

Domestic Services and Female Earnings: Panel Register Data Evidence from a Reform¹

by

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Abstract

Empirical studies have shown that changes in household time constraints affect female labor supply, but evidence of the precise quantitative relation is scarce. In 2007, a tax discount reform in Sweden reduced prices of domestic services by 50 percent. Analyzing population register data of changes in tax discounts and annual earnings, our estimates indicate for married women that outsourcing of 10 extra hours per year generates one percent increase in log annual earnings, with a “cap” occurring at about 10 percent. “Placebo” estimates confirm that earnings trends do not drive the results.

Keywords: household work, outsourcing, female labor supply

JEL classification: J13, J22

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1 Introduction

Within the OECD countries, a majority of couple families are dual earners with women on average working fewer hours and at lower wages compared to their partners (OECD 2010). Theory and empirical observations link a major part of women's labor market disadvantages to the fact that they do most of the routine housework. To date, several studies have shown evidence of that female labor supply is sensitive to changes in time constraints following e.g. expansions of access to child care, flows of immigrants enhancing the supply of low skilled workers or technological improvements in household appliances (references in Section 2). These studies typically report average estimates but contain limited information on the more precise relation between hours set free and female labor supply. An improved knowledge about the functional form of women's labor supply is relevant to efficiently design policies aiming to relax households' time constraints, e.g. child care (kindergarten, preschool), afterschool activities or elderly care, as it requires correct predictions on the labor supply response of time set free in households. The topic is important from the perspective of gender equality, but also more generally as most OECD governments share the ambition to increase labor supply.

The purpose of this article is to quantify the relation between hours set free through housework outsourcing and female labor supply. To this end, we analyze individual register data of the Swedish population 2000-2010 which include information on households' purchases of domestic services from July 1st 2007, when the implementation of a tax discount reduced the consumer price by 50 percent.² The data enable us to study marginal changes in households' domestic outsourcing 2007-2010, and link these to individual changes in annual labor earnings. In terms of labor supply, we interpret short-term earnings increases as primarily due to extra work hours but

² The types of domestic services that were allowed tax subsidies were restricted. 89 percent of the purchases regarded cleaning (Swedish Tax Agency, 2011), a routine task which may accumulate into a large number of hours in one year. Policies which directly subsidize and stimulate the demand for domestic services (and/or stimulate the supply) exist in e.g. Belgium (*Tütres-services*), Denmark (*Hjemmeservice*), Finland (*Kotitalousvähenmys*), France (*Cheque emploi service*), Germany (*Haushaltsnahe Beschäftigungsverhältnisse*) and Spain (*Special Regime*).

do not rule out that it may partly also reflect hourly wage increases. To take into account that purchases of domestic services are endogenous, we employ difference-in-differences propensity score matching where we account for an unusually large set of covariates, dating back at least seven years prior to year t , the first year the household received a tax discount. “Placebo” estimations on earnings changes in year $t-1$ assure that the estimates do not capture trends in earnings – a key assumption for a causal interpretation of our results.

The major contribution of our study lies in the quality of the data. It allows us; 1) to single out households that outsource housework; 2) to use the yearly change in the amount of the household’s tax discounts as a proxy for the number of hours set free; 3) to use the yearly change in household members’ log annual earnings as a proxy for labor supply decisions; 4) to examine the more exact relation between a relaxed household time constraint and changes in annual earnings. Thus, our presented results concern earnings changes of individuals from households actually outsourcing domestic services so that unaffected (untreated) households do not dilute our estimates.³ Our estimates imply that 10 extra hours of outsourced domestic services is roughly associated with one percent increase in log annual earnings, up to about 10 percent. For the interpretation of the result, one hour of services releases more than 1.5 hours for 70 percent the purchasing households according to survey data (Swedish Tax Agency 2011). We infer from this that freed hours are increasingly transformed to leisure rather than labor market work as their number exceeds three weeks of full time work. The results are obtained in a setting where high quality public child care is universally available at a low cost (e.g., Lundin et al. 2008). This could influence the estimates downwards if there is a decreasing marginal effect of housework outsourcing on earnings. Alternatively, they could be influenced upwards to the extent that child care is a prerequisite for labor force participation. We follow the literature to focus on exploring how household time constraints influence women’s earnings, but we also present estimates for married men.

³ This is an advantage as we seek to pin-down the functional form of female labour supply, but not necessarily if one wishes to evaluate a reform, e.g. of public child care provision, where some households have prior arrangements.

These results point towards an expected gender difference in the impact of domestic outsourcing on earnings.

The paper is structured as follows. The following section gives an account of previous research while Section 3 describes the 2007 tax reform, the data and the samples studied. Section 4 presents the empirical strategy. The results are provided in Section 5 while section 6 concludes with a discussion.

2 Stylized facts, theory and earlier empirical studies

In this section, we give some stylized facts about gendered patterns in housework. Section 2.2 presents theoretical understandings of these related to social norms, household bargaining and specialization within the household. In Section 2.3, we discuss the empirical literature analyzing the relation between housework outsourcing and female labor supply. We conclude by summarizing the existing evidence regarding their quantitative relation.

2.1 Stylized facts

It is well-established that women on average do a majority of all housework (e.g. Treas & Drobnič, 2010). The difference in men's and women's hours of domestic work is largest among couples with children. Longitudinal studies show that men's hours of housework changes only little when becoming a parent while women increase their time in domestic work (Baxter, Hewitt, & Haynes, 2008; Boye, 2008; Kühhirt, forthcoming). The gender gap in housework has declined over time, mainly because women spend less time in unpaid work and less as a result of men increasing their time in housework (Evertsson & Nermo, 2004; Hook, 2010, 2006). The European Social Survey (2010) reveals that Swedish women spend approximately 15 hours a week on do-

mestic work compared to ten hours for men. This is a moderate gender difference compared with many other European countries.

2.2 Theory

One type of explanation for this observed gender gap in housework is that women are *doing gender*, i.e., express themselves as womanly when they do housework (West and Zimmerman 1987, Fenstermaker and West, 2002, Bianchi et al. 2000). In addition, gender norms could make a neat household more important for a woman in displaying herself as a good spouse compared to a man (Ibid.). Behaviors attributable to social norms are often difficult to disentangle from individual preferences.

The relative resource bargaining perspective assume household work is intrinsically bad, and that women have a weaker bargaining power due to lower income or education relative to their male partners (Blood and Wolfe, 1960). This hypothesis has received empirical support (Bianchi et al. 2000, Bittman et al. 2003, Evertsson and Nermo, 2004, 2007, Killewald and Gough, 2010) though some researchers (e.g., Gupta, 2007) have claimed that it is the absolute rather than the relative resources that are important.⁴ Several studies based on cross-sectional data have also argued support for the buying out hypothesis (Cohen 1998, Gupta 2007 and Treas and de Ruijter 2008), predicting that women use their own resources to purchase domestic services and allow their labor supply to increase. Killewald (2011) questions the relevance of this hypothesis as she only finds a weak link between female earnings and time in housework, suggesting market substitutes “*play a smaller role in explaining variation in wives’ time in household labor than has previously been hypothesized*”.

⁴ Interestingly, Evertsson and Nermo (2004) who compare Sweden and U.S. find that American women tend to increase housework if their husband is economically dependent on them, “*as if to neutralize the presumed gender deviance*”. This result could be interpreted as partly supporting a “doing gender” approach.

The theory on *specialization* within the household (Mincer and Polachek 1974, Becker 1985, 1991) is based on the assumption of increasing returns to specialized human capital. To maximize household utility, family members specialize in different activities. If women put in more hours of effort intensive housework, it may (i) reduce women's hours of labor market work, (ii) reduce women's wages compared to men through a lower labor market effort, given similar hours of labor market work, and/or (iii) reduce labor market hours *and* wages by altering females' investments in labor market human capital.

While the above theories explain the existence of gendered division of housework, they do not predict what happens if time is set free by outsourcing.⁵ Microeconomic theory is very general, stipulating that time set free is allocated either to leisure, to other housework or to labor market work depending on the relative marginal utility of the activities. This includes non-monetary aspects, e.g. if parents' utility also depend on children's welfare, produced by time invested in children (Blundell et al. 2005, Cherchye et al. 2012), if labor market work yields social appreciation, social networks or if there is a consumption value in a labor market career. The effects are heterogeneous as the relative preferences for leisure, housework and labor market work, are likely to vary both between household members (e.g. depending on earnings, life cycle situation) and between households (e.g. living conditions and/or social norms). All things considered, compared with males, one would expect female labor supply to be more sensitive to outsourcing.

2.3 Quantifying the relation between domestic work and female labor supply

⁵ If relaxed time constraints are associated with increased female earnings it could in turn improve women's household bargaining power and in a longer perspective cause changes of social norms (e.g. Herch and Stratton 1994; Hook 2010).

There are several studies which have reported that relaxed time constraints (due to outsourcing of household duties) affect the labor supply of, foremost, high skilled women and for women with children. For example, Attanasio et al. (2008) analyze the observed increase in female labor supply between cohorts born in the 1940s and the 1950s and emphasize relaxed time restrictions through lower prices for child care as a key explanation. Hook (2010) presents cross country evidence indicating that public child care stimulates female labor supply and, in their survey, Maani and Cruickshank (2010) argue that the negative relation they discern between housework and wages is difficult to explain without partly inferring a causal relation.

During the last decade, studies exploring changes in the access to and/or prices of domestic services have attempted to identify a more direct causal link between time restrictions and female labor supply. Below, we primarily focus on a few studies of child care reforms which arguably have reported the most precise quantified relationship. A caveat when interpreting the results from these studies is that they concern very different frameworks. Cascio (2009) used Decennial Censuses from 1950 to 1990 to explore variations between U.S. states, and between cohorts within states, in the timing of the introduction of kindergarten grants in the 1960s and 1970s. The findings indicate an increase in the labor supply of mothers whose youngest child was five years old (who were the children primarily affected by the grants), but the effects were limited to single mothers, with no significant effects for married. In this study, kindergarten implied child care for most of the work day and the effect on weekly work hours was estimated to a maximum of 2.8 hours (11 percent increase).

Baker et al. (2008) and Lefebvre and Merrigan (2008) analyzed the introduction of a universal subsidy for public child care in Quebec for children aged five or below. Both studies, although using slightly different data and empirical approaches, find a significant increase in the employment of 7- 8 percent for women with at least one child aged below five, but Baker et al. also re-

port that about one third of the effect crowds out private child care which existed prior to the reform. In terms of working hours, Lefebvre and Merrigan find average increases between 2 and 4.5 hours per week. Estimated effects encompass women from all social classes, thereby also addressing family poverty to the extent that mothers' time gains were turned into labor market work. For Argentina, Berlinski and Galliani (2007) also find an increase in maternal employment following an expansion of preschool for children aged 3-5 years between 1991 and 2001, when the child care coverage increased from 49 percent to 64 percent. They tentatively quantify the average increase in female working hours to approximately 3 hours per week. Existing studies from both Norway and Sweden have provided an insignificant link between child care and women's employment (Havnes and Mogstad 2011, Lundin et al. 2008), plausibly due to that IV estimates were valid only for special groups with child care arranged already prior to the reforms which are explored.

There is also a rapidly growing literature exploiting information on immigrant flows, presumed to reduce the price for domestic services and increase housework outsourcing.⁶ Cortés and Tessada (2011) find evidence that low skilled immigration flows increased the spending on housekeeping services among women in the top quartile of the wage distribution; increased their average work hours and the probability of working long hours while it decreased their time in housework. Work hours increased by 20 minutes per week (app. 16 hours per year). Farré et al. (2011) analyze annual register data, exploring an immigrant wave in Spain. They find an increase in the labor supply of skilled women (completed college degree) with family responsibilities (small children or elderly present in home) compared with equally skilled women without such family responsibili-

⁶ We also fully acknowledge Greenwood et al. (2005) and Coen-Pirani et al. (2010) who argue that relaxed time constraints following ownership of household appliances contributed to increase American women's labor force participation during the second half of the 20th century.

ties. Calculations suggest that a 10 percent increase in immigrant population increased high skilled female labor supply by 15 minutes per week.⁷

All in all, the evidence suggests that relaxed time constraints have a causal effect on female labor supply. In terms of magnitude, estimates on weekly work hours following increased access to child care hovers around 3 hours, implying an average of approximately 10 percent for women working 25-40 hours per week. However, the interpretation of such an average estimate is not straightforward as the time set free varies across households; from potentially a substantial amount of hours to close to zero if child-care arrangements already existed. Without information for individual households, it is difficult to draw any inference on the more specific relation between the number of hours set free and female labor supply increases.

3 The reform, data, and sample

3.1 The reform

On July 1st 2007, a 50 percent tax discount was implemented for individuals purchasing domestic services such as housecleaning and minor gardening tasks. The right wing coalition government argued that the reform would make the allocation of labor more efficient, increase women's possibilities to a labor market career, increase employment and decrease the use of informal labor. However, the reform was highly controversial with the political opposition arguing that it would imply a division of society by class, as it mainly would benefit high income families, and it was also inferred as a moral or ethical problem in that one would subsidize "servants" (or "maids")

⁷ Furtado and Hock (2010) find that immigration reduced the trade-off between fertility and work in the US. Cortés and Pan (2011) explore the arrival of foreign domestic workers 1976-2006 in Hong-Kong, during which the domestic service uptake increased from 2 percent to 8 percent. They report an increase in the relative labor force participation among women in Hong-Kong with a child below age five, compared to women with a child 5-14 years old. See also Barone and Mocetti (2011; Italy), Freire (2013; Singapore) and Freire (2011) who consider migration of low skilled Brazilian women from rural to urban areas.

for the rich. This argument is related to a relatively common view in Sweden, at least at the time, which complied with a norm that made the outsourcing of domestic services partly surrounded by a stigma, as it could represent re-establishment of a society of servants and masters, contrasting to a trend in the 20th Century towards more egalitarian values.⁸

The number of individuals with registered purchases was 46,000 in 2007 and then almost doubled each year and was 325,000 in 2010. Couples with children and older individuals were particularly attracted, with half of the individuals aged above 55. Survey data presented by Statistics Sweden (2011) indicate that cleaning was by far the most frequently purchased service (89 percent), with lawn mowing or snow shoveling being the second most common service (8 percent). Among married couples where the female partner was aged between 25-55 years, the numbers with a tax discount increased from 14,239 in 2007 to 29,075 in 2008 and about 54,000 in both 2009 and 2010, representing about 8 percent of all married women in the aged of 25 to 55 years. It seems plausible that several explanations combined to produce the gradual increase in the demand for domestic services. First, the supply of domestic services grew during this time. Survey data presented by Statistics Sweden (2011) indicate that about 60 percent of the firms claiming tax discount reimbursements in 2010 did not exist prior to 2007. In addition, most households stated that they did not previously purchase domestic services whereas 6-10 percent previously purchased the services from the informal market (Swedish Tax Agency 2011). Second, according to economic theory, the amount of services purchased is related to the price elasticity. However, the decision process may be sluggish with different short-term and long-term demand elasticities, where one would expect families with the highest marginal utility of domestic services to have higher short-run demand elasticity. Third, there may have been network effects (or “demonstration effects”) as relatives, colleagues and friends might spread information about their experiences with firms, on how to purchase the services or on how the tax discount works.

⁸ A similar proposal from 1993 was followed by a turbulent debate which has been given its own historical name (*pigdebatten*). The heated debate preceding the 2007 reform is often referred to with this label.

Fourth, plausibly relevant for the case of Sweden 2008-2010, Baxter, Hewitt and Western (2009), emphasize that attitudes toward using paid domestic services could generate important differences between households in outsourcing of housework.⁹ The left wing critique gradually toned down and their demand to abolish the reform was abandoned in 2011, first by the Green party and later in the same year also by the Social Democratic party. It is consistent with that the reform first changed behaviors and then social norms (Hook 2010). While it is difficult to document a shift in social norms, one may at least state that in Sweden it would not be controversial to claim that one occurred.

3.2 Data and sample

This study is based on register data from LISA (*Integrated database for labor market research*), administered by Statistics Sweden, and include the total Swedish population aged 16 years and older for the period 2000 to 2010. The data includes detailed information on individuals' incomes, e.g., annual labor earnings, family disposable income and various social insurance benefits related to parental leave, sick-leave, unemployment insurance and social welfare. The LISA register data has been merged with records from the Swedish Tax Agency on tax discounts for purchases of domestic services.

Tax discounts may be attributed to either adult individual in a household. This means that the husband might apply for the tax discount even though it is the wife who gets time set free by purchase of domestic services. We therefore limit our sample to married individuals as it enables us to identify partners in the same household. Since labor supply is our outcome of interest, we also limit the sample to individuals aged 25-55. However, tax discounts made by partners outside this age-range are included.

⁹ Households' norm change is a potential confounding factor if it is correlated with e.g. earnings levels. However, for our empirical model to be biased it must be correlated with *changes* in earnings, which must occur in the same year as norms change, and conditional on our very rich set of covariates. See further Section 4.

There is little information on the amount of domestic purchases prior to the reform, but we need to make some assessment of to what extent these purchase should be seen as changes in time constraints. The 50 percent tax discount made a legal purchase roughly equivalent in price to acquiring the services from the black market. There are thus incentives from both buyers (legal) and sellers (social security) to make the transaction legally. If this is what happens, one would expect it to occur in 2007. Our samples 2008-2010 are conditioned to have had zero tax discounts until 2007, 2008 and 2009 respectively. If the transition from illegal to legal occurs gradually across years, the problem would decrease and be smallest for the 2010 sample

We use log annual earnings as our outcome variable, thus a generic measure comprising both hourly wages (effort) and work hours. One may expect the demand for purchasing domestic services to be larger among the highest earners and the relatively affluent middle class. Using log earnings compresses the earnings distribution and reduces the risk that high earnings increases for relatively few individuals drive positive results. On the other hand, changes from small absolute values may translate into very large percentage (log) changes. We therefore exclude individuals with annual earnings below SEK 100,000 (approximately € 11,000) in $t-1$. This restriction reduces the number of women who outsourced domestic work by 15 to 20 percent.¹⁰ We also condition our sample in year t to have zero tax discounts until $t-1$. This makes our female samples with first time tax discounts mutually exclusive between years, constituting 13,446 individuals in 2008, 23,765 in 2009 and 10,307 in 2010.

Table 1 displays some selected descriptive characteristics of married women in households with and without tax discounts for domestic services, referred to henceforth as treated and untreated respectively. Stars indicate that the treated average is significantly different from the average of

¹⁰ Including these households would increase our estimates in the empirical section by one or two percentage points (with placebo tests, described in Section 4, insignificant throughout).

untreated women aged 25-55 in the particular year, almost in all cases with p -values below .0001 (not displayed). Since the untreated characteristics remain relatively stable, Table 1 only shows the average characteristics of untreated in 2008 while the characteristics of the treated category is presented for 2008, 2009 and 2010. The average amounts are slightly higher in the 2008 sample. The top rows then show the treated proportions within four different intervals of tax discounts which range from corresponding to on average less than 10 hours per year to a month of full time work (discussed further in a moment).¹¹ Women in treated households are on average one or two years younger, have .10 more children at home, have completed one more year of schooling, are less likely to be employed in the public sector and have higher individual labor earnings. These households are also characterized by substantially higher family disposable incomes. Earlier studies of domestic services and labor supply have often focused on high skilled women. In the present sample, about 60 percent of the treated women in 2008 had a three year college degree. This fraction was just above 50 percent among treated in 2009 and 2010, compared to around 30 percent of the non-treated. Treated women lived in Stockholm to a higher extent than women in households not outsourcing domestic services. This overrepresentation decreased from 37 percent in 2008, compared with 20 percent of the non-treated, to about 30 percent in 2009 and 2010. The decrease may reflect that firms first were more quickly established in the Stockholm area, but it may also be that attitudes towards outsourcing domestic work shifted first in the capital and later in other parts of Sweden.

The composition of households using the tax deductions varied somewhat across years. The fractions above the 95th percentile in annual earnings decreased from 22.3 percent in 2008 to 14.0 percent in 2009 (measured in year prior). Table 2 displays other characteristics, separately between groups with different amounts of tax discounts. Interesting aspects include that the pres-

¹¹ The high frequency of relatively small amounts of tax discounts is explained by the effect of timing on annual tax discounts. If households decide continuously to purchase domestic services of, say, SEK 1,000 per month, only the ones starting in January or February will have tax discounts exceeding SEK 10,000 and those starting in August or later will be found in the lowest interval. Of the full population with positive tax discounts in 2010, the lowest interval represented 46 percent.

ence of a child aged below seven does not seem to affect the amount of tax discounts, once it exceeds SEK 5,000 (our lowest interval). Also, the average amount of tax discounts for the highest interval is substantially higher in 2009 than in surrounding years.

To interpret the tax discounts as a proxy for hours of domestic services, a survey directed to households with tax discounts in 2010 is informative (Swedish Tax Agency 2011). Survey data indicate that the average tax discount for an hour of domestic services is SEK 175. Further, among individuals aged 18-64, 25 percent stated that each purchased hour of domestic services saved them three hours or more and two thirds said it saved at least 1,5 hours. A rudimentary average from these answers suggest that each hour of outsourced domestic services correspond to on average 1.8 hours of time set free. This would imply that the interval SEK 5,000 – 9,999, with average discounts of about 7,000, would represent roughly 40 hours of domestic services per year (7,000 divided by 175), but that the hours set free may be about 70 hours (1.8 x 40 hours). For the interval 10,000-14,999, the average tax discount of about SEK 12,000 implies some 70 hours of domestic services per year (we disregard that reduced travel costs may make prices cheaper for families purchasing a large number of hours). Taking the survey average of freed time literally would imply that the time constraint of this group on average was relaxed by 126 hours (70 x 1.8), i.e. more than three weeks of full time work. For the groups with above SEK 15,000, the average tax discount varies between 2008-2010 from 21,000 in 2008, 29,000 in 2009 and 24,000 in 2010. The number of hours outsourced would then hover around 150 hours. Multiplying this number by 1.8 would imply time set free representing well over one month of full time labor market work.

4 Empirical strategy

The reform generated large variation across years in household behavior. Available register data provides us with very precise information each year on individual annual earnings and each household’s tax discount. For our purposes, these are interesting aspects, but as with the studies referred to in Section 2, the decisions to outsource housework following a reform are not randomly assigned but instead systematically related to household and individual characteristics. To estimate if and how relaxed time constraints influence annual earnings, we use difference-in-differences propensity score matching (Rosenbaum and Rubin, 1983, Smith and Todd 2005). Below, we describe the underlying assumptions of this approach before turning to the empirical implementation in subsection 4.2. We discuss potential sources of bias in subsection 4.3.

4.1 Difference-in-difference propensity score matching

Assume annual earnings of individual i at time t , Y_{it} , is a function of the number of hours of purchased domestic services h_{it} , such that $Y_{it} = f(h_{it})$. Taking differences between time periods t and $t-1$ gives $\Delta Y_{it} = f(\Delta h_{it})$. We denote a treatment indicator variable D_{it} equal to 1 if $\Delta h_{it} > 0$ and 0 otherwise. With superscripts 1 and 0 denoting treated and untreated respectively, the “treatment effect” of interest to us is given by the difference-in-difference outcome (superscript T for treatment)

$$\Delta Y_{it}^1 - \Delta Y_{it}^0 = \Delta^T_{it}$$

The idea behind propensity score matching is to generate treated and untreated samples which are identical (matched) on observable characteristics. Conditional on the observable individual characteristics X_{it-} observed prior to time t and Y_{it-} , ΔY_{it} is assumed independent of the decision to take treatment D_{it} .¹² Formally, $\Delta Y_{it} \perp D_{it} | Y_{it-}, X_{it-}$.

¹² That is, the outcome ΔY_{it} must be independent of the *decision* to outsourcing housework in year t (although the household services themselves may eventually influence annual earnings).

If this conditional independence function holds, Rosenbaum and Rubin (1983) showed the important result that it also holds for some function of the covariates such that

$$\Delta Y_{it} \perp D_{it} | P(Y_{it-}, X_{it-})$$

P is the so called propensity score, i.e., the probability that $D_{it} = 1$. Adding the assumptions that the treatment does not affect untreated outcomes, and the probability of treatment must be strictly positive and smaller than one, Δ_{it}^T provide an unbiased estimate of the average treatment effect on the treated (ATT).

4.2 Empirical implementation

The propensity score is used to match treated individuals' with comparable untreated individuals. Thus, to empirically derive Δ_{it} , the outcome of the treated ($\Delta Y_{it}^1 | D_{it} = 1$) is observable whereas the counterfactual outcome ($\Delta Y_{it}^0 | D_{it} = 1$) is not and instead estimated with ($\Delta Y_{it}^0 | D_{it} = 0$). Propensity score matching is based on selection on observables but, as our outcome variable is the difference in log earnings, defined as $\Delta \ln(Y_{it}) = \ln(Y_{it}) - \ln(Y_{it-1})$, we also control for time consistent unobserved individual characteristics (fixed effects).

For each treated individual, we use one-to-one matching to construct the counterfactual state. This minimizes bias at the cost of less precision. In the empirical section, we therefore also present robustness checks where matching is based on the closest four matches.

One advantage with the matching approach is that one explicitly controls the weighting scheme, to compare comparable individuals, and that it allows for an estimate of the ATT even in the case of effects being heterogeneous across individuals. The quality of the matching procedure is

checked by balancing tests of the treated and the matched comparison group. Table 1 gives an account of these tests for the samples of married women in 2008, 2009 and 2010 (columns to the far right). Note that we condition all individuals to have behaved identically up and until time t with $\hat{h}_{it-} = 0$. Due to space limitations, Table 1 only presents selected covariates. In total, the tests cover age (31 categories), number of children at home (6), age of children (6), education (10), profession (15), sector of employment (7), rural or metropolitan area (2), different kinds of social insurance benefits related to unemployment (UI), sick-leave, as well as study allowances and social welfare, applying both continuous measures in amounts of SEK and dummy variables (incidence of the various benefits) in 2000 and in year $t-1$.¹³ Further, treated and untreated persons are also balanced every year from 2000 until $t-1$ in terms of levels and incidence of parental leave benefits, average family disposable income and annual earnings (which means we also control for earning trajectories). The disposable income and earnings variables also include the proportions above various percentile levels in each year from 2000 until $t-1$ (99th, 95th, 90th, 75th and 50th) and the respective average disposable incomes of these subgroups. The number of variables in our balancing tests thereby exceeds 200 in each matching process. The probit regressions are based on selected variables and their interactions. These results are not displayed for reasons of space (available from the authors on request).¹⁴

Each estimate presented in Section 5 is based on a probit regression and an ensuing balancing test which fulfill the requirements as stated here. To explore the variation in the intensity of treatment, the change in the household's approved tax discount, $\Delta\hat{h}_i$ is used to approximate the

¹³ Parents are entitled to 12 months of parental leave benefits equal to 80 percent of the previous earnings level, or a minimum transfer of about € 600 a month net of taxes. Most of the parental leave is used before the child is two years old (Ekberg *et al.* 2005). One concern is the possibility that our results would be biased upwards by women returning to work after child rearing. Hence, we balance treated and untreated individuals on the number of children in 2000 and in $t-1$, on the age of children in 2000 and in $t-1$, and on parental leave every year (both trends and levels) from 2000 until $t-1$ (i.e. at least seven years).

¹⁴ Including many non-significant explanatory variables may deteriorate the balancing properties (Caliendo and Kopeinig 2008). Explanatory variables with poor predictive power (p-value above .20) are therefore excluded from the probit regressions except if they are necessary for the balancing test to hold.

true value of Δh_{it} . We remain agnostic about the more exact relation between $\Delta \hat{h}_{it}$ and ΔY_{it} and test the null hypothesis for four intervals of $\Delta \hat{h}_{it}$ (as presented in Table 1). Positive effects on earnings may occur smoothly with the amount of purchased domestic services, e.g., if the probability of accepting extra work hours gradually increases, or with threshold effects if individuals make discrete decision on whether to increase work hours or not, potentially generating "jumps" in the relation between $\Delta \hat{h}_{it}$ and work hours (Becker 1985, p34-35).

4.3 Interpreting the estimated effects

We presume increases in female earnings primarily reflect changes in work hours, but we do not rule out that it may enable a higher effort to enhance hourly wages. Disregarding these differing mechanisms, an estimated treatment effect always includes indirect effects. For example, it is possible that domestic services decrease stress and the number of days of sick-leave, which in turn increases work hours. It may also be that an increase in work hours occurs through the complementarities between housework outsourcing and e.g. leaving a child greater independence (needing slightly less care), by reducing the time for cooking meals, or some other form of change which relax time constraints.

Potential bias in our estimates may occur if some confounding factor is related to both earnings and housework outsourcing. Given that our observables are very rich and that our difference-in-differences outcome variable takes into account unobserved fixed effects, the main threat to a causal interpretation comes from unobserved dynamic effects, e.g., systematic differences in earning trends.¹⁵

¹⁵ Experimental data is something of a gold standard to derive treatment effects. However, non-experimental evaluations based on high quality data have typically been difficult to reject on the grounds of endogeneity (Heckman et al. 1999 (ch 10), Heckman and Smith 1999, Glazerman 2003, Smith and Todd 2005, Diaz and Handa 2006).

To deal with positive earnings trends, “placebo” estimations are conducted by assuming treated received tax discounts in $t-1$, i.e., one year earlier than they actually did. For the 2008 sample, the “placebo” difference-in-differences estimates concern the earnings change 2006-2007 (instead of 2007-2008), with explanatory variables collected from 2006 and earlier (instead of from 2007 and earlier). This estimate should be insignificantly different from zero if our methodology has managed to control for relevant characteristics determining earnings trends.

However, our estimates may still be upward biased if the decision to outsource housework is caused by an increase in earnings in year t (i.e., an income effect which increases the demand for domestic services). In the absence of detailed data on the mechanisms behind the decisions, it is not possible to rule this out but it would require two assumptions to hold; a) an increase in wages and/or work hours must occur independently of our control variables: b) the short run demand for domestic services must be income elastic. We earlier observed that decisions to purchase domestic services 2007-2010 were characterized by sluggishness, which is at odds with the latter assumption. Note also that an increase in work hours is likely to be linked to relaxed time restrictions in the first place, which would *decrease* the demand for domestic services.¹⁶

If the approved tax discounts fail to account for purchased domestic services in $t-1$, either if this service was purchased from the informal sector or if there was no application for a tax discount, our estimates should be regarded as lower bounds.¹⁷ Our rich set of covariates also serve to con-

¹⁶ Alternatively, an unforeseen increase in work hours, e.g. following promotion or on the request from the employer, may be associated with purchases of domestic services. In such a case, it is unclear if the housework outsourcing is necessary to perform the extra hours of labor market work (no bias) or whether the extra hours would have been put in anyway, even without the domestic services (upward bias).

¹⁷ In the aftermath of the reform, purchases of domestic services from the informal sector were only reduced by between 6 and 12 percent (Gavanas and Darin Mattson, 2011; Swedish Tax Agency, 2011). The price of domestic services in the informal sector seem to have been strongly reduced, possibly because of a lower willingness to pay informally for the services or because these services are now performed by illegal immigrant residents with lower reservation wages.

trol for h_{it} and h_{it-1} and correct for such measurement errors in $\Delta\hat{h}_{it}$. An additional source of downward bias is if outsourcing leads to more labor market work hours which potentially would not be picked up in the earnings measure in the following year, e.g. if individuals are on fixed (monthly) salaries.

5 Results

Our results from one-to-one propensity score matching, and one-to-four matching as robustness checks, are presented in Tables 3, 4, 5 and 6. To simplify the discussion, the specific estimates we refer to below are the one-to-one matching estimates unless otherwise stated.

The results for the sample of married women are presented in Table 3. The six columns to the left contain estimates of the 2008, 2009 and 2010 samples respectively whereas the six rightmost columns present the “placebo” estimates for each of the samples, which we expect to be insignificantly different from zero (this is also the case throughout). If we first consider the full samples of households outsourcing, the average estimates imply a positive impact on annual earnings of 4.8 percent (2008 sample), 1.3 percent (2009 sample) and .6 percent (2010 sample), with the last insignificantly different from zero. The decreasing estimates plausibly reflect both that the average tax discount was higher in 2008 sample (by about SEK 1000 – or 6 hours of outsourcing), but also heterogeneity in treatment effects as we expect those with a higher marginal utility of domestic services to have a higher short-run price elastic demand. For instance, women expecting threshold effects on labor working hours following the time set free may react more quickly to the reform. In terms of an overall effect on the population of married females, these estimates represent very small earnings increases, less than .1 percent even if one generalizes the effect to all who received a tax discount. Cascio (2009) reported a price elasticity of child care on female labor supply between -.22 and -.79. Our estimates imply elasticities below -.01, since the fractions

reacting to the tax discount reform are less than 10 percent. If we instead only consider the treated, and assume a 50 percent price reduction, the ATT indicates a price elasticity of domestic services on female labor supply of about -0.10 in 2008, of -0.04 in 2009 and of -0.02 in 2010.¹⁸

To analyze subgroups, we separate treated according to intervals of tax discounts. Our lowest two intervals concern tax discounts below SEK 5,000 and SEK 5,000-9,999, where the average tax discounts correspond to on average of about 10 hours and 40 hours of domestic services in a year (dividing by SEK 175 the average tax discounts of about SEK 2,000 and SEK 7,000 respectively). One should also interpret these in light of that one hour of paid work on average represents tasks requiring 1.8 hours if conducted by the purchaser (Swedish Tax Agency 2011). The higher of these two intervals would then represent 70-80 freed hours, or two weeks of full time work. As for the estimates, they are for the lowest interval small and statistically insignificant except in 2008 (2.5 percent). For the groups with tax discounts of SEK 5,000-9,999, the point estimates across years are significantly positive; 6.6 per cent 2008, 5.1 percent 2009 and 4.6 percent for the 2010 sample. To assess these in more general terms, the estimates associated with 40 hours of outsourced domestic services are on average 4 percent in 2010 and 4.4 if one takes the average from 2009 and 2010, implying roughly one percent from 10 hours of services.¹⁹

Turning to the two highest intervals, tax discounts of SEK 10,000-14,999 correspond to around 70 hours outsourced per year, and thus about 125 hours released. Results pertaining to this group are also significant and positive for all three samples, 7.2, 6.0 and 8.9 percent. These point estimates indicate only a weak increase compared with the preceding group in 2008 and 2009, but

¹⁸ E.g. for 2008; $(4,5/-50)$. If domestic services were above zero in $t-1$, part of the change in tax discount merely represents a replacement of pre-existing services. If the prior purchases were legal, the price reduction implies an income elasticity on the labor supply (positive or negative), with our elasticity a hybrid between the price and the income elasticities.

¹⁹ The average of all three years is 5.2 percent. Since the highest estimates, for both intervals, are recorded in 2008 and then gradually decreasing across years, the validity of this result may be limited.

with a clearer incremental effect in 2010.²⁰ Taking the average estimates of this interval and the preceding one, they differ by roughly three percent, which one may set in relation to the difference in average hours of outsourcing, which should be around 30 hours (70 hours minus 40 hours). Thus, it is once more relatively close to a relation where 10 hours outsourced on average produce a one percent earnings increase. In contrast, the point estimates of the groups with the highest tax discount interval, SEK 15,000 or above, are a couple of percentage points lower, 5.6, 4.1 and 5.9 percent, with the last (2010) estimate insignificantly different from zero. A possible interpretation is that these households already purchased domestic services from the informal sector in $t-1$ and that our data and empirical model fail to control for this. However, one would then perhaps not expect the decrease in point estimates to remain across years despite that, as displayed in Table 2, the average tax discounts vary from corresponding to approximately 120 hours (2008) to 170 hours (2009). Also, the amount of tax discounts is strongly correlated with annual earnings and disposable income in 2008, but while this relation is almost dissolved in 2009, and weakly re-appears in 2010, the pattern in the estimates persists across all the three years.²¹ The “cap” in earnings effects may instead reflect that outsourcing becomes more likely to be transformed into leisure rather than labor market work as the number of hours set-free approaches a month of full time work. If each hour outsourced releases 1.8 hours to a purchasing household, already the interval with 70 hours outsourced have on average about 125 hours set free.

Tables 4 reports estimates where treated are separated into two groups depending on whether there is at least one child below the age of seven in the household. The magnitudes are overall more modest if there is no child aged 0-6 in the household. The statistical power of the one-to-

²⁰ The incremental effect between these two groups is also slightly clearer for the subsamples in Table 4.

²¹ Compared with the interval with tax discounts of SEK 10,000-15,000, annual earnings $t-1$ of the 2009 sample are lower through the whole distribution (which is contrary to 2008 sample, 21 percent higher, and 2010, 8 percent higher) and the distribution of disposable income is wider with incomes at percentiles 1 through 50 lower but incomes for the 75th to the 99th percentile higher.

one estimates is weak but the one-to-four matching estimates confirm the same impression with improved precision. It is expected since the gender gap in housework to the disadvantage of women is largest among couples with children (see Section 2.1). It is interesting to note that the gap in estimated effects between the intervals outsourcing 40 hours and 70 hours differ by 3-4 percent, again implying something relatively close to one percent earnings increase per 10 hours of outsourcing.²²

Table 5 presents results pertaining to treated males, i.e. husbands in treated households. While the results partly indicate positive effects on male earnings, the placebo estimates are in several cases significantly different from zero.²³ The impression is that it is very uncertain whether we capture effects of time gains or selection effects. When we separate the men into samples with and without children below seven at home, we find that males in 2008 and 2009 with tax discounts above SEK 10,000 or above SEK 15,000 are associated with positive earnings increases and insignificant placebo estimates. While one should be cautious about interpreting these findings, it is nevertheless interesting to note that males appear more affected by outsourcing of domestic services when there are children aged below seven in the household. On the one hand, this may be expected in the case of Sweden as males are the beneficiaries of 22.3 percent of days of parental leave. On the other hand, a general pattern reported in the literature is that women tend to increase their time in housework when becoming mothers, but the time men spend in housework barely changes with the transition into parenthood (for Australia, Sweden and Germany, see Baxter, Hewitt, and Haynes, 2008; Boye, 2008; Kühhirt, 2011).

²² Household averages in the respective intervals are very similar whether with or without a child aged below seven.

²³ Note that for our estimates pertaining to females, the empirical model did control for family disposable income. Failing to control for husbands' earnings increase may cause upward bias in the female estimates if it provides means for the wife to earn more (e.g. if an extra car is bought to facilitate home to work transit), but no bias if the extra earnings induces the household to purchase domestic services which in turn influence female earnings (that is the effect we are after). It could also be a reason for downward bias if the husband performs fewer tasks at home. One can imagine all three scenarios, at least to some extent, are present in our data. However, we do not consider it a threat to the overall implications of estimates pertaining to females, as this would require very strong assumptions.

The estimates we report are likely to at least partly reflect increases in total production (GDP). In 2010, six out of ten purchasing households stated that they would not have purchased domestic services if it were not for the tax discount, and six out of ten firms performing the housework were not registered until in 2007 or later (Swedish Tax Agency 2011). However, public interventions which stimulate the domestic service sector may have a number of effects on a society, not least in terms of redistribution effects. A comprehensive assessment is therefore a formidable task which is outside the scope of this study. It remains an open question whether the state resources spent on subsidizing domestic services (which in 2010 amounted to about € 140,000,000) could have been used more efficiently to promote gender equality and female labor market supply for broader groups of the Swedish population (since domestic outsourcing is concentrated to high to middle income households), e.g., by prolonging the opening hours of kinder gardens or increasing home assistance to elderly.

6 Summary and discussion

Previous empirical studies support the hypothesis that decreased housework responsibility increases female labor supply. While many of these analyses are compelling in terms of deriving causal average effects, it has been difficult to establish a more precise functional form between female earnings and the number of hours set free in a household. We use population register data of tax discounts for housework outsourcing to measure changes in households' time constraints, and analyze their relation to individual labor earnings. The results confirm a statistically significant impact of relaxed time restrictions on female earnings, roughly following a rule of thumb that 10 hours of domestic services increases female earnings by one percent, with a "cap" at about 10 percent.

The literature on labor supply effects of child care reforms have shown that the validity of results is important to consider. The seminal work by Gelbach (2002) has been followed by several important contributions to shape our understanding of the results. In the typical case of a reform, the short run price elasticity of domestic services should correlate with the expected marginal utility of households. This implies that estimates pertaining to households with their first tax discounts in 2008 may be larger than for those in 2010. However, a study of Sweden 2007-2010 is interesting as a change in norms likely occurred. A norm change which partly occurs independently of the marginal utility expectations could make the validity of our results more general, and consistent with this claim is that our results pertaining to the highest intervals of tax discounts display no apparent pattern across years.

Regarding gender equality, our results imply that subsidized domestic services may mitigate the gender earnings gap compared with the counterfactual state of no subsidy, *ceteris paribus*. By facilitating changes in behaviors within households, the reform may also change gendered norms (Hook, 2010).²⁴ However, the reform could serve to cement gender roles, e.g. if women perform the purchased domestic services and to the extent that the time set free in households does not affect female labor supply.

²⁴ Grunow et al. (2012) suggest that patterns in the division of housework within couples are established early, and are difficult to change later.

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Table 1. Descriptive averages of married females with and without tax discounts for domestic services in 2008, 2009 and 2010, aged 25-55 in 2008, 2009 and 2010 respectively. Amounts in thousands of SEK (2006 prices), € 100 is approximately SEK 970.

Samples: Earnings at least SEK 100,000 in the year of tax discount

	Treated: year of first tax discounts			Untreated (2008):	Balancing tests of matched comparisons:					
	2008	2009	2010	No tax discount	2008	<i>p</i> -value ^{a)}	2009	<i>p</i> -value ^{a)}	2010	<i>p</i> -value ^{a)}
Average tax discounts	5.7	4.5	4.6							
Tax discounts 1-4999	.615	.757	.715							
Tax discounts 5000-9999	.206	.145	.162							
Tax discounts 10000-14999	.100	.047	.071							
Tax discounts >14999	.078	.050	.052							
Age (<i>t-1</i>) ^{b)}	41.30*	41.51*	42.28*	43.47	41.40	.361	40.61	.177	40.32	.897
Fraction aged 25	.001*	.002*	.001*	.003	.001	.957	.002	.545	.001	.817
Fraction aged 55	.029*	.029*	.035*	.045	.029	.985	.031	.540	.037	.474
No. of children at home ^{b)}	1.67*	1.64*	1.67*	1.55	1.66	.514	1.64	.540	1.66	.436
Zero children at home ^{b)}	.141*	.155*	.152*	.222	.144	.515	.157	.462	.158	.295
Years of schooling	14.39*	13.92*	14.03*	12.78	14.39	.955	13.93	.932	14.04	.847
No upper secondary schooling	.014*	.025*	.021*	.073	.014	.990	.025	.929	.021	.951
Less than 3yrs of upp. sec. sch	.104*	.163*	.142*	.352	.104	.976	.164	.758	.142	.920
At least 3 years of college	.602*	.507*	.523*	.295	.600	.748	.507	.897	.525	.903
Stockholm county ^{c)}	.370*	.299*	.293*	.203	.369	.982	.302	.365	.290	.756
Inland of Norrland ^{c)}	.011*	.013*	.018*	.036	.011	.837	.013	.951	.019	.676
Farming and mining ^{d)}	.005	.003	.004	.004	.005	.964	.004	.711	.004	.955
Construction ^{d)}	.013*	.014*	.014*	.011	.011	.273	.015	.716	.014	.906
Manufacturing ^{d)}	.120*	.096*	.090	.096	.121	.775	.097	.895	.093	.607
Finance, insurance ^{d)}	.266*	.207*	.211*	.116	.265	.934	.210	.408	.212	.742
Other private sector ^{d)}	.200*	.213*	.207	.189	.204	.385	.209	.555	.203	.671
Public sector ^{d)}	.394*	.463*	.471*	.578	.391	.464	.462	.702	.471	.815
Unemployment benefits > 0	.030*	.030*	.026*	.062	.029	.541	.029	.956	.025	.762
Sick-leave benefits > 0	.107*	.112*	.093*	.136	.105	.796	.108	.391	.095	.429
Social welfare benefits > 0	.001*	.003*	.001*	.007	.001	.731	.002	.309	.001	.927
Parental leave benefits t-3	18.1*	15.8*	17.3*	8.1	18.2	.776	15.6	.742	17.1	.694
Parental leave benefits t-2	19.3*	17.8*	16.7*	7.1	19.1	.700	17.5	.587	16.3	.555
Parental leave benefits t-1	15.5*	12.7*	12.2*	4.2	14.7	.267	12.6	.427	11.8	.485

Family disposable income	864.2*	723.7*	792.6*	548.8	46.5	.465	724.5	.931	777.6	.150
– above 99 th percentile	.052*	.028*	.036*	.009	.048	.397	.028	.813	.0322	.189
– above 95 th percentile	.223*	.140*	.176*	.053	.222	.915	.141	.735	.173	.528
– above 90 th percentile	.369*	.247*	.318*	.107	.369	.899	.248	.662	.318	.973
– above 75 th percentile	.670*	.528*	.617*	.293	.676	.358	.531	.602	.625	.257
– above 50 th percentile	.887*	.808*	.880*	.610	.887	.885	.808	.772	.881	.884
Earnings in 2000 ^{b)}	238.8*	193.8*	195.7*	171.5	239.1	.959	195.7	.252	195.6	.913
Earnings t-3 ^{b)}	303.9*	269.0*	286.0*	219.7	304.2	.945	270.9	.405	285.7	.767
Earnings t-2 ^{b)}	326.2*	286.6*	301.7*	236.0	325.2	.650	288.9	.365	302.2	.983
Earnings t-1 ^{b)}	362.5*	313.0*	330.7*	257.0	360.3	.358	315.3	.348	331.4	.980
– above 99 th percentile	.074*	.033*	.035*	.011	.072	.488	.035	.287	.034	.795
– above 95 th percentile	.251*	.150*	.155*	.061	.255	.650	.154	.265	.156	.931
– above 90 th percentile	.383*	.264*	.266*	.128	.390	.416	.268	.504	.270	.600
– above 75 th percentile	.606*	.492*	.500*	.337	.608	.965	.497	.600	.506	.595
– above 50 th percentile	.811*	.744*	.749*	.699	.818	.305	.747	.891	.755	.579
Earnings change $t - (t-1)$	8.3*	9.3*	4.5*	-.3	-	-	-	-	-	-
Log earnings change $t - (t-1)$	-.019*	-.030	-.049	-.050	-	-	-	-	-	-
N	13,346	23,765	10,307	538,102						

^{a)} *t*-test for equality between average of treated and untreated matched comparisons.

^{b)} Balancing tests are always balanced also on age dummies (14 categories), dummies for number of children (six categories) in *t-1* and in 2000, age of children (six categories) in *t-1* and in 2000, dummies for completed years of schooling (10), for employment profession (15), levels and incidence of transfers in 2000 and *t-1* regarding social welfare, unemployment benefits, sick-leave benefits, child allowances, study allowances, average age at immigration, indicator of zero annual earnings in *t-1* and in 2000, average annual earnings and family disposable income each year from 2000 until year *t-1*, the five earnings percentiles and disposable income percentiles each year from 2000 until year *t-1*. For reasons of space, these are not displayed. Complete accounts of these tests are available from the authors on request.

^{c)} The inland of Norrland is a sparsely populated area in the north of Sweden with permanently higher than average unemployment rates. Stockholm County hosts 20 percent of the population, and the overall employment level is higher than in any other region of Sweden.

^{d)} If sector is not reported in 1993, we use the latest reported sector from previous years, back to 1990.

Table 2; Descriptive statistics of treated females, by amount of tax discounts 2008-2010.

FEMALES			
	2008	2009	2010
Average tax discounts (1000s of SEK)			
Tax discounts 1-4999	2.1	1.8	1.8
Tax discounts 5000-9999	7.2	6.9	7.1
Tax discounts 10000-14999	12.2	12.0	12.1
Tax discounts >14999	21.5	30.0	24.6
Fraction of families with disposable income > 99th percentile			
Tax discounts 1-4999	.025	.020	.030
Tax discounts 5000-9999	.055	.043	.043
Tax discounts 10000-14999	.089	.061	.046
Tax discounts >14999	.209	.078	.076
Fraction of families with disposable income > 95th percentile			
Tax discounts 1-4999	.156	.116	.154
Tax discounts 5000-9999	.246	.198	.199
Tax discounts 10000-14999	.350	.241	.250
Tax discounts >14999	.529	.251	.301
Fraction of families with disposable income > 75th percentile			
Tax discounts 1-4999	.682	.490	.665
Tax discounts 5000-9999	.749	.638	.787
Tax discounts 10000-14999	.862	.686	.888
Tax discounts >14999	.912	.632	.902
Annual earnings t-1			
Tax discounts 1-4999	322.2	300.8	319.3
Tax discounts 5000-9999	384.0	347.3	346.5
Tax discounts 10000-14999	436.3	361.7	364.0
Tax discounts >14999	528.5	352.3	392.4
Fraction of families with child at home aged below 7			
Tax discounts 1-4999	.434	.411	.373
Tax discounts 5000-9999	.506	.469	.485
Tax discounts 10000-14999	.512	.473	.480
Tax discounts >14999	.498	.368	.474
Completed college			
Tax discounts 1-4999	.571	.495	.499
Tax discounts 5000-9999	.640	.549	.563
Tax discounts 10000-14999	.667	.581	.617
Tax discounts >14999	.664	.493	.603

Table 3 Married females, one-to-one and one-to-four propensity score matching estimates. Bootstrap standard errors based on 500 replications within parentheses.

Dependent variable: Log earnings difference: $\Delta \ln(Y_{it}) = \ln(Y_{it}) - \ln(Y_{it-1})$.

Sample restriction: annual earnings at least SEK 100,000 in year prior to tax discount.

Year of tax discount Matching algorithm	2008		2009		2010		"Placebo" estimations ^{a)}					
	1:1	1:4	1:1	1:4	1:1	1:4	2008 sample		2009 sample		2010 sample	
							1:1	1:4	1:1	1:4	1:1	1:4
Total sample	.0475 *	.0383 *	.0129 *	.0127 *	.0057	.0087	.0113	-.0002	-.0053	-.0012	.0017	-.0055
	(.0071)	(.0055)	(.0053)	(.0042)	(.0076)	(.0061)	(.0080)	(.0063)	(.0054)	(.0044)	(.0080)	(.0063)
N ^{RUT}	13,367	13,367	23,599	23,599	10,241	10,241						
N ^{COMP} (weighted)		43,656		76,667		36,410						
Tax discounts 1-5000	.0246 *	.0190 *	.0055	.0006	-.0134	-.0101	-.0106	-.0115	-.0005	.0023	-.0072	-.0021
N ^{TR} : 8226/17874/7322	(.0090)	(.0071)	(.0062)	(.0048)	(.0090)	(.0074)	(.0100)	(.0080)	(.0063)	(.0050)	(.0091)	(.0072)
Tax discounts 5000-9999	.0665 *	.0713 *	.0512 *	.0447 *	.0459 *	.0352 *	.0128	.0016	-.0074	-.0062	-.0272	-.0109
N ^{TR} : 2766/3422/1656	(.0152)	(.0115)	(.0133)	(.0105)	(.0187)	(.0141)	(.0178)	(.0139)	(.0143)	(.0113)	(.0207)	(.0172)
Tax discounts 10000-14999	.0774 *	.1000 *	.0596 *	.0595 *	.0893 *	.0858 *	.0061	.0027	.0084	.0186	.0153	.0132
N ^{TR} : 1329/1108/730	(.0207)	(.0150)	(.0244)	(.0183)	(.0258)	(.0194)	(.0237)	(.0189)	(.0233)	(.0193)	(.0293)	(.0233)
Tax discounts > 15000	.0563 *	.0619 *	.0406 *	.0403 *	.0593	.0642 *	.0271	.0281	-.0211	-.0127	.0022	.0315
N ^{TR} : 1044/1188/531	(.0221)	(.0175)	(.0202)	(.0162)	(.0328)	(.0252)	(.0262)	(.0199)	(.0228)	(.0187)	(.0321)	(.0282)

* indicates 95% confidence interval of estimate does not include zero.

^{a)} Difference-in-differences estimates based on years t-1 and t-2, see further Section 3.

Table 4. Married females, one-to-one and one-to-four propensity score matching estimates. Bootstrap standard errors based on 500 replications within parentheses.

Dependent variable: Log earnings difference: $\Delta \ln(Y_{it}) = \ln(Y_{it}) - \ln(Y_{it-1})$.

Sample restriction: annual earnings at least SEK 100,000 in year prior to tax discount.

No child at home aged 0-6 in $t-1$

Year of tax discount Matching algorithm	2008		2009		2010		2008 sample		2009 sample		2010 sample	
	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4
Total sample	.0174 *	.0151 *	.0090	.0070	.0021	.0068	.0032	.0021	.0018	.0010	.0130	.0079
	(.0065)	(.0051)	(.0048)	(.0037)	(.0069)	(.0054)	(.0073)	(.0059)	(.0049)	(.0039)	(.0068)	(.0053)
N ^{RUT}	7,210		13,706		6,119							
Tax discounts 1-5000	-.0048	-.0013	-.0041	-.0024	-.0181 *	-.0124	-.0030	-.0124	-.0009	-.0004	.0066	.0007
N ^{TR} : 4665/10539/4605	(.0080)	(.0065)	(.0054)	(.0043)	(.0077)	(.0065)	(.0094)	(.0074)	(.0055)	(.0044)	(.0076)	(.0058)
Tax discounts 5000-9999	.0249	.0432 *	.0223	.0234 *	.0178	.0185	.0296	.0200	.0154	.0083	-.0043	.0127
N ^{TR} : 1370/1823/853	(.0144)	(.0110)	(.0120)	(.0096)	(.0153)	(.0120)	(.0157)	(.0119)	(.0138)	(.0102)	(.0176)	(.0154)
Tax discounts 10000-14999	.0549 *	.0473 *	.0535 *	.0404 *	.0586 *	.0777 *	.0110	.0192	-.0172	.0252	.0298	.0160
N ^{TR} : 649/585/379	(.0174)	(.0131)	(.0207)	(.0156)	(.0228)	(.0182)	(.0236)	(.0196)	(.0242)	(.0226)	(.0270)	(.0231)
Tax discounts > 15000	-.0098	.0112	.0387 *	.0404 *	.0112	.0385	.0216	.0299	.0149	.0166	.0260	.0294
N ^{TR} : 526/755/278	(.0208)	(.0178)	(.0166)	(.0130)	(.0278)	(.0210)	(.0223)	(.0189)	(.0194)	(.0148)	(.0329)	(.0278)

At least one child at home aged 0-6 in $t-1$

Total sample	.0643 *	.0701 *	.0185	.0242 *	.0113	.0279 *	-.0174	-.0104	-.0071	-.0042	-.0165	-.0059
	(.0135)	(.0106)	(.0110)	(.0088)	(.0165)	(.0131)	(.0151)	(.0122)	(.0118)	(.0095)	(.0172)	(.0139)
N ^{RUT}	6,157		9,893		3,896							
Tax discounts 1-5000	.0409 *	.0371 *	.0030	.0100	-.0059	-.0004	-.0255	-.0161	-.0003	.0117	.0083	-.0206
N ^{TR} : 3561/7335/2711	(.0178)	(.0141)	(.0126)	(.0101)	(.0211)	(.0167)	(.0200)	(.0160)	(.0139)	(.0111)	(.0211)	(.0171)
Tax discounts 5000-9999	.0577 *	.0870 *	.0635 *	.0476 *	.0442	.0525 *	.0015	-.0191	-.0147	-.0071	-.0226	-.0055
N ^{TR} : 1396/1594/796	(.0258)	(.0204)	(.0244)	(.0193)	(.0346)	(.0266)	(.0319)	(.0250)	(.0271)	(.0214)	(.0402)	(.0319)
Tax discounts 10000-14999	.1291 *	.1154 *	.0807	.0832 *	.0915	.1125 *	.0313	.0140	-.0011	.0224	.0258	-.0156
N ^{TR} : 681/523/349	(.0360)	(.0262)	(.0465)	(.0350)	(.0481)	(.0363)	(.0400)	(.0318)	(.0405)	(.0325)	(.0544)	(.0391)
Tax discounts > 15000	.0761	.0671 *	.0582	.0398	.0740	.1117 *	.0536	.0330	-.0131	-.0421	.0323	.0636
N ^{TR} : 517/430/248	(.0392)	(.0308)	(.0481)	(.0388)	(.0523)	(.0347)	(.0430)	(.0342)	(.0576)	(.0457)	(.0580)	(.0471)

* indicates 95% confidence interval of estimate does not include zero.

a) Difference-in-differences estimates based on years t-1 and t-2, see further Section 3.

Table 5 Married males, one-to-one and one-to-four propensity score matching estimates. Bootstrap standard errors based on 500 replications within parentheses.

Dependent variable: Log earnings difference: $\Delta \ln(\hat{Y}_{it}) = \ln(\hat{Y}_{it}) - \ln(\hat{Y}_{it-1})$.

Sample restriction: annual earnings at least SEK 100,000 in year prior to tax discount.

Year of tax discount Matching algorithm	2008		2009		2010		"Placebo" estimations ^{a)}					
	1:1	1:4	1:1	1:4	1:1	1:4	2008 sample		2009 sample		2010 sample	
							1:1	1:4	1:1	1:4	1:1	1:4
Total sample	.0177 *	.0159 *	.0124 *	.0131 *	.0137 *	.0081 *	.0103 *	.0110 *	.0082 *	.0072 *	.0205 *	.0218 *
	(.0042)	(.0034)	(.0034)	(.0027)	(.0049)	(.0038)	(.0044)	(.0035)	(.0032)	(.0025)	(.0046)	(.0036)
N ^{RUT}	14,239	14,239	24,502	24,502	10,376	10,376						
N ^{COMP} (weighted)												
Tax discounts 1-5000	.0096	.0053	.0042	.0053	.0031	.0082	.0121 *	.0079	.0086 *	.0055	.0174 *	.0179 *
N ^{TR} : 8711/18595/7351	(.0055)	(.0043)	(.0039)	(.0031)	(.0056)	(.0044)	(.0053)	(.0041)	(.0037)	(.0028)	(.0054)	(.0042)
Tax discounts 5000-9999	.0332 *	.0379 *	.0307 *	.0340 *	.0190	.0198 *	.0148	.0147	.0010	.0094	.0042	.0182 *
N ^{TR} : 2938/3532/1721	(.0090)	(.0069)	(.0085)	(.0067)	(.0111)	(.0094)	(.0101)	(.0083)	(.0079)	(.0065)	(.0100)	(.0086)
Tax discounts 10000-14999	.0291	.0316 *	.0471 *	.0514 *	.0210	.0206	.0297 *	.0302 *	.0163	.0243	.0178	.0293 *
N ^{TR} : 1450/1127/760	(.0156)	(.0110)	(.0137)	(.0112)	(.0191)	(.0127)	(.0126)	(.0103)	(.0150)	(.0128)	(.0187)	(.0144)
Tax discounts > 15000	.0567 *	.0521 *	.0232	.0213	-.0017	.0256	.0368 *	.0383 *	.0161	.0148	.0176	.0340 *
N ^{TR} : 1130/1242/535	(.0159)	(.0133)	(.0158)	(.0112)	(.0189)	(.0162)	(.0162)	(.0132)	(.0151)	(.0116)	(.0209)	(.0166)

* indicates 95% confidence interval of estimate does not include zero.

^{a)} Difference-in-differences estimates based on years t-1 and t-2, see further Section 3.

Table 6. Married males, one-to-one and one-to-four propensity score matching estimates. Bootstrap standard errors based on 500 replications within parentheses.

Dependent variable: Log earnings difference: $\Delta \ln(Y_{it}) = \ln(Y_{it}) - \ln(Y_{it-1})$.

Sample restriction: annual earnings at least SEK 100,000 in year prior to tax discount.

No child at home aged 0-6 in $t-1$

Year of tax discount Matching algorithm	2008		2009		2010		2008 sample		2009 sample		2010 sample	
	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4
Total sample	.0256 *	.0191 *	.0102 *	.0169 *	.0184 *	.0150 *	.0077 *	.0132 *	.0105 *	.0111 *	.0259 *	.0232 *
	(.0065)	(.0048)	(.0043)	(.0035)	(.0063)	(.0046)	(.0059)	(.0047)	(.0041)	(.0033)	(.0059)	(.0046)
N ^{RUT}	6,697	6,697	12,449	12,449	5,430	5,430						
Tax discounts 1-5000	.0058	.0071	.0118 *	.0088 *	.0130 *	.0150 *	.0088	.0103	.0065	.0074 *	.0156 *	.0296 *
N ^{TR} : 4237/9558/4024	(.0071)	(.0060)	(.0050)	(.0043)	(.0065)	(.0053)	(.0067)	(.0053)	(.0044)	(.0036)	(.0066)	(.0052)
Tax discounts 5000-9999	.0417 *	.0413 *	.0273 *	.0266 *	.0078	.0193	-.0047	.0091	.0319 *	.0285 *	-.0099	.0128
N ^{TR} : 1297/1648/802	(.0127)	(.0099)	(.0121)	(.0084)	(.0135)	(.0126)	(.0148)	(.0128)	(.0118)	(.0096)	(.0146)	(.0133)
Tax discounts 10000-14999	.0125	.0275	.0486 *	.0461 *	-.0013	.0039	.0386 *	.0245	.0177	.0178	.0287	.0312
N ^{TR} : 639/541/350	(.0189)	(.0170)	(.0213)	(.0137)	(.0226)	(.0161)	(.0179)	(.0132)	(.0244)	(.0205)	(.0215)	(.0166)
Tax discounts > 15000	.0358 *	.0395 *	.0185	.0075	.0010	.0045	.0588 *	.0616 *	.0138	.0198	.0159	.0235
N ^{TR} : 517/698/250	(.0225)	(.0176)	(.0190)	(.0144)	(.0291)	(.0219)	(.0229)	(.0179)	(.0183)	(.0148)	(.0311)	(.0265)

At least one child at home aged 0-6 in $t-1$

Total sample	.0187 *	.0124 *	.0161 *	.0130 *	.0023	.0035	.0156 *	.0063	-.0014	-.0001	.0224 *	.0213 *
	(.0062)	(.0049)	(.0051)	(.0041)	(.0076)	(.0061)	(.0066)	(.0051)	(.0048)	(.0039)	(.0075)	(.0057)
N ^{RUT}	7,342	7,342	12,053	12,053	4,946	4,946						
Tax discounts 1-5000	.0082	.0069	.0057	.0064	-.0129	-.0046	.0077	.0150 *	.0040	.0028	.0084	.0116
N ^{TR} : 4474/9037/3327	(.0077)	(.0062)	(.0057)	(.0047)	(.0090)	(.0072)	(.0079)	(.0063)	(.0057)	(.0046)	(.0090)	(.0069)
Tax discounts 5000-9999	.0186	.0364 *	.0367 *	.0326 *	.0222	.0153	.0224	.0281 *	-.0070	-.0092	.0546 *	.0259 *
N ^{TR} : 1641/1883/920	(.0122)	(.0099)	(.0123)	(.0100)	(.0162)	(.0138)	(.0133)	(.0108)	(.0113)	(.0093)	(.0183)	(.0114)
Tax discounts 10000-14999	.0737 *	.0512 *	.0518 *	.0734 *	.0309	.0314	.0352	-.0053	-.0078	.0064	.0255	.0052
N ^{TR} : 808/586/410	(.0214)	(.0146)	(.0213)	(.0177)	(.0242)	(.0192)	(.0211)	(.0150)	(.0210)	(.0156)	(.0259)	(.0213)
Tax discounts > 15000	.0694 *	.0712 *	.0527 *	.0408 *	.0529	.0369	.0188	-.0027	.0138	.0092	.0644 *	.0433 *
N ^{TR} : 613/544/285	(.0244)	(.0193)	(.0225)	(.0177)	(.0322)	(.0223)	(.0216)	(.0184)	(.0224)	(.0178)	(.0282)	(.0204)

* indicates 95% confidence interval of estimate does not include zero.

a) Difference-in-differences estimates based on years t-1 and t-2, see further Section 3.