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## **Evaluation of the Swedish Earned Income Tax Credit**

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# **An evaluation of the Swedish earned income tax credit \***

by

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## **Abstract**

Over the last twenty years we have seen an increasing use of in-work tax subsidies to encourage labor supply among low-income groups. In Sweden, a non-targeted earned income tax credit was introduced in 2007, and was reinforced in 2008, 2009 and 2010. The stated motive of the reform was to boost employment; in particular to provide incentives for individuals to go from unemployment to, at least, part-time work. In this paper we try to analyze the extensive margin labor supply effects of the Swedish earned income tax credit reform up to 2008. For identification we exploit the fact that the size of the tax credit, as well as the resulting average tax rate, is a function of the municipality of residence and income if working. However, throughout the analysis we find placebo effects that are similar in size to the estimated reform effects. In addition, the results are sensitive with respect to how we define employment, which is especially true when we analyze different subgroups such as men and women, married and singles. Our conclusion is that the identifying variation is too small and potentially endogenous and that it is therefore not possible to use this variation to perform a quasi-experimental evaluation of the Swedish EITC-reform.

Keywords: Labor supply, labor force participation, tax incentives  
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# 1 Introduction

One of the most marked features of contemporary tax policy is the use of in-work tax subsidies to combat ‘poverty traps’ and to encourage labor supply among low-income families with children. Leading examples are the Earned Income Tax Credit (EITC) in the U.S. and the Working Families Tax Credit (WFTC) in the U.K. There is a large supply-side-oriented literature evaluating the effects of these programs. In general, these papers indicate that EITC policies have been successful in enhancing labor supply at the extensive margin.<sup>1</sup> In Sweden, an earned income tax credit was introduced in 2007 and was further expanded in 2008, 2009 and 2010. The announced motive of the reform was to boost employment; in particular to provide incentives for individuals to go from unemployment to, at least, part-time work (Prop. 2006/07:1, p. 136). By lowering marginal tax rates on labor income for low and middle-income earners the reform had large effects on work incentives.

The purpose of this paper is to assess the employment effects up to 2008 of the Swedish EITC reform at the extensive margin in the working age population. To this end we use high-quality individual based register data that cover the entire Swedish population up to the tax year of 2008. While in-work credit policies implemented elsewhere typically have been targeted towards certain demographic groups (e.g. lone mothers), *all* Swedish citizens aged below 65 were exposed to the same tax credit schedule.<sup>2</sup> Therefore, in the Swedish case the empirical researcher lacks possibilities to compare employment growth between treated and non-treated groups in a transparent way.

To identify the effect of the Swedish EITC-reform we instead exploit variation along two dimensions. First, the size of the tax credit is a function of the local tax rate. An individual who lives in a high tax municipality will *ceteris paribus* receive a higher tax credit compared to an individual who resides in a low tax municipality. Second, the absolute size of the tax credit – and even more importantly the tax credit as a share of the annual wage income – is a function of the wage income. A low-income individual

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<sup>1</sup> See Blundell (2006) and Eissa and Hoynes (2005) for overviews.

<sup>2</sup> For people older than 64 a more generous tax credit applied.

will *ceteris paribus* receive a larger reduction in the average tax rate, which is the relevant tax rate when analyzing the participation margin.

As the goal of the paper is to evaluate the effects of the EITC we perform the analysis on a sample of individuals who 2006 resided in a municipality whose local tax rate did not change between 2006 and 2008. Since the ‘treatment’ is a function of municipality of residence, potential income in the state of work and the timing of the reform we can estimate a triple-differences specification. In the main specification where we use 2006 as the pre-reform year and 2008 as the post-reform year and define employment as having a positive amount of earnings, we obtain a semi-elasticity of 0.52, which is statistically significant from 0 at a level of 1 %. This suggests that a one percent increase in the net-of-tax rate (1-average tax rate) leads to a 0.52 percentage point increase in the probability to be employed. Expressed in terms of the number of new jobs, this estimate translates into an employment increase of 3.3 percentage points. Given that the 2006 mean employment rate was around 86 percent in our sample, this treatment effect must be thought of as large.

This baseline result should be interpreted with severe caution though. When we perform a ‘placebo test’, pretending that the EITC reform occurred between 2004 and 2006, we obtain a sizable and highly statistically significant semi-elasticity of 0.24. Since the validity of the triple-differences specification hinges on the assumption that the effect of the ‘placebo treatment’ should be 0, this estimate is worrying. Thus, even though we control in a flexible way for trends in employment growth in the main specification we still believe that there is a substantial upward bias in the obtained coefficient estimate. In addition, both the reform and the placebo estimates are sensitive to how we define employment. In sum, as the reform estimates typically are larger than the placebo estimates it cannot be ruled out that the EITC policy had a positive impact on employment, but the degree of uncertainty is large.

We also show regression results from a subgroup analysis. A general pattern is that the placebo estimates are fairly close to (but typically smaller) than the reform estimates. However, the estimates changes in a non-systematic way when we use different definitions of employment, and we sometimes even find negative semi-elasticities. A major concern is therefore that there are trends present in the data that we fail to control for. To our knowledge, there is no other exogenous variation that can be

used to evaluate the Swedish EITC, and we come to the conclusion that it is not possible to evaluate the Swedish tax credit using quasi-experimental methods.

The disposition of the rest of the rest of the paper is as follows: section 2 describes the Swedish tax system and, in particular, the earned income tax credit; section 3 discusses the previous literature; section 4 presents our empirical specification; section 5 presents the data and the sample used; section 6 contains a descriptive analysis of the data; and section 7 presents the results of the empirical analysis. Finally, section 8 concludes.

## 2 The Swedish Earned Income Tax Credit Reform

### 2.1 The Swedish income tax system

The basic structure of the Swedish statutory income tax system is simple. A proportional local tax rate applies to all earned income and taxable transfers.<sup>3</sup> The individual – and not the household – is the taxpaying unit. The mean local income tax rate in 2006 was 31.60 %, with a minimum rate of 28.89 (Vellinge), and a maximum rate of 34.24 (Dals-Ed).<sup>4</sup> For total labor incomes above a certain threshold, (SEK 317,700 in 2007), the taxpayer also has to pay a central government income tax. The central government income tax schedule consists of two brackets; the marginal tax rates in each bracket are 20 % (for incomes between 317,700 and 503,900 in 2007) and 25 % (for incomes above 503,900 in 2007) respectively. Before computing the individual's tax liability, a *basic deduction* is mechanically made by the tax authorities against the individual's assessed total labor income. The basic deduction is phased in at lower income levels and phased out at higher income levels with consequences for the marginal tax rate facing the individual in these income intervals. In 2006 the basic deduction was phased in between SEK 39,400 and SEK 107,900 and phased out

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<sup>3</sup> Owing to the nature of the Swedish tax system we need to make a distinction between *earned income* and *taxable transfers*. We call the sum of the two *total labor income*. In this paper we let earned income denote wage income and income from active self-employment, while taxable transfers refer to indirect employment-related incomes such as sickness or parental leave benefits, unemployment benefits and pension income.

<sup>4</sup> The local income tax rate is the sum of the income taxes set by the two lower tiers of government; the municipalities and the counties. The local income tax finances the public services provided by the municipalities (childcare, primary and secondary schooling, care of the elderly), and the counties (health care, infrastructure).

between SEK 124,200 and SEK 312,200. In terms of price base amounts the basic deduction was held constant 2006–08.

## 2.2 EITC

The Swedish EITC reform was implemented on January 1<sup>st</sup> 2007 and was reinforced in 2008, 2009 and 2010. The EITC is a non-refundable tax credit, which means that the EITC cannot reduce the individual's tax liability below zero. The Swedish EITC has a very general character: all individuals aged below 65 face the same tax credit formula, regardless of marital status or number of children in the household.<sup>5</sup> In addition, in contrast to EITC policies elsewhere, the taxpayer does not need to apply for the tax credit. The individual's tax liability is automatically reduced with the size of the tax credit.

We will examine employment growth for individuals aged 20–64 between 2006 and 2008. During this time period the EITC was implemented in two steps. Table 1 shows how the tax credits for 2007 and 2008 are calculated. The EITC amount depends on the local (municipality specific) tax rate as well as earned income. As is clear from the formulas in the table, it is not straightforward for the individual taxpayer to calculate the tax credit. Besides the fact that the formulas are rather complex, the calculation is complicated by the fact that the basic deduction, which is a function of both earned income and taxable transfers, is part of the tax credit formula. As a consequence, the income limits in the left columns of Table 1 cannot directly be translated into kink points in terms of earned income, but the kink points, and the tax credit, vary as a function of taxable transfers.

Table 1: Formula for the Swedish EITC in 2007 and 2008

	Earned income ( $E$ )	Tax credit formula
2007	0 – 31,837SEK	$\text{Max}[0, (E - \text{BD}) * t]$
	31,837 – 109,616 SEK	$(0.79 * \text{BA} + 0.2 * (E - 0.79 * \text{BA}) - \text{BD}) * t$
	109,616 – SEK	$(1.176 * \text{BA} - \text{BD}) * t$
2008	0 – 37,310 SEK	$\text{Max}[0, (E - \text{BD}) * t]$
	37,310 – 111,520 SEK	$(0.91 * \text{BA} + 0.2 * (E - 0.91 * \text{BA}) - \text{BD}) * t$
	111,520 – 287,000 SEK	$(1.272 * \text{BA} + 0.033 * (E - 2.72 * \text{BA}) - \text{BD}) * t$
	0 – 37,310 SEK	$\text{Max}[0, (E - \text{BD}) * t]$

**Note:** E = Earned income, BA = Basic amount = 40,300 SEK in 2007 and 41,000 SEK in 2008, BD = basic deduction, t = local income tax rate.

<sup>5</sup> A more generous tax credit scheme applies to those aged above 64.

Figure 1 shows the size of the EITC from 2007 to 2010 as a function of earned income under the assumption that the individual does not receive any social transfers (i.e. the tax credit is a function of earned income only). An essential feature of the Swedish EITC is that there is no phase-out region.<sup>6</sup> The tax credit is phased in up to the point where the individual reaches a level of annual earnings of SEK 323,000 in 2008. This approximately corresponds to an earnings level that applies to a white-collar worker in the private sector with a median wage.<sup>7</sup> The maximum annual tax credit in 2007 was SEK 14,250 in 2008. As can be seen from *Figure 1* the EITC amount increased substantially in 2008–10.

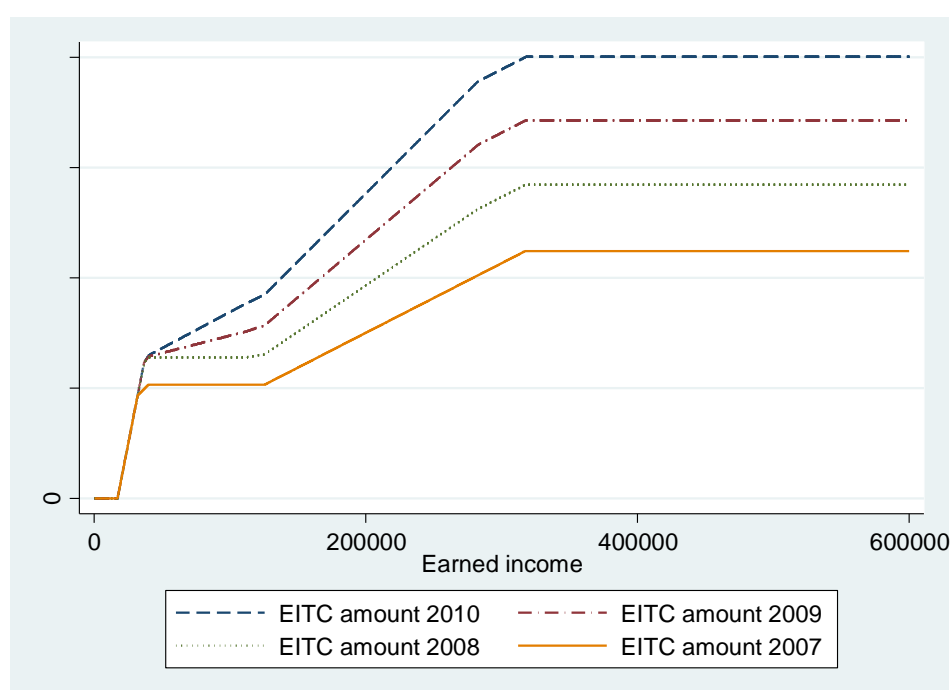


Figure 1 The EITC as a function of earned income 2007–10 in the price level of 2007. Computed for an average local tax rate as of 2007 (31.55 %)

Figure 2 visualizes the marginal and average tax rate as functions of earned income – with and without the 2008 EITC – again under the assumption that the taxpayer does not receive any taxable transfers. It is clear that the EITC policy implies sharp

<sup>6</sup> According to the somewhat dated international overview in Owens (2005) of in-work subsidies in OECD countries in 2001 this feature of the EITC is only shared with the Netherlands.



reductions in the average tax rate at the very bottom of the income distribution. At higher income levels, in particular beyond the point where the EITC is fully phased in, the gap between the average tax rate with and without EITC becomes smaller and smaller – and asymptotically approaches 0. Figure 1 and Figure 2 both illustrate that the EITC has led to fairly large tax decreases, and – in terms of average tax reductions – especially for low-income individuals.

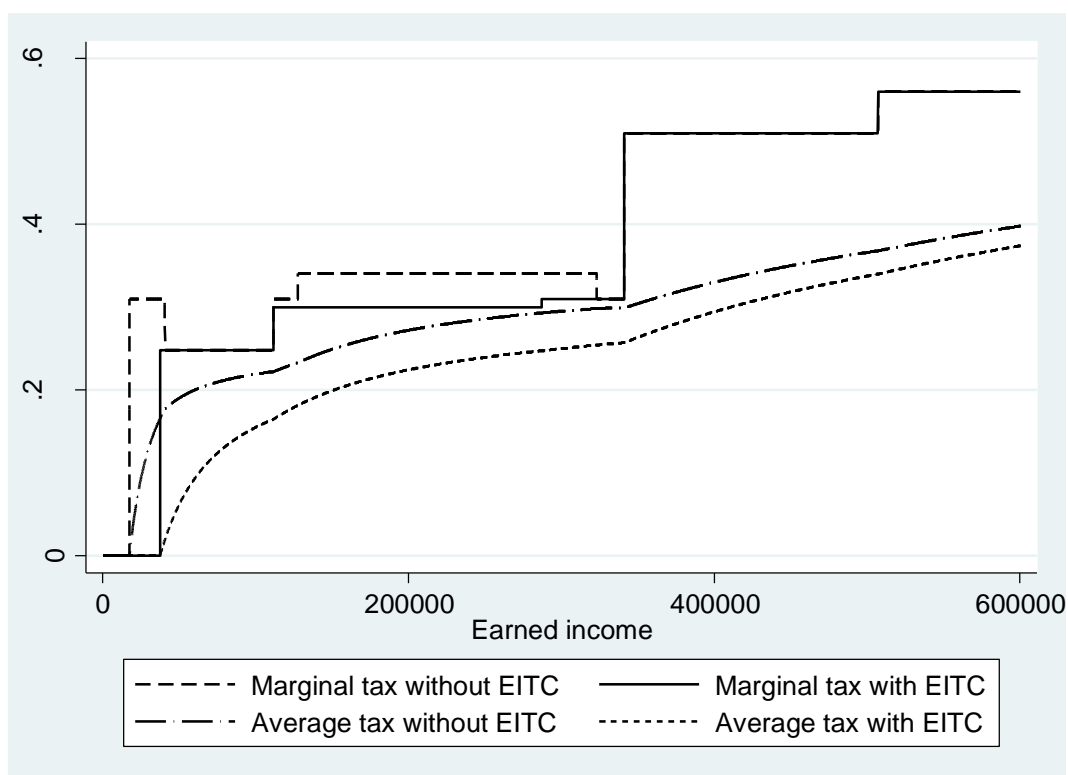


Figure 2 Marginal and average tax rate with and without EITC in 2008.

### 2.3 Announcement of the reform and simultaneous rule changes

The Swedish EITC reform was part of a broader reform package launched by the centre-right wing coalition government that came into power after the parliamentary elections in September 2006. The key components of this broad reform were first presented at a well-attended press conference on August 31, 2005. The details of the 2007 legislative changes were announced as a part of the government budget proposal in October, 2006.

<sup>7</sup> Median monthly wage was 27,100 in 2007 for white-collar workers in the private sector (SCB, strukturlönestatistik).

Recent reforms in the unemployment insurance system and in the sickness insurance system have been surveyed by Forslund (2009). In general, these reforms aimed at making the state of non-work financially more un-attractive. Taken together, these reforms have had a substantial impact on participation tax rates (Pirttilä and Selin, 2011).<sup>8</sup> Another reform that was introduced on January 1, 2007 was an increase in the insurance contribution rates to the unemployment insurance funds. The reform aimed to link the contribution to the unemployment situation, and different sectors were hence hit differently by the reform. To some extent, the reform did offset the increase in disposable income caused by the tax credit for those who were members of the unemployment insurance fund. However, the size of the increase in the unemployment insurance contribution was in most cases modest in size in comparison to the tax credit.<sup>9 10</sup>

### 3 Previous literature

There is an enormous literature that analyzes how labor supply responds to taxation. The literature can be divided into two strands where the first builds on structural estimation techniques within a discrete choice framework. While the main advantage of structural models is that they enable policy reform simulations and provide means to carry out welfare analysis, they typically build on statistical assumptions that can be considered as controversial. On the other hand, quasi-experimental techniques which make up the second strand of the literature do not require as strong assumptions, but cannot be used for ex-ante simulations of planned policy reforms. The common way to go about is to exploit group level variation in eligibility to a new policy and then to use the difference-in-differences (DD) estimator that recovers treatment effects for distinct subpopulations (like lone mothers).

Since the 1990's an extensive literature evaluating EITC policies along the DD-lines has emerged. Around the turn of the millennium, in-work tax credits were in place in a

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<sup>8</sup> The 'participation tax rate' (PTR) is the 'average tax rate' if one also takes the transfer system into account (Brewer et. al. 2010). The PTR is different for each transfer system.

<sup>9</sup> Kjellberg (2011) presents the unemployment insurance contribution rates (A-kasseavgifterna) in 2006 and 2007 for a sample of labor unions (See Table 2A in his paper). The largest increase in the monthly fee was 300 SEK for the Musicians union, which corresponds to 4200 SEK annually, while for most unions, the monthly increase was around 200 SEK. As can be seen in Figure 1, the annual EITC exceeds 5,000 SEK even at low income levels, so the tax reduction resulting from the EITC is higher than the unemployment insurance rate for the vast majority.

<sup>10</sup> To our knowledge, no evaluations of the labor supply effects of these reforms have so far been presented.

number of countries (Owens 2005), including Belgium, Canada, Finland, France, Ireland, Netherlands and New Zealand. However, most of the literature centers on the EITC expansions in the US (e.g. Eissa and Liebman 1996, Eissa and Hoynes 2004 and Meyer and Rosenbaum 2001) and similar policy reforms in the UK (e.g. Blundell et al., 2005; Blundell and Shephard, 2011). A general lesson from this literature is that the labor supply response appears to be concentrated to the extensive margin, whereas the intensive margin tends to be quite unresponsive.<sup>11</sup>

The Swedish EITC-reform has until now only been investigated using structural estimation techniques. With special emphasis on lone mothers, Lennart Flood and co-authors (Flood et al 2004, 2007, Aaberge and Flood 2008) have simulated the labor supply response to reforms in the tax and transfer system based on estimated discrete choice models on Swedish data sources.

With special relevance to our paper, Ericson et al (2009) have made an ambitious *ex ante* simulation of the Swedish EITC reform as of 2007, i.e. the same reform that we evaluate *ex post* in the present paper. The simulation exercise does not only evaluate the change in the number of work hours, but also the effects on social welfare along the lines of Aaberge and Colombino (2010). The data used by Ericson et al is the 2006 wave of LINDA<sup>12</sup>. The estimation model contains both a structural discrete-choice labor supply model and reduced form equations for in- and outflows from disability, unemployment, long term sickness and old age pension. While taking the tax law changes implemented in 2007–09 into account the authors conclude that the reforms *ceteris paribus* should have led to an increase in labor supply – both along the extensive and intensive margins. More specifically, they find that the number of working hours increased by 1.5% and the number of working individuals by 1.1%. They also show that social welfare increased *ex ante* according to four different social welfare criteria owing to the reform.

Sacklén (2009) exploits an econometric model of the same kind. He estimates a labor supply model for different demographic groups based on the Income Distribution

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<sup>11</sup> In an evaluation of a Dutch tax credit reform of 2001 Bosch and van der Klaauw (2009) conclude that the reform led to a positive significant effect on labor force participation. They also found an insignificant negative response on work hours.

Survey of 2004 (HEK). He finds quite low participation elasticities (0.12 for women and 0.08 for men). Still, his simulations suggest that men increased their work hours by 1.9 percent and women by 2.8 percent in response to the reforms occurring between 2007 and 2009. Most of the response is driven by changes along the extensive margin. Also in a discrete choice framework, Andersson and Hammarstedt (2008), has performed an *ex ante* evaluation of the impact of the tax credit on the labor force participation of female immigrants who are single mothers. This exercise is interesting since the pre-reform participation rate was quite low in some of the female immigrant groups. Andersson and Hammarstedt find rather modest effects of the 2007 tax credit on the participation of female immigrants.

While the above mentioned works exclusively focus on the supply side of the economy, a policy oriented literature also discusses effects on wage formation. In an informal policy discussion, Henrekson (2010) notes that minimum wages in Sweden (80 percent of the average wage) exceed those in the U.S. (30 percent of the average wage).<sup>13</sup> Given that a minimum wage policy is binding on a certain segment of the labor market, an outwards shift of the labor supply curve caused by the tax credit will not affect the level of employment – it will only increase involuntary unemployment.<sup>14</sup>

In a search-matching framework Kolm and Tonin (2011) make the theoretical point that an earned income tax credit will reduce equilibrium unemployment, moderate wages, boost participation and search effort. Bennmarker et al (2011) depart from a related theoretical framework and takes the model to the data for the years 2004–09. The idea is to examine the effect of tax progressivity (which has been affected by the introduction of the EITC) and the effect of the net replacement rate in the unemployment insurance. As mentioned above, the latter was reduced at the same time as the introduction of the EITC. The paper by Bennmarker et al also contains an interesting descriptive section on the growth in log wages 2004–09. When partitioning the total population into four wage quartiles it actually appears (from their Figure 1) as

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<sup>12</sup> LINDA is panel of register based data covering a representative sample of around 3 percent of the Swedish population; see Edin and Fredriksson (2000) for a description.

<sup>13</sup> There are no legal minimum wages in Sweden. However, collectively agreed minimum wages play a similar role.

<sup>14</sup> See Lee and Saez (2010) for a recent analysis on the optimality of minimum wage policies in a freely competitive labor market in the presence of non-linear income taxation. An interesting analysis of the incidence effects of the U.S. EITC, which both takes the supply and demand side into account, is provided by Rothstein (2009).

if the percentage wage growth has been rather stable in all quartiles 2004–09, including the years when the EITC has been in place. If the EITC had indeed moderated wages, then we would expect to see a slower growth in wages in the lowest quartile, which is not the case.

## 4 Evaluation strategy

### 4.1 Underlying model framework

The empirical specification of this paper is motivated by a standard theoretical labor supply model, where the individual belonging to a certain skill group chooses whether or not to work in a certain occupation specific to each skill group.<sup>15</sup> Thus, the individual's decision is a binary one. The individual derives utility from disposable income and disutility from work. The individual chooses to work if the utility in the state of work exceeds utility in the state of non-work. As an earned income tax credit increases disposable income in the state of work it will *ceteris paribus* increase the probability to work.<sup>16</sup> In the main analysis, we make the assumption that individuals react in the same way to the same percentage increase in consumption in the state of work. One can then show that, under some further assumptions, the relevant right-hand side regressor is the natural log of the net-of-tax share (1-the average tax rate). In Appendix A we motivate the empirical model.

### 4.2 Identification

The aim of our analysis is to measure how the EITC-reform changed individuals' employment participation decisions. Since the tax credit is general in the sense that everyone is entitled to EITC, we cannot use a simple difference-in-differences approach with a control and a treatment group. Instead we utilize the fact that the EITC-reform affected individuals differently depending on the local (municipal) tax rate and the level of earnings in the state of work. In other words, the reform generates variation in the

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<sup>15</sup> Our model can be thought of as being similar to the extensive margin model in Saez (2002).

<sup>16</sup> A caveat is that cohabitating and married individuals who pool their income can be affected by an increase in their partner's net-of-tax earnings. Such an income effect is likely to go in the opposite direction: it will decrease the probability to work. In the empirical model described below we address this issue by including spousal income in the vector of control variables.

average tax rate  $\tau^A$  that is specific for: (i) an individual's earnings potential ( $y$ ); and (ii) the municipality of residence ( $m$ ). Our identification strategy hence strives to compare the labor supply before and after the introduction of the EITC among individuals with different earnings potential and different municipality of residence.

Let us start by considering the following regression equation for the employment decision:

$$h_{iynt} = \alpha + \beta \log[1 - \tau_{mt}^A(y)] + \chi_t + \delta_i + \varepsilon_{iynt}. \quad (1)$$

In equation (1), the dependent variable ( $h_{iynt}$ ) is a dummy variable that measures employment for individual  $i$ , with earnings potential  $y$ , residing in municipality  $m$ , year  $t = \{2006, 2008\}$ . We take the natural log of  $1 - \tau_{mt}^A(y)$ . Thus, (1) assumes that all individuals respond in the same way to the same percentage change in the net-of-tax share.<sup>17</sup>  $\beta$  can hence be interpreted as the semi-elasticity, which is a measure of how many percentage points employment increases when the net of tax rate changes by one percent. In order to control for different work patterns between different income groups and municipalities, we include an individual fixed effect  $\delta_i$  that absorbs all possible interactions between potential income and municipality of residence ; and we include year fixed effects,  $\chi_t$ , to control for common time trends. (1) defines a linear probability model. An advantage with the linear probability model, as compared to, e.g., the probit model, is that time-invariant individual heterogeneity (individual fixed effects) can be controlled for. As our data is longitudinal,  $\delta_i$  can be removed by taking the first difference (over  $i$ ) of (1).

One complicating issue in the estimation of equation (1) is that an individual's income in the state of work is endogenous to the employment decision. Hence, the key right hand side regressor of equation (1),  $\log[1 - \tau_{mt}^A(y)]$ , would also be endogenous. The standard way to address this issue in labor supply estimation is to impute either

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<sup>17</sup> We have also estimated equations where  $(1 - \tau^A)$  enters linearly. The elasticity results are insensitive to the choice of functional form.

annual or hourly earnings by performing Mincer-type regressions. Recent examples include Eissa and Hoynes (2004), Blau and Kahn (2007) and Gelber and Mitchell (2011). We will follow this literature and impute annual earnings by regressing earnings for all employed individuals on a set of demographic characteristics, defined based on their 2006 values and then compute tax rates.<sup>18</sup> The socio-demographic variables include age, level of education, type of education, country of origin, and when appropriate, spousal income. The Mincer-regressions are run separately for men and women, singles and couples.<sup>19</sup> By using predetermined 2006 values we ensure that the predicted earnings are unaffected by the reform. We denote the vector with demographic variables  $X_i$ . The variables in  $X_i$  are assumed to be time-invariant in the regression model. Predicted potential earnings will then be denoted  $\hat{y}_i(X_i)$ . All demographic variables are defined as dummy variables.

Equation (1) is similar to a difference-in-difference equation, which compares the differential EITC-treatment before and after the introduction of the tax credit, across municipalities and income groups. Under the assumption that there are no income nor municipality specific time trends in employment, equation (1) will give an unbiased estimate of the labor supply response of the tax credit. However,  $\log[1 - \tau_{mt}^A(y)]$  is still endogenous in the presence of municipality specific and/or potential income specific trends in employment that covary with the EITC. In order to control for this possibility we will add time-varying municipality specific effects (captured by the term  $\chi_t * \eta_m$ ) as well as time varying effects for the different regressors  $X$  in the potential earnings-equation (captured by the term  $X_i * \chi_t$ ). This yields the following specification:

$$h_{iyt} = \alpha + \beta \log[1 - \tau_{mt}^A(\hat{y})] + \chi_t + \eta_m * \chi_t + X_i * \chi_t + \delta_i + \varepsilon_{iyt} \quad (2)$$

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<sup>18</sup> As explained below, we will work with two employment concepts. In the Mincer-type regressions we include all individual who are employed according the specific definition in use. Thus, potential income differs somewhat depending on the dependent variable in the main regression.

<sup>19</sup> For individuals who are married and/or have common children we can correctly observe if they are living in the same household. However, individuals who are cohabiting but are not married nor have common children, are observed as single households in the data. This means that the single household indicator will also capture cohabitants that are unmarried and that do not have common children.

Since we are using two years of data in the main regression (2) will be estimated in a first differenced form:

$$h_{iym08} - h_{iym06} = \beta \log \left\{ \frac{1 - \tau_{m08}^A(\hat{y})}{1 - \tau_{m06}^A(\hat{y})} \right\} + \Delta\chi + \eta_m * \Delta\chi + X_i * \Delta\chi + (\varepsilon_{iym08} - \varepsilon_{iym06}) \quad (3)$$

where  $\Delta\chi = \chi_{08} - \chi_{06}$ . Equations (2) – (3) are in the spirit of a triple-difference equations (DDD). The critical assumption for this specification is that there are no unobserved trends in labor supply that are specific for income group *and* municipality.

To some extent, this identifying assumption can be tested by performing placebo-tests on the pre-reform period. We do this by estimating the following regression equation on data from 2004-2006.

$$h_{iym06} - h_{iym04} = \beta_{placebo} \log \left\{ \frac{1 - \tau_{m08}^A(\hat{y})}{1 - \tau_{m06}^A(\hat{y})} \right\} + \beta \log \left\{ \frac{1 - \tau_{m06}^A(\hat{y})}{1 - \tau_{m04}^A(\hat{y})} \right\} + \Delta\chi + \eta_m * \Delta\chi + X_i * \Delta\chi + (\varepsilon_{iym06} - \varepsilon_{iym04}) \quad (4)$$

where  $\Delta\chi = \chi_{06} - \chi_{04}$ . If the identifying assumption is valid we expect  $\hat{\beta}_{placebo}$  to be 0 since the EITC reform did not occur 2004–06. The second term on the right hand side captures the small modifications of the income tax schedule that were made 2004–06.<sup>20</sup> When estimating (4) we impute earnings in the state of work based on the value of the socio-demographic variables as of 2004.

### 4.3 Threats to identification

One may ask whether forward-looking individuals might have changed their behavior in anticipation of the reform, in which case reform effects would show up before the implementation of the reform on January 1, 2007? We find it unlikely that the reform would have had any large such effects. The reason is that while forward-looking individuals might have been aware that a centre-right victory in the elections would lead

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<sup>20</sup> There were changes in the basic allowance and in the general pension contribution rate during the pre-reform period 2004–06.



to the introduction of the EITC, they would still have faced substantial uncertainty regarding the election outcomes, as the margin of victory was small.<sup>21</sup> Hence, it is first starting from the last couple of months of 2006 that we would expect that people started to adapt their labor supply as a response to the reform. If this is the case we will underestimate the effect of the EITC.

Another issue that should be discussed is whether the results of our analysis could capture effects of other simultaneous reforms? We can conclude that there are a few other reforms that may also have affected the labor supply between 2006 and 2008. The crucial thing for our identification strategy, however, is that these reforms did not vary at the level of the municipality. Therefore, at the outset we find it plausible that our empirical strategy, which uses the variation between income groups *and* across municipalities, will be robust to policy changes mainly treated different income groups differently.

## 5 Data and sample selection

The evaluation is based on the IFAU data base that includes individual level data from the national registers held by Statistics Sweden. We have information on various sources of incomes as well as socio-demographic characteristics of individuals and households. The data set contains information on all individuals aged 20–64, and covers the period up to 2008. The data contain an identifier for each individual, which allows us to observe each individual over time. Regional data that is used in the analysis, such as the local income tax, is collected from the Statistics Sweden home page. In the main analysis we only include individuals who in 2006 lived in a municipality which did not change local tax rate between 2006 and 2008. We also exclude the municipalities that were part of Regional Support Area A from the analysis, since most individuals in these municipalities temporarily were eligible to a higher basic deduction in 2006. The reason for these exclusions is that we want all the variation in the change in the log net-of-tax share to be generated by the introduction of the EITC. Our final estimation sample

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<sup>21</sup> The vote share for the centre/right-parties forming government was 48.24 percent (resulting in 178 parliamentary seats), and the vote share for the three left/green opposition parties was 46.08 percent (resulting in 171 seats in parliament). ([www.val.se](http://www.val.se)).

consists of 2,573,000 individuals, or around half of the Swedish population aged 20–64.<sup>22</sup>

In Table 2 we report summary statistics for both the total population of taxpayers and the estimation sample as of 2006. It shows that the mean local tax rates are fairly similar in the total sample and the estimation sample. Notably, the share of people born outside Sweden is smaller in the estimation sample as compared to the total sample.

Table 2 Summary statistics for 2006, all individuals aged 20–64.

	Total population		Estimation sample	
	Mean	St. dev.	Mean	St. dev.
Share male	0.50	0.50	0.50	0.50
Share married	0.56	0.50	0.60	0.49
Age	43.5	12.3	44.2	12.1
Share with kids	0.35	0.48	0.36	0.48
Share born outside Sweden	0.16	0.37	0.12	0.33
Taxable income	215,325	207,120	208,241	176,974
Local tax rate	0.32	0.01	0.32	0.01
No. of observations	5,011,336		2,572,599	

We will use two definitions of employment. According to the first definition, an individual is defined as working if his or her earned income is positive. Wage income and business income form the bulk of earned income, but some smaller components have also been taken into account.<sup>23</sup> Hence, the indicator variable for employment is defined to be 1 if earned income is positive and is 0 otherwise. According to the second definition, the individual is considered as being employed if his or her earnings exceed one income base amount. The income base amount tracks the general income growth and amounted to SEK 44,500 in 2006. Figure 3 compares our two employment measures (in the total sample of taxpayers) with aggregate employment-population ratios in the Labor Force Surveys. The latter employment variable is a yearly average of monthly employment figures based on survey data. Figure 3 reveals that our employment measures track the employment-population ratios reported in the Labor

<sup>22</sup> We reach the same qualitative conclusions if we instead use the full sample.

<sup>23</sup> These are for example the first period of sick pay that is financed by the employer, the Marie Curie scholarships for Ph D students and taxable education grants for Ph D students.

Force Surveys fairly well, even though the line reflecting our first employment-measure (earnings larger than 0) is considerably flatter than the other two. In levels, the Labor Force series lies in between our two measures. There was a reduction in employment up to 2002–04, followed by increases in 2006 and 2007. Despite the financial crisis there was no drop in employment in 2008.<sup>24</sup> The repercussions of the global depression did, however, hit the Swedish labor market in 2009 when employment fell.

While merely visual inspection does not indicate any clear effects of the EITC on the employment level one should keep in mind though that the year-to-year trends in employment are affected by many factors, including macro-economic factors. In order to identify the effects of the EITC separate from such confounding factors, we should instead rely on the results from a regression analysis that will be given in the following section.

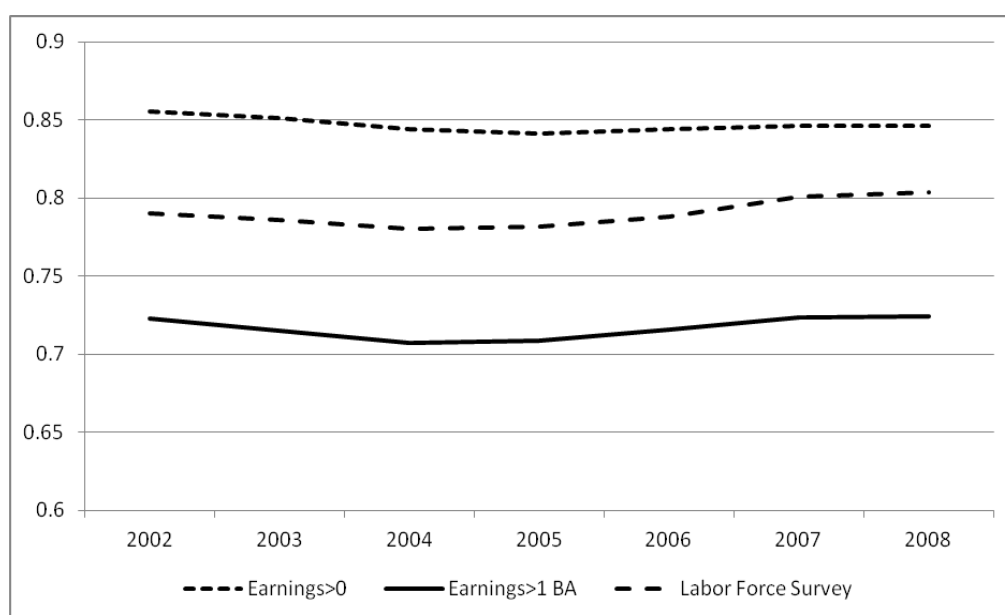


Figure 3 Employment-population ratios 2002–08 for the total Swedish population (not the estimation sample).

<sup>24</sup> Real GDP rose between 2006 and 2007, but did, however, fall between 2007 and 2008. When 2005 is used as a base year real GDP was 104.2 in 2006, 108.3 in 2007 and 107.5 in 2008. (Source: Statistics Sweden)

## 6 Descriptive analysis

Before we move to the estimation results, it is useful to give a description of the tax changes resulting from the introduction of the EITC, and of the employment trends over the period studied.

### 6.1 Tax changes

We have already seen in Figure 2 that the EITC decreased the average income tax significantly, at least in lower income intervals. Figure 4 displays the distribution of the change in the average net-of-tax share for all Swedish tax payers aged 20–64 between 2006 and 2008 using their predicted earnings if working. There is a large mass of taxpayers around a 5 percentage point increase. Since we have restricted the sample to those who 2006 lived in a municipality that did not change local tax rate between 2006 and 2008, all of this change is attributable to the EITC.

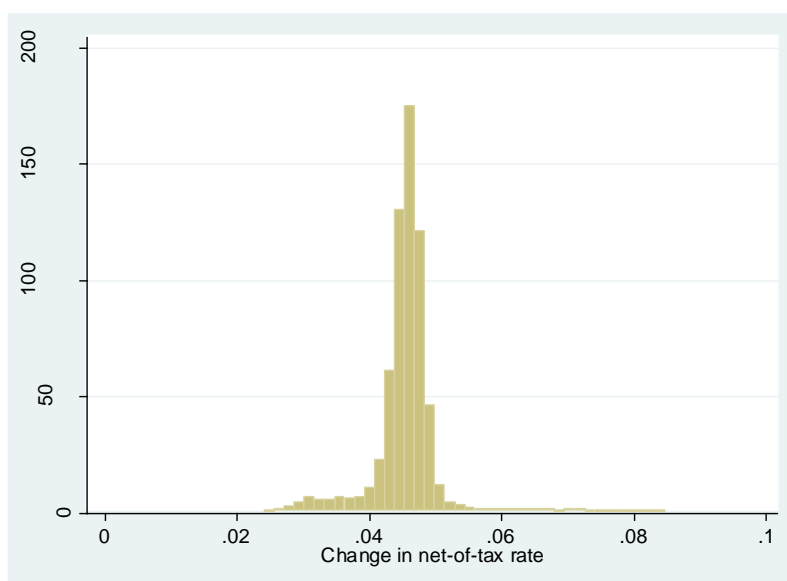


Figure 4 The distribution of the change in  $(1 - \text{average tax rate})$  between 2006 and 2008.

Since the regression specification in equation (3) controls for a number of confounding factors, the identifying variation is not equal to the net-of-tax share changes that are shown in Figure 4. Rather, the identifying variation will stem from the differential response to the EITC-induced net-of-tax share changes, due to the difference in the tax change depending on income class (earnings potential) and municipality of residence. In

other words, while Figure 4 shows that the EITC did give rise to substantial tax reductions, our identification strategy hinges on the assumption that individuals that experienced different tax cuts due to living in different municipalities and having different earnings potential responded differently on the margin. An illustration of this variation is given by Figure 5 and Figure 6. These figures plot the change in the predicted average net of average tax rate versus the local tax rate and predicted income if working respectively. Starting with the variation in net of average tax induced by the fact that individuals living in different municipalities experienced different tax credits we see from Figure 5 that there is a clear positive correlation between the change in net of average tax and municipal tax rate, although the variation along this dimension is small.

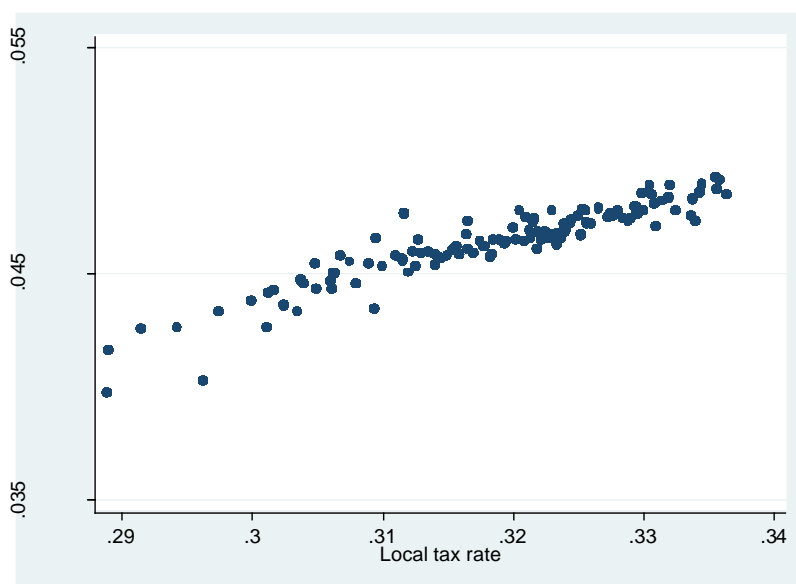


Figure 5 Change in predicted average net of average tax rate 2006–08 versus local tax rate 2006

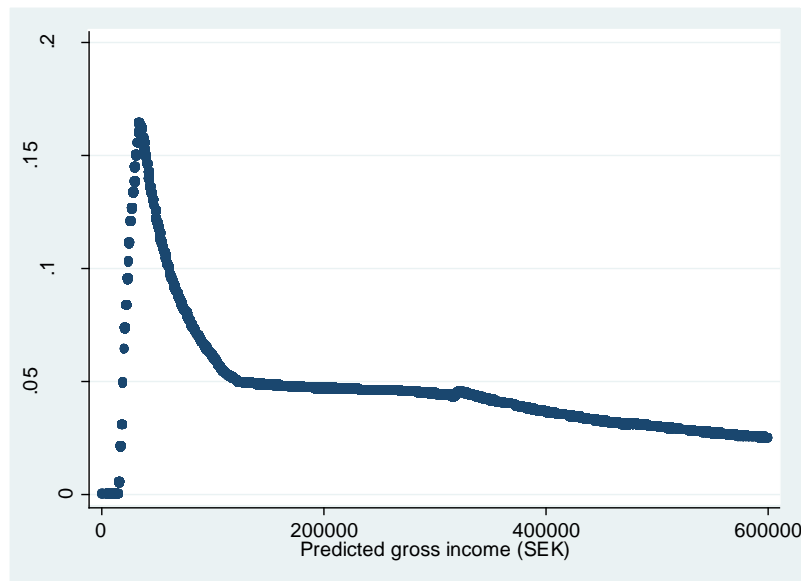


Figure 6 Change in predicted average net of average tax rate 2006–08 versus predicted income if working

If we instead focus on the variation induced by differences in potential income if working, we see from Figure 6 that this variation is considerably larger, especially for low income individuals, and that there is a negative correlation between predicted income and the change in net of average tax for incomes higher than approximately 40,000 SEK.

Hence, our approach relies on using a relatively small source of variation in the data, even though the EITC in itself generated reasonably large tax decreases. In order to get a picture of the nature of the variation that we use to identify the effect of EITC we report results from “first stage regressions” in Table 3 where we analyze the change in the log net-of-tax share (that in the next section will be our key independent variable). More specifically, in column (1) we regress the change in the log net-of-tax share on the local tax rate. In column (2) potential income (expressed in SEK 100,000) is the sole independent variable. Columns (3)–(4) add individual specific control variables (measured in 2006). In column (5), finally, the change in the log net-of-tax share has been regressed on an interaction term between the local tax rate and potential income while controlling both for a full set of municipality dummies (fully collinear with the local tax rate) and the variables used for imputing potential income (fully collinear with imputed income).

Consistent with Figure 5 and Figure 6, columns (1)–(4) reveal that the percentage change in net-of-tax share increases in the local tax rate and decreases with potential income. Given these correlations, we would expect the interaction term to be negative. Column (5) confirms that this is indeed the case. The coefficient, -0.019, should be interpreted as the effect on the change in the log net-of-tax share of increasing the local tax rate by 100 percentage points *and* potential income by SEK 100,000 while holding everything else constant. Thus, this variation must be thought of as very small.

Table 3 "First stage" regression. Dependent variable: the change in the log net-of-tax share 2006–08.

	(1)	(2)	(3)	(4)	(5)
Local tax rate	0.275 (0.001)***		0.271 (0.000)***		
Potential income/100,000		-0.006 (0.000)***		-0.006 (0.000)***	
Local tax rate X Potential income/100,000					-0.019 (0.003)***
Controls	No	No	Yes	No	Yes
Municipality dummies	No	No	No	Yes	Yes

Note: Robust standard errors are in parentheses. \* denotes significance at 1 %, \*\* at 5 % and \*\*\* at 1%. The number of individuals is 2,572,599 and the number of municipalities is 181.

## 6.2 Employment trends

It is a key issue to understand the trends in employment during the period of study in the estimation sample. Table 4 has the same structure as Table 3, but now the change in employment (earnings>0) is the dependent variable. Since the increase in the log net-of-tax share depends positively on the local tax rate, if individuals responded to the tax credit and if confounding factors are absent, we expect a positive relationship between the local tax rate and employment growth 2006–08. However, when the control variables that are used to impute potential income are left out from the regression (column 1) there is a clear negative correlation between the local tax rate and employment growth. Interestingly, when we include the control variables (column 3) the coefficient takes on the expected sign though. The change of sign is probably due to the fact that the demographic structure differs between high tax and low tax municipalities.

Table 4 The change in employment (earnings>0), 2006-08.

	(1)	(2)	(3)	(4)	(5)
Local tax rate	-0.097 (0.018)***		0.044 (0.018)**		
Potential income/100,000		-0.008 (0.000)***		-0.008 (0.000)***	
Local tax rate X potential income/100,000					-0.083 (0.027)***
Controls	No	No	Yes	No	Yes
Municipality dummies	No	No	No	Yes	Yes

Note: Robust standard errors are in parentheses. \* denotes significance at 1 %, \*\* at 5 % and \*\*\* at 1%. The number of individuals is 2,572,599 and the number of municipalities is 181

As the increase in the net-of-tax share depends negatively (for incomes exceeding approximately SEK 40,000) on potential income we expect to see a negative correlation between potential income and employment growth. In fact, this is what we observe from Table 4. A SEK 100,000 increase in potential income implies a 0.8 percentage points decrease in employment. Nor the magnitude or the sign is affected by the inclusion of municipality dummies.

In our most preferred specification, defined by equation (3) in Section 4.2, we will control for all the dummy variables used to impute potential and municipality dummies. What we do not control for is the effect of the interaction between potential income and local tax rate on the employment growth. It is therefore particularly interesting to look at the coefficient for this interaction term, which is reported in column (5). It does actually take on the expected sign, the point estimate, which is significant at a level of 1%, is -0.083. Hence, increasing the local tax rate by 100 percentage points *and* potential income by SEK 100,000 while holding everything else constant gave rise to an 8.3 percentage points decrease in employment 2006–08.

It is also interesting to examine how these trends looked like in the pre-reform period, 2004–06.<sup>25</sup> Importantly, we see from Table 5 that the same qualitative pattern is present also in the placebo period. In particular, the local tax rate has a negative effect when controls are omitted (column 1) and a positive effect when controls are included (column 3). Also, there is a negative relationship between potential income and



employment growth. Finally, the interaction term in column (5) takes on a negative sign, even though it is not statistically distinct from 0.

The message to take away from this descriptive analysis is first of all that the variation that we use to identify the effects is relatively small. Second, already before the reform, there existed some trends in the data and that we must be very cautious in interpreting the results from any estimations unless we can make it credible that we have controlled for these trends.

Table 5 Change in employment (earnings>0), 2004–06

	(1)	(2)	(3)	(4)	(5)
Local tax rate	-0.169 (0.018)***		0.066 (0.018)***		
Potential income/100,000		-0.002 (0.000)***		-0.002 (0.001)**	
Local tax rate x potential income/100,000					-0.023 (0.025)
Controls	No	No	Yes	No	Yes
Municipality dummies	No	No	No	Yes	Yes

Note: Robust standard errors are in parentheses. \* denotes significance at 1 %, \*\* at 5 % and \*\*\* at 1%. The number of individuals is 2,991,863 and the number of municipalities is 147.

In Table 4 and Table 5 we reported trends for the first binary employment variable, which takes the value of 1 if earnings are larger than 0. The same qualitative pattern holds also for our second binary employment variable, which takes the value of 1 if earnings are larger than 1 base amount. We have also examined subgroups with respect to gender and marital status. On the subgroup level there is some heterogeneity.

## 7 Results

In this section we first present some baseline results for the whole estimation sample. Thereafter, we present a subgroup analysis where we divide the sample according to sex and marital status, as well as focusing on groups with a potential weak attachment to the labor market.

<sup>25</sup> There were some minor tax changes during this period, but these did not have the same structure as the tax change caused by the EITC 2006–08. In the placebo sample we include those who 2004 lived in a municipality whose local tax rate did not change between 2004 and 2006.

## 7.1 Baseline results

Let us now turn to the results from the estimation of equation (3). Using a linear probability model we estimate a fixed-effects (first-difference between 2006 and 2008) model for both men and women aged between 20 and 64. As mentioned earlier, we run these regressions with two different binary dependent variables. The first employment dummy takes the value of 1 if earnings are positive and the second employment dummy is 1 if earnings are larger than one income base amount. Table 6 reports these results from four different model specifications. In addition to presenting the parameter estimate which can be interpreted as the semi-elasticity<sup>26</sup>, we also present the average treatment effect (ATE) that shows by how many percentage points employment has changed due to the reform.<sup>27</sup>

In column (i) we show the results from the estimation of equation (1) where we do not include potential income specific trends and the municipality specific trends (i.e. the diff-in-diff-specification). In column (ii) we also allow for a municipality specific time effect, and in column (iii) we instead control for the covariates in the differenced-model, hence allowing for different time effects for the different ages, education etc. Finally, in column (iv) we combine the specifications from columns (ii) and (iii) which corresponds to the difference-in-difference-in-difference specification (DDD) of equation (2) estimated in first-differenced-form of equation (3).

In order to make credible that we do in fact estimate causal effects of the EITC-reform rather than some underlying trends that we are unable to control for we also perform

<sup>26</sup> To obtain a more traditional labor supply elasticity one can divide the semi-elasticity by the mean participation rate in the relevant estimation sample. This elasticity illustrates the percentage change in participation that follows from a one percent increase in the net wage.

<sup>27</sup> We define the average treatment effect as the difference between the actual employment growth 2006-08 and the counterfactual employment growth 2006-2008 assuming that the EITC reform did not occur (i.e. we hold the tax rates fixed between the two years):

$$ATE = \frac{\sum_i \left\{ \Delta \hat{h}_{iy08}(EITC_{08} = 1) - \Delta \hat{h}_{iy08}(EITC_{08} = 0) \right\}}{N} = \overline{\hat{\beta} \log \left\{ \frac{1 - \tau_{08}^A}{1 - \tau_{06}^A} \right\}} - \overline{\hat{\beta} \log \left\{ \frac{1 - \tau_{06}^A}{1 - \tau_{06}^A} \right\}} = \overline{\hat{\beta} \log \left\{ \frac{1 - \tau_{08}^A}{1 - \tau_{06}^A} \right\}}$$

, where  $\overline{\log \left\{ \frac{1 - \tau_{08}^A}{1 - \tau_{06}^A} \right\}}$  is the average change in log net-of-tax share in the sample and  $N$  is the number of individuals in the sample.

some placebo-tests. Doing this, we pretend that the EITC took place between 2004 and 2006, i.e. we estimate equation (4). The estimated placebo ATE is obtained in the same way as in the main regressions, i.e. we multiply the estimated beta-coefficient by the mean change in the log net-of-tax share 2006–08. The placebo sample consists of individuals who 2004 lived in a municipality whose local tax rate did not change between 2004 and 2006.<sup>28</sup>

Focusing first on the case when employment is defined as having positive earnings, we see from the first column in Table 6 that when not controlling for trends we find a statistically significant semi-elasticity of 1.239, which implies an ATE of 7.8 percentage points. Taken literally, this would be an enormous employment effect of the introduction of the tax credit. This estimate increases when we include municipality specific time trends in column 2. However, when we control for trends in the covariates that we use to impute potential income the point estimate of the semi-elasticity drops to 0.476. Still, this highly significant estimate implies a large reform effect, a 3.0 percentage point increase in employment due to the earned income tax credit. In the most preferred specification (column 4), where we control both for potential income specific trends and municipality specific trends, we find an estimate of the semi-elasticity is 0.521 and of the reform effect 3.3 percentage points. All these estimates are significant at a level of 1%.

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<sup>28</sup> We have also run placebo regressions on a sample only including those individuals who 2006 lived in a municipality whose local tax rate did not change between 2006–08 and who did not changed municipality between 2004 and 2006. The elasticity results for earnings>0 were very similar to those presented in Section 7.

Table 6 The effect of log net of average tax on employment: The full sample 2006–08

		(1)	(2)	(3)	(4)
		DD		DDD	
Reform: change in employment between 2006 and 2008					
Earnings>0	Beta	1.239 (0.027)***	1.358 (0.042)***	0.476 (0.037)***	0.521 (0.052)***
	ATE	7.828 (0.171)***	8.842 (0.265)***	3.007 (0.234)***	3.292 (0.329)***
Earnings> 1bb	Beta	1.820 (0.044)***	2.556 (0.069)***	0.246 (0.072)***	0.353 (0.122)***
	ATE	11.253 (0.272)***	15.803 (0.427)***	1.521 (0.445)***	2.183 (0.754)***
Placebo: change in employment between 2004 and 2006					
Earnings>0	Beta	0.79 (0.029)***	0.865 (0.07)***	0.149 (0.031)***	0.244 (0.042)***
	ATE	4.985 (0.183)***	5.632 (0.442)***	0.941 (0.196)***	1.542 (0.265)***
Earnings> 1bb	Beta	0.487 (0.048)***	0.855 (0.139)***	-0.336 (0.056)***	0.281 (0.128)**
	ATE	3.011 (0.297)***	5.286 (0.859)***	-2.077 (0.346)***	1.737 (0.791)**
Reform - Placebo					
Earnings>0	Beta	0.450	0.493	0.327	0.277
	ATE	2.843	3.210	2.066	1.750
Earnings> 1bb	Beta	1.333	1.701	0.582	0.072
	ATE	8.242	10.517	3.598	0.445
Loc gov		No	Yes	No	Yes
Con		No	No	Yes	Yes

Note: Robust standard errors are in parentheses. \* denotes significance at 1 %, \*\* at 5 % and \*\*\* at 1%

How should we understand the rather large estimated semi-elasticities? Remember that when we regressed the employment change on potential income while controlling for municipality dummies (Table 4, column 4) we found that a SEK 100,000 increase in potential income led to a 0.8 percentage point decrease in employment. At first sight, this does not appear to be a large effect. However, the “first stage” regressions reported in Table 3 (column 4) showed that the marginal effect of potential income on the change in the log net-of-tax rate was also small – a SEK 100,000 increase in potential income translated into a 0.06 percentage points decrease in the log net-of-tax change. Since the change in the log net-of-tax share is a non-linear transformation of potential income there is no direct mapping between the linear trends and our final elasticity estimates. However, this illustrates that even a small correlation (spurious or not) between potential income and the employment change may generate high elasticities since the tax rate variation is small.

One way to test whether the reform estimate is in fact an effect of the reform is to conduct a placebo analysis where we pretend that the tax credit was implemented already in 2005. If our identifying assumption of no other trends than those that we control for holds we would expect to find a parameter estimate of zero for this placebo reform. The second panel of Table 6 reports  $\hat{\beta}_{placebo}$  from such an analysis (equation (4)). It turns out that the placebo estimates reported in column (1) and (2) exhibit the same pattern as the main estimates. The only difference is that they are smaller: when the covariates from the Mincer equation are controlled for they are roughly half the size. In the DDD specification (column 4) the semi-elasticity is 0.244, which translates into a ‘reform effect’ of 1.542 percentage points. Hence, we believe that our main estimates of the reform effect are biased upwards. There seems to be some underlying trend in employment that covaries with the triple-interaction term, which we have not been able to take into account.

The bottom part of Table 6 reports the difference between the 2006–08 estimates and the 2004–06 estimates. If one is willing to assume that  $\hat{\beta}_{placebo}$  in equation (4) captures the bias in  $\hat{\beta}$  from equation (3) one can interpret this difference as a reform effect. Conceptually, this would correspond to a DDDD-specification. As can be seen from column (4), it turns out that this difference corresponds to a 1.75 percentage point increase in employment in the most preferred specification. This is still a large reform effect, but it is not so much higher than the employment effects found in earlier micro-simulations of the same reform (Sacklén, 2009, Ericson et al, 2009). However, we do not want to draw too strong conclusions from the difference between  $\hat{\beta}$  and  $\hat{\beta}_{placebo}$ , since it is very unsatisfactory to not understand the nature of the underlying trends..

Table 6 also reports results from estimations where the dependent variable is employment defined as having earnings exceeding one income base amount. In the 2006–08 regressions the semi-elasticity estimates are now higher when the variables used to imputed potential income are not controlled for (column 1–2). However, once controlling for these variables (column 3–4) the estimates are lower. In the most preferred DDD-specification of column 4, the estimated semi-elasticity is 0.353 and highly significant. This estimate implies an estimated treatment effect of 2.2 percentage

points. In the placebo regression the same pattern as for earnings $>0$  prevails. It is noteworthy, however, that the semi-elasticity is actually estimated to be negative when the variables used to impute potential income are controlled for while omitting municipal dummies (column 3). In the placebo DDD-specification, along the lines of equation (4), the semi-elasticity is estimated to be 0.281 and highly significant. If one subtracts the placebo estimate from the reform estimate one obtains a modest semi-elasticity of 0.072 and a treatment effect of 0.445. The latter figure is below the simulated employment effects obtained in the earlier mentioned micro-simulation studies on Swedish data.

Hence, when using a higher cut-off limit for earnings (one income base amount) we obtain a lower aggregate response to the EITC as compared to the case when we only require earnings to be positive. We want to emphasize that these results should be interpreted with severe caution, especially in the light of the subgroup analysis that we will present in the next subsection. Still, there are *a priori* reasons to believe that the elasticity should be lower when using a higher cut-off limit for earnings. In fact, the 2008 design of the Swedish EITC implied that the individual faced a marginal tax rate of zero up to SEK 37,200 (whereas one income base amount was SEK 48,000). Thus, the 2008 system gave strong incentives to earn very low incomes. Part of this response would not be captured by our second employment definition.

To conclude, since the reform estimates are typically larger than the placebo estimates, we cannot rule out that the earned income tax credit did indeed increase employment. However, there is a large degree of uncertainty and it may also be the case that there are underlying trends that we have not managed to control for and that are captured by the estimated coefficients.

## **7.2 Subgroup analysis**

Since it is well known that men and women as well as married couples and singles typically respond differently to changes in tax rates we have also conducted the analysis above separately for four different subgroups: single women, single men, married women, and married men. The results are presented in Table 7 below. We now present results from the most preferred DDD-specification only (corresponding to column 4 in Table 6).

Two general patterns can be discerned. First, with one exception (married males, earnings over one income base amount) the reform estimate is always larger than the placebo estimate. In some cases (married females, earnings over one income base amount, and married males, earnings over zero) the difference is close to zero though. Second, the placebo estimate tends to follow the reform estimate fairly close. When the reform estimate is high (e.g. single men, one income base amount) the placebo estimate also tends to be high. We believe that this reflects underlying trends in the data, trends that render any interpretation of the reform estimates very difficult.

One further issue that makes us reluctant to interpret the difference between the reform estimate and the placebo estimate as an effect is that the results are extremely sensitive to which definition of employment we use. For example, focusing on singles, we find a larger semi-elasticity for men when using the first definition of employment, but a large semi-elasticity for women if we instead use the second definition. Finally, for married men, the difference between the reform- and the placebo-estimates are negative.

Table 7 The effect of net of average tax on employment: Heterogeneous effects with respect to gender and marital status

		Reform	Placebo	Difference
Single women	Beta, >0	0.138 (0.088)	-0.123 (0.08)	0.261
	ATE, >0	0.898	-0.801	1.699
	Beta, >1BA	-0.092 (0.231)	-0.877 (0.226)***	0.785
	ATE, >1BA	-0.583	-5.555	4.972
Single men	Beta, >0	1.09 (0.121)***	0.448 (0.126)***	0.642
	ATE, >0	6.866	2.822	4.044
	Beta, >1BA	3.588 (0.806)***	3.437 (0.592)***	0.151
	ATE, >1BA	22.253	21.317	0.937
Married women	Beta, >0	0.504 (0.069)***	0.315 (0.062)***	0.189
	ATE, >0	3.301	2.063	1.238
	Beta, >1BA	0.053 (0.212)	0.047 (0.244)	0.006
	ATE, >1BA	0.336	0.298	0.038
Married men	Beta, >0	0.959 (0.137)***	0.921 (0.162)***	0.038
	ATE, >0	5.712	5.485	0.226
	Beta, >1BA	0.669 (0.205)***	0.919 (0.231)***	-0.25
	ATE, >1BA	3.944	5.417	-1.474

**Note:** Robust standard errors are in parentheses. \* denotes significance at 1 %, \*\* at 5 % and \*\*\* at 1%. All regressions include the variables used to impute potential and a full set of municipality dummies.

We have also estimated the same models for different subsamples that we consider as being particularly interesting to look at. These are individuals with predicted potential income if working under SEK 100,000, immigrants born outside the Western world, young persons under 26 years of age and individuals with low education. These groups all have a lower employment rate in 2006 than the full sample.



A quick view of the results from the subgroup analysis in Table 8 show that the estimated coefficients are all over the place. In some cases we even find negative semi-elasticities. Overall, the results from the subgroup analysis strengthen our view that we have not been able to control for all underlying trends in employment and that we therefore cannot draw any conclusions about the effects of the earned income tax credit on employment from this type of quasi-experimental set up.

Table 8 The effect of net of average tax on employment: Heterogeneous effects for groups with a potentially weak attachment to the labor market

		Reform	Placebo	Difference
Low income earners	Beta, >0	0.14 (0.086)	-0.04 (0.047)	0.18
	ATE, >0	1.382	-0.395	1.777
	Beta, >1BA	-0.914 (0.493)*	-0.713 (0.418)*	-0.201
	ATE, >1BA	-8.141	-6.351	-1.79
Non-western immigrants	Beta, >0	0.228 (0.089)**	0.104 (0.061)*	0.124
	ATE, >0	1.565	0.714	0.851
	Beta, >1BA	-0.166 (0.152)	-0.381 (0.145)***	0.215
	ATE, >1BA	-1.063	-2.439	1.376
Young people	Beta, >0	0.23 (0.083)***	0.038 (0.048)	0.192
	ATE, >0	1.696	0.28	1.416
	Beta, >1BA	-0.571 (0.21)***	-0.275 (0.211)	-0.296
	ATE, >1BA	-3.76	-1.811	-1.949
Low education	Beta, >0	0.565 (0.096)***	0.179 (0.069)**	0.386
	ATE, >0	3.702	1.173	2.529
	Beta, >1BA	0.011 (0.192)	0.134 (0.162)	-0.123
	ATE, >1BA	0.07	0.853	-0.783

**Note:** Robust standard errors are in parentheses. \* denotes significance at 1 %, \*\* at 5 % and \*\*\* at 10 %. All regressions include the variables used to impute potential and a full set of municipality dummies

## 8 Conclusions

It is a very difficult task to evaluate the Swedish earned income tax credit policy. The reason is that all Swedes face the same tax credit schedule. Hence, the structure of the reform does not allow us to compare treated and non-treated groups in a simple way. To identify the effect of the Swedish EITC-reform we have instead tried to exploit smaller sources of variation along two dimensions. First, the size of the tax credit is a function of the local tax rate. An individual who lives in a high tax municipality will *ceteris paribus* receive a higher tax credit compared to an individual who resides in a low tax municipality. Second, the absolute size of the tax credit – and even more importantly the tax credit as a share of the annual wage income – is a function of the wage income an individual would receive if she would have chosen to work. A low-income individual will *ceteris paribus* receive a larger reduction in the average tax rate, which is the relevant tax rate when analyzing the participation margin.

We have estimated a triple-differences specification where we rely on a small source of identifying variation over municipality and predicted income if working in the net of average tax rate implied by the EITC. Summarizing our results we reach the conclusion that despite estimating a DDD model we do not seem to have succeeded in controlling for all underlying trends 2006–08. The placebo tests clearly indicate that there are trends that covary with the EITC reform. The same message is given by the descriptive analysis that shows similar correlations between employment on the one hand and local tax rates and predicted income on the other hand for the treatment period as for the placebo period. Unfortunately, since the identifying variation is small, minor trends may generate large elasticity estimates.

Our conclusion is hence that it is not possible to evaluate effects on employment of the Swedish earned income tax credit using credible quasi-experimental methods. The reason for this is that there is too little variation in treatment between different individuals and that there are underlying trends in employment that covaries with the tax credit in ways that are very hard to control for.

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## Appendix A

The empirical model chosen in this paper can be motivated in the following way. Let  $U(C, l)$  be the utility of an individual.  $l$  is a binary variable that takes the value of 1 if the individual chooses to work and is 0 otherwise. The individual earns a gross income of  $y$  if he/she chooses to work. Suppose that utility is separable in consumption and leisure, i.e.  $U(C, l) = g(C) - v(l)$ ,  $v(0) = 0$ . Since the aim is not to estimate taste parameters for leisure, the disutility from work can be given a broad interpretation, also including mental and financial fixed costs of working. The disposable income of the individual can be written as  $C = y - T(y) + m$ , where  $y$  denotes earnings in the state of work and  $m$  is non-labor income (e.g. spousal income).

$T(y)$  is the income tax function, representing the taxes paid and government transfers foregone when the individual earns  $y$  kronor. If the individual works,  $T(y)$  is typically positive. If the individual chooses not to work,  $T(0)$  is typically negative, since the individual receives a transfer from the government. Individuals differ with respect to  $v(1)$  in a random way. We refer to the cumulative distribution of  $v(1)$  as  $F(v)$ , The probability that an individual chooses to work can be written

$$\begin{aligned} \Pr(l = 1) &= \Pr\{U(C_1, 1) > U(C_0, 0)\} = \\ &= \Pr\{v(l_1) < g(C_1) - g(C_0)\} = \\ &= F\{g(C_1) - g(C_0)\} \end{aligned} \tag{A.1}$$

Suppose that  $g(C) = \beta \log C$  and that disutility of work is distributed according to a uniform distribution. The individual's utility is increasing in consumption, albeit at a decreasing rate. We then have that

$$\Pr(l = 1) = \beta \log C_1 - \beta \log C_0 = \beta \log \left( \frac{C_1}{C_0} \right) \tag{A.2}$$

Introducing a time index  $t$  and taking the first difference between  $t$  and  $t-k$  gives

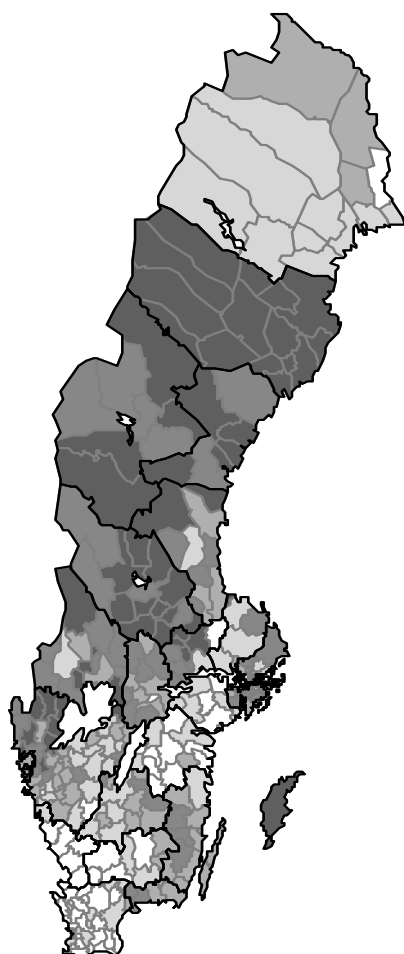
$$\begin{aligned}
& \Pr(l_t = 1) - \Pr(l_{t-k} = 1) \\
&= \beta \log \left( \frac{C_{1,t}}{C_{0,t}} \right) - \beta \log \left( \frac{C_{1,t-k}}{C_{0,t-k}} \right) = \\
&= \beta \log \left( \frac{C_{1,t}}{C_{1,t-k}} \right) - \beta \log \left( \frac{C_{0,t}}{C_{0,t-k}} \right)
\end{aligned}
\tag{A.3}$$

Remember that  $\left( \frac{C_{0,t-k}}{C_{0,t-k}} \right)$  was unaffected by the EITC reform as such since the EITC only increased disposable income in the state of work. Substituting the expression for the budget constraint into (A.3) gives

$$\begin{aligned}
& \Pr(l_t = 1) - \Pr(l_{t-k} = 1) = \\
&= \beta \log \left( \frac{[y_t - T_t(y_t) + m_t](1+g)y_{t-k}}{[y_{t-k} - T_{t-k}(y_{t-k}) + m_{t-k}](1+g)y_{t-k}} \right) - \beta \log \left( \frac{m_t - T_t(0)}{m_{t-k} - T_{t-k}(0)} \right) = \\
&= \beta \log \left( \frac{1 - \tau_t^A + q_t}{1 - \tau_{t-k}^A + q_{t-k}} \right) + \beta \log(1+g) - \beta \log \left( \frac{m_t - T_t(0)}{m_{t-k} - T_{t-k}(0)} \right) = \\
&= \beta \log \left( \frac{1 - \tau_t^A + q_t}{1 - \tau_{t-k}^A + q_{t-k}} \right) + \text{other\_terms}
\end{aligned}
\tag{A.4}$$

, where  $\tau^A = \frac{T(y)}{y}$  and  $q = \frac{m}{y}$ . When  $m$  is 0 we get an estimation equation similar to equation (3) in the paper. In general, non-labor income,  $m$ , is not 0. To account for income effects in the empirical model, we control for spousal income and a number of sociodemographic characteristics that are correlated with non-labor income in a flexible way.

## Appendix B



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