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ORIGINAL RESEARCH

The Spending Effects of a Tax Rebate in Italy: A Fuzzy RKD Experiment

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Abstract
This paper examines the spending effects of a popular tax rebate in Italy - the so-called 'Renzi Bonus' (RB) - across four key expenditure categories: total expenditure, food, durables, and other goods and services. We focus on a specific normative income threshold providing a well-defined quasi-experimental setting. A key innovation of our study is the use of a novel administrative-survey linked dataset for Italy which allows for the precise identification of RB recipients, thus improving on previous studies relying solely on survey data. Furthermore, our analysis employs a fuzzy Regression Kink Discontinuity (RKD) design, which leverages the rebate allocation rule to construct a credible quasi-experimental framework. The findings indicate that, at the EUR 24, 000 income kink, the RB does not have a statistically significant effect on household semi-elasticity of consumption. However, positive, albeit limited, effects are detected on the consumption of 'other goods and services' - the residual category remaining after netting out total expenditure for food and durables. These results are consistent across multiple model specifications and robustness checks. Overall, our study contributes to the broader literature on fiscal policy and household economic behaviour by providing robust evidence on the limited short-term spending effects of the RB.

Keywords Regression Kink Discontinuity · Tax rebates · Consumption expenditure

1 Introduction

Tax rebates are tax refunds for specific categories of taxpayers. Such policies reduce the overall tax burden faced by individuals increasing disposable income and, potentially, consumption expenditure. Thus, such policies are commonly used by policymakers as business cycle fluctuation stabilisers (Shapiro & Slemrod, 2009).

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However, their effectiveness, which relates to the broader topic of intertemporal spending choices, is widely debated. According to standard economic theory, consumption expenditure only depends on permanent income, implying that tax rebates are spent only to the extent to which they are perceived as permanent. This is the basic tenet of both life cycle theory (Modigliani & Brumberg, 1954) and the permanent income hypothesis (Friedman, 1957). Both theories indeed frame spending decisions as the outcome of optimal plans over long time horizons in the context of frictionless economies and predict that consumption expenditure reacts to unpredictable income fluctuations influencing future expectations but does not react to predictable - and thus anticipated - income fluctuations. On the other hand, the large body of the literature framing spending choices within a more realistic setting - thus introducing uncertainty, more complex preference structures, and market frictions (e.g., in capital markets) - reaches opposite results. For instance, spending decisions will track income fluctuations more closely when rational consumers face liquidity constraints: being unable to borrow against expected future income, households whose current income is below the life-cycle average are constrained in their spending levels and will thus exhibit large propensities to consume out of transitory income fluctuations (see Deaton, 1991; Jappelli & Pistaferri, 2010; Heathcote et al., 2009, for an extensive review of the main theoretical implications).

The spending response to transitory income fluctuations is also shaped by precautionary savings behaviour (Carroll & Samwick, 1997 and Deidda, 2014 for the Italian case) with more ‘prudent’ households less prone to consume out of the windfall due to buffer stock savings.

From a theoretical standpoint, the spending effects of tax rebates thus depend on the complex interaction of a number of factors as the demographic structure, the working of capital markets, the relevance of liquidity constraints, the preference structures, and many others. Not surprisingly, estimation of the spending effects of tax rebates has been at the heart of a large body of literature (Heim, 2007 for a series of tax rebates between 1995 and 2001, Johnson et al., 2006; Misra and Surico, 2014; Shapiro and Slemrod, 2009 for the 2001 and the 2008 tax rebates, Neri et al., 2017; Andini et al., 2018; Lucchetti et al., 2022).

While methodological approaches differ, US-based studies show that non-durable spending absorbs roughly 25% of the rebates in the same quarter it is received, a finding which is consistent with a determinant role for ‘market frictions’.

Italy introduced a large tax rebate targeted to low and middle incomes - the so-called ‘Renzi Bonus’ – hereafter RB – in May 2014. As will be further specified in Section 2.1 the ‘Renzi Bonus’ was introduced in May 2014 by Decree Law 66/2014 and stayed in place with the same structure until 2020 when it was extensively reformed. In a nutshell, the RB consisted in a flat-rate EUR 80 payment per month for individuals meeting some income requirement. In 2017, the bonus had already been established as a permanent feature of the Italian tax system. Evidence on the spending effects of the Italian tax rebate is mixed.

Lucchetti et al. (2022) use data from the Survey on Household Income and Wealth (SHIW) by the Bank of Italy - which records information on various income sources and synthetic information on consumption expenditure - and a bayesian model averaging propensity score matching approach to estimate the change in various expenditure categories due to the tax rebate in 2014 (the year of introduction). They find that the impact of the tax rebate on spending differs across the expenditure categories considered and is mainly driven by an increase in the expenditure for non-durable goods. However, they also find that such increase is statistically insignificant for all categories considered with a key role of model specification uncertainty.

Neri et al. (2017), using the same dataset and a diff-in-diff approach find a statistically significant effect of the RB on both food and means of transportation. In line with Lucchetti et al. (2022), the effect is greater for non-durable spending category.

Andini et al. (2018), also using SHIW 2014 data, find a positive and statistically significant effect of the RB on food expenditure but not on other spending categories. Their focus, however, is to propose the adoption of machine learning techniques to select beneficiaries to maximise spending effects.

In the present paper, we contribute to the literature by exploring the spending effects in 2017 of the Italian tab rebate - the RB - arguably one of the most debated economic policies of the last decade in Italy. Differently from the previous articles on the topic, our study presents several advancements.

First, the high data quality. We take advantage of a unique dataset - named AD-HBS - which jointly records detailed administrative income information and survey-based information on household sociodemographic characteristics. The administrative data we use is thus an extraction from the universe of tax files (which is available to the Finance Department of the Italian Ministry of Economics). This extraction provides us with the true amount received by each taxpayer, which can greatly differ from the one imputed based on the type of occupation. This information is extremely important for our purposes because the amount received can now be treated as a continuous variable. Additionally, the (monthly) information on consumption expenditure recorded in the HBS is more granular and precise compared to most other datasets (as the SHIW). The AD-HBS dataset is discussed in greater detail in Section 2.2.

Second, the availability of the actual amount received allows us to exploit an exogenous variation in the rebate amount that cannot be easily manipulated. This advantage contrasts sharply with other cutoff-based identification strategies, which often suffer from confounding effects. Consequently, our identification hypothesis and the resulting findings are quite credible. Notably, unlike most of the literature that mainly relies on diff-in-diff experiments (as Heim, 2007) or regressions with the change in spending as dependent variable (as in Shapiro & Slemrod, 2009 or Misra & Surico, 2014) we perform a fuzzy Regression Kink Design (FRKD) experiment exploiting a policy-determined kink in the allocation mechanism of tax rebate. We focus on different spending categories. The categories considered are total expenditure (net of imputed rents), food expenditures, durable goods, and 'other good and services' expenditure, i.e. the remainder after netting total expenditure for food and durables.[1]

Third, we estimate the effect of the rebate on both the households' long- and short-run consumption behaviour, estimating both the change in the semi-elasticity of consumption and the marginal propensity to consume. We find no statistical evidence of a change in the semi-elasticity of consumption for any spending category, while positive effects are detected only on the consumption of 'other goods and services'.

[1] The durable goods category includes means of transportation, home appliances, furniture, and some personal items such as jewelry. A complete list by spending category is available in the appendix. As specified above, total expenditure is net of imputed rents. Including imputed rents arising from real estate possession unrelated to the RB could indeed wrongly amplify the spending effects of the RB.

We argue that a thorough assessment of the RB is crucial in light of the high budgetary cost of the measure (roughly EUR 9.5 billion every year) and its potential impact on macroeconomic aggregates.

The remainder of this paper is organised as follows. Section 2 describes the RB policy and provides an overview of the dataset available, specific attention is devoted to the kinked allocation mechanism that will prove useful to identify the causal effect of the policy. Section 3 discusses the empirical strategy adopted, while Section 5 shows the main results. Several robustness checks are presented in Section 6; finally Section 7 concludes.

2 The Institutional Context and the Dataset

2.1 The Italian Tax Rebate

At the end of April 2014 the Italian government introduced a tax rebate with the idea of countering the long-lasting economic downturn by stimulating internal demand.

The budgetary cost of this policy measure was significant: EUR 6,655 million in 2014 and EUR 9,503 million from 2015 according to the technical reports. Coverage of the RB in the national press was also rather extensive. Specific attention was devoted to the spending effects of the reform and potential equity issues. Since the first years of the policy, different estimation methodologies were proposed with contrasting preliminary results. Gagliarducci and Guiso (2015) propose using a dataset very similar to the one employed in the present paper, but due to time misalignment, the amount of the rebate and the beneficiaries were imputed with substantial imputation errors. Up to 2020, the RB was a monthly-paid EUR 80 flat-rate for employees whose annual total income lay in a fixed interval. It could be received for 12 months a year implying a maximum rebate amount of EUR 960. The RB was established at the individual level and could thus be received by more than one earner per household.

Notably, to be eligible for the full tax rebate an employee - or worker belonging to minor categories assimilated to employees - had to have an annual taxable income, i.e. gross income, between the no-tax-area threshold (EUR 8,145) and EUR 24,000.[2] Differently, employees with an annual taxable income between EUR 24,000 and EUR 26,000 were entitled to a reduced amount. The reduction was linear in income: every additional euro of income entailed a benefit reduction of EUR 0.48.[3] Therefore, we can outline the allocation of the annual rebate as follows:

$$RB = \begin{cases} 960 & \text{if income} \in (8,145; 24,000] \\ \frac{960}{2000}(26,000 - \text{income}) & \text{if income} \in (24,000; 26000) \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

In monthly terms, this translates into a EUR 0.04 in the bonus for every additional euro of income.

[2] Indeed, as pointed out by Neri et al. (2017), the RB acts as a reduction in the so-called ‘tax wedge’ - i.e. the difference between the cost of an employee for the employer and the employee compensation.

[3] See Circolare N. 9/E Agenzia delle Entrate, 14-May-2014, page 8.

The allocation scheme exhibits a jump at 8, 145 and a kink at 24, 000, see Fig. 1. The former coincides with the no-tax area and cannot be used to identify the effect of the RB for at least two reasons. First, there are multiple benefits accruing to individuals having income below that threshold; second, the no-tax area was established in 2002, many years before the RB, which in principle gave rise to the possibility of strategic positioning of taxpayers along the running variable, i.e., taxable income. Conversely, the kink was not announced beforehand, the RB is computed on the basis of income earned in the previous calendar year making manipulation of past employee income unlikely and there are no other benefits and supports at the same value or in a close interval of taxable income. It follows that, after appropriate checks, the kink lends itself to be exploited to empirically identify the effect of the rebate. Originally, the RB was financed only for 2014 as a one-off tax rebate. However, the 2015 budgetary law (Law 193/2014) made the tax rebate permanent. This is particularly relevant to the scope of the present paper since our empirical application refers to 2017 when the permanent nature of the rebate was already established. From a fairness standpoint, three main issues have been pointed out by economists and commentators. First, excluding individuals with annual income below the no-tax-area threshold of EUR 8,145 was problematic. Indeed, considering the average rebate received by equivalent income decile, Baldini et al. (2014) showed that the poorest households would have received much less than those in the middle of the distribution. Second, since eligibility for the rebate was established at the beginning of each year, an individual who has lost his/her job at a given point in time during the calendar year would have to give the money back in the following year. Third, the two income thresholds implied sharp discontinuities in marginal tax rates. The RB was extensively reformed in 2020. Crucially, the amount of the bonus was increased to EUR 100 per month and the eligibility thresholds were increased to EUR 28,000 for the full amount and EUR 40,000 for the partial amount.

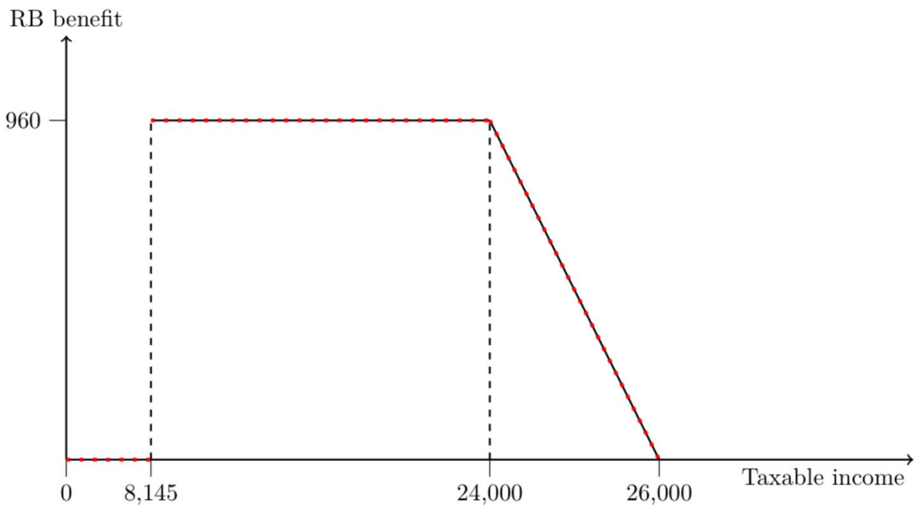


Fig. 1 Theoretical allocation design of the RB

2.2 Data

Understanding if and to what extent the RB policy affects spending behaviour requires detailed data on both individual income - the running variable - used to distinguish eligible and non-eligible individuals - and household consumption expenditure categories - the outcome variables. In accordance with the approach advocated by Gagliarducci and Guiso (2015), we exploit a dataset combining detailed information on household expenditure and fiscal data on income to identify the RB beneficiaries. More specifically, we employ the 2017 wave of the AD-HBS, a novel dataset that was recently developed within a joint research project between the Italian Ministry of Economy and Finance and the Department of Economics and Law of Sapienza University.

The AD-HBS is obtained by merging deterministically various waves of the Italian Household Budget Survey (HBS) with several administrative (AD) archives managed by the National Social Security Institute (INPS) and, for selected waves, also with tax files data managed by the Finance Department of the Ministry of Economy and Finance (Aprea et al., 2023). The matching key is an anonymous fiscal code. The 2017 wave is one of the waves recording personal income tax file data. This type of information is indeed crucial to the scope of our analysis since it allows us to identify RB beneficiaries and RB amounts received by individuals interviewed in the HBS in that specific tax year.[4] On the other hand, the HBS records detailed information on monthly household consumption expenditure classified according to the Classification of individual consumption by purpose (COICOP) system and provides the outcome variables for our analysis. Additionally, the HBS provides a set of socio-demographic variables which may be used as covariates in our RDK estimation framework. Finally, the sampling design of the HBS ensures the sample is representative of the Italian population in the corresponding year.

The 2017 HBS sample has information on 39,365 individuals living in 16,946 households (average household size is just above 2.3 members). Income tax file data are available for 25,697 of these individuals (slightly more than 70% of the HBS sample, which also includes the unemployed and non-working minors). Since the eligibility requirements for the RB are based on individual income, we drop non-matched individuals (mostly children and unemployed persons).

In summary, our data jointly record individual-level income information and household-level information on monthly consumption expenditure and socio-demographic characteristics. Expenditure on goods and services is indeed a household-level decision depending on both household structure (size, age composition, specific needs) and the preferences of household members. In the HBS survey, each household member is assigned the same level of (household) consumption expenditure so that dropping unmatched individuals does not affect our dependent variable. A visual representation of the matching procedure generating the dataset is provided by Fig. 2.

It must be highlighted once again that what we are estimating is the effect of an individual-level policy on a household-level outcome. In this specific setting, estimation results could be biased by two types of confounding factors: the number of subsidies in the household (which determines a higher *intensity* of treatment, and the overall household income, which directly influences consumption expenditure. To address these issues, we limit our

[4] Notice that, in Italy as in most other countries, the income tax file refers to income in the preceding year. In out dataset, the tax year is 2017, so the tax declarations are from 2018.

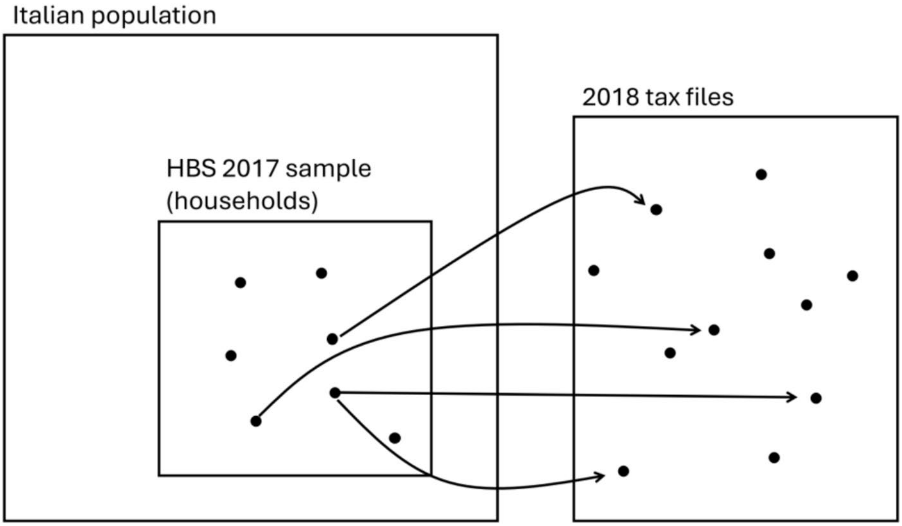


Fig. 2 Data matching procedure: a visual representation. Note: The HBS sample is a representative sample of the Italian population in 2017. Data is matched at the household level, so each dot in the HBS sample may match with more than one 2018 tax file. 2018 tax files record 2107 income

analysis to individuals in single-income households. This additional sample selection kills two birds with one stone: it ensures that consumption expenditure is only influenced by the income of one household member and one rebate.

Finally, in line with our goal of estimating the effects of the RB on several categories of consumption expenditure, our treatment variable only considers the RB amounts effectively received during the fiscal year. Indeed, some of the individuals entitled to the RB do not receive it (or part of it) during the year for various reasons (mostly non-compliance of the employer). These households may claim the benefit with the tax file declaration. To only consider the RB amounts effectively received in the fiscal year we thus compute the treatment variable as the difference between the amount to which the individual is entitled and the amount that can be claimed with the tax declaration. Similarly, our treatment variable does not take into account the amounts of RB received above the entitlements and that should be returned with the tax declaration.

In summary, note that the AD-HBS dataset strongly improves on other potentially available datasets (as the Bank of Italy SHIW data) in three main ways: first, tax file data allow to directly observe the individuals receiving the RB and the exact amount of the rebate. Second, the income variables recorded in the tax files are the same available to the tax authority and are not affected by recall bias. Third, consumption expenditure in the HBS is measured much more precisely than in other datasets.

2.3 Descriptive Statistics

Some descriptive statistics for employees are provided in Table 1. The first five rows refer to the running, treatment, and dependent variables. The following blocks of rows refer instead to some of the socio-demographic information used in the econometric specification.

Table 1 Descriptive evidence by treatment status, single income households

Variables	Not receiving RB	Receiving RB
Fiscal income (EUR)	21,749.5	17,328.7
RB (EUR)	0.0	804.7
Total spending (EUR)	1,590.6	1,766.2
Food spending (EUR)	385.0	395.7
Durables spending (EUR)	105.6	120.9
Other spending (EUR)	1,100.5	1,250.0
Number of household members		
1 member	59.8%	42.7%
2 members	23.1%	21.7%
3 members	8.6%	16.9%
4+ members	8.6%	18.7%
Geographical area of residence		
North	43.4%	45.7%
Centre	19.5%	19.8%
South and Islands	37.1%	34.5%
Municipality type		
Metropolitan city	17.0%	15.6%
Medium-size city	27.6%	28.1%
Small city	55.4%	56.3%
Parents' maximum education level		
Primary	30.3%	3.7%
Lower secondary	22.6%	29.7%
Upper secondary	30.4%	49.6%
Tertiary	16.7%	17.1%
Self-assessed economic condition		
Not adequate	41.4%	48.9%
Adequate	58.6%	51.1%

Notes: the value of total spending slightly differs from the sum of the single components (food, durables, and other) due to missing values in the single components which are replaced by 1

Descriptive evidence is divided by treatment status and refers to the estimation sample of single-income households. Some patterns emerging from the table are worth a brief discussion.

First, the average value of the running variable is higher for non-treated households - i.e. those not receiving RB, the value for households receiving RB is, as expected, within the policy range discussed at the beginning of Section 2. Second, the average RB amount for eligible earners is almost EUR 805, which is close to—but lower than—the maximum amount of EUR 960. This is due to the decreasing amount for incomes greater than EUR 24, 000 and the fact that not all earners have been employed for 12 months. Third, all dependent variables take lower values, on average, for households where the single earner receives the RB. This result may appear counterintuitive, since individuals in the richest households are not entitled to the RB due to the upward income limit (EUR 26, 000). However, it should be pointed out that the poorest individuals (those with a gross income below EUR 8, 145) and the corresponding households are also excluded. Finally, while the distribution of the geographical area of residence and municipality type is rather balanced across treatment status, some sharp differences emerge for the number of household members, the maximum level of education of the parents, and the self-assessed economic condition. More specifically, single-earner households receiving RB tend to be larger than those not receiving RB and tend to be better educated. This pattern has to do with the high presence of pensioners among the single-earner households not receiving the RB. The higher share of households with an inadequate self-perceived economic condition may have the same explanation.

3 Empirical Strategy

As mentioned above, the present paper contributes to the empirical literature on the spending effects of tax rebates by assessing the effects of the RB on four consumption expenditure aggregates. To do that, we exploit the kink in the RB schedule to estimate the impact of the RB on households' spending. Let us denote Y as spending, T as the amount of RB one is entitled to, X as the taxable income for employees (also referred to as the running or forcing variable, or score), and finally, let c represent the specific value of X at the kink, i.e., 24, 000. The marginal effect of T on Y, i.e., $Y'(T)$ is a key policy parameter as it measures the variation in outcome Y in response to a unit variation in the amount of the treatment received. Card et al. (2015) show that this quantity identifies the treatment-on-the-treated effect as defined by Florens et al. (2008) or equivalently the so-called local average response, as defined by Altonji and Matzkin (2005). However, the empirical derivative of $E[Y|T]$ with respect to T does not immediately identify this parameter because of the endogeneity of T. Luckily, for $X = c$ the change in T is exogenously given by law, that is as saying that at the kink the change in the amount of RB is as good as randomly assigned and this discontinuity can provide us with the basis to identify the policy-relevant parameter. Indeed, the change in slope of $E[Y|X]$ at X=c is due only to the change in slope of $T(X)$, hence, it is proportional to the average marginal effect of T on Y with a 'known' coefficient of proportionality. It follows that the marginal effect of T on Y can be recovered as the ratio of two changes in slope:

$$E[Y'(T)|X = c] = \frac{E'[Y|X = c^+] - E'[Y|X = c^-]}{T'(c^+) - T'(c^-)} \tag{2}$$

where $E'[Y|X] = \frac{\partial E[Y|X]}{\partial X}$ and similarly $T'(X) = \frac{\partial T}{\partial X}$.

In principle, according to the monthly equivalent of Eq. 1, $T(X)$ is a deterministic function of income, X, and the denominator of Eq. 2 is known and equal to $960/2000 = -0.48$, meaning that each extra euro of income with respect to 24, 000 generates a yearly drop of EUR 0.48 of the rebate. In monthly terms, the drop is equal to $(960/12)/2000 = -0.04$. However, there are some exceptions to the allocation mechanism which make the denominator no longer deterministic and its value must be estimated from the data, in other words, there is no perfect compliance and the set up is of the fuzzy type. Specifically, the RB is paid out in year t as a function of total taxable annual income earned in the same year. During the year, the total amount that will be earned by the end of the period is not perfectly known, and its prediction is based on income at $t - 1$, but some changes over time may occur. This can lead to discrepancies between the amount actually paid and the theoretical amount as in Eq. 1. To illustrate a case of non-perfect compliance, let us consider an employee who earned €23, 000 at time $t - 1$ and €27, 000 at time t. If the employer does not promptly adjust the benefit to the new income, the employee will receive over the year t an amount of €960 (€80 per month). A similar situation occurs for individuals whose income drops below the kink from a higher level. Of course, over and underpayments would be adjusted by the tax authorities in the following year.

From an empirical point of view the numerator of Eq. 2 is given by α_1 of the following regression equation estimated in a narrow bandwidth around the kink:

$$Y_i = \alpha_0 + \alpha_1(Z_i * x_i) + \alpha_2 f(x_i) + \varepsilon_i \tag{3}$$

and the denominator is given by β_1 of the following regression equation estimated in the same narrow interval:

$$T_i = \beta_0 + \beta_1(Z_i * x_i) + \beta_2 f(x_i) + u_i \tag{4}$$

for $i = 1, \ldots, N$, where $x = X - c$ is the normalised or centered running variable, $Z = \mathbb{1}\{x \geq 0\}$ and $f(x_i)$ is a power function of x_i and its interaction with Z_i intended to capture nonlinearities. Analogously to the popular LATE interpretation, Imbens and Angrist (1994) in the fuzzy setup RK estimate will be the local average response among compliers - i.e., employees who experience a kink, with non-zero $T'(c^+) - T'(c^-)$. The coefficient α_1 measures the change in (log)spending, *Y*, for a unit increase of income above 24, 000 in a narrow interval around the kink. Given that the rebate changes by β_1 for each additional euro of income above the kink, we can interpret the ratio $\frac{\alpha_1}{\beta 1}$ as the change in (log)spending for a change in the rebate of β_1, see Deiana et al. (2022). Since *Y* is in log the estimate measures the semi-elasticity of consumption.

Equations 2 - 4 represent the baseline model that we also re-run including only those predeterminate covariates that increase precision of the estimates.

Finally, we run a battery of robustness checks. Starting from our baseline model structure, among other things, we change: i) covariates inclusion, ii) the order of the polynomial used to approximate the slope of the outcome variable on each side of the threshold, iii) the bandwidth, iv) the kernel type, and v) we cluster the standard errors according to two different variables, vi) the kink. The details and the main results are presented in Section 6.

3.1 The Identification Hypothesis

In order to validly estimate the treatment effect at the kink some identifying assumptions must hold. First, there must be no sorting at the kink, that is, employees must not have manipulated their income with the intention of positioning themselves on either side of the kink. This occurrence would invalidate the "as if randomly assigned" condition. In our case manipulation, or sorting, is unlikely as employees do not self-report income, but rather it is reported by employers directly to the national fiscal authorities. In addition, employees' income is determined by national labour contracts based on the type of work, the worker's qualification and their length of service, all aspects independent of the RB, leaving no room for manipulation. However, just to make sure of consistency between the data and the institutional setup we also proceed to formally test this identifying condition by means of the McCrary (2008) manipulation test and its subsequent developments by Cattaneo et al. (2018) and Cattaneo et al. (2020) which substantially compare the conditional probability density function at the left- and at the right-hand-side of the kink. A discontinuity of the density is taken as a signal in favour of manipulation.

A second important identifying assumption consists in the continuity of the slope of the conditional distribution of the pre-determined covariates at the kink. Testing this condition is straightforward and it can be accomplished by estimating Eq. 2 for each covariate, instead of Y. In other words, we must substitute Y by pre-determined covariates, one at a time, in Eq. 3 and testing $\alpha_1/\beta_1 = 0$.

A third important condition is analogous to the monotonicity in the LATE framework, which properly adapted to the RKD setup claims that the direction of the kink is either non-negative or nonpositive for the entire population - i.e. the sign of $T'(c+) - T'(c-)$ does not vary across units, i.e. either higher income generates lower T or *viceversa*. Monotonicity implies that there may be heterogeneity in the size of the kink across individuals (for this reason the first stage must be estimated from the data) yet the direction of the kink must be the same for everybody in the population, ruling out situations where some individuals experience a negative kink at $X = 24,000$, but others experience a positive kink at $X = 24,000$. As pointed out by Card et al. (2015) and Simonsen et al. (2016), monotonicity can be restated as implying that agents must not have full control of the forcing variable, x, which is indeed our case. Put differently, monotonicity rules out the existence of *defiers*, those who receive the opposite treatment to the one they are assigned, that is a plausible assumption.

4 Graphical Inspection and Tests of the Identification Hypotheses

In this section, we examine whether the identification hypotheses discussed above hold true in the data and whether the intuition underlying the RKD adapts well to the dataset available. As a first step, we check if the kinked allocation design that we exploit as a key identification point shows up in the data. To this aim, Fig. 3 plots the amount of RB tax rebates received by eligible households against the running variable. In case of perfect allocation, i.e., sharp design, all points in the figure would lie on the flat branch of the line in correspondence of EUR 960 to the left of the kink and on a decreasing line with slope $-\frac{dy}{dx}$ of $\frac{960}{2000} = -0.48$ to the right of EUR 24, 000, exactly as in Fig. 1. Its empirical counterpart is given by Fig. 3 where the points lie very close to, or in some cases even on, these hypothetical "policy" line resembling very closely Fig. 1, with some limited variation due to the fuzziness of the allocation mechanism.

Once we have confirmed the presence of the kink in the data and thus the appropriateness of the RKD estimator, we proceed further in the graphical analysis by looking at the change in the slope of the relationship between spending, Y, and income at the kink; in other words, we want to graphically visualise a change in $E'[Y|X = c]$.

Figure 4 plots spending for all goods and services (panel a), food (panel b), durables (panel c), and other goods and services (panel d) as a function of annual taxable income. The dots represent average spending computed over evenly spaced bins of EUR 2, 000 and the continuous lines represent linear interpolations of such average values. In all the cases, expenditure is increasing up to EUR 24,000 (especially for food and other spending components) and levels off afterward. For food and durables components, the trend

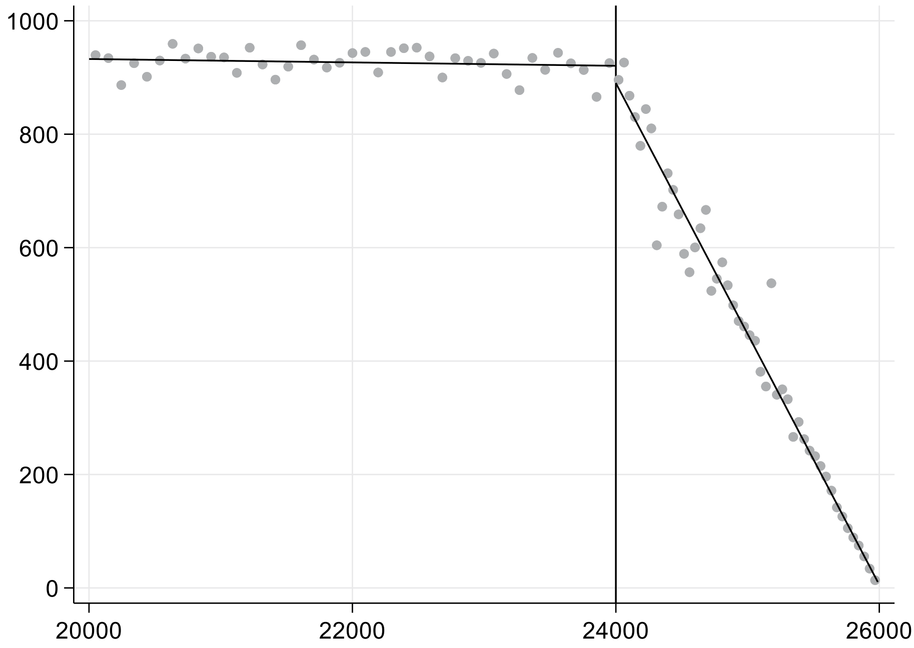
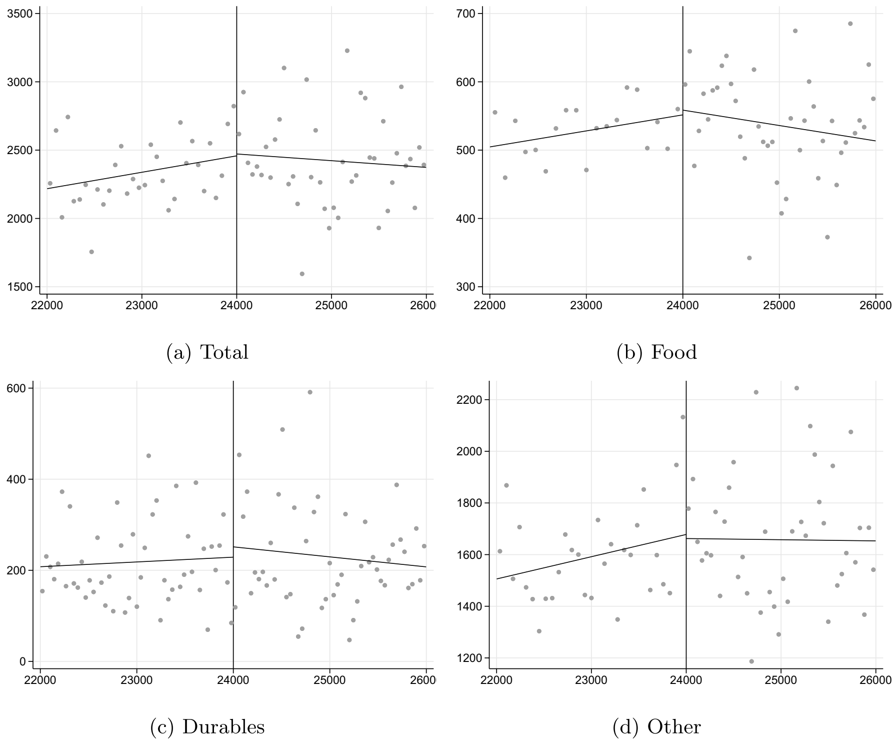


Fig. 3 Empirical allocation design. Note: the figure is the empirical counterpart of Fig. 1 representing the empirical take up of the policy. On the x-axis taxable income (the running variable), on the y-axis the amount of RB

turns decreasing to the right of the cutoff. At first sight, these figures seem to be consistent with the decreasing amount of RB received by consumers earning more than EUR 24,000. However, one must be cautious in considering this as evidence of a 'policy' effect as many confounding factors may be at work. The results of our formal estimations are presented in the following section.

We now proceed to test the hypothesis of continuity of the running variable around the policy threshold. Figure 5 reports graphical evidence of continuity of the density of the running variable around the kink point, as suggested by McCrary (2008) and implemented by Cattaneo et al. (2018) and Cattaneo et al. (2020), showing no sign of jumps around EUR 24,000 (highlighted by the red vertical line). More formally, the null hypothesis of no manipulation cannot be rejected with a p-value equal to 0.852. Further additional visual inspection of the continuity of the running variable in the range relevant to this work is provided by Fig. 6 in the Appendix.

The second test consists in detecting treatment effects in pre-determined covariates. For every pre-determinate covariate, the balancing test checks for slope changes around the policy threshold in the running variable (EUR 24,000). Table 2 reports the test. Column 3 of Table 2 reports the robust p-values of the test showing that we always reject the null of treatment effects on the predeterminate covariates, corroborating the identification hypothesis.



(a) Total (b) Food

(c) Durables (d) Other

Fig. 4 Rdplots, different spending categories, cutoff at EUR 24,000

5 Results

We present hereafter the main results of our estimation approach. We estimate two baseline models for each outcome variable: one without covariates and one including the covariates resulting from a selection procedure we will describe shortly. Several robustness checks are reported in Section 6.

For each model, we present the results for both the first and the second stage. Recall that the first stage measures the effect of the running variable on the treatment variable. Non-significant first stage estimates would thus imply that the running variable does not impact the (average) amount of RB received and, consequently, impair the whole identification approach. Regression tables also include information on the number of observations, the effective number of observations used for the estimation left and right of the cutoff point, the kernel and the bandwidth type, and the bandwidth size. Robust standard errors for both the first and the second stage estimates are reported in parentheses beneath each regression coefficient. As usual, stars refer to different confidence levels.

The results for the baseline models without covariates are presented in Table 3, broken down by expenditure category. Specifically, each column refers to the (log) of each consumption category used. The procedure for choosing the kernel and bandwidth type has been discussed in Section 3.

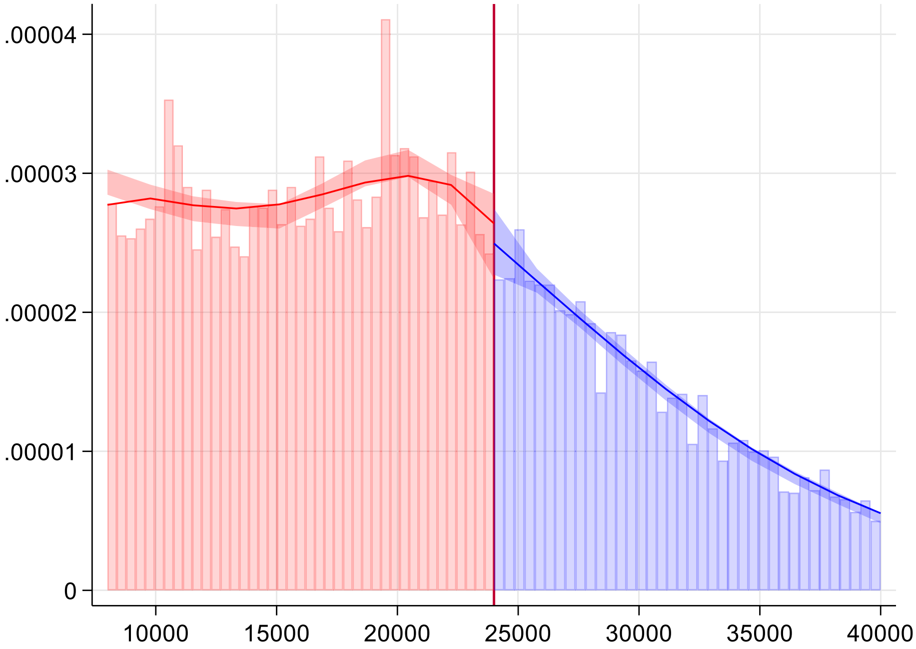


Fig. 5 McCrary test for manipulation of the running variable. Note: the figure reports graphical evidence of continuity of the density of the running variable around the kink point, as suggested by McCrary (2008) and implemented by Cattaneo et al. (2018) and Cattaneo et al. (2020)

The first key result is that the first stage coefficients are negative and statistically significant for all dependent variables. This indicates that the RB has a measurable discontinuity at the threshold, which is a necessary condition for identifying treatment effects. Additionally, note that first stage coefficients change across the outcomes and this is because the optimal bandwidth changes and, accordingly, the number of observations. All coefficients, however, exhibit the expected negative sign: an increase in the running variable generates a reduction in the amount of treatment received. Also, for smaller bandwidths, the point estimate is closer to the theoretical value of -0.04 (considering the monthly reduction in the bonus).

RK estimates capture the causal effect of the RB treatment on (log) consumption expenditure and can be interpreted as semi-elasticities, that is, as the percentage change in the outcome variable associated with a marginal variation in the rebate equal to the first-stage coefficient (approximately EUR 0.015). For interpretative clarity, it is useful to recall that RK coefficients are obtained as a ratio (Section 3).

The denominator of this ratio coincides with the first stage estimate and is consistently negative. This reflects the structure of the rebate schedule, which mechanically reduces the marginal amount of the rebate as individual taxable income increases around the kink.

The numerator captures the local change in the slope of the (log) consumption-income relationship at the kink and corresponds to the coefficient β_1 in Eq. 3. Formally, it measures the difference between the right- and left-hand derivatives of expected (log) consumption with respect to income. This quantity should not be interpreted as a marginal propensity to consume; rather, it reflects a local difference in the income-consumption gradient between relatively richer and relatively poorer households within the optimal bandwidth - i.e., a

Table 2 Balancing test on pre-determinate covariates

Covariate	RK coeff.	Robust p-value
2 hh members	0	0.693
4+ hh members	0	0.730
2+ over 67 members	0	0.335
2+ minors	0	0.411
0 under 3 members	0	0.132
1 under 3 member	0	0.161
2+ under 3 members	0	0.512
Foreigners	0	0.145
Married couple	0	0.955
1+ unemployed	0	0.546
1+ inactive	0	0.431
North	0	0.546
Centre	0	0.365
South and Islands	0	0.349
Large city	0	0.338
Medium city	0	0.379
Small city	0	0.156
Primary education	0	0.778
Lower secondary education	0	0.793
Upper secondary education	0	0.723
Tertiary education	0	0.565
Manages to save	0	0.589
Adequate economic condition	0	0.398
Improved economic condition	0	0.997
Renters	0	0.923
Has property	0	0.462
Has property above family home	0	0.251
Income losses	0	0.299

Note: optimal data-driven bandwidth according to the Coverage Error Rate criterion (CER). *2 hh members* takes value 1 if the household has two members and zero otherwise; the same interpretation holds for *4+ hh members*, *2+ over 67 members*, *2+ minors*, *0 under 3 members*, *1 under 3 members*, *2+ under 3 members*; *Foreigners* takes value one if all household members are foreign-born and zero otherwise; *Married couple* takes value one if the spouses in the household are married and zero otherwise; *1+ unemployed* and *1+ inactive* take value one if, respectively, there is at least 1 unemployed or inactive member in the household; *North*, *Centre*, *South* and *Large city*, *Medium city*, *Small city* take value one or zero depending on the geographical area of residence and the size of the city of residence; *Primary education*, *Lower secondary education*, *Upper secondary education*, and *Tertiary education* take value one or zero depending on the maximum level of education reached within the household; *Manages to save* takes value one is the interviewee self-declares to manage to save some money every month; *Adequate economic condition*, and *Improved economic condition* take value one if the interviewee self-declares the economic situation of the household to be, respectively, 'at least adequate' or 'improved'; *Renters* takes value one if the household lives in a rented home; *Has property* and *Has property above family home* take value one if the household has receives income, respectively, from any property or from property above the family home; *Income losses* takes value one if the household incurred income losses (self-employed)

deeper parameter of the expenditure-income relationship. In line with standard consumption theory, this gradient is typically lower for households with higher income, especially for basic expenditure categories such as food.

Table 3 Baseline model without covariates

	(0)	(1)	(2)	(3)
Outcomes	Total	Food	Durables	Other
First stage	−0.01886***	−0.01604***	−0.01555***	−0.01943***
	(0.00215)	(0.0016)	(0.0015)	(0.00231)
RK coef.	0.00255	0.00465	−0.00332	0.00124
	(0.00229)	(0.00324)	(0.00747)	(0.00260)
Observations	8,305	8,305	8,305	8,305
Eff. Number of obs left	2259	2749	2853	2149
Eff. Number of obs right	1338	1527	1579	1303
Kernel Type	Triangular	Triangular	Triangular	Triangular
BW Type	cerrd	cerrd	cerrd	cerrd
Order Loc. Poly. (p)	2	2	2	2
Symmetric BW	8502	10298	10803	8129

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Since both the numerator and the denominator are negative, the resulting RK estimates are positive. They quantify the percentage increase in consumption expenditure associated with a marginal increase in the rebate of approximately EUR 0.015 induced by the policy kink. For example, Table 3 reports an estimated semi-elasticity of 0.255% for total expenditure and 0.465% for food expenditure. An analogous interpretation applies to the remaining coefficients.

Results in Table 3, however, suggest that the treatment effect is not statistically significant broadly in line with Lucchetti et al. (2022). Point estimates are positive for total expenditure, food and other expenditure (0.255%, +0.465% and +0.124%) and negative for durables expenditure (-0.332%).

Even though not statistically significant, the negative estimate for durables may appear puzzling at first, as it would suggest a decline in expenditure following receipt of the RB. However, this result is more plausibly explained by the inherently volatile and lumpy nature of durable consumption. In addition, most of the items included in this category - listed in Appendix A.1 - are measured in the HBS with reference to expenditure in the preceding 6 to 12 months. This introduces a potential misalignment between the timing of bonus receipt and the period over which expenditure is recorded, thereby increasing measurement noise and contributing to the instability of estimates for durables.

In order to increase the precision of the estimates, we take advantage of the covariates in Table 2 selecting only those reducing the confidence interval as in Calonico et al. (2017).

First, to reduce the number of possible combinations, we group the covariates into five different categories according to their reference area. We then test them groupwise in an algorithm designed to select the 'best' combination of groups of covariates.[5] The idea underlying the algorithm is the following: first, for a given set of n variables (or groups of variables), define the total number of unique combinations of sizes 1 to n. This number is given by

$$\text{Total combinations} = \sum_{k=1}^{n} \binom{n}{k}$$

which simplifies to $2^n - 1$. According to the above formula, our five groups of covariates translate into $2^5 - 1 = 31$ combinations. Each of these 31 combinations are then plugged into the RKD model for the four outcome variables of interest.[6] The chosen combination of covariates maximises the reduction in the standard error of the estimated treatment effect. The results of the covariate-augmented estimates are presented in Table 4.

Concerning the first stage, covariate inclusion does not substantially alter the results.

In spite of the decrease in the standard errors determined by the covariate-selection mechanism treatment effect coefficients remain small in magnitude and statistically insignificant across all expenditure categories, indicating limited evidence of a treatment effect. The negligible changes in the magnitude and significance of the coefficients suggest that the results in Table 3 are robust to the inclusion of covariates.

In summary, while the inclusion of covariates marginally refines the estimates by reducing standard errors, it does not fundamentally change the interpretation of the results.

6 Robustness Checks

6.1 Further Evidence

To show that the main implications of the estimates presented in Section 5 do not depend on specific model assumptions, we run a series of robustness checks. More specifically, for each dependent variable, we change i) the order of the local polynomial used to construct the point estimator, ii) the optimal bandwidth algorithm, and iii) the kernel function used to construct the local-polynomial estimator. Additionally, we allow for clustering of errors and falsification tests. Tables 5, 6, 7, and 8 are organized by consumption category and report the different perturbations of the benchmark by columns. Differently, Tables 9 and 10 report the placebo thresholds and the breakdown by household characteristic for each consump-

[5] The five categories of covariates refer to: demographic information, labour market position of household members, geographical information, maximum education level of household members, and economic conditions of the household.

[6] An alternative strategy would be to avoid grouping covariates and test each one by itself. The main issue with this approach is that, given the exponential nature of the total number of combinations, the models to estimate increase too quickly as the number of covariates to test increases

Table 4 Baseline model with covariates

	(0)	(1)	(2)	(3)
Outcomes	Total	Food	Durables	Other
First stage	−0.01436***	−0.01437***	−0.01545***	−0.01412***
	(0.00131)	(0.0013)	(0.00146)	(0.00125)
RK coef.	0.00274	0.00406	−0.00294	0.00301
	(0.00189)	(0.00312)	(0.00734)	(0.00208)
Observations	8,305	8,305	8,305	8,305
Eff. Number of obs left	2969	2983	2894	2969
Eff. Number of obs right	1924	1626	1597	1624
Kernel Type	Triangular	Triangular	Triangular	Triangular
BW Type	cerrd	cerrd	cerrd	cerrd
Order Loc. Poly. (p)	2	2	2	2
Symmetric BW	11299	11343	10984	11300

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 5 Robustness checks, total expenditure, no covariates

Type of check	p(1)	Alternative bandwidth			Alternative kernel		Error clustering	
		mserd	msetwo	certwo	Uniform	Epanechnikov	Province	hh members
RK coef.	0.00200	0.00423*	0.00444	0.00381	0.00253	0.00253	0.00358	0.00326***
	(0.00297)	(0.00173)	(0.00279)	(0.00444)	(0.00226)	(0.00235)	(0.00208)	(0.000976)
Observations	8,305	8,305	8,305	8,305	8,305	8,305	8,305	8,305

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

tion category, respectively. The first stages are largely consistent throughout the alternative specifications and omitted for sake of space.

Similar exercises including covariates are reported in the Appendix from Table 12 to Table 15.

In greater detail, the first column across Tables 5 through 8 uses a local polynomial of order one to construct the point estimator. The second through fourth columns employ different bandwidth-selection algorithms: symmetric Mean Squared Error (*mserd*), asymmet-

Table 6 Robustness checks, food expenditure, no covariates

Type of check	p(1)	Alternative bandwidth			Alternative kernel		Error clustering	
		mserd	msetwo	certwo	Uniform	Epanechnikov	Province	hh members
RK coef.	0.00599	0.00123	000356	000956	0.00294	0.00440	0.00430	0.00277
	(0.00383)	(0.00260)	(0.00377)	(0.00503)	(0.00350)	(0.00334)	(0.00281)	(0.00237)
Observations	8,305	8,305	8,305	8,305	8,305	8,305	8,305	8,305

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 7 Robustness checks, durable goods expenditure, no covariates

Type of check	p(1)	Alternative bandwidth			Alternative kernel		Error clustering	
		mserd	msetwo	certwo	Uniform	Epanechnikov	Province	hh members
RK coef.	−000753	0.00256	0.00290	−0.00290	−0.00815	−0.00454	−000586	000764
	(0.00859)	(0.00650)	(0.00905)	(0.0120)	(0.00860)	(0.00784)	(0.00581)	(0.00624)
Observations	8,305	8,305	8,305	8,305	8,305	8,305	8,305	8,305

Robust Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 8 Robustness checks, other expenditure, no covariates

Type of check	p(1)	Alternative bandwidth			Alternative kernel		Error clustering	
		mserd	msetwo	certwo	Uniform	Epanechnikov	Province	hh members
RK coef.	−000134	0.00508**	0.00476	0.00298	0.00267	0.00146	0.00338	0.00461***
	(0.00327)	(0.00190)	(0.00333)	(0.00582)	(0.00255)	(0.00258)	(0.00239)	(0.000939)
Observations	8,305	8,305	8,305	8,305	8,305	8,305	8,305	8,305

Robust Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 9 Regression kink estimates at placebo thresholds

Kink	Total		Food		Durables		Other	
	RK coef.	P-val.	RK coef.	P-val.	RK coef.	P-val.	RK coef.	P-val.
EUR 20,000	−0.016	0.244	−0.047	0.551	0.016	0.500	−0.009	0.198
EUR 22,000	0.001	0.916	−0.002	0.208	−0.003	0.460	0.002	0.379
EUR 26,000	−0.004	0.180	−0.001	0.853	−0.003	0.868	−0.005	0.138
EUR 28,000	0.001	0.511	0.004	0.239	−0.005	0.643	0.003	0.342

ric Mean Squared Error (*msetwo*), and asymmetric Coverage Error Rate (*certwo*). The fifth and sixth columns use the Uniform and Epanechnikov kernel functions, while the last two columns cluster standard errors by province and the number of household members.

Table 10 Regression kink estimates by household characteristics

Condition	Obs.	Total		Food		Durables		Other	
		RK coef.	P-val.	RK coef.	P-val.	RK coef.	P-val.	RK coef.	P-val.
Minors	1,395	−0.002	0.546	−0.003	0.636	−0.018	0.229	0.000	0.832
No minors	6,910	0.004	0.121	0.006	0.172	−0.003	0.703	0.002	0.643
Adequate	4,741	0.004	0.244	0.004	0.277	−0.004	0.671	0.004	0.313
Inadequate	3,564	0.003	0.387	0.005	0.453	−0.001	0.904	0.002	0.596
Not 'old'	5,424	0.000	0.903	0.003	0.447	−0.003	0.694	0.000	0.848

Despite some qualitative differences with respect to the baseline estimates, the robustness checks confirm the main result of non-significant effect of the RB on consumption expenditure at the EUR 24,000 kink for single-earner households. The only exception seems to concern 'other' expenditure - i.e., expenditure for all non-durable goods different from food. In fact, for this specific spending category, the effect of the RB is statistically significant in two cases (with the *mserd* bandwidth-selection algorithm and when clustering errors according to the number of household members). In qualitative terms, the effect has the same (positive) sign of the baseline estimates, but the magnitude is slightly higher. More precisely, while in the baseline model a EUR 0.015 increase in the rebate leads to an increase in consumption expenditure of 0.124%, results in Table 8 indicate values between 0.461% and 0.508%.

The results of this robustness test battery broadly align with the results of our main specifications. The general picture is that the coefficients remain small and insignificant in all but a few cases. Seemingly, there are few estimates that seem to point to significant results, but this is to be expected because we run so many different regression specifications that the results should fluctuate into positive and negative regions by mere chance in some cases. Overall, the results do not reliably present seizable effects of the policy. In addition to that, even if one wanted to take at face value the statistical significance, these results would be negligible in economic terms.

We also complement the analysis by re-estimating the models at different 'placebo' kinks – i.e., thresholds of the running variable unrelated to the policy allocation rule. The results of this exercise for four placebo kinks are reported in Table 9. Consistent with the baseline specification, all placebo estimates are statistically insignificant, supporting the absence of spurious slope changes at non-policy thresholds.

Finally, we examine whether the main finding – namely, the absence of a statistically significant kink in the consumption–income relationship at the EUR 24, 000 policy threshold – varies across household subgroups. Table 10 presents the results of the models estimated by subgroups with specific reference to i) the presence of minors in the household, ii) the self-perceived level of economic conditions, and iii) the household composition in terms of old-age members. Specifically, for what concerns this last subgroup, we exclude all households whose members are all aged 67 or above. These households are indeed largely not eligible for the RB and may have specific consumption patterns.

Across all subgroups and consumption categories, the estimated effects remain statistically insignificant, corroborating the baseline result and suggesting that the absence of detectable slope changes is not driven by specific household characteristics.

6.2 How is the Bonus Spent?

The evidence presented so far relies on specifications in which the four expenditure aggregates of interest enter in logarithms. Since the rebate was introduced as a long-run policy, the use of the log is meant to capture long-run effects by estimating the semi-elasticity of consumption with respect to income. However, the rebate amount is small compared to the average income around the kink, and its percentage impact on consumption is likely too small to be detected within this framework.

This interpretation helps explain why our benchmark estimates - and most of the additional robustness checks - yield non-significant local effects of the bonus at the kink.

To investigate instead whether the bonus affects, at least to some extent, the level of consumption expenditure - i.e., the marginal propensity to consume - we re-estimate the model using the ‘levels’ counterparts of the dependent variables used in the baseline models. In this ‘levels’ specification, the discontinuity in the slope of the consumption–income relationship thus corresponds to an absolute variation rather than a proportional one, making the design more sensitive to small changes generated by the rebate. The main results are reported in Table 11.

The first stage coefficients are always negative and statistically significant, however, in this specification the RK coefficients turn statistically significant for ’other goods’, that, in turn, makes significant also total expenditure. Conversely, expenditures for food and, especially, for durable goods are not sensitive to the benefit. This pattern is consistent with the notion that households located around EUR 24, 000 face no binding constraints on essential food expenditure, and therefore allocate the rebate to less primary items, such as: clothing, personal services, or leisure-related goods. Taken together, these results suggest that the rebate does not alter the way households adjust consumption to proportional income variations (elasticity), but it does raise consumption levels - although by a small amount (less than EUR 9). This indicates a higher marginal propensity to consume at the kink.

Table 11 Baseline model without covariates - levels

	(0)	(1)	(2)	(3)
Outcomes	Total	Food	Durables	Other
First stage	−0.01517***	−0.0148***	−0.01522***	−0.01574 ***
	(0.00144)	(0.00137)	(0.00145)	(0.00154)
RK coef.	8.423**	1.705	0.858	5.365*
	(3.885)	(0.982)	(1.260)	(2.936)
Observations	8,305	8,305	8,305	8,305
Eff. Number of obs left	2925	3021	2912	2810
Eff. Number of obs right	1610	1644	1603	1554
Kernel Type	Triangular	Triangular	Triangular	Triangular
BW Type	cerrd	cerrd	cerrd	cerrd
Order Loc. Poly. (p)	2	2	2	2
Symmetric BW	11143	11541	11087	10621

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

7 Conclusions

In this paper, we study the effects of an important tax rebate in Italy – the so-called *Renzi Bonus* – on the main categories of consumption expenditure. Assessing the spending effects of a tax rebate is a crucial task, as such policies are often introduced to stimulate household spending and economic growth, implying a non-negligible effort in terms of public finances. Understanding whether these policies achieve their intended effects is essential for designing effective fiscal interventions and for assessing their impact on well-being. This study exploits a strong identification setup, i.e the FRKD, which, on the one side, allows to credibly identify the effect of the bonus, while on the other side, restricts attention to a specific point of the income distribution (EUR 24, 000) and a sub-population of the beneficiaries, single earner households. Put another way, the specificity of the results is the price to be paid for a valid identification setup.

Another contribution of our work is the use of a novel administrative-survey linked dataset for Italy, the AD-HBS, which was developed within a joint research project between the Italian Ministry of Economy and Finance and the Department of Economics and Law of Sapienza University and greatly improves on other available datasets. A major advantage of this dataset is the precise identification of RB beneficiaries. The results indicate that the RB does not have a statistically significant effect on the semi-elasticity of consumption of any of the spending categories considered, while it has a limited effect on the marginal propensity to consume of ‘other goods and services’, including clothing, personal services, or leisure-related goods. This finding is consistent with the fact that households with an income around EUR 24, 000 do not suffer from binding constraints on food and other primary needs, so they can afford spending the rebate in less primary goods and services. As a consequence, the results point to the absence of a change in the long-term structure of households’ demand, the semi-elasticity, in favor of a short-term effect, namely a small increase in the level of consumption.

Some issues remain open and deserve further investigation. We dropped from the sample households with more than one income earner, for a correct identification of the effect. However, in principle, individual-level targeting poses some equity issues if *individual* well-being also depends on *household* disposable income. To see why, let us consider the following RB-related example. A family with two income earners, one earning EUR 23, 000 and the other EUR 200, 000 would receive EUR 960 of RB, despite having an overall income of EUR 223, 000. By contrast, a family with two earners with income of EUR 27, 000 each, is not eligible for the support despite having a much lower household income (EUR 54, 000). As the example shows, these equity issues are inversely related to the degree of mating homogamy - i.e., the tendency of individuals to mate with ‘similar’ partners.

Overall, our study contributes to the broad literature on fiscal policy and household behavior by providing robust evidence on the limited short-term spending effects of the ’Renzi Bonus’. The methodological approach and data employed in this paper offer valuable insights for evaluating similar income-support policies in other contexts. For future research, it would be relevant to extend the analysis to different household structures and income levels, also studying heterogeneous effects among different occupational types.

A Appendix

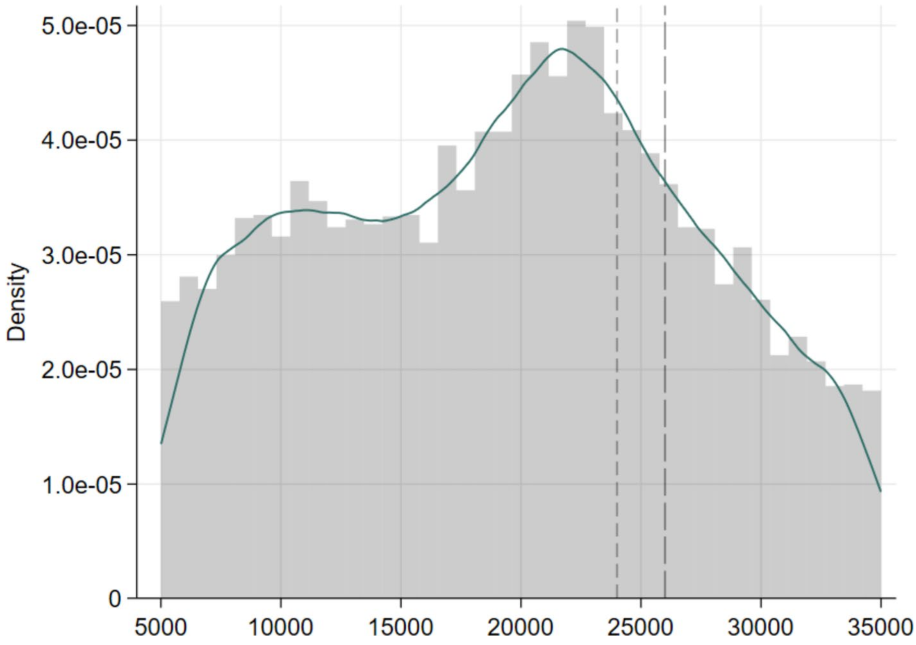


Fig. 6 Continuity of the running variable

Table 12 Robustness checks, food expenditure, with covariates

Type of check	p(1)	Alternative bandwidth			Alternative kernel		Error clustering	
		mserd	msetwo	certwo	Uniform	Epanechnikov	Province	hh members
RD_Estimate	0.00479	−1.40e−05	−000722	0.00202	0.00465	0.00370	0.00298	0.00217
	(0.00351)	(0.00294)	(0.00492)	(0.00624)	(0.00367)	(0.00317)	(0.00278)	(0.00279)
Observations	8,305	8,305	8,305	8,305	8,305	8,305	8,305	8,305

Robust Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 13 Robustness checks, durable goods expenditure, with covariates

Type of check	p(1)	Alternative bandwidth			Alternative kernel		Error clustering	
		mserd	msetwo	certwo	Uniform	Epanechnikov	Province	hh members
RD_Estimate	−000587	0.00263	0.00295	−0.00295	−0.00685	−0.00424	−000381	000660
	(0.00841)	(0.00649)	(0.00910)	(0.0116)	(0.00854)	(0.00767)	(0.00577)	(0.00583)
Observations	8,305	8,305	8,305	8,305	8,305	8,305	8,305	8,305

Robust Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 14 Robustness checks, other expenditure, with covariates

Type of check	p(1)	Alternative bandwidth			Alternative kernel		Error clustering	
		mserd	msetwo	certwo	Uniform	Epanechnikov	Province	hh members
RD_Estimate	0.00184	0.00343	0.00204	0.00455	0.00206	0.00221	0.00334	0.00339**
	(0.00259)	(0.00210)	(0.00366)	(0.00494)	(0.00243)	(0.00222)	(0.00217)	(0.00119)
Observations	8,305	8,305	8,305	8,305	8,305	8,305	8,305	8,305

Robust Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 15 Baseline model with covariates - levels

	(0)	(1)	(2)	(3)
Outcomes	Total	Food	Durables	Other
First stage	−0.01516***	−0.01502***	−0.01536***	−0.0138 ***
	(0.00142)	(0.00139)	(0.00145)	(0.00126)
RK coef.	8.542**	1.678*	0.855	5.147*
	(3.843)	(0.969)	(1.250)	(2.868)
Observations	8,305	8,305	8,305	8,305
Eff. Number of obs left	2957	3001	2909	3045
Eff. Number of obs right	1619	1634	1600	1655
Kernel Type	Triangular	Triangular	Triangular	Triangular
BW Type	cerrd	cerrd	cerrd	cerrd
Order Loc. Poly. (p)	2	2	2	2
Symmetric BW	11257	11405	11061	11628

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

A.1 Classification of Durable Goods Expenditure

Durable goods are grouped into broad expenditure categories. **Household appliances** include major items such as refrigerators, washing machines, dishwashers, kitchen units or cookers, air conditioners, heaters and other large domestic appliances, together with small kitchen and personal-care appliances (e.g. coffee machines, kettles, irons, toasters, grills and similar devices). **Electronics and ICT equipment** comprise televisions, aerials, video and audio players (portable and non-portable), personal computers, software, accessories, calculators, landline and mobile telephones, and fax machines. **Photography, optics and musical equipment** include cameras and related accessories, binoculars, microscopes and musical instruments. **Vehicles and transport equipment** cover new and used cars and motorcycles, bicycles, boats, canoes, camper vans and other recreational vehicles. **Furniture and home furnishings** include indoor and outdoor furniture, lighting equipment, decorative items, carpets, floor coverings, curtains, furnishings fabrics and household textiles. **Household utensils and kitchenware** comprise cutlery, tableware, pots and other non-electric kitchen tools, as well as ironing boards and similar household items. **Personal goods and leisure equipment** include jewellery and ornaments, watches and clocks, bags and luggage, sewing machines and billiard tables.

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Declarations

Conflicts of Interest The authors do not declare any conflict of interest or competing interest.

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