Changing the incentive to pollute: Heterogeneous effects of waste pricing policies

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Abstract The externalities of household waste generation and treatment can be internalized with policies pricing unsorted waste disposal known as Pay-As-You-Throw (PAYT). This paper estimates PAYT heterogeneous causal effects on municipal wastes and analyzes the determinants of policy adoption and compliance. Empirically, I employ an R-learning random forest estimator on a unique high-dimensional data panel of Italian municipalities. Controlling for possibly endogenous adoption with a large set of variables, results show that PAYT is on average effective, decreasing per capita unsorted waste up to −60%, an effect driven by households preferring recycling (+41%) over total waste reduction. Both adoption and compliance are heterogeneous in initial waste levels, and explained by complex interactions of covariates. Targeting municipalities with low opportunity costs of recycling through, e.g., awareness-raising campaigns seems recommendable to achieve total waste reductions. Continuous treatment analyses reveal a modest heterogeneity arising from different price levels. Social costs savings are positive, and predicted to increase if all municipalities were to introduce the policy.

Keywords: Municipal solid waste, Pay-As-You-Throw, Heterogeneous policy effects, Machine learning, Random forests
JEL Codes: C14, C21, C52, Q53

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1 Introduction

In the presence of pollution externalities, environmental policy aims to internalize the difference between social and private costs into individuals' polluting behavior. Municipal Solid Waste (MSW) generation characterizes a classic example of negative externality when households are not required to internalize the full environmental and treatment costs of waste generation, collection, and disposal. Under standard flat fees on MSW collection, households face a zero marginal cost of waste disposal, leading to greater-than-optimal waste quantities (Jenkins, 1993). Therefore, charging a positive price increment for using more collection service is a growingly adopted quantity-based policy instrument, a practical example of a Pigouvian fee (Pigou, 1932) oft-called unit pricing or Pay-As-You-Throw (PAYT) program. The common PAYT approach consists of pricing unsorted waste generation leaving recycling unpriced so as to promote the European Union's (EU) waste management hierarchy (European Union, 2018). In fact, by changing the relative price of waste disposal options, PAYT provides (indirect) incentives for both waste avoidance[1] and recycling which are, respectively, the first and second most important goal of EU's waste management. Because PAYT may reward these two behaviors, PAYT has become an attractive policy for communities working to implement circular economy strategies for climate change mitigation.[2] In this respect, this paper analyzes PAYT effectiveness in increasing recycling and reducing total waste, the determinants of its effectiveness, and social cost savings from its implementation.

Using a unique and high-dimensional dataset spanning from 2010 to 2015 for about 3,600 Italian municipalities, this paper looks at the way households change waste gen-

[1]Waste avoidance (or prevention/minimization) is defined as using household resources (time, knowledge, purchased goods) to limit total waste generation. For this, households can purchase durable goods with, e.g., small amounts of packaging and other excess material, and by using waste-avoidance techniques such as reuse and refill (Ebreo *et al.* , 1999).

[2]By encouraging increased recycling and waste avoidance, PAYT programs contribute to decrease emissions from landfilling, and reduce the use of virgin materials and the environmental externalities associated with their extraction (Skumatz, 2008).

eration behavior in response to price incentives, both theoretically and empirically. For this, I first build a theoretical model of household utility maximization that accommodates waste avoidance and recycling behaviors. Assuming that households get disutility from the time/effort spent in waste generation and allowing for complementarities/substitutabilities between the two waste reduction behaviors within a standard microeconomic cost function, the model predicts that households' responses to PAYT depend on their opportunity costs. Such heterogeneity can be explained by differences in socio-economic factors as well as initial (lagged) waste amounts accounting for experience (knowledge capital) in waste reduction activities.

Next, I estimate individual (municipal-level) causal effects of PAYT policies on per capita unsorted, recycling, and total waste. These effects are estimated as heterogeneous in municipal characteristics and dynamic, i.e., short- versus long-run. To the best of my knowledge, this is the first paper that analyzes policy adoption and effectiveness including a large set municipal attributes, cross-sections and time periods, and estimates PAYT effects heterogeneously using nonparametric machine learning methods. Thereby, my empirical strategy allows to estimate social cost savings from the policy for treated (with PAYT) as well as untreated (non-PAYT) municipalities, and predict policy compliers, i.e., those non-PAYT municipalities that, based on their characteristics, are most likely to reduce waste and increase recycling if they were to introduce PAYT.

Although the literature generally estimates negative average effects on unsorted waste, there is no consensual evidence on the behavioral mechanisms behind such reduction, i.e., on whether households adjust their behavior by increasing recycling and decreasing total waste. Existing results vary depending on the method, data and municipalities under study.[3] This may be due to several reasons. First, available data is often small

[3]Some studies estimate waste reductions but no statistically significant effects on recycling, e.g., Jenkins *et al.* (2003) and Fullerton & Kinnaman (2000, 1996) for the U.S., Allers & Hoeben (2010) for the Netherlands, Dahlèn & Lagerkvist (2010) for Sweden, and Valente & Bueno (2019) for Italy. Differently, Carattini *et al.* (2018) and Bucciol *et al.* (2015) find a positive effect on the frequency of

in either time or cross-sectional dimension or both, which questions its representativeness and results' external validity (Allers & Hoeben, 2010). Second, the set of control variables is usually limited, not allowing to include all relevant municipal characteristics explaining waste generation and, if non-random, policy adoption. As a result, omitted variable bias and policy endogeneity threaten the internal validity of existing studies. Third and last, parametric model assumptions limit model flexibility and may lead to misspecification. In light of these limitations, the contribution of this article is to extend previous analyses by fitting heterogeneity of policy adoption, waste generation, and policy effects in several variables accounting for the high-dimensionality of the data in a flexible nonparametric framework.

My empirical strategy uses the potential outcome approach (Rubin, 1974) to identify individual heterogeneous policy (treatment) effects under unconfoundedness, aka selection on observables, and no spillover (SUTVA) assumptions[4]. Estimation proceeds via residualization to account for non-random policy adoption, and the potential impact of covariates on both waste generation and adoption (Neyman, 1979; Robinson, 1988). Residualization, known as R-learning if performed in high-dimensional settings, amounts to, first, partial out the effect of the covariates on outcomes and treatment propensities and, second, predict the residualized outcomes with the residualized propensity scores. This two-step procedure makes the treatment effect estimator insensitive to small errors in the nuisance components (Chernozhukov *et al.* , 2017; Nie & Wager, 2019). Conditional means and treatment effects are heterogeneously estimated nonparametrically by random forests (RFs) (Athey *et al.* , 2019)[5]. RFs are conditional mean estimators employed in this

recycling for Switzerland, and on recycling rates for Italy, respectively. For Korea, Hong & Adams (1999) estimate positive effects recycling, but no source-reductions. For Japan, Usui & Takeuchi (2014) find a sustained increase in recycling, and a short-run (long-run) total waste reduction (increase).

[4]These assumptions mean that, respectively, policy adoption is as good as random once controlled for the covariates, and potential outcomes of one unit are unaffected by policy adoption by other units, respectively.

[5]This method builds upon, e.g., Athey & Wager (2018); Athey & Imbens (2016); Breiman (2001).

paper to handle a high-dimensional set of (often correlated) covariates, capture possibly complex interaction using a data-driven model specification, and consistently estimate the full conditional treatment effect distribution for policy targeting and to predict effects for non-PAYT municipalities based on their characteristics.

Forest-based estimates show that PAYT adoption probabilities (propensity scores, PS) are especially heterogeneous in recycling rates, as well as socio-economic and political variables, and complex interactions thereof. This points towards the importance to include a high-dimensional set of covariates to account for endogenous adoption. In particular, a one percent point increase in recycling rates between the first and second quartile (34-53%) leads to about 20% higher PS, an effect decreasing over higher quartiles. This suggests that PAYT was possibly adopted to further improve recycling rates towards EU's goals, namely, 50% within 2020 (European Union, 2008), and it was thereby less likely for municipalities which already meet these targets.

Estimates of average policy effects accounting for endogenous adoption show that PAYT is effective, decreasing per capita unsorted waste up to -60%. These effects are mostly driven by an increase in recycling (+41%), and only to a lesser extent by total waste reductions (-15%). This indicates that households prefer recycling over waste avoidance on average, and the two behaviors are not complementary. Highest compliance occurs in the first policy year after which households continue substituting unsorted waste disposal with more recycling in lower quantities. Waste avoidance occurs only in the longer-run and for those municipalities which further incentivize this behavior as a cost-saving option in alternative for recycling.

Moreover, this analysis reveals considerable effect heterogeneity, namely, the null hypothesis of no treatment heterogeneity is rejected for all outcomes. This can be explained by heterogeneous opportunity costs of waste recycling and avoidance, and complex interactions of covariates. Concerning the first, there is a significant difference in policy effects for different initial waste levels (used to proxy opportunity costs), other things equal. In particular, municipalities with high recycling levels pre-policy (low opportunity costs of sorting) increase recycling more than municipalities with low levels in response to PAYT. Similarly, municipalities with low total waste pre-policy (low opportunity costs of avoidance) react to PAYT by reducing total waste more than municipalities with high levels. This suggests that targeting municipalities with low opportunity costs of recycling through, e.g., awareness-raising campaigns could increase PAYT effectiveness on total waste reductions.

Focusing on socio-economic heterogeneity, variables such as education and income are found to interact in complex ways. These are two variables typically considered by the literature as important determinants of policy effectiveness, although their effects are rarely assessed due to data limitations (Usui *et al.* , 2017), and oft ambiguous (for reviews see, e.g., Fiorillo, 2013; Schultz *et al.* , 1995). Estimating ceteris paribus effects of this interaction shows that, short-term, relatively low-educated municipalities comply with the policy the least. Yet, if high-income, these municipalities prefer recycling over avoidance by increasing the former and reducing the latter.

I also find that short- and long- term heterogeneity differ showing that recycling and avoidance behaviors are substitutes over time. This means that, e.g., while low-educated municipalities avoided waste the least in the short-term, they avoid waste the most in the long-term.

To contribute to the literature that investigates price elasticities of household demand for MSW, I also analyze policy heterogeneity that may arise from variation in price levels. Forest-based estimates for continuous treatments can be obtained under similar assumptions to the binary treatment framework thanks to the generalization of propensity score methods developed in Hirano and Imbens (2004) and Imai and van Dyk (2004). Estimates reveal that [...]

Additionally, I show that the use of a forest-based rather than a difference in differences approach is justified in this application because the parallel trend assumption needed for the validity of the latter is violated. Assuming selection based on time-constant variables is the likely source of this bias, pointing towards the need to control for time-varying characteristics and, possibly, allow variables to have time-varying effects on policy adoption and waste generation. The successful estimation by random forests including a high-dimensional set of variables in both outcome and selection equations, on the other hand, allows to control for the remaining unobserved heterogeneity.

[Lastly, from a social cost view point...]

The remainder of the paper is organized as follows: Section 2 describes the household waste generation problem and derives theoretical predictions of PAYT heterogeneous effects. Section 3 outlines the random forest estimation method. Background and data are described in Section 4. Section 5 shows the results, and Section 6 concludes and provides policy implications of this study.

2 Theoretical Predictions

By pricing conventional (unsorted) waste generation, PAYT policies provide monetary incentives that decrease the relative price (opportunity cost) of other waste disposal options, in particular, waste avoidance and recycling.[6] In this way, PAYT alters households' optimization problem regarding waste generation.

Consider the household problem to maximize utility from consumption net of the disu-

[6]The terms waste "generation" and "disposal" are used as synonyms throughout the text. Disposal may differ from generation if, e.g., unlawful options are available such as littering and illegal dumping. Yet, including illegal disposal as a further choice alters the strength but not the sign of the predicted effects.

tility of avoidance and recycling efforts. The corresponding expressions is

$$max_{w_A,w_R} : V(\cdot) = U(y - t(W - (w_A + w_R))) - C(w_A, w_R, W_0, W_R(W_0), X, Z) \qquad (1)$$

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 takes as exogenous in the following derivations. Total waste is defined by the sum of unsorted and recycling waste, and equals $W - w_A$. 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. 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 the time/effort spent in generating w_i. $C(\cdot)$ is a standard microeconomic cost function for which the following conditions hold:

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

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 condition, importantly, points to either complementarities or substitutabilities between the two waste reduction behaviors with $\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_C} < 0$ or $\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_C} > 0$, respectively. Empirically, complementarities after PAYT introduction mean that both avoidance and recycling occur, namely, causal effects on recycling are positive and on total waste (avoidance) are negative (positive). This translates into increases in both $\{w_A, w_R\}$ after PAYT. Substitutabilities occur instead when households prefer one behavior over the other, namely, causal effects on recycling and total waste are positive, or vice versa. This translates into increases in either w_R or w_A after PAYT.

The arguments of the cost function, $C(\cdot)$ are current (w_i) and initial (lagged) waste amounts (W_0, $W_R(W_0)$) with the latter accounting for experience (knowledge capital) in waste reduction activities. Additionally, $C(\cdot)$ depends on waste-reduction technology shifters (X) which are exogenous socio-economic factors such as household size and age, 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.[7]

Solving problem (1) for w_i ($i = A, R$) leads to the following first-order conditions:

$$\frac{\partial V(\cdot)}{\partial w_i} = U'(\cdot)t - \frac{\partial C(\cdot)}{\partial w_i}, \tag{3}$$

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 (3) 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:[8]

$$sgn\left(\frac{\partial w_A}{\partial t}\right) = sgn\left[\frac{\partial^2 C(\cdot)}{\partial w_R^2} - \frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R}\right]; sgn\left(\frac{\partial w_R}{\partial t}\right) = sgn\left[\frac{\partial^2 C(\cdot)}{\partial w_A^2} - \frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R}\right] \tag{4}$$

where $sgn(\cdot)$ is a function that extracts the sign of PAYT causal effects. Specifically, equation 4 means that the sign of PAYT causal effects depends on complementarities/substitutabilities between waste reduction behaviors. As a result, theoretical predictions of PAYT causal

[7] For a discussion see, e.g., Abbott *et al.* (2013); Bénabou & Tirole (2003); Brekke *et al.* (2010); D'Amato *et al.* (2014); Gilli *et al.* (2018); 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.

[8] Complete derivations are reported in the Appendix.

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 $|\frac{\partial^2 C(\cdot)}{\partial w_R^2}| > |\frac{\partial^2 C(\cdot)}{\partial w_A \partial w_R}| > |\frac{\partial^2 C(\cdot)}{\partial w_A^2}|$.

Complementarities (case i) occur if, for example, households change their purchasing behavior towards both more durable and recyclable items. In this case, PAYT causes more recycling and less waste overall. Also, complementarities take place if PAYT - which is usually coupled with government campaigns to inform and raise awareness about the waste problem – stimulates pro-environmental lifestyles, and affects people's cultural learning of new preferences by crowding in social preferences of intrinsically and extrinsically motivated individuals (Bowles & Polania-Reyes, 2012).

Instead, substitutabilities (case ii or iii) occur if households find one of the two waste reduction behaviors more convenient (lower opportunity costs). This can be due to easier recycling/avoidance opportunities and higher experience (knowledge capital) in recycling/avoiding (Morris & Holthausen Jr., 1994). In case (ii), PAYT causes a rebound effect on total waste, namely, has the unintended adverse effect of increasing total waste. This can occur if recycling is very convenient, thus its opportunity costs are low due to, for instance, households' socio-economic characteristics (e.g., higher education level and lower housing density) and a higher recycling experience acquired from, e.g., information campaigns and efficient recycling collection systems like curbside recycling.

Households may prefer recycling over avoidance also due to "licensing" and "multi-tasking" effects. The first refers to the fact that recycling allows households to abstain from other environmentally responsible actions (see, e.g, Catlin & Wang, 2013, for a literature review). The second refers to households being induced to focus on one behavior and reallocate some attention away from other tasks (Holmstrom & Milgrom, 1991). Since PAYT does not directly target avoidance and governments often put excessive emphasis on recycling, a trade-off between the two pro-environmental behaviors may arise, namely, opportunity costs of recycling decrease because households feel a stronger obligation to pursue this behavior (Cecere *et al.* , 2014).

Empirically, accounting for initial (pre-policy) waste quantities as well as for other socio-economic characteristics (X) allows to capture some heterogeneity in opportunity costs, and explain the magnitude of PAYT causal effects. Heterogeneity in PAYT responses are determined by the following relations:

$$\frac{\partial^3 C(\cdot)}{\partial w_i^2 \partial W_0} \gtreqless 0; \ \frac{\partial^3 C(\cdot)}{\partial w_i^2 \partial W_R(W_0)} \gtreqless 0; \ \frac{\partial^3 C(\cdot)}{\partial w_i^2 \partial X} \gtreqless 0, \tag{5}$$

which measure slope changes of waste marginal cost curves at different levels of initial total waste (W_0), initial recycling ($W_R(W_0)$), and socio-economic status (X), respectively. For example, I expect that higher recycling experience decreases marginal costs of recycling, and lower initial total waste is associated to higher avoidance experience and, thereby, to decreasing marginal costs of avoidance. How this impacts policy reactions is then determined by complementarities/substitutabilities between the two behaviors. This paper analyzes these behavioral responses empirically by estimating PAYT heterogeneous causal effects on all waste streams: unsorted, recycling, and total.

3 Method

To empirically learn not only the magnitude of policy effects but also the determinants of policy adoption and effectiveness, I estimate policy heterogeneous effects as a function of a large set of municipal characteristics that may influence policy adoption and responsiveness. Following the potential outcome approach (Rubin, 1974), let (X_i, Y_i, D_i) be the available data for municipality $i = 1, ..., n$, where $X_i = x \in \mathbb{R}^p$ is a vector of p covariates, Y_i is the waste outcome, and $D_i \in \{1, 0\}$ is the policy (treatment) dummy under {PAYT, non-PAYT}. Additionally, let $\{Y_i(1), Y_i(0)\}$ be the waste outcomes that we would observe under $D_i = 1$ or 0 respectively, such that $Y_i = Y_i(D_i)$. For each municipality i defined by its vector of characteristics x, the goal is to estimate the conditional Treatment Effect (TE) $\delta(x) = \mathbb{E}\left[Y_i(1) - Y_i(0)|X_i = x\right]$ under unconfoundedness,[9], and the no spillover assumption, aka SUTVA (Rubin, 1974). To fulfill unconfoundedness in the likely case of non-random PAYT assignment, it is necessary to control for the potential sources of self-selection into policy, i.e., capture the effect of X_i on D_i. Consider the waste outcome model:

$$Y_i = \mathbb{E}\left[Y_i(0)|X_i\right] + D_i\delta(X_i) + \epsilon_i, \tag{6}$$

where $\mathbb{E}\left[\epsilon_i|X_i, D_i\right] = 0$ holds under unconfoudedness. Let $\mathbb{E}\left[Y_i|X_i\right]$ and $\mathbb{E}\left[D_i|X_i\right]$ be the conditional means of the waste outcome Y_i and policy adoption D_i respectively, with $\mathbb{E}\left[D_i|X_i\right]$ representing policy adoption probabilities aka propensity scores. After few algebraic transformations, model (6) can be rewritten as:

$$Y_i - \mathbb{E}\left[Y_i|X_i\right] = (D_i - \mathbb{E}\left[D_i|X_i\right])\delta(X_i) + \epsilon_i. \tag{7}$$

[9]Namely, $\{Y_i(0), Y_i(1)\} \perp\!\!\!\perp D_i|X_i$. This assumption is plausible as we include a large set of covariates with a potential impact on outcome levels, policy adoption, and policy effectiveness.

Thereby, estimation of the treatment effect $\delta(X_i)$ in (7) proceeds via residualization of the conditional means $\mathbb{E}[Y_i|X_i]$ and $\mathbb{E}[D_i|X_i]$ which are separately estimated in a previous step (Neyman, 1979; Robinson, 1988). In particular, estimating $\mathbb{E}[D_i|X_i]$, namely, the selection equation, and $\delta(X_i)$, namely, heterogeneous policy effects by observed covariates, allows to analyze the determinants of policy adoption (self-selection) and effectiveness, respectively. Since there is no prior information on the functional form of these possibly high-dimensional relations, employing a parsimonious parametric model could not possibly capture complex interactions or nonlinear terms. Also, including a large set of potential predictors in such a model may lead to, e.g., variance inflation and incorrect signs. Thus, to reduce the risk of model misspecification and ad-hoc model selection, these functions are determined nonparametrically using RFs (Athey & Wager, 2018; Athey & Imbens, 2016; Breiman, 2001) and, specifically, the generalized version of this estimator developed in Athey *et al.* (2019).

RFs are ensamble of trees, and each tree is a nonparametric conditional mean estimator used to estimate the quantities of interest defined as $y(x) = \mathbb{E}[Y_i|X_i = x]$, $d(x) = \mathbb{E}[D_i|X_i = x]$ and $\delta(x)$. Such estimator works according to the principle "divide and rule": Every tree recursively partitions the covariate space into binary regions according to an optimal criterion. In this case, such criterion is chosen to maximize outcome heterogeneity between regions. Thereby, estimation of a tree, namely, a sequence of binary regions, proceeds greedily, i.e., partitions that reduce outcome heterogeneity the most are performed first. As a result, final regions aka leaves contain a number of observations, set to be greater than a given minimum,[10] that are as homogeneous as possible in terms of those covariates able to explain most of the variation in the quantity of interest. As a result, the estimated conditional expectation function is a nearest-neighbor function that matches units with similar characteristics by partitioning them into optimal regions, and

[10]Note that minimum leaf-size and other parameters such as a penalty for imbalanced partitions are chosen via cross-validation according to Tibshirani *et al.* (2018).

estimates the conditional mean of interest within each final region. In addition, each tree in a RF is built using a random subsample of data and covariates, aka training sample. The estimated variance from one single tree is usually large because each tree is built to minimize bias. In this respect, RFs avoid overfitting, i.e., reduce such variance, by averaging estimates over trees. Once the RF is estimated (trained), unbiased conditional mean estimates for each vector $X_i = x$ are obtained via out-of-bag prediction. This amounts to obtain (predict) the quantity of interest for $X_i = x$ from trees (bags) estimated without it.[11]

Formally, for each i in a random subsample of training data s^{tr}, define $\theta(x)$ the quantity to estimate, namely, $y(x), d(x)$, and $\delta(x)$. As defined above, these quantities are conditional expectations using outcome(s) $Q_i = \{Y_i, D_i\}$. A tree recursively partitions the covariate space into binary regions such as to maximize $\hat{\theta}$-heterogeneity between regions (Athey *et al.* , 2019):

$$\frac{N_{R_1} N_{R_2}}{N_{R_0}^2} (\hat{\theta}_{R_1}(s^{tr}) - \hat{\theta}_{R_2}(s^{tr}))^2, \tag{8}$$

with $\frac{N_{R_{1,2}}}{N_{R_0}}$ being the fraction of training examples $i : X_i \in s^{tr}$ assigned to two new regions $R_{1,2}$ from the original region R_0. Such new regions are defined by optimal covariate cutoffs that maximize (8) greedily. As in Athey *et al.* (2019), $\hat{\theta}_{R_j}(s^{tr})$ is identified by the local estimating equation $\mathbb{E}\left[\rho_\theta(Q_i)|X_i = x\right] = 0$ where ρ_θ is the moment function. The solution to this estimating equation is defined as $\hat{\theta}_{R_j}(s^{tr})$, and it is estimated as follows:[12]

$$\hat{\theta}_{Rj}(s^{tr}) \in \underset{\theta}{\operatorname{argmin}} \| \textstyle\sum_{i:X_i \in R_j} \rho_\theta(Q_i) \|_2. \tag{9}$$

[11]Without out-of-bag 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).

[12]Due to computational inefficiency, Athey *et al.* (2019) optimize a gradient-based approximation to (8) and (9) which requires to evaluate $\hat{\theta}_{R_0}$ and the moment function only in R_0, and not in each new region $R_{1,2}$ (see Athey *et al.* , 2019, for more details).

The moment function for the conditional mean estimation of $y(x)$ and $d(x)$ writes $\rho_\theta(Q_i) = Q_i - \theta(x)$, with $Q_i = \{Y_i, D_i\}$ respectively. In this case, equation (9) gives the standard random forest's solution $\hat{\theta}(x) = \bar{Q}$ from Breiman (2001), which is the mean outcome computed over $\forall i : X_i \in R_j$ of the b-th tree and then averaged over all B trees ($b = 1, ..., B$). Predictions for each $X_i = x$ are obtained out-of-bag, and $\{\hat{y}(x), \hat{d}(x)\}$ are residualized as $\{\tilde{Y}_i = Y_i - \hat{y}(x), \tilde{D}_i = D_i - \hat{d}(x)\}$. Similarly, the moment function that estimates $\delta(x)$ uses these residualized outcomes, and writes $\rho_{\theta,\alpha}(\tilde{Y}_i, \tilde{D}_i) = (\tilde{Y}_i - \delta(x)\tilde{D}_i - \alpha(x))(1\ \tilde{D}_i)^\top$ where $\alpha(x)$ is an intercept term. Further, the local moment condition $\mathbb{E}\left[\rho_{\theta,\alpha}(\tilde{Y}_i, \tilde{D}_i)|X_i = x\right] = 0$ identifies the TE as in model (7), namely, $\delta(x) = Var[\tilde{D}_i|X_i = x]^{-1}Cov[\tilde{D}_i, \tilde{Y}_i|X_i = x]$. These treatment effects are estimated from (9) and plugged into the criterion (8) to estimate each tree.[12] For out-of-bag prediction, forest-based weights $w_i(x)$ are computed $\forall i : X_i = x$ as the frequency with which the training example $i : X_i \in s^{tr}$ is estimated in the same region as the held-out x. This is meant to correct for randomization bias, e.g., noisy leaves.[13] Therefore, the solution to the (weighted) estimating equation for the TE yields:

$$\hat{\delta}(x) = (\sum_{i=1}^{n} w_i(x)(\tilde{D}_i - \bar{D}_w)^2)^{-1} \sum_{i=1}^{n} w_i(x)(\tilde{D}_i - \bar{D}_w)(\tilde{Y}_i - \bar{Y}_w) \tag{10}$$

where $\bar{D}_w = \sum_{i=1}^{n} w_i(x)\tilde{D}_i$ and $\bar{Y}_w = \sum_{i=1}^{n} w_i(x)\tilde{Y}_i$. Note that training examples i not falling in the same leaf of x do not enter the computation of $\hat{\delta}(x)$ because $w_i(x) = 0$. In words, treatment effects for each x are estimated in locally weighted nearest neighborhoods of x (leaves) produced by different trees.

Finally, the variance of $\hat{\theta}(x)$ is estimated by evaluating the estimator on bootstrapped half-samples of the training data, aka bootstrap of little bags (see, for a discussion, Athey *et al.* 2019, and Sexton & Laake 2009).

[13]See Athey *et al.* (2019) for the formal definition of such forest-based weights.

4 Background and Data

4.1 PAYT policies

PAYT policies in Italy, as in many municipalities worldwide, require households to pay a fix price per unit of unsorted waste according to either its volume, i.e., per bag or bin of waste, or weight, i.e., per kilogram of waste. By linking price to service and pollution levels, PAYT fulfills the equivalence principle for which service consumers pay for its consumption, and the polluter pays principle for which households pay according to their MSW generation. This occurs either ex ante via prepaid bags or ex post via identification (tag on bags, chip on bins, electronic keys). Weight systems are generally less diffused because they require an increase in administrative capacity due to data processing and in technology investments on collection vehicles. Volume systems are preferred because of their simplicity and less expensive technology. However, there is asymmetry in incentives between these systems. For example, on the one side, a per-emptying system encourages households to present full bins, which reduces waste collection frequency, thus, costs. On the other side, volume systems encourage waste compacting and, thereby, may be less effective in reducing waste amounts. In this data, about 7% of PAYT municipalities pay per weight, 16% per bag, 72% per emptying, and 5% have a mixed system - which allows to check for policy heterogeneity in this respect.

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). Municipalities are cost minimizers such that fee revenues finance fixed and variable waste management costs, a principle known as budget balance. PAYT municipalities implement PAYT - the "incentive" fee - on top of the flat fee, and reduce the latter because PAYT covers variable costs of waste collection. The flat part of such binomial fee is usually maintained to safeguard against budget deficits due to the difficulty in anticipating the impact of the incentive fee on households' behavior. To ensure budget balance in each year, local governments adjust both flat and PAYT fees based on expected fee revenues and on waste generation and management costs in the previous year. Enforcement and monitoring by both the waste collection company and the municipal police are in place to prevent policy adverse effects.[14]

At the time of writing, there is no open-source database on PAYT prices for each municipality. This information has been reported upon request. Unit prices vary between 0.01 and 0.18 euros per liter of unsorted waste, which corresponds to an annual cost between 25 and 80 euros per capita.[15] In light of this variability, this paper also estimates whether PAYT causal effects are heterogeneous in price and type of system. In this respect, the literature generally estimates small and non-significant price elasticities and stronger policy effects for weight-based systems (see Bel & Gradus, 2016, for a review).

4.2 Outcomes

I collected data for about all Italian municipalities over the years 2010-2015. Waste data come from the database of the Italian Institute for Environmental Protection and Research (ISPRA) which also provided information about PAYT introduction for some municipalities. Full data on PAYT adoption were collected upon direct request, and provided by waste collection companies and the National Association of Italian Municipalities (ANCI). The outcome of the selection model is a binary variable that takes the value of one if the municipality introduced PAYT and zero otherwise. Outcomes of the treatment ef-

[14]E.g., trash bins are locked, waste collectors do not remove bags or cans if sensed to weigh excessively (under volume-based PAYT), all households in a building and/or single households are fined if unsorted waste is found inside the recycling and in case of illegal dumping.

[15]For the conversion from kg to liter of unsorted waste municipalities use a specific weight (if not reported, the median is used). Also, municipalities charge households a fix cost for the generation of q liters of unsorted waste. Based on municipalities' most reported values, I use $q = \{240, 480\}$ for emptying- and bag-based systems, respectively (see, e.g., Iren & Dolomiti Ambiente, 2019). PAYT costs are thereby computed on the remaining per capita unsorted waste ($> q$ for every municipality).

fect model are kilograms of per capita unsorted (UW), recycling waste (RW), and total (TW) which is the sum of the previous two. I also estimate effects on the recycling rate ($RWrate$) for comparison. Figure 1 provides information of the geographical distribution of per capita waste amounts at municipality level. In general, northern municipalities generate higher amounts of total waste per capita but also higher recycling and recycling rate.

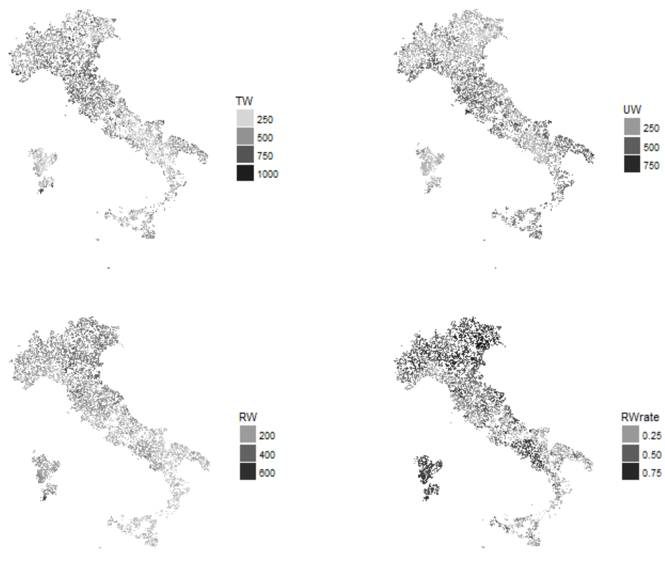

Figure 1: Average per capita waste amounts by municipality over 2010-2015 (white, no data) - unsorted (UW), total (TW), recycling (RW) waste and rate ($RWrate$).

I consider as treated units those municipalities that implemented PAYT from 2012 onwards, and discard PAYT municipalities before 2012. This choice is due to the fact that lagged waste amounts are used as predictors in all equations (selection, waste generation, and treatment effect), and that first-order lags for PAYT municipalities are likely biased predictors. This is due to two reasons. First, as common in PAYT policy settings, households likely anticipate policy in the year before treatment and vary waste generation behaviors. Second, PAYT municipalities usually introduce a training period, a pilot program or a phased-in approach for the new system before its official start in which waste is generated under unit pricing but it is not priced (Skumatz, 2008). Since policy adoption is likely decided in the year prior to the training period, I predict policy adoption with second-order lagged data with respect to the official PAYT implementation, and I estimate treatment effects including second-order lags of waste amounts. Also, I use first-order lags to estimate policy anticipation effects.

The resulting treated municipalities are 194, of which 102 in the North-West, 82 in the North-East, and six in the Center. Additionally, since mismeasurement of waste data for the South is more likely due to illegal waste disposal, I exclude all observations in the South from the data.[16] Further, 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). I analyze dynamic treatment effects by running separate RFs for the first to fourth year from treatment as well as the year before official policy start (anticipation). Figure 2 shows the distribution of PAYT (black) and non-PAYT (grey) municipalities over 2012-2015. Excluded municipalities (white) include the South, those adopting PAYT before 2012, and those with missing values due to, e.g., merging municipal administration and change in denomination.

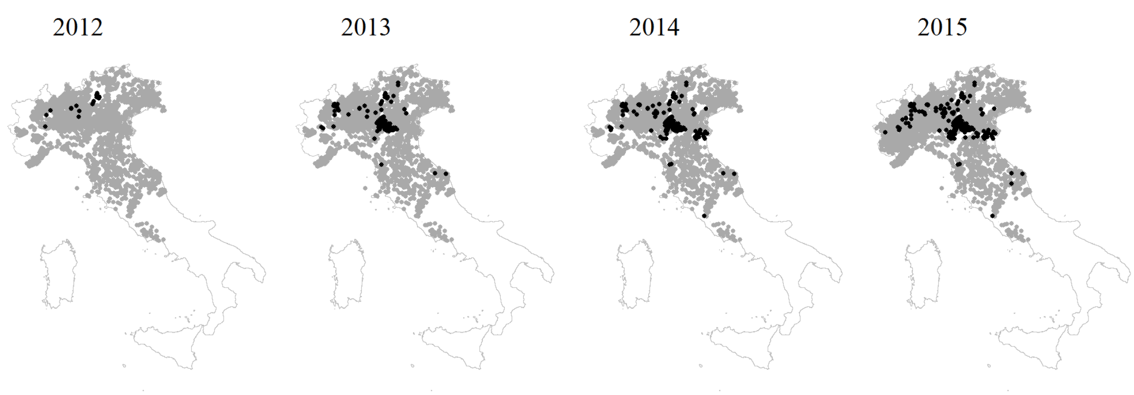

Figure 2: Map of PAYT (black) and non-PAYT (grey) municipalities over 2010-2015 (white: South, PAYT municipalities with missing values and those adopting PAYT before 2012).

[16]Between 2010 and 2015, average crime rates on illegal incineration and traffic of waste and violations of waste management regulations including, e.g., illegal dumping, amount to 29% and 14%, respectively, for the South compared to a national average of 10% for both crimes (ISTAT, 2018).

For a descriptive comparison of waste generation at the beginning (2010) and at the end (2015) of the sample, Figure 3 plots the distribution of UW, TW, and RW for PAYT (grey) and non-PAYT (white) municipalities.

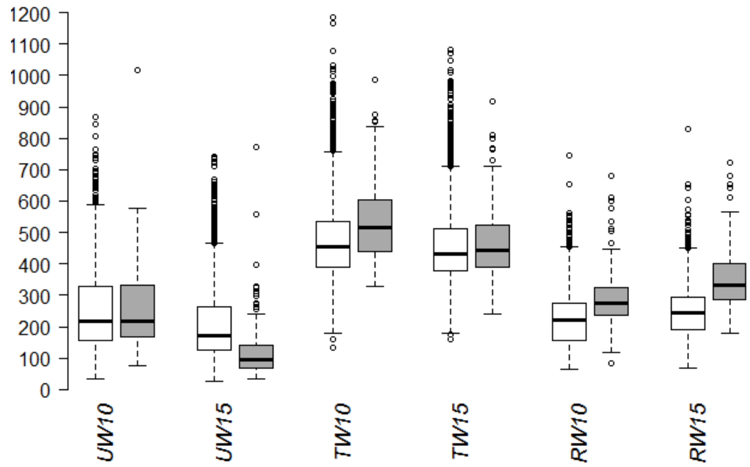

Figure 3: Boxplot of 2010 and 2015 per capita unsorted (*UW10, UW15*), total (*TW10, TW15*), recycling (*RW10, RW15*) waste for PAYT (grey) and non-PAYT (white) municipalities.

In both PAYT and non-PAYT municipalities we observe a decrease in UW and TW, and an increase in RW over time. These changes are especially visible for UW. Further, comparing untreated (2010) waste amounts in PAYT and non-PAYT municipalities reveals possible self-selection into policy. Figure 3 shows that PAYT municipalities present relatively higher TW and RW levels. The first may suggest that municipalities introduced PAYT due to high waste pollution and collection/treatment costs. The second indicates that municipalities introducing PAYT have already developed recycling habits before adoption. In fact, to minimize the risk of adverse effects such as illegal dumping, PAYT is usually implemented only if household opportunity costs of recycling are already low.

4.3 Determinants of PAYT adoption and waste generation

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

Socio-economic characteristics

The covariate set includes socio-economic variables based on the literature[17] such as average household size (*hhSize*); per capita income (*incomePc*); educational attainment as measured by the share of the population with a graduate degree or higher (*collegeDeg*) as well as with an elementary degree or none (*elemDeg*); tourism intensity, measured as touristic capacity per capita (*tourism*); and age structure, namely shares of population aged under 15 (*age14*), over 65 years old (*age65*), and under 5 (*age0*) with the latter accounting for the amount of infant-related waste, e.g., diapers. Among these, the literature especially focuses on income and education levels as the most likely drivers of PAYT adoption and effectiveness, with education estimated to be the most important (Gradus *et al.* , 2019). I also control for labor market characteristics such as the share of unemployed and out-of-the-labor force population (*unempOutLab*), and an index of the labor market activity in terms of commuting intensity (*labMarket*) which may determine the time spent at home and the waste generated.

Geographic characteristics

I control for the distance (in km) of each municipality to waste incinerator (*distInc*), landfill (*distLandf*), and hazardous waste treatment facilities (*distHaz*) because this may impact not only waste generation incentives, but also policy adoption. In fact, adoption may be more likely in a municipality with a saturated landfill and a distant incinerator. However, the opposite may also occur because the vicinity to an incinerator may stimulate

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

households' environmental awareness and cause concerns for environmental and health damages. In addition, municipalities with distant landfills may adopt PAYT to save transportation costs. Further, I account for observables that possibly serve as fixed effects, such as the degree of urbanization (*urban*, *highUrban*), and the type of municipality, i.e., regional and provincial seats (*regSeat*, *provSeat*). Together with population density, these variables may also determine the type of recycling infrastructure and PAYT system, e.g., weight-based systems are rarely implemented in rural, sparsely populated areas because of their limited administrative capacity and resources to finance weight-based technology on a large number of collection vehicles (Dunne *et al.* , 2008).

Neighborhood effects

PAYT adoption could be also explained by neighborhood effects for two reasons. First, information dissemination, namely, municipalities may adopt PAYT to mimic successful neighbors' policies (Allers & Hoeben, 2010). Second, municipalities often decide to group in so-called consortia to save, e.g., waste collection costs. Consortia consists of neighboring municipalities sharing a number of relevant structural characteristics, e.g., urban infrastructure and services such as waste management. Thereby, although every municipality in a consortium has full decision power on waste management matters, PAYT adoption may not only depend on its characteristics but also on proximity. To control for neighborhood effects, I include the distance (in km) to each non-PAYT municipality in a given year to the closest municipality that implemented PAYT in earlier years. As a result, this variable takes similar values for neighboring municipalities that adopt PAYT and belong to the same consortium. Indeed, PAYT municipalities seem clustering in certain areas (see Figure 2). Descriptive statistics (Table 2 in the Appendix) show that municipalities adopting PAYT dist on average 22 km from the nearest municipality that adopted PAYT in previous years, while non-PAYT municipalities are on average farther away (50 km).

Political variables

The last group of covariates includes a proxy for political participation (*polPart*), and political preferences (*votesLeft*, *votesRight*, *votesBigTent*), as well as mayors' characteristics such as age (*mayorAge*), term length (*yearsOffice*), and political party (e.g., *mayorGreen*). These variables may determine, especially, PAYT introduction as a result of ideology and interest group influence (Dijkraaf Gradus 2011, EPA). In particular, political variables reflect the fact that, since waste fees imposed on users are proportional to waste management costs, local governments may introduce PAYT to save money, decrease fees, and increase political consensus. However, economic theory suggests that household price elasticity may be low in the short-run because fewer alternatives to source reduction are available (Usui, 2009). Thereby, PAYT may cause higher costs to households in the short-run, which may be disliked by, e.g., liberal political parties (Gradus *et al.* , 2019). For this reason, mayors may have different incentives to adopt PAYT depending on their party, term length, and age. In particular, I expect new and younger mayors to be more willing to invest in new waste collection technology and environmental quality. Further, by applying the polluter pays principle, PAYT is often adopted to increase fairness (Batllevell & Hanf, 2008). Yet, its adoption and effectiveness likely depend on public acceptance and participation, proxied by the share of voting population and share of votes for each major national party. In this case, long-term mayors with an established public support and trust may be more likely to introduce PAYT.

Pre-policy waste amounts

As mentioned in Section 2, initial waste amounts (W_0, $W_R(W_0)$) are used as predictors for policy adoption and effectiveness, as well as for other unobserved waste generation habits. Among the latter, pre-policy recycling, unsorted and total waste amounts control for the opportunity costs of recycling waste avoidance habits, and for existing differences in the recycling infrastructure, e.g., at the curb, that may favor sorting habits. Since a common reason to adopt PAYT is to increase household recycling, pre-policy recycling rates and levels may be important predictors of PAYT adoption. However, the direction of the correlation is ambiguous. On the one hand, municipalities with low recycling seeking to improve sorting performance may be more likely to adopt PAYT (Kinnaman *et al.* , 2014). On the other hand, low pre-policy recycling levels are likely due to a poor recycling infrastructure and high opportunity costs of sorting. In this setting, PAYT adoption may not decrease the relative price of recycling unless the unit price is sufficiently high, which could result in low public acceptability due to perceived unfairness, and adverse policy effects such as illegal dumping. For this reason, high recycling pre-policy may positively correlate with adoption probabilities. Finally, since municipalities introduce PAYT to internalize waste pollution externalities, high pre-policy total waste levels may be the main driver of policy adoption.

Costs of MSW services

Municipal management, collection, and treatment costs of total, recycling[18] and unsorted waste are included in the analysis, both in per capita ($costTW$, $costRW$, $costTW$) and per kg terms ($avgCostTW$, $avgCostRW$, $avgCostUW$). On the one hand, policymakers may adopt PAYT to lower per capita and average costs (increase efficiency), which may be desirable to, e.g., increase political consensus. On the other hand, waste-aware municipalities with an already efficient waste management may decide to implement PAYT to further decrease environmental and private costs. Lastly, if salient to the household, (lagged) per capita costs may impact household waste generation, thus, not only the selection but also the outcome equation.

Detailed information on variables' denomination, descriptive statistics, and analysis is presented in the Appendix B. Socio-economic, tourism, and geographic data is collected from the Italian National Institute of Statistics (ISTAT) and through webscraping the online database of comuni-italiani.it. Data on municipal political elections and mayors' char-

[18]Recycling costs are net of revenues from selling products and energy derived from the process.

acteristics are obtained from the Ministry of the Interior upon request, while MSW costs from ISPRA. The geographic location of landfills, incinerators and other waste treatment facilities is computed with data of the European Pollution Release and Transfer Register (E-PRTR).

5 Empirical results

5.1 Difference in differences estimation

For a conditional comparison of waste generation between treated and control group that accounts for fixed differences between municipalities, Figure 4 presents the results from an event-study-like Difference in Differences (DiD) estimation.[19] This corresponds to a DiD regression including lead and lagged policy dummies for PAYT municipalities equal to one in each pre- and post- policy year, respectively. As a result, I include four of such leads and lags using the last lead before policy as a baseline dummy. Since the number of potentially relevant (and partly collinear) covariates is large, and there is no a priori guidance on which one to exclude, this DiD only controls for time-invariant waste generation determinants absorbed by municipal fixed effects.

The aim of such DiD regression is twofold. First, DiD gives an indication of the magnitude of average policy effects in the short- and in the long-run. Second, DiD allows to test for statistically significant differences in waste generation trends between PAYT and non-PAYT units pre-policy. In this case, causal effect estimates would be biased due to violation of the DiD identifying assumption of parallel trends.

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

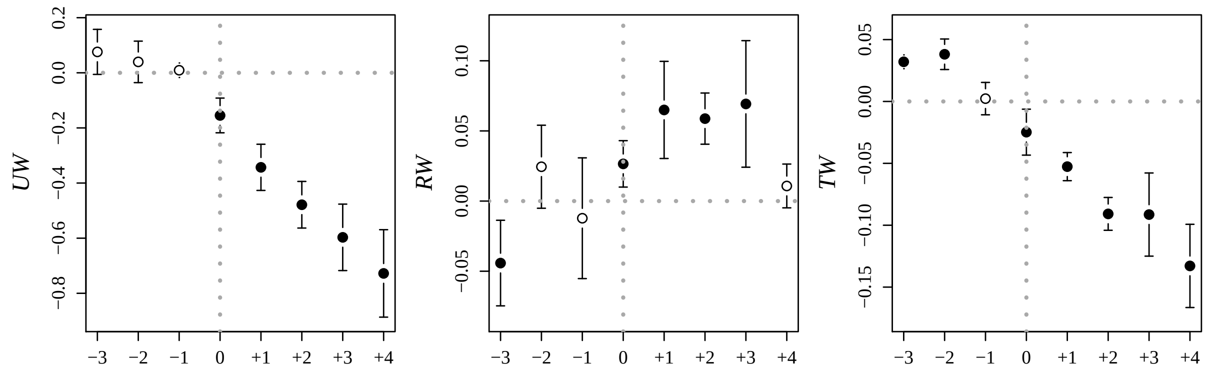

Figure 4: DiD estimates (2010-2015, $n = 19,982$) of average policy effects on UW, RW, TW (logs) with time and unit fixed effects from the fourth year pre-policy (-4) to the fourth policy year (+4). Clustered standard errors (Driscoll & Kraay, 1994). Statistical significance (5%) indicated by black dots.

Figure 4 plots estimated lead and lagged average policy effects with their confidence intervals. Black dots represent statistical significant effects at 5%, and vertical dotted lines indicate the year of policy decision which usually includes PAYT training, namely, households discard their waste per bag/weight/emptying but do not pay accordingly. Post-policy estimates show that PAYT is effective in reducing UW and TW with larger effects over time in the range of 15-73% (13-25%) for UW (TW). PAYT seems effective also in increasing recycling (3-7%) at least until the third (+3) year after policy. Yet, considering the pre-policy period, the DiD estimates statistically effects for RW and TW. This reveals that waste generation of PAYT and non-PAYT municipalities differ already before policy, leading to the bias of post-PAYT effects due to non-parallel trends.

Assuming selection based on time-constant variables is the likely source of this bias, pointing towards the need to control for time-varying characteristics and, possibly, allow variables to have time-varying effects on policy adoption and waste generation. In fact, DiD only allows for time-invariant effects on the outcome, which might not be the case in this setting (Gobillon & Magnac, 2016; Valente & Bueno, 2019). For instance, opportunity costs of waste disposal may have evolving effects over time. Controlling for this variation with a high-dimensional set of covariates is at the heart of this paper's motivation to employ a random forest (RF) approach.

5.2 Random forests estimation

I have explored from 500 to 10,000 trees in the RF, and treatment effect estimates become stable after 1,000 trees, thus, results are obtained using this value. Further, each tree is built with data for the same year to account for common shocks to all units. All trees are grown with cross-validated values for the number of randomly subsampled covariates, minimum leaf size, and penalty for imbalanced splits, namely, splits in which the size of parent and child node are very different are penalized. Additionally, since the treatment group is substantially smaller than the control group, each node is required to include a minimum number of both treated and control units, i.e., enough information about both factual and counterfactual to estimate the treatment effect reliably. For this reason, a penalization is imposed also to nodes including an unbalanced number of treated and control units. Values for such parameters are taken following Athey *et al.* (2019) and obtained via cross-validation.[20]

To obtain ceteris paribus partial and interaction effects, I estimate the shape of the forest-based regression function allowing the variable(s) of interest to vary while holding all other attributes equal. For this, I assign each value of the variable of interest being, e.g., income to all N observations, namely, to all row vectors $X_i = x$, and feed this new data into the forest to predict TEs. This gives N^*v estimates of TE associated to all possible income values, v, and the other unaltered covariate values of each x. By averaging these estimates over the same income value and observation, I obtain heterogeneous TE for different income values while holding all other attributes equal. Next, I estimate the magnitude and statistical significance of TE heterogeneity by the local linear nonparametric regression method proposed by Li and Racine (2004) employing cross-validated bandwidth selection

[20] All RFs and related codes are developed using the software R-3.4.2 (Tibshirani *et al.* , 2018).

using the method of Hurvich et al. (1998).[21]

5.3 Heterogeneous propensity score estimation

Forest-based estimates show that PAYT adoption probabilities (propensity scores, PS) are especially heterogeneous in recycling rates, as well as socio-economic and political variables, and complex interactions thereof. This points towards the importance to include a high-dimensional set of covariates to account for endogenous adoption. Figure 5 plots values of initial recycling rates, $W_R(W_0)$, versus the estimated gradient (partial effect) with its variability bounds around the smoothed function.

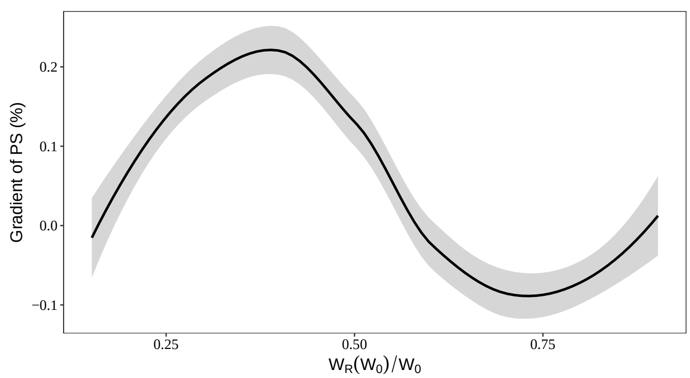

Figure 5: Partial local linear nonparametric regression plot for partial effects of $W_R(W_0)/W_0$ on PS.

Gradient estimation in Figure 5 shows that a one percent point (pp) increase in recycling rates ($W_R(W_0)/W_0$) between the first and second quartile (34-53%) leads to 20.3% higher PS, an effect decreasing over higher quartiles. Yet, a one pp increase in recycling rates between the third and fourth quartile (53-74%) and above the fourth quartile (>74%) leads to 4.6% and 3.7% lower PS, respectively.[22] These results suggest that PAYT is possibly adopted to further improve recycling rates towards EU's goals,[23] and it is thereby

[21] In particular, bandwidth selection is performed by AIC cross-validation and with Gaussian kernel weights using the np package (Hayfield & Racine, 2008). Results are checked for robustness to locally constant estimation.

[22] P-values are below the conventional 0.05 level and can be reported upon request.

[23] The EU mandated its member states to increase recycling rates to 50% by 2020 (European Union,

less likely for municipalities which already meet these targets.

Moreover, to a smaller extent, PS are estimated to increase with pre-policy recycling and total waste levels (at a decreasing rate). The first suggests that, to prevent policy adverse effects such as illegal dumping, policymakers are more likely to implement PAYT where recycling habits are developed. The second seems to confirm the hypothesis that municipalities tend to introduce PAYT if waste pollution (and related costs) are high.

To a smaller extent, sources of heterogeneity are also determined by other municipal characteristics and complex interdependences thereof. This is shown, for instance, by the interaction plots in Figures 6 and 7 where PS are denoted by color, from dark (high) to light (low).

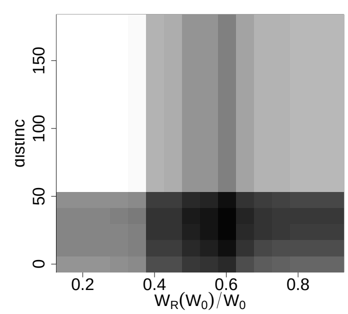

Figure 6: PS heterogeneity in recycling rates ($W_R(W_0)$) and distance to incinerators ($distInc$).

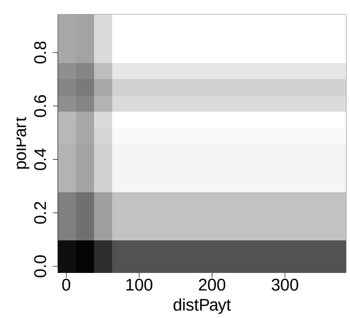

Figure 7: PS heterogeneity in neighborhood effects ($distPayt$) and political participation ($polPart$).

Figure 7 indicates that PS are higher for municipalities with initial recycling rates between first and second quartile and located within 50 km from the closest incinerator, probably because more aware of the waste pollution problem. For these municipalities, a one pp increase in recycling rate increases PS by 23.2%. Also, Figure 7 shows that PS are highest where political participation is low, which may simply capture a fixed effect, and neighborhood (mimicking) effects are present. This suggests that nearby municipalities - including those belonging to the same consortium - are more likely to implement PAYT.

Moreover, I find that pre-policy average and per capita costs of MSW services explain

2008).

only a very small portion of the variation in PS ($< 1\%$). Looking at statistically significant heterogeneity in other socio-economic and political variables, PS are higher for municipalities with ceteris paribus higher income per capita ($incomePc \geq 15$k euros and independently on education), lower household density and medium levels of population density ($hhPerSqMeter < 2$; $popDens \approx 5,000$), higher share of infants ($age0 > 4\%$), lower share of foreigners and higher tourism ($foreignPop < 10\%$; $tourism > 4$k), and young mayors ($mayorAge < 40$) elected since at least two years ($yearsOffice > 2$). Also, adoption is not strongly associated to vote shares for any political party. Further, municipalities with a landfill or close to one ($distLandf < 6$ km) are most likely to adopt PAYT, which may be due to landfills reaching capacity.[24]

To assess the balancing property of the PS and overlap assumption (Imbens & Rubin, 2015), the Appendix C reports overlap statistics for PAYT and non-PAYT municipalities. Results show that overlap is satisfied after deleting around 2% of the sample, and covariate distributions are balanced between treated and untreated units.

5.4 Heterogeneous treatment effect estimation

Forest-based estimates of average policy effects accounting for endogenous adoption with a large set of covariates are summarized in Table 1. Results show that PAYT is effective, decreasing per capita unsorted waste up to -60% relative to pre-policy waste amounts. These effects are mostly driven by an increase in recycling (up to 41%), and only to a lesser extent by total waste reductions (up to -15%). This indicates that households prefer recycling over waste avoidance on average, and the two behaviors are substitutes. Highest compliance occurs in the first policy year (+1) after which households continue substituting unsorted waste disposal with more recycling, though in lower quantities. Waste avoidance occur only in the longer-run, and if further incentivized by municipalities as a cost-saving

[24] All results are available upon request.

option in alternative for recycling.

Table 1: Average policy effect estimates from training year (0) to fourth policy year (+4). Standard errors clustered by year. Statistical significance (5%) indicated in bold.

Year	UW %	kg	s.e.	RW %	kg	s.e.	TW %	kg	s.e.
0	-1.7	-4.3	22.53	**27.6**	65.3	9.53	**6.2**	28.8	10.27
+1	**-22.5**	-55.2	18.6	**32.3**	81	14.48	3.43	16.5	15.62
+2	**-34.8**	-82.7	14.39	**35.0**	85.3	10.7	-0.42	-2	16.25
+3	**-51.7**	-120.7	7.91	**40.7**	94.5	15.91	-5.05	-23.3	21.08
+4	**-59.7**	-143	3.6	**25.2**	57.4	4.98	**-15.3**	-70.5	7.31

Table 1 shows that in the training and first policy years (0 and +1) PAYT causes a substitution behavior between unsorted and recycling with a rebound effect on total waste. In the training year (0), this corresponds to an increase of 27.5% in recycling driven, to a minor extent, by an insignificant decrease in unsorted waste of 1.7% and, to a major extent, by an increase in total waste of 6.2% relative to pre-policy waste amounts. In terms of kg, this translates into an increase in recycling of about 50 kg, 30 kg of which are subtracted from the unsorted waste and the remaining approximately 20 kg are generated from an increase in total waste. Yet, due to the non-significance of the causal effects on unsorted and total waste, the exact mechanisms behind the increase in recycling in the training year are uncertain.

For all subsequent years, causal effects on unsorted and recycling waste are statistically significant either at or below the 5% level, which leads to the conclusion that recycling and avoidance efforts are substitutes. In particular, from the first to the third year after policy (+1 to +3), effects on unsorted waste and recycling increase in magnitude up to -51.7% and +40.7% of the respective waste amounts pre-policy. No rebound effects on total waste occur from the third policy year (+3) onward and, instead, households start avoiding waste, though in relatively small and non-significant amounts.

Interestingly, results for the fourth policy year (+4) – estimated on treated and untreated municipalities in the year 2012 – show complementarities between waste reduction behaviors, namely, both waste recycling and avoidance take place. Looking at the geographic and administrative characteristics of the treated municipalities, most of them belong to the same consortium which, four years after PAYT introduction, decided to publicly encourage avoidance behaviors as a cost-saving option in alternative for recycling (Gazzetta delle Valli, 2014). Thereby, reminded of the opportunity of saving through avoidance behaviors, households generated less unsorted waste by 59.7% from an increase in recycling of 25.2% and a decrease in total waste of -15.3% relative to pre-policy waste amounts.[25]

Moreover, the analysis reveals considerable effect heterogeneity. This can be explained by heterogeneous opportunity costs of waste recycling and avoidance, and complex interactions of covariates. Concerning the first, there is a significant difference in policy effects for different initial waste levels (used to proxy opportunity costs), other things equal. In particular, municipalities with high recycling levels pre-policy (low opportunity costs of sorting) increase recycling more than municipalities with low levels in response to PAYT. Similarly, municipalities with low total waste pre-policy (low opportunity costs of avoidance) react to PAYT by reducing total waste more than municipalities with high levels.

Partial regression and interaction plots in Figures 8 and 9 show TE heterogeneity on recycling by plotting initial waste levels versus the estimated TE for the first policy year.[26]

[25] In terms of kg, the estimated causal effects for UW and RW approximately correspond to those for TW only when considering the respective standard errors. This may indicate the presence, to a small degree, of illegal dumping or alternative waste disposal options such as waste burning.

[26] Due to computational limitations, the new data used to estimate ceteris paribus effects varies over a subset of covariate values and not over all values as described in Section 5.2, resulting in less smooth predictions.

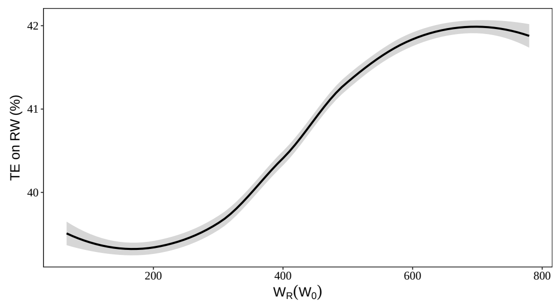

Figure 8: **Partial effects** of $W_R(W_0)$.

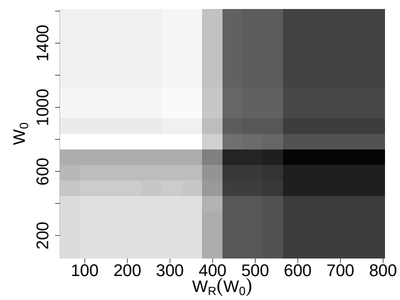

Figure 9: **Interaction effects** with W_0.

Figure 8 reveals that TE heterogeneity is not constant. For instance, a 100 kg increase in $W_R(W_0)$ at the 50th percentile ($\approx$ 400 kg) leads to stronger policy effects on recycling by about one pp on average. Also, heterogeneity in $W_R(W_0)$ seems to occur where PAYT was highly effective, namely, where recycling increases by about 39-42% due to the policy. Concerning heterogeneity in initial total waste, a 100 kg increase in W_0 at the second quartile ($\approx$ 500 kg) leads to higher rebound effects on total waste by about half pp on average.[27]

These results suggest that TE heterogeneity in initial waste levels is significant but not large in magnitude, which points towards the importance of interactions with other covariates. In this respect, Figure 9 shows that PAYT is highly effective on increasing recycling especially in those municipalities with ceteris paribus high pre-policy recycling and total waste levels around 600 kg per capita (causal effects are denoted by color, from dark/high to light/low).

Focusing on socio-economic heterogeneity, variables such as education and income are found to interact in complex ways. These are two variables typically considered by the literature as important determinants of policy effectiveness, although their effects are rarely assessed due to data limitations (Usui *et al.* , 2017), and oft ambiguous (for reviews see, e.g., Fiorillo, 2013; Schultz *et al.* , 1995). Interaction plots of income (*incomePc*) and

[27]Estimates for total waste are available upon request.

education levels (*college*) are displayed in Figures 10, 11, 12, 13.

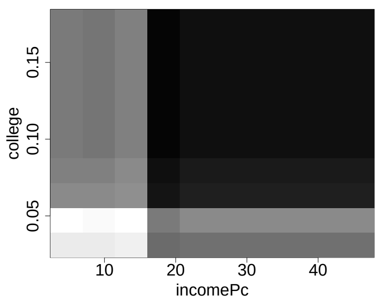

Figure 10: Short-term TE heterogeneity on recycling by income and education levels.

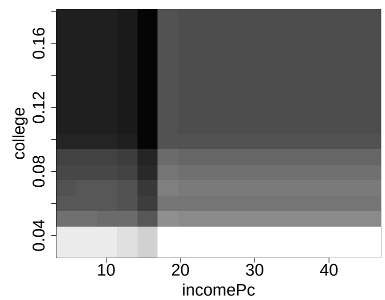

Figure 11: Short-term TE heterogeneity on avoidance by income and education levels.

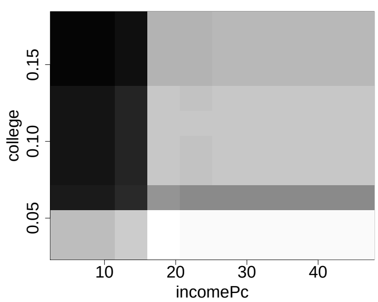

Figure 12: Long-term TE heterogeneity on recycling by income and education levels.

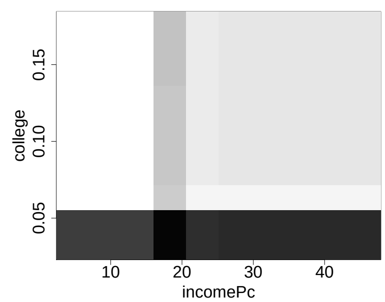

Figure 13: Long-term TE heterogeneity on avoidance by income and education levels.

Interactions plots indicate that, short-term, low-educated municipalities comply with the policy the least. Yet, if high-income, these municipalities prefer recycling over avoidance by increasing the former and reducing the latter (see Fig. 10 and 11). I also find that short- and long- term heterogeneity differ. Figure 12 shows that the strongest long-term recycling increases are estimated for lower-income and higher-educated municipalities (in line with prior results on income heterogeneity by, e.g., Usui & Takeuchi, 2014). The latter are those municipalities showing the strongest effects on avoidance in the first policy year (see Fig. 11). Similarly, while low-educated municipalities avoided waste the least short-term (see Fig. 11), they avoid waste the most long-term.

Heterogeneity in other variables is often statistically significant but of very small magnitude. For instance, PAYT effectiveness on recycling is significantly higher for municipalities with a low share of infants (*age0*), low share of student population (*students*), with larger households (*hhSize*), and farther away from incinerators (*distInc*). This likely indicates that families with infants as well as students have higher time opportunity costs of sorting while larger families may have increasing returns.

Finally, since policy heterogeneity may not only be due to waste generation habits pre-policy, other covariates, and interactions thereof, I analyze how much of the variation of the estimated TE may be explained by different PAYT prices and systems.

5.5 Unconfoundedness

The RF approach relies on unconfoundedness as an identifying assumption, aka selection on observables. The factors influencing municipalities' policy adoption and waste generation are of socio-economic, geographic, administrative, and political nature as well as depend on households' recent waste generation history and costs of MSW service (see Sections 2 and **??**). The information that policymakers base their decision on are contained in our data, in particular previous waste generation behaviors capturing waste-reduction experience and opportunity costs of alternative waste disposal options (recycling/avoidance). Accounting for all these variables, this analysis aims to make the impact of leftover unobserved factors negligible, and the unconfoundedness assumption plausible. Moreover, to address the concern that unaccounted-for factors may bias the estimation of policy effects, I run in-time placebo tests by estimating causal effects in the pre-policy period. Similarly to the DiD framework (Autor, 2003), statistically significant estimates for these years would indicate the presence of unaccounted-for factors in either the selection or the outcome equation or both. Conditioning on the same set of covariates and all possible outcome lags, forest-based estimates plotted in Figure 14 show that this is not the case.

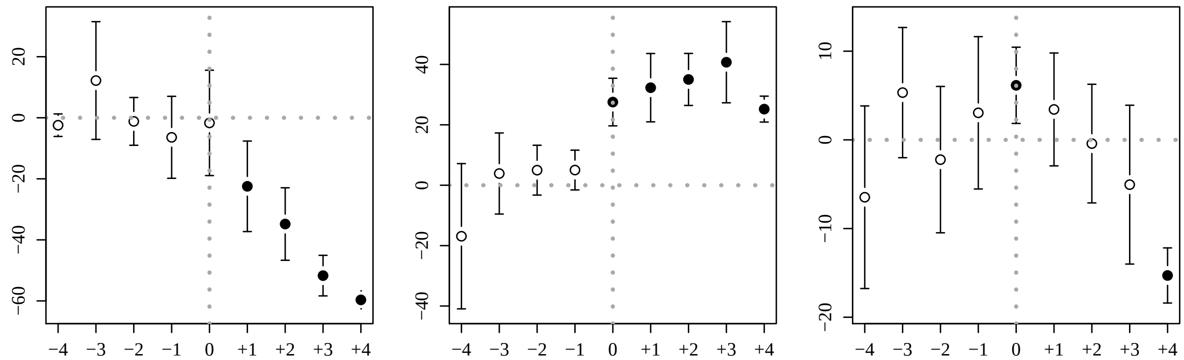

Figure 14: Average policy effect estimates from the fourth year pre-policy (-4) to fourth policy year (+4). Standard errors clustered by year. Statistical significance (5%) indicated by black dots.

Lastly, as robustness checks against a possible omitted variable bias, I estimate policy effects on transformed waste outcomes, namely, after unit-demeaning and detrending waste quantities to eliminate the effect of any time-invariant municipal characteristic and linear trend in waste generation, respectively. In both cases, causal estimates have similar magnitude and statistical significance to those obtained above, suggesting that the main estimates do not suffer from such omitted variable bias (results are presented in Tables x and y in the Appendix D).

5.6 Heterogeneity in PAYT prices and systems

[Work in progress] The previous analysis focuses on binary policy effects without differentiating by price levels. Unit prices, however, are heterogeneous and vary between 0.01 to 0.18 euros per liter of unsorted waste. In this section, I contribute to the literature that investigates price elasticities of household demand for MSW by analyzing policy heterogeneity that may arise from variation in price levels. Under similar assumptions to the binary treatment framework, R-learning random forests can be used to estimate the heterogeneity of policy effects along the price dimension after partialling out the impact of covariates on outcomes and price levels, where the latter amounts to estimate the oft-called generalized propensity score (GPS) function (Hirano & Imbens, 2004; Imai & van

Dyk, 2004). The estimated price coefficients in the outcome equation have no direct causal interpretation but they reflect the relative effectiveness of PAYT programmes with different price levels. For instance, a flat functional relation between prices and waste amounts would imply that the household demand for MSW is price inelastic, which may be the case if prices are not salient resembling the effects of a nudge policy.

5.7 Estimation of social cost savings

[Work in progress] Idea: Estimate actual and potential social costs savings (environmental plus municipal) by combining estimated individual TE for treated and predicted TE for untreated on all waste types (unsorted, recycling, total) with data on MSW management, collection and treatment costs as well as monetized pollution costs for all municipalities.

6 Conclusions

Using an R-learning random forest estimator on a unique and high-dimensional panel data on Italian municipalities, this paper shows that PAYT is effective, reducing per capita unsorted waste amounts by 60% on average over four years after policy. This effect seems to be driven by behavioral changes towards recycling (up to 41%) and by a smaller increase in waste reduction (up to -15%). This indicates that households prefer recycling over waste avoidance on average, and the two behaviors are not complementary. Highest compliance occurs in the first policy year after which households continue substituting unsorted waste disposal with more recycling in lower quantities. Waste avoidance occurs only in the longer-run and for those municipalities which further incentivize this behavior as a cost-saving option in alternative for recycling.

Forest-based estimates show that PAYT adoption probabilities (propensity scores, PS) are especially heterogeneous in recycling rates, as well as socio-economic and political variables, and complex interactions thereof. This points towards the importance to include a high-dimensional set of covariates to account for endogenous adoption. Results show that PAYT was possibly adopted to further improve recycling rates towards EU's goals, namely, 50% within 2020 (European Union, 2008), and it was thereby less likely for municipalities which already meet these targets.

Moreover, this analysis reveals considerable effect heterogeneity. This can be explained by heterogeneous opportunity costs of waste recycling and avoidance, and complex interactions of covariates. Concerning the first, there is a significant difference in policy effects for different initial waste levels (used to proxy opportunity costs), other things equal. In particular, municipalities with high recycling levels pre-policy (low opportunity costs of sorting) increase recycling more than municipalities with low levels in response to PAYT. Similarly, municipalities with low total waste pre-policy (low opportunity costs of avoidance) react to PAYT by reducing total waste more than municipalities with high levels. This suggests that targeting municipalities with low opportunity costs of recycling through, e.g., awareness-raising campaigns could increase PAYT effectiveness on total waste reductions.

Focusing on socio-economic heterogeneity, variables such as education and income are found to interact in complex ways. I also find that short- and long- term heterogeneity differ showing that recycling and avoidance behaviors are substitutes over time. This means that, e.g., while low-educated municipalities avoided waste the least short-term, they avoid waste the most long-term. Altogether, these findings add further evidence as to the oft-ambiguous findings in the literature on the relation between recycling to income and education (for reviews see, e.g., Fiorillo, 2013; Schultz *et al.* , 1995; Usui *et al.* , 2017).

Additionally, to contribute to the literature that investigates price elasticities of household demand for MSW, I analyze policy heterogeneity that may arise from variation in price levels. My estimates show that [...]

[Lastly, from a social cost view point...]

This study has several possible policy implications. Adopting PAYT seems to lead to waste pollution reduction and changes in household waste generation behavior. However, PAYT effects are dynamic and heterogeneous, i.e., depend on time from adoption and municipal characteristics, respectively. Accounting for this, municipalities could increase policy effectiveness by, e.g., providing incentives for sorting and source-reduction behaviors already before policy implementation.

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A Appendix

Total derivatives of optimality conditions in (3) for $i = A, R$ write:

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

$$\frac{\partial^2 V(\cdot)}{\partial w_R \partial t}\frac{\partial t}{\partial t} + \frac{\partial^2 V(\cdot)}{\partial w_R \partial w_A}\frac{\partial w_A}{\partial t} + \frac{\partial^2 V(\cdot)}{\partial w_R^2}\frac{\partial w_R}{\partial t} = 0 \tag{12}$$

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

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

$$\frac{\partial w_R}{\partial t} = \frac{\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R}\frac{\partial V(\cdot)}{\partial w_A \partial t} - \frac{\partial^2 V(\cdot)}{\partial w_A^2}\frac{\partial V(\cdot)}{\partial w_R \partial t}}{\frac{\partial^2 V(\cdot)}{\partial w_A^2}\frac{\partial^2 V(\cdot)}{\partial w_R^2} - (\frac{\partial^2 V(\cdot)}{\partial w_A \partial w_R})^2}, \tag{14}$$

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

Moreover, from equation (3) compute the following derivatives:

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

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

$$\frac{\partial^2 V(\cdot)}{\partial w_i^2} = -U''(\cdot)t^2 - \frac{\partial^2 C}{\partial w_i^2} \tag{17}$$

Substituting (15), (16), (17), and soc in equations (13) and (14) gives:

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

$$\frac{\partial w_R}{\partial t} = \frac{(\frac{\partial^2 C}{\partial w_A^2} - \frac{\partial^2 C}{\partial w_A \partial w_R})(U'(\cdot) + U''(\cdot)t(w_A + w_R - W))}{soc}, \tag{19}$$

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 (4).

B Appendix

This appendix presents descriptive statistics and denominations of the X variables included in the estimation of propensity scores and treatment effects. In particular, economic theory and knowledge of the policy setting leads to the inclusion of $k = 50$ covariates. Differently from past literature, this analysis does not ex-ante select among possibly relevant variables. In fact, the collected data is richer than that available in previous studies, and the employed random forest approach works well with large and high-dimensional data.

Waste data for PAYT municipalities with either incomplete or mistaken data is obtained upon request from the respective waste collection company. Data errors due to, e.g., typing and misreporting, are analyzed by looking at anomalies in MSW generation over time, specifically, by comparing deviations to the municipal-specific median with the sample upper bound (SUB) defined by $Q_3 + (Q3 - Q1)$ where Q is the quartile value, $(Q3 - Q1)$ the interquartile range. Municipalities with deviations to the median beyond the SUB are eliminated from the data.

Looking at possible outliers, the sample includes all extreme outcome values but excludes units with extremely low values in recycling shares and per capita recycling to increase the comparability of municipalities' waste management practices. Extreme values are eliminated if they are below the sample lower bound (SLB) defined by $Q_3 - 0.5(Q3 - Q1)$ where 0.5 is chosen to avoid that Q_3 takes negative values.

Table 2: Descriptives for treated (T) and control (C) municipalities (2010-2015)

	mean(T)	min(T)	max(T)	SD(T)	mean(C)	min(C)	max(C)	SD(C)

Continued on next page

RW	315.41	84.79	721.51	93.14	233.94	13.13	828.52	82.84
TW	486.00	230.62	1594.06	139.08	457.40	107.63	1193.25	127.03
UW	170.59	35.83	1016.42	112.62	223.47	21.62	867.26	120.53
RWrate	0.66	0.17	0.90	0.15	0.53	0.03	0.91	0.17
costUW	51.30	0.11	294.66	22.39	40.34	0.11	327.44	22.48
costRW	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
avgCostUW	0.17	0.00	0.60	0.07	0.18	0.00	2.24	0.12
avgCostRW	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	0.00	1.93	0.09
distPayt	22.55	0.00	339.55	35.02	50.00	1.00	371.26	63.23
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	2381.23	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	0.23	0.03
age65	0.22	0.11	0.34	0.04	0.23	0.05	0.51	0.05
elemDeg	0.31	0.19	0.42	0.04	0.31	0.14	0.57	0.05
collegeDeg	0.09	0.03	0.17	0.03	0.09	0.03	0.18	0.03
rentedHouses	0.09	0.04	0.17	0.02	0.09	0.00	0.44	0.03
hhPerSqMeter	2.24	1.68	3.08	0.26	2.30	1.28	3.27	0.27
oneParentFam	0.10	0.06	0.16	0.01	0.10	0.00	0.19	0.02
students	0.06	0.04	0.08	0.01	0.06	0.05	0.09	0.01
commuters	0.26	0.09	0.36	0.05	0.26	0.06	0.37	0.06
deprIndex	-1.87	-6.42	1.70	1.32	-1.50	-7.62	6.14	1.58
outLabRate	0.62	0.51	0.78	0.05	0.63	0.46	0.90	0.06
unempOutLab	0.06	0.01	0.13	0.02	0.07	0.00	0.23	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	0.23	0.09	0.37	0.05	0.23	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

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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	0.48
urbanHigh	0.07	0.00	1.00	0.26	0.07	0.00	1.00	0.25
urbanLow	0.14	0.00	1.00	0.35	0.11	0.00	1.00	0.32

Table 3: Variables' description. Census indicates 2011 values (ISTAT, 2011).

	Variables' description
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 Management, Collection, Treatment (MCT) costs of UW (euros)
costRW	per capita MCT costs of RW, net of recycling revenues (euros)
costTW	per capita MCT costs of TW (euros)
avgCostUW	average MCT costs of UW (euros per kg)
avgCostRW	average MCT costs of RW, net of recycling revenues (euros per kg)
avgCostTW	average MCT costs of TW (euros per kg)
distPayt	distance to closest municipality with PAYT in t-1 (km)
distHaz	distance to closest hazardous waste treatment facility (km)
distInc	distance to closest waste incinerator (km)
distLandf	distance to closest waste landfill (km)
popDens	population density (inhabitants per km2)
hhSize	average household size (n. household members)
income	income per capita (x 1,000 euros)
migrNet	net migrant flow per capita
pop	population (x 1,000 inhabitants)
foreignPop	share of foreign population
males	share of male population
popGrowth	population growth
tourism	capacity of tourist accommodation per capita (x 1,000)
age0	share of population aged less than 5 (census)
age14	share of population aged less than 14

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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 index (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 left-wing parties in the IGR13
votesRight	vote shares for right-wing parties in the IGR13
localMayor	mayor born in the municipal province (dummy)
mayorCenter	centre-party mayor (dummy)
mayorGreen	green-party mayor (dummy)
mayorLeft	left-wing mayor (dummy)
mayorOther	mayor of other party (dummy)
mayorReg	mayor of local party (dummy)
mayorRight	right-wing mayor (dummy)
mayorAge	mayor's age
yearsOffice	mayor's term of office (years)
noSeat	no capital (dummy)
provSeat	provincial capital (dummy)
regionSeat	regional capital (dummy)
urban	mediumly urbanized municipality (dummy)
urbanHigh	highly urbanized municipality (dummy)
urbanLow	lowly urbanized municipality (dummy)

C Appendix

This section presents overlap statistics for the estimated propensity scores (PS). 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 we would observe terminal nodes including only either treated or untreated units and, thus, PS 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). Therefore, PS overlap guarantees that a comparable unit can be found for each municipality. As shown in Figure 15, PS of treated and untreated (control) group overlap.

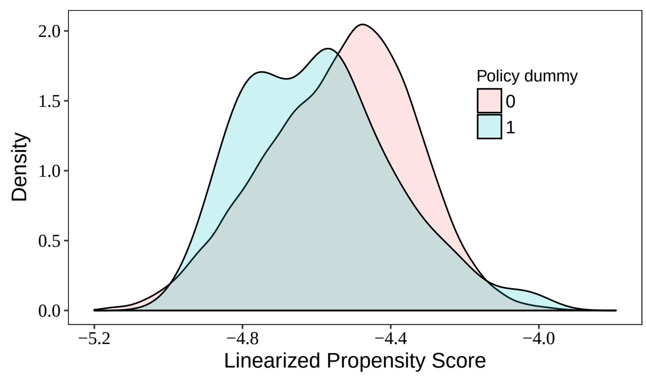

Figure 15: Distribution of propensity scores (logs) for treated (PAYT=1) and untreated (PAYT=0).

In particular, treated and control groups are similar in their PS means, first and third quartiles. Statistics computed following Imbens & Rubin (2015) show that the two groups are not apart, namely, the (normalized) difference in estimated PS is 0.16 which is far less than one standard deviation. Also, there is good coverage frequency for both treated and control group meaning that, specifically, 96% (94%) of the treated (control) units have PS values inside the .025 and .975 quantiles of the PS distribution of the control (treated) units. Further, all units have close comparisons in the opposite treatment group. In particular, for all treated units and 97% of the control units there are units with the other treatment status that have differences in PS less than 10%, a threshold that guarantees unbiased estimates of the causal effects without extrapolation (Imbens & Rubin, 2015). Therefore, causal effects for the control group, and not only for the subpopulation of treated units, can be credibly estimated under unconfoundedness.

D Appendix