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**THE RELATIONSHIP BETWEEN HOURS OF DOMESTIC
SERVICES AND FEMALE EARNINGS:
PANEL REGISTER DATA EVIDENCE FROM A REFORM**

by

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**The Relationship between Hours of Domestic Services and Female Earnings:
Panel Register Data Evidence from a Reform¹**

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Abstract

We provide evidence on the quantitative relationship between household time constraints and female earnings. In 2007, a tax discount reform in Sweden reduced prices of outsourced domestic services (ODS) by 50 percent. Linking population register data on yearly tax discounts with annual earnings, we find that 60 percent of married women's freed hours are applied to labor market work, with ODS of approximately 2-4 percent of full-time work in a year being linked to a 2-4 percent earnings increase, but with no additional increase thereafter. The analysis carefully considers potential bias and outlines the required assumptions regarding reverse causality.

Keywords: household work, outsourcing, female labor supply

JEL classification: J13, J22

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1 Introduction

Empirical studies have shown that changes in household time constraints affect female labor supply. Our study sheds light on the quantitative relationship between a relaxed household time constraint and the labor supply response. 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 report average estimates but contain limited information on the more precise relationship between hours saved by outsourcing housework and female labor supply. An improved understanding of the functional form of women's labor supply is relevant to efficiently design policies aiming to increase women's labor supply by relaxing households' time constraints, for example through child care (kindergarten, preschool), afterschool activities or elderly care, as it requires precise predictions of the labor supply response to time saved in households. The topic is important from the perspective of gender equality, as 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.

To quantify the relationship between hours saved by outsourcing housework and female labor supply, we analyze individual register data on the Swedish population for the period 2000-2010, which include 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 study marginal changes in households' domestic outsourcing over the period 2007-2010 and link these to individual changes in annual labor earnings. A textbook theoretical model of household labor supply implies that reducing the price of domestic outsourcing decreases the marginal value of household work and domestic services purchased (in terms of units of time) will be substituted into leisure or labor market work (or other household work). In terms of labor supply, we interpret short-term earnings changes as primarily reflecting additional working hours, although they may also partly involve hourly wage increases.³ The groups of main interest represent around .3 percent of the overall population samples. We therefore use propensity score matching to control for endogeneity 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. "Placebo" estimates of earnings changes in year $t-1$ are employed as a check to ensure that our empirical model controls for trends in earnings – a key assumption for a causal interpretation of our results.⁴

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 as a proxy for 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 household time constraint and changes in annual earnings. To interpret our estimates, responses to a

² The types of domestic services eligible for tax subsidies were restricted. A total of 89 percent of the purchases pertained to cleaning (Swedish Tax Agency 2011), 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 in, for example, Belgium (*Titres-services*), Denmark (*Hjemmeservice*), Finland (*Kotitalousväbenny*), France (*Cheque emploi 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.

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 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 (representing 2-4 percent of a working year) is also associated with earnings increases of 2-4 percent. Our preferred conservative interpretation of the results is that for a married woman working full-time, 60 percent of the time outsourced is allocated to labor market work, whereas for a woman working 50 percent part-time, 30 percent of the time outsourced is devoted to labor market work. When the total hours saved exceed one month of full-time work, our estimates tend to decline. Given the generous child care policy in Sweden, outsourced hours may then increasingly be allocated to leisure or other housework rather than additional labor market work. Nevertheless, our estimates suggest that housework subsidies might facilitate women's labor market careers. 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.

It is important to assess the validity of these results. Our estimates are obtained in a setting in which the number 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 should reasonably be characterized by a relatively high expected marginal utility of outsourcing house-

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.

work, i.e., women with high earnings. However, a change in norms likely occurred in Sweden during the 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 validity of our results.⁶ The remainder of paper is structured as follows. The following section provides 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, and 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, within a microeconomic framework, 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

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.

and less as a result of an increase in men's time spent on housework (Evertsson & Neremo 2004; 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) reveals 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

⁷ Becker emphasizes the role of human capital, but as noted by Pollak (2011), specialization may follow without any assumptions on human capital if spouses' inputs are perfect substitutes.

received empirical support (Bianchi et al. 2000; Bittman et al. 2003; Evertsson & Neramo 2004, 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.⁸ 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.). 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 (allocated to) leisure, labor market work or other housework. The outcome de-

⁸ Interestingly, Evertsson and Neramo (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, see below.

depends on the relative marginal utility of the activities.⁹ In the context of our analysis, the utility function may 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 primarily 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

⁹ Provided there is no household outsourcing prior to the reform, the purchase of domestic services also implies a negative income effect that could in itself stimulate labor supply (see also Section 4.2).

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 mothers, 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), on Norway, and Lundin et al. (2008), on Sweden, find no effect on female labor supply; the former studying the Norwegian expansion of public child care between 1976 and 1979 and the latter exploring a price reduction due to a price cap on child care, which was introduced in 2002. 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. 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 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.¹⁰

Overall, the evidence suggests that relaxed time constraints have a causal effect on female labor supply.¹¹ 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. However, the interpretation of such an average

¹⁰ 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).

¹¹ 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.

estimate is not straightforward, as the time saved varies across households, from potentially a substantial 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. Subsection 3.2 presents the data and the sample we use for our analyses (3.3), including a more detailed description of the distribution of tax discounts among married women (3.4).

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 serv-

ants and masters, contrasting the 20th century trend towards more egalitarian values (Gavanas 2013).¹²

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 shoveling being the second most common (8 percent) (Swedish Tax Agency 2011). Among married couples where the female partner 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. Various mechanisms may explain this gradual increase. First, according to economic theory, the volume of services purchased is related to the price elasticity. If the decision process is sluggish, it may result in different short- and long-term demand elasticities.¹³ Second, the supply of domestic services grew rapidly during this period. Approximately 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 might 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

¹² 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.

¹³ One would then perhaps expect families with higher short-run demand elasticity to have a higher marginal utility for domestic services.

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 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).¹⁴

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 include 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.

¹⁴ 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.

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 tax discounts claimed by partners outside this age-range 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 $t-1$. This restriction reduces the number of women who outsourced domestic work by 15 to 20 percent.¹⁵ Importantly, to facilitate the interpretation of our estimates, we impose the condition that our sample in year t has zero tax discounts until $t-1$. This makes our treated samples containing first time tax discounts mutually exclusive across years, constituting 13,826 females in 2008, 24,425 in 2009 and 33,382 in 2010. Average household disposable income (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.

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, re-

¹⁵ Including these households would increase our log estimates in the empirical section by 1-2 percentage points.

spectively. 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, 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 (these subgroups are discussed further below).¹⁶ 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 treated women 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 high earners are overrepresented, but it may also be that attitudes towards outsourcing domestic work shifted first in the capital and subsequently in other parts of Sweden.¹⁷

¹⁶ 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 on the total annual tax discounts. 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.

¹⁷ 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.

3.4 Descriptive statistics – by tax discount intervals

Our analyses in the empirical section are conducted separately for treated women with 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 implications of these intervals, it is useful to consider survey data collected by the Swedish Tax Agency. According to respondents from households with tax discounts in 2010, the average tax discount for an hour of domestic services is SEK 175 (Swedish Tax Agency 2011). 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.¹⁸ 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 percen-

¹⁸ 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).

tile. 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 earnings 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.

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. 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 IV-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. 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; 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 endogeneity bias in evaluation studies: 1) a lack of common support in the explanatory variables between treated and untreated; 2) differences in the distributions of the explanatory variables; 3) differences in unobservables. 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 compared to the overall samples, 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 $\Delta Y_{it} \approx f'(\Delta h_{it})$. To empirically analyze the hypothesis that $f' > 0$, we denote a treatment indicator variable D_{it} equal to 1 if $\Delta h_{it} > 0$ and 0 otherwise.

The aim of propensity score matching is to generate treated and untreated samples that are matched on observable characteristics X_{it-} and Y_{it-} , observed prior to time t . Formally, let us assume the assumption holds that $\Delta Y_{it} \perp D_{it} | Y_{it-}, X_{it-}$. Rosenbaum and Rubin (1983) presented the important result that it then also holds for some function of the covariates such that

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

where P is the so called propensity score, i.e., the probability that $D_{it} = 1$. For each treated individual, an untreated individual with the nearest the propensity score value is selected as comparison. For each treated individual, we use one-to-one matching to construct the counterfactual. This minimizes bias at the cost of reducing precision. In the empirical section, we therefore also present robustness checks where matching is based on the nearest four matches. The difference

in average outcomes between the treated and their matched counterparts provide an unbiased estimate of the average treatment effect on the treated (ATT), even if treatment effects are heterogeneous across individuals. Propensity score matching is based on selection on observables, but because 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). 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), as it is difficult to technically exclude the possibility that unobservable factors are correlated with both the decision to take up treatment and with the outcome of interest.¹⁹ The rest of this section will describe the approach and section 4.2 considers unobservable confounding factors.

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 we condition all individuals to have behaved identically 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 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 $t-1$.²⁰ Further, treated and untreated persons are also balanced in each year from 2000 until $t-1$ on

¹⁹ Experimental data are ideal for deriving treatment effects. The assessments of program evaluations based on non-experimental, high-quality data have typically been difficult to reject on the grounds of endogeneity (Diaz & Handa 2006; Glazerman et al. 2003; Heckman et al. 1999; Heckman & Smith 1999; Smith & Todd 2005).

²⁰ 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

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 $t-1$ (99th, 95th, 90th, 75th and 50th) and 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 some 200 variables) that fulfils these requirements. The probit regressions are based on limited set of variables.²¹

To explore the variation in outsourced domestic services, we exploit the change in households' tax discounts, $\Delta \hat{h}_i$, to approximate the true value of Δh_i . 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, or with threshold effects if individuals make discrete decisions regarding whether to increase hours worked, potentially generating "jumps" in the relationship between $\Delta \hat{h}_i$ and hours worked (Becker 1985, p34-35). We remain agnostic with respect to the precise relationship between $\Delta \hat{h}_i$ and ΔY_i and test the null hypothesis that $\Delta \ln(Y_i) = 0$ for four intervals of $\Delta \hat{h}_i$ (as presented in tables 1 and 2).

4.2 Interpretation and causality

A causal interpretation of our estimates would not hold if some unobserved factor, which is not captured by our control variables and/or individual fixed effects, influences both housework

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).

²¹ 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.

outsourcing and future earnings. To determine whether our models control for such confounding factors, “placebo” estimations are conducted in which we assume that treated women received tax discounts in $t-1$, i.e., one year earlier than they actually did. 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.

However, our estimates may nevertheless be upward biased if the decision to outsource housework is caused by an increase in earnings in year t (i.e., an income effect that increases the demand for domestic services). In the absence of detailed data on the mechanisms underlying the decisions, it is not possible to exclude this possibility; but it would require two assumptions to hold: a) an increase in wages and/or hours worked must occur independent of our control variables; 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 sluggishness, which is at odds with the assumption regarding short-run income elasticity (which also needs 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.²² 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 strengthen our identification strategy, to the extent that changes in norms occur (conditional on our covariates) independent of earnings changes. It is not possible to analyze this with the data currently available.

²² Alternatively, an unforeseen increase in hours worked, for example following a promotion or request from an employer, 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).

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 due to negligence or because they purchase services on the black market. These two possibilities have opposing implications for our estimates.

Black market purchases in $t-1$ would cause us to exaggerate the change in time constraints between years (bias our estimates downward). 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.²³ If this were the case, one would expect it to occur in July 2007. If the transition from illegal to legal transactions instead occurs gradually over time, any potential downward bias in our estimates (conditional on our rich set of covariates) would decrease and be smallest for the 2010 sample. In 2010, less than 10 percent reported having previously purchased domestic services on the black market (Swedish Tax Agency 2011). 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. The increase in tax discounts was similar in 2009 and 2010 and the percentages increase is stable from 2007 until 2012.²⁴

²³ 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 that black market seems to have remained in parallel to the legal one (Gavanas & Darin Mattson 2011).

²⁴ Underreporting in year t generates upward bias but is counterbalanced to the extent that the same households also underreported outsourcing in $t-1$, which we condition to be zero. If underreporting is more common among wealthy

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 specific estimates we refer to below are the one-to-one matching estimates unless stated otherwise.

The results for the full sample of married women are presented in the top rows of Table 3. 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. The average full sample estimates imply a positive impact on annual earnings of 3.3 percent (2008 sample), 1.5 percent (2009 sample) and .3 percent (2010 sample), with the last being insignificantly different from zero. The shares in the lowest interval of tax discounts increase gradually over time, partly explaining the decreasing estimates. In terms of the overall effect on the population of married females, these estimates represent an earnings increase of less than .1 percent, even if one generalizes the estimates to all females associated with a tax discount.

The two lowest intervals of tax discounts, 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, saving approximately 20 and 70 hours annually (see Section 3.4 and 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 treatment effects if those with a higher marginal utility of domestic services had a higher short-run price elasticity,

households with large amounts of outsourcing, the improved reporting in July-Dec 2009 could cause them to be located within lower tax discount intervals. This would compress the differences in characteristics between groups for 2009. Data on income and earnings in Table 2 present some indications of this.

but one would then expect higher estimates in 2008 for all intervals. A more likely 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.

Treated women with tax discounts of SEK 10,000-14,999 outsource an average of 70 hours annually, and the estimates in Table 3 are significant and positive at 8.5, 5.6 and 7.4 percent. Figure 2 displays trajectories of the difference between treated women and their respective matched counterparts of these groups, in terms in log earnings and the difference-in-difference estimates. Overall, there are no strong trends, at least not to an extent that could explain the increased earnings in 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 .²⁵ Nonetheless, we consider the lowest one-to-one estimates. 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. This interpretation yields a close relationship between the number of hours of outsourced 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.²⁶ However, survey data indicated that the average hours saved was approximately

²⁵ 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 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.

²⁶ Comparing the groups outsourcing 40 and 70 hours, a marginal increase of 30 hours outsourced would save 54 hours (1.8*30), which is approximately 2.5 percent of a working year. In Table 3, the difference in effects between

1.8 hours greater than those purchased, which for these two groups would imply roughly 3.5 and 6.3 percent of a working year, respectively. If we assume that married women allocate 60 percent of the time saved to labor market work, this would imply effects equal to 2.2 and 3.8 percent ($2\% * 1.8 * .6 = 2.2$ and $3.5\% * 1.8 * .6 = 3.8$), which again are similar to the 2.4 and 3.6 percent we arrived at for these groups previously. Thus, for a woman working full-time, this implies that 60 percent of the time saved is devoted to increasing hours worked (or 45 percent if working 75 percent part-time, 30 percent if working 50 percent part-time, etc.). It is possible that the true effect is larger, but we leave that for future research when an opportunity arises to move even closer to a causal interpretation of these types of estimates.

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, there is no evidence that the point estimates increase, despite that the hours outsourced in this interval free the equivalent of five to seven weeks of full-time work. 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. However, one would then perhaps not expect the decrease in point estimates to persist across years despite that, as displayed in Table 2, the average tax discounts in this interval vary from corresponding to approximately 120 hours (2008) to 170 hours (2009). Moreover, the amount of tax discounts is strongly correlated with annual earnings and disposable income in 2008, but this relationship nearly disappears in 2009 before weakly reappearing in 2010. The pattern in the estimates persists across all three years.²⁷ It may be that there is a “cap” in earnings effects, reflecting that time saved via outsourcing becomes more likely

the groups is small in 2008, below 2 percent in 2009 and above 4 percent in 2010 (the average for 2009 and 2010 is 3.2 percent).

²⁷ Annual earnings in $t-1$ among the highest interval in the 2009 sample are lower than the interval containing households receiving tax discounts of SEK 10,000-15,000, which is contrary to the results for the 2008 (21 percent higher) and 2010 samples (8 percent higher).

to be allocated to leisure rather than labor market work as the number of hours saved approaches a month of full-time work.

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 above, 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. Unfortunately, the instability in our estimates from Table 3 is now exacerbated, as the samples are smaller. We therefore refrain from interpreting the estimates individually and instead focus on patterns in the results. First, in the upper half of Table 5, samples conditioned on a completed college degree, or no college degree, are associated with estimates that are relatively similar. Many earlier studies have, for good reasons, focused on high-skill women. These results are interesting in that there does not seem to be any large systematic difference in the treatment effect between the two groups, one they are subject to similar changes in time constraints. Regarding the other results, limiting the comparison to groups with tax discounts of SEK 10,000-15,000, the estimates tend to be higher if annual earnings were below the median in the year prior to the tax discount, or if there is at least one child in the household below age seven, but none of these patterns hold in all years and/or if one includes all intervals in the comparisons.

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. In addition, estimates for the SEK 10,000-15,000 interval generally hover around 6 percent. This group is of primary interest for our purposes, and it is encouraging for our framework that the placebo estimates are statistically insignificant, despite certain point estimates being at levels that are slightly disturbing (but also standard errors up to 3-4 percent).²⁸

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.²⁹ 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 appear to 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 transi-

²⁸ In some cases, notably for the subsample of “below median earnings”, low earners exhibiting high percentage increases affect the placebo estimates. If one considers earnings in absolute terms, the placebo tests for the 2010 sample with below median earnings (.0462 and .0470 in Table 4) 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.

²⁹ Note that for our estimates pertaining to women, the empirical model controlled for household disposable income. Failing to control for increases in husbands’ earnings may generate upward bias in the female estimates if such an increase allows the wife to earn 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. However, we do not consider it a threat to the overall implications of the estimates pertaining to women, as this would require strong assumptions.

tion to parenthood (for Australia, Sweden and Germany, see Baxter et al. 2008; Boye 2008; Kühhirt 2012). In the case of Sweden, bearing in mind that males are the beneficiaries of 22.3 percent of total days of parental leave, the results in Table 6 are, in a sense, as expected.

6 Summary and discussion

Previous empirical studies support the hypothesis that a decrease in housework responsibilities 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 for the relationship between domestic work hours saved and female earnings. We use population register data on tax discounts for housework outsourcing to measure changes in households' time constraints and analyze their relationship with individual labor earnings. The results confirm a statistically significant impact of relaxed time restrictions on female earnings.

Our quantitative assessment reveals an approximate rule of thumb, whereby married women working 100 (50) percent of full-time devote 60 (30) percent of the time saved to labor market work. This interpretation may be slightly conservative and is only valid for households purchasing at least 40 hours of domestic services annually, which saves approximately 70 hours annually. Our results also imply an intuitive relationship between hours outsourced and increased earnings, as 40 and 70 hours represent approximately 2 and 3.5 percent of a full-time working year, yielding estimates similar to these percentages. This relationship 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).³⁰ 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 formidable task. The state resources spent on subsidizing domestic services in 2010 amounted to approximately € 140,000,000, 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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³⁰ Grunow et al. (2012) suggest that patterns in the division of housework within couples are established early and are difficult to change later.

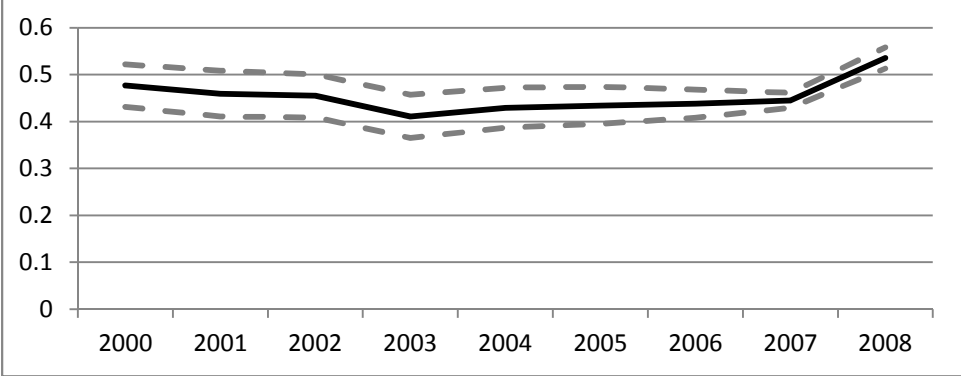
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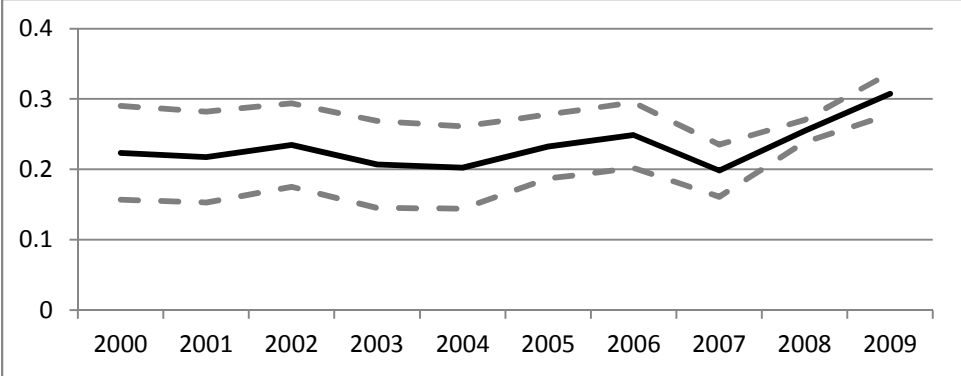
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Figure 1 Raw data descriptive statistics of differences between treated with tax discounts of SEK 10,000-15,000 and untreated sample in each year.

2008 sample



2009 sample



2010 sample

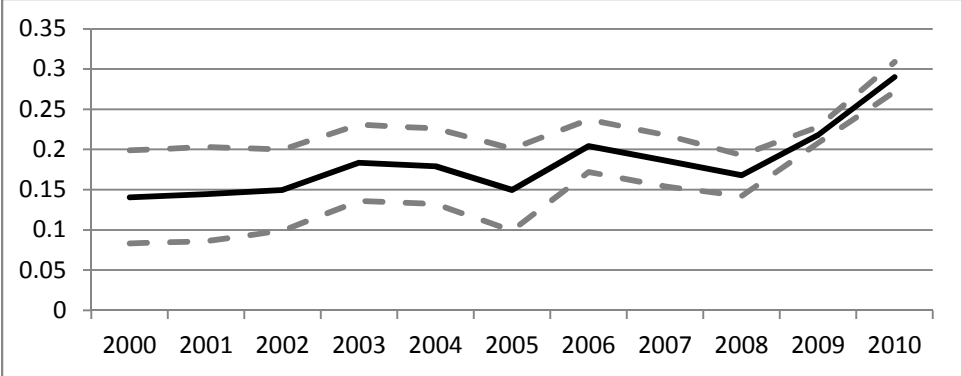
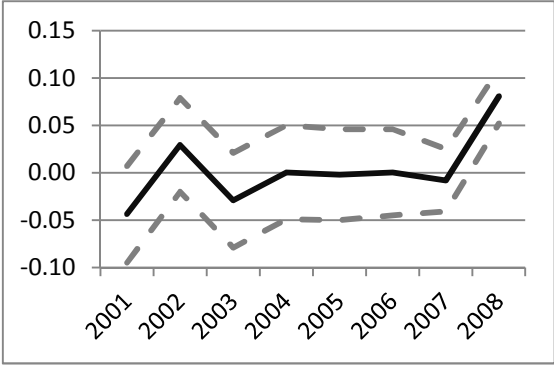
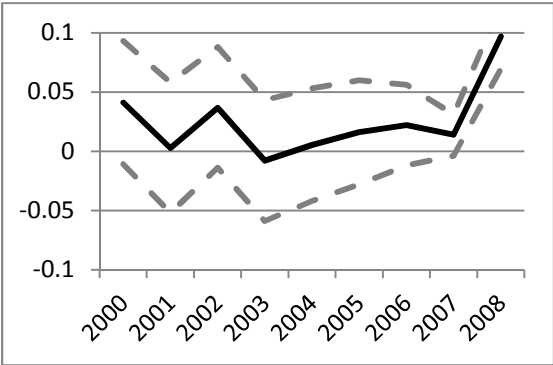


Figure 2 Treated with tax discounts of SEK 10,000-15,000 and matched comparisons.

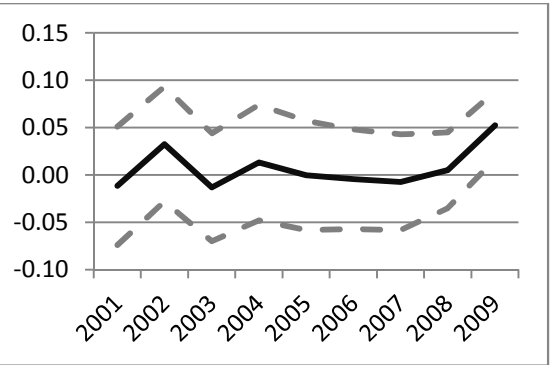
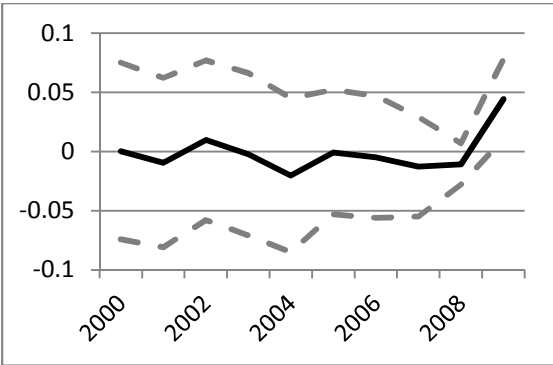
2008 - differences

2008 – difference-in-differences



2009 - differences

2009 – difference-in-differences



2010 - differences

2010 – difference-in-differences

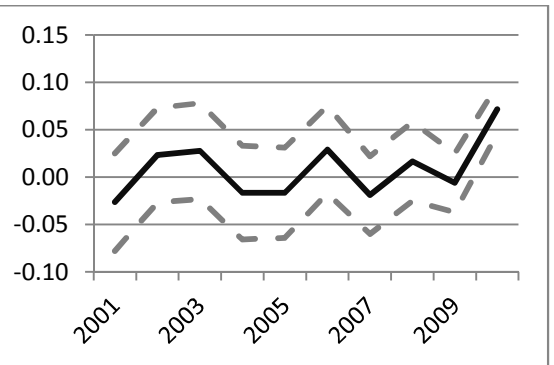
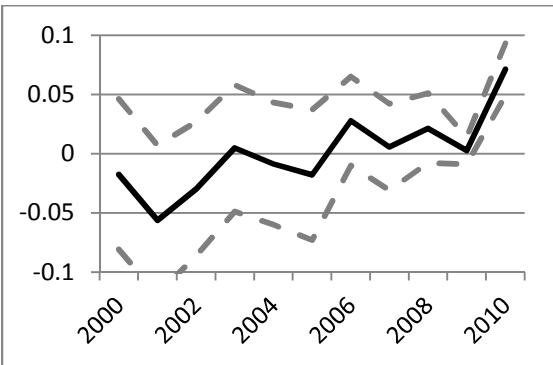


Table 1. Descriptive averages of married females with and without tax discounts for domestic services in 2008, 2009 and 2010, aged 25-55 in 2008, 2009 and 2010 respectively. Amounts in thousands of SEK (2006 prices), € 100 is approximately SEK 970.

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	4.1							
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 (<i>t-1</i>) ^{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 99 th percentile	.053*	.028*	.023*	.008	.048	.121	.027	.609	.022	.860
– above 95 th percentile	.226*	.140*	.116*	.052	.224	.954	.141	.752	.116	.999
– above 90 th percentile	.371*	.247*	.224*	.106	.3373	.656	.249	.504	.224	.919
– above 75 th percentile	.671*	.526*	.492*	.292	.677	.294	.529	.556	.494	.584
– above 50 th 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 99 th percentile	.073*	.033*	.023*	.011	.072	.656	.035	.358	.025	.311
– above 95 th percentile	.249*	.148*	.119*	.061	.254	.461	.151	.506	.120	.814
– above 90 th percentile	.380*	.262*	.220*	.128	.387	.406	.263	.943	.220	.721
– above 75 th percentile	.605*	.491*	.445*	.336	.610	.620	.493	.996	.449	.625
– above 50 th 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) <i>t-1</i>			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.

Year of tax discount Matching algorithm	2008		2009		2010		"Placebo" estimations ^{a)}					
							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	.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 ^{TR}	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: $\Delta Y_{it} = Y_i - Y_{it}$.

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

Year of tax discount Matching algorithm	2008				2009				2010			
	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	-0.0%	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.

Year of tax discount Matching algorithm	2008		2009		2010		"Placebo" estimations ^{a)}					
							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
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, 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
Females, no child at home aged 0-6 in <i>t-1</i>												
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 <i>t-1</i>												
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 <i>t-1</i>												
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.