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ABSTRACT

The Relationship between Hours of Domestic Services and Female Earnings: Panel Register Data Evidence from a Reform[1]

In 2007, a tax discount reform in Sweden reduced prices of outsourced domestic services (ODS) by 50 percent. Unlike most previous studies, population register data enable us to directly link a proxy for relaxed time constraints, annual changes in households' tax discounts, to changes in earnings. We find that 60 percent of married women's freed hours are applied to labor market work, which tapers off when ODS corresponds to approximately three weeks of full time work. This is substantially higher than previously reported estimates. A causal interpretation is supported by "placebo tests", and we carefully outline the assumptions required.

JEL Classification: H2, J13, J22

Keywords: household work, outsourcing, female labor supply

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1 Introduction

In OECD countries, a majority of coupled families are dual earners, with women on average working fewer hours and at lower wages than their partners (OECD 2010). Theory and empirical observations link a major share of women's labor market disadvantages to the fact that they perform most of the routine housework. Several current studies indicate that female labor supply is sensitive to changes in household time constraints due to, for example, increased access to child care, flows of immigrants enhancing the supply of low skilled workers or technological improvements in household appliances (see references in Section 2). These studies typically contain limited information on the more precise relationship between hours saved by outsourcing housework and female labor supply. An improved understanding of the labor supply response to time saved in households, for example through child care (kindergarten, preschool), afterschool activities or elderly care, is relevant to efficiently design policies aiming to increase women's labor supply. From the perspective of gender equality, such measures could improve married women's ability to combine family and career, release skill reserves and mitigate glass ceiling effects, as high skilled women may suffer large earnings penalties from shorter working hours (Albrecht et al. 2003; Bertrand et al. 2010), but also more generally, as most OECD governments share the ambition to increase labor supply.

The aim of this study is to shed light on the quantitative relationship between a relaxed household time constraint and the female labor supply response. We analyze individual register data on the Swedish population for the period 2000-2010. This includes information on households' purchases of domestic services from July 1st 2007, when a tax discount was implemented that reduced the consumer price of outsourced domestic services by 50 percent.[2] The data enable us to

[2] The refund was 50 percent of the charged amount and not related to marginal tax rates. The types of domestic services eligible for tax subsidies were restricted. According to Swedish Tax Agency (2011), a total of 89 percent of the purchases pertained to cleaning, a routine task that may account for a large number of hours over the course of a year. Policies that directly subsidize and stimulate the demand for domestic services (and/or stimulate supply) exist

study changes in households' domestic outsourcing over the period 2007-2010 and link these to individual changes in annual labor earnings. Theoretically, purchased domestic services (in terms of units of time) will be substituted into leisure or labor market work (or other household work). We interpret short-term earnings increases as primarily reflecting additional working hours, although they may also partly involve hourly wage increases.[3] We use propensity score matching to control for selection in purchases of domestic services, accounting for an unusually large set of covariates dating back at least seven years prior to year *t*, the first year in which households received a tax discount for outsourcing domestic work. As for unobservable characteristics, our main concern regards changes in behaviors which correlate with both tax discounts and annual earnings, notably females returning to work after child rearing. To check that our empirical model controls for such trends in earnings, we employ "placebo" estimates of earnings changes between year *t-1* and *t-2* (using controls observed *t-2* or earlier), which we know are unaffected by tax discounts.[4] Because women in general perform more housework than men, we focus on how household time constraints influence women's earnings, but we also present estimates for married men. The results indicate, as expected, a gender difference, with a modest impact of outsourcing on male earnings.

The major contribution of our study lies in the quality of the data. The data allow us: 1) to identify households that outsource housework; 2) to use the annual change in the amount of the a household's tax discounts to approximate the change in the number of hours of outsourced domestic services; 3) to use the annual change in household members' log annual earnings as a proxy for labor supply decisions; 4) to examine the precise relationship between a relaxed house-

in, for example, Belgium (*Titres-services*), Denmark (*Hjemmeservice*), Finland (*Kotitalousvähennys*), France (*Cheque emploie service*), Germany (*Haushaltsnahe Beschäftigungsverhältnisse*) and Spain (*Special Regime*).

[3] Concerning earnings as a measure of work hours, it is most likely substantially more appropriate than self reported hours of work, as it is difficult for an individual to accurately assess a five percent change in an average working week (say, from 20 to 21 hours), unless there is a change in his or her employment contract.

[4] Hypothetically, reverse causality may also drive earnings increases in year *t*, but the assumptions required, as outlined in Section 4.2, arguably make it implausible that this would call the overall implications of our results into question.

hold time constraint and changes in annual earnings. To interpret our estimates, responses to a survey conducted by the Swedish Tax Agency (2011) provide us with information on the average tax discount for one hour of domestic services. Importantly, respondents also state that one hour of outsourcing releases roughly 1.8 hours of time on average. Our estimates indicate that outsourced domestic services tend to increase female log annual earnings. An intuitive relationship emerges between hours of outsourced domestic services and earnings, as a 40-80 hour increase in outsourcing, which represents 2-4 percent of a working year, is also associated with earnings increases of 2-4 percent. Our interpretation of the results is that for a married woman working full-time, 60 percent of the time outsourced is devoted to labor market work. This is substantially higher than what is implied by estimates reported in earlier studies, but these typically cannot separate between households' saving a large number of hours and those saving close to zero hours (e.g. if child-care arrangements already existed). Our estimates tend to decline when the total hours saved exceed one month of full-time work. Given the generous child care policy in Sweden, outsourced hours may then increasingly be substituted into leisure or other housework rather than additional labor market work. Nevertheless, our estimates suggest that housework subsidies might facilitate women's labor market careers.

It is important to assess the external validity of these results. Our estimates are obtained in a setting in which the share of households benefiting from the tax discount increased rapidly from 1.7 percent in 2007 to 9.9 percent in 2010 and where high-quality public child care is universally available at low cost (e.g., Lundin et al. 2008). This may affect the income level at which domestic outsourcing ceases to have an effect on earnings.[5] The households of interest in this study are as expected characterized by relatively high earnings, and presumably a high expected marginal utility of outsourcing housework. However, a change in norms likely occurred in Sweden during the

[5] Child care availability could influence the estimates downwards if there were a decreasing marginal effect of housework outsourcing on earnings. Alternatively, estimates could be influenced upward to the extent that child care is a prerequisite for labor force participation.

period 2007-2010, which dissolved a widespread reluctance among the general public to hire individuals to provide domestic services (Lütz 2011; see also Section 3.1). To the extent that the change in norms is independent of the expected marginal utility of domestic services, this would increase the internal validity of our results.[6] The remainder of the paper provides an account of previous research, while Section 3 describes the 2007 tax reform, the data and the samples studied. Section 4 and Section 5 present the empirical strategy and the results. Section 6 concludes with a discussion.

2 Stylized facts, theory and previous empirical findings

In this section, we begin by presenting some stylized facts regarding gendered patterns in housework from the US, Europe and Sweden. Section 2.2 provides theoretical interpretations of these gendered differences, and we discuss the specific issue of a price reduction in domestic services and its effects on time use. Section 2.3 contains a brief summary of the existing empirical evidence regarding the quantitative relationship between housework outsourcing and female labor supply.

2.1 Stylized facts

It is well-established that women generally perform the majority of all housework (e.g., Treas & Drobnič 2010), with the difference between men's and women's hours of domestic work being largest among couples with children. Longitudinal studies demonstrate that there is little change in men's hours of housework when they become parents, while the amount of time women spend on domestic work increases (Baxter et al. 2008; Boye 2008; Kühhirt 2012). The gender gap in housework has declined over time, primarily because women spend less time on unpaid work and less as a result of an increase in men's time spent on housework (Evertsson & Nermo 2004;

[6] Our study does not aspire to assess the reform *per se* or its overall effect on labor supply. The employees of the firms that provide domestic services are spread across numerous branches, and it is currently not possible to identify their (potentially substantial) increase in the supply of working hours.

Hook 2010, 2006). Geist (2010) employed data from the International Social Survey Programme (2002) and compared men's and women's hours of weekly housework in 35 countries. On average, women spent 19 hours on domestic work, while the corresponding figure for men was 8.5 hours, but there was substantial cross-country variation. Women from Sweden, the other Nordic countries and the U.S. report housework hours that are clustered at the lower end of the housework time distribution. Swedish and American men spent approximately 8 hours per week on housework. More recent data from the European Social Survey (2010) indicate that Swedish females spend approximately 15 hours per week on domestic work compared to 10 hours for males. This gender difference of 5 hours is moderate compared to the figures for many other European countries.

2.2 Theory

There are several plausible explanations for the observed gender gap in housework. One is the theory on *specialization* in the household among family members (Becker 1985, 1991; Mincer & Polachek 1974), which is most often based on the assumption of increasing returns to specialized human capital.[7] Family members then specialize in different activities to maximize household utility. If women devote more hours of effort to intensive housework, it may (i) reduce women's hours of labor market work, (ii) reduce women's wages relative to men through reduced labor market effort, given a similar number of hours of labor market work, and/or (iii) reduce labor market hours *and* wages by altering females' investments in labor market human capital.

The relative resource bargaining perspective provides a second potential explanation and assumes that household work is intrinsically bad and women have weaker bargaining power due to lower income or education relative to their male partners (Blood & Wolfe 1960). This hypothesis has received empirical support (Bianchi et al. 2000; Bittman et al. 2003; Evertsson & Nermo 2004,

[7] Becker emphasizes the role of human capital, but as noted by Pollak (2011), the key assumption that leads to specialization is that spouses' inputs are perfect substitutes.

2007; Killewald & Gough 2010), although some researchers (e.g., Gupta 2007) have claimed that it is the absolute rather than relative resources that are important. Several studies based on cross-sectional data have also argued in favor of the buying out hypothesis (Cohen 1998; Gupta 2007; Treas & de Ruijter 2008), which predicts that women will 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 devoted to housework, suggesting that market substitutes "*play a smaller role in explaining variation in wives' time in household labor than has previously been hypothesized*".

A third explanation is that women are doing gender, i.e., expressing themselves as feminine, when they engage in housework (Bianchi et al. 2000; Fenstermaker & West 2002; West & Zimmerman 1987). Moreover, gender norms could make a neat household more important in demonstrating one's character as a good spouse for women than for men (Ibid.).[8] We will not be able to formally test these different theories, but a possible interpretation is that women will substitute their time into labor market work if the household bargaining or specialization hypotheses are more relevant but into leisure or other housework if preferences or norms explain gender differences in housework. However, behaviors attributable to norms are difficult to disentangle from preferences.

A textbook microeconomic model of household labor supply is very general. A sudden price reduction in domestic services, which falls below the household's marginal value of housework, will induce a household to outsource domestic services. Each additional unit of time saved is then substituted into leisure, labor market work or other housework. The outcome depends on the relative marginal utility of the activities. In the context of our analysis, the utility function may

[8] Interestingly, Evertsson and Nermo (2004), who compare Sweden and the U.S., find that American women tend to increase the time spent on housework if their husbands are economically dependent on them, "*as if to neutralize the presumed gender deviance*". This result could be interpreted as partly supporting a "doing gender" approach.

also include non-monetary incentives for labor market work, e.g., if there is a consumption value in a labor market career or it yields social appreciation or social networks that are of value. Relative to men, one would expect women's labor supply to be more sensitive to outsourcing routine tasks. One may also expect the effects of outsourcing to be heterogeneous, as relative preferences for leisure, housework and labor market work vary across households and individuals, e.g., due to living conditions, life cycle situation and/or prevailing social norms.

2.3 Empirical studies of outsourcing domestic work and female labor supply

Several empirical studies have reported that relaxed time constraints (due to outsourced household duties) affect the labor supply of, primarily, high skilled women and 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 due to reduced child care prices 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 relationship they observe between housework and wages is difficult to explain without partly inferring causality.

Over the last decade, studies exploring changes in 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 focus on a few studies of child care reforms that arguably have reported the most precise quantified estimates of the relationship. A caveat when interpreting the results from these studies is that they concern very different frameworks. Cascio (2009) analyzed Decennial Censuses from 1950 to 1990 to explore variations across U.S. states, and across 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). The effects were limited to single moth-

ers, with no significant effects for married women. In terms of a quantified effect, kindergarten implied child care for most of the workday, and the effect on weekly working hours was estimated at a maximum of 2.8 hours (an 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, while employing slightly different data and empirical approaches, find a significant increase in the female employment rate (7- 8 percent for women with at least one child aged below five), but Baker et al. also report that approximately one third of the effect crowds out private child care that existed prior to the reform. In terms of working hours, Lefebvre and Merrigan find average increases of between 2 and 4.5 hours per week. The estimated effects encompass women from all social classes, thereby also addressing family poverty to the extent that maternal time gains were transformed into labor market work. For Argentina, Berlinski and Galiani (2007) also find an increase in maternal employment following an expansion of preschool for children aged 3-5 years between 1991 and 2001, when child care coverage increased from 49 percent to 64 percent. They quantify the average increase in women's working hours at approximately 3 hours per week.

Existing studies from the Nordic countries have provided less clear evidence of a link between child care and female employment. Havnes and Mogstad (2011) found no effects on female labor supply following an expansion of public child care in Norway between 1976 and 1979. The authors' interpretation is that the households affected by the policy changes to a large extent consisted of dual earner couples that already had child care arrangements. With Swedish data, Lundin et al. (2008) find no effect on female labor supply when exploring a price reduction due to a price cap on child care, introduced in 2002. The price cap came as an addition to an already heavily subsidized child care sector. For Denmark, Simonsen (2010) explored the variation in child care costs across municipalities and found small but significant effects on maternal employment. The

author's assessment is that the result implies substantial overall price sensitivity, as the validity of the result is related to a wealthier subset of the population and the public child care sector in Denmark is also highly subsidized.

There is also a more recent literature exploiting information on immigration flows, which reduce the prices of domestic services and increase housework outsourcing. Cortés and Tessada (2011) find evidence that among women in the top quartile of the wage distribution: increased spending on housekeeping services increases their average hours worked, the probability of working long hours and decreases the time they spend on housework. Hours worked increased by 20 minutes per week (app. 16 hours per year). Cortés and Pan (2013) explore the increase in foreign domestic workers in Hong-Kong during the period 1978-2006 and report an increase in the relative labor force participation of women with children below age five of 10-14 percent compared to women with children 6-17 years old. Farré et al. (2011) analyze Spanish annual register data and find that an immigrant wave increased the labor supply of skilled women (completed college degree) with family responsibilities (small children or seniors present in the home) relative to equally skilled women without such family responsibilities. Calculations suggest that a 10-percent increase in the immigrant population increased high-skilled female labor supply by 15 minutes per week. [9]

Overall, the evidence suggests that relaxed time constraints have a causal effect on female labor supply. [10] In terms of magnitude, estimates of changes in weekly hours worked due to increased access to child care are clustered around 3 hours, implying an average of approximately 10 percent for women working 25-40 hours per week. The interpretation of such an average estimate is not straightforward, as the time saved varies across households, from potentially a substantial

[9] Furtado and Hock (2010) find that immigration reduced the trade-off between fertility and work in the US. See also Barone and Mocetti (2011; Italy), Freire (2013; Singapore) and Freire (2011; Brazil).
[10] We also fully acknowledge the contributions of Greenwood et al. (2005) and Coen-Pirani et al. (2010), who argue that relaxed time constraints following ownership of household appliances contributed to the increase in American women's labor force participation during the second half of the 20th century.

number of hours to close to zero if child-care arrangements already existed. Without information on individual households, it is difficult to draw any inference on the more specific relationship between the reduction in the number of hours devoted to housework and the increase in female labor supply.

3 The reform, data, and sample

In this section, we explain the context of the Swedish tax discount reform in 2007. In subsections 3.2 through 3.4 we present the data, the sample we use for our analyses and provide a detailed description of the distribution of tax discounts among married women.

3.1 The reform

We exploit the variation in household behavior that followed a sudden decline in prices for domestic services. On July 1st 2007, a 50 percent tax discount was implemented in Sweden 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 potential for labor market careers, increase employment and decrease the use of informal labor. However, the reform was highly controversial, with the political opposition arguing that it would exacerbate class-based divisions in society, as it would primarily benefit high-income families, and it was also regarded as a moral or ethical problem, in that one would subsidize "servants" (or "maids") for the wealthy. This argument is related to a relatively common view in Sweden, at least at the time, which complied with a norm that made outsourcing domestic services somewhat stigmatized, representing the re-establishment of a society of servants and masters, contrasting the 20th century trend towards more egalitarian values (Gavanas 2013).[11]

[11] A similar proposal from 1993 was followed by a turbulent debate that was named "the maid debate" (*pigdebatten).* The heated debate preceding the 2007 reform is often referred to with the same label.

The number of individuals who registered purchases of household services in 2007 was 46,000, and this figure nearly doubled each year to reach 325,000 in 2010 (and 492,000 in 2012). The take up of the tax discounts broadened over time but were still primarily used by high-income households and became particularly popular among couples with children and older individuals, with half of the individuals being above 55. Cleaning was the most frequently purchased service (89 percent), with lawn mowing or snow shovelling being the second most common (8 percent) (Swedish Tax Agency 2011).[12] Among married couples where the wife was aged between 25 and 55 years, the number claiming a tax discount increased from 13,321 in 2007 to 27,601 in 2008, 50,512 in 2009 and 91,826 in 2010, representing 9.9 percent of all married women aged 25-55. This increase may partly indicate that the decision making of the households was sluggish and that the availability through the supply of domestic services grew during the period. About 60 percent of the firms claiming tax discount reimbursements in 2010 did not exist prior to 2007 (Swedish Tax Agency 2011). This does not reflect illegal services becoming legal, as survey data indicate that only 6-11 percent of the purchasing households in 2010 had previously purchased these services on the informal market (Ibid.). Third, there may have been network effects (or "demonstration effects"), as relatives, colleagues and friends spread information on their experiences with firms, how to purchase the services or how to claim the tax discount. From July 1st 2009, the administration of the tax discount was facilitated such that the buyer only paid 50 percent of the amount stated on the bill, with the firm requesting the remainder directly from the Swedish Tax Agency. This eliminated the households' reporting burden.

Fourth, a shift in norms likely occurred that mitigated the widespread aversion in Sweden to purchasing domestic services (Lütz 2011). While this is not a controversial statement in Sweden, it is

[12] It may be that respondents only answer the most predominant service purchased. For our purposes, it would not question the relation between hours freed and earnings if purchased services also included e.g. nanny services picking up children from childcare, to enable women to work longer hours, but potentially blur an analyses of the true mechanisms behind our results.

nevertheless difficult to document. Gavanas (2010) provides a useful overview of the debate, describing it as having transitioned from a discussion on inequality and subsidies for rich households to one on labor market integration, where the actual subsidy was less questioned. The critique voiced by the major opposition political parties also moderated gradually, and their demand to abolish the reform was abandoned in 2011, first by the Green Party and followed by the Social Democratic Party. Baxter et al. (2009) emphasize that attitudes toward using paid domestic services could be an important determinant of households' decision to outsource housework. The increased popularity of the tax discount over time is also consistent with, and could serve to support, the view that a shift in norms occurred. Potentially, the reform first changed behaviors and then social norms (Hook 2010).

3.2 Data and sample

The analyses are based on register data from LISA (*Integrated database for labor market research*), administered by Statistics Sweden, and include the entire Swedish population aged 16 years and older for the period from 2000 to 2010. The data contain detailed information on individual incomes, for example annual labor earnings, family disposable income and various social insurance benefits related to parental leave, sick-leave, unemployment and social welfare. The LISA register data were 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 if it was the wife whose time was saved by purchasing domestic services. We therefore limit our sample to married individuals, as this enables us to identify partners within the same household. As labor supply is our outcome of interest, we also limit the sample to individuals aged 25-55, but partners outside this age-range and their claimed tax rebates are included.

We use log annual earnings as our outcome variable and hence a generic measure comprising both hourly wages (effort) and hours worked. One may expect the demand for 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 could drive positive overall results. However, 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* (the year prior to the first tax discount). This restriction reduces the number of women who outsourced domestic work by 15 to 20 percent.[13] Importantly, to facilitate the interpretation of our estimates, we also impose the condition that our sample in year *t* has zero tax discounts until *t-1*. This ensures that all individuals are identical in this respect up and until year *t*. It also makes our samples mutually exclusive across years.

3.3 Descriptive statistics – overall

Table 1 displays selected descriptive characteristics for married women in households with and without tax discounts for domestic services, referred to henceforth as treated and untreated, respectively. Our treated samples of first time tax discounts constitute 13,826 females in 2008, 24,425 in 2009 and 33,382 in 2010. The average disposable income of these households (measured in *t-1*) represented the 93rd percentile in our 2008 sample. This declined to the 86th percentile in the 2009 sample and the 82nd in the 2010 sample. In Table 1, stars indicate that the treated average is significantly different from the average of untreated women aged 25-55 in a particular year, in nearly all cases with *p*-values below .0001 (not displayed). As the characteristics of untreated women remain relatively stable, Table 1 only displays the average characteristics of untreated women in 2008, while the characteristics of the treated category are presented for 2008,

[13] Including these households would increase our log estimates in the empirical section by 1-2 percentage points.

2009 and 2010. The top rows present the proportions of treated women for four different intervals of tax discounts, which range, on average, from corresponding to less than 10 hours annually to one month of full-time work.[14] Women in treated households are on average a few years younger, have completed more schooling and are less likely to be employed in the public sector. They are also characterized by substantially higher individual labor earnings and higher family disposable incomes. Prior studies of domestic services and labor supply have often focused on high-skilled women. In the present sample, approximately 60 percent of the treated women in 2008 had a three-year college degree. This share was just above 50 percent among the women defined as treated in 2009 and 2010, compared to approximately 30 percent of the non-treated. Treated women were also more likely to reside in Stockholm than women in households not outsourcing domestic services. This overrepresentation decreased from 37 percent in 2008, compared to 20 percent of the non-treated, to approximately 30 percent in 2009 and 2010. This may reflect that domestic service firms were first established in the Stockholm area, or that attitudes towards outsourcing domestic work shifted first in the capital and subsequently in other parts of Sweden.[15]

3.4 Descriptive statistics – by tax discount intervals

Our analyses in the empirical section are conducted for each year 2008-2010, and separately for different amounts of tax discounts, divided into intervals of SEK 5,000 (app. €600). Table 2 presents selected characteristics for these groups. To assess the meaning of these intervals, it is useful to consider survey data collected by the Swedish Tax Agency (Swedish Tax Agency 2011).

[14] The high frequency of relatively small amounts of tax discounts is explained by many households only purchasing small amounts, but partly also by the effect of the timing of the first tax discount. This is because if households decide to continuously purchase domestic services and have tax discounts of, for example, SEK 1,000 per month, only those making initial purchases in January or February would have tax discounts exceeding SEK 10,000, and those making initial purchases in August or later will be found in the lowest interval. Of the total population with positive tax discounts in 2010, households in the lowest interval represented 59.5 percent.

[15] As late as in the first half of 2013, tax discounts increased by 25 percent in Norrbotten and Västernorrland, counties with a tradition of left-wing political preferences. The spokesperson for the Swedish Tax Agency explained this in terms of increased social acceptance and individuals receiving information from colleagues and friends regarding how the services can be contracted and how to claim the tax discount.

According to respondents from households with tax discounts in 2010, the average tax discount for an hour of domestic services is SEK 175 (app. €19.00). Further, one hour of domestic services saved at least 1.5 hours for two thirds of the respondents aged 18-64. A rudimentary average based on these responses suggests that each hour of outsourced domestic services corresponds to 1.8 hours of time saved.[16] For simplicity, we assume that the quality of households work is the same whether outsourced or not. This would imply that the interval SEK 5,000 – 9,999, with average tax discounts of approximately SEK 7,000, represents roughly 40 hours of domestic services per year (7,000 divided by 175) and approximately 70 hours are saved (1.8*40 hours). For the interval SEK 10,000-14,999, the average tax discount of approximately SEK 12,000 implies some 70 hours of domestic services per year and that time constraints among this group were relaxed by 126 hours on average (70*1.8), i.e., by more than three weeks of full-time work (we disregard the possibility that reduced travel costs may reduce prices for families purchasing a large number of hours). For the group with tax discounts above SEK 15,000, the averages vary over the period considered from 21,600 in 2008 to above 30,000 in 2009, which represent 125 and 170 hours outsourced. Multiplying this number by 1.8 implies that the time saved is well over one month of full-time labor market work.

Table 2 also provides details on individual labor earnings and family incomes. Once tax discounts exceed SEK 5,000 (our lowest interval), nearly 60 percent are above the 75th earnings percentile in 2010, one in four are also above the 95th percentile and 5-10 percent are above the 99th percentile. The highest percentiles tend to be reached more frequently for individual earnings than for family income. This is consistent with the buying out hypothesis, i.e. that high earning women use their own resources to purchase domestic services. In contrast, the presence of a child under age seven does not seem to affect the amount of tax discounts.

[16] The question asked was: If you or another member of the household had performed work of the same quality as the professional – how many hours would that require compared to that needed by the firm? The responses (and their shares) were less time (.03), as much time (.24), 1.5 times longer (.17), two times longer (.27), 3 times longer or more (.17), and cannot perform the work (.12).

As trends in earnings are a concern, Figure 1 depicts the raw data on the difference in log earnings between each of the groups with tax discounts of SEK 10,000-15,000 and their respective untreated samples. The trajectories are closer to zero for each year, showing that the difference in average earnings between treated and untreated gradually decreased across years. While there is clearly a jump in earnings in the year in which the first tax discount was received, there is also an earnings increase between *t-2* and *t-1* for the 2009 and 2010 samples that could be interpreted as the beginning of a trend. Similarly to the studies referred to in Section 2.3, it is necessary to account for e.g., common trends as decisions to outsource housework are not randomly assigned but are instead systematically related to household and individual characteristics. We now turn to the task of accounting for differences in characteristics between the treated and untreated samples.

4 Empirical strategy

Our empirical strategy exploits that the reform generated substantial variation in household behavior over time, as the number of individuals with tax discounts increased from 46,000 in 2007 to 325,000 in 2010. Available register data provide us with very precise annual information on individual earnings and each household's tax discount. To estimate if and how relaxed time constraints influence annual earnings, we employ difference-in-differences propensity score matching (Rosenbaum & Rubin 1983; Harding 2003; Smith & Todd 2005). Below, we motivate this approach, describe the underlying assumptions (4.1) and discuss potential sources of bias, including earnings trends, black market purchases and reverse causality (4.2).

4.1 Difference-in-difference propensity score matching

Heckman et al. (1999, ch 8.2) characterize three sources of selection bias in evaluation studies: 1) a lack of common support in the explanatory variables between treated and untreated; 2) differ-

ences in the distributions of the explanatory variables; 3) differences in unobservable characteristics. Propensity score matching (PSM) explicitly addresses the first two of these sources of bias, which are related to overlap in the values of explanatory variables. As we are interested in relatively small groups, those of primary interest representing between .2 and .4 percent of the respective total samples, the advantages of PSM are obvious. We assume that the annual earnings of individual *i* at time *t*, Y_{it}, are a function of the number of hours of purchased domestic services; h_{it}, that is $Y_{it} = f(h_{it})$. Differencing between time periods *t* and *t-1* yields

$$Y_{it} - Y_{it-1} = \Delta Y_{it} \approx f(\Delta h_{it}).$$

To empirically analyze the hypothesis that a change in h_{it} influences ΔY_{it}, we denote a treatment indicator variable D_{it} equal to 1 if $\Delta h_{it} > 0$ and zero otherwise (to comply with the evaluation literature, we refer to uptake of the tax discount as "treatment"). Since individuals are either defined as treated or untreated, one of the two states is always unobserved. The aim of propensity score matching is to estimate the unobserved counterfactual outcome of treated. This is achieved by matching treated and untreated individuals on characteristics observed prior to time *t*; X_{it-} and Y_{it-}. To match exactly on each value of the explanatory variables quickly exhausts the number of observations, but Rosenbaum and Rubin (1983) presented the important result that if exact matching on X_{it-} and Y_{it-} controls for selection bias, it also holds for some function of the covariates $P_{it}(Y_{it-}, X_{it-})$, the so called propensity score..Matching is then reduced to conditioning on a scalar, P_{it}, in our case a probit estimate of the probability that $D_{it} = 1$. Formally, our estimated model yields the average treatment effect on the treated (ATT):

$$\Delta_{ATT} = [\Delta Y_{it} \mid D_{it} = 1, P_{it}(Y_{it-}, X_{it-}), h_{it-} = 0] - [\Delta Y_{it} \mid D_{it} = 0, P_{it}(Y_{it-}, X_{it-}), h_{it-} = 0]$$

For each treated individual, an untreated individual with the nearest propensity score value, P_{it}, is selected as comparison. This "one-to-one" matching to construct the counterfactual minimizes bias, but at the cost of precision. In the empirical section, we therefore also present results based on the nearest four matches. Propensity score matching is based on selection on observables, but because the outcome variable compares treated and untreated 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). To further increase comparability in each year, we only compare individuals from households with no history of tax rebates in previous years (i.e. $h_{it-} = 0$).

The "difference in differences" between the treated and their matched counterparts provide an unbiased estimate of the ATT, even if treatment effects are heterogeneous across individuals. The assumptions necessary for a causal interpretation are: (i) the treatment does not affect untreated outcomes; (ii) the probability of treatment must be strictly positive and smaller than one; (iii) conditional on the control variables, the mechanisms behind the decision to take up the treatment D_{it} are independent of expected future earnings. The crucial assumption is (iii), which we refer to as the unconfoundedness assumption (e.g. Gangl 2010, Imbens 2014, further discussed in section 4.2).

To explore the variation in outsourced domestic services, we exploit the change in households' tax discounts, $\Delta\hat{h}_{it}$, to approximate the true value of Δh_{it}. Treatment is defined as the first year in which $\Delta\hat{h}_{it} > 0$, given that $\Delta\hat{h}_{it-} = 0$ up until that point. We verify the quality of the matching procedure using balancing tests of the treated and the matched comparison group. Table 1 provides an account of these tests for the samples of married women in 2008, 2009 and 2010 (columns to the far right). Note that in each respective sample, we condition all individuals to have behaved identically until treatment at 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 in the home (6), children's ages (6), education (10), profession (15), sector of employment (7), rural or metropolitan area (3), different types of social insurance benefits related to unemployment (UI), sick-leave, as well as study allowances and social welfare, applying both continuous measures in SEK and dummy variables (incidence of the various benefits) in 2000 and in year *t-1*.[17] Further, treated and untreated persons are also balanced in each year from 2000 until *t-1* on levels and incidence of parental leave benefits, household disposable income, individual disposable income and individual annual earnings (which means we also control for earning trajectories). The household disposable income and individual earnings variables also include the proportions above various percentile levels in each year from 2000 until *t-1* (99^{th}, 95^{th}, 90^{th}, 75^{th} and 50^{th}) and, in addition, the household disposable incomes of each of these respective subgroups. Each estimate presented in Section 5 is based on a probit regression and an ensuing balancing test (encompassing about 200 variables) that fulfils these requirements. The probit regressions are based on limited set of variables.[18]

Positive effects on earnings may occur smoothly in the amount of purchased domestic services, for example if the probability of accepting additional working hours increases gradually. Alternatively, there may be threshold effects if individuals make discrete decisions regarding whether to increase hours worked, generating "jumps" in the relationship between $\Delta\hat{h}_{it}$ and hours worked (Becker 1985, p34-35). We remain agnostic with respect to the precise relationship between $\Delta\hat{h}_{it}$

[17] Parents are entitled to 12 months of parental leave benefits equal to 80 percent of the previous earnings level or a minimum transfer of approximately €600 a month net of taxes. Most of the parental leave is used before the child is two years old (Ekberg et al. 2013). One concern is the possibility that our results would be biased upwards by women returning to work after child rearing. Therefore, we balance treated and untreated individuals with respect to the number of children in 2000 and in *t-1*, the age of children in 2000 and in *t-1*, and parental leave in each year (both trends and levels) from 2000 until *t-1* (i.e., at least seven years).

[18] These are available from the authors on request. Including explanatory variables with poor predictive power (p-value above .20) may deteriorate the balancing properties (Caliendo & Kopeinig 2008) and are therefore excluded from the probit unless they are necessary for the balancing test to hold.

and ΔY_{it} and test the null hypothesis that $\Delta \ln(Y_{it}) = 0$ for four intervals of $\Delta \hat{h}_{it}$ (as presented in tables 1 and 2).

4.2 Assessing the unconfoundedness assumption

A causal interpretation of our estimates requires that there is no confounding unobserved factor that influences both housework outsourcing and future earnings. To assess the credibility of this assumption, we follow Imbens (2014) to estimate the effects on a pseudo outcome which we know is unaffected by treatment because it is determined prior to the treatment itself. We refer to these as "placebo" estimations, where the outcome variable is the change in earnings $Y_{it-1} - Y_{it-2}$. As an example, for the 2010 sample, the "placebo" difference-in-differences estimates concern the change in earnings from 2008 to 2009 (instead of 2009-2010), using explanatory variables collected from 2008 and earlier (instead of from 2009 and earlier). If our empirical strategy controls for the relevant characteristics determining earnings trends, this estimate should be insignificantly different from zero.

Our estimates may nevertheless be upward biased if the decision to outsource housework is caused by an increase in earnings in year *t*. In the absence of detailed data on the mechanisms underlying the decisions, it is not possible to exclude this possibility. However, it would require two assumptions to hold: a) an increase in wages and/or hours worked must occur independent of our control variables and/or individual fixed effects; b) the demand for domestic services must be income elastic in the short run. Above, we observed that decisions to purchase domestic services in the period 2007-2010 were characterized by substantial sluggishness, which suggests that the demand for outsourcing is not sensitive to short-run changes (which also need to be independent of our control variables). Note also that an increase in hours worked is likely to be linked

to relaxed time constraints, which would *decrease* the demand for domestic services.[19] A change in norms, as discussed above, may to some extent be correlated with future earnings but does not appear likely to be a confounder when conditioning on a large set of covariates. One could also argue that a change in norms serves to mitigate the selection problem to the extent that it occurs independently of earnings changes. It is not possible to analyse this with the data currently available. [20]

A further potential source of bias is measurement errors in our key variables. We presume that earnings increases primarily reflect changes in hours worked but accept that they may, to a lesser extent, also enable a higher effort to enhance hourly wages. If individuals are on fixed (monthly) salaries and lack offers of additional assignments and overtime pay, our estimates may be downward biased and interpreted as lower bounds. Regarding measurement errors in households' outsourcing behavior, households may underreport their purchases either because they purchase services on the black market or due to negligence. These two possibilities have opposing implications for our estimates.

Black market purchases in *t-1* would cause downward bias in our estimates, since it would make us exaggerate the change in time constraints between years. The 50 percent tax discount made a legal purchase approximately equivalent in price to acquiring the services on the black market. There are thus incentives for both buyers (legal) and sellers (social security) to conduct the transaction legally. [21] If this were the case, one would expect the transition from illegal to legal transac-

[19] Following a promotion or request from an employer, an *unforeseen* increase in hours worked may be associated with purchasing domestic services. In such a case, it is unclear whether outsourcing housework is necessary to perform the additional hours of labor market work (no bias) or the additional hours would have been worked regardless, even in the absence of domestic services (upward bias).

[20] Empirically, a change in households' norms could be a confounding factor, e.g., if it is correlated with earnings levels. However, to bias our empirical model it must be correlated with *changes* in earnings, which must occur in the same year as the change in norms, and conditional on our very rich set of covariates. See Section 4.2.

[21] After the 2007 reform, prices on the black market were reduced by half, possibly because of a decline in the willingness to pay for the services informally. Purchases in the informal sector were only reduced by approximately 10 percent, and the black market seems to have remained in parallel to the legal one (Gavanas & Darin Mattson 2011).

tions to occur in July 2007. If it instead occurs gradually over time, any potential downward bias would decrease and be smallest for the 2010 sample. For individuals with tax discounts in 2010, the survey of the Swedish Tax Agency (2011) reported that less than 10 percent previously purchased domestic services on the black market. Finally, while there is a substantial economic incentive to apply for the tax discount, households may nonetheless underreport. Our estimates would then exaggerate the effect of housework outsourcing. From July 1st 2009, households were liberated from the burden of reporting their purchases (see Section 3.1), potentially improving the accuracy. Even so, the increase in tax discounts was similar in 2009 and 2010 and the percentages increase is stable from 2007 until 2012.

5 Results

Our main results, pertaining to married women, are presented in Table 3. The estimates include one-to-one propensity score matching and one-to-four matching. To simplify, the specific estimates we refer to below are the one-to-one matching estimates unless stated otherwise. The six columns to the left contain estimates for the 2008, 2009 and 2010 samples, whereas the six rightmost columns present the "placebo" estimates for each of the samples, which we expect to be insignificantly different from zero. As described in Section 4, placebo estimates apply earnings in *t-1* as outcome variable with no controls for observations recorded later than *t-2*.

We are primarily interested in estimates pertaining to different intervals of tax discounts. The two lowest intervals, below SEK 5,000 and between SEK 5,000-9,999, represent groups with average tax discounts corresponding to approximately 10 hours and 40 hours of domestic services in a year (see Table 2). The estimates are small for the lowest interval and statistically insignificant, except in 2008 (2.1 percent). For those with tax discounts of SEK 5,000-9,999, the point estimates are significant and positive: 8.8 per cent in 2008, 3.7 percent in 2009 and 2.4 percent for the 2010 sample. Hypothetically, the high estimates in 2008 could reflect heterogeneous treat-

ment effects if those with a higher marginal utility of domestic services also had higher short-run price elasticity, but one would then expect higher estimates in 2008 for all intervals. An alternative explanation is that expenses on outsourced domestic services were partly underreported in 2008. This problem should be less severe in 2009, as firms from the 1st of July reported the expenses to receive half of the payment from the Tax Agency, and virtually eliminated in 2010. However, alternative estimates using parental leave as outcome, indicate that treated in 2008 had a lower uptake of parental leave benefits of about SEK 20,000. Our corresponding estimates for 2009 and 2010 are close to zero and statistically insignificant. The model may in 2008 fail to appropriately take into account treated-untreated differences in decisions to take up work earlier after child birth.

Treated women with tax discounts of SEK 10,000-14,999 are associated with significant and positive estimates at 8.5, 5.6 and 7.4 percent. Figure 2 displays trajectories from 2000 and onwards of the log difference between treated women and their respective matched comparisons. There are no trends indicating increased earnings before the first year of the tax discounts. However, the point estimates of the placebo tests in 2009 and 2010 are between 1.8 and 2.6 percent, and although their standard errors are of nearly the same magnitude, a conservative interpretation of our estimates is arguably preferable. One should note that while these placebo tests may suggest that there are dynamic factors that are difficult to capture properly, the individuals driving the positive placebo estimates are not the same individuals as those driving the estimates at time *t*. [22]

Turning to the groups with the highest tax discount interval (above SEK 15,000), these estimates are also positive and significant at 5.3, 4.2 and 4.5 percent. Compared to the preceding interval,

[22] As propensity score matching (PSM) generates one estimate for each treated individual, we conducted an estimate excluding individuals with difference-in-differences estimates above the 95th percentile in the placebos, i.e., excluding the 5 percent of the treated that partly drive the four placebo estimates in 2009 and 2010. Re-estimating the PSM then yields more modest placebo estimates, .0109 and .0039 in 2009 and .0100 and .0041 in 2010, while estimates at time *t* are relatively unchanged, .0490 and .0550 in 2009 and .0705 and .0671 in 2010 (all standard errors are similar to those in Table 3). Of course, this approach does not eliminate the potential upward bias emanating from other individuals.

the point estimates are modest considering that the hours outsourced in this interval are more than twice as high. Possible interpretations include that these women are less interested in increasing their labor supply, or that these households had already purchased domestic services from the informal sector in *t-1* and our data and empirical model fail to control for this, but one would then perhaps expect greater variation in these estimates. As displayed in Table 2, the average tax discounts in this interval vary from corresponding to approximately 120 hours (2008) to 170 hours (2009). It may instead be that there is a "cap" in earnings effects, and that time saved via outsourcing becomes more likely to be allocated to leisure rather than labor market work as the number of hours purchased approaches three weeks of full-time work.

To apply a cautious interpretation, we consider the lowest estimate for each interval. For the 40-hour group, this would leave us with 2.4 percent (in 2010). For the 70-hour group, the lowest estimate is 5.6 percent (in 2009), but we also withdraw 2 percent due to the placebo, thus landing at 3.6 percent. For the highest interval, it would be 4.2 percent (2009). The results may at first seem large, on par with an extra year of education, but there is here an intuitive relationship between the number of hours of purchased services and earnings increases. A standard workweek is 40 hours, which is approximately 2 percent of a working year (1/50), whereas 70 hours equates to 3.5 percent. For the groups outsourcing 40 and 70 hours, one may observe that they are associated with earnings increases in the same order of magnitude, 2.4 percent and 3.6 percent. In addition, survey data indicated that the average hours saved was approximately 1.8 hours greater than those purchased. If married women allocate 60 percent of the time saved to labor market work, the effects on earnings would equal 2.2 and 3.8 percent (2% * 1.8 * .6 = 2.2 and 3.5% * 1.8 * .6 = 3.8), which are similar to the 2.4 and 3.6 percent we reported for these groups. Thus, for a woman working full-time, the results imply that roughly 60 percent of the time saved is devoted to increasing hours worked. This interpretation presumes that the quality of cleaning is the same with and without outsourcing.

Table 4 presents estimates based on absolute values, which assign greater weight to increases among high earners. These results may be more interesting from a policy perspective as an indication of additional tax revenues. The specifications employed are identical to those using log earnings. Specifically, the estimates for 2009 are now higher than those for 2010, and placebo tests are slightly more modest for the 70-hour group. The overall implications are very similar to those in Table 3, including the calculation implying that 60 percent of the time saved is devoted to labor market work for a woman working full-time.

Tables 5 and 6 report estimates from subsamples, in an attempt to discern whether there are heterogeneous effects of relaxed time constraints. Many earlier studies have, for good reasons, focused on high-skill women. In the upper half of Table 5, samples are conditioned on a completed college degree, or no college degree. Other results regard groups with annual earnings below or above the median in the year prior to the tax discount, or if there is at least one child in the household below age seven. Unfortunately, as the samples are smaller, the instability in our estimates from Table 3 is now exacerbated, and we only discern tendencies in the results with few of the patterns stable across 2009 and 2010. The variation in the results notwithstanding, the groups in the highest interval still tend to display point estimates that are similar or below those for the group with fewer hours saved, strengthening the impression from earlier that there might be an upper bound on the earnings effects of relaxed time constraints. It is also encouraging for our framework that for females, the placebo estimates are almost always statistically insignificant, despite certain point estimates being at levels that are slightly disturbing (but also standard errors up to 3-4 percent).[23]

[23] In some cases, notably for the subsample of "below median earnings", low earners exhibiting high percentage increases affect the placebo estimates. If one instead considers earnings in absolute terms, the placebo tests for the 2010 sample with below median earnings (.0462 and .0426 in Table 5) yield estimates of 4,992 (std err. 4,767) and 5,743 (4,767), representing 2.1 and 2.4 percent, while the estimates at time *t* are 16,884 (4,669) and 19,341 (3,806), which represent 7.1 and 8.1 percent, respectively.

The results presented in the lower half of Table 6 concern treated males, i.e., husbands in treated households. For the total sample of treated males, there is no interval in which all six cases (2008-2010) are statistically significant (in Table 3, this was the case for all three intervals), and all the intervals display more modest results relative to those in Table 3. A few of the placebo estimates are also positive and significantly different from zero. [24] When we isolate married men with children below seven in the household, we find that males in 2008 and 2009 with tax discounts in the two highest intervals are associated with positive earnings increases and insignificant placebo estimates. While one should interpret these findings with caution, as they do not hold in 2010, it is interesting to note that males might be affected by outsourcing domestic services to a greater extent when there are children below seven in the household. A general pattern reported in the literature is that women tend to increase the time they devote to housework when they become mothers, but the time men spend on housework changes very little following the transition to parenthood (for Australia, Sweden and Germany, see Baxter et al. 2008; Boye 2008; Kühhirt 2012). In the case of Sweden, the results in Table 6 may be influenced by that males are the beneficiaries of 22.3 percent of total days of parental leave.

6 Summary and discussion

Previous empirical studies have generally struggled to establish a precise relationship between domestic work hours saved and female earnings. We are able to link population register data on tax discounts for housework outsourcing, as a measure of changes in households' time con-

[24] Note that for our estimates pertaining to women, the empirical model controlled for household disposable income, which includes husband's earnings. If one fails to control for husbands' earnings increases, the female estimates may be upward biased if such an increase allows the wife to work more (e.g., if an additional car is bought to facilitate commuting). However, no bias would emerge if the additional earnings induce the household to purchase domestic services that in turn influence female earnings (that is the effect we are attempting to capture). Downward bias may also emerge if the husband performs fewer tasks at home. All three scenarios are plausibly, at least to some extent, present in our data, but we do not consider it a threat to the overall implications of the estimates pertaining to women, as this would require strong assumptions.

straints, to changes in individual labor earnings. The findings imply an intuitive relationship between hours outsourced and increased earnings. The number of hours purchased, 40 and 70 hours, represent approximately 2 and 3.5 percent of a full-time working year. Our estimates imply log earnings increases similar to these percentages. Survey data of the Swedish Tax Agency (2011) indicate that the number of hours released may be about 1.8 times larger than the number purchased. Assuming the cleanliness of the household is the same with or without outsourcing, our results yield an approximate rule of thumb, whereby married women working 100 percent of full-time devote 60 percent of the time saved to labor market work. This is substantially higher than previously reported estimates. The relationship with earnings tends to weaken if total hours outsourced exceed 100 hours, at which point the saved hours represent more than one month of full-time work. The "cap" in earnings effects may to some extent be related to the availability of high-quality child care. Future studies from different countries and contexts are necessary to further explore how women's labor market supply reacts to changes in time constraints.

Regarding gender equality, our results indicate that the gender earnings gap may be mitigated by subsidizing domestic services, relative to the counterfactual of no subsidies. Theory also implies that increased female earnings could, in turn, improve women's bargaining power within the household. If social norms are an important determinant of the gender earnings gap, changes in behaviors within households may, over the longer term, lead to changes in social norms (e.g., Hersch & Stratton 1994; Hook 2010).[25] However, the reform could also serve to cement gender roles as women constitute a majority of domestic services workers or to the extent that the time saved in households does not affect female labor supply.

Public interventions that stimulate the domestic service sector may have a number of effects on a society, not least in terms of redistributive effects. A comprehensive assessment is therefore a

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

formidable task. The state resources spent on subsidizing domestic services in 2010 amounted to approximately € 150,000,000, with outsourcing exceeding SEK 5,000 for 4.5 percent of the married women's households (extrapolation implies a figure of 7.5 percent in 2012). It remains an open question whether it would have been more efficient to promote female labor market supply and/or gender earnings equality by, for example, extending the open hours of kindergartens or increasing home assistance for the elderly.

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Figure 1 Descriptive statistics; treated with tax discounts of SEK 10,000-14,999 and their earnings differences compared with the untreated sample in each respective year.

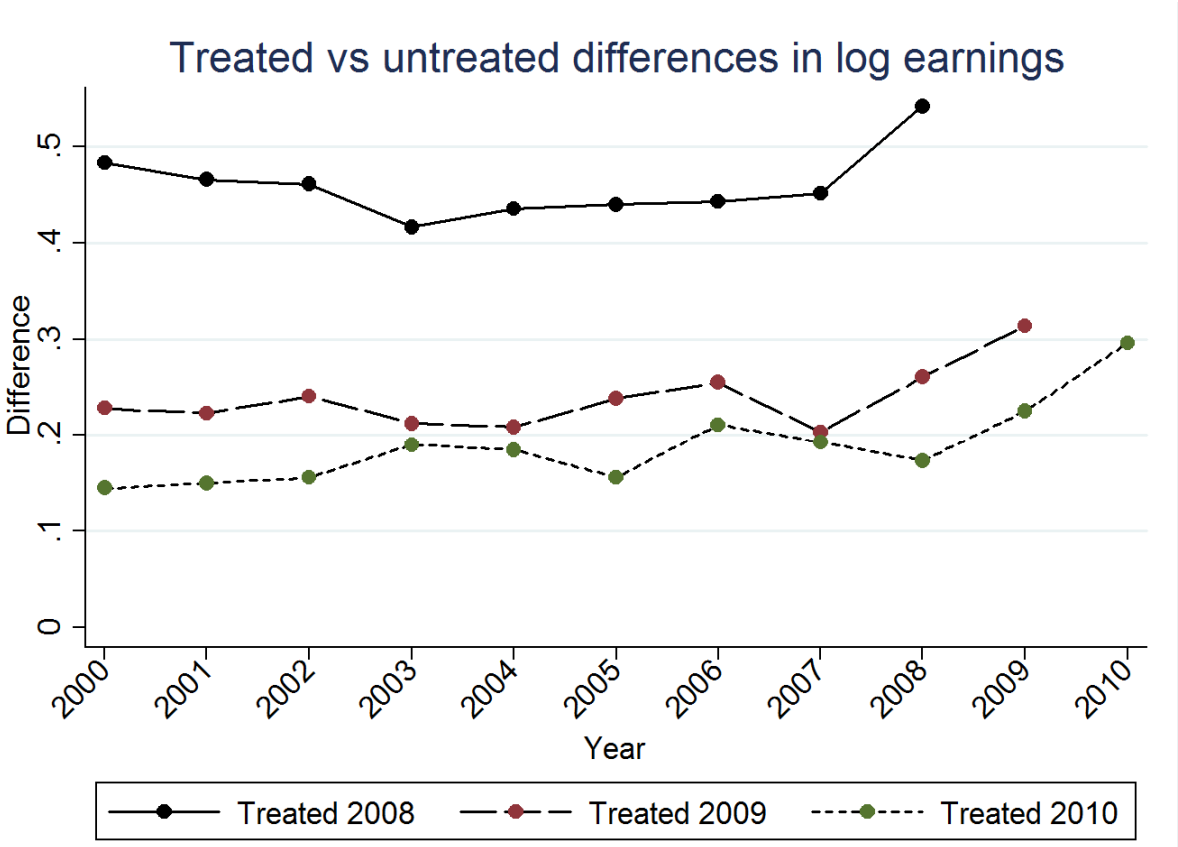
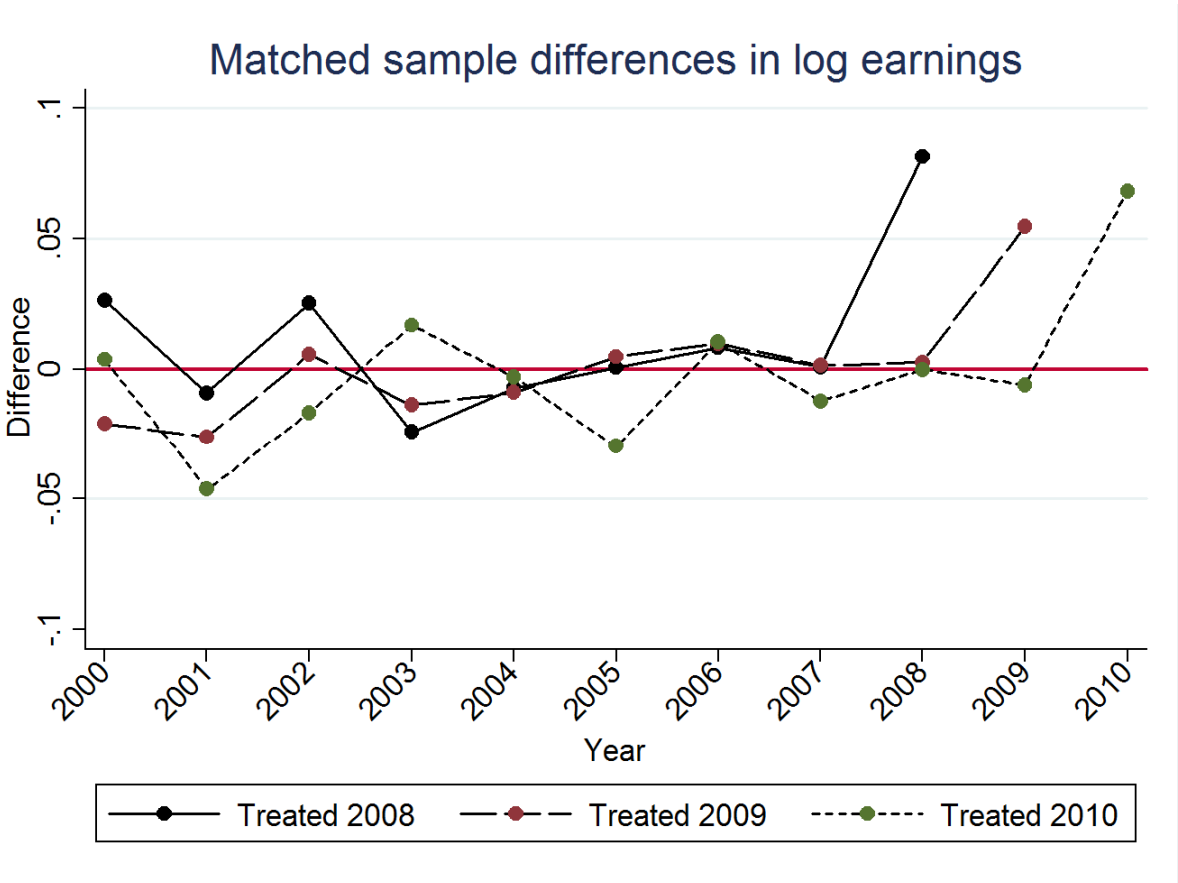


Figure 2 Treated with tax discounts of SEK 10,000-15,000 and matched comparisons.



Note: Confidence intervals are omitted for expositional reasons. There are no statistically significant differences apart from the last year of each trajectory.

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): No tax discount	Balancing tests of matched comparisons:					
	2008	2009	2010		2008	*p*-value[a]	2009	*p*-value[a]	2010	*p*-value[a]
Average tax discounts	5.7	4.5	4.1							
Tax discounts 1-4999	.617	.758	.764							
Tax discounts 5000-9999	.205	.144	.139							
Tax discounts 10000-14999	.099	.047	.056							
Tax discounts >14999	.079	.051	.041							
Age (*t-1*) [b]	41.64*	40.85*	39.83*	43.46	41.80	*.108*	40.94	*.280*	39.93	*.211*
Fraction aged 55	.036*	.037*	.036*	.044	.037	*.847*	.037	*.779*	.036	*.917*
No. of children at home [b]	1.64*	1.61*	1.57	1.55	1.64	*.666*	1.61	*.877*	1.57	*.831*
Zero children at home [b]	.151*	.165*	.180*	.211	.144	*.322*	.168	*.832*	.183	*.333*
Years of schooling	14.36*	13.89*	13.76*	12.79	14.36	*.885*	13.89	*.993*	13.76	*.832*
No upper secondary schooling	.015*	.027*	.029*	.073	.015	*.863*	.027	*.850*	.029	*.778*
Less than 3yrs of upp. sec. sch	.109*	.170*	.181*	.352	.110	*.744*	.170	*.859*	.181	*.866*
At least 3 years of college	.593*	.499*	.477*	.295	.594	*.677*	.499	*.937*	.476	*.556*
Stockholm county [c]	.366*	.296*	.310*	.203	.367	*.866*	.296	*.738*	.308	*.819*
Inland of Norrland [c]	.011*	.013*	.018*	.037	.012	*.799*	.014	*.513*	.019	*.652*
Farming and mining [d]	.005	.003*	.004*	.004	.004	*.805*	.003	*.660*	.003	*.538*
Construction [d]	.013*	.014*	.014*	.011	.012	*.416*	.014	*.916*	.015	*.579*
Manufacturing [d]	.118*	.095*	.085	.096	.118	*.780*	.094	*.563*	.085	*.884*
Finance, insurance [d]	.262*	.204*	.191*	.116	.263	*.738*	.203	*.989*	.191	*.965*
Other private sector [d]	.198*	.212*	.212	.189	. 199	*.848*	.212	*.678*	.213	*.613*
Public sector [d]	.400*	.468*	.490*	.578	.401	*.998*	.469	*.995*	.489	*.673*
Unemployment benefits > 0	.030*	.030*	.034*	.062	.029	*.858*	.029	*.887*	.032	*.965*
Sick-leave benefits > 0	.107*	.113*	.107*	.136	.108	*.637*	.112	*.844*	.107	*.585*
Social welfare benefits > 0	.001*	.003*	.003*	.007	.001	*.670*	.002	*.303*	.003	*.361*
Parental leave benefits t-3	17.6*	15.2*	15.8*	8.1	17.2	*.526*	15.2	*.971*	15.5	*.316*
Parental leave benefits t-2	18.7*	17.2*	16.2*	7.1	18.3	*.432*	16.9	*.540*	15.7	*.224*
Parental leave benefits t-1	14.8*	12.3*	11.7*	4.2	14.4	*.466*	12.1	*.602*	11.4	*.207*
Family disposable income	865.7*	726.3*	697.5*	548.1	838.6	*.186*	716.2	*.234*	695.3	*.584*

− above 99th percentile	.053*	.028*	.023*	.008	.048	*.121*	.027	*.609*	.022	*.860*
− above 95th percentile	.226*	.140*	.116*	.052	.224	*.954*	.141	*.752*	.116	*.999*
− above 90th percentile	.371*	.247*	.224*	.106	.3373	*.656*	.249	*.504*	.224	*.919*
− above 75th percentile	.671*	.526*	.492*	.292	.677	*.294*	.529	*.556*	.494	*.584*
− above 50th percentile	.886*	.806*	.795*	.610	.887	*.715*	.806	*.993*	.797	*.773*
Earnings in 2000 [b]	239.1*	194.9*	180.1*	171.4	239.1	*.894*	195.9	*.584*	181.0	*.595*
Earnings t-3 [b]	304.0*	269.4*	266.7*	219.6	305.3	*.734*	270.0	*.970*	267.3	*.886*
Earnings t-2 [b]	326.0*	286.7*	280.7*	235.9	326.0	*.889*	287.4	*.971*	281.7	*.663*
Earnings t-1 [b]	361.5*	312.4*	309.0*	256.8	360.2	*.534*	313.5	*.771*	310.2	*.612*
− above 99th percentile	.073*	.033*	.023*	.011	.072	*.656*	.035	*.358*	.025	*.311*
− above 95th percentile	.249*	.148*	.119*	.061	.254	*.461*	.151	*.506*	.120	*.814*
− above 90th percentile	.380*	.262*	.220*	.128	.387	*.406*	.263	*.943*	.220	*.721*
− above 75th percentile	.605*	.491*	.445*	.336	.610	*.620*	.493	*.996*	.449	*.625*
− above 50th percentile	.811*	.745*	.720*	.699	.820	*.135*	.748	*.878*	.724	*.614*
N	13,906	24,596	33,621	534,216						

[a] *t*-test for equality between average of treated and untreated matched comparisons.

[b] Balancing tests always confirm balance also on age dummies (30 categories), dummies for number of children (6 categories) in *t-1* and in 2000, age of children (6 categories) in *t-1* and in 2000, dummies for completed years of schooling (10), dummies for employment profession (15) and sector (6), 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 the tests are available 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.

	2008	2009	2010	2008	2009	2010	2008	2009	2010
	Average tax discounts (1000s SEK)			**Hrs outsourced (tax discount/175)**			**Hrs set free (= hrs outsourced x 1.8)**		
Tax discounts 1-4999	2.1	1.8	1.8	12.3	10.3	10.4	22.1	18.5	18.7
Tax discounts 5000-9999	7.2	6.9	7.1	41.0	39.6	40.6	73.8	71.3	73.1
Tax discounts 10000-14999	12.2	12.1	12.1	69.8	69.4	69.4	125.6	124.9	124.9
Tax discounts >14999	21.6	30.2	25.1	122.6	171.0	143.4	220.7	307.8	258.1
	Fraction with individual annual earnings								
	> 99th percentile			**> 95th percentile**			**> 75th percentile**		
Tax discounts 1-4999	.046	.031	.026	.188	.142	.120	.540	.472	.436
Tax discounts 5000-9999	.091	.069	.044	.295	.225	.194	.652	.562	.546
Tax discounts 10000-14999	.144	.077	.055	.410	.270	.229	.723	.590	.580
Tax discounts >14999	.209	.069	.087	.446	.230	.249	.739	.540	.570
	Fraction with family disposable income								
	> 99th percentile			**> 95th percentile**			**> 75th percentile**		
Tax discounts 1-4999	.025	.020	.018	.157	.116	.098	.582	.489	.449
Tax discounts 5000-9999	.057	.044	.030	.250	.196	.142	.750	.634	.596
Tax discounts 10000-14999	.094	.059	.038	.355	.245	.194	.861	.686	.669
Tax discounts >14999	.210	.076	.059	.534	.247	.293	.915	.627	.693
	Annual earnings(1000s SEK) *t-1*			**Completed college**			**Fractions with child aged below 7**		
Tax discounts 1-4999	321.2	300.5	298.5	.562	.488	.459	.419	.397	.373
Tax discounts 5000-9999	383.0	345.8	333.3	.638	.545	.523	.492	.459	.453
Tax discounts 10000-14999	435.2	360.1	348.1	.666	.576	.576	.498	.461	.497
Tax discounts >14999	527.9	350.8	367.7	.663	.481	.528	.482	.353	.450

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.

							"Placebo" estimations [a]					
Year of tax discount	**2008**		**2009**		**2010**		*2008 sample*		*2009 sample*		*2010 sample*	
Matching algorithm	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	.0331 *	.0371 *	.0154 *	.0112 *	.0031	.0085 *	.0102	-.0036	.0013	-.0029	-.0171 *	-.0148 *
	(.0068)	(.0054)	(.0053)	(.0041)	(.0045)	(.0037)	(.0078)	(.0062)	(.0054)	(.0042)	(.0045)	(.0037)
N^{RUT}	13,826	13,826	24,425	24,425	33,382	33,382	13,073	13,073	23,600	23,600	32,582	32,582
N^{COMP} (weighted)	12,843	45,322	22,705	79,528	32,582	102,452	12,255	43,721	22,218	79,059	30,101	104,709
Tax discounts 1-5000	.0210 *	.0241 *	-.0069	.0040	-.0003	-.0025	-.0075	-.0068	-.0010	.0014	-.0154 *	-.0185 *
N^{TR}: 8529/18528/25493	(.0088)	(.0069)	(.0058)	(.0047)	(.0053)	(.0042)	(.0099)	(.0078)	(.0060)	(.0048)	(.0053)	(.0042)
Tax discounts 5000-9999	.0880 *	.0655 *	.0368 *	.0336 *	.0239 *	.0294 *	-.0020	-.0179	-.0084	.0072	.0038	-.0147
N^{TR}: 2845/3508/4626	(.0147)	(.0112)	(.0126)	(.0102)	(.0112)	(.0042)	(.0170)	(.0135)	(.0138)	(.0110)	(.0130)	(.0103)
Tax discounts 10000-14999	.0847 *	.0806 *	.0560 *	.0562 *	.0740 *	.0663 *	.0074	-.0026	.0178	.0265	.0213	.0226
N^{TR}: 1367/1135/1879	(.0207)	(.0145)	(.0248)	(.0180)	(.0173)	(.0133)	(.0229)	(.0188)	(.0235)	(.0187)	(.0184)	(.0139)
Tax discounts > 15000	.0527 *	.0528 *	.0423 *	.0448 *	.0448 *	.0452 *	.0133	.0210	.0151	-.0129	.0097	.0133
N^{TR}: 1083/1254/1372	(.0216)	(.0166)	(.0198)	(.0153)	(.0218)	(.0158)	(.0250)	(.0196)	(.0221)	(.0177)	(.0218)	(.0174)

* 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 Absolute values, married females, one-to-one and one-to-four propensity score matching estimates. Bootstrap standard errors based on 500 replications.

Dependent variable: Log earnings difference: $\Delta Y_{it} = Y_t - Y_{it-}$.
Sample restriction: annual earnings at least SEK 100,000 in year prior to tax discount.

Year of tax discount	**2008**				**2009**				**2010**			
Matching algorithm	1:1		1:4		1:1		1:4		1:1		1:4	
Total sample	9363 *	2.7%	9902 *	2.9%	3137 *	1.0%	3485 *	1.1%	2812 *	.9%	3191 *	1.1%
	(1903)		(1599)		(1013)		(792)		(812)		(653)	
N^{RUT}	13,906		13,906		24,596		24,596		33,621		33,621	
N^{COMP} (weighted)	12,880		45,490		22,806		79,834		30,531		103,140	
Tax discounts 1-5000	6014 *	1.9%	4870 *	1.5%	1261	.4%	876	.3%	-100	-.0%	427	.1%
N^{TR}: 8577/18647/25673	(1690)		(1359)		(1045)		(853)		(891)		(702)	
Tax discounts 5000-9999	15345 *	4.1%	16674 *	4.5%	8831 *	2.5%	10238 *	2.9%	9036 *	2.7%	6933 *	2.1%
N^{TR}: 2856/3540/4663	(5681)		(4474)		(2950)		(2338)		(2365)		(1867)	
Tax discounts 10000-14999	21824 *	5.0%	26464 *	6.3%	30171 *	8.1%	27560 *	7.6%	17119 *	4.9%	20115 *	5.8%
N^{TR}: 1377/1145/1890	(5479)		(4430)		(8923)		(5358)		(3667)		(3037)	
Tax discounts > 15000	-3124	-.6%	8247	1.7%	17212 *	4.9%	12151 *	3.5%	16404 *	4.4%	20820 *	5.6%
N^{TR}: 1094/1249/1383	(14043)		(11434)		(7956)		(4496)		(5089)		(4193)	

"Placebo" estimations [a)]

Year of tax discount	*2008 sample*				*2009 sample*				*2010 sample*			
Matching algorithm	1:1		1:4		1:1		1:4		1:1		1:4	
Total sample	3856 *	1.1%	4226 *	1.2%	246	.1%	316	.1%	-2198 *	-.7%	-2116 *	-.7%
	(1816)		(1601)		(1106)		(882)		(807)		(660)	
Tax discounts 1-5000	-2403	-.7%	-2677	-.8%	255	.1%	20	.0%	-3008	-1.0%	-2318	-.8%
N^{TR}: 8577/18647/25673	(1725)		(1459)		(1013)		(815)		(868)		(708)	
Tax discounts 5000-9999	1638	.4%	3050	.8%	2102	.6%	-2160	-.6%	-1404	-.4%	-2498	-.8%
N^{TR}: 2856/3540/4663	(4007)		(3593)		(4725)		(3486)		(2339)		(1983)	
Tax discounts 10000-14999	8227	1.9%	4101	1.0%	5968	1.6%	8103	2.2%	4042	1.2%	4690	1.4%
N^{TR}: 1377/1145/1890	(6034)		(4981)		(7822)		(6479)		(3889)		(3076)	
Tax discounts > 15000	11875	2.4%	21043	4.5%	2515	.7%	3755	1.1%	2543	.7%	1919	.5%
N^{TR}: 1094/1249/1383	(16282)		(11697)		(6061)		(4461)		(6116)		(5165)	

* 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 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.

							"Placebo" estimations [a]					
Year of tax discount	**2008**		**2009**		**2010**		*2008 sample*		*2009 sample*		*2010 sample*	
Matching algorithm	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4
College degree												
Tax discounts 5000-9999	.0741 *	.0739 *	.0559 *	.0469 *	.0050	.0349 *	-.0007	-.0079	-.0058	-.0107	-.0465 *	-.0410 *
N^{TR}: 1808/1902/2382	(.0188)	(.0149)	(.0186)	(.0150)	(.0167)	(.0140)	(.0230)	(.0183)	(.0205)	(.0160)	(.0197)	(.0166)
Tax discounts 10000-14999	.0639 *	.0792 *	.0522	.0439	.0664 *	.0850 *	-.0226	-.0198	.0209	.0266	.0025	.0218
N^{TR}: 914/649/1081	(.0239)	(.0187)	(.0341)	(.0261)	(.0241)	(.0190)	(.0283)	(.0244)	(.0302)	(.0239)	(.0267)	(.0203)
Tax discounts > 15000	.0457	.0302 *	.0828 *	.0630 *	.0696 *	.0687 *	.0362	.0307	-.0266	-.0126	.0424	.0007
N^{TR}: 718/601/723	(.0278)	(.0208)	(.0324)	(.0242)	(.0270)	(.0217)	(.0322)	(.0236)	(.0359)	(.0300)	(.0348)	(.0248)
No college degree												
Tax discounts 5000-9999	.0755 *	.0698 *	.0095	.0156	.0191	.0206	-.0123	-.0052	.0084	-.0094	-.0018	.0094
N^{TR}: 1030/1581/2139	(.0219)	(.0167)	(.0177)	(.0137)	(.0143)	(.0106)	(.0237)	(.0191)	(.0205)	(.0150)	(.0152)	(.0120)
Tax discounts 10000-14999	.0621 *	.0766 *	.0667 *	.0596 *	.0548 *	.0601 *	.0210	.0398	-.0072	-.0056	.0297	.0327
N^{TR}: 452/482/798	(.0260)	(.0221)	(.0310)	(.0224)	(.0265)	(.0186)	(.0336)	(.0266)	(.0348)	(.0294)	(.0242)	(.0189)
Tax discounts > 15000	.0465	.0345	-.0036	.0133	.0291	.0287	.0168	.0261	-.0090	-.0198	.0276	.0230
N^{TR}: 365/648/645	(.0344)	(.0275)	(.0221)	(.0196)	(.0329)	(.0233)	(.0421)	(.0345)	(.0256)	(.0208)	(.0311)	(.0244)
Above respective treatment group's median earnings in *t-1*												
Tax discounts 5000-9999	.0635 *	.0598 *	.0479 *	.0422 *	.0095	.0122	-.0007	.0006	-.0065	-.0161	-.0303 *	-.0326 *
N^{TR}: 1426/1759/2309	(.0142)	(.0112)	(.0153)	(.0114)	(.0117)	(.0093)	(.0193)	(.0155)	(.0150)	(.0116)	(.0140)	(.0120)
Tax discounts 10000-14999	.0686 *	.0619 *	.0456 *	.0449 *	.0531 *	.0739 *	-.0173	.0171	.0204	.0268	.0085	.0180
N^{TR}: 688/568/941	(.0202)	(.0144)	(.0208)	(.0163)	(.0174)	(.0138)	(.0203)	(.0176)	(.0306)	(.0240)	(.0186)	(.0150)
Tax discounts > 15000	.0648 *	.0463 *	.0151	.0031	.0278	.0258	.0312	.0473 *	-.0196	-.0182	-.0245	-.0244
N^{TR}: 567/632/691	(.0245)	(.0173)	(.0217)	(.0181)	(.0219)	(.0182)	(.0268)	(.0202)	(.0279)	(.0235)	(.0254)	(.0211)
Below respective treatment group's median earnings in *t-1*												
Tax discounts 5000-9999	.0448	.0613	.0367	.0336	.0319	.0387 *	.0195	.0118	-.0178	-.0105	-.0026	-.0045
N^{TR}: 1419/1743/2225	(.0240)	(.0193)	(.0217)	(.0170)	(.0205)	(.0154)	(.0278)	(.0227)	(.0230)	(.0185)	(.0214)	(.0169)
Tax discounts 10000-14999	.1023 *	.0974 *	.1030 *	.0939 *	.0689 *	.0696 *	-.0268	-.0075	.0176	.0269	.0462	.0426
N^{TR}: 678/565/938	(.0309)	(.0247)	(.0419)	(.0320)	(.0291)	(.0229)	(.0414)	(.0334)	(.0385)	(.0291)	(.0315)	(.0238)
Tax discounts > 15000	.0544	.0521	.0475	.0483 *	.0757 *	.0675 *	-.0065	-.0037	-.0144	-.0143	.0321	.0306
N^{TR}: 516/622/681	(.0372)	(.0289)	(.0321)	(.0240)	(.0324)	(.0250)	(.0398)	(.0327)	(.0314)	(.0262)	(.0351)	(.0269)

* 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 females/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.

							"Placebo" estimations [a]					
Year of tax discount	**2008**		**2009**		**2010**		*2008 sample*		*2009 sample*		*2010 sample*	
Matching algorithm	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4	1:1	1:4
Females, no child at home aged 0-6 in *t-1*												
Tax discounts 5000-9999	.0493 *	.0326 *	.0320 *	.0209 *	.0070	.0038	.0084	.0085	.0178	.0051	-.0091	-.0053
N^{TR}: 1449/1902/2528	(.0144)	(.0105)	(.0122)	(.0094)	(.0101)	(.0079)	(.0157)	(.0121)	(.0137)	(.0097)	(.0111)	(.0091)
Tax discounts 10000-14999	.0617 *	.0364 *	.0340	.0538 *	.0568 *	.0432 *	.0115	.0199	.0176	.0244	.0228	.0231
N^{TR}: 686/613/945	(.0167)	(.0119)	(.0186)	(.0153)	(.0184)	(.0126)	(.0253)	(.0204)	(.0254)	(.0206)	(.0190)	(.0133)
Tax discounts > 15000	.0338	.0057	.0434 *	.0306 *	.0344	.0280 *	.0241	.0194	.0343	.0099	.0300	.0201
N^{TR}: 563/816/754	(.0231)	(.0166)	(.0180)	(.0117)	(.0180)	(.0134)	(.0236)	(.0178)	(.0193)	(.0147)	(.0234)	(.0186)
Females, at least one child at home aged 0-6 in *t-1*												
Tax discounts 5000-9999	.1010 *	.1031 *	.0655 *	.0660 *	.0578 *	.0523 *	-.0159	-.0083	-.0189	-.0080	-.0289	-.0342
N^{TR}: 1396/1594/796	(.0267)	(.0204)	(.0249)	(.0195)	(.0216)	(.0169)	(.0311)	(.0251)	(.0265)	(.0213)	(.0256)	(.0205)
Tax discounts 10000-14999	.1223 *	.1395 *	.0628	.0754 *	.0761 *	.0831 *	.0092	.0080	.0360	.0408	.0192	.0274
N^{TR}: 681/523/349	(.0340)	(.0271)	(.0457)	(.0349)	(.0318)	(.0268)	(.0417)	(.0314)	(.0416)	(.0328)	(.0326)	(.0248)
Tax discounts > 15000	.0238	.0680 *	.0428	.0234	.0859 *	.0749 *	.0560	.0330	.0227	-.0457	.0314	.0221
N^{TR}: 517/430/248	(.0337)	(.0300)	(.0456)	(.0379)	(.0391)	(.0304)	(.0444)	(.0338)	(.0597)	(.0454)	(.0422)	(.0314)
Married males												
Tax discounts 5000-9999	.0337 *	.0374 *	.0350 *	.0283 *	.0113	.0146 *	.0073	.0156	.0068	.0131 *	.0157 *	.0177 *
N^{TR}: 2938/3532/1721	(.0092)	(.0069)	(.0083)	(.0066)	(.0072)	(.0058)	(.0104)	(.0084)	(.0086)	(.0065)	(.0070)	(.0056)
Tax discounts 10000-14999	.0319 *	.0190	.0576 *	.0584 *	.0199	.0243 *	.0214	.0331 *	.0136	.0209	.0070	.0033
N^{TR}: 1450/1127/760	(.0139)	(.0107)	(.0146)	(.0116)	(.0119)	(.0099)	(.0130)	(.0101)	(.0157)	(.0128)	(.0124)	(.0093)
Tax discounts > 15000	.0446 *	.0526 *	.0272	.0347 *	.0267 *	.0141	.0562 *	.0413 *	.0281	.0199	.0524 *	.0391 *
N^{TR}: 1130/1242/535	(.0172)	(.0131)	(.0147)	(.0114)	(.0134)	(.0106)	(.0184)	(.0131)	(.0175)	(.0111)	(.0140)	(.0101)
Married males, at least one child at home aged 0-6 in *t-1*												
Tax discounts 5000-9999	.0186	.0364 *	.0367 *	.0326 *	.0221*	.0167 *	.0224	.0281 *	-.0070	-.0209	.0197	.0072
N^{TR}: 1641/1883/920	(.0122)	(.0099)	(.0123)	(.0100)	(.0109)	(.0083)	(.0133)	(.0108)	(.0113)	(.0111)	(.0104)	(.0080)
Tax discounts 10000-14999	.0737 *	.0512 *	.0518 *	.0734 *	.0260	.0396 *	.0352	.0158	-.0078	.0177	-.0160	.0155
N^{TR}: 808/586/410	(.0214)	(.0146)	(.0213)	(.0177)	(.0155)	(.0127)	(.0211)	(.0129)	(.0210)	(.0212)	(.0259)	(.0162)
Tax discounts > 15000	.0782 *	.0745 *	.0576 *	.0493 *	.0258	.0155	.0106	.0095	.0138	.0133	.0644 *	.0541 *
N^{TR}: 613/544/285	(.0262)	(.0194)	(.0235)	(.0177)	(.0195)	(.0160)	(.0241)	(.0186)	(.0224)	(.0218)	(.0282)	(.0193)

* 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.