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ABSTRACT

Policy makers are generally interested in both the anti-poverty impact and the efficiency of reforms. To connect these two dimensions, I measure the poverty gap change per unit of net revenue that tax-benefit reforms produce. To isolate the impact of reforms and account for labour supply responses, I apply a microsimulation decomposition framework to poverty gap and net revenue changes. Labour supply responses are accounted for using reduced-form models, partly exploiting variation over time that reforms produce. I measure this indicator in Belgium between 2005 and 2014, focusing on revenue changes at the bottom half of the income distribution. Without considering labour supply reactions, reforms reduced the poverty gap among the poor by €0.6 for each euro of net revenue decline. However, this drops to €0.4 when negative labour supply reactions are included, which were caused by unemployment benefits growing faster than in-work compensations. These results highlight the importance of looking simultaneously at reforms to in- and out-of-work benefits.

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1   Introduction

Policy makers are generally interested in both the anti-poverty impact and the efficiency of tax-benefit reforms targeted at low-income households. Many studies evaluate the impact of reforms using tax-benefit microsimulations techniques. These techniques allow isolating the distributional impact of reforms from the environment in which they operate (for a formal framework and examples see e.g. Figari, Paulus, & Sutherland, 2015). Many of these studies consider only the mechanical effect that reforms have on incomes. This leaves aside other effects such as reform-driven labour supply reactions that can also affect poverty outcomes. At the same time, studies that do consider labour supply reactions, generally do not estimate the cost of reducing poverty in terms of the net revenue changes provoked by those same reforms. In addition, those studies usually do not identify labour supply effects exploiting the same changes that reforms produce. To connect the anti-poverty and efficiency concerns regarding policy reforms, I measure the poverty gap change per unit of net revenue that tax-benefit reforms produce. In doing so, I include labour supply effects that are partly identified by the same changes that reforms produce.

Connecting anti-poverty and efficiency concerns is particularly relevant because there can be inherent tensions between them. In this regard, it has been argued that the goals of redistribution, encouraging labour market participation and limiting government costs often conflict with each other. This has been referred to as the 'iron triangle' of welfare reform (Adam, Brewer, & Shephard, 2006; Blundell, 2002). The idea behind this trilemma is that increasing transfers to the poorest would come at the cost of hampering financial work incentives or at a high budgetary cost (e.g. if in-work transfers were also increased). In line with this, in the last decades in Belgium and other Northwestern EU welfare states, tax-benefit reforms that deal with these interrelated challenges have been implemented. An illustration of this is the fact that 'making work pay' policies that deal simultaneously with in-work poverty and work incentives have become widespread. For their part, the evolution of out-of-work benefits across countries has been more diverse.

I measure the proposed indicator in Belgium for two reasons. First, Belgium was one of the many countries were in-work benefits were implemented and expanded during the last two decades. Moreover, also out-of-work benefits were increased and considerably more than in-work benefits. Second, previous research using tax-benefit microsimulation techniques studied the tensions between redistribution, work incentives and government budgets in Belgium (Decoster, Perelman, Vandelannoote, Vanheukelom, & Verbist, 2015). Nonetheless, this research analysed each element separately, and did not fully account for reform-driven labour supply reactions. The empirical methodology that I propose improves on these elements.

That being said, I study the (cash) tax-benefit reforms implemented in Belgium between 2005 and 2014, focusing on households with members available for the labour market and at the bottom half of the income distribution. This means comparing poverty and revenue indicators based on the income distribution of 2005 and based on a counterfactual distribution in which the policies of 2014 are applied to the population of 2005. By utilising

the same population, changes other than policy reforms are not considered. For their part, labour supply reactions are taken into account using a reduced-form model. Without considering labour supply reactions, results indicate that tax-benefit changes reduced the average poverty gap among the poor by €31, while they reduced net revenue at the bottom half of the income distribution by €56 per person. This implied a ratio of €0.6 of poverty gap reduction for each euro of net revenue decline. However, this drops to €0.4 when including labour supply reactions because policy changes reduced the probability of being in the labour market. This reduction occurred because unemployment benefits grew faster than in-work compensations. As a reference, up to 2009 the 'mechanical' ratio between poverty and revenue changes was €0.08 and it goes down to €0.07 when including labour supply reactions. This meant that reforms in later years were more targeted to the poor and created somewhat more deadweight cost. These results highlight the importance of looking simultaneously at and balancing reforms to in- and out-of-work tax-benefits.

The next section reviews previous related research. Section 3 describes the empirical methodology. Section 4 presents the empirical application and section 5 concludes.

2 Previous related research

By studying changes in a welfare measure per unit of net revenue, my research is related to the literature on the marginal cost of public funds (MCF). The MCF measures the *welfare* cost of raising an additional unit of income, taking into account both the mechanical and behavioural effects of reforms (see e.g. Kleven & Kreiner, 2006). In this way, this concept connects the three elements of the aforementioned trilemma: redistribution (that affects welfare), net revenue and labour market participation. Alternately, this concept can also measure the marginal benefit of spending an additional unit of income. After Saez (2002) showed the importance for theoretical optimal taxation of including responses both at the intensive and extensive margin, just a few papers have studied the MCF using this insight (Figari, Gandullia, & Lezzi, 2018; Kleven & Kreiner, 2006). In line with the concept, these studies used a welfarist approach.

With respect to previous studies on the MCF, this paper differs in the following. I argue that it is also relevant to study the marginal benefit of public funds using a non-welfarist and official measure such as poverty. This is the same argument that Kanbur, Keen, and Tuomala (1994, p. 1613) used to initiate the study of optimal taxation for poverty alleviation, to thus 'capture the tone of much policy debate'. Regarding analytical approaches, the recent studies on the MCF have used a *small reform* approach combined with microsimulation techniques. Although I follow the same general logic of those studies, my approach is solely based on microsimulations, which will not generate many differences when focusing on poverty—instead of welfare. With respect to the reforms studied, in my empirical application I study all reforms between two points in time, but the concept could also be applied to a subgroup of reforms or to hypothetical ones. Within the MCF literature using microsimulations, all earlier research studied hypothetical reforms (Browning, 1978; Browning & Johnson, 1984; Kleven & Kreiner, 2006; Triest, 1996), while more recent

research focusing on actual (supposedly marginal) reforms has been scant. Moreover, this recent literature had some limitations such as using stylised labour supply elasticities (Eissa, Kleven, & Kreiner, 2008), identifying those elasticities not using the variation that the analysed reforms produce, and making strong analytical simplifications to represent those reforms (Figari et al., 2018). I come back to the identification issue at the end of this section.

With the advent of tax-benefit microsimulation models, many studies have analysed the mechanical effects of tax-benefit reforms using the decomposition framework formalised by Bargain and Callan (2010). In this framework, by generating 'intermediate' counterfactual income distributions that hold constant the underlying population and allow policies to change, policy effects are separated from the environment in which they operate. This 'no population change' decomposition has been applied to many European countries (Bargain et al., 2015; Hills, Paulus, Sutherland, & Tasseva, 2019; Matsaganis & Leventi, 2014; Paulus, Figari, & Sutherland, 2017; Paulus & Tasseva, 2017) and elsewhere (e.g. Bargain et al., 2015). In Belgium, studies have analysed periods that somewhat overlap with the one I investigate, finding that policies have generally reduced inequality or relative poverty, and—accordingly—impacted negatively public budgets (Decoster et al., 2015; Hills et al., 2019; Paulus & Tasseva, 2017).

These studies reveal the 'morning-after effect' of policies and therefore give a partial account by not considering other policy-driven effects such as labour supply responses. In Belgium in particular, Decoster et al. (2015) did study the related issue of financial work incentives, finding that reforms have weakened them. This means that the marginal benefit per euro spent was actually less than one euro due to the distortions produced by the same reforms. Because Decoster et al. (2015) did not translate the reform-driven changes in work incentives into behavioural changes, they were not able to calculate the precise marginal benefit of each euro spent[1]. To include reform-driven labour supply responses when isolating the effect of reforms on distributional outcomes, few studies for other countries (e.g. for the UK Bargain, 2012; and for Australia Creedy & Hérault, 2015; Herault & Azpitarte, 2016) have utilised the framework that Bargain (2012) extended to include policy-driven labour supply effects. This consists of first estimating a structural labour supply model exploiting cross-sectional variation in base- or end-period data. Subsequently, an income distribution where the underlying population is held constant and policies are allowed to change is compared to a similar distribution in which the labour supply of people is also 'allowed' to respond to the reforms.

I utilise the framework of Bargain (2012) with two differences to previous studies. First, previous studies did study distributional outcomes such as poverty but did not calculate changes per unit of net revenue. Second, I identify labour supply responses exploiting partly the same variation that reforms produced, which is more aligned with decomposing changes

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[1] They did calculate a special case of the marginal cost of public funds. They mainly used this concept to aggregate incentives across people and margins. However, in the formula of the MCF they used stylised elasticities and instead of parameterising the reforms studied, they use a simple proportional tax increase (the latter is the same simplification of Figari et al., 2018). Therefore, the only parameters representing the reforms studied were incentive measures.

over time. With respect to the latter, using a similar decomposition framework for the US, Hoynes and Patel (2017) studied the mechanical and behavioural effects on poverty of changes in the Earned Income Taxed Credit. While their behavioural model did not estimate the impact of policy changes on labour supply but directly on poverty[2], for identification they did exploit the same variation that reforms produced, which is closer to my identification strategy.

3 Empirical methodology

In this section I elaborate on my empirical methodology to measure the poverty gap change per unit of net revenue that tax-benefit reforms produce. In particular, I define this concept and explain my microsimulation approach and the strategy to identify labour supply responses.

3.1 The anti-poverty marginal benefit of public funds

In this paper I measure the poverty gap change per unit of net revenue that tax-benefit reforms produce. For brevity I refer to this indicator as the Anti-poverty Marginal Benefit of Public Funds (AMBF). This is simply defined as the ratio between changes in the average poverty gap among the poor ΔP (in monetary terms) and changes in net revenue ΔR that tax-benefit reforms produce, that is, ΔP/ΔR. This bears resembles with the concept of the Marginal Cost of Public Funds (MCF). The MCF measures the *welfare* cost of raising an additional unit of income. Alternatively, it can also measure the marginal benefit of spending an additional unit. To allow for distributional concerns, Dahlby (1998) defined the *Social Marginal Cost of Public Funds* (SMCF) which measures the same as the MCF but weighting differently the welfare changes of people according to their income position. In this way, the AMBF can also be seen as a special case of the SMCF whereby changes in welfare come only from the incomes of people below the poverty threshold, attaching to them equal weights.

With respect to the specific components of the AMBF, I study the average poverty gap among the poor due the intuitiveness of this indicator. In other words, I study the average distance between the poverty threshold and the incomes of the poor[3]. In relation to net revenue, I obtain it for each household by summing taxes and social contributions and subtracting social benefits, and later I calculate the average per person (at the bottom half of the income distribution in my empirical application). Consequently, because poverty is

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[2] Their model therefore includes more margins of reaction. This comes at the cost of a model for a more multifaceted outcome and relying on a 'larger' parametric prediction when contrasting behavioural effects to mechanical ones coming from a tax-benefit microsimulation model. Furthermore, they would have needed another behavioural model for net revenue to be able to estimate poverty changes per unit of net revenue (whereas a labour supply model serves both indicators).

[3] This indicator considers only the depth of poverty and not its extent in the population. Alternatively, the poverty gap *index* (Ravallion, 2017) apportions the sum of the poverty gaps among the whole population instead of only among the poor. However, because it represents the average poverty gap among poor and non-poor, its units are less intuitive.

driven by a smaller segment of people than revenue, the AMBF without taking into account labour supply reactions reflects how targeted to the poor are tax-benefit changes. In turn, including those reactions gives the actual AMBF, and comparing both indicators shows the size of the behavioural effects. Regarding the reforms that one can study, they can be all those implemented between two points in time, a subgroup of them or hypothetical ones.

3.2   A microsimulation approach

The intention of this paper is measuring the poverty gap change per unit of net revenue that tax-benefit reforms produce. Because both outcomes can change due to tax-benefit reforms and many other factors, I must isolate the effect of reforms from the effect of those other factors. To do so, the recent literature estimating the MCF has used a *small reform* approach combined with tax-benefit microsimulations. This approach allows deriving an analytical formula that depends—among others—on the derivatives of marginal and participation tax rates with respect to a *small* tax-benefit change, and on labour supply elasticities. These multipliers represent rates of change while holding other factors constant. Thus, by applying small tax-benefit changes, one can obtain counterfactual outcomes under those scenarios. Instead of this approach to obtain the necessary counterfactual outcomes, I propose using an approach fully based on microsimulations, which will not generate many differences when focusing on poverty—instead of welfare[4].

To isolate the effect of tax-benefit reforms on poverty and revenue changes, I start from the decomposition framework of Bargain (2012). Equation 1 shows this decomposition for a change between two points in time in any aggregate index I. d_t refers to the function that transforms household gross incomes into net incomes, z_t to the monetary parameters of policies, y_t to the gross income distribution and α to an uprating factor to make monetary values of year 1 comparable to those of year 2. Subscripts refer to the year of the element, whereas the gross income distribution of year 1 with a superscript indicates that people are 'allowed' to react to the policies of year 2[5]. First, the index $I[d_2(z_2, \alpha y_1)]$ based on a counterfactual income distribution that holds constant the underlying population y_1 (including their gross incomes) and applies the policies of year 2, is compared to the observed index in year 1 $I[d_1(\alpha z_1, \alpha y_1)]$. Because the only discrepancy between these indices is the policies applied, their difference reflects the effect of those policies. Second, to obtain the policy-driven labour supply effect, the same index $I[d_2(z_2, \alpha y_1)]$ is compared to the index $[d_2(z_2, \alpha y_1^2)]$ which differs by 'allowing' people to adapt their behaviour to the imposed policies of year 2. Lastly, the difference between the index $[d_2(z_2, \alpha y_1^2)]$ and the

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[4] In addition, the small reform (or sufficient statistics) approach does not have the same advantages when applied to a non-welfarist measure. Within a welfarist approach, when reforms are small, the aggregate welfare formula does not depend on the functional form of utility. This is because when people's reactions are small, they end up with a similar post-reform utility. In turn labour supply responses do not influence welfare and these effects come only from mechanical changes in income due to the reforms. Estimating labour supply effects on welfare would require specifying the functional form of utility, which under these assumptions becomes conveniently unnecessary.

[5] It is relevant to mention that this decomposition is path dependent. I chose this path combination based on base-period data because it considers the impact of policy changes in prospect.

observed index in year 2 $I[d_2(z_2, y_2)]$ represents other effects (changes in wage inequality, demographics, etc.). To calculate the AMBF I use the policy and the policy-driven labour supply effects, which are now separated from other effects[6].

$$\Delta = I[d_2(z_2, \ y_2)] - I[d_1(\alpha z_1, \alpha y_1)] =$$
$$\{I[d_2(z_2, \ y_2)] - I[d_2(\ z_2, \alpha y_1^2)]\} + \qquad \text{other effects}$$
$$\{I[d_2(z_2, \alpha y_1^2)] - I[d_2(\ z_2, \alpha y_1)]\} + \qquad \text{labour supply effect} \qquad (1)$$
$$\{I[d_2(z_2, \alpha y_1)] - I[d_1(\alpha z_1, \alpha y_1)]\} \qquad \text{policy effect}$$

To account for labour supply effects, previous studies using the framework of Bargain (2012) have estimated structural discrete choice models of labour supply exploiting cross-sectional variation in base- or end-period data. Instead, using reduced-form models I identify labour supply responses exploiting partly the same variation that reforms produce, which is more aligned with decomposing changes over time. As in the small reform approach, I separate the contribution of labour supply responses in the intensive and extensive margins. This implies estimating models at each margin. Equation 2 shows how I implement this for poverty gap changes ΔP by further specifying the policy and policy-driven labour supply effects of equation 1. To simplify the notation, I assume that monetary values are already expressed in values of the year 2. $p(.)$ represents a function that returns the contribution of a household to the average poverty gap among the poor. That is, the extra income that would be required to mechanically bring a household to the poverty threshold, divided by the number of poor people in the population. Expressed as a formula $p(d_i(.)) = max(0, \bar{d} * s_i - d_i(.))/N_p$, where $\bar{d}$ is the poverty threshold, s_i an equivalence scale (e.g. the OECD one), d_i the *household* net disposable income of *person i* and N_p the number of poor people in the population. In contrast to equation 1, $d_{it}(.)$ and $y_{i1}(.)$ also have the subscript i because now they do not represent the whole distribution of gross and net incomes but the household net and gross incomes of person i. Continuing with equation 2, N is the number of people available for the labour market, N_i the number of available persons in the household of person i (e.g. two for couples and one for singles), E_{i1} the probability of being employed of person i, h_{i1} hours worked, 0_{i1} describes the situation in which person i is unemployed, and E_{i1}^2 and h_{i1}^2 represent the probability of working and hours worked under the policies of year 2. In the next paragraph I explain the combined use of household and individual concepts, and the simultaneous use of in- and out-of-work incomes. The formulas for decomposing net revenue are the same but instead of summing the poverty gap contributions $p(.)$, I sum the balance between taxes and benefits of households $r(.)$.

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[6] It is possible that changes in revenue from *personal* taxes and benefits are counterbalanced by increases in other fiscal sources not considered among the policies (e.g. VAT). Another limitation of this approach is that, besides considering labour supply reactions, the counterfactual scenarios are partial equilibriums and therefore do not include other potential effects of policies.

$$policy\ effect =$$

$$\sum_{i=1}^{N}\left\{\frac{p\left(d_{i2}(z_2,y_{i1}(h_{i1}))\right)}{N_i}E_{i1} - \frac{p\left(d_{i2}(z_2,y_{i1}(0_{i1}))\right)}{N_i}(1-E_{i1})\right\} -$$

$$\sum_{i=1}^{N}\left\{\frac{p\left(d_{i1}(z_1,y_{i1}(h_{i1}))\right)}{N_i}E_{i1} - \frac{p\left(d_{i1}(z_1,y_{i1}(0_{i1}))\right)}{N_i}(1-E_{i1})\right\}$$

$$labour\ supply\ effect =$$

(2)

$$\sum_{i=1}^{N}\left\{\frac{p\left(d_{i2}\left(z_2,y_{i1}(h_{i1}^{2})\right)\right)}{N_i}E_{i1}^{2} - \frac{p\left(d_{i2}(z_2,y_{i1}(0_{i1}))\right)}{N_i}(1-E_{i1}^{2})\right\} -$$

$$\sum_{i=1}^{N}\left\{\frac{p\left(d_{i2}(z_2,y_{i1}(h_{i1}))\right)}{N_i}E_{i1} - \frac{p\left(d_{i2}(z_2,y_{i1}(0_{i1}))\right)}{N_i}(1-E_{i1})\right\}$$

Equation 2 deserves some further explanations. First, each person available for the labour market is represented twice: when s/he is employed and unemployed. The non-observed incomes are obtained using microsimulations. This approach corresponds to the pseudo-distribution approach of Creedy and Kalb (2005) (and is similar to the theoretical model of Eissa et al. (2008) within the small reform approach literature[7]). While people are counted twice in this approach, their two pseudo-observations are weighted by the probabilities of being employed and unemployed, which add-up to one. Second, the household poverty gap $p(.)$ of person i is divided by the number of persons available for the labour market N_i because of the following. While the poverty gap and net revenue are household concepts and labour supply decisions will also consider household incomes, non-structural reduced-form models are traditionally individual, and therefore also my general approach. This implies that, for example, for a couple in which both partners are employed, the household incomes when each partner is unemployed can be different. Normally one would count the poverty gap and net revenue of the common household once, whereas in my approach I count the possibly somewhat different poverty gaps and net revenues twice but then they are divided in half. Third, the labour supply models (detailed in the next sub-section) allow estimating E_{i1}^{2} and h_{i1}^{2} and also getting the baseline probability E_{i1} and the likely hours that unemployed might work (while for employed I use their observed hours).

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[7] E.g. the change in the in the average poverty gap among the poor applying a small reform approach after a small reform dz would be

$\frac{dP}{dz} = \Sigma^{N}\left\{\frac{\Delta p_i^{z}}{\Delta d_i^{z}}\frac{\partial T_i}{\partial z}E_i - \frac{\Delta p_{0\,(i)}}{\Delta d_{0\,(i)}}\frac{\partial T_0}{\partial z}(1-E_i) + \frac{\Delta p_i^{h}}{\Delta d_i^{h}}(1-m_i)w_i\frac{dh_i}{dz}E_i + (p_i - p_{0(i)})\frac{dE_i}{dz}\right\}$, where T_i and T_0 are net tax-benefits when working and not working, m_i the effective marginal tax rate and w_i hourly wages. Because p_i is not differentiable near the poverty threshold, the terms $\frac{\Delta p}{\Delta d}$ would adjust the derivatives for people crossing the threshold (simply making the change in income proportional to the change in the poverty gap and thus correctly leaving out changes above the threshold). The four terms of this equation represent the mechanical and labour supply effects in each margin respectively.

3.3 Estimating labour supply responses

Most previous studies decomposing the labour supply effect of tax-benefit reforms have estimated labour supply models exploiting static variation in budget constraints and behaviour *across* people. I differ from those studies by identifying these responses exploiting partly the variation that reforms produced *over time*, which is more aligned with decomposing the effect of those reforms (also over time). The following is the basic model to take into account labour supply responses. I start from the standard static model of labour supply. Individuals are assumed to maximise utility with respect to consumption (which as in other studies will be approximated by disposable income) d_i and labour supply h_i subject to the budget constraint $d_i = w_i h_i + O_i - T_i (w_i h_i + O_i)$, where w_i is the (assumed exogenous) gross hourly wage rate, O_i other household incomes, $T_i (.)$ the tax-benefit function, $-T_0 (.)$ net benefits when not working, and then gross earnings are $e_i = w_i h_i$. Similar to Immervoll, Kleven, Kreiner, and Saez (2007), utility is assumed quasi-linear in consumption and specified as $u_i(d_i, h_i) = d_i - v_i(h_i)$. $v_i(.)$ is an increasing function for the disutility of work that includes the fixed cost of working. This specification rules out income effects which in the labour supply literature are often small or not statistically significant (e.g. Bargain, Orsini, & Peichl, 2014; Collado, 2018; Jäntti, Pirttilä, & Selin, 2015; Selin, 2014). At the intensive margin, first order conditions lead to a labour supply function h_i that depends on the slope of the budget constraint: $h_i(w_i(1 - EMTR_i))$, where EMTR is the Effective Marginal Tax Rate. At the extensive margin, the condition for being employed is that utility in this state is higher than when unemployed, which implies $e_i(1 - \frac{T_i - T_0}{e_i}) > v_i(h_i)$. Defining the Participation Tax Rate as $PTR_i = \frac{T_i - T_0}{e_i}$, the previous condition defines an individual employment function E_i that depends on the net-of-PTR earnings: $E_i(e_i(1 - PTR_i))$. The PTR measures the proportion of household earnings taken in (effective) tax and withdrawn (net) benefits when a household member moves from unemployment to employment[8]. In turn, net-of-PTR earnings represent the net gain of moving to employment in income units. More details on the calculation of work incentives can be found in Appendix A. The derived labour supply functions have the empirical counterparts showed in equations 3 and 4, where $\boldsymbol{X}_{it}$ is a vector of control variables and in the extensive margin I assume $v_i(.)$ follows a logistic distribution. Once I have identified the relationship between work incentives and labour supply, for the decomposition analysis I can predict labour supply under the policies of a given year.

$$h_{it} = \beta_{EMTR} w_{it} (1 - EMTR_{it}) + \boldsymbol{\beta}_{it}' \boldsymbol{X}_{it} + \varepsilon_{it} \qquad (3)$$

$$Pr(E_{it} = 1) = \Lambda(\beta_{PTR} e_{it} (1 - PTR_{it}) + \boldsymbol{\beta}_{it}' \boldsymbol{X}_{it}) \qquad (4)$$

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[8] Taxes in relation to earnings represent tax rates and benefits in relation to earnings represent replacement rates. Then for a person who only receives a benefit when unemployed and only pays taxes when employed, the PTR can be understood as the sum of her replacement and tax rates. Note that $T(.)$ is negative every time benefits are higher than taxes paid.

To overcome potential omitted-variable bias between labour supply and work incentives, with repeated cross-sectional data (as it is available for my empirical application) one can estimate these models in the following way[9]. Following the method pioneered by Cutler and Gruber (1996) and similar to the application of Kalíšková (2018) estimating the effect of work incentives on female employment in EU countries, one can instrument work incentives with a group-level simulated variable. This simulated instrumental variable (IV) is built by 'freezing' a population (e.g. of a first year) and recalculating group average work incentives applying the policy changes that took place over the years. Groups must be defined by the differential treatment that reforms have on different people. In this way, those group-averages reflect exclusively mechanical policy changes. This approach can be understood as a parameterised difference-in-difference framework (Hoynes & Patel, 2017). In my case, the treatments correspond to the policy changes that took place between the two periods of the decomposition. In this regard, including more points in time would allow adding more policy variation[10] and estimating more accurately group fixed effects to remove constant differences (while adding time fixed effects to absorb common shocks). There is a (exclusion) restriction and a (relevance) condition for using such a framework. The restriction is that tax-benefit treatments must be (conditionally) exogenous to labour supply outcomes. For its part, the condition is that after adding control variables such as group and time fixed effects, there must be enough variation left in the instrumented variable to be explained by the instrument. In this context, this means that reforms must have affected different groups differently.

It is common that important policy changes take place but that they do not have (sufficiently) different impacts on different groups (e.g. a general increase in unemployment benefits). This would violate the condition to use the previous approach. Then the IV approach of Blundell, Duncan, and Meghir (1998) could be an alternative. This approach consists in defining groups by exogenous variables (e.g. birth cohort and education achieved), adding group and time fixed effects, and instrumenting marginal net earnings by their group's averages. Thus, this approach exploits the differential growth in marginal net earnings between groups. This means that it exploits both the differential impact of reforms on groups as well as the differential growth in their gross earnings. Compared to the aforementioned simulated IV, on the one hand this approach might lose some variation coming from policy reforms because the groups are not necessarily defined by those changes. On the other hand, it gains variation from differential growth in gross earnings. Originally this approach was applied to the intensive margin, and more recently Bartels and Shupe (2018) and Jäntti et al. (2015) applied it to the extensive margin as well.

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[9] As it will be seen, in my application I only estimate an extensive margin model. I do not estimate an intensive margin model because there was very little policy variation affecting that margin. If one did estimate an intensive margin model, there might also be a self-selection bias because that model would be estimated on the observed hours of work of employed people. In that case one could test and correct for this.

[10] Adding more points in between implies that the mean of the predicted individual probabilities matches the observed aggregate employment in all years combined but not necessarily in a specific year. For this reason, for the decomposition I scale the predicted probabilities of the first year to match the observed aggregate employment levels (in my empirical application I also scale them to match the somewhat different subsample used in the decomposition).

4 Empirical application

In this section, I measure the poverty gap change per euro of net revenue provoked by the (cash) tax-benefit reforms implemented in Belgium between 2005 and 2014.

4.1 Data and microsimulation model

I utilise data from the tax-benefit microsimulation model EUROMOD H1.0+ (Figari et al., 2015; Sutherland & Figari, 2013) which is mostly based on the cross-sectional EU-SILC survey[11]. With EUROMOD it is possible to calculate net incomes, given gross incomes and personal/household characteristics[12]. I use counterfactual incomes produced by this type of model for several purposes: i) to obtain counterfactual net income distributions for the decomposition analysis in which policies from one year are applied to another (and to 'reobtain' the observed distributions for comparability), ii) to simulate the non-observed incomes in my pseudo distribution approach and when calculating work incentive measures, and iii) to estimate econometric models exploiting group variation over time in policy, earnings and labour supply. My decomposition analysis goes from the earliest to the most recent year with available data, that is, from 2005 to 2014. For the labour supply models I also include available data from years in between (2006, 2007, 2009 and 2011) to add more variation. I make some modifications to the default simulation of out-of-work benefits in EUROMOD, which are specified in Appendix A.

I study people living in households that contain persons available for the labour market, that is, (self-defined) employed or unemployed[13]. I only consider households composed by either couples or singles, with or without (non-working) children. This represents 78% of the people in households with persons available for the labour market. For the decomposition analysis, I concentrate on the bottom half of the income distribution to leave out reforms targeted at the top of the distribution (while to estimate models I also use the top half of the distribution to have more statistical power).

For the decomposition analysis I use the following uprating factor (see equation 1) and poverty threshold. As uprating factor I use average wages[14]. Thus, tax-benefit parameters growing differently than average wages contribute to the policy effects and affect work

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[11] Standard errors take into account the sample design of EU-SILC following the Stata files of Zardo Trindade and Goedemé (2016) and Goedemé (2011). EUROMOD uses other EU-SILC versions than these files. In the versions used by EUROMOD some sample design variables are missing: in 2008 primary sampling units (PSU) are missing so I use instead household identifiers as PSU, and in 2012 the strata is missing so I use a single strata instead.

[12] More information can be found in the EUROMOD country report in https://www.euromod.ac.uk/using-euromod/country-reports.

[13] Therefore, available for the labour market means people aged between 18 and 65 years old excluding self-employed (due to the limited quality of their income data (Immervoll, 2004)), (early) retired, students, disabled, or other inactive.

[14] While the average wages grew 22.4% in the period analysed, CPI grew 19.48%. Thus, policy effects are only slighly larger using CPI.

incentives[15]. Some less relevant taxes and benefits that are not simulated are simply uprated by the wage index and therefore do not contribute to these effects. As poverty threshold I use the official At-risk-of-poverty threshold of the European Commission, which is defined as 60% of median equivalised household income. I anchor this relative poverty line in the first year analysed (in real terms) to exclude possible 'poverty line effects'.

4.2   Most relevant policy changes

Next I present the most relevant changes in the Belgian (cash) tax-benefit system between 2005 and 2014 (a similar description until 2012 can be found in Decoster et al. (2015)). Reforms refer mainly to policies directed to households with members available for the labour market. In Table B1 of the Appendix, I present the main changes in the parameters of policies. Figure 1 exemplifies some of these changes for a hypothetical single parent household with an hourly wage of €13 in 2014 prices (equivalent to around €2150 monthly if the person worked full-time, i.e. 38 hours per week). The flat continuous net income line until around 20 hours of work reflects the 100% withdrawal of social assistance (SA). This can also be seen in the declining dashed-dotted benefits line representing decreasing SA as people increase their hours of work. Benefits for this household also include child benefits (CBs).

Figure 1. Budget constraints, single parent with an hourly wage of €13, 2005-2014
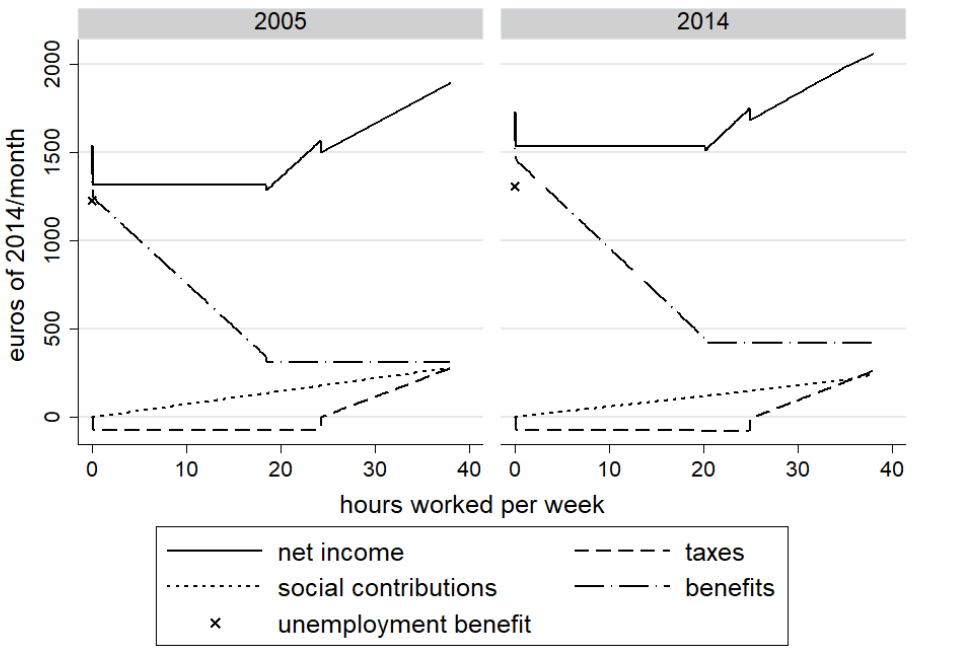
Note: EUROMOD's wage index is used to bring amounts to euros of 2014. In this way, the evolution of policy parameters is contrasted to the evolution of wages.
Source: EUROMOD's Hypothetical Households Tool (HHoT).

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[15] E.g. suppose that to calculate PTR changes between two points in time the same underlying population is used, all wages are uprated by the average wage growth, and out-of-work benefits grow more than that. Then the benefits withdrawn if one moves to work represent a larger proportion of wages in the second period, which increases PTRs.

The main policy change affecting poverty, revenue and participation incentives was increases in unemployment benefits (only somewhat noticeable in Figure 1 due to the specific characteristics of this household). The primary rises occurred in 2009 and 2014. Increases were generally the same for the three types of recipients defined by the rules of unemployment benefits (UBs). An exception to this was in 2009 when the 55% replacement rate for cohabitants was equalised with the rate for singles and head of households at 60%[16]. The other across the board changes corresponded to an increase in replacement rates from 60% to 65% for the three first months of a spell in 2014, and important increases in the maximum and minimum limits of UBs in 2009 and 2014. It is relevant to mention here a data limitation of my analysis: it is not possible to identify people according to the length of their unemployment spells. For this reason, UBs are simulated as if everybody was in their first year of unemployment, while there were also reductions to these benefits for the long-term unemployed. In consequence, probably the mechanical effect of UB changes on poverty and spending will be overestimated, while the misestimations of labour supply effects are less clear-cut[17].

Other policies that went through some changes were social contribution (SC) and SA. SC rebates were expanded in 2006, while in 2009 there was also an increase in the base reduction – although there are few workers with incomes low enough to be entitled to the full base reduction (for a description of SC rebates see Appendix A). This can be noticed in the decline in the steepness of the dotted SC line in Figure 1. With respect to SA, though relatively few jobless households receive this benefit (in the survey data), in some years it grew considerably faster than average wages.

There were also other smaller changes to CBs and special SCs for employees. With respect to child benefits, in 2007 two supplements were implemented. A 'back-to-school' premium was gradually introduced and a special means-tested supplement for single parents was implemented (somewhat complemented by increases in the income limit for social supplements for single parents on replacement incomes). This can be noticed in Figure 1 by the rise of the dashed-dotted benefits line. In relation to special SCs, their brackets were not uprated during the whole period. This might have increased payments for people who were just below the lower bound of a bracket and whose earnings grew (e.g. going above the bound exempting this payment).

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[16] Singles are considered as people living alone (and not paying inter-household transfers); head of households as people whose partners have earnings and UBs below certain limits and do not have other replacement incomes, or whose dependent children have no or low earnings; people not fulfilling the last two conditions are considered cohabitants. The earnings limit for partners was €384.27 per month in 2005 and in real terms doubled in 2009 and grew around another 10% in 2011.

[17] Using longitudinal data to study *transitions* into employment, in Collado (2018) I found that among the long-term unemployed, those unemployed between 12 and 24 months (around one eighth of the unemployed) were somewhat sensitive to changes in participation incentives, while results for those unemployed for more were not statistically significant. Thus, in the current paper, elasticities could be slightly larger if I had retrospective information. However, for those (relatively few) unemployed between 12 and 24 months, the labour supply effect in the simulations would run in the opposite direction compared to the current simulations (because their UBs actually decreased).

4.3 Work incentives variation, econometric specification and simulation

The specific work incentives variation generated by the policy reforms affects the econometric specification chosen. To analyse the effect of policy reforms on the evolution of work incentives I utilise two approaches. First, I do this on a selection of hypothetical households to remove compositional effects, and I break down the evolutions by the categories defined by the main policy changes. Second, I analyse the evolution of the average incentives of groups defined by those categories.

With respect to the first approach, I present some selected results in Figure 2. At the top left corner we observe the evolution of Participation Tax Rates (PTRs) by UB recipient type. We see that the evolution of PTRs was similar for singles and head of households, while PTRs increased importantly in 2009 for cohabitants due to the large increase in UBs for this category. At the top right corner of the figure, we see that PTRs have evolved somewhat similarly for people working for different levels of hourly wages, except in 2006. This household corresponds to a cohabitant and therefore the curves are generally driven by increases in UBs. The somewhat different evolutions in 2006 are due to the expansion of SC rebates, which made work pay more at medium and low full-time equivalent (FTE) earnings. Moving to the bottom left corner of the figure, we do not see many differences between a single person with or without children, except in 2007. This was due to the introduction of the special mean-tested supplement for single parents, which for specific earnings levels could affect how much work paid. Lastly, at the bottom right corner we see the evolution of Effective Marginal Tax Rates (EMTRs). We see that the increase in SC reductions in 2006 had an effect on them but not a large one. The increase in EMTRs for people with high hourly wages in 2011 was caused by special SCs based on taxable income. The brackets for these contributions were not uprated during the whole period and this hypothetical household happened to cross the exemption limit in this year.

Figure 2. Policy effects on work incentives of hypothetical households, 2005-2014
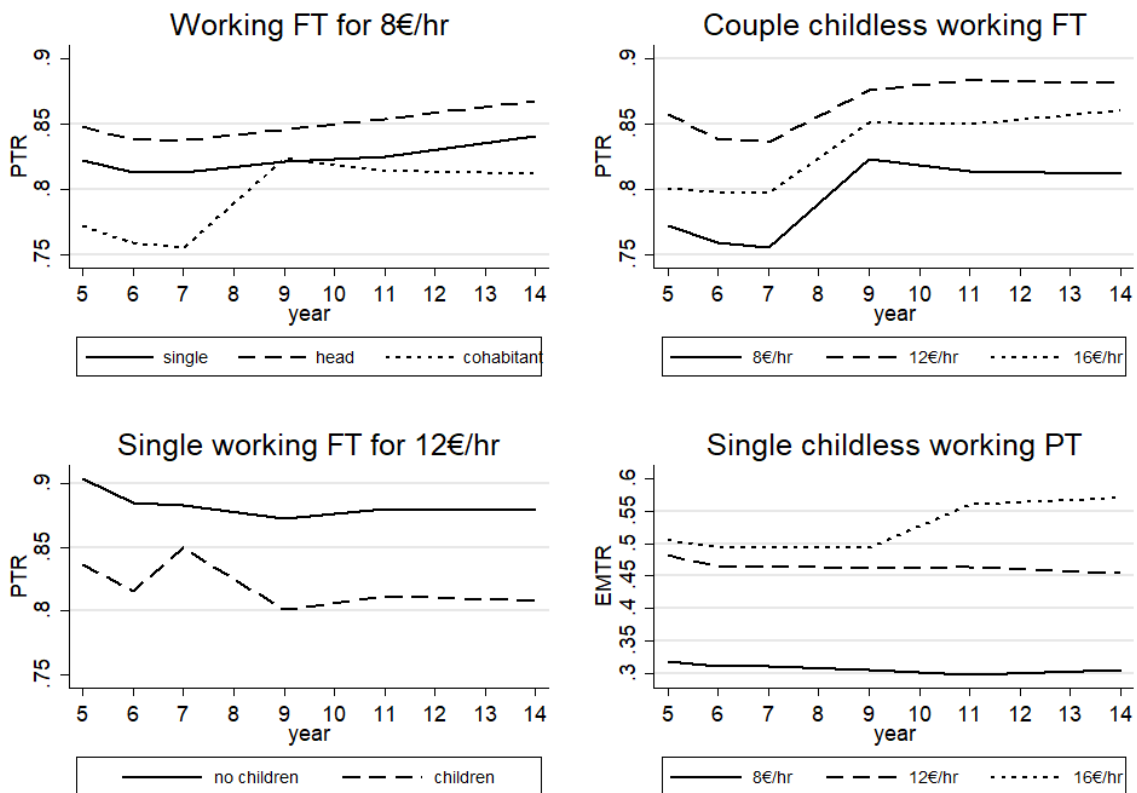
Note: I analyse hypothetical household formed by singles and couples, with and without children, with different hourly wages, and here I present some selected results. Full-time (FT) means working 38 hours per week, while part-time (PT) 30 hours. Families with children have two children of 7 and 14 years old. The partners of the analysed cohabitants are working FT for the same hourly wage.
Source: EUROMOD's Hypothetical Households Tool (HHoT)

The second approach to explore the effect of policies on work incentives consists of analysing the evolution of the average incentives of groups defined by the aforementioned policy changes (Figure B1). Accordingly, at the extensive margin I define (10) groups for PTR evolutions using (5) groups based on UB type and having or not children (relevant for CBs), and (2) pertinent groups based on FTE earnings deciles (relevant for SC reductions). To define (4) groups for EMTRs I use the same (2) FTE earnings decile groups and (2) groups based on taxable income deciles (relevant for special SCs). From the evolutions of EMTRs I exclude people working full-time or more hours because the design of the SC rebates creates a discontinuity at this point[18]. Results show that PTRs have increased for most groups, while the differences between EMTRs in 2005 and 2014 are negligible[19]. Although the increases in

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[18] Beyond FT (equal to 38 hours), to calculate the reduction (see Appendix A for a description) FTE earnings are computed under the assumption that the person still works 38 hours. If hours worked go beyond FT, the mentioned assumption implies that FTE increase (since one earns more while still working FT). After a plateau-area limit, social contribution rebates are withdrawn when FTE increase. This implies that after 38 hours the slope of the budget constraint decreases, creating a discontinuity. Furthermore, people working FT probably have other type of restrictions to work overtime.

[19] The peak in EMTRs in 2007 was due to a tax credit that included civil servants only in this year and that was strongly targeted at low earnings.

PTRs were important, they were not very different across groups. This might complicate the usage of a model only exploiting this type of variation. I explore this in the next sub-section.

Since there were not many policy changes affecting the intensive margin and elasticities in this margin tend to be small (e.g. Bargain et al., 2014; Collado, 2018), I only estimate models for the extensive margin. I try estimating two models: one exploiting variation in Participation Tax Rates (PTRs) (Kalíšková, 2018), and another exploiting earnings net of PTRs (Bartels & Shupe, 2018; Jäntti et al., 2015).

The first model regresses the probability of being employed on individual PTRs and other control variables. To account for possible omitted variable bias I instrument individual PTRs with a group–level simulated instrumental variable (IV) (Cutler & Gruber, 1996). This is expressed in equations 5 and 6. Being employed and unemployed is defined by being seven or more months in the respective state. Most people in my sub-sample are either employed or unemployed 12 months (more than 90%)[20]. The instrumental variable IV_PTR_{gt} is built in the following way. First I use the groups g defined above based on the main policy changes. Then, using the population of the first year I calculate the average PTR for these groups in each year following the respective year's policies. In this way, those group-averages reflect exclusively mechanical policy changes[21]. PTRs use predicted earnings (more details in Appendix A) for people both in and out of work; therefore, I solely utilise the variation I am interested in. Other controls are included in vector X, and I also include year fixed effects α_t to control for common shocks, and group fixed effects α_g to control for constant group differences. Including year and group fixed effects and the fact that the variation from the IV is at the group level, implies that I exploit only *within-group* variation.

$$\text{1st stage: } PTR_{it} = \beta_{IV} IV_{PTR_{gt}} + \alpha_t + \alpha_g + \beta_{it}' X_{it} + \varepsilon_{it} \tag{5}$$

$$\text{2nd stage: } Pr(E_{it} = 1) = \Lambda(\beta_{PTR} \widehat{PTR_{it}} + \alpha_t + \alpha_g + \beta_{it}' X_{it}) \tag{6}$$

The second model I attempt estimating also regresses the probability of being employed, but this time on Net-of-PTR earnings (NPTRE). The rest of the model is similar to the previous one (see equations 7 and 8) except for the way of dealing with endogeneity. Because I also exploit changes in gross earnings (e_{it}), to deal with endogeneity one cannot use the previous approach of 'freezing' the population and recalculating policies. Instead, I define (16) groups based on (2) education level, (4) birth cohort and (2) gender, and utilise the averages of those groups as IV (Blundell et al., 1998). This and the fact that I include year and group fixed effects imply that the model exploits differential growth between groups. Although this model exploits variation in both gross earnings and PTRs, later for the decomposition analysis I will predict employment probabilities modifying only PTRs.

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[20] The very few people exactly six months in each state are not considered in the regression analysis (but are considered for prediction). Regression results are very similar if I compare only those 12 months in each state, or if I compare those unemployed 12 months to those employed at least one month.

[21] The increase in the earnings limit for partners mentioned in footnote 16 practicaly did not provoke changes in group composition, which could have weakened the instrument. For instance, only 0.4% of the observations in the 2005 sub-sample changes group when applying the policies of 2014.

1st stage: $e_{it}(1 - PTR_{it}) = NPTRE_{it} = \beta_{IV}IV\_NPTRE_{gt} + \alpha_t + \alpha_g + \boldsymbol{\beta}_{it}'\boldsymbol{X}_{it} + \varepsilon_{it}$ (7)

2nd stage: $Pr(E_{it} = 1) = \Lambda(\beta_{NPTRE}\widehat{NPTRE}_{it} + \alpha_t + \alpha_g + \boldsymbol{\beta}_{it}'\boldsymbol{X}_{it})$ (8)

Besides predicting the probability of employment under the policies of a given year, the pseudo-distribution approach of Equation 2 requires the *household* incomes of *individuals* when both employed and unemployed. For the observed status of individuals in employment or unemployment 12 months, I use their observed household incomes. For their unobserved status, I change individuals into this state and use EUROMOD to obtain simulated full-year incomes (for the estimation of earnings for the unemployed see Appendix B). For people observed in both employment and unemployment during the year, I do the same assuming as observed the state that was observed for seven or more months. For people observed exactly six months in each state, I use the observed incomes regardless their status.

4.4   Results

I start this results section by studying the direct (or mechanical) contribution that changes to different (cash) tax-benefit components had on poverty, net revenue and PTRs. I do so before including labour supply reactions because those reactions are caused by the combined effect of tax-benefit changes to different components. To study these mechanical effects, I calculate each indicator using the population of 2005 and applying the policies of the different years involved. For the poverty gap and net revenue, this corresponds to estimating the policy effects of equation 1. For the detailed decomposition, I assign to each tax-benefit component its proportional contribution to the policy effects[22]. Because tax-benefit components can correspond to different employment states, in addition I separate their contributions by the household work intensity declared at the moment of the interview. Results are presented in Figure B2. Looking at the total policy effects and consistent with previous research, we see that policy changes have decreased poverty as well as net revenue (Decoster et al., 2015; Hills et al., 2019; Paulus & Tasseva, 2017). In terms of the specific tax-benefit components, policy effects have mainly been driven by unemployment benefits (UBs). This was also somewhat the case for 'employed' household in the case of net revenue because people in these households might have been receiving UBs in a different moment than the interview. Taxes seem important; however, this is mainly the response to more generous UBs as they are partially taxable. Child benefits (CBs) and Social contribution (SC) reductions somewhat contributed as well to poverty reduction and higher expenditure. For their part, in Figure B3 average PTRs are decomposed in terms of changes

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[22] For the poverty gap, as people might enter and exit poverty due to policy changes, to assign the proportional contribution of a tax-benefit component I consider households that were poor under the policies of both periods.

in the different tax-benefit components. Here we see that UBs also drove most of the changes in average PTRs[23].

Coming to the behavioural responses, for the two types of labour supply models I try to estimate, Table 1 presents the results of statistical tests of the relevance of instrumental variables (IVs) and of the exogeneity of the variables being instrumented. Given that the variation from the IVs is at the group level and that I include year and group fixed effects, the first-stage relevance tests assess whether there are different evolutions across the groups defined in the previous sub-section. As suspected, in the first row we see that the conditional relationship between the simulated group-average PTRs and individual PTRs is very low. Though there were important policy changes affecting PTRs, they tended to be common across groups and therefore the year fixed effects absorb this variation. Fortunately, the relevance of the group-average net of PTR earnings (NPTRE) is higher. The corresponding exogeneity test shows that results are not very different whether I use this instrument or not. Using the instrument would come at the cost of high standard errors because its variation is much lower than that of the instrumented variable. For these reasons, I use this model without the instrument.

Table 1. Relevance and exogeneity test results

Instrumental variable	Relevance		Wald exogeneity test
	Partial R-sq.	F	p-value
Mean group PTR	0.00	21	-
Mean group NPTRE	0.02	559	0.59

Note: To estimate relevance I use the Stata command *ivregress* since the reduced form for the endogenous explanatory variable is linear. To test exogeneity I use the Stata command *ivprobit*. These commands are able to accommodate the survey's sample design except the strata. Results were obtained using all control variables (without interactions). People above and below the first and last percentile of the PTRs distribution are excluded.
Source: EUROMOD

Table 2 shows the results of the selected model. To simulate later more heterogeneous effects, I include in the model interactions that the literature traditionally studies: gender, education, having children and age. Column 1 shows the results in odds. Column 2 shows the Average Marginal Effects (AME) of the variables and of the *difference* between the categories of the interactions. Column 3 shows the *level* of the AME of the categories of the interactions. Results for NPTREs are presented for an increase of €100 per month. All monetary amounts are in monthly euros of 2014. That being said, in column 2 we see that for an increase of €100 in the monthly income difference between working and not working, the probability of being in the labour market increases by 0.6 percentage points. This AME is equivalent to an elasticity of 0.05, with the employment rate in the sub-sample (including the top half of the income distribution for estimation) being 88% and the average NPTRE €784. My elasticity is between the ones that Jäntti et al. (2015) and Bartels and Shupe (2018)

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[23] *Average* PTRs might hide changes for specific groups. I did the same decomposition for the aforementioned groups defined by the policy reforms. This showed a very similar picture, with SC rebates and CBs having slightly stronger but pratically unnoticeable effects for people at the bottom of FTE earnings distribution and with children respectively.

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found, which are 0.01 and 0.08 respectively. Among the interactions, elderly people seem less sensitive to changes in their marginal net earnings, while males and more educated people somewhat more sensitive. The results of this model are the ones I use for the decomposition analysis.

Table 2. Labour supply model results

| VARIABLES	(1) DV: Emp. logit odds	(2) DV: Emp. logit AME	(3) DV: Emp. logit AME (levels)
NPTRE (100 euro 2014)	1.050***	0.006***	
	[1.018,1.082]	[0.005,0.007]	
NPTRE * Male = 0			0.004***
			[0.003,0.006]
NPTRE * Male = 1	1.053***	0.004***	0.008***
	[1.028,1.079]	[0.002,0.005]	[0.006,0.010]
NPTRE * High Edu. = 0			0.006***
			[0.004,0.007]
NPTRE * High Edu. = 1	1.034**	0.002**	0.008***
	[1.008,1.060]	[0.001,0.003]	[0.006,0.009]
Children = 1	1.073	0.023***	
	[0.927,1.242]	[0.014,0.032]	
NPTRE * Children = 0			0.006***
			[0.004,0.007]
NPTRE * Children = 1	1.028**	0.001	0.007***
	[1.010,1.047]	[-0.000,0.003]	[0.006,0.008]
NPTRE * 18-34			0.008***
			[0.006,0.009]
NPTRE * 35-49	1.005	0.000	0.008***
	[0.980,1.030]	[-0.001,0.001]	[0.007,0.009]
NPTRE * 50-64	0.921***	-0.006***	0.002
	[0.896,0.946]	[-0.008,-0.004]	[0.000,0.003]
Group FE	Yes		
Year FE	Yes		
Pseudo-R2	0.0969		
Log likelihood	-5.921e+06		
AIC	1.180e+07		
N	24508	24508	24508

Note: *** p<0.01 ** p<0.05 * p<0.1, 90% confidence intervals in brackets. AME=average marginal effects. In column 3 AMEs refer to the *level* of the effect of increasing the NPTRE in €100 for the categories of the interacted variables. The 16 groups are defined by education (higher education or not), birth cohort (before-the-60s, 60s, 70s and 80-90s) and gender. All groups have at least 133 observations except four groups that have between 16 and 55 observations. People above and below the first and last percentile of the PTR distribution are excluded for estimation but included for prediction. Information criteria are able to accommodate the survey's sample design except the strata.
Source: EUROMOD

With the results of the labour supply model I am able to make the decomposition described in equation 2. Figure 3 presents the results of this decomposition for the policy and the

policy-driven labour supply effects. In relation to the mechanical policy effect, tax-benefit reforms reduced the average poverty gap among the poor and net revenue per person at the bottom half of the income distribution by €31 and €56 per month. This means that the poverty gap decreased in €0.6 for each euro of net revenue decline. This corresponds to the defined Anti-poverty Marginal Benefit of Public Funds (AMBF) without taking into account labour supply reactions.

Figure 3. Decomposition of policy and policy-driven labour supply effects on the poverty gap and net revenue at the bottom half of the income distribution, 2005-2014
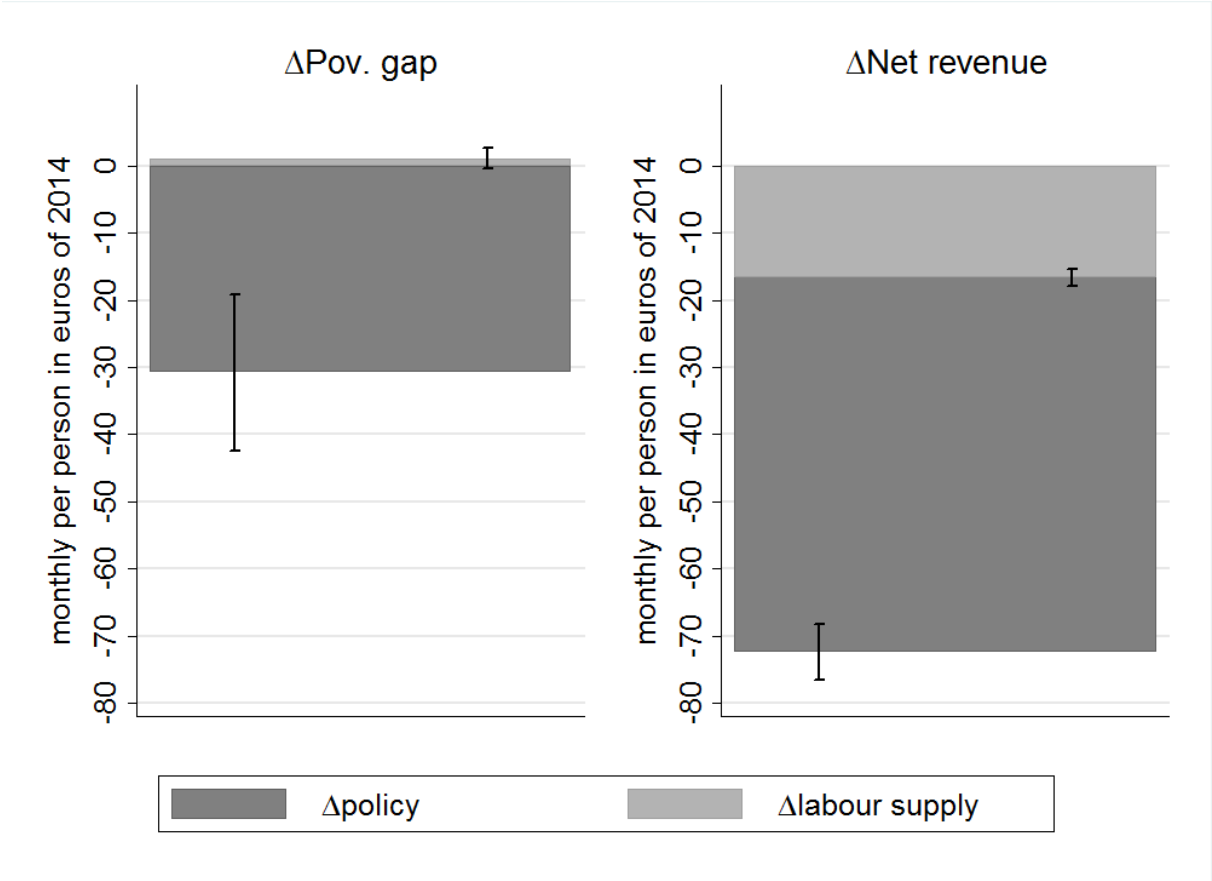
Note: See Figure B2. 90% confidence intervals.
Source: EUROMOD

When looking at the labour supply effect, we see an extra effect on budget deficits. This happened because unemployment benefits grew faster than in-work compensations, which increases PTR and therefore decreases NPTRE. This is translated into a reduction in the probability of being in the labour market according to the labour supply model. Applying the policies of 2014 to the population of 2005 reduces NPTRE by €132 per month on average, which the model's elasticity translates into a 1% decrease in employment. Then the probability of being a taxpayer decreases and of receiving out-of-work benefits increases. Labour supply effects on the poverty gap are much less noticeable than on net revenue. This is because benefits cushion income drops when unemployed. In other words, from the perspective of income maintenance there are not many changes. However, from the perspective of revenue it does make a difference whether people pay taxes or receive

benefits. For this reason, the policy-driven labour supply effect is much more visible on net revenue (-€16.69). Accordingly, when including labour supply reactions the AMBF decreases to €0.4.

Lastly, as a comparison for the AMBF of the reforms between 2005 and 2014, I calculate the AMBF between 2005 and 2009. Up to 2009 most of the large policy changes had already been implemented, except for the additional increase in UBs in 2014. Without considering labour supply reactions, the AMBF up to 2009 was €0.08. This reflects the fact that until that period there was less poverty reduction with respect to the decline in net revenue. When including labour supply reactions, the AMBF up to 2009 decreases to €0.07. This mainly reflects the fact that policy changes until 2009 increased PTRs less than changes up to 2014 (Figure B3).

5   Conclusion

To connect the anti-poverty impact and the efficiency of tax-benefit reforms, in this paper I measured the poverty gap change per unit of net revenue that reforms produce. I refer to this measure as the Anti-Poverty Marginal Benefit of Public Funds (AMBF). Through a microsimulation decomposition framework, I separate the impact of reform from the environment in which they operate. While most previous decompositions considering labour supply reactions have exploited cross-sectional variation in base- or end-period data to, I identify those reactions partly exploiting the same changes that reforms produce. The AMBF can also be seen as a special case of the *Social* Marginal Cost of Public Funds (MCF) (Dahlby, 1998) in which changes in welfare come only from the incomes of people below the poverty threshold. While the recent literature estimating the MCF has used a *small reform* approach combined with tax-benefit microsimulations, my approach is fully based on microsimulations.

In an empirical application to the (cash) tax-benefit reforms implemented in Belgium between 2005 and 2014, I estimated the reduction in the poverty gap per euro of net revenue that these reforms provoked. Without taking into account labour supply reactions, results indicate that reforms reduced the average poverty gap among the poor in €0.6 for each euro of net revenue decline per person at the bottom half of the income distribution. This reduction in poverty with a concomitant increase in budget deficit was mainly due to large increases in unemployment benefits, and secondarily, to augmentations in social contribution reductions and child benefits. The AMBF decreases to €0.4 when including labour supply reactions because policy changes reduced the probability of being in the labour market. This means that the probability of being a taxpayer decreased and of receiving out-of-work benefits increased. The decline in the probability of being in the labour market was because unemployment benefits grew faster than in-work compensations, which weakened participation incentives. As a reference, up to 2009 the 'mechanical' ratio between poverty and revenue changes was €0.08 and it goes down to €0.07 when including labour supply reactions. This meant that reforms in later years were more targeted to the poor and created somewhat more deadweight cost. These results highlight the importance

of looking simultaneously at and balancing potential reforms to in- and out-of-work benefits. At a broader level—and bearing in mind that these outcomes come only from tax-benefit policy—, results show the difficulty of dealing with a social trilemma: reducing poverty while not discouraging work nor running large public deficits.

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Acknowledgment

Appendix

Appendix A

Work incentives

For the formula of PTRs one needs household taxes and benefits (therefore also net incomes) when people are both employed and unemployed. In the non-observed state, household net incomes are simulated changing individual i into this state and running EUROMOD assuming other household members do not change their behaviour. To predict gross wages for unemployed people I utilise a Heckman selection equation, while for hours of work I impute the most likely option (see below). Incomes when employed and unemployed are made comparable by estimating earnings and out-of-work benefits on a full-year basis. In the formula, the difference between household $T(.)$ when in- and out-of-work is expressed in relation to the earnings of individual i. In this way, PTRs take into account household incomes but represent an individual measure. This implies that I calculate them separately for each (available) partner in a couple: one time modifying the earnings of one partner, keeping constant the income sources of the other partner, and then vice versa. With respect to EMTRs, they follow a similar logic measuring the proportion of household earnings taken in tax and withdrawn benefits when a household member increases her hours of work by 5%.

Estimating earnings

- I predict log hourly wages using a Heckman selection model. This model controls for sample selection bias given that those currently in work might have unobserved characteristics different from those currently out of work. I partially follow Bargain et al. (2014) in estimating separate wage equations for men and women containing age and experience (including squared terms), education, number of children and number of children below three years old. The extra exclusion variables in the selection equation are other household incomes and the number of children younger than three years old, between four and six, between seven and 12, and between 13 and 17. To improve my estimations, I do not include in the model people with too high/low hours (below 30 and above 70 for full-timers, and more than 36 for part-timers) and with a second job. In EU-SILC, income and employment information refer to the year before the interview, while weekly hours worked to the year of the interview. For this reason, I also exclude from the model employed people who changed their job or were not in the same full/part-time regime during the whole year. I impute wages for these excluded employed people based on the same model but using an OLS regression. I also bottom code wages using minimum wages from OECD (2014).
- In relation to hours of work, I assign people to their most likely option among the most common options by gender. Thus, I assume that the unemployed (employed)

men censored (excluded) from the previous model work 39 hours. For women, I assign them to either 20, 30 or 38 hours of work according to their highest predicted probability using a multinomial logistic model. This model contains the same variables as the selection equation of the Heckman model.

Simulating out-of-work benefits

Unemployment benefits (UBs) are not ready to be simulated in EUROMOD's baseline. Moreover, even when activated, they are programmed only for people observed in unemployment and not for 'new unemployed'. I implement a few modifications in both cases[24]. To check the eligibility of new unemployed, I extrapolate the observed months in work to the previous two years. For UBs amounts, I utilise observed wages (e.g. to apply the replacement rates) when possible and suitable, and otherwise I predict them (see above). For PTRs, UBs in the out-of-work state are calculated using predicted earnings for both employed and unemployed people. This is consistent with the earnings I utilise in the corresponding in-work state. When calculating work incentives in the intensive margin, the UBs of unemployed partners are estimated using observed wages if these partners happened to be employed at the moment of the interview and therefore declared those wages, and otherwise they are predicted.

By default, social assistance (SA) is simulated for every entitled household assuming a given amount of random non-take up. I do not apply the random non-take up and instead do the following:

- For people observed in unemployment: I only simulate SA if their households are actually taking it up. I make one exception for the counterfactual situation in which an unemployed person works: if when not working she is not receiving SA but UB, when I assume she works I allow her household to take up SA if they are entitled to it.
- For people observed in employment: I only simulate SA in the situation in which they work if their households are actually taking it up. In the counterfactual situation in which these people do not work, I assume their households would take up social assistance if entitled to it.

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[24] The following special UBs are not simulated either and instead regular UB simulations are used. First, if people work involuntarily part-time they could receive an income guarantee connected to their UBs (though instead EUROMOD allows combining earnings and SA). As a reference, the EUROMOD country report shows that part-time employees with income guarantee only amounted to around 6% among the categories that should be simulated. Second, those working part-time voluntarily should receive a 'halved' UB as well. According to my own calculations based on EU-SILC, only around ¼ of people working less than 30 hours did so because they did "not want to work more". Third, benefits for the category "temporary unemployed" are not simulated either. This category is for people still bounded by a contract while work is temporarily suspended (e.g. because of economic circumstances). Replacement rates are slightly higher for this group and do not decrease over time. According to the EUROMOD country report, among the categories that should be simulated, temporary unemployed represented around 20%. Fourth, seniority supplements and UBs after studies are not simulated either.

Social contribution reductions

SCs generally correspond to 13.07% of gross earnings (e.g. if the person of Figure 1 works 38 hours per week earning around €2150 monthly, SC are approximately €280). Reductions to these contributions are calculated based on full-time equivalent (FTE) gross earnings and, given an hourly wage, they are proportional to hours worked up to full-time hours[25]. Below a plateau-area limit, workers are entitled to a full base reduction. The plateau-area limit was around €1500 in 2014, which is quite close to the minimum wage, while the base reduction was around €183. Above the plateau-area limit, FTE earnings are withdrawn from the base reduction at a given rate until a phase-out-area limit. The withdrawal rate was around 20% in 2014 and the phase-out-area limit around €2395 (this implied a SC reduction of around €55 at 38 hours for the person in Figure 1).

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[25] This means that SC reductions do not distort financial incentives to work more hours but do distort incentives to work more than full-time (as the rebate does not increase beyond that) and incentives to increase hourly wages.

Appendix B

Table B1. Main changes in the parameters of policies with respect to average wages, 2005-2014

	2005	2005-2006	2006-2007	2007-2009	2009-2011	2011-2014	2005-2014
				Year-to-year growth			
Wage index	100	3.65	2.47	3.98	4.65	5.91	22.40
			Year-to-year growth in relation to wage index growth				
Unemployment benefits							
Replacement rate single and head months 1 to 6/3	60%	0.00	0.00	0.00	0.00	8.83	10.20
Replacement rate cohabitating months 1 to 6/3	55%	0.00	0.00	9.45	0.00	8.83	22.25
Max UB months 1 to 6	€2,054.56	-1.65	-0.46	20.06	0.70	9.04	33.90
Min UB*	€1,106.70	-1.64	-0.46	4.29	1.43	2.33	6.96
Max UB months 6 to 12	€2,054.56	-1.65	-0.46	11.63	0.69	0.21	12.07
Social insurance contributions							
Base reduction	€125.00	8.35	-0.33	18.39	-4.65	-0.78	24.78
Plateau-area limit of reduction	€1,234.23	-3.65	1.56	4.08	-0.61	-1.87	-0.72
Phase-out area limit of reduction	€1,703.42	15.87	-0.47	2.14	-0.61	-1.86	17.64
Exemption from special contribution	€1,549.34	-3.65	-2.47	-3.98	-4.65	-5.91	-22.40
Social assistance							
Max social assistance*	€817.77	-1.65	0.55	8.63	-0.61	2.34	10.87
Means-test disregard*	€310.00	-3.65	-2.47	-3.98	-4.65	-5.91	-22.40
Child benefits							
Special supplement for single-parents's			€20.00	108.32	-0.62	-1.86	
Back-to-school premium 0-5 years old				€25.50	-0.61	-1.88	
Back-to-school premium 6-11 years old			€51.00	2.13	-0.60	-1.86	
Back-to-school premium 12-17 years old			€71.40	2.14	-0.61	-1.86	
Back-to-school premium 18-24 years old				€50.00	54.53	32.83	
Income limit for single-parents's social supplement	€1,672.38	-1.65	-2.47	16.83	-0.61	1.81	15.70

Note: At the top, the year-to-year wage index growth is displayed. In the rest of the table, the growth of each parameter with respect to the growth of the wage index is displayed (e.g. from 2007 to 2009, the upper limit of UBs grew 20.06 percentage points more than the wage index). For replacement rates I present the difference that changes would have caused to an average wage. In the first column are presented the 2005 parameters (monthly and in prices of that year for monetary values). When policies were implemented in a year after 2005, their initial value is presented in the respective column. In the rows marked with an asterisk, the parameter corresponds to the minimum and maximum across the categories defined by the policies (for other rows involving categories, the growth was the same across them). '6/3' refers to 6 months except in 2014 when it refers to 3. Social supplements are for parents on replacement incomes.
Source: EUROMOD

Figure B1. Mean work incentives by groups defined by main policy changes, 2005-2014
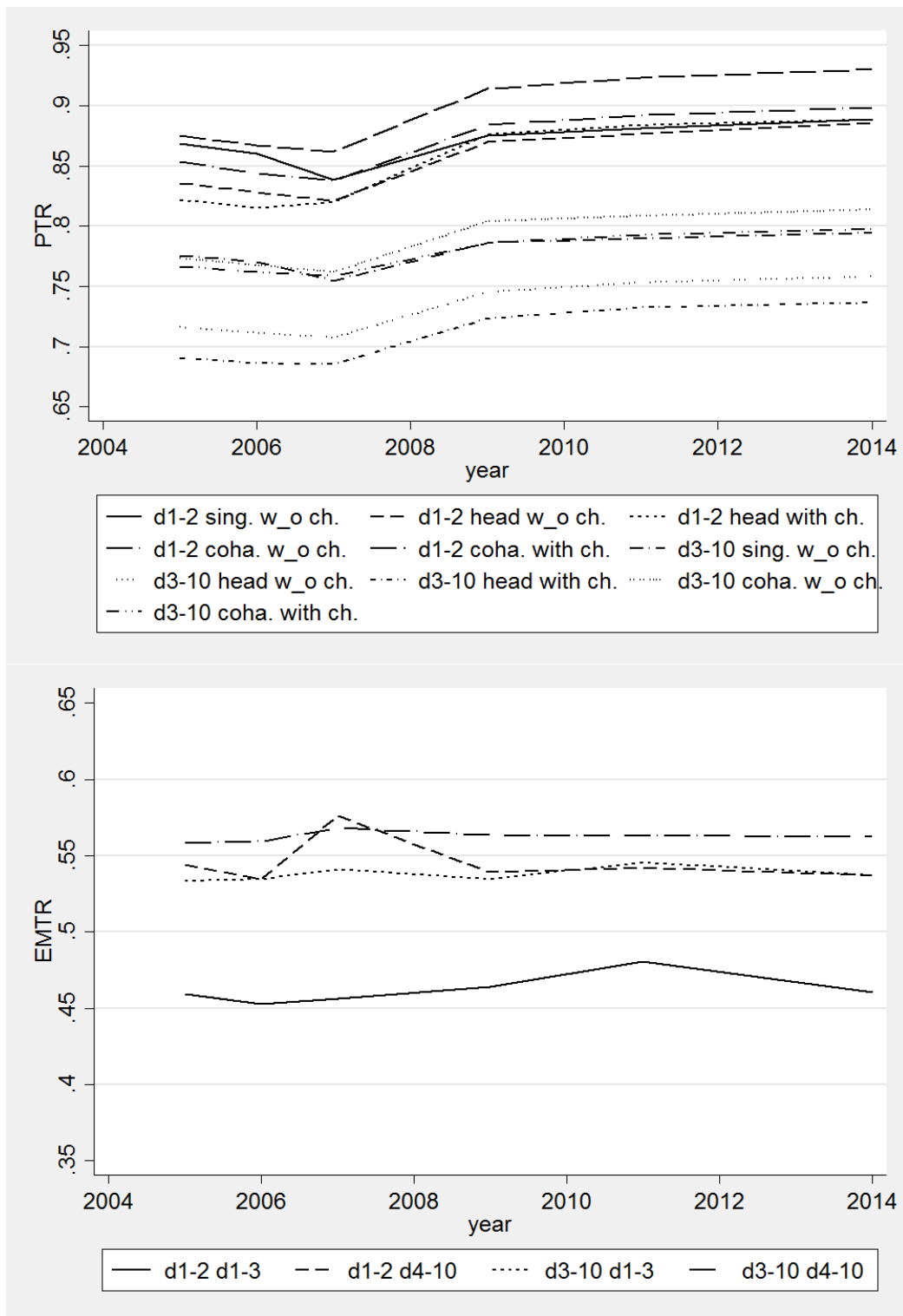
Note: FTE earning deciles are divided from 1 to 2 and 3 to 10, while taxable income deciles from 1 to 3 and 4 to 10. sing=single, coha=cohabitant, head=head of household, w/o=without, ch.=children. For EMTRs, people working full-time or more are excluded because the design of SC rebates creates a discontinuity at this point. Source: EUROMOD

Figure B2. Policy effects on poverty and net revenue at the bottom half of the income distribution by tax-benefit component, 2005-2014
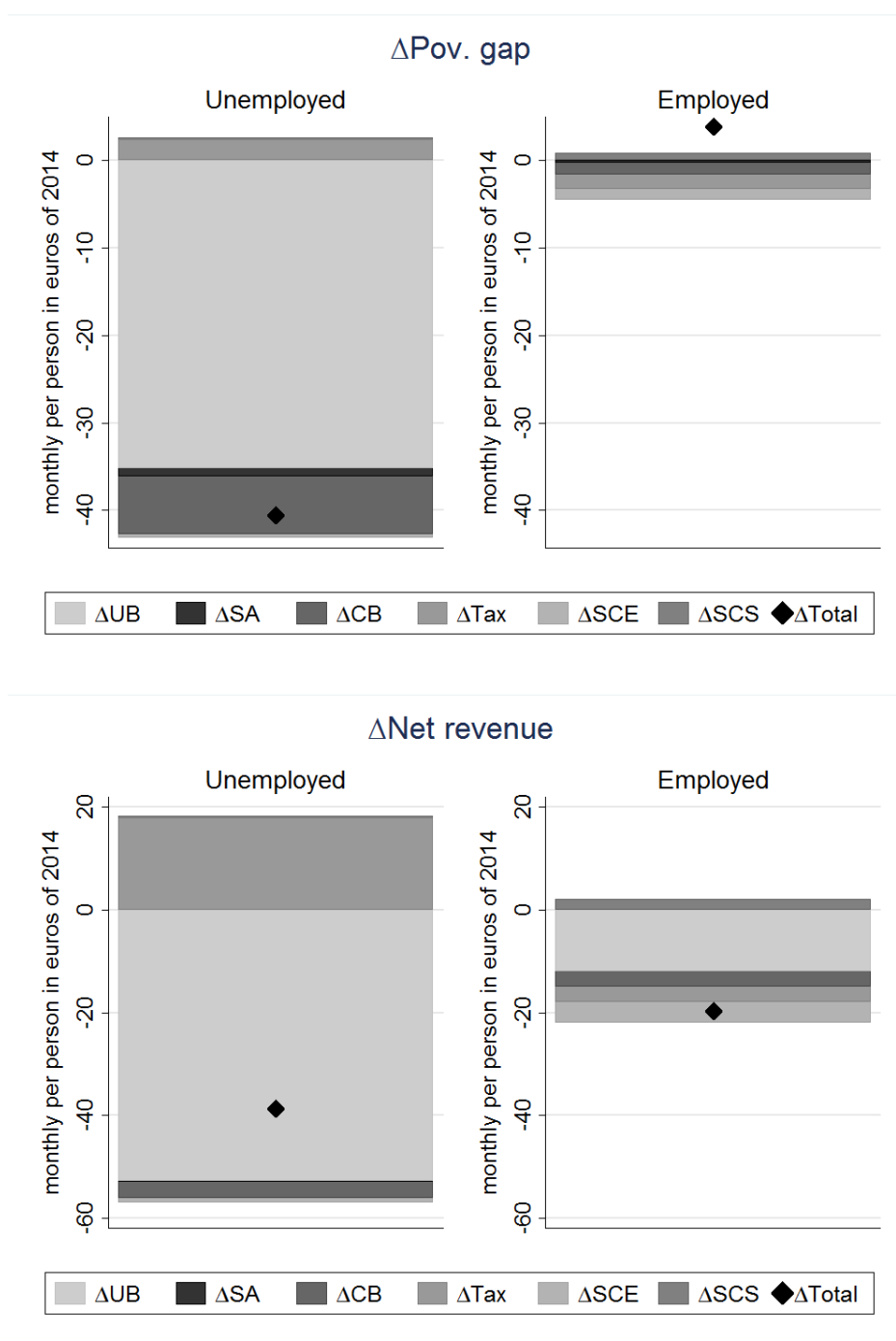
Note: UB=unemployment benefit, SA=social assistance, CB=child benefit, SCE=social contribution employee, SCS=social contribution self-employed. I separate households into the ones with unemployed members and the ones where everybody available for the labour market works. The poverty gap for 'employed' households is positive while the contribution of most components negative because the number of people living in 'employed' poor households decreased slightly more than the sum of the poverty gaps (which slightly increased the average poverty gap). Although I do not focus on the behaviour of self-employed, I do include some of them as partners of employed people. I exclude households with negative equivalised disposable incomes that are below -1.5 times median equivalised disposable income (following Paulus & Tasseva, 2017). Source: EUROMOD

Figure B3. Decomposition of average participation tax rates at the bottom half of the income distribution by tax-benefit component, 2005-2014
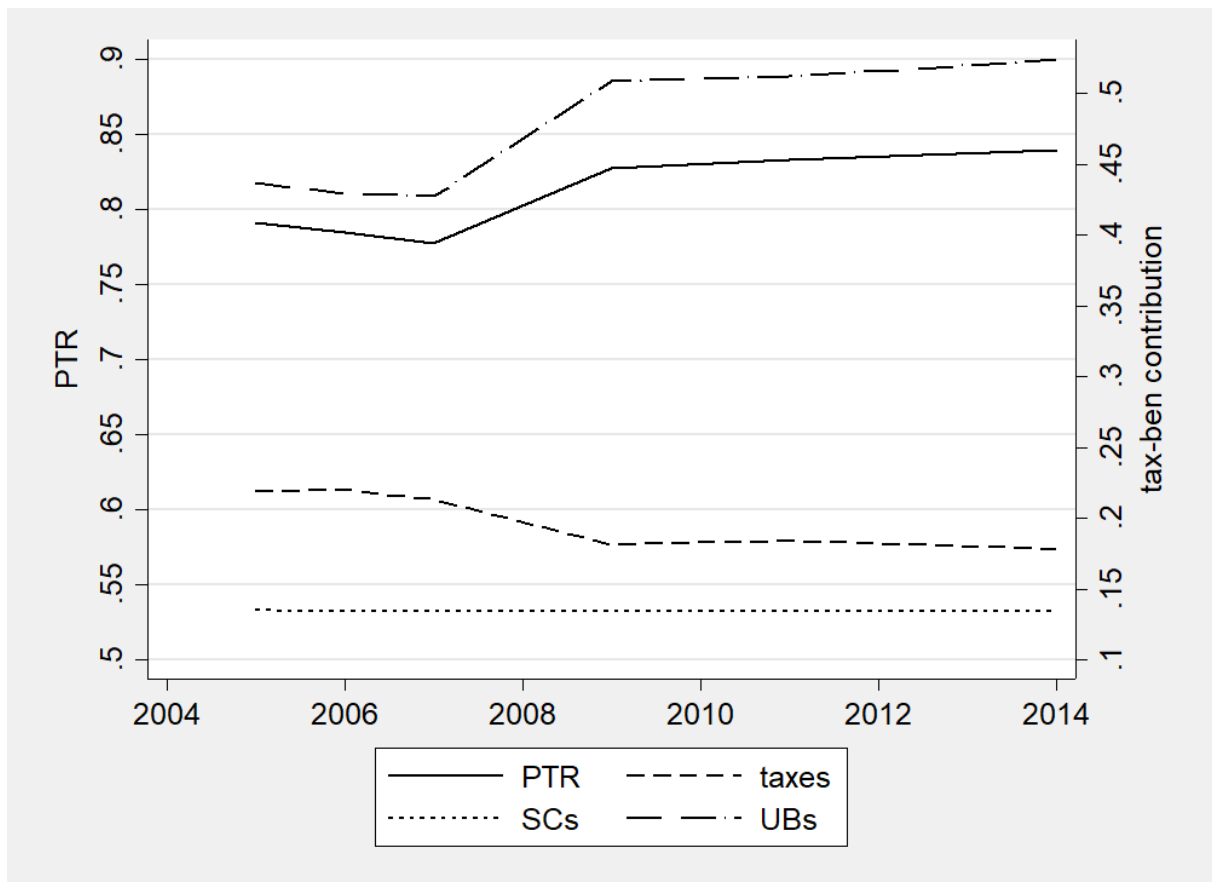
Note: UB=unemployment benefit, SC=social contribution. The sum of the components of the right axis adds up to the average PTR on the left axis. Other tax-benefit components are not displayed because they practically do not contribute to the average PTR.
Source: EUROMOD