The Anatomy of the Extensive Margin Labor Supply Response*

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Abstract

This paper presents a first systematic analysis of the relationship between the extensive margin labor supply response and the employment level in a quasi-experimental setting. We model the labor force participation margin and estimate participation responses for married women in Sweden using population-wide administrative data, exploiting a reform in the tax/transfer-system for identification. We present compelling graphical evidence on the behavioral response to the reform as well as an estimate of the participation elasticity that is more than twice as large in the lowest-skill sample (with relatively low employment) as compared to the highest-skill sample (with high employment). Our analysis suggests that cross- and within country comparisons of participation elasticities always should be made with reference to the relevant employment level.

Keywords: labor supply; social assistance; housing allowance; in-work tax credits; take up of transfer programs

JEL Classification: H20; J22

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1 Introduction

The primary goal of in-work tax credit programs, such as the Earned Income Tax Credit (EITC) in the United States and the Working Tax Credit (WTC) in the United Kingdom, is to support low income families and encourage labor force participation. The consensus view in the literature is that these policies increased labor supply at the extensive margin for single mothers (Eissa and Liebman 1996, Meyer and Rosenbaum 2001) but at the same time discouraged work for a large number of secondary earners in couples (Eissa and Hoynes 2004, Francesconi et al. 2009). The reason is that the tax credits are phased out as a function of family income rather than individual income. Accordingly, if the primary earner's income is sufficiently large, the family will experience a reduction in benefits if the secondary earner choses to work. This is a central policy issue: Kearney and Turner (2013) documented that under the 2013 U.S. federal tax and transfer system, a family with standard child care costs and a primary earner with an annual income of $25,000, would take home less than 30 percent of the earnings of the secondary earner. Therefore, the participation elasticity of secondary earners is of great policy interest.[1] Nonetheless, there are very few quasi-experimental estimates of this key policy parameter, as evident from the meta-analysis by Chetty (2012).[2]

In this paper we provide new quasi-experimental evidence on how labor force participation reacts when the secondary earners' work incentives change as well as provide a first systematic analysis of how participation elasticities differ across different skill groups with different initial employment levels. The latter is a key contribution of our paper, as the relationship between the labor supply response and the employment level previously only has been highlighted in the structural labor supply literature.[3] Clearly, there is a need to fill this gap in the

[1]The participation elasticity is the percentage change in secondary earners' labor force participation in response to a percentage change in the financial reward of working. This elasticity determines the efficiency gains from reducing participation tax rates applying to secondary earners and is a key concept in the literature on optimal tax- and transfer systems, see, e.g., Immervoll et al. (2011).

[2]The enormous literature on in-work tax credit policies focuses on singles. Eissa and Hoynes 2004, Francesconi et al. (2009),Bosch and van der Klaauw (2012) and Ellwood (2000) are notable exceptions. To our knowledge, the only previous quasi-experimental studies *explicitly* reporting the secondary earner's participation elasticity are Selin (2014) and Kosonen (2014). Related papers using quasi-experimental methods to estimate the effect of childcare prices on female labor supply are Lundin et al. (2008) for Sweden and Havnes and Mogstad (2011) for Norway. None of them found an effect of child-care prices.

[3]When surveying a large number of elasticity estimates from the structural labor supply literature, Bargain and Peichl (2016) noted that married women's elasticites tend to be larger in countries with low female labor force participation. Bargain et al. (2014) find a similar pattern when using a coherent structural estimation approach on

literature, because heterogeneous elasticites is a key concern when, for example, calibrating micro-simulation models using quasi-experimental estimates, see, e.g, Immervoll et al. (2007).

Exploiting high-quality administrative data on the full population of Swedish taxpayers, we make two primary contributions. First, we present an estimate of 0.13 of the average participation elasticity in a population of women where the average labor force participation already is high. Second, exploiting our large sample size, we partition the sample and systematically investigate the participation responses for different subgroups of individuals with different baseline employment rates. We divide the sample into four quartiles based on the wife's skill (predicted income) and, interestingly, find elasticities that are monotonically falling in the skill level of the wife (ranging from 0.24 to 0.09). The results suggest that cross- and within country comparisons of participation elasticities always should be made with reference to the relevant employment level. Our work complements, and is also broadly in line with, earlier structural labor supply studies on Swedish data. Flood et al. (2004), for example, also found fairly low elasticities for Swedish married women.

For identification, we use a reform in the Swedish system for housing allowances for couples with children in 1997. Before 1997 the housing allowance was means-tested based on family income - a family received maximal housing allowance if the joint income of the household did not exceed SEK 117,000 (appr. USD 15,000). After the reform the system was individualized so that the housing allowance was phased out if the individual labor income of either spouse in the household exceeded SEK 58,500. Both before and after 1997 the phase-out rate was 20%. The reform substantially lowered participation tax rates of secondary earners married to low- and middle income spouses, mainly by making not working less attractive.[4] We carefully calculate the participation tax rates, which reflect the financial gain from working, in the treatment- and control groups before and after the reform.

Following earlier work on secondary earners' labor supply on survey data (e.g. Eissa and Hoynes 2004, Francesconi et al. 2009) we compare eligible households (with children) with in-

micro data from 17 E.U. countries and the United States.

[4]From a different angle the same reform has earlier been analyzed by Enström Öst (2012). Using data from the Swedish Social Insurance Agency she compares earnings growth in households with different income compositions in 1996. She estimates significant earnings responses for women. In an experimental study on U.S. data Jacob and Ludwig (2012) estimated a negative effect of housing assistance on labor supply.

eligible households (without children) before and after the 1997 reform, and provide compelling graphical evidence on the reactions to the reform. Since we have access to several pre-reform years of data we can examine the parallel trends assumption. We focus on wives married to husbands with an income below the median and document that female employment increases in households with children relative to households without children in the post-reform period.

The paper is organized as follows. In the next section we describe the 1997 reform in the Swedish housing allowance system. In section 3 we describe our data sources, section 4 develops a model for interpreting the evidence and section 5 presents the empirical strategy. A graphical analysis is provided in section 6, whereas the regression results and implied elasticities are reported in section 7. Finally, section 8 offers concluding remarks.

2 The reform

We begin by describing the reform in 1997 that we exploit to identify extensive margin labor supply responses.

2.1 General description of the transfer program

The housing allowance system can be characterized as an *out-of-work program* as there is no work-requirement for eligibility and the associated transfer is reduced as a function of the income of the members of the household (means-testing). The program is administered by the Social Insurance Agency ("Försäkringskassan") and payments are given on a monthly basis. To receive the transfer (which is a cash transfer), the household has to apply for it by the end of each year. In 1996, 180,000 Swedish couples received housing allowance and the transfer made up an important budget share of many low income households. The particular program that we analyze in this paper applies to low income families with children.[5] We will motivate our choice of control group in section 5.1.

[5]There is also a separate and different housing allowance system applying to young families without children that was not subject to reform and that we do not analyze in this paper.

2.2 Incentive effects

To ease the description of the incentive effects of the housing allowance we introduce some notation. The housing allowance can be written as a function $B(\tilde{z}^p, \tilde{z})$ where $\tilde{z}^p$ and $\tilde{z}$ are, respectively, the two spouses' *qualifying income* or "bidragsgrundande inkomst", which is the income concept used to assess eligibility for welfare programs in Sweden.[6] Without loss of generality we assume $\tilde{z}^p > \tilde{z}$ making one spouse the "primary earner" and the other spouse the "secondary earner". The function B is weakly decreasing in both its arguments which reflects that the housing allowance is a means-tested program. The maximal level of the housing allowance is obtained when neither spouse has any qualifying income and is equal to $B(0, 0)$ which we denote B^{00}. The value of B^{00} depends on a number of non-income characteristics such as the number of children in the household, housing costs and the living space (sq.m.) of the household.[7]

Before the reform in 1997 the transfer was reduced as a function of *the sum* of the two spouses qualifying incomes, i.e. the housing allowance pre-reform could be written $B(\tilde{z}^p, \tilde{z}) = B^{pre}(\tilde{z}^p + \tilde{z})$ and took the following form:

$$B^{pre}(\tilde{z}^p + \tilde{z}) = \begin{cases} B^{00} & \text{if } \tilde{z}^p + \tilde{z} \leq 117,000 \\ \max\left\{B^{00} - h^{pre}(\tilde{z}^p + \tilde{z}), 0\right\} & \text{if } \tilde{z}^p + \tilde{z} > 117,000. \end{cases}$$

where $h^{pre}(x) = 0.2 \times (x - 117,000)$. Thus, a family received the maximum transfer if the joint income of the household did not exceed SEK 117,000 SEK. If the joint income exceeded this exemption level, the transfer was reduced at a phase-out rate of 20 percent. Hence, if say, family income was 118,000 SEK, the transfer was reduced by 200 SEK [$= 0.2\times(118,000-117,000)$].

After the 1997 reform, the system was individualized so that the household received the maximum transfer only if the income of *neither* spouse exceeded SEK 58,500. The phase-out rate was kept at 20 %.[8] Thus the post-1997 housing allowance can be written as $B(\tilde{z}^p, \tilde{z}) =$

[6]Qualifying income does not only include earnings, but also capital income and a fraction of wealth.

[7]In appendix A we describe in more detail how the value of B^{00} is determined.

[8]The reform implied no change to the income thresholds, the level of the housing allowance or the phase-out rates for single parents. Therefore, singles with children could *a priori* be considered to serve as a control group to married with children in the empirical analysis. However, owing to differential employment trends and levels we have not chosen this strategy.

$B^{post}(\tilde{z}^p, \tilde{z})$ defined as:

$$B^{post}(\tilde{z}^p, \tilde{z}) = \begin{cases} B^{00} & \text{if } \tilde{z}^p \leq 58,500 \quad \text{and} \quad \tilde{z} \leq 58,500 \\ \max\left\{B^{00} - h^{post}(\tilde{z}_p), 0\right\} & \text{if } \tilde{z}^p > 58,500 \quad \text{and} \quad \tilde{z} \leq 58,500. \\ \max\left\{B^{00} - h^{post}(\tilde{z}_p) - h^{post}(\tilde{z}), 0\right\} & \text{if } \tilde{z}^p > 58,500 \quad \text{and} \quad \tilde{z} > 58,500. \end{cases}$$

where $h^{post}(x) = 0.2 \times (x - 58,500)$.

How did the 1997 reform affect work incentives? To answer this question we need to make an assumption about how economic decisions within the family are organized. Even though there is individual taxation in Sweden, the transfer system depends on the income of both spouses hence the total tax/transfer relevant for the labor force participation decision of one member of the family depends on the economic decision of his/her spouse. We analyze the incentive changes from the point of view of a sequential model, where the secondary earner decides whether to work or not conditional on the labor supply choice of the primary earner. For the moment we abstract from the take-up issue, and simply assume that the household always takes up the transfer when eligible.

In figure 1 we have illustrated the pre- and post-reform transfers $B^{pre}(\tilde{z}^p + \tilde{z})$ and $B^{post}(\tilde{z}^p, \tilde{z})$ for a family with two children as a function of the secondary earner's income $\tilde{z}$ while fixing $\tilde{z}^p$ to $170,000$ (a typical value of the primary earner's qualifying income in our estimation sample). We assume that if neither spouse would work, the household would be entitled to the maximum level of housing allowance for households with two children, $B^{00} = 38,100$. Given these assumptions, in the pre-reform scenario, the household is eligible for a transfer amounting to $38,100 - 0.2 \times (170,000 - 117,000) = 27,500$ when the secondary earner has zero earnings. According to the pre-reform rules, as soon as the secondary earners supplies any amount of positive earnings, the housing allowance is reduced. More specifically, it is reduced by 0.2 SEK for every SEK of secondary earnings up until the point where the total amount of 27,500 SEK is phased out (which happens at 137,500 SEK) . In the post-reform scenario, on the other hand, the transfer at zero earnings of the secondary earner is significantly smaller: $38,100 - 0.2 \times (170,000 - 58,500) = 15,800$ but the phase-out does not kick in until the secondary earner exceeds the income level of $58,500$. At this point the pre- and post-reform transfers are equal and the functions B^{pre} and B^{post} coincide for secondary earnings exceeding 58,500.

The important lesson from figure 1 is that if the potential earnings of the secondary earner is SEK 58,500 or more, the difference between the household's disposable income in the state of work and non-work, respectively, will entirely be driven by the difference in the transfer in the state of non-work. Since most married women earn annual incomes above SEK 58,500 when working we therefore conclude that the variation used to recover participation elasticities in this paper is a variation in the housing allowance at zero earnings of the secondary earner. In summary, the reform makes not working much less attractive for the secondary earner. Accordingly, even though households may not be perfectly aware of the income splitting rules, one-earner households will certainly recognize that the size of the transfer will be reduced after the reform.

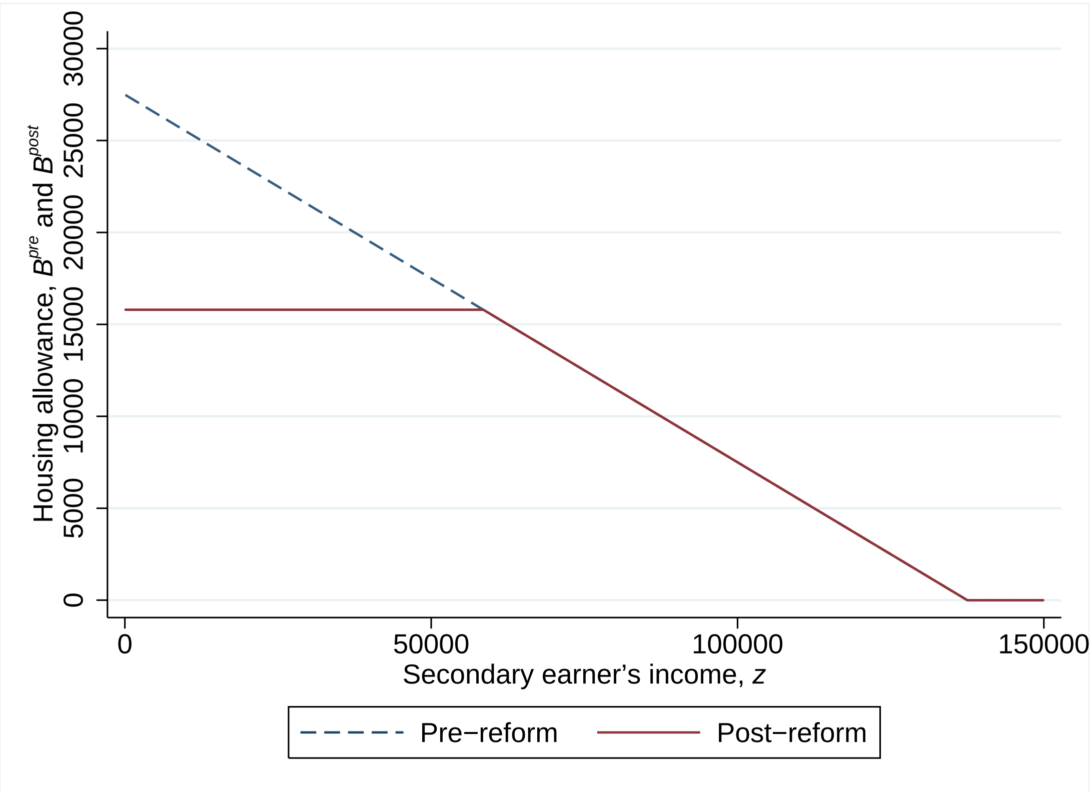

Figure 1: Housing allowance before and after the reform according to the functions $B^{pre}(\tilde{z}^p + \tilde{z})$ and $B^{post}(\tilde{z}^p, \tilde{z})$ as a function of secondary income $\tilde{z}$ for a family with two children. The primary earner's income is fixed at $\tilde{z}^p = 170,000$.

2.3 Time line and anticipation issues

The main objective of the 1997 reform was to cut government expenditures related to the housing allowance program. The size of the program more than doubled between 1990 and 1995 (Boverket 2006). In April 1995, when the annual expenditures were projected to amount to more than SEK 9 billion, the Social Democratic government appointed a government committee (Kommittédirektiv 1995:65). The mandate of the committee was straightforward: The committee was supposed to propose expenditure reductions, e.g. by changing the rules for means-testing. The committee issued their report in December, 1995. The committee's proposal was similar to the reform that was to be implented on January 1, 1997. The Social Democratic government presented a government bill in March 1996 and the bill was passed in parliament on May 8, 1996.[9]

Did households anticipate the 1997 reform? This is a key issue when interpreting the estimated elasticities (Blundell et al. 2011). In principle, well-informed households could have adjusted their behavior already in December 1995 when the committee's report became publicly known.[10]. However, we think that large-scale pre-reform anticipatory responses are unlikely. As far as we can tell, there was no public discussion about the income limits when the committee's report was presented. [11] According to Enström Öst (2012) the Social Insurance Agency ("Försäkringskassan") informed beneficiaries about the reform by sending out letters in June and October 1996. Accordingly, it is likely that the vast majority became aware of the new earnings limits close to the implementation of the reform on January 1, 1997.

[9]The Social Democratic party was in minority in the parliament, but was supported by the Centre (agrarian) party ("Centerpartiet").

[10]As discussed by Blundell et al. it is not *a priori* clear in which direction such anticipatory responses would go. If intertemporal substitution is the dominating mechanism, we would observe people working less in anticipation of the reform. If, on the other hand, labor market frictions is the key mechanism we would expect people to start searching for new jobs already in the pre-reform period.

[11]A search on "bostadsbidrag" in the media archive "Newsline" suggests that the main media focus was on actions against fraud in the system for housing allowances, rather than work incentives when the committee presented their report. The media coverage was larger when the reform was legislated on May 8, 1996, but the focus was not on the earnings limits.

3 Data

3.1 Administrative data

This study primarily exploits large population-wide administrative data sets provided by Statistics Sweden. We have access to all key variables from 1991 and onwards. These include earned income (which we define as the sum of wage income and self-employment income), education level, geographical indicators, the number of children in the household and region of origin. Our graphical analysis of section 6 will cover the years 1991-2010 whereas, as we motivate in section 5.1 below, we focus on the years 1994-2001 in the regression analysis.

Since the variables that we use are collected from administrative registers, the overall quality is very good. A caveat is that the data quality on variables for non-natives might be slightly lower in some cases. In particular, in the 1990's data on education level for many non-natives (who obtained their education degrees from other countries) was missing. We have been able to correct the missing values by using leads of the education variable. The Swedish authorities later on actively sent questionnaires to immigrants where they were asked to report their education level.[12]

In the Swedish register data non-married cohabiting couples without common children are observed as singles in the administrative data. Therefore, even though the housing allowance system applies both to married and cohabiting couples, we limit the sample to formally married couples. We simply do not observe cohabiting couples *without* children.

3.2 Supplementary survey data and micro-simulation model

The housing allowance interacts with other parts of the transfer system, most notably social assistance. Therefore, it is important to take into account the entire tax-and transfer system when constructing households' budget sets. To achieve this, we use the microsimulation model FASIT developed by the Swedish Ministry of Finance and Statistics Sweden.

As FASIT relies on a larger set of variables than is available in our population data, we use as input to FASIT, the smaller supplementary data set HEK ('Hushållens ekonomi') that

[12]Unless the individual died or migrated between year t and year 2000 we use education information as of 2000 when constructing the variable for education level.

is based on both surveys and administrative registers. After having imposed the same sample restrictions on HEK as on the administrative data, the size of the HEK sample varies between 1000 and 2000 observations across years. Since HEK both includes the full set of variables that determine eligibility for the housing allowance program and the size of the beneift actually received (from registers), we also use HEK to compute the take-up of the housing allowance.

3.3 Participation tax rates

Let us now formally define participation tax rates (PTR) and describe in more detail how they are computed. We let $T^{total}(z^p, z)$ refer to all taxes paid and benefits received by a household with primary earnings z^p and earnings of the secondary earner equal to z, assuming the household takes up all transfers.[13] The PTR for the secondary earner is defined in the following way:

$$\tau(z^p, z) = \frac{T^{total}(z^p, z) - T^{total}(z^p, 0)}{z}. \quad (1)$$

This is the key independent variable that appears in our estimation equations (11) and (12) below. Importantly, we compute PTR:s for all households assuming that households eligible for housing allowance and social assistance take up the transfers. As mentioned already, when calculating PTR:s we leverage on the micro-simulation model FASIT and the HEK data set that are tailor-made to measure the impact of taxes and transfers on households' disposable incomes.

The PTR concept implies that the household chooses between two hypothetical disposable incomes; the disposable incomes when the secondary earner is working and non-working, respectively. To be able to estimate the impact of PTR:s on employment we need to compute PTR:s for all individuals, both labor force participants (with positive earnings) and labor force non-participants (with zero earnings) in our population-wide register data. Two issues arise. First, earnings in the state work are observed for those who are working only. Second, some of the variables needed to compute PTR:s (e.g. housing costs and dwelling space) are present in HEK, but not in the population wide data. Hence, we need to impute PTR:s.

We proceed in the following way. We start by calculating the PTR:s for all secondary earners

[13]The function T^{total} corresponds to $T + B$ below in section 4.

with *positive* earnings in the HEK data. This is achieved by computing the disposable income for each household while setting the secondary earner's earnings to zero in the HEK data. We then subtract the household's disposable income at zero earnings from the household's actual disposable income (in the state of work) to obtain the household's financial gain from secondary earner employment. Finally, we divide the financial gain by the secondary earner's earnings to obtain the PTR according to equation (1).[14]

Next, pooling the HEK data for the years 1994-2001, we regress PTR:s on four dummies based on the actual qualifying income of the husband (year-specific quartiles), four dummies based on the number of children in the household and eight year dummies as well as the full set of interactions between the income, children and year dummies. The estimated coefficients from these regressions are then used to impute PTR:s for *all* secondary earners in the *population wide register data*, both participants (with positive earnings) and non-participants (with zero earnings). Since the imputation model is fully interacted, the predictions can be interpreted as group means for women who are working.

While the HEK sample is too small to be used in the labor supply analysis described in section 5, it is still very useful for the purpose of estimating PTR:s. Remember that the households' budget sets are given *deterministically* by the micro-simulation model and the variables in the HEK data. Of course, this does not mean that the sample size of HEK is unimportant, because the precision of the estimated group means become more precise the larger is the number of households represented in the HEK sample.

As already mentioned, the FASIT model is very detailed and should, in principle, be able to account for the entire tax- and transfer system. Since the main purpose of FASIT has been to assess revenue effects of changes in the tax- and transfer system we had to rewrite the code carefully so that it served our purposes. Most importantly, there were no modules computing social assistance benefits for the years 1994-1995. Hence, for these years, we wrote the code ourselves based on national guidelines for social assistance.[15]

[14]We acknowledge that earnings in the state of work may differ for employed and unemployed women, even conditional on observable characteristics, which may induce a selection bias. We have however not been able to find any valid instruments that enable us to use a selection correction term. In this respect, our approach bares some similarities with Gelber and Mitchell (2011) and Meyer and Rosenbaum (2001).

[15]Rules for social assistance differ across municipalities. For some, but not all, years we can compute social assistance both as a function of municipality-specific parameters and national guidelines. For coherency, we have

4 A model to interpret the evidence

4.1 The model

To support the interpretation of our empirical evidence we sketch a simple model that will allow us to (i) clarify conditions under which there is a very simple relationship between elasticities describing the responsiveness to transfers with imperfect take-up and elasticities with respect to changes in taxes (which by assumption have perfect take-up) (see section 4.2 below) and (ii) highlight how estimated participation elasticities depend on the skill-specific employment level (see section 4.3 below).

We consider a model with a discrete set of household types $\mathcal{H}$ indexed by $h \in \mathcal{H}$. There are π_h number of households of each household type. Each household consists of two agents with earnings capacaties z_h^p and z_h, where $z_h^p > z_h$, making one household member the "primary earner" and the other household member the "secondary earner". In a given household type all households are identical with respect to their potential earnings z_h^p and z_h. We focus on the optimal decision-making of the secondary earner from the perspective of the household, treating the primary earner as a passive agent with fixed income z_h^p. Thus, in line with earlier literature (see e.g. Eissa 1995; Eissa and Hoynes 2004) we treat the primary earner as exogenous.[16]

The household decides whether the secondary earner should enter the labor force or not and whether the household should take up the transfer or not. There is no intensive margin hours choice in the theoretical model. As the reform changed marginal work incentives at very low earnings levels of the secondary earner we *a priori* consider the extensive margin to be the important the one.[17] Within a given household type households differ along two dimensions, 'fixed costs of working', q_h, and 'take-up costs', χ_h. Each household i of household type h

chosen to use national guidelines for all years. We have verified that the two methods produce similar results for the years that both methods are available to us.

[16]Several remarks are in order. The model of household behavior is closely related to Immervoll et al. (2011) and their case without income effects on labor supply. In line with these authors, we assume Pareto efficiency and a sharing rule (dictating how resources are divided in the family) that is unaffected by taxes. In contrast to these authors, to simplify the interpretation of our empirical results, we assume the extensive margin of the primary to be inelastic. This does not seem unreasonable ex-ante given the high participation rate of primary earners in Sweden. Moreover, the non-responsiveness of primary earners along the extensive margin is supported by our empirical results in table D in the appendix. The omission of income effects is not without loss of generality, but simplifies the analysis considerable and has become a standard practice in the literature (see Brewer et al. 2010))

[17]We have also conducted a reduced form analysis which strongly points in this direction, see section 7.1 and Table A1.

makes a draw from the joint distribution of q_h and χ_h with the associated bi-variate probability density function $f_h(q_h,\chi_h)$. In the tradition of Cogan (1981) and Hausman (1980) the fixed cost of working, q_h, can be interpreted broadly to accomodate the utility costs (stemming from foregone leisure or the psychological costs associated with leaving a child under the supervision of a non-parent) or monetary costs (such as commuting or child care costs) associated with secondary earner labor market entry. The take-up cost, χ_h, can be interpreted as a cost from gathering information about the transfer program, a time-cost associated with filling out the paperwork, a complexity cost (understanding, and gathering the correct information about how to fill out the paperwork) or simply the social stigma associated with accepting transfers from the government.[18]

The two binary decisions at the household level implies that each household selects between four different states: (i) working without transfers, (ii) working with transfers, (iii) not-working and not taking up transfers, and, finally, (iv) not working and taking up transfers. We denote the decision of the household by $(M, L) \in \{0, 1\} \times \{0, 1\}$ where M is the take-up decision and L is the labor force participation decision of the secondary earner. Let c_{ih} denote household consumption of household i in household type h. The utility function for each household is:

$$u_{ih}(c_{ih}, M_{ih}, L_{ih}) = c_{ih} - q_{ih}L_{ih} - \chi_{ih}M_{ih}, \tag{2}$$

and the budget constraint of the household is given by:

$$c_{ih} \leq z_h^p + z_h L_{ih} - T(z_h^p, z_h L_{ih}) + B(z_h^p, z_h L_{ih})M_{ih} \tag{3}$$

where $T(z_h^p, z_h L_{ih})$ is the total tax liability (possibly negative) and $B(z_h^p, z_h L_{ih})$ is a non-negative transfer received from the government. It is a standard practice in the public finance literature to treat the nonlinear income tax T as representing the complete tax system (including transfers). In this paper we follow this approach with the exception that we leave out the *particular components* of the transfer system that are associated with costly take-up and designate these to

[18]Using a large-scale policy experiment, conducted in collaboration with the Internal Revenue Service (IRS) in the US, Bhargava and Manoli (forthcoming) find that incomplete take-up among low-income earners can at least partially be attributed to lack of program awareness and understanding combined with an aversion to program complexity.

the B-function.

Each household of type h chooses, based on its realized characteristics $(q_{ih}, \chi_{ih}) \in \mathbb{R}^2_+$, one out of the four different alternative states to maximize their utility (2) subject to the budget constraint (3). The mass of individuals choosing each state (M, L) correspond to different regions in the (q, χ)-space. We denote the share of households of household type h in each state with e_h^{ML}, $M = 0, 1; L = 0, 1$. Employment in household h is defined as $e_h = e_h^{11} + e_h^{01}$.

4.2 Participation elasticites with imperfect take-up

We now introduce the following simplified notation based on the T and B functions introduced in the budget constraint (3): $T_h^1 = T_h(z_h^p, z_h)$, $T_h^0 = T(z_h^p, 0)$, $T_h = T_h^1 - T_h^0$ and $B_h^1 = B_h(z_h^p, z_h)$, $B_h^0 = B_h(z_h^p, 0)$. We assume $B_h^0 > B_h^1$ and $T_h^1 > T_h^0$, which is the relevant case that applies when transfers are means-tested and participation taxes are less than 100%. In terms of the variables above, the participation tax introduced in (1) can be decomposed as:

$$\tau_h = \frac{T_h(z_h^p, z_h) - T(z_h^p, 0) + \left[B_h(z_h^p, 0) - B_h(z_h^p, z_h)\right]}{z_h} = \frac{T_h + B_h^0 - B_h^1}{z_h}. \quad (4)$$

This is the relevant participation tax rate for an individual who takes up both the work-related transfer and the non-work transfer and allows us to distinguish, for theoretical purposes, between three possible sources of variation in the incentives to participate in the labor force. These are, (i) a variation in T_h (the difference in taxes between the work and non-work state), (ii) a variation in transfer in the state of non-employment B^0, and, (iii) a variation in the transfer in the state of employment B^1.[19]

We define $\epsilon_h = -\frac{de_h}{dT_h}\frac{z_h - T_h - B_h^0 + B_h^1}{e_h}$ as the *participation elasticity* which yields the percentage increase in employment following a one percent increase in the financial reward from working $z_h - T_h - B_h^0 + B_h^1$ due to a change in T_h. Moreover, we define $\epsilon_h^{B^0} = -\frac{de_h}{dB_h^0}\frac{z_h - T_h - B_h^0 + B_h^1}{e_h}$ and $\epsilon_h^{B^1} = \frac{de_h}{dB_h^1}\frac{z_h - T_h - B_h^0 + B_h^1}{e_h}$ as the *transfer elasticities*, i.e. the elasticities obtained when using variation in the transfer system (which are subject to take-up costs).[20] We can then derive the following

[19]The difference between T^{total} entering equation (1) and T entering (4) is that T excludes those components of the transfer system that are associated with costly take-up which we instead capture with the B-function. In our empirical analysis the variation in T^{total} stems mainly from variation in B^0.

[20]Notice that we have chosen to evaluate all elasticities at the point $z_h - T_h - B_h^0 + B_h^1$ which is the financial

proposition which is very useful:

Proposition 1. *Suppose that at the household-type level, namely, for each $h \in \mathcal{H}$, (i) the random variables q_h and χ_h are independent, and, (ii) q_h is locally uniform on the open interval $(z_h - T_h - B_h^0, z - T_h) \subset \mathbb{R}^+$ and unrestricted elsewhere. Then, letting G_h denote the CDF of χ_h,*

$$\epsilon_h = \frac{\epsilon_h^{B^0}}{G_h(B_h^0)} = -\frac{\epsilon_h^{B^1}}{G_h(B_h^1)},$$

where $G_h(B_h)$ is the take-up rate in household type h when the level of transfers is B_h, or, equivalently, the fraction of type-h workers with take-up costs less than B_h.
Proof *See appendix C.* □

The above proposition specifies sufficient conditions under which reforms in transfers (that are subject to take-up decisions) can readily be used to assess the sensitivity of employment to taxes. The only necessary adjustment in this case is to scale the transfer-elasticities with the inverse of the take-up rate. Notice that the distributional assumptions in Proposition 1 are not very restrictive since they apply at the *household-type* level. Even though we in this paper study an out-of-work program (a variation in B^0), Proposition 1 can also be fruitfully applied when studying in-work tax credits (variations in B^1).

4.3 Heterogeneous responses and aggregate elasticities

It is well-known that the responsiveness along the extensive margin is not captured by a single structural parameter but instead by the number of workers who are, at the margin, indifferent between working and not working. To illustrate this in the simplest possible way, consider our model while assuming *identical* fixed cost distributions for all $h \in \mathcal{H}$, with pdf $f(q)$ and cdf $F(q)$. In this simple example we abstract from the take-up decision. Hence, employment in household type h can be written $e_h = \int_0^{z_h - T_h} f(q)dq = F(z_h - T_h)$. Notice that when the fixed cost functions are identical across h, the employment level will solely depend on disposable income in the state of work, $z_h - T_h$, and employment will be larger in household types with

reward from work for a person who takes up transfers both in the state of work and non-work.

larger potential earnings. We have that $z_h - T_h = F^{-1}(e_h)$ where $F^{-1}(e_h)$ is the generalized inverse distribution function defined as $F^{-1}(e_h) = \inf\{x \in \mathbb{R} \mid F(x) \geq e_h\}$. Moreover,

$$\frac{de_h}{dT_h} = -F'(z_h - T_h) = -F'(F^{-1}(e_h)). \tag{5}$$

This shows that the employment effect depends on the mass (density) of the fixed cost distribution at the *quantile* $F^{-1}(e_h)$. Specifically, $\frac{de_h}{dT_h}$ will depend on e_h, unless F is uniform. A related observation is made by Chetty et al. 2012 who notes that the size of the extensive margin responses depend on the density of the distribution of reservation wages around the economy's equilibrium and that these elasticities vary with the wage rate unless the density of the reservation wage distribution happens to be uniform.[21]

In the empirical analysis we will recover participation elasticities for different subgroups by using variation in the secondary earner's PTR. Recall that the PTR conditional on taking up the transfer is $\tau_h = \frac{T_h + B_h^0 - B_h^1}{z_h}$. As explained in section 2, the variation in τ_h mainly originates from changes in transfers received in the state of non-work, B^0. We now assume that there are Θ subsets of $\mathcal{H}$ and denote each subset by $\mathcal{H}_\theta$. One possibility, that we consider in the empirical analysis below, is to group household types into four groups (quartiles) $\{\mathcal{H}_\theta\}_{\theta=1}^4$ based on the secondary earners' predicted income. The average employment in each set $\mathcal{H}_\theta$ is $\bar{e}_\theta = \sum_{h\in\mathcal{H}_\theta} \frac{\pi_h e_h}{\sum_{h\in\mathcal{H}_\theta}\pi_h}$. Consider now how this quantity responds to a marginal increase in the PTRs $\{\tau_h\}_{h\in\mathcal{H}_\theta}$ induced by marginal increases in B_h^0, $h \in \mathcal{H}_\theta$. The marginal effect on $\bar{e}_\theta$ of such a change can, invoking the assumptions in Proposition 1, be written as:

$$\nabla_{\mathbf{v}}\bar{e}_\theta = -\sum_{h\in\mathcal{H}_\theta} \frac{\pi_h}{\sum_{h\in\mathcal{H}_\theta}\pi_h} \frac{de_h}{dB_h^0} z_h \tag{6}$$

$$= -\sum_{h\in\mathcal{H}_\theta} \frac{\pi_h}{\sum_{h\in\mathcal{H}_\theta}\pi_h} \gamma_h z_h G_h(B_h^0) \tag{7}$$

$$= \beta_\theta, \tag{8}$$

where $\nabla_{\mathbf{v}}\bar{e}_\theta$ is the directional derivative of the average employment in group $\mathcal{H}_\theta$ along the direc-

[21] The model analyzed by Chetty et al. (2012) is isomorphic to ours. The reservation wage corresponds to the fixed-cost threshold for labor force participation that appear in the derivation of proposition 1 in section C. Moreover, in a perfectly competitive labor market equilibrium, there is a one-to-one relationship between the wage rate and the employment level.

tion $\mathbf{v}$ specified by the change in the PTRs $\{\tau_h\}_{h\in\mathcal{H}_\theta}$ (which operate through changes in $\{B_h^0\}_{h\in\mathcal{H}_\theta}$). G_h is the CDF of the take-up cost distribution and γ_h is the density of the fixed cost of work distribution (see appendix C.2 for details). The parameter of interest that we will estimate is β_θ. It is, however, more in line with previous literature to transform marginal effects into elasticities. We define the average participation elasticity in subpopulation $\mathcal{H}_\theta$ as:

$$\bar{\epsilon}_\theta^T = -\sum_{h\in\mathcal{H}_\theta} \frac{\pi_h}{\sum_{h\in\mathcal{H}_\theta}\pi_h} \frac{de_h}{dT_h} \frac{z_h - T_h - B_h^0 + B_h^1}{e_h} = -\sum_{h\in\mathcal{H}_\theta} \frac{\pi_h}{\sum_{h\in\mathcal{H}_\theta}\pi_h} \frac{de_h}{dT_h} z_h \frac{(1-\tau_h)}{e_h}.$$

Using equations (6)-(8), we can approximate the average participation elasticity in subgroup $\mathcal{H}_\theta$ as

$$\bar{\epsilon}_\theta^T \approx \beta_\theta \frac{(1-\bar{\tau}_\theta)}{\bar{e}_\theta \overline{G}_\theta(B^0)}, \tag{9}$$

where for a variable x, $\bar{x}_\theta$ denotes an average over the subset $\mathcal{H}_\theta$. Finally, note that we could use the same reasoning as that behind (9) to aggregate over the entire treated population.

5 Empirical labor supply analysis

5.1 Econometric method

Our aim is to estimate the following relationship on secondary earners in (formally) married couples where both spouses are aged 30-55

$$e_{ihkt} = \alpha + \beta\tau_{ihkt} + \eta_{ihkt} \tag{10}$$

where β can be given the interpretation in equations (6)-(8). The time period of study is 1994 to 2001. The dependent variable e_{ihkt} is a dummy which takes on the value of 1 if individual i with k children in household type h in year t is employed and is zero otherwise. In our baseline specification we define employment as having positive earnings. Moreover, k will be binary in the analysis and equal to 1 if there is at least one child aged below 20 in the household and 0 otherwise. The independent variable τ_{ihkt} is individual i's PTR which is calculated assuming that eligible households take up the housing allowance. Finally, η_{ihkt} is an error term.

We define household types, h, based on the two spouses' age (five groups) and education (four groups). This leaves us with $4^2 \times 5^2 = 400$ household types. In the empirical analysis, the household types primarily function as fully saturated controls for age and education. We will estimate the model on broad aggregates of household types (discussed in section 4.3).

As already described in section 3.3, we estimate τ_{ihkt} on a smaller survey data set that contains all variables necessary to compute the household's taxes and transfers accurately. Let W denote a vector of variables that are contained both in the main (population wide) data set and in the smaller survey data set (W is a subset of the variables needed to compute the PTR). We refer to the coefficient vector in the regression of τ_{ihkt} on W_{it} on the smaller data set as ρ and focus on the following regression model for the population wide data set:

$$e_{ihkt} = \alpha + \beta\hat{\tau}_{ihkt} + \eta_{ihkt}, \tag{11}$$

where $\hat{\tau}_{ihkt} = \hat{\rho}W_{it}$. To account for the fact that $\hat{\rho}$ is estimated with uncertainty we have checked that the standard errors are robust to the corrections suggested by Murphy and Topel (1985), see section 7.2 below.

If we were to estimate (11) in a cross section without any control variables one would fear $\widehat{\beta}$ being biased. The reason is of course that $\widehat{\beta}$ also would capture direct effects of W on e. If, on the other hand, one would include controls for W in a flexible way, identification would be lost. The leading idea of our paper is to exploit the 1997 housing allowance (HA) reform to address the potential endogeneity of $\hat{\tau}_{ihkt}$ in equation (11). The HA reform substantially reduced PTRs for households with children in certain income intervals, but left households without children unaffected. Hence, if there are no direct effects on the outcome variable of the interactions between the children dummy, λ_k, and the time dummies, λ_t, (conditional on λ_k and λ_t) the HA reform can be used as an instrument for τ.

The richness of the data enables us to control for covariates and time trends in a very flexible way. We let λ_{kt} be the vector of excluded instruments. λ_{kt} is the full set of interactions between the child and time dummies. Ultimately, we wish to estimate the equation

$$e_{ihkt} = \alpha + \beta\hat{\tau}_{ihkt} + \lambda_t + \lambda_k + \lambda_h + \lambda_{hk} + \lambda_{ht} + \gamma X_{ihkt} + \eta_{ihkt}, \tag{12}$$

where X_{ihkt} is a rich set of pre-determined control variables not used to construct the household types. In the X vector we include seven dummies for region of origin as it is well-known that foreign-born on average exhibit lower employment rates than natives.[22] In addition, we include 21 dummies for county of residence to account for regional employment differences. Moreover, we interact the dummies for region of origin and the county dummies with the children and the time dummies. Finally, we also include detailed age dummies (one dummy per age), which we interact with the children dummmy. Technically, due to the very large number of dummy variables included, we estimate (12) by the control function method, which under linearity produces identical point estimates as 2SLS. [23]

Notice that, at the *individual* level, the imputed participation tax rate $\hat{\tau}$ in equation (11) will often be measured with error. The reason is that the imputations are made at the group level (see section 3.3). However, since we instrument $\hat{\tau}$ with λ_{tk}, the requirement for consistent estimation of β in equation (12) is that the year-specific group averages are correct.

Why do we compare low income households with and without children? An alternative would be to focus only on households with children and define treatment status according to the income of the husband. That is, wifes with low income husbands would be assigned to the treatment group and wives married to high income husbands would be assigned to the control group. Remember, however, that for the structural interpretation of β to hold we need to impose the assumption that the marginal effect of τ on e is the same in the treatment and control groups.

[22]These regions are (i) Sweden , (ii) Western Europe, North America and Oceania, (iii) Eastern Europe and former Soviet Union , (iv) South America, (v) Sub-Saharan Africa, (vi) Northern Africa and Middle East and (vii) Asia.

[23]We plug in the residuals from the first stage regression into equation (12). We use the Stata areg command while demeaning the data with respect to time-specific household fixed effects. A potential issue is that standard errors will be biased. Fortunately, for specifications with a smaller set of covariates we can compare the standard errors obtained from standard 2SLS regressions with the standard errors obtained from the control function method. We find that the confidence intervals are quite similar. In a specification with time, children and household dummies only, the point estimate for the PTR is -0.102. The 95 percent confidence interval ranges from -0.121 to -0.084 with 2SLS and from -0.125 to -0.079 with the control function method. Hence, we do not believe that a correction substantially would change the interpretation of the results. We have therefore chosen not to make such a correction, which is computational burdensome with a very large number of control variables.

In practice, this means that we will not only have to consider common trends for households with and without children, but we also need to check that the employment *levels* are reasonably similar between the groups. As emphasized in Section 4.3, we expect the employment response to depend on the employment level. It will be apparent from figure 3 below that this is indeed the case for couples with and without children. In contrast, female employment is systematically higher in high income households than in low income households. Therefore, as explained below in Section 5.3 we instead exploit untreated high-income households for making placebo tests. Reduced form results are, however, quite similar if we keep 'low income households with children' as the treatment group, but instead use 'high income households with children' as the control group.

Throughout the results section we will report standard errors that are clustered at the individual level rather than the household type level. The logic is the following. In our analysis we compare labor supply behavior in similar household types with and without children. This is conceptually different from using within-individual variation to identify the response. [24] However, recall that we are using individual level data on the entire population. Hence, over time, individuals will change household type (as they grow older). The reported standard errors are robust to non-independence of the error terms for the same individual.

5.2 Sample restrictions

In line with previous literature (e.g. Eissa and Hoynes 2004) we assume that the wife is the secondary earner and that the husband is the primary earner.[25] We make the following sample restrictions. First, we restrict the sample on that the husband has positive earnings in order to guarantee that the secondary earner's PTR is well-defined.[26] Second, we estimate equation (12) on the subsample of household types substantially affected by the differential drop in PTRs. This is achieved by restricting the sample as a function of the husband's actual qualifying income.[27]

[24]The fundamental problem of exploiting within-individual variation in this context is that aging parents' and aging non-parents' labor supply are likely to evolve differentially also in the absence of a housing allowance reform. When using household types we compare parents of the *same age* both before and after the reform. This approach also circumvents issues related to child births.

[25]In our data, the vast majority of secondary earners are women.

[26]If the husband has zero earnings the wife's PTR will be the PTR of the primary earner.

[27]In the register data, we compute qualifying income based on information on earnings and capital income and imputing financial assets from information on capital income.

More specifically, a household is included in the main estimation sample if the actual qualifying income falls below the median level of qualifying income. The cut-off at the median income was chosen because it corresponds to an income level of around 230,000 SEK in 1996, and households with levels of qualifying income exceeding this threshold were not eligible to any sizable housing allowances prior to the reform.[28] As described below in section (5.3) we will also run placebo regressions on a separate sample of high-income couples, which is identical to the main sample in all other respects. Finally, we drop households where any of the two spouses are aged below 30 or above 55. As described in section 2, households with two spouses aged below 30 were subject to different housing allowance rules both before and after the reform. The upper age limit is imposed as we are interested in the labor supply behavior of prime-aged individuals and not in retirement behavior.

As already mentioned, equations (12) and (13) are estimated on the time period 1994 to 2001 while the graphical analysis of section 6 covers the years 1991-2010. The reason for focusing on the time period 1994-2001 in the regression analysis is that reliable estimates from the micro-simulation FASIT are available from 1994 and onwards. There was also a severe macro-economic crisis in the beginning of the 1990's in Sweden. The reason for not using years after 2001 is that a large childcare fee reform was implemented in 2002 (see Lundin et al. 2008).

5.3 Reduced form and placebo regressions

We also estimate reduced form regressions. To be more specific, we will estimate

$$e_{ihkt} = \mu_{kt} + \mu_t + \mu_k + \mu_h + \mu_{hk} + \mu_{ht} + \delta X_{ihkt} + \upsilon_{ihkt} \tag{13}$$

where μ_{kt} is a shorthand for the interactions between the children dummy and the time dummies.

Since the housing allowance reform occurred in 1997, the estimation sample contains three pre-reform years and five post-reform years. We chose 1996 as the reference year. Due to

[28]The upper limits of qualifying income (i.e. the income level where the entire housing allowance was phased out) differed depending on the number of children below 20 in the household. In 1997, the upper limit was SEK 267,000 for 1 child, SEK 307,500 for 2 children and SEK 351,000 for 3 or more children. Since we pool all households in the main analysis, we cannot use separate income cut-offs.

the length of the estimation sample we are able to account for both pre-reform trends in the estimation as well as estimate how responses evolve across post-reform years. The dynamic dimension is crucial: In the presence of adjustment costs we expect the long-run response to be larger than the short-run response.

The identifying assumption in the difference-in-difference specification is that labor supply behavior of secondary earners with and without children would have evolved similarly in the absence of the reform. The fact that we have access to several years of pre-reform data allows us to test this ’parallel-trends’ assumption for the years before the reform. For obvious reasons, we cannot verify if this assumption holds in our low income sample for the years after the reform. However, given that the housing allowance reform only affected low income households we can run ’placebo’-regressions on the sample of rich households. If the labor force participation of secondary earners in high income households with and without children (which were essentially all untreated) evolved similarly after the reform, this provides some evidence on the likelihood that the post-reform trends for the low income sample would be similar as well and thereby serve as an important robustness test. More specifically, we have constructed a placebo-test by estimating equation (13) on females married to husbands with qualifying incomes above the 75th percentile which in 1996 corresponded to an income level of around 310,000 SEK.[29] If there is a ’response’ of high-income households in the post-reform period there is a concern that the estimated effect in the low-income sample reflects some underlying employment trend of women with children rather than a causal effect of the reform.

6 Graphical analysis

In Figure 2 we plot the evolution of the average PTR for the treatment and control groups (households with and without children) over the time period 1994-2001 which is the focus of our regression analysis. The PTR:s have been calculated on HEK-data using the micro-simulation model FASIT (which takes the entire Swedish tax- and transfer system into account). As can be seen from the Figure, the reform in 1997 implied a sharp drop in the average PTR for the treatment group. This drop was caused by the housing allowance reform and demonstrates the

[29]In fact, some households with 3 or more children could be eligible for housing allowance up to 351,100 SEK.

strength in the first stage of our IV strategy. Before the housing allowance reform of 1997 the gap in the average PTR:s for couples with and without children respectively exceeded 10 percentage points and was substantially smaller in the post-reform period.

In Figure 3 we show how the employment of married women (defined as having positive earnings) evolved in couples with and without children between 1991 and 2010. A nice feature of Figure 3 is that it illustrates the evolution of employment outside the more narrow time period of our regression analysis. [30] We make the following observations. In the beginning of the 1990's, there was a sharp decline in employment due to a deep economic recession. Figure 3 suggests that female employment decreased slightly more among households with kids 1991-1993. However, between 1993 and 1996 the two lines moved in parallel. Note also that the employment *levels* are strikingly similar. After the 1997 reform, employment continued to evolve similarly until 1998. Then there was a relative employment increase of women with children, which continued in the post-reform period.

7 Results

In the following sections we present and discuss our empirical findings.

7.1 Reduced form effects

We start off by presenting results from the simplest and most transparent specification, equation (13), where we are interested in the interactions between the indicator variables for having children and the year dummies. The coefficients on these interactions for the post-reform years capture the dynamics of the reform effect and the coefficients on the interactions for the pre-reform years allow us to test that the pre-reform trends were parallel for households with and without children.

Our complete set of results for the reduced form effects analysis are presented in Table 1. Columns 1-4 show the coefficients for the main 'low income' sample where most households

[30] In both figures 2 and 3 we maintain the same sample restrictions as in the regression analysis, i.e. we focus on households where the husband's qualifying income falls below the 50th percentile and where the husband reports positive earnings.

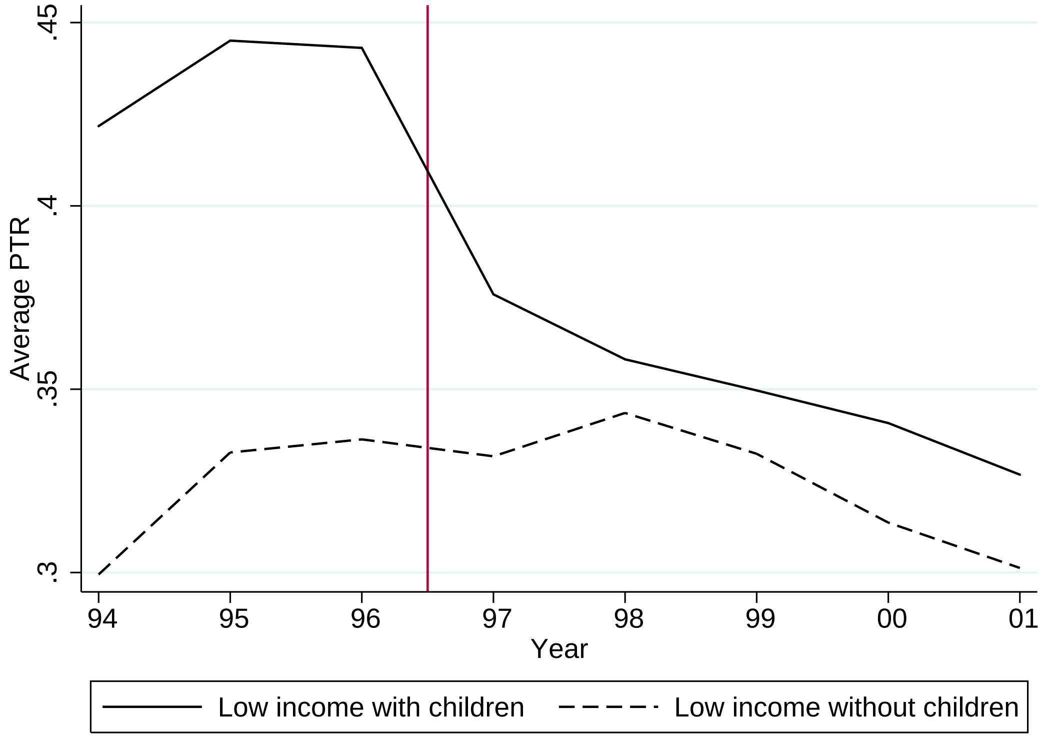

Figure 2: (*Graphical first-stage*) Average participation tax rates (PTR) by child status on HEK data. PTR:s are calculated in FASIT. The sample is restricted to households where the husband's qualifying income falls below the 50th percentile and where the husband reports positive earnings.

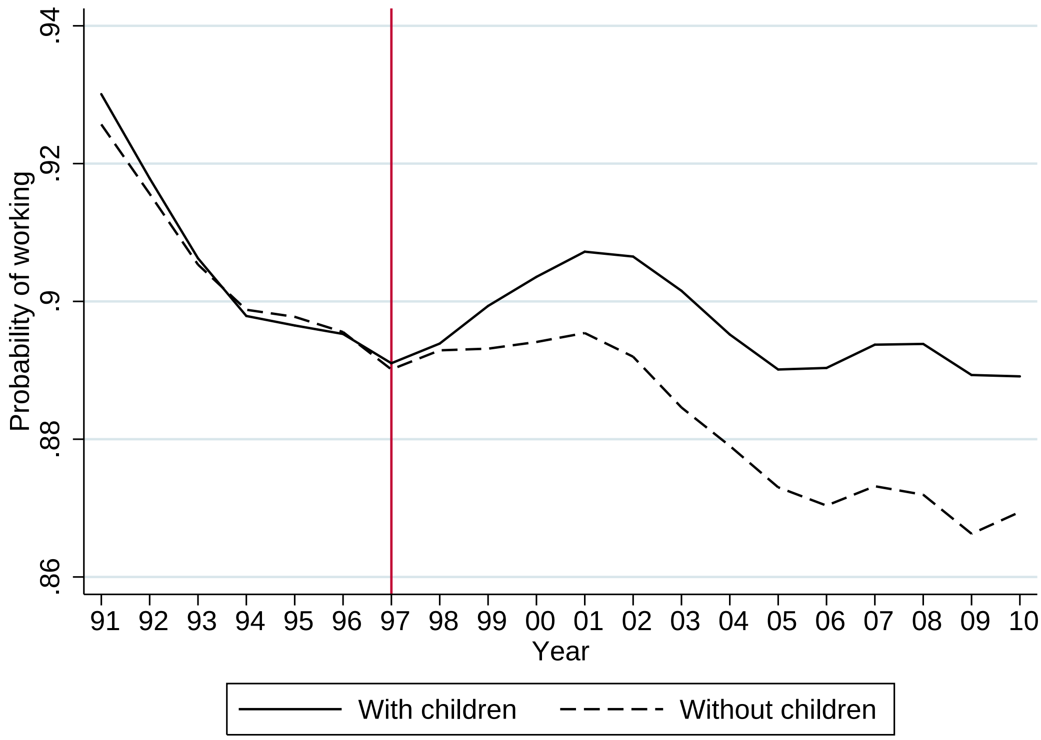

Figure 3: (*Graphical reduced form evidence and long term trends*). Female participation (share with positive earnings) in low income households where the husband participates in the labor force.

with children were eligible for housing allowances (at zero earnings of the wife). The first column reports the results of a difference-in-difference specification without any control variables. In this column, the first thing to notice is that the coefficients for the pre-reform years, 1994 and 1995, are statistically insignificant, confirming the visual evidence of Figure 3 that the pre-reform trends were very similar for the treatment and control group. In fact, the coefficients for the pre-reform years remain insignificant for all the specifications that we have considered as evident by columns 1-4. Moreover, also consistent with Figure 3, we see that there is a statistically significant response to the reform in 1999 and that the response grows monotonically across the post-reform years. For 2001 the estimated effect amounts to 1.2 percentage points.

In column 2 we have added household type controls and the estimated effects become somewhat larger. In column 3 we control for trends in a flexible way including the full set of interactions between the time dummies and the household type dummies as well as the interactions between the household type dummmies and the dummy for having children. Interestingly, in this specification, the reduced form effect estimates are also significant for the two post-treatment years 1996 and 1997 (at the 5 percent level). Finally, when the full set of controls are included in column 4, the overall pattern of coefficients is similar to column 3, but the reform effect estimate for 2001 is more in line with that obtained in the specification without controls in column 1. Our preferred estimate of the reform effect is the coefficient for 2001 in our most ambitious specification of column 4 and amounts to a 1.12 percentage point increase in the probability of married women to participate in the labor force.[31]

We see that the response in general is increasing in each post-reform year. This suggests that adjustment costs, e.g. the search cost of finding a new job, are important. As discussed above in section 2.3 information about the reform became publicly available close before its implementation, and it probably takes some time for households to adjust.

In column 5 we report the results from a 'placebo-regression' with the full set of controls, where we have estimated equation (13) on a sample consisting of women married to husbands with qualifying income over the 75th percentile (which were essentially all untreated by the reform). In all other respects, the selection criteria are identical to the main low-income sam-

[31] These results are robust to excluding cells (defined based on year×children×household type) that contain less than 100 observations.

ple. It is striking that all estimated coefficients are insignificant at the 5 percent level. One interaction, the interaction for 2001, is significant at the 10 percent level, but the coefficient estimate is considerably smaller than the corresponding point estimate in the low income sample. The results of this placebo regression, considered in conjunction with the results in column 1-4 showing that the trends before the reform were parallel, and the visual evidence in 3, allow us to be reasonably confident that the identifying assumption in our difference-in-difference setup is satisfied.

In order to examine the validity of 'primary-secondary earner' assumption we have estimated equation (13) on a sample of males. Our idea has been to construct the male sample as a *mirror image* of the female low income sample by conditioning the male sample on the wife's qualifying income falling below the 50th percentile. The results are presented in column 1 of Table A1 of Appendix D where it can be inferred that the estimated coefficients for this male sample are very different from the female sample. For 1994-2000 none of the interaction terms are statistically significantly different from zero. For 2001 we estimate a *negative* effect on male employment equal to -0.36 percentage points which is significant at the 5 percent level. To dig deeper into the potential mechanisms at play we have also examined the males' potential *earnings* responses (intensive margin response). We found no clear evidence of a response in log earnings after including the full set of controls, see columns 2 and 3 of of Table A1.

Finally, we have also estimated equation (13) on the main *female* sample with log earnings instead of employment on the left hand side. We first transformed earnings into log earnings in the standard way, thereby *excluding* women with zero earnings. The estimation of this pure 'intensive margin' response resulted in small positive coefficients for the post-reform years (see column 4 of Table A1). However, we then used log of (earnings+1) as dependent variable, thereby *including* females with zero earnings in the regression and found that the estimated coefficients were significant in all post-reform years and also substantially larger (see column 5 of Table A1). The results from these two exercises lead us to conclude that women primarily reacted to the reform along the extensive margin, i.e. they went from zero earnings to a positive amount of earnings.

Table 1: Reduced form effects (in percentage points)

	Low income	Low income	Low income	Low income	High income
	(1)	(2)	(3)	(4)	(5)
Year 1994 × children	-0.060	-0.152	0.000	-0.097	-0.264
	(0.130)	(0.129)	(0.163)	(0.159)	(0.171)
Year 1995 × children	-0.097	-0.121	-0.095	-0.140	0.016
	(0.111)	(0.110)	(0.140)	(0.137)	(0.149)
Year 1997 × children	0.120	0.154	0.348**	0.404***	-0.117
	(0.114)	(0.113)	(0.144)	(0.141)	(0.153)
Year 1998 × children	0.129	0.245*	0.331**	0.392**	0.000
	(0.134)	(0.132)	(0.169)	(0.164)	(0.178)
Year 1999 × children	0.652***	0.833***	0.681***	0.813***	0.189
	(0.145)	(0.144)	(0.181)	(0.177)	(0.192)
Year 2000 × children	0.976***	1.24***	0.790***	0.992***	0.245
	(0.154)	(0.152)	(0.189)	(0.185)	(0.202)
Year 2001 × children	1.214***	1.485***	0.863***	1.120***	0.385*
	(0.160)	(0.159)	(0.196)	(0.193)	(0.211)
Household type dummies	No	Yes	Yes	Yes	Yes
Household type × children	No	No	Yes	Yes	Yes
Household type × year dummies	No	No	Yes	Yes	Yes
Additional controls	No	No	No	Yes	Yes
Nr of observations	2,770,100	2,770,100	2,770,100	2,770,100	1,385,071

Note: Dependent variable: probability of having positive earnings. ‘Low income’ sample consists of wives married to husbands with a positive qualifying income, which falls below the 50th percentile. ‘High income’ sample consists of wives married to husbands with a positive qualifying income that falls above the 75th percentile. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level.

7.2 Elasticities

We now turn to our participation elasticity estimates. Before we discuss the results we briefly comment on how we construct the participation elasticities based on the regression coefficients (marginal effects). The elasticities are calculated according to equation (9) where we have multiplied the estimate $\hat{\beta}$ of β of equation (12) with the ratio $\frac{\overline{1-\tau}}{\bar{e}\bar{G}(B^0)}$. In this expression, $\overline{1-\tau}$ and $\bar{e}$ are the averages of $1-\tau$ and e (the employment rate) over the years 1994-2001 in the low income sample and $\bar{G}(B^0)$ is the average take-up rate of one-earner households in the pre-reform period, which is observed to be around 0.6 in the HEK sample.[32] Moreover, if the conditions specified in Proposition 1 in section 4.2 are satisfied, the participation elasticities that we construct based on the marginal effect in regression (12), can be given a structural interpretation.

The results are presented in Table 2. Columns 1-4 show estimates using different sets of control variables. The instruments are strongly correlated with the PTR. In the 2SLS regression presented in column 1 the first-stage F-statistic of the excluded instruments is as large as 66,834. In each case we obtain precise estimates of the participation elasticity. Our preferred estimate is obtained for our most ambitious set of controls (column 4) in which case the elasticity estimate amounts to 0.13. The exact magnitude of the elasticity estimate varies somewhat depending on the set of control variables used in the regressions. This is perhaps not too surprising in light of the results for the reduced form effects in Table 1.

Before closing this section we would like to point out that we are aware of the fact that since the PTR:s have been estimated in a separate step, our standard errors might be slightly biased due to presence of a generated regressor in equation (12). As performing a proper correction of the covariance matrix for the full specification, which contains a huge amount of dummy variables, would be computationally very burdensome we have instead made a correction á la Murphy and Topel (1985) for the specification without control variables reported in column 1. More specifically, we have computed the covariance matrix given by equation (15') of Murphy and Topel (1985) and verified that the correction did not, at least in this case have any profound

[32]We obtained this figure by pooling the pre-reform years, 1994-1996. Due to the fact that the sample is restricted to only include households where the wife does not work, the sample size is too small to provide a more disaggregated estimate of the take-up rate.

Table 2: Participation elasticity estimates

	(1)	(2)	(3)	(4)
Participation elasticity	0.088***	0.117***	0.098***	0.127***
	(0.013)	(0.013)	(0.020)	(0.020)
Household type dummies	No	Yes	Yes	Yes
Household type × children	No	No	Yes	Yes
Household type × year dummies	No	No	Yes	Yes
Additional controls	No	No	No	Yes
Nr of observations	2,770,100	2,770,100	2,770,100	2,770,100

Note: Elasticities are evaluated at the mean values of employment (0.897) and (1-PTR) (0.659) over the years 1994-2001 in the total 'low income sample'. 2SLS regressions are run on 'low income sample', which consists of wives married to husbands with a qualifying income below the 50th percentile. The average take-up rate is set to 0.6. The interactions between the year dummies and the dummy for having children are the excluded instruments. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level. Standard errors for elasticities are obtained by the delta method.

impact on the standard errors. The implied standard error increased only slightly from 0.013 to 0.014. We therefore conclude that the generated regressor bias is likely to be small and of little practical importance.

7.3 Heterogenous response

As emphasized in section 4 above we anticipate the elasticity to differ across subpopulations with different baseline employment rates. In the past, extensive margin responses to taxes have been estimated on relatively small data sets. Since we have access to population wide registers we are able to examine how the elasticity differs across subpopulations in a systematic way.

We divide the low income sample into four *quartiles* based on imputed log earnings. In the imputation regressions, which are run separately for each year, we control for household type (as defined above). In addition we include dummies for 7 regions of origin, dummies for municipality of residence and a full set of age dummies. After partitioning the sample into four quartiles, we rerun equation (12) on each quartile. Following the procedure suggested by equation (9) we evaluate the elasticity at the *subsample-specific* mean values of employment and $(1 - \tau)$.

Table 3: Heterogenous response

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Participation elasticity	0.235***	0.117*	0.109**	0.090***
	(0.058)	(0.047)	(0.038)	(0.027)
Mean employment level	0.808	0.903	0.923	0.955
Household type dummies	Yes	Yes	Yes	Yes
Household type × children	Yes	Yes	Yes	Yes
Household type × year dummies	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Nr of observations	692,559	692,542	692,476	692,523

Note: Elasticities are evaluated at the mean values of each subsample. 2SLS regressions are run on 'low income sample', which consists of wives married to husbands with a qualifying income below the 50th percentile. Quartiles are created based on tbe wife's predicted income. The average take-up rate is set to 0.6. The interactions between the year dummies and the dummy for having children are the excluded instruments. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level. Standard errors for elasticities are obtained by the delta method.

Table 3 reports the subsample analysis with the full set of control variables. As we move across the four quartiles we see that the elasticities are falling monotonically in the wife's skill level mirrored by a corresponding monotonic increase in the employment level. In line with our expectations, the elasticity is the largest in the first quartile, where the employment level is substantially smaller than in the other three quartiles. The elasticity estimate for the first quartile (0.235) and the fourth quartile (0.09) are statistically different at a level of 95 percent.[33]

8 Concluding remarks

In this paper we have estimated participation elasticities of secondary earners exploiting a reform in the tax/transfer-system for identification. Our central estimate of the participation elas-

[33]Following e.g. Clogg et al. (1995), p.1276, we test this using the fact that differences between the coefficients from a regression run on two independent large samples x and y can be assessed by the statistic $Z = \left(\hat{\beta}_x - \hat{\beta}_y\right) / \sqrt{se_x^2 + se_y^2}$, which follows a standard unit normal distribution. $\hat{\beta}_j$ and se_j are the coefficient and the standard error of sample $j = x, y$. Since we are interested in testing for differences in elasticities, we have made the proper adjustments by multiplying the coefficients and standard errors by different constants. Using the values for the elasticities and standard errors in column 1 and 4 of Table 2 we obtain a Z-ratio of 2.266, which is larger than the critical value 1.96.

ticity is 0.13, arguably a lower value than many earlier estimates obtained in the literature. Crucially, we have also presented quasi-experimental estimates of participation elasticities for subgroups of the population with different employment levels. This exercise was made possible by virtue of our large sample size. Dividing up the population into four quartiles based on the wife's skill level we find participation elasticities ranging from 0.24 at the bottom to 0.09 at the top. The point estimates of the elasticities fall monotonically in skill level, and the elasticity differences between the bottom and the top are statistically significant.

Intuitively, the higher the employment level, the smaller the pool of unemployed that can be incentivized to enter the labor force. Following e.g. Chetty et al. (2012) we have emphasized that the participation elasticity is determined by the number of individuals who are indifferent between working and not working, which in the context of our simple model, depends on the local shape of the distribution of fixed costs of work. In line with the public finance literature, we have assumed that employment is voluntary and focused on the decision to enter the labor force. If involuntary unemployment is more common among the low-skilled we potentially underestimate the participation elasticity in this group.

It is useful to compare our results to Selin (2014) who exploited the switch from joint to individual taxation in Sweden in 1971 to estimate participation elasticities for married Swedish women. Selin found estimates in the range 0.5-1.0, which are well above our estimates. However, the results are actually completely consistent when adopting the perspective of our paper. Selin (2014) reports that the pre-reform share of married women with positive earnings was 67% (Table 8) whereas the corresponding share in our study is 90%. This further highlights the important relationship between the participation elasticity and employment level that we have emphasized in this paper.

The key insight from this paper is that the participation elasticity is fundamentally different in nature from the intensive margin labor elasticity. When 'plugging in' the participation elasticity into simulation models it is indeed important to consider the employment level in the subpopulation of interest.[34] This point has been made before, see e.g. Chetty et al. (2012); our

[34]Our quasi-experimental estimates provide a useful contrast against estimates obtained using microsimulation models. Immervoll et al. (2007) analyze welfare reforms in 15 European countries including Sweden, and calibrate the average participation elasticity for the whole economy to 0.2, but decreasing across deciles. In a related exercise, which is more focused on participation responses, Immervoll et al. (2011) assume participation elasticities

contribution is to examine this feature of the participation response using administrative data and a quasi-experimental identification strategy.

for secondary earners in the range 0.3-0.7. In light of this paper these elasticities appear to be too large, at least for a country like Sweden.

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Online Appendix (not for publication)

A Formula for calculating the HA

Both for 1996 and 1997 the maximum monthly housing allowance (MMHA) can be written

$$MMHA = 600 + \max\{0, (\min(QHE, 3000) - 2000) \times 0.75\} \\ + \max(0, (QHE - 3000) \times 0.50) \quad (14)$$

where HA = household housing allowance [SEK/month], QHE = qualifying household housing expenses [SEK/month], and I = household income before tax [SEK/month]. However, the qualifying housing expenses changed between 1996 and 1997.

In 1996 QHE was simply the rent paid by the tenant. There was also a minimum guaranteed housing expense level (which was a function of the number of children).

For 1997 the QHE can be written

$$QHE = \max\left(MHE, HE \times \frac{\min(SC, AS)}{AS}\right), \quad (15)$$

where MHE =minimum guaranteed housing expense level , HE = actual housing expense (rent), SC = space constraint and AS = actual space constraint. The space constraint depends on the number of kids in the houshold.[35] [36]

[35] 1 child: 80 sqm, 2 children: 100 sqm, 3 children: 120 sqm, 4 children: 140 , 5 or more: 160 sqm.

[36] The yearly rent per square meter was approximately SEK 700, 1996-97. Rent statistics: http://www.boverket.se/Global/Webbokhandel/Dokument/2011/Hyror-i-Sverige-1975-2009.pdf, figure 2.1.

B Other components of the reform

In the discussion of section (2.2) we only considered the individualization of the exemption level, which is the main focus of our paper. However, two other potentially important components of the reform deserve to be mentioned as well; the new space restriction and the *ex post* adjustment of the allowance.

Although the upper cap on the transfer before phase-out, B^{00}, did not change, many households nevertheless experienced a decrease in B^{00}. In the 1997 reform package the government introduced an upper limit to the qualifying housing space, i.e. the number of square-meters of dwelling space the household could be compensated for. We take this space restriction into account when calculating the participation tax rates. It lowered the transfers, especially for couples who tend to live in larger apartments than singles.

Both before and after 1997 the beneficiary had to repay the benefit if the household's qualifying income substantially increased and the household did not report this increase in income.[37] However, before 1997 the household never had to repay an allowance it was eligible for at the month of the monthly benefit payment. From 1997 and onwards, the monthly allowance receipt was labeled as 'preliminary'. In the new system, the beneficiary applies in December year t for housing allowance in year $t + 1$. In year $t + 1$ the beneficiary each month receives the housing allowance based on the qualifying income reported in the application in December year t. In year $t + 2$ the two spouses file their tax returns. By the end of $t + 2$ the Social Insurance Agency receive information from the Tax Agency on the household's *ex post* qualifying incomes in $t+1$. Finally, in the spring of year $t + 3$ the Social Insurance Agency charge/reimburse households where the incomes reported in year t deviate from the realized income in year $t + 1$.

From the point of view of fiscal sustainability, the reform was a great success, to say the least. As can be seen from Figure A1, the government's expenditures on the program fell dramatically in the years following 1997 (marked with a vertical line). Moreover, we see that there was a huge decrease in the number of couples receiving the transfer between 1996 and 1997. The decrease among singles was arguably more modest. Single households were affected both by the space restriction (but to a smaller degree than couples as their dwellings typically were

[37] See Boverket (2006) (in Swedish) for a description of these pretty complex rules.

smaller) and by the new rules for *ex post* repayments/reimbursements. However, the income limits of singles were unchanged. Why did the size of the program decrease in the years following 1997? In the post-reform period the benefit levels and the income limits were kept at their nominal levels of 1996; they were not indexed. Accordingly, with inflation and real wage growth, a growing fraction of couples and singles became eligible only for small amounts, or became ineligible.

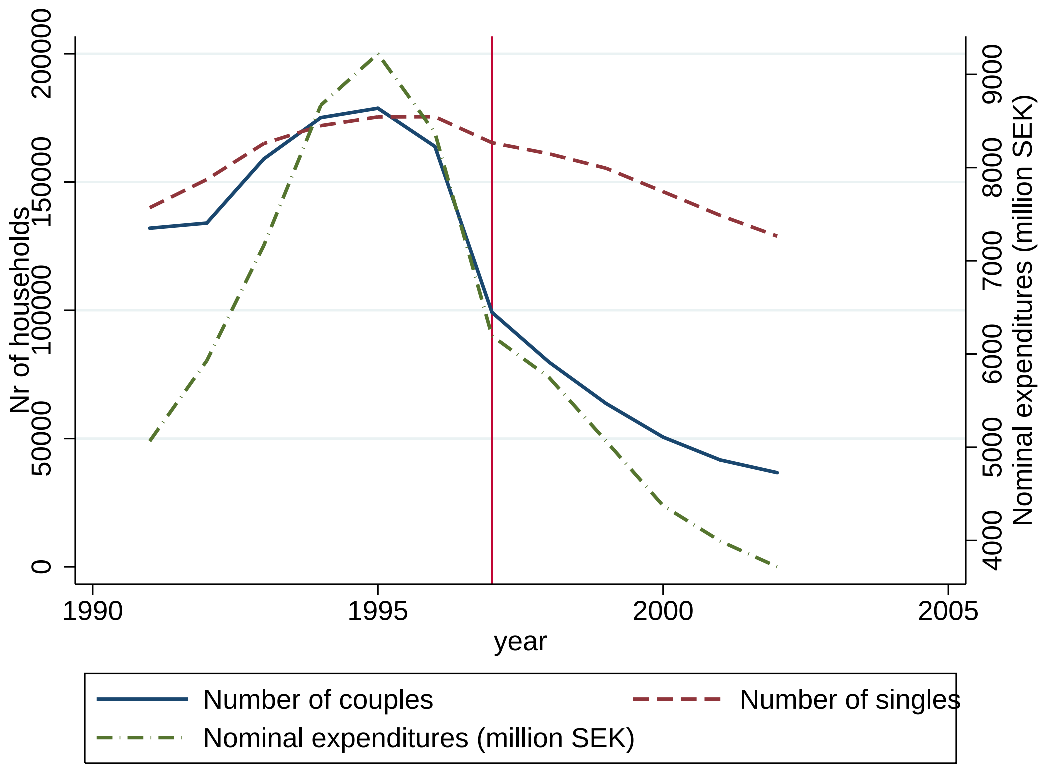

Figure A1: Number of couples and singles receiving housing allowances, as well as nominal expenditures on housing allowances in million SEK. Source Boverket 2006, Table A.

C Proof of proposition 1

First we characterize the fractions of the population in each of the four household states emphasized on page 14 (i.e. $e^{ij}, i, j = 0, 1$) without making any distributional assumptions. Thereafter we impose the assumptions in Proposition 1 to derive the relevant derivatives of e with respect to T, B^0, and, B^1, that can be used to establish the relationship between the two key elasticities given in the proposition. To simplify the exposition in this appendix we omit the h index. All calculations are valid at the household-type level.

C.1 A general characterization

We describe the decision-making of the household by considering the labor-market entry conditions for the secondary earner depending for different values of the take-up cost χ.

If $0 \leq \chi \leq B^1$ the household always takes up the transfer (both when working and not working) and therefore participates in the labor force when the following condition is met:

$$z - (T^1 - T^0) - (B^0 - B^1) \geq q \quad \text{(low)} \tag{16}$$

If $\chi > B^0$ the household does not take up the transfer in the state of work nor in the state of non-work, and the participation equation becomes:

$$z - (T^1 - T^0) \geq q \quad \text{(high)} \tag{17}$$

If $B^1 < \chi \leq B^0$ the household takes up the transfer when unemployed, but not when working, which implies that the participation equation becomes:

$$z - (T^1 - T^0) - B^0 \geq q - \chi \quad \text{(intermediate)} \tag{18}$$

Note that this last condition depends on χ. That is, the incentive to enter the labor force depends on the size of the take-up cost. It appears in (18) because households with $B^1 < \chi \leq B^0$ only experience the take-up cost when they are outside the labor force.

As the above conditions only depend on the difference between T^1 and T^0 we set $T = T^1 -$

$T^0 \geq 0$ without loss of generality. We denote the threshold values of q which cause inequalities (16), (17), and (18) to bind by q^L, q^H, and q^I, respectively. We have that $q^L \leq q^I \leq q^H$ by virtue of the fact that $B^0 > B^1$ (and the fact that q^I only applies for values of χ satisfying $B^1 < \chi \leq B^0$). Notice that q^L and q^H are fixed and can be expressed in terms of observable quantities as $q^L \equiv q^L(z, T, B^1, B^0)$ and $q^H \equiv q^H(z, T)$ [specifically, $q^L = z - (T^1 - T^0) - (B^0 - B^1)$ and $q^H = z - (T^1 - T^0)$] whereas q^I depends on the take-up cost χ and takes on the value $q^I = q^L$ when $\chi = B^1$ and $q^I = q^H$ when $\chi = B^0$.[38]

In the following we assume q and χ are jointly distributed according to the probability density function $f(q, \chi)$.

Based on conditions (16)-(18) we can write down the number of workers in each state e^{ML}, $M = 0, 1; L = 0, 1$. Note that the division of agents into the four categories above based on their innate characteristics (q, χ) completely characterizes the optimal behavior of agents.

The number of households who work and take-up transfers are:

$$e^{11} = \int_0^{q^L} \int_0^{B^1} f(q, \chi) d\chi dq$$

The number of households who work and do not take up transfers are:

$$e^{10} = \int_0^{q^L} \int_{B^1}^{\infty} f(q, \chi) d\chi dq + \int_{q^L}^{q^H} \int_{q-q^I}^{\infty} f(q, \chi) d\chi dq$$

The number of households who do not work and take up transfers are:

$$e^{01} = \int_{q^H}^{\infty} \int_0^{B^0} f(q, \chi) d\chi dq + \int_{q^L}^{q^H} \int_0^{q-q^I} f(q, \chi) d\chi dq$$

Finally, the number of households who neither work nor take up transfers are:

$$e^{00} = \int_{q^H}^{\infty} \int_{B^0}^{\infty} f(q, \chi) d\chi dq.$$

It follows by construction that the total number of workers is $e^1 = e^{11} + e^{10}$ and that the total number of unemployed agents is $e^0 = e^{00} + e^{01}$ with $e^0 + e^1 = 1$.

[38]Notice that q^I will be a line in the (χ, q)-space.

C.2 Derivation of marginal effects of tax/transfer instruments

Assuming q and χ are independent we can write the number of individuals in each group as follows:

$$
\begin{aligned}
e^{11} &= F(q^L)G(B^1) \\
e^{10} &= F(q^L)[1 - G(B^1)] + \int_{q^L}^{q^H} f(q)\left[\int_{q-q^I}^{\infty} g(\chi)d\chi\right]dq \\
&= F(q^L)[1 - G(B^1)] + \int_{q^L}^{q^H} f(q)[1 - G(q - q^I)]dq \\
e^{01} &= [1 - F(q^H)]G(B^0) + \int_{q^L}^{q^H} f(q)\left[\int_{0}^{q-q^I} g(\chi)d\chi\right]dq \\
&= [1 - F(q^H)]G(B^0) + \int_{q^L}^{q^H} f(q)G(q - q^I)dq \\
e^{00} &= [1 - F(q^H)][1 - G(B^0)]
\end{aligned}
$$

To establish Proposition 1 we need to compute the derivatives of $e = e^{11} + e^{10}$ with respect to the tax/transfer instruments T, B^0 and B^1. That is, we are interested in computing:

$$
\begin{aligned}
\frac{de}{dB^0} &= \frac{de^{11}}{dB^0} + \frac{de^{10}}{dB^0} \\
\frac{de}{dB^1} &= \frac{de^{11}}{dB^1} + \frac{de^{10}}{dB^1} \\
\frac{de}{dT^1} &= \frac{de^{11}}{dT^1} + \frac{de^{10}}{dT^1}.
\end{aligned}
$$

To make progress we impose the additional assumption that $F(q)$ is *locally* uniform on the open interval $(z - T - B^0, z - T)$ in the sense that it has constant pdf with density γ on this interval and is unrestricted elsewhere. In the derivations below, recall that $q^L = z - (T^1 - T^0) - (B^0 - B^1)$ and $q^H = z - (T^1 - T^0)$.

Then, we first notice that:

$$\begin{aligned}
\frac{de^{11}}{dB^0} &= -\gamma G(B^1) \\
\frac{de^{11}}{dB^1} &= \gamma G(B^1) + G'(B^1)F(q^L) \\
\frac{de^{11}}{dT^1} &= -\gamma G(B^1).
\end{aligned}$$

For example, the first condition above states that as B^0 is marginally increased, there will be an outflow from the group of workers who take-up transfers according to their number $G(B^1)$ times the marginal density of the fixed-cost distribution γ (which simply reflects the number of individuals who are indifferent between working and not working).[39] In the second condition, the first term states that as B^1 is increased, the fraction of workers who take up the transfer when working will be incentivized to join the labor force, according to the marginal density γ. In addition, there will be an increase in take-up represented by the second term.

Applying slightly more effort we can apply Leibniz integral rule and derive:

$$\begin{aligned}
&\frac{de^{10}}{dB^0} = \frac{d}{dB^0}\Big(F(q^L)[1 - G(B^1)]\Big) + \\
&\gamma \int_{q^L}^{q^H} \frac{d}{dB^0}[1 - G(q - q^I)]dq + \gamma \frac{dq^H}{dB^0}[1 - G(q^H - q^I)] - \gamma \frac{dq^L}{dB^0}[1 - G(q^L - q^I)] = \\
&= -\gamma[1 - G(B^1)] + \gamma[-G(q - q^I)]_{q^L}^{q^H} + \gamma[1 - G(B^1)] = \\
&= -\gamma[G(B^0) - G(B^1)]
\end{aligned}$$

This condition gives the change in the group who works and does not take up transfers in response to an increase in the out-of-work transfer B^0. An increase in B^0 increases non-participation proportionally to $[G(B^0) - G(B^1)]$ which is the fraction of workers with intermediate take-up costs in the sense that they only take-up transfers when unemployed.

[39] Notice that any worker who belongs to the group e^{11} will by assumption also take up the transfer when not-working since $B^1 \leq B^0$.

Similarly, we can derive:

$$\frac{de^{10}}{dB^1} = \frac{d}{dB^1}\Big(F(q^L)[1 - G(B^1)]\Big) +$$
$$\gamma \int_{q^L}^{q^H} \frac{d}{dB^1}[1 - G(q - q^I)]dq + \gamma\frac{dq^H}{dB^1}[1 - G(q^H - q^I)] - \gamma\frac{dq^L}{dB^1}[1 - G(q^L - q^I)] =$$
$$= -F(q^L)G'(B^1).$$

This expression states that as B^1 increases, there will be a dynamic take-up response. Some who previously worked without transfers will now work and take up transfers.

Finally, we derive:

$$\frac{de^{10}}{dT^1} = \frac{d}{dT^1}(\gamma q^L + \rho)[1 - G(B^1)] +$$
$$\gamma \int_{q^L}^{q^H} \frac{d}{dT^1}[1 - G(q - q^I)]dq + \gamma\frac{dq^H}{dT^1}[1 - G(q^H - q^I)] - \gamma\frac{dq^L}{dT^1}[1 - G(q^L - q^I)] =$$
$$= -\gamma[1 - G(B^1)]$$

To understand this effect note that e^{10} is the fraction of workers who do not take up the transfer while working represented by the fraction $[1 - G(B^1)]$ of the population. A number of these individuals will drop out of the labor force in response to the tax increase according to the marginal density γ.

Putting things together we get:

$$\frac{de}{dB^0} = \frac{de^{11}}{dB^0} + \frac{de^{10}}{dB^0} = -\gamma G(B^1) - \gamma[G(B^0) - G(B^1)] = -\gamma G(B^0)$$
$$\frac{de}{dB^1} = \frac{de^{11}}{dB^1} + \frac{de^{10}}{dB^1} = \gamma G(B^1) + G'(B^1)F(q^L) - F(q^L)G'(B^1) = \gamma G(B^1)$$
$$\frac{de}{dT^1} = \frac{de^{11}}{dT^1} + \frac{de^{10}}{dT^1} = -\gamma G(B^1) - \gamma[1 - G(B^1)] = -\gamma.$$

This establishes Proposition 1.

D Alternative empirical specifications

Regression results from alternative specifications are reported in Table A1.

E Summary Statistics

Summary statistics are reported in Table A2.

Table A1: Reduced form effects in alternative specifications

	Male employment (1)	Male log(earnings) (2)	Male log(earnings+1) (3)	Female log(earnings) (4)	Female log(earnings+1) (5)
Year 1994 × children	0.045	-0.000	0.002	-0.008	-0.015
	(0.159)	(0.006)	(0.012)	(0.005)	(0.011)
Year 1995 × children	-0.061	0.005	-0.001	-0.004	-0.013
	(0.139)	(0.005)	(0.011)	(0.005)	(0.010)
Year 1997 × children	0.049	-0.000	0.005	0.002	0.031***
	(0.141)	(0.005)	(0.011)	(0.005)	(0.010)
Year 1998 × children	-0.036	0.0039	0.003	0.000	0.029**
	(0.161)	(0.006)	(0.013)	(0.006)	(0.012)
Year 1999 × children	-0.081	0.007	0.004	0.012*	0.068***
	(0.170)	(0.006)	(0.013)	(0.006)	(0.013)
Year 2000 × children	-0.096	0.011*	0.009	0.029***	0.095***
	(0.177)	(0.006)	(0.014)	(0.006)	(0.014)
Year 2001 × children	-0.364**	0.009	-0.012	0.038***	0.112***
	(0.182)	(0.007)	(0.015)	(0.007)	(0.015)
Household type dummies	Yes	Yes	Yes	Yes	Yes
Household type × children	Yes	Yes	Yes	Yes	Yes
Household type × year dummies	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes
Nr of observations	2,658,815	2,521,767	2,658,815	2,485,259	2,770,100

Note: Dependent variable: probability of having positive earnings. 'Male sample' consists of husbands married to wives with a positive qualifying income, which falls below the 50th percentile.'Female sample' sample consists of wives married to husbands with a positive qualifying income, which falls below the 50th percentile. All specifications contain a dummy for having children and a full set of year dummies. 400 household types are defined based on 5 age dummies for each spouse and 4 education level dummies for each spouse. The additional control variables are specified in section 5.1. Standard errors reported below the estimates are robust to heteroscedasticity and clustered at the household level. * indicates significance at 10%. level, ** 5% level and *** at 1% level.

Table A2: Summary Statistics

	With children		Without children	
Labor force participation	0.898	(0.303)	0.895	(0.307)
Net of tax rate $1 - \tau$	0.603	(0.067)	0.663	(0.033)
Age of secondary earner	39.720	(5.962)	47.649	(5.765)
Age of primary earner	42.201	(6.246)	49.286	(5.560)
Earnings	1245.602	(841.517)	1395.388	(868.631)
Qualifying income of primary earner	1891.367	(1121.539)	1895.504	(758.296)
Education				
At most 9 years of education	0.156	(0.363)	0.282	(0.450)
At most high school education	0.573	(0.495)	0.536	(0.499)
College education	0.265	(0.441)	0.172	(0.377)
Country of origin				
Sweden	0.920	(0.271)	0.949	(0.220)
Western Europe, North America and Oceania	0.058	(0.233)	0.039	(0.194)
Eastern Europe and former Soviet Union	0.015	(0.121)	0.010	(0.100)
South America	0.001	(0.030)	0.000	(0.016)
Sub-Saharian Africa	0.000	(0.016)	0.000	(0.010)
Northern Africa and Middle East	0.005	(0.069)	0.001	(0.025)
Asia	0.001	(0.033)	0.000	(0.019)
Number of observations	2,069,793		700,307	

Note: Standard deviations reported in parenthesis. Incomes are expressed in 100 SEK. Summary statistics refer to the period 1994-2001.