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Luca VILLAMAINA, Paolo ACCIARI

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Dipartimento delle Finanze
Via dei Normanni, 5
00184 Roma

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Taxation and labour supply decisions: an evaluation of the earned income tax credit in Italy*

Luca Villamaina† Paolo Acciari‡

Abstract

The earned income tax credit - so-called 'monthly €80 bonus' - introduced in Italy in 2014, has been characterized by a rapid phase-out area for budget-constraints reasons, leading to very high effective marginal tax rates. The aim of our analysis is to empirically investigate whether this policy design has effectively determined a reduction of the intensive margin of the labour supply of employees. The empirical analysis is based on the longitudinal electronic database of *Personal Income Tax returns* from 2011 to 2017 assembled by the Department of Finance of the Italian Ministry of Economy and Finance. The timing and the structure of the reform allow us to exploit the difference-in-discontinuities design using the before/after with the discontinuous policy change. Despite a close to 100% effective average tax rate for a substantial income range, a unique feature among EU and OECD countries, we found that the tax credit design had no negative effect on changes of the labour effort in Italy, challenging the economic theory but confirming previous empirical evidence.

Il credito d'imposta erogato sui redditi da lavoro dipendente ed introdotto in Italia nel 2014, comunemente denominato 'Bonus Irpef 80€', è stato caratterizzato, nell'area di decrescenza del bonus per soglia massima di reddito, da un rapido azzeramento, determinando aliquote fiscali marginali effettive molto elevate per alcuni livelli di reddito. Lo scopo della nostra analisi è indagare empiricamente se l'introduzione di questa misura abbia effettivamente determinato una riduzione nel margine intensivo dell'offerta di lavoro dei lavoratori dipendenti. L'analisi empirica si basa sul database elettronico longitudinale delle dichiarazioni dei redditi delle persone fisiche, dal 2011 al 2017, raccolto presso il Dipartimento delle Finanze del Ministero dell'Economia e delle Finanze. La tempistica di introduzione e il disegno della misura consentono di sfruttare l'approccio controfattuale per la valutazione delle politiche tramite l'utilizzo della tecnica *difference-in-discontinuities*. Nonostante l'aliquota media effettiva vicina al 100% per una fascia di reddito piuttosto ampia, una caratteristica unica tra paesi dell'UE e dell'OCSE, abbiamo riscontrato come l'introduzione della policy non abbia avuto alcun effetto negativo nel margine intensivo dell'offerta di lavoro in Italia, in contrasto la teoria economica ma confermando le precedenti evidenze empiriche.

JEL codes: H21, H24, H30, J22, J38

Keywords: EITC, Public Economics, Labor Market, Labor supply, Personal Income Tax

*The opinion expressed in this work are solely by the authors and do not represent the view of the institution where they work

†MEF - Department of Finance, Directorate for Studies and Research on Tax Economics, Rome, Italy

‡MEF - Department of Finance, Rome, Italy

1 Introduction

During the Great Recession, governments on both sides of the Atlantic have enacted large fiscal packages aimed at stimulating internal demand.

In May 2014, the Italian Government introduced a tax credit aimed at both increasing house holds' consumption and reducing the tax wedge on labour, targeted to employees with gross annual income between €8,145 and €26,000. The bonus resulted in an average net salary increase of about €80 per month. For earnings between €24,000 and €26,000, the amount of the bonus was smaller, linearly decreasing to zero[1] (Neri et al., 2017).

In the following years the bonus was slightly modified: as of tax year 2018, the thresholds were increased by €600 to €24,600 of income for the full bonus and to 24,600-26,600 euros of income for the decreasing range; as of July 2020, the bonus was increased to €100 up to €28,000 of income and the decreasing range was set at 28,000-40,000 euros of income.

Beyond the main goal of increasing households' consumption, the typology of this tax rebate can be directly categorized as an 'earned income tax credit' (EITC) policy, which helps to boost labor supply, by reducing the tax wedge on labour and improving subsidization among low-wage workers in the Italian labor market. In-work benefits of this kind aim at achieving both re-distributive (Baldini et al., 2015) and labor supply effect (Bettendorf et al., 2014; Eissa and Liebman, 1996; Stancanelli, 2008).

As a consequence, changes in the personal income tax also influence, through marginal and average tax rates, labour supply decisions: as shown by Di Nicola et al. (2017) by using a micro-simulation model with a representative sample, the tax rates in the Italian tax-benefit system may generate a broad range of effective marginal tax rates (EMTRs), with positive and negative values, determining, in some cases, also a 'low-wage trap' or 'poverty trap' (that is, a marginal tax rate close to 100% or even higher).

In fact, marginal and average tax rates also decrease as taxable income grows, while at low income levels such high tax rates are observed as to act as a disincentive for labour supply.

In the specific case of a spouse with two children, at a low income level, taking into account local taxation, the taxpayer has to pay an additional local rate on her overall income, not just on her exceeding income, leading to a tax rate above 100%.

[1] In this specific income bracket the bonus was calculated as follows: [80*(26,000 - *Income*)/2,000].

Moreover, it is worth noting that, again according to Di Nicola et al. (2017), the tax credit determines two spikes, one negative and one positive, as showed in the figures: the former just above the exemption income level and the latter when the bonus decreases within the €24,000-€26,000 income range, which corresponds to 80.80% and 87.53% of the average wage in Italy used as a reference for the OECD (2014).

More precisely, in the specific case of a single employee in the decreasing area of the bonus, the amount of the bonus decreases rapidly, so that the specific implicit rate becomes 48% while the effective marginal tax rate (EMTR) is higher than 80%.

In this context, according to the above authors, high EMTRs may constitute a disincentive for enhancing labour supply, as high EMTRs may affect people's incentives to work by influencing the behavioural response to labour supply.

More broadly, Immervoll and Pearson (2009) write: "*one consequence of targeting payments to low-income or part-time workers is that they reduce recipients' incentives to increase work effort or working hours, or to invest in training, since reduced in-work benefits will partly offset any associated income gain*".

Carone et al. (2004) and OECD (1997) define the "low-wage trap" (or "poverty trap") which is related *"not to a transition into work but to the financial consequences of increasing working hours (or work effort) for those already in (lowpaid) work. The "trap" refers to a situation where an increase in gross in-work earnings fails to translate into a net income increase that is felt by the individual to be a sufficient return for the additional effort. [...] In terms of their potential effect on labour-supply, these instruments therefore trade off higher participation against lower working hours. Given this trade-off it is therefore essential to monitor both the financial incentives of taking up employment as well as the incentives to change working hours for those already in work.*"

The OECD and the European Commission are concerned about the existence of this "low-wage trap", and they annually compute and publish a "low-wage trap indicator" for all EU countries[2].

The aim of our analysis is, hence, to empirically investigate whether the introduction of the tax-credit bonus has effectively determined a reduction of labour effort in Italy in the area

[2]See website: https://europa.eu/economy_finance/db_indicators/tab/ by selecting European Commission, Economic and Financial Affairs, Economic Databases and indicators, Tax and benefits indicators, Tax and Benefits

where the EMTR is extremely high.

In other words, our research question is: do extremely high EMTRs for certain income intervals generate disincentives to the labour supply at the intensive margin?

In this sense, the results of this analysis could be beneficial not only in the Italian case, but also to all policy makers in designing an equitable and efficient PIT: indeed, an EMTR of around 80% represents such an high rate that it can be used as an experiment.

In this work we do not investigate the positive effect of labour participation at the extensive margin (*i.e.*, the decision, especially for second earners, to participate to the labour market because of the increase of the potential net wage generated by the tax credit).

2 Literature Review

The use of 'in-work benefits' policies (IWBs) in the form of EITCs has become increasingly popular over the last decades: the design of the policy has varied across countries, depending on whether the purpose of country-specific policy makers was the reduction of in-work poverty or the increase of work incentives (Laun, 2019).

According to Van der Linden (2021), high risk of poverty and low employment rates are widespread among low-skilled groups, especially in the case of some household compositions (e.g. single mothers). These kinds of policies have been advocated and implemented to address these issues, through the alleviation of the above-mentioned problems without providing a disincentive to work.

As a matter of fact, the early uses of IWBs in the US, the UK and Ireland were closely targeted towards low-income families with children as a working poverty alleviation measure through cash transfers or tax benefits paid to workers, conditional on employment (Eissa and Hoynes, 2004; Hernández-Murillo et al., 2001).

Additionally, labour policies are often complex and interact with other schemes; they may thus be poorly understood, particularly by their target beneficiary group and, therefore, it should be crucial for policy makers to take into account the underlying interactions among taxation, labour market and welfare in order to design a balanced tax-benefit system.

Thus, most of the recent economic literature has attempted to address this policy issue by estimating the impact from both a theoretical *ex-ante* than an empirical *ex-post* perspective.

In this context, following a theoretical perspective, when low-income married taxpayers are targeted to benefit for the EITC and the unit of analysis for the tax design is based on family income, the second earners, mostly women, might be structurally discouraged to enhance their own labour supply (Eissa and Hoynes, 2004; Triest, 1990) indeed, as noted in Crandall-Hollick and Hughes (2018), married couples may consciously and jointly decide to keep the second earner out of the labour force in order to carry out childcare activities, arguing that having two earners would not only reduce their EITC, but may also increase the cost of other expenses, like child care, and ultimately lower their disposable income.

Having said this, IWBs may have a different impact on the budget curves of single eligible employee eligible, depending on which effect prevails between the negative effect on labour supply of the increase in net income and the positive effect on labour supply of the increase in cost-opportunity of leisure.

On one hand, permanent IWBs have unambiguously increased work incentives by increasing the difference between in-work and out-of-work income, since they increase the cost-opportunity of leisure and, more specifically, have generally increased the decision to participate in the labour force (*i.e.* 'extensive margin') as well as the transition from welfare to employment of single mothers, also through a positive effect on their mental health and life satisfaction (Van der Linden, 2021).

On the other hand, IWBs may negatively affect the decision of the extent of work among those already working, (*i.e.* 'intensive margin'), given the negative effect on labour supply of the increase in net income as well as the possible decrease in the employment level of second earners since the behavioural response also strongly depends on the level of household income.

Moreover, from a micro-simulation perspective and still focusing more on the impact on the intensive margin, in this respect most of the economic literature measures through EMTRs the incentive to increase labour supply, with a lower EMTRs tax rate associated with a greater 'incentive to progress', in the sense of the incentive for individuals to increase their earnings slightly, whether by working more hours, getting a promotion, qualifying for bonus payments or getting a better paid job (Brewer and Shephard, 2005).

In this sense, this has also motivated more structural analyses of labor supply, based on parametric specifications of the individual's utility function and on the assumption that the observed

choices are utility maximizing (Blundell et al., 2004; Blundell and MaCurdy, 1999; Meyer and Rosenbaum, 2001).

As a consequence, theory suggests that we should observe a decline in the phase-out portion of the tax-credit as most people should be encouraged to reduce their hours under the EITC, but an empirical analysis shows that there is no impact on hours worked in any evaluation, giving room for a "puzzle" in the economic literature (Eissa and Hoynes, 2006; Eissa and Liebman, 1996; Meyer, 2002, 2010).

However, it is worth noticing that empirical economic literature has stressed how the introduction of EITC policies in the case of single individuals has generally achieved positive results in the increase of their labour supply, especially for single mothers (Brewer et al., 2006; Meyer and Rosenbaum, 2001) and unmarried individuals (Eissa and Hoynes, 2006), that is at least for those social categories considered with a higher probability of poverty risk.

More in a detail, from an empirical perspective, several studies have attempted to address this issue by estimating, commonly through the use of a difference-in-differences econometric design, the impact of the EITC on second earners or single women with low education and one or more children, comparing them to the treated group and analyzing the effect on employment and participation rates for the extensive margins and on working hours for the intensive margins. As discussed by Eissa and Hoynes (2006), considering both the introduction of the EITCs and their different expansions, the main empirical findings have stressed the positive effect on the extensive margin and little evidence on the intensive margin (Eissa and Hoynes, 2004; Eissa and Liebman, 1996), also given the fact that measuring the hours worked response of employees to the EITC budget constraint is fundamentally a harder empirical problem.

In fact, there are different tasks that occur following this perspective when estimating the effects of policies on hours of work, such as the fact that employees' choice between labour and leisure may depend on taxpayers' preferences, with this endogeneity possibly giving an estimation bias especially for those EITC recipients who lie in the phase-out region of the EITC (Eissa and Hoynes, 2004; Hotz and Scholz, 2003), as well as challenges related to other confounding factors for a correct estimation of the intensive-margin response, due to the existence of behavioural and composition effects on the treated and the non-treated both as a consequence of the introduction and the expansion of the EITC (Nichols and Rothstein, 2015).

According to the literature, various interpretations have been used to explain these findings: on one hand, it is worth noting the possible existence of a measurement error in working hours as they are mainly self-reported, as well as the inability of employees to freely decide their own working hours; on the other hand, it might also be that EITC recipients are not fully aware of the structure of the EITC schedule or have an imperfect perception of the EMTRs (Chetty and Saez, 2013).

More in a detail, especially focusing on this last interpretation, both Liebman (1998) and Saez (2010) have found no bunching of taxpayers at the beginning and the end of the phase-out range, as might be expected if the EITC significantly affects hours and taxpayers are conscious of the discontinuities in EMTRs generated by the credit, a result that is consistent with the hypothesis that people are simply unaware of their EMTRs, with important implications for the welfare impacts of the EITC and its "optimal" design when recipients do not fully understand the tax schedules given the complexity of eligibility rules and instructions.

Furthermore, following Eissa and Hoynes (2011), the finding of a significant extensive margin effect, especially for single mothers, but of no intensive margin effect is consistent with the current consensus that intensive labor supply elasticities are relatively small.

Having said this, for a more comprehensive review of literature on the matter, see Hotz and Scholz (2003); Meyer (2002) and Nichols and Rothstein (2015).

Thus, in this regard, it is quite clear why, in our opinion, it has become an empirical matter to widely investigate whether changes in the EMTRs effectively affect changes in the intensive margin of taxpayers in the phase-out area of the EITC.

2.1 Institutional background and evidence from Italy

During the second dip of the economic crisis, total consumption of Italian households dropped from nearly 970 billion euros in 2010 to 909 in 2013 (Andini et al., 2018).

As a consequence, in the 2014 the Italian government enacted for a new monetary transfer to households, announced to the press as a transfer on the 12th of March, starting from May. As already mentioned, the policy was designed as a tax-credit and, in this sense, the timing of the reform should prevent the existence of possible anticipation effects that could bias our estimates.

In line with worldwide economic literature, the most recent economic studies related to the Italian case has followed a micro-simulation perspective by focusing on the effect of in-work benefit schemes on labour supply of women and second-earners. More in detail, Figari (2011) analyses the incentive and redistributive effects of family- and individual-based IWBs by simulating two alternative marginal reforms of the tax system, showing that the EITC scheme has stronger effects for women in couples; Colonna and Marcassa (2015) compare a set of alternative policy reforms by estimating a structural labour supply model for women and use the estimated parameters to simulate the effects of alternative revenue-neutral tax systems, finding that provisions for a WTC scheme and gender-based taxation boost the participation rate, while De Luca et al. (2014) focus on EITC and WTC IWBs schemes showing that reforms have strong positive effects on labour supply of wives, weak negative effects on labour supply of husbands, and strong positive effects on equity, with the EITC more effective than the WTC in boosting employment of wives and the WTC more effective than the EITC in fighting poverty. Moreover, looking at the specific case of the monthly €80 bonus, previous papers have focused exclusively on estimating the consumption response of individual households to the monthly €80 bonus (Gagliarducci and Guiso, 2015; Neri et al., 2017), on its potential redistributive effects (Baldini et al., 2015) or on the effectiveness of targeting methods adopted (Andini et al., 2018; Lucchetti et al., 2021).

Specifically in the first case, Neri et al. (2017), by building a structural model of household with heterogeneous agents and using SHIW household data to test whether predictable changes in household income have occurred, specifically estimate the change in household expenditure due to the tax credit to be between 50 and 60 per cent of the sum received.

Therefore, in light of this, the contribution of our paper to the existing literature on EITC is twofold. First, we expand previous empirical economic literature on the effects of EITCs on the intensive margins of labour supply offering a unique quasi-natural experiment of the effect of close to 100% EMTRs on labour effort. In this context, we try to measure the effective influence of EMTRs on the intensive margins of labour supply from a different perspective than the one followed by previous micro-simulation studies on EITC, which have always considered employment income as an effective proxy of the labour effort. Second, to the best of our knowledge, this is the first implementation of an *ex-post* counterfactual evaluation of the

monthly €80 bonus on the intensive margin of labour supply, contributing to the empirical economic literature related to tax-rebates and their influence on the Italian labour market. Indeed, paying attention to the behavioural response of employees falling in the phase-out area, who represent a high percentage of the labour force in Italy[3], is particularly useful to better guide policy makers in future changes of the tax-benefit schemes in the country.

3 Dataset and sample selection

The empirical analysis is based on the electronic database of '*Personal Income Tax returns*' assembled by the Department of Finance of the Italian Ministry of Economy and Finance, used for the official tax return statistics published annually.

Computations based on income tax return data are widely used in the recent taxation literature, on the one hand, due to their higher level of accuracy and population coverage, on the other hand, due to their lower misreporting and measurement error (Slemrod, 2016).

Moreover, the longitudinal dataset is particularly suitable for the purpose of this analysis because it combines information from all three income tax forms available to Italian taxpayers jointly with numerous variables on demographic characteristics (Acciari et al., 2022).

In order to avoid other confounding factors, we focus on people already employed in 2013 to avoid the influence of the 2015 Jobs-Act reform and the reduced contribution to new hiring, with a gross income between €24,000 and €28,000: in this way, by focusing on individuals already employed, we try to minimize the between-group differences following the labour market reform due the so-called composition effect on the outcome of interest that might prevent the estimates from being correct.

Furthermore, we choose the pre-treatment period 2011-2013 in order to stay close to year 2014, when the policy came into force, and to have a balanced sample before/after its implementation, while we consider only the time range 2011-17, since in this way employees were not influenced by the economic cycle (*i.e.* the 2010-11 debt crisis) to avoid any bias on our estimates and to prevent the non-randomization of the treatment.

[3] According to the electronic database of '*Personal Income Tax returns*' assembled by the Department of Finance of the Italian Ministry of Economy and Finance, the percentage of employees with a gross income between €24,000 and €26,000 (*i.e.* the phase-out area of the EITC) equals 5.49% of the total number of employees in the 2014 tax year.

In addition, to complete the missing variables in our database and to avoid potential sources of endogeneity (that risk to provide a biased estimation in our econometric models), we add as control variables, in both cases at the province level, the '*activity rate*' (by gender and age classes derived from the ISTAT database) and the '*median income*', in order to control for both the economic cycle and any exogenous variations in earnings.

Hence, in the general sample, employees who did not match the criteria concerning job sector, public/private, type of contract, firm dimension and province of work were dropped, together with employees outside the 21,000-30,000 euros income range interval and 21-60 years range interval in the pre- treatment period.

Additionally, we have also dropped employees who worked part-time in the pre-treatment period and those who have at least once registered a total gross or an employment income higher than €60,000, a net income from '*regime di vantaggio*' or a '*regime forfetario*' higher than €20,000 and total tax reliefs higher than €12,000 in the 2011-17 period, in order to drop outliers, as they may bias our estimates as well.

Hence, after these filters, our general sample is composed by 2,511,247 employees, while our final sample (selected using the CER-optimal bandwidth procedure suggested by Calonico et al. (2018)) is composed by 901,419 employees in the neighborhood of the cutoff of €26,000 in gross income over the period 2011-17, as shown in the Table 1.

4 Econometric strategy

4.1 Regression Discontinuity Design: an overview

Following Lee and Lemieux (2010), the potential outcome framework in the economic literature assumes the form of counterfactual causality, as it compares the outcome Y_i of the same i^{th} individual when it is assumed to be assigned or not to a specific policy rule (*i.e.* treatment variable), indicated as $D_i \in (0, 1)$.

. In this conceptualization, the evaluation of the policy effect (i.e. treatment effect) is defined as the effect of a specific treatment variable on a target variable $Y_i(0)$, once any potential confounder X_i is ruled out, *i.e.* $[Y_i(1) - Y_i(0)|X_i]$.

The fundamental problem of causal inference is that we cannot observe the pair $Y_i(0)$ (individ-

ual not assigned to the policy, treated) and $Y_i(1)$ (individual assigned to the policy, control) simultaneously for the same i^{th} individual.

We therefore typically focus on the average treatment effect (*i.e.* ATE) on sub-populations rather than on unit-level effects, that is the averages of $[Y_i(1) - Y_i(0)|X_i]$, which equals $E[Y_i(1) - Y_i(0)|X_i]$.

Among the methods used for the estimation of the ATE, we can consider the Regression Discontinuity Design (*i.e.* RDD) as firstly introduced by Thistlethwaite and Campbell (1960) in their study of the impact of merit awards on the future academic outcomes (career aspirations, enrolment in postgraduate programs etc.) of students.

Their study exploited the fact that these awards were allocated on the basis of an observed test score: i^{th} student with test scores, X_i, greater than or equal to a cut-off value c, received the award, while those with scores below the cut-off were denied the award.

This generated a sharp discontinuity in the "treatment" (receiving the award) as a function of the test score, where the main idea behind the research design was that individuals with scores just below the cut-off (who did not receive the award, control group) were good comparisons to those just above the cut-off (who did receive the award, treated group)[4].

. Let the receipt of treatment be denoted by the dummy variable $D_i \in (0, 1)$, so that we have $D_i = 1$ if $X_i \geq c$ and $D_i = 0$ if $X_i < c$.

Therefore, assuming that the relationship between Y and X is otherwise linear, a simple way of estimating the causal treatment effect of the award though the coefficient δ, is by fitting the following linear regression:

$$Y_i = \alpha_i + \beta' X_i + \delta D_i + \epsilon_i$$

which equals $E[Y_i|X_i, D_i]$. In this context, in the RD setting we can only observe $E[Y_i(1)|X_i, c^+]$ to the right of the cut-off and $E[Y_i(0)|X_i, c^-]$ to the left of the cut-off and the inference is possible because of the continuity of the two underlying functions (awards and test score), which enables us to use the average outcome of those right below the cut-off (control group) as

[4]Following Cattaneo et al. (2020), in the RD continuity-based framework the goal of bandwidth selection methods is to find the bandwidth that optimally balances bias and variance, with the mean squared error (MSE) criterion optimal for point estimation, and the coverage error probability (CER) criterion optimal for conducting inference, with the CER-optimal bandwidth smaller than the MSE-optimal bandwidth, at least in large samples.

a valid counterfactual for those right above the cut-off (treated group).

$$ATE = \lim_{\gamma \to 0} E[Y_i(1)|X_i = c^+ + \gamma] - \lim_{\gamma \to 0} E[Y_i(0)|X_i = c^- + \gamma]$$

which would equal $E[Y_i(1) - Y_i(0)|X_i = c]$, *i.e.* the ATE at the cut-off c.

In fact, we can assume that individuals strictly around the cut-off c are identical in the observables: consequently, the jump in the outcome Y_i (*i.e.* future academic outcomes) is totally driven by the causal effect of the merit award (*i.e.* policy or treatment effect), once we controls for all the confounders factors X_i.

4.2 Regression Discontinuity in Time: an application to the 'monthly €80 bonus'

Most recent works aimed at integrating the RDD in a temporal context[5] (De Benedetto and De Paola, 2019; Grembi et al., 2016; Leonardi and Pica, 2013), in order to better achieve the casual identification of the policy effect.

In the typical RDiT (Regression Discontinuity in Time) framework, the researcher knows the date c of a policy change, assuming that for all dates $t>c$, the unit is treated, and for all dates $t<c$, the unit is not treated.

More in detail, in the idea of Grembi et al. (2016), the diff-in-disc estimator identifies the (local) causal effect of the treatment in a neighbourhood of the threshold ($P_{it}=P_c$) and can be implemented by estimating the boundary points of four regression functions of Y_{it} on P_{it}: two on both sides of P_c, both before and after $t=c$, applying a Local Linear Regression (LLR hence- forth), following Gelman and Imbens (2014), which consists in fitting linear regression functions to the observations distributed within a distance h on either side of P_c, both before and after $t=c$.

Thus, in the case of our study, we compare the change in employees' labour effort just below €26,000 (treated group) before and after the 2014 reform to the change in employees' labour effort just above €26,000 (control group)[6].

[5] See (Hausman and Rapson, 2018) for a general overview.

[6] In other words, the assumption that guarantees that the effect of bonus tax-credit on work effort can be interpreted as causal is that the characteristics of employees should not display any discontinuity at the threshold, building an RDD combined with a DID strategy to estimate the causal effects (Angrist and Krueger,

In this context, employees with earnings lower but really close to €26,000 may prefer to gain higher income and overcome the inclusion threshold, thus preferring to reject the bonus, as the effective average tax rate is lower for the part of income higher than €26,000: consequently, the counterfactual design would be invalid as the treatment would result endogenous.

For this reason, to prevent the invalidity because of this particular behaviour, in the analysis we will run tests to verify the homogeneity of the two groups in the controls and randomization of the policy to be fully respected.

This reflects the identification assumption that the average work effort of individuals earning an income marginally below the €26,000 employees' threshold is expected to diverge from the one of the control group just above the threshold for no other reason than the law change. Therefore, if employees were exogenously assigned to the treatment and control groups and following the previous studies using the RDiT, in order to identify the impact of bonus tax-credit on labour effort, our empirical design combines the RD with a before-after comparison by restricting the sample to employees in the interval $P_i \in [P_c - h, P_c + h]$ and by choosing the CER-optimal bandwidth as suggested by Calonico et al. (2018) through an estimation of the following specification by means of a fixed effects modeling using OLS with fixed effects at individual level (α_i), to control for the sorting of employees into lower earnings according to time-invariant employees characteristics, either observable or unobservable:

$$\Delta Y_{it} = \beta' X_{it} + \delta_1 Bonus_i + \delta_2 Post_t + \delta_3 (Treatment_{it}) + \alpha_i + \varepsilon_{it}$$

The dependent variable ΔY_{it} is the log-yearly variation of the total employment income, that in our opinion could be considered a good proxy of the work effort[7].

Moreover, in our counterfactual evaluation setting we find the use of this proxy more robust than other variables for different reasons.

As already said, the hours worked are mostly self-reported and, consequently, the variable could be strongly affected by a measurement error that damages its level of accuracy.

1999).

[7] More in detail, in a efficiency-wage setting the basic hypothesis states that employees' effort depends positively on the wages paid by the firm (Yellen, 1984) as, for example, employers cannot directly monitor employee performance due to moral hazard issues forcing firms to pay higher wages to avoid contraction (Shapiro and Stiglitz, 1984) or to minimize turnover costs (Salop, 1979) in order to induce effort: in this context, we can assume that changes in the total employment income can be classified as changes in employees' work effort

Furthermore, by focusing only on private sector employees , we agreed that in this context employees are more able to choose their total annual employment income with great precision and freedom, since the higher level of dynamism should incentivize private sector employees to freely move from a job to another to earn, even marginally, higher incomes and, consequently, the choice of this variable allows us to proxy with a higher level of accuracy the labour effort than the use of the hours worked or productivity of the individual employee, which has no general consensus for its measurement.

The variable $Post_t$ is a dummy that takes the value of 1 starting from 2014 and zero otherwise while the variable $Bonus_i$ is a dummy that takes the value of 1 if the employee earns an income within the area concerned for the tax-rebate. Consequently, the coefficient δ_3 is the diff-in-disc estimator and identifies the treatment effect of receiving the tax-rebate, as $Treatment_{it} = (Bonus_i * Post_t)$ is the interaction term included to capture the effect of the reform.

The matrix X_{it} includes the following individuals' characteristics:'*personal*' (Gender, Age, Age squared), *familiar*' (Spouse support, Housing deductions, Number of sons, Family deductions, Marital status, Dependent spouse), '*job*' (Macroregion of residence, Yearly working days, Wit-holding agent, Number of wit-holding agents, ATECO-5-digit, Firm dimension, Contractual arrangement), '*taxation*' (Gross/Net income tax, Total Surcharge taxes[8], Total tax reliefs[9], Substitutive tax at 5% (*i.e.* 'regime di vantaggio'), Substitutive tax at 15% or 5% (*i.e.* 'regime forfetario'), '*income*' (Taxable income, Taxable surcharge income, Tax base at flat-tax rate of 10% or 21% ('*reddito da fabbricati*'), Total net income excluding flat-taxed base, Net income from '*regime di vantaggio*', Net income from'*regime forfetario*', Total gross income), '*economic*' (Yearly province activity rate, Yearly province median income).

Additionally, in order not to run into an omitted variable bias, we include also as other covariate the amount of hours of payroll subsisidies (*'Cassa integrazione e guadagni'*) at province level to proxy for the ability of employees to increase their own income.

As a conclusion, in all of our regressions the standard errors are robust to heteroskedasticity.

[8]Regional and Municipal

[9]Job deductions + Total tax deductions + Tax reliefs + Other tax reliefs

4.3 Identification strategy and validity tests

As previously discussed, this work will use a combination of RDD and DID approach, using worker-side fixed effect to control the endogeneity of the treatment status.

According to De Benedetto and De Paola (2019) and Leonardi and Pica (2013) who applied the same econometric strategy for their research, we run different specification tests.

Firstly, according to the main assumptions related to the RDD, the eligibility index must be continuous around the cut-off point so employees do not manipulate their income in order to increase their chances of being included in or excluded from the policy[10] finding the presence of self-selection.

In this regard, we initially test the null hypothesis of continuity of the density of yearly increase of earnings at €26.000 as proposed by McCrary (2008)[11] to confirm the continuity of the forcing variable around the threshold. In fact, in case of presence of discontinuities at the cut-off point, employees would be able to manipulate the increase in their annual earnings. Figure 5 shows that the log-difference between the frequency of the annual increase of earnings at €26,000 in 2014 to the right and to the left of the threshold is not statistically significant at conventional levels.

Moreover, following Cattaneo et al. (2018), we implement data-driven manipulation tests based on density discontinuity and constructed using the results for local-polynomial density estimators, which are useful for falsification of RD designs and for empirical research by analysing whether employees are self-selected into a particular group or treatment status.

Table 2 shows the result of the tests checking the manipulation of the running variable through checking its density around the threshold, stressing that we can strongly reject the null hypothesis of manipulation for each year of the interval considered, as graphically showed in Figure 6.

Secondly, the eligibility should be mostly determined by the assignment rule and must solely influence the outcome variable around the cut-off point, thus homogeneity of the two groups in the controls and the absence of employees facing discontinuities on other observable characteristics at the threshold must be required for the randomization of the policy to be fully

[10] Moreover, in the MEF (2022), the authors follow a strategy similar to ours to understand whether there has been evidence of manipulation of the declared revenues around the established substitutive tax-rate threshold due to the introduction of the so-called '*regime forfetario*'.

[11] The test studies the statistical significance of the log-difference between the frequency to the right and to the left of the threshold, by running kernel local linear regressions of the log of the density separately on both sides of the threshold, to avoid the assumption of manipulation.

respected.

In this context, we further perform the balancing tests in the neighbourhood of €26,000 of individual income with the goal to check whether employees with the same observable characteristics have discontinuities at the threshold after the reform, thus checking whether the introduction of the policy in 2014 is predictive of a set of control variables.

The balance tests are performed running the individual-level regression:

$$X_{it} = \beta' X_{it} + \delta_1 Bonus_i + \delta_2 Post_t + \delta_3 (Treatment_{it}) + \alpha_i + \varepsilon_{it}$$

More in detail, we perform the balance test by implementing a diff-in-discontinuity regression, with a CER-optimal bandwidth and with a linear polynomial of the normalized forcing variable along with a first order interaction term, in which we consider as outcome variable X_{it} a number of labour and personal characteristics (bonus productivity, spouse support, ATECO- 5-digit, firm dimension and total tax reliefs).

In fact, considering that in our baseline specification we use employee fixed effects control for all time-invariant unobserved factors that may affect the propensity of workers to self-select into (or out of) treatment, we are not able to control for the time-varying factors that affect employees self-selection, including the reform itself.

According to this, if a non-random sorting of workers were to occur, we would expect these characteristics to differ systematically between treated and untreated workers around the €26,000 threshold, as shown by the significance of δ_2 coefficient that gives insights on whether other (unobserved) policies differentially affect workers, thus confounding the effect of the reform itself.

Results reported in Table 3 highlight how all of these variables are balanced around the €26,000 threshold before/after 2014, since the coefficients attached to the diff-in-disc estimators are not statistically significant at conventional levels.

Finally, we indirectly test the assumption, underlying a diff-in-discontinuities setting, according to which the effect of the confounding policy discontinuity at the cut-off is constant over time in the case of no treatment. In other words, this assumption requires employees just above and just below the cut-off to be on local parallel trends in the period before the introduction of

the reform (2011-2013)[12]. In particular, we take our treated and control employees within the CER-optimal bandwidth in the pre-reform period, and show the trends in the level of yearly log-variation of employment income. Figure 7 reassures us that observations just below and just above the threshold were not on a differential trend before the policy shift.

4.4 Main empirical results

Table 3 shows the main results of the diff-in-discontinuities estimates, noting that the introduction of the bonus had a decidedly insignificant impact on the yearly log-variation of employment income and that the negative absolute value is really close to zero and quite stable across the different specifications.

Thus, our estimates could stress how the introduction of EITC policies does not significantly negatively affect the work effort in treated employees (below the €26,000 cut-off) compared to untreated employees (above the €26,000 cut-off).

Moreover, in order to run further in-depth analysis, we investigate whether the introduction of the policy had any impact on different targeted individuals, considering employees within different regions of residence, age classes or gender. All the estimated results have confirmed the absence of evidence of any effect on the outcome of interest.

Additionally, following De Benedetto and De Paola (2019), our estimation results are also consistent with the descriptive graph presented in Figure 8, where we plot the estimated difference between post-treatment yearly log-variation of employment income and pre-treatment values against the normalized gross income (income minus €26,000) close to the threshold. The central line is a spline third-order polynomial fit, whereas the lateral lines represent the 95% confidence intervals. This graph allows us to check whether the difference in the outcome variable shows a discontinuity in the neighbourhood of 0 (the €26,000 cut-off). As we can notice, the difference in yearly log-variation of employment income sharply changes at 0.

The absence of any effect of the tax-rebate on the intensive margin can be explained by different reasons. The main explanation, in line with Liebman (1998), Saez (2010) and Chetty and Saez (2013) is that employees could be simply unaware of their EMTRs and the discontinuities in EMTR generated by the tax-rebate, with the consequential implication of this not having any

[12] As a matter of fact, the x-axis shows just 2012 and 2013, as we lost the first year observation considering the yearly variation of our variable of interest.

influence on their intensive margins. This seems to be particularly relevant in Italy with EMTR close to 100%, where the substitution effect should in theory lead the employee to the choice of free-time compared to work, or completely discourage the employee in engaging in training activities to enhance his/her skills.

Another explanation could be the limited room that employees might have in deciding their working hours, which are usually given by the organizational choices of the employers and by ex-ante contractual agreements.

On the other hand, the employees who are fully aware of their EMTR and with the possibility to decide their working hours (or work effort), might decide to continue to increase their work effort in the perspective of a medium-long term increase in their income, so as to overcome the income range where EMTR are extremely high.

Having said this, as the results will be corroborated by additional future analysis, this could have several implications for policy makers. First, if they are considering using EITC to increase labour participation (at the extensive margin) they should not worry about the negative effects on the intensive margin. In other words, it seems a good idea to intervene with EITC to tackle the "unemployment trap" since in practice there does not seem a "low-wage trap" and thus no dilemma between the two . Second, when designing the PIT system, policy makers could focus more on equity, since workers with average incomes do not seem to react to extremely high EMTRs, at least if those are limited to income intervals that are not very large.

5 Robustness check and placebo tests

Following De Benedetto and De Paola (2019), a first robustness check will consist in choosing as optimal bandwidth the one proposed by Calonico et al. (2018), in which the alternative method leads to an MSE-optimal bandwidth. Table 6 shows that even with this choice of bandwidth the coefficients are not significant with a positive magnitude, but equally close to zero.

Moreover, following Leonardi and Pica (2013), a second class of robustness check will consist in implementing placebo tests by estimating the treatment effect at fake thresholds (years and income), where there should be no effect.

Firstly, considering that the reform started only in May 2014 and thus affects individuals only

for a part of year, we check whether the results will change when we assume the 2015 as the threshold year for receiving the bonus as well as the 2013 and the 2016: by selecting both the CER-optimal and the MSE-optimal bandwidth and controlling as before for a linear polynomial of the forcing variable and for a first-order interaction term, results displayed in Table X confirm the expectation that the diff-in-discontinuities estimates are not statistically significant at conventional levels within these fake years.

Secondly, we implement several diff-in-discontinuity estimations at 82 fake income thresholds, 41 below (from €25,000 to €25,975, every 25 units) and 41 above (from €26,025 to €27,000, every 25 units) the €26,000 cut-off to stay far away from the true policy threshold, in order to check if our results can be interpreted as causal or are due by random chance according what suggested by Della Vigna and La Ferrara (2010) and Grembi et al. (2016).

More in detail, as proposed by De Benedetto and De Paola (2019), Figure 9 shows the cumulative density function of these 82 placebo point estimates normalized with respect to our baseline point estimate. The idea behind this test is to highlight that not too many normalized coefficients lie outside the interval from -1 to +1. In fact, only 13 over 82 (roughly 16 percent) of all placebo coefficients are larger than our true estimated term limit coefficient in absolute value, and the cumulative density function of the normalized coefficients is much steeper around zero, providing strong support for the robustness of our main results presented.

6 Conclusion

The debate on the pros and cons of using EITC policies to encourage labour effort is still open among both academics and policy makers.

On the one hand, the use of EITC policies through a tax-rebate boosts earnings among low-wage employees as a redistributive measure and could indirectly affect the labour supply of total employees as a consequence of increasing net incomes.

On the other hand, some observers believe that higher marginal tax rates could discourage increased labour supply for employees as, at the margin, the single employee might not be advantaged in increasing his own labour supply as the correlated net monetary benefit does not satisfy his working time-leisure balance.

The value added in this paper comes from the capacity to analyse these different aspects in

combination with each other, as often happens in labour market achievement.

To best of our knowledge, this is the first empirical work that tries to directly and empirical measure the effect of EITC policies in the Italian case and, more specifically, the effect of this particular tax rebate.

In fact, regarding previous works, as already noted, on one hand, Neri et al. (2017) studied the effect of this tax rebate on households' consumption, not measuring the secondary effect of the policy on the relative labour supply response, while, on the other hand, Baldini et al. (2015) focused on the potential redistributive effects of the bonus on individuals and households, but from a micro-simulation perspective.

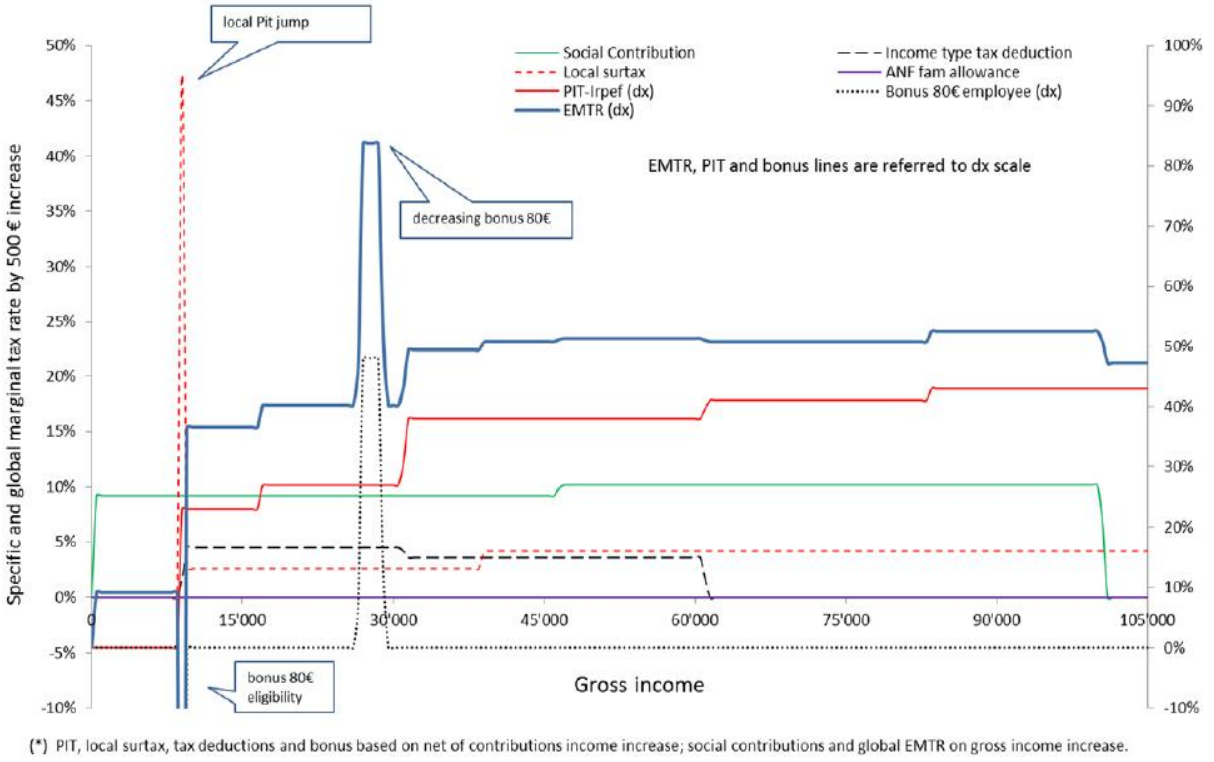
Moreover, in contrast to the extensive treatment of the topic in tax-benefit microsimulation studies, the aim of the economists' empirical studies is to properly verify whether the policy that is under analysis had a significant impact on the outcome of interest, rather than to simulate it.

Our findings, relevant for policy makers, are that employees earning middle incomes do not seem to react to extremely high EMTRs, at least if these are limited to income intervals that are not very wide. Moreover, even with EMTR close to 100%, we did not find any evidence of "low-wage trap" or "poverty-trap" for those already employed.

In light of this, future research should continue to contribute to the empirical evaluation of tax-rebates and their influence on the Italian labour market.

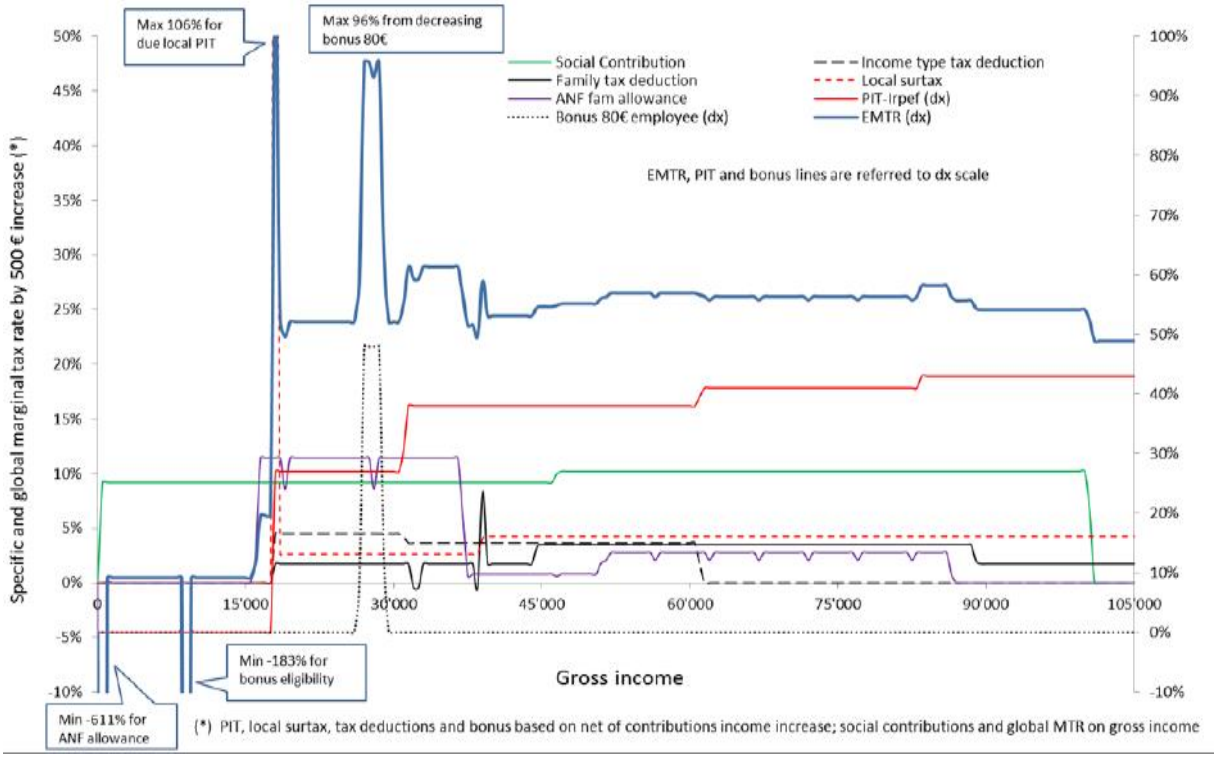
7 Appendix

Figure 1: Marginal tax rates for a single employee



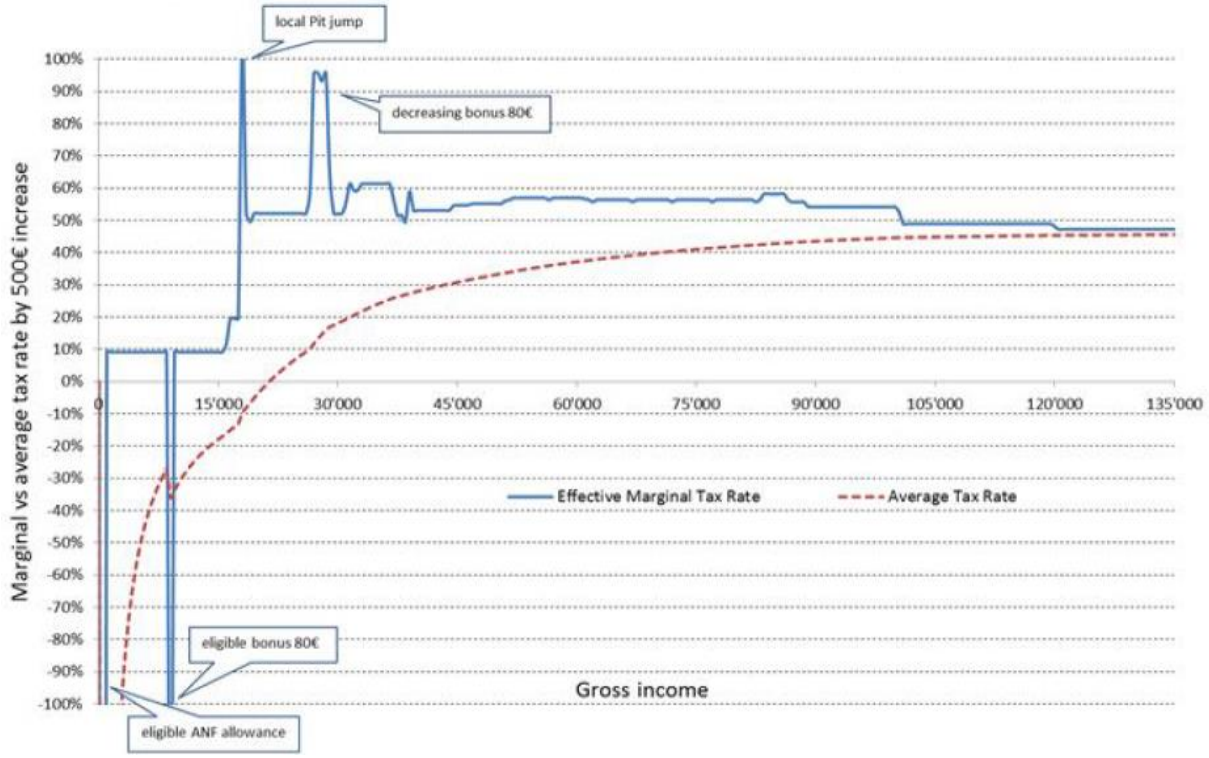
Di Nicola et al. (2017)

Figure 2: Marginal vs. average tax rates for a single employee, including family tax-deduction



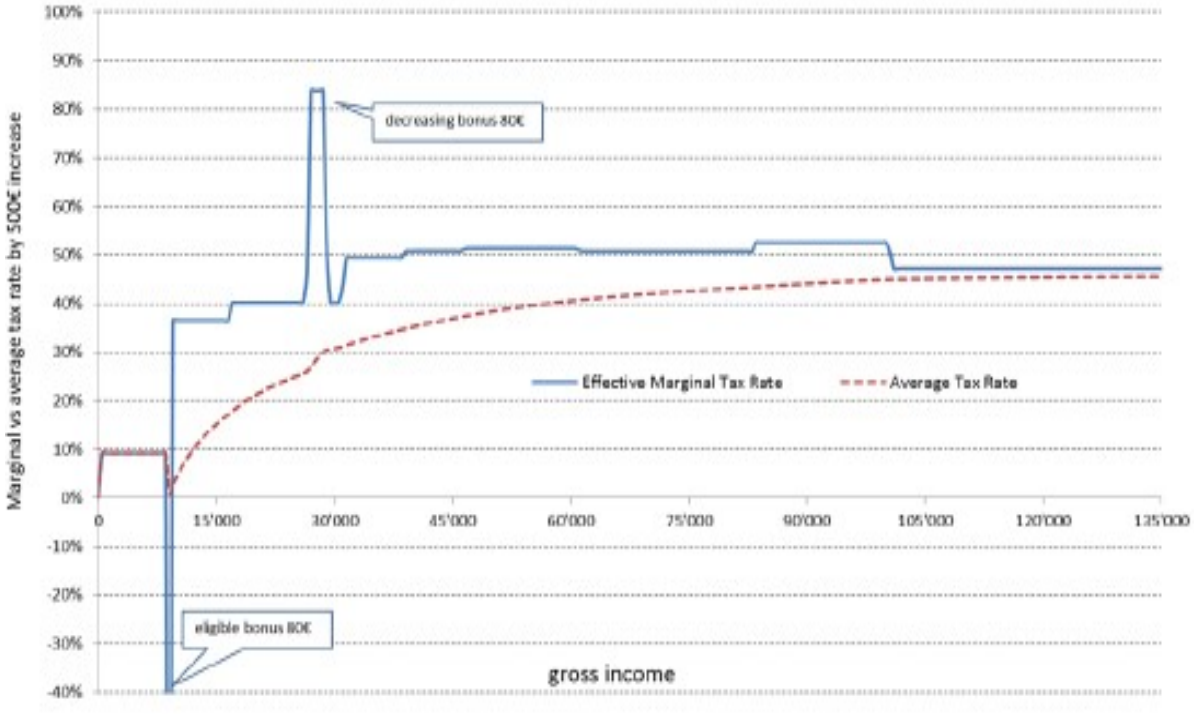
Di Nicola et al. (2017)

Figure 3: Marginal tax rates for an employee with dependent spouse and two children



Di Nicola et al. (2017)

Figure 4: Marginal vs. average tax rates for an employee with dependent spouse and two children



Di Nicola et al. (2017)

Table 1: Descriptive statistics (Discontinuity Sample)

	Sum	Mean	SD	Min	Max	Obs.
Personal						
Female	273,566	0.30	0.46	0	1	901,419
Age	40,087,413	44.47	8.67	22	64	901,419
Familiar						
Spouse support	8,716,835	9.67	188.26	0	9,600	901,419
Housing deductions	188,007,681	208.57	277.56	0	5,457	901,419
Family deductions	544,338,271	603.87	834.77	0	9,871	901,419
Marital status	503,944	0.56	0.50	0	1	901,419
Dependent spouse	150,957	0.17	0.37	0	1	901,419
Dependent children	747,871	0.83	0.94	0	14	901,419
Job						
Productivity bonus	285.209.395	316.40	699.03	0	4,596	901,419
Number of wit-holding agents	1,914,192	2.12	0.40	2	8	901,419
Taxation						
Total tax reliefs	3,035,943,900	3,367.96	1,310.53	0	11,998	901,419
Total surcharges taxes	481,482,004	534.14	12.54	0	1,169	901,419
Net income tax	3,795,260,254	4,210.32	1,101.05	0	9,987	901,419
Tax-rebate '€ 80 bonus' recipients	458,232	0.51	0.50	0	1	901,419
Income						
Yearly income change	426,772,870	473.45	1,796.51	-31,841	26,372	901,419
Labour gross income	23,000,684,146	25,516.09	1,278.70	370	38,491	901,419
Net income from '*regime di vantaggio*'	5,378,714	5.97	239.78	0	20,000	901,419
Net income from '*regime forfetario*'	3,607,631	4.00	196.22	0	19,935	901,419
Total gross income	23,431,678,544	25,994.21	290.80	25,497	26,503	901,419

Figure 5: McCrary test - Manipulation of Yearly Log-Variation of Employment Income (2014)

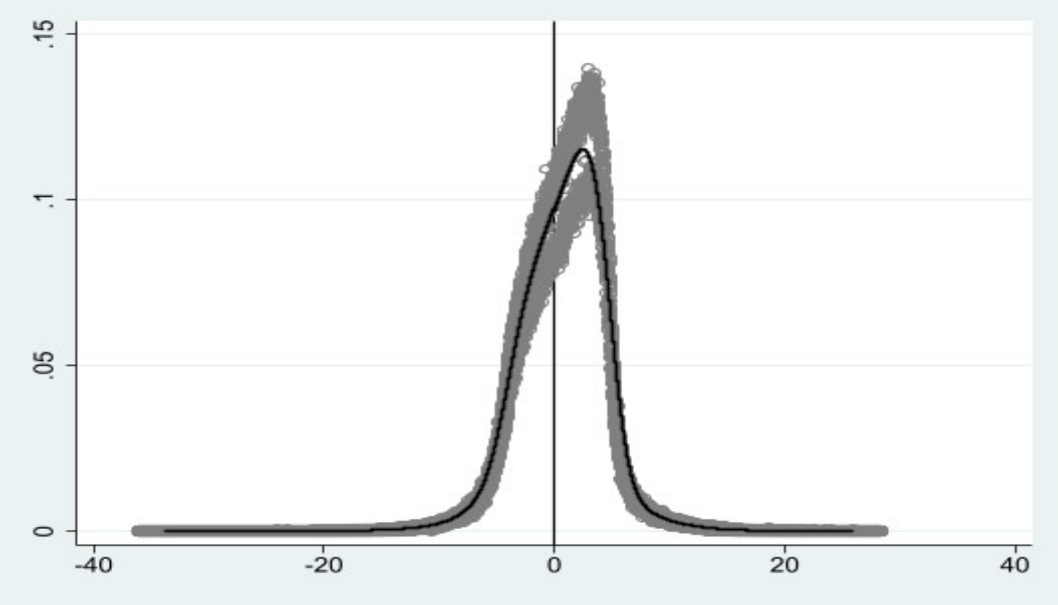


Table 2: Density test - Manipulation of Gross Income - Robust Method

Year	**T**	**P>|T|**
2011	0.9228	0.3561
2012	0.4347	0.6638
2013	-1.3191	0.1871
2014	-0.9792	0.3275
2015	-0.8725	0.3829
2016	0.5155	0.60625
2017	-0.3634	0.7163

Table 3: Balance test on Control Variables

Variables	**Diff-in-disc estimates**	***h* CER-Optimal Bandwidth**	**Observations**
Firm-dimension	1.06e-15	0.497	890,686
	(3.49e-07)		
Spouse support	-.4241714	1.435	2,573,507
	(.4497821)		
Bonus productivity	-9.59e-14	0.413	740,245
	(4.30e-06)		
ATECO 5-digit	4.82e-12	0.502	899,638
	(.0000921)		
Total tax reliefs	-1.25e-15	0.587	1,052,518
	(2.17e-07)		

Notes: Baseline Diff-in-discontinuities estimates. We focus on the period 2011-2017. Estimation method: FE with a first order polynomial of the forcing variable and a linear interaction term. Standard Errors are robust to heteroskedasticity (reported inside the brackets). The dependent variable 'bonus productivity' follows a logarithmic transformation. Significance at the 10% level is represented by *, at the 5% level by **, and at 1% level by ***.

Figure 6: Density test - Manipulation of Gross Income - Years 2011-2017

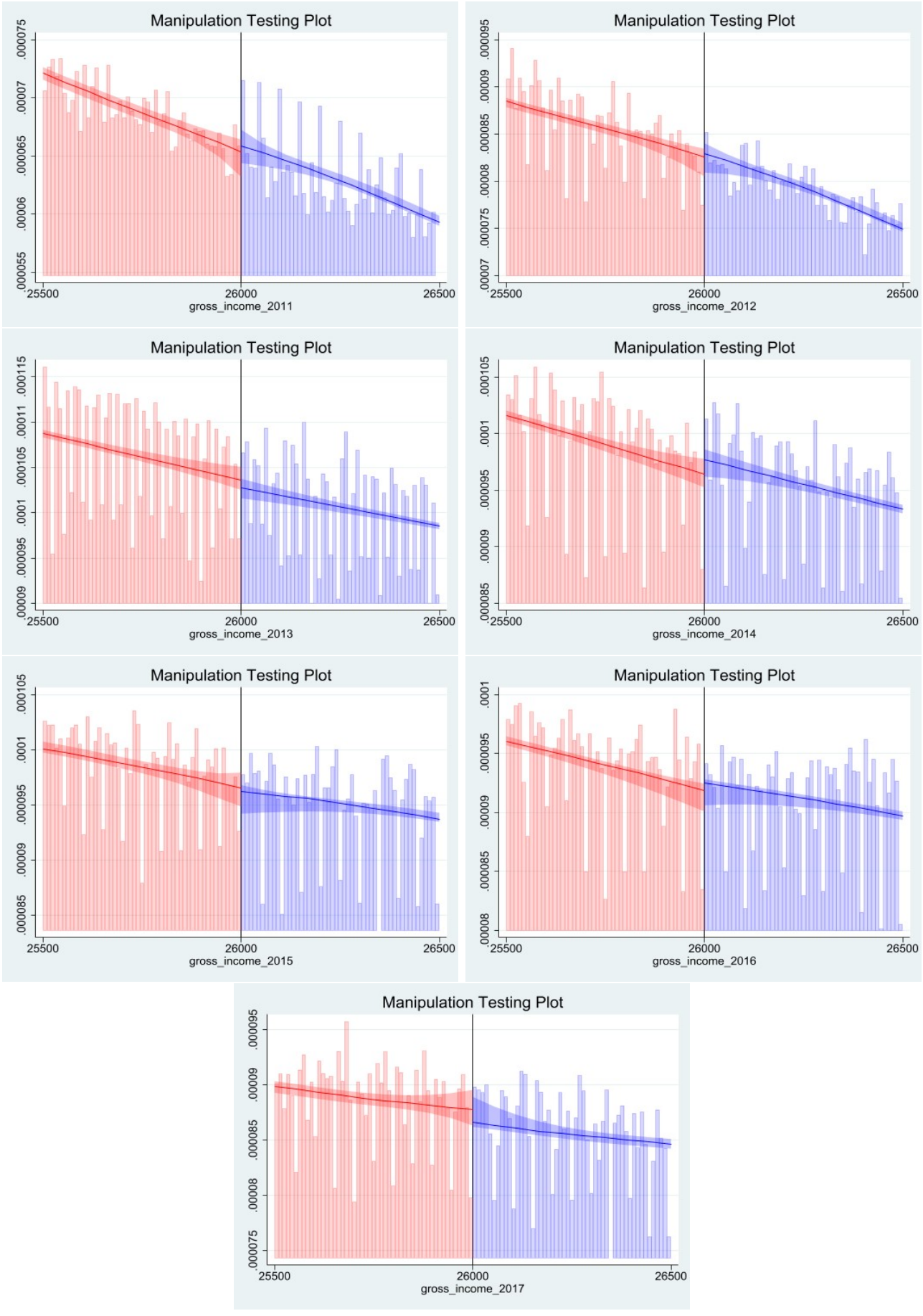


Figure 7: Pre (local) parallel trend (2011-2013).

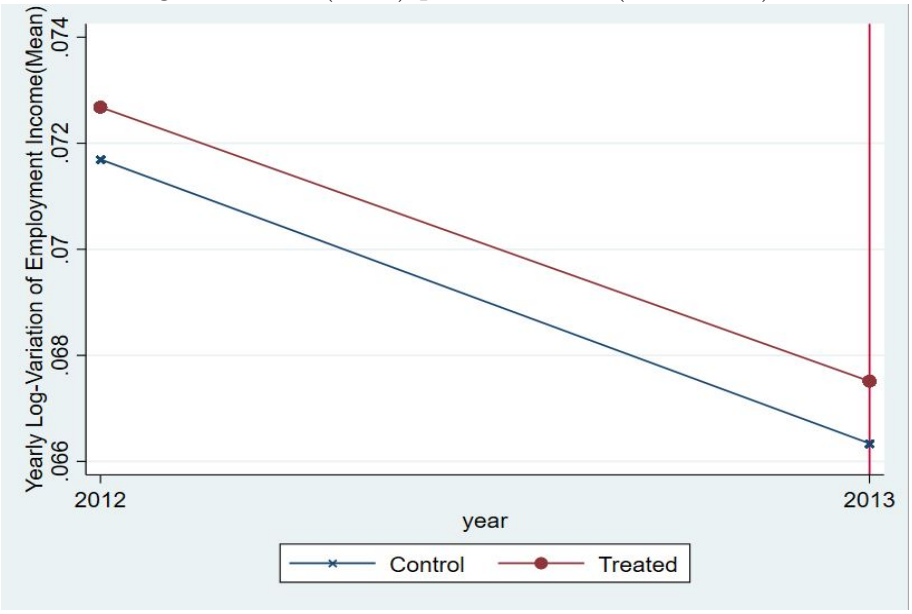


Table 4: Diff-in-disc estimates on Yearly Log-Variation of Employment Income

Variables	(1)	(2)	(3)	(4)	(5)
Bonus introduction	0.00165	-0.000589	-0.000579	-0.000490	-0.000396
	(0.466)	(0.632)	(0.638)	(0.696)	(0.760)
Diff-in-disc controls	X	X	X	X	X
Personal		X	X	X	X
Familiar		X	X	X	X
Job		X	X	X	X
Taxation			X	X	X
Income				X	X
Geography					X
Constant	0.0741***	0.458***	0.448***	-2.956	-17.28
	(0)	(0)	(0)	(0.976)	(0.865)
Interaction Term	First	First	First	First	First
Labour Earnings Polynomial	First	First	First	First	First
h	0.503	0.503	0.503	0.503	0.503
Bandwidth	CER	CER	CER	CER	CER
Observations	1,413,801	941,292	941,292	941,292	901,419
R-squared	0.022	0.130	0.130	0.138	0.138

Notes: The dependent variable is the log of the yearly variation of employment income. The independent variables *'spouse support', 'total taxes', 'productivity bonus', 'total tax reliefs', net income from "reddito di vantaggio", net income from "reddito forfetario","total gross income', 'Cassa integrazione e guadagni'* follow a log transformation. In all the regressions we apply individual fixed effects and correct for heteroskedasticity. Pre-treatment period is 2011-2013. P-values are reported in parentheses. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 5: Diff-in-disc estimates on Yearly Log-Variation of Employment Income

Variables	(1)	(2)	(3)	(4)	(5)
Bonus introduction	-0.00172	0.000400	0.000404	0.000371	0.000475
	(0.161)	(0.517)	(0.513)	(0.552)	(0.458)
Diff-in-disc controls	X	X	X	X	X
Personal		X	X	X	X
Familiar		X	X	X	X
Job		X	X	X	X
Taxation			X	X	X
Income				X	X
Geography					X
Constant	0.0746***	0.579***	0.569***	-39.81***	-38.54***
	(0)	(0)	(0)	(0.000856)	(0.00167)
Interaction Term	First	First	First	First	First
Labour Earnings Polynomial	First	First	First	First	First
h	1.105	1.105	1.105	1.105	1.105
Bandwidth	MSE	MSE	MSE	MSE	MSE
Observations	3,106,780	2,072,694	2,072,694	2,072,694	1,984,498
R-squared	0.022	0.166	0.166	0.176	0.176

Notes: The dependent variable is the log of the yearly variation of employment income. The independent variables *'spouse support', 'total taxes', 'productivity bonus', 'total tax reliefs', net income from "reddito di vantaggio", net income from "reddito forfetario",'total gross income', 'Cassa integrazione e guadagni'* follow a log transformation. In all the regressions we apply individual fixed effects and correct for heteroskedasticity. Pre-treatment period is 2011-2013. P-values are reported in parentheses. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 6: Diff-in-disc estimates on Yearly Log-Variation of Employment Income. Fake reform year.

Variables	2013 fake year (1)	2013 fake year (2)	2015 fake year (3)	2015 fake year (4)	2016 fake year (5)	2016 fake year (6)
Bonus introduction	0.000372	0.000704	-0.000409	0.000342	-0.000271	8.45e-05
	(0.000951)	(0.000749)	(0.00109)	(0.000608)	(0.000644)	(0.000471)
Constant	-36.08	-26.98***	-39.90	-42.56***	-153.8***	-82.26***
	(22.27)	(5.661)	(61.26)	(10.55)	(31.52)	(8.016)
Interaction Term	First	First	First	First	First	First
Labour Earnings Polynomial	First	First	First	First	First	First
h	0.861	1.476	0.600	1.180	0.762	1.280
Bandwidth	CER	MSE	CER	MSE	CER	MSE
Observations	1,543,742	2,646,848	1,073,993	2,119,367	1,367,411	2,298,505
R-squared	0.161	0.197	0.147	0.179	0.167	0.204

Notes: The dependent variable is the log of the yearly variation of employment income. The independent variables *'spouse support', 'total taxes', 'productivity bonus', 'total tax reliefs', net income from "reddito di vantaggio", net income from "reddito forfetario",'total gross income', 'Cassa integrazione e guadagni'* follow a log transformation. In all the regressions we apply individual fixed effects and correct for heteroskedasticity. Pre-treatment period is 2011-2013. P-values are reported in parentheses. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Figure 8: Difference-in-Discontinuities graph

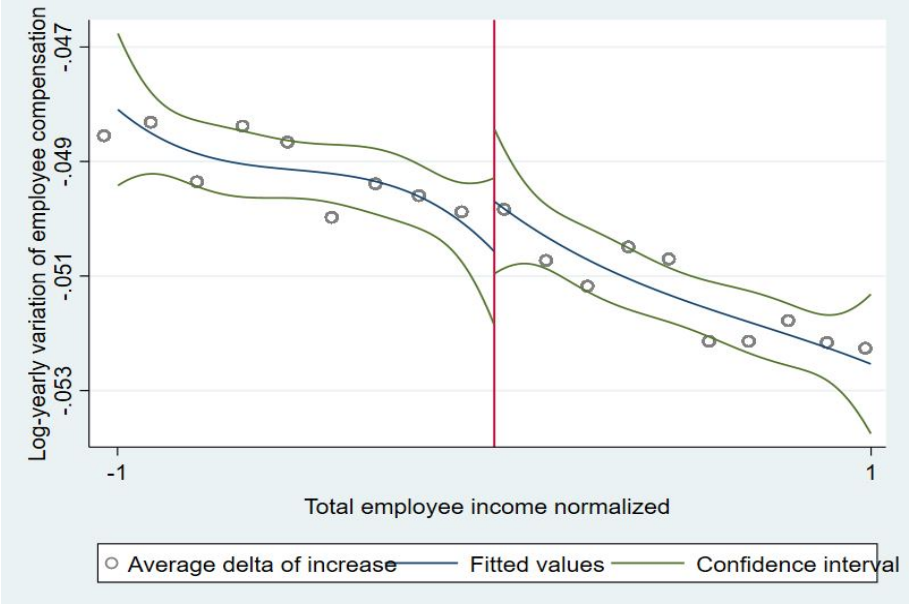
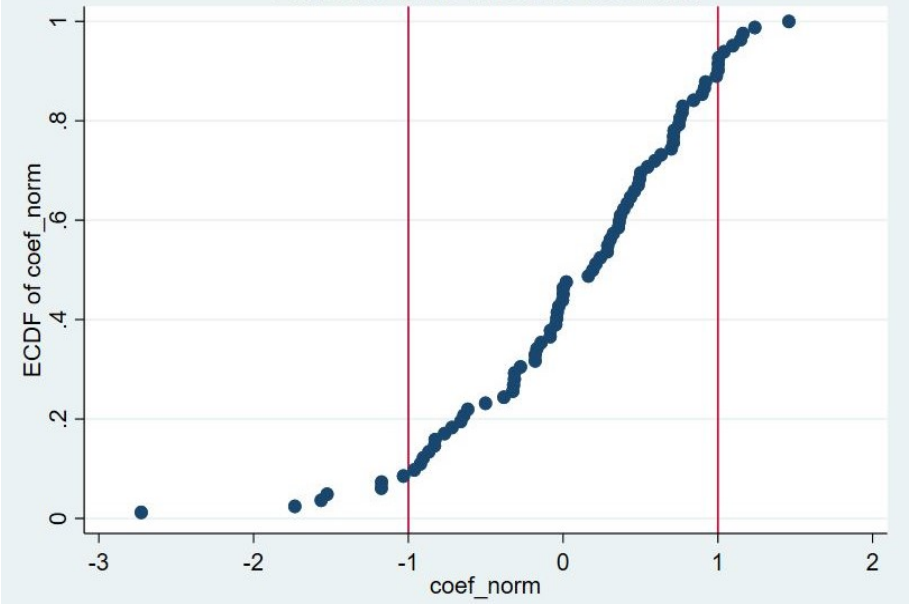


Figure 9: Placebo test for yearly log-variation of employment income



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