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The effects of pricing waste generation: A synthetic control approach

Matheus Bueno* and Marica Valente†

Abstract To internalize pollution externalities into household waste generation, Unit Pricing Systems (UPS) have been adopted worldwide. This paper evaluates the causal effects of a UPS on the disposal of municipal solid waste in Trento, Italy. Using a unique panel dataset of monthly waste generation in Italian municipalities, we employ the synthetic control method, which allows us to account for possible time-varying effects of unobservables. Our results show that the policy was effective, with a significant decrease of the priced waste stream, unsorted waste, by 37.5%. This effect seems to be largely driven by behavioral changes towards waste avoidance (−8.6%) and possibly by a smaller increase in recycling (+6.1%). By comparing these results to those obtained by a difference-in-differences approach, we show that failing to account for time-varying effects of unobservables may lead to a mismeasurement of policy effects.

Keywords: Waste generation, Unit pricing, Synthetic controls, Policy endogeneity, Selection on unobservables

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1 Introduction

The generation of Municipal Solid Waste (MSW) characterizes a classic example of negative externality, as its environmental and treatment costs are higher than its private ones. Standard flat fees on MSW collection, however, do not usually suffice for internalizing the cost difference into individuals' waste generation behavior. Therefore, pricing waste per collected unit, a form of Pigouvian fee (Pigou, 1932), has been increasingly used as a policy instrument, known as Unit Pricing Systems (UPSs) or pay-as-you-throw programs. Existing empirical evaluations of such policies generally find them to be effective, when controlling for selection bias due to unobserved determinants of their adoption with time-invariant effects on waste generation.¹

Nevertheless, despite their growing popularity, there is an empirical gap on UPSs' effectiveness when the effects of unobservables on waste generation vary over time, and on the behavioral mechanisms behind UPSs' effects. Using a unique data on monthly waste generation for nineteen municipalities in Italy between 2008 and 2016, this paper attempts to fill this gap by evaluating the causal effects of a UPS implemented on the disposal of unsorted MSW in the municipality of Trento in 2013.

The first contribution of this paper is to address the endogeneity associated with UPS adoption due to selection on unobservables with time-varying effects. Waste generation in municipalities adopting this policy may have unobserved determinants that spawn diverging waste generation trends with respect to municipalities not adopting it. For example, the effects of pro-environmental attitudes on waste generation may vary over time depending on, e.g., learning effects from awareness-raising campaigns at the local level that inform citizens on the excess waste problem, and promote recycling and waste reduction behaviors. The impacts of unobservables on waste generation may cause non-parallel trends in outcomes, invalidating the identification assumption of standard policy evaluations based on differencing out fixed individual characteristics with time-invariant effects, such as with fixed effects and difference-in-differences (DID) estimation (Gobillon and Magnac, 2016).

To account for time-varying effects of unobservables, we implement the Synthetic Control Method (SCM) (Abadie and Gardeazabal, 2003; Abadie et al., 2010). This method reconstructs the counterfactual outcomes using a combination of untreated municipalities with similar outcome trajectories that did not select into treatment, hence replicating the unobserved heterogeneity and allowing it to have time-varying effects. Several empirical applications in other areas of study have been implementing the SCM², but, in the authors' best knowledge, this is the first paper doing so to evaluate waste pricing policies.

Secondly, this paper adds to the literature by studying the behavioral mechanisms

¹See, e.g., the survey in Huang et al. (2011), and the literature review in Section 2.

²See, e.g., Abadie et al. (2015), Acemoglu et al. (2016), Billmeier and Nannicini (2013), Cavallo et al. (2013), and Pinotti (2015).

behind the partial effects on the priced waste stream. When the relative price of unsorted waste increases, individuals may shift away from generating unsorted waste either by recycling more or by avoiding waste generation of any sort, e.g., by using less packaging.³ Thereby, this paper looks at which behavioral response prevails by evaluating policy causal effects separately for total, recycling and unsorted waste.

Our results indicate policy effects of sizable magnitude, and changes in waste generation behavior. Unsorted waste generation fell immediately after UPS implementation and levelled off after around two years, with an average decrease of 37.5% in the post-treatment period. This partial effect seems to have been largely driven by waste avoidance and a relatively smaller increase in recycling, with total waste decreasing by 8.6% and recycling waste increasing by 6.1%. Placebo tests show that the unsorted waste reduction is strongly significant, while weaker and no significance is found for the causal effects on total and recycling waste, respectively. Finally, we show that the use of the SCM rather than a DID approach is justified in this application because the parallel trend assumption needed for the validity of the latter is violated. The successful reconstruction of the unobserved heterogeneity by the SCM, on the other hand, allowed to control for its time-varying effects.

The remainder of the paper is organized as follows: Section 2 provides a brief review of the related literature on UPS, and discusses potential methodological shortcomings. Section 3 outlines the SCM used to estimate the policy causal effects under study. Background and data are described in Section 4. Section 5 provides the results, and Section 6 concludes and discusses implications of this study.

2 Literature

Starting in the 1970s, UPSs have been broadly implemented both in Asia and the United States, e.g., in municipalities of Japan, California, and Michigan (Miranda and Bauer, 1996). By the mid-2000s, UPSs have been used in 30 of the 100 largest municipalities in the country as, e.g., Seattle and San José (Skumatz, 2008). Afterwards, UPS was adopted in many European countries, particularly Switzerland, the Netherlands, the northeastern area of Germany, Denmark and Italy (for a review, see, e.g., Reichenbach, 2008).

Since the 1990s, following their implementation, UPSs' effects have been extensively studied in the literature. Evaluations in the United States respond for a large chunk of the early and current literature (Fullerton and Kinnaman, 1996, 2000; Hong et al., 1993; Huang et al., 2011; Miranda and Bauer, 1996; Podolsky and Nestor, 1998; Reschovsky and Stone, 1994; Van Houtven and Morris, 1999; Wright et al., 2018). Studies for other regions have followed through for, e.g., Korea (Hong and Adams, 1999; Kim et al., 2008),

³Note that a potential policy response is also to dump waste illegally. We discuss this possibility in Section 5.

Japan (Usui, 2009; Usui and Takeuchi, 2014; Yamakawa et al., 2002), and the Netherlands (Allers and Hoeben, 2010; Dijkgraaf and Gradus, 2009, 2004; Linderhof et al., 2001). However, to the authors' knowledge this is one of the first UPS policies to be evaluated in Italy and Europe, except from the Dutch case, another study on Italy (Buccioli et al., 2015) and Switzerland (Carattini et al., 2018). The scarce empirical evidence on the effects of European UPSs lingers despite its increasing use in the region, and the EU mandate targeting both waste avoidance and diversion (EEA, 2009, 2013; EU, 2008).

Moreover, results on UPSs' effectiveness differ across studies and geographic areas, suggesting a general lack of external validity. A potential reason for this is that policy effectiveness is likely to vary across different social environments (see, e.g., Kipperberg, 2007; Reschovsky and Stone, 1994, for a thorough analysis). Based on economic theory, one would expect that households respond to economic incentives in a similar way, i.e., by decreasing (increasing) priced (unpriced) waste generation. Empirically, instead, some studies estimate small, often insignificant substitution between priced and unpriced waste generation, as well as unclear prevention efforts, i.e., waste reductions possibly due to reuse and adjustments in purchasing habits (see, e.g., Allers and Hoeben, 2010; Fullerton and Kinnaman, 2000, 1996; Jenkins et al., 2003).

A challenge that the literature faces is to address the endogeneity of UPS adoption (see, e.g., a discussion in Dijkgraaf and Gradus, 2009). As mentioned earlier, a UPS may be implemented in municipalities where environmental awareness has dynamic effects on waste generation through, e.g., learning effects from additional information provision, and recycling and waste reduction promotion. On the contrary, UPS may also be adopted where low learning effects and environmental awareness result in high waste levels requiring local governments to consider policy alternatives to standard flat fees. Hence, policy adoption is likely not orthogonal to municipalities' observed and unobserved waste generation determinants. To the best of our knowledge, the existing literature only considers selection bias due to observables and/or fixed unobservables with time-invariant effects.

To account for unobserved heterogeneity, various approaches have been used. A part of the literature that controls for observed determinants within a regression framework to estimate price elasticities of waste demand finds them to be mostly inelastic (see Huang et al., 2011, for a discussion of these studies). Among this literature, a number of studies explicitly take into account selection biases with instrumental variable approaches (see, e.g., Fullerton and Kinnaman, 2000; Usui, 2008; Huang et al., 2011). Most of these evaluations are based on cross-sectional data and on a large number of UPS municipalities with different price levels which are instrumented in 2SLS regression (for a review, see Bel and Gradus, 2016). Findings from this literature generally vary, depending on the method, data and municipalities under study.

Other investigations account for unobserved heterogeneity with time-invariant effects in DID and fixed effects estimations, concluding that UPSs are mostly effective in reducing

unsorted waste and increasing recycling (Dijkgraaf and Gradus, 2004, Van Houtven and Morris, 1999, Linderhof et al., 2001, Allers and Hoeben, 2010, and Usui and Takeuchi, 2014,⁴ Carattini et al., 2018). While controlling for time-invariant unobservables can be an improvement over previous analyses, DID (fixed effects) methods do not allow the impacts of unobservables on waste generation to vary over time. In fact, although DID allows for different outcome levels between units, it assumes that outcome differences between UPS and non-UPS municipalities do not change over time.

However, in some applications, it can be the case that unobserved determinants of waste generation do not only lead to level differences in waste generation, but have also time-varying effects (as discussed in, e.g., Usui and Takeuchi, 2014). Miranda and Bauer (1996) report that UPS municipalities often engage in citizen education efforts to strengthen environmental awareness, informing households on the excess waste problem with informative campaigns. This seems also the case for the municipality of Trento, where, over the years, citizens and retailers were involved in education and information programs promoting recycling and waste avoidance (ComuneTrento, 2012).⁵

Therefore, waste generation in UPS municipalities may vary over time depending not only on the above-mentioned learning effects but also on the target and the intensity of such awareness-raising programs. Indeed, pro-environmental behavior seems to be affected by the ability of local governments in motivating civic mindedness, i.e., the sense of voluntary public good provision through a shared sense of obligation (Alesina et al., 2017; Besley and Reynal-Querol, 2011). Further, also the availability of time and space may impact waste generation of UPS municipalities differently over time, altering the opportunity costs of waste reduction and recycling.⁶

Hence, if determinants of selection bias have time-varying effects on waste generation, failing to account for them would return biased estimates of UPS' true impacts. In this direction, the SCM allows for time-varying effects of the unobserved determinants by recreating them in the treated municipality with a combination of untreated municipalities. This generalizes the DID estimation, as shown by Gobillon and Magnac (2016).

Finally, the literature provides scant empirical evidence on the behavioral mechanisms behind the reduced-form estimated effects of UPS implementations. Theoretical studies point towards both monetary and non-monetary incentives as important determinants of household waste generation behavior: While recycling waste or avoiding its generation are associated with opportunity costs in terms of time and effort spent, its benefits involve not

⁴Note that Allers and Hoeben (2010) and Usui and Takeuchi (2014) also tested the use of instruments to correct for the endogeneity of the price.

⁵As a result, in some years, the National Institute of Statistics ranked Trento as one of the most environmental-friendly municipalities for, e.g., recycling rates, green spaces, air pollution, energy consumption, and public transport (ISTAT, 2011; La Repubblica, 2006).

⁶For example, time (space) opportunity costs of recycling refer to the time spent (space needed) to sort and transport (store) recycling materials.

only saving eventual fees on waste disposal but also the psychological reward, aka “warm-glow”, associated with environmentally friendly attitudes (see, e.g., Bénabou and Tirole, 2003; Brekke et al., 2004; Jenkins et al., 2003; Kahn, 2007; Morris and Holthausen Jr., 1994; Thøgersen, 2006). Thus, the direction and relative strength of UPSs’ effects likely varies with levels of intrinsic motivation, recycling and avoidance habits as well as socio-economic characteristics such as education, income, and age. On the one hand, pricing waste per unit could lead to increased recycling and waste avoidance. On the other hand, waste generation may stagnate or even increase due to the crowd out of intrinsic motivations (Abbott et al., 2013; Bruvoll and Nyborg, 2003). This paper analyzes these behavioral responses empirically by estimating causal effects of UPS on all waste streams: unsorted, recycling, and total.

3 Methodology

Instead of differencing out fixed unobserved heterogeneity, the synthetic control method, introduced by Abadie and Gardeazabal (2003), attempts to replicate the unobserved heterogeneity by matching the observable characteristics predicting post-treatment outcomes of the treated unit to those of a convex combination of untreated units, denoted as a synthetic control. Abadie et al. (2010) argue, using a linear factor model, that matching on observed confounders and on a long set of pre-treatment outcomes is possible as long as unobserved and observed confounders are also matched. It remains to be verified for any particular application, however, to which extent this matching is achieved. Importantly, the proposed linear factor model allows individual effects (factor loadings) to have time-varying effects (factors) on the outcome, which generalizes the DID approach that restricts the latter to be constant over time, as discussed in Gobillon and Magnac (2016).

In this specific application, for $i = 1, \dots, J + 1$ municipalities and $t = 1, \dots, T$ time periods with $T_0, 1 \leq T_0 < T$, pre-UPS treatment periods, let Y_{it}^N be the waste generation of municipality i in time t in the absence of the UPS policy and Y_{it}^{UPS} be its waste generation if exposed to the policy. Without loss of generality, let the first municipality, $i = 1$, be exposed to the policy intervention while the remaining J municipalities are not. The policy causal effect to be estimated is given by the Treatment effect on the Treated, $TT_t = Y_{1t}^{UPS} - Y_{1t}^N$ for $t > T_0$, and the empirical challenge is to reconstruct the counterfactual Y_{1t}^N , i.e., the waste generation outcome of the treated municipality after the intervention had it not been treated.⁷ Once the counterfactual outcome, \hat{Y}_{1t}^N , is estimated, the average causal effect of the policy, i.e., the Average Treatment effect on

⁷Note that, as usual, it is assumed that the intervention has no anticipation effects on the waste generation before its implementation in any of the $J + 1$ cities, i.e. $Y_{it}^{UPS} = Y_{it}^N, \forall i$ and $t, 1 \leq t \leq T_0$; and that there are no spillovers from the intervention on waste outcomes of untreated municipalities, i.e. $Y_{it}^{UPS} = Y_{it}^N \forall t$ and $i > 1$.

the Treated (ATT), is computed as $\frac{1}{T-T_0} \sum_{t>T_0} (Y_{1t} - \hat{Y}_{1t}^N)$.

The synthetic control method recreates this counterfactual with a convex combination of untreated municipalities, i.e., $\hat{Y}_{1,t>T_0}^N = \sum_{i \neq 1} w_i^* Y_{i,t>T_0}^N$, by choosing a vector of weights $W^* = \{w_i^*\}_{i \neq 1}$ through an optimization program. More specifically, let $\mathbf{H} = (\eta_1, \dots, \eta_{T_0})$ be a set of weights that generates a linear combination of pre-treatment waste generation outcomes $Y_i^H = \sum_{t=1}^{T_0} \eta_t Y_{it}$ of a municipality i , and take M of such combinations. Now, let X_1 be a $(L+M) \times 1$ predictor vector composed by all M selected linear combinations of pre-treatment outcomes and L observed waste generation determinants of the treated municipality, i.e., L covariates. Finally, take X_0 to be a matrix of dimensions $(L+M) \times J$, in which each column is the equivalent of the X_1 vector for an untreated municipality.⁸ Then, for the treated municipality, W is chosen to minimize $\sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)}$ subject to $w_i \geq 0, \forall i$ untreated municipalities, and $\sum_{i \neq 1} w_i = 1$, where V is a $(L+M) \times (L+M)$ diagonal weighting matrix for each predictor variable in the matrices X .

Different specifications of the estimator are possible by changing which M combinations of pre-treatment outcomes are used and how they are weighted, along with the L observed covariates, in the matrix V . In this paper, as in Abadie and Gardeazabal (2003), we select a sole combination of pre-treatment outcomes, i.e. $M = 1$, corresponding to their unweighted average, i.e. $Y_i^H = \frac{1}{T_0} \sum_{t=1}^{T_0} Y_{it}$. The predictors weighting matrix V , in turn, is chosen, also as in Abadie and Gardeazabal (2003) and Abadie et al. (2010), among positive definite and diagonal matrices such that the Mean Squared Error (MSE) of the outcome is minimized for the pre-treatment periods.

Since this comparative case study setting is not suitable for large sample inferential techniques, placebo tests are performed by applying the SCM to each control municipality in the sample. If the estimated treatment effect in the treated municipality is large relative to the one estimated for a control municipality chosen at random, it is possible to conclude that the UPS policy had a significant impact on waste generation in the treated municipality. Placebo tests are also performed in a restricted sample of control units for which the estimated synthetic controls provide a better pre-treatment fit, in the form of a lower MSE than the actual treated unit. Finally, the probability of finding treatment effects as high as in the treatment unit is reported as $\frac{\sum_{j=2}^{J+1} \mathbf{1}(\frac{1}{T-T_0} \sum_{t>T_0} TT_{tj} \geq \frac{1}{T-T_0} \sum_{t>T_0} TT_{t1})}{J+1}$.

Lastly, in order to illustrate the methodological innovation with respect to the previous UPS literature, the SCM results are compared to the ones obtained through the conventional difference-in-differences approach. For this, the same L covariates specified in the matrices X above are used to estimate the ATT. Finally, the bias of the DID estimator is evidenced by showing that the parallel trend assumption fails in the pre-treatment period.

⁸In order to avoid interpolation biases the control group is limited to municipalities with similar observed covariates and pre-treatment outcomes.

4 Background and Data

We analyze the UPS policy implemented in Trento, Italy, in January 2013. Trento’s implementation of UPS consists of adding, on top of the already existing flat fee for MSW collection,⁹ a unit price of € 0.09 per liter of unsorted waste. Therefore, UPS was introduced without changing any other ongoing baseline policy. In particular, recycling waste remains unpriced and collected at the curb. Starting from January 2013, each household was mandated to discard its unsorted waste either in 30 liter distinctively marked waste bags that cost € 2.69 each or, in buildings with a maximum of four flats and single houses, in 120 liter waste bins equipped with a coded microchip that is activated at every emptying of the bin at the price of € 10.8. To avoid free riding problems, every building’s bin is locked. If non-compliant bags are found, the whole building is charged for the offense to guarantee self-enforcing and mutual monitoring among residents (see, e.g., Reschovsky and Stone, 1994, for a discussion).

We collected monthly data on waste generation, Y , between January 2008 and December 2016 for nineteen Italian municipalities in different regions: Trento – the treated city –, Bari, Benevento, Bergamo, Brindisi, Chivasso, Ciriè, Collegno, Milano, Moncalieri, Novara, Pesaro, Pinerolo, Rivoli, Salerno, Settimo Torinese, Taranto, Torino, and Venaria Reale. MSW data for the region of Piedmont, Apulia, and Campania are collected from the respective regional observatory on municipal waste,¹⁰ while MSW data for Trento come from the official website of the municipality¹¹. For the other municipalities, MSW data was provided upon request either by the regional environmental protection agency (ARPA - Pesaro) or by the waste collection company (A2A Bergamo, AMSA Milano, and ASSA Novara).¹²

The outcome variables of interest are the three waste streams measured as log per capita kilograms of unsorted waste ($\ln UW$), recycling waste ($\ln RW$), and total waste ($\ln TW$). The amount of total waste is computed as the sum of recycling and unsorted waste, with recycling being recyclable items (e.g., glass, paper, textiles) that arrive to the recycling facility for treatment, and unsorted waste being the residual fraction that goes to landfill and incineration.¹³ The covariates determining waste generation were chosen by literature review, and represent socio-economic predictors of waste generation and recycling habits.¹⁴ These are: average household size ($hhSize$); log per capita income in

⁹This flat fee is paid by all Italian municipalities, and depends on factors such as the floor area of the house and the number of inhabitants, and not on the actual amount of waste produced.

¹⁰Respectively, www.cittametropolitana.torino.it, www.sit.puglia.it, www.mysir.it.

¹¹www.comune.trento.it.

¹²The control group includes municipalities with available data that approximate Trento’s waste generation determinants including, e.g., curbside recycling.

¹³Note that in the aggregation of recycling amounts we did not correct for the seasonality of sub-streams (e.g., green waste), as this seasonality is common to all cities.

¹⁴See, e.g., Grossmann et al. (1974); Jenkins et al. (2003); Miranda and Bauer (1996); Richardson and Havlicek (1978); Van Houtven and Morris (1999), and Wertz (1976).

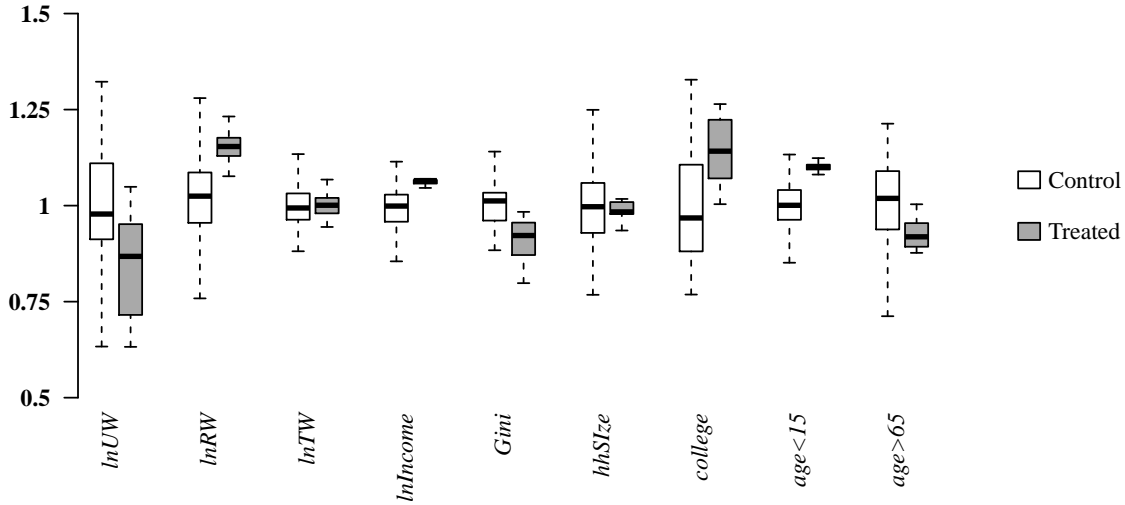
thousand euros (*lnIncome*); income inequality, as measured by the Gini coefficient (*Gini*); educational attainment, measured as the share of the population with a graduate degree or higher (*college*); tourism intensity, measured as the nights spent by tourists divided by the local population (*tourism*); and age structure, decomposed into citizens under 15 (*age<15*) and over 65 years old (*age>65*). This data was obtained from the Italian National Institute of Statistics (Istat), as well as Comuni-Italiani.it and Tuttitalia.it.

Importantly for the SCM, the values of waste generation outcomes and predictors of the treated municipality have to lie within the convex hull spanned by the control municipalities' values, such that a convex combination of control municipalities can fit the actual treated municipality. Figure 1 provides evidence for the validity of this common support assumption by showing boxplots of mean-corrected variables for treated (grey) and control units (white).

In addition, Figure 1 gives indication of possible reasons for policy endogeneity.¹⁵ In terms of waste generation, Trento has on average higher (lower) levels of recycling (unsorted) waste than other control municipalities, and higher total waste, while, in terms of socio-economic characteristics, Trento has, for instance, higher average education levels and per capita income. In this respect, previous studies suggest that low unsorted waste, high recycling as well as high education and income levels are likely associated to high environmental awareness and selection bias due to an environmental activism effect (Dijkgraaf and Gradus, 2004). On the other hand, this could also point to lower opportunity costs of recycling due to, e.g, time and space. Additionally, relatively high total waste may point to selection bias due to excess overall waste and social costs.

¹⁵Note that pre-policy data for treated and control units have similar distributions to the full data plotted in Figure 1.

Figure 1: Data Descriptives: Boxplots of Mean-corrected Variables



5 Results

Using the SCM, we estimate the counterfactual, aka synthetic Trento, as a linear combination of outcomes of the J control municipalities. Thereby, we estimate optimal weights, $\{w_i^*\}_{i \neq 1}$, for each waste stream separately.

Table 1: Synthetic Trento: Estimated Weights for Control Units

Control Units	UW	RW	TW	Control Units	UW	RW	TW
Bari	0	0	0	Novara	0.645	0.559	0.006
Benevento	0.058	0	0.001	Pinerolo	0	0	0
Bergamo	0	0.217	0.536	Pesaro	0.291	0.224	0.073
Brindisi	0	0	0	Rivoli	0	0	0.001
Collegno	0	0	0.002	Salerno	0	0	0
Chivasso	0.001	0	0.363	Settimo Torinese	0	0	0.001
Ciriè	0	0	0.012	Taranto	0	0	0
Milano	0	0	0	Torino	0	0	0.001
Moncalieri	0	0	0.001	Venaria Reale	0.005	0	0.003

Table 1 reports the estimated weights. The control cities of Novara and Pesaro best resemble Trento in terms of unsorted waste (UW); Novara, Pesaro, and Bergamo in terms of recycling (RW); Bergamo and Chivasso in terms of total waste (TW). The SCM provides unbiased counterfactual estimates if predictor variables as well as outcomes of the treated unit are sufficiently close to those of the synthetic unit pre-treatment. Table 2 shows that, compared to the sample average assigning equal weights to all control units,

synthetic Trento is most similar to Trento in terms of outcomes and predictor averages, and it has a lower MSE for each waste type.

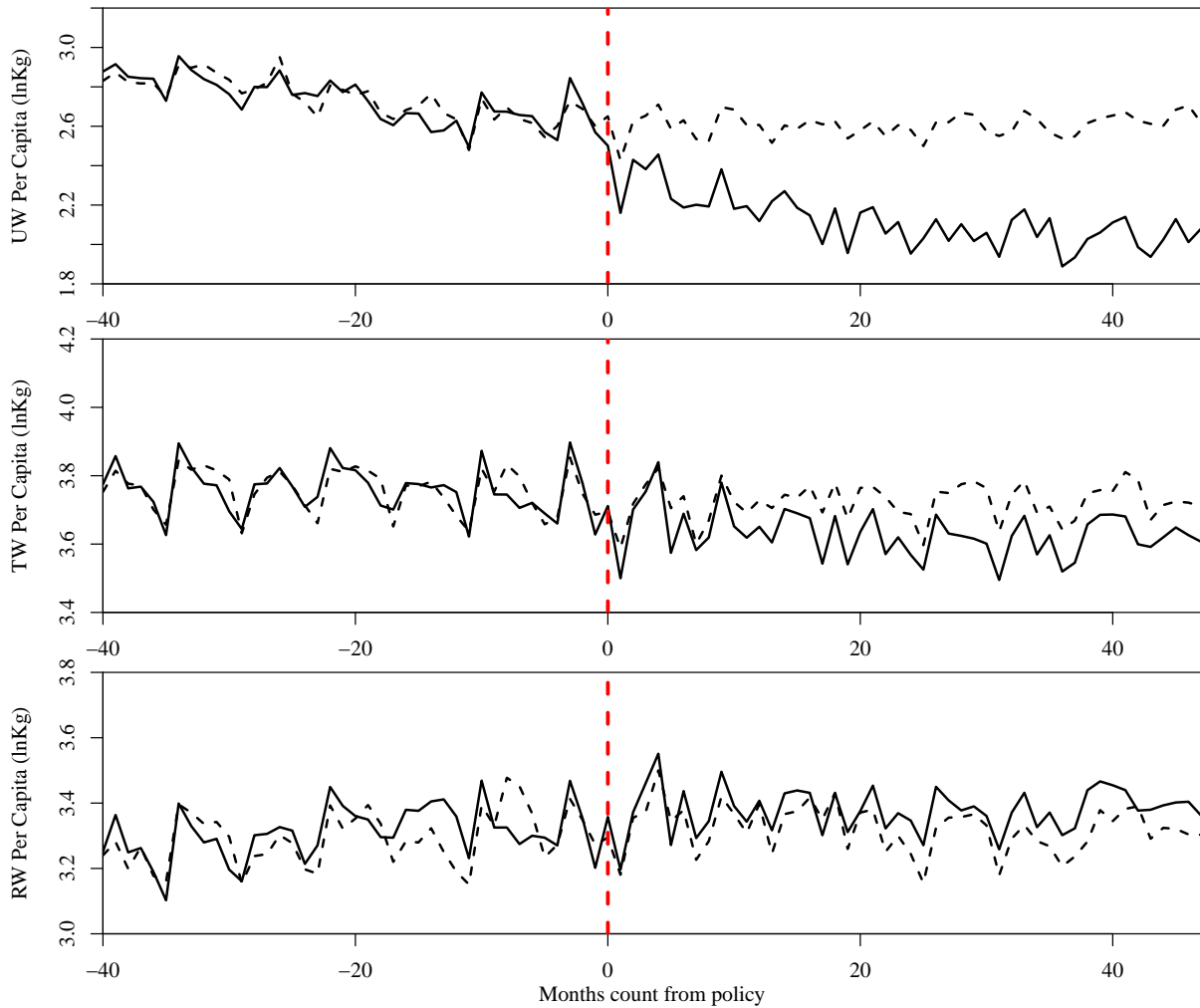
Table 2: Outcome and Predictor Means (2008-2012)

<i>Y: lnUW</i>	Treated	Synthetic	Sample	<i>Y: lnRW</i>	Treated	Synthetic	Sample
<i>lnUW</i>	2.735	2.741	3.049	<i>lnRW</i>	3.310	3.293	2.848
<i>lnRW</i>	3.270	3.257	2.811	<i>lnUW</i>	2.819	2.820	3.088
<i>hhSize</i>	2.892	2.770	2.709	<i>hhSize</i>	2.243	2.198	2.280
<i>lnIncome</i>	2.847	2.734	2.664	<i>lnIncome</i>	2.892	2.840	2.709
<i>Gini</i>	0.274	0.299	0.309	<i>Gini</i>	0.274	0.296	0.309
<i>tourism</i>	4.736	4.245	2.237	<i>tourism</i>	4.736	4.173	2.237
<i>age<15</i>	0.144	0.128	0.129	<i>age<15</i>	0.144	0.129	0.129
<i>age>65</i>	0.197	0.218	0.213	<i>age>65</i>	0.197	0.224	0.213
<i>college</i>	0.117	0.115	0.101	<i>college</i>	0.117	0.117	0.101
MSE		0.029	0.732	MSE		0.036	0.730

<i>Y: lnTW</i>	Treated	Synthetic	Sample
<i>lnTW</i>	3.758	3.759	3.721
<i>hhSize</i>	2.243	2.146	2.280
<i>lnIncome</i>	2.892	2.876	2.709
<i>Gini</i>	0.274	0.296	0.309
<i>tourism</i>	4.736	3.465	2.237
<i>age<15</i>	0.144	0.117	0.129
<i>age>65</i>	0.197	0.205	0.213
<i>college</i>	0.117	0.112	0.101
MSE		0.203	0.785

To show the outcome fit in each time period, Figure 2 plots waste time series for Trento (solid) and synthetic Trento (dotted), and a vertical dotted line that separates pre- from post-treatment periods in time zero (January 2013).

Figure 2: UW, TW, RW Time Series for Trento (solid) and Synthetic Trento (dotted)



The behavior of the treated outcome and its counterfactual are very similar in the pre-treatment period. This suggests that counterfactuals capture treated-specific unobserved heterogeneity in waste generation, which indicates unbiasedness of the synthetic control estimators. As for the post-treatment period, Figure 2 displays the waste paths and gaps between treated and synthetic unit.¹⁶ For UW, we observe a large and persistent divergence between Trento’s UW path and its counterfactual. While the latter remains fairly stable over time, the former drops immediately after policy, and levels off in the long-run. The ATT, computed as the average post-treatment percent gap, amounts to 37.5%. For TW, post-treatment gaps are negative, and of moderate magnitude at first, but then increasing in the longer-run. In particular, the treated outcome decreases to a lower level after about one year from policy implementation, while the counterfactual level stays constant over time. The negative ATT amounts to 8.6%. For RW, we observe mostly positive post-treatment gaps, though of very small magnitude, and increasing

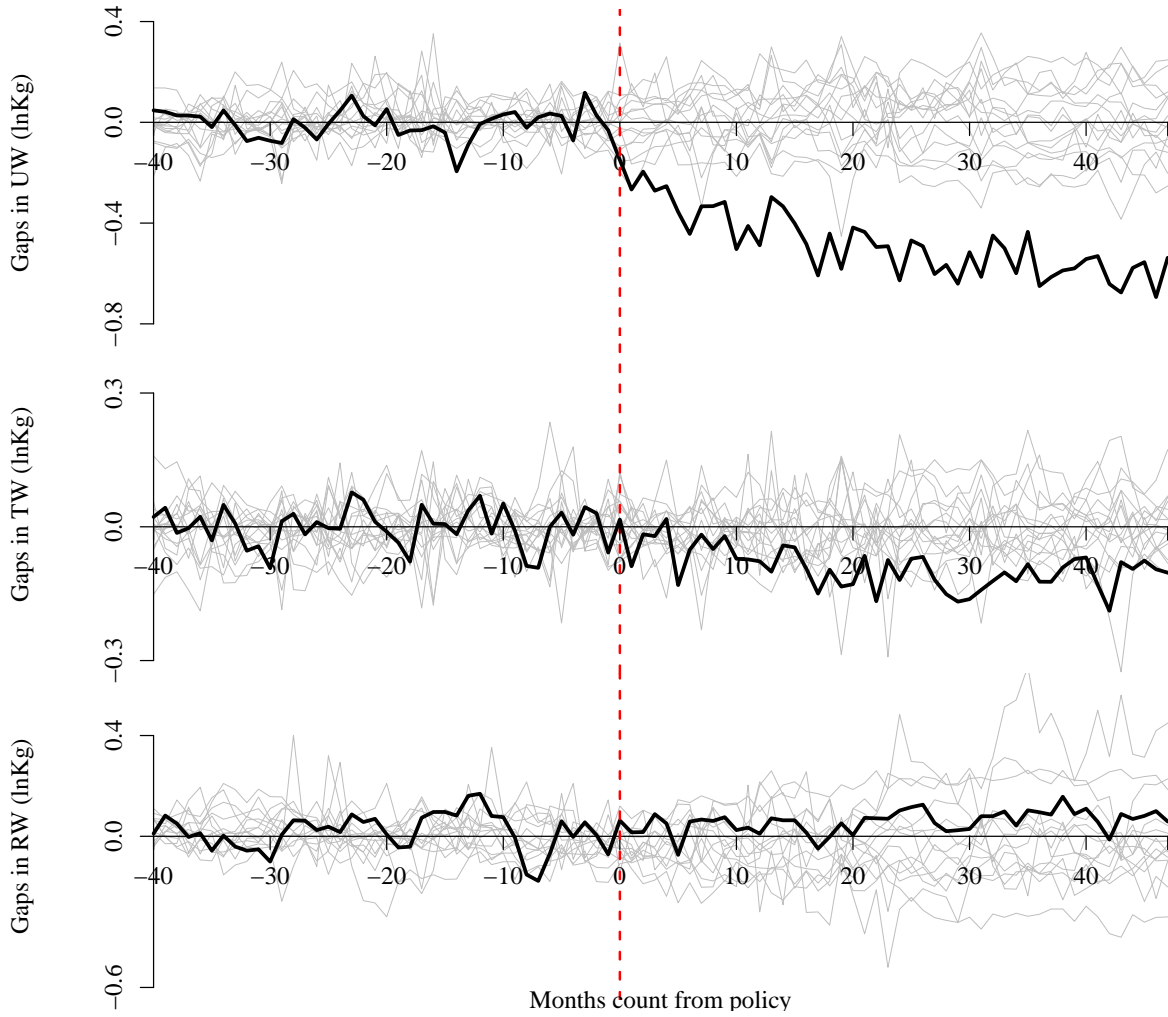
¹⁶The sum of the counterfactual UW and RW should approximately equal the counterfactual TW. This is indeed the case, with a negligible overestimation of TW post-treatment by 0.4% on average.

only after about two years from the policy. Further, both the treated outcome and its counterfactual follow a positive trend, indicating overall increasing trends in recycling habits. The ATT of 6.1% for RW, therefore, may not be such a sizable post-policy gap for the treated unit as it was for the other waste streams.

To assess the statistical significance of the gaps, we perform placebo tests by applying the synthetic control method to each control municipality in the sample. This means to consider control units as treated one at a time, to estimate their respective synthetic control, and to compute the treatment effect given by the post-policy differences between control unit outcomes and their counterfactuals. If the estimated treatment effect for the actually treated unit, Trento, is large relative to the ones estimated for the control municipalities, the significance of the estimated effects is ascertained. Figure 3 plots gaps for the treated unit (black) and placebo control units (grey) against their respective synthetic estimate (x-axis).¹⁷

¹⁷We exclude the treated unit from placebo counterfactual estimation because it self-selected into the policy. Additionally, counterfactuals with a pre-treatment MSE five times higher than the treated unit's MSE are discarded to safeguard against out-of-support estimations (Abadie et al., 2010).

Figure 3: Placebo Tests: Trento (black line) and Control Units (grey lines)



For UW and TW, the estimated ATT for the treated unit is the largest in absolute terms over the other 16 placebo ATT estimates, however, this does not hold for RW. Thereby, placebo tests indicate that the policy has a statistically significant impact on UW and TW, but not on RW.

A second way to account for pre-treatment goodness of fit is to compare the distribution of the ratio of post- and pre-treatment MSE for treated and placebo units (Abadie et al., 2010). Finally, since the MSE criterion overweights large discrepancies, the ATT of each unit is also plotted, for robustness, against its pre-treatment Mean relative Absolute Deviation (MAD), the pre-treatment average percent gap between treated and synthetic outcomes (Seifert and Valente, 2018).

Figure 4: MSE Ratio and ATT-MAD Tests for Trento (black) and Control Units (white)

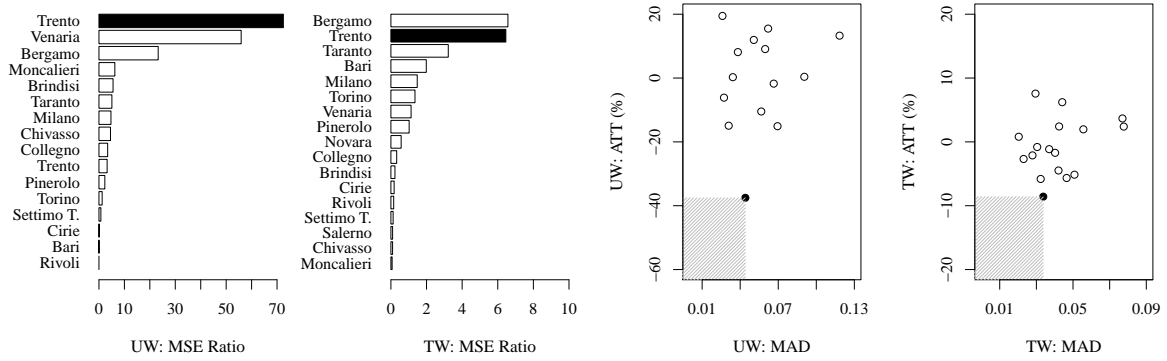


Table 4 plots the MSE ratio test and the ATT-MAD test for UW and TW. MSE ratio tests show that the treated unit has the largest, and second largest MSE ratio for UW and TW, respectively. The policy effect, hence, seems to be significant on UW and not largely on TW. Nevertheless, since no control unit shows a higher ATT as well as a smaller MAD than the treated unit, ATT-MAD tests suggest statistical significant causal effects for both UW and TW.

The statistically significant effect of the policy, stronger on UW, and weaker on TW, may be explained by changes in waste generation behaviors of households due to the policy. As intended by the policy, households decreased priced waste generation. Furthermore, reductions in TW occurring especially in the long-run suggest behavioral adjustments towards waste avoidance. This behavioral response can be also explained by increased reuse opportunities as, e.g., many supermarkets started providing facilities for refilling of beverages and detergents. On the other hand, causal effects on RW are not significant, which might be due to two reasons: First, households may further adjust their purchasing behavior by, e.g., increasing the use of lightweight packaging while also recycling more items. Because our outcome is measured in kilograms, we are not able to observe such an increase in recycling, which would lead to the underestimation (overestimation) of households' substitution (prevention) behaviors. Second, due to decreasing marginal returns to recycling efforts, further increases in recycling are less likely in municipalities with already high recycling levels such as Trento.

In conclusion, the SCM estimates suggest that the policy was effective on the priced waste, and further caused households to avoid waste generation, likely as a consequence of pro-environmental changes in waste generation behavior. However, the policy did not cause households to significantly substitute priced waste with recycling. Concerning negative side effects of the policy, illegal dumping of waste could have happened. Yet, official statistics provided by Trento's municipal police indicate that this was not the case.¹⁸

¹⁸As reported by the National Institute of Statistics and by official statistics of Trento's municipal police, the illegal dumping of MSW follows a rather constant, linearly decreasing path, with five (twelve)

Finally, in order to compare our empirical strategy with the previous literature, we estimate policy effects using the conventional DID approach. The DID estimator (ATT) is defined as the coefficient of the interaction between a dummy for the treated unit ($treated$), i.e., a unit fixed effect equal to one for Trento in all time periods, and a time fixed effect for the post-policy periods ($Post$) equal to one only from 2013 onward. Table 3 reports estimates accounting for possible dependence in the residuals using Driscoll and Kraay (1994) adjustment.¹⁹

Table 3: DID Regression Estimates (2008-2016) for UW, RW and TW (lnKg)

	$lnUW$	$lnRW$	$lnTW$
$lnIncome$	-0.233 (0.308)	-0.443** (0.210)	-0.416*** (0.099)
$Gini$	-7.169*** (1.165)	2.408 (1.749)	-3.638*** (0.194)
$tourism$	0.046*** (0.011)	0.003 (0.006)	0.029*** (0.006)
$age < 15$	0.057*** (0.021)	-0.184*** (0.018)	-0.020** (0.008)
$age > 65$	0.008 (0.019)	0.079*** (0.013)	0.028*** (0.006)
$college$	-0.189*** (0.025)	0.184*** (0.015)	-0.034** (0.016)
$treated$	-0.316*** (0.120)	0.687*** (0.071)	0.055 (0.040)
$post$	0.216* (0.117)	-0.543*** (0.106)	-0.095** (0.046)
ATT	-0.555*** (0.057)	0.012 (0.061)	-0.076*** (0.024)
Constant	9.348*** (0.504)	4.583*** (0.594)	8.413*** (0.114)
Obs.	171	171	171
Time effects	Yes	Yes	Yes
F Statistic	29.90***	12.81***	16.56***
Adjusted R ²	0.478	0.464	0.559
<i>Notes:</i>	***p=.01; **p=.05; *p=.1		

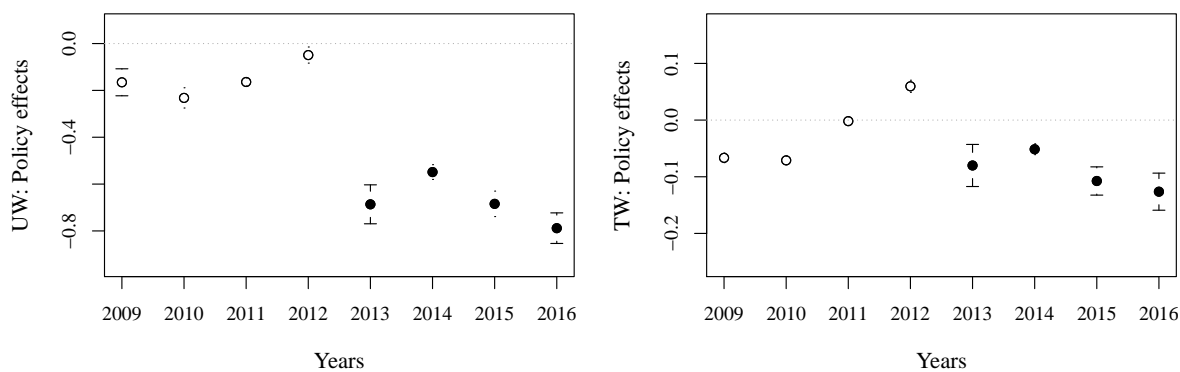
cases on average in the four years after (before) policy (ISTAT, 2018; Police, 2018).

¹⁹Note that while waste outcomes are measured monthly, covariates are measured annually, which is consistent with SCM application. For DID, however, we measure all dependent and independent variables annually. Further, $hhSize$ is excluded to decrease the noise generated by the linear dependence of the covariates (based on diagnostics in Fox and Monette, 1992).

Control variable estimates present overall plausible signs and magnitudes. In particular, we find that higher income municipalities produce less total and recycling waste, with wealthier households potentially having higher opportunity costs of recycling and being overall less incline to public good provision (Magnani, 2000). In addition, the income inequality measure (*Gini*) negatively correlates with unsorted and total waste, possibly because income inequality is increasing in most cities, and at country-level, while waste streams are progressively diminishing. In addition, cities with more nights spent by tourists per inhabitant (*tourism*) produce more UW and TW, likely because tourists have less incentives to avoid waste and recycle. We also find that older cities (*age*>65) recycle more and produce more waste in total, while cities with on average larger families (*age*<15) generates less total waste, likely because they buy in bulk; they also recycle less and produce more unsorted waste as a possible consequence of more stringent time constraints, and higher opportunity costs of recycling. Then, higher-educated cities (*college*) produce less waste and recycle more, suggesting that higher education could correlate with higher environmental awareness levels.

Concerning the estimates of the policy average causal effect, we obtain a statistically significant negative ATT of about 55% for UW, and 7.6% for TW, and, as with SCM estimates, no statistically significant ATT for RW. Potential violation of the DID Parallel Trend Assumption (PTA) for UW and TW is assessed by in-time placebo tests (Autor, 2003). Figure 5 plots the estimated ATTs post-treatment (black) and placebo pre-treatment (white) for each year with their respective confidence interval.

Figure 5: In-time Placebos: Pre- (white) and Post-treatment (black) Policy Effects



For UW and TW, pre-treatment effects are jointly statistically significant at the 1% and 10% level, respectively, indicating a failure of the PTA assumed in the DID regression model. Specifically, for UW, we find statistically significant, negative effects in each pre-policy year. This suggests that UW generation trend in the treated unit prior to the policy was diverging from that of the DID counterfactual, leading to an overestimation of the UPS' treatment effect. Concerning TW, both negative and positive statistically

significant effects are estimated already pre-policy, suggesting diverging trends, and the bias of the DID estimator.

As discussed, DID only allows for individual effects that have time-invariant effects on the outcome, which might not be the case in our setting. If, e.g., environmental-friendly attitudes or time and space availability have evolving effects over time as outlined earlier, then unobserved determinants will have varying effects in time. Controlling for this variation is at the heart of this paper’s motivation to employ the SCM.

6 Conclusions

This paper evaluates the effects of a Unit Pricing System (UPS) implemented in Trento, Italy, in 2013, on the disposal of municipal solid waste. We use a unique data on monthly waste generation for nineteen municipalities in Italy over the period 2008-2016. To account for time-varying effects of unobserved determinants of waste generation, we employ the synthetic control method (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010), in which the counterfactual outcome of the treated municipality is reconstructed with a convex combination of untreated municipalities with similar outcome trajectories but not selecting into treatment.

We find that UPS was effective, and caused changes in household waste generation behavior. In particular, households responded by reducing priced waste, i.e. unsorted waste, by 37.5% on average in the post-treatment period. This partial causal effect seems to have been driven by waste avoidance and, possibly, by increased recycling. In fact, our results show that total waste decreases by 8.6% on average after policy, and recycling increases by 6.1%. The statistical significance of the results, found to be stronger for unsorted waste, and weaker for total waste, may be explained by changes in waste generation behaviors of households after policy. As intended, UPS induced households to decrease the generation of the priced waste and sustain increasing recycling levels. However, the policy did not cause households to significantly substitute priced waste by recycling. In this respect, our results are in line with the literature that finds evidence for sizable unsorted waste reductions and relatively smaller, often insignificant increases of recycling (see, e.g., Allers and Hoeben, 2010; Dahlén and Lagerkvist, 2010; Fullerton and Kinnaman, 2000; Jenkins et al., 2003).

Our findings contribute to the policy debate on policy instruments for municipal waste reduction. We find strong evidence that monetary incentives are effective to induce behavioral changes in household waste generation. The results show that unit pricing systems may help to apply the European Union’s priority of waste reduction (EU, 2008), and to partly internalize negative externalities of waste generation.

Moreover, municipalities might support UPS’ effectiveness in several ways, given that rational households adjust their behavior such that their marginal cost of waste reduction

equals the unit price (Palmer et al., 1997). For example, municipalities may consider decreasing costs of policy compliance by, for instance, facilitating curbside recycling and awareness-raising campaigns that make recycling more appealing, i.e., less time and space consuming. Yet, introducing UPS in municipalities with high recycling may have a limited impact because households sort most of the goods in their bundle already before policy. For this reason, municipalities with lower recycling than Trento may experience stronger UPS' effects on this type of waste.

Policy makers should also take into account municipalities' heterogeneity. In fact, opportunity costs of waste avoidance, recycling, and illegal dumping likely depend on municipal characteristics such as income and education (Callan and Thomas, 2006). For example, higher-income municipalities may value their time more greatly leading to higher opportunity costs of policy compliance. On the other hand, these municipalities may face lower opportunity costs due to higher education and environmental awareness levels. Further, law enforcement, civic mindedness and social norms likely determine opportunity costs of illegal dumping. To increase the efficiency of UPS, municipalities should reduce illegal disposal opportunities, and possibly adjust the unit price accounting for households' opportunity costs. In this respect, future research would benefit from analyzing the optimality of the unit price, and how municipal heterogeneity impacts policy effects.

Moreover, future work could also investigate the amount of social and, especially, external cost savings from waste avoidance. The latter is indeed the most desirable outcome from a social viewpoint because, differently from recycling, it causes no private and external costs of, e.g., collection and treatment. Finally, studies are needed for the estimation of private savings due to waste reductions induced by UPS.

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