Female Labor Supply and Intergenerational Spillovers: Evidence from a Tax Reform *

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Abstract

Female labor supply has increased substantially over the past century. Consequently, more women and men have observed their mother employed which may affect their own labor supply. In this paper, I exploit a tax reform which stimulated labor supply among women with low labor force attachment. Mothers directly affected by the reform increased their labor supply. However, daughters of these women reduce their labor supply and are more likely to have kids and be married. I do not find any effects on the labor supply or fertility of sons. Hence, my results show that a reform which stimulated maternal labor supply can have unintended negative effects on the labor supply of their daughters.

Keywords: Female labor force participation, maternal labor force participation, intergenerational transmission

JEL codes: J13, J16, J24,

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1 Introduction

Female labor supply has increased substantially over the past century, although the increase has slowed down or plateaud more recently (Blau and Kahn, 2017; Goldin, 2006; Olivetti and Petrongolo, 2016). In particular, labor supply of mothers has increased (Kuziemko et al., 2020). Consequently, more women and men have observed their mother employed which may influence their own labor supply. As the remaining gender gaps in labor market outcomes are mostly related to having children (e.g. Albanesi et al., 2022; Cortés and Pan, 2020), a mother's experience combining market and household work may be particularly relevant for her daughter's labor market decisions. Understanding such spillover effects is important as they contribute to future trends in female labor supply. Furthermore, these spillover effects impact the effectiveness of any policy which stimulates female labor supply.

The effect of maternal labor supply on her daughter's labor supply can be positive or negative. Maternal labor supply may increase daughter's labor supply as working mothers may change gender norms or provide favorable information on the benefits and costs of maternal labor supply (Fernández et al., 2004; Fernández, 2013; Fogli and Veldkamp, 2011; Gay, 2023).[1] However, combining work and family may be very costly. This is illustrated by women experiencing upon motherhood a decline in labor market outcomes (e.g. Angelov et al., 2016; Kleven et al., 2019a,b; Andresen and Nix, 2022) and mental health (Ahammer et al., 2023). A working mother may increase the salience of these costs to her children. As women have been shown to underestimate these costs prior to motherhood (Kuziemko et al., 2020), observing the realized costs for their mother may negatively affect the beliefs of daughters.

In this paper, I use a 2012 tax reform in the Netherlands which stimulated married women with weak labor force attachment to increase their labor supply to estimate the effects of this on the labor supply of these women and their daughters and sons. Prior to 2012, married women with low or no earnings received the full amount of a tax credit

[1] These channels may partially overlap. Norms are seen as the part of culture that describes how people should behave (Boelmann et al., 2021). Culture is commonly defined in economics as the systematic differences in beliefs and preferences (Fernández, 2011). Hence, learning about the costs of maternal employment may change the beliefs about the costs of maternal labor supply and thus change culture (Fernández, 2013).

even if they did not fully qualify for this through their own earnings, essentially a tax subsidy.[2] As a result, when their earnings increased, the amount of tax credit received did not change and therefore these women faced much higher participation and marginal tax rates compared to single women. I exploit that this tax subsidy was phased out for women born on or after January 1^{st}, 1963, while those born before this date retained the subsidy. This lowered the household income and the participation tax rate for these younger cohorts, who turned 49 years old in the year of the reform. Hence, I expect these women to increase their labor supply in response to the increased financial incentive. I use this to study whether there are spillover effects onto the labor supply of their daughters and sons, who are aged 12 to 25 at the time of the reform and live in the same household as their mother.

This reform is an interesting setting to study intergenerational spillovers of female labor supply. First, as people born before 1963 are exempted from the reform, this allows me to estimate the causal effect of this reform in a regression discontinuity framework by comparing women born before and after 1963, and their children. Importantly, their children all face the same tax regime. Identification relies on the assumption that in absence of the reform, all outcomes for these women (children) should evolve smoothly by birth month (birth month of their mother). Any discontinuity in outcomes at the cutoff can then be attributed to the reform. Second, the administrative data in the Netherlands allows me to link women and men to their mothers which allows me to examine the impact of the reform both on mothers' and childrens' outcomes. Moreover, I can combine labor supply, education and family formation outcomes, which allows me to study both the labor supply response and other, related outcomes. Third, although stimulating the labor force participation of older mothers may have different spillover effects on children than stimulating younger mothers, as children have already observed their mothers out of employment for many years and for mothers re-entering the labor market at a later age may be difficult, they are a relevant group for policy makers who want to increase the labor force. These mothers likely reduced their labor supply when their children were younger, but as their children are older and require less care these mothers may want to increase their labor supply. Finally, the Netherlands is an interesting context as there are

[2]More precisely, this applied to married people who are the lowest earning partner. In the vast majority of eligible families, the least earning partner is female (Lok, 2009).

large differences in labor supply between men and women.[3]

First, I study the effects on the mothers directly affected by the reform. As expected, I find that these mothers increase their labor force participation in response to the reform. Eight years after the introduction of the reform, at age 57, they are 2.3 percentage point more likely to be employed. Their income increases, but only along lower parts of the income distribution. Moreover, they are more likely to work at least one hour a week, but I do not find an effect on working 12 or more hours a week. Hence, these mothers increase their labor supply but most do so by working few hours in low-paid jobs.

Second, I study spillover effects of the reform on daughters and sons. Daughters decrease their labor supply in response to the reform. This effect is concentrated among older daughters aged 19 to 25 at the time of the reform. In 2020, when these daughters are aged 27 to 33, they are 3.5 percentage points less likely to work and rank 2 percentiles lower in the income distribution. Furthermore, these older daughters are more likely to live with a partner and have children. I do not find any effect on the labor supply and fertility of sons. This implies that there exists a gendered component in how children are affected by their mothers' labor supply.[4]

To test the validity of my identifying assumption, I perform various placebo checks. First, I do not find differences in predetermined characteristics or outcomes in pre-reform years. Moreover, I estimate a placebo-in-time, where I shift the threshold to preceding birth years. I do not find an effect on these other birth years. Furthermore, my results are robust to changing the bandwidth and other sensitivity checks, such as omitting control variables, including a quadratic polynomial and a uniform kernel.

Several mechanisms could potentially explain the spillover effects I find. A standard labor-leisure model suggest that there are three potential channels that could drive the

[3]Females work on average ten fewer hours a week than men (CBS, 2022b). There is a substantial child penalty for women of around 47%, (Artmann et al., 2022; Rabaté and Rellstab, 2022). Moreover, these differences also exist among younger cohort as 46% of women aged 25-30 work part-time, compared to 20% of men (CBS, 2022a).

[4]The reform also directly affects mothers' income through the loss of the tax subsidy which may also affect daughters and sons outcomes. However, it is unlikely that this income loss drives the spillover effect I find on daughter's labor supply. If there is an effect of income loss on labor supply of children, this would likely operate through a reduction in human capital. I find no evidence for a change in education level among daughters nor sons. Moreover, this income reduction would likely affect sons and daughters similarly.

reduction in labor supply of the daughters. First, mothers increase labor supply and spend more time outside the home which may decrease daughters' human capital and subsequent wages. Hence, the benefits of working would decrease for daughters. However, I do not find an effect on education level of the daughters which is not in line with this mechanism. Second, as mothers increase their labor supply they are less available to provide child care for grandchildren. This would increase the costs of employment for daughters as they have to purchase formal childcare. However, I show that the change in daughters' labor supply is similar for daughters who live close to their mothers, for whom this channel is arguably most relevant, and those who live far from their mothers, for whom this channel is less relevant. Hence, the results are unlikely to be driven by this channel.

Finally, the preferences for work and leisure, or the perceived benefits and costs of work may change for daughters following the reform. Daughters may observe their mother struggle when combining market and household work. This can increase daughters' perceptions on the costs of combining work and family. As a result, some daughters may decide to not follow the example set by their mother and therefore reduce their labor supply and have children earlier. Alternatively, daughters may observe their mother successfully re-enter the labor market. This may decrease daughters beliefs about the costs of career interruptions and lead them to temporarily reduce their labor supply after having children.

My paper contributes to the literature on the intergenerational transmission of female labor force participation. Maternal labor supply is correlated with daughters labor supply (Farré and Vella, 2013; Morrill and Morrill, 2013; Johnston et al., 2014) and daughter-in-law's labor supply (Morrill and Morrill, 2013; Bütikofer, 2013; Schmitz and Spiess, 2022). Studying the causal effect of maternal labor force supply on the labor force participation of their daughters and sons is challenging as it requires exogenous variation in maternal labor supply. Hence causal studies on this are scarce. Fernández et al. (2004) use World War II as a shock to female labor participation and show that this has positive spillover effects on the next generation of women, by affecting the preferences sons have for a working partner. Gay (2023) uses a similar strategy using World War I in France and shows positive spillover effects on female labor supply driven through mother-daughter, mother-daughter-in-law and local social interactions. Another set of papers uses the

epidemiological approach, comparing migrants who have different norms or culture in their home country or region but are exposed to the same current norms and institutions, and finds that home culture affects female labor supply (e.g. Fernández and Fogli, 2009; Blau et al., 2011; Boelmann et al., 2021). This approach however, cannot distinguish between the effect of one own mothers' labor supply and culture. The labor supply of other women such as peers (Cavapozzi et al., 2021), mothers of peers (Olivetti et al., 2020), and family (Nicoletti et al., 2018) has also been shown to positively affect women's labor supply. I add to this literature by using a reform which results in exogenous variation in mothers' labor supply, not related to culture, and which allows me to estimate the causal effect of maternal labor supply on daughter's labor supply in a modern context.

In contrast to the existing literature, I find negative spillover effects of maternal labor supply on daughters. Fernández (2013) proposes a model of intergenerational transmission of female labor supply in which women learn about the costs of maternal employment both from their mother and women working in the previous generation. Through the lens of this model, there are multiple potential explanations for this difference between my results and the existing literature. First, the existing causal evidence on the intergenerational transmission of female labor supply is from World War II (Fernández et al., 2004; Gay, 2023), when maternal labor supply was low. As maternal labor supply is more common today, daughters may already learn about maternal labor supply by observing these women and the additional effect of their mother's labor supply may be smaller. Second, the costs of maternal labor supply may have increased over time and therefore observing your mother working today may give a negative signal of the costs of maternal labor supply. Third, I study mothers who increase their labor supply due to a financial incentive and thus are likely to have high disutility of labor. This may affect how daughters perceive the costs of maternal employment. In other settings, such as World War II (Fernández et al., 2004; Gay, 2023) and using exogenous variation in peers' labor supply (Cavapozzi et al., 2021; Olivetti et al., 2020; Nicoletti et al., 2018), the working women are more likely to have lower disutility for labor as they may select into employment for other reasons. Finally, children are teenagers and in their early twenties when they observe their mother being employed. It could be that at older ages, the costs their mothers face combining market and household work are more salient to daughters.

Second, I contribute to the literature on female labor supply and taxation. The tax

subsidy I study shares characteristics with joint taxation; second earners face higher participation tax rates than singles or main earners. A small literature has looked at the effect of joint taxation on female labor supply. This literature has found that joint taxation is associated with lower female labor supply (Bick and Fuchs-Schündeln, 2017). Moreover, a large literature exists on the EITC and its effect on female labor supply. Most of this literature finds positive effects on the labor supply of single mothers, in particular along the extensive margin, and negative effects on married mothers, for whom the EITC creates a disincentive to work (Nichols and Rothstein, 2016; Hoynes and Rothstein, 2017).[5] Bastian (2020) shows that introduction of the EITC in the US not only increased the labor supply of mothers, but also changed views on gender equality. Hence, these types of public policies can affect gender norms.[6] I contribute to this literature by estimating the spillover effects of taxation on the labor supply of the next generation.

The rest of the paper is as follows. In Section 2, I discuss the institutional setting in more detail. In Section 3, I discuss the data I use. Next, I discuss the methodology in Section 4. In Section 5, I discuss the results and robustness checks. In Section 6, I discuss potential mechanisms that could drive these results. Finally, in Section 7 I conclude.

2 Institutional setting

The reform I study is part of the tax system in the Netherlands. First, I describe the policy prior to the reform. Second, I describe the reform and policy after the reform. In the Dutch tax system, income is taxed individually, with the possibility of transferring certain tax deductions between partners. The general tax credit ('algemene heffingskorting') is accessible to all taxpayers and is deducted from their income tax. As this is a tax credit, only the amount that covers the income tax bill is paid out. In 2009, the tax credit was 2,007 euros. As a result, people effectively do not pay taxes over the first thousands of euros earned as the tax credit covers the tax payment (see Figure 1, left panel).

Households with two partners in the labor force can claim the tax credit twice, while single earner families can only claim it once. This was deemed unfair, hence the government allows partners to transfer the tax credit between partners. Partners can 'transfer' the tax credit when one partner cannot claim the full credit based on their own income

[5] Although Kleven (2020) argues that the EITC only had limited effects on female labor supply.

[6] Ichino et al. (2023) show the reverse: the labor supply response to taxation depends on gender norms.

tax, and the other partner pays sufficient income tax to cover the tax credit twice. The full tax credit is paid to the partner with lower income, although they only qualify for part of it based on their individual income. For brevity, I will refer to this as the tax subsidy. A person qualifies for this tax subsidy under two conditions. First, they must not fully qualify for the general tax credit through their own income. In 2009, this is equivalent to earning less than 5,991 euros a year (36% of the minimum yearly income). Second, their partner must pay sufficient income taxes, which are equal to at least twice the general tax credit. In 2009, this is equivalent to earnings at least 16,472 euros a year (99% of the minimum yearly wage).

The tax subsidy has two consequences. First, it increases income for eligible households. Second, it increases the participation tax rate for the eligible second earners. Figure 1, Panel a illustrates this, the left figure shows net income, tax credit and income tax as a function of gross income for single women in 2009. For the first 5,991 euros of gross income, the tax credit and income tax fully cancel out. As a result, people effectively do not pay income taxes; post-employer contributions income and net income are equal. The right figure, shows this figure for married women with a partner who earns at least the minimum wage full time. These women receive the full tax credit as a tax subsidy if they have no income themselves. Once they start earning income, both the income tax and the tax credit increase and fully cancel out. However, the tax subsidy decreases by the same amount as the tax credit increases. The tax subsidy and tax credit decrease and increase one for one. Hence, they effectively start paying income taxes from the first euro earned. As a result, the participation tax rate for these women is much higher compared to that for single women. If married women earn enough to fully claim the tax credit themselves, they face the same tax rates as single women.

The Dutch government introduced two consecutive reforms which limited the transferability of the general tax deduction in order to stimulate female labor force participation. These reforms gradually reduced the portion of tax deduction specific groups could transfer to a partner, effectively phasing out the program for these groups. I focus on the second reform, which expanded the group of women for whom transferability was phased out, creating two distinct tax treatments for married people solely based on the birth year of the least earning partner.[7]

[7]The first reform is not relevant for my paper, as it applied only to household in which the least

This reform reduced the amount of the tax credit that can be transferred for households in which the least earning partner was born in 1963 or later. This reform was announced by the newly formed government in September 2010, and went into effect on January 1^{st}, 2012. In the first three years, the amount of the tax credit 'treated' households could transfer reduced by 13.3 percentage points per year. From 2014 on, the amount reduced by 6.6 percentage point a year.[8] The amount to be transferred was fully phased out in 2023. Households in which the least earning partner was born before 1963 are not affected, they remain eligible to transfer the full amount of the tax credit. Figure 2 shows the euro amount and percentage of the general tax deduction that can be transferred to a partner based on year of birth of the least earning partner. Figure 1, Panel b displays the tax scheme for married women who are the least earning partner in their household when the tax subsidy is fully phased out. For women born before 1963, the tax scheme is the same as before. For women born in 1963 or later, the tax scheme has changed. These women lose the tax credit and phase a lower participation tax rate. The phase-out of the tax subsidy increases the gap between these two tax schemes over time.

3 Data and descriptive statistics

3.1 Data

I use various administrative data sets from Statistics Netherlands. Individuals can be linked across these different data sets using a unique identifier. Moreover, I can link children to their parents, which allows me to study the effects of the reform both on mothers directly affected by the reform and their children.

I obtain earnings from income records, this includes income from paid and self-employment. From these income records, I also obtain the percentile rank in the earnings

earning partner was born after January 1^{st}, 1972 and without children aged under six; hence none of the women I study are affected by this reform. This reform reduced the part of tax deduction that could be transferred by 6.6 percentage points a year for this group, thereby fully abolishing it in 15 years. It was announced in 2007 and went into effect in 2010.

[8]The phase-out rate was higher in the first three years to align the phase-out scheme of this reform with the phase-out scheme of the first reform.

distribution in the Netherlands. I classify people as 'employed' if they have positive earnings in the year. Furthermore, I observe whether people are in paid-employment or self-employed. For people in paid-employment, I obtain the weekly hours worked during the year of all their jobs combined.

Furthermore, I obtain household and family formation outcomes. I observe whether someone is cohabiting or married on December 31^{st} of each year. I also use this to identify partners. Furthermore, I observe the number of children mothers have and whether these live in the same household as their mother on December 31^{st} of each year. From municipality records, I obtain the muncipality and province in which mothers and their children were residing on December 31^{st}, 2009.

For the children's generation, I observe both highest education level and current education enrollments from administrative education records. From this, I construct the finished education level and current education level for children. For mothers, I observe highest obtained education from two sources. First, for a random subsample I observe the education level in *Enquête Beroepsbevolking*, a large representative survey. Second, for mothers who were enrolled in university after 1983, I observe their education level from administrative records. Hence, I do not observe education level for all mothers. As I will include education level as a control variable, I recode missings to zero and create a dummy indicating education level is unknown.

I restrict the sample to women who are born between 1960 and 1965 in the Netherlands.[9] I link these women to their partner, married or cohabting, on December, 31^{st} 2009. I identify women who are eligible for the tax subsidy in 2009, i.e. before the reform was announced. Eligibility is based on their own earnings, which should be lower than 5,991 euros a year, and their partners earnings, which should exceed 16,471 euros a year. I further restrict the sample to women who have children aged 12 to 25 in 2012 that live in the same household prior to the announcement of the reform. I choose this age range as these children are old enough at the end of the time frame to have meaningful labor market and educational outcomes, and are young enough for these choices to still be changed.[10] I impose the restriction that children live in the same household as their mother prior to the reform as this makes it more likely that children are exposed

[9]Later, I further restrict this sample to women born in a 10 month bandwidth around the threshold.

[10]I can also observe educational outcomes such as tracking in high school for younger children. However, the number of women in this age range who have younger children is rather low.

to their mothers' treatment.[11] This sample consists of 57,110 mothers, 52,126 daughters and 58,338 sons. In my analysis, I further restrict the sample to mothers who were born in an 10 month bandwidth around the threshold, which results in 26,136 mothers, 23,864 daughters and 26,900 sons.[12]

3.2 Descriptive statistics

Table 1 displays some descriptive statistics for mothers measured prior to the reform. For comparison, the descriptive statistics for all women in these birth cohorts are displayed in the last column. The mothers were on average 28 years old when their first child was born and have 2.4 children on average, of which most still live in their household. The mothers in my sample have more children compared to the average women of their age, who only has 1.8 child. I only observe education level for 22% of the mothers. Among those for whom I do observe their education level, 17% has finished vocational education, 15% upper vocational education, 11% college and 5% academic. The remaining group did not finish post-secondary education. A third of the mothers resides in the 'Bible Belt', a region with more conservative gender norms. In the seven years preceding the announcement of the reform, the mothers are on average employed in 2.4 of the years. In 2009, 35% of the mothers are employed, 20% is in paid-employment and 16% is self-employed.[13]. However, average earnings and hours worked are rather low at 1,165 euros and 1 hour per week. respectively. The labor supply of these mothers is much smaller than the average labor supply of women of their age. This is not surprising, as I eligibility for the tax subsidy is based on low labor supply. One exception, is that the eligible mothers are more likely to be self-employed compared to the average women. Their partner's labor supply is much higher and also higher than the labor supply of the partner of the average women. The partners of my sample are all employed in 2009 as this is necessary for eligibility for the tax subsidy. The far majority, 88% is in paid-employment and 16% are self-employed although this is slightly more common in the treatment group. Partners earn on average

[11]It is not uncommon for children to live at home at these ages. I lose 13% of the sample of children by imposing this restriction. In 2009, 87% of all 18-year-olds, 66% of 20-year-olds and 24% of 25-year-olds lived with one of their parents (CBS, 2023a).

[12]The number of sons is larger than the number of daughters as daughters leave their parental home at younger ages on average (CBS, 2023b).

[13]Note that these two are not mutually exclusive

80,415 euros a year. Hence, on average the mothers in my sample have high earning partners.

Table 2 displays descriptive statistics for daughters and sons separately. I further split the sample by age of the child in 2012, children aged 12-18 and 202-5, as the outcomes differ between the younger and older children and I will also analyse the effects on daughters and sons split by age. Among the older daughters and sons, around 84% are employed in 2009. Daughters work on average 11 hours per week, sons slightly more, around 14%. Employment can be their main activity or a side job next to education. Labor supply of the younger children is much lower, around 9% of them are employed. Daughters have slightly higher education level compared to sons. Two-thirds of the older daughters are enrolled in education, this is slightly lower among the sons. Moreover, daughters tend to be enrolled or have finished more often the higher levels of education. Among younger daughters and sons, 63% are enrolled in post-primary school education. In the Netherlands, children typically enroll in high school at age 12, hence some of the children will still be enrolled in primary school in 2009. For the children not yet enrolled in high school, I do not observe the education track. Academic track enrollment appears to be very high among these younger children, however in the first three years of high school I cannot distinguish between academic and college track and therefore I assign these children to college track. Finally, family formation is a rare to observe for these children prior to the reform. In 2009, 0.4% of the older daughters has a partner and 0.64% has a child. Among the older sons, these outcomes are even rarer.[14]

4 Methodology

4.1 Identification strategy and empirical model

I exploit a discontinuity in tax treatment based on birth date of the mother that arises after the reform. The reform phased out the tax subsidy for women born on or after January 1^{st}, 1963.[15] The identifying assumption is that in absence of the reform, outcomes

[14]I do not report these summary statistics among the younger children, as the outcomes are extremely rare and thus too low to report on due to privacy concerns.

[15]More precisely, the tax treatment is based on the birth year of the least earning partner, irrespective of gender. In my sample, all mothers are the second earner in the household prior to the reform and hence this reform applies to them. Moreover, in the majority of eligible families, the least earning partner is

for mothers and their children would have evolved smoothly across the cutoff (Hahn et al., 2001).[16] This assumption is reasonable because women born just before or just after January 1^{st} are likely to be very similar.[17] Hence, outcomes are expected to evolve smoothly across this threshold. Moreover, birth month cannot be manipulated ex-post. To address anticipation effects that could change the composition of the treatment and control group, I select women who are eligible for the tax subsidy prior to the announcement of the reform. Finally, I do not know of any other cut-off on the same birth date.[18] This assumption naturally extends to their children. Importantly, all children in my sample have been born before the announcement of the reform.

I use the local linear approach with a polynomial of order one and the triangular kernel function as suggested by Cattaneo et al. (2020). I use a bandwidth of 10 months on either side of the cut-off, which corresponds the median of the MSE-optimal bandwidths, hence women born between March 1962 and November 1963 are included or their children.[19] In the specifications for daughters and sons I cluster the standard errors at the mother level.

female (Lok, 2009). Since the sample of eligible men around the threshold is small, I restrict my analysis to women.

[16]Hence, I use the continuity based framework for identification. An alternative framework is local randomization, which views the RD-setting as an experiment in a small window around the cutoff and uses an exclusion restriction as identifying assumption: within a window around the cutoff the running variable can only affect potential outcomes through treatment assignment (Cattaneo et al., 2015). The birth month of the mother is likely to directly affect potential outcomes, either through 'age' or 'generation' effects. Therefore, the identifying assumption of the continuity based framework is more appropriate in this setting. The continuity-based framework can be used with a discrete running variable if extrapolation from below the cutoff to the cutoff is accurate and if the number of unique values of the running variable is sufficiently large (Cattaneo et al 2022). As the running variable is birth month, the extrapolation is a birth month difference and is likely sufficiently small.

[17]Seasonality in parental characteristics could be a concern (Buckles and Hungerman, 2013). However, I do not find evidence for differences in pre-reform characteristics of mothers (Table 3). I discuss the validity of this assumption in more detail below in subsection 4.2

[18]For these cohorts, October 1^{st} was used to assign children to grades in the education system rather than January 1^{st}. Hence, the reform cut-off does not coincide with a school cut-off which can affect educational outcomes (see e.g. Bedard and Dhuey, 2006).

[19]To keep the sample constant across outcomes, I use the median of the MSE-optimal bandwidths rather than selecting the corresponding MSE-optimal bandwidth for each outcome. As a robustness check, I vary the bandwidth from 6-24 months and my results remain stable (Figure 16-17, Appendix Figure A.14-A.16).

To increase precision of my estimates, I include control variables. All control variables are measured in 2009, the year before the reform announcement, and hence are exogenous. For mothers, I control for previous labor supply through employment status, number of hours worked and a cubic for mothers earnings pre-reform. In addition, I control for their education level. As education level is not observed for all mothers, I include a dummy for not observing the education level and dummies which correspond to various education levels. Moreover, I control for partners' labor supply through their employment status and a cubic for partners earnings. I control for the number of children, the number of children in the household and age at first birth.[20] Finally, I include dummies for provinces and a dummy for whether the municipality of residence belongs to the 'Bible Belt', a region with more conservative views and hence lower average female labor supply.[21] For children, I include birth year fixed effects and birth order dummy variables in addition to the controls included for mothers.

Hence, the specification is as follows for mothers and children, respectively:

$$y_i^M = \alpha^M + \delta^M 1\{a_i^M \geq c\} + \beta_1^M a_i^M + \beta_2^M 1\{a_i^M \geq c\} a_i^M + \gamma_1^M X_i^M + \varepsilon_i^M$$

$$y_i^C = \alpha^C + \delta^C 1\{a_i^M \geq c\} + \beta_1^C a_i^M + \beta_2^C 1\{a_i^M \geq c\} a_i^M + \gamma_1^C X_i^M + \gamma_2^C X_i^C + \varepsilon_i^C$$

In which y_i^M is the outcome for mother i and y_i^C for child i. a_i^M is the birth month of the mothers, the threshold is c. I include a linear trend in birth month of the mother, separately before and after the threshold. X_i^M is a vector of mother covariates, X_i^C is a vector of child covariates. ε_i^M and ε_i^C are the error term for mother and child respectively. Under the assumption that in absence of the reform the outcomes would evolve smoothly across the threshold, the effect of the reform is captured by δ^M for mothers and δ^C for children. I estimate the specification separately for daughters and sons because they may be affected differently by the reform.

[20] The number of children and the number of children in the household are included as dummy variables for one, two, or three and more. Age at first birth (in months) is included as a dummy variable for each quartile of the distribution.

[21] Following Gielen and Zwiers (2018), I define Bible Belt as municipalities with at least 1% of votes for the SGP.

4.2 Threats to identification

Identification relies on the assumption that if mothers faced the same tax regime, their outcomes and their children's outcomes would evolve smoothly across the threshold. One concern regarding this assumption is that birth month is related to parental characteristics, which results in differences in outcomes later in life and this could drive any differences I observe around the threshold (Buckles and Hungerman, 2013). I cannot test for the identifying assumption directly, but I can test for discontinuities in outcomes determined prior to the reform. If there is seasonality in parental selection, one would expect these difference to be realized prior to the reform. Figure 3 displays RD-plots for mother's labor supply in 2009, prior to the reform, there are no visible discontinuities around the January 1963. In addition, I test whether I find any discontinuities around the threshold in the control variables. I estimate my main specification, excluding controls, with as dependent variable the control variables which are determined prior to the reform. Table 3 displays these estimates for the mothers, I do not find any evidence for any discontinuities around the threshold. Nor do I find any discontinuities when I do this for daughters and sons (Table 4).

Regression discontinuity designs also require no manipulation of the running variable. In this setting, this is not a concern as birth month has been recorded many years prior to the reform.[22] I still check whether the distribution of births is smooth across the threshold. Appendix Figure A.1 shows that slightly more women are born just after the threshold compared to just before, however it also shows that this is effect is present in other years and thus likely due to seasonality in births. I formally test for this and find a discontinuity in the density as I do around January 1^{st}'s in earlier and later years (see Table A.1). As the discontinuity I find around the true cut-off is not larger than the discontinuity I find around January 1^{st} in other years and I also observe it for women not eligible for the tax subsidy, this is likey due to seasonality in births rather than manipulation. More importantly, I have shown above that this seasonality does not result in discontinuities in characteristics prior to the reform (Table 3).

Anticipation of the reform could change the composition of the target sample as women who will be subject to the reform may change their labor supply earlier and

[22] I exclude non-Dutch born women from my sample as historically birth month has not always been reported accurately for immigrants.

no longer belong to the eligible sample. To circumvent this, I select the target sample based on 2009 eligibility, the year before the reform was announced. Prior to the reform announcement in 2010, the 1963-birth year threshold was not discussed.[23] Moreover, children in the sample are aged 12-25 in 2012, and hence were born prior to the reform.

Finally, I am not aware of any other policies that use January 1^{st}, 1963 as a cut-off. Importantly, January is also not used in the Dutch education system to assign students to grades. [24]

5 Results

5.1 Labor supply effect of the reform on mothers

First, I estimate the effect of the reform on the labor force participation and income of mothers who are directly affected by the reform. Figure 4 shows the average labor supply outcomes for mothers in 2020, eight years after the reform when the mothers are aged 57.[25] These figures show a positive trend in employment, income percentile and hours worked for mothers; younger cohorts have a higher labor supply on average. Moreover, the jump in the fraction of mothers employed around the threshold suggests that employment is positively affected by the reform. For percentile rank in the income distribution, there is a small jump. There does not seem to be an effect on number of hours worked. Figure 5 further zooms in on the effect on earnings by looking at various points in the earnings distribution. The figure shows an increases in the number of mothers earning at least 10, 20 and 30% of the minimum wage just after the cut-off, but does not find an increase in earnings at higher levels of earnings. Figure 6 displays the RD-plots for four categories of hours worked per week. This figure shows a positive jump in the probability of working any hours, but not for higher thresholds.

Table 5 displays the RD-estimates for mothers' labor market outcomes in 2020. I find

[23] Plans to reduce or abolish the tax subsidy were included in the platforms of multiple parties leading up to the June 2010 elections following the collapse of the previous government in February 2010. However, none of these plans included the 1963 threshold, nor any other threshold. Hence, the first time the general public became aware of the 1963 threshold was in September 2010 when the new government announced their plans.

[24] In the robusness section I discuss results of a placebo test using other January 1^{st} cut-offs.

[25] See Appendix Figures A.2-A.4 for figures with one month bins.

an increase in employment among mothers of 2.25 percentage point, although marginally significant, which is a 5% increase relative to the mean. Figure 16 suggests that the marginal significance is due to limited power, when I further increase the bandwidth and hence sample size, the coefficient remains stable but is estimated more precisely. This increase in employment is driven by paid-employment rather than self-employment (Table 5). The number of hours of work does not increase, although the probability of working any hours does increase. I find positive coefficients income percentile and earnings, but these are not significant.[26] Next, I estimate the effect along the earnings distribution by estimating the effect on earning at least a certain percentage of the minimum wage. The probability of earning at least 10, 20 and 30% increases by 3.4, 3.2 and 2.5 percentage points respectively. I do not find an effect on higher earnings levels. Hence, overall these estimates show that mothers' labor supply increases but only at modest levels of hours and earnings.

Next, I estimate the effect on mothers' labor supply over time. Labor supply may increase over time as the financial incentive increases over time or if labor market frictions prevent mothers from finding a job immediately. Figure 7 shows that mothers labor supply increases over time. I do not find an effect on employment in 2012, but the effect increases afterwards and stabilizes from 2014 onward. For other outcomes, such as the probability of earning at least 10% of the minimum wage or the number of hours worked per week, the effect continues to increase over time although the increases are smaller in later years. In addition, Table 6 displays the estimates for the effect on cumulative labor supply.

Finally, I consider the effect on partners' labor supply and marriage stability. I find no effect on the labor supply of partners (Table 7). I find a positive point estimate for the number of hours partners work, but the estimate is not significant. Marriage stability can be affected as the reform changes the surplus of marriage directly as the subsidy is no longer in place. Another channel could be the increase in maternal labor force participation, which can change the bargaining power of women within the household.

[26] I do not estimate the effect on log of earnings or the inverse hyperbolic sine of earnings as I have a large number of observations with zero earnings. Chen and Roth (2023) show that in such cases the point estimate will depend on the measurement unit, i.e. earnings in euros vs. earnings in thousands of euros or monthly vs. yearly earnings. Indeed, when I estimate the effect on the inverse hyperbolic sine of earnings in cents, euros or thousand of euros the point estimate ranges from 0.34, to 0.23 and .07.

However, I do not find an effect on divorce (Table 7).

5.2 Effect of the reform on daughters

In this section, I discuss the results for daughters. Figure 8 displays the regression discontinuity plots for the daughters.[27] These figures suggest that the reform decreased employment, income percentile and hours of work for daughters. Furthermore, an increase in the probability of having a partner and a child is suggested.

Table 9 displays the estimates for daughters' labor supply and family formation. I find that the reform decreased daughters' employment in 2020 by 2.8 percentage points. This decrease is driven by a decrease in paid-employment. Furthermore, daughter are less likely to work positive hours or more than twelve hours per week, although the last estimate is only marginally significant. Daughters income percentile is 1.1 point lower, but this estimate is only marginally significant. Daughters are less likely to earn above 10 to 50% of the minimum wage, for higher levels of income point estimates are smaller and no longer significant. Finally, daughters leave their mothers household earlier, they spend on average 0.2 fewer years there. Moreover, they are 3.4 percentage point more likely to have a partner. Fertility also increases, daughters are 2.9 percentage points more likely to have a child and number of children increases by 0.056.

Next, I split the sample based on daughters' age at the time of the reform. I do this for two reasons. First, the age at which daughters are affected may impact how they are affected by the reform. In this context, older daughters may have made more labor market decisions prior to the reform and thus could be less able to respond. Moreover, older daughters may also be more aware of the labor market decisions their mothers make. Second, the age at which I currently observe the daughters may matter for their outcomes, especially for their labor market decisions and family formation decisions. Older daughters are more likely to have children, hence as they are making different labor market decisions they may also be affected differently by the reform.

In Figure 9, I plot the outcomes for daughters who were aged between 19 and 25 at the time of the reform. The outcomes are observed in 2020, when these daughters are aged 27 to 33. I find similar effects on labor supply and fertility for these daughters. Figure 10 displays the RD-plots for younger daughters, who were aged 12-18 at the time

[27] See Appendix Figures A.5 - A.7 for RD-figures with one month bins.

of the reform and 20-26 when I observe them in 2020. I do not see discontinuities at the threshold for these younger daughters.

The results I find for the overall sample of daughters are driven by older daughters, who were aged 19 to 25 in 2012 at the time of the reform and hence are aged 27 to 33 in 2020 when I observe their outcomes. Older daughters are 3.5 percentage points less likely to be employed and are 2 percentiles lower in the income distribution (Table 9). The decline in income is found for various income levels, older daughters are less likely to earn 10% of the minimum wage up to 150% of the minimum wage. Furthermore, these older daughters spend on average 0.3 fewer years in the same household as their mother. They are 4.8 percentage point more likely to have a partner. Finally, they are almost 6 percentage points more likely to have a child and the number of children increases by 0.1.

For younger daughters, the estimates are much smaller. They are 2 percentage points less likely to be employed, but this estimate is not statistically significant (Table 9). Table 9 shows that daughters are less likely to earn at least 20 and 30% of the minimum wage. However, I do not find an effect for other levels and the estimates are positive and marginally significant at higher levels.[28] I do not find an effect on the family formation of younger daughters.

Both labor supply and fertility of older daughters are affected. These outcomes may be related. However, I cannot condition on either one of the outcomes as these are endogenous. Therefore, I create four mutually exclusive categories that are the four possible combinations of employment and fertility. I estimate the effect of the reform on these four categories. Following the reform, daughters are 6.7 percentage point less likely to be employed and not have a child at the same time. Daughters are more likely to be employed and have a child, as well as to not be employed and have a child. The magnitude of these estimates is comparable, 3.2 percentage points and 2.8 respectively. However, the former one is marginally statistically significant, the latter is statistically significant. Moreover, the category of not being employed and having a child is much less common among daughters. Hence, in relative terms this increase is much larger. Overall, this suggests that the decrease in labor supply and increase in fertility are linked.

Hence, I find a decrease in labor supply among daughters following the reform. Moreover, these daughters are more likely to have a partner and children. These effects are

[28]The estimate for earnings at least 300% of the minimum wage is statistically significant.

concentrated among daughters who were aged at least 19 at the time of the reform. This may be because the age at which daughters are impacted matters. Alternatively, the age at which I observe daughters may matter. In particular, as the labor supply and fertility response of daughters seem linked. Younger daughters are much less likely to have children in 2020 as they are young, it is therefore possible that these daughters may also respond to the reform at older ages.

5.3 Effect of the reform on sons

Next, I estimate the effect of the reform on sons. First, I plot the outcomes for sons. Figure 11 displays the RD-plots for sons.[29] I do not find a discontinuity in sons' labor market outcomes nor in their family formation outcomes. Figure 12 and 13 display the RD-plots for sons by age, there are also no discontinuities in these figures. The RD-estimates in Table 10 confirm that I find no effect of the reform on sons labor market and family formation outcomes. Most of these estimates are also close to zero, suggesting that there is no effect rather than my estimates being underpowered.

5.4 Placebo and robustness

I perform various placebo and robustness checks. First, I address the concern that my results are driven by another discontinuity at January 1^{st}, not related to the reform, such as seasonality in births (see e.g. Buckles and Hungerman, 2013). In the methodology section, I have already shown that I find no discontinuities in the control variables which are measured prior to the reform (Table 3). As a placebo test, I estimate the effect on mother's labor supply during the years 2006 to 2008. There should not be an effect of the reform in these years, as the reform has not been announced or implemented yet. Indeed, I find no effect on labor supply in these earlier years (Table 11).

Furthermore, I estimate the effect of the reform on a placebo group of single mothers. These women were not eligible for the tax subsidy in 2009 as eligibility requires a partner and are therefore unlikely to be affected by the reform. Some of these women may become eligible after 2009 as they find a partner, however this group is likely small. Table 5 display the RD-estimates for single mothers. These estimates are much smaller

[29] See Appendix Figures A.8 - A.10 for RD-figures with one month bins.

than the estimates I find for the main sample. None of these estimates are significant. I also do not find an effect on the outcomes of the daughters and sons of these single mothers (Table 13-14).

As another placebo test, I estimate the main specification using as cut-off January 1^{st} in earlier birth cohorts. In these specifications, I only include mothers or children in the control group. Figure 16 displays these placebo estimates for mothers. I do not find an effect at these placebo cut-offs. For the older daughters, for most outcomes I do not find a significant effect at the placebo cut-offs (Figure 15). I do find a significant, negative point estimate for income percentile for the placebo cut-off 1961. However, I do not find this for the other outcomes. For younger daughters, I do not find significant estimates at the placebo cut-offs, except for income percentile in 1961 (Figure A.11). For younger sons, I find a significant positive effect on income percentile and hours worked at the 1962 cut-off, but not for the other outcomes or cut-offs (Figure A.13). For older sons, I find a significant negative effect on income percentile at the 1962 cut-off and marginally significant effects on hours of work and having a partner (Figure A.12). Hence, I do not find effects on placebo cut-offs for mothers. For the children, I find some significant estimates at placebo cut-offs, but at non of the placebo cut-offs do I find significant estimates on the family formation outcomes.

Next, I vary the bandwidth to check that my results are not the result of a specific bandwidth choice. Figure 16 displays the estimates with varying bandwidths for mothers. The estimates are stable across bandwidths. As the bandwidth increases, the precision increases due to the larger sample size but the point estimates do not change. Hence, my results are not driven by a specific bandwidth choice. In Figure 17 the results for older daughters are displayed. For daughters, the point estimates decrease slightly as the bandwidth increases. However, the overall patterns remain stable. Hence, again the results are not driven by a specific bandwidth choice. The estimates for younger daughters (Figure A.14), older sons (Figure A.15) and younger sons (Figure A.16) also do not depend much on bandwidth choice.

Finally, I perform various sensitivity checks. First, I estimate the specification using a one month donut to check that the results are not driven by observations just around the cutoff. Second, I estimate the specification using uniform weighting, rather than kernel weighting. Third, I estimate the specification using a quadratic polynomial rather than

a linear polynomial. Finally, I estimate the specification excluding control variables. The sensitivity checks do not change the estimates for mothers (Figure 18). Neither does it change the estimates for the children (Figure 19-A.19).

6 Mechanism

Several mechanisms could potentially explain the spillover effects on daughters' labor supply and family formation. In a simple labor leisure model a decrease in daughters' labor supply could stem from three sources; a decrease in the benefits of labor, an increase in the costs of labor, or a change in the preferences for labor or leisure and the perceived benefits and costs.

In this context, the benefits of work may decrease as a result of a decrease in maternal inputs. If mothers increase their labor supply, they spend less time in the household and this may reduce daughters' human capital and subsequent wages. As a result of this, daughters may decrease labor supply. To test for this mechanism, I estimate the effect on daughters' education level as a proxy for their human capital (Table 17, Panel A). I do not find a significant effect on education level. The point estimate for years of education is not a precise zero. However, the estimates for the different levels of education are very small. In addition, I estimate the effect on the education level of younger daughters and sons (Panels B-D) and I do not find an effect on their education level either.

The second potential channel is an increase in the costs of labor. If mothers increase their labor supply, they are less available to provide child care for grandchildren. This increases the costs of labor for daughters as they have to purchase formal child care to remain in the labor force. This mechanism is more relevant for daughters who live geographically close to their mother as mothers who live far from their daughters are less relevant child care providers regardless of their employment status. Hence, if this mechanism is indeed driving the decrease in daughters' labor supply, the results should be concentrated among daughters who live relatively close to their mothers. To test this, I split the estimates for older daughters based on whether the geographical distance between mother and daughter in 2020 is below or above median.[30] Table 18 shows that

[30]I measure the distance between two coordinates, this measure does not have unit interpretation. As I select daughters who live in the same household as their mother pre-reform, I cannot use distance measured pre-reform. Distance in 2020 may be endogenous as it is determined prior to the reform.

the results are similar for daughters who live closer to their mothers and daughters who live farther from their mothers. Hence, it is unlikely that this mechanism drives the decline in labor supply.

The remaining third source is a change in the preference for labor and leisure or a change in the perceived benefits and costs of work. One possibility is that mothers who re-enter the labor market after age 49 find unattractive jobs or struggle to combine market and household work. Through this, employment or combining work and family may seem less desirable for daughters. Daughters at the margin of employment, may decide to no longer combine work and family and rather drop out of the labor market or reduce their labor supply. This can also explain the effect on fertility as daughters no longer need to delay motherhood to a moment which minimizes the career costs of having children.

Alternatively, daughters observe their mothers successfully re-enter the labor market at older ages. From this, daughters may infer that the costs of career interruptions are lower than previously expected. This may lead daughters to have children earlier and temporarily drop out of the labor market to later return.

7 Conclusion

In this paper I have evaluated a reform which stimulated female labor supply among married women with low labor force attachment. I evaluate its effect both on women directly affected and spillover effects on their children. As this reform only affects women born on January 1^{st}, 1963 or later, I can causally estimate its effect using a regression discontinuity design. I use administrative data from the Netherlands, which allows me to estimate the effects of the reform on the women directly affected and their daughters and sons. Moreover, I can estimate the effect both on labor market and family formation outcomes.

Women directly affected by the reform increase their labor force participation in response, although at low levels. However, their daughters reduce their labor supply. Moreover, these daughters advance their family formation; they are more likely to have a partner and children. These effects are concentrated among older daughters who are aged 27-33 when I observe them. I do not find effects on these outcomes for sons.

However, I do not find of the reform effect on distance in 2020: -758.1 (1,302), mean is 16364.

I explore several mechanisms that could potentially explain these results. I show that the decline in labor supply among daughters is unlikely to be driven by changes in education level. Furthermore, the effects are unlikely to be explained by mothers being less available to provide child care to grandchildren. I argue that the effects may be caused by daughters changing their preferences for labor and leisure or changes in the perceived costs of labor and leisure. One possible explanation is that mothers' labor market experience increases the salience of the costs of combining work and family for daughters, which leads them to opt-out. Another possibility is that daughters observe their mother successfully re-enter the labor market at older ages and from this infer that the costs of career interruptions are low, which leads them to temporarily reduce their labor supply.

My paper shows that a tax reform which successfully stimulated female labor supply can have unintended consequences on the labor supply of the next generation. I find a decline in the labor supply of daughters. This is in contrast to the existing literature which finds positive spillovers of labor supply from mothers to daughters (Fernández et al., 2004; Gay, 2023), from women to other women (Boelmann et al., 2021; Cavapozzi et al., 2021; Nicoletti et al., 2018) and from peers' mothers to women (Olivetti et al., 2020). This difference may arise because the mothers in my setting enter the labor market due to a financial incentive, and thus likely have a high disutility of labor, which may spillover to their daughters. Another explanation is that these women experience higher costs of labor as they are older when they increase their labor supply. Finally, daughters perceptions of career interruptions may change if they observe their mothers re-enter the labor market at older ages. The negative spillover effect I find may be specific to this setting or this target group. However, some of the potential explanations may also be relevant for other groups. Hence, further research could focus on understanding the conditions under which maternal labor supply has negative or positive effects on the labor supply of their daughters. Moreover, policies which stimulate maternal labor force participation should consider the spillover effect on daughter's labor supply.

Overall, my results show that female labor supply does not always have positive spillover effects to labor supply in the next generation. Future research could study what the drivers are of these spillover effects, to understand under which conditions these spillover effects are negative or positive.

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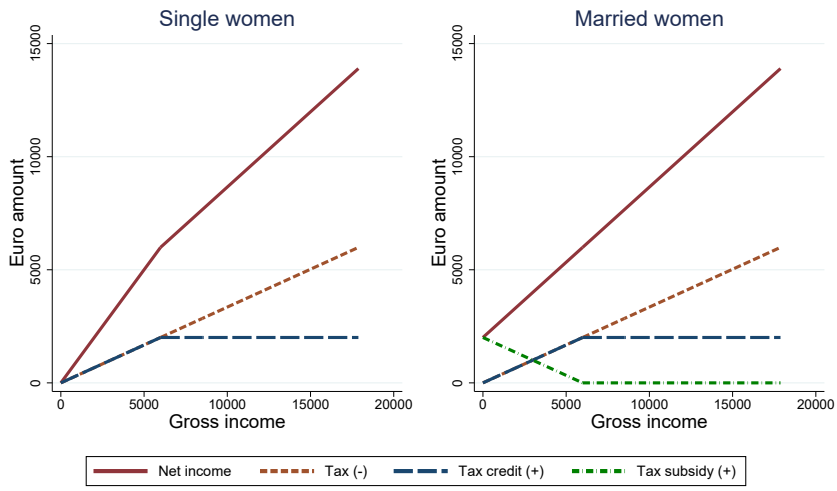
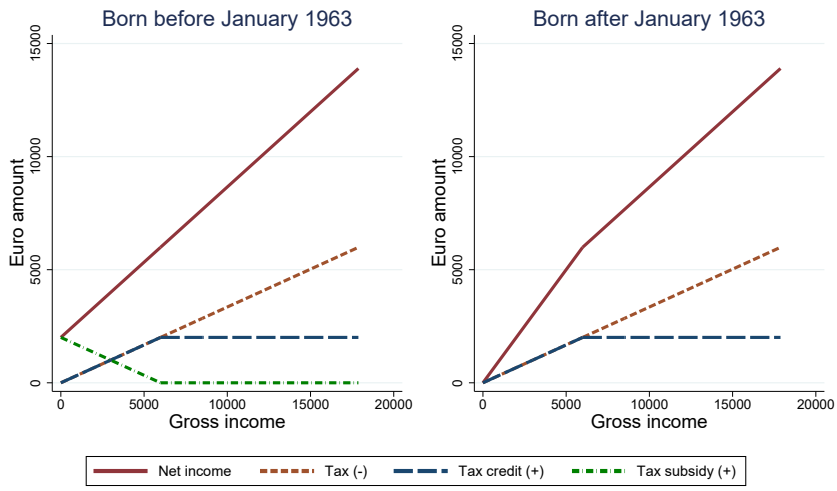
Figures and Tables



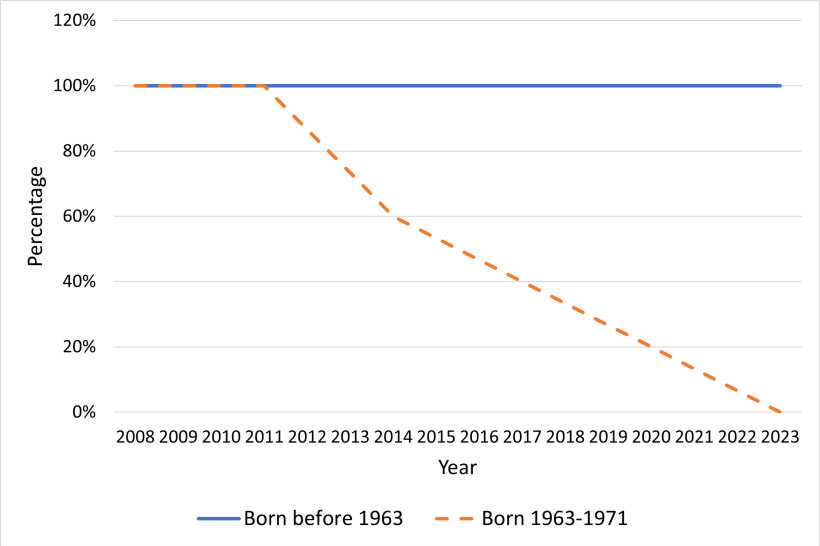
(a) Pre-reform: Tax scheme for single and married women in 2009



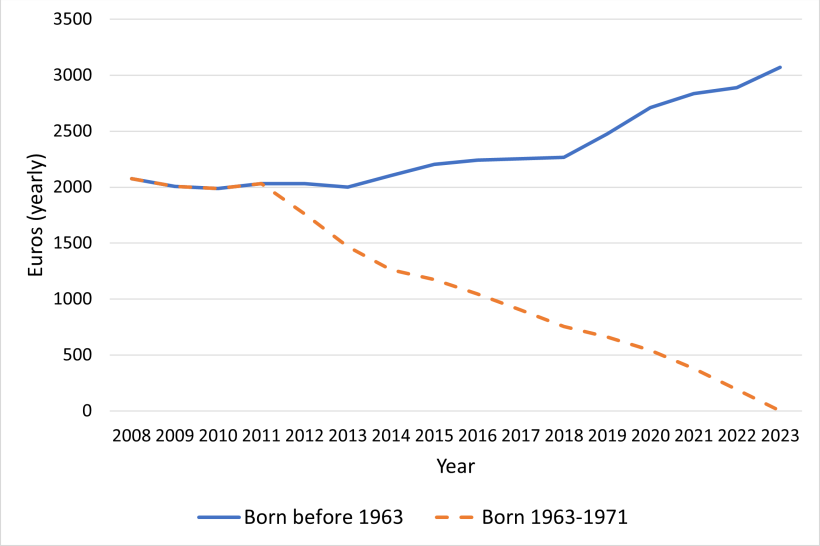
(b) Post-reform: Tax scheme for married women by birth year post reform

Figure 1: Tax scheme for single and married women

Notes: The figure displays a simplified version of the tax scheme for single and married women. Married women are married or cohabiting women who are the least earning partner and have a partner who earns at least the minimum wage full time. Panel a) displays the tax scheme in 2009 for single and married women. Panel b) displays the tax scheme for married women after the tax subsidy is fully phased out due to the reform. Net income is the sum of gross income without employer contributions, tax credit and tax subsidy; tax is deducted from this. Tax credit is based on individual income, 'tax subsidy' is the part of the tax credit that is obtained through the partner.



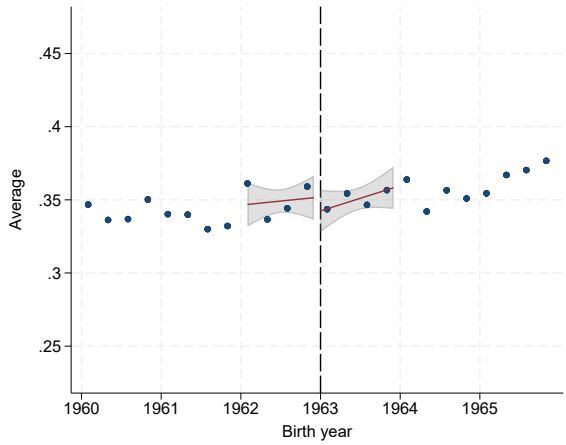
(a) Percentage



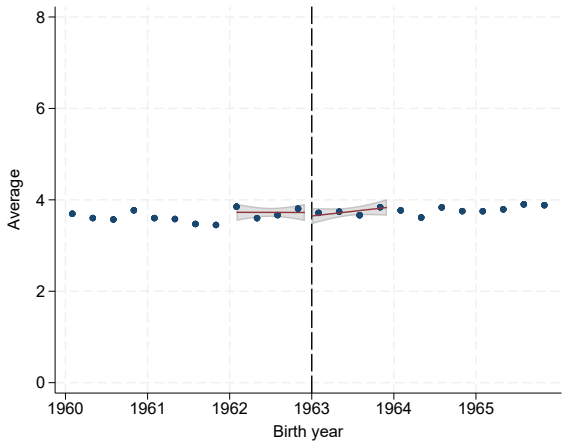
(b) Euro's

Figure 2: Reform: Tax subsidy by year of birth least earning partner

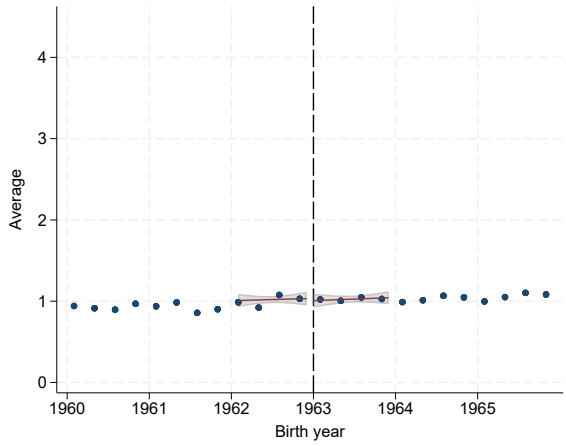
Notes: Tax subsidy by year of birth of the least earning partner and over time. In (a) the tax subsidy is displayed as percentage of the general tax deduction. In (b) the tax subsidy is displayed in euros, not adjusted for inflation. For ease of exposition, the scheme for people born after January 1^{st}, 1972 is not displayed. These people are not included in the analysis.



Employed

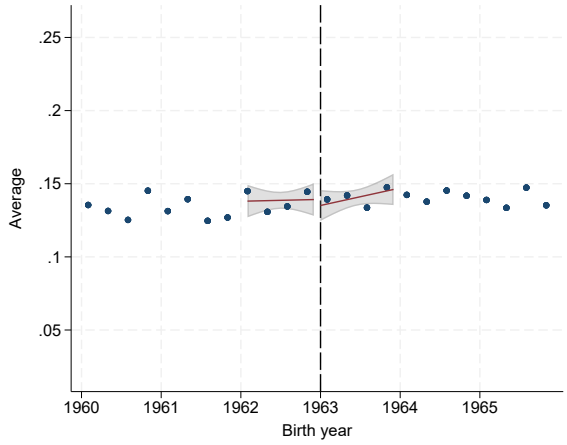


Income percentile



Hours of paid-employment per week

Earnings $\geq$ 10% of minimum wage



Earnings $\geq$ 20% of minimum wage

Earnings $\geq$ 30% of minimum wage

Figure 3: Placebo test: Labor supply of mothers in 2009

Notes: See notes to Table 5. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

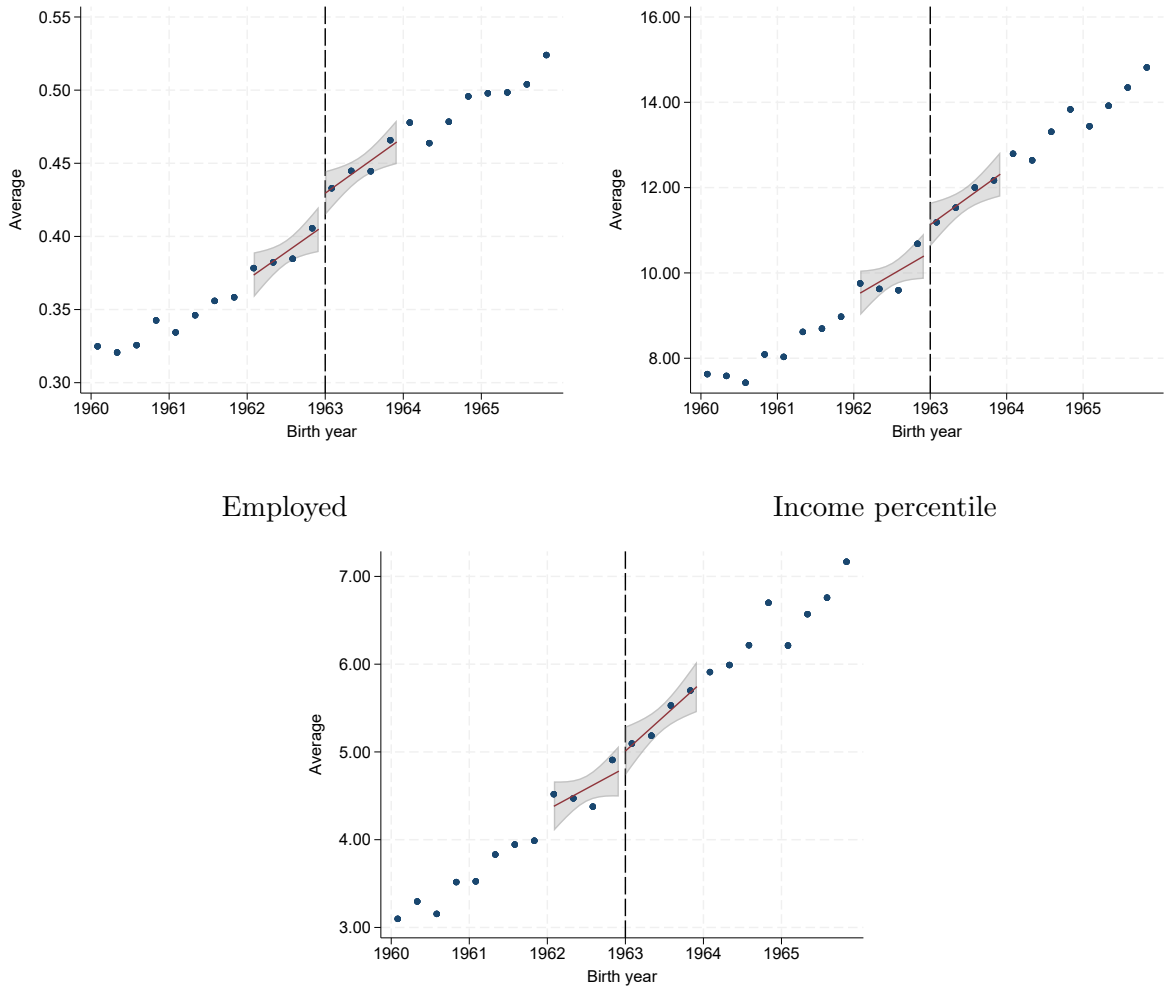
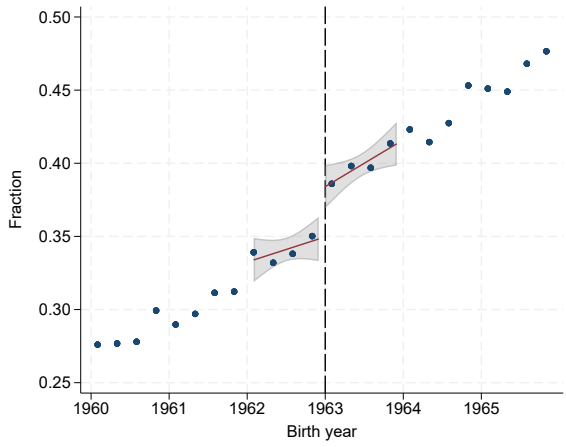
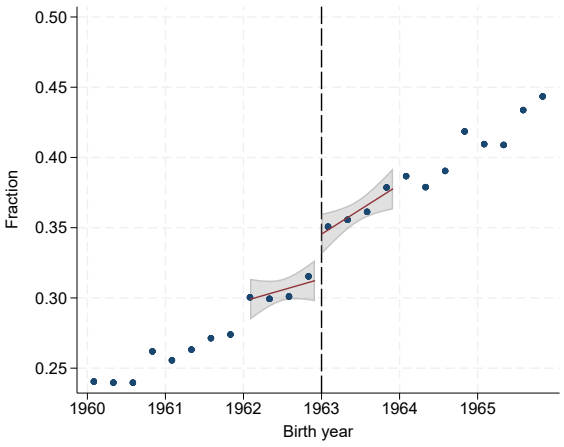


Figure 4: Effect of the reform on the labor supply of mothers in 2020

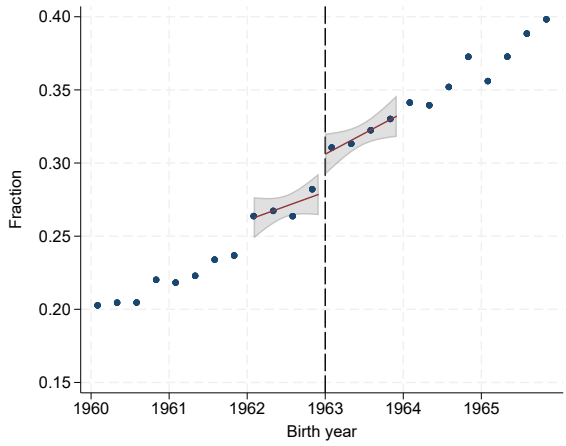
Notes: See notes to Table 5. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.



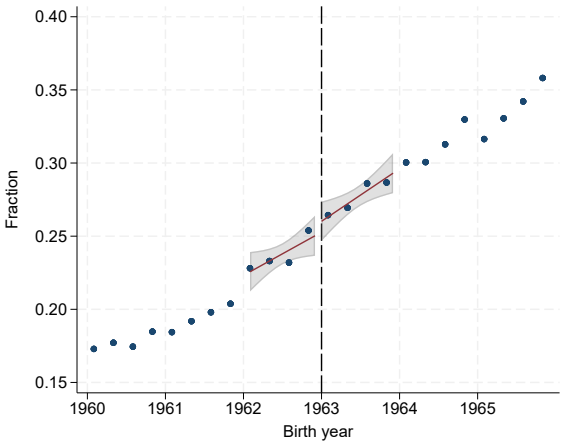
≥ 10% of minimum wage



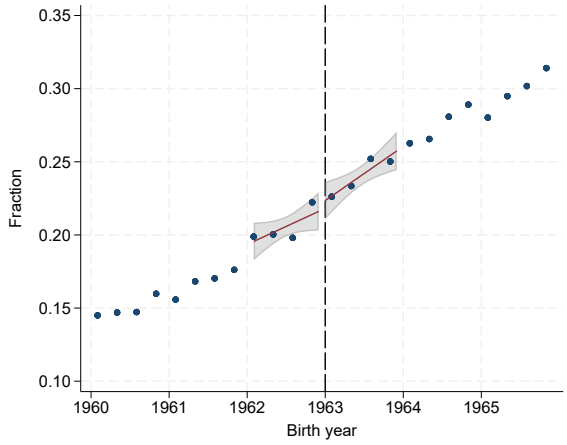
≥ 20% of minimum wage



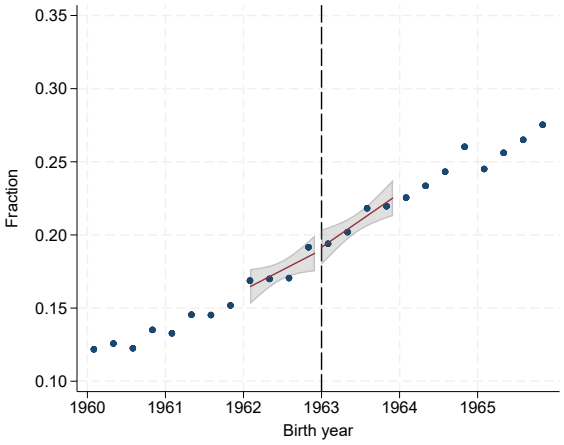
≥ 30% of minimum wage



≥ 40% of minimum wage



≥ 50% of minimum wage



≥ 60% of minimum wage

Figure 5: Effect of the reform on the earnings of mothers in 2020

Notes: See notes to Table 5. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

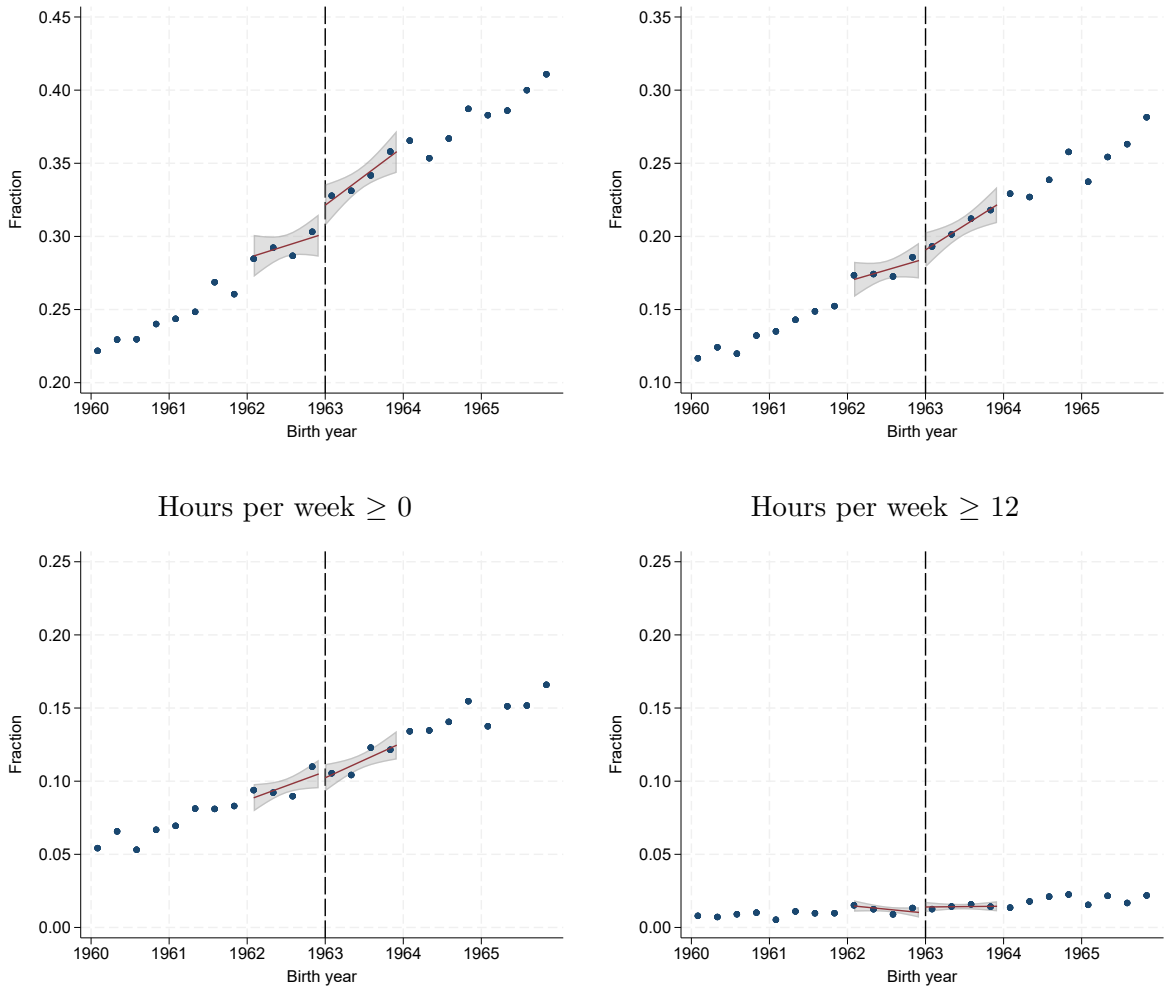
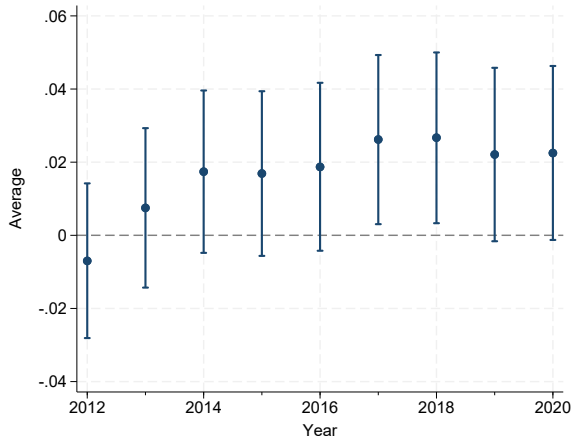
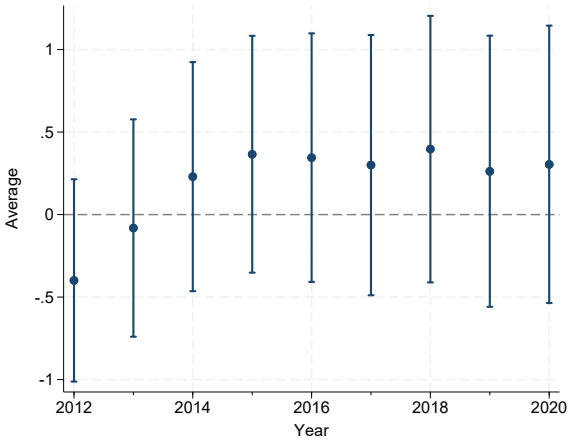


Figure 6: Effect of the reform on hours worked per week of mothers in 2020

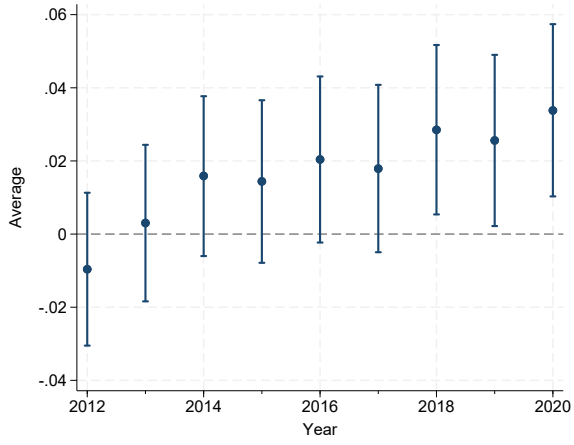
Notes: See notes to Table 5. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.



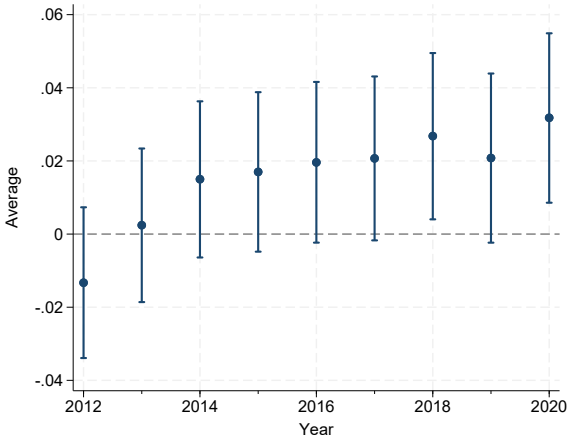
Employed



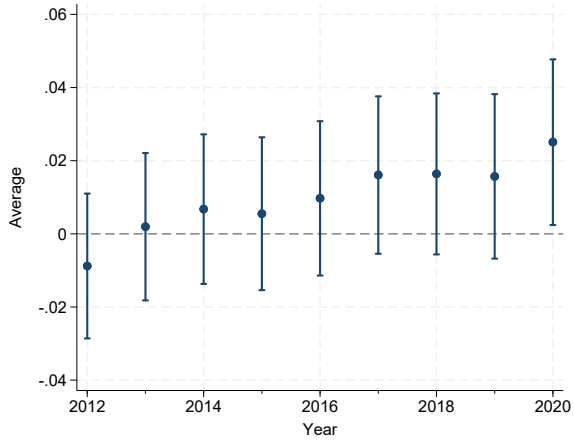
Income percentile



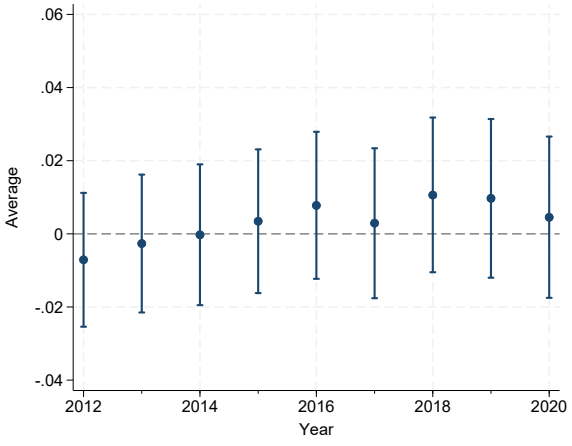
Earnings $\geq$ 10% of minimum wage



Earnings $\geq$ 20% of minimum wage

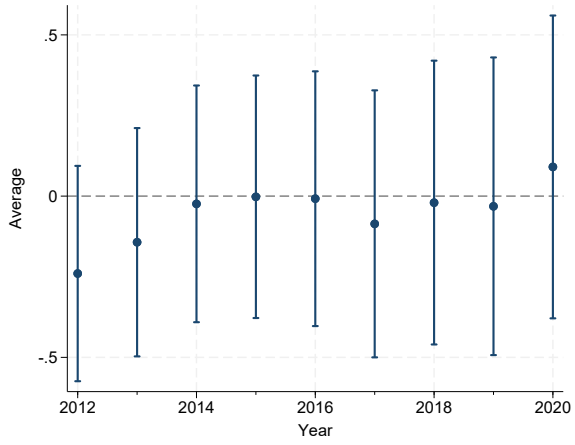


Earnings $\geq$ 30% of minimum wage

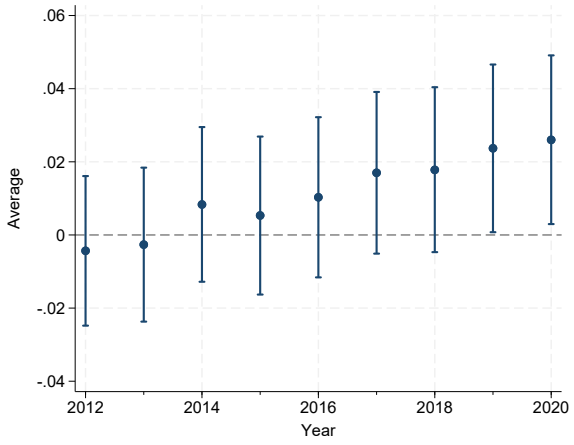


Earnings $\geq$ 40% of minimum wage

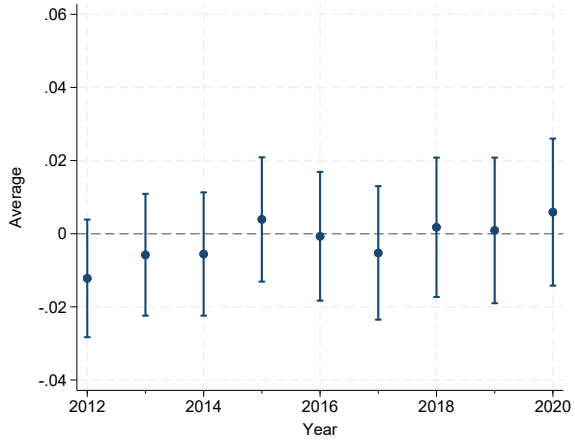
Figure 7: Effect of the reform on labor supply of mothers over time



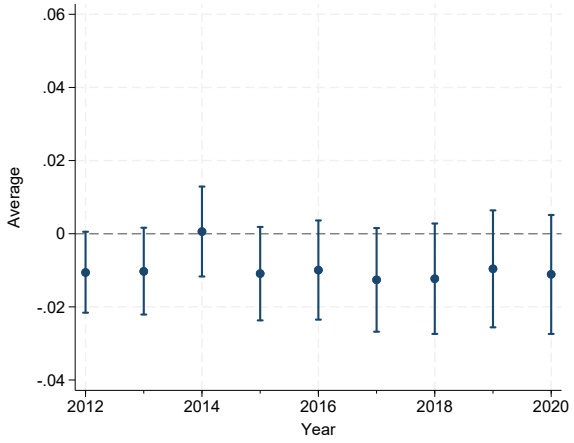
Hours worked per week



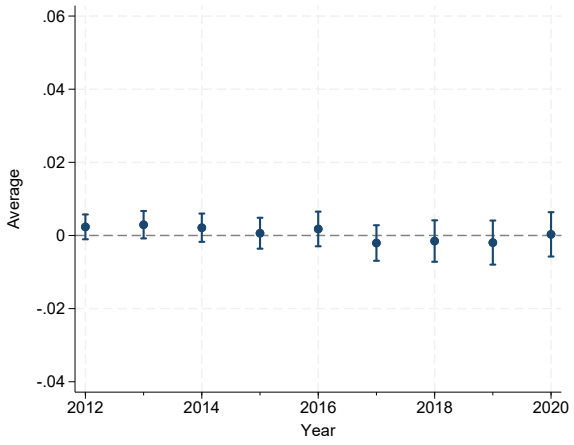
Hours worked per week ≥ 0



Hours worked per week ≥ 12



Hours worked per week ≥ 20



Hours worked per week ≥ 35

Figure 7: Effect of the reform on labor supply of mothers over time (continued)

Notes: See notes to Table 5. Regression discontinuity estimates and 95% confidence intervals are displayed from separate regressions for each year.

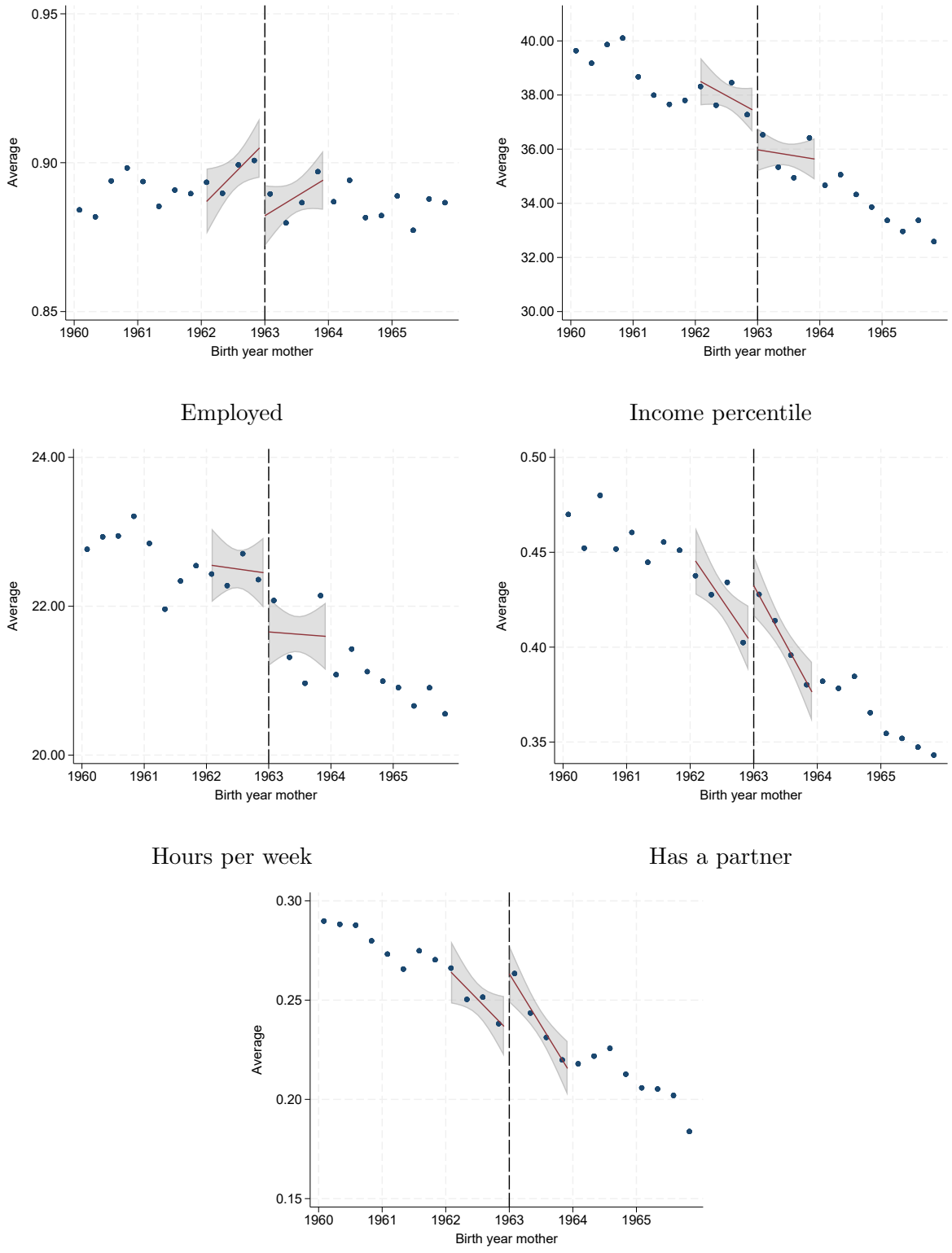


Figure 8: Effect of the reform on daughters in 2020

Notes: See notes to Table 9. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

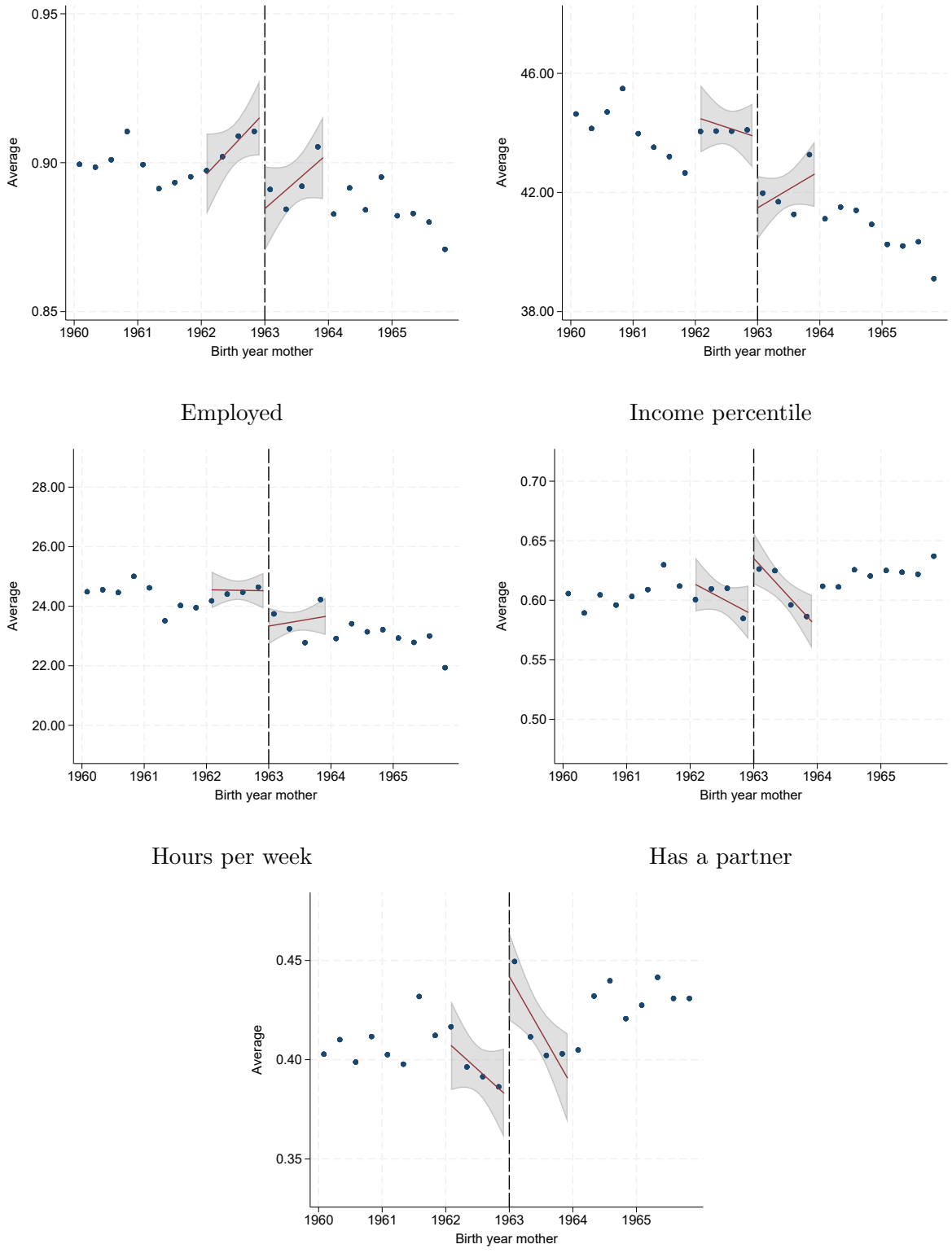
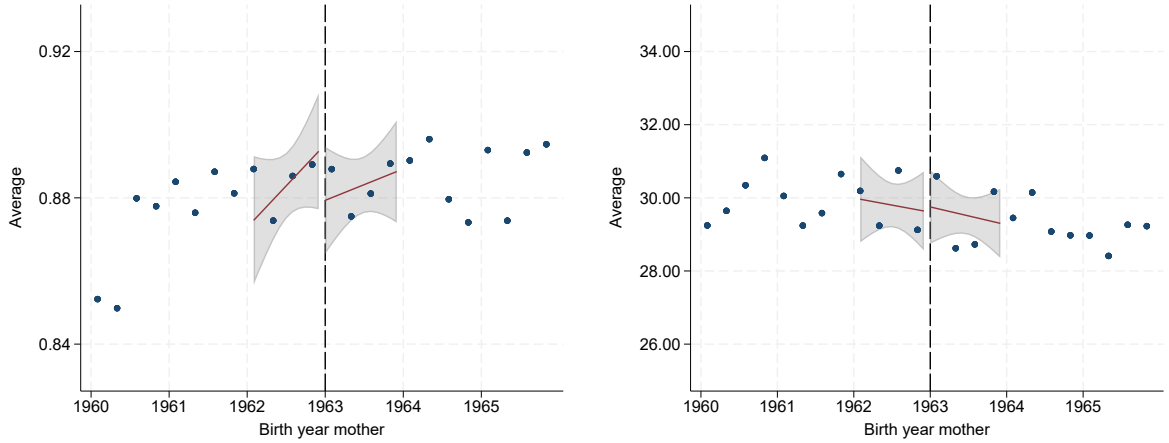


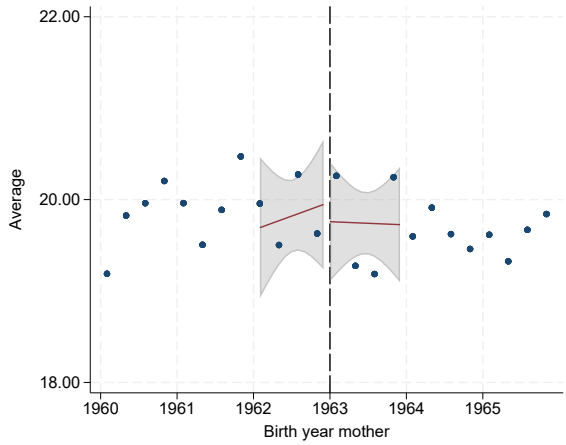
Figure 9: Effect of the reform on daughters aged 27-33 in 2020

Notes: See notes to Table 9. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

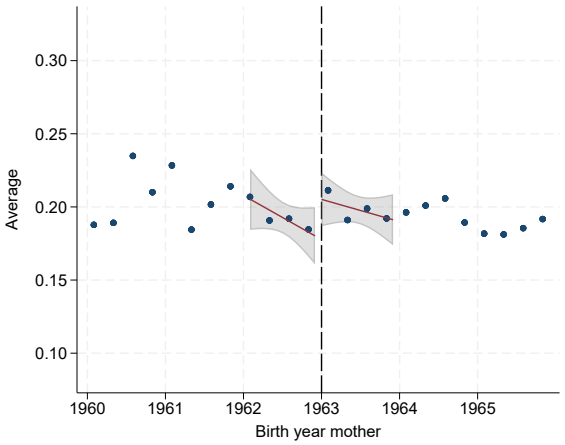


Employed

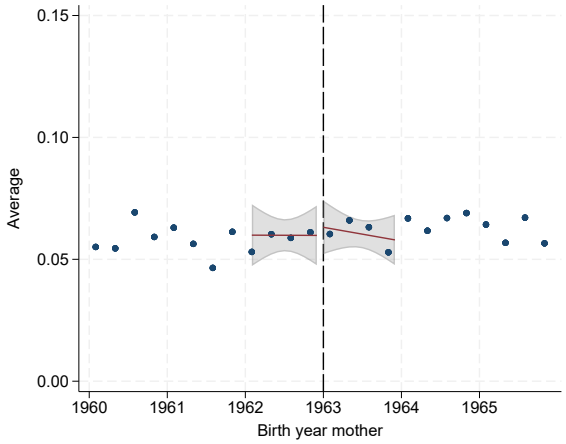
Income percentile



Hours per week



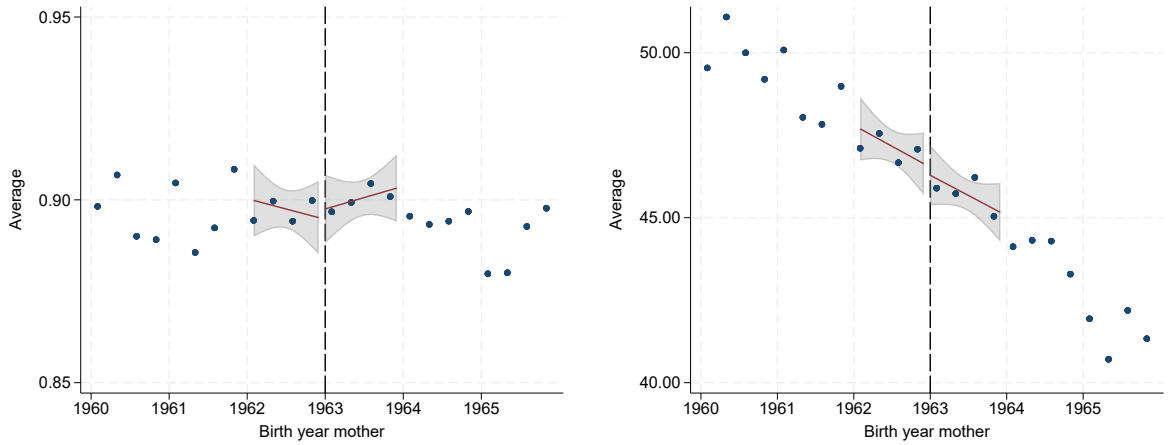
Has a partner



Has a child

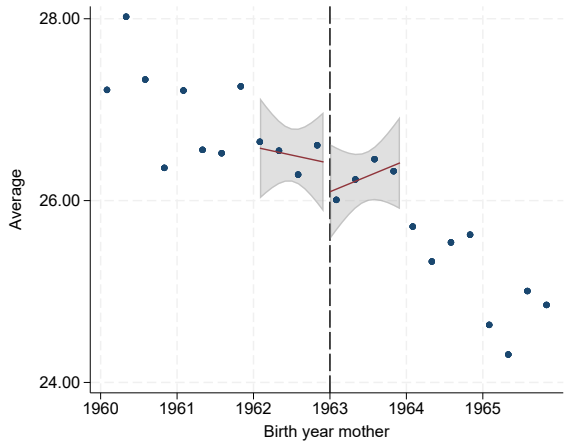
Figure 10: Effect of the reform on daughters aged 20-26 in 2020

Notes: See notes to Table 9. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

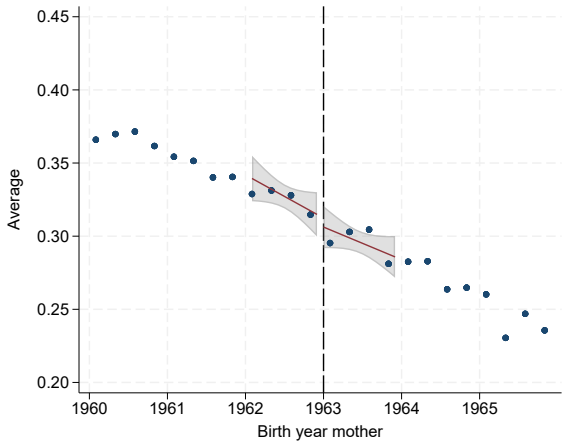


Employed

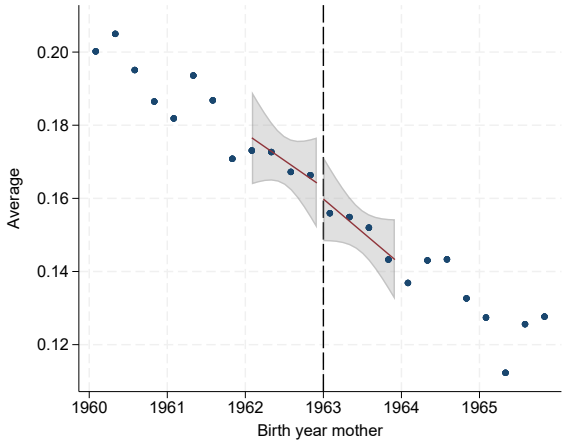
Income percentile



Hours per week



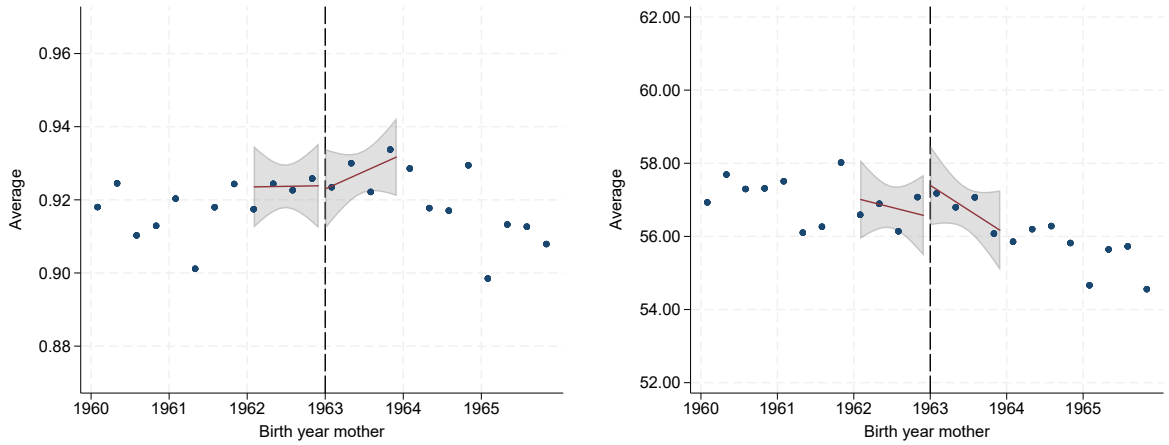
Has a partner



Has a child

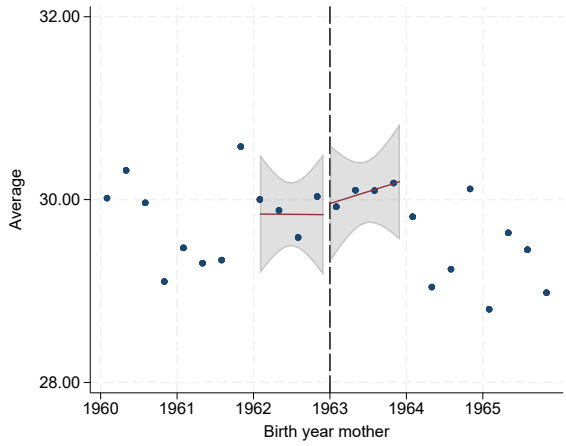
Figure 11: Effect of the reform on sons in 2020

Notes: See notes to Table 10. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

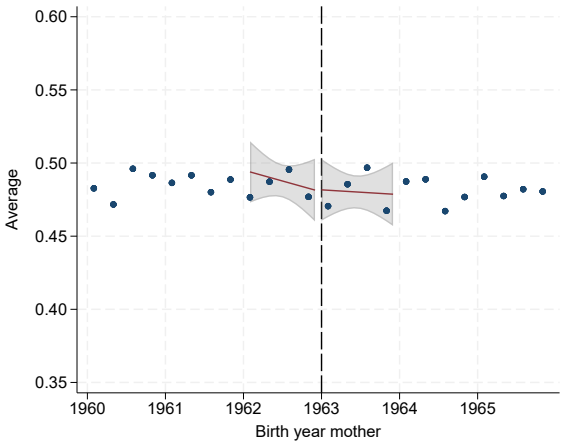


Employed

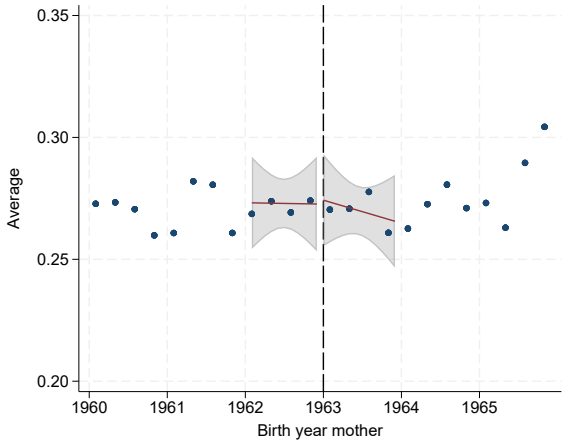
Income percentile



Hours per week



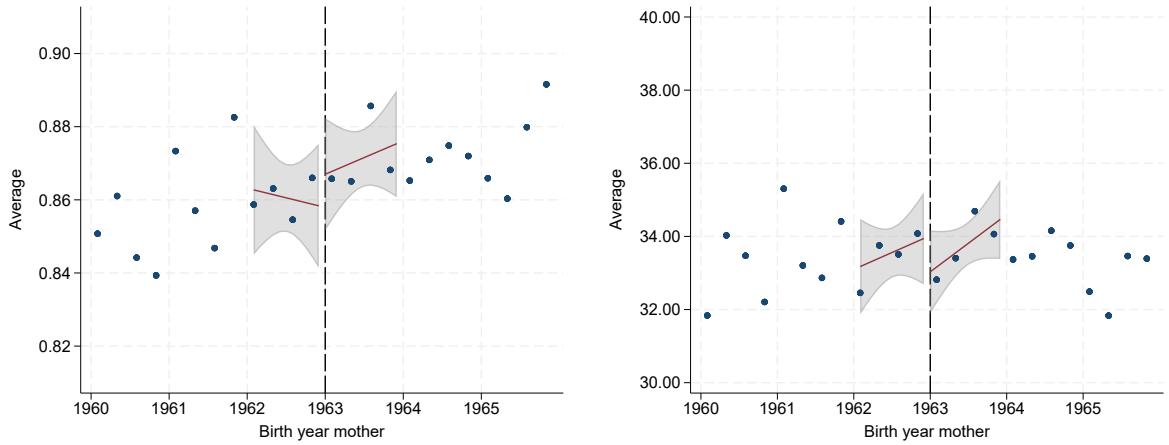
Has a partner



Has a child

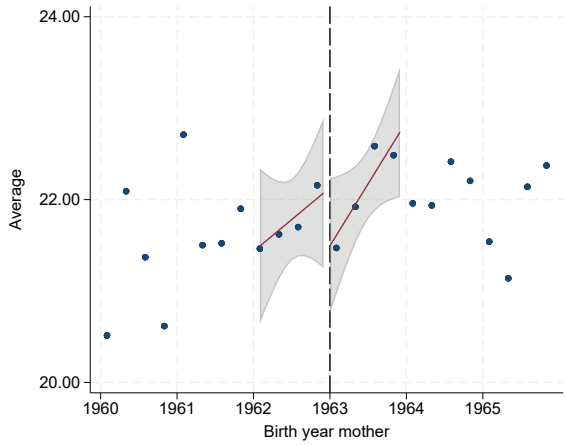
Figure 12: Effect of the reform on sons aged 27-33 in 2020

Notes: See notes to Table 10. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

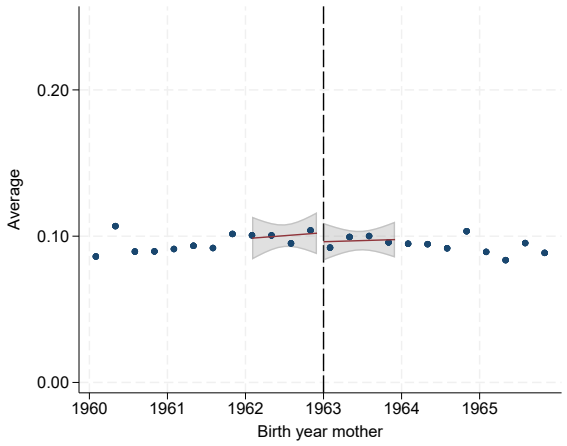


Employed

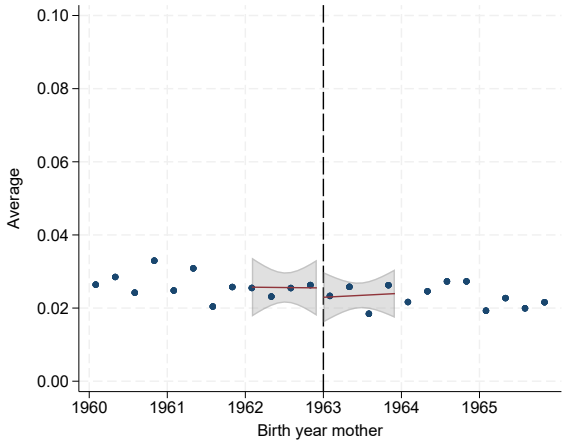
Income percentile



Hours per week



Has a partner



Has a child

Figure 13: Effect of the reform on sons aged 20-26 in 2020

Notes: See notes to Table 10. Each dot is the average in a three-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

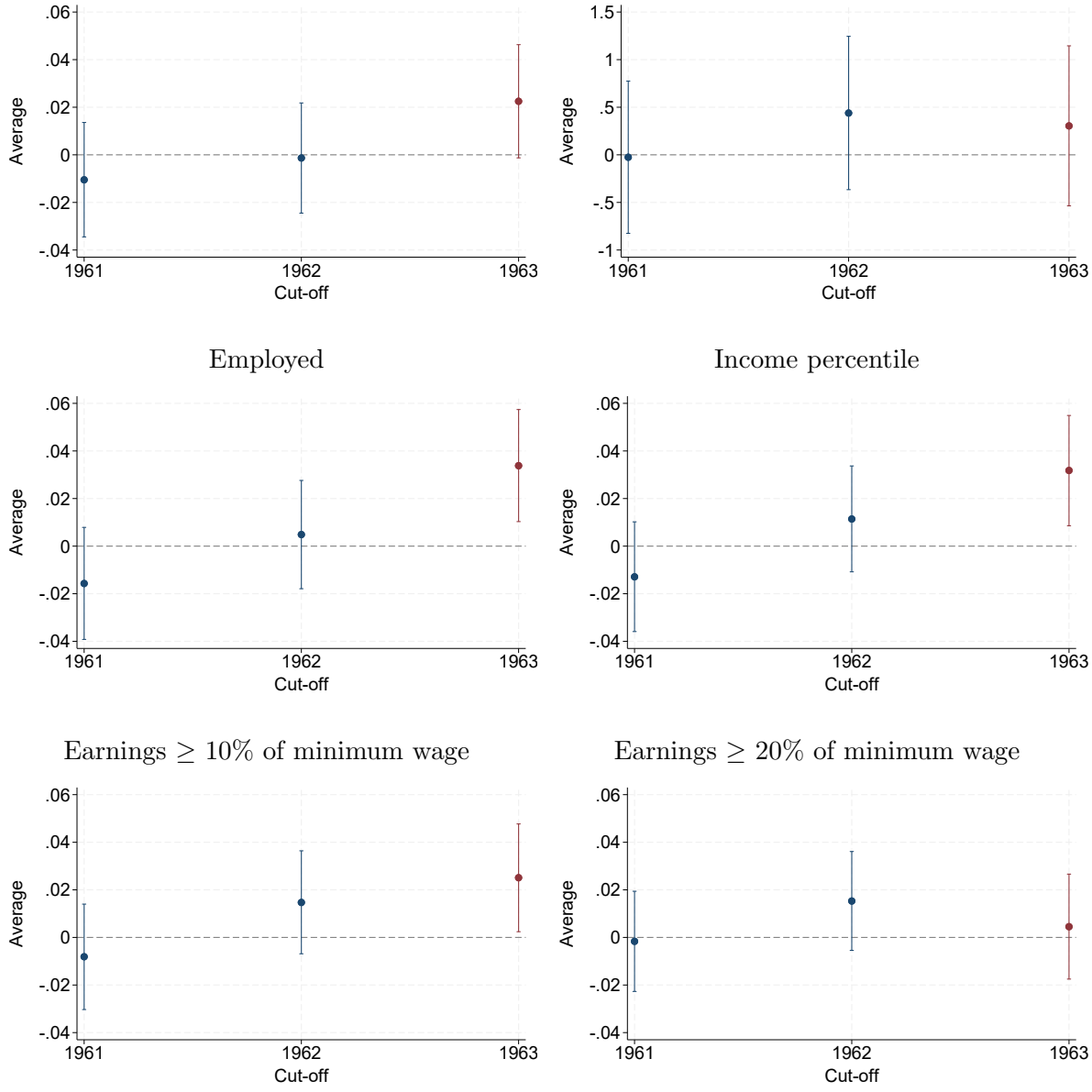
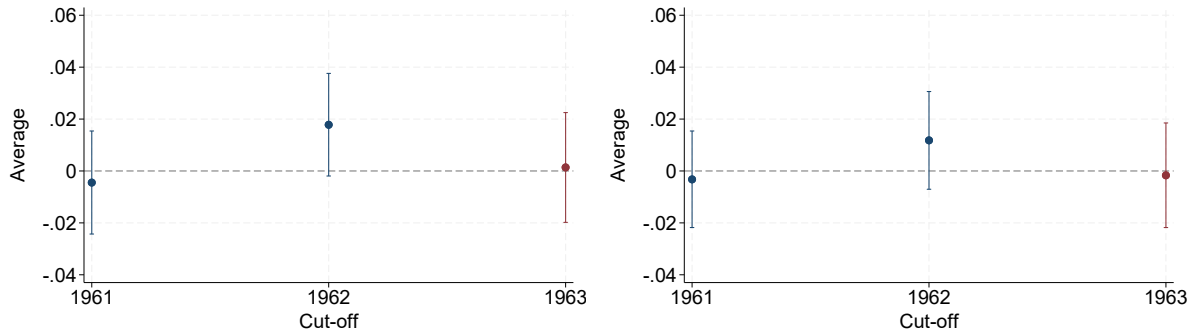
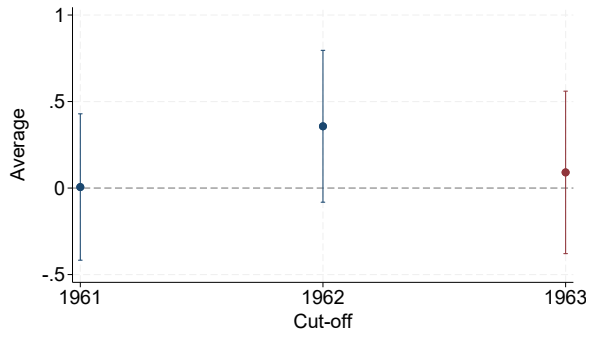


Figure 14: Effect of placebo cut-offs on labor supply of mothers in 2020

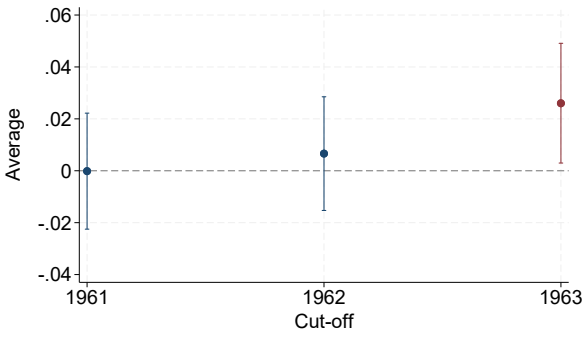


Earnings $\geq$ 50% of minimum wage

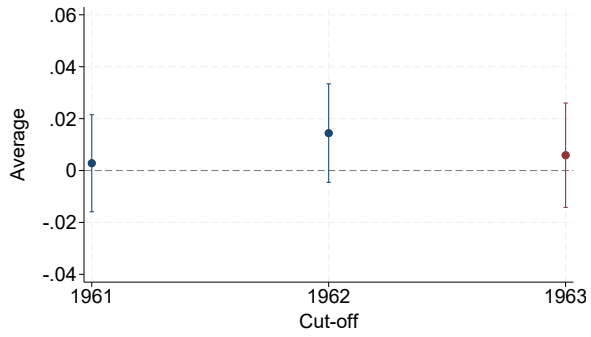
Earnings $\geq$ 60% of minimum wage



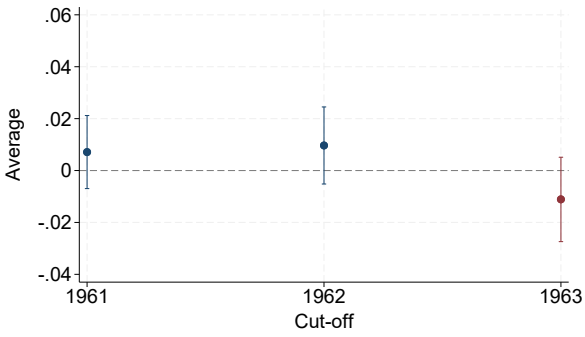
Hours worked per week



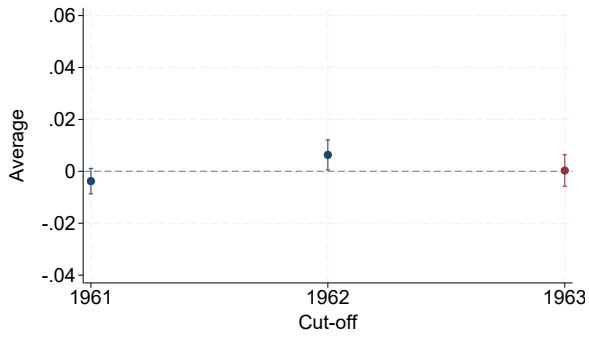
Hours worked per week $\geq$ 0



Hours worked per week $\geq$ 12



Hours worked per week $\geq$ 20



Hours worked per week $\geq$ 35

Figure 14: Effect of placebo cut-offs on labor supply of mothers in 2020 (continued)

Notes See notes to and estimates reported in Table 15. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays the birth year which is used as a cut-off. In blue are placebo cut-offs, in red is the true cut-off.

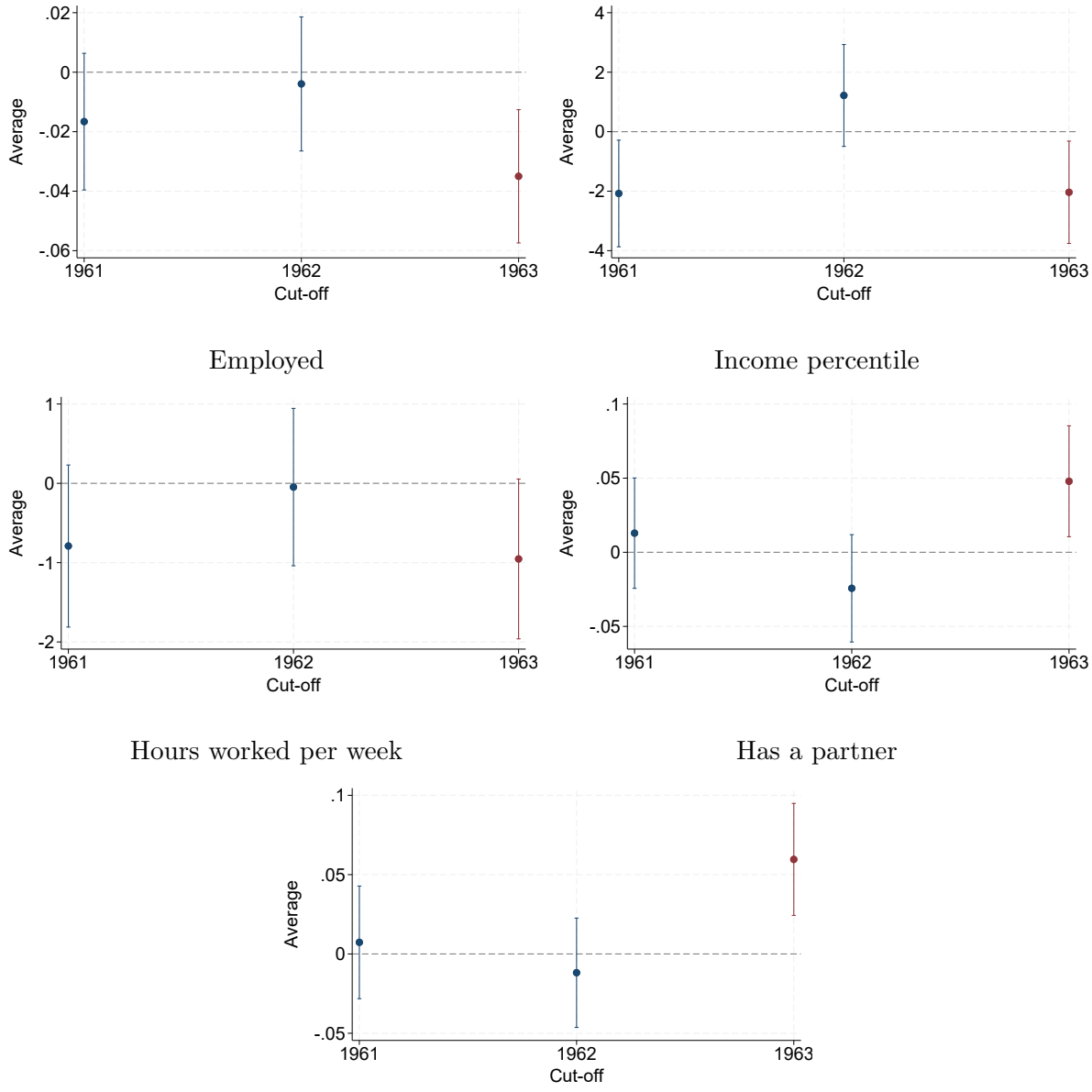


Figure 15: Effect of placebo cut-offs on daughters aged 27-33 in 2020

Notes: See notes to and estimates reported in Table 16. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays the birth year which is used as a cut-off. In blue are placebo cut-offs, in red is the true cut-off.

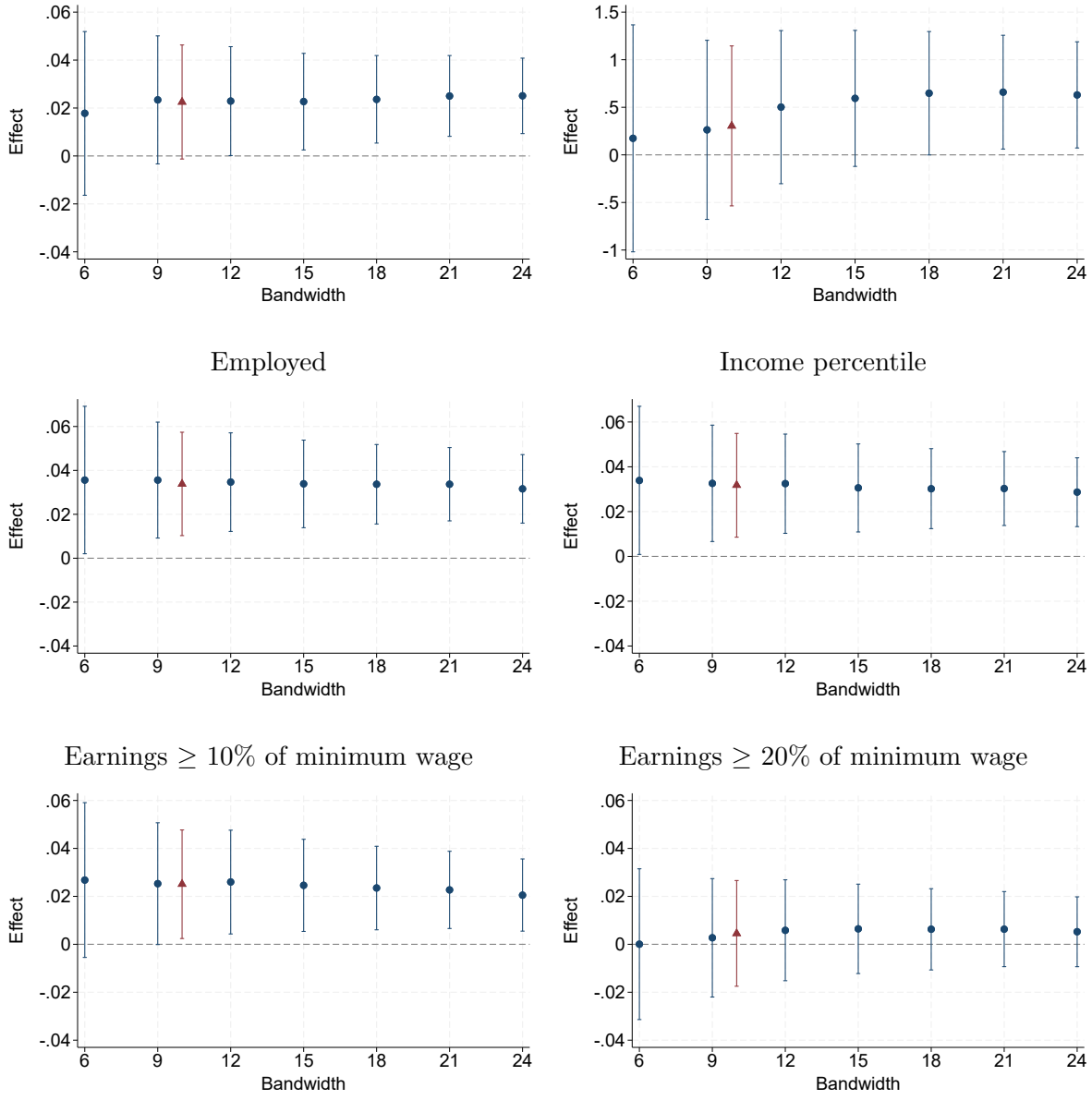


Figure 16: Effect of the reform on labor supply of mothers with varying bandwidth

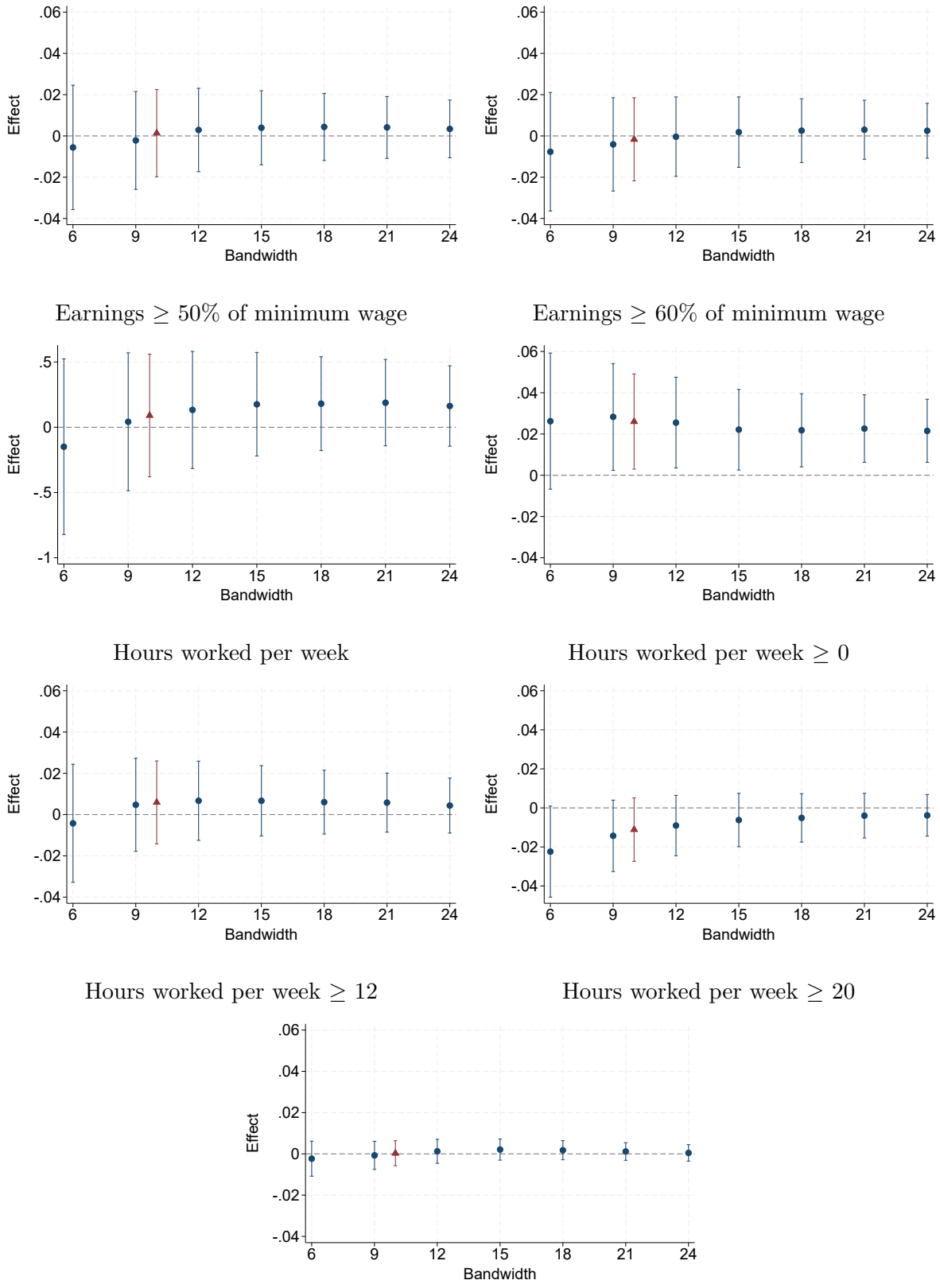


Figure 16: Effect of the reform on labor supply of mothers with varying bandwidth (continued)

Notes See notes to Table 5. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays bandwidth which is used. In blue are alternative bandwidths, in red is the main bandwidth.

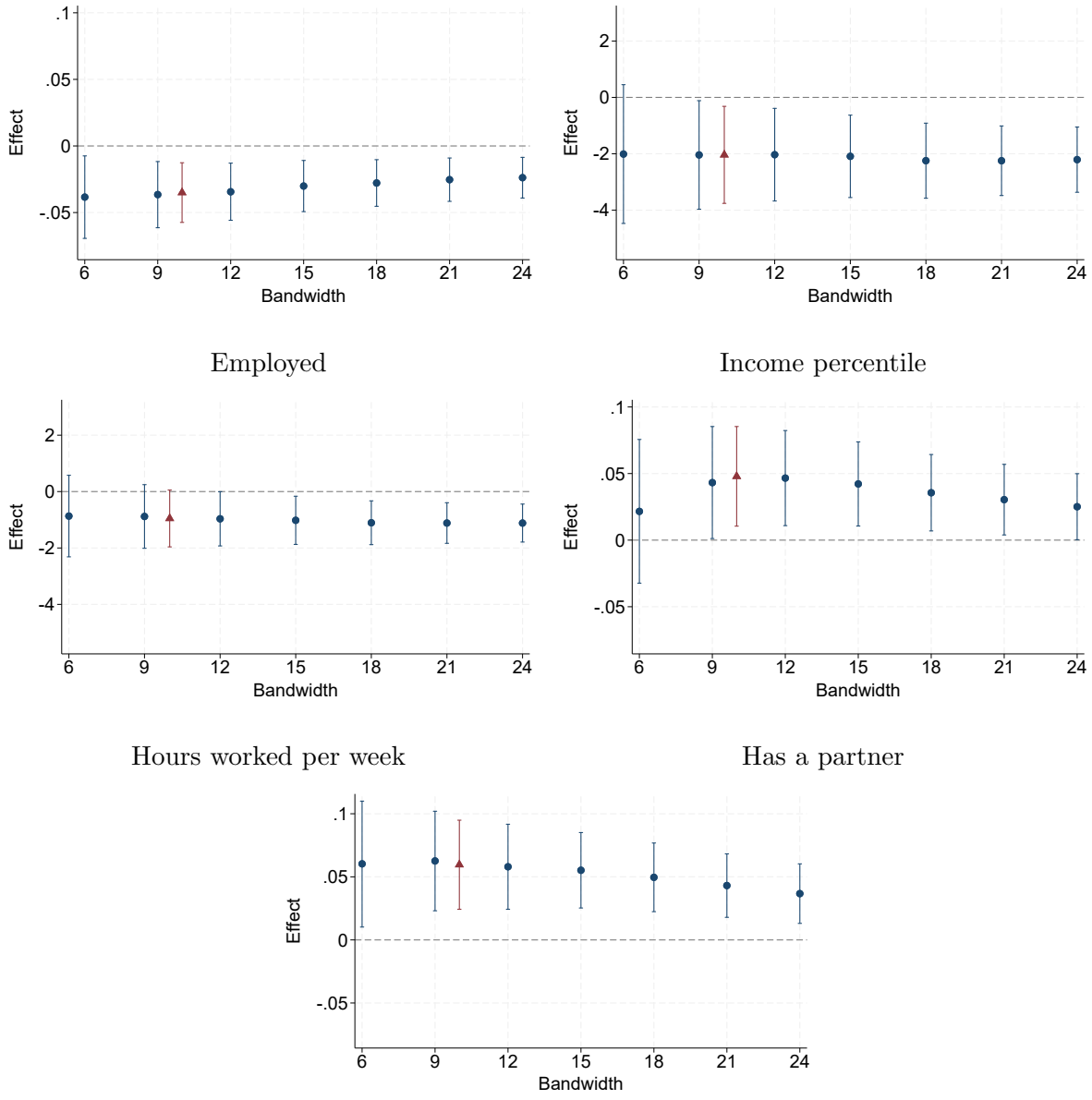


Figure 17: Effect of the reform on daughters aged 27-33 in 2020 with varying bandwidth

Notes See notes to Table 9. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays bandwidth which is used. In blue are alternative bandwidths, in red is the main bandwidth.

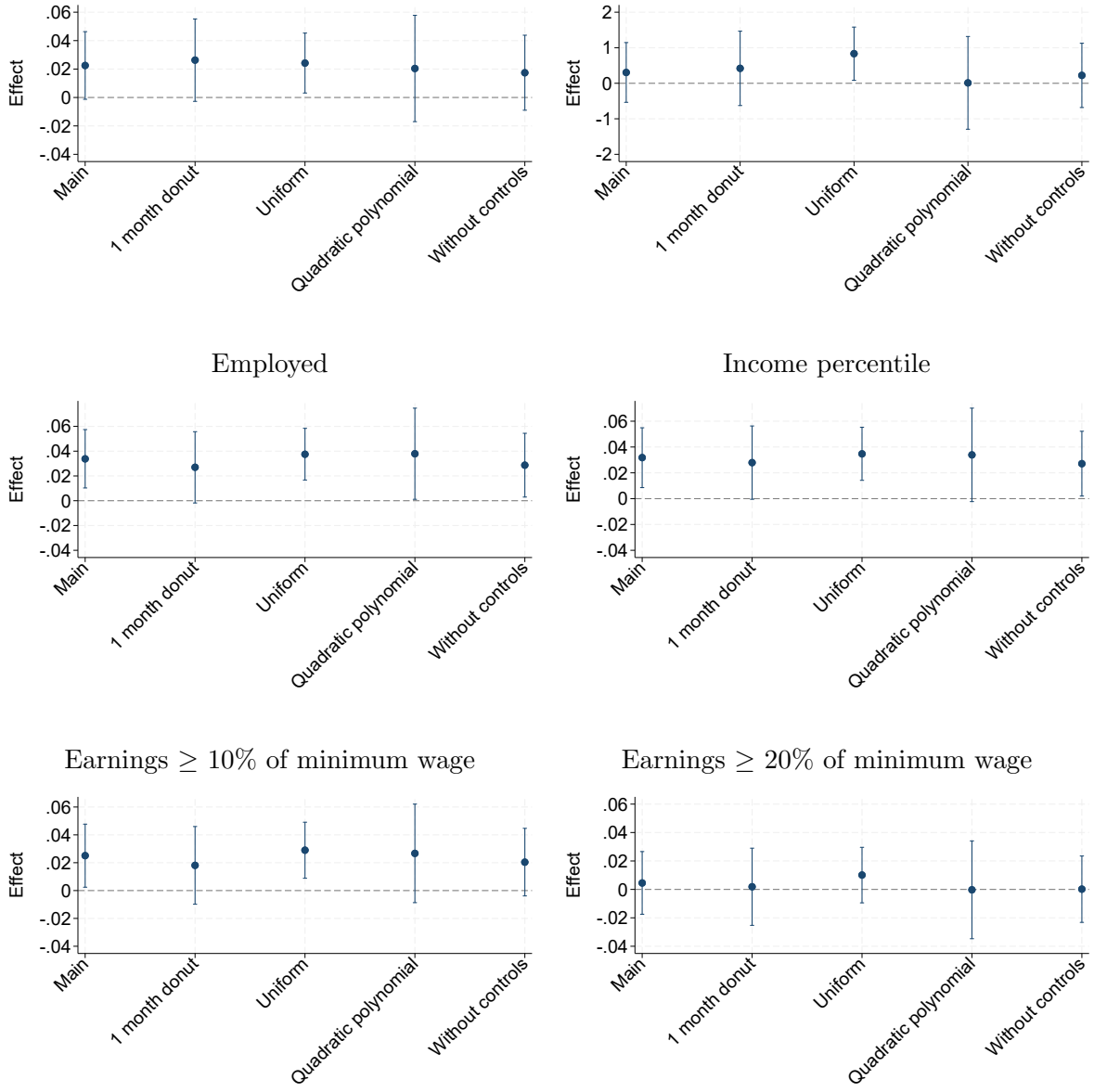
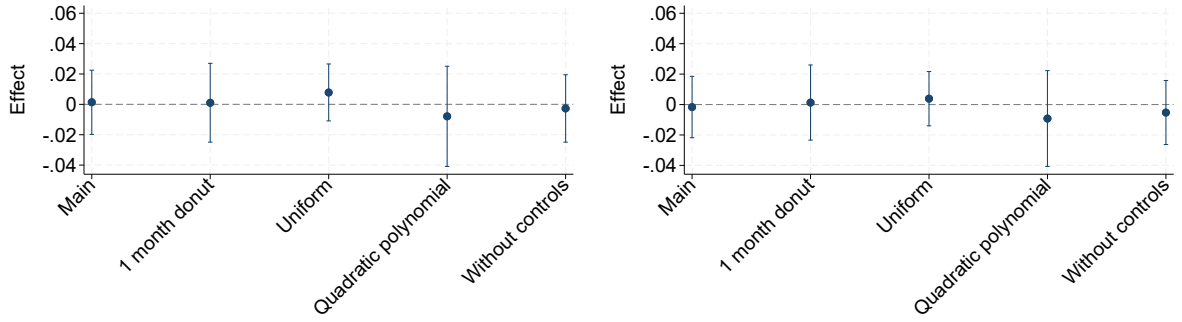
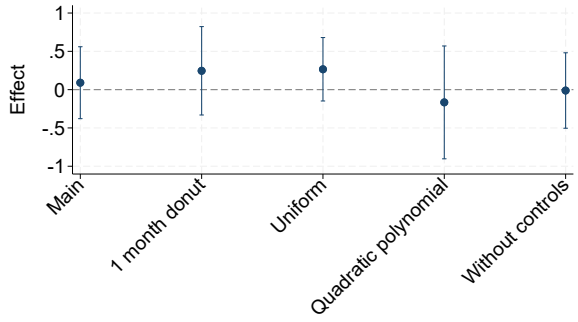


Figure 18: Sensitivity to specification choices: Effect of reform on mothers

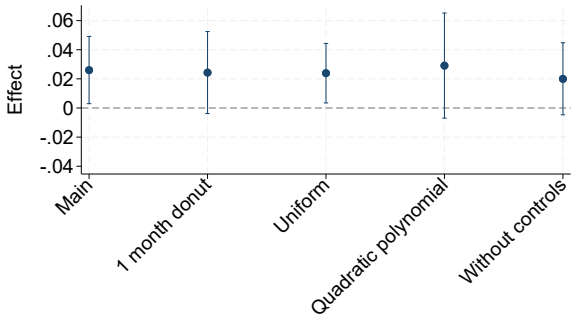


Earnings $\geq$ 50% of minimum wage

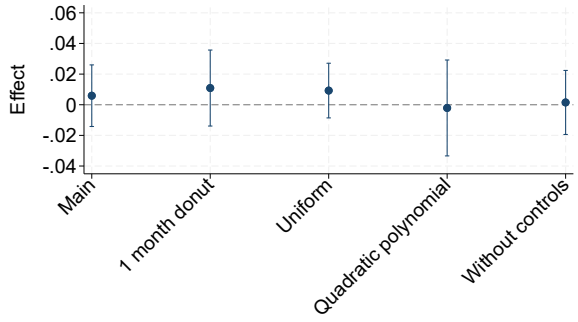
Earnings $\geq$ 60% of minimum wage



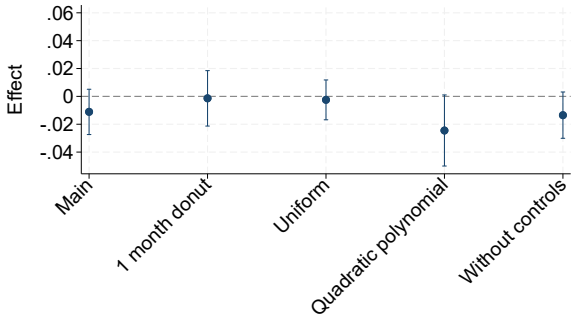
Hours worked per week



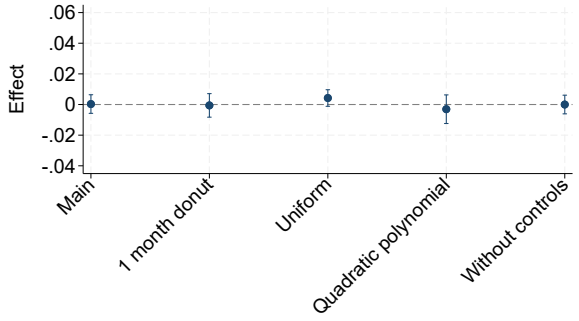
Hours worked per week $\geq$ 0



Hours worked per week $\geq$ 12



Hours worked per week $\geq$ 20



Hours worked per week $\geq$ 35

Figure 18: Sensitivity to specification choices: Effect of reform on mothers (continued)

Notes: See notes to Table 5. Regression discontinuity estimates and 95% confidence intervals are displayed. Main is the main specification. '1 month donut' uses a one month donut. 'Uniform' uses a uniform kernel. 'Quadratic polynomial' includes a quadratic polynomial. 'Without controls' omits control variables.

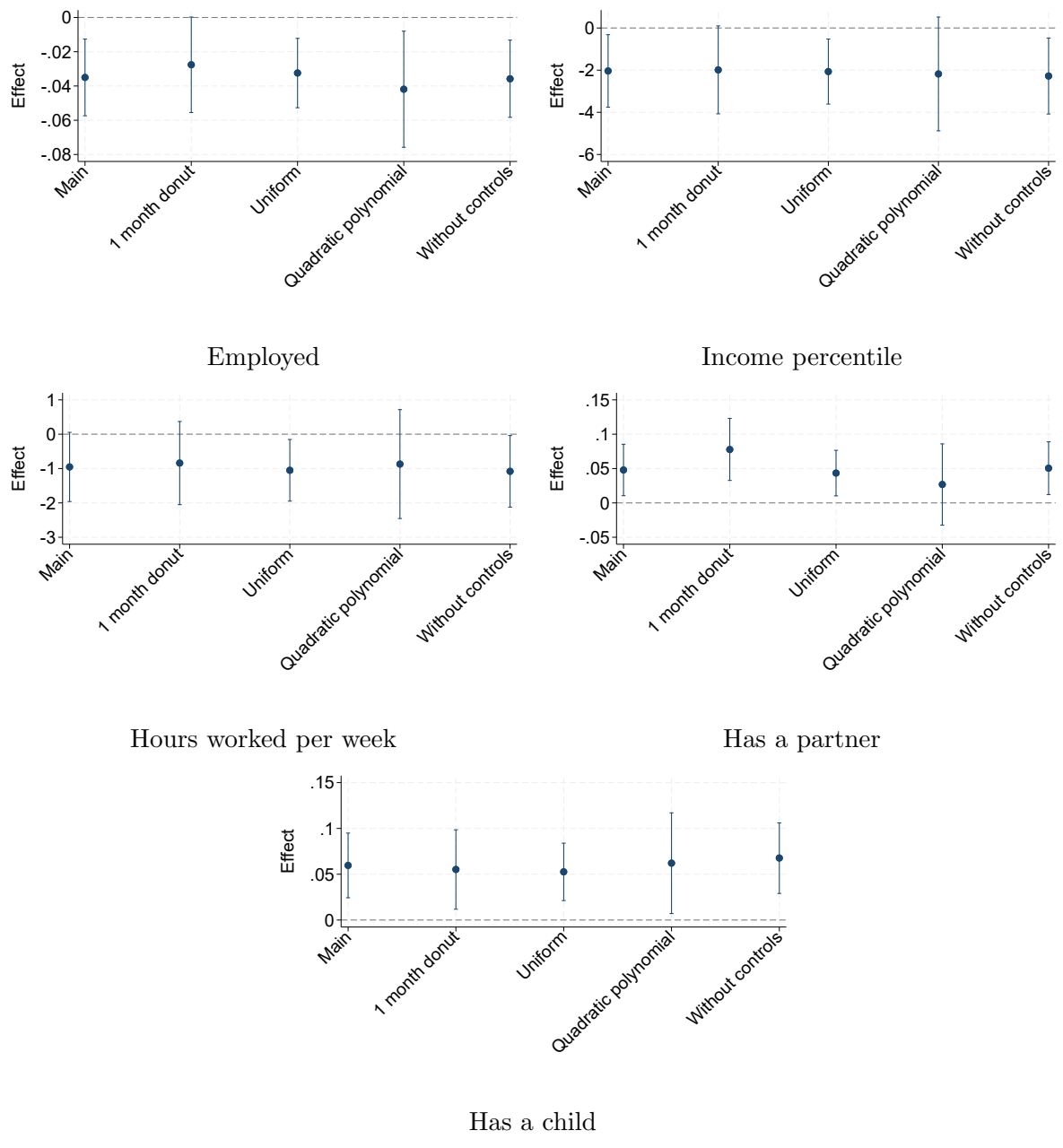


Figure 19: Sensitivity to specification choices: Effect of reform on daughters aged 27-33 in 2020

Notes: See notes to Table 9. Regression discontinuity estimates and 95% confidence intervals are displayed. Main is the main specification. '1 month donut' uses a one month donut. 'Uniform' uses a uniform kernel. 'Quadratic polynomial' includes a quadratic polynomial. 'Without controls' omits control variables.

Table 1: Summary statistics of mother's characteristics and pre-treatment outcomes

	Mother sample			All women
	Overall	Treatment	Control	
Background characteristics				
Age at first birth (years)	28.0	27.9	28.0	28.2
Number of children	2.4	2.4	2.4	1.8
in the household	2.4	2.4	2.4	1.8
in the household aged 9-22	2.1	2.1	2.1	1.5
Education level observed	22.5%	22.7%	22.2%	32.9%
Years of education	10.2	10.3	10.1	11.1
Vocational education	16.8%	16.8%	16.8%	16.8%
Upper vocational education	15.0%	15.5%	14.4%	16.8%
College education	11.4%	11.2%	11.6%	19.2%
Academic education	5.4%	6.0%	4.8%	8.2%
Lives in Bible Belt	29.6%	29.8%	29.4%	24.0%
Labor supply prior to reform				
Cumulative employment history (2003-2009, years)	2.4	2.5	2.4	5.6
Employed	35.0%	35.0%	34.9%	82.3%
Paid-employment	20.4%	20.3%	20.6%	73.2%
Self-employment	16.1%	16.4%	15.9%	11.8%
Earnings (euros)	1165.44	1168.93	1161.57	27448.40
Hours per week	1.0	1.0	1.0	17.6
Partner's labor supply prior to reform				
Paid-employment	88.2%	87.6%	88.9%	63.9%
Self-employment	16.3%	17.0%	15.6%	14.0%
Earnings (euros)	80407.34	80533.58	80267.14	52437.96
Hours per week	32.9	32.7	33.1	23.0
Observations	26,136	13,761	12,375	211,194

Notes: This table contains summary statistics for mothers who were eligible for the tax subsidy in 2009, have a child aged 9-22 living at home in 2009 and are born within a 10-month bandwidth around January 1963. Treatment column includes the mothers born before January 1963, control column includes the mothers born in January 1963 or later. For comparison, the last column displays summary statistics for all women born in this time frame. All outcomes are measured in 2009. Age at first births is measured in years. The number of children is the total number of children a women has regardless of age or living situation. Number of children in the household includes only those who live in the same household as their mother, and 'in the household aged 9-22' also restricts the age range of children. Bible Belt indicates the municipality of residence belongs to the Bible Belt. Cumulative employment history indicates the number of calendar years during 2003 to 2009 in which the mother was employed. Employed indicates paid employment or self-employment, these are not mutually exclusive. Hours per week worked are hours of paid worked and is unconditional on employment. Earnings are measured in euros, includes both earnings from paid and self-employment and is unconditional on employment.

Table 2: Summary statistics of daughter's and son's characteristics and pre-treatment outcomes

	Daughters		Sons	
	12-18	19-25	12-18	19-25
Background characteristics				
Age (2012)	15.6	21.4	15.6	21.6
Birth order	2.3	1.7	2.3	1.7
Labor supply				
Employed	8.8%	84.8%	9.2%	83.7%
Hours p/wk	0.2	11.2	0.3	14.7
Earnings (euros)	52.37	5388.41	68.37	8440.53
Education				
Enrolled in secondary or tertiary education	62.7%	67.3%	60.8%	63.1%
Years of educationn	5.2	10.9	5.1	10.5
Vocational track	30.0%	32.6%	30.8%	39.5%
Upper vocational track	10.6%	26.4%	10.4%	23.1%
College track	2.9%	24.6%	2.4%	21.3%
Academic track	19.3%	13.9%	17.2%	11.5%
Family formation Has a partner	-	0.40%	-	0.15%
Has a child	-	0.64%	-	0.21%
Number of children	-	0.69	-	0.21
Observations	11,168	12,696	12,075	14,825

Notes: This table contains summary statistics for daughters and sons who are aged 12-15 in 2012, live in the same household as their mother in 2009, have a mother who is eligible for the tax subsidy in 2009 and their mother is born within a 10-month bandwidth around January 1963. Summary statistics are displayed separately for daughters and sons, and based on child's aged in 2012. Outcomes are measured in 2009, except age. Employment includes paid-employment and self-employment. Hours per week worked are hours of paid worked and is unconditional on employment. Earnings are in euros and includes both earnings from paid and self-employment. Enrolled in education is a dummy for being enrolled in secondary or tertiary education during the year. Years of education is based on the current education if a child is still enrolled in education, otherwise it is based on the highest finished education level. The four school track dummies indicate whether a child is currently enrolled in this track or in case of no enrollment has finished education within this track. The first three years of high school, I do not distinguish between college and academic track and I assign these children to academic track. For younger children family formation outcomes are not displayed as these are too rare at this age and hence the numbers are too low to report on due to privacy concerns.

Table 3: Balancing table of mother's characteristics pre-reform

Control variable (2009)	RD-estimate	S.E.	p-value
Age at first birth (months)	-0.593	(1.385)	0.669
Paid employment	-0.0131	(0.0109)	0.228
Self-employment	-0.000297	(0.0101)	0.977
Hours p/wk	-0.00895	(0.0732)	0.903
Income (euros)	-26.15	(54.14)	0.629
Income (inverse hyperbolic sine)	-0.121	(0.111)	0.275
Partner self-employed	0.00898	(0.0101)	0.372
Partner's income (euros)	-601.1	(1,887)	0.750
Partner's income (ihst)	-0.00675	(0.0157)	0.667
Biblebelt	0.0150	(0.0125)	0.229
Education unknown	0.0109	(0.0115)	0.341
Years of education	-0.124	(0.124)	0.318
Number of children	0.0112	(0.0313)	0.721
Number of children in the household	0.0107	(0.0310)	0.730
Observations	26,136		

Notes: The sample consists of mothers who were eligible for the tax subsidy in 2009, have a child aged 9-22 living at home in 2009 and are born within a 10-month bandwidth around January 1963. The RD-estimate, standard error and p-value from a local-linear regression discontinuity specification with a triangular kernel, a polynomial of order one, a bandwidth of 10 months and without control variables are displayed. All outcomes are measured in 2009.

Table 4: Balancing table of daughter's and son's characteristics pre-reform

	Daughters			Sons		
Control variable (2009)	RD-estimate	S.E.	p-value	RD-estimate	S.E.	p-value
B. Daughters						
Mothers' characteristics						
Age at first birth (months)	-0.752	(1.643)	0.647	-0.710	(1.584)	0.654
Paid employment	-0.0115	(0.0145)	0.425	-0.00682	(0.0142)	0.630
Self-employment	-0.00477	(0.0139)	0.732	-0.00187	(0.0133)	0.888
Hours p/wk	0.0260	(0.0981)	0.791	-0.00239	(0.0945)	0.980
Income (euros)	-67.63	(72.17)	0.349	-32.52	(70.36)	0.644
Income (ihst)	-0.236	(0.148)	0.112	-0.125	(0.144)	0.388
Partner self-employed	0.00613	(0.0137)	0.655	0.00640	(0.0140)	0.649
Partner's income (euros)	-1,233	(2,642)	0.641	1,318	(2,646)	0.618
Partner's income (ihst)	-0.0148	(0.0212)	0.486	0.00909	(0.0210)	0.665
Bible Belt	0.0188	(0.0172)	0.274	0.0109	(0.0172)	0.526
Education unknown	0.0218	(0.0152)	0.152	0.0123	(0.0150)	0.412
Years of education	-0.312*	(0.169)	0.0650	-0.154	(0.167)	0.355
Number of children	0.0250	(0.0643)	0.697	0.0773	(0.0727)	0.288
Number of children in the household	0.0246	(0.0641)	0.702	0.0728	(0.0723)	0.314
Daughters' characteristics						
Age of child (years)	-0.0186	(0.104)	0.859	-0.119	(0.102)	0.244
Birth order	0.0357	(0.0414)	0.388	0.0513	(0.0423)	0.225
Observations	23,864	11,168	12,696	26,900	12,075	14,825

Notes: The sample consists of daughters or sons aged 9-22 in 2009 and living with their mother, who have a mothers who is eligible for the tax subsidy in 2009 and was born within a 10-month bandwidth around January 1963. The RD-estimate, standard error and p-value from a local linear regression discontinuity specification with a triangular kernel, a polynomial of order one, a bandwidth of 10 months and without control variables are displayed. All outcomes are measured in 2009.

Table 5: Effect of the reform on mothers' labor supply in 2020

	RD-Estimate	Mean
Employed	0.0225*	0.420
	(0.0121)	
Paid-employment	0.0260**	0.318
	(0.0118)	
Self-employment	0.00460	0.117
	(0.00820)	
Hours of work per week	0.0906	4.987
	(0.240)	
Hours of work per week ≥ 0	0.0260**	0.318
	(0.0118)	
Hours of work per week ≥ 12	0.00590	0.192
	(0.0103)	
Hours of work per week ≥ 20	-0.0111	0.105
	(0.00829)	
Hours of work per week ≥ 35	0.000304	0.0132
	(0.00309)	
Income percentile	0.304	10.87
	(0.429)	
Earnings (euros)	0.0977	3.024
	(311.8)	
Earnings $\geq$ % of minimum wage		
$\geq 10\%$	0.0338***	0.371
	(0.0120)	
$\geq 20\%$	0.0318***	0.334
	(0.0118)	
$\geq 30\%$	0.0251**	0.295
	(0.0116)	
$\geq 40\%$	0.00452	0.258
	(0.0112)	
$\geq 50\%$	0.00138	0.224
	(0.0108)	
$\geq 60\%$	-0.00168	0.193
	(0.0103)	
$\geq 70\%$	0.000480	0.165
	(0.00972)	
$\geq 80\%$	-0.00851	0.142
	(0.00919)	

Table 5: Effect of the reform on mothers' labor supply in 2020 (continued)

	RD-Estimate	Mean
$\geq$ 90%	-0.00702	0.122
	(0.00872)	
$\geq$ 100%	-0.00917	0.104
	(0.00820)	
Observations	26,136	

Notes: Sample consists of mothers who were eligible for the tax subsidy in 2009, have a child aged 9-22 living at home in 2009 and are born within a 10-month bandwidth around January 1963. Outcomes are measured in 2020. Employment is a dummy which includes both paid and self-employment, which are not mutually exclusive. Paid-employment and self-employment are also dummies. Hours of work per week measures the number hours of paid work per week. Hours of work per week $\geq$ X is a dummy variable for the hours of work exceeding X. Income percentile is the income percentile in the income distribution in the Netherlands. Earnings are measured in euros and include income from paid-employment and self-employment. Earnings $\geq$ X% is a dummy variable for earnings exceeding X% of the minimum wage. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order one and a bandwidth of 10 months are displayed. Controls are included. Standard errors are in parentheses. * p<0.1; ** p<0.05; *** p< 0.01

Table 6: Effect of the reform on mothers' cumulative labor supply in 2012-2020

	RD-Estimate	Mean
Employed (years)	0.151*	3.704
	(0.0873)	
Paid-employment(years)	0.102	2.688
	(0.0856)	
Self-employment (years)	0.0892	1.177
	(0.0604)	
Average hours of work per week	-0.0516	4.062
	(0.180)	
Hours of work per week $\geq$ 0 (years)	0.102	2.688
	(0.0856)	
Hours of work per week $\geq$ 12 (years)	-0.0171	1.338
	(0.0688)	
Hours of work per week $\geq$ 20 (years)	-0.0868*	0.665
	(0.0514)	
Hours of work per week $\geq$ 35 (years)	0.00460	0.0801
	(0.0156)	
Income percentile (Average)	0.191	9.785
	(0.336)	
Earnings (cumulative, euros)	629.1	44857
	(2,064)	
Cumulative earnings $\geq$ % of yearly minimum wage		
$\geq$ 10%	0.0320***	0.523
	(0.0117)	
$\geq$ 20%	0.0412***	0.503
	(0.0118)	
$\geq$ 30%	0.0392***	0.485
	(0.0118)	
$\geq$ 40%	0.0409***	0.471
	(0.0118)	
$\geq$ 50%	0.0384***	0.459
	(0.0118)	
$\geq$ 100%	0.0330***	0.402
	(0.0118)	
$\geq$ 150%	0.0259**	0.356
	(0.0117)	
$\geq$ 200%	0.0235**	0.318
	(0.0114)	
$\geq$ 250%	0.0170	0.282
	(0.0112)	
$\geq$ 300%	0.00667	0.246
	(0.0108)	

Table 6: Effect of the reform on mothers' cumulative labor supply in 2012-2020 (continued)

	RD-Estimate	Mean
Cumulative earnings ≥ % of yearly minimum wage		
≥ 400%	-0.00355	0.192
	(0.0100)	
≥ 450%	-0.00736	0.171
	(0.00967)	
≥ 500%	-0.0104	0.151
	(0.00922)	
Observations	26,136	

Notes: See notes to Table 5. Outcomes are measured as the cumulative outcome during 2012-2020. Employment, paid-employment and self-employment measure the number of years. Hours of work per week is the average during this time period. Hours of work per week ≥ X is a the number of year in which the hours of work exceeds X. Income percentile is the average income percentile over the years 2012-2020 in the income distribution in the Netherlands. Cumulative earnings are measured in euros and include income from paid-employment and self-employment. Cumulative earnings ≥ X% is a dummy variable for the cumulative earnings during 2012-2020 exceeding X% of the yearly minimum wage. * p<0.1; ** p<0.05; *** p< 0.01

Table 7: Effect of the reform on mothers' partner's labor supply and marriage stability

	RD-Estimate	Mean
Partners' labor supply		
Employed	0.00237	0.871
	(0.00894)	
Hours worked per week	0.758*	26.83
	(0.430)	
Income percentile	0.996	
	(0.805)	
Divorce	0.0128	0.153
	(0.00961)	
Observations	26,136	

Notes: See notes to Table 5. Outcomes are measured in 2020. The labor supply of the partner they had in 2009 is used as an outcome measure, regardless of whether they are partners in 2020. Divorce is a dummy for being being divorced from this partner in 2020. * p<0.1; ** p<0.05; *** p< 0.01

Table 8: Effect of the reform on daughter's labor supply and family formation in 2020

	All		Daughters by age			
			20-26		27-33	
	RD-Estimate	Mean	RD-Estimate	Mean	RD-Estimate	Mean
Employed	-0.0278***	0.892	-0.0212*	0.883	-0.0350***	0.900
	(0.00855)		(0.0128)		(0.0114)	
Paid-employment	-0.0203**	0.857	-0.0150	0.862	-0.0261*	0.852
	(0.00987)		(0.0140)		(0.0136)	
Self-employment	-0.00172	0.0427	-0.00468	0.0234	0.000889	0.0597
	(0.00554)		(0.00617)		(0.00886)	
Hours of work per week	-0.602	22.02	-0.207	19.81	-0.953*	23.96
	(0.382)		(0.547)		(0.514)	
Hours of work per week ≥ 0	-0.0203**	0.857	-0.0150	0.862	-0.0261*	0.852
	(0.00987)		(0.0140)		(0.0136)	
Hours of work per week ≥ 12	-0.0206*	0.723	-0.0194	0.651	-0.0232	0.785
	(0.0124)		(0.0192)		(0.0158)	
Hours of work per week ≥ 20	-0.00766	0.611	0.00597	0.515	-0.0195	0.696
	(0.0136)		(0.0201)		(0.0178)	
Hours of work per week ≥ 35	-0.00345	0.225	0.0104	0.185	-0.0144	0.259
	(0.0117)		(0.0158)		(0.0167)	
Income percentile	-1.117*	36.77	0.596	30.10	-2.037**	43.99
	(0.625)		(0.801)		(0.878)	
Earnings $\geq$ % of minimum wage						
$\geq 10\%$	-0.0280***	0.849	-0.0249	0.810	-0.0322***	0.883
	(0.00990)		(0.0157)		(0.0121)	
$\geq 20\%$	-0.0334***	0.813	-0.0432**	0.750	-0.0258**	0.868
	(0.0106)		(0.0172)		(0.0127)	
$\geq 30\%$	-0.0322***	0.777	-0.0374**	0.690	-0.0287**	0.853
	(0.0113)		(0.0185)		(0.0133)	
$\geq 40\%$	-0.0264**	0.740	-0.0209	0.633	-0.0317**	0.834
	(0.0117)		(0.0189)		(0.0140)	
$\geq 50\%$	-0.0246**	0.707	-0.0149	0.585	-0.0337**	0.814
	(0.0120)		(0.0191)		(0.0148)	
$\geq 60\%$	-0.0160	0.675	-0.00183	0.545	-0.0286*	0.790
	(0.0124)		(0.0193)		(0.0155)	
$\geq 70\%$	-0.0119	0.644	0.00405	0.507	-0.0259	0.765
	(0.0127)		(0.0193)		(0.0163)	
$\geq 80\%$	-0.0135	0.615	0.00819	0.475	-0.0323*	0.738
	(0.0130)		(0.0192)		(0.0169)	
$\geq 90\%$	-0.0148	0.585	0.0102	0.444	-0.0366**	0.710
	(0.0131)		(0.0190)		(0.0175)	
$\geq 100\%$	-0.0199	0.554	0.00347	0.414	-0.0394**	0.677
	(0.0133)		(0.0188)		(0.0179)	
$\geq 150\%$	-0.0240*	0.366	0.0127	0.247	-0.0557***	0.471
	(0.0132)		(0.0170)		(0.0191)	

Table 8: Effect of the reform on daughters labor supply and family formation in 2020 (continued)

	All		Daughters by age			
			20-26		27-33	
	RD-Estimate	Mean	RD-Estimate	Mean	RD-Estimate	Mean
$\geq$ 200%	-0.00343	0.193	0.0218*	0.106	-0.0247	0.269
	(0.0109)		(0.0122)		(0.0170)	
$\geq$ 300%	-0.00128	0.0300	0.00677**	0.00886	-0.00838	0.0486
	(0.00476)		(0.00344)		(0.00830)	
Years lived w/ mother	-0.227***	4.421	-0.109	6.320	-0.327***	2.750
	(0.0775)		(0.107)		(0.105)	
Partner	0.0338***	0.415	0.0166	0.196	0.0479**	0.608
	(0.0126)		(0.0153)		(0.0191)	
Child	0.0299***	0.246	-0.00507	0.0604	0.0596***	0.408
	(0.0109)		(0.00968)		(0.0180)	
Number of children	0.0560***	0.393	-0.00235	0.0792	0.105***	0.669
	(0.0196)		(0.0139)		(0.0335)	
Observations	23,864		11,168		12,696	

Notes: Sample consists of daughters aged 20-33 in 2020, who lived in the same household as their mother in 2009, have a mother who was eligible for the tax subsidy in 2009 and was born within a 10-month bandwidth around January 1963. Outcomes are measured in 2020. Employment is a dummy which includes both paid and self-employment, which are not mutually exclusive. Paid-employment and self-employment are also dummies. Hours of work per week measures the number hours of paid work per week. Hours of work per week $\geq$ X is a dummy variable for the hours of work exceeding X. Income percentile is the income percentile in the income distribution in the Netherlands. Earnings are measured in euros and include income from paid-employment and self-employment. Earnings $\geq$ X% is a dummy variable for earnings exceeding X% of the minimum wage. Years lived with mother is the number of years during 2012-2020 in which the child lived in the same household as their mother. Partner is a dummy for having a partner, either married or cohabiting. Child is a dummy for having a child, number is the number of children unconditional on having any children. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order one and a bandwidth of 10 months are displayed. Controls are included. Standard errors in parentheses and clustered at the mother level. * p<0.1; ** p<0.05; *** p< 0.01

Table 9: Effect of the reform on older daughter's employment and fertility in 2020

	Daughter employed & has a child	Daughter employed & does not have child	Daughter not employed & has a child	Daughter not employed & does not have a child
RD-estimate	0.0321*	-0.0671***	0.0275***	0.00752
	(0.0179)	(0.0184)	(0.00782)	(0.00870)
Mean	0.366	0.534	0.0424	0.0580
Observation	12,696	12,696	12,696	12,696

Notes: See notes to Table 9. Sample includes only daughters aged 27-33 in 2020. Outcomes are four mutually exclusive dummy variables.

Table 10: Effect of the reform on sons' labor supply and family formation in 2020

	All		Sons by age			
			20-26		27-33	
	RD-Estimate	Mean	RD-Estimate	Mean	RD-Estimate	Mean
Employed	0.00557	0.899	0.00619	0.866	0.00450	0.926
	(0.00840)		(0.0138)		(0.0100)	
Paid-employment	0.00259	0.837	-0.00128	0.825	0.00554	0.847
	(0.0105)		(0.0156)		(0.0135)	
Self-employment	-0.00150	0.0765	0.00271	0.0446	-0.00527	0.103
	(0.00744)		(0.00851)		(0.0111)	
Hours of work per week	-0.0555	26.38	-0.602	21.95	0.373	29.98
	(0.436)		(0.632)		(0.566)	
Hours of work per week ≥ 0	0.00259	0.837	-0.00128	0.825	0.00554	0.847
	(0.0105)		(0.0156)		(0.0135)	
Hours of work per week ≥ 12	0.00610	0.738	-0.00358	0.648	0.0136	0.811
	(0.0122)		(0.0192)		(0.0148)	
Hours of work per week ≥ 20	0.00184	0.675	-0.00994	0.544	0.0102	0.781
	(0.0125)		(0.0192)		(0.0156)	
Hours of work per week ≥ 35	-0.0102	0.520	-0.0197	0.368	-0.00195	0.644
	(0.0131)		(0.0186)		(0.0176)	
Income percentile	0.254	46.48	-0.819	34.24	0.635	58.17
	(0.691)		(0.934)		(0.892)	
Earnings $\geq$ % of minimum wage						
$\geq 10\%$	4.17e-05	0.863	-0.00545	0.798	0.00385	0.916
	(0.00953)		(0.0163)		(0.0104)	
$\geq 20\%$	0.00156	0.835	-0.00838	0.745	0.00847	0.908
	(0.0100)		(0.0172)		(0.0110)	
$\geq 30\%$	0.00846	0.808	0.00811	0.696	0.00800	0.899
	(0.0105)		(0.0180)		(0.0113)	
$\geq 40\%$	0.0103	0.782	0.0180	0.649	0.00314	0.891
	(0.0109)		(0.0188)		(0.0117)	
$\geq 50\%$	0.00466	0.762	0.00330	0.614	0.00513	0.882
	(0.0111)		(0.0189)		(0.0123)	
$\geq 60\%$	0.00389	0.741	-0.000565	0.581	0.00650	0.872
	(0.0113)		(0.0190)		(0.0127)	
$\geq 70\%$	0.00440	0.722	-0.00247	0.553	0.00917	0.860
	(0.0114)		(0.0190)		(0.0131)	
$\geq 80\%$	-0.00291	0.704	-0.00859	0.525	0.00145	0.849
	(0.0115)		(0.0190)		(0.0134)	
$\geq 90\%$	-0.00587	0.688	-0.00999	0.502	-0.00268	0.840
	(0.0116)		(0.0188)		(0.0138)	
$\geq 100\%$	-0.00538	0.674	-0.00924	0.481	-0.00293	0.830
	(0.0117)		(0.0188)		(0.0140)	
$\geq 150\%$	-0.0176	0.567	-0.0346*	0.349	-0.00262	0.745
	(0.0122)		(0.0178)		(0.0160)	

Table 10: Effect of the reform on sons labor supply and family formation in 2020 (continued)

	All		Sons by age			
			20-26		27-33	
	RD-Estimate	Mean	RD-Estimate	Mean	RD-Estimate	Mean
$\geq$ 200%	0.0108	0.388	-0.0103	0.187	0.0293	0.552
	(0.0121)		(0.0149)		(0.0181)	
$\geq$ 300%	0.00701	0.103	0.00580	0.0278	0.00839	0.165
	(0.00815)		(0.00645)		(0.0137)	
Years lived w/ mother	-0.0540	5.322	-0.0696	7.080	-0.0538	3.890
	(0.0782)		(0.102)		(0.108)	
Partner	-0.00438	0.312	-0.00665	0.0990	-0.00401	0.485
	(0.0111)		(0.0115)		(0.0176)	
Child	-0.00238	0.161	-0.00215	0.0241	-0.00262	0.273
	(0.00897)		(0.00621)		(0.0153)	
Number of children	-0.0163	0.249	-0.00792	0.0306	-0.0224	0.427
	(0.0162)		(0.00887)		(0.0280)	
Observations	26,900		12,075		14,825	

Notes: Sample consists of sons aged 20-33 in 2020, who lived in the same household as their mother in 2009, have a mother who was eligible for the tax subsidy in 2009 and was born within a 10-month bandwidth around January 1963. Outcomes are measured in 2020. Employment is a dummy which includes both paid and self-employment, which are not mutually exclusive. Paid-employment and self-employment are also dummies. Hours of work per week measures the number hours of paid work per week. Hours of work per week $\geq$ X is a dummy variable for the hours of work exceeding X. Income percentile is the income percentile in the income distribution in the Netherlands. Earnings are measured in euros and include income from paid-employment and self-employment. Earnings $\geq$ X% is a dummy variable for earnings exceeding X% of the minimum wage. Years lived with mother is the number of years during 2012-2020 in which the child lived in the same household as their mother. Partner is a dummy for having a partner, either married or cohabiting. Child is a dummy for having a child, number is the number of children unconditional on having any children. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order one and a bandwidth of 10 months are displayed. Controls are included. Standard errors in parentheses and clustered at the mother level. * p<0.1; ** p<0.05; *** p< 0.01

Table 11: Placebo test: Effect of the reform on mothers' labor supply in 2006-2008

	2006		2007		2008	
	RD-Estimate	Mean	RD-Estimate	Mean	RD-Estimate	Mean
Employed	0.000492	0.345	0.000297	0.358	0.00434	0.361
	(0.0110)		(0.0101)		(0.00874)	
Paid-employment	-0.00797	.219	-0.00175	0.220	-0.00376	0.209
	(0.00986)		(0.00913)		(0.00774)	
Self-employment	0.00135	0.131	-0.00149	0.143	0.00650	0.157
	(0.00752)		(0.00702)		(0.00608)	
Hours of work per week	-0.138	2.111	-0.0508	1.939	0.0568	1.513
	(0.145)		(0.132)		(0.0916)	
Hours of work per week ≥ 0	-0.00797	0.219	-0.00175	0.220	-0.00376	0.209
	(0.00986)		(0.00913)		(0.00774)	
Hours of work per week ≥ 12	-0.00409	0.0648	-0.00186	0.0548	0.00856**	0.0336
	(0.00649)		(0.00593)		(0.00436)	
Hours of work per week ≥ 20	-0.00301	0.0285	0.00315	0.0229	0.00452*	0.0114
	(0.00433)		(0.00378)		(0.00265)	
Hours of work per week ≥ 35	0.00229	0.00432	0.00138	0.00367	0.00232*	0.00187
	(0.00179)		(0.00177)		(0.00126)	
Percentile in the income distribution	-0.221	5.829	-0.300	5.722	-0.170	5.083
	(0.299)		(0.286)		(0.239)	
Earnings (euros)	-176.3	2814	-238.3	2695	-51.25	2253
	(224.8)		(232.8)		(214.0)	
Earnings $\geq$ % of minimum wage						
$\geq 10\%$	0.00150	0.254	-0.00963	0.261	-0.00731	0.256
	(0.0103)		(0.00976)		(0.00847)	
$\geq 20\%$	0.00680	0.183	-0.0123	0.184	-0.00964	0.172
	(0.00953)		(0.00925)		(0.00832)	
$\geq 30\%$	-0.00462	0.122	-0.0107	0.117	-0.00471	0.0947
	(0.00844)		(0.00827)		(0.00750)	
$\geq 40\%$	0.00475	0.0914	-0.00309	0.0835	0.000515	0.0596
	(0.00753)		(0.00724)		(0.00616)	
$\geq 50\%$	-0.00850	0.0740	-0.00635	0.0658	-0.00166	0.0437
	(0.00697)		(0.00657)		(0.00544)	
$\geq 60\%$	-0.00675	0.0603	0.000258	0.0514	-3.93e-05	0.0344
	(0.00630)		(0.00591)		(0.00485)	
$\geq 70\%$	-0.00989*	0.0499	-0.000624	0.0424	0.00143	0.0272
	(0.00577)		(0.00530)		(0.00435)	
$\geq 80\%$	-0.00778	0.0423	0.000805	0.0354	-0.00149	0.0227
	(0.00528)		(0.00489)		(0.00394)	
$\geq 90\%$	-0.00661	0.0363	8.51e-05	0.0304	-0.00141	0.0189
	(0.00454)		(0.00367)			
$\geq 100\%$	-0.00786*	0.0310	-0.000903	0.0265	-0.00116	0.0158
	(0.00467)		(0.00428)		(0.00340)	
Observations	26,136		26,136		26,136	

Notes: See notes to Table 5. The outcomes are measured in 2006-2008, prior to the reform. * p<0.1; ** p<0.05; *** p< 0.01

Table 12: Placebo test: Effect of the reform on single mother's labor supply in 2020

	RD-Estimate	Mean
Employed	0.0137	0.681
	(0.0107)	
Paid-employment	0.0137	0.616
	(0.0113)	
Self-employment	0.00255	0.0717
	(0.00626)	
Hours of work per week	0.242	16.69
	(0.343)	
Hours of work per week ≥ 0	0.0137	0.616
	(0.0113)	
Hours of work per week ≥ 12	0.0115	0.561
	(0.0117)	
Hours of work per week ≥ 20	0.00290	0.498
	(0.0119)	
Hours of work per week ≥ 35	0.00459	0.139
	(0.00888)	
Income percentile	0.345	35.25
	(0.636)	
Earnings (euros)	653.6	26245
	(568.8)	
Earnings $\geq$ % of minimum wage		
$\geq 10\%$	0.00701	0.662
	(0.0109)	
$\geq 20\%$	0.00827	0.650
	(0.0110)	
$\geq 30\%$	0.00914	0.637
	(0.0111)	
$\geq 40\%$	0.0122	0.623
	(0.0112)	
$\geq 50\%$	0.00676	0.607
	(0.0113)	
$\geq 60\%$	0.00219	0.591
	(0.0114)	
$\geq 70\%$	-0.00359	0.572
	(0.0114)	
$\geq 80\%$	0.00217	0.552
	(0.0115)	

Table 12: Placebo test: Effect of the reform on single mother's labor supply in 2020 (continued)

	RD-Estimate	Mean
$\geq$ 90%	0.00147	0.532
	(0.0114)	
$\geq$ 100%	0.00109	0.509
	(0.0115)	
Observations	24,597	

Notes: See notes to Table 5. Sample consists of mothers who were single in 2009 and hav a child aged 9-22 living at home in 2009 and are born within a 10-month bandwidth around January 2020. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order on and a bandwidth of 10 months are displayed. Controls are included. Standard errors are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01

Table 13: Placebo test: Effect of the reform on labor supply and family formation in 2020 of daughters of single mothers

	Daughters by age			
	20-26		27-33	
	RD-estimate	Mean	RD-estimate	Mean
Employed	-0.0211	0.864	-0.0168	0.829
	(0.0172)		(0.0153)	
Paid-employment	-0.0244	0.843	-0.0202	0.785
	(0.0184)		(0.0167)	
Self-employment	-0.00583	0.0262	-0.000134	0.0568
	(0.00823)		(0.00895)	
Hours of work per week	-0.471	18.77	-0.577	21.81
	(0.690)		(0.582)	
Hours of work per week ≥ 0	-0.0244	0.843	-0.0202	0.785
	(0.0184)		(0.0167)	
Hours of work per week ≥ 12	-0.0176	0.622	-0.0206	0.710
	(0.0244)		(0.0183)	
Hours of work per week ≥ 20	-0.0247	0.485	-0.0277	0.632
	(0.0247)		(0.0195)	
Hours of work per week ≥ 35	0.00948	0.166	-0.00876	0.235
	(0.0192)		(0.0169)	
Income percentile	-0.175	28	-1.877*	39.49
	(1.023)		(1.055)	
Earnings $\geq$ % of minimum wage				
$\geq 10\%$	-0.0255	0.790	-0.0177	0.809
	(0.0205)		(0.0160)	
$\geq 20\%$	-0.0123	0.727	-0.0138	0.790
	(0.0225)		(0.0166)	
$\geq 30\%$	-0.0122	0.668	-0.00987	0.773
	(0.0236)		(0.0170)	
$\geq 40\%$	0.00440	0.609	-0.0137	0.755
	(0.0241)		(0.0174)	
$\geq 50\%$	-0.00614	0.556	-0.0234	0.737
	(0.0238)		(0.0177)	
$\geq 60\%$	-0.00238	0.516	-0.0205	0.715
	(0.0236)		(0.0182)	
$\geq 70\%$	-0.0174	0.469	-0.0258	0.688
	(0.0237)		(0.0187)	
$\geq 80\%$	-0.0260	0.436	-0.0257	0.661
	(0.0237)		(0.0191)	
$\geq 90\%$	-0.0193	0.405	-0.0323*	0.633
	(0.0233)		(0.0193)	
$\geq 100\%$	-0.0207	0.375	-0.0253	0.604
	(0.0229)		(0.0195)	

Table 13: Placebo test: Effect of the reform on labor supply and family formation in 2020 of daughters of single mothers (continued)

	Daughters by age			
	20-26		27-33	
	RD-estimate	Mean	RD-estimate	Mean
$\geq$ 150%	0.0116	0.219	-0.00127	0.427
	(0.0203)		(0.0199)	
$\geq$ 200%	0.0166	0.0939	-0.0153	0.255
	(0.0150)		(0.0174)	
$\geq$ 300%	0.00433	0.00868	-0.00681	0.0564
	(0.00471)		(0.00916)	
Lives w/ mother	-0.0155	0.253	-0.0127	0.0513
	(0.0222)		(0.00907)	
Partner	0.00248	0.201	-0.0232	0.550
	(0.0193)		(0.0197)	
Child	0.0155	0.0853	-0.00148	0.441
	(0.0136)		(0.0187)	
Number of childre	0.0125	0.116	0.0161	0.743
	(0.0216)		(0.0386)	
Observations	7,682		12,076	

Notes: See notes to Table 9. Sample consists of daughters aged 20-33 in 2020, who lived in the same household as their mother in 2009, have a mother who was single in 2009 and was born within a 10-month bandwidth around January 2020. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order on and a bandwidth of 10 months are displayed. Controls are included. Standard errors are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01

Table 14: Placebo test: Effect of the reform on labor supply and family formation in 2020 of sons of single mothers

	Sons by age			
	20-26		27-33	
	RD-estimate	Mean	RD-estimate	Mean
Employed	0.0133	0.829	-0.00930	0.850
	(0.0182)		(0.0140)	
Paid-employment	-0.00181	0.790	0.00798	0.778
	(0.0200)		(0.0165)	
Self-employment	0.000351	0.0432	-0.0139	0.0950
	(0.0107)		(0.0120)	
Hours of work per week	-0.318	19.42	0.150	26.09
	(0.752)		(0.663)	
Hours of work per week ≥ 0	-0.00181	0.790	0.00798	0.778
	(0.0200)		(0.0165)	
Hours of work per week ≥ 12	-0.0150	0.597	0.00666	0.722
	(0.0241)		(0.0178)	
Hours of work per week ≥ 20	-0.0341	0.478	0.00742	0.684
	(0.0243)		(0.0187)	
Hours of work per week ≥ 35	0.0116	0.285	-0.00709	0.525
	(0.0211)		(0.0202)	
Income percentile	-0.490	29.46	-0.556	48.43
	(1.119)		(1.151)	
Earnings $\geq$ % of minimum wage				
$\geq 10\%$	0.0244	0.752	-0.0134	0.834
	(0.0214)		(0.0148)	
$\geq 20\%$	0.00709	0.697	-0.0192	0.821
	(0.0224)		(0.0153)	
$\geq 30\%$	-0.00414	0.648	-0.0187	0.811
	(0.0235)		(0.0156)	
$\geq 40\%$	-0.0113	0.601	-0.0183	0.796
	(0.0240)		(0.0159)	
$\geq 50\%$	-0.0143	0.560	-0.0157	0.783
	(0.0241)		(0.0163)	
$\geq 60\%$	-0.0158	0.522	-0.00877	0.769
	(0.0241)		(0.0168)	
$\geq 70\%$	-0.0174	0.485	-0.00398	0.756
	(0.0239)		(0.0172)	
$\geq 80\%$	-0.0113	0.456	0.00101	0.743
	(0.0236)		(0.0175)	
$\geq 90\%$	-0.00358	0.432	-0.00926	0.731
	(0.0230)		(0.0177)	
$\geq 100\%$	-0.00490	0.407	-0.0135	0.715
	(0.0228)		(0.0181)	

Table 14: Placebo test: Effect of the reform on labor supply and family formation in 2020 of sons of single mothers (continued)

	Sons by age			
	20-26		27-33	
	RD-estimate	Mean	RD-estimate	Mean
$\geq$ 150%	0.00209	0.278	0.000479	0.608
	(0.0210)		(0.0200)	
$\geq$ 200%	-0.0143	0.138	0.00483	0.427
	(0.0165)		(0.0200)	
$\geq$ 300%	-0.00970	0.0213	-0.00156	0.125
	(0.00748)		(0.0130)	
Lives w/ mother	0.0137	0.348	-0.000816	0.0986
	(0.0233)		(0.0127)	
Partner	0.00659	0.103	-0.0103	0.417
	(0.0146)		(0.0196)	
Child	-0.00787	0.0267	-0.00170	0.252
	(0.00861)		(0.0170)	
Number of children	-0.0105	0.0319	0.00283	0.380
	(0.0106)		(0.0285)	
Observations	8,172		12,203	

Notes: See notes to Table 10. Sample consists of sons aged 20-33 in 2020, who lived in the same household as their mother in 2009, have a mother who was single in 2009 and was born within a 10-month bandwidth around January 2020. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order on and a bandwidth of 10 months are displayed. Controls are included. Standard errors are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01

Table 15: Placebo test: Effect of placebo cut-offs on mothers' labor supply in 2020

	True cut-off	Placebo cut-off	
	1963	1961	1962
Employed	0.0225*	-0.0105	-0.00134
	(0.0121)	(0.0123)	(0.0118)
Paid-employment	0.0260**	-0.000148	0.00661
	(0.0118)	(0.0114)	(0.0112)
Self-employment	0.00460	-0.00895	0.00200
	(0.00820)	(0.00829)	(0.00786)
Hours of work per week	0.0906	0.00578	0.357
	(0.240)	(0.216)	(0.224)
Hours of work per week ≥ 0	0.0260**	-0.000148	0.00661
	(0.0118)	(0.0114)	(0.0112)
Hours of work per week ≥ 12	0.00590	0.00281	0.0144
	(0.0103)	(0.00955)	(0.00967)
Hours of work per week ≥ 20	-0.0111	0.00711	0.00966
	(0.00829)	(0.00717)	(0.00758)
Hours of work per week ≥ 35	0.000304	-0.00378	0.00633**
	(0.00309)	(0.00251)	(0.00295)
Income percentile	0.304	-0.0254	0.439
	(0.429)	(0.408)	(0.411)
Earnings (euros)	116.8	-110.9	288.4
	(311.8)	(297.7)	(301.0)
Earnings $\geq$ % of minimum wage			
$\geq 10\%$	0.0338***	-0.0157	0.00486
	(0.0120)	(0.0120)	(0.0116)
$\geq 20\%$	0.0318***	-0.0129	0.0114
	(0.0118)	(0.0117)	(0.0114)
$\geq 30\%$	0.0251**	-0.00811	0.0147
	(0.0116)	(0.0113)	(0.0110)
$\geq 40\%$	0.00452	-0.00164	0.0153
	(0.0112)	(0.0107)	(0.0106)
$\geq 50\%$	0.00138	-0.00447	0.0178*
	(0.0108)	(0.0101)	(0.0101)
$\geq 60\%$	-0.00168	-0.00324	0.0118
	(0.0103)	(0.00949)	(0.00960)
$\geq 70\%$	0.000480	0.00438	0.0116
	(0.00972)	(0.00886)	(0.00901)
$\geq 80\%$	-0.00851	0.00442	0.00947
	(0.00919)	(0.00825)	(0.00848)
$\geq 90\%$	-0.00702	0.00994	0.0141*
	(0.00872)	(0.00778)	(0.00792)
$\geq 100\%$	-0.00917	0.00355	0.0102
	(0.00820)	(0.00717)	(0.00744)
Observations	26,136	23,654	25,365

Notes: See notes to Table 5. True cut-off is January 1963. Placebo cut-offs are January 1961 and January 1962. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order on and a bandwidth of 10 months are displayed. Controls are included. Standard errors are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01

Table 16: Placebo test: Effect of placebo cut-offs on daughters' and sons' labor supply and family formation in 2020

	Daughters by age					
	20-26			27-33		
	True cut-off	Placebo cut-off		True cut-off	Placebo cut-off	
	1963	1961	1962	1963	1961	1962
Employed	-0.0212*	-0.0140	0.000107	-0.0350***	-0.0166	-0.00393
	(0.0128)	(0.0171)	(0.0148)	(0.0114)	(0.0117)	(0.0115)
Income percentile	0.596	-1.976**	-0.521	-2.037**	-2.077**	1.218
	(0.801)	(0.997)	(0.880)	(0.878)	(0.914)	(0.874)
Hours of work per week	-0.207	-0.808	-0.947	-0.953*	-0.790	-0.0472
	(0.547)	(0.691)	(0.601)	(0.514)	(0.521)	(0.506)
Partner	0.0166	-0.00924	-0.0106	0.0479**	0.0129	-0.0243
	(0.0153)	(0.0206)	(0.0171)	(0.0191)	(0.0190)	(0.0185)
Child	-0.00507	-0.00751	-0.00317	0.0596***	0.00733	-0.0118
	(0.00968)	(0.0121)	(0.0102)	(0.0180)	(0.0181)	(0.0176)
Observations	11,168	7,519	9,299	12,696	12,501	13,199
	Sons by age					
	20-26			27-33		
	True cut-off	Placebo cut-off		True cut-off	Placebo cut-off	
	1963	1961	1962	1963	1961	1962
Employed	0.00619	0.0302	-0.0237	0.00450	0.00387	-0.0110
	(0.0138)	(0.0184)	(0.0152)	(0.0100)	(0.0102)	(0.00988)
Income percentile	-0.819	2.471**	-1.522	0.635	-0.454	-2.131**
	(0.934)	(1.160)	(1.055)	(0.892)	(0.918)	(0.864)
Hours of work per week	-0.602	1.676**	-0.349	0.373	0.496	-0.966*
	(0.632)	(0.800)	(0.694)	(0.566)	(0.560)	(0.539)
Partner	-0.00665	0.0138	-0.000938	-0.00401	0.00304	-0.0323*
	(0.0115)	(0.0140)	(0.0128)	(0.0176)	(0.0176)	(0.0173)
Child	-0.00215	-0.00368	0.000870	-0.00262	0.00664	-0.00416
	(0.00621)	(0.00830)	(0.00707)	(0.0153)	(0.0150)	(0.0147)
Observations	12,075	7,476	9,718	14,825	15,133	15,434

Notes: See notes to Table 9 and Table 10. True cut-off is January 1963. Placebo cut-offs are January 1961 and January 1962. RD-estimates of an RDD-specification with a triangular kernel, polynomial of order on and a bandwidth of 10 months are displayed. Controls are included. Standard errors are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01

Table 17: RD-estimates of the reform on education level

	Years of education	Enrolled in 2020	Education level ≥			
			Vocational	Upper vocational	College	Academic
Panel A. Daughters aged 27-33						
RD-estimate	-0.118	0.00611	-0.0104	-0.00787	-0.0243	0.00272
	(0.0929)	(0.00689)	(0.0128)	(0.0180)	(0.0186)	(0.0126)
Mean	12.26	0.0298	0.879	0.663	0.406	0.120
Observations	12,696	12,696	12,696	12,696	12,696	12,696
Panel B. Daughters aged 20-26						
RD-estimate	0.0290	-0.0196	-0.0209	0.00265	0.0114	0.0124
	(0.0886)	(0.0175)	(0.0136)	(0.0183)	(0.0203)	(0.0165)
Mean	12.20	0.281	0.871	0.729	0.520	0.221
N	11,168	11,168	11,168	11,168	11,168	11,168
Panel C. Sons aged 27-33						
RD-estimate	0.0998	-0.00182	-0.00379	-0.00369	0.00798	0.0117
	(0.0975)	(0.00682)	(0.0143)	(0.0184)	(0.0172)	(0.0104)
Mean	11.67	0.0360	0.824	0.537	0.327	0.103
N	14,825	14,825	14,825	14,825	14,825	14,825
Panel D. Sons aged 20-26						
RD-estimate	0.00572	0.00354	-0.0146	-0.0130	-0.00685	0.0218
	(0.0922)	(0.0170)	(0.0162)	(0.0196)	(0.0195)	(0.0151)
Mean	11.59	0.283	0.815	0.599	0.428	0.186
N	12,075	12,075	12,075	12,075	12,075	12,075

Notes: See notes to Table 9 and Table 10. Outcomes are measured in 2020. Education level includes finished education and current enrollment. Coefficients of an RDD-specification with a triangular kernel, polynomial of order one and a bandwidth of 10 months are displayed. Standard errors are in parentheses and clustered at the mother level. * p<0.1; ** p<0.05; *** p< 0.01

Table 18: RD-estimates of the reform on daughters aged 27-33 in 2020 split by distance to mother

	Employed	Hours p/wk	Partner	Child
	Daughters who live close to mother in 2020			
RD-estimate	-0.0328**	-0.143	0.0539*	0.0530**
	(0.0153)	(0.726)	(0.0278)	(0.0262)
Mean	0.909	23.56	0.578	0.0436
Observations	6,023	6,023	6,023	6,023
	Daughters who live far from mother in 2020			
RD-estimate	-0.0294*	-1.546**	0.0612**	0.0700***
	(0.0151)	(0.714)	(0.0267)	(0.0255)
Mean	0.921	25.12	0.662	0.387
Observations	6,034	6,034	6,034	6,034

Notes: See notes to Table 9. Distance is measured as the distance between the x,y coordinates of mother's and daughter's neighborhood. The sample is split based on whether this distance is below or above the median distance. Coefficients of an RDD-specification with a triangular kernel, polynomial of order one and a bandwidth of 10 months are displayed. Standard errors are in parentheses and clustered at the mother level. * p<0.1; ** p<0.05; *** p< 0.01

Appendix: Additional Figures and Tables

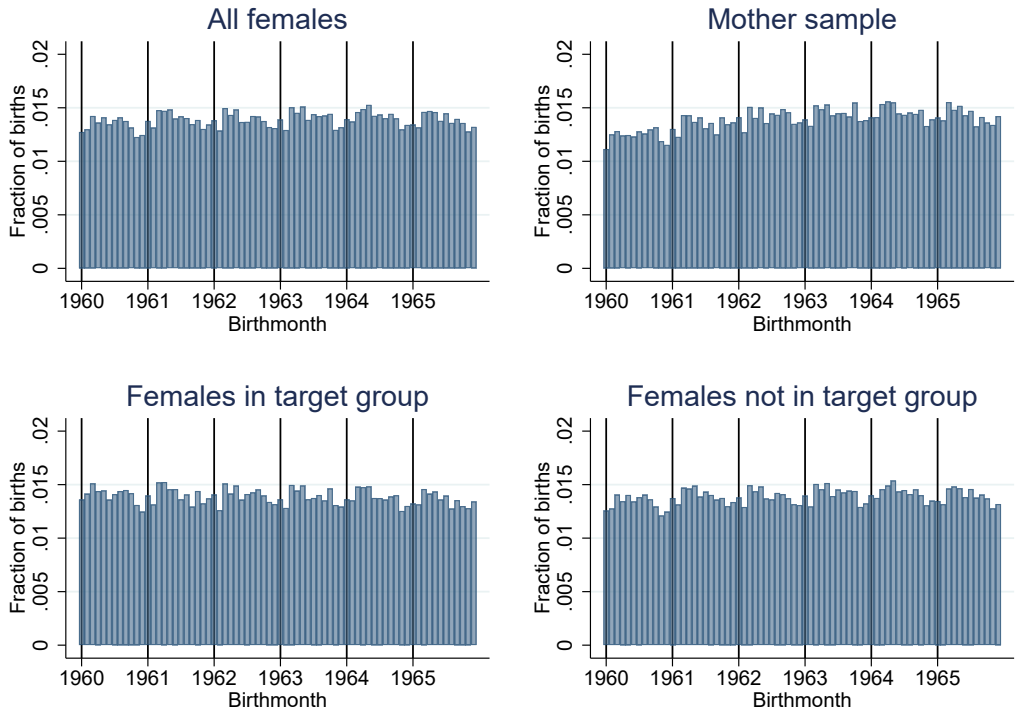


Figure A.1: Histogram of number of women born per month

Notes: Each bar represents the fraction of women born in a month. The black lines are placed in January. All females consists of all women born in the Netherlands. Mother sample is the main sample. Females in target group consists of all females who are eligible for the tax subsidy in 2009, females not in target group consists of all females who are not eligible for the tax subsidy in 2009.

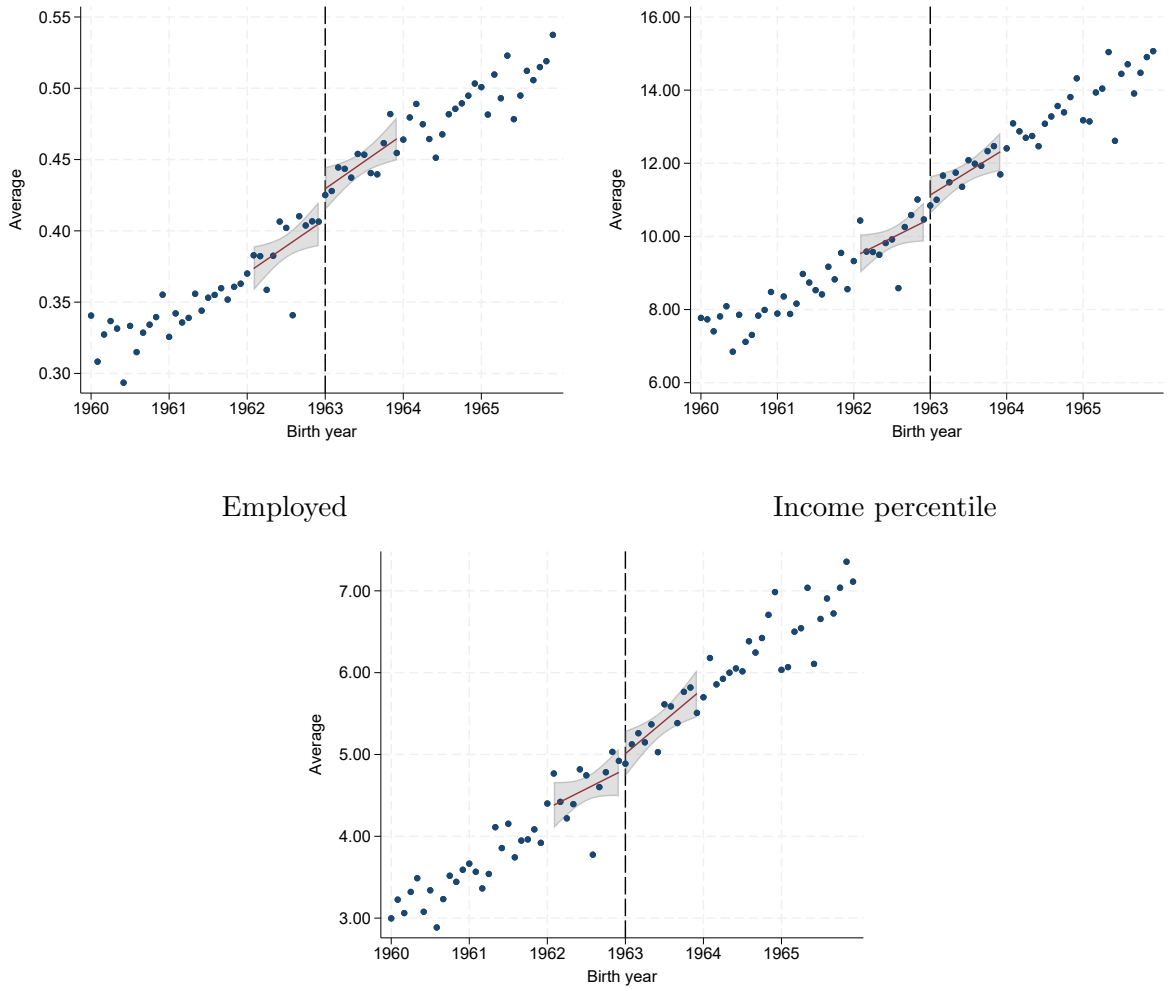


Figure A.2: Effect of the reform on the labor supply of mothers in 2020

Notes: See notes to Table 5. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

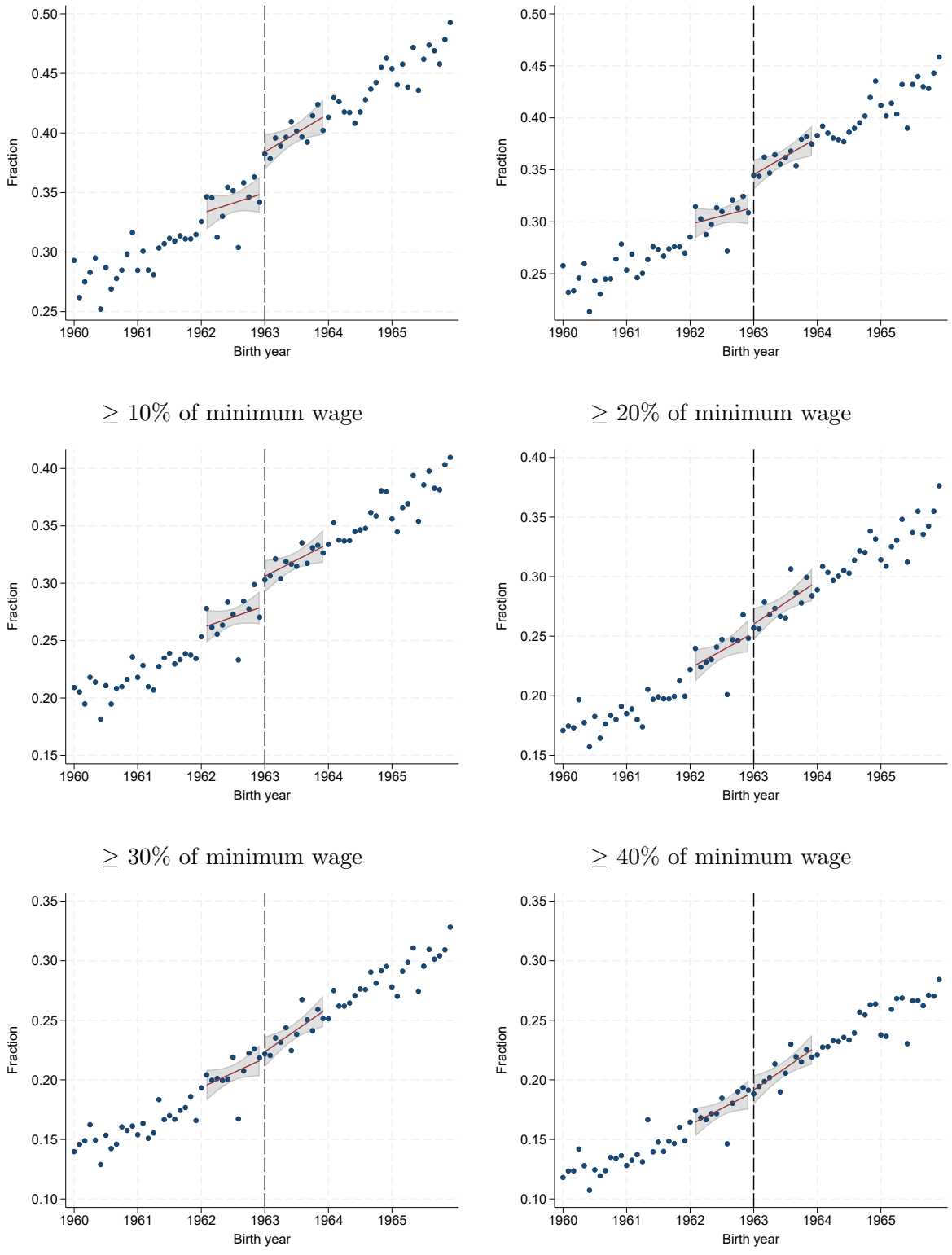


Figure A.3: Effect of the reform on the earnings of mothers in 2020

Notes: See notes to Table 5. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

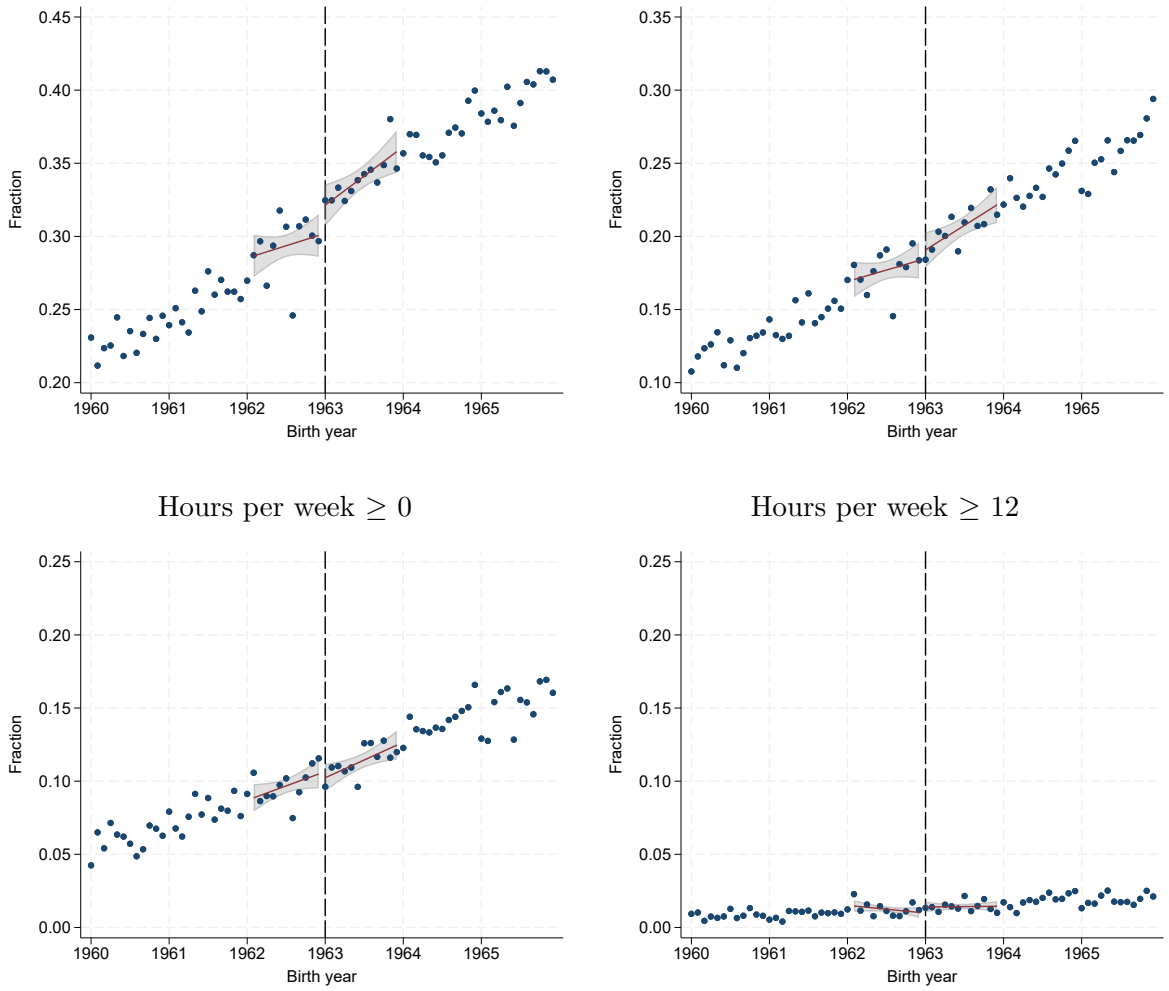


Figure A.4: Effect of the reform on hours worked per week of mothers in 2020

Notes: See notes to Table 5. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

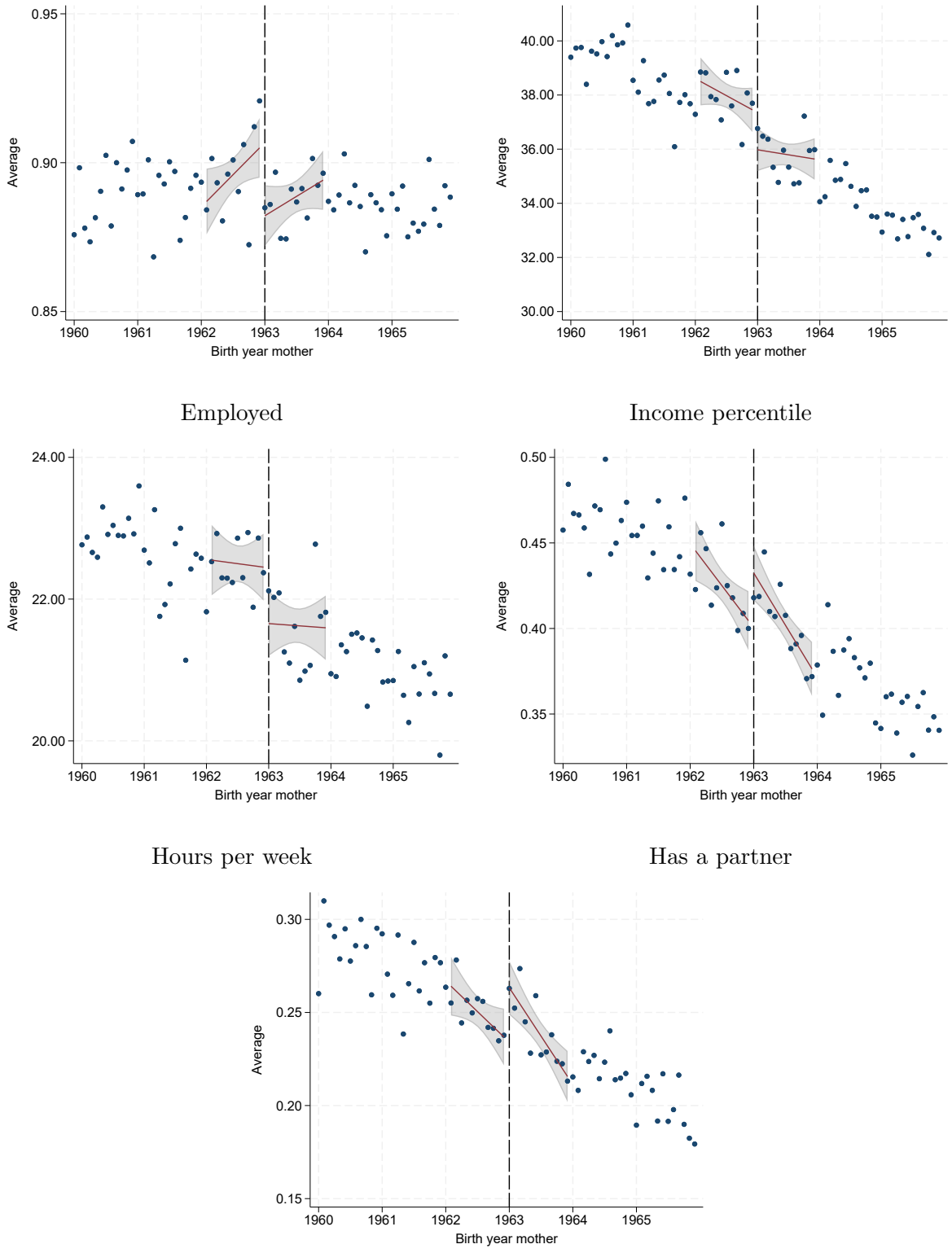
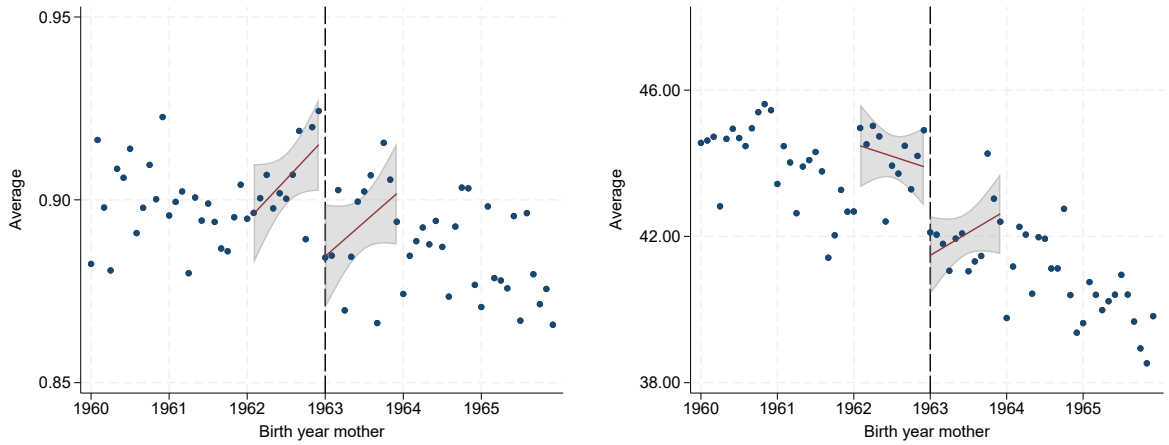


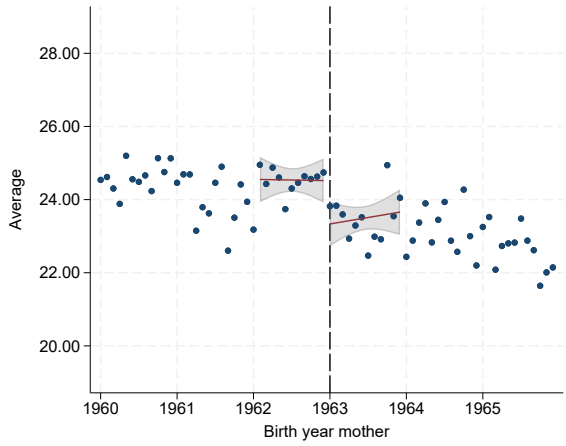
Figure A.5: Effect of the reform on daughters in 2020

Notes: See notes to Table 9. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

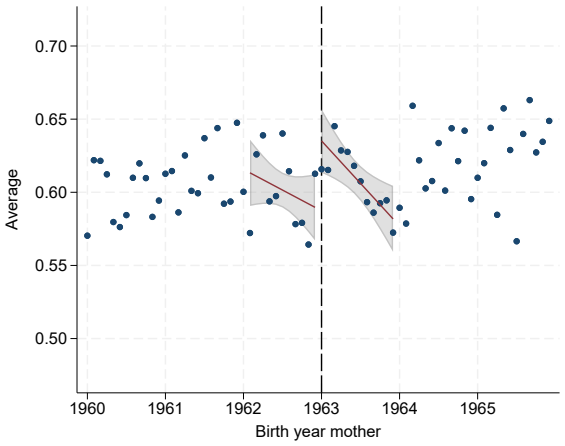


Employed

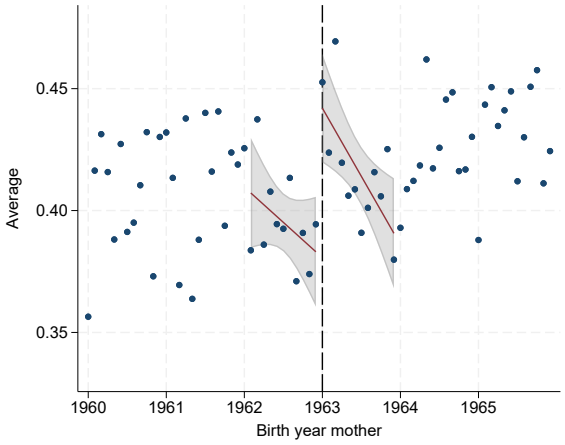
Income percentile



Hours per week



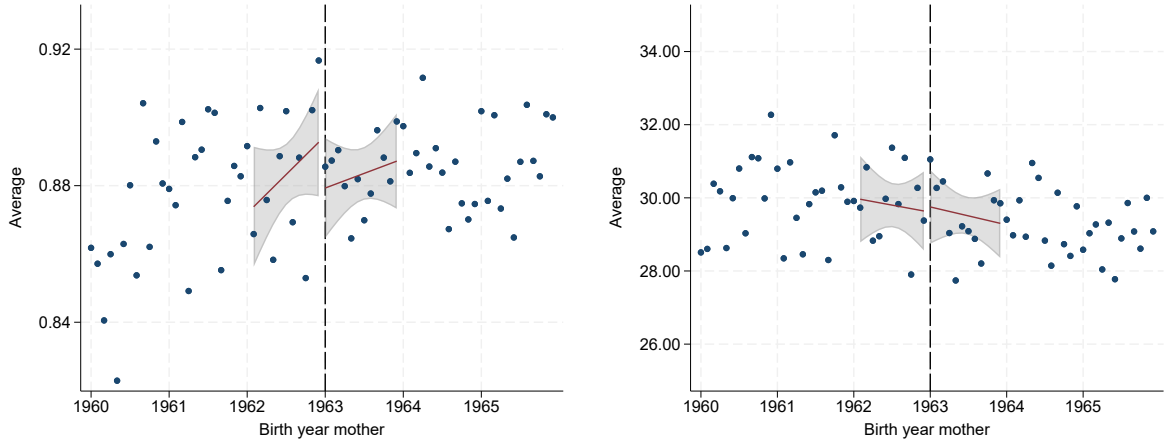
Has a partner



Has a child

Figure A.6: Effect of the reform on daughters aged 27-33 in 2020

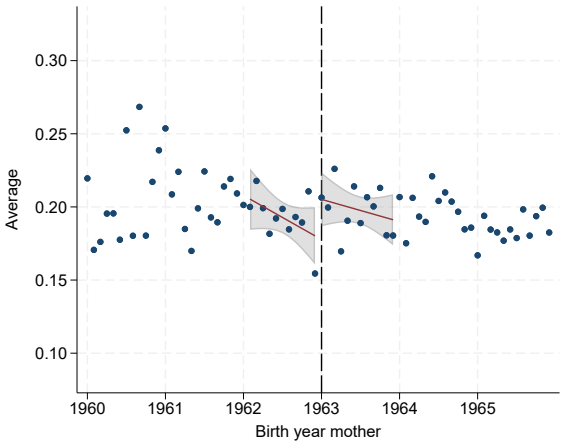
Notes: See notes to Table 9. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.



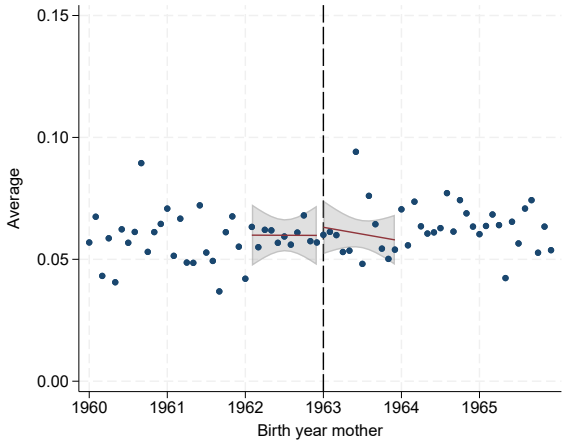
Employed

Income percentile

Hours per week



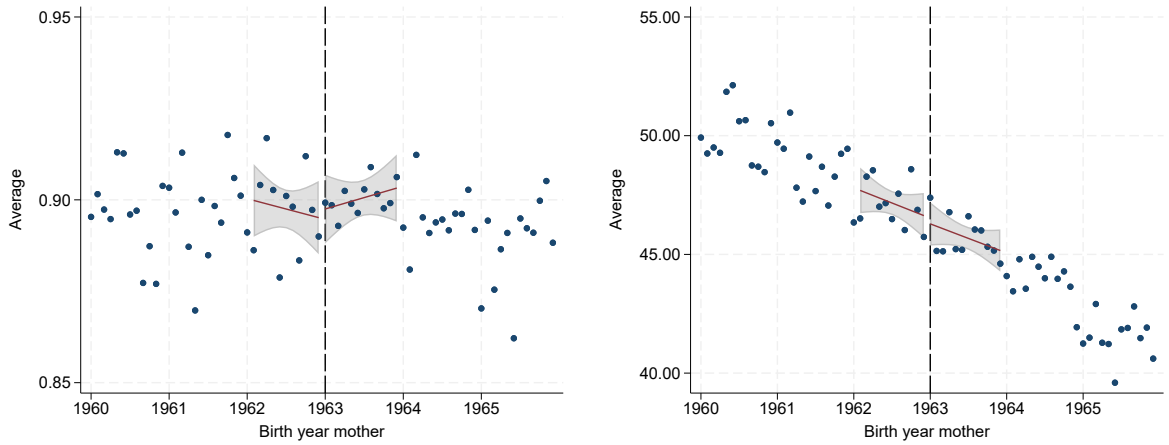
Has a partner



Has a child

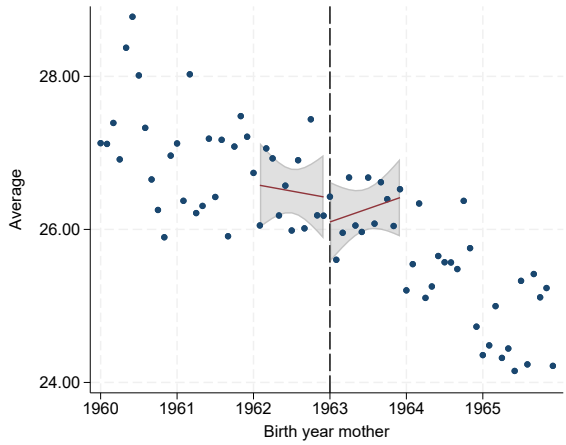
Figure A.7: Effect of the reform on daughters aged 20-26 in 2020

Notes: See notes to Table 9. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

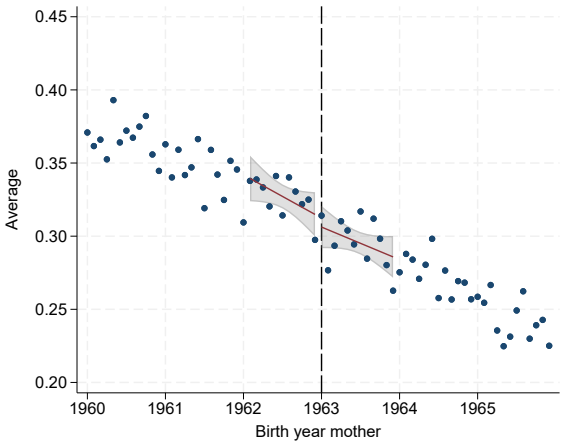


Employed

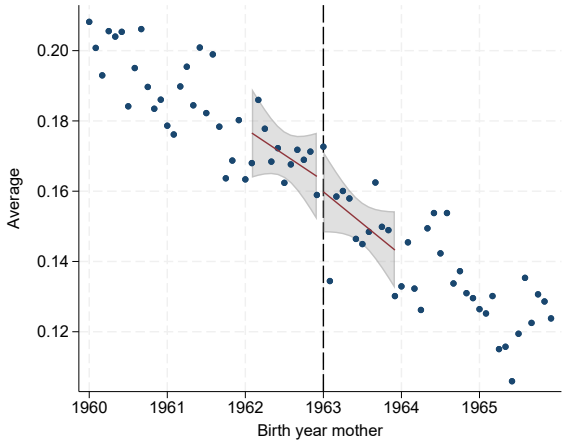
Income percentile



Hours per week



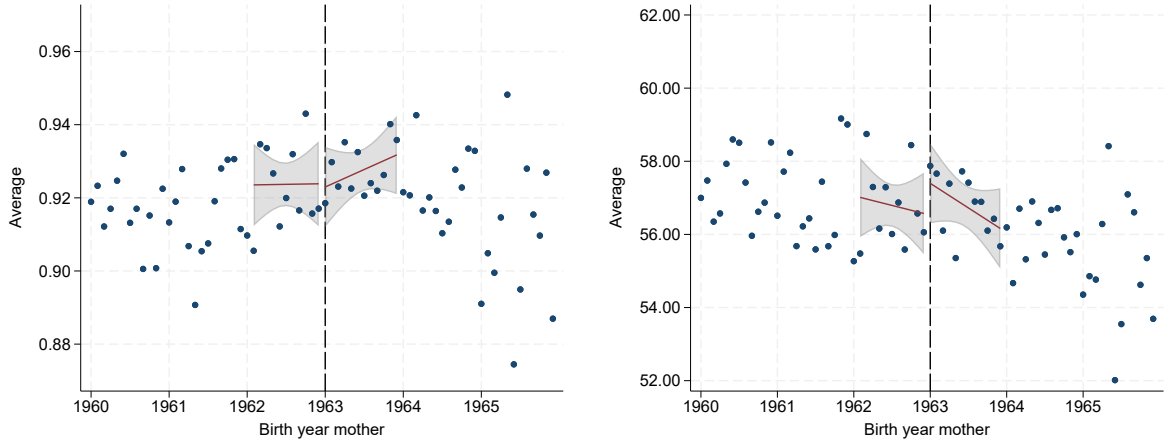
Has a partner



Has a child

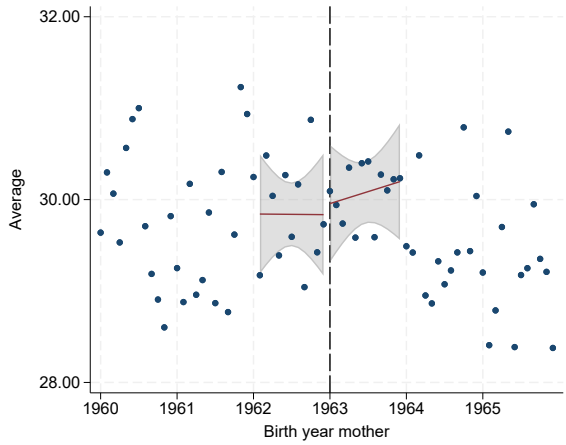
Figure A.8: Effect of the reform on sons in 2020

Notes: See notes to Table 10. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

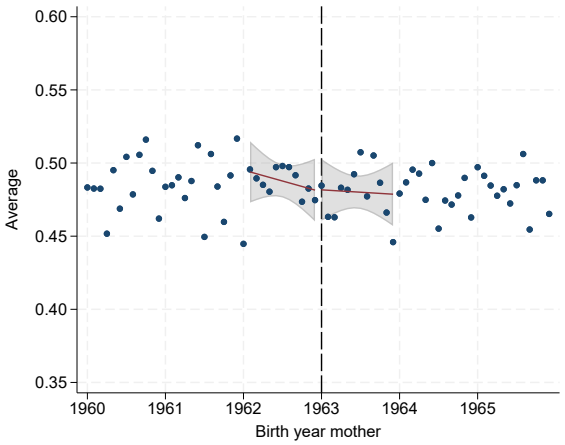


Employed

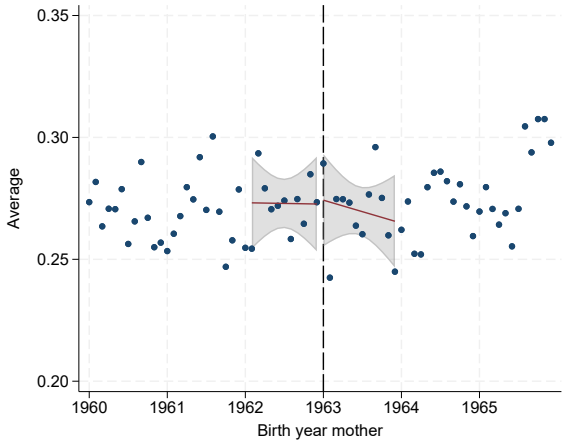
Income percentile



Hours per week



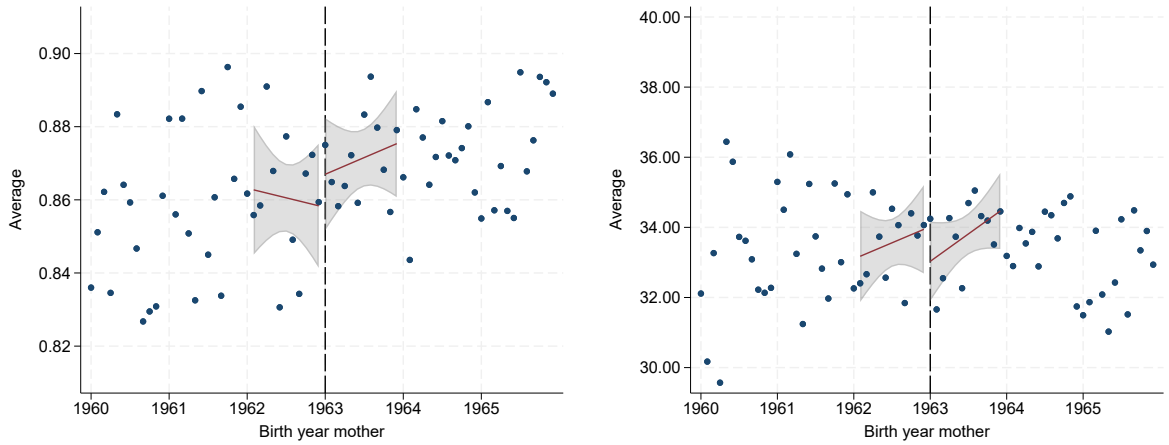
Has a partner



Has a child

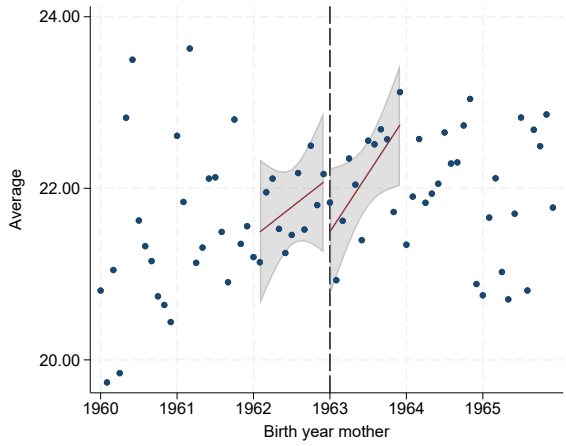
Figure A.9: Effect of the reform on sons aged 27-33 in 2020

Notes: See notes to Table 10. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

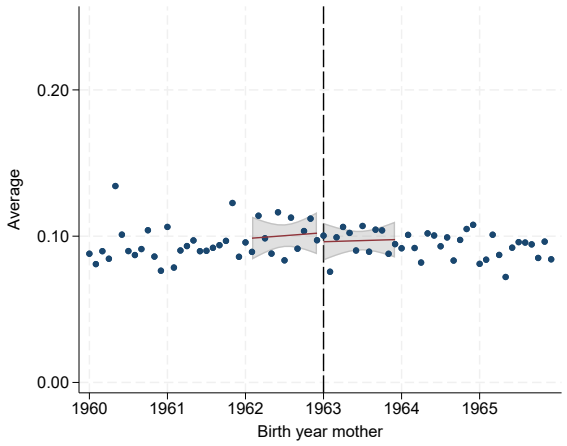


Employed

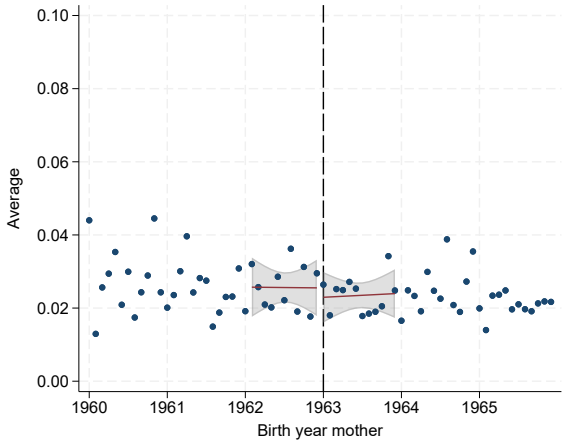
Income percentile



Hours per week



Has a partner



Has a child

Figure A.10: Effect of the reform on sons aged 20-26 in 2020

Notes: See notes to Table 10. Each dot is the average in a one-month birth month bin. The lines are fitted regression lines on the unbinned data in a 10-month window around January 1963, excluding control variables. Shaded areas are 95% confidence intervals of these predictions.

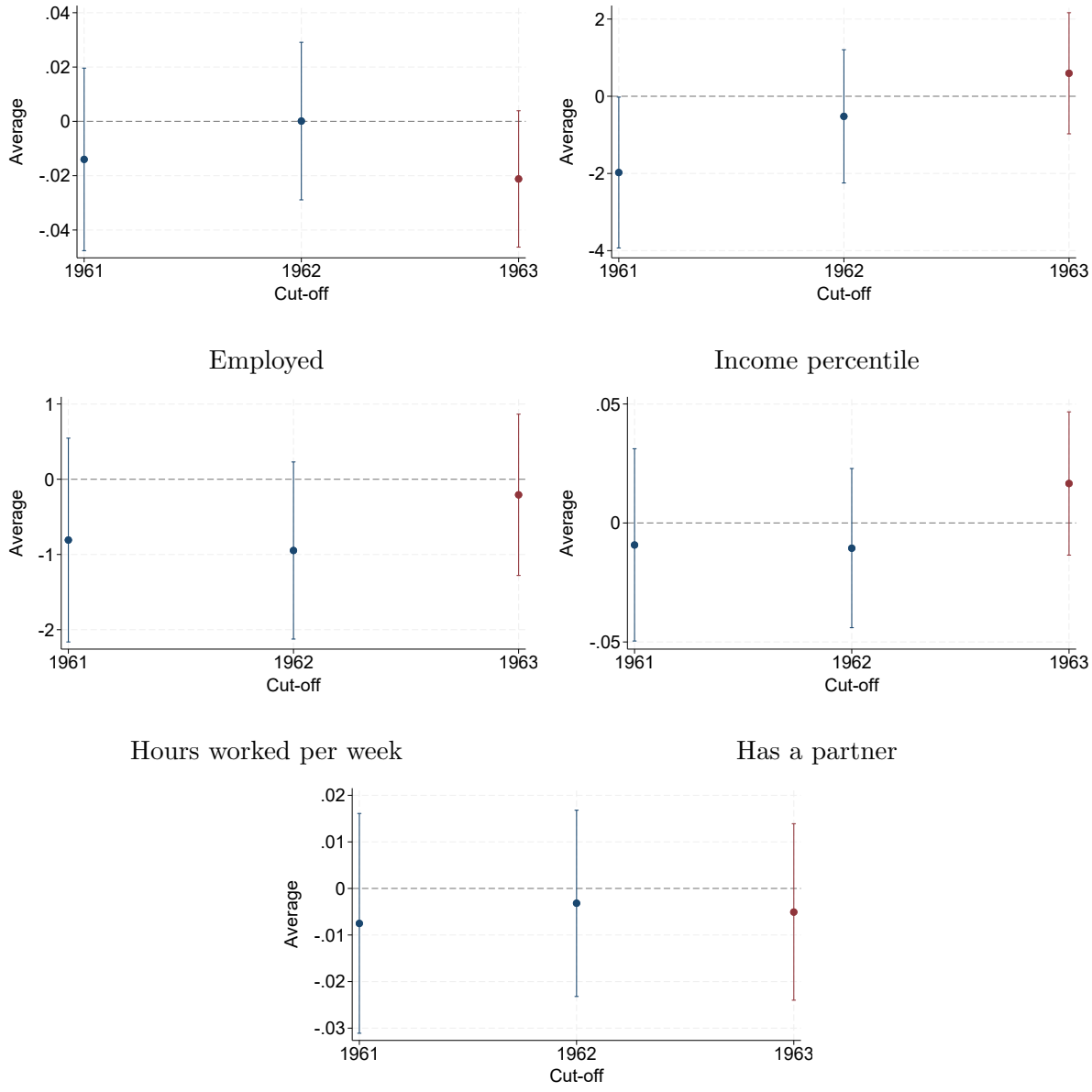


Figure A.11: Effect of placebo cut-offs on daughters aged 20-26 in 2020

Notes: See notes to and estimates reported in Table 16. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays the birth year which is used as a cut-off. In blue are placebo cut-offs, in red is the true cut-off.

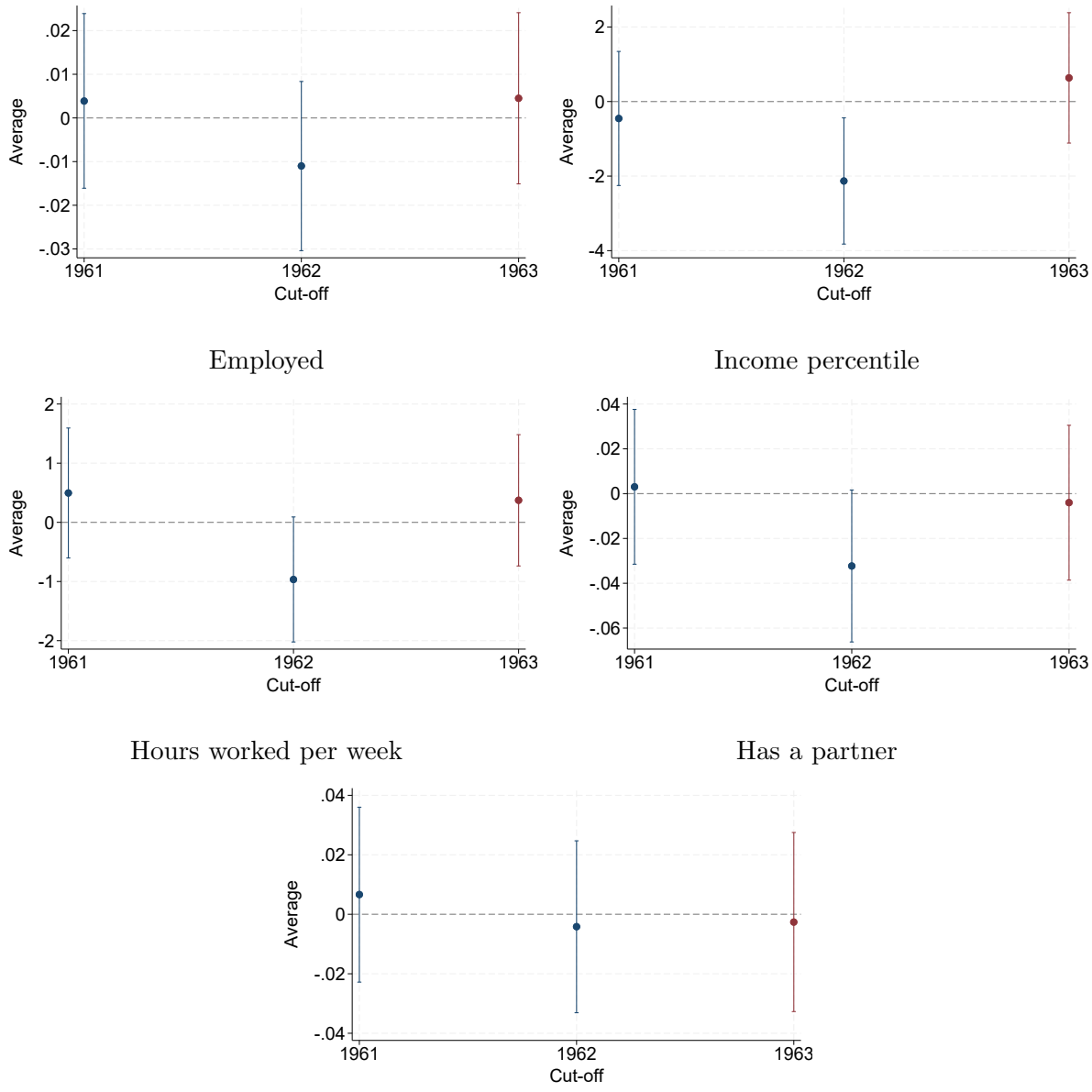


Figure A.12: Effect of placebo cut-offs on sons aged 27-33 in 2020

Notes: See notes to and estimates reported in Table 16. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays the birth year which is used as a cut-off. In blue are placebo cut-offs, in red is the true cut-off.

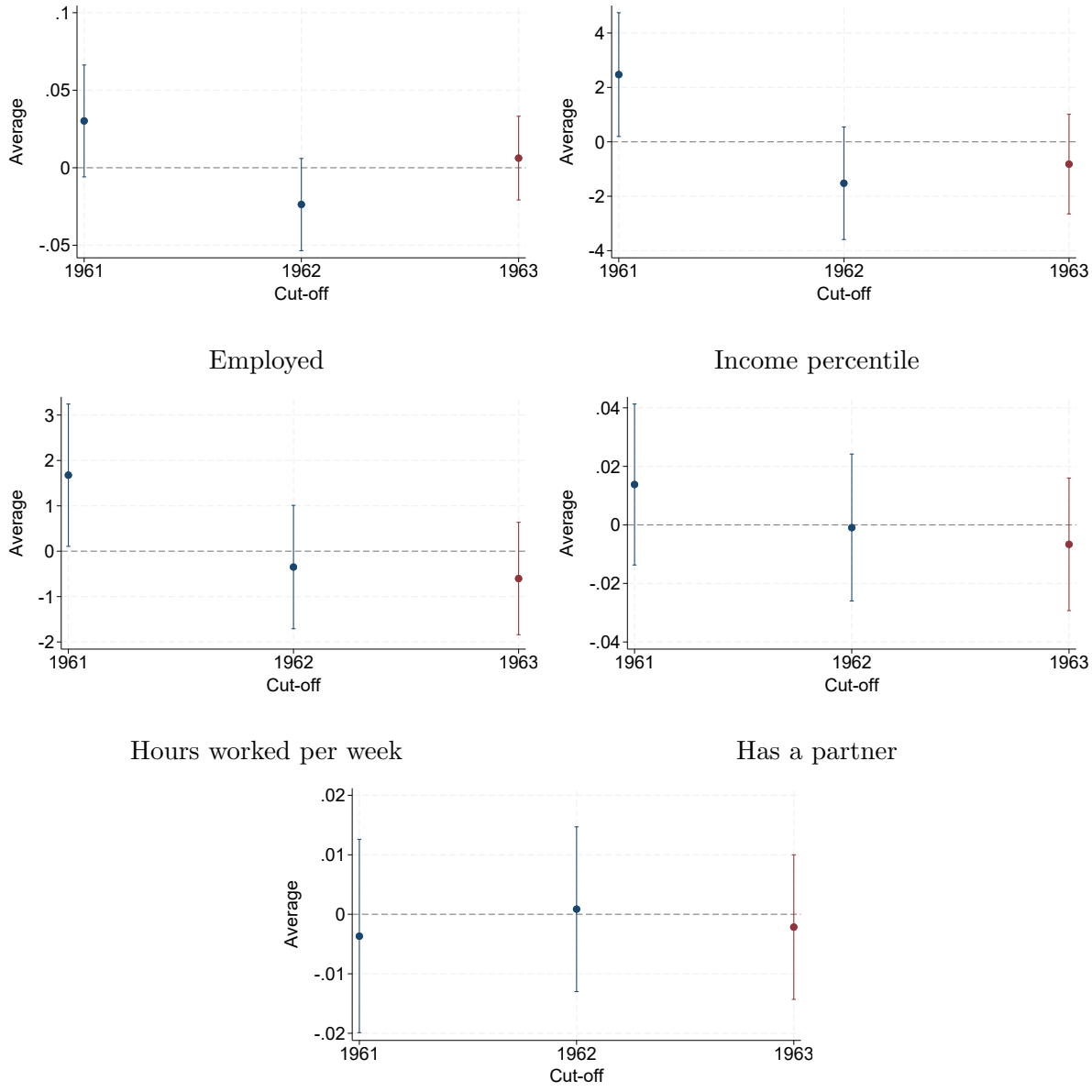


Figure A.13: Effect of placebo cut-offs on sons aged 20-26 in 2020

Notes: See notes to and estimates reported in Table 16. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays the birth year which is used as a cut-off. In blue are placebo cut-offs, in red is the true cut-off.

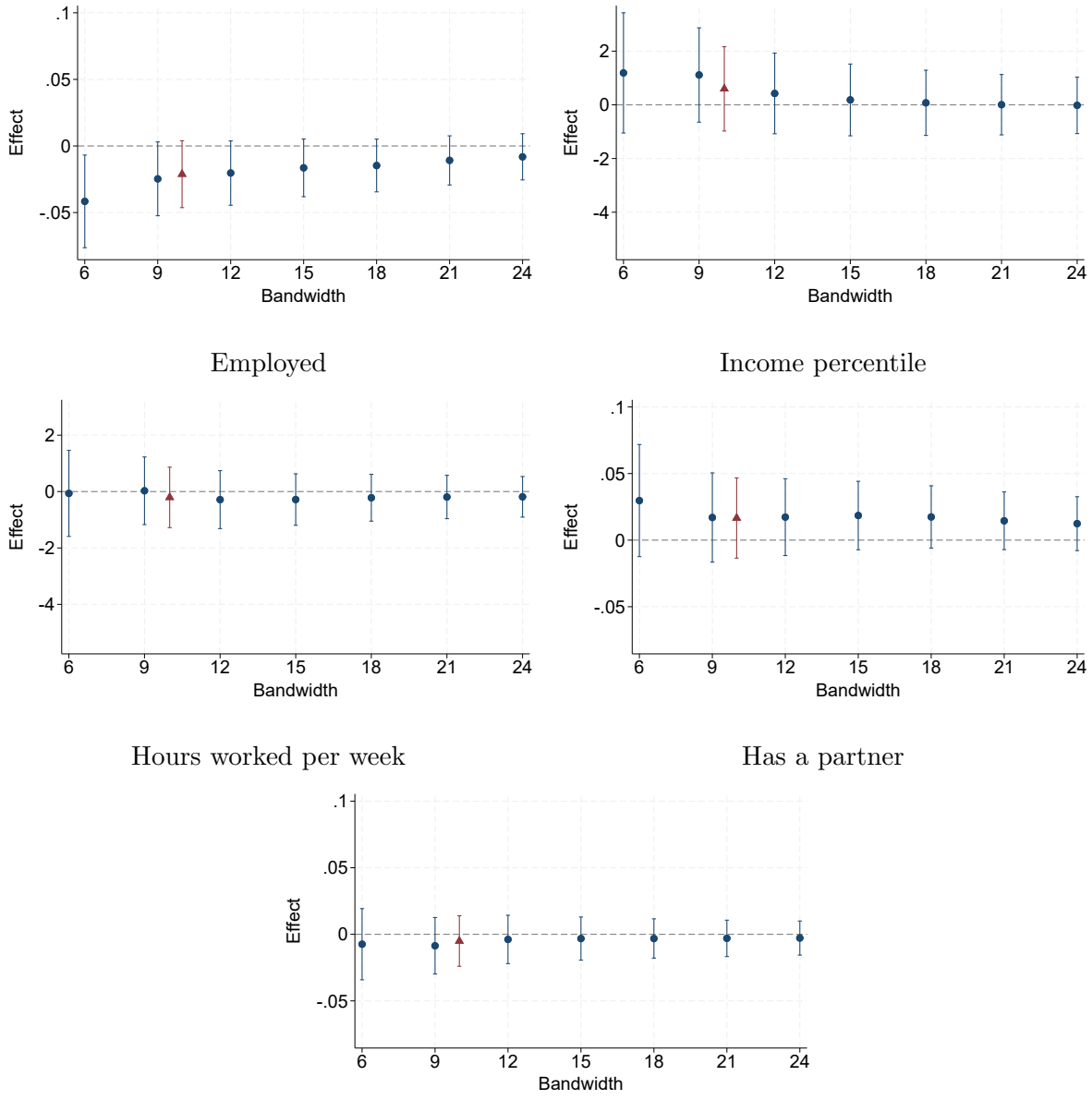


Figure A.14: Effect of the reform on daughters aged 20-26 in 2020 with varying bandwidth

Notes See notes to Table 9. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays bandwidth which is used. In blue are alternative bandwidths, in red is the main bandwidth.

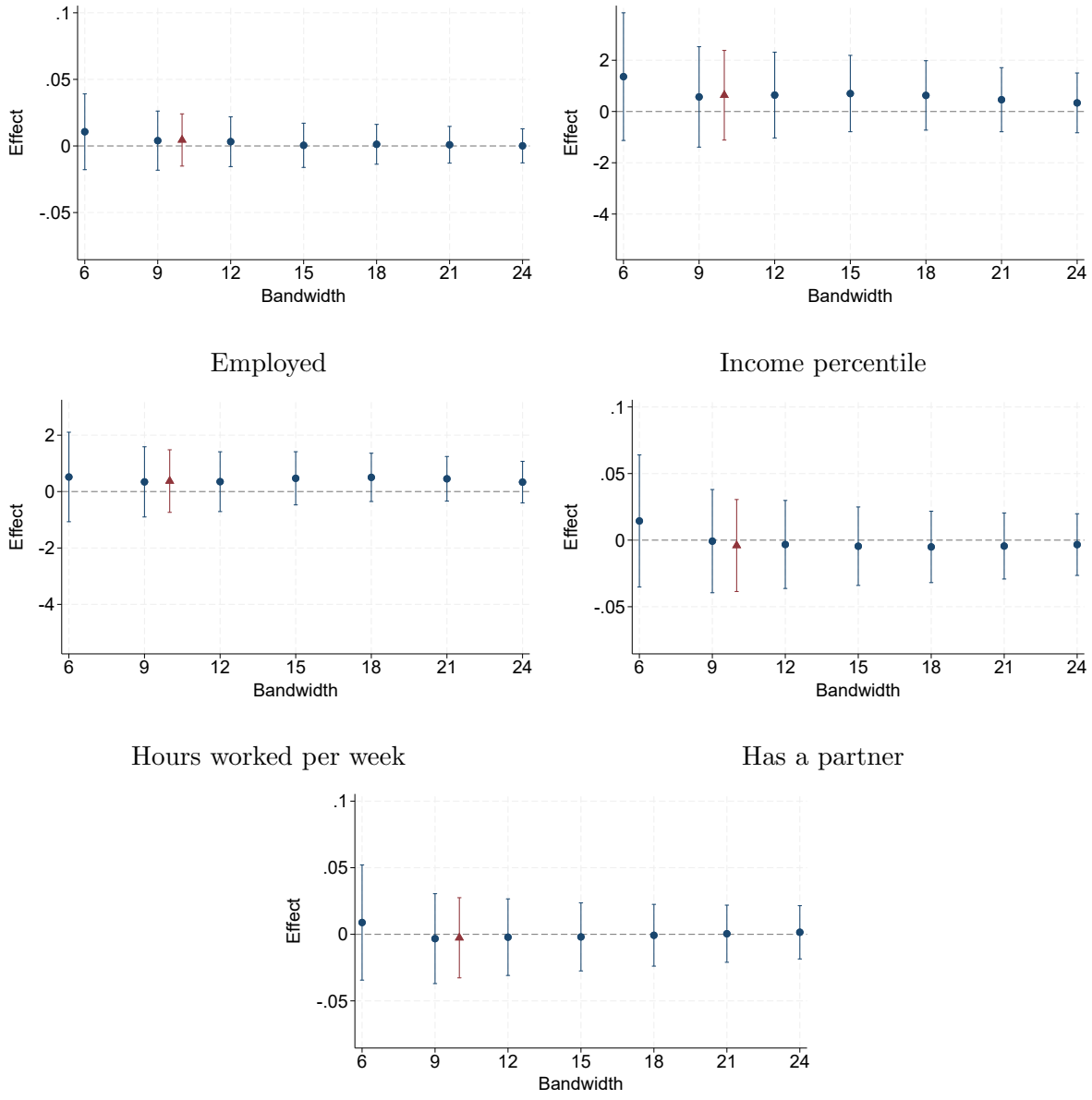


Figure A.15: Effect of the reform on sons aged 27-33 with varying bandwidth

Notes See notes to Table 10. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays bandwidth which is used. In blue are alternative bandwidths, in red is the main bandwidth.

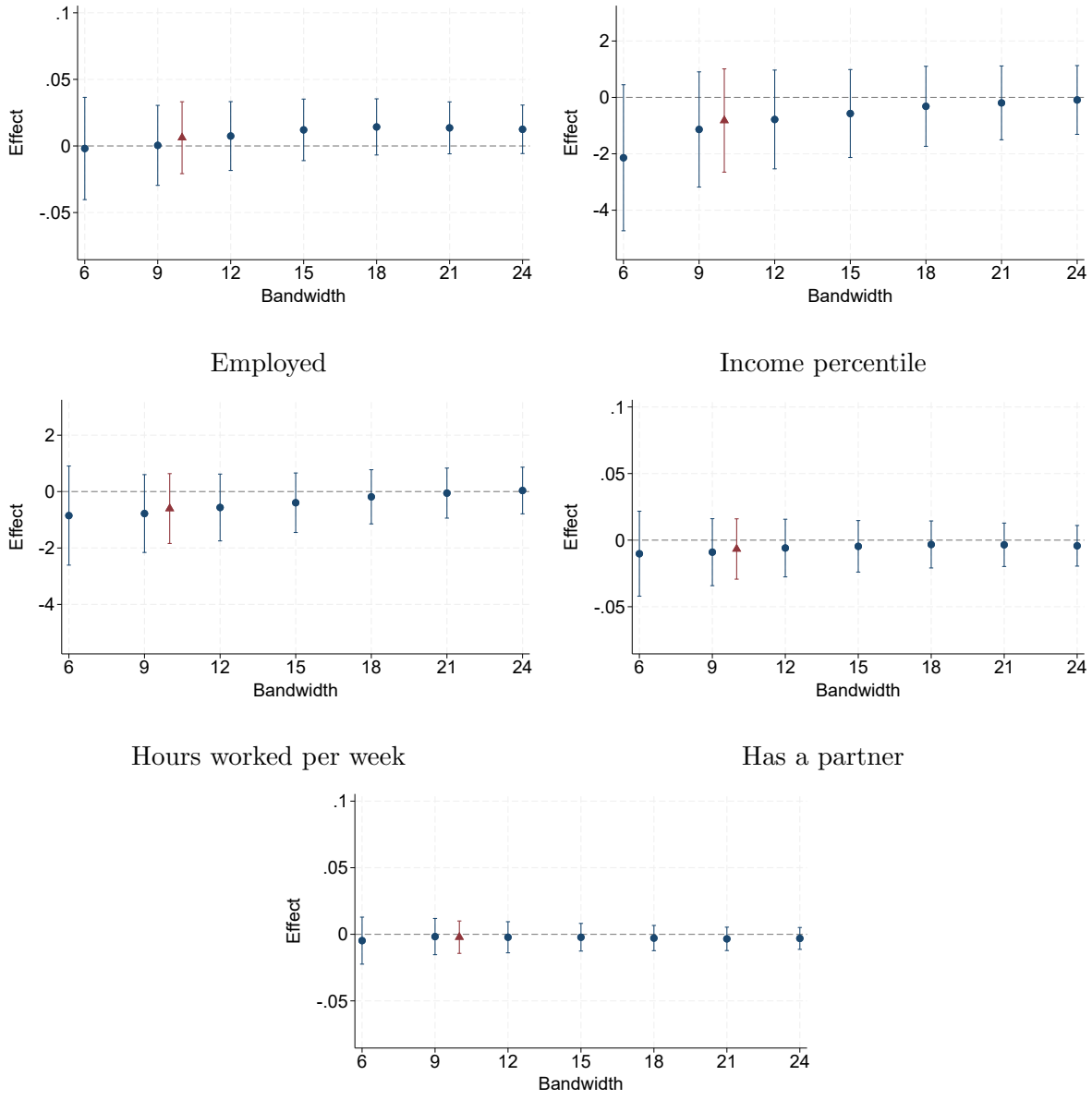


Figure A.16: Effect of the reform on sons aged 20-26 with varying bandwidth

Notes See notes to Table 10. Regression discontinuity estimates and 95% confidence intervals are displayed. The y-axis displays bandwidth which is used. In blue are alternative bandwidths, in red is the main bandwidth.

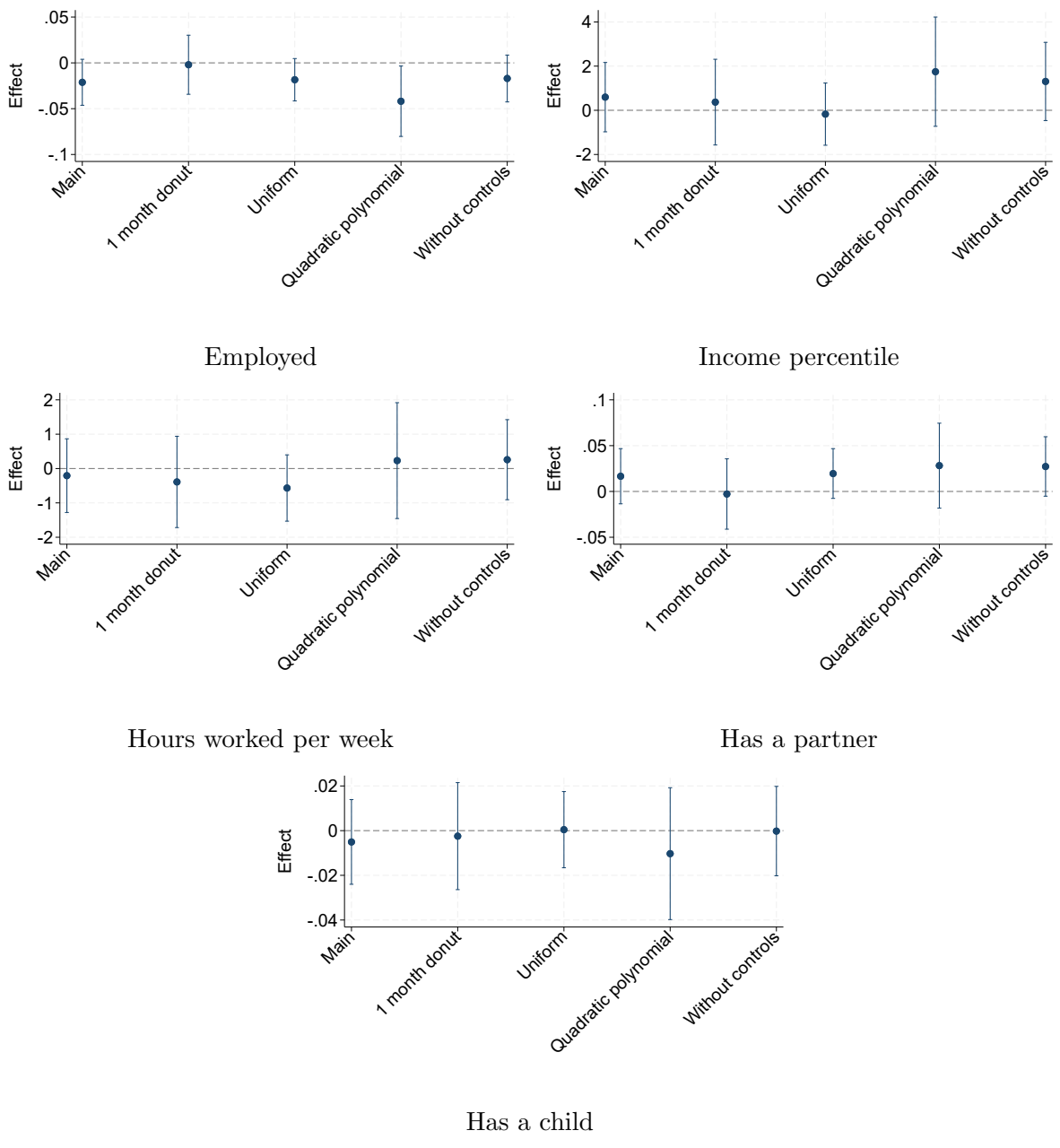


Figure A.17: Sensitivity to specification choices: Effect of reform on daughters aged 20-26 in 2020

Notes: See notes to Table 9. Regression discontinuity estimates and 95% confidence intervals are displayed. Main is the main specification. '1 month donut' uses a one month donut. 'Uniform' uses a uniform kernel. 'Quadratic polynomial' includes a quadratic polynomial. 'Without controls' omits control variables.

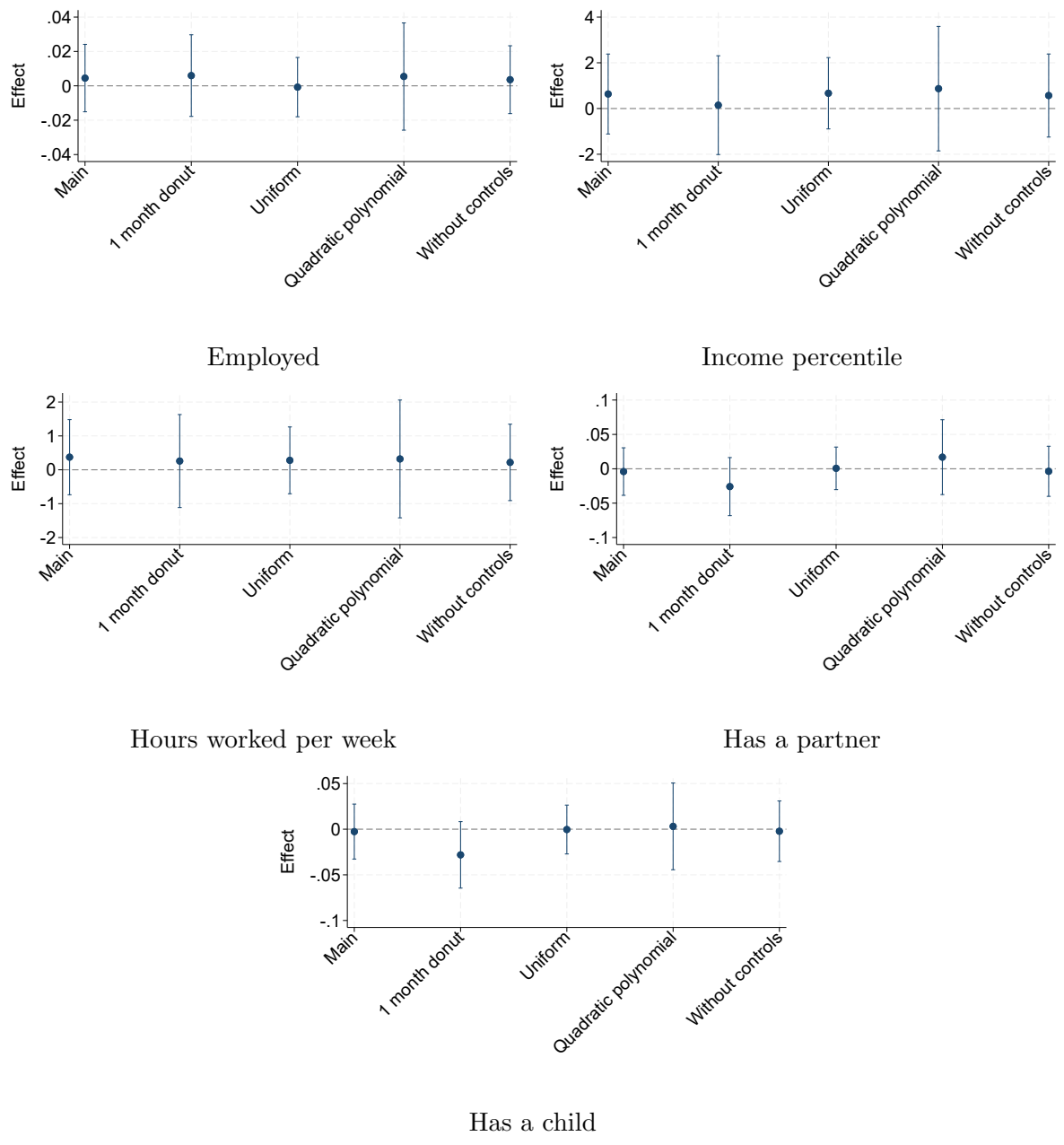


Figure A.18: Sensitivity to specification choices: Effect of reform on sons aged 27-33 in 2020

Notes: See notes to Table 10. Regression discontinuity estimates and 95% confidence intervals are displayed. Main is the main specification. '1 month donut' uses a one month donut. 'Uniform' uses a uniform kernel. 'Quadratic polynomial' includes a quadratic polynomial. 'Without controls' omits control variables.

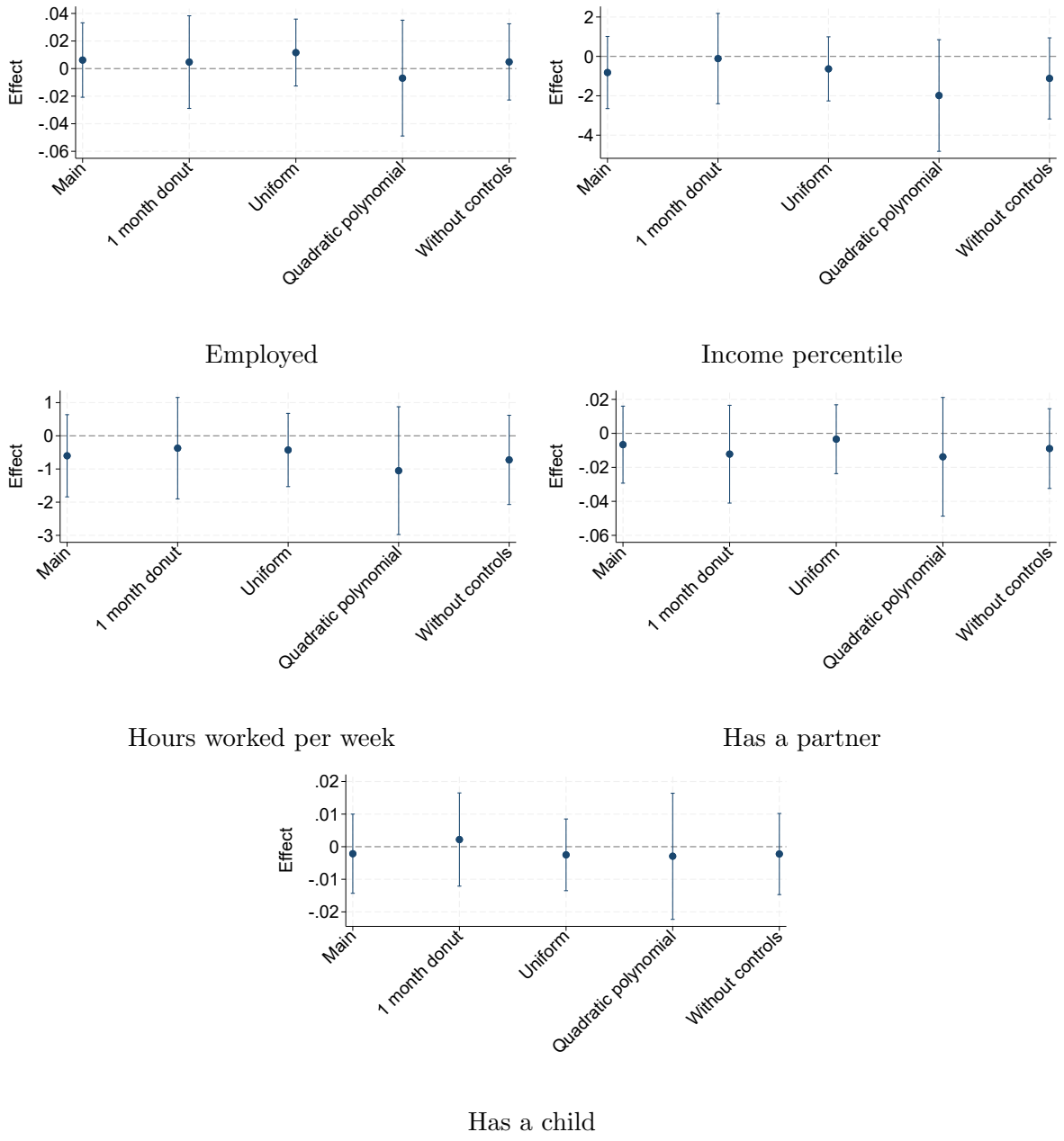


Figure A.19: Sensitivity to specification choices: Effect of reform on sons aged 20-26

Notes: See notes to Table 10. Regression discontinuity estimates and 95% confidence intervals are displayed. Main is the main specification. '1 month donut' uses a one month donut. 'Uniform' uses a uniform kernel. 'Quadratic polynomial' includes a quadratic polynomial. 'Without controls' omits control variables.

Table A.1: Density test of number of women born around January 1^{st}

	True cut-off	Placebo cut-off			
	1963	1961	1962	1964	1965
A. All females					
T-statistic	4.25	11.47	3.56	6.39	4.86
p-value	0.000	0.000	0.000	0.000	0.000
B. Target sample					
T-statistic	2.07	4.22	-0.44	2.23	2.24
p-value	0.039	0.000	0.658	0.026	0.025
C. Mother sample					
T-statistic	1.78	3.75	-0.88	1.59	1.81
p-value	0.088	0.000	0.379	0.111	0.070
D. Non-target sample					
T-statistic	3.73	10.68	4.11	6.01	4.33

Notes: Table reports the t-statistic and p-value of a test for a discontinuity in the density of births around January 1^{st}. All females includes all females born in the Netherlands between 1960-1965. Target sample includes all females born in the Netherlands between 1960-1965 and eligible for the tax subsidy in 2009, non-target sample those not eligible for the tax subsidy in 2009. Mother sample is the sample as described in notes to Table 1.