Subsidized small jobs and maternal labor market outcomes in the long run

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Abstract

This paper investigates whether incentives generated by public policies contribute to motherhood penalties. Specifically, we study the consequences of subsidized small jobs, the German Minijobs, which are frequently taken up by first-time mothers upon labor market return. Using a combination of propensity score matching and an event study applied to administrative data, we compare the long-run child penalties of mothers who started out in a Minijob employment versus unsubsidized employment or non-employment after birth. We find persistent differences between the Minijobbers and otherwise employed mothers up to 10 years after the first birth, which suggests adverse unintended consequences of the small jobs subsidy program for maternal earnings and pensions.

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This paper investigates whether incentives generated by public policies contribute to motherhood penalties. Specifically, we study the consequences of subsidized small jobs, the German Minijobs, which are frequently taken up by first-time mothers upon labor market return. Using a combination of propensity score matching and an event study applied to administrative data, we compare the long-run child penalties of mothers who started out in a Minijob employment versus unsubsidized employment or non-employment after birth. We find persistent differences between the Minijobbers and otherwise employed mothers up to 10 years after the first birth, which suggests adverse unintended consequences of the small jobs subsidy program for maternal earnings and pensions.

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Keywords: Motherhood penalty, small job subsidies, Minijob, maternal employment, labor market policy, propensity score matching

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1. Introduction

Most developed countries experienced substantial gender convergence in the labor market throughout the past century (e.g., Goldin 2014, Olivetti and Petrongolo 2016, England et al. 2020). However, labor market outcomes of women still differ considerably from those of observationally equivalent men: women are less likely to participate in the labor force, work fewer hours, and earn lower wages. These differences typically emerge with first parenthood, persist during prime working ages, and carry over to retirement as gender pension gaps. Recent contributions (e.g., Angelov et al. 2016, Bütikofer et al. 2018, Kleven et al. 2019a) point to the motherhood penalty as an increasingly important determinant of gender wage differences. While gender wage gaps and child penalties are pervasive phenomena, there are large differences in their magnitudes across countries (Olivetti and Petrongolo 2016, Kuziemko et al. 2018, Kleven et al. 2023a). For example, using data from Austria, Denmark, Germany, Sweden, the United Kingdom, and the United States, Kleven et al. (2019a) estimate long-run child penalties in maternal earnings of between 20 and 60 percent, where Scandinavian countries feature the lowest, and German-speaking countries the highest penalties among the developed countries under study.

This paper adds to the literature by investigating the role of a large-scale payroll tax subsidy program for the emergence of the significant motherhood penalty in Germany. Specifically, we study the Minijob program that subsidizes small job arrangements (*geringfügige Beschäftigung*). The program refers to employment relationships that yield less than a given amount of monthly earnings (400 Euro in the period under study, currently 520 Euro). Introduced decades ago, the original purpose of Minijobs was to reduce bureaucratic burdens and to facilitate flexible minor employment relationships. Very similar programs exist in other countries. For employees, Minijobs are exempt from social insurance contributions and income tax obligations. Thus, from an individual perspective, a Minijob constitutes a payroll tax subsidy, which is especially appealing to mothers seeking a limited number of working

hours per week after birth. Compared to regular part-time employment, a Minijob yields higher net earnings. This monetary gain can be amplified by the high marginal taxes for secondary earners in married couples. However, a vast majority of Minijobs are in low-skill domains, which in the long run, may limit individual earnings potentials, prevent investments in human capital, and inhibit the accumulation of pension claims.

Given these features, taking up Minijob employment after birth may have long-run negative consequences for maternal labor market outcomes, and, consequently, for social inequalities. We carefully test this hypothesis using detailed administrative data on employment biographies from German social security records. These data allow us to follow the labor market outcomes of first-time mothers who gave birth between 1999 and 2006 for up to 10 years thereafter.[1] To address potential differences between mothers who take up a Minijob after birth and those who do not, we apply propensity score matching. Specifically, we use a comprehensive set of individual characteristics, including detailed pre-birth employment biographies and firm-level information to match the Minijobbers to various control groups (alternative employment statuses after birth). The richness of our main data, a complementary analysis that controls for partner characteristics, and additional evidence from survey data on household characteristics and preferences give us confidence that our approach accounts for a wide range of relevant observable and unobservable differences between the groups.[2] Eventually, we compare the development of long-run labor market outcomes of the matched groups using an event-study approach.

[1] In this period, relatively modest child-rearing benefits were provided to recent parents conditional on a means test. In an auxiliary analysis looking at medium-run outcomes of mothers who gave birth between 2007 and 2012 (see section 4.4), we find that the results in the first years (up to 5 years) after birth are very similar to those presented here for mothers who gave birth before 2007.

[2] Nevertheless, establishing conditional independence is a challenging endeavor in any setting. Therefore, we are careful in our interpretations. Unfortunately, there is no quasi-experimental variation that we could exploit for the question under study.

We confirm staggering and persistent motherhood penalties with respect to labor market outcomes in Germany and show that they significantly differ by Minijob status. Specifically, mothers who took up a Minijob after birth have a nearly 10 percentage points lower probability of being regularly employed ten years after birth compared to mothers who directly returned to regular employment after birth. In addition, returning to the labor market as a "Minijobber" increases the motherhood penalty in earnings persistently by at least 10 percentage points compared to returning to regular employment. Compared to the group of non-employed mothers, the differences are generally smaller but in the long run, Minijobbers do not benefit substantially either. Thus, responding to the policy-induced incentives to take up small jobs seems to negatively affect maternal labor market outcomes in the long run.

We find that the accumulation of less valuable labor market experience (i.e., in unskilled tasks) and more frequent employer switches may be plausible, though not necessarily exclusive, mechanisms behind the long-lasting detrimental consequences of Minijobs. This unintended impact of the small jobs subsidy is of substantial policy relevance and has been disregarded so far. Using auxiliary data on a subset of our main sample, we show that our main results are highly robust to accounting for partner characteristics. This and alternative robustness tests yield even slightly larger differences between the Minijobbers and regularly employed mothers than our conservative baseline estimates.

This paper makes three contributions to several literatures. First, we add to the broad literature studying various explanations for persistent gender gaps and motherhood penalties. Extensive research focuses on gender-specific differences in productivity, preferences, labor market discrimination, and institutions (see, e.g., Olivetti and Petrongolo 2016 for a review). Several studies investigated the role of direct consequences of giving birth and breastfeeding, which seem to matter in the short run but cannot explain the persistence of the effects (Kleven et al. 2021, Andresen and Nix 2022b). Increasingly, research emphasizes the importance of social norms, attitudes, and culture (see, e.g., Bertrand 2011, Kleven et al. 2019a, Boelmann et

al. 2020, Schmitz and Spiess 2022) although capturing such impacts is challenging. Generally, the determinants of the long-run impact of children on observed gender inequalities in the labor market are still largely unclear.[3]

Second, our study is most closely related to the literature evaluating the effects of governmental policies on maternal labor market outcomes.[4] Several studies show that family policies such as paid leave and public child care may generate short-run effects of children on maternal earnings and employment (e.g., Lalive and Zweimüller 2009, Rossin-Slater et al. 2013, Schönberg and Ludsteck 2014, Bauernschuster and Schlotter 2015, Dahl et al. 2016, Kleven et al. 2023b, Andresen and Nix 2022a); however, their impact on long-run outcomes seems to be limited. Thus, the findings are somewhat consistent with family policies being typically designed to support mothers in the first years following childbirth. In contrast, we are among the first to offer insights into the role of policies that create long-run incentives to move down the job ladder after birth. Our findings are consistent with the argument that incentives generated by public policies might contribute to the explanation of the long-run motherhood penalty (Kleven et al. 2023b), especially, if they affect the "experience capital" accumulated in work over time (Costa Dias et al. 2020).

Third, we add to the literature evaluating employment effects of payroll tax subsidies by pointing to an important dimension that has, so far, been largely ignored: their unintended impacts on mothers. Earlier research on the Minijobs focused on evaluating their effectiveness as a stepping stone from unemployment into regular employment (see, e.g., Freier and Steiner 2008, Caliendo et al. 2016, or Böheim and Weber 2011 for the Austrian case), mostly using samples of unemployed men. However, the Minijob subsidy generates strong incentives for new mothers to enter a small-job arrangement, which potentially affects their career prospects

[3] Hotz et al. (2018) discuss the role of family-friendly workplaces and conclude that while these workplaces yield short-term benefits, they may hinder women's long-run careers.

[4] For prior discussions of the role of institutions see, e.g., Drange and Rege (2013), Dustmann and Schönberg (2012), or Brady et al. (2020).

and wage growth in the long run.[5] Thus, our findings are of broad interest because minor employment relationships with poor job characteristics similar to the German Minijobs exist in many advanced economies (see Adams and Prassl 2018, Table 1). Moreover, "atypical" contractual arrangements are particularly widespread among women.[6] Messenger and Wallot (2015) show that in several countries (Australia, Austria, Germany, Ireland, The Netherlands, UK), more than 10 percent of women in dependent employment actually work in such non-standard arrangements with low working hours and poor job characteristics. Thus, our results are relevant for various national labor market settings.

The paper proceeds as follows. We explain the relevant institutions in the next section. Section 3 describes our empirical approach and provides details on our data. The results follow in section 4, and section 5 concludes.

2. Institutional Background

2.1 Minijobs

The Minijob program is one of the largest labor market programs in Germany and subsidizes small jobs. As of 2019, more than 7.5 million individuals, i.e., one-sixth of the labor force took advantage of this subsidy (BA 2020). Legally, individuals performing Minijobs are part-time employees. They benefit from sick pay, maternity leave benefits, employment protection, and are entitled to paid vacation just as regular employees.

At the same time, Minijobbers differ from regular employees in that they are exempt from otherwise mandatory contributions to social insurance and income taxes. Instead,

[5] Indeed, Gudgeon and Trenkle (2022) and Tazhitdinova (2020) show a substantial bunching at the Minijob earnings threshold, particularly among females.

[6] Kalleberg et al. (2000), Kalleberg (2009), and Henly and Lambert (2014) investigate 'bad jobs' in the United States, Adams and Prassl (2018), McGovern et al. (2004), Koumenta and Williams (2019) study 'zero hours contracts' in the United Kingdom, Laß and Wooden (2020) discuss 'non-standard employment' in Australia, and OECD (2014) offers a broader international survey.

employers pay a fixed share of a worker's gross earnings to social insurance and tax authorities; for employers, the Minijobs are attractive due to the flexibility of small employment contracts. Currently, the Minijob subsidy is available if monthly earnings do not exceed 520 Euro. Labor earnings above that amount are fully taxable and subject to social insurance contributions by workers and employers (for details see e.g. Eichhorst et al. 2012, Berthold and Coban 2013).

Similar regulations existed for many decades but over time, the rules were modified with varying objectives, e.g., to reduce the bureaucratic burden, to incentivize labor supply, to raise social insurance contributions, or to provide incentives for unsubsidized part-time employment (see, e.g., Collischon et al. 2020). In 1999, the earnings limit was fixed at 325 Euro per month and a limit of at most 15 working hours per week was set. A reform in 2003 abolished the working hours limit and raised the monthly earnings threshold from 325 to 400 Euro. It also introduced the so-called Midijobs for monthly earnings between 400 and 800 Euro[7]. The main aim was to reduce illicit employment and increase employment opportunities for the unemployed in that Minijobs were considered a stepping stone to the unsubsidized labor market.[8] The 2003 reform also allowed to take up a Minijob in addition to a regular employment contract. In 2013, the earnings limits for Minijobs and Midijobs were shifted to 450 and 850 Euro per month.

2.2 Income Tax System

Germany uses a progressive income tax system that applies a tax splitting rule for married couples based on their joint income: if y_M and y_F are the gross incomes of the two spouses and y_C is the total income of the married couple, then the progressive tax function T(.)

[7] In contrast to Minijobbers, Midijob employees pay regular income taxes. Their social insurance contributions are subsidized and increase on a sliding scale.

[8] Existing research yields mixed results on this issue (see, e.g., Freier and Steiner 2008, Caliendo et al. 2016, or for the case of Austria, Böheim and Weber 2011). Some studies point to unintended substitution effects (e.g., Jacobi and Schaffner 2008, Collischon et al. 2020).

yields that $2*T(0.5 * y_C) \leq T(y_M) + T(y_F)$. Typically, this generates a tax benefit from marriage, which is largest for couples where one spouse earns the total income: if a second earner starts to earn above the Minijob earnings threshold, these earnings are taxed at the marginal tax rate of the first earner. This can generate large disincentives for labor supply as, e.g., between 1999 and 2006 - our analysis period - marginal tax rates could reach up to 45 percent. **Figure A.1** in the appendix illustrates the relationship between gross and net earnings at the earnings threshold of Minijobs for three average tax rate scenarios. The figure shows that even with relatively low income tax rates, gross earnings from regular employment had to reach approximately 600 Euro in order to match similar net earnings via Minijob employment of 400 Euro. This indicates the disincentive of extending earnings beyond the Minijob threshold.[9] Gudgeon and Trenkle (2022) and Tazhitdinova (2020) illustrate that the notch in the earnings distribution causes substantial bunching at the Minijob earnings limit, particularly among females.

2.3 Family Benefits

German family policy offers different programs aiming at the well-being of parents and newborns: fully paid maternity leave is available from six weeks before to eight weeks after childbirth and mothers are prohibited to work during this period. Parents can take parental leave, which provides job protection for up to three years (cf. Dustmann and Schönberg 2012).

Also, parents are entitled to parental leave benefits. In the period prior to 2007, which we study here, "child-rearing benefits" (*Erziehungsgeld*) were provided. They were means-tested and paid a maximum of 300 Euro monthly for up to 24 months.[10] The means test related to family income: parents were eligible for full child-rearing benefits if their annual net income was below a specific threshold. For example, first-time parents received the maximum benefit

[9] In **Appendix B,** we test whether the motherhood penalty of Minijobbers varies depending on husband's earnings, which generate different tax burdens at the Minijob ceiling.

[10] Alternatively, families could choose payments of 450 Euros for up to 12 months.

of 300 Euro during months 1-6 after birth, if their annual income did not exceed 30,000 Euro. They received the full benefit in months 7-24 after birth if their annual income did not exceed 16,500 Euro.[11] In practice, often solely the father's income was relevant because earnings of the leave-taking parent, i.e., usually of the mother, were omitted in the means test as long as she did not work during leave-taking.[12] Generally, employment up to 30 hours per week was allowed during leave-taking but this labor income added to the means test save for Minijob earnings. For mothers who sought employment after birth, typically with a limited number of weekly hours, this Minijob exception provided an additional incentive to take up a Minijob instead of regular part-time employment.

2.4 Minijob take-up after birth

Figure 1 panel a shows that German mothers experience a sharp, immediate, and persistent increase in Minijob employment after the birth of their first child. While about 15 percent of first-time mothers take up a Minijob after birth, fathers remain essentially unaffected. There are several reasons for this pattern. First, given the still prevailing traditional gender roles within families, after birth, German mothers typically return to the labor market with only a limited number of weekly hours of work. Thus, compared to regular part-time employment, Minijobs, which are exempt from both social insurance contributions and income taxes, are very attractive. Second, once having started a Minijob, the income tax system creates strong disincentives to extend labor supply, especially for married women. This might permanently deter the lower-earning spouse, typically the female, from moving on to regular employment,

[11] The threshold differed for single parents, it varied depending on the parity of the child and also for benefits for months 1-6 vs. 7-24 after birth. Generally, the thresholds referred to annual joint family income measured in the calendar year before the childbirth for benefits in months 1–12 and measured in the year of the childbirth for benefits in months 13–24. If net income exceeded the threshold, payouts were reduced.

[12] While the law equally treated both parents, in practice, the share of fathers utilizing paid parental leave was below 2.5 percent (Cygan-Rehm et al. 2018).

and lead to dead-end jobs in low-wage occupations, instead (e.g., Eichhorst et al. 2012). Indeed, the jump in maternal Minijob employment after birth (see panel a of **Figure 1**) coincides with persistent losses in women's post-birth earnings (see panel b of **Figure 1**) suggesting that the motherhood penalty might be (at least to some extent) connected to Minijobs. However, this is descriptive evidence and so far, there is no research on the direct impact of Minijob employment on the maternal labor market status after birth.

3. Empirical Approach and Data

3.1 Empirical Approach

Propensity score matching

The key econometric challenge in analyzing the long-run consequences of a Minijob employment shortly after birth is that mothers who take up a Minijob might differ from mothers who decide not to do so. To deal with the potentially non-random self-selection into the program, we apply a matching strategy, more specifically, radius-caliper matching with a caliper of 0.001. We focus on first-time mothers who 6 months before birth were in regular full-time employment. We split this sample into subgroups depending on maternal employment status in month 13 after birth when we capture the first employment choice after birth.[13] In particular, our main analysis compares mothers in subsidized Minijob employment and three alternative control groups: all regularly (full-time or part-time) employed mothers, those in low-paying regular employment (lowest earnings quartile in month 13), and - in section 4.5 - those without employment.

[13] We chose month 13 after birth in order to ensure that the data are informative on the first employment choice after birth. In the first 12 months after birth, German mothers are typically on parental leave. However, it is still possible that pre-birth employment contracts run out or provide social security relevant payouts (e.g., bonuses or premiums at the end of calendar year) during parental leave; this would then erroneously appear like continued full-time employment in the data. In section 4.4, we offer robustness tests with respect to the choice of the cutoff of 13 months (versus, e.g., 12 or 14) after birth.

For the matching procedure, we use a rich set of maternal pre-birth characteristics such as age at birth (annual indicators), tenure, total employment experience, unemployment experience, Minijob experience (all measured in years[14]), education (5 indicators), establishment size (7 indicators), and federal state main effects and interacted with dummies for year of birth. Furthermore, we match on daily earnings (6, 12, 18 months prior to birth), employment status (indicator 6, 12, and 18 months prior to birth), and job complexity (6 months steps prior to birth in 4 categories). Additionally, we also match on 1-digit-occupation (Blossfeld) as well as 1-digit- industry classifications (NACE) in the last job prior to giving birth. To flexibly capture any time-specific effects, we also include a full set of indicators for the interaction terms between the year of birth dummies and quarter of childbirth indicators and their main effects. Thereby, our matching procedure accounts for any differences due to seasonality and institutional changes.

Using this set of covariates, we match mothers in a Minijob in month 13 after birth to mothers who in month 13 after birth, are (i) regularly employed, (ii) regularly employed in low-paying jobs, and (iii) non-employed using separate matching procedures. In a complementary analysis based on linked social security records of married partners, we show that our results are virtually identical if we additionally account for an extensive set of partner characteristics (see discussion in section 4.4 below). Finally, using auxiliary survey data, we also provide evidence on household characteristics and preferences, which further underpins the conditional independence assumption.

Event study analysis

To determine the consequences of Minijob employment after a first birth on subsequent maternal labor market outcomes, we adopt a quasi-experimental approach using an event study

[14] We consider mothers who are fulltime employed in the year prior to birth and with first births between 1999 and 2006. As the Minijob information is available in our data only since 1999, we can match on prior Minijob experience only for the later cohorts of mothers. For those giving birth in 1999 and 2000 prior Minijob employment cannot be fully observed.

design similar to Kleven et al. (2019b) for the matched samples. Specifically, using a balanced panel, we study the development of maternal employment status and daily earnings 5 years before and 10 years after birth. The approach provides consistent estimates under the identifying assumption that the event, i.e., the first birth, is not determined by the outcome variables. Kleven et al. (2019b) provide various checks that support the plausibility of this assumption. In the fully dynamic specification, the outcomes Y of a mother i at event time t (measured relative to birth) and calendar time s are regressed on event-time and calendar-time fixed effects, which we measure in 6-month steps. The point estimates for the post-event periods flexibly capture the average treatment effect of the birth over time. It is assumed to be homogenous across observations (Borusyak and Jaravel 2018, Freyaldenhoven et al. 2019, Schmidheiny and Siegloch 2019).

However, we are not only interested in the general pattern of post-birth developments in employment and earnings, which have been studied before (see e.g., Kleven et al. 2019a). Instead, we aim at identifying the consequences of Minijob employment after birth by comparing the long-run trajectories in maternal labor market outcomes for mothers who took-up a Minijob employment shortly after birth and those who did not. Specifically, for each subgroup (g), we estimate the following equation

$$Y_{ist}^{g} = \sum_{j \neq -1} \alpha_j^g \cdot \mathbf{I}[j = t] + \sum_{y} \gamma_y^g \cdot \mathbf{I}[y = s] + \sum_{k} \beta_k^g \cdot \mathbf{I}[k = age_{is}] + \varepsilon_{ist}^{g}$$

where event time runs in 26 steps from month 60 before to month 120 after a birth covering a span of 5 years before to 10 years post birth. Since we omit the event time dummy at t = -1, the event time coefficients (α_t) measure the relative impact of the first birth on the outcome in period t relative to the outcome value in month 6 prior to birth. The group indicator g represents mothers who took up a Minijob, and the control groups of either mothers who are in regular employment or in low-pay regular employment in month 13 after birth. Data limitations do not allow us to reliably separate part-time vs. full-time employment after birth. Besides a full set of

event-time (t) and calendar-time (s) indicators in 6-month steps, our regressions also include a full set of indicators for maternal age at time t. Thus, we control non-parametrically for underlying life-cycle patterns and developments over time such as wage inflation, business cycles, or institutional changes. We use estimation weights to account for the repeated use of control group observations based on the matching algorithm. Standard errors are clustered at the individual level.

To depict the estimation results, we follow the literature (see Kleven et al. 2019b) and calculate the relative motherhood penalty at every event time (t) separately for each group (g). This penalty is defined as the ratio of the estimated coefficient α_t over the predicted outcome at time t when omitting the contribution of the event indicator.[15] It provides the relative difference in the outcome at time t versus the value observed in month 6 prior to birth as a percentage of the counterfactual outcome absent children. Intuitively, we compare long-run motherhood penalties for observationally similar mothers with and without Minijobs.

3.2 Data

We use administrative data from the Sample of Integrated Labour Market Biographies (SIAB) (Antoni et al., 2019).[16] The data offer a 2 percent random sample of all individuals registered at least once due to employment, unemployment, or take-up of other public transfers (i.e., welfare benefits) by the social security system between 1975 and 2017.

[15] The value in the denominator is calculated by first predicting the outcome for each panel observation, then deducting the coefficient α_t and, finally, averaging across the observations in the relevant group and event time cell.

[16] Specifically, we use the weakly anonymous version of the SIAB 1975-2017 and accessed the data via a Scientific Use File at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) in Nuremberg and via remote data access at the FDZ. DOI: 10.5164/IAB.FDZD.1902.en.v1

As our data report Minijob employment only since 1999, we consider West German mothers who gave birth between 1999 and 2006.[17] Unfortunately, German law prohibits combining the data with birth certificates. However, it is possible to identify first-time mothers based on the method of Müller and Strauch (2017) that uses employer-provided information on the starting day of the obligatory maternity leave (see section 2.3). Given that there are no household identifiers in SIAB and maternity leave is provided only to mothers, it is not possible to reliably identify fathers in the data and we focus on mothers.[18] Specifically, we focus on mothers with a strong labor market attachment before giving birth, i.e., those who were employed in regular full-time employment 6 months prior to birth. In case of multiple job holding at a particular time, we focus on the main employment which is determined by the highest earnings. Finally, we exclude first-time mothers below age 20 given that their labor market biographies typically do not provide sufficient information to allow for reliable matching.[19]

We are interested in the consequences of taking up a Minijob after the first birth for subsequent maternal employment and earnings. To capture the initial post-birth employment choices, we use maternal employment at month 13 after birth to define the treatment. Traditionally, German mothers take relatively long employment interruptions after birth and tend to return to the labor market when the child turns one, two, or three years old (Bergemann and Riphahn 2023).

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17 These sample restrictions allow us to observe labor market outcomes of all mothers over a 10-year period after birth. We exclude East German states, which until 1990 formed the German Democratic Republic, because during our period of study, East and West Germany differed in various dimensions including female labor market participation, fertility patterns, and social norms. Many of those differences still persist (see, e.g., Bauernschuster et al. 2012, Boelmann et al. 2020).

18 However, prior literature convincingly shows that paternal labor market outcomes remain unaffected by a childbirth, which holds in various countries and settings (Kleven at al. 2019a, 2019b, 2020, 2021).

19 Our results are robust to including teenage mothers, which is not surprising because teenage motherhood is generally rare in West Germany (Cygan-Rehm and Riphahn 2014).

Our sample contains 21,630 first-time mothers who gave birth between 1999 and 2006. For them, we generate a balanced panel that covers 60 months (5 years) before and 120 months (10 years) after birth. As we use six-month observation intervals, this 16-year panel yields 32 observations per woman and a total sample size of 670,530 observations.[20] **Figure 2** depicts the overall development of our samples' labor force status over time. Because maternity leave is mandatory for at least 8 weeks after giving birth, all mothers interrupt active work during this period by definition. In the first two years after birth, up to 18 percent of previously full-time employed mothers take up Minijob employment and the share remains at 14 percent in the long run. In contrast, in the third year after birth, about 25 percent of mothers have returned to regular employment and this share increases to 59 percent 10 years after the first birth.

The Minijobber group consists of mothers who were in a Minijob as their main employment in month 13 after birth (N=3,234 or 15 percent of all mothers). Our main control group contains mothers who were in regular (part-time or full-time) employment (N=4,618 or 21 percent). The remaining 64 percent of mothers (N=13,778) did not work at all 13 months after birth; we consider them as a control group in section 4.5. Interestingly, among all mothers who are employed at month 13 after their first birth, 41 percent hold a Minijob which underlines the relevance of this labor market institution. As an additional control group, we consider a subgroup of regular employees who are in the lowest earnings quartile 13 months after birth (N=1,151 or 5.3 percent of all mothers). We consider this group to be the most comparable to the Minijob employees because they typically work a limited number of hours per week or at moderate earnings. Mothers who temporarily worked on a Minijob before month 13 but stopped

[20] Given that regular and Minijob employment is registered in our data, we assume non-employment if there is no register entry for a given woman at a particular time. This introduces a measurement error if individuals became civil servants or self-employed, which is not recorded by the social security system. We checked the probability of such events using survey data from the German Socioeconomic Panel Study (SOEP) and found that among mothers who were full-time employed prior to first birth, no more than 3.5 percent shifted into either civil service or self-employment by year 10 after birth. Thus, the measurement error is relatively small and should not affect our results in important ways.

before month 13 and mothers who initiated a Minijob after month 13 are both in the control group.

Given that we do not observe fathers, we do not impose any restrictions on the relationship status of the parents. However, during the period of our analysis about 80 percent of all births in West Germany occurred to married mothers (DESTATIS 2020) although the share may be slightly lower for first births. In a robustness test, we take advantage of recent attempts to match married couples in the German social security records (see Bächmann et al. 2021) and demonstrate that our results remain virtually identical when we additionally account for a father's characteristics.

We focus on two main maternal labor market outcomes: the propensity to be employed in regular (i.e., non-subsidized) employment and gross daily earnings.[21] Following Kleven et al. (2019b), we keep observations with zero earnings for those who are not in employment. In parallel analyses, we drop these observations. Unfortunately, there is no information on working hours, so we cannot study hourly wages and instead focus on daily earnings, i.e., the total amount paid over the duration of an employment contract divided by the number of calendar days covered by the contract.

Figure 3 describes the development of the outcomes for the full sample. The top left panel shows that the share of regularly employed mothers increases in the pre-birth period and (by definition) reaches 100 percent in month 6 prior to birth.[22] The share drops to about zero immediately after birth and then slowly returns to about 40 percent in year 10 after the birth, a decline by 60 percent relative to the pre-birth situation. In the top right panel, we add Minijob employment to look at the changes in overall participation rates. Now the return path to the

[21] Focusing on gross vs. net earnings follows the literature. Also, our data do not provide net earnings which would vary depending on married couples' income tax choices. Furthermore, it is gross earnings which are relevant for pension claims.

[22] Prior to month 6 before birth, some women were not (yet) in fulltime employment, which is why the share increases over time.

labor force after birth is somewhat steeper and reaches employment rates of about 60 percent (or minus 40 percent) 10 years after birth. The bottom left panel describes the development of nominal average daily earnings when non-employed mothers contribute a value of zero. We see that the long-run decline in earnings exceeds the long-run drop in employment. The long-run penalty of about 60 percent matches the survey-based evidence presented by Kleven et al. (2019b). The bottom right panel depicts the development of earnings when only positive earnings of employed mothers are considered. Here, the drop after birth is not as vast and the long-run penalty is slightly lower than on the left-hand side, reaching about 54 percent in the long run.

Table 1 describes the relevant subsamples and the matching quality. Overall, mothers who took up Minijobs after birth were on average younger at birth, had lower pre-birth earnings, longer tenure with their employer, and less pre-birth unemployment experience compared to the control groups. with the Minijobbers were also more likely to have vocational training, less likely to have higher education, and worked in smaller establishments pre-birth than the control groups. While we find significant differences in these characteristics in the raw data, the significant differences disappear after we apply the propensity score matching. It is not obvious that any one of the three control groups (regular employees, low-paid regular employees, and non-employed) is most similar to Minijobbers in terms of unbalanced characteristics.[23] In section 4.4, we draw on auxiliary survey data to show that we also do not find differences in other dimensions such as household composition or individual attitudes, which we cannot observe in social security records (cf. **Figure A.5**). In each of the comparison groups, our matching procedure finds appropriate matching partners for nearly all Minijob observations.

[23] Regular employees are most similar to Minijobbers in terms of past labor market experience, the firm-size distribution of Minijobbers is most similar to low-paid regular employees, while job complexity is most similar between Minijobbers and the non-employed.

Figures A.2-A.3 in the appendix depict the distribution of propensity scores and show that common support is not a concern.

4. Results

4.1 Baseline results

Our main analysis compares mothers who start out in Minijobs to mothers in regular employment. Panel A of **Table 2** shows our main results for the estimated coefficients regarding the probability of being in regular employment and on daily earnings 2, 5, and 10 years after birth relative to the situation 6 months prior to birth. Specifically, the first two rows show the estimated coefficients on the relevant event time dummies and the corresponding standard errors. The results imply that for mothers who returned to regular employment after birth, the probability of being in regular employment 10 years after birth decreases by 43.2 percent compared to 6 months before birth. The corresponding decrease for Minijobbers is by 51.3 percent, so that the difference between the two groups after 10 years is 8.2 percentage points, which is substantial and statistically significant. This absolute difference implies that after 10 years, Minijobbers experience nearly 20 percent larger decreases in the probability to be regularly employed than the comparison group. The magnitudes of the absolute and relative differences in earnings losses 10 years after birth are similar (see next rows of panel A).

To shed more light on the dynamics over the entire 10-year period, we use a graphical representation of the estimation results. In particular, we plot the change in outcomes over time compared to the situation 6 months prior to birth as a percentage of the counterfactual outcome without a birth. **Figure 4.1** graphically presents our baseline results for mothers who picked up a Minijob after birth in comparison to those matched observations who returned to regular employment after birth. Due to the matching on pre-birth characteristics, we observe identical developments for the Minijobbers and the control group until childbirth. Nevertheless, for all outcomes, there are sharp differences post-birth. For example, in the top left panel, we see that

mothers in Minijobs have a significantly and substantially reduced propensity to return to regular employment for many years after birth but they seem to catch up in the long run.[24] Specifically, 9 to 10 years after birth, their regular employment probability is still below that of mothers who returned to regular employment after birth by nearly 10 percentage points but this difference is statistically not significant. When we add Minijobs to look at 'any employment' (see top right panel), the differences between the two groups disappear. This is an important finding because it suggests that our results are not driven by differences in labor force participation at the extensive margin. Instead, our results imply that for a long time, mothers starting out with a Minijob after birth have a substantially reduced probability of returning to regular employment ('Minijob trap').

Not surprisingly, this disadvantage extends to earnings: the bottom left panel depicts the motherhood penalty in daily earnings when we consider non-employed mothers with a value of zero earnings. The bottom right panel exclusively considers the earnings of employed mothers. The motherhood earnings penalty is rampant for all mothers: they never catch back up to their pre-birth earnings. Additionally, we find a substantial and durable disadvantage for the mothers in Minijobs relative to those taking up regular employment after birth: even 10 years after birth, the difference in child penalties between the groups reaches 10 percentage points, which (although not clearly visible in the figure) is statistically significant.

In **Figure 4.2**, we compare the outcomes of mothers who took up a Minijob after birth to those of mothers who returned to regular employment, instead, but with earnings in the bottom quartile of the earnings distribution in month 13 after birth. These are either full-time jobs with low daily earnings or part-time employments. The results are similar to those in **Figure 4.1**: mothers starting out in low-paid regular employment after birth have a significantly

[24] The control group status is assigned based on activities in month 13 after birth whereas **Figure 4** depicts employment status in month 12. This difference in timing combined with slight imprecisions in the assignment of the date of birth generates less than 100 percent employment in month 12 after birth for the control groups in **Figure 4**.

higher propensity of regular employment even 8 years after birth (see top left panel) than those taking up Minijob employment early on. However, the differences decline over time and by year 10 are no longer statistically significant. In the first six years after birth, there is also a significant gap in child penalties in earnings (see bottom row), which nevertheless in the longer run remains at approximately 6-7 percentage points.

To show how these earnings losses accumulate over time, **Figure 5** describes the development of cumulative earnings over ten years after the first birth net of age and year effects. The left panel implies that by year 10, mothers who started out with a Minijob after birth, accumulate not even one-half of the total labor income of mothers who returned to regular employment after birth. This long-run difference is smaller but still substantial in the right panel, which considers mothers who started out with a low-paid regular job after birth as a comparison group. Over time these differences translate into considerable pension gaps. Overall, we find that taking up Minijob employment after birth causes long-run disadvantages in earnings and pension claims. This holds even when we compare mothers who take up Minijobs to those in the bottom quartile of the earnings distribution in month 13 after birth.

4.2 Transmission mechanisms

Several mechanisms may explain the substantially larger and persistent losses in earnings for mothers taking up Minijob employment after birth compared to mothers who return to regular (even if low-paid) employment instead. For example, given that firms typically use Minijobs for low-skill tasks, Minijobbers accumulate qualitatively different work experience over time. Generally, the work experience accumulated over the life cycle might play an important role in explaining maternal earnings losses after the first birth (Costa Dias et al. 2020). We illustrate this in **Figures 6.1** and **6.2** using an event study approach similar to our main analysis. Here, the outcome variables represent the labor market experience in jobs with different types of complexity. Our data report occupation-specific job complexity in four categories: unskilled

(level 1), skilled (level 2), complex (level 3), and highly complex (level 4). The vast majority of jobs (about 80 percent) fall into the skilled (level 2) category. We show results for labor market experience in unskilled vs. skilled and (highly) complex jobs (levels 2-4).

Both panels of **Figure 6.1** show that mothers who start out in a Minijob after birth experience significantly more accelerated growth in unskilled (level 1) labor market experience after birth compared to the control groups.[25] This disadvantage emerges shortly after birth and increases over time. **Figure 6.2** shows the reverse pattern for labor market experience in skilled and complex jobs (levels 2-4). We find a statistically significant gap between Minijobbers and control groups which widens over time. If experience in more complex jobs yields higher earnings, the pattern can contribute to the explanation of the long-run earnings disadvantage of Minijobbers. Importantly, we showed in section 4.1 that these results are not driven by labor force participation differences between the Minijobbers and the control groups.

Another potential explanation for the substantially larger long-run earnings penalty for Minijobbers might be connected to differences in the accumulation of firm-specific human capital. This may follow if mothers who take up a Minijob after birth are more likely to change employers than mothers who start out in regular employment post-birth; the latter may be more likely to return to their previous employers. In this situation, we expect to observe more volatile employment biographies and more frequent job switches for the Minijobbers. We investigate this issue in **Figures 7.1** and **7.2** again using event study designs.

The outcome in **Figure 7.1** is the probability of having a different employer than the one a mother had six months prior to birth, which we determine using unique establishment

[25] Given the low incidence of unskilled jobs in our data, in **Figure 6.1** we extend the earnings threshold for the definition of low-paid regular employment from the bottom quartile to 40th earnings percentile. Here, using the first-quartile threshold (as in the other figures) leads to implausible and outlying relative effects in the right panel of **Figure 6.1**. For comparability, we also use the 40th percentile when defining low-paid employment in **Figure 6.2** on experience in complex jobs. Nevertheless, **Figure 6.2** looks nearly identical if we use the 25th percentile, which makes us confident that the sensitivity of the results for unskilled jobs (**Figure 6.1**) is solely due to limited outcome variation in the data.

identifiers. In this analysis, we have to exclude mothers who are not employed. Both panels confirm that Minijobbers are more likely to leave their pre-birth employer shortly after birth compared to mothers who start out in regular (even if low-paid) employment after birth. However, to some extent, the control groups catch up over time as they also start to leave their pre-birth employers after birth, though somewhat later than the Minijobbers. Consequently, the difference between the Minijobbers and the control groups declines over time. While the gap persists in the long run, it is statistically no longer significant. We extend this analysis to subsequent employer switches in **Figure 7.2** where the outcome variable is the cumulative number of main employers starting 5 years before birth. Generally, we observe more employers for the Minijobbers. Although the differences are statistically significant only in the first four years after birth, the gap seems to increase in magnitude over time.

Overall, entering a Minijob employment shortly after birth is closely associated with a long-run disadvantage in the accumulation of qualitatively valuable labor market experience. Minijobbers also experience more volatile employment biographies after birth due to the relatively higher mobility across employers compared to mothers returning to regular employment after birth. These are likely determinants of Minijobbers' larger long-run labor market penalties after childbirth.[26]

4.3 Heterogeneity by prior earnings

Next, we investigate whether the consequences of Minijob employment after birth vary by pre-birth earnings reflecting differences in maternal human capital. We group all mothers in three terciles based on their position in the pre-birth earnings distribution. We expect that women with higher human capital suffer higher opportunity costs of not working in a regular job and

[26] In addition, Minijob subsidies can generate labor market traps for mothers facing high income tax rates upon exceeding the Minijob earnings ceiling. In **Appendix B,** we take advantage of additional data (described in detail in section 4.4) to test for responses based on an interaction between the income tax regime and the Minijob subsidy.

therefore return to regular employment faster. We also expect that the relative decline in earnings is larger at the upper end of the earnings distribution because human capital depreciation may not be relevant for unskilled employment in the bottom tercile. With respect to the results of taking up a Minijob instead of regular employment, we expect that Minijobs generate greater earnings losses for previous high earners. However, that group may then leave Minijobs faster in order to return to regular employment than women with low human capital and low earnings before birth.

Figures 8.1 and **8.2** depict the regular employment and earnings outcomes for mothers in Minijobs versus regular employment by pre-birth earnings tercile. **Figure 8.1** indeed shows that those with previously higher earnings are most likely to return to regular employment. At the same time, **Figure 8.2** shows that the relative motherhood earnings penalty when regularly employed is highest for those with the highest pre-birth earnings which confirms findings for the United States (England et al. 2016).[27] Thus, the magnitude of the motherhood penalties differs across the pre-birth earnings distribution. However, there are no clear patterns with respect to the long-run impact of Minijobs.

4.4 Robustness checks

To investigate the sensitivity of our estimates, we return to **Table 2** showing the estimated coefficients of the event time dummies for mothers taking up regular employment vs. those in Minijobs. Panel A presents the baseline estimation results as used for the calculation of the relative child penalties depicted in **Figure 4.1**.

We estimated the baseline equation as specified in section 3.1 separately for the Minijobbers and the control groups. However, as we use child age and birth year for matching, these variables are not strictly necessary as controls in the regressions. In a first robustness test,

[27] Appendix **Figure A.4** shows the tercile-specific results when the outcome measure considers zero-valued earnings.

we omit these controls. The results yield much larger differences in the motherhood penalties for the Minijobbers and control groups (see panel B in **Table 2**): in year 10 after birth, the propensity to be regularly employed differs significantly by 30 percent and the earnings penalty of Minijobbers exceeds the earnings penalty of the regular employed by more than 70 percent. Possibly, our baseline regression model is too conservative if age and year effects respond to Minijob take up.

Panel C shows the results after using the nearest neighbor instead of radius caliper matching. The estimates are similar to those in panel A except for larger estimates after 10 years.[28] In panel D, we aim to reduce the impact of extreme outliers in our matching procedure by dropping 5 percent of the matched control observations at the tails of the propensity score distributions. Again, the estimated penalties are somewhat larger 10 years after birth. In panel E, we match mothers in Minijobs to mothers in regular employment both measured 18 months after birth instead of 13 months. The estimates are also larger after this change. Next, we inspect whether our results are driven by potential differences in subsequent childbearing. In panel F, we present the estimates after omitting the observations for mothers after their second birth. The resulting relative disadvantages of Minijob vs. regular employment are now again larger than in our baseline sample. However, we observe additional births only for mothers who remain subject to social security within employment, i.e., with a strong attachment to the labor market. This is why the results need to be interpreted with caution.[29] In panel G, we consider a more recent sample of mothers who gave birth after the paid parental leave reform in 2007. Due to the time window of the data, we can only evaluate their motherhood penalties in the first 5 years after birth. However, the patterns of significantly larger employment and earnings

[28] The Minijob penalties are identical because mainly the matched control observations change (mothers in regular employment) whereas the Minijob group remains constant.

[29] For the same reason, we cannot reliably use subsequent fertility as an additional outcome; instead, we show additional evidence on this issue based on survey data in **Figure A.5** (panel H).

penalties with Minijob vs. regular employment are confirmed in this sample as well. In sum, the results from alternative matching procedures and sample restrictions strongly support our main conclusions.

A remaining issue is that the administrative data we use for our estimations do not provide any information on household composition or individual attitudes. Thus, our results would be potentially biased if the Minijobbers and the control groups differed in these dimensions. We use two strategies to evaluate the relevance of this issue. First, we study a similarly defined sample of first-time mothers in survey data from the German Socioeconomic Panel (SOEP). The sample sizes are much smaller, which is an important disadvantage compared to our primary data source. Nevertheless, the SOEP includes relevant information on a woman's household composition and wealth, her attitudes with respect to occupational success, family and children, overall life satisfaction, and the number of subsequent children. Thus, the SOEP allows us to investigate whether mothers who take up a Minijob shortly after birth differ in these characteristics from other mothers**.** Reassuringly, **Figure A.5** shows that this is not the case.[30] Neither the probability of single motherhood nor the household income and wealth, or maternal attitudes differ across the groups. At the time of birth mothers with and without subsequent Minijob employment do not differ in their evaluation of occupational success, Importantly, also subsequent fertility is almost identical for both groups until the first child reaches the age of 6 and differs only slightly by year 10 after the first birth, which might be endogenous to Minijob employment itself.

Second, to incorporate the partner's characteristics in our estimations, we take advantage of a recently developed strategy. It approximately identifies married couples in the social security records after 2001 based on shared addresses and last names and conditional on

[30] Given the small sample size, we use an unbalanced panel for **Figure A.5** and compare Minijobbers to all other mothers. The outcomes reflect information at the time of the annual survey responses, which varies in its distance to birth. The patterns are less smooth but qualitatively similar when we use a reduced sample after balancing the panel.

a maximum age difference of 15 years (for details see Bächmann et al. 2021). Due to data protection, we cannot apply this procedure to our data and thus had to obtain an alternative dataset comprising identified couples instead. We apply similar sample restrictions as in our main analysis but by construction, this alternative sample differs in some dimensions from our main data. Specifically, it excludes births before 2001 and does not consider mothers who live as single parents, are not married, do not share the spouses' surname, or have a large age difference relative to their spouses. Panel G in **Table 2** shows the results when we repeat our analyses using the sample of couples, which allows us to additionally match on partner characteristics.[31] Despite some differences in the sample composition, the estimates confirm our baseline results and yield even larger Minijob penalties after ten years. More importantly, using the sample of couples, we also carefully investigate whether the estimation results differ with and without controls for partner characteristics in the matching procedure. **Figure A.6** in the appendix shows that the additional controls do not affect the results. Across all outcomes, the development of Minijob penalties is virtually identical for the two procedures. Therefore, we are confident that the lack of controls for household background in our main analysis does not harm our main results.[32]

Overall, these auxiliary analyses support the interpretation of our main estimates. It is also worth noting that all performed robustness tests suggest that our baseline approach yields very conservative results. For example, when looking at the absolute differences in the earnings penalties between Minijobbers and regular employees 10 years after birth, our main results in panel A imply a 10 percentage point difference. In comparison, in all presented sensitivity tests, this gap increases, mostly to 13-16 percentage points. Similarly, in panel A, the relative

[31] We consider indicators of the spouses' year of birth, education, earnings and employment status, tenure, and labor force experience 6 months prior to birth in the matching.

[32] This is consistent with recent results on maternal job search after birth based on Austrian administrative data, which suggest that husbands do not play an important role in maternal labor market choices (Lafférs and Schmidpeter 2021).

difference after 10 years suggests that the earnings penalty for the Minijobbers is 21 percent higher compared to the penalty of regular employees. In comparison, most tests yield differences of 27-35 percent, instead. We observe similar patterns for the employment penalty. Overall, we find larger estimates in our robustness tests than in our baseline estimation.

4.5 Comparing mothers in Minijob employment to those not employed

So far, we considered (low-paid) regular employment as the relevant alternative to a Minijob employment after birth. Nevertheless, the vast majority of mothers in our data are neither in regular nor in Minijob employment at month 13 after their first birth. Thus, in this section, we complete the picture by extending the main analysis to the matched sample of mothers who start out in a Minijob after birth and those who remain non-employed.

Figure A.7 in the Appendix depicts the baseline results for this control group.[33] We find that over time, Minijob employees are statistically but not economically significantly more likely to subsequently take up regular employment than mothers who are not employed in month 13 after birth (top panels). We also do not observe large long-run differences in earnings (bottom panels**)** even though the difference is statistically significant. Thus, while the comparison group of initially non-employed mothers, on average, stay out the labor force for longer than Minijobbers, they do not suffer significantly larger earnings losses.

In sum, compared to the large group of mothers who do not return to employment by month 13 after birth, Minijobbers do not enjoy advantages in earnings and regular employment. Therefore, Minijobs do not seem to be a stepping stone to employment for recent mothers who would otherwise remain out of the labor force.

5. Conclusions

[33] **Figure A.8** shows the distribution of the corresponding propensity scores.

In this study, we investigate the role of incentives generated by public policy instruments as potential drivers of the motherhood effect for female labor market outcomes in Germany where motherhood penalties are particularly large (e.g., Kleven et al. 2019a). Specifically, we focus on a large labor market program, the German Minijobs, which refers to subsidized employment relationships where workers earn up to a monthly earnings threshold (400 Euro in the period under study). The small job subsidy amounts to between 20 and 60 percent of gross earnings, depending on the individual income tax situation. This makes a Minijob more attractive than regular part-time employment over a wide range of gross earnings beyond the Minijob threshold. Although intended as a stepping-stone to regular employment for the unemployed, Minijobs might trap some individuals in unskilled jobs with low earnings (e.g., Blömer and Peichl 2020). This applies especially to mothers who typically reduce their working hours after birth. Surprisingly, so far, there is no research on the unintended and potentially adverse consequences of Minijobs for the development of maternal labor market outcomes after birth.

This study uses detailed administrative data on employment biographies to compare the long-run labor market outcomes for mothers who take up Minijob employment after first birth versus those who take up regular employment or remain non-employed. To address the possibility of selection into Minijobs after birth, we apply propensity score matching based on a rich set of pre-birth characteristics. Using the matched samples, we then follow the different groups of mothers up to 10 years after birth within an event time study to test whether Minijobs contribute to the long-run motherhood penalties in Germany.

Generally, we confirm large motherhood penalties in Germany with post-birth earnings declining by about 60 percent in the long run. This loss is partly due to a persistent 40 percent drop in labor force participation. Although within the group of mothers who return to the labor market shortly after birth the long-run penalty is somewhat lower, we find significant differences by initial job type after birth. Specifically, when we compare the penalties of mothers who start out in subsidized Minijob employment to observationally identical mothers

who return to regular unsubsidized employment after birth, we find that for many years, Minijobbers suffer much larger motherhood penalties. This applies to both the propensity of regular employment and earnings, and is robust to accounting for partner characteristics. At the same time, Minijobbers do not realize substantially better long-run labor market outcomes compared to initially non-employed mothers.

Among first-time mothers who initially take up a Minijob after birth, the long-run earnings penalty after 10 years is 60 percent relative to pre-birth earnings. For those starting out in regular employment, it reaches 50 percent. This absolute difference of 10 percentage points is statistically and economically significant. It implies that Minijobbers experience long-run earnings penalties which are 21 percent larger than for this comparison group. This is a rather conservative estimate as a wide range of robustness tests suggests even larger differences. In the long run, mothers who started out with a Minijob after birth, accumulate only one-half of the labor income of mothers who returned to regular employment after birth. This has considerable implications for maternal pension claims. In line with our results, Germany displays one of the highest gender pension gaps internationally (OECD 2021).

When investigating the potential mechanisms behind the negative consequences of Minijobs compared to regular employment, we find that the lower earnings and employment opportunities are potentially due to worse job characteristics and losses in firm-specific human capital. Specifically, mothers taking up a Minijob after birth are more likely to accumulate additional labor market experience in unskilled jobs and are less likely to gain experience in complex jobs compared to those who return to regular employment after birth. Minijobbers also more frequently switch employers, which yields more volatile employment biographies.

Overall, our results indicate adverse unintended labor market consequences of a small job subsidy program for mothers. The Minijob program seems to fail not only as a stepping stone into regular employment it also harms maternal labor market outcomes, increases the aggregate motherhood penalty, and impedes the availability and development of a qualified

workforce. Even if mothers who initially limit their labor supply after birth optimize their short-run employment choices, our results suggest that they may suffer long-term career disadvantages and, consequently, reduced pension benefits.

Similar programs supporting small jobs exist in numerous countries. It is important to understand and acknowledge that subsidizing small jobs might contribute to motherhood penalties and inhibits gender convergence in the labor market.

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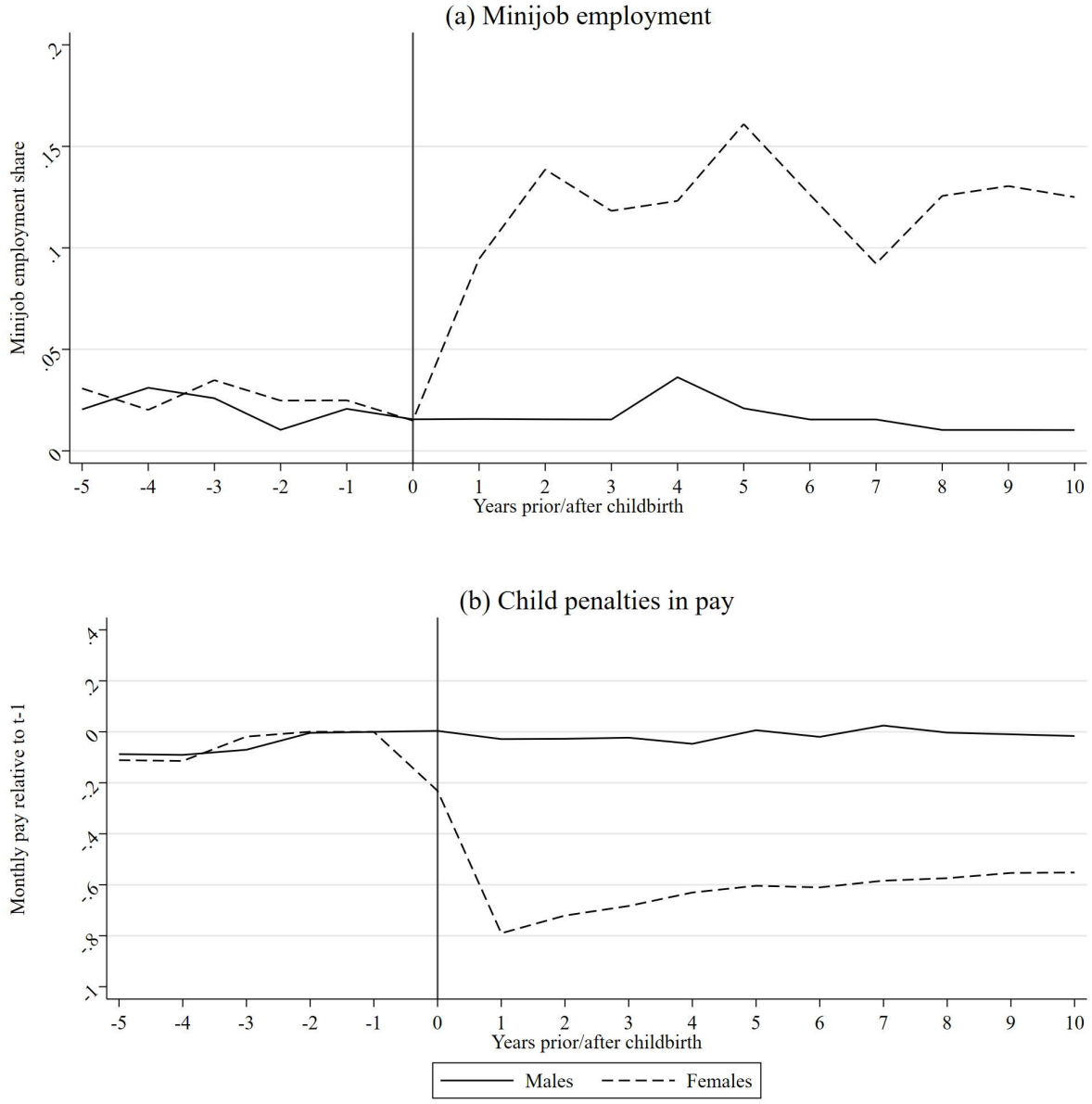
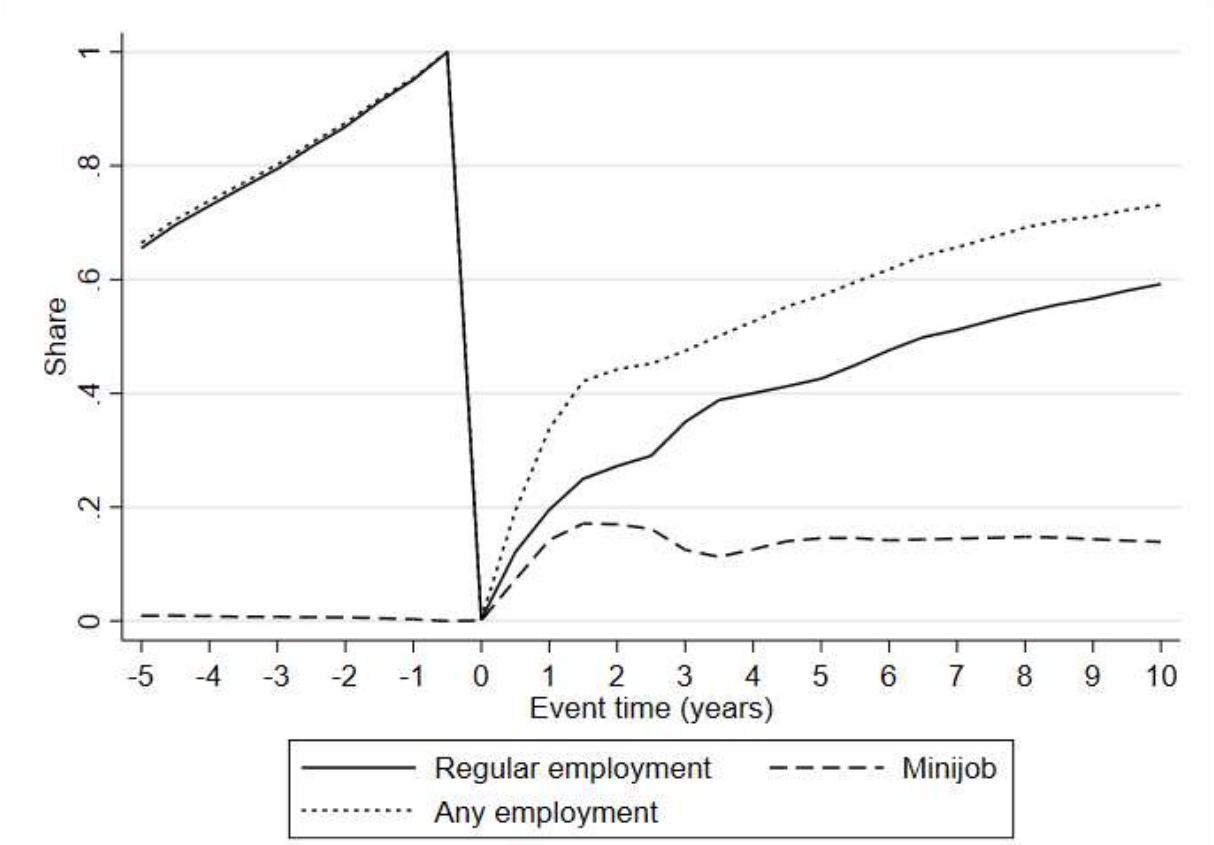
Figure 1 Average Minijob employment around birth and the motherhood earnings penalty



Note: The Minijob employment share in panel (a) describes the share of Minijob employees among all mothers and fathers. Panel (b) shows the estimated child penalties using the methodology by Kleven et al. (2019b). Thus, it replicates the results for Germany presented in Figure 3 in Kleven et al. (2019a) using a slightly different sample.

Source: SOEP (1984-2018, v35, DOI: 10.5684/soep-core.v35i). The sample contains first-time parents of children born between January 1999 and December of 2006 where the parents are observed every year between 5 years before having their first child and 10 years thereafter. Only individuals in West Germany are considered.

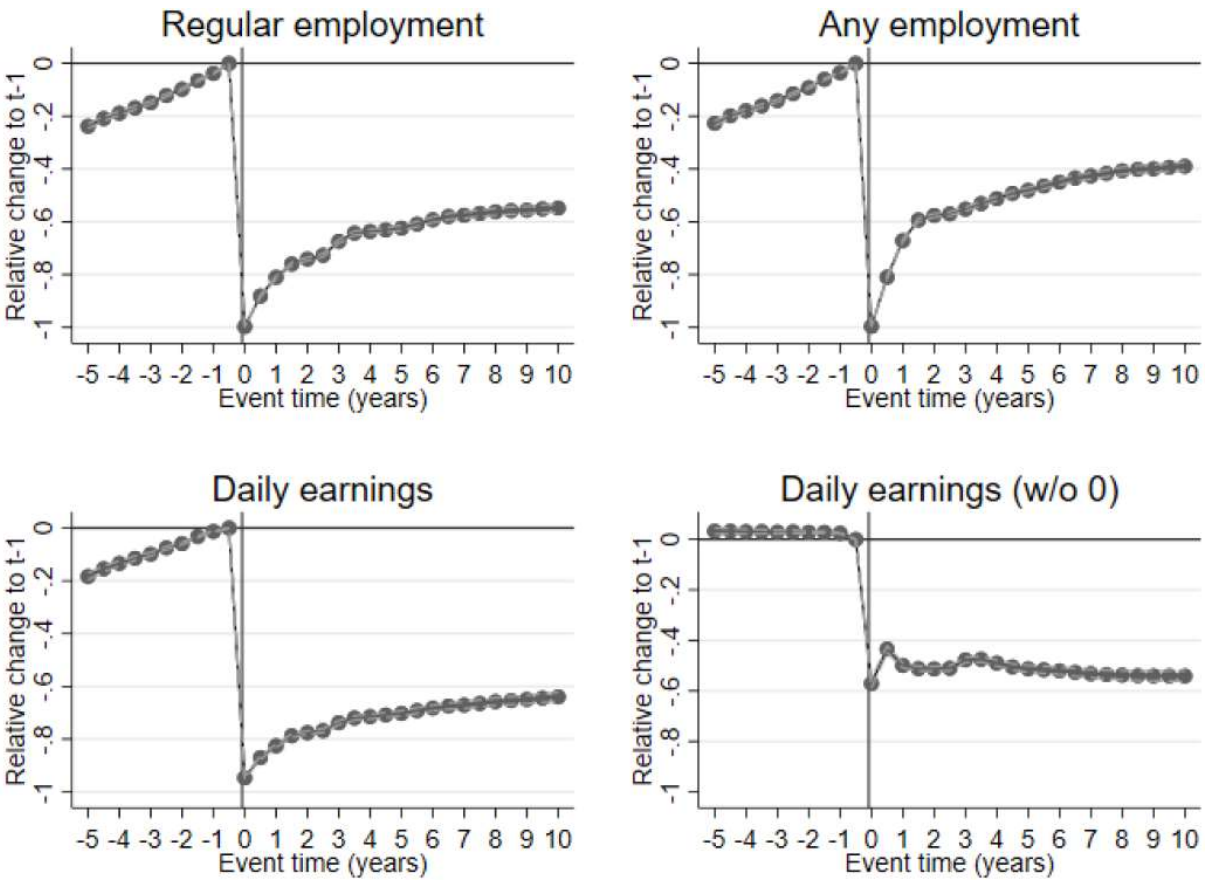
Figure 2 Development of average labor force status of sampled mothers over time



Note: The x-axis shows the time relative to the event of first childbirth (indexed to 0). Regular employment refers to full-time and part-time jobs subject to social security contributions and income tax. Minijobs are exempt from these, otherwise mandatory, contributions. Any employment provides the sum of regular employment and Minijobs.

Source: SIAB (1999-2017) and own calculations.

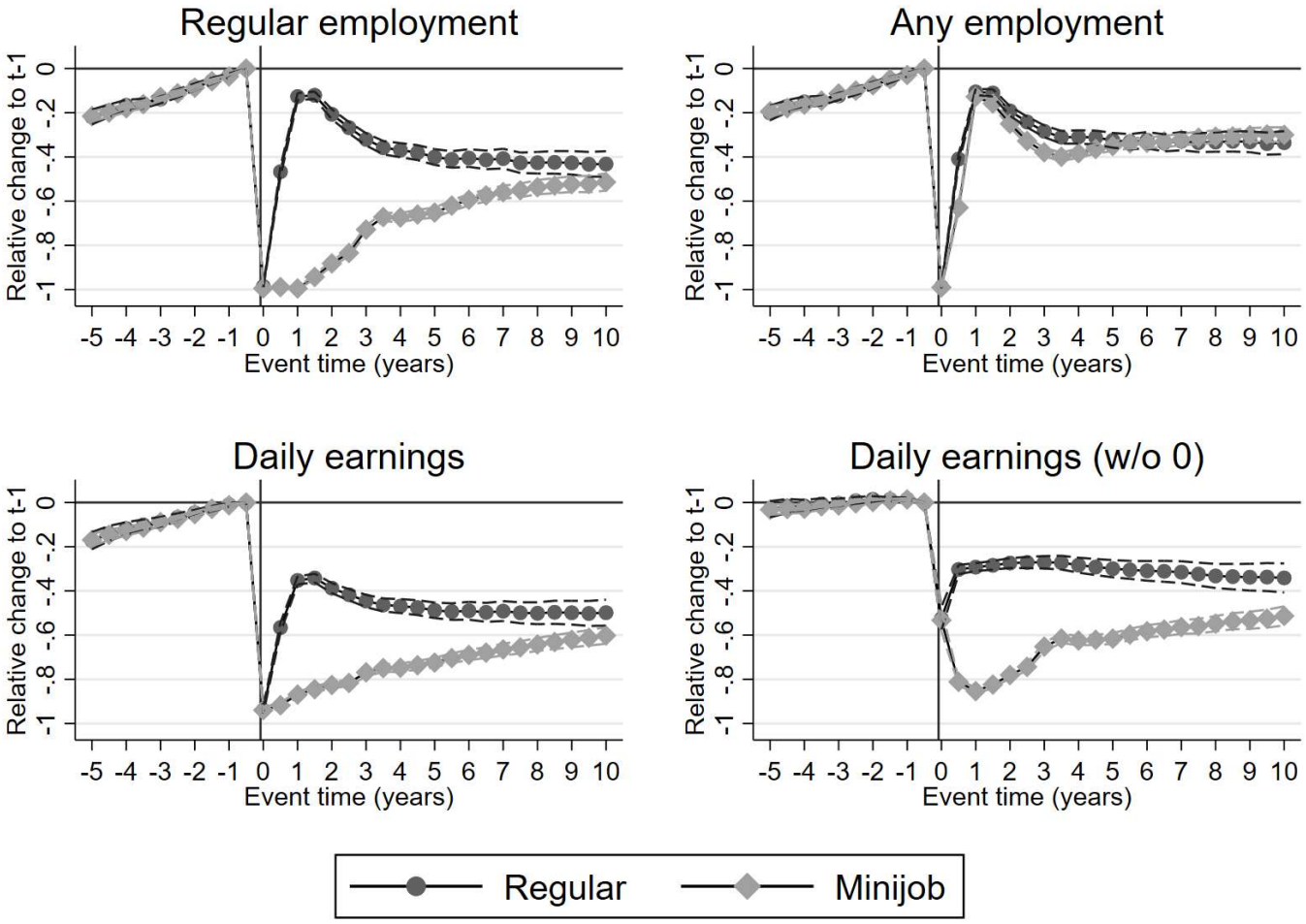
Figure 3 Development of outcome measures for the full sample



Note: The x-axis shows the time relative to the event of first childbirth (indexed to 0). The dots depict the employment status and pay at child's age t relative to the respective outcome 6 months prior to birth and net of age and year-specific effects. The shaded areas represent 95 percent confidence intervals, which turn out to be rather narrow. The estimation samples cover the period from 60 months prior to 120 months post birth.

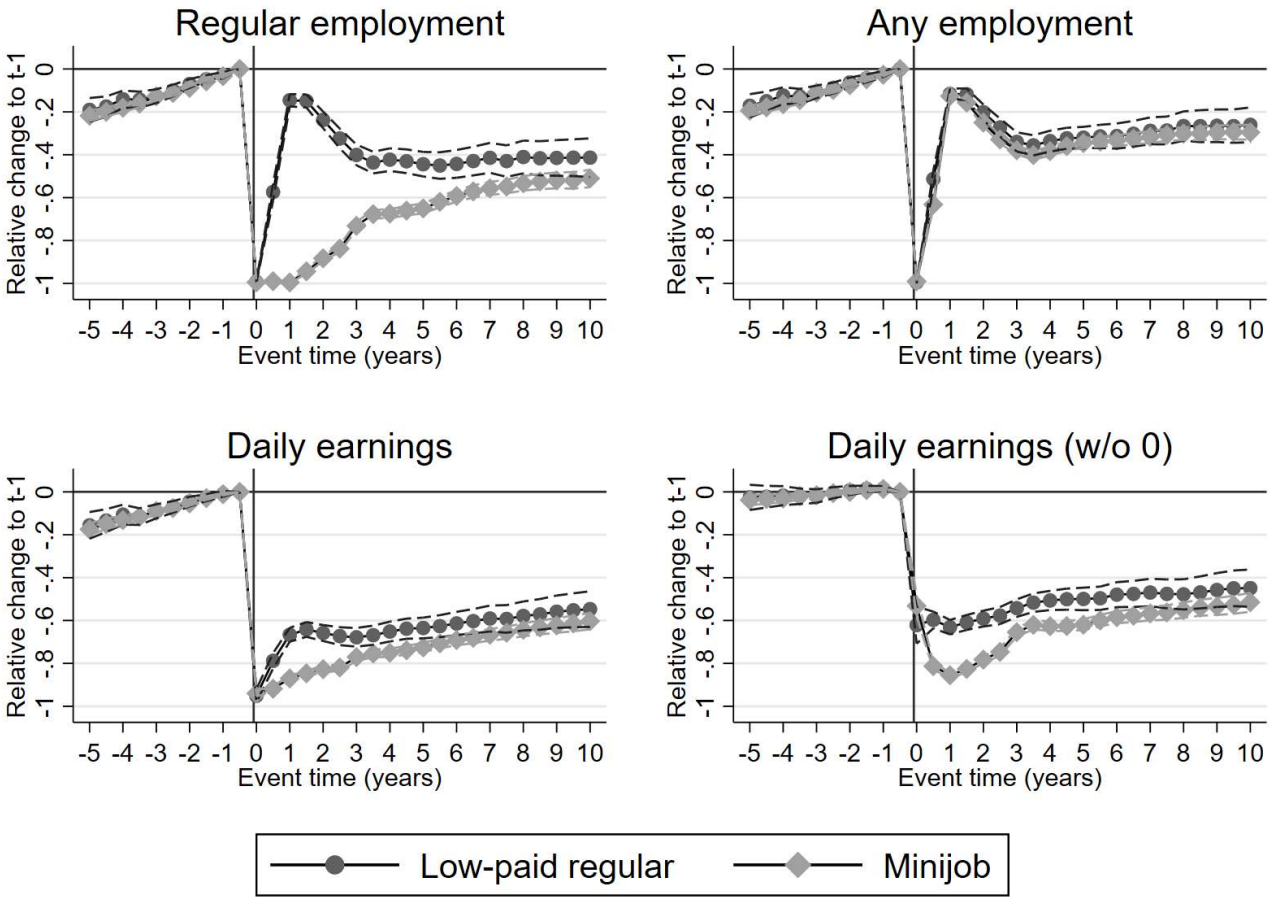
Source: SIAB (1999-2017) and own calculations.

Figure 4.1 Motherhood penalties for mothers starting out in Minijob versus in regular employment after birth (control) for four outcomes



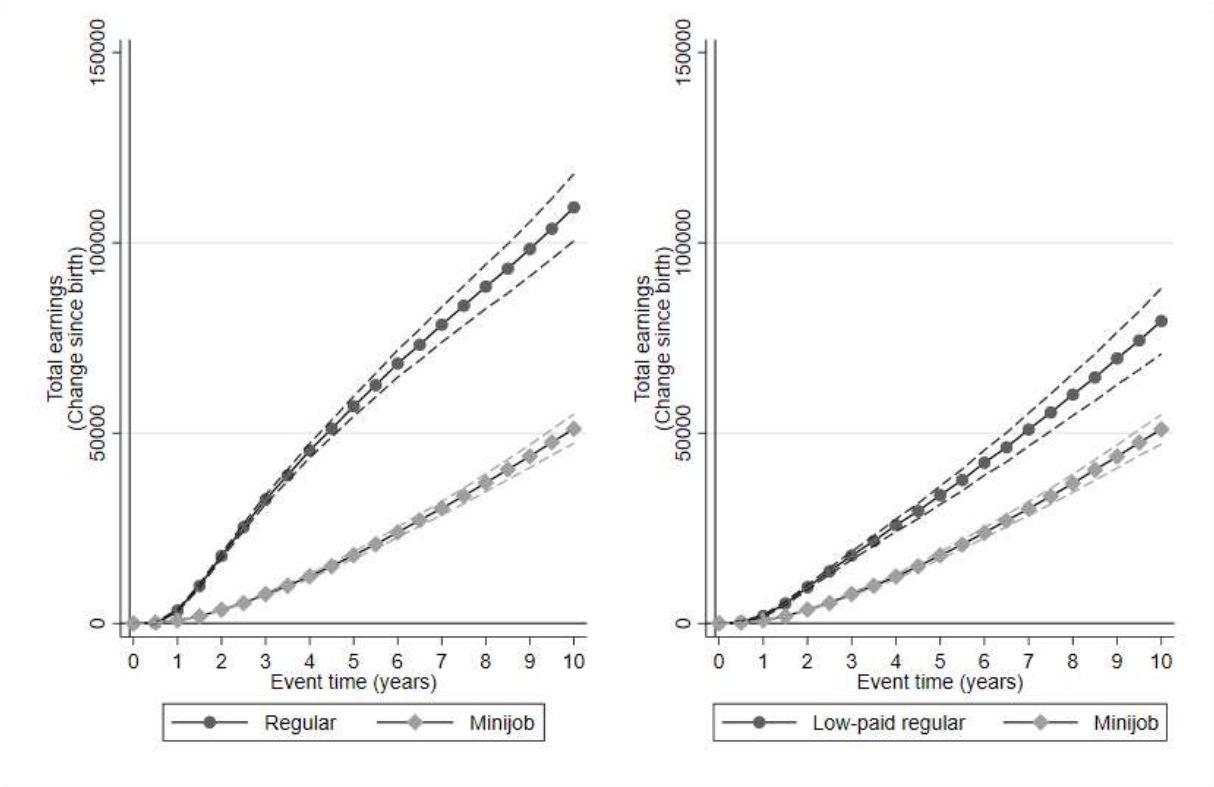
Note: The x-axis is indexed to 0 in the month of birth. The figures show the employment status and pay at child's age t relative to the respective outcome 6 months prior to birth. The estimation samples are a result of a matching of the Minijob group to the control group of regularly employed mothers. The figures show 95 percent confidence intervals and cover the period from 60 months prior to 120 months post birth.
Source: SIAB (1999-2017) and own calculations.

Figure 4.2 Motherhood penalties for mothers starting out in Minijob versus low-paid regular employment after birth (control) for four outcomes



Note: The x-axis is indexed to 0 in the month of birth. The figures show the employment status and pay at child's age t relative to the respective outcome 6 months prior to birth. The estimation samples are a result of a matching of the Minijob group to the control group of low-paid regularly employed mothers (bottom earnings quartile). The figures show 95 percent confidence intervals and cover the period from 60 months prior to 120 months post birth.
Source: SIAB (1999-2017) and own calculations.

Figure 5 Post-birth cumulative earnings for mothers starting out in Minijob versus regular employment (left panel) and versus low-paid regular employment (right panel) after birth



Note: The x-axis is indexed to 0 in the month of birth. The figures show the sum of post-birth earnings (in 2010 EUR) at child's age t net-of age and year-specific effects starting at birth. The samples are a result of a matching of the Minijob group to the two alternative groups. The figures show 95 percent confidence intervals and cover the period up to 120 months post birth.

Source: SIAB (1999-2017) and own calculations.

Figure 6.1 Labor market experience in unskilled jobs for mothers starting out in Minijob versus those in regular (left panel) and low-paid regular employment (right panel) after birth

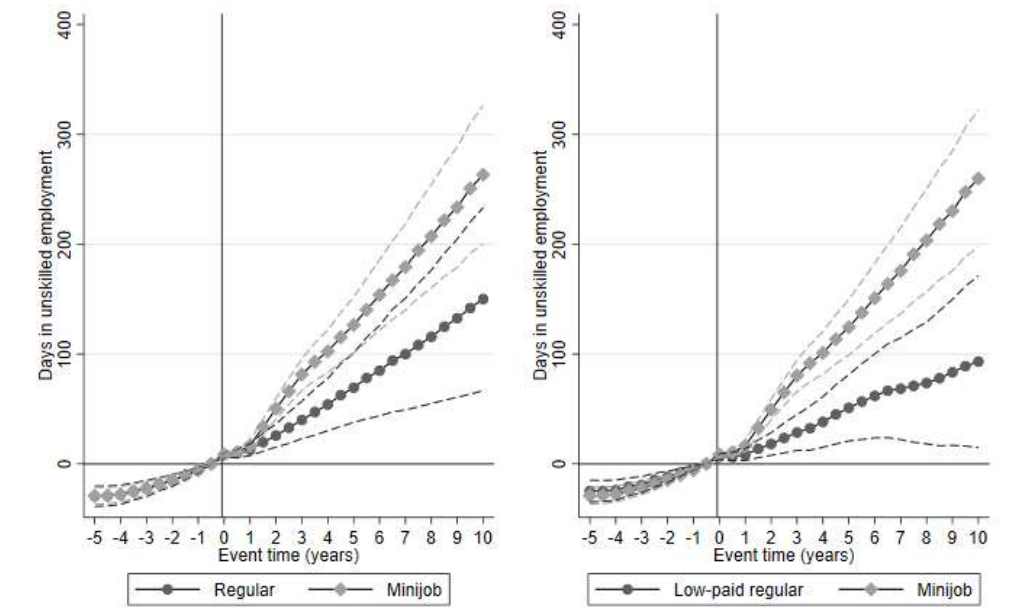
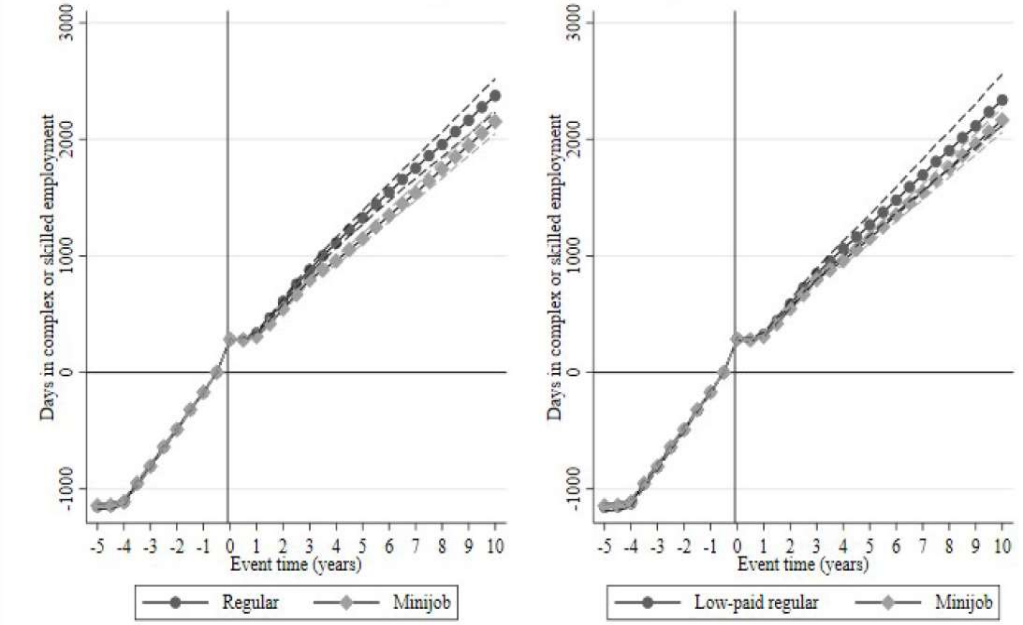


Figure 6.2 Labor market experience (in days) in skilled and complex jobs for mothers starting out in Minijob versus those in regular employment (left panel) and low-paid regular employment (right panel) after birth



Note: The x-axis is indexed to 0 in the month of birth. The figures show maternal labor market experience by job complexity at child's age t. The estimation samples are a result of a matching of the Minijob group to the two alternative groups. The figures show 95 percent confidence intervals and cover the period from 60 months prior to 120 months post birth. Note that the vast majority of employment spells is classified as skilled employment, which explains the large differences in the scaling of the y-axis between panel 6.1 and 6.2.
Source: SIAB (1999-2017) and own calculations.

Figure 7.1 Change of the pre-birth employer for mothers starting out in Minijob versus those in regular employment (left panel) and low-paid regular employment (right panel) after birth

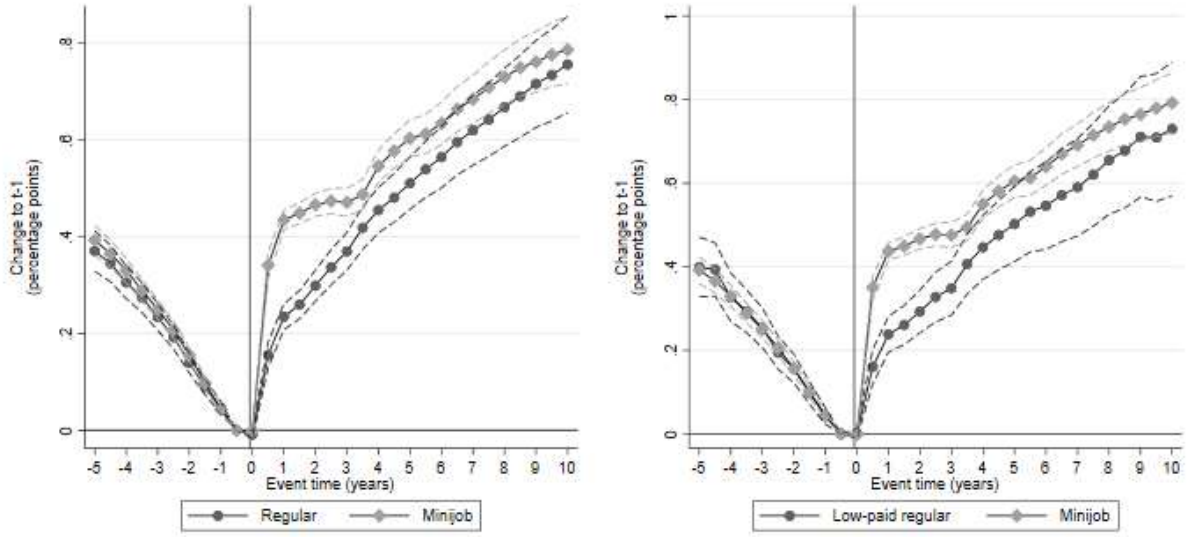
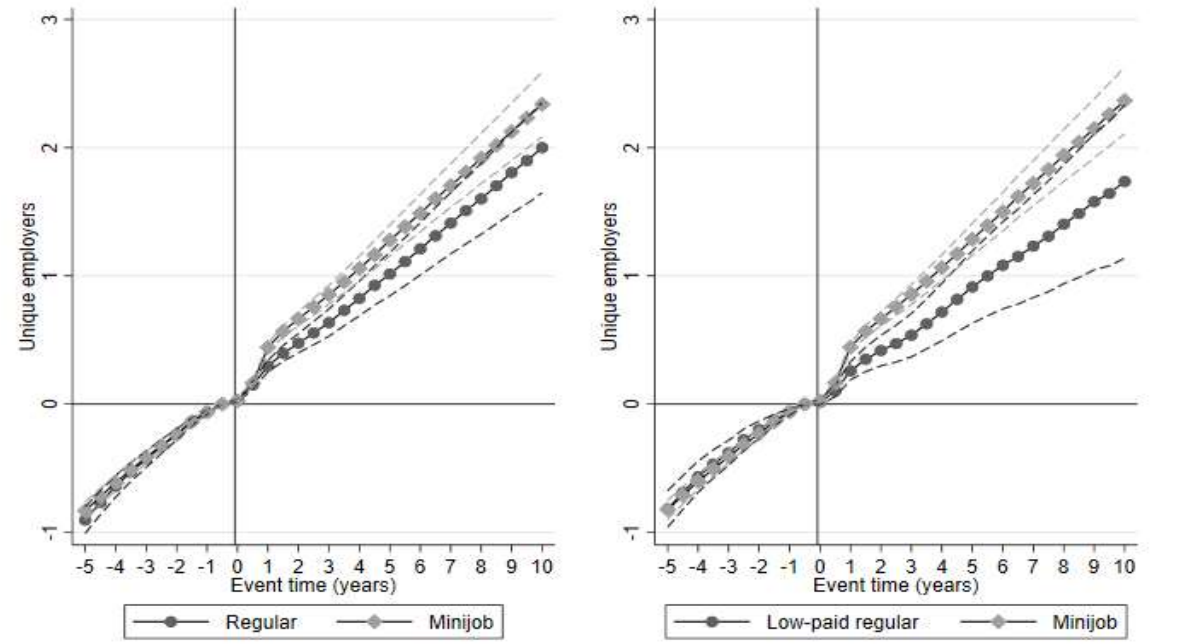


Figure 7.2 Number of employers for mothers starting out in Minijob versus those in regular employment (left panel) and low-paid regular employment (right panel) after birth



Note: The x-axis is indexed to 0 in the month of birth. **Figure 7.1** shows the probability of a change in the establishment number at child's age t compared to the establishment number 6 months prior to birth. **Figure 7.2** shows the cumulative number of unique employers starting 5 years before birth and up to a child's age t. The estimation samples are a result of a matching of the Minijob group to the two alternative groups. The figures show 95 percent confidence intervals and cover the period from 60 months prior to 120 months post birth.
Source: SIAB (1999-2017) and own calculations.

Figure 8.1 Baseline results for the probability of regular employment by pre-birth earnings (Minijob vs. regular employment)

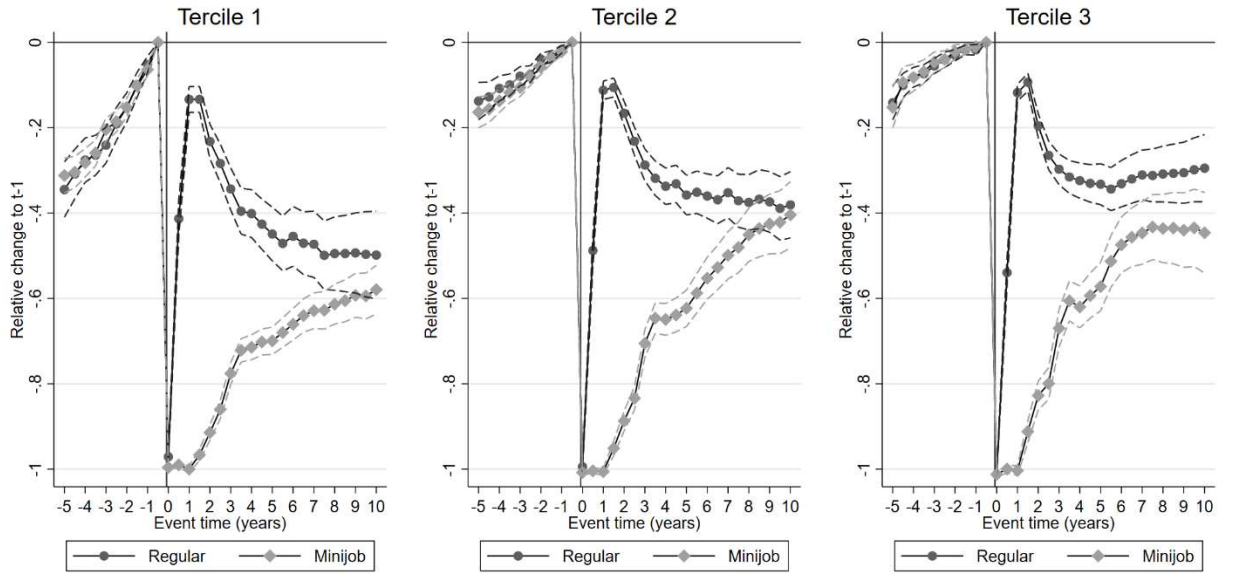
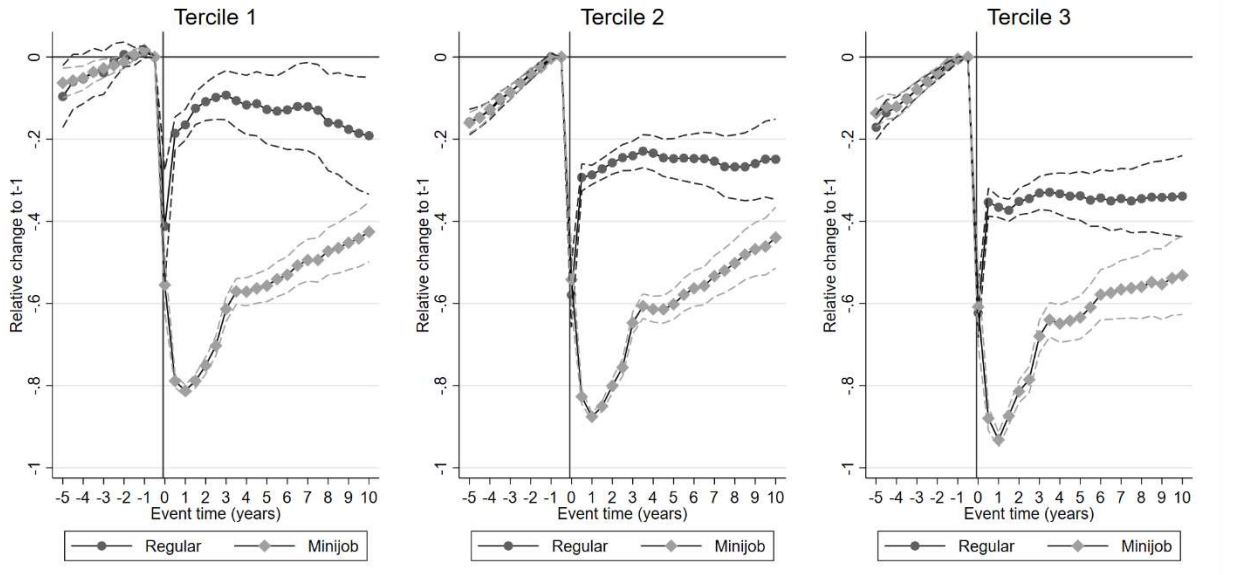


Figure 8.2 Baseline results for earnings excluding zero values by pre-birth earnings (Minijob vs. regular employment)



Note: Terciles are defined based on the earnings distribution 6 months prior to birth. Tercile 1 reflects the lowest and tercile 3 the highest earning mothers. The x-axis is indexed to 0 in the month of birth. The figures show the employment status (top) and pay (bottom) at child's age t relative to the respective outcome 6 months prior to birth. The estimation samples are a result of a matching of the Minijob group to the control group of regularly employed mothers. The figures show 95 percent confidence intervals and cover the period from 60 months prior to 120 months post birth.

Source: SIAB (1999-2017) and own calculations.

Table 1 Balancing of covariates

		Minijob	Low-paid regular		Minijob	Regular employee		Minijob	Not employed	
		Mean	Mean	Diff	Mean	Mean	Diff	Mean	Mean	Diff
Age at birth	U	28.351	28.993	-0.642 ***	28.351	30.305	-1.954 ***	28.351	29.262	-0.911 ***
	M	28.405	28.412	-0.007	28.380	28.481	-0.101	28.362	28.339	0.023
Daily earnings 6 ms pre-birth	U	66.614	73.621	-7.007 ***	66.614	88.786	-22.172 ***	66.614	73.148	-6.534 ***
	M	67.280	66.903	0.377	67.274	66.767	0.507	66.683	66.186	0.497
Reg. employed 12 ms pre birth	U	0.958	0.948	0.010	0.958	0.964	-0.006	0.958	0.945	0.013 **
	M	0.958	0.963	-0.005	0.967	0.959	0.008	0.957	0.960	-0.003
Labor Market Experience										
Tenure pre-birth (years)	U	4.740	4.500	0.240 +	4.740	4.590	0.150 +	4.740	4.501	0.239 **
	M	4.714	4.566	0.148	4.710	4.652	0.058	4.733	4.694	0.039
Experience pre-birth (years)	U	9.607	9.268	0.339 **	9.607	9.795	-0.188 +	9.607	9.477	0.130
	M	9.614	9.500	0.114	9.574	9.578	-0.004	9.607	9.575	0.032
Unemploym. exp. pre-birth (years)	U	0.212	0.220	-0.008	0.212	0.229	-0.017 ***	0.212	0.282	-0.070 ***
	M	0.214	0.221	-0.007	0.214	0.214	0.000	0.213	0.219	-0.006
Minijob exper. pre-birth (years)	U	0.052	0.062	-0.010	0.052	0.047	0.005	0.052	0.045	0.007
	M	0.054	0.046	0.008	0.052	0.057	-0.005	0.052	0.050	0.002
Education										
No vocational Training	U	0.052	0.059	-0.007	0.052	0.051	0.001	0.052	0.079	-0.027 ***
	M	0.050	0.048	0.002	0.054	0.055	-0.001	0.052	0.053	-0.001
Vocational Training	U	0.745	0.652	0.093 ***	0.745	0.557	0.188 ***	0.745	0.645	0.100 ***
	M	0.742	0.737	0.005	0.740	0.724	0.016	0.744	0.748	-0.004
Upper Secondary	U	0.032	0.037	-0.005	0.032	0.043	-0.011 **	0.032	0.037	-0.005
	M	0.033	0.030	0.003	0.032	0.040	-0.008	0.032	0.031	0.001
Upper Secondary + voc training	U	0.109	0.153	-0.044 ***	0.109	0.153	-0.044 ***	0.109	0.133	-0.024 ***
	M	0.112	0.119	-0.007	0.109	0.118	-0.009	0.109	0.107	0.002
Tertiary degree	U	0.062	0.100	-0.038 ***	0.062	0.196	-0.134 ***	0.062	0.105	-0.043 ***
	M	0.062	0.066	-0.004	0.064	0.064	0.000	0.053	0.061	-0.008
Establishment size										
1 to 9	U	0.292	0.205	0.087 ***	0.292	0.130	0.162 ***	0.292	0.194	0.098 ***
	M	0.287	0.288	-0.001	0.278	0.281	-0.003	0.291	0.291	0.000
10 to 19	U	0.159	0.117	0.042 ***	0.159	0.095	0.064 ***	0.159	0.111	0.048 ***
	M	0.157	0.151	0.006	0.157	0.159	-0.002	0.157	0.159	-0.002
20 to 49	U	0.159	0.133	0.026 *	0.159	0.129	0.030 ***	0.159	0.127	0.032 ***
	M	0.160	0.175	-0.015	0.162	0.159	0.003	0.159	0.157	0.002
50 to 99	U	0.106	0.107	-0.001	0.106	0.108	-0.002	0.106	0.113	-0.007
	M	0.107	0.110	-0.003	0.108	0.110	-0.002	0.106	0.107	-0.001
100 to 199	U	0.086	0.083	0.003	0.086	0.102	-0.016 *	0.086	0.116	-0.030 ***
	M	0.085	0.080	0.005	0.088	0.086	0.002	0.086	0.085	0.001
200 to 499	U	0.088	0.140	-0.052 ***	0.088	0.159	-0.071 ***	0.088	0.140	-0.052 ***
	M	0.091	0.096	-0.005	0.091	0.094	-0.003	0.088	0.089	-0.001
more than 500	U	0.111	0.215	-0.104 ***	0.111	0.277	-0.166 ***	0.111	0.199	-0.088 ***
	M	0.114	0.100	0.014	0.115	0.111	0.004	0.112	0.112	0.000
Job complexity										
Unskilled job	U	0.027	0.028	-0.001	0.027	0.026	0.001	0.027	0.031	-0.004
	M	0.025	0.022	0.003	0.028	0.028	0.000	0.027	0.028	-0.001
Skilled job	U	0.835	0.789	0.046 ***	0.835	0.734	0.101 ***	0.835	0.823	0.012
	M	0.834	0.817	0.017 +	0.831	0.823	0.008	0.839	0.840	-0.001
Complex job	U	0.050	0.051	-0.001	0.050	0.055	-0.005	0.050	0.050	0.000
	M	0.051	0.064	-0.013 *	0.051	0.056	-0.005	0.050	0.051	-0.001
Highly complex job	U	0.088	0.133	-0.045 ***	0.088	0.185	-0.097	0.088	0.095	-0.007
	M	0.091	0.097	-0.006	0.090	0.093	-0.003	0.084	0.082	0.002
Number of mothers	U	3,234	1,151		3,234	4,618		3,234	13,778	
	M	2,964	1,078		3,124	4,179		3,214	13,731	

Note: U and M stand for unmatched and matched. In addition to the shown covariates, our matching procedure also uses federal state at birth (dummies), quarter of birth of child (dummies), year of birth of child (dummies), daily earnings prior to birth (dummies), labor force participation prior to birth (dummies). The columns entitled "Diff" show the difference of the means provided in the two preceding columns. ***, **, *, and + indicate statistical significance of the difference at the 0.1, 1, 5, and 10 percent significance level.

Source: SIAB (1999-2017) and own calculations.

Table 2 Estimation results

		2 Years after birth		5 Years after birth		10 Years after birth	
		b	se	b	se	b	se
(A) Baseline							
Regular Employment	Regular	-0.207***	(0.011)	-0.400***	(0.018)	-0.432***	(0.030)
	Minijob	-0.882***	(0.007)	-0.651***	(0.012)	-0.513***	(0.020)
	Absolute Diff.	0.675***	(0.013)	0.251***	(0.022)	0.082***	(0.036)
	Relative Diff.	-327%		-63%		-19%	
Daily Earnings (incl. 0)	Regular	-0.387***	(0.012)	-0.488***	(0.017)	-0.498***	(0.030)
	Minijob	-0.825***	(0.007)	-0.724***	(0.011)	-0.602***	(0.019)
	Absolute Diff.	0.438***	(0.014)	0.237***	(0.020)	0.103***	(0.036)
	Relative Diff.	-113%		-49%		-21%	
(B) No control variables							
Regular Employment	Regular	-0.175***	(0.009)	-0.347***	(0.012)	-0.306***	(0.012)
	Minijob	-0.881***	(0.006)	-0.627***	(0.009)	-0.399***	(0.009)
	Absolute Diff.	0.706***	(0.011)	0.280***	(0.015)	0.093***	(0.015)
	Relative Diff.	-403%		-81%		-30%	
Daily Earnings (incl. 0)	Regular	-0.337***	(0.010)	-0.395***	(0.012)	-0.294***	(0.012)
	Minijob	-0.820***	(0.006)	-0.693***	(0.008)	-0.498***	(0.009)
	Absolute Diff.	0.482***	(0.012)	0.298***	(0.014)	0.204***	(0.015)
	Relative Diff.	-143%		-76%		-70%	
(C) 1 Nearest Neighbor matching, caliper 0.001							
Regular Employment	Regular	-0.216***	(0.014)	-0.388***	(0.021)	-0.376***	(0.034)
	Minijob	-0.882***	(0.007)	-0.651***	(0.012)	-0.513***	(0.020)
	Absolute Diff.	0.665***	(0.016)	0.263***	(0.024)	0.137***	(0.040)
	Relative Diff.	-307%		-68%		-36%	
Daily Earnings (incl. 0)	Regular	-0.398***	(0.015)	-0.488***	(0.023)	-0.468***	(0.039)
	Minijob	-0.825***	(0.007)	-0.724***	(0.011)	-0.602***	(0.019)
	Absolute Diff.	0.427***	(0.017)	0.236***	(0.025)	0.134***	(0.043)
	Relative Diff.	-107%		-48%		-29%	
(D) Drop bottom and top 5% of matching weights							
Regular Employment	Regular	-0.204***	(0.009)	-0.377***	(0.015)	-0.391***	(0.025)
	Minijob	-0.882***	(0.007)	-0.651***	(0.012)	-0.513***	(0.020)
	Absolute Diff.	0.678***	(0.012)	0.274***	(0.019)	0.122***	(0.032)
	Relative Diff.	-332%		-73%		-31%	
Daily Earnings (incl. 0)	Regular	-0.394***	(0.010)	-0.480***	(0.016)	-0.472***	(0.026)
	Minijob	-0.825***	(0.007)	-0.724***	(0.011)	-0.602***	(0.019)
	Absolute Diff.	0.432***	(0.013)	0.245***	(0.019)	0.129***	(0.032)
	Relative Diff.	-110%		-51%		-27%	
(E) Matching 18 months after birth							
Regular Employment	Regular	-0.155***	(0.010)	-0.378***	(0.018)	-0.383***	(0.029)
	Minijob	-0.925***	(0.006)	-0.658***	(0.012)	-0.515***	(0.021)
	Absolute Diff.	0.770***	(0.012)	0.280***	(0.022)	0.132***	(0.036)
	Relative Diff.	-497%		-74%		-34%	
Daily Earnings (incl. 0)	Regular	-0.355***	(0.011)	-0.460***	(0.019)	-0.448***	(0.032)
	Minijob	-0.841***	(0.008)	-0.729***	(0.011)	-0.606***	(0.019)
	Absolute Diff.	0.486***	(0.014)	0.269***	(0.022)	0.158***	(0.037)
	Relative Diff.	-137%		-58%		-35%	

Table 2 Estimation results (continued)

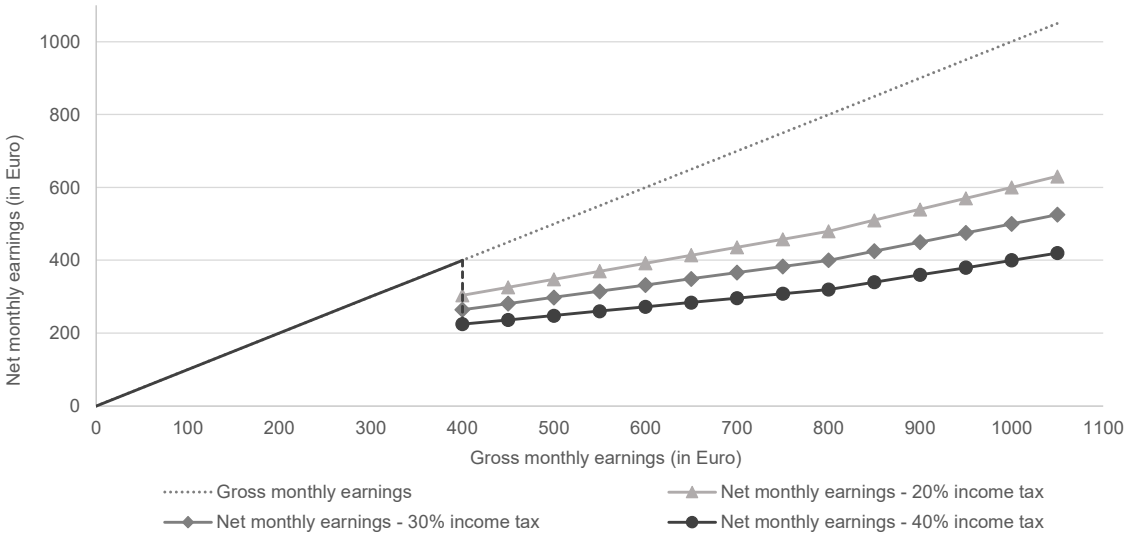
		2 Years after birth		5 Years after birth		10 Years after birth	
		b	se	b	se	b	se
(F) Dropping mothers after identified second birth							
Regular Employment	Regular	-0.142***	(0.009)	-0.244***	(0.017)	-0.287***	(0.034)
	Minijob	-0.883***	(0.007)	-0.606***	(0.013)	-0.524***	(0.023)
	Absolute Diff.	0.741***	(0.011)	0.362***	(0.022)	0.237***	(0.041)
	Relative Diff.	-523%		-148%		-82%	
Daily Earnings (incl. 0)	Regular	-0.384***	(0.010)	-0.366***	(0.019)	-0.370***	(0.037)
	Minijob	-0.827***	(0.007)	-0.695***	(0.012)	-0.595***	(0.022)
	Absolute Diff.	0.443***	(0.012)	0.329***	(0.022)	0.225***	(0.043)
	Relative Diff.	-115%		-90%		-61%	
(G) Births 2007-2012							
Regular Employment	Regular	-0.183***	(0.012)	-0.321***	(0.020)	-	
	Minijob	-0.758***	(0.013)	-0.560***	(0.023)	-	
	Absolute Diff.	0.575***	(0.018)	0.239***	(0.031)	-	
	Relative Diff.	-315%		-75%		-	
Daily Earnings (incl. 0)	Regular	-0.370***	(0.013)	-0.428***	(0.023)	-	
	Minijob	-0.752***	(0.013)	-0.646***	(0.021)	-	
	Absolute Diff.	0.381***	(0.018)	0.217***	(0.032)	-	
	Relative Diff.	-103%		-51%		-	
(H) Alternative sample: Matching with partner characteristics, births 2001-2006							
Regular Employment	Regular	-0.228***	(0.011)	-0.393***	(0.025)	-0.430***	(0.048)
	Minijob	-0.875***	(0.004)	-0.641***	(0.013)	-0.610***	(0.029)
	Absolute Differe	0.647***	(0.012)	0.248***	(0.028)	0.180***	(0.056)
	Relative Diff.	-284%		-63%		-42%	
Daily Earnings (incl. 0)	Regular	-0.425***	(0.013)	-0.512***	(0.024)	-0.576***	(0.043)
	Minijob	-0.817***	(0.005)	-0.714***	(0.010)	-0.714***	(0.024)
	Absolute Differe	0.392***	(0.014)	0.202***	(0.026)	0.138***	(0.049)
	Relative Diff.	-92%		-39%		-24%	

Note: In each panel, the first two rows show the estimated coefficients on selected event time dummies (i.e., 2, 5, and 10 years after birth). Each row comes from a separate linear estimation of equation 1 in the matched samples of mothers who returned to regular employment after birth and those who took up a Minijob after birth, respectively. The third row in each panel shows the absolute difference in the estimates between the two groups. The fourth row in panel A provides the ratio of the absolute difference over the coefficient of regular employment. Panel G is based on an auxiliary dataset (see text for details).
Source: SIAB (1999-2017) and own calculations.

Online Appendix (not for publication)

Appendix A: Additional Figures and Tables

Figure A.1 Tax schedule in the Minijob earnings range (as of 2003)



Note: Own presentation based on actual total social insurance contributions of 20 percent and assumed average tax rates of 20, 30, and 40 percent.

Figure A.2 Propensity score distributions when matching mothers starting out in Minijob and regular employment after birth

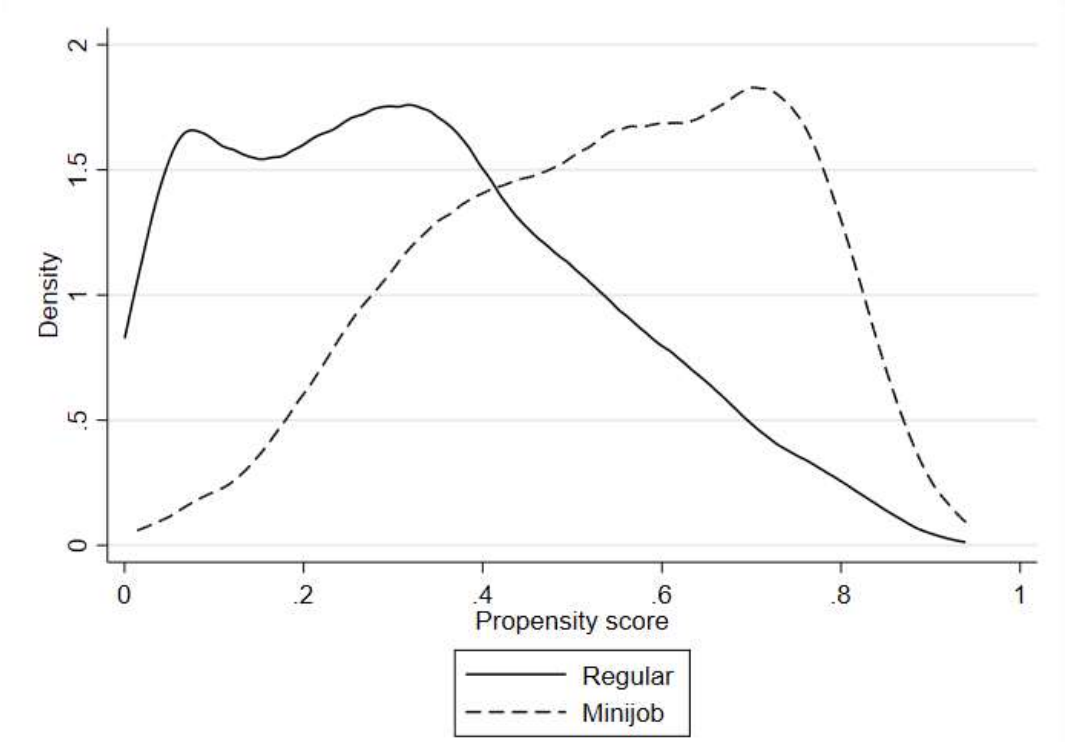
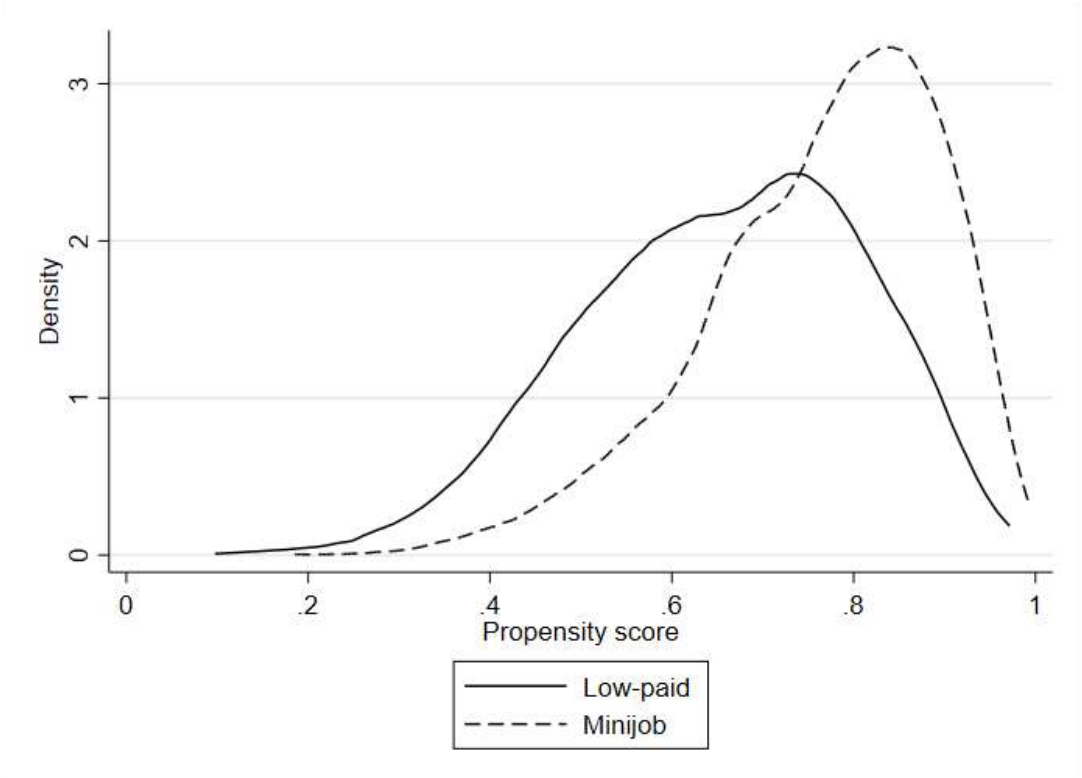
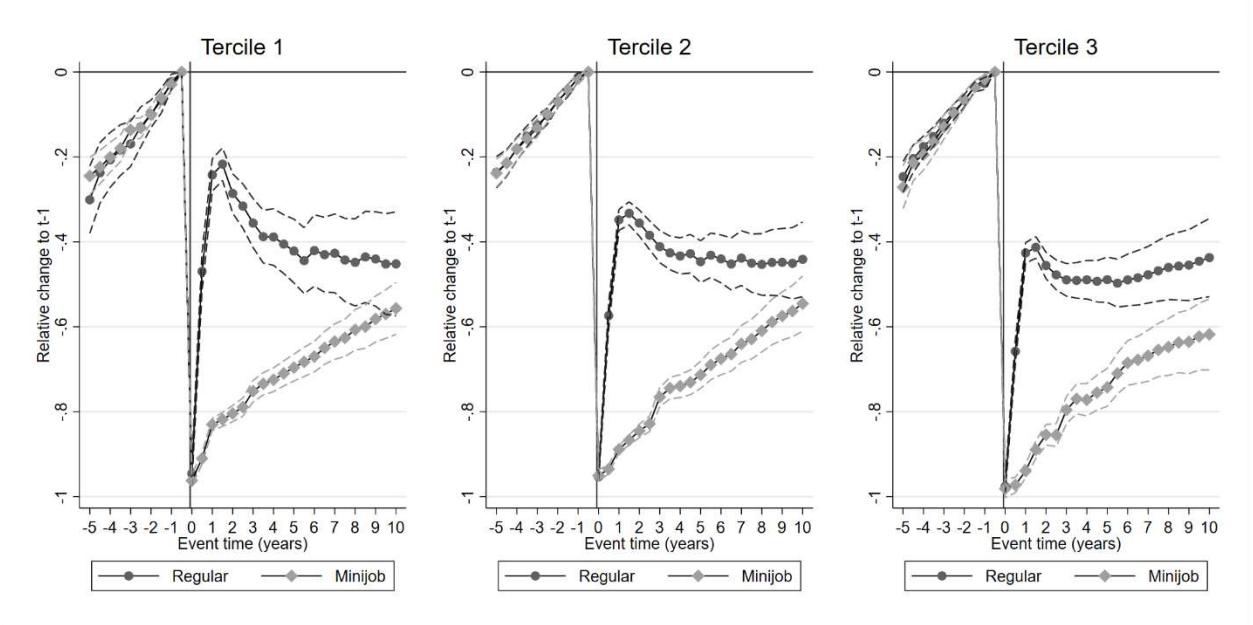


Figure A.3 Propensity score distributions when matching mothers starting out in Minijob and low-paid regular employment after birth



Source: SIAB (1999-2017) and own calculations.

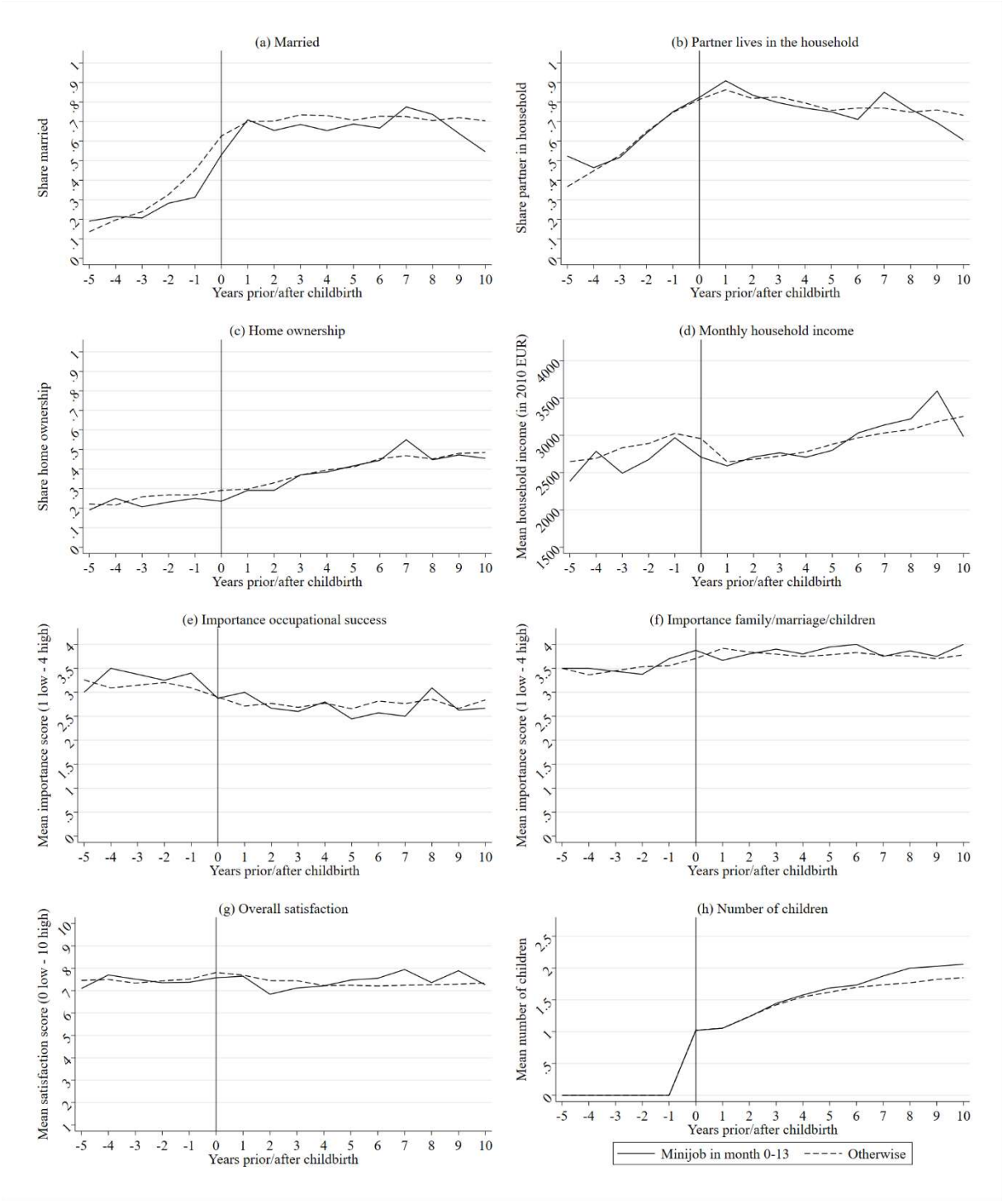
Figure A.4 Baseline results for earnings including zero values by pre-birth earnings (Minijob vs. regular employment)



Note: Terciles are defined based on the earnings distribution 6 months prior to birth. Tercile 1 reflects the lowest and tercile 3 the highest earning mothers. The x-axis is indexed to 0 in the month of birth. The figures show the employment status (top) and pay (bottom) at child's age t relative to the respective outcome 6 months prior to birth. The estimation samples are a result of a matching of the Minijob group to the control group of regularly employed. The figures show 95 percent confidence intervals and cover the period from 60 months prior to 120 months post birth.

Source: SIAB (1999-2017) and own calculations.

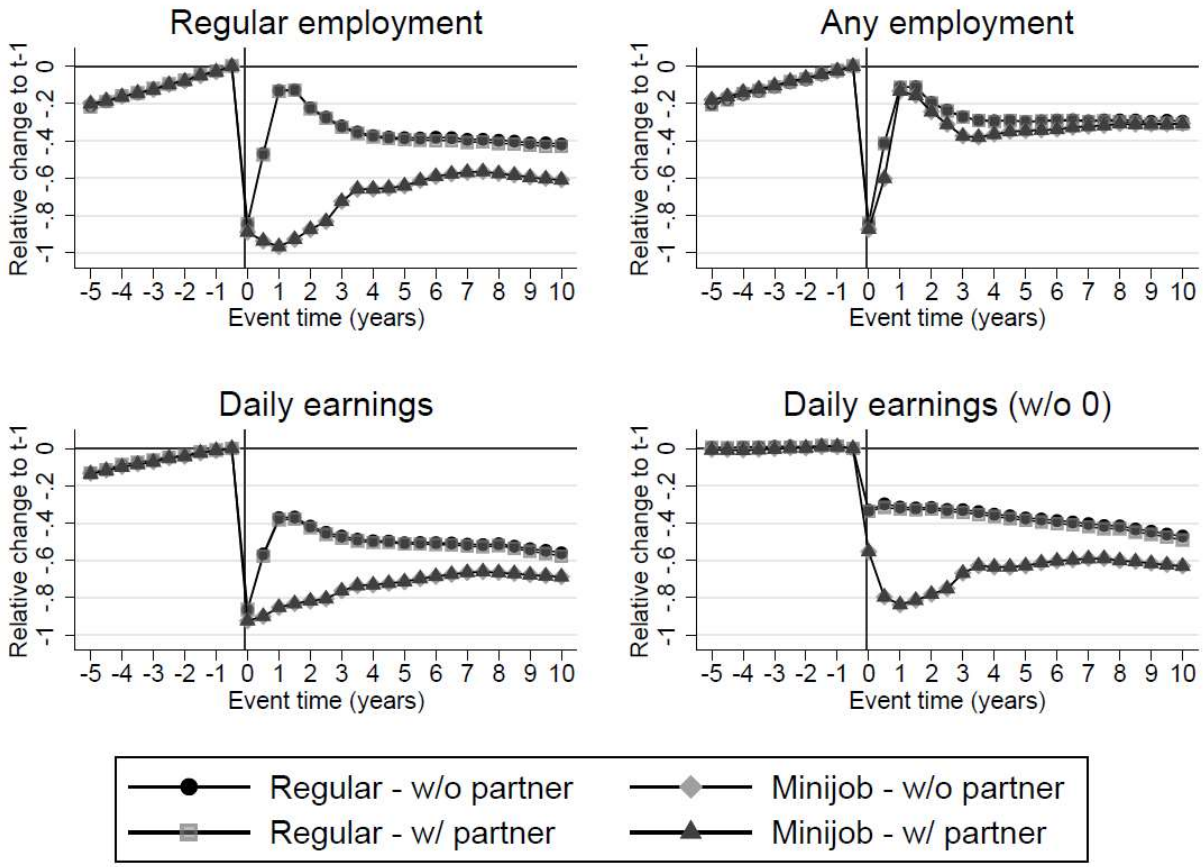
Figure A.5 Household characteristics and attitudes around birth for mothers starting out in Minijob employment after birth and remaining mothers



Note: The sample contains an unbalanced panel of 792 first-time mothers of children born between January 1999 and December of 2006 in West Germany. 55 of these mothers took up a Minijob in months 0-13 after birth. Formal tests do not yield significant differences in means between the groups prior to birth.

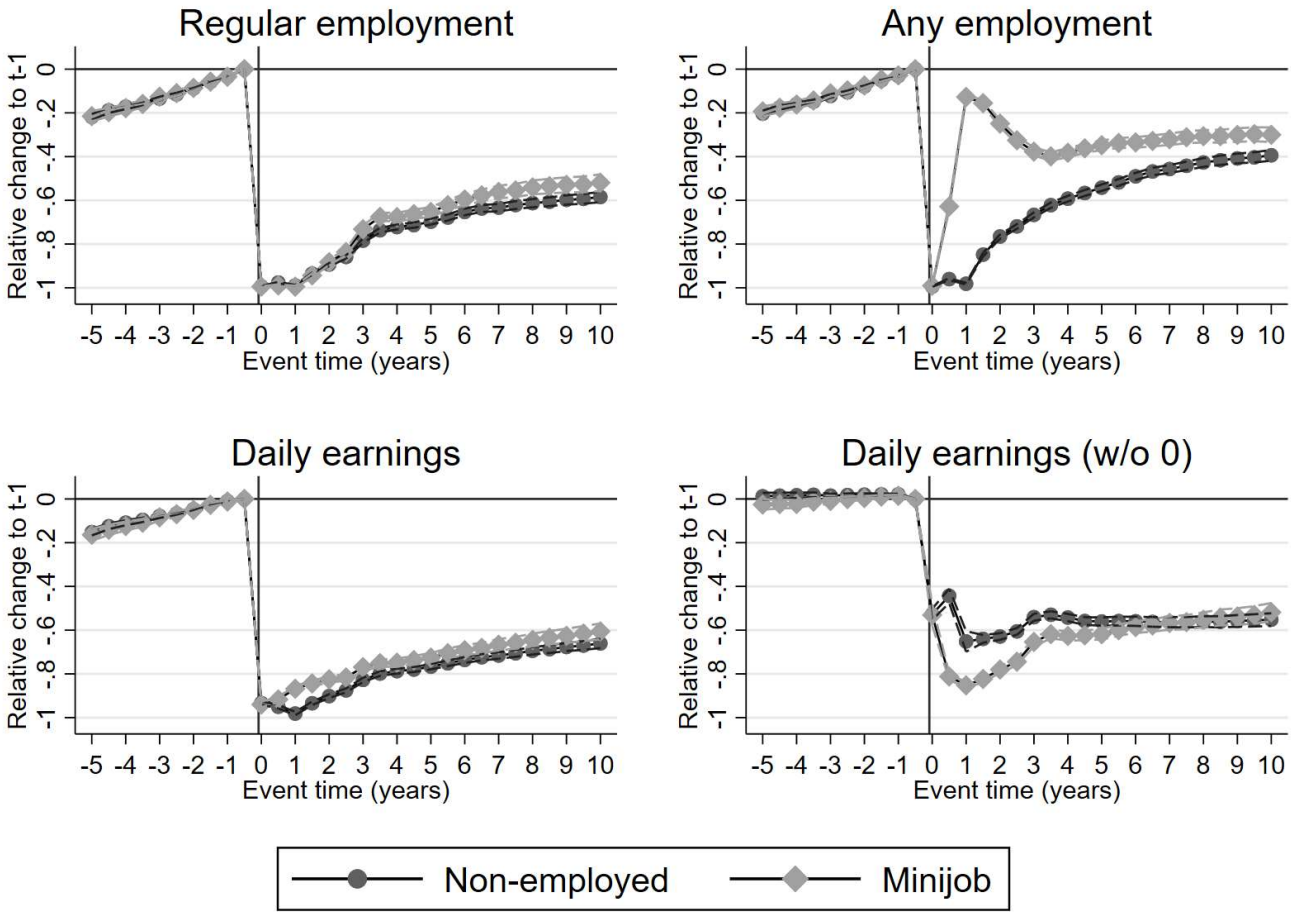
Source: SOEP (1984-2018, v36, 10.5684/soep.core.v36eu).

Figure A.6 Motherhood penalties for mothers starting out in Minijob versus in regular employment after birth (control) for four outcomes - Couple sample with and without controls for partner characteristics in the matching process



Note: The x-axis is indexed to 0 in the month of birth. The figures show the employment status and pay at child's age t relative to the respective outcome 6 months prior to birth. The estimation samples are a result of a matching of the Minijob group to the control group of regularly employed mothers. The figures cover the period from 60 months prior to 120 months post birth.
Source: IEB (2001-2017) and own calculations.

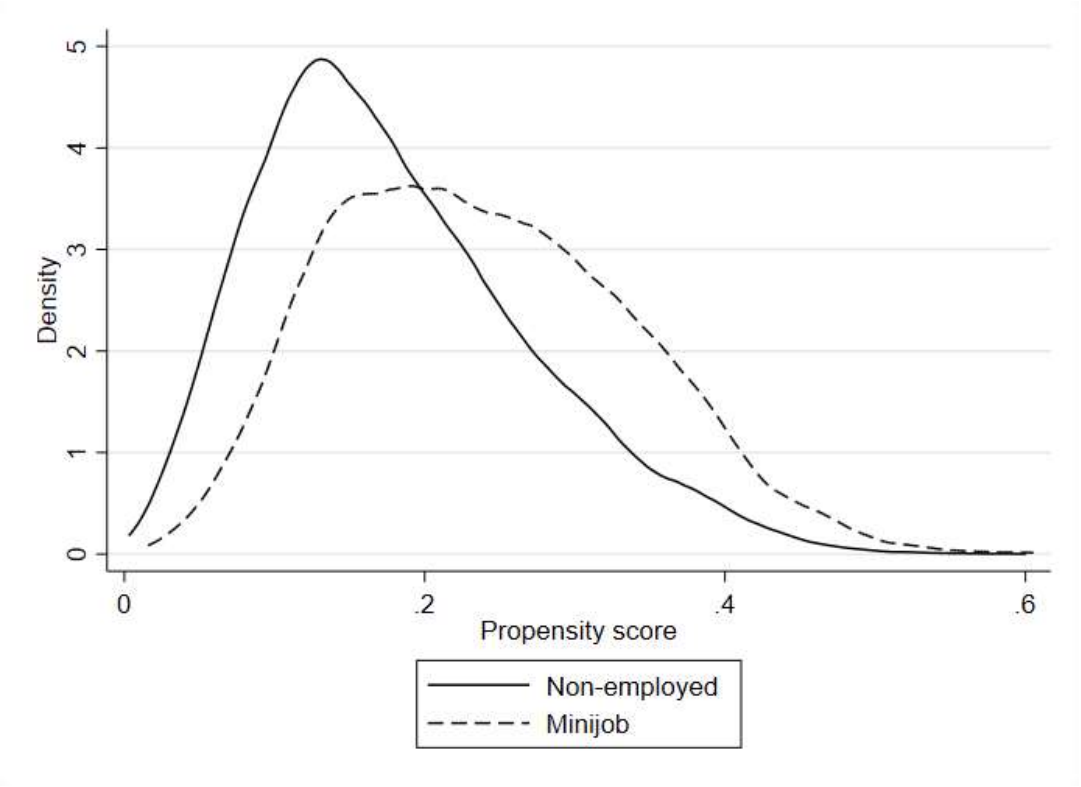
Figure A.7 Motherhood penalties for mothers starting out in Minijob versus those not employed after birth



Note: The x-axis is indexed to 0 in the month of birth. The figures show the employment status and pay at child's age t relative to the respective outcome 6 months prior to birth. The estimation samples are a result of a matching of the Minijob group to the control group of non-employed mothers. The figures show 95 percent confidence intervals and cover the period from 60 months prior to 120 months post birth.

Source: SIAB (1999-2017) and own calculations.

Figure A.8 Propensity score distributions when matching mothers starting out in Minijob and non-employment after birth



Source: SIAB (1999-2017) and own calculations.

Appendix B Testing for the relevance of an additional mechanism

The tax splitting rule for married couples affects the income taxes of secondary earners' (mostly females) and generates heterogeneous net earnings schedules for them. The magnitude of the notch in the net earnings distribution at the Minijob earnings ceiling varies with the first earner's income (see **Figure A.1**): the higher the primary earner's income, the higher the secondary earner's income tax rate at the Minijob earnings ceiling. If secondary earners face a larger notch in their net earnings schedule at the Minijob earnings ceiling, they have a lower incentive to increase their earnings beyond the Minijob earnings ceiling and to extend their labor supply beyond a Minijob by taking up regular unsubsidized employment.

To validate this incentive mechanism, we test whether the Minijob motherhood penalty varies with spousal income. For this purpose, we use the sample of observations with matched partner information (see section 4.4) and group mothers in three terciles depending on the relative earnings of their spouse prior to birth. We expect mothers with high-income husbands to be less likely to leave Minijob employment and take up regular employment. **Figures B.1** and **B.2** present the estimation results for mothers in the regular and low-paid regular control group whose husbands earn in the bottom and in the top terciles of the husband earnings distribution. In both sets of results, we find that the gap in the motherhood penalty for mothers initially employed in Minijobs is larger in the top than in the bottom tercile of husband earnings. This pattern is consistent with the tax incentives trapping mothers in the small job subsidy.

Figure B.1 Motherhood penalties for mothers starting out in Minijob versus in regular employment after birth (control) by partner earnings

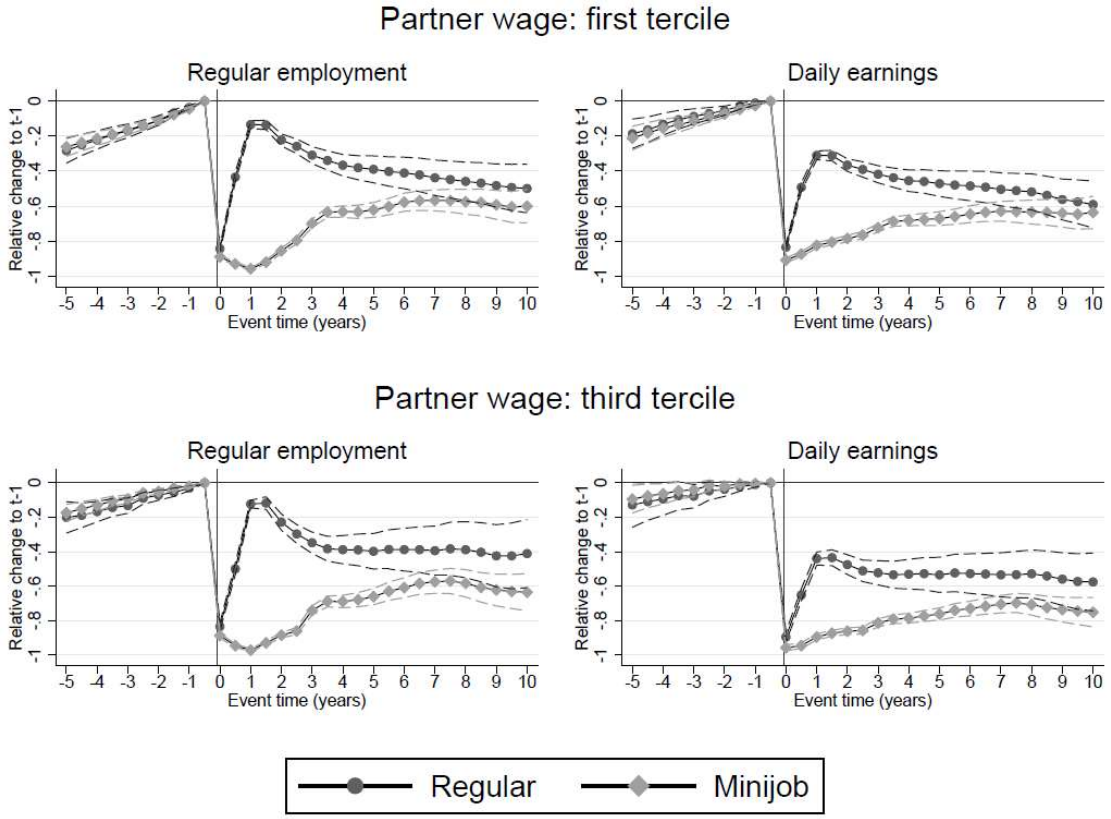


Figure B.2 Motherhood penalties for mothers starting out in Minijob versus in low-paid regular employment after birth (control) by partner earnings