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Curbside Collection and Participation in Household Waste Recycling: A Causal Analysis

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Abstract This paper estimates the causal effect of reducing behavioral costs of participation in household waste recycling. We use panel data collected in three city districts in Cologne, Germany (n=1567), in one of which a curbside scheme replaced the traditional bring scheme between observations. Using propensity score matching and triple-differences-estimation we are able to identify the causal effect of curbside collection, its variation between types of recyclables, and its elasticity with regard to the distance to collection containers in the bring scheme condition. We find that a curbside scheme is most effective for plastics, metal cans and packaging but less so for paper. Furthermore, the effect of implementing a curbside scheme is stronger when the initial distance to a collection container has been greater. The results of our causal analysis therefore have important implications for effective and cost-efficient implementation of environmental protection policies.

JEL Classification *C1, C93, Q53*

Keywords *environmental behavior, environmental policy, recycling, natural experiment, propensity score matching, differences in differences, causal analysis*

1 Introduction

Over the past several decades, municipalities have faced enormous growth in solid waste output (World Bank 2012). Beyond more practical problems of local waste management, this development also relates to global threats, i.e. environmental pollution and inefficient use of scarce resources. Thus, a reduction of total waste, either by decreasing total output of packaging materials and other disposable goods or by increasing the level of recycling, is the generally preferred manner of improving waste management (see e.g. van den Bergh, 2008). Two policy instruments have been discussed primarily as promising to redirect waste quantities from landfills or incineration to recycling: pricing systems in which fees depend on the actual amount of waste generated and curbside recycling schemes which reduce the effort required for individual participation.

Pricing systems are essentially either 'upstream' or 'downstream' taxes (Bartelings et al., 2004). The former involve incorporating waste treatment costs into the prices of consumption goods that generate waste. The latter usually take the form of unit-based pricing, where the unit is either weight or, more commonly, volume. Although most economists agree that a pricing system in which total fees are independent of waste output leads inevitably to inefficient regulations of environmental externalities, empirical research has put forth mixed results, particularly on the impact of unit pricing. In general, studies employing community-level data tend to report substantial price incentive effects, whereas studies employing individual household data indicate that such effects are rather small (Hong, 1999). In fact, the precise effect of unit pricing on waste reduction or recycling remains unclear and is possibly close to zero (van den Bergh, 2008). This may particularly be the case for volume-based pricing in cases when households can try to reduce the volume but not the weight of garbage output through compression (e.g. stomping) (Fullerton and Kinnaman, 1996). Moreover, unit pricing policies provide incentives for illegal disposal such as dumping or burning (Fullerton and Kinnaman, 1995).

Curbside recycling programs constitute a different approach to reducing residual waste output. Essentially, they reduce households' recycling costs relative to a drop-off system by making recycling more convenient and less time-consuming. In particular, reducing the costs of storing and transporting recyclables can increase recycling participation (Ando and

Gosselin, 2005). Previous research has analyzed the impact of curbside recycling programs on recycling participation. One of the most extensive studies is that of Jenkins et al. (2003), using U.S. nationwide household-level data and distinguishing five different materials. They conclude that access to curbside recycling significantly raises the percentage recycled for all materials irrespective of whether the program is mandatory or voluntary. Unit pricing is found to have no significant effect in this study. This is consistent with a study by Rechovsky and Stone (1994), who found that curbside recycling is more effective than unit-based pricing. Using data from 20 Korean cities, Hong (1999) found that households respond to a rise in waste collection fees only when accompanied by increased recycling opportunities. Thus, apart from its effect on recycling participation, curbside recycling might also affect solid waste output by mediating waste tax effects on residual waste output.

A possible shortcoming of most previous studies on curbside recycling is that they rely on cross-sectional data. Kinnaman and Fullerton (2000) point to the potential endogeneity of a curbside program implementation and resulting biased estimates. A policy choice is endogenous if the decision of the administration to implement a curbside program is affected by the *status quo ante* rate or the anticipated probability of recycling participation, i.e. due to differential effectiveness of the treatment over groups. They model local governments' decisions about curbside recycling as a function of observable exogenous variables to control for possible endogenous policy choices. Correcting for endogeneity increases the estimated effect of curbside recycling on recycling quantities. Beatty at al. (2007) exploit within-county variation over time in their data to account for endogeneity. They find that the marginal impact of expanding curbside programs upon total recycled quantities is small. This is in part due to changes in curbside access reducing returns to co-existing recycling centers. Thus, their contribution emphasizes that the performance of curbside recycling has always to be evaluated in the light of its alternatives. Similarly, Tsai and Sheu (2009) promote a difference-in-difference approach to identify the effect of unit pricing on garbage reduction and recycling. According to their results, the investigated fee-per-bag program significantly reduced garbage output but had no effect on recycling. However, more than 60% of the garbage reduction was found to be due to increased illegal dumping. A drawback of these studies is that they use community-level data while individual-household-level data would be preferable in that households are the decision-making units recycling policies target (Jenkins et al., 2003). A recent paper by Best and Kneip

(2011) analyzed the mediating role of environmental attitudes on recycling behavior using individual-level panel data and fixed effects regression. They report a positive main effect of curbside collection on reported recycling participation; however, the study does not account for possible bias due to selection into treatment or induced over-reporting.

The present paper strives to identify the causal effect of curbside recycling on households' propensity to recycle by evaluating the implementation of a curbside-recycling program for paper and packaging in Cologne, Germany using individual-level panel data and propensity score matching. We are particularly interested in effect heterogeneity with regard to differences in the arrangement of the pre-treatment bring-in scheme and different types of recyclables: Is the implementation of a curbside scheme effective when the previously used bring-in scheme used a dense grid of collection containers, or is this the case only when replacing a situation with larger distances to the collection containers? And is curbside recycling equally effective for paper, plastics, and packaging? Estimating treatment effects for these different conditions allows us to comment on the efficiency of curbside recycling and may help policy makers in choosing an effective yet cost-efficient solution for the collection of recyclable household waste.

In order to estimate the treatment effect in an unbiased way we use a semi-parametric difference-in-difference-in-differences (DDD) approach. We exploit a natural experiment and complement DDD with propensity score matching to account for self-selection into treatment and control group. Using this approach we are able to account for time-constant unobserved heterogeneity due to self-selection into the treatment groups or policy endogeneity. Additionally, by differencing out changes in recycling of a material unaffected by the treatment (i.e. glass) we are also able to account for possible bias due to time-variant heterogeneity over groups. This may be of particular importance when only a self-reported measure of recycling behavior is available, as in our case, but may also capture other sources of bias.

The paper is structured as follows. In Section 2 we present the conceptual framework for our analyses and derive testable hypotheses. Section 3 describes the data and methods used. This includes a description of the underlying research design and the resulting data and central variables used for analyses, the delineation of the pursued analytic strategy, as well as some notes on how propensity matching was performed. Section 4 starts with a discussion of pre-treatment recycling rates in treatment and control groups. After that, we present our estimations of treatment effects by type of recyclable and contrast results obtained using unmatched vs. matched data as well as employing a DD vs. DDD approach. Finally, we investigate effect heterogeneity with regard to individual pre-treatment conditions (i.e. initial distance to container). We conclude with a summary and discussion of our findings.

2 Theory and hypotheses

2.1 Conceptual framework

Previous research has developed conceptual frameworks to analyze effects of features of curbside recycling on recycling participation. We draw upon a model proposed by Kinnaman and Fullerton (2000) that has since been commonly applied, sometimes with slight modifications (e.g. Beatty et al., 2007; Jenkins et al., 2003; Sidique et al., 2010). According to this class of models, households maximize a utility function over consumption and waste disposal, subject to a budget constraint incorporating prices for different disposal options. This maximization process then yields demand functions d_j for different disposal options j. Essentially, these take costs of recycling (p_r), garbage disposal (p_g), and illegal disposal like dumping or burning (p_b) as well as socio-demographic characteristics (σ), including income, as arguments:

$$d_j = f(p_r, p_g, p_b, \sigma), \tag{1}$$

where j=[r, g, b]. Prices may include fees (or, in the case of illegal disposal, fines) but also time and effort associated with the respective disposal options. Time costs may themselves be a function of σ. Socio-demographic characteristics may also influence other cost aspects of recycling participation, e.g. the volume of recyclables, cost for individual storage, or for transportation. This system of equations can serve as the basis for our empirical analysis. Since the right-hand-side variables in each demand equation are identical, the system can be estimated employing separate equations without introducing bias. However, as policy measures like the introduction of curbside recycling constitute only a quasi-experimental design with non-random assignment of households to the treatment group, the underlying selection process has to be accounted for as well.

Suppose that a community with a drop-off system for recycling in period t=0 introduces curbside recycling in period t=1, which is assumed to reduce recycling costs p_r. Further suppose that the costs for other disposal options as well as socio-demographic characteristics remain stable over time. The (marginal) curbside effect is then given by the difference in demand for recycling between $t=0$ and $t=1$ if the policy measure is exogenous:

$$\Delta d_r \equiv d_r^{t=1} - d_r^{t=0} = f(p_r^{t=1}, p_g, p_b, \sigma) - f(p_r^{t=0}, p_g, p_b, \sigma) \tag{2}$$

Equation (2) reflects the logic of a before-after-estimator. In the presence of time-invariant unobservable confounding factors, a difference-in-differences (DD) approach can be employed. DD accounts for such heterogeneity by comparing a change in d_r between units prone to a change in treatment variable p_r and such which were not. In spite of its advantages, DD could still be prone to endogeneity-bias, as the administration's decision to introduce curbside recycling in particular districts at particular times may well be influenced by the same variables as recycling participation itself. As discussed in more detail below, this bias may be accounted for by using matched data (Heckman et al., 1997; Ravaillion and Chen, 2005).

2.2 Hypotheses

Based on our conceptual framework and the demand function given in (1), we can expect a reduction in the cost of recycling due to a curbside scheme to lead to an increase in recycling participation. The reduction in cost is due to lower effort required in terms of time, storage, and transport for constant monetary cost of recycling. We can therefore formulate

H1: The introduction of curbside recycling increases recycling participation.

This reduction in cost, however, is not necessarily constant over all respondents. Rather, it can be assumed to vary depending on respondent and household characteristics σ as well as on characteristics of the prior bring scheme. Holding σ constant, we can expect the cost reduction to be lower when the grid of collection containers under the bring condition is dense. Therefore,

H2: The lower the distance to collection containers at time t_0, the lower the effect of a curbside scheme.

Finally, the effort of recycling participation varies between kinds of recyclables because of variations in storage and transport costs. Therefore

H3: The effect of curbside recycling differs between plastic and packaging and paper.

3 Data and Methods

3.1 Research design

In most regions of Germany a two- or three-stream curbside system has been used for collecting recyclables since the 1990s: in addition to bins for residual waste, households have bins for paper, as well as other bins – or yellow bags – for packaging materials (mainly plastic, Tetra Paks and metal cans), and sometimes yet others for glass. In other cases, the collection of glass is organized as a drop-off scheme with containers at street corners. By law, the industry is responsible for the collection and recycling of paper, plastics, and glass. The cost of recycling is added to products' prices; the consumer, therefore, pays for the recycling of the packaging materials when buying packaged products – regardless of his/her decision to recycle or not. Due to the upstream waste tax on recyclables, actual *participation* in recycling activities is free of charge. Residual waste, however, is charged with a volume-based downstream tax.

The city of Cologne relied on a drop-off system with drop-off containers at street corners for all kinds of recyclables. In 2006, the waste management authorities commenced a stepwise implementation of curbside collection. Between February 2006 and October 2007, the drop-off scheme for recyclable waste was replaced by a curbside recycling scheme for paper and packaging.[1] In one city district after another, households received blue and yellow bins for the collection of paper and plastic/metal cans free of charge. In one neighborhood, *Lindenthal*, the curbside scheme had already been implemented during a pilot study a few years earlier. This stepwise implementation provided an opportunity to design a field-experimental study with one treatment group and two control groups. In this natural experiment, the change in collection systems can be considered a (quasi-)experimental

[1] The curbside scheme was not used for the collection of glass. Rather, recyclable glass continued to have to be brought to drop-off containers by participants.

treatment to modify the behavioral cost of recycling (p_r). As noted above, there was no change in the collection system for glass bottles.

The inhabitants of the district of *Nippes* served as the experimental group. In this district, the curbside recycling scheme took effect in September/October 2006. Control groups came from two districts not subject to any change in recycling scheme over the relevant period. Inhabitants of Cologne-*Innenstadt* served as the first control group, as curbside pickup had not been introduced in that district until September/October 2007. Cologne-*Lindenthal* served as the second control group; in this district paper and plastic had been picked up at the curbside for some years. Members of the study and control groups were randomly selected from their respective districts and interviewed in a postal survey at two points in time (see below for details). The first wave of interviews was conducted in all districts before the introduction of curbside pickup in *Nippes* (the study group). The second panel wave followed about half a year later but well before the transition to curbside recycling in *Innenstadt*.

3.2 Data and Central variables

All analyses in this paper are based on a two-wave panel postal survey. The participants were randomly selected from the population register of Cologne, distributed equally across the three selected districts: *Nippes*, *Innenstadt* and *Lindenthal*. The survey was designed following Dillman's tailored-design method (Dillman, 2000), using incentives and two follow-up reminders. The first panel wave was conducted during July/August 2006 and yielded a response rate of 64%. The second panel wave followed in May/June 2007 with a retention rate of 83%. Overall, 1567 persons provided sufficient information in both waves of the panel (*Nippes*: 507, *Innenstadt*: 491, *Lindenthal*: 569).

The questionnaire of the first wave comprised questions on socio-demographic individual and household characteristics, a number of questions on environmental attitudes, the location of the collection containers for recyclables, and a detailed account of recycling behavior. For each of the types of recyclable (paper, glass and packaging), the frequency of participation in recycling was to be indicated on a four-point ordinal scale. For the purposes of this paper, recycling participation was dichotomized, with persons declaring that they "always" participated in recycling being coded as 1, the rest as 0. Dichotomization of the variable was necessary as the variable is highly skewed. A reproduction of our analyses with the four-category variable leads to results not substantially different from the results reported here. In the second wave, the measurement of recycling behavior, the location of collection containers, and environmental attitudes was replicated, employing the same questions as in the first wave.

3.3 Analytic strategy

In order to test our hypotheses it is necessary to isolate the effect of p_r on d_r (as in equation 2) or, more precisely, of the availability of curbside recycling *D*. An unbiased identification of the treatment effect requires a variation in *D*, holding p_g, p_b, and σ (as well as other factors affecting p_r apart from D) constant. Formally, the general identification problem can be described by the equation system

$$Y = f(D, X, Z) + U \tag{3a}$$

$$D = g(X, W) + V \,, \tag{3b}$$

where (3a) is the outcome equation for recycling participation *Y* and (3b) gives the treatment assignment equation. Let *X* denote a set of observable factors which may affect *Y* as well as *D*, e.g. socio-economic characteristics like age, education, or income. *Z* and *W* represent potential exogenous factors contributing to either *Y* or *D*. Disregarding such factors will not introduce bias in the estimation of the treatment effect but will usually reduce precision. *U* and *V* reflect unaccountable variation in *Y* and *D* due to unobservable factors. The usual identifying assumption is conditional independence of the error terms, i.e. $U \perp V | X$. As we have variation in *D* over time – induced by the implementation of a curbside scheme – we can use difference-in-difference estimation to considerably relax this assumption. At the same time, we can eliminate spurious effects due to aggregate changes in environmental awareness, large-scale policy changes, etc., as these would equally affect all three Cologne districts. However, the remaining identifying assumption of the DD approach is exogeneity of time-varying idiosyncratic errors. Thus, in the presence of non-parallel time trends for treated and untreated groups, DD will give a biased estimate. In our setting, this will be the case if changes in recycling participation unrelated to changes in the recycling theme are a function of initial conditions that also influenced the likelihood of treatment assignment. Estimates will also be biased if certain groups react differently to the introduction of curbside collection than others and group membership is systematically related to the district of residence – and thus to treatment assignment. To address this serious concern, a combination of DD with propensity score matching methods has been proposed (Heckman et al., 1997; Ravaillion and Chen, 2005). By doing so, the identifying assumption can be further relaxed to exogeneity of time-varying errors conditional on *X* (or, respectively, the propensity of treatment assignment based on *X*; c.f. Smith and Todd, 2005). A further advantage of this approach, e.g. vis-à-vis alternative regression methods, is that it is a largely non-parametric method of controlling for initial heterogeneity, thus avoiding potential bias due to a misspecification of the functional form of *f(.)*.

Following this approach, we adjust study and control groups prior to treatment using propensity score matching (Caliendo and Kopeinig, 2008; Rosenbaum and Rubin, 1983; Rosenbaum and Rubin, 1985). The resulting DD matching algorithm (Gangl, 2006; Heckman et al., 1997) provides a nonparametric estimate of the effect of interest from

$$DD_M = \frac{1}{N_{E_1 \cap S}} \sum_{i \in E_1 \cap S} \left[\Delta Y^1_{i,M,T+1} - \sum_{j \in E_0 \cap S} W_{ij} \Delta Y^0_{j,M,T+1} \right], \tag{4}$$

where

$\Delta Y^d_{i,M,T+1} = Y_{i,M,T+1} - Y_{i,M,T} \mid D = d$

E_d – treatment (d=1) or control (d=0) sample

S – area of common support

$N_{E_1 \cap S}$ – number of observations in treatment group in the area of common support

D – causal factor of interest (curbside introduction)

W_{ij} – kernel weight

and M – type of recyclable (paper, packaging, glass).

Controlling for observable initial states, however, may still not capture all other sources of bias. To account for this possibility, we construct a triple-difference estimator by subtracting changes in participation in the collection of a recyclable *unaffected* by the introduction of a curbside scheme, namely glass. Note that this strategy also accounts for a possible over-reporting of *general* recycling participation as a reaction to treatment, e.g. due to increased awareness, social desirability, etc.

Equation (4) can easily be extended to obtain a DDD based estimation of the treatment effect from

$$DDD_A = \frac{1}{N_{E_1 \cap S}} \sum_{i \in E_1 \cap S} \left[\left(\Delta Y^1_{i,A,T+1} - \Delta Y^1_{i,U,T+1} \right) - \sum_{j \in E_0 \cap S} W_{ij} \left(\Delta Y^0_{j,A,T+1} - \Delta Y^0_{j,U,T+1} \right) \right], \qquad (5)$$

where

A – type of recyclable affected by curbside introduction (paper, packaging)

U – recyclable unaffected by curbside introduction (glass).

By disaggregating recyclables by type and distance to collection containers under the bring scheme condition, we can additionally assess the relative effectiveness of curbside recycling in different pre-treatment settings.

3.4 Propensity score matching

Equations (4) and (5) specify the use of propensity score matching to account for endogeneity and self-selection into the treatment group. We decided to use kernel matching (see Heckman et al., 1998) because of its relatively high efficiency and the possibility of bootstrapping standard errors of the treatment effect (see Abadie and Imbens, 2008). We estimated the propensity score in a multinomial probit model using the Stata ado *psmatch2* and the default bandwidth of 0.06 to perform the matching analysis.[2] The selection model used cohabitation, presence of children in household, number of persons in household, education, labor-force participation, age, gender, income, nationality, migration background, environmental attitudes, and type of dwelling as covariates (refer to table 4 in the appendix for selection model).

As Fig. 1 in the appendix shows, common support of all values of the propensity score and kernel matching leads to a very good adjustment of the propensity score distributions in treatment and control groups. After matching, no statistically significant differences in the covariates of the selection model between the study and control groups remain. The standardized bias is below 2.5 % for all matching variables except part-time employment (4.1 %).

[2] We tested different varieties of propensity score matching (different bandwidth, nearest neighbor, logit selection model instead of probit), and the results are robust against changes of these specifications.

4 Results

In the following sections we present the results of our empirical study. We start with a brief discussion of the pre-treatment setting, then identify the treatment effect and finally evaluate the elasticity of the treatment effect with regard to characteristics of the bring scheme.

4.1 Pre-Treatment-Setting

In 2006, paper and packaging were collected at the curbside in *Lindenthal*, but had to be brought to containers at street corners in *Innenstadt* and *Nippes*. Glass was collected in a bring scheme in all three districts. As can be seen from table 1, recycling rates differ substantially among materials and neighborhoods. Overall recycling participation is relatively low for packaging (ca. 38–62%), as compared to glass (70–78%) and paper (71–86%). For all materials, rates are lowest in *Innenstadt* and highest in *Lindenthal*.

Table 1: Recycling participation T0 ("always recycling")

	Paper	Packaging	Glass	N
C1 (Innenstadt)	.705	.379	.701	491
C2 (Lindenthal)	.859	.624	.784	569
T (Nippes)	.787	.539	.771	507
				1567

We also find that cross-district variation in rates is lowest for glass and substantially higher for packaging. As at T_0 curbside recycling had already been in place in *Lindenthal* for paper and plastic; these results could be regarded as a first indicator for the effectiveness of a curbside scheme: Not only are participation rates highest in the district with a curbside scheme, but the variation in rates by material is also larger when there is a variation in the recycling scheme (as opposed to glass recycling). Clearly, such an inference would be premature, as the results could well be due to endogenous policy or unobserved heterogeneity. Valid identification of the treatment effect of curbside recycling therefore requires that we turn to a discussion of changes over time in relation to a policy change.

4.2 Estimation of the treatment effect

Table 2 presents changes in recycling participation from 2006 to 2007 in the treatment and control groups. As the collection scheme for paper and packaging changed in *Nippes* from a bring to a curbside collection, one would expect an increase particularly in this group.

However, as there were aggregate influences as well (namely the publication of the IPCC report and a corresponding public debate on climate change, see IPCC, 2007), these changes could in theory be due to these influences as well. Such background noise, however, can easily be controlled for by calculating differences-in-differences. In the unmatched sample, positive effects of the implementation of curbside collection remain: participation rates rose by approximately 6 percentage points regarding paper and 19 percentage points for packaging. For glass, a 4 percentage-point increase – significant on the 10% level only – can be observed even though there was no change in the collection scheme.

The results change slightly when we control for policy endogeneity and socio-demographic composition of study and control groups by using propensity score matching. We estimate a DD of 5 percentage points for paper recycling and 20 percentage points for packaging, both statistically significant (using bootstrapped standard errors). The DD estimator for glass remains at 3.5 percentage points, but loses statistical significance.

Table 2: Changes in recycling participation (D and DD)

	Unmatched			Matched			N
	Paper	Packaging	Glass	Paper	Packaging	Glass	
C1 (Innenstadt)	.020	-.012	.026	.036	-.015	.022	491
C2 (Lindenthal)	.019	-.009	-.007	.027	-.022	.002	569
T (Nippes)	.081	.178	.043	.081	.178	.043	507
DD	**.061**	**.188**	**.035**	**.053**	**.196**	**.035**	1567
	(.019)***	(.024)***	(.020)*	(.022)**	(.028)***	(.022)	

* p ≤ 0.1; ** p ≤ 0.05; *** p ≤ 0.01. Standard errors in brackets

As there was no change in the collection system of glass, the remaining DD estimator for glass recycling points to some influences that are clearly not attributable to a cost reduction in recycling participation. We can use this effect to calculate an alternative estimate of the treatment effect, the DDD. After further differencing we obtain a DDD of 0.161 (0.031) for packaging and of 0.018 (0.025) for paper recycling (see also line total in table 3 below). These results indicate that the implementation was far more effective for packaging than for paper. While the average treatment effect on plastic recycling remains substantial and statistically significant, the effect on paper recycling loses statistical significance when estimating DDD.

4.3 Heterogeneity in the treatment effect

The conditions prior to implementation of a curbside scheme, however, were not identical for all members of the treatment group. Rather, some had to bring their recyclables to a distant container while others found a collection container next to their house. Therefore the reduction in behavioral cost was lower for the latter group than for the former. This gives us the opportunity to test the relative effectiveness of curbside recycling with different configurations of the bring scheme for different types of materials empirically.

Table 3 disaggregates the treatment effect of curbside recycling for prior distances to a collection container of 0-100 and more than 100 meters. For paper we find the DD to be statistically significant in the high-distance condition only. Furthermore, the effect reduces to 4.9 percentage points and loses statistical significance when estimating DDDs. Regarding plastic recycling, we find a substantially strong and statistically significant treatment effect under both conditions. We find the effect to be much stronger when the prior distance – and hence behavioral cost – was high (DDD 20 points vs. 10 points). Hence, the effectiveness of the introduction of curbside recycling varies greatly with materials and the *status quo ante*. For paper, a curbside scheme does not seem to offer advantages over a dense grid of collection containers. For the collection of packaging, on the contrary, collection containers at street corners are simply not good enough. Here, a curbside scheme outperforms the bring scheme even under the condition of a dense grid of collection containers.

Table 3: DD and DDD by distance to container (t1), matched data

	Paper		Packaging		Glass
	DD	DDD	DD	DDD	DD
Total	.053**	.018	.196***	.161***	.035
	(.022)	(.025)	(.028)	(.031)	(.022)
Low distance (0-100m)	.022	-.026	.139***	.098**	.041
	(.032)	(.035)	(.038)	(.043)	(.027)
High distance (above 100m)	.075***	.049	.235***	.204***	.031
	(.028)	(.032)	(.035)	(.038)	(.028)

* p ≤ 0.1; ** p ≤ 0.05; *** p ≤ 0.01. Standard errors in brackets. N=1567. Note: for some respondents the distance to collection containers differs between materials.

5 Summary and Discussion

This study examines the effect of implementing a curbside scheme on participation in household waste recycling. Using a quasi-experimental design and a unique individual-level dataset, we are able to estimate the treatment effect of a reduction in required effort, and thus a reduction in behavioral cost, on the probability of recycling activities. Our data additionally allow us to investigate treatment-effect heterogeneity by materials and characteristics of the prior bring scheme.

After controlling for policy endogeneity, self-selection into study and control groups and exogenous aggregate influences, we find an overall increase in recycling participation of 16 percentage points for packaging and a small, insignificant increase for paper. The effect of curbside recycling varies with distance to a collection container under the preceding bring scheme condition: For packaging we can observe a treatment effect of 20 percentage points when the distance was larger than 100m and of 10 percentage points when the distance was lower or equal to 100m. The effect on paper recycling is statistically insignificant under all conditions but, consistent with our expectations and the trend we observed regarding packaging, point estimates are larger under the high-distance condition.

We used a DDD matching strategy to identify the causal effect of implementing a curbside recycling scheme on recycling behavior. The DDD matching estimator is analogous to the standard DDD regression estimator. However, it also accounts for selection into treatment based on observable characteristics without imposing functional restrictions in estimating the conditional expectation of the outcome variable. Our approach thus combines the advantages of differencing and matching methods. By using triple differences, we can additionally account for further unobserved heterogeneity. This may be particularly important as our outcome variable is based on self-reporting rather than objective measures. If treatment leads to over-reporting recycling participation but does so for *all* recyclables, DDD effectively controls for this by ruling out any effect on recycling of materials where recycling costs have not been affected by the treatment under study.

Despite our efforts to identify the causal effect of curbside recycling, potential shortcomings remain and must be addressed. To begin with, the estimation still relies on the assumption of exogeneity of time-varying idiosyncratic errors. Conditional independence of treatment and outcomes is required, ruling out endogenous selection into treatment based on agents' predictions about treatment impact. While triple differences should contribute to minimizing possible bias, they may also produce excessively conservative results. This would be the case if, for example, an increase in participation in glass recycling came along as a by-product of an increased participation in recycling other goods and should thus be considered as due to the introduction of curbside recycling.

Another possible problem with our estimates relates to the distribution of our outcome variable. We consider the binary outcome of recycling participation versus non-participation so that the treatment effect can be interpreted as (additive) marginal effect on the participation rate. Given a pre-treatment participation rate of about 80% with regard to paper, some ceiling effect is likely to occur. Thus, the differences in effect size for paper as compared to packaging could to some extent be a result of differential baseline probabilities of recycling participation.

A related concern pertains to the generalizability of our findings. We have argued that the effect of curbside recycling should vary over types of recyclables as well as over pre-treatment recycling options inasmuch as both affect relative cost savings for recycling. In our case, the implementation of the curbside scheme occurred when a rather well-planned bring scheme had been running for several years. In the presence of treatment-effect heterogeneity with regard to other factors, estimates may have limited external validity. Consequently, the treatment effect could only be interpreted locally, i.e. with regard to the specific characteristics of the population in the treatment group in our analysis.

That said, our results point to some important implications for the implementation of environmental policies. First, we could show that curbside recycling can, in many circumstances, be an effective tool to enhance recycling rates – even when compared to an extended bring scheme. This is especially the case when the effort required for storage and transport of recyclables is substantial, as is the case with packaging (e.g. cans, yoghurt jars, Tetra Paks, plastic wrapping, and boxing). Such materials are quite bulky and therefore require substantial space to store and may additionally lead to nuisances either due to bad smells or, alternatively, to the necessity of frequent transport to the collection container. For paper, which is simple to store and relatively easy to transport, our results indicate that a comfortable bring scheme with a very dense grid of collection containers is likely to be the more efficient policy.

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Appendix

Table 4: Selection models

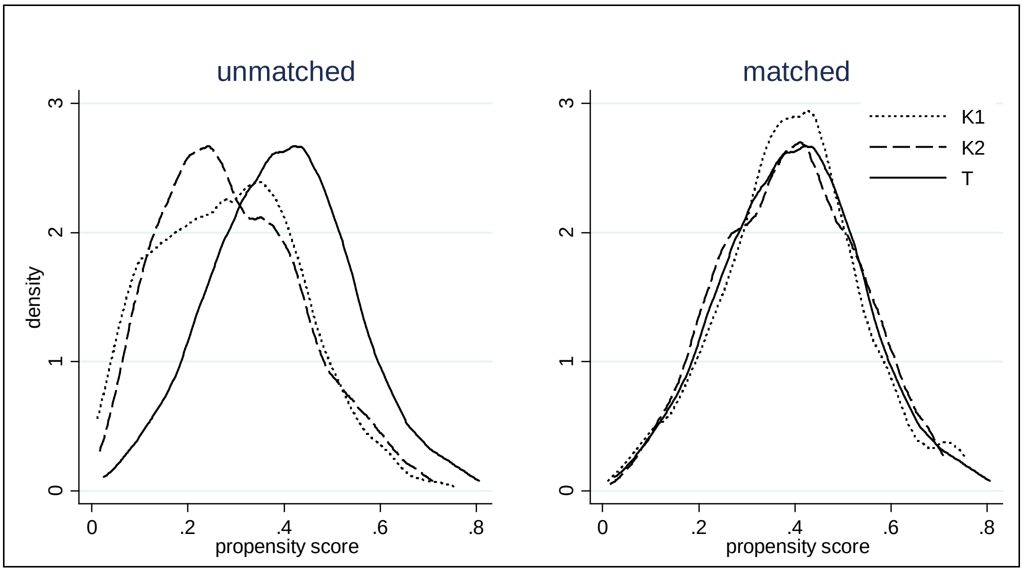
	Innenstadt		Lindenthal	
	b	se	b	se
Intercept	-0.179	(0.653)	1.568	(0.611)
Age (deciles) (ref 18-32)				
32-36	-0.914***	(0.264)	-0.974***	(0.268)
37-40	-1.281***	(0.269)	-1.104***	(0.272)
41-44	-1.275***	(0.269)	-1.177***	(0.269)
45-48	-1.247***	(0.281)	-1.214***	(0.281)
49-53	-1.487***	(0.292)	-1.273***	(0.284)
54-60	-1.133***	(0.286)	-0.755***	(0.280)
61-66	-1.101***	(0.309)	-0.721**	(0.304)
67-73	-1.423***	(0.323)	-0.685**	(0.315)
74-93	-1.340***	(0.338)	-0.356	(0.325)
Missing	-1.275***	(0.295)	-0.767***	(0.288)
Female	0.095	(0.115)	0.228**	(0.111)
Secondary Education (ref low)				
Medium	0.128	(0.172)	0.477***	(0.168)
High	0.374**	(0.183)	0.755***	(0.179)
Missing	0.272	(0.447)	0.900**	(0.425)
University degree	-0.052	(0.148)	0.191	(0.143)
Missing	-0.021	(0.331)	-0.459	(0.355)
Employment (ref unemployed)				
Full time employed	-0.083	(0.157)	-0.068	(0.152)
Part time employed	-0.394**	(0.192)	-0.272	(0.185)
Missing	-0.308	(0.299)	-0.337	(0.289)
Income group (ref no income)				
> 400 €	-0.079	(0.550)	-0.545	(0.590)
400 € - 749 €	-0.130	(0.408)	-0.140	(0.398)
750 € - 999 €	-0.158	(0.345)	-0.734**	(0.351)
1.000 € - 1.249 €	0.381	(0.334)	-0.427	(0.341)
1.250 € - 1.499 €	0.170	(0.299)	-0.124	(0.284)
1.500 € - 1.749 €	0.502	(0.308)	-0.203	(0.303)
1.750 € -2.999 €	0.163	(0.293)	-0.039	(0.275)
2.000 € - 2.249 €	-0.069	(0.305)	-0.327	(0.284)
2.250 € - 2.499 €	0.190	(0.300)	0.014	(0.281)
2.750 € - 3.999 €	0.174	(0.315)	-0.118	(0.293)
3.000 € - 3.249 €	0.424	(0.312)	-0.230	(0.302)
3.250 € - 3.499 €	0.154	(0.329)	-0.138	(0.304)
3.500 € - 3.749 €	0.999***	(0.362)	0.670**	(0.340)
3.750 € - 4.999 €	0.734**	(0.354)	0.311	(0.327)
4.000 € - 4.249 €	0.622*	(0.366)	0.230	(0.334)
4.250 € - 4.499 €	0.698*	(0.396)	0.535	(0.261)
4500 € and more	0.760***	(0.288)	0.517**	(0.261)

Table 4 cont.

Missing	0.198	(0.369)	-0.223	(0.349)
Cohabitation				
Yes	-0.057	(0.155)	0.092	(0.151)
Missing	0.665	(0.570)	0.030	(0.589)
Children in HH				
no	-0.051	(0.139)	0.179	(0.135)
Missing	-0.624	(0.762)	-0.914	(0.712)
Persons in HH	-0.072	(0.076)	0.005	(0.073)
German citizen	-0.695***	(0.272)	-0.323	(0.273)
Migration background (parents)	-0.257	(0.187)	-0.239	(0.185)
Apartments (ref detached)				
2-4 apartments	1.347***	(0.235)	-0.237	(0.180)
5-10 apartments	1.700***	(0.208)	-0.020	(0.147)
11-15 apartments	1.947***	(0.248)	-0.025	(0.205)
16+ apartments	1.712***	(0.258)	-0.041	(0.216)
Specific environmental concern	0.040	(0.099)	-0.171*	(0.095)
General environmental concern	0.027	(0.108)	-0.139	(0.102)

* $p \leq 0.1$; ** $p \leq 0.05$; *** $p \leq 0.01$. N=1567. Coefficients from multinomial probit; standard errors in brackets.

Figure 1: common support and distribution of propensity scores



Note: K1 = Innenstadt; K2 = Lindenthal; T = Nippes