The Effect of Curbside Collection on Packaging Recycling

A Difference-in-Differences Analysis of Swedish Municipalities, 2011-2023

Emma Lundqvist

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Emma Lundqvist

Supervisor:	**Levi Soborowicz, Swedish University of Agricultural Sciences, Department of Economics**
Examiner:	Shon Ferguson, Swedish University of Agricultural Sciences, Department of Economics

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Swedish University of Agricultural Sciences
Faculty of Natural Resources & Agricultural Science
Department of Economics

Abstract

By 2027, all Swedish municipalities must implement curbside collection of packaging recycling. This was decided with hopes to increase collected packaging material, while also decreasing the dilution of the sorted waste. Projections made up until this point show that recycling rate targets of 70% for 2030 in Sweden will not be met. This study investigates the effects from curbside collection on collected packaging and residual waste, and discuss these effects applied to the 2030 target. The research design is a staggered Difference-in-Differences method applied over the years 2011-2023. The Two-Way Fixed Effects model is used as a baseline, while the estimator from Abraham & Sun (2021), which corrects for heterogeneity across time and units is the headline model. Furthermore, the Callaway & Sant'Anna (2021) estimator is used as robustness check, alongside with a pseudo-timing test to control for parallel trends. Data has been collected from AvfallWeb, a website that gathers waste statistics from Swedish municipalities.

The estimation from Abraham & Sun (2021) showed increasing, yet fading effects in packaging waste of 10,06kg/capita/year as a result of curbside collection. Meanwhile, residual waste showed decreasing and persistent effects of 29,2kg/capita/year. Put in regard to the 2030 target, these results are projected to carry Swedish recycling across the 70% mark.

Keywords: curbside collection, packaging waste, residual waste, recycling, Difference-in-Differences

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Abbreviations

Abbreviation	Description
EPR	Extended Producer Responsibility
PRO	Producer Responsibility Organization
PPP	Polluter Pays Principle
LIP	Lättillgängliga insamlingsplatser (Easily Accessed Drop-off Points)

1. Introduction

Packaging waste management represents a major environmental and policy challenge. In 2024, the total volume of waste collected from households and businesses in Sweden amounted to 4.5 million tons, 15% was from packaging waste collected from households (Avfall Sverige, 2024a). Plastic waste is particularly concerning; historically, only 9% of all generated plastic waste has been recycled. A pattern of mismanaged plastics risks causing disruptions to biodiversity, through ingestion by wildlife and marine entanglement. Substitutions to other materials such as paper are not without complications, as paper production from virgin materials causes deforestation (Interreg Baltic Sea Region, 2024).

Because waste generation causes environmental externalities, governments increasingly rely on institutional and economic policy to increase recycling. The Swedish recycling management system depends on multiple actors: municipalities, producers, producer responsibility organizations (PROs), and consumers, which together determine the cost distribution, efficiency of the collection, and material recovery rates (Avfall Sverige, 2024a).

In 2024, Sweden's total recycling of packaging waste reached 76% of all packaging placed on the market (Naturvårdsverket, 2024), surpassing the EU target of 65% by weight set for the end of 2025 (Regulation (EU) 2025/40). Despite this, Sweden is projected to miss the national target of 70% by weight for 2030. The main driver of this possible gap is the presence of unreported producers of packaging waste. Free riding actors that place packaging on the market, without paying producer responsibility fees, nor reporting their waste statistics, causes increases in the true generation of waste. Furthermore, the collection and recycling rates of plastic waste face big challenges. Depending on the efficiency of the improvement measures taken up until 2030, plastic recycling rates are estimated to land between 37-51%, which means that the best-case scenario still fails to reach the goal of 55% in recycled plastic waste (Näringslivets Producentansvar, 2025a).

With enrollment starting January 1st, 2023, the Swedish government implemented an ordinance on extended producer responsibility (EPR) with multiple regulations that redefine the recycling industry. By the start of January 2027, Swedish municipalities must implement curbside collection of recyclable materials such as plastic, paper, metal, and glass as part of the ordinance on extended producer responsibility (Swedish Government 2022). The goal of the packaging ordinance is to reduce packaging, packaging waste, and littering, while also affirming the responsibility of collection and recycling on producers. (Näringslivets Producentansvar, 2025b)

To address these upcoming challenges, this paper evaluates the historic efficiency of curbside collection in Sweden. More specifically, this paper addresses the question:

- Are there effects on outcomes for packaging and residual waste when curbside collection is introduced?

Prior work have identified effects in collected material as a result of curbside collection implementation. Dahlén et al. (2007) showed that the effects on packaging collection roughly doubled with curbside collection compared to drop-off point systems. Furthermore, Sidique et al. (2009), Czajkowski, Hanley & Nyborg (2017) and Takahashi point to the importance of convenience to recycle. The literature on the effects on generation of residual and packaging waste show diverging results, where some literature point to decreases in generated waste (Ek & Miliute-Plepiene 2018; Alacevich et al. 2021; Miliute-Plepiene & Plepys 2015), while others show increasing effects (Maier, Geyer & Steigerwald, 2023)

While studies have been performed on the effects of curbside collection on packaging waste, most studies have been conducted on food waste implementation. Furthermore, the studies performed on packaging waste have mostly pre-dated the modern studies on Difference-in-Differences (DiD) literature (Abraham & Sun, 2021; Callaway & Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2023), and biased, underestimated effects as results of heterogeneity across time and units could consequently have been overlooked.

In contrast, this paper investigates the effects of curbside collection on packaging and residual waste with modern DiD framework (Abraham & Sun, 2021 and Callaway & Sant'Anna, 2021). The found effects from the estimation is discussed in regard to the 2030 projections, which give insight on the expected results of the policy adoption.

The paper is structured as follows; chapter two presents background information on recycling and the Swedish waste management system. The third chapter touches relevant theory, and the fourth chapter describes the previous literature in the field. Chapter five presents the data used in the analysis while chapter six presents the applied methodology. The results obtained are presented in chapter seven, followed by a discussion in chapter eight. Chapter nine gives concluding remarks.

2. Background

European and Swedish waste management prioritizes the prevention of waste generation in their waste hierarchy, followed by reuse, material and biological recycling, other recycling, and disposal. The market consists of multiple actors: municipalities, producers, Producer Responsibility Organizations (PRO's), and consumers (Avfall Sverige, 2024a).

In 1994, a new ordinance establishing the principle on extended producer responsibility (EPR) was enacted, transferring the previous responsibility of collection and recycling of packaging waste and newspapers from municipalities to producers. Producers were responsible for providing households with appropriate collection systems for waste management, as well as meeting government-set targets for collection, and material and energy recovery for specific recyclables (SFS 1994:1235, 1994). This is in accordance with the Polluter Pays Principle (PPP) upon which the EPR system relies. To meet the obligations, the producers organized Producer Responsibility Organizations (PRO's), who managed their packaging waste. Today there are two approved PRO's, Näringslivets Producentansvarsorganisation (previously called Repa-registret and FTI AB) and TMR AB.[1]

From 1994 and onward, the main collection system has been drop-off points (LIP) where large containers are distributed across municipalities. In municipalities that utilize the drop-off point system, households collect their packaging waste at home and drive to recycling stations to sort out the material. Bulky waste is dropped off at larger recycling centers. While residual waste has long been collected at home under municipal responsibility, it was decided that food waste was to be mandatorily managed through curbside collection by the start of 2024 (Avfall Sverige, 2026a).

The Swedish waste management industry evolved over the years, the latest issued ordinance on EPR (SFS 2022:1274, 2022) brought reformative provisions. Firstly, the management of waste collection was transferred from producers to municipalities. The collection of packaging waste from the recycling stations was, up until 2024, under the liability of the producers, which was organized by PRO's. After this shift, the municipalities are in control of managing the waste at the recycling stations. Producers continue to administer the material

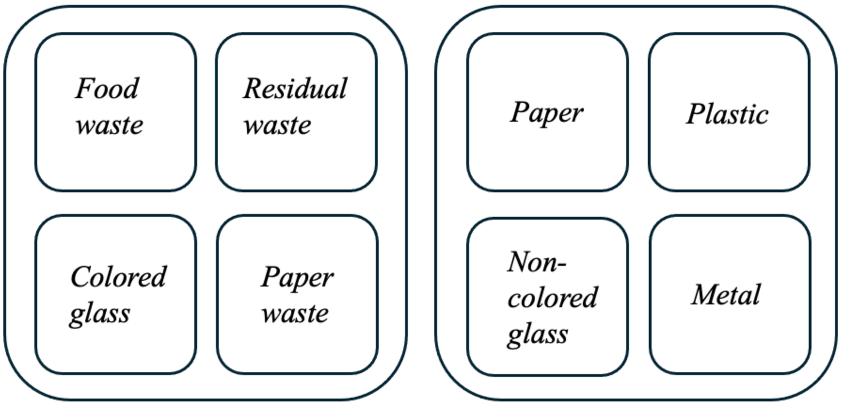
[1] Information from the Swedish Environmental Protection Agency (Naturvårdsverket) website, [Kommuners insamling av förpackningsavfall], available at https://www.naturvardsverket.se/vagledning-och-stod/producentansvar/producentansvar-for-forpackningar/kommuners-insamling-av-forpackningsavfall-fran-hushall-och-vissa-verksamheter/, accessed at 2026-05-16.

recycling of packaging while bearing the costs of the packaging waste recycling system, in accordance with the PPP (Avfall Sverige, 2026b).

Secondly, the Ordinance on Producer Responsibility for Packaging (SFS 2022:1274) decided that all Swedish municipalities will implement curbside collection for packaging waste by January 1st, 2027. This means that paper, plastic, metal, and glass will be collected by the property from which it has been produced, e.g. by the plot boundary or apartment building property. Since curbside collection for residual and food waste is already in place, the 2027 reform means that all day-to-day household waste - residual, food, and packaging - will be collected at the property, while bulky waste continues to be dropped off at recycling centers. So far, 100 municipalities have adopted curbside collection, which means that 190 municipalities are yet to implement.

Figure 1 depicts the two compartment bins that are placed by the plot boundary with curbside collection. The waste bins have eight separate sections; paper packaging, plastic packaging, metal packaging, colored glass, non-colored glass, paper waste, food waste, and residual waste.

Figure 1. Example of structure for collection bins



The reform in the Swedish waste collection was implemented partly as a result of insufficient recycling rates. SFS 2022:1274 establishes that the total material recycling rate for packaging waste should be at least 65% per year up to 2029, rising to 70% from 2030. Material-specific targets are stricter: 85% per year for paper and cardboard, 90% for glass, and 50% for plastic, rising to 55% from 2029 (Avfall Sverige, 2024b).

While paper (99%) and glass (98%) greatly exceed these targets, the recycling rate for plastic packaging waste was measured at 31%. However, these statistics are based on reports from producers and other actors in the collection and recycling system. Through waste composition analyses, it stands clear that a lot of packaging that is placed on the market is not reported as such. One reason behind the underestimation of packaging waste is so called free riders, i.e. producers fail to meet their producer responsibility by not reporting the correct amount of packaging that they produce. When the data is sharpened using the waste composition analyses, glass drops from 98% to 83%, paper from 99% to 82%, and plastic from 31% to 29%. The total recycling rate decreases from 76% to 66% (Naturvårdsverket, 2025).

3. Theory

Consumers allocate time and effort across a set of activities to maximize their utility. A consumer will choose the option that yields the most utility. When households consume, waste is created, and the consumer needs to decide whether to recycle or not to recycle that waste. A consumer would choose to recycle only if the utility derived from recycling exceeds the utility of disposing of waste without sorting. The evaluation of this decision includes the efforts of recycling, such as learning correct sorting practices and transporting waste to a recycling station. The policy of mandatory curbside collection tries to reduce the disutility of recycling by minimizing these efforts.

This can be defined as transaction costs, which originate from Coase (1937, p 390), who argued that economic exchanges bring costs such as information search, negotiating agreements, and monitoring outcomes. In terms of the recycling decision, transaction costs are the costs of seeking information regarding the proper sorting of the waste material, storing recyclable materials at home, transporting sorted waste to recycling stations, and disposing of it in their appropriate bins.

Furthermore, the household needs to consider the opportunity costs of spending time recycling. An opportunity cost is the value a consumer gives up when choosing one alternative over another. For individuals who could otherwise be working, the wage rate serves as a useful proxy for the opportunity cost of their time (Varian 2014, p 174). However, for individuals who recycle during their leisure time, the wage rate can be seen as the lower bound of their opportunity cost. This holds because the individual already has revealed that they value leisure time at least as much as their wage rate. Households have a limited number of hours in a day that they can choose to spend between working, leisure and unpaid activities such as recycling. The higher the value that the individual places on their time, regardless of whether this is measured in time spent working or leisure, the greater the opportunity cost of spending that time on recycling.

However, Berglund (2006) argues that people may derive utility through "warm glow" effects to recycling, where individuals benefit from contributing to environmental activities. This would partially decrease the cost of time spent recycling. According to Berglund, the household's willingness to pay (WTP) to avoid an hour of recycling is lower than their hourly wage rate. This means that the opportunity cost to recycle is the income net of the benefit from warm glow. If the sorting costs are reduced by the introduction of curbside collection, then households that have a lower intrinsic motivation to sort will show higher behavioral responses than households that are driven by warm glow-effects.

Curbside collection reduces several aspects of transaction and opportunity costs (Jenkins et al., 2003). Transaction costs are lowered in terms of less information search on appropriate disposal, no demand for storage between trips to drop-off points as well as no need for waste transportation. Moreover, curbside collection reduces opportunity costs in terms of the time spent on recycling. As a result, it is possible to assume that the introduction of curbside collection increases the share of the packaging that is correctly sorted while simultaneously increasing the net utility of recycling.

Consequently, the analysis in this paper is divided into the following testable hypotheses:

H1: Curbside collection increases packaging recycling

H2: Curbside collection decreases residual waste

4. Related literature

Dahlén et al. (2007) compares different collection systems for sorted households in Sweden and find that when introducing separate collection of biodegradables, the overall sorting of dry recyclables increased. They present one possible explanation to this, where the separate handling of biodegradables eases the sorting of dry recyclables. They also saw that municipalities with curbside collection sorted out approximately twice the amount of packaging per capita per year compared to municipalities with drop-off systems. Dahlén et al. (2009) found that there was no correlation between larger amounts of separated dry recyclables and smaller amounts of residual waste generation. Conversely, they found a small effect on the opposite trend, i.e. the more waste, the more dry recyclables that were left off at drop-off points. They also found that the more accessible recycling centers were, the more waste per capita was delivered.

Sidique et al. (2009) study the profile of people who use drop-off points when recycling and analyze the factors that influence their usage of the sites. They find that recyclers are more prone to recycling frequently if the distance between home and drop-off points is shorter. They also find that households tend to use drop-off sites more often if they perceive recycling as a convenient activity and are familiar with the recycling facilities. Furthermore, Czajkowski, Hanley & Nyborg (2017) find that households prefer to sort their waste at home rather than in central sorting facilities. They found that this preference was motivated by a belief that sorting waste at home led to more thorough results. Moreover, the focus on providing information and improving facilities to make sorting and discharging easier in the Augustenborg district in Sweden resulted in a recycling rate of 70% (Takahashi, 2020).

Ek & Miliute-Plepiene (2018) investigate whether there are behavioral spillover effects from food waste collection and find that there is a positive spillover of about 5-10% in increased packaging collection, which increases gradually over time. They argue that a policy that encourages recycling one waste stream may affect their willingness to recycle other waste streams as well. Ek & Miliute-Plepiene conclude that household residual waste decreases with the introduction of food waste collection, thus any increases in collected packaging waste are not due to increased household consumption. This implies that any increases in packaging collection are explained by households sorting more, where packaging waste is diverted from residual streams. A similar study looked into the behavioral spillover effects from implementation of food waste curbside collection on residual waste generation (Alacevich et al., 2021). Comparably to Ek & Miliute-Plepiene, they find that food waste collection reduced residual waste by up to 9%. However, these effects fade after five to eight months. Further support to these results is given by Miliute-Plepiene & Plepys (2015), that conducted a survey with 117 residents in Vellinge in Swede, where 47% of recipients reported that the introduction of separate food waste sorting improved their recycling habits for packaging. They too find that the introduced food waste collection decreases total waste generation (by about 10-20%), and improved sorting rates for packaging.

Maier, Geyer & Steigerwald (2023) study the effects of household consumption with the introduction of curbside collection in North Carolina between 1999 and 2019. In contrast to the findings in Ek & Miliute-Plepiene (2018) and Alacevich et al (2021), they find that consumption increases by 6-10%, proving rebound effects to curbside collection.

Jenkings et al. (2003) showed that there are no statistically significant effects on recycling when making it mandatory, i.e. forcing recycling upon households have no clear effect on recycling rates. Similar results are found in Kinnaman & Fullerton (2000), where communities with mandated recycling do not significantly increase recycling rates. More findings from Czajkowski, Hanley & Nyborg (2017) showed that households have an intrinsic motivation to recycle, 84% of their recipients preferred to sort waste into two categories, rather than not sorting at all. 70% of the recipients rathered sorting into five categories in contrast to not sorting at all.

5. Data

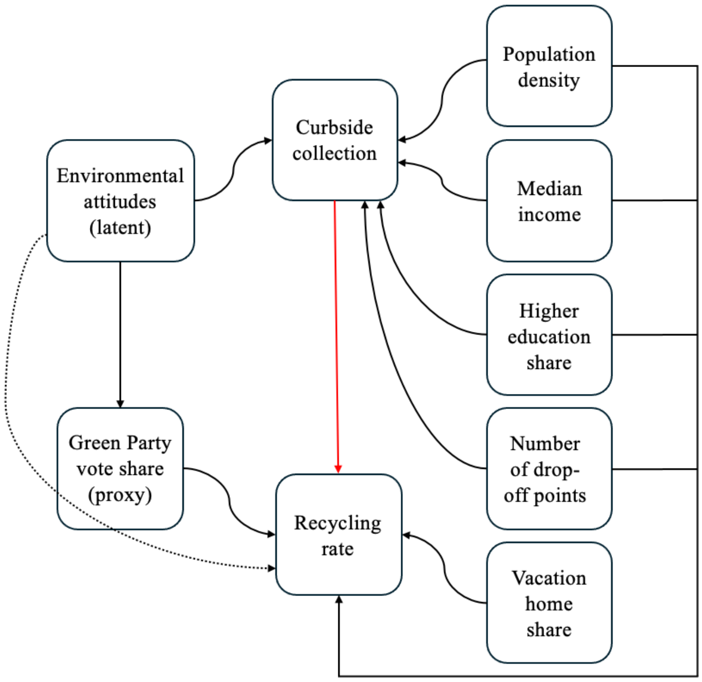
Sweden consists of 290 municipalities, 270 are included in this data set, a full list of the included municipalities can be found in Tables A1, A2, A3 and A6 in the appendix. The data has been gathered from Statistics Sweden and AvfallWeb, a website that collects data on Swedish municipal waste management governed by Sverige Avfall. The data set runs between the years 2011-2023.

The number of treated municipalities between the years 2011-2023 is 55. The control group consists of 190 untreated units, 26 already treated units before 2011 and six units that adopted policy after 2023, which sums to 215 control units. The data consists of 337 time by unit observations in the treatment group, and 3137 time by unit observations in the control group.

The policy adoption is the independent variable; a binary dummy variable equal to 1 when curbside collection is adopted. The dependent variables vary between hypotheses. For testing H1; curbside collection increases packaging recycling, the dependent variables are total collected recyclable packaging, paper-, plastic-, glass-, and metal packaging. For testing H2; diversion effects from residuals, the dependent variable is residual waste.

Furthermore, six control variables are included in the regressions; population density, vacation home share, share with higher education, median income, the vote share for the green party Miljöpartiet2, and the number of drop-off points (LIP) per municipality. Each covariate and their relations to the policy and outcome variable is depicted in the directed acyclic graph (DAG) below.

Figure 2. Directed acyclic graph for covariate variables



While Kinnaman and Fullerton (2000) find a significant negative effect for population density on recycling rates per person, Abbott et al. (2011) finds a significant positive effect for the same coefficient. The population density matters in terms of recycling rates as a low density in a municipality could influence the amount of, and distance to drop-off points. The distance to the nearest drop-off point could influence the rate in which people choose to recycle as a longer distance implies a higher transaction cost. More educated populations have been found to exhibit higher recycling rates, which could be attributed to a better understanding of environmentally friendly activities and greater environmental concern (Kinnaman & Fullerton, 2000). Furthermore, a higher income is often related to more consumption and, by extension, more waste generation. Sidique, Joshi and Lupi (2010) argues that higher income leads to lower recycling rates both in terms of a higher generation of waste when households consume more, but also because a higher income yields a higher opportunity cost to allocating time to recycling. Conversely, a low income is related to a higher rate of unemployment, part-time working or retirement, all situations where it is easier to allocate time to recycle in comparison to full-time workers.

The vote share for Miljöpartiet has been shown to be a statistically significant predictor of recycling rates, suggesting that municipalities with stronger green political preferences exhibit higher rates in recycling (Hage, Söderholm, and Berglund, 2009). However, the vote share is only evidence of election results, not necessarily an indicator of how the population behaves.

The share of vacation homes per municipality is included to account for the possible seasonal variation in waste generation, as vacant dwellings do not produce waste. Municipalities with higher vacation home shares could therefore show systematically lower rates of recycling rates due to lower waste generation. Lastly, the number of drop-off points (LIP) per municipality is included to capture the pre-existing recycling infrastructure as many drop-off points in a municipality lowers the transaction cost of recycling. Such a municipality could then show a smaller increase in collected recyclables by the introduction of curbside collection than a municipality with few drop-off points, as the recycling infrastructure already is accessible. Municipalities with fewer drop-off points may face stronger pressure to adopt curbside collection, as residents have more limited access to recycling infrastructure - potentially influencing the timing of curbside collection adoption and thus the selection into treatment and control groups. However, as shown in Table 3, the pre-treatment means for the number of drop-off points are nearly identical across groups, with treated municipalities averaging 20.2 and control municipalities averaging 20.0, suggesting the risk for selection bias is insignificant.

Table 1. Descriptive statistics

Variable	Mean	Std. Dev.	Min	Max
Total collected recyclable packaging	1 635,5	3 613,8	40	55 980
Paper	555,2	1 026,7	17,3	14 819
Plastic	262,8	485,4	2	7 621
Metal	63,3	114	0,0	1 605
Total glass	765,6	2 136,8	12,2	33 635
Paper waste (returpapper)	828,1	1 690,2	0,0	33 622
Food waste	1 592,2	2 846,6	0,0	40 711
Residual waste	6 613,3	16 357,9	28	229 348
Residents	34 405,2	73 848,8	2 348	988 943
Population density (residents/km²)	159,2	576,4	0,2	6 441,5
Vacation home share	0,4	0,1	0,0	1
Share with higher education	0,3	0,1	0,2	0,8
Median income (tkr)	316,7	42,9	222,3	542,4
Miljöpartiet vote share (%)	4,3	2,6	0,0	16,6
Number of drop-off points (LIP)	18,8	28,5	1,6	339

Note: Summary statistics for 270 Swedish municipalities over the years 2011-2023 (N = 3,302). The waste variables are measured in tonnes per municipality per year, and population density is measured as residents per square kilometer. Vacation home share and share with higher education is measured as fractions ranging between 0 and 1. Median income is measured in thousands of SEK (kr). The share of votes for Miljöpartiet is denoted in per cent. The total number of LIP means the total number of recycling stations and recycling centrals.

Table 2. Treatment timing

Year of adoption	New adoptions	Cumulative total	Cumulative share (%)
2012	1	1	0,4
2013	3	4	1,8
2014	7	11	4,8
2015	2	13	5,7
2016	4	17	7,5
2017	5	22	9,6
2018	4	26	11,4
2019	12	38	16,7
2020	6	44	19,3
2021	9	53	23,2
2023	2	55	24,1

Note: The cumulative share is a percentage of the cumulative number of municipalities with curbside collection and the total number of municipalities in the data set (270).

Table 3. Mean values for key variables in pre-treatment years

Variable	Control (mean)	Treated (mean)
Total collected recyclable packaging	1,545.1	1,541.1
Paper	515.0	534.6
Plastic	242.0	225.7
Metal	58.1	63.1
Total glass	741.3	720.0
Paper waste (returpapper)	790.5	1,089.0
Residual waste	6,205.6	8,233.4
Food waste	1,445.2	2,020.3
Residents	32,475.4	37,020.1
Population density (residents/km²)	163.7	125.6
Vacation home share	0.4	0.4
Share with higher education	0.3	0.3
Median income (tkr)	316.7	298.1
Miljöpartiet vote share (%)	4.1	5.0
Number of drop-off points (LIP)	18.5	19.7

Note: The means are calculated only on pre-treatment years, i.e. if a treated municipality introduced curbside collection in 2018 the mean is based on the years 2011-2017.

5.1 Demarcations

Due to lack of observations in the surrounding years, the data is restricted between 2011-2023. Table A5 in the appendix depicts the share of missing observations for each year. As seen in Table A4 in the appendix, there are 20 municipalities that adopted curbside collection before 2011 and after 2023. Due to the low share of observations these years, the parallel trends' assumption is untestable, and they have been excluded from the analysis.

Sweden consists of 290 municipalities, whereof 42 of them are jointly administered by waste management companies, see Table A6 in the appendix. This means that multiple municipalities are collectively managed through one company rather than the municipality itself organizing their own waste management. Because of this, their waste statistics are reported as one merged observation, rather than separated between municipalities. This causes unmatched covariate observations, as the covariates are separated by municipalities and not by the jointly administered waste management units. To include these units in the regressions, the waste statistics have been constructed into weighted averages based on the population share of the respective municipality.

Multiple-family housing is not included in the data, as the market structure is different in comparison to one-or two-family housing and vacation homes, where the municipality oversees curbside collection. Up until now, property owners of multiple-family housings have been free to contact waste contractors for curbside collection setups, resulting in a non-unison implementation. Because of this market structure, there is no available data that oversees the curbside collection adoption time for multi-family housing.

6. Methodology

6.1 Empirical strategy – the canonical 2x2 DiD

When comparing a simple before/after outcome without a control group, one can never be certain that the change in outcomes was not due to something other than the treatment. The difference-in-differences (DiD) framework addresses this problem by combining differences across time and across groups. The simple 2x2 canonical DiD setup includes two groups: treated and untreated, and two periods: before and after treatment. Given that $D_i = 1$ denotes the treated group and $t = 1$ the post-treatment period, the observed outcome will be:

$$Y_{it} = Y_{0it} + D_{it}(Y_{1it} - Y_{0it})$$

The average treatment effect (ATT) is then calculated as:

$$ATT = E[Y_{0i} - Y_{1i}|D_i = 1]$$

As the counterfactual Y_{0i} for treated groups, i.e. what they would have experienced without treatment, is never observed; the DiD setup uses the control group as an approximation of the potential outcome of the treated group without treatment. The key identifying assumption of the canonical DiD setup is the parallel trends assumption. This tells us that in the absence of treatment, the two groups would move in parallelism. The untreated group outcome can be modeled as:

$$Y_{it}^0 = \alpha + \delta_t + \gamma \cdot 1(\alpha_i = 1) + \varepsilon_{it}$$

Where δ_t is the time effect, γ is the group effect and ε the error term. Without treatment, the average change in outcomes for the treated group would equal the average change in outcomes for the untreated group. If we were to simply compare the change in outcomes between the two groups before and after treatment, the analysis would be contaminated by selection bias. Their relative results could differ due to reasons external to the treatment (Angrist & Pischke, 2008).

When the adoption timing is different across treatment groups, the simple 2x2 canonical DiD setup is no longer appropriate; the analysis demands a staggered DiD setup. The next best method, the standard two-way fixed effects (TWFE) estimator can produce biased estimates under the staggered adoption timing as a result of contaminated comparison groups (Goodman-Bacon, 2021), which motivates the use of alternative estimators that are better designed for this setting (Abraham & Sun, 2021; Callaway & Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2023).

6.2 Identification assumptions

The key identifying assumptions that need to be met to guarantee causality of the results are parallel trends, random assignment, no anticipation and SUTVA.

The parallel trends assumption, which is the primary identifying assumption in a canonical DiD, introduces the idea that in the absence of treatment the control and treated group would follow the same trend over time. This implies that, without treatment, the amount of sorted recyclables would be the same in the treated and control groups over time. (Angrist & Pischke, 2009) However, in a staggered treatment setup, it is not possible to test the parallel trends assumption by a visual comparison of pre-treatment trends. Instead, it is tested using an event study plot. An event study plot names the adoption year for each unit as “year 0” and the three years before/after the adoption year as “years -3, -2-1,”/” years +1, +2, +3”. In the before treatment years, it is observed whether the control and treated groups were already behaving differently before the introduction of curbside collection. The parallel trends assumption is validated if this sits around 0 (Abraham & Sun, 2021).

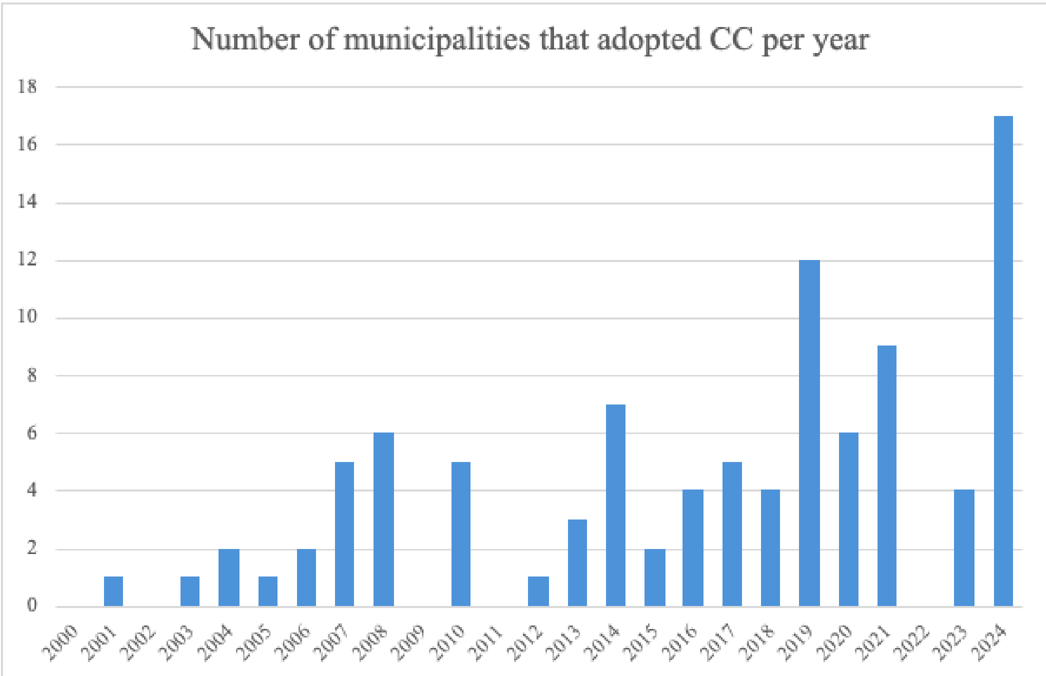
Furthermore, the assumption of random assignment, i.e. when the only systematic difference between the treatment and control group, is the treatment itself, and not some external effect that affects the assignment of individuals to treatment/control group. However, the DiD setup relaxes this assumption as it allows units to differ in time-invariant ways while it requires parallel trends to hold. Reasonably, municipalities do not adopt curbside collection randomly, but the timing for adoption is likely related to e.g. demographics, where more wealthy, educated or urban municipalities could be driving incentives for earlier adoption. Similarly, municipalities with larger budgets might implement curbside collection sooner, or it could be politically incentivized where municipalities with greener local governing may adopt earlier. The concern here could be both a violation of parallel trends, but also that early adopters and late adopters are on diverging trends in pre-treatment. To account for any non-random selection into the treatment, the analysis includes a set of control variables that capture the municipal characteristics that could influence adoption timing and recycling rates, i.e. median income, share of high education, number of drop-off points, population density and vacation home share.

Abraham & Sun (2021) require another identifying assumption, “no anticipation”. This ensures that units do not change their behavior before treatment is installed as an effect of anticipation. If participating individuals expect a policy change and thus change their behavior pre-treatment, the pre-treatment outcomes will be contaminated, and the parallel trends assumption will be invalidated. The event study plots in chapter 7 address the no anticipation assumption.

Another identifying assumption is SUTVA or spillover effects, where the potential outcome of one unit is not affected by the treatment status of another. Put differently, the outcome of a municipality in the control group is not affected by the treatment of another municipality. Since curbside collection of recyclables is a locally administered service only available to the residents of the implementing municipality, spillover effects seem unlikely, and reasonably SUTVA in the context of recycling rates can be considered satisfied.

Furthermore, spillover effects also pose a threat in terms of adoption timing, where municipalities neighboring already treated municipalities may adopt curbside collection after having witnessed successful policy installations. However, the distribution among treated municipalities points to adoption timing being correlated to regulatory events, rather than learning-by-doing spillovers.

Figure 3. Adopting municipalities per year, 2000-2024



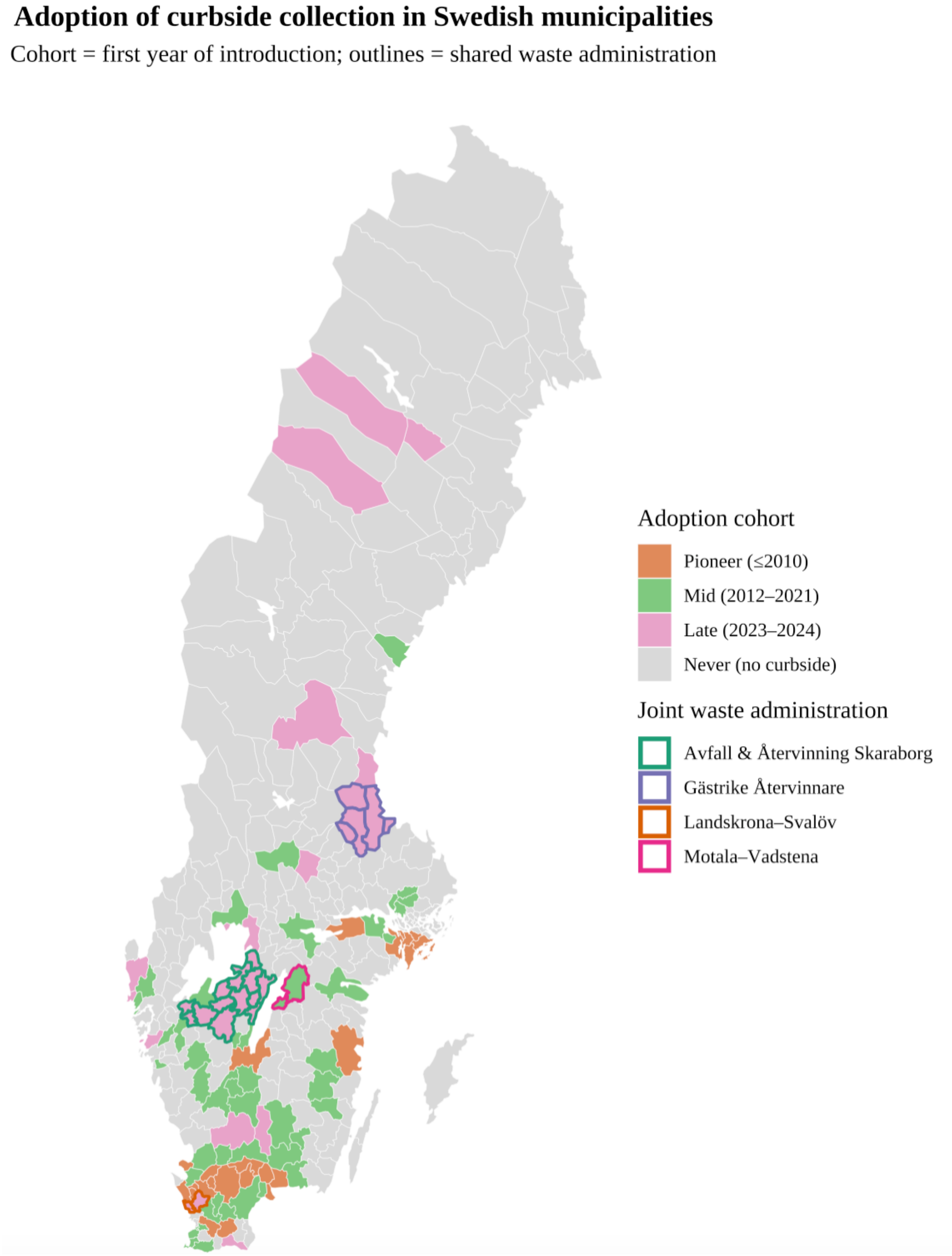
The first "peak" in curbside collection installment with six adopting municipalities happened in 2008, the same year as the European Parliament and Council (2008) established binding targets for recycling levels and demanded member states to implement separate collection of paper, plastic, glass and metals before 2015. This could explain the increase in adopting municipalities in 2014, with the upcoming deadline of separate collection implementation. The increase in adopting municipalities in 2019 can be explained as a result of the enforcement of the Swedish Ordinance on Producer Responsibility for Packaging (2018).

A further rise in adoptions is observed in 2024, which can plausibly be attributed to the transfer of operational responsibility for packaging waste collection from producers to municipalities under SFS 2022:1274, which mandated that all Swedish municipalities are to implement curbside collection by 2027. While this does not fully argue against the possibility of adoption timing being dependent on spillover effects from neighboring municipalities, it indicates that the installment of curbside collection is primarily driven by regulatory events.

Figure 4 depicts the distribution of adoption across Sweden. The four jointly administered waste companies that have adopted curbside collection each implement policy for all collectively managed municipalities. Therefore, any clustering they seem to produce are not due to spillover in adoption timing.

Beyond these groupings, adoption timing seems to shows signs of some clustering, mostly concentraded in southern Sweden. This can be due to several reasons and they are not necessarily mutually exclusive. Curbside collection is more cost-efficient to implement in areas with higher population density, concentration in the south could therefore partly be a result of urbanization rather than spillover effects across municipalities. Shared underlying characteristics such as political composition or distance to waste infrastructure could also be possible drivers to adoption, which then could contribute to similar adoption timing. As a result, SUTVA in adoption timing cannot be fully ruled out in this setting.

Figure 4. Distribution of policy adoption across Sweden, ≤2010-2024



6.3 The TWFE bias – Goodman-Bacon (2021)

When performing a traditional DiD analysis, there are two groups and two time periods. In this case, however, municipalities adopt curbside collection at different points in time, which creates a staggered adoption design with multiple treatment groups.

Recent research has shown that the TWFE only creates a causal interpretation of the ATT if; 1) the parallel trends assumption hold, and 2) the treatment effect is constant over time and groups. While the first is easy to test, the second is unlikely to hold (Goodman-Bacon, 2021). Assuming the TWFE, the expected effect of introducing curbside collection within a municipality will be constant from year one and forward. Due to the domestic structure of household sorting, it is reasonable to assume that sorting rates differ over time. After policy adoption, households may need time to thoroughly adjust to the new policy and learn-by-doing. Secondly, the rollout of curbside collection within a municipality is not introduced overnight but might take multiple years to fully implement. In the early post-adoption years, not all households are physically connected to the new service, so the treatment indicator captures assignment to an adopting municipality more than what is actually implemented. The estimated early-period effect therefore resembles an intention-to-treat effect, converging toward the full average treatment effect on the treated (ATT) as implementation completes.

Moreover, Table 3 in section 5 shows that the already-adopted vs. not-yet-adopted municipalities differ before treatment, where treated groups have on average lower median income, a higher vote share for the green party, and a greater number of residents.

Goodman-Bacon (2021) shows that the standard two-way fixed effects (TWFE) estimator in a staggered setting yields a weighted average for all possible two-group/two period comparisons in the data set. This means that TWFE does not estimate one clean treatment effect but rather combines many different comparisons. Some of these comparisons use units that have been treated at some time as the treatment group and untreated units as their controls. Other comparisons include units that are treated at two different times, where the group that gets treatment later on is used as a control group before its treatment begins, and the group that is treated early on as a control group after its treatment begins. The timing group sizes and the variance of the treatment dummy in the respective pairs decide the weights on the 2x2 comparisons, where the units that are treated in the middle of the panel have the highest weights.

Table 4. 2x2 comparisons in Goodman-Bacon decomposition

	Treatment group	*Control group*	*Status*
Earlier vs Later Treated	Early adopters	Late adopters	Clean comparison
Later vs Earlier Treated	Late adopters	Early adopters	Bad comparison
Treated vs Untreated	Adopters	Non-adopters	Clean comparison

When ATT vary over time, negative weights can occur as a result of already-treated units being used as controls, which causes any changes in their outcomes to be subtracted. These changes may include time-varying treatment effects, and if removed from the estimation, there is cause for caution when using the TWFE estimators as explanators for the ATT.

6.4 The fix – Abraham & Sun (2021)

Abraham & Sun (2021) introduce a method more robust to heterogeneity in treatment effects than the canonical TWFE regression. They estimate the shares of cohorts as weights, resulting in easily interpreted estimates. $C\ ATT_{e\ell}$ is the cohort-specific average treatment effect on the treated ℓ periods from initial treatment, for the cohort first treated at time e. In this setting, the cohorts are the years for adopting curbside collection, which ℓ 2011-2023, while the relative period ℓ represent the years since introduction. Furthermore, they present three identifying assumptions; 1) parallel trends in baseline outcomes, 2) no anticipatory behavior prior to treatment, and 3) treatment effect homogeneity. While the first two assumptions have been addressed in 6.2, the third has been addressed in section 6.3.

While the Goodman-Bacon decomposition shows that the static TWFE estimation is dominated by clean comparisons, Sun & Abraham (2021) extends the estimation to dynamic TWFE that includes leads and lags. They show that, in event studies with staggered adoption settings, the coefficient on any lead or lag can be a non-convex weighted combination of cohort specific ATT from multiple periods. Even if the static estimation produces positive aggregate weights for the clean comparisons, individual event-time comparisons can be contaminated by treatment effects from other relative periods and, by extension, make interpretating results difficult. Moreover, Sun & Abraham prove that there can be non-zero coefficients for leads in pre-trends for canonical TWFE, even though the true pre-trend leads are zero. This is a result of the pre-trends being contaminated by post-treatment effects from other periods. A visual inspection of the canonical TWFE pre-trends is therefore not reliable for the parallel trends assumption, which justifies the use of the Sun & Abraham estimator.

To address both of these issues, I will use the Sun & Abraham interaction weighted estimator. The IW-estimator estimates the cohort-specific average treatment effects C ATTe,ℓ, which is the average treatment effect for the cohort first treated at time e, ℓ periods after treatment. The cohort-specific estimates are then aggregated using weights from the sample share of the cohorts. The resulting coefficients are convex averages of the underlying C ATT and will not be contaminated by other effects from other periods. Standard errors are clustered at municipality level.

The panel data is unbalanced, particularly the data for residual waste is sparsely reported. The number of observations for H1 regarding packaging is 2952, whereas for H2 regarding residuals have 1881. The unbalanced panel and sporadic residual data motivate the Abraham & Sun estimator as headline, as this estimator is more forgiving toward the data gaps in comparison to the Callaway & Sant'Anna estimator. Also, the Abraham & Sun estimator uses not-yet-treated control units, while Callaway & Sant'Anna only uses the not-treated controls. Because of the thin treated cohorts, it is beneficial to take advantage of as many control units as possible. With this, the Callaway & Sant'Anna estimator is used as a robustness check to validate the results of the Abraham & Sun estimation, as Callaway & Sant'Anna is more flexible when handling covariates.

6.5 Limitations

The small sample of 55 treated units in the analysis is one limitation to this paper. While there are 100 treated units in Sweden today, the poorly reported waste statistics from years before 2011 prevents any analysis to be performed on these years. The same conclusion is drawn on the years between 2024-2025. Furthermore, the data for residual waste is sparsely reported, which limits the ability to draw causal conclusions from the estimation on hypothesis 2.

As stated in the section on demarcations, the data does not allow for estimation to occur on multiple-family housing as there are no reports on joint adoption timing. As a result, the analysis is limited to single-family households and vacation homes, which limits the interpretation of the effects of curbside collection to these groups, and not full municipal effects. Also, there is no data on the specific number of residents that live in single-family housing and vacation homes per municipality per year. Instead, the data set includes a number of dwellings in these two categories. This could introduce imprecision as residents vary across municipalities and time. By nature, vacation homes are not occupied in the same intensity as year-round accommodation. As vacant vacation homes are still counted as dwellings while they do not produce waste, this too can introduce imprecision on the causal effect of the estimator.

7. Results

In the following chapter, the results from different estimations are presented. The first part (7.1.) presents the results from the estimation on Hypothesis 1; "does curbside collection increase packaging recycling?", between the years 2011-2023. The second part (7.2.) presents the results from the estimation in Hypothesis 2; "does curbside collection cause diversion from residual waste to packaging recycling?", estimated for the same years. In the third part (7.3.), the estimation is extended using the Callaway & Sant'Anna model as a robustness check.

7.1 H1: Does curbside collection increase packaging recycling?

7.1.1 Two-Way Fixed Effects model

Table 5 shows the results, in terms of both levels and logarithms, from the two-way fixed effects model – with and without cocovariates. The first estimates present the results when no covariates are included, while the second estimate includes all six covariates; population density, vacation home share, share with higher education, median income, the vote share for the green party "Miljöpartiet", and the number of drop-off points (LIP) per municipality. The policy variable is a dummy that indicates if a municipality has adopted curbside collection. Standard errors are clustered at municipal level.

Table 5. Results of the TWFE estimation for H1, 2011-2023

H1: Effect of CC on total recyclable packaging — Levels and logarithmic specifications						
Specification	**Estimator**	**ATT**	**Std. Error**	**95% CI Lower**	**95% CI Upper**	**Obs.**
Levels	TWFE (no covariates)	10.26***	1.570	7.190	13.340	3,258
	TWFE (with covariates)	10.93***	1.630	7.740	14.120	2,953
Logarithm	TWFE (no covariates)	0.167***	0.028	0.113	0.221	3,257
	TWFE (with covariates)	0.178***	0.028	0.123	0.232	2,953

Significance codes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the municipality level. 95% confidence intervals reported. Levels specification: ATT in kg per capita per year. Logarithmic specification: ATT approximately interpretable as a percentage change; exact percentage = (exp(β) - 1) × 100.

Running the TWFE estimator without the inclusion of covariates yields positive and statistically significant results at the 1% level. It estimates that the introduction of curbside collection is associated with an increase of about 10,26 kg per capita per year in total recyclable packaging collection. When logging the same estimate, this results in a yearly increase of about 16,7%. Running the same regression included the covariates, still positive and statistically significant at the 1% level, estimates an increase of about 10,93 kg per capita per year. This means that the TWFE estimator is not driven largely by confounders. All covariates except “number of drop off points (LIP)” are statistically insignificant which means that the effect we see from curbside collection on recycling rates (10,93 kg per capita per year) is robust when controlling for socioeconomic and political variables.

However, as previously stated in chapter 6.3, there can be hidden bias in the TWFE estimator due to negative weights across comparison groups. The following section investigates the risk for biased estimation in TWFE according to Goodman-Bacon.

7.1.2 Goodman-Bacon decomposition

Figure 5. Goodman-Bacon decomposition, 2011-2023 (N = 192)

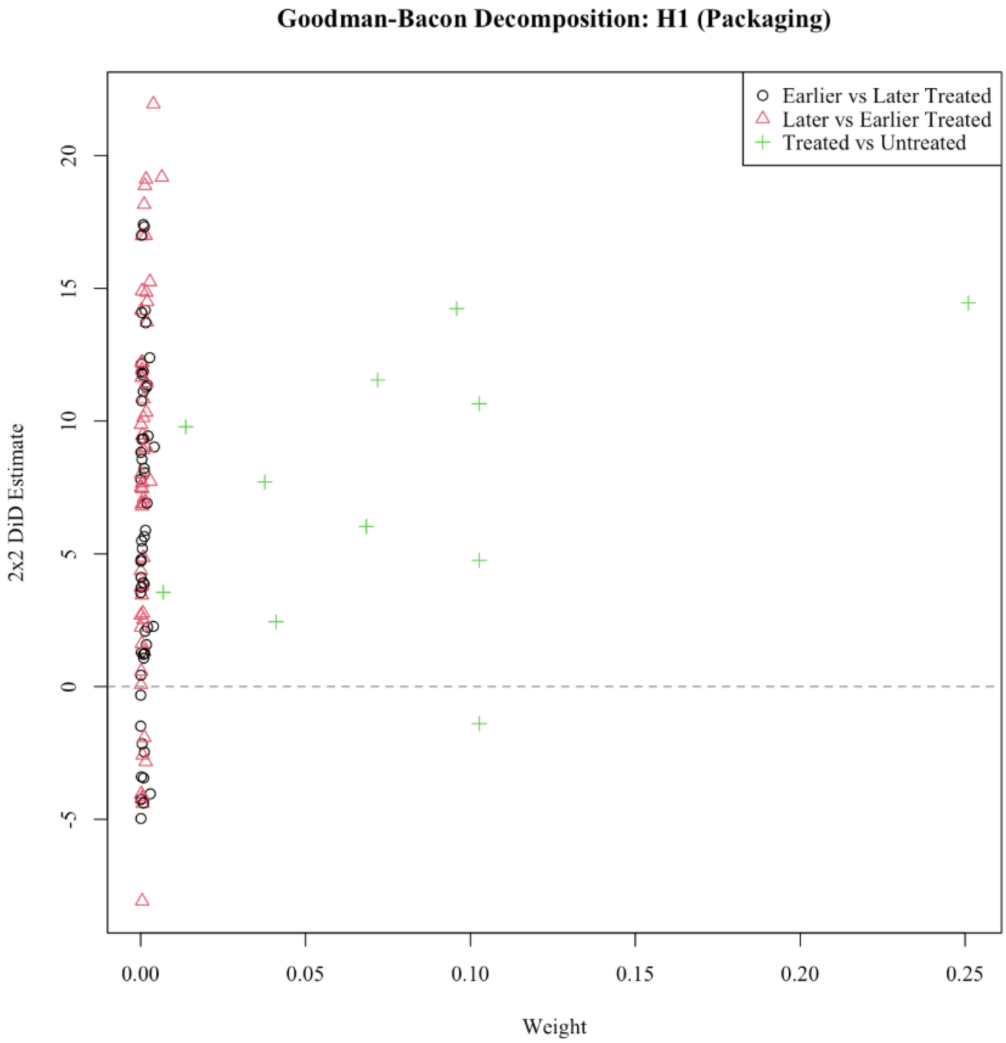


Figure 5 depicts the distribution of weights in the TWFE estimate in this data set. The black circles represent comparisons between one unit that is in the treatment group to a unit that is not yet in the treatment group but will be later on. The green crosses represent the clean comparisons of units in the treatment group to units in the control group. These two (black circles and green crosses) are "good" comparison observations. The red triangles, however, are the comparison observations that Goodman-Bacon detected as a problem for the TWFE estimator, as they represent comparisons between late adopters as the treatment group and early adopters as the control group, even though they have already undergone treatment.

The "Treated vs. Untreated" group is the only comparison that shows evidence of substantial weight to the estimate, while the "Earlier vs. Later Treated" and

“Later vs. Earlier Treated” comparisons are clustered around 0. This is a positive result for the TWFE estimate as the individual comparisons of “Later vs Earlier Treated”, which are the sources to bias in the estimate, carry small weights. However, there are many red triangles, and their weights sums to an aggregate weight. If this aggregate weight is large, then there is a cause for concern regarding the biasedness of the estimate.

Furthermore, the spread of the comparisons is wide, ranging from around –5 to + 20 for the DiD estimate. This means that the treatment effects across the 2x2 comparisons are highly heterogeneous, where some comparisons produce large positive effects, some around zero and some very negative. Had the comparison observations been clustered around 0 on the y-axis, then the treatment effect would have been constant over time and groups which would have satisfied the second condition needed to yield causal TWFE estimates.

Table 6. Aggregate weights and estimates in Goodman-Bacon decomposition (kg/capita)

Type	Weight	Average estimate
Earlier vs Later Treated	0,05209	6,27075
Later vs Earlier Treated	0,05340	11,25806
Treated vs Untreated	0,89452	9,19252

Table 6 shows the aggregate weights for each comparison group, which reports small weights in the “bad” comparison group, and large weights in the “good” comparison groups. This is reasonable given that the control group consists of 215 municipalities, whereas the treatment group consists of only 55 municipalities. Also, the rollout of curbside collection is heavier in recent years, resulting in less risk of pairing treated observations with each other. Only around 5,3% of the TWFE estimate is explained by the “bad” comparisons, while 89% + 5,2% = 94,6% is explained by the “good”.

By examining the average DiD estimates for each group, it is visible that the “Earlier vs. Later Treated” average estimate (6,27 kg/capita) is smaller than the “Treated vs. Untreated” average estimate (9,19 kg/capita). This could point to the later adopters among “Earlier vs. Later Treated” gear up for upcoming policy introduction, which could result in the DiD estimate being smaller compared to the “Treated vs. Untreated” estimate. Also, the “Later vs. Earlier Treated” average estimate (11,25 kg/capita) is larger than the “Treated vs. Untreated” average estimate (9,19 kg/capita), which could be caused by late adopters being in the treatment group. As these adopt curbside collection later in respect to the early adopters, there might be structural improvements in the adoption strategies that late adopters can benefit from, which could increase the average treatment effects for this group.

Given that the average estimates for the three different comparison groups are broadly consistent, it suggests that the TWFE estimator is not severely biased and can thus be used as a baseline model. However, further investigation using heterogeneity-robust estimators is needed to establish causal effects. Abraham & Sun (2021) identifies the problem with heterogeneity in a staggered adoption setting and proposes an alternative estimator.

7.1.3 Abraham & Sun (2021)

As previously stated in chapter 6.5, the Abraham & Sun estimation is carried out to avoid the possible issues related to the TWFE model. Table 7 shows the results, in terms of both levels and logarithms, from the two-way fixed effects model and the Abraham & Sun model.

Table 7. Results of the TWFE and Abraham & Sun estimation for H1, 2011-2023

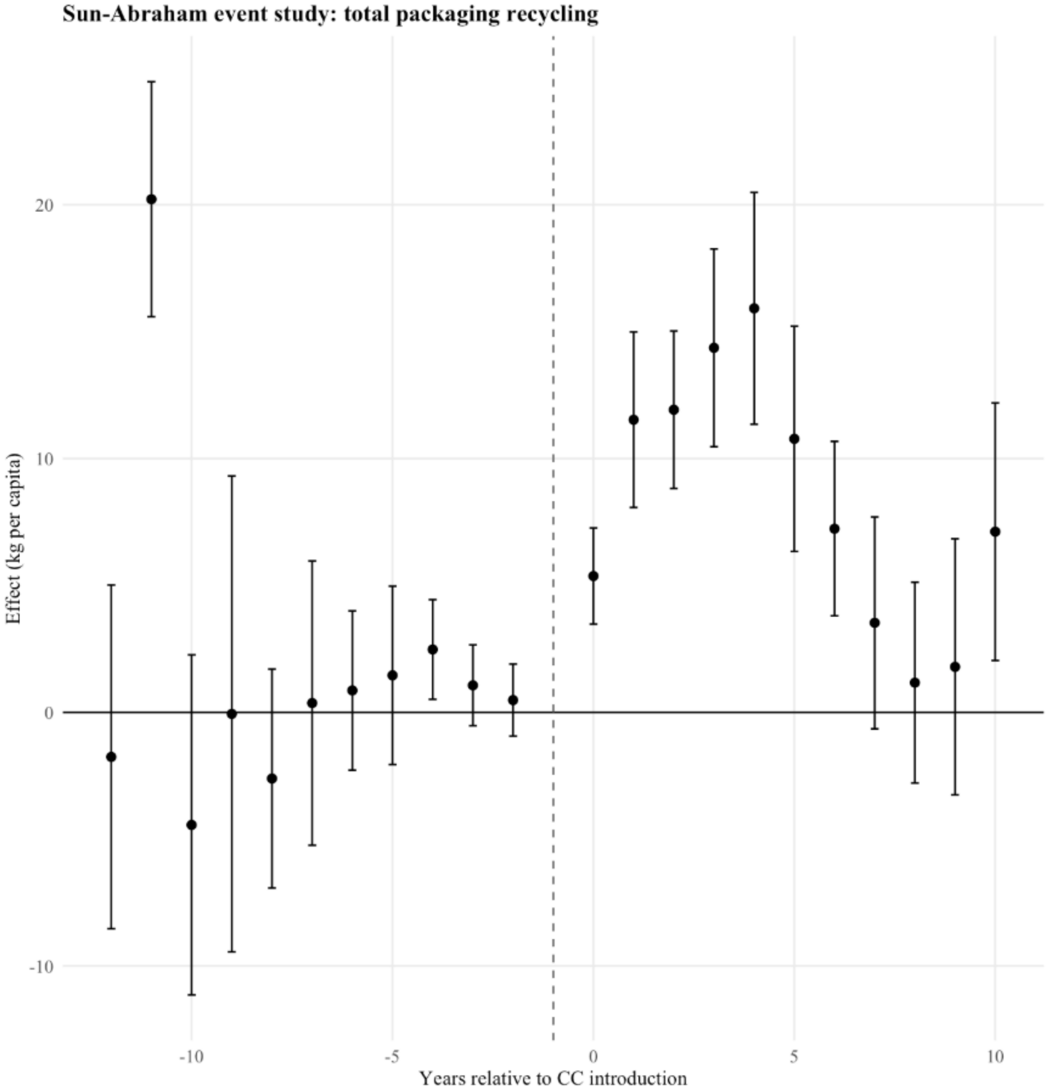
H1: Effect of CC on total recyclable packaging — Levels and logarithmic specifications						
Specification	**Estimator**	**ATT**	**Std. Error**	**95% CI Lower**	**95% CI Upper**	**Obs.**
Levels	TWFE (no covariates)	10.26***	1.570	7.190	13.340	3,258
	TWFE (with covariates)	10.93***	1.630	7.740	14.120	2,953
	Sun-Abraham	**10.06*****	**1.210**	**7.690**	**12.430**	**2,952**
Logarithm	TWFE (no covariates)	0.167***	0.028	0.113	0.221	3,257
	TWFE (with covariates)	0.178***	0.028	0.123	0.232	2,953
	Sun-Abraham	**0.165*****	**0.021**	**0.125**	**0.205**	**2,952**

Significance codes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the municipality level. 95% confidence intervals reported. Levels specification: ATT in kg per capita per year. Logarithmic specification: ATT approximately interpretable as a percentage change; exact percentage = (exp(β) - 1) × 100.

The estimate for Sun & Abraham on the effect of curbside collection on packaging recycling results in positive and statistically significant estimates at the 1% level. The results show an increase in packaging waste of approximately 10,06 kg/capita per year, on average across all post-treatment years. In logged terms, this translates to an increase of about 16,5%.

In Figure 6, the event study plot for Sun & Abraham is depicted. This depicts several noteworthy things; if parallel trends hold pre-treatment, if there are anticipation effects pre-treatment, and how the effect from curbside collection evolves over time.

Figure 6. Event study plot of Abraham & Sun results for packaging, 2011-2023



The pre-treatment coefficients are generally around zero, which supports the parallel trends assumption. However, the coefficient for eleven years before treatment is an outlier in the pre-treatment environment. The coefficient for year –11 is only driven by the municipalities that have observations in the data set for that long back in time. In this case, eleven years back from adoption timing is only driven by two municipalities, i.e., the municipalities that adopted curbside collection in 2023 (see Table A7.1. in the appendix). That is, the coefficient for –11 years from adoption of curbside collection is made up of a tiny subsample and is therefore noisy. The expanding confidence intervals further back in the pre-treatment environment can be explained by few subsamples of observations, as seen in Table A7.1 in the appendix.

There seems to be no anticipation of treatment as the coefficients show no clear indication of an upward trend in the years before adoption. As seen in the environment of post-treatment, there is a positive effect of policy introduction which gradually decreases over time. The observed increase in the last coefficients in post-treatment of the event study could be the result of a small subsample, where the cohort for the tenth year post-treatment only includes four municipalities.

7.2 H2: Does curbside collection decrease residual waste?

7.2.1 Two-Way Fixed Effects model

Similarly to the results for TWFE for Hypothesis 1, Table 8 shows the results for the estimation in terms of both levels and logarithms, with and without covariates. Standard errors are clustered at municipal level.

Table 8. Results of the TWFE estimation for H2, 2011-2023

H2: Effect of CC on residual waste — Levels and logarithmic specifications						
Specification	**Estimator**	**ATT**	**Std. Error**	**95% CI Lower**	**95% CI Upper**	**Obs.**
Levels	TWFE (no covariates)	-25.59***	4.590	-34.590	-16.590	2,016
	TWFE (with covariates)	-17.64***	3.820	-25.130	-10.150	1,882
Logarithm	TWFE (no covariates)	-0.143***	0.022	-0.187	-0.099	2,016
	TWFE (with covariates)	-0.099***	0.020	-0.139	-0.060	1,882

Significance codes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Standard errors clustered at the municipality level. 95% confidence intervals reported. Levels specification: ATT in kg per capita per year. Logarithmic specification: ATT approximately interpretable as a percentage change; exact percentage = $(\exp(\beta) - 1) \times 100$.

The TWFE estimation without covariates shows that there is a negative and statistically significant effect at the 1% level. This estimation indicates a decrease in residual waste of about 25,59 kg/capita per year as a result of introducing curbside collection. This coincides with a percentual reduction of approximately 14,3%. The same estimation including covariates reports an estimate of –17,64 kg/capita per year, or when logged a decrease of 9,99%.

7.2.2 Abraham & Sun (2021)

Table 9. Results of the TWFE and Abraham & Sun estimation for H2, 2011-2023

H2: Effect of CC on residual waste — Levels and logarithmic specifications						
Specification	**Estimator**	**ATT**	**Std. Error**	**95% CI Lower**	**95% CI Upper**	**Obs.**
Levels	TWFE (no covariates)	-25.59***	4.590	-34.590	-16.590	2,016
	TWFE (with covariates)	-17.64***	3.820	-25.130	-10.150	1,882
	Sun-Abraham	**-29.20*****	**3.850**	**-36.740**	**-21.660**	**1,881**
Logarithm	TWFE (no covariates)	-0.143***	0.022	-0.187	-0.099	2,016
	TWFE (with covariates)	-0.099***	0.020	-0.139	-0.060	1,882
	Sun-Abraham	**-0.149*****	**0.019**	**-0.186**	**-0.112**	**1,881**

Significance codes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the municipality level. 95% confidence intervals reported. Levels specification: ATT in kg per capita per year. Logarithmic specification: ATT approximately interpretable as a percentage change; exact percentage = (exp(β) - 1) × 100.

Looking at the estimates for Abraham & Sun, they are negative and statistically significant at the 1% level. While the Sun & Abraham estimate shows a decrease in residual waste of 29,2 kg per capita per year, on average across all post-treatment cohorts, the TWFE estimator with covariates indicates a decrease of only 17,64 kg/capita per year. This means that the TWFE estimators for hypothesis 2 are biased downward.

Figure 7. Event study plot of Abraham & Sun results for residual waste, 2011-2023

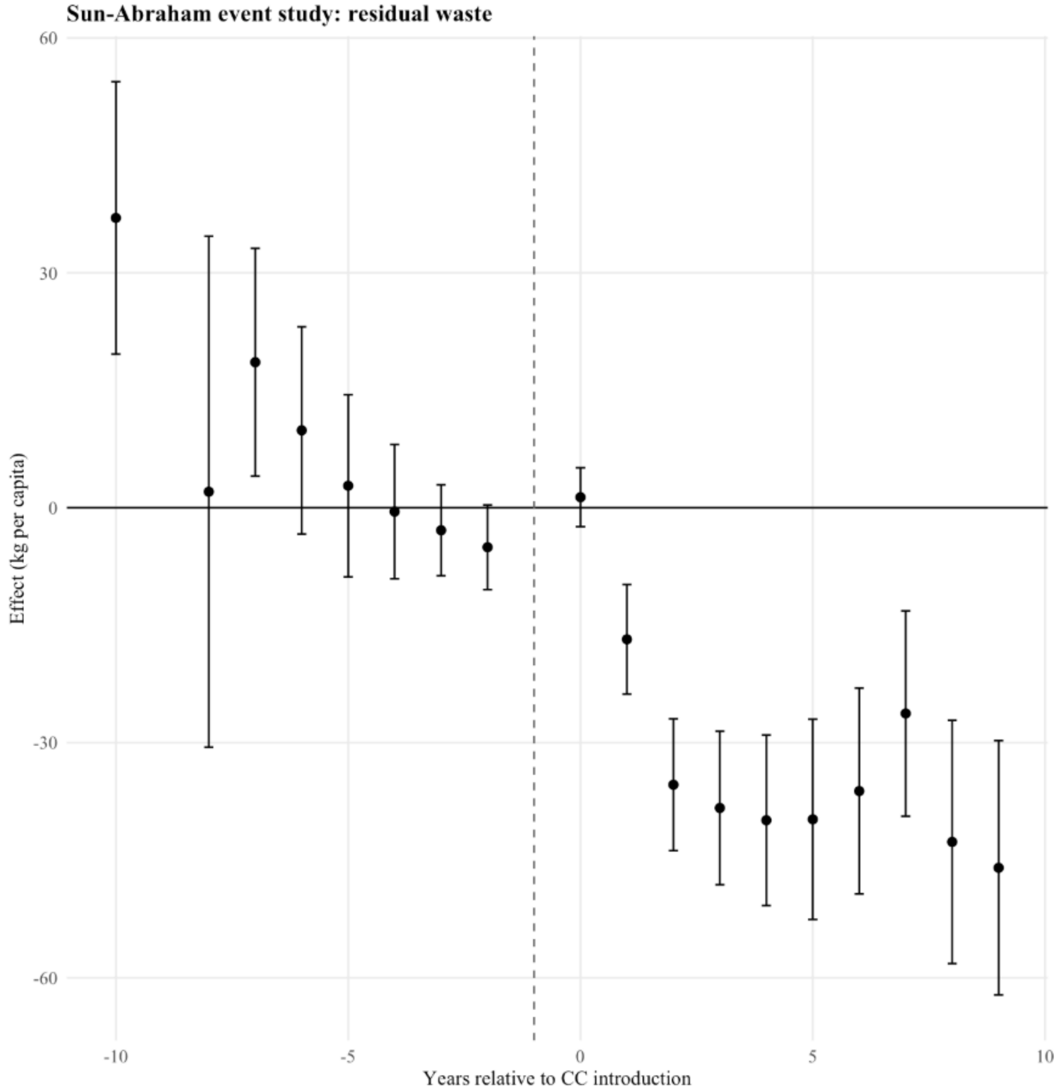


Figure 7 depicts the results for the event study plot on residual waste, which shows a downward trend in treatment effects. The coefficient for year –10 shows a relatively high value of around 35-40kg/capita per year, pre-treatment, with a wide confidence interval.

As seen in Table A7.2. in the appendix, the coefficient for year –8 is dependent on two cohorts that consist of three municipalities, which explains the wide confidence interval. While years –6 and –5 have 5, respective 6 observations, creating noisy coefficients, points –4 to –1 are hovering around zero with tighter confidence intervals. The four years before adoption show no anticipation and flat pre-trends.

The coefficient for year 0 is close to zero with tight confidence interval, which makes sense as behavioral effects don't necessarily happen at once, but households might need time to adapt. The effect on residual waste appears immediately one year after adoption, and the effect is persistent over time.

7.3 Robustness checks

7.3.1 Estimator choice – Callaway & Sant'Anna (2021)

In this chapter, the Goodman-Bacon bias-robust Callaway & Sant'Anna estimation is introduced as a robustness check for the choice of the estimator. Similar to Abraham & Sun, the Callaway & Sant'Anna estimator computes the aggregate treatment effect on 2x2 comparisons across specific cohorts. However, there are differences in the mechanical setup. If the treatment effects from all estimators point in the same direction, it strengthens the credibility of the results and further dismisses the risk of the treatment effects as results of a particular design choice. While Callaway & Sant'Anna is not necessarily a good estimator given the composition of this data, it is used as a control for the validity of Sun & Abraham.

Table 10. Results from TWFE, Abraham & Sun, and Callaway & Sant'Anna, 2011-2023

Robustness: the Callaway & Sant'Anna estimator for H1 and H2						
Outcome	**Estimator**	**ATT**	**Std. Error**	**95% CI Lower**	**95% CI Upper**	**Obs.**
Total packaging	TWFE (no covariates)	10.26***	1.57	7.19	13.34	3,258
	TWFE (with covariates)	10.93***	1.63	7.74	14.12	2,953
	Sun-Abraham	10.06***	1.21	7.69	12.43	2,952
	Callaway-Sant'Anna (simple)	**8.94****	**1.60**	**5.81**	**12.08**	
Residual waste	TWFE (no covariates)	-25.59***	4.59	-34.59	-16.59	2,016
	TWFE (with covariates)	-17.64***	3.82	-25.13	-10.15	1,882
	Sun-Abraham	-29.20***	3.85	-36.74	-21.66	1,881
	Callaway-Sant'Anna (simple)	**-32.56****	**9.78**	**-51.73**	**-13.38**	

Significance codes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the municipality level. 95% confidence intervals reported. The Callaway-Sant'Anna estimator does not report a single p-value; significance based on whether the 95% confidence interval excludes zero. H2 is supported if packaging rises and residual falls — both directions hold across all estimators.

The Callaway & Sant'Anna estimates for packaging waste report somewhat lower estimates (8,94kg/capita) than the TWFE and Sun & Abraham estimates. The results are statistically significant at the 5% level. The estimate for residual waste is –32 kg/capita per year, which is slightly lower, while still in the same direction, as the Abraham & Sun estimation of -29,2 kg/capita. The slightly smaller estimates for Callaway & Sant'Anna could be explained by the estimator giving smaller weight to earlier treated cohorts, which by default have more observations in post-treatment. On the contrary, Abraham & Sun give more weight to these earlier treated cohorts in the ATE.

For the results on residual waste, both TWFE estimators understate the effect in relation to Abraham & Sun and Callaway & Sant'Anna. This is consistent with the interpretation presented in chapters 6.3 and 6.4. When treatment effects are heterogeneous over time and cohorts, the TWFE estimator understates the true effect. The event study plots for Callaway & Sant'Anna compare well with the event study plots for Abraham & Sun and can be found in the appendix as Figure A1 and A2.

7.3.2 Placebo outcomes test: pseudo-timing

In order to validate the results for parallel trends from the event study plot, a pseudo timing outcomes tests over three pseudo adoption years – two, three, and four years before the true adoption year. By simulating adoption before the true adoption timing, any detected effects from treatment are warning signs to the actual ATE.

Table 11. Pseudo-timing placebo, aggregated ATT by fake-adoption offset

Pseudo-timing placebo: aggregated ATT across fake-adoption offsets (total packaging, kg per capita)					
Fake offset (years)	**ATT**	**Std. Error**	**p-value**	**Treated municipalities**	**Observations**
-2	-0.93	0.58	0.108	55	2,625
-3	-1.96***	0.68	0.004	55	2,625
-4	-0.60	1.50	0.691	55	2,625

*Notes: SEs clustered at the municipality level. Significance codes: * p<0.10, ** p<0.05, *** p<0.01.*

Treatment effects across all faked adoption years report negative results, in contrast to the true Abraham & Sun estimate of +10,06kg/capita. While there are negative values across all placebo ATE's, a trend pre-existent to the true treatment would have to be similar in size and follow the same direction.

Years –2 and –4 have statistically insignificant results while also being close to zero, whereas the adoption year for –3 estimates an effect of –1,96kg/capita with significance at the 1% level. However, for there to be diverging trends between treated and controls, the effect would need to be stable over the fake offset years, not just appear for –3 and then vanish again. This can be explained by noisy estimates rather than violations against the parallel trends assumption.

8. Discussion

In this chapter, the results are discussed and interpreted. The findings are reviewed in relation to the 2030 targets that were presented in the introduction of this paper.

While the ATT for packaging is +10,06kg, the residual waste estimation is of -29,2kg. The estimations in chapter 7 measure two outcomes in parallel, and do not trace where the kilograms from residual waste end up nor where packaging waste increases originate from. While this paper is unable to causally prove diversion effects from residual waste to packaging, it is plausible that some of the reduction in residual waste is diverted into packaging waste sorting. However, the effect is not one-to-one, as the decrease of 29kg/capita in residuals is a larger effect than an increase of 10,06kg/capita in packaging waste. Assuming that the full effect of packaging waste originates from residual waste, there is still a reduction of 19kg/capita in residuals that need to go somewhere. One possible explanation could be that households generate less waste in total as a result of policy adoption, as was found by Miliute-Plepiene & Plepys (2015). Another explanation could be diversion effects into waste streams that are not observed in this paper, like food waste, paper waste, or bulky waste.

Furthermore, the event study plot for packaging waste shows a sharp rise in collected material after adoption, possibly due to novelty and warm-glow effects. Between year +4 and +8, packaging waste decreases to pre-treatment levels; however, there are declining observations in the cohorts which could enforce this effect. Nevertheless, as time goes on, warm-glow effects and novelty may attenuate, and the efforts to sort packaging materials with it. Increases seen from year +8 to +11 could be driven by few observations in the cohorts as these years are based on 1 to 13 municipalities. The small cohorts with few observations explain imprecisely estimated coefficients toward the edges of both event study plots; they do not, however, invalidate the flat pre-trends nor no anticipation-assumption.

Meanwhile, the event study plot for residual waste depicts persistent effects over time. Since curbside collection systems include the eight compartments depicted in Figure 1, whereof food waste is one of them, it is plausible that the persistent decrease in residual waste is partly explained by the inherent implementation of food waste. Any leftover residual waste could then be interpreted as non-recyclable, which could explain the persistent effect.

The TWFE estimator yielded similar results as Abraham & Sun and Callaway & Sant'Anna for the regression run on packaging (+10,93 / +10,06 / +8,94kg/capita). The consistency across the estimators assures confidence in the estimated effect and rejects the possibility of the estimated outcome to be an artifact of one specific estimator. At the same time, the estimations differed greatly in the regression on residual waste (-17,64 / -29,2 / -32,56kg/capita). While the estimators that account for heterogeneity across time and units are somewhat similar in outcomes, the TWFE estimation largely diverges. This proves the Goodman-Bacon bias presented in chapter 6, where negative weights in the 2x2 comparisons cause underestimation of the true effect. Furthermore, the robustness check for parallel trends validation through the pseudo timing test showed no clear signs of diverging trends pre-treatment.

The mandatory implementation of curbside collection for all Swedish municipalities is set to be carried out by the start of 2027. With targets for recycled packaging materials set to 2030, the policy is effectively granted three years to take effect. By 2030, Sweden is set to recycle 70% of all generated packaging materials. It is important to note that the target of 70% is measured in recycled material, while the result from this paper presents the effects of policy adoption for collected material. The two are not equivalent: collected packaging is not necessarily fully recyclable, as households may sort incorrectly, and a share of collected material is rejected during sorting and sent to incineration rather than recycled. Any projection from collected quantities to the recycling target therefore represents an upper bound on the achievable recycling rate.

Another distinction regarding the reported recycling rate is also necessary. As briefly mentioned in the introduction and background, the recycling rate for all reported packaging material placed on the market in 2024 was 76%, which exceeds the 2030 target of 70%. However, this is likely overstated as waste composition analyses that account for unreported packaging from free riders result in a recycling rate of 66%, below the 2030 target (Naturvårdsverket, 2025). Given this gap in reported and true recycling rates, the effect of curbside collection becomes relevant. Table 12 summarizes the thus far presented values that will be used in further discussion.

Table 12. Summary of relevant values

The 2030 target (in recycled packaging)	70%
2024 reported recycling rate	76%
2024 estimated true recycling rate	66%
Abraham & Sun estimate for curbside collection (collected packaging)	16,5%

By treating the collected packaging as an upper bound on recyclable material, it is possible to quantify the effect of curbside collection in relation to the 2030 target. By assuming there are no losses during sorting, and that all collected material is recyclable, the most optimistic estimate of recycling rates in 2030 can be attained. Under this assumption, in combination with the assumption that total generated waste is unchanged, the reported 2024 rate of 76% yields an upper bound of approximately:

$$76\% \cdot 1{,}165 \approx 88\%$$

The same logic can be applied in the free rider-setting, where the recycling rate of 66% results in an upper bound of about:

$$66\% \cdot 1{,}165 \approx 76{,}9\%$$

However, it is important to note that this is the upper bound limit to what the curbside collection implementation can yield, under assumptions regarding no dilution and fixed generation.

According to Christian Håkansson (Packaging & Sustainability Manager, TMR AB, personal communication, 20 May 2026), 89% of all collected packaging waste is recycled. By accounting for this, a middle-case scenario can be achieved, while still holding household generation fixed. An updated recycling rate estimate can be calculated as:

$$16{,}5\% \cdot 0{,}89 \approx 14{,}7\%$$

The recycling rate for reported packaging waste would then be:

$$76\% \cdot 1{,}147 \approx 87{,}2\%$$

While the middle-case recycling rate while accounting for free-riding would be:

$$66\% \cdot 1{,}147 \approx 75{,}7\%$$

Also, a worst-case scenario can be achieved by utilizing the lower bound confidence interval from hypothesis 1, while still holding waste generation fixed. The Abraham & Sun estimator reported a lower bound confidence interval of 12,5%. Accounting for 89% recycled packaging out of total collected yields a worst-case percentage increase of:

$$12{,}5\% \cdot 0{,}89 \approx 11{,}13\%$$

The worst-case scenario in the reported recycling of packaging is thus:

$$76\% \cdot 1{,}1113 \approx 84{,}5\%$$

The same case while accounting for free riders yields:

$$66\% \cdot 1{,}1113 \approx 73{,}3\%$$

Table 13. Summary of projected recycling rates for 2030

2030 target: 70%	*Reported rate*	*Free rider-rate*
Best-case scenario	*88%*	*76,9%*
Middle-case scenario	*87,2%*	*75,7%*
Worst-case scenario	*84,5%*	*73,3%*

The above projections are all based on the assumption that household waste generation is constant over time; however, it is not. Between 2020 and 2024, the change in generated household waste in Sweden declined with about 10,4% (Sverige Avfall 2024b). If this is used as a guideline for 2030 household generation rates, any mentioned projections are pessimistic as the denominator (waste generation) declines over time.

$$\frac{x}{total\ waste \downarrow} = recycling\ rate \uparrow$$

There are real limitations to this paper in the exclusion of multi-family housing. The results from the regressions, as well as any projections toward a national 2030 target, are bounded to single-family housing and vacation homes. These two do not necessarily accurately represent the market structure in terms of waste generation or household behavior, and the analysis is therefore tied to this constraint. Also, there seems to be some spatial pattern in adoption timing across municipalities, which unables full disregard of SUTVA bias. Furthermore, the restriction in residual data weakens the statistical claim, as well as the small treated sample of 55 municipalities. Thin cohorts further away from adoption year in the event studies limit the interpretation of long-term effects.

9. Conclusions

This paper investigates whether there are causal effects to recyclable packaging and residual waste rates when introducing curbside collection, as a projection of the outcomes for 2027 mandatory implementation. I found increasing, yet fading effects of +10,06kg/capita, or +16,5% in collected packaging, while residual waste shows decreasing and persistent effects of -29,2kg/capita, or -14,9%. The decreasing outcomes in recycled packaging over time could be explained by fading warm-glow effects as novelty wears off. The regressions performed on collected packaging were robust across all estimators, while regressions across residual waste showed bias in the TWFE estimation due to heterogeneity across time and units. The key identifying assumptions are supported by evidence in the event study plot, with caveats in terms of thin cohorts further away from policy adoption year, as discussed in chapter 7 and 8.

This paper holds relevance in light of the upcoming mandatory adoption of curbside collection and pressing targets for 2030 levels of recycling, where the policy implementation is hoped to yield satisfactory results. Given the estimated outcomes and constant household waste generation, the reported recycling rate for 2030 is projected to hit 88% as a best-case scenario. The more credible best-case-scenario, which includes non-reported waste from free rider actors, yields a recycling rate of 76,9%. Furthermore, this paper finds that even in the worst-case scenario that accounts for free riders, the 70% target for 2030 is projected to be met as a result of the introduction of curbside collection.

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Appendix

Table A1. Treated municipalities that are included in the data set and their introduction years for curbside collection

Municipality	Introduction year
1. Ale	2021
2. Alingsås	2012
3. Borås	2021
4. Botkyrka	2014
5. Bromölla	2021
6. Burlöv	2019
7. Eslöv	2013
8. Gislaved	2019
9. Gnosjö	2019
10. Habo	2018
11. Haninge	2014
12. Huddinge	2014
13. Hultsfred	2017
14. Härnösand	2014
15. Håbo	2020
16. Högsby	2017
17. Hörby	2013
18. Höör	2013
19. Karlstad	2019
20. Knivsta	2021
21. Kristianstad	2017
22. Laholm	2018
23. Lessebo	2019

Municipality	Introduction year
24. Lidköping	2019
25. Ljusdal	2023
26. Ludvika	2019
27. Lysekil	2016
28. Malmö	2019
29. Markaryd	2020
30. Motala	2021
31. Mullsjö	2018
32. Munkedal	2019
33. Mölndal	2015
34. Norrköping	2016
35. Nykvarn	2017
36. Nynäshamn	2014
37. Ronneby	2018
38. Salem	2014
39. Sigtuna	2021
40. Sotenäs	2023
41. Staffanstorp	2020
42. Strängnäs	2015
43. Svenljunga	2019
44. Tingsryd	2019
45. Trelleborg	2014
46. Upplands-Bro	2021

Municipality	Introduction year
47. Vadstena	2021
48. Vaggeryd	2020
49. Vellinge	2021
50. Vimmerby	2017
51. Värnamo	2019

Municipality	Introduction year
52. Växjö	2020
53. Älmhult	2020
54. Ängelholm	2016
55. Örebro	2016

Table A2. Municipalities that have not adopted curbside collection and are used as controls

1. Alvesta	27. Essunga	53. Heby
2. Aneby	28. Fagersta	54. Hedemora
3. Arboga	29. Falkenberg	55. Herrljunga
4. Arjeplog	30. Falköping	56. Hjo
5. Arvidsjaur	31. Falun	57. Hofors
6. Arvika	32. Filipstad	58. Hudiksvall
7. Askersund	33. Finspång	59. Hylte
8. Avesta	34. Flen	60. Hällefors
9. Bengtsfors	35. Forshaga	61. Härjedalen
10. Berg	36. Färgelanda	62. Härryda
11. Bjurholm	37. Gagnef	63. Höganäs
12. Boden	38. Gnesta	64. Jokkmokk
13. Bollebygd	39. Gotland	65. Järfälla
14. Bollnäs	40. Grums	66. Kalix
15. Borgholm	41. Grästorp	67. Kalmar
16. Boxholm	42. Gullspång	68. Karlsborg
17. Bräcke	43. Gällivare	69. Karlskoga
18. Dals-Ed	44. Gävle	70. Karlskrona
19. Danderyd	45. Göteborg	71. Katrineholm
20. Degerfors	46. Götene	72. Kil
21. Dorotea	47. Hagfors	73. Kinda
22. Eda	48. Hallsberg	74. Kiruna
23. Ekerö	49. Hallstahammar	75. Kramfors
24. Eksjö	50. Halmstad	76. Kristinehamn
25. Emmaboda	51. Hammarö	77. Krokom
26. Enköping	52. Haparanda	78. Kumla

79. Kungsbacka	106. Mörbylånga	133. Skellefteå
80. Kungsör	107. Nacka	134. Skinnskatteberg
81. Kungälv	108. Nora	135. Skurup
82. Kävlinge	109. Norberg	136. Skövde
83. Köping	110. Nordanstig	137. Smedjebacken
84. Landskrona	111. Nordmaling	138. Sollefteå
85. Laxå	112. Norrtälje	139. Sollentuna
86. Lekeberg	113. Norsjö	140. Solna
87. Leksand	114. Nybro	141. Stenungsund
88. Lidingö	115. Nyköping	142. Stockholm
89. Lilla Edet	116. Nässjö	143. Storfors
90. Lindesberg	117. Ockelbo	144. Storuman
91. Linköping	118. Orsa	145. Strömstad
92. Ljungby	119. Orust	146. Strömsund
93. Ljusnarsberg	120. Oskarshamn	147. Sundbyberg
94. Lomma	121. Ovanåker	148. Sundsvall
95. Luleå	122. Oxelösund	149. Sunne
96. Lycksele	123. Pajala	150. Surahammar
97. Malung-Sälen	124. Partille	151. Svalöv
98. Malå	125. Piteå	152. Svedala
99. Mariestad	126. Ragunda	153. Säffle
100. Mark	127. Robertsfors	154. Säter
101. Mellerud	128. Rättvik	155. Sävsjö
102. Mjölby	129. Sala	156. Söderhamn
103. Mora	130. Sandviken	157. Tibro
104. Munkfors	131. Simrishamn	158. Tidaholm
105. Mönsterås	132. Skara	159. Tierp

160.	Timrå	177.	Uppvidinge	194.	Älvkarleby
161.	Tjörn	178.	Valdemarsvik	195.	Älvsbyn
162.	Tomelilla	179.	Vallentuna	196.	Åmål
163.	Torsby	180.	Vansbro	197.	Ånge
164.	Torsås	181.	Vara	198.	Åre
165.	Tranemo	182.	Varberg	199.	Årjäng
166.	Tranås	183.	Vaxholm	200.	Åsele
167.	Trollhättan	184.	Vetlanda	201.	Åtvidaberg
168.	Trosa	185.	Vindeln	202.	Öckerö
169.	Tyresö	186.	Vingåker	203.	Ödeshög
170.	Täby	187.	Vännäs	204.	Örnsköldsvik
171.	Töreboda	188.	Värmdö	205.	Östersund
172.	Uddevalla	189.	Västerås	206.	Österåker
173.	Ulricehamn	190.	Vårgårda	207.	Östhammar
174.	Umeå	191.	Ydre	208.	Överkalix
175.	Upplands Väsby	192.	Ystad	209.	Övertorneå
176.	Uppsala	193.	Älvdalen		

Table A3. Treated municipalities with adoption years 2024-2025, included as control units in the data set

1. Borlänge	2025
2. Lerum	2025
3. Vänersborg	2025
4. Sorsele	2024
5. Tanum	2024
6. Vilhelmina	2024

Table A4. Municipalities with adoption year before 2011 that are excluded from the data set

Municipality	Introduction Year
1. Söderköping	1991
2. Hässleholm	1997
3. Lund	2001
4. Perstorp	2003
5. Helsingborg	2004
6. Klippan	2004
7. Båstad	2005
8. Osby	2006
9. Östra Göinge	2006
10. Bjuv	2007
11. Södertälje	2007
12. Västervik	2007
13. Åstorp	2007
14. Örkelljunga	2007
15. Jönköping	2008
16. Eskilstuna	2010
17. Karlshamn	2010
18. Olofström	2010
19. Sjöbo	2010
20. Sölvesborg	2010

Table A5. Share of missing observations in the panel, 2000-2025

Year	**Total municipalities**	**Missing observations for "Total collected recyclable packaging"**	**Share missing**
2000	254	253	0.996
2001	254	253	0.996
2002	254	239	0.941
2003	254	241	0.949
2004	254	240	0.945
2005	254	239	0.941
2006	254	237	0.933
2007	254	116	0.457
2008	254	104	0.409
2009	254	72	0.283
2010	254	75	0.295
2011	254	2	0.008
2012	254	37	0.146
2013	254	30	0.118
2014	254	26	0.102
2015	254	6	0.024
2016	254	2	0.008
2017	254	2	0.008
2018	254	2	0.008
2019	254	2	0.008
2020	254	20	0.079
2021	254	12	0.047
2022	254	7	0.028
2023	254	0	0.000

2024	254	9	0.035
2025	254	80	0.315
			1.000

Table A6. Municipalities that are jointly administered by waste management companies, included in the data set

Avfall & Återvinning Skaraborg	1. Essunga 2. Falköping 3. Grästorp 4. Gullspång 5. Götene 6. Hjo 7. Karlsborg 8. Mariestad 9. Skara 10. Skövde 11. Tibro 12. Töreboda 13. Vara
Gästrike Återvinnare	14. Hofors 15. Ockelbo 16. Sandviken 17. Älvkarleby 18. Gävle
Kretslopp Sydost	19. Kalmar 20. Mörbylånga 21. Nybro 22. Oskarshamn 23. Sävsjö 24. Torsås 25. Uppvidinge 26. Vetlanda
LSR Landskrona - Svalövs Renhållnings AB	27. Landskrona 28. Svalöv
Vafab Miljö	29. Arboga 30. Enköping 31. Fagersta 32. Hallstahammar 33. Heby 34. Kungsör 35. Köping 36. Norberg 37. Sala 38. Skinnskatteberg 39. Surahammar 40. Västerås
Motala/Vadstena	41. Motala 42. Vadstena

Table A7.1. Sample contribution by event time for packaging, Abraham & Sun estimator

Event time	Municipalities	Cohorts
-12	2	1
-11	2	1
-10	11	2
-9	15	3
-8	28	4
-7	30	5
-6	35	6
-5	39	7
-4	41	8
-3	49	9
-2	52	10
-1	54	11
0	54	11
1	53	10
2	53	10
3	44	9
4	38	8
5	26	7
6	22	6
7	17	5
8	13	4
9	11	3
10	4	2
11	1	1

Table A7.2. Sample contribution by event time for residual waste, Abraham & Sun estimator

Event time	Municipalities	Cohorts
-10	1	1
-9	1	1
-8	3	2
-7	5	3
-6	5	3
-5	12	3
-4	20	5
-3	29	6
-2	35	7
-1	38	9
0	42	10
1	48	9
2	49	9
3	42	8
4	38	8
5	26	7
6	22	6
7	17	5
8	13	4
9	11	3
10	4	2
11	1	1

Figure A1. Event study plot for packaging waste, Callaway & Sant'Anna

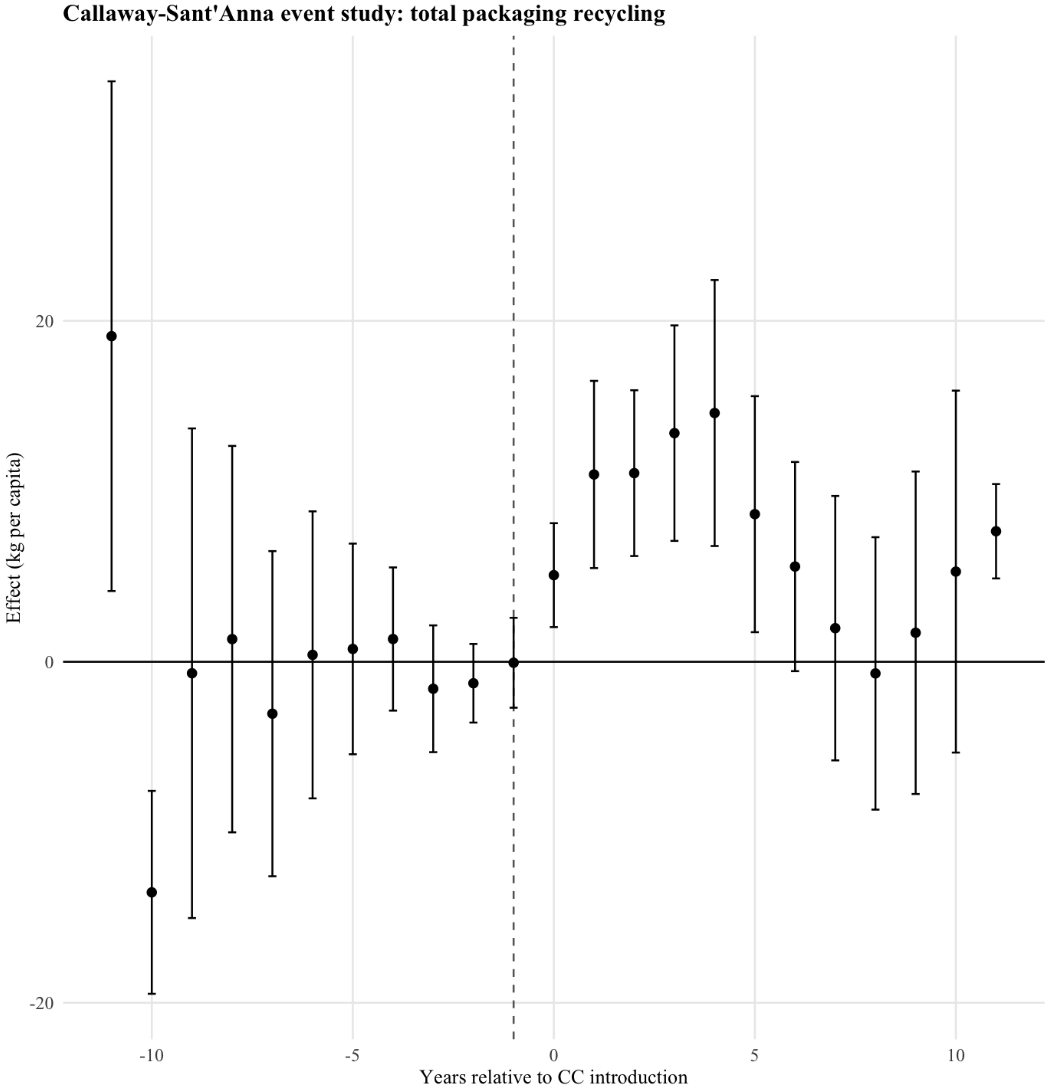
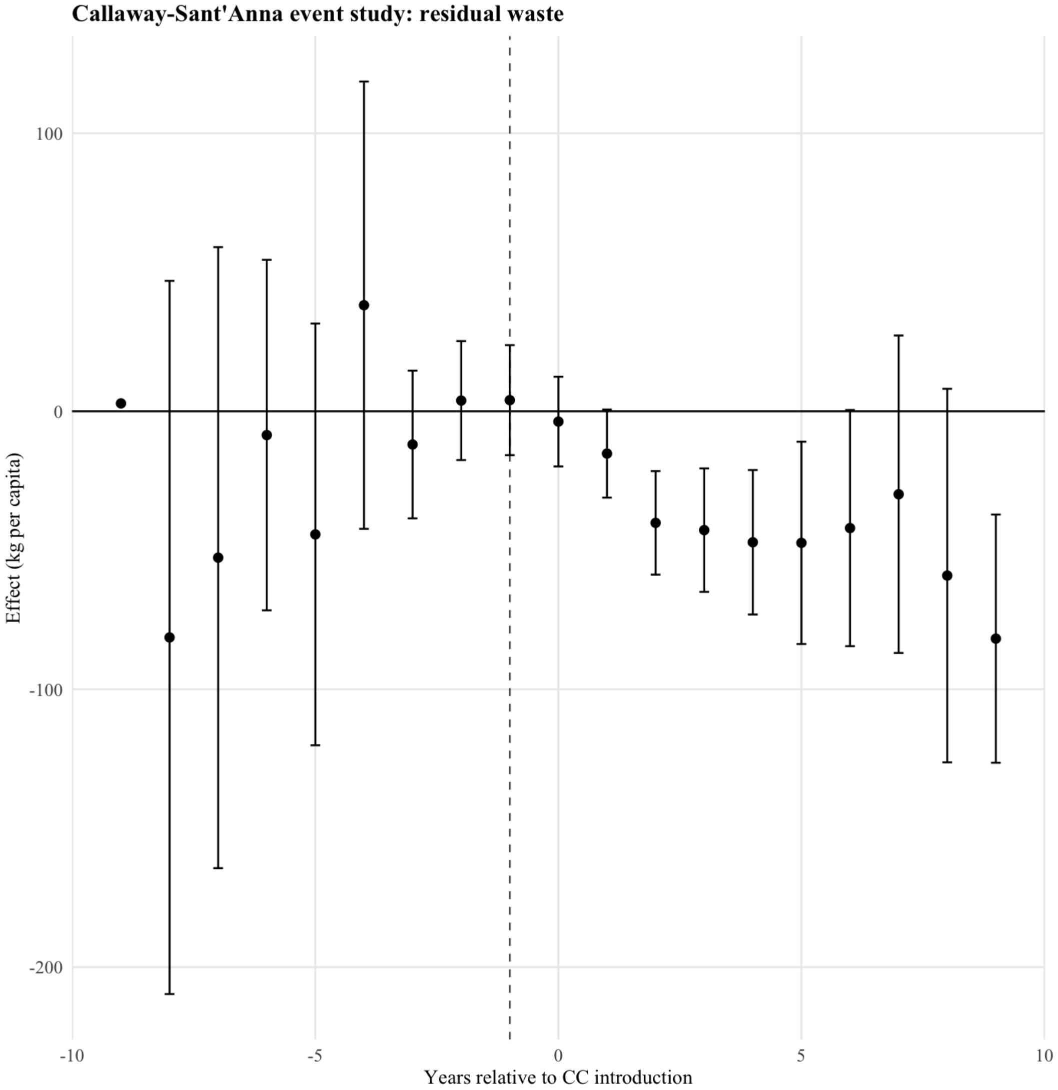


Figure A2. Event study plot for residual waste, Callaway & Sant'Anna



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