the risk of policy adverse effects. Estimates show a significant increase in RW that leads to higher TW, indicating that households largely substitute towards more recyclable products as, e.g., bulky items (Hong & Adams, 1999; Podolsky & Nestor, 1998). In terms of magnitude, the APE on TW approximately equals the sum of APE on RW and UW, however, a large standard error makes the latter statistically insignificant.

System effects.–I assess another possible threat to identification: unaccounted-for heterogeneity in system adoption.[42] In the sample, about 7% of treated units pay per weight, 16% per bag, 72% per emptying, and 5% have a mixed system. I first test for whether the included covariates can explain system adoption. Second, I test for significant differences in price elasticities between systems. Using all covariates, system choices are predicted by RF for classification. Reassuringly, observables can predict the adoption of volume systems correctly, out-of-sample, and misclassify 33% of weight systems only. Next, I test for effect heterogeneity. Results of pairwise mean comparison tests (Games & Howell, 1976) show that price elasticities do not statistically differ between systems.[43]

Waste tourism.–Violation of the no spillover assumption (SUTVA) could possibly come from waste tourism, namely, households may discharge their waste for free in surrounding municipalities without PAYT. I geocode neighbors as never-treated units sharing a border with treated units. The remaining untreated units represent the control group. I find that PAYT has no significant effect on the waste amounts collected in surrounding municipalities on average. As a further robustness check, I re-estimate causal effects for PAYT municipalities excluding neighbors from the control group, finding no significant change in estimates. As in other empirical studies (e.g. Carattini *et al.*, 2018; Allers & Hoeben, 2010; Dijkgraaf & Gradus, 2004), waste tourism is not relevant on any significant scale.

3.4.2 Supporting analyses: Robustness to alternative assumptions

I compare my average estimates to *(i)* the binary treatment case, which assumes no effect heterogeneity in prices; *(ii)* the dynamic (event-study-like) difference-in-differences design, which assumes no effect heterogeneity, and selection based on time-constant variables; and *(iii)* penalized linear regression methods known as LASSO, which assumes no effect heterogeneity, sparsity and linearity in a high-dimensional set of covariates.

Binary treatment case.–Results show that mean elasticities (APE) on UW are overestimated by 28% on average (see Table 14 in Appendix D.4.3). Why does this happen? The forest kernel assigns similarity weights to units based on a residual-on-residual regression

[42] I cannot include system dummies in RF because, as for any policy indicator, I would force treated and untreated units into different neighborhoods.

[43] Results are reported in Table 13 in Appendix D.4.2.

that ignores the price variation and uses a binary indicator instead. Hence, the algorithm may place units adopting high and low prices in the same neighborhood, and assign the same prediction to both units. However, I have shown that price responses at high and low prices are not the same, and the sources of effect heterogeneity differ.

Difference-in-Differences.–Results from a two-way fixed effect regression show that APE on UW are underestimated by 29% on average (see first panel of Figure 13 in Appendix D.4.3). The identifying parallel trend assumption is violated. There are at least three possible sources of bias. First, unaccounted-for time-varying effects on the outcomes (Bueno & Valente, 2019). Second, non-interpretable causal coefficients (Callaway & Sant'Anna, 2018). Due to the staggered policy adoption, APE represent weighted averages of effects computed using both treated and untreated outcomes, which may not allow for a comparison with my estimates. Third, falsely assuming policy homogeneity (Abraham & Sun, 2018). Intuitively, pre-trends can arise solely from effect heterogeneity because the coefficient on a given lead or lag can be contaminated by effects from other periods.

LASSO.–Similarly to RF, doubly robust LASSO regression estimates the APE after removing the correlation of covariates with outcomes and prices. Yet, differently from RF, it assumes causal effect homogeneity and, typically, linearity in covariates (Athey *et al.*, 2017; Belloni *et al.*, 2017). I find that my APE estimates are almost insensitive to these assumptions when differences in covariates are effectively adjusted. Yet, this method does not allow to consistently estimate unit level causal effects and identify effect heterogeneity.

Summary.–Table 7 summarizes all APE estimates by estimation method. Residualized reg via RF is my main estimation method; residualized reg via RF (bin.) assumes no heterogeneity in prices; dynamic difference-in-differences (DiD) assumes constant APE and selection due to time-constant variables; double selection and residualized LASSO reg assume constant APE and linearity after effectively removing differences in covariates.

Table 7: Method comparison: APE estimates in the third policy year. Estimates represent average kg changes in waste for a one cent price increase.

Method/APE estimates	UW kg	s.e.	RW kg	s.e.	TW kg	s.e.
residualized reg via RF	-11.50	1.50	8.10	2.20	-2.70	0.90
residualized reg via RF (bin.)	-16.50	1.10	13.0	2.20	-3.20	2.90
dynamic DiD reg	-7.40	0.30	2.70	0.70	-4.70	0.50
double selection LASSO reg	-9.30	0.40	9.40	0.60	-1.00	0.50
residualized LASSO reg	-9.00	1.30	8.80	1.00	-1.00	1.30

4 Conclusions

This paper models and estimates causal effects of waste prices (PAYT) on recycling and avoidance behaviors by explicitly accounting for heterogeneity with machine learning methods. I analyze unique data with large variation in price levels and a high-dimensional set of municipal characteristics. I estimate municipal level price elasticities using a random forest and R-learning estimator robust to confounding that affects outcomes and prices. By mapping municipal attributes to causal effects, I predict heterogeneous policy effects for all municipalities in the sample, treated or not. The welfare analysis is motivated by a theoretical model that predicts heterogeneous reactions on recycling and total waste. On the one hand, higher recycling brings environmental benefits. On the other hand, it increases municipal management costs. Theoretically, total waste reductions seem crucial to decrease municipal social costs substantially, and generate overall welfare benefits.

Disentangling effect heterogeneity in prices from other sources, I find that waste demands are nonlinear. Low prices, nudging households toward similar reactions, trigger large unsorted waste reductions, increase recycling, and decrease total waste. High prices make waste demands increasingly elastic, and induce recycling rather than waste avoidance. On average, policy causal effects amount up to -50% on unsorted waste, 32% on recycling, and -5% on total waste. Income effects and waste habits pre-policy well explain nonlinearities at high prices. I find that lower-income municipalities are more price elastic, which indicates that PAYT is not regressive. Despite large effect heterogeneity, the policy reduces social costs in most municipalities after three years of adoption, especially where total waste reductions are large. This implies that higher prices may be less desirable from a social cost perspective although they cause relatively larger unsorted waste reductions.

In sum, this study shows that price levels matter for household waste behavior as well as social welfare. Estimating heterogeneous responses on unsorted waste, and especially on recycling and total waste is crucial to assess overall welfare effects. Behavior changes towards waste avoidance, however, may not be immediate, and social costs may increase in the short-run. One upshot of this paper is that low prices can have a big impact on waste behavior and social welfare. Analyzing the long-term effects of PAYT policies presents an exciting research opportunity that I hope to tackle in future work.

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A Appendix: Theoretical predictions

The model.–Consider the household problem to maximize utility from consumption net of the disutility of avoidance and recycling efforts. The corresponding expression is:

$$max_{w_A,w_R} : V(\cdot) = U(y - t(W - (w_A + w_R))) - C(w_A, w_R, W_0, W_R(W_0), X, Z) \quad (6)$$

where $U(\cdot)$ is the utility from consumption, $y-t(W-(w_A+w_R))$, with $W-(w_A+w_R)$ being the amount of unsorted waste subject to the PAYT price, t, which is taken as exogenous in the following derivations. Avoided (A) and recycling (R) waste amounts, w_i ($i = A, R$), are generated from a potential waste amount, W, associated to the consumption of goods purchased with income y. Total waste is defined by the sum of unsorted and recycling waste, and equals $W - w_A$. Regarding consumption utility, the household is assumed to prefer more consumption to less ($\frac{\partial U(\cdot)}{\partial y - t(W-(w_A+w_R))} > 0$), waste avoidance and recycling to unsorted waste generation. This implies that marginal consumption utility with respect to waste avoidance and recycling is positive ($\frac{\partial U(\cdot)}{\partial w_i} > 0$) and decreasing ($\frac{\partial^2 U(\cdot)}{\partial w_i^2} < 0$).

Moreover, households get disutility, $C(\cdot)$, from time and effort spent in disposing w_i. $C(\cdot)$ is a standard microeconomic cost function for which the following conditions hold:

$$\frac{\partial C(\cdot)}{\partial w_i} > 0;\ \frac{\partial^2 C(\cdot)}{\partial w_i^2} > 0;\ \frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R} \lesseqgtr 0. \tag{7}$$

The first two conditions indicate that avoiding and recycling waste is costly, and marginal costs are increasing with the reduction of unsorted waste. The third relationship, importantly, captures the degree to which households are able to shift between disposal methods, pointing to either complementarities or substitutabilities between the two behaviors with $\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_C} < 0$ or $\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_C} > 0$, respectively. Empirically, an indication of substitute (complement) behaviors may derive from the estimation of a negative (positive) correlation between individual price reactions on recycling and on avoidance.

The arguments of the cost function, $C(\cdot)$ are current $\{w_i\}$ and initial (lagged) waste $\{W_0, W_R(W_0)\}$ with the latter accounting for waste reduction and recycling habits. Additionally, $C(\cdot)$ depends on waste-reduction technology shifters (X) which are exogenous socio-economic factors such as household income and education, and taste shifters (Z) such as preferences for the environment. In particular, households may derive utility from waste avoidance and recycling due to the psychological reward of contributing to public good provision or "warm-glow", and external rewards (peer approval) for pro-social and environmentally responsible actions.[44]

Solving problem (6) for w_i ($i = A, R$) leads to the following first-order conditions:

$$\frac{\partial V(\cdot)}{\partial w_i} = U'(\cdot)t - \frac{\partial C(\cdot)}{\partial w_i}, \tag{8}$$

with $U'(\cdot)t$ being the marginal utility of avoiding or recycling waste, and $\frac{\partial C(\cdot)}{\partial w_i}$ its marginal

[44] For a discussion see, e.g., Abbott *et al.* (2013); Bénabou & Tirole (2003); Brekke *et al.* (2010); D'Amato *et al.* (2016); Gilli *et al.* (2018a); Kahn (2007); Jenkins *et al.* (2003); Thøgersen (2006). The PAYT price may also enter $C(\cdot)$ directly since it may partly crowd out households' intrinsic and extrinsic motivation, increasing the disutility from waste recycling and avoidance (see, e.g., Bowles & Polania-Reyes, 2012; Cecere *et al.*, 2014; Chi-ang & Zheng, 2017; Ferrara & Missios, 2012, and citations therein). Yet, this section outlines a simpler model predicting PAYT heterogeneous effects.

cost. Optimality conditions (8) for both w_A and w_R allow to derive the comparative statics about the impact of PAYT pricing (t) on waste avoidance (w_A) and recycling (w_R), implying that:

$$sgn\left(\frac{\partial w_A}{\partial t}\right) = sgn\left[\frac{\partial^2 C(\cdot)}{\partial w_R^2} - \frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R}\right]; sgn\left(\frac{\partial w_R}{\partial t}\right) = sgn\left[\frac{\partial^2 C(\cdot)}{\partial w_A^2} - \frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R}\right] \quad (9)$$

where $sgn(\cdot)$ is a function that extracts the sign of PAYT causal effects. Specifically, equations (9) mean that the sign of PAYT causal effects depends on complementarities/substitutabilities between waste reduction behaviors. As a result, theoretical predictions of PAYT causal effects are threefold:

(i) PAYT causes avoidance and recycling to increase ($\frac{\partial w_A}{\partial t} > 0$, $\frac{\partial w_R}{\partial t} > 0$) if A, R are *complements* ($\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R} < 0$) or if A, R are *substitutes* ($\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R} > 0$) but substitutabilities are small such that $\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R} < \frac{\partial^2 C(\cdot)}{\partial w_i^2}$.

(ii) PAYT causes total waste and recycling to increase ($\frac{\partial w_A}{\partial t} < 0$, $\frac{\partial w_R}{\partial t} > 0$) if A, R are *substitutes* ($\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R} > 0$) and marginal costs of avoidance are steeper than marginal costs of recycling such that $|\frac{\partial^2 C(\cdot)}{\partial w_A^2}| > |\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R}| > |\frac{\partial^2 C(\cdot)}{\partial w_R^2}|$.

(iii) PAYT causes avoidance to increase and recycling to decrease ($\frac{\partial w_A}{\partial t} > 0$, $\frac{\partial w_R}{\partial t} < 0$) if A, R are *substitutes* and marginal costs of recycling are steeper than marginal costs of avoidance such that $|\frac{\partial^2 C(\cdot)}{\partial w_R^2}| > |\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R}| > |\frac{\partial^2 C(\cdot)}{\partial w_A^2}|$.

Formulas show that whether PAYT causes an increase in both pro-environmental behaviors depends on household marginal costs of recycling versus avoidance relative to the degree to which behaviors are substitutes or complements in individual preferences.

Full derivations.–Total derivatives of optimality conditions in (8) for $i = A, R$ write:

$$\frac{\partial^2 V(\cdot)}{\partial w_A \partial t}\frac{\partial t}{\partial t} + \frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R}\frac{\partial w_R}{\partial t} + \frac{\partial^2 V(\cdot)}{\partial w_A^2}\frac{\partial w_A}{\partial t} = 0 \quad (10)$$

$$\frac{\partial^2 V(\cdot)}{\partial w_R \partial t}\frac{\partial t}{\partial t} + \frac{\partial^2 V(\cdot)}{\partial w_R \partial w_A}\frac{\partial w_A}{\partial t} + \frac{\partial^2 V(\cdot)}{\partial w_R^2}\frac{\partial w_R}{\partial t} = 0 \quad (11)$$

Solving for $\{\frac{\partial w_A}{\partial t}, \frac{\partial w_R}{\partial t}\}$, and considering that $\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R} = \frac{\partial^2 V(\cdot)}{\partial w_R \partial w_A}$ gives:

$$\frac{\partial w_A}{\partial t} = \frac{\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R}\frac{\partial V(\cdot)}{\partial w_R \partial t} - \frac{\partial^2 V(\cdot)}{\partial w_R^2}\frac{\partial V(\cdot)}{\partial w_A \partial t}}{\frac{\partial^2 V(\cdot)}{\partial w_A^2}\frac{\partial^2 V(\cdot)}{\partial w_R^2} - (\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R})^2} \quad (12)$$

$$\frac{\partial w_R}{\partial t} = \frac{\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R}\frac{\partial V(\cdot)}{\partial w_A \partial t} - \frac{\partial^2 V(\cdot)}{\partial w_A^2}\frac{\partial V(\cdot)}{\partial w_R \partial t}}{\frac{\partial^2 V(\cdot)}{\partial w_A^2}\frac{\partial^2 V(\cdot)}{\partial w_R^2} - (\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R})^2}, \tag{13}$$

where the denominator is the determinant of the Hessian, and is therefore positive. Define it as $soc := \frac{\partial^2 V(\cdot)}{\partial w_A^2}\frac{\partial^2 V(\cdot)}{\partial w_R^2} - (\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R})^2 > 0$.

Moreover, from equation (8) compute the following derivatives:

$$\frac{\partial^2 V(\cdot)}{\partial w_i \partial t} = -U''(\cdot)t(W - (w_A + w_R)) + U'(\cdot) \tag{14}$$

$$\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R} = -U''(\cdot)t^2 + \frac{\partial^2 C}{\partial w_A \partial w_R} \tag{15}$$

$$\frac{\partial^2 V(\cdot)}{\partial w_i^2} = -U''(\cdot)t^2 - \frac{\partial^2 C}{\partial w_i^2} \tag{16}$$

Substituting (14), (15), (16), and *soc* in equations (12) and (13) gives:

$$\frac{\partial w_A}{\partial t} = \frac{(\frac{\partial^2 C}{\partial w_R^2} - \frac{\partial^2 C}{\partial w_A \partial w_R})(U'(\cdot) + U''(\cdot)t(w_A + w_R - W))}{soc} \tag{17}$$

$$\frac{\partial w_R}{\partial t} = \frac{(\frac{\partial^2 C}{\partial w_A^2} - \frac{\partial^2 C}{\partial w_A \partial w_R})(U'(\cdot) + U''(\cdot)t(w_A + w_R - W))}{soc}, \tag{18}$$

where $U'(\cdot) + U''(\cdot)t(w_A + w_R - W) > 0$ since $w_A + w_R - W < 0$, and standard conditions on the utility function are satisfied such that $U'(\cdot) > 0, U''(\cdot) < 0$. Thereby, the sign of $\frac{\partial w_R}{\partial t}$ and $\frac{\partial w_A}{\partial t}$ is determined as in equation (9).

B Appendix: Descriptive statistics

This appendix presents descriptive statistics and denominations of the X variables included in the estimation of propensity scores and treatment effects. In particular, economic theory and knowledge of the policy setting leads to the inclusion of 90 control variables. Differently from past literature, this analysis does not ex-ante select among possibly relevant variables. The collected data is richer than that available in previous studies, and the employed random forest approach works well with large and high-dimensional data.

Data errors due to, e.g., typing and misreporting, are analyzed by looking at anomalies in MSW generation over time, specifically, by comparing deviations to the municipal-specific median with the sample upper bound (SUB) defined by $Q_3 + (Q3 - Q1)$ where Q is the quartile value, $(Q3 - Q1)$ the interquartile range. Municipalities with deviations to the median beyond the SUB are eliminated from the data. Looking at possible outliers, the sample includes all extreme outcome values but excludes units with extremely low values in recycling shares and per capita recycling to increase the comparability of municipalities' waste management practices. Extreme values are eliminated if they are below the sample lower bound (SLB) defined by $Q_3 - 0.5(Q3 - Q1)$ where 0.5 is chosen to avoid that Q_3 takes negative values.

Table 8: Descriptives for treated (T) and never-treated (NT) municipalities (2010-2015)

	mean(T)	min(T)	max(T)	Sd(T)	mean(NT)	min(NT)	max(NT)	Sd(NT)
PAYT price	0.08	0.01	0.18	0.05	0	0	0	0
RW	315.41	84.79	721.51	93.14	353.94	13.13	828.52	82.84
TW	486.00	350.62	1594.06	139.08	457.40	107.63	1193.25	127.03
UW	170.59	35.83	1016.42	112.62	235.47	21.62	867.26	120.53
RWrate	0.66	0.17	0.90	0.15	0.53	0.03	0.91	0.17
costRW	51.30	0.11	294.66	22.39	40.34	0.11	327.44	22.48
costUW	44.73	6.65	134.79	21.47	53.83	0.11	682.79	34.26
costTW	96.03	21.68	301.31	26.75	94.17	2.30	805.79	38.21
avgCostRW	0.17	0.00	0.60	0.07	0.18	0.001	2.24	0.12
avgCostUW	0.37	0.02	1.78	0.23	0.28	0.00	4.40	0.19
avgCostTW	0.21	0.03	0.67	0.07	0.22	0.005	1.93	0.09
distPayt	22.55	0.00	339.55	35.02	50.00	1.00	371.26	63.35
distHaz	10.71	0.00	48.13	9.92	13.80	0.00	115.40	15.40
distInc	26.29	0.00	72.49	21.30	29.00	0.00	178.01	28.39
distLandf	7.70	0.00	24.51	6.49	11.21	0.00	89.93	10.95
popDens	263.87	3.80	3581.35	396.69	337.29	1.33	7765.52	579.97
hhSize	2.49	1.12	6.95	0.83	2.31	1.00	7.09	0.34
income	14.44	6.97	22.98	1.95	13.86	4.66	45.62	2.29
migrNet	0.00	-8.80	14.68	0.55	0.00	-0.14	0.18	0.01
pop	8.52	0.12	192.84	19.27	7.35	0.03	1345.85	34.72
foreignPop	0.10	0.01	0.19	0.04	0.08	0.00	0.41	0.04
males	0.49	0.41	0.54	0.01	0.49	0.39	0.69	0.02
popGrowth	0.01	-0.72	0.56	0.12	0.00	-1.28	1.49	0.15
tourism	0.36	0.00	9.34	1.01	0.33	0.00	11.64	0.85
age0	0.05	0.03	0.06	0.01	0.05	0.03	0.06	0.01
age14	0.14	0.07	0.21	0.02	0.13	0.00	0.35	0.03
age65	0.22	0.11	0.34	0.04	0.35	0.05	0.51	0.05
elemDeg	0.31	0.19	0.42	0.04	0.31	0.14	0.57	0.05
collegeDeg	0.09	0.03	0.17	0.03	0.09	0.03	0.18	0.03
rentedHouses	0.09	0.04	0.17	0.02	0.09	0.00	0.44	0.03
hhPerSqMeter	2.24	1.68	3.08	0.26	2.30	1.28	3.27	0.27
oneParentFam	0.10	0.06	0.16	0.01	0.10	0.00	0.19	0.02
students	0.06	0.04	0.08	0.01	0.06	0.05	0.09	0.01
commuters	0.26	0.09	0.36	0.05	0.26	0.06	0.37	0.06
deprIndex	-1.87	-6.42	1.70	1.32	-1.50	-7.62	6.14	1.58
outLabRate	0.62	0.51	0.78	0.05	0.63	0.46	0.90	0.06
unempOutLab	0.06	0.01	0.13	0.02	0.07	0.00	0.35	0.02
labMarket	6.27	0.11	57.94	15.03	0.44	0.01	0.66	0.13
polPart	0.61	0.01	0.82	0.24	0.69	0.14	0.91	0.09
votesBigTent	0.35	0.09	0.37	0.05	0.35	0.00	0.58	0.07
votesLeft	0.06	0.02	0.24	0.03	0.07	0.00	0.84	0.10
votesRight	0.12	0.03	0.30	0.06	0.13	0.00	0.57	0.08
localMayor	0.85	0.00	1.00	0.36	0.76	0.00	1.00	0.42

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mayorCenter	0.00	0.00	0.00	0.00	0.00	0.00	1.00	0.05
mayorGreen	0.03	0.00	1.00	0.16	0.01	0.00	1.00	0.11
mayorLeft	0.07	0.00	1.00	0.26	0.07	0.00	1.00	0.25
mayorOther	0.14	0.00	1.00	0.35	0.11	0.00	1.00	0.32
mayorReg	0.66	0.00	1.00	0.47	0.71	0.00	1.00	0.46
mayorRight	0.10	0.00	1.00	0.30	0.10	0.00	1.00	0.30
mayorAge	51.14	21.00	78.00	10.47	52.04	22.00	87.00	10.47
yearsOffice	1.98	0.00	5.00	1.46	1.84	0.00	8.00	1.38
noSeat	0.97	0.00	1.00	0.17	0.97	0.00	1.00	0.17
provSeat	0.03	0.00	1.00	0.16	0.01	0.00	1.00	0.10
regionSeat	0.01	0.00	1.00	0.07	0.00	0.00	1.00	0.06
urban	0.48	0.00	1.00	0.50	0.38	0.00	1.00	0.49
urbanHigh	0.14	0.00	1.00	0.35	0.20	0.00	1.00	0.40
urbanLow	0.38	0.00	1.00	0.49	0.42	0.00	1.00	0.49

Table 9: Variables' description. Census indicates 2011 values (ISTAT, 2011).

	Variables' description
PAYT price	treatment: unit price on unsorted waste in € per liter
RW	recycling waste (RW) per capita (kg)
TW	total waste (TW) per capita (kg)
UW	unsorted waste (UW) per capita (kg)
RWrate	recycling rate (% of total waste)
costUW	per capita costs of UW management (euros)
costRW	per capita costs of RW management less recycling revenues (euros)
costTW	per capita costs of TW management (euros)
avgCostUW	average costs of UW management (euros per kg)
avgCostRW	average costs of RW management less recycling revenues (euros per kg)
avgCostTW	average costs of TW management (euros per kg)
distPayt	distance to closest municipality with PAYT in t-1 (km)
distHaz	distance to closest hazardous waste treatment facility (km)
distInc	distance to closest waste incinerator (km)
distLandf	distance to closest waste landfill (km)
popDens	population density (inhabitants per km2)
hhSize	average household size (n. household members)
income	income per capita (x 1,000 euros)
migrNet	net migrant flow per capita
pop	population (x 1,000 inhabitants)
foreignPop	share of foreign population
males	share of male population
popGrowth	population growth
tourism	capacity of tourist accommodation per capita (x 1,000)
age0	share of population aged less than 5 (census)
age14	share of population aged less than 14
age65	share of population aged more than 65
elemDeg	share of population with elementary degree or lower (census)
collegeDeg	share of population with college degree (census)
rentedHouses	share of rented houses (census)
hhPerSqMeter	housing density (inhabitants per 100m2, census)
oneParentFam	share of single-parent families (census)
students	share of population older than 15 and students (census)
commuters	share of commuters (census)

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deprIndex	social deprivation index (-/+ less/more deprived, census)
outLabRate	out-of-the-labor force rate (census)
unempOutLab	unemployed and out-of-the-labor force rate (census)
labMarket	commuting intensity between municipalities (IIRFL index 0-100, census)
polPart	voter turnout in the 2013 Italian general election (IGR13)
votesBigTent	vote shares for big-tent parties in the IGR13
votesLeft	vote shares for extreme left-wing parties in the IGR13
votesRight	vote shares for extreme right-wing parties in the IGR13
localMayor	mayor born in the municipal province (dummy)
mayorCenter	centre-party mayor (dummy)
mayorGreen	green-party mayor (dummy)
mayorLeft	left-wing mayor (dummy)
mayorOther	mayor of other party (dummy)
mayorReg	mayor of local party (dummy)
mayorRight	right-wing mayor (dummy)
mayorAge	mayor's age
yearsOffice	mayor's term of office (years)
noSeat	no capital (dummy)
provSeat	provincial capital (dummy)
regionSeat	regional capital (dummy)
urban	mediumly urbanized municipality (dummy)
urbanHigh	highly urbanized municipality (dummy)
urbanLow	lowly urbanized municipality (dummy)

C Appendix: The overlap assumption

Overlap analysis indicates that differences in treatment status can be explained to some extent by the included covariates. If X would fully (not) explain treatment assignment I would observe terminal nodes including only either treated or untreated units and, thus, Generalized Propensity Scores (GPS) equal to one and zero. This would indicate that treatment assignment is deterministic, namely there is no randomness that allows municipalities with identical characteristics to be observed in both states (Heckman *et al.*, 1997). Hence, overlap guarantees that a comparable unit can be found for each municipality. Figure 10 shows that PAYT units are fully in the support of non-PAYT units.

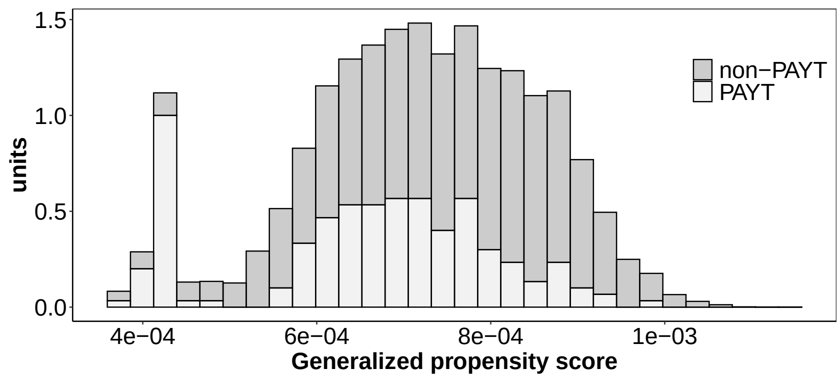

Figure 10: Common support condition for treated (PAYT) and never-treated (non-PAYT).

In particular, treated and control groups are similar in their GPS means, first and third quartiles. Statistics computed following Imbens & Rubin (2015) and Kluve *et al.* (2012) show that the two groups are not apart, namely, the (normalized) difference in estimated GPS is less than one standard deviation (0.62). Also, there is good coverage frequency for both treated and control group meaning that, specifically, 100% (96%) of the treated (control) units have GPS values inside the .025 and .975 quantiles of the GPS distribution of the control (treated) units. Further, all units have close comparisons in the opposite treatment group. In particular, for all treated units and for 96% of the control units there are units with the other treatment status that have differences in GPS less than 10%, a threshold that guarantees unbiased estimates of the causal effects without extrapolation (Imbens & Rubin, 2015). Therefore, causal effects for the control group, and not only for the subpopulation of treated units, can be credibly estimated under unconfoundedness.

D Appendix

D.1 Average point-price elasticities

Table 10 shows a comparison with the literature values of average point-price elasticities of waste demands. Typically, the literature relies on the assumption that the mean price and waste quantity are on one point along the linear demand curve (see e.g. Fullerton & Kinnaman, 2000). I derive the slope of the demand curve by estimating the APE in kg, and multiply this value by mean price and waste quantity. Table 10 reports estimates for all waste types and policy years.

Table 10: Average point-price elasticity estimates calculated at mean values with 95% confidence intervals [ci.low; ci.up].

Year	UW	ci.low	ci.up	RW	ci.low	ci.up	TW	ci.low	ci.up
+1	-0.38	-0.25	-0.51	0.18	0.12	0.24	-0.00	-0.06	0.05
+2	-0.56	-0.43	-0.68	0.16	0.10	0.35	-0.03	-0.02	0.01
+3	-0.90	-0.66	-1.14	0.17	0.08	0.25	-0.05	-0.09	-0.02

The estimate of -0.38 is in line with the literature's value of -0.344, the average elasticity estimate of UW demands one year after policy computed across 72 studies (Bel & Gradus, 2016). Cross-price elasticity estimates for RW are also in the range of previous estimates: I find higher values than Fullerton & Kinnaman (1996) (0.073), and lower values that Fullerton & Kinnaman (2000) (0.22) and Callan & Thomas (2006) (0.387).

Similarly to the literature, this study consistently estimates price inelastic waste demands. However, comparing elasticities across studies is not straightforward because mean price and waste quantity vary across data sets. The mean price in this study is generally higher, which can explain point-elasticity estimates above the literature's average.[45] In the first two policy years, my results are in line with e.g. Hong & Adams (1999) who finds that higher prices do not statistically influence total waste on average. However, after three policy years, higher prices lead to significant total waste reductions on average, suggesting that waste avoidance is rather a long-term effect.

D.2 Testing for effect heterogeneity

Levene's tests (1960) reject the null hypothesis of no CAPE heterogeneity for all outcomes. Moreover, as suggested in Athey *et al.* (2017), I perform the following heuristic to test for effect heterogeneity. First, I group observations into a high and low APE group using the median CAPE as a threshold. Next, I derive an estimate of the APE for each subgroup by residual-on-residual regression. Results are provided in the Table 11, and show statistically significant differences in APE between subgroups for all outcomes and policy years.

Table 11: Difference in APE (dUW, dRW, dTW in kg) between high and low APE group with 95% confidence intervals [ci.low; ci.up].

Year	dUW	ci.low	ci.up	dRW	ci.low	ci.up	dTW	ci.low	ci.up
1	-2.72	-2.98	-2.46	-5.05	-5.29	-4.82	-5.77	-6.04	-5.51
2	-3.69	-3.95	-3.43	-5.15	-5.38	-4.91	12.01	11.62	12.41
3	-4.49	-4.75	-4.24	-6.13	-6.35	-5.91	-3.32	-3.57	-3.07

[45] For instance, Callan & Thomas (2006) use a mean price of $0.012 per gallon which translates into less than one cent per liter (vs. mean price of about 8 cents per liter in this study).

D.3 Price causal effects on waste demands

Figure 11 reports effect heterogeneity across price levels. Price effects on the y-axis (CAPE Kg) are estimated as quantity changes for a one cent price increase. CAPE heterogeneity varies at low versus high prices, ceteris paribus: a one cent price increase reduces UW by 10.8 kg at low prices vs. 14.2 kg at high prices, ceteris paribus. Higher prices lead to increased recycling by 6.8 kg at low prices vs. 11.6 kg at high prices. The remaining UW amounts are due to significant TW reductions.

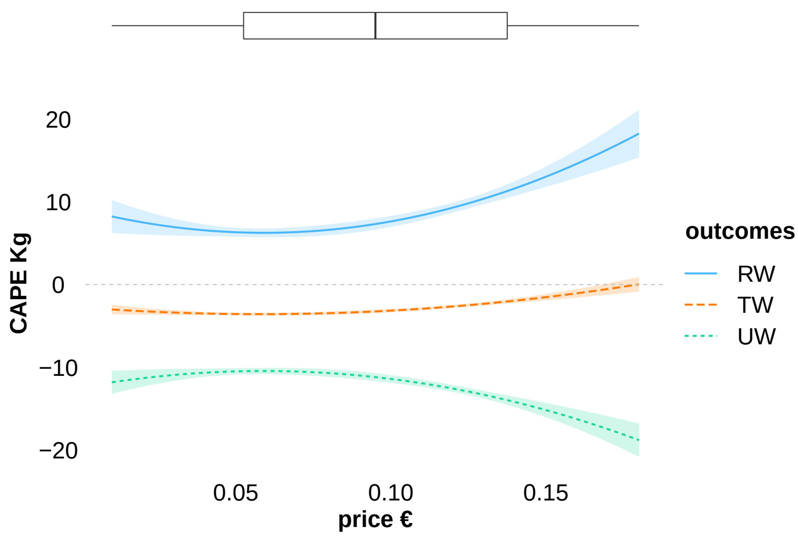

Figure 11: Fitted price effects on waste demands (CAPE Kg) as quantity changes for a one euro cent price increase.

Figure 12 reports effect heterogeneity across pre-policy RW levels. Effect heterogeneity is statistically significant only at high prices: municipalities recycling little before policy have generally higher elasticities, ceteris paribus, by on average 1.4 percent points.

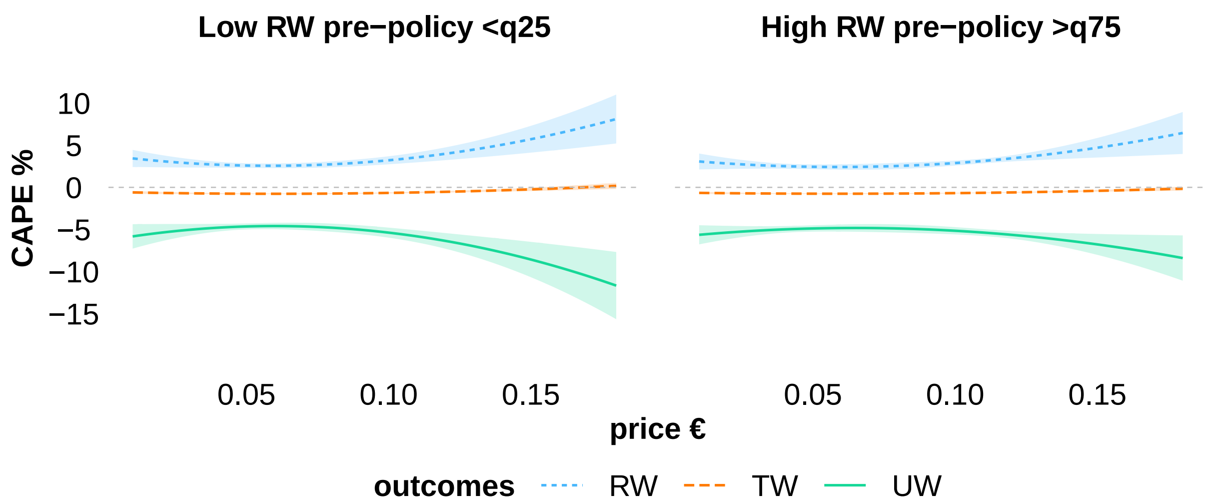

Figure 12: Fitted price semi-elasticities (CAPE) of waste demands by pre-policy recycling levels. Thresholds q25 and q75 indicate first (248 kg) and third (344 kg) quartiles of annual recycling per capita.

D.4 Heterogeneity in PAYT systems

D.4.1 Determinants of system choice

Evidence provided by manufacturers of PAYT technology (e.g., magnetic cards, tags and RFID readers) suggests that system choices are driven by three main factors. First, proximity: neighboring municipalities may either share the same waste hauler or influence each other through information dissemination. Second, population density: sparsely populated municipalities are more likely to adopt weight systems (PartItalia, 2020).[46] Third, geography: physical characteristics of the territory such as uphills and one-way roads impact transportation costs and collection modes.

I run a multinomial logit regression in order to assess whether observables can partly explain the choice of the adopted PAYT system. These are: population size (pop), distance to neighboring PAYT municipalities (distPayt), population density (popDens) as described in Table 9 in the Appendix B. Additionally, I distinguished communities by their metro status: urban, peri-urban, semi-peripheral, and very peripheral (the reference category). Categories are defined based on travel times (t) from each municipality to the closest urban center. Table 12 shows the results including all treated observations in the first

[46] Implementing curbside collection is especially difficult in low-density areas. Also, transportation costs would be high. Thus, communities generally prefer to organize waste collection in centralized disposal areas rather than door-to-door. As volume systems would imply carrying heavy bags and bins, weight systems facilitate households allowing them to carry smaller quantities of waste.

policy year. The dummy for weight systems is the reference choice.

Table 12: Average partial effect estimates of multinomial logit regression. Weight systems are the reference choice. Very peripheral areas (t>75') are the reference category for geographic dummies.

	Dependent variable:		
	emptying	bag	mixed
	(1)	(2)	(3)
pop	-0.085^{***}	-0.076^{**}	0.032
	(0.032)	(0.036)	(0.022)
popDens	0.009^{*}	0.011^{**}	0.011^{**}
	(0.005)	(0.005)	(0.005)
distPayt	-0.006	-0.017^{*}	-0.073
	(0.008)	(0.010)	(0.046)
peripheral(40<t<75')	1.189^{*}	1.655^{***}	28.377^{***}
	(0.663)	(0.000)	(0.663)
semi-peripheral(20<t<40')	-21.713^{***}	5.599^{***}	4.797^{***}
	(0.924)	(0.952)	(0.962)
semi-urban(t<20')	-24.078^{***}	2.523^{***}	2.132^{***}
	(0.699)	(0.718)	(0.781)
peri-urban	-24.819^{***}	1.111^{**}	-36.060^{***}
	(1.618)	(0.525)	(0.000)
urban	-20.275^{***}	4.625^{***}	-5.571^{***}
	(1.097)	(0.912)	(0.223)
Constant	36.339^{***}	13.313^{***}	0.799
	(1.145)	(0.980)	(0.706)
Observations	194	194	194
Year and area dummies	Yes	Yes	Yes
Akaike Inf. Crit.	284.558	284.558	284.558
Note:			*p<0.1; **p<0.05; ***p<0.01

Lower population density (popDens) significantly increases the probability to adopt weight programs over any other, as expected. Higher populated municipalities (pop) are more likely to implement weight vs. bag/emptying programs, perhaps to exploit returns to scale. Information dissemination effects (distPayt) positively increase the choice of bag programs. Finally, geographic characteristics seem to have similar effects for bag and mixed program choices with less peripheral municipalities being more likely to adopt these systems (with the exception of "peri-urban" for mixed systems). Most of emptying programs seem to be adopted in peripheral municipalities (rather than very peripheral or urbanized ones).

D.4.2 CAPE heterogeneity by system

Table 13: Pairwise comparison of price elasticities (CAPE) on waste amounts by PAYT system: Mean differences (dUW, dRW, dTW in kg per euro cent), standard errors and p-values estimated by Tukey-Kramer's method (1956).

PAYT system	dUW	s.e.	p-value	dRW	s.e.	p-value	dTW	s.e.	p-value
emptying - bag	0.12	0.27	0.96	0.47	0.24	0.18	0.55	0.23	0.06
mixed - bag	0.19	0.52	0.98	0.56	0.46	0.59	0.62	0.44	0.47
weight - bag	0.76	0.52	0.44	0.16	0.46	0.98	1.06	0.44	0.07
mixed - emptying	0.07	0.48	1.00	0.09	0.42	1.00	0.07	0.40	1.00
weight - emptying	0.64	0.48	0.52	-0.31	0.42	0.87	0.50	0.40	0.57
weight - mixed	0.57	0.66	0.81	-0.40	0.57	0.89	0.44	0.55	0.85

Differences are overall statistically insignificant at conventional confidence levels. Estimates only suggest that pay-per-bag systems are (weakly) associated to more waste avoidance (p-values < 0.1): Price semi-elasticities on avoidance are 0.55 and 1.06 kg higher than with pay-per-emptying and weight-based systems, respectively. Overall, my study does not point towards significant differences in price elasticities between PAYT systems.

D.4.3 Supporting analyses

Binary treatment case.–The binary case assumes that the policy has homogeneous effects across prices, and that policy adoption is homogeneous in prices. Table 14 reports average price effects (APE) estimated as average effects of a binary treatment at the mean price level ($ATE/\bar{P}$). The binary case overestimates APE on UW by 28% on average.

Table 14: Continuous APE vs. Binary APE estimates (kg) at the mean price (ATE/$\bar{P}$) from (level-level) residual-on-residual regression with year clustered standard errors.

	Continuous			*Binary*	
Year	UW kg	s.e.	*vs.*	UW kg	s.e.
1	-7.38	1.30		-7.60	2.55
2	-8.86	1.03		-11.3	1.97
3	-11.5	1.53		-16.5	1.08

Difference-in-differences.–When the timing of treatment varies across units, Difference-in-Differences (DiD) designs are commonly extended to allow for dynamic average treatment effects by including leads and lags of treatment as regressors (Jacobson *et al.*, 1993). These dummy variables allow policy effects to vary by the number of years relative to separation from policy. Figure 13 presents the results from a dynamic (event-study-like)

DiD regression estimated using a standard two-way (unit and time) fixed effects model.[47] The regression includes three leads and lags, and uses the second (unaffected) lead before policy (-2) as a baseline dummy. Since potentially relevant covariates are many and partly collinear, and there is no a priori guidance on which one to exclude, this DiD only controls for time-invariant waste generation determinants captured by municipal fixed effects. Figure 13 plots lead and lagged average price effect (APE) estimates (continuous treatment) with their confidence intervals. Black dots represent statistically significant effects at 5%, and vertical dotted lines indicate the policy adoption year (1).

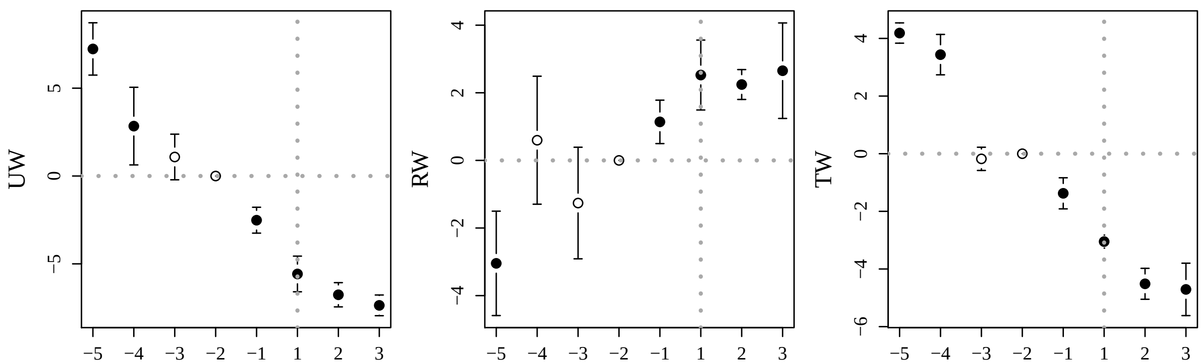

Figure 13: Dynamic DiD estimates (2010-2015, $n = 19,982$) of average price effects (APE kg) on UW, RW, TW with year and unit fixed effects. Clustered standard errors (Driscoll & Kraay, 1994). Statistical significance (5%) indicated by black dots.

Considering the pre-policy period, DiD estimation shows non-zero and statistically significant coefficients on leads. This indicates the presence of pre-trends which invalidate causal effect estimates due to violation of the generalized parallel trends assumption necessary to identify the dynamic DiD estimator (Freyaldenhoven *et al.*, 2019; Abraham & Sun, 2018). The bias possibly derives from having erroneously assumed selection based on time-constant variables. This could be corrected by including time-varying characteristics and allowing variables to have time-varying effects on policy adoption and waste generation. Yet, DiD only allows for time-invariant unobserved effects on the outcome, which might not be the case in this setting (Bueno & Valente, 2019; Gobillon & Magnac, 2016). For instance, opportunity costs of waste disposal may have evolving effects over time. Including pre-policy waste outcomes and a high-dimensional set of covariates aims to capture types of heterogeneity that jointly influence the dependent variable and the price. Controlling for this variation is at the heart of this paper's motivation to employ a forest-based approach.

[47]For the analysis, I use the software R, and, particularly, the package plm (Croissant & Millo, 2008).