RP No. 9
SSE Riga/BICEPS Research papers

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November 2021

SSE RIGA

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Million Dollar Baby: Should Parental Benefits Depend on Wages When the Payroll Tax Evasion is Present?[*]

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Abstract

This paper explores the effect of tying social security benefits to declared wages on firm-worker collusion and strategic income reporting before the benefit entitlement. We use administrative data from Latvia covering the entire working population over a 15-year period from 1996 to 2010 to study generous parental benefits, which depend on the reported wage in the time period before the childbirth. Our analysis delivers three principal results. First, we observe a sharp increase in the wage during the time period taken into account to calculate parental benefits, and interpret the obtained result as a collusive legalization of previously unreported income with an aim to increase the future benefit. Depending on the specification, we conclude that during this period the wage on average increases by 5.4%-7.5%. Second, obtained effects are highly heterogeneous. We find that the wage growth is much higher in small firms, where it is presumably easier to sustain collusion between employees and employers. Finally, we demonstrate that legalization of wages is temporary and lasts only until the end of the period taken into account to calculate parental benefits.

JEL Code: H26

Keywords: Payroll tax evasion, employee-employer collusion

[*]We thank Stéphane Straub for a continuous guidance throughout this project. This paper has benefited from conversations with Matteo Bobba, Pascaline Dupas, Nicolas Gavoille, Sergei Guriev, Colin Kuehnhanss, Elena Paltseva, Konstantin Sonin, Giancarlo Spagnolo, Jesper Roine and Gabriel Ulyssea and comments from seminar participants at the Baltic International Centre for Economic Policy Studies, 2nd (2021) SSE Riga/BICEPS Conference on Corruption, Tax Evasion and Institutions, and Toulouse School of Economics. Additionally we would like to thank Sabīna Rauhmane from Latvian Social Security Agency for answering endless inquiries about the data. We gratefully acknowledge funding from the Latvian Council of Science's FLPP research grant "Institutions and Tax Enforcement in Latvia" (INTEL) (grant No. LZP-2018/2-0067).

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1 Introduction

Tax evasion in its various forms has serious implications for many aspects of social and economic development. It is particularly important in the context of developing and middle-income countries (Besley and Persson (2014), La Porta and Shleifer (2014), Besley and Persson (2010)), where weak state capacity and the absence of adequate tax enforcement policies make tax collection an especially challenging task.

To induce one form of tax compliance – the payroll tax compliance – it is often recommended to tie benefits to declared wages. The underlying rationale behind such policies is an assumption that instead of evading taxes, people will optimally choose to comply with prevailing tax rules today and enjoy social security benefits later (Kumler et al. (2013), Becerra (2017)). A potential concern with such policies is that employees and employers might collude by manipulating the level or timing of declared wages, thereby affecting the size of social insurance benefits. For example, Christofides and McKenna (1996) and Doornik et al. (2018) document job transition patterns that are consistent with firm-worker collusion to manipulate workers' eligibility for unemployment benefits. Using data from Brazil, Doornik et al. (2018) show that many workers are laid off exactly when they become eligible for unemployment benefits and then re-employed once the benefits expire. They explain this result by workers' shifts in and out of formal employment with an aim to extract rents from the unemployment insurance system. From the point of view of an employer, such an arrangement allows sustaining lower equilibrium wages. For an employee, it ensures a higher take-home pay.

In this paper we explore a different type of firm-worker collusion in the context of incentives provided by social insurance systems. In particular, we study the effect of income-dependent parental benefits on declared wages during women's pregnancy. We draw on the example of Latvia: weak tax enforcement capacity and generous contributory parental benefits make this country an ideal testing ground to understand whether tying benefits to wages creates incentives for firms and employees to collude.

We use administrative data on wages and social security benefits covering the entire working population of Latvia over a 15-year period from 1996 to 2010. To elicit the causal effect of contributory benefits on firm-worker collusion, we use an event study analysis with three sources of identifying variation. At the most basic level, we compare wage growth of pregnant women during the benefit qualification period with wage growth of other women. To test if the observed differences in wage growth are driven by incentives to report higher wages before the childbirth or by other time varying unobservables occurring at the same time as pregnancy, we supplement our analysis with two robustness checks. First, as an additional control group, we use public sector employees, who cannot collude with the employer to manipulate reporting of their wages. Second, we exploit a reform that was implemented in Latvia in 2005, when the contributory parental benefit replaced a universal fixed-amount benefit. Hence we use women who became pregnant before 2005 as a second control group. These additional control groups allow us to more credibly difference out time-varying unobservables occurring concurrently with the pregnancy and strengthen the validity of our identification strategy. We find strong evidence of wage growth acceleration that starts shortly after a woman becomes pregnant, and rationalize this phenomenon by collusive legalization of previously undeclared earnings.

From the policy perspective, it is essential to understand whether the legalization of earnings in response to pregnancy is temporary or permanent. If it is permanent, the short-run fiscal losses resulting from the abuse of the social security benefits can be more than compensated by the improved tax compliance in the long run. If it is temporary, the firm-worker collusion represents a pure loss to the budget. We cannot answer this question using the sample of women, because many women change employers after giving birth. This itself might depend on whether women collude with their employers before the childbirth and therefore the set of women not switching employers after the childbirth is highly non-representative and cannot be used to study persistence of earnings legalization. We address this question by exploiting a unique feature of our institutional setup allowing any of the parents to receive the parental benefit. Since men do not normally change employers after their partner has given birth, we use the sample of men to understand whether the legalisation of earnings is temporary or permanent.

Our analysis delivers three principal results. First, we observe a sharp wage growth during the months of pregnancy that overlap with the benefit qualification period. Depending on the specification, we conclude that during this period the wage on average increases by 5.4%-7.5%. Second, obtained effects are highly heterogeneous. Like the previous literature, we show that the wage growth is much higher in small firms, where it is presumably easier to sustain collusion between employees and employers (see Kleven et al. (2016) for micro-foundations of this result): we find that in small firms (with one to five employees) wage increases by 17.4% more than in large firms (with more than fifty employees). Finally, we demonstrate that legalisation of wages is temporary and lasts only until the end of the period taken into account to calculate parental benefit.

Related Literature. This paper contributes to several strands of the literature. First, we complement nascent empirical literature arguing in favor of tying various social security benefits to declared wages and provide results on the important negative side effects, which should be taken into account when designing benefits. Papers arguing in favor of tying social security benefits to reported wages mainly provide evidence for Latin America, and among others include Kumler et al. (2013), who show that payroll tax evasion declines in response to tying pension benefits more closely to reported wages in Mexico, Becerra (2017), who provide similar results for pension benefits in Colombia, and studies by Cruces and Bergolo (2013) and Bergolo and Cruces (2014), who argue that a reform tying health insurance of children to the formal labor supply of parents increased formal labor supply in Uruguay. Other studies analyzing the impact of benefit-induced incentives on informality mainly focus on the effect of expansion of non-contributory assistance schemes in Latin American countries and on their effect on formal employment. In general, these studies show that the provision of non-contributory benefits leads to disincentives for the creation of formal jobs (see Bosch and Campos-Vazquez (2014), Garganta and Gasparini (2015) and Bergolo and Cruces (2016)).

Second, we show that results from the analysis of the inter-temporal income shifting apply to social security benefits which depend on the reported wages. For example, le Maire and Schjerning (2013), Kreiner et al. (2014), Kreiner et al. (2016) and Kreiner et al. (2017) all provide evidence from Denmark on how people in intertemporally adjust their behavior to enjoy significantly more beneficial tax regimes.

More broadly, we contribute to the literature analyzing what should be taken into account when designing social security system in the presence of informality and worker-firm collusion (see Gerard and Gonzaga (2016) on how to design unemployment benefits in the context of high informality and Doornik et al. (2018) who studies how worker-firm collusion affects the take-up of unemployment benefits in Brazil). We document one more margin of adjustment - income shifting in and out of informality, which along with income shifting between tax bases (documented for example in Slemrod (1995) for US, Harju and Matikka (2016) for Finland and by Kleven and Waseem (2013) for Pakistan) and income shifting over time should be taken into account when designing tax and social security systems. To the best of our knowledge, we are among the first alongside with Waseem (2015) to provide evidence on this important but currently understudied margin. Finally, our results provide a cautionary tale for the literature trying to identify the effect of financial incentives on fertility (for excellent recent examples see Raute (2016) for Germany and González (2013) for Spain). In light of our results, if informality is present, it might be empirically challenging to disentangle whether financial incentives increase fertility of high earning women or of women who manage to temporarily increase their earnings to inflate future parental benefits. Hence the analysis of response of fertility to financial incentives in the presence of informality is an important avenue for future research.

2 Institutional Background

This section provides the institutional details we later exploit in our empirical strategy. First, we describe the child-related benefits in Latvia. Then we give an overview of the taxes that are applied to wages. Finally, we present survey evidence on the prevalence of payroll tax evasion in Latvia.

2.1 Child-Related Benefits in Latvia

The main focus of this paper is on the parental benefit – a benefit paid to one of the parents of a newborn baby. Before 2005 it was a universal lump-sum monthly benefit[4], but with a reform that was implemented in 2005 it was replaced with a monthly benefit that depended on the declared wage.

Announced in August 2004 and enacted in January 2005, the reform introduced a contributory benefit equaling 70% of the parent's average gross wage (which is approximately equivalent to the net wage) and lasting until the child becomes one year old. The benefit was bounded by the floor of EUR 80 and by the ceiling of EUR 558 per month. The benefit qualification period, in which the reported wage counts towards the benefit, was 12 months ending three months before the childbirth, and therefore included five months of pregnancy. This overlapping of pregnancy with the benefit qualification period is key to our identification strategy: in 5 out of the 12 months that count towards the benefit a woman has incentives to increase her reported wage to enjoy a higher benefit after the childbirth.

The reform produced a considerable increase in the size of the benefit: the country-wide average monthly benefit went up from EUR 43 in 2004 to EUR 124 in 2005. At the same time, the reform introduced some employment restrictions for the benefit recipients. Before the reform, a benefit recipient could work part-time, but after the reform employment was made incompatible with the benefit recipiency. The employment restrictions were later lifted following two decisions of the constitutional court. Starting March 2006, working parents became eligible for 50% of the benefit, and one year later, beginning March 2007, working parents became eligible for the full benefit amount.

Several other reforms refining the reform of 2005 are exploited in the second part of the paper to understand what happens with wages of the parental benefit recipients after

[4] For a detailed description of the lump sum benefit see Appendix B.1.

the end of the benefit qualification period. First, a reform removing the cap on the benefit came into force beginning January 2008[5]. Second, in response to the economic crisis of 2008, the government again made employment incompatible with the benefit. This reform was announced in June 2009 and was enacted starting July 2009, and stipulated that working parents were eligible only for 50% of the benefit amount. Parents whose children were born after May 2010 were not eligible even for 50% of the benefit amount.

The removal of the benefit cap and changes in employment restrictions changed the gender composition of the benefit recipients. In the vast majority of cases, women received the parental benefit when it was not compatible with employment or was compatible only with the part-time work. Wages of men in Latvia are on average higher than those of women. Therefore, once employment became compatible with the benefit recipiency, men started to receive the parental benefit and simultaneously continued to work. The share of men among the recipients of the parental benefit went from about 4% in 2005 to 26% in 2007 and further to 37% in 2008, when the benefit cap was removed (see Figure 1).

Throughout the sample period women were eligible for another contributory benefit – the maternity benefit. The benefit was introduced in 1997, and was paid for a maximum of 140 days (70 days before and 70 days after the childbirth). The size of the maternity benefit depended on the average reported gross monthly wage over the six months period ending two months before the benefit entitlement, i.e., approximately four months before the childbirth[6]. Therefore, after the reform of 2005, the qualification periods for the parental benefit and the maternity benefit overlapped – the qualification period for the parental benefit started five months earlier and ended one month later.

2.2 Social Contributions and Income Tax

Contributory benefits are financed from the social security tax that is remitted by employers. All employees below the retirement age are subject to a flat social security tax rate, which

[5] This reform also raised the minimum amount to EUR 90.

[6] Initially, the period that was taken into account to calculate average wage was 2 months. Starting August 1998, the period was prolonged to 6 months ending this period two months before the benefit entitlement, which is approximately four months before the childbirth. See Appendix B.2 for more details.

during our sample period was equal to 33.09%. Apart from the social security tax, wages are subject to a flat personal income tax of 25%[7]. Similarly to the social security tax, income tax is remitted by employers. No other taxes were applied to wages during our sample period.

2.3 Payroll Tax Evasion

Available cross-country evidence suggests that payroll tax evasion is widely prevalent in Latvia - the share of employees who admit having evaded payroll taxes is 11% – the highest in the EU (European Commission (2014)). The amount of lost revenue to tax authorities is quite substantial - 40% of employees who have evaded payroll taxes have not declared between 50% and 100% of their wage. Similar results are obtained by Putnins and Sauka (2015) who focus exclusively on the Baltic states. They provide survey evidence on the payroll tax evasion showing that in recent years the share of unreported income in the Latvian private sector has varied between 18% and 35% of the gross wage. Thus, although there is some uncertainty about the exact estimates of the payroll tax evasion in Latvia, available evidence suggests that it is widely prevalent and hence makes this country an ideal testing ground to explore the impact of various incentives on the payroll tax evasion.

3 Data

Throughout this paper we use administrative data provided by the Latvian Social Security Agency. The structure of this database is very similar to the Austrian Social Security Database (Zweimüller et al. (2009)). It provides individual level monthly information on wages and various social security benefits described in the previous section, thus making it an ideal data source to answer our question of interest. Below we provide a detailed

[7] The personal income tax rate was cut to 23% in 2009 and was changed several times after that, but our baseline empirical analysis does not cover this period.

description of the various measures used in the empirical section.

3.1 Wages

Information on gross wages comes from a monthly matched employee - employer panel that covers the entire working population (aged 15 and above) from 1996 to 2015. It allows us to track individuals and firms over time and to study wage dynamics within employee - employer pairs. Additionally, from this panel we obtain a limited set of demographic characteristics (notably age and gender) and an indicator showing whether a firm is privately or publicly owned. The dataset has two drawbacks. First, it does not contain information on the hours worked, hence we are not able to account for labor supply responses on the intensive margin. Second, social security contributions are capped, which introduces truncation at the top. The cap was in place in all years except 1996 and 2009-2013, but fortunately, it is binding only for a very small share of employees (throughout our sample period this share did not exceed 0.7% - for more details see Appendix B.3).

3.2 Child-Related Benefits

We use data on various child-related benefits to impute pregnancy periods for parental benefit recipients or, in case of male recipients, pregnancy periods of their partners.

First, we use data on monthly maternity benefit payments to identify our sample of women. The data covers all women who received the benefit for children born starting 1999 and provides information on months when the maternity benefit was paid. As the maternity benefit is paid only for several months - shortly before and shortly after the childbirth, i.e. during the period when we believe most mothers would prefer not to work, the take-up rate of this benefit is likely to be very high. Therefore the data on the maternity benefit should capture most of the socially insured women who gave birth starting 1999.

Unfortunately, the data on the maternity benefits does not contain information on the child's birth date, which we need to identify pregnancy periods. To obtain a child's birth date, we match the data on the maternity benefits with the data on the recipiency of the family state benefit. The family state benefit is a lump sum monthly benefit that is paid to one of parents until a child becomes 15 years old[8]. It is a non-contributory universal benefit that is paid independently of parents' employment status. Therefore the data on the family state benefit covers all individuals - both women and men, employed and non-employed, who received the benefit. The data contains information on parents who received the benefit starting year 2008, which, given that the benefit is paid at least until a child is 15 years old, implies that it should capture majority of children born in 1993 or later. It contains information on child's month and year of birth, and we merge the data on the maternity benefits with the data on the family state benefits to obtain the sample of socially insured women for whom we know a child's birth date and therefore can identify the start of pregnancy.

Finally, in the second part of the paper, we use the data on men receiving the parental benefits to understand whether the legalization of earnings is temporary or permanent. The data on the parental benefits contains a child's birth date only starting with January 2007. In case of men we cannot use data on the family state benefit to identify a child's birth date, because the proportion of men among recipients of the family state benefits is low[9] (for details see Figure 2). Therefore, to analyze what happens after the qualification period for the parental benefits ends, we use the data only on the parental benefits.

3.3 Sample Selection and First Look at the Data

Our baseline sample of women who gave birth consists of women born from 1975 to 1983 whose first child was born in January 1999 or later, and who for the first time received

[8]Before 2010 the benefit was paid unconditionally until a child becomes 15 years old and conditionally on her continuing education until she becomes 20 years old. Starting 2010 the benefit was paid unconditionally until a child becomes 15 years old and conditionally on her continuing education until she becomes 19 years old.

[9]As we see from the Figures 1 and 2, following the reform of 2005 the proportion of men increased both among the recipients of the family state benefit and among the recipients of the parental benefit. In case of the parental benefit it is due to relaxation of employment restrictions for the benefit recipients. In the case of the state family benefit the growing share of men among the benefit recipients is not clear, given that the state family benefit is a universal benefit and is not tied to earnings. A possible explanation is that in many cases the person applying for one benefit will apply also for all other benefits for the particular child.

the maternity benefit before January 2009. The lower age bound corresponds to the first cohort of women for whom we are likely to observe all children[10], whereas the upper bound is chosen so that we would have at least 500 women giving birth both before and after the reform of 2005. We focus only on the first child.

As explained in the previous section, to impute pregnancy periods we match the data on the maternity benefits with the data on the family state benefits. Figure 3 shows the match quality for the two data sources. Although there is some variation over time, the proportion of matched women is never below 93%[11]. For our baseline sample we analyze wages during the 1996-2008 period. In order not to introduce effects caused by the financial crisis of 2008 we omit years following 2008[12]. Additionally our baseline sample includes employed women (born from 1975 to 1983) who did not become pregnant during the sample period[13].

In this way we are left with 4937317 person-firm-time observations corresponding to monthly reported wages of 38349 women who became pregnant during the sample period and 47284 women who did not become pregnant during the sample period.

Figures 5 and 6 display the evolution of the average wage for different groups of women and provide the basis for our identification. In what follows we label the period when pregnancy overlaps with the benefit qualification period for the parental benefit the conversion period, meaning that in this period a woman and her employer can collude and "convert" undeclared income into declared income. In the figures we see that on average the wage of women who become pregnant during our sample period is higher than the wage of those

[10] We observe all children born starting 1993 and majority of women in Latvia give birth after they become 18 years old.

[11] In what follows we use the sample of women for whom we were able to match the data on the maternity benefits with the data on the family state benefits and not the sample of women for whom we were able to match the data on the maternity benefits with the data on the family state benefits and with the data on the parental benefits. In this way we are not introducing the sample selection bias associated with the fact that once the benefit recipiency becomes compatible with employment only the women whose earnings exceeded those of men applied to receive the parental benefit. Figure 4 shows the proportion of women receiving the maternity benefit (conditional on receiving the family state benefit) which also received the parental benefit. In the Figure 4 we can see that once the benefit recipiency became compatible with employment the proportion of women receiving both the maternity benefit and the parental benefit sharply decreased.

[12] Omitting year 2008 does not qualitatively change our baseline results

[13] More precisely, we include women who either did not have children at the time of the data preparation or their children were born either before 1996 or after 2009. We assume that if a women gave birth in 1995 then three years later she can be used as a control to identify the effect of interest. Similarly if a women gave birth in 2010 then we assume that her wage for the first time was affected in 2009 and hence she can be used as control group in 2008.

who do not become pregnant. Additionally, the difference increases during the conversion period (both with respect to women who do not become pregnant during the sample period and those women who have not yet become pregnant). Finally, we see that these differences become larger in the private sector starting 2005.

Similarly as in the case of women, our sample of men consists of two groups: men whose partners became pregnant during the sample period and who received the parental benefit, and all other men. For the former, we select men born between 1975 and 1984[14], whose partners gave birth between July 2008 and May 2010[15]. For the latter, we make similar adjustments as in the case of women[16]. We analyze wages from January 2005 to August 2010[17]. In this way we are left with 6497939 observations of monthly reported earnings corresponding to 6411 men who received the parental benefit, and 136978 men who did not receive the parental benefit during the sample period. Figures 7 and 8 display the evolution of the average wage for different groups of men. Similarly as for women, we see that wages of men whose partners became pregnant and who received the parental benefit are almost always higher than wages of other men. Additionally, wages of these men increase during the conversion period. Finally, after the conversion period wages in the public sector do not change, whereas in the private sector we see a clear decrease once the conversion period is over. We next proceed to the formalization of these descriptive results.

[14] So that we would have 500 men in each cohort whose partners gave birth during the sample period.

[15] As explained in the previous section, before the reform of 2008 the parental benefit was capped at EUR 558. Figures 7 and 8 show the evolution of the average wage for different groups of men. From these figures we can witness that during the sample period the average wage of men whose partner became pregnant exceeded EUR 558 even before the conversion period. As men did not have incentives to inflate wages before the cap on parental benefits was removed we do not include the time period before the reform of 2008 into our analysis. The reform of 2008 was announced in October 2007, therefore men whose partners gave birth in July 2008 are the first group who were aware of the new rules from the beginning of the partner's pregnancy. Starting May 2010 benefit recipiency became incompatible with employment, therefore men whose children were born in April 2010, were the last group who could simultaneously work and receive full benefit amount.

[16] We select men born from 1975 to 1984 who either did not receive the parental benefit during the sample period or received the parental benefit either before 2006 or after 2011.

[17] We analyze wage growth for men whose partners became pregnant in November 2007 or later, and we leave around three years for the baseline period. Starting September 2010 a new form of legal entity was introduced in Latvia for which we observe only net wages. Since it is unclear how to compare wages of people working for this new legal entity with wages of other people, we analyze time period only until August 2010.

4 Empirical Strategy and Results

This section describes the empirical strategy and obtained results. We start by presenting difference in differences results where we compare wages of women who became pregnant with those of women who did not (or became later in time as compared with women under consideration). Then, to strengthen the validity of our identification strategy, we add additional control groups resulting in a triple difference and a quadruple difference setup. To understand whether our results can be falsified, we then proceed with a battery of falsification tests. Next, we present heterogeneity of the obtained results with respect to the firm size. Finally, we conclude with the analysis of the persistency of the obtained effect over time.

4.1 Baseline Results

The basis of our empirical strategy is the comparison of the wage growth during the conversion period relative to the period before the conversion period, compared to the wage growth of a woman who did not become pregnant at that point in time. Figure 9 illustrates which variation can be used to identify the effect of interest. In the Figure 9 we see that the effect of interest can be identified either by using only the sample of women who became pregnant during the period of interest (that is, we can use only Person 1 and Person 2 by exploiting the fact that they gave births at different points in time) or by adding to these women also women who did not become pregnant during the period of interest (Person 3 in Figure 9). Therefore, to obtain the effect of interest we estimate variants of the following specification:

$$log(w_{ijt}) = \alpha_1 Conv_{it} + \eta_i + \gamma_j + \xi_t + \epsilon_{ijt} \tag{4.1}$$

where w_{ijt} denotes wage of a person i in a firm j in a year-month t, $Conv_{it}$ equals 1 for pregnant women during their conversion periods and 0 otherwise and η_i, γ_j and ξ_t denote person, firm and year-month fixed effects, respectively. Once their conversion periods end, we remove pregnant women from the sample.

Table 1 presents results from estimating variants of the specification (4.1), where we subsequently add more heterogeneity: we add firm specific or worker-firm specific time constant heterogeneity to account for firm-worker match effects, and cohort specific year-month fixed effects to account for lifecycle effects. Depending on the specification, we conclude that during the conversion period wage increases by 5.7%-8.7%.

Before proceeding with other specifications it is worth doing a remark on what we need to assume to interpret the results obtained from the specification (4.1) as causal, i.e. to interpret estimate $\hat{\alpha}_1$ as an effect of pregnancy on the wage. To interpret estimate $\hat{\alpha}_1$ as causal we need to assume that absent pregnancy the difference in wages for those who became pregnant and those who did not would be constant over time. That is, we need to make the "parallel trends" assumption. To provide some evidence in favor of this assumption we further decompose the period before the conversion period into planning period and the period before the planning period. We define planning period as the time period that precedes pregnancy, but which is still taken into account to calculate parental benefits. We study plausible validity of the parallel trends assumption using the following specification:

$$log(w_{ijt}) = \alpha_1 Plann_{it} + \alpha_2 Conv_{it} + \eta_i + \gamma_j + \xi_t + \epsilon_{ijt} \tag{4.2}$$

where all the notation corresponds to that used in the specification (4.1) and $Plann_{it}$ equals 1 for pregnant women during their planning periods and 0 otherwise. Table 2 presents results obtained from estimating variants of the specification (4.2). We conclude that depending on the specification, even before the pregnancy wages of those who eventually became pregnant and those who did not differ by 2.9%-6.2%. This result motivates our further analysis where we use additional control groups to "difference out" pre-pregnancy divergence in wages between those who eventually became pregnant and those who did not.

4.2 Triple Difference and Quadruple Difference Results

We next use two additional sources of variation to difference out pre-pregnancy divergence in wages between those who became pregnant during the period of interest and those who did not.

As a first additional control group, we use public sector employees which we assume cannot engage in the payroll tax evasion. The idea is that by using data only on the public sector employees when estimating specification (4.1) we will obtain an estimate of the selection effect which arises from the non-random selection of women into pregnancy. Whereas estimates obtained from the specification (4.1) by using data only on private sector employees will provide us with a sum of the selection effect and the true treatment effect arising from the conversion of the undeclared payments into declared wages. Under the assumption that selection effects are the same in two sectors the difference between the two should provide an effect of interest. To obtain this effect we estimate variants of the following specification:

$$\begin{aligned} log(w_{ijt}) = \alpha_1 Treat_i \cdot Private_j + \alpha_2 Plann_{it} + \alpha_3 Conv_{it} + \alpha_4 Plann_{it} \cdot Private_j + \\ \alpha_5 Conv_{it} \cdot Private_j + \eta_i + \gamma_j + \xi_t \cdot Private_j + \epsilon_{ijt} \end{aligned} \tag{4.3}$$

where $Treat_i$ equals 1 for women who became pregnant during the period of interest and 0 otherwise and $Private_j$ equals 1 for private firms and 0 otherwise, all other notation corresponds to that used in other specifications. The main parameter of interest in this specification is an estimate of the triple-difference term, $\hat{\alpha}_5$. Estimates obtained from the specification (4.3) are presented in the Table 3. Depending on the specification we conclude that on average during the conversion period wage increase in the private sector is 5.4% to 7.1% larger as compared to the public sector. Two additional remarks regarding Table 3 are in order. First, only in one of the four specifications, we find a significant coefficient on $Plann_{it} \cdot Private_j$. Second, once we account for firm-specific heterogeneity, the only coefficient that stays robust across specifications is that on the variable $Conv_{it} \cdot Private_j$. The non-existence of the differential planning effect between two sectors and robustness of the coefficient on $Conv_{it} \cdot Private_j$ across a wide range of specifications gives us additional confidence that public sector employees can be used as a viable control group to difference out pre-trends between women who became pregnant during the sample period and those who did not.

We next incorporate a second source of additional variation - a reform linking parental benefits to wages - into our baseline specification. Instead of assuming that the selection effect is constant across people working in different sectors, we assume that the selection effect is constant over time. The idea remains that the estimate obtained from the specification (4.1) using the data before the reform should provide the estimate of the selection effect whereas the estimate obtained using the data after the reform should provide us with an estimate of the sum of the selection effect and of the treatment effect. Under the assumption that the selection effect is constant over time, the difference between the two should provide us with an effect of interest. We study this additional source of the variation using the following specification:

$$\begin{aligned} log(w_{ijt}) = \alpha_1 Plann_{it} + \alpha_2 Conv_{it} + \alpha_3 Plann_{it} \cdot After_t + \\ \alpha_4 Conv_{it} \cdot After_t + \eta_i + \gamma_j + \xi_t + \epsilon_{ijt} \end{aligned} \tag{4.4}$$

where $After_t$ equals 1 after August 2004 (the time when the reform was announced) and 0 otherwise. The main parameter of interest in this specification is an estimate of the triple difference term, $\hat{\alpha}_4$. Table 4 presents estimation results. Depending on the specification we conclude that after the reform the increase in wage during the conversion period is 1.4% to 2.3% larger as compared to the period before the reform. On the other hand, we do not find a significant change in the planning effect after the reform. Additionally, once we account for the firm specific heterogeneity again the only robust coefficient across specifications in the table 4 is that on the variable $Conv_{it} \cdot After_t$. We see that the obtained effect is much smaller as compared to the case when we used private sector employees to difference out pre-trends. There are two explanations of this phenomenon. First, because of the existence of the wage-dependent maternity benefit, even before the reform women had incentives to inflate earnings during the conversion period. Second, we obtain this treatment effect using the data both on public and private sector employees. Since tax evasion is not possible in the public sector, the reform linking parental benefits to earnings should not affect women becoming pregnant in the public sector. These two facts presumably attenuate the treatment effect of pregnancy on wages which we are obtaining using this source of variation.

We next incorporate two previous sources of variation in a unified quadruple difference framework. This allows us to further relax some of the assumptions regarding non-random selection into pregnancy. More precisely, instead of assuming that selection effects are constant either across women working in two sectors or over time we assume that the difference in selection effects between two sectors is constant over time. To obtain a causal effect of pregnancy on wages in this framework we analyze variants of the following specification:

$$\begin{aligned} log(w_{ijt}) = {} & \alpha_1 Treat_i \cdot Private_j + \alpha_2 Plann_{it} + \alpha_3 Conv_{it} + \alpha_4 Plann_{it} \cdot After_t + \\ & \alpha_5 Conv_{it} \cdot After_t + \alpha_6 Plann_{it} \cdot Private_j + \alpha_7 Conv_{it} \cdot Private_j + \\ & \alpha_8 Plann_{it} \cdot After_t \cdot Private_j + \alpha_9 Conv_{it} \cdot After_t \cdot Private_j + \\ & \eta_i + \gamma_j + \xi_t \cdot Private_j + \epsilon_{ijt} \end{aligned} \tag{4.5}$$

the parameter of interest in the specification (4.5) is a quadruple difference estimate $\hat{\alpha}_9$. Table 5 presents estimation results. Depending on the specification we conclude that wage increase in the private sector relative to the public sector during the conversion period is 6.8%-7.5% larger after the reform as compared to the same difference before the reform. Similarly as before we do not find significant planning effects, the coefficient on $Plann_{it} \cdot After_t \cdot Private_j$ is marginally significant only in one of four specifications and is on average around four times smaller as compared to our main effect of interest.

Throughout this section we have used two control groups - women who became pregnant and women who did not become pregnant during the period of interest. We next omit women who did not become pregnant during the sample period. Table 6 presents estimation results. Comparing obtained results to those presented in Tables 1-5 we conclude that both control groups provide us with qualitatively very similar estimates.

4.3 Falsification Checks and Heterogeneity Analysis

We next proceed with a battery of placebo tests and the heterogeneity analysis of the obtained effect. First, to understand whether inflation of wages during the conversion period becomes more pronounced immediately after the reform of 2005, we present yearly triple difference estimates (comparing the increase in the wage during the conversion period in the private sector to that in the public sector). Second, we study the dynamics of the conversion to understand during which months of the pregnancy women start to inflate wages. Finally, at the end of this section we study the heterogeneity of the obtained effect with respect to firm size.

We obtain yearly triple difference estimates using the following specification:

$$\begin{aligned} log(w_{ijt}^{ly}) = \alpha_1 Conv_{it} + \alpha_2 Conv_{it} \cdot Private_j + \sum_{y \neq 2003} \theta_y Conv_{it} \cdot Year_y + \\ \sum_{y \neq 2003} \Theta_y Conv_{it} \cdot Year_y \cdot Private_j + \eta_{ij} + \gamma_t \cdot Private_j \cdot Yob_l + \epsilon_{ijt} \end{aligned} \tag{4.6}$$

where $Year_y$ equals 1 in year y and 0 otherwise and Yob_l equals 1 if a women is born in a year l and 0 otherwise, all other notation corresponds to that used in other specifications. In Figure 10 we plot yearly triple difference coefficients $\hat{\Theta}_y$. From figure 10 we can see that while before the reform of 2005 the difference in the wage growth during the conversion period between private and public sectors was either 0 or negative, then starting with the reform of 2005 it became positive. Even more telling are the Figures A.1 and A.2 provided in the Appendix A where we plot estimates $\hat{\delta}_y$ and $\hat{\Delta}_y$ obtained from the following specification:

$$
\begin{aligned}
log(w_{ijt}^{ly}) = {} & \alpha_1 Plann_{it} + \alpha_2 Conv_{it} + \alpha_3 Plann_{it} \cdot Private_j + \alpha_4 Conv_{it} \cdot Private_j + \\
& \sum_{y \neq 2003} \theta_y Plann_{it} \cdot Year_y + \sum_{y \neq 2003} \Theta_y Conv_{it} \cdot Year_y + \\
& \sum_{y \neq 2003} \delta_y Plann_{it} \cdot Year_y \cdot Private_j + \sum_{y \neq 2003} \Delta_y Conv_{it} \cdot Year_y \cdot Private_j \\
& + \eta_{ij} + \gamma_t \cdot Private_j \cdot Yob_l + \epsilon_{ijt}
\end{aligned} \tag{4.7}
$$

While we do not see that yearly triple difference estimates of the planning effect, $\hat{\delta}_y$, become positive after the reform, we clearly see this for yearly triple difference estimates of the conversion effect, $\hat{\Delta}_y$.

Next, using our triple difference strategy we study the dynamics of conversion. To do that we estimate the following specification:

$$
\begin{aligned}
log(w_{ijt}^{l}) = {} & \alpha_1 Before_{it} + \sum_{p \neq 0} \theta_p Pm_{ip} + \alpha_2 Before_{it} \cdot After_t + \\
& \sum_{p \neq 0} \Theta_p Pm_{ip} \cdot After_t + \alpha_3 Before_{it} \cdot Private_j + \\
& \sum_{p \neq 0} \delta_p Pm_{ip} \cdot Private_j + \alpha_4 Before_{it} \cdot After_t \cdot Private_j + \\
& \sum_{p \neq 0} \Delta_p Pm_{ip} \cdot After_t \cdot Private_j + \gamma_t \cdot Private_j \cdot Yob_l + \epsilon_{ijt}
\end{aligned} \tag{4.8}
$$

where $Before_{it}$ equals 1 for women who became pregnant, before the start of the benefit qualification period, and 0 otherwise, and Pm_{ip} equals 1 during the p^{th} month of pregnancy (with $p = 0$ denoting one month before the pregnancy) for women who became pregnant during the sample period and 0 otherwise. Figure 11 displays quadruple difference estimates, $\hat{\Delta}_p$. From this figure one can see that there is a sharp jump in the wage growth in the private sector as compared to the public sector after the reform, and this jump starts to appear exactly in the first month of the pregnancy.

We finish this section by studying how the obtained result varies with the firm size. To study the heterogeneity of the obtained effect with respect to the firm size we use only the sample of private firms and employ the following specification:

$$
\begin{aligned}
log(w_{ijt}^{syl}) = {} & \alpha_1 Plann_{it} + \alpha_2 Conv_{it} + \alpha_3 Plann_{it} \cdot After_t + \alpha_4 Conv_{it} \cdot After_t + \\
& \sum_{s \neq 0} \theta_s Plann_{it} \cdot Size_{jy}^s + \sum_{s \neq 0} \Theta_s Conv_{it} \cdot Size_{jy}^s + \\
& \sum_{s \neq 0} \delta_s Plann_{it} \cdot After_t \cdot Size_{jy}^s + \sum_{s \neq 0} \Delta_s Conv_{it} \cdot After_t \cdot Size_{jy}^s + \\
& \eta_{ij} + \gamma_t \cdot Size_{jy}^s \cdot Yob_l + \epsilon_{ijt}
\end{aligned}
\tag{4.9}
$$

where $Size_{jy}^s$ equals 1 if a firm j in a year y is in a size category s and 0 otherwise. As a baseline category we use firms with more than 50 employees in a given year. Figures 12 and 13 display triple difference estimates $\hat{\delta}_s$ and $\hat{\Delta}_s$, respectively. While we do not see any association between $\hat{\delta}_s$ and the firm size, we see that $\hat{\Delta}_s$ monotonically decreases in the firm size for firms with 1-20 employees and then stays constant (and still positive as compared to the baseline category) for firms with 21-50 employees.

To conclude, in this section we have shown that the difference in the wage growth during the conversion period between private and public sectors starts to appear after the reform of 2005. Additionally, we have also shown that the inflation of wages starts with the first month of the conversion period. Finally, similarly as in the previous literature, our results indicate that the magnitude of the conversion is the largest in small firms. Taken together these more disaggregated results strongly resonate with the results presented in the previous section and point towards the existence of the worker-firm collusion.

4.4 Alternative explanations of the identified wage growth during pregnancy

An alternative explanation of the identified wage growth in the private sector is an increase in supplied hours of work during pregnancy. Arguably, a change in supply of hours is more likely to occur in the private sector, and especially in small private firms, where work-time arrangements are the most flexible and allow a woman to temporarily switch to longer working hours. This alternative explanation is consistent with our findings on the wage growth in the private sector and the identified heterogeneity of the effect across firms of different size, and it therefore questions our interpretation of the results as a wage legalization.

Formally, we cannot rule out this alternative because, as mentioned previously, we do not have information on hours of work. But there are several reasons why we believe that an increase in the supply of labor is not the main mechanism that drives our results. First, our yearly triple difference estimates increase almost monotonically from 2005 to 2008. If the identified wage growth is a result of wage legalization, this time pattern can be explained by the removal of legal restrictions on employment and the benefit ceiling, which were enacted in 2006-2008. But this time pattern is difficult to explain if one believes that our results are driven by changes in the supply of labor, because this period covers the beginning of the economic recession in Latvia that led to a strong negative adjustment in employment on both intensive and extensive margins.

Second, the size of our estimates of the wage growth, especially in small firms, imply a pretty sizeable increase in the hours of work. Using the central estimate of Frisch labor supply elasticity for women in Attanasio et al (2018), 0.87, we conclude that pregnant women in small private firms supply about 15% more hours of work than their counterparts in large firms[18]. Many studies find that Frisch elasticity at the intensive margin tends to be higher for married women, and especially for married women with children. The upper end of Frisch elasticity estimates for married women reported in Whalen and Reichling (2017) exceed one, and using these estimates (1.2 – 1.4), we conclude that the difference in hours of work can be as large as 21 -24%. This is quite a substantial increase in supplied hours during pregnancy. In 2005-2008, 75%-79% of women in Latvia worked 40+ hours per week. Assuming that this share is similar across small and large firms, even a 15% increase in supplied hours during pregnancy for a majority of women implies 46+ hours of work per week in small firms.

In addition, there can be other collusive arrangements between an employee and an employer that are consistent with the identified pattern of wage growth. For example, an employer and an employee can agree on shifting the employee's future income to the

[18]Calculated as 17.4%*0.87.

pregnancy period, or they can collude to temporarily increase the employee's income only "on paper" to inflate the size of the benefit. Like income legalization during the employee's pregnancy, these types of arrangements would increase the employee's declared income before the childbirth, and therefore it is not possible to empirically distinguish between these alternative explanations. However, we believe that arrangements such as front-loading of the employees' income or straight fraud involving reporting of unpaid wages are less plausible than income legalization, because they are more difficult to sustain. Front-loading of employees' income implies that a woman should keep the job with the same employer after the childbirth, but reporting of the non-existing income involves considerable extra risks for the employer in exchange for future gains from the inflated social benefits. In practice an employer cannot enforce receipt of this anticipated future income, because a woman can leave the job and keep the entire gain from the higher benefit. Hence, we believe that legalization of the previously unreported income represents the main source of income growth that starts after the woman becomes pregnant.

4.5 Results for the Sample of Men

In the previous sections we have shown that wages of pregnant women increase during the conversion period. We interpret this finding as an evidence of a firm-worker collusion to legalize the previously unreported earnings with an aim to increase parental benefits. Looking from the tax collection perspective this might be a harmless side effect - if the observed wage increase is permanent then in the long run the government could recoup losses of inflated parental benefits via collection of the payroll tax. If on the other hand the legalization of wages is temporary, the net fiscal effect can be negative. Unfortunately, it is impossible to infer the nature of wage legalization by looking at the sample of women. During our sample period, after giving birth only 53% of women returned to the employer where they had worked during the conversion period (see Figure 14 for an evolution of this fraction over time). To answer what happens with wages once the conversion period is over we turn to our sample of men (around 83% of men continue to work for the same firm after their partner has given birth; for an evolution of this fraction over time see Figure 15).

We analyze what happens after the conversion period is over using variants of the following specification:

$$\begin{aligned} log(w_{ijt}) = {} & \alpha_1 Treat_i \cdot Private_j + \alpha_2 Plann_{it} + \alpha_3 Conv_{it} + \alpha_4 Aft\_Conv_{it} + \\ & \alpha_5 Plann_{it} \cdot Private_j + \alpha_6 Conv_{it} \cdot Private_j + \\ & \alpha_7 Aft\_Conv_{it} \cdot Private_j + \eta_i + \gamma_j + \xi_t \cdot Private_j + \epsilon_{ijt} \end{aligned} \tag{4.10}$$

where Aft_Conv_{it} equals 1 after the conversion period is over for men whose partners gave birth during the sample period and 0 otherwise. All other notation corresponds to that used in other specifications.

Results presented in Tables 7 and 8 show that once we do the analysis at the level of the firm-worker pair we find that during the conversion period the wage increase in the private sector is 7%-9% larger as compared to that in the public sector. We also find that even during the planning period wage increase in the private sector is larger than that in the public sector but the magnitude of this effect is almost three times smaller as compared to the main effect of interest. Finally and most importantly, we do not find any positive wage differentials in the private sector as compared to the public sector after the end of the conversion period - we find either no differential growth, or we find that wage decreases in the private sector relative to that in the public sector once the conversion period is over. Therefore, our results indicate that the wage increase is not permanent and suggest that in response to incentives provided by social security benefits firms and workers collude to only temporarily declare higher earnings and inflate the future benefits.

5 Discussion and Conclusions

In this paper, we study the effect of tying social security benefits to declared wages on firm-worker collusion and strategic income reporting shortly before the benefit entitlement.

Drawing on the example of the contributory parental benefit in Latvia, we show that declared wages sharply increase in pregnancy months that overlap with the benefit qualification period. Depending on the identifying assumptions, we show that during this period wages increase by 5.4%-7.5%. We interpret the obtained result as a collusive legalization of previously unreported income with an aim to increase the size of the parental benefit.

Our result is robust with respect to many falsification tests - we show that the wage growth during pregnancy sharply increases in the private vis-a-vis the public sector starting with the year when the parental benefit became dependent on the wage. Additionally, we also show that the wage growth starts with the first month of pregnancy and we do not find the differential growth in wage in the private sector vis-a-vis public sector before pregnancy. Similarly as in the previous literature on the payroll tax evasion we also find that the obtained effect is heterogenous with respect to the firm size and is larger in small firms. Importantly, our findings indicate that the wage legalization is temporary and does not continue once the benefit qualification period is over. This temporary legalization of earnings is possible, because the benefit qualification period is relatively short (12 months), and includes 5 months of pregnancy, which makes the average wage during the qualification period relatively easy to affect. Such setting creates bad incentives – an employee and an employer can collude to increase the average wage that determines the size of the benefit.

Many countries implement wage-dependent benefits. Our results show that social security benefits can and will be abused if people are given wrong incentives. Therefore to achieve the best outcome policy makers when deciding whether to tie social security benefits to declared wages should take into account the possibility of a firm-worker collusion.

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Tables

Table 1: Impact on Wage: Difference in Differences Results

	ln(wage)			
Conv	0.087***	0.073***	0.064***	0.057***
	(0.004)	(0.004)	(0.004)	(0.004)
Fixed effects:				
Ind	Y	Y	N	N
Firm	N	Y	N	N
Ind x Firm	N	N	Y	Y
Time	Y	Y	Y	N
Time x Year of Birth	N	N	N	Y
N	4,936,477	4,934,911	4,910,582	4,910,582
Adjusted R^2	0.63	0.77	0.86	0.86

Notes: Difference in differences estimates from various variants of the specification 4.1. For details see section 4.1. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

Table 2: Impact on Wage: Difference in Differences Results

	ln(wage)			
Plann	0.062***	0.042***	0.036***	0.029***
	(0.004)	(0.003)	(0.002)	(0.002)
Conv	0.103***	0.086***	0.080***	0.070***
	(0.005)	(0.004)	(0.004)	(0.005)
Fixed effects:				
Ind	Y	Y	N	N
Firm	N	Y	N	N
Ind x Firm	N	N	Y	Y
Time	Y	Y	Y	N
Time x Year of Birth	N	N	N	Y
N	4,936,477	4,934,911	4,910,582	4,910,582
Adjusted R^2	0.63	0.77	0.86	0.86

Notes: Difference in differences estimates from various variants of the specification 4.2. For details see section 4.1. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

Table 3: Impact on Wage: Triple Difference Results - Using Public Sector to Difference Out Pre Trends

	ln(wage)			
Treat x Private	-0.031^{*}	-0.030^{**}		
	(0.018)	(0.013)		
Plann	0.065^{***}	0.055^{***}	0.043^{***}	0.034^{***}
	(0.007)	(0.006)	(0.005)	(0.005)
Conv	0.049^{***}	0.043^{***}	0.041^{***}	0.029^{***}
	(0.008)	(0.007)	(0.006)	(0.006)
Plann x Private	-0.003	-0.015^{**}	-0.007	-0.004
	(0.008)	(0.007)	(0.006)	(0.006)
Conv x Private	0.071^{***}	0.058^{***}	0.054^{***}	0.057^{***}
	(0.010)	(0.009)	(0.009)	(0.009)
Fixed effects:				
Ind	Y	Y	N	N
Firm	N	Y	N	N
Ind x Firm	N	N	Y	Y
Time x Private	Y	Y	Y	N
Time x Private x Year of Birth	N	N	N	Y
N	4,936,477	4,934,911	4,910,582	4,910,580
Adjusted R^2	0.63	0.78	0.86	0.86

Notes: Triple difference estimates from various variants of the specification 4.3. For details see section 4.2. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

Table 4: Impact on Wage: Triple Difference Results - Using Reform to Difference Out Pre Trends

	ln(wage)			
Plann	0.065***	0.043***	0.035***	0.029***
	(0.005)	(0.003)	(0.003)	(0.003)
Conv	0.095***	0.075***	0.067***	0.058***
	(0.006)	(0.005)	(0.005)	(0.005)
Plann x After	−0.007	−0.005	0.001	−0.001
	(0.007)	(0.005)	(0.004)	(0.004)
Conv x After	0.014*	0.021***	0.024***	0.023***
	(0.008)	(0.007)	(0.007)	(0.007)
Fixed effects:				
Ind	Y	Y	N	N
Firm	N	Y	N	N
Ind x Firm	N	N	Y	Y
Private	Y	Y	Y	N
Time x Year of Birth	N	N	N	Y
N	4,936,477	4,934,911	4,910,582	4,910,582
Adjusted R^2	0.63	0.77	0.86	0.86

Notes: Triple difference estimates from various variants of the specification 4.4. For details see section 4.2. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

Table 5: Impact on Wage: Quadruple Difference Results

	ln(wage)			
Treat x Private	-0.032^{*}	-0.030^{**}		
	(0.018)	(0.013)		
Plann	0.075^{***}	0.061^{***}	0.050^{***}	0.041^{***}
	(0.009)	(0.008)	(0.006)	(0.006)
Conv	0.074^{***}	0.061^{***}	0.059^{***}	0.048^{***}
	(0.011)	(0.010)	(0.008)	(0.008)
Plann x After	-0.021^{*}	-0.012	-0.012	-0.015^{*}
	(0.012)	(0.010)	(0.008)	(0.008)
Conv x After	-0.045^{***}	-0.032^{***}	-0.033^{***}	-0.035^{***}
	(0.014)	(0.012)	(0.010)	(0.010)
Plann x Private	-0.010	-0.019^{**}	-0.016^{**}	-0.013^{*}
	(0.011)	(0.009)	(0.007)	(0.007)
Conv x Private	0.032^{**}	0.022^{*}	0.015	0.017^{*}
	(0.014)	(0.011)	(0.010)	(0.010)
Plann x After x Private	0.014	0.007	0.016	0.017^{*}
	(0.015)	(0.012)	(0.010)	(0.009)
Conv x After x Private	0.074^{***}	0.068^{***}	0.073^{***}	0.075^{***}
	(0.017)	(0.014)	(0.013)	(0.013)
Fixed effects:				
Ind	Y	Y	N	N
Firm	N	Y	N	N
Ind x Firm	N	N	Y	Y
Time x Private	Y	Y	Y	N
Time x Private x Year of Birth	N	N	N	Y
N	4,936,477	4,934,911	4,910,582	4,910,580
Adjusted R^2	0.63	0.78	0.86	0.86

Notes: Quadruple difference estimates from various variants of the specification 4.5. For details see section 4.2. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

Table 6: Impact on Wage: Results From Various Specifications Using Only Women Who Got Pregnant During the Period of Interest

	ln(wage)				
Plann		0.013***	0.023***	0.016***	0.029***
		(0.002)	(0.005)	(0.003)	(0.006)
Conv	0.041***	0.049***	0.019***	0.040***	0.031***
	(0.004)	(0.005)	(0.007)	(0.005)	(0.008)
Plann x After				−0.003	−0.016**
				(0.005)	(0.008)
Conv x After				0.021***	−0.030***
				(0.007)	(0.011)
Plann x Private			−0.011**		−0.015**
			(0.006)		(0.007)
Conv x Private			0.043***		0.015
			(0.009)		(0.010)
Plann x After x Private					0.015
					(0.010)
Conv x After x Private					0.066***
					(0.015)
Fixed effects:					
Ind x Firm	Y	Y	Y	Y	Y
Time x Year of Birth	Y	Y	Y	N	N
Time x Private x Year of Birth	N	N	N	Y	Y
N	1,923,431	1,923,431	1,923,422	1,923,431	1,923,422
Adjusted R^2	0.84	0.84	0.84	0.84	0.84

Notes: This table presents estimates from various specifications. The sample consists of only women who got pregnant during the period of interest. For details see section 4.2. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

Table 7: Impact on Wage: Triple Difference Results for the Sample of Men

	ln(wage)			
Plann	-0.068	-0.009	-0.001	0.003
	(0.054)	(0.035)	(0.009)	(0.009)
Conv	0.016	0.010	-0.006	-0.002
	(0.013)	(0.011)	(0.011)	(0.012)
After_Conv	0.023	0.013	0.031^{**}	0.036^{***}
	(0.016)	(0.013)	(0.012)	(0.011)
Plann x Private	0.056^{***}	0.047^{***}	0.033^{***}	0.031^{***}
	(0.017)	(0.014)	(0.011)	(0.011)
Conv x Private	0.100^{***}	0.033^{***}	0.090^{***}	0.089^{***}
	(0.016)	(0.012)	(0.014)	(0.015)
After_Conv x Private	0.148^{***}	0.083^{***}	0.004	0.002
	(0.019)	(0.015)	(0.015)	(0.014)
Fixed effects:				
Ind	Y	Y	N	N
Firm	N	Y	N	N
Ind x Firm	N	N	Y	Y
Time x Private	Y	Y	Y	N
Time x Private x Year of Birth	N	N	N	Y
N	6,495,964	6,494,247	6,460,407	6,460,407
Adjusted R^2	0.55	0.75	0.82	0.82

Notes: Triple difference estimates from various variants of the specification 4.10. For details see section 4.5. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

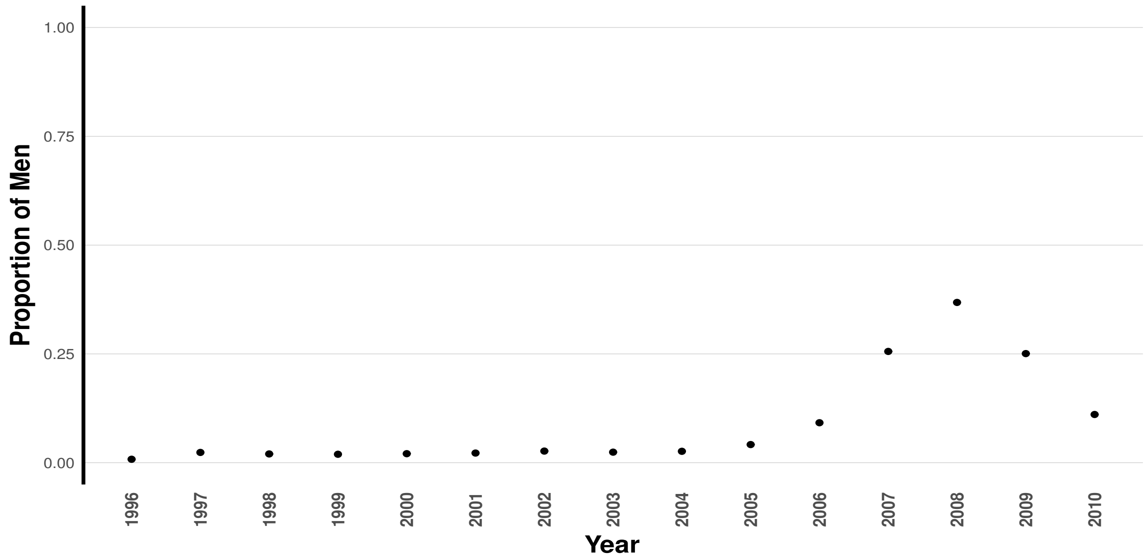
Table 8: Impact on Wage: Triple Difference Results for the Sample of Men

	ln(wage)			
Plann	0.020	0.016	0.004	0.004
	(0.024)	(0.014)	(0.012)	(0.012)
Conv	0.033	0.024	0.005	0.004
	(0.038)	(0.020)	(0.015)	(0.015)
After_Conv	0.079	0.086^{***}	0.067^{***}	0.068^{***}
	(0.056)	(0.029)	(0.024)	(0.023)
Plann x Private	0.023	0.011	0.020	0.020
	(0.029)	(0.016)	(0.014)	(0.014)
Conv x Private	0.049	0.050^{**}	0.071^{***}	0.070^{***}
	(0.045)	(0.023)	(0.019)	(0.019)
After_Conv x Private	-0.104	-0.073^{**}	-0.051^{*}	-0.052^{*}
	(0.066)	(0.033)	(0.028)	(0.027)
Fixed effects:				
Ind	Y	Y	N	N
Firm	N	Y	N	N
Ind x Firm	N	N	Y	Y
Time x Private	Y	Y	Y	N
Time x Private x Year of Birth	N	N	N	Y
N	426,945	426,406	425,514	425,514
Adjusted R^2	0.5	0.79	0.82	0.82

Notes: Triple difference estimates from various variants of the specification 4.10. The sample consists of men whose partner gave birth during the sample period and who chose to receive the parental benefit instead of them. For details see section 4.5. Standard errors in parentheses, robust and clustered at the individual and firm level. Level of significance: ***p<0.01, **p<0.05, *p<0.1

Figures

Figure 1: Proportion of Men Among the Recipients of the Parental Benefit



Notes: This figure displays proportion of men among the recipients of the parental benefit. Year in this figure corresponds to the first year when the benefit was received.

Figure 2: Proportion of Men Among the Recipients of the Family State Benefit

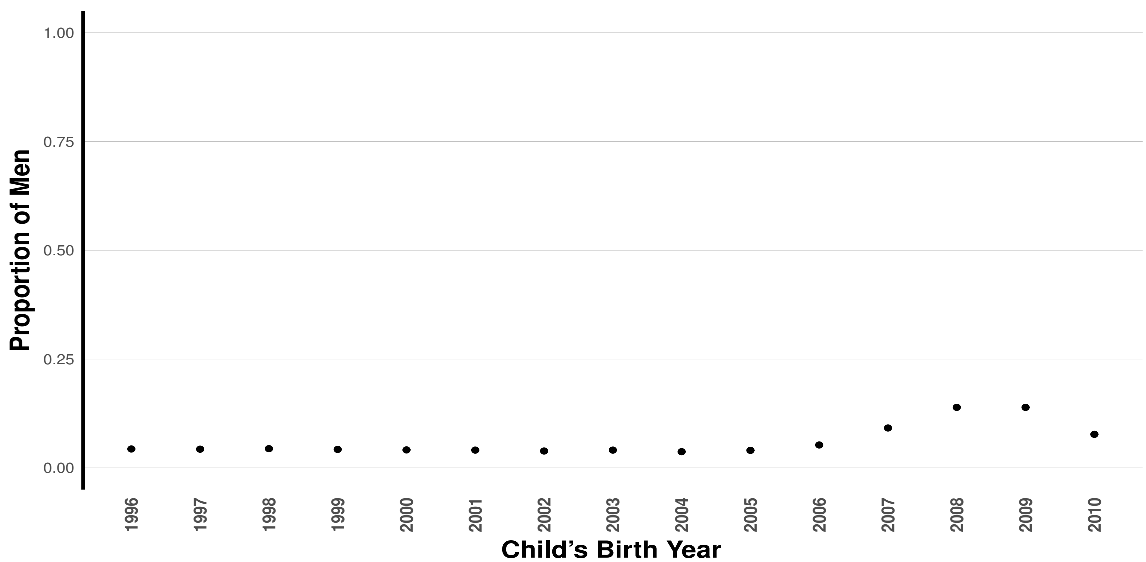
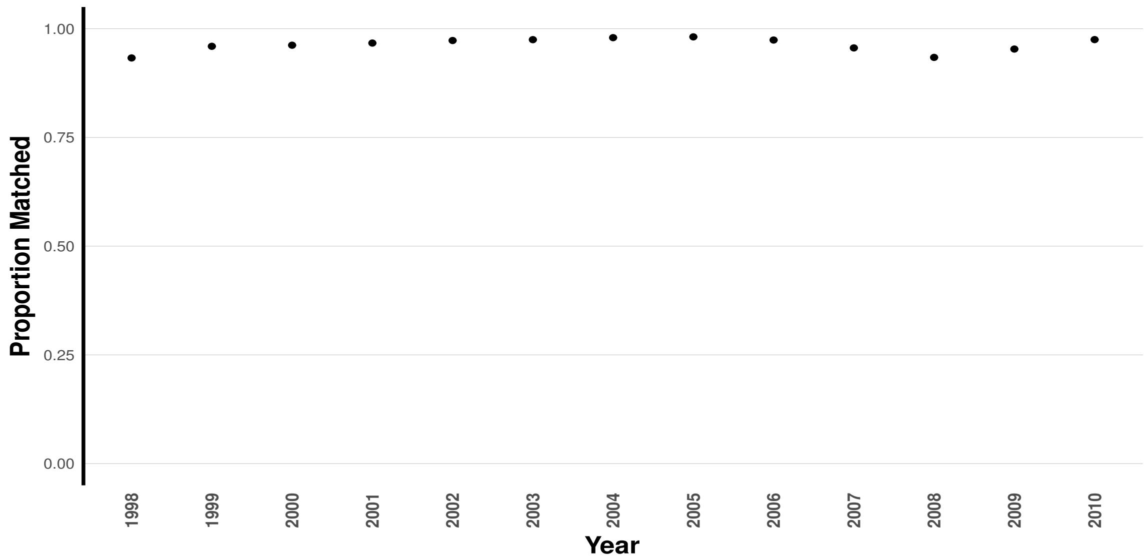
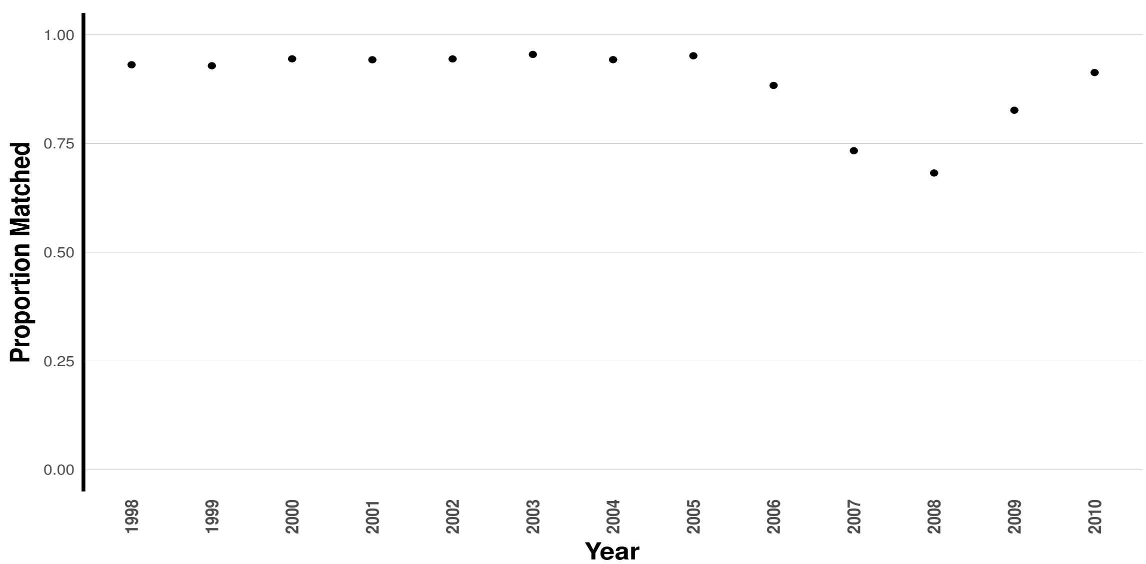


Figure 3: Match Quality



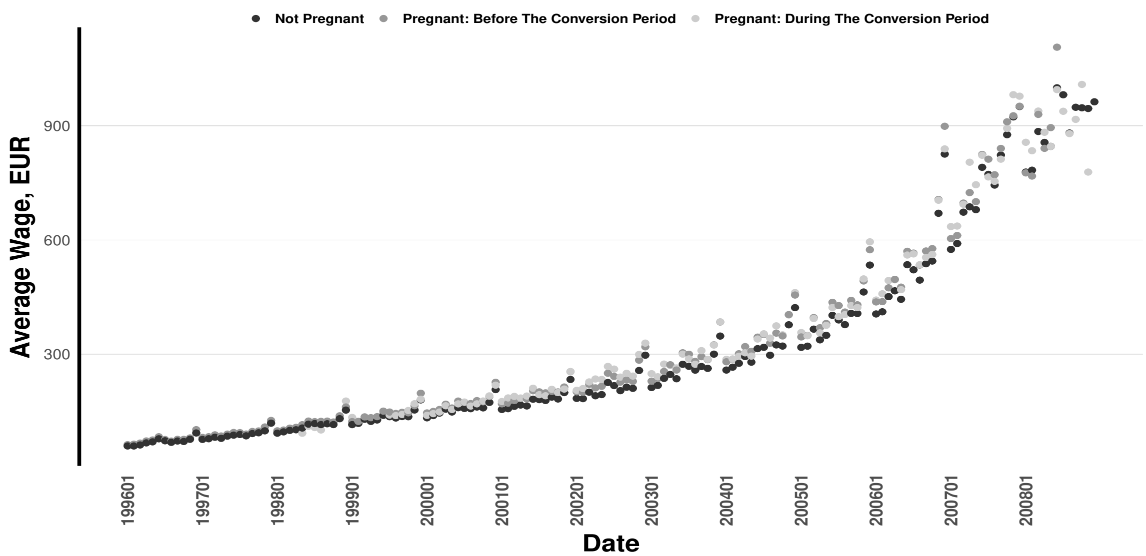
Notes: This figure displays proportion of women contained in the data on the maternity benefits which we managed to match with the data on the family state benefits. Year in this figure corresponds to the first year when the maternity benefit was received.

Figure 4: Proportion of Women Receiving the Parental Benefit



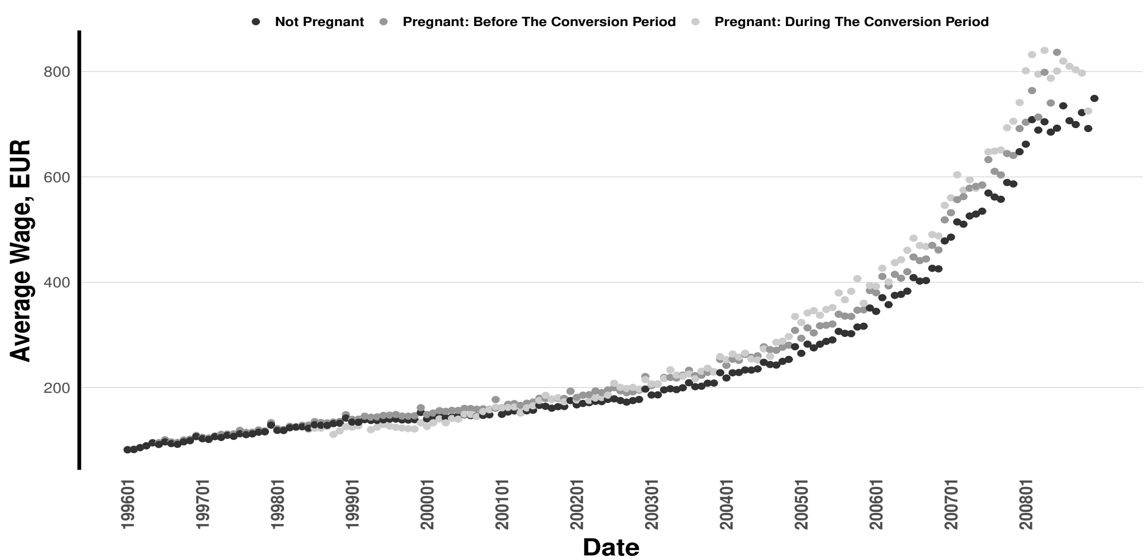
Notes: This figure displays proportion of women contained in the data on the maternity benefits (conditional on receiving also the family state benefits) which we managed to match with the data on the recipiency of the parental benefit. Year in this figure corresponds to the first year when the maternity benefit was received.

Figure 5: Average Wage in the Public Sector: Sample of Women



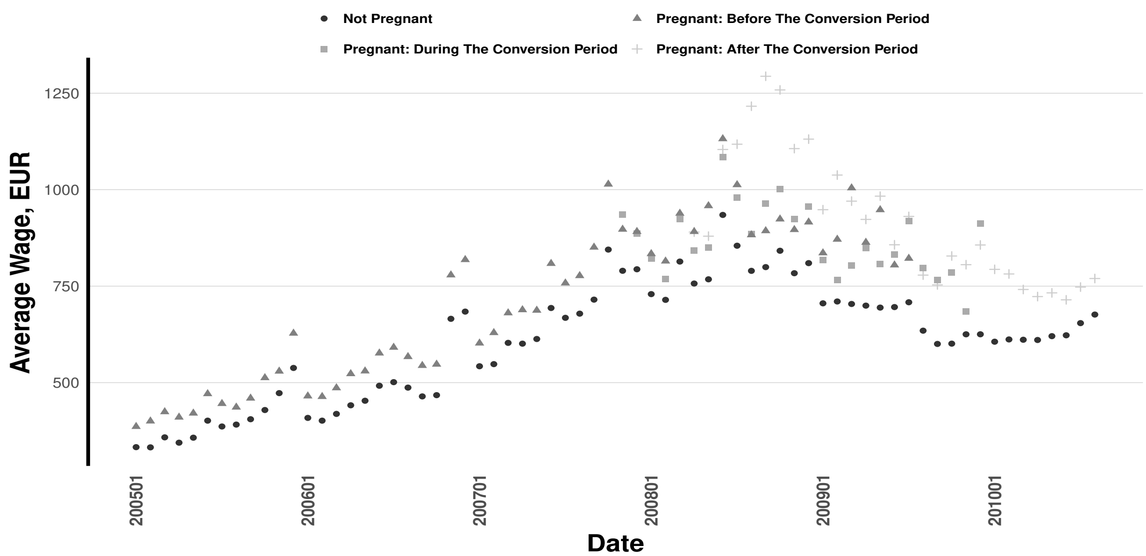
Notes: This figure displays the evolution of an average wage over time for different groups of women in the public sector. Label "Pregnant" in this figure corresponds to women who became pregnant during the sample period.

Figure 6: Average Wage in the Private Sector: Sample of Women



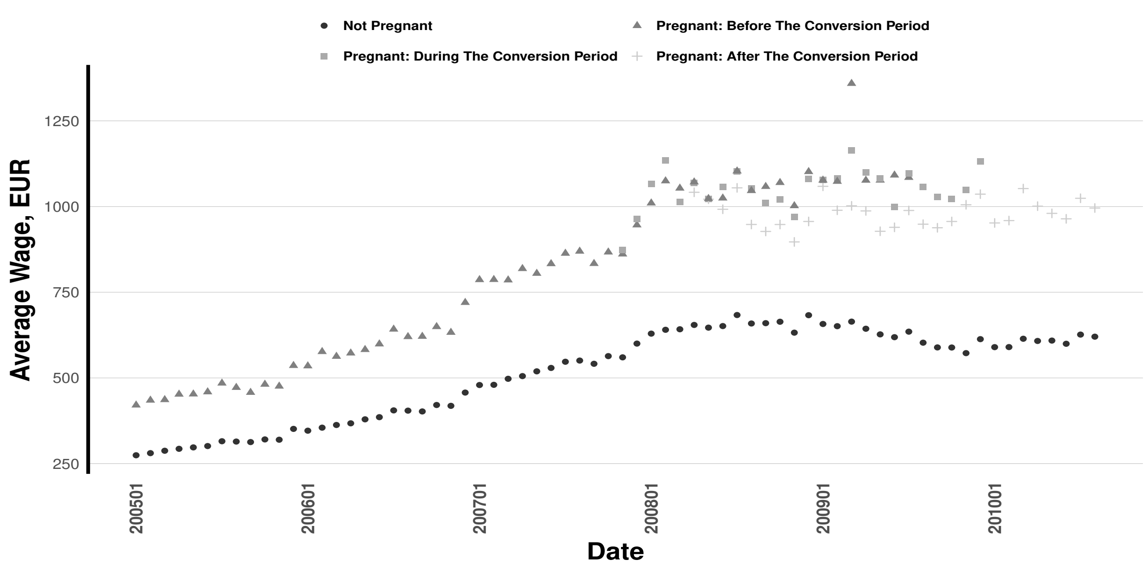
Notes: This figure displays the evolution of an average wage over time for different groups of women in the private sector. Label "Pregnant" in this figure corresponds to women who became pregnant during the sample period.

Figure 7: Average Wage in the Public Sector: Sample of Men



Notes: This figure displays the evolution of an average wage over time for different groups of men in the public sector. Label "Pregnant" in this figure corresponds to men whose partner became pregnant during the sample period and who chose to receive the parental benefit instead of them.

Figure 8: Average Wage in the Private Sector: Sample of Men



Notes: This figure displays the evolution of an average wage over time for different groups of men in the private sector. Label "Pregnant" in this figure corresponds to men whose partner became pregnant during the sample period and who chose to receive the parental benefit instead of them.

Figure 9: Illustration of the Identification Strategy

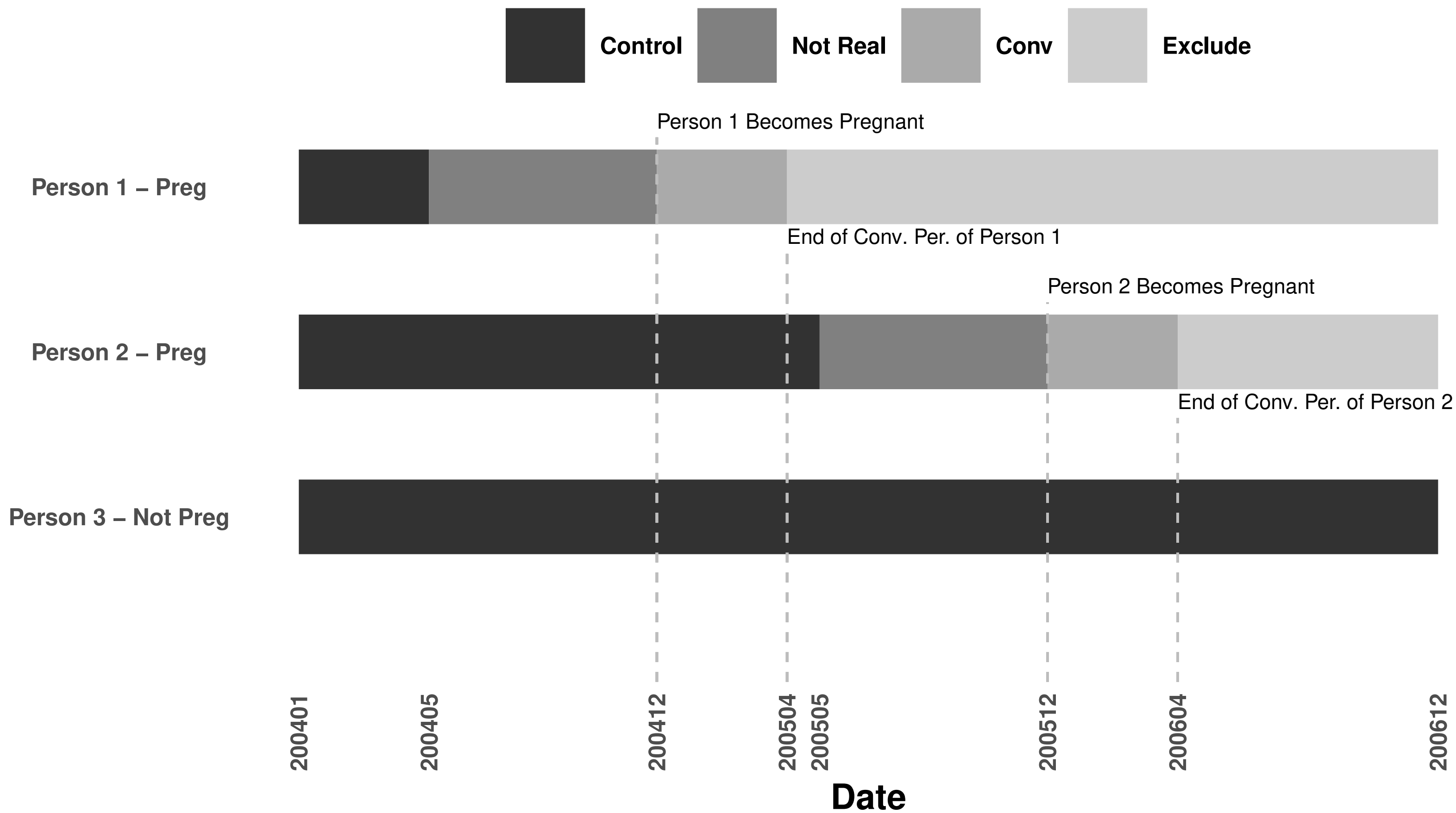
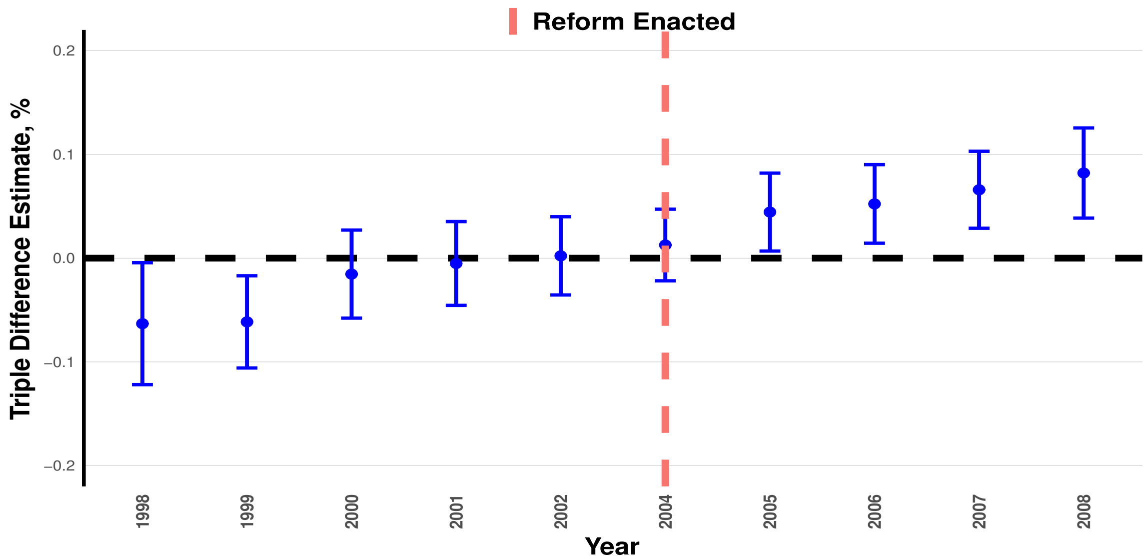
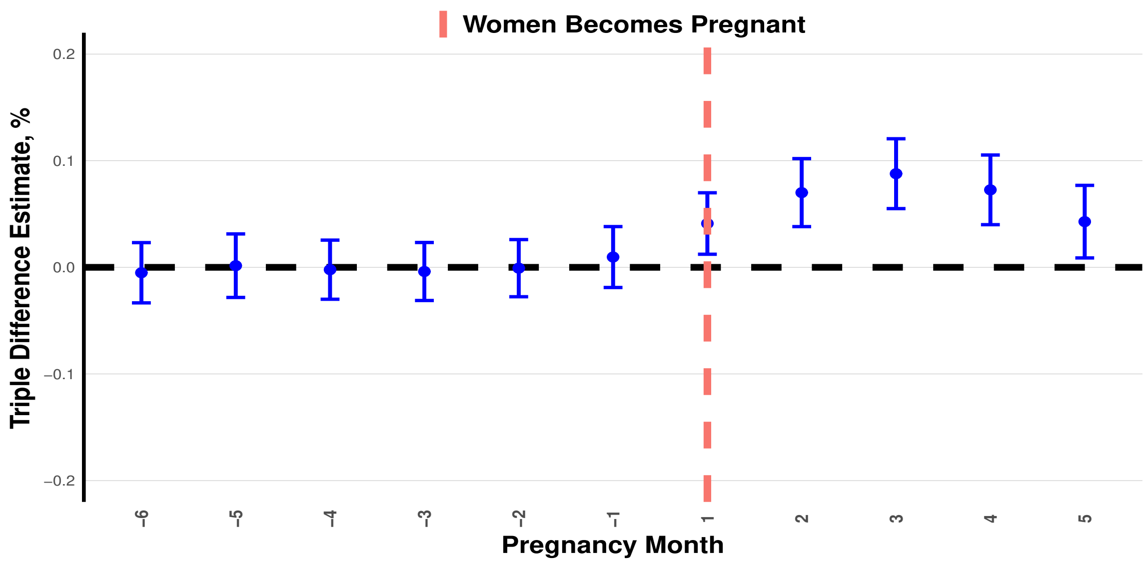


Figure 10: Triple Difference Estimates by Year - Coefficient on $Conv_{it} \times Year_y \times Private_j$



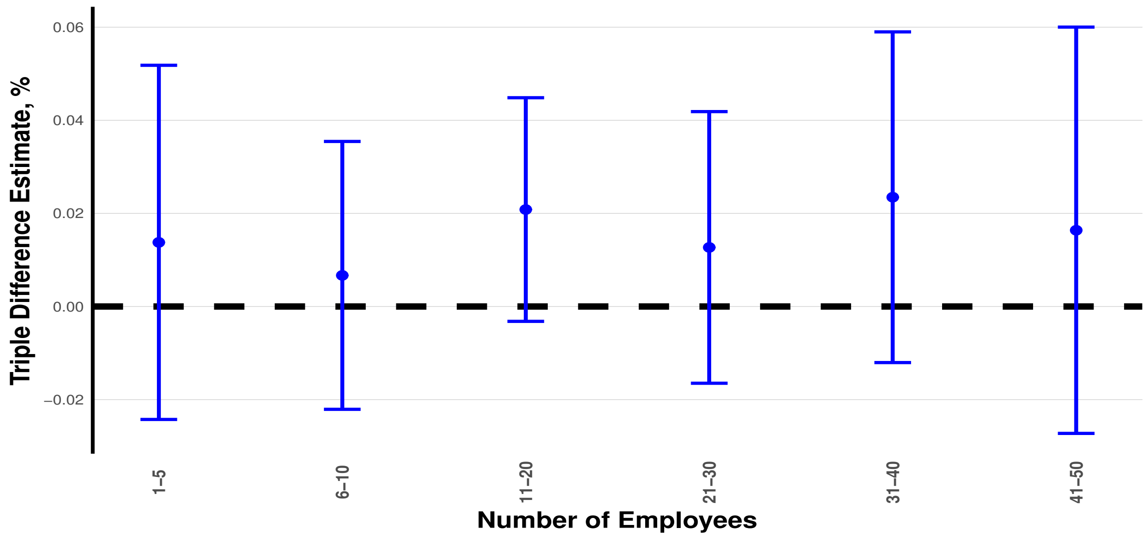
Notes: This figure displays estimates of coefficients on $Conv_{it} \times Year_y \times Private_j$ from the specification 4.6. For details see section 4.3. Planning period is pooled together with a time period before the qualification period. Year 2003 is taken as a baseline year in this specification.

Figure 11: Triple Difference Estimates by Pregnancy Month - Coefficient on $Pm_{ip} \times After_t \times Private_j$



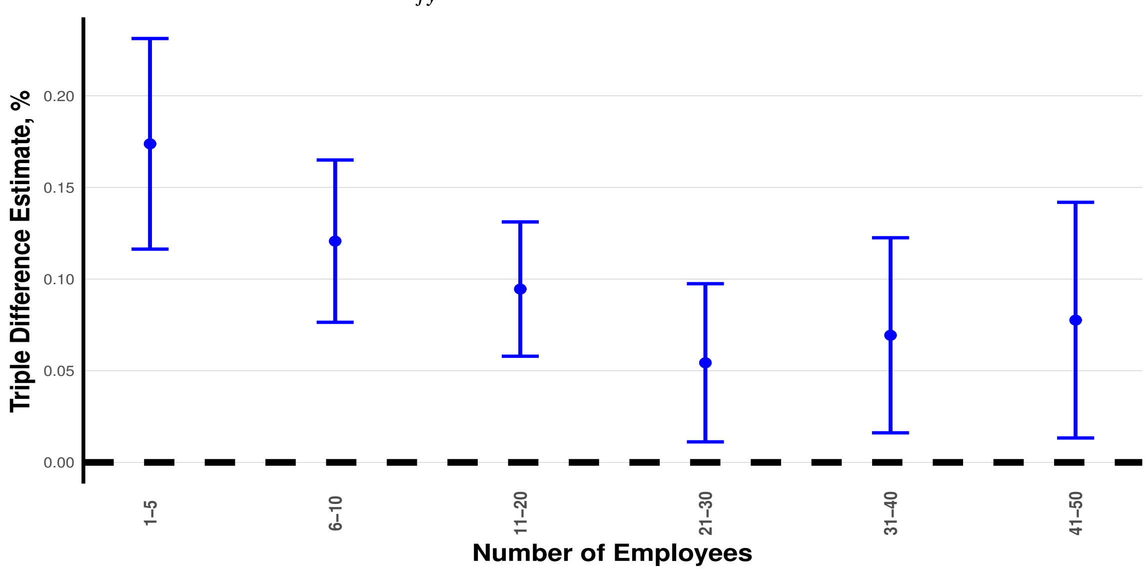
Notes: This figure displays estimates of coefficients on $Pm_{ip} \times After_t \times Private_j$ from the specification 4.8. For details see section 4.3. One month before the pregnancy (month 0) is taken as a baseline month in this specification.

Figure 12: Triple Difference Estimates by Firm Size - Coefficient on $Plann_{it} \times After_t \times Size^s_{jy}$



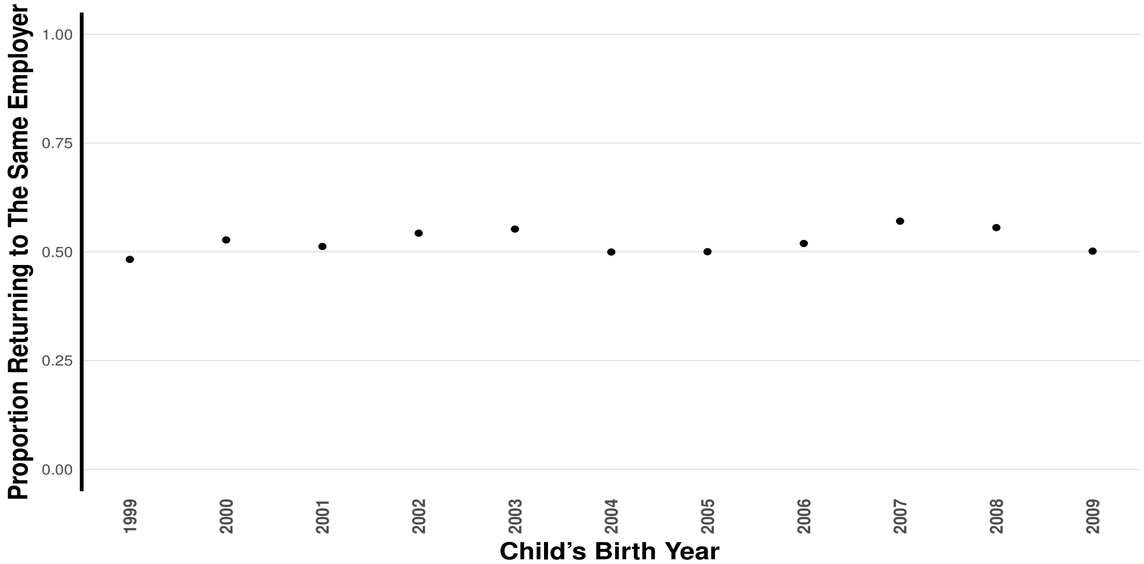
Notes: This figure displays estimates of coefficients on $Plann_{it} \times After_t \times Size^s_{jy}$ from the specification 4.9. For details see section 4.3. Firms with more than 50 employees serve as a baseline category in this specification.

Figure 13: Triple Difference Estimates by Firm Size - Coefficient on $Conv_{it} \times After_t \times Size^s_{jy}$



Notes: This figure displays estimates of coefficients on $Conv_{it} \times After_t \times Size^k_{jy}$ from the specification 4.9. For details see section 4.3. Firms with more than 50 employees serve as a baseline category in this specification.

Figure 14: Proportion of Women Returning to The Same Employer



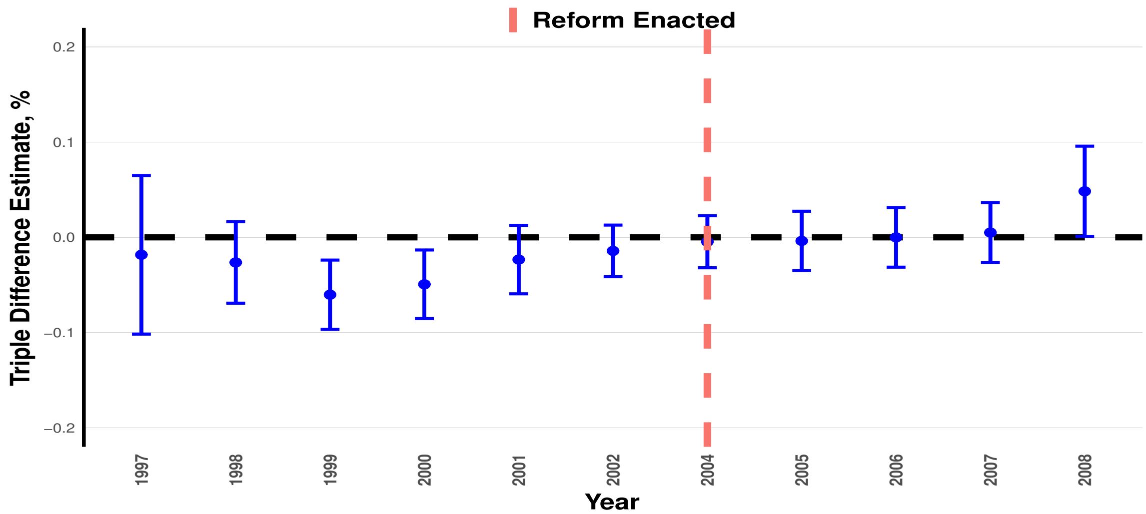
Notes: This figure displays the proportion of women who after giving birth return to the same firm where they worked during the conversion period.

Figure 15: Proportion of Men Returning to The Same Employer

Notes: This figure displays the proportion of men who after their partner has given birth return to the same firm where they worked during the conversion period.

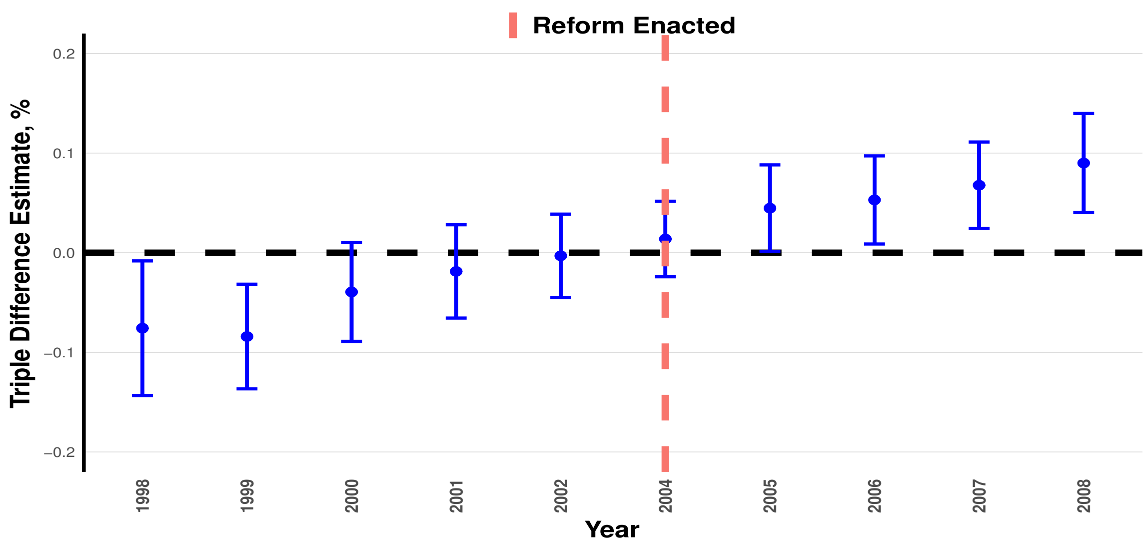
Appendix A Additional Figures

Figure A.1: Yearly Triple Difference Estimates - Coefficient on $Plann_{it} \times Year_y \times Private_j$



Notes: This figure displays yearly tripple difference estimates of coefficients on $Plann_{it} \times Year_y \times Private_j$ from the specification 4.7. See section 4.3 for details. Year 2003 is taken as a baseline year in this specification.

Figure A.2: Yearly Triple Difference Estimates - Coefficient on $Conv_{it} \times Year_y \times Private_j$



Notes: This figure displays yearly tripple difference estimates of coefficients on $Conv_{it} \times Year_y \times Private_j$ from the specification 4.7. See section 4.3 for details. Year 2003 is taken as a baseline year in this specification.

Appendix B Additional Institutional Details

B.1 Parental benefit before the 2005 reform

The parental benefit was introduced in 1995. Until 2005 it was a lump sum benefit paid on a monthly basis to either one of the parents. The benefit was paid for the period that started right after the end of the maternity benefit (i.e. approximately 2 months after childbirth) until the child became 3 years old. As of 2003, the period was shortened to 2 years.

Although the benefit was paid for a long period, it was a relatively modest benefit. During the1997-2003 time period its size was tied to the minimum wage - 90% of the minimum wage until the child became 1.5 years old and 70% of the minimum wage afterwards (amounting to approximately 45% and 35% of the average economy net monthly wage in 2003, respectively). In 2003-2004 the benefit set in absolute terms - EUR 43 per month until the child reached 1.5 years and EUR 11 afterwards (approximately 20% and 5% of the average net wage, respectively). The benefit was not compatible with full-time employment, but it was possible to combine the benefit with a part-time job.

B.2 Maternity benefit

Maternity benefit was introduced in January 1997. This is a contributory short-term benefit aimed at replacing a working woman's earnings shortly before and shortly after the childbirth.

The benefit consists of two parts. The first part is paid for the pregnancy period, and covers the last 70 days of pregnancy if the woman registered with a doctor until the 12th week of pregnancy, and 56 days, if the woman did not register with a doctor until the 12th week of pregnancy. The second part of the benefit covers 56 days after the childbirth if there are no particular childbirth-related health complications, or 70 days in case of health complications or in case of multiple births. The benefit is not compatible with employment, i.e., the woman has to be on maternity leave to be eligible for the benefit.

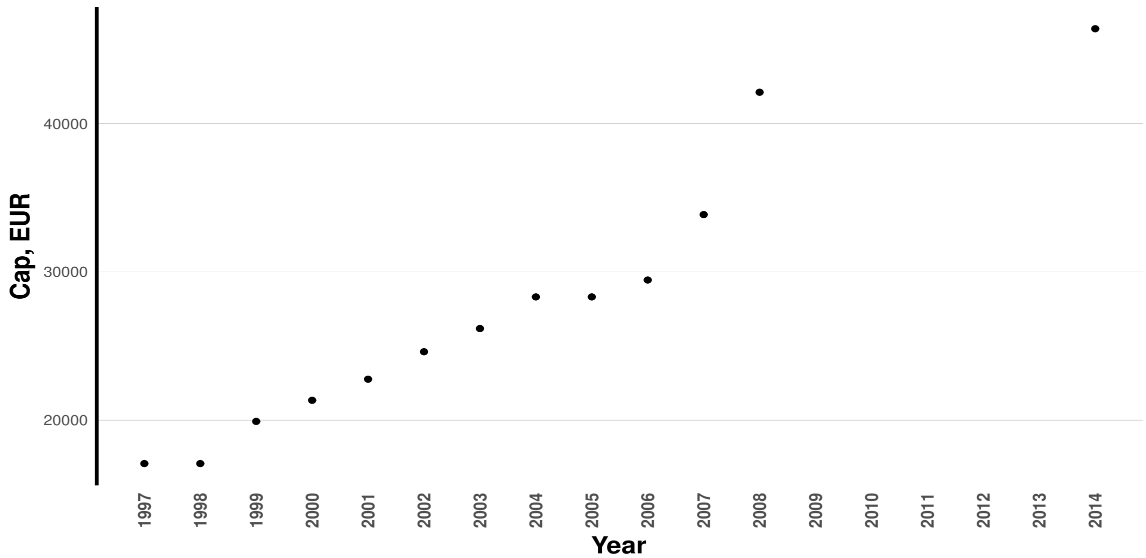
Until November 2010, the size of the benefit was equal to 100% of the mother's average gross monthly earnings, but as of November 2010, it was cut to 80% of the gross earnings. The period that is taken into account to calculate the average earnings has been changed two times since the introduction of the benefit. When the benefit was initially introduced, the average wage was calculated over the last two months in which the woman had non-zero earnings before the childbirth. In August 1998, the period was extended to 6 months and tied to the time of the childbirth - according to the new rules, the period ended 2 months before the benefit entitlement, which is equivalent to approximately 4 months before childbirth. As of January 2010, the period was further extended to 12 months.

B.3 Income cap for social tax

Income subject to social security contributions is capped in annual terms, i.e., it is subject to social contributions only until the cumulative amount received in the course of the year reaches the defined threshold. Any income received above the threshold is not subjected to social security contributions.

The cap was not applied in 1996 (the law "On State Social Insurance" came into force in 1997) and in 2009-2013, when the cap was temporarily removed to constrain the fall in budget revenues during recession. The level of the cap was adjusted upwards almost every year (with the exception of 1998 and 2005) to account for growth in wages (see figure A.3).

Figure A.3: Income Cap for Social Tax



The cap affects a very small number of employees: the share of employees whose wage was capped did not exceed 0.7% in any of the years. For women, the share did not exceed 0.4% (see figure A.4).

Figure A.4: Income Cap for Social Tax