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Working Paper

Policy evaluation of waste pricing programs using heterogeneous causal effect estimation

DIW Discussion Papers, No. 1980

Provided in Cooperation with:

German Institute for Economic Research (DIW Berlin)

Suggested Citation: Valente, Marica (2021) : Policy evaluation of waste pricing programs using heterogeneous causal effect estimation, DIW Discussion Papers, No. 1980, Deutsches Institut für Wirtschaftsforschung (DIW), Berlin

This Version is available at:

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1980

Discussion
Papers

Policy Evaluation of Waste Pricing Programs Using Heterogeneous Causal Effect Estimation

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IMPRESSUM

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ISSN electronic edition 1619-4535

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Policy evaluation of waste pricing programs using heterogeneous causal effect estimation

Marica Valente*

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 constant elasticities 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 are positive for all municipalities after three years of adoption, when waste prices cause significant waste avoidance.

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

JEL Codes: C14, C21, C52, Q53

*ETH Zurich (mvalente@ethz.ch) and DIW Berlin. This paper was previously circulated under the title “Heterogeneous effects of waste pricing policies” which is one chapter of my PhD dissertation written at DIW Berlin and Humboldt University. I thank Bernd Fitzenberger, Jeffrey Wooldridge, and Lorenzo Trapani for their invaluable advice. I also thank Pio Baake, Marco Caliendo, Juan Carlos Escanciano, Elia Lapenta, Stefan Seifert, and Jann Spiess for fruitful discussion. I am grateful for seminar invitations and feedback received from participants of the NBER Environmental & Energy Economics Workshop, LSE Causal Inference Meeting, University Carlos III of Madrid, CREST, Aix-Marseille University, University of Innsbruck, ECB, Bank of Spain, Trinity College Dublin, Mines ParisTech, University of the Balearic Islands, University of Nottingham, University of Barcelona, University of Trento, LMU Munich, University of Hohenheim, University of Ferrara, Berlin Research Institute for Global Commons and Climate Change, University of St.Gallen, Institute for Employment Research (IAB) of Nuremberg, DIW Berlin, Berlin Network of Labor Market Research (BeNA), University of Southern Denmark, University of Potsdam, the Lindau Nobel Laureate Meetings, and the ETH/UZH Seminars in Economics & Data Science. This work did also benefit from presentations in the (2021) ESEM Annual Congress, New York Camp Econometrics, (2020) ES World Congress, EAERE Annual Conference, NERI Workshop in Naples, Causal Machine Learning Workshop in St. Gallen, and (2019) ES Winter Meeting, European Conference of the Society for Benefit-Cost Analysis, EEA Annual Congress, EAERE Annual Conference, IAERE Annual Conference, and EUI Annual Conference.

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 municipalities¹ have implemented 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. Policy average causal effects on waste quantities can be estimated, e.g., with fixed effects regression methods and synthetic controls.² However, empirical estimates provide mixed evidence on the magnitude of PAYT average effect on unsorted waste as well as on possible indirect causal effects on recycling and total waste. Moreover, policy impacts may vary across municipalities depending, e.g., on the adopted price level and socio-economic characteristics (Kinnaman, 2006). In light of this, heterogeneity in causal effects plays an essential role in evaluating policy efficacy, allowing to ascertain subpopulations for which the policy is most beneficial and to generalize causal effect estimates to a new target population. On top of this, heterogeneous causal effect estimates can be combined with waste management costs and benefits for municipal budgets and for the environment to deliver heterogeneous welfare evaluations of waste policies.

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 causal effects on unsorted and recycling waste with their impacts on pollution and municipal costs. I find that waste prices generate social cost savings for most municipalities since the first adoption year. Cost savings are highest in the long-term

¹PAYT is used in more than 800 U.S. cities, and is broadly implemented in Europe, Japan and Korea. China planned a number of pilots in big cities as Beijing (Central Maine, 2015).

²See, e.g., Bueno & Valente (2019) and the survey in Bel & Gradus (2016).

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

(after three adoption years) when municipalities show the highest reductions of total waste. Avoiding waste generation improves social welfare by reducing pollution and waste management costs for municipalities.⁴

This paper aims to contribute to the causal inference literature. I use machine learning methods to estimate heterogeneous causal effects when units are heterogeneous not only in their observable characteristics but also in their treatment level. I provide a simple approach to interpret causal effect heterogeneity in the treatment level, and to exploit unit level estimates for policy targeting and social welfare evaluation. Also, I apply machine learning methods in such a way to account for a high-dimensional set of possible sources of self-selection and for staggered policy adoption. In addition, while few applications of machine-learning to binary treatment effect models can be found in labor economics,⁵ no study used high-dimensional methods for causal effect estimation in environmental economics.

My work also adds to a large and growing literature on waste prices. Prior research estimates the average price elasticity of waste demands or the average causal effect of PAYT (binary treatment) on waste amounts.⁶ Compared to these studies, this paper requires less restrictive identifying assumptions, in particular, allowing for possibly heterogeneous price effects across units (heterogeneity in prices and observables). Further, it aims to deliver more precise estimates as it exploits a large variation in prices and flexibly accounts for many possible sources of self-selection.

Beyond the methodological contribution, this paper looks for the first time at demand responses at high prices, providing new evidence on PAYT effectiveness.⁷ Previous studies typically estimate low price elasticities of waste demands (for a review see, e.g., Kinnaman, 2006). In line with prior work I find low elasticities for municipalities adopting low prices. However, for municipalities adopting high prices, I find significantly larger elasticities. Moreover, at high prices waste demands also respond to other factors, in particular, elasticities are largest for municipalities with low income and little recycling before policy. Finally, this is the first study to perform a welfare analysis at municipal level, showing significant social cost savings from PAYT adoption for most municipalities.

⁴Waste avoidance is defined as using effort to reduce total waste (via e.g. product reuse). Throughout the paper, waste avoidance and total waste reductions are used interchangeably.

⁵See, e.g., Gulyas & Pytka 2019; Athey & Wager 2019; Davis & Heller 2017.

⁶See, e.g., Bueno & Valente 2019; Carattini *et al.* 2018; Bucciol *et al.* 2015; Huang *et al.* 2011; Allers & Hoeben 2010; Fullerton & Kinnaman 2000, 1996. See Kinnaman 2014 for a review.

⁷The highest price in, e.g., Fullerton & Kinnaman (2000) is \$2.18 per 32-gallon bag (120L) or \$0.02/L. My data include prices up to €0.18/L. See Bel & Gradus (2016) for a survey.

A body of work in behavioral environmental economics analyzes drivers of waste behaviors.⁸ Building on this literature, I rationalize welfare effect heterogeneity by showing that policy responses on recycling and waste avoidance are ambiguous from a theoretical viewpoint. In the empirical part, I account for several hypothesized sources of response heterogeneity, and I disentangle their relative importance.

I study household 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).⁹ 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).¹⁰ The random forest instantiation of the R-learner allows to flexibly control for a high-dimensional set of outcome determinants and sources of self-selection. Further, thanks to data subsampling and aggregation, random forests can be adapted to account for staggered adoption designs.¹¹

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 allows for parameter heterogeneity in the large set of (often correlated) covariates.¹² 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.

⁸See, e.g., D’Amato *et al.* 2016, Bowles & Polania-Reyes 2012, and Gilli *et al.* 2018b.

⁹RFs build upon Athey & Wager (2018), Athey & Imbens (2016) and, firstly, Breiman (2001).

¹⁰Robust to confounding affecting outcome and treatment (Chernozhukov *et al.*, 2017).

¹¹See implementation details in Section 3.

¹²Differently, standard regression methods are justified if treatment effects are constant, observables have linear or pre-specified effects, and unobservables are time-invariant (Wager, 2020).

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 and interpret municipal level causal estimates, 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.¹³ 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. At high prices, 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%) compared to a counterfactual zero-price (no PAYT) scenario. Analyzing price indirect effects on recycling and total waste, I find that higher prices do not cause further total waste reductions, instead they simply reallocate waste to the recycling pile.

Having established heterogeneity of price effects, the second part of the empirical analysis focuses on municipal waste management costs and benefits. I estimate that PAYT leaves unit costs of waste mostly unaffected, suggesting constant returns to scale.¹⁴ In a small share of municipalities, I estimate that PAYT causes an increase (decrease) of unit costs of unsorted waste management (recycling). Municipal private benefits of recycling from selling the fraction of recycled material are included in the analysis.

Next, I simulate the impact of PAYT adoption on social welfare combining predicted (causal) changes in unsorted and recycling waste for each municipality with unit costs of waste management under PAYT as well as prior estimates for the relative environmental costs of unsorted versus recycling waste (Kinnaman *et al.*, 2014). After three years of adoption, I predict welfare benefits for all municipalities of on average €30 up to €170 per person. As unit costs of unsorted waste are higher than those of recycling for most municipalities, total waste reductions trigger large welfare benefits. This implies that also low prices – by causing significant total waste reductions – improve waste behavior and social welfare substantially.

I present additional analyses that show robustness of my results against the presence of unaccounted-for heterogeneity and spillover effects (waste tourism).

¹³Their relevance is discussed in, e.g., Bueno & Valente (2019) and Gilli *et al.* (2018b).

¹⁴This is consistent with results on Italian municipalities by, e.g., Abrate *et al.* (2014).

As often found in the literature (see Bel & Gradus, 2016, for a review), my results are robust to confounding from adoption of heterogeneous PAYT systems (weight versus volume). Further, I find that waste tourism is not relevant on any significant scale. Finally, I contrast my average estimates to the binary treatment case, two-way (event-study-like) fixed effects estimation (with/without re-weighting á la de Chaisemartin & D’Haultfœuille 2020), and R-learning LASSO regression. Results highlight the importance to account for continuous rather than binary treatment, the bias of the fixed effects estimator due to non-parallel trends pre-policy, 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 empirical framework. 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. Yet, political and socio-economic factors may also matter (see Gradus *et al.* 2019 and Section 1.4 for details). In sum, drivers of price adoption and waste generation often overlap, and are possibly many. Their relevance is, therefore, an empirical question.

Credible enforcement and monitoring systems are crucial for policy success.¹⁵ Illegal dumping is the main possible adverse effect. However, after about fifty years of PAYT experiences worldwide, adverse effects seem a bigger fear than reality.¹⁶ 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, amounts of household waste (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.¹⁷ Treated municipalities for which second-order lags are missing are also excluded.¹⁸

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.

¹⁵E.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).

¹⁶See, 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.

¹⁷Missing values are due to merging administrations and data errors. 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).

¹⁸PAYT is typically announced in the first year pre-policy. Thus, adoption decisions correlate with second- or earlier-order outcome lags, and first-order lags are biased predictors due to anticipation effects.

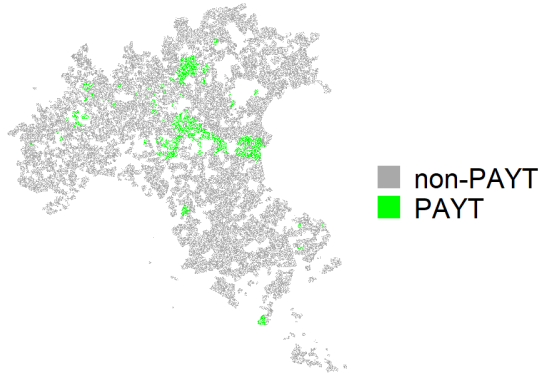


Figure 1: Map of PAYT and non-PAYT municipalities in the sample. White areas show the excluded South and Islands, municipalities with missing values, and those adopting PAYT before 2012.

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.¹⁹ I collected a cross-section of price data directly from municipalities and companies upon request, and indirectly from municipal balance sheets. Each municipality sets the same price over the three post-policy years analyzed in this study. Detailed information on data management, variable denomination and descriptive statistics is presented in Appendix B.

¹⁹E.g., Aimag, Ascit, Clara, Dolomiti Energia, Hera, Iren, SEAB, Soraris, etc.

1.3 Summary Statistics

Table 1 compares key attributes of never-treated (non-PAYT) and treated (PAYT) units before policy.²⁰ 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 statistics of the included attributes are presented in Tables 7 and 8 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 in kilograms (kg) per capita (p.c.). Over 2010-2015, observations equal 18,812 for never-treated (unbalanced panel) and 636 for treated before policy (balanced panel).

		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
	(... up to 90 variables)	(...)	(...)	(...)	(...)

Municipalities generate relatively more RW than UW on average. Managing one unit of UW costs on average twice as much as RW. However, municipal unit costs are largely heterogeneous, and for some of these municipalities (40%) unit costs of RW are higher than those of UW. Table 1 also shows that average costs for UW are slightly higher 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.²¹ This implies that UW reductions that do not translate into RW increases will potentially drive cost savings for both households and municipalities. Previous studies point to education and income levels as key determinants of policy adoption and effectiveness (see, e.g., Gradus *et al.*, 2019; Kinnaman, 2006). In my

²⁰To simplify the exposition, variable values are averaged over all years but interpretation of summary statistics does not change when values are averaged over individual years.

²¹Municipal 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.

sample, PAYT is implemented in municipalities with on average slightly higher income and comparable education levels.

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

Table 2: Before-and-after comparison of waste outcomes and management costs of treated units. Observations equal 1,164 for 194 treated units observed over all years (balanced panel).

		<u>Before</u>		<u>After</u>	
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.²² 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

²²I use waste haulers' data for liter-to-kg conversion. If missing, I use the median value.

questions. After partialling out confounding factors, do reactions in municipalities setting high versus low prices differ? What role do income effects play in driving these differences? More broadly, how do behaviors toward recycling and waste avoidance adjust to a decrease in unsorted waste?

1.4 Determinants of waste generation and policy adoption

A large number of variables (confounders) can explain the decision to adopt PAYT as well as differences in waste generation across municipalities. I control for a large set of observables (deconfounders) that potentially affect household waste generation and policy adoption decisions at municipal level. Municipalities decide to implement PAYT because of possibly many single or combined factors (Gradus *et al.*, 2019): high levels of waste and/or waste management costs pre-policy, awareness of the waste pollution problem, citizens' demand for higher-quality services, political variables, social norms, information effects, a developed recycling infrastructure and large administrative capacity. Further, as shown in Bueno & Valente (2019), leftover unobserved time-varying heterogeneity can be controlled for by including outcome lags. As a result, 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, information 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.²³ 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

²³See, 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).

share of unemployed and out-of-the-labor force population, and indices 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).

Information effects.—Vicinity to other PAYT municipalities may matter because of information dissemination: municipalities may adopt PAYT to mimic successful neighbors' policies (Allers & Hoeben, 2010). As a proxy, I control for the distance to the closest municipality that implemented PAYT in prior years.

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; Batllell & Hanf, 2008). Public engagement may correlate with waste behaviors as well as with policy acceptance and reactions to policy. 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 responses on waste generation. Protest votes and extreme ideology are proxied by vote shares for big tent parties, and for extreme left- and right-wing parties. Moreover, mayor characteristics can matter for policy adoption decisions and also for waste outcomes because they may shape waste-friendly behaviors. 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 promote pollution-saving behaviors, and 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.—Policy 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, and administrative services, net of recycling revenues from selling products and energy recovery.

2 Empirical Framework

2.1 Motivation

I am interested in how a price on household unsorted waste disposal affects waste generation and social welfare at municipal level. 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 increased recycling. Thus, empirically, it is key to understand the behavioral mechanism behind PAYT causal effects, namely, the extent to which unsorted waste reductions translate into more recycling and waste avoidance (less total waste).

I present a simple model in Appendix A to fix ideas on the fact 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. The 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. In light of possi-

bly heterogeneous demand responses on unsorted and recycling waste as well as the large heterogeneity in municipal waste management costs (see Table 1), social welfare effects of PAYT are ex-ante ambiguous, and remain an empirical question.

2.2 Identification and Empirical Specification

As described in Section 1.4, 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 PAYT adoption and waste generation.

I follow the potential outcome approach (Rubin, 1974). Let (X_{it}, Y_{it}, P_{it}) be the available data for municipality $i = 1, \dots, n$ at time $t = 2010, \dots, 2015$, 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 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 $\Delta(x) = \mathbb{E}[Y_i(p) - Y_i(0) | X_i = x]$ 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 $Y_i(p) \perp P_i | X_i \forall p \in [0; p_{max}]$, 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.²⁴ While this

²⁴Yet, 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,

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 3.6.1.

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 make this assumption more reliable, I control for information effects across treated units, and I assess robustness to spillovers in untreated units (see Sections 1.4 and 3.6.1).

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:

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

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

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

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

2000, for details).

in high-dimensional settings, make the parameter estimate insensitive to small errors in the nuisance components, thus improving its robustness. The final estimator combining predictions from two models is robust to possible misspecifications in either the outcome model or the propensity score model (Nie & Wager, 2019). 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).²⁵ 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 (Nie & Wager, 2019).

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 may allow to consistently estimate the full 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

²⁵Without regularization, the inclusion of a large set of partly correlated predictors may lead to, e.g., variance inflation and incorrect signs.

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 $b = 1, \dots, B$, draw a random subsample of training data $s_b^{tr} \subseteq \{1, \dots, n\}$; (2) Estimate a tree via recursive partitioning on each such subsample of the data:²⁶ 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:

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

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 $i \notin s_b^{tr}$.²⁷

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:

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

²⁶The 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.

²⁷Without 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).

where $\hat{s}^{-i}(X_i)$ are out-of-sample predictions of the propensity scores $s(x)$. Equation (4) defines 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.²⁸ This distinguishes the CAPE from the APE (Average Price Effect) estimator which is the unweighted version of (4).²⁹ 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 $i = 1, \dots, n$ 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:

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

$CATE_{UW}(x)$ 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

²⁸RFs 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.

²⁹Motivated by the R-learning equation (2), I estimate the APE as $\frac{\sum_{i=1}^n (Y_i - \hat{y}^{-i}(X_i))(P_i - \hat{s}^{-i}(X_i))}{\sum_{i=1}^n (P_i - \hat{s}^{-i}(X_i))^2}$.

marginal costs of unsorted waste and recycling, respectively, where the first is positive and the second is negative (in €/kg).³⁰ 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.

$CATE(x)$ is estimated for each waste type (here, UW and RW) as $p\hat{\delta}(x)$ with $\hat{\delta}(x)$ being *statistically significant* CAPE estimates for each municipality and 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. PAYT adoption may cause a significant change in the unit costs of treated units. Counterfactual estimates of unit costs are needed to predict welfare effects for untreated units with similar characteristics to those treated units whose unit costs are significantly affected by PAYT adoption. In order to predict counterfactual values for untreated units, I use the RF running the R-learner 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 whether PAYT affects unit costs significantly. If this is the case, I replace $PC^{P>0}(x)$ for untreated units with $\hat{P}C^{P>0}(x)$, their predicted counterpart under PAYT.

EC rely on prior estimates and are assumed to be constant across units.³² Unsorted waste disposal causes external costs associated to landfill and incineration.³³ A review of the life-cycle literature shows that external costs are constant and rather small. 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

³⁰The treatment process of recycling also causes external costs. Yet, benefits overcompensate costs (Kinnaman *et al.*, 2014).

³¹As 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).

³²Municipal level estimates of the environmental impacts of unsorted and recycling waste are not available. This is generally the case as engineers' and geologists' estimates are not monetized.

³³External costs include pollution from waste emissions and transportation, and local disamenity externalities such as nuisance effects to neighboring properties (Kinnaman *et al.*, 2014).

unsorted waste.³⁴

Recycling waste causes, to a large extent, external benefits. 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 and unsorted waste are higher than their external benefits for most municipalities. 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, demand responses on total waste matter for welfare.

3 Main results

I estimate separate random forests for each outcome (unsorted, recycling, total waste per capita) and policy year.³⁵ A tree is built with data for the same calendar year to account for common shocks to all units. In order to account for the staggered nature of the intervention, treated units in a given policy year are matched with never-treated units only. In other terms, late adopters are not included in the control group of early adopters. This is important especially since lagged outcomes are included among the covariates.

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. 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. To overcome the issue that 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

³⁴Data on amounts sent to landfills and incinerators are unavailable. Thus, I consider the average cost.

³⁵Policy years range between one and three as treated units are observed up to three years after policy implementation.

Athey *et al.* (2019), values for such parameters are obtained via cross-validation.³⁶

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 Policy causal effects on waste demands

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 closer estimate to the long-term effects of PAYT.³⁷ 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.³⁸

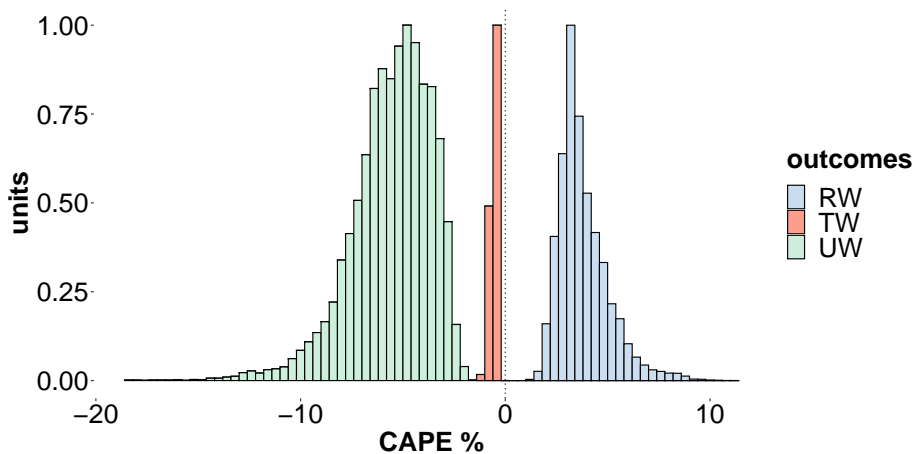


Figure 2: Unit level estimates of price semi-elasticities (CAPE) of waste demands. CAPE are measured as percent changes of waste amounts for a one cent price increase.

The estimated price effects on waste demands for UW and RW are large in mag-

³⁶I use the software R-3.4.2 and the grf package version 0.10.0 (Tibshirani *et al.*, 2018).

³⁷I analyze the dynamics of price effects in Section 3.4. One caveat is that these elasticities cannot be estimated for late adopters. Reassuringly, there are no statistically significant differences in the price effect distribution of early and late adopters in the first two policy years.

³⁸See Levene's tests (1960) and heuristics (Athey *et al.*, 2017) in Appendix D.1.

nitude. I use the estimated CAPE to compute factual and counterfactual waste amounts, and the percent (causal) change in waste relative to the counterfactual. I find 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%.³⁹ The large substitution effect of UW with RW caused by the policy, therefore, represents a strong change in waste behaviors.

3.2 Sources of causal effect heterogeneity

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 paribus*.⁴⁰ Pointwise estimates are obtained by fitting a smooth regression line. Shaded regions are the 95% confidence intervals.

³⁹This corresponds to -110 kg (UW), +80 kg (RW) and -25 kg (TW) per capita. Thereby, estimates from separate forests are consistent with each other ($CATE_{TW} \approx CATE_{UW} + CATE_{RW}$).

⁴⁰Data 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.2.

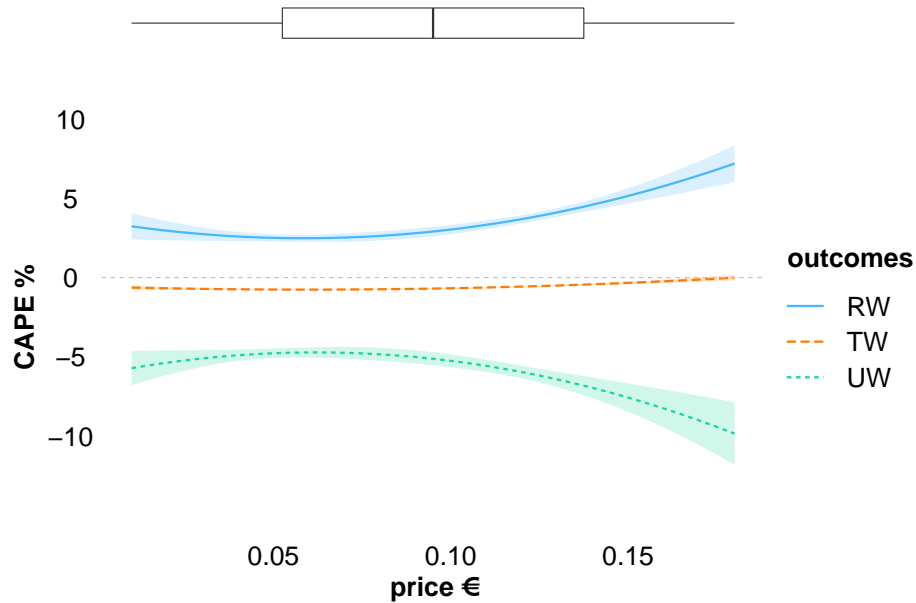


Figure 3: Fitted price semi-elasticities (CAPE) of waste demands at each price level.

Price elasticities 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. 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%) compared to a counterfactual zero-price (no PAYT) scenario. Further, demand responses at high prices shed 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 substitute behaviors.⁴¹ Thus, higher prices do not cause further total waste reductions, 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*.

⁴¹Section 3.5 provides estimates of the elasticity of substitution between recycling and waste avoidance behaviors.

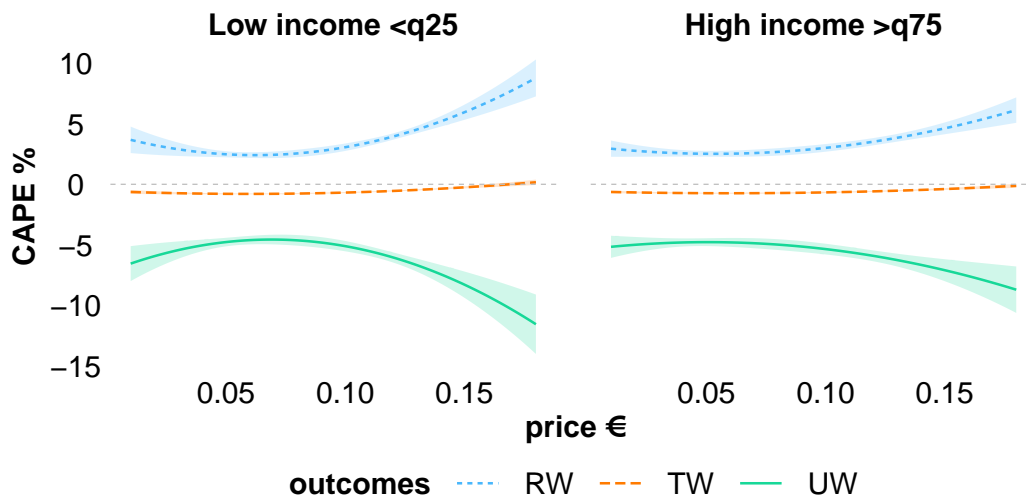


Figure 4: Fitted price semi-elasticities (CAPE) of waste demands by income levels. Thresholds q25 and q75 indicate first (€13k) and third (€16k) quartiles of annual income per capita.

Income differences across municipalities significantly explain price effect heterogeneity at high prices: Low-income municipalities are on average one percent point more elastic than high-income municipalities.⁴² This result is new to the literature as previous studies typically consider the effects of low prices.⁴³ Differently, income effects are not statistically significant at low prices. This may be explained 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.

Additionally, price effects at high prices are heterogeneous also in recycling habits before policy, as predicted by Kinnaman (2006). In particular, high prices are effective in increasing recycling levels in low-recycling municipalities which have large margin to improve on this behavior.⁴⁴

Finally, looking at heterogeneity across education levels, I estimate no significant effects, suggesting that education plays, if any, a relatively minor role.

⁴²In 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.

⁴³The 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.

⁴⁴See Figure 12 in Appendix D.2.

3.3 Comparison with the literature

Typically, the literature estimating demand responses of waste prices relies on the assumption that the mean price and waste quantity are on one point along the linear demand curve, and finds low average price elasticities (see, e.g., Kinnaman, 2006; Fullerton & Kinnaman, 2000). However, prior work analyzes waste programs with low adopted prices.⁴⁵ The average point-price elasticity of unsorted waste demand across 72 studies amounts to -0.34 (Bel & Gradus, 2016). For comparison, I compute the point-price elasticity at mean price and quantity in the first and fourth quartile of the price distribution. I find a value of -0.26 in the lowest quartile, and a value of -2.2 in the highest quartile. In line with the literature, this study consistently estimates low price elasticities of waste demands when prices are low. Additionally, I show that waste demands are significantly more elastic at high prices.

3.4 Welfare analysis

Welfare effect heterogeneity is driven by both demand and cost effects. 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. In particular, 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.⁴⁶ Figure 5 plots the distribution of statistically significant demand effects on TW by policy year.

⁴⁵For instance, Callan & Thomas (2006) use a mean price of \$0.012 per gallon which translates into less than one cent per liter.

⁴⁶For instance, households need to learn how to reuse and buy products with less packaging.

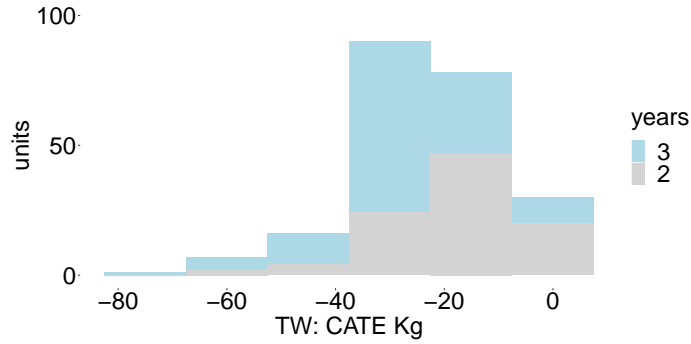


Figure 5: Unit level estimates of causal effects (CATE) on total waste demands in the second and third policy year (p-values < 0.05). The y-axis refers to the number of treated municipalities.

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) 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) at municipal level. Positive SWE indicate welfare benefits from PAYT adoption estimated as reductions of private (management), external (environmental) and social cost per capita (p.c.) by policy year.

Year	SWE € p.c.	Mean	Sd	SWE>0
1	Private	0.13	10.16	36%
	External	6.87	5.33	all
	Social	7.01	10.35	91%
2	Private	3.14	7.10	89%
	External	5.96	4.75	all
	Social	9.11	9.08	99%
3	Private	20.17	16.28	all
	External	4.45	3.52	all
	Social	24.62	19.71	all

Welfare effects are mostly positive from the first adoption year. However, waste prices raise social costs in a small fraction of municipalities where the reduction of unsorted waste is largely driven by an increase in recycling, and management costs of recycling are high compared to those of unsorted waste. Further, in year 3, PAYT generates welfare benefits in all 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.

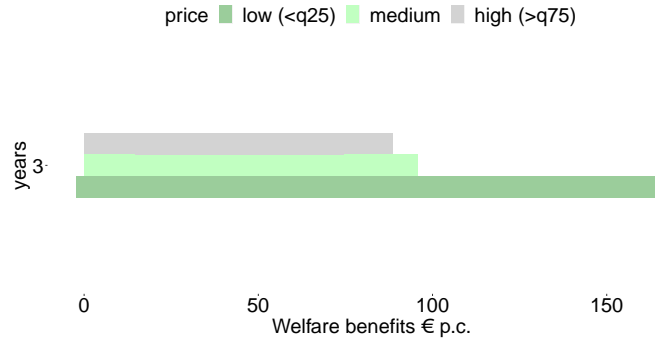


Figure 6: Social welfare effects (largely, welfare benefits) at different price levels after three years of adoption. First and third quartiles correspond to 3 and 13 cents, respectively.

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.

The final implication of this paper may help local governments to overcome their initial fears regarding the implementation of PAYT policies.⁴⁷ This paper provides evidence that short-term costs may indeed increase due to higher recycling, but these costs are outrun by the long-term savings due to reductions in overall waste.

⁴⁷In 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).

3.5 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.

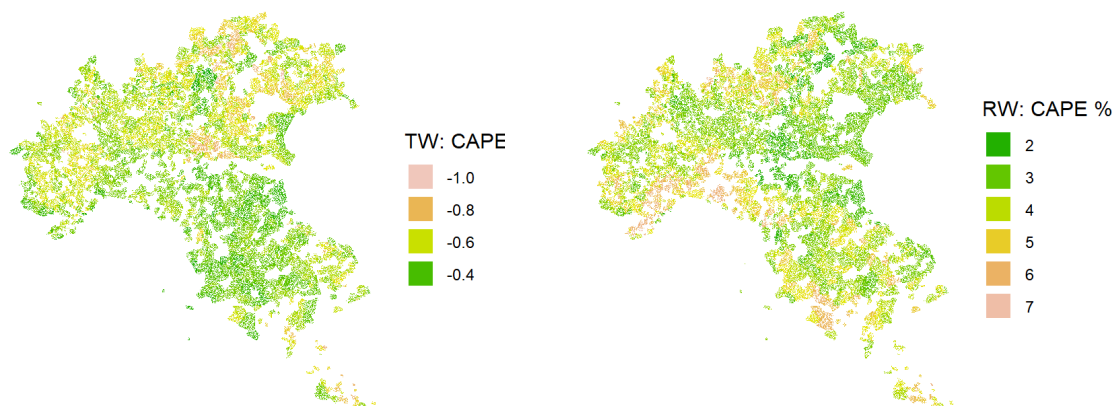


Figure 7: Predicted semi-elasticities (CAPE) on total waste and recycling demands for all municipalities three years after policy. Higher elasticities on waste avoidance and recycling indicated by warmer colors.

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

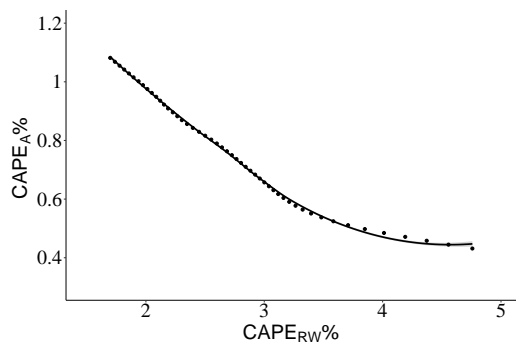


Figure 8: CAPE (semi-elasticities) for recycling (x -axis) plotted against CAPE for avoidance (y -axis). Note that $-CAPE_{TW} = CAPE_A$. A smooth line is fitted through percentile values.

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 decreases that of waste avoidance between 0.03 and 0.4 percent points.

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 (distributions are plotted in Figure 13 in Appendix D.3).

Table 5: Summary statistics for predicted Social Welfare Effects (SWE) at municipal level. Positive SWE indicate welfare benefits from PAYT adoption estimated as reductions of private (management), external (environmental), and social costs per capita (p.c.) by policy year.

Year	SWE € p.c.	Mean	Sd	SWE>0
1	Private	1.45	10.90	50%
	External	7.05	4.64	all
	Social	8.50	10.51	90%
2	Private	3.95	9.33	73%
	External	6.90	4.90	all
	Social	10.86	10.54	98%
3	Private	23.85	18.33	99.7%
	External	5.44	4.20	all
	Social	29.29	21.97	99.9%

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.

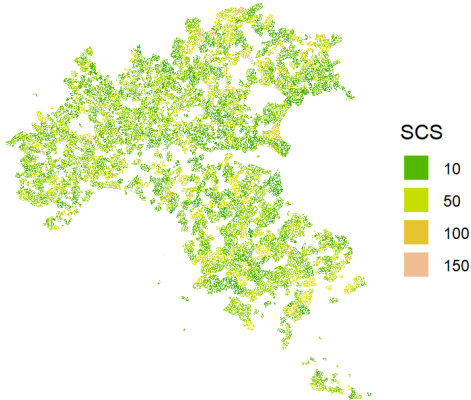


Figure 9: Distribution of predicted welfare benefits in the third policy year in € per capita.

3.6 Robustness and sensitivity

3.6.1 Possible unaccounted-for heterogeneity and spillover effects

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 geocoded neighbors as never-treated units sharing a border with treated units. The remaining untreated units represent the control group. In order to estimate causal effects of PAYT on waste amounts of surrounding municipalities without PAYT, I use the RF running the R-learner described in Section 2.2. 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.

System effects.—Unaccounted-for heterogeneity may threaten identification. Adoption of different PAYT systems (e.g. volume versus weight) may have heterogeneous effects on waste generation.⁴⁸ 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. Sec-

⁴⁸I cannot include system dummies in RF because, as for any policy indicator, I would force treated and untreated units into different neighborhoods.

ond, 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 (out of sample) the adoption of volume systems correctly for most municipalities. 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.⁴⁹

3.6.2 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 two-way (event-study-like) fixed effects regression, 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 12 in Appendix D.4.3). Why does this happen? The forest kernel assigns similarity weights to units based on a residual-on-residual regression 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.

Two-way fixed effects regression.—Results from a two-way (event-study-like) fixed effects regression show that APE on UW are underestimated by 29% on average (see first panel of Figure 14 in Appendix D.4.3). Standard event study pretrend tests (Autor, 2003) present evidence of non-parallel trends, thus, a bias in the estimation of average price elasticities.⁵⁰ There are at least two possible sources of bias. First, unaccounted-for time-varying effects on the outcomes (Bueno & Valente, 2019). Second, heterogeneous treatment effects (de Chaisemartin & D’Haultfoeuille, 2020). Due to staggered policy adoption, APE represent weighted averages of effects computed using both treated and untreated outcomes,

⁴⁹Results are reported in Table 11 in Appendix D.4.2.

⁵⁰Including time trends in the regression does not induce parallel trends either.

which may not allow for a comparison with my estimates. Re-weighting the estimator according to de Chaisemartin & D’Haultfoeuille (2020) gives, however, similar results and the bias persists. Therefore, bias may be due to effect heterogeneity across (covariate-specific) groups which are ex-ante unknown. RF methods allow for a data-driven way to estimate these groups and detect ex-post the relevant heterogeneities.

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 6 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 fixed effects (FE) 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 6: 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 FE reg	-7.40	0.30	2.70	0.70	-4.70	0.50
dynamic FE reg (re-weighted)	-9.06	0.98	3.96	0.76	-5.10	0.40
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 make waste demands similarly elastic, and 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 significantly explain price effect heterogeneity at high prices. I find that lower-income municipalities are more price elastic. Despite large effect heterogeneity, the policy reduces social costs in all 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 significant 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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Online Appendix

A Theoretical predictions

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 recycling versus waste avoidance, as well as behaviors' substitutability or complementarity. For the sake of simplicity, the theoretical model falsely assumes linear demands. In the empirical part, however, I will be able to detect possible nonlinearities by allowing price elasticities to differ across units.

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:

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

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 $\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R} < 0$ or $\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R} > 0$, 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.⁵¹

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

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

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

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

where $\text{sgn}(\cdot)$ 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.

Behavioral explanations.—What makes recycling and waste avoidance more or less costly for

⁵¹For 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.

households? The behavioral economic literature offers at least three (non mutually-exclusive) theories. Cecere *et al.* (2014) provides a first 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.

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 $\left\{ \frac{\partial w_A}{\partial t}, \frac{\partial w_R}{\partial t} \right\}$, 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:

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

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

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

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

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

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

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

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

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

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

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 Descriptive statistics

Table 7: 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

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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
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 8: 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 km ²)
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

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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)
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 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.

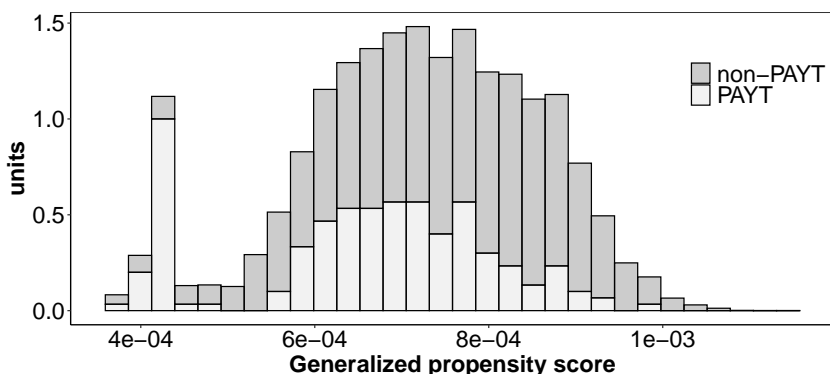


Figure 10: Common support condition for treated (PAYT) and never-treated (non-PAYT).

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 Additional results

D.1 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 subgroup using the median CAPE as a threshold. Next, I derive an estimate of the APE for each subgroup by residual-on-residual regression. Table 9 shows statistically significant differences in APE between subgroups for all outcomes and policy years.

Table 9: Absolute difference in APE ($|dUW|$, $|dRW|$, $|dTW|$ in kg) between high and low APE subgroup 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

D.2 CAPE heterogeneity across price levels

Figure 11 reports effect heterogeneity across price levels, ceteris paribus. 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.

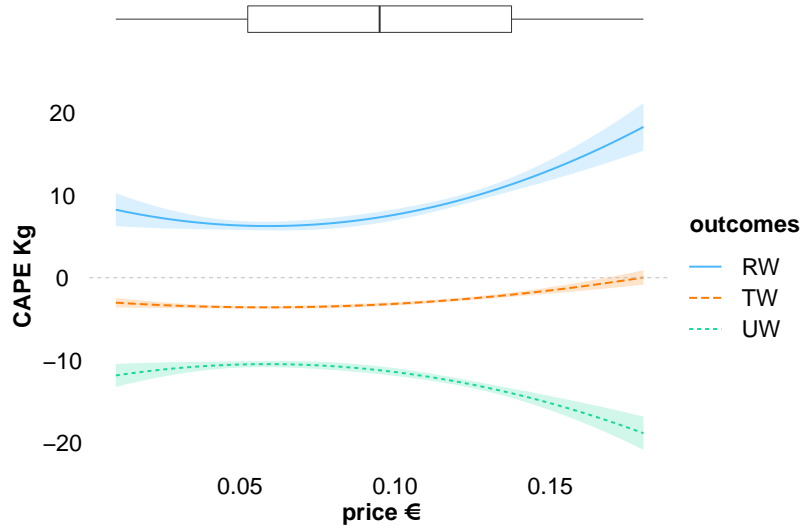


Figure 11: Fitted price effects on waste demands (CAPE Kg) as quantity changes for a one euro cent price increase.

Figure 12 reports effect heterogeneity across prices by pre-policy RW levels, ceteris paribus. 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.

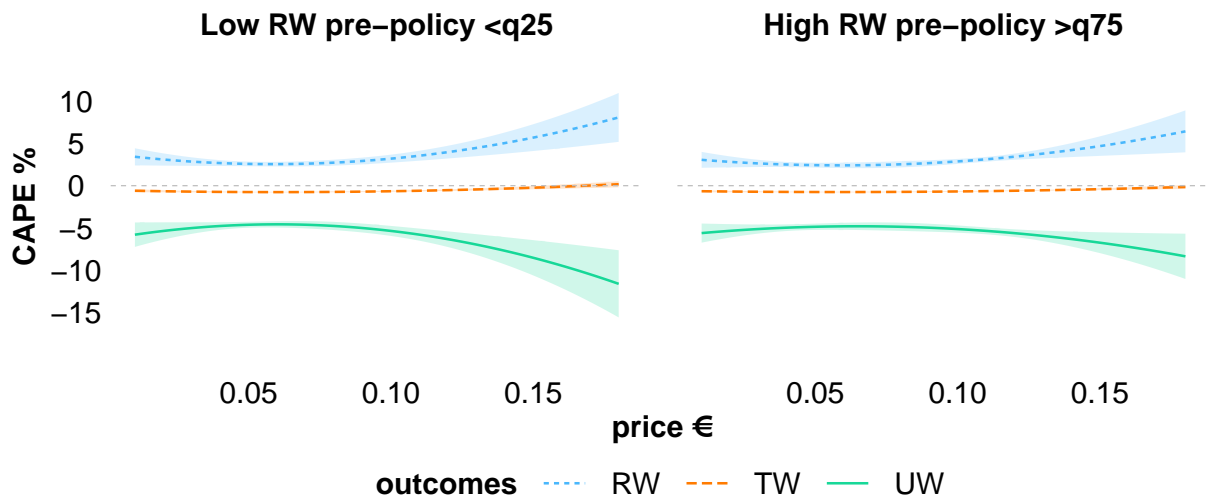


Figure 12: Fitted price semi-elasticities (CAPE) of waste demands by pre-policy recycling levels. Thresholds q25 and q75 indicate first (248 kg) and third (344 kg) quartiles of annual recycling per capita.

D.3 Distribution of social welfare effects

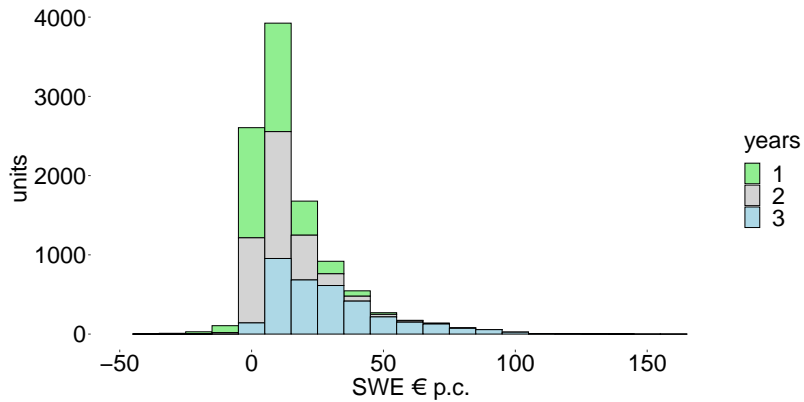


Figure 13: Distribution of Social Welfare Effects (SWE) at municipal level after one, two, and three years of policy adoption. The y-axis refers to the number of treated and yet-to-be-treated municipalities.

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).⁵² Third, geography: physical characteristics of the territory such as uphill 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 8 in 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 10 shows the results including all treated observations in the first policy year. The dummy for weight systems is the reference choice.

⁵²Implementing 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.

Table 10: 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.

	<i>Dependent variable:</i>		
	emptying (1)	bag (2)	mixed (3)
pop	-0.085*** (0.032)	-0.076** (0.036)	0.032 (0.022)
popDens	0.009* (0.005)	0.011** (0.005)	0.011** (0.005)
distPayt	-0.006 (0.008)	-0.017* (0.010)	-0.073 (0.046)
peripheral($40 < t < 75'$)	1.189* (0.663)	1.655*** (0.000)	28.377*** (0.663)
semi-peripheral($20 < t < 40'$)	-21.713*** (0.924)	5.599*** (0.952)	4.797*** (0.962)
semi-urban($t < 20'$)	-24.078*** (0.699)	2.523*** (0.718)	2.132*** (0.781)
peri-urban	-24.819*** (1.618)	1.111** (0.525)	-36.060*** (0.000)
urban	-20.275*** (1.097)	4.625*** (0.912)	-5.571*** (0.223)
Constant	36.339*** (1.145)	13.313*** (0.980)	0.799 (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 11: 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 12 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 12: 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.

Year	<i>Continuous</i>		<i>vs.</i>	<i>Binary</i>	
	UW kg	s.e.		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

Two-way fixed effects regression.—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 14 presents the results from a dynamic (event-study-like) DiD regression estimated using a standard two-way (unit and time) fixed effects model.⁵³ The regression includes three leads

⁵³For the analysis, I use the software R, and, particularly, the package plm (Croissant & Millo, 2008).

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 14 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).

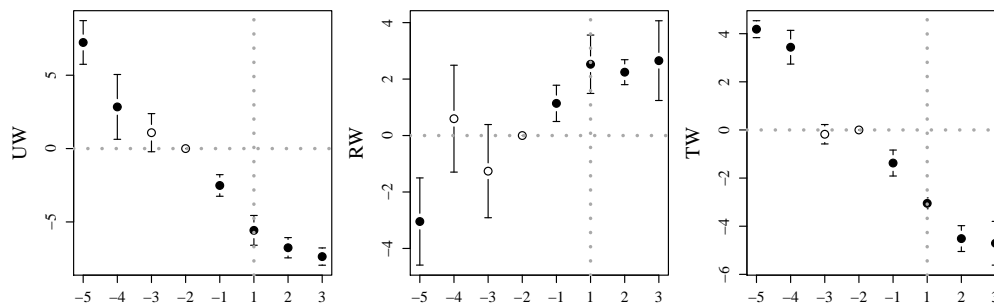


Figure 14: Dynamic DiD estimates (2010-2015, $n = 19,982$) of average price effects (APE kg) on UW, RW, TW with year and unit fixed effects. Clustered standard errors (Driscoll & Kraay, 1994). Statistical significance (5%) indicated by black dots.

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.