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**Working Paper**

## Subsidized Small Jobs and Maternal Labor Market Outcomes in the Long Run

CESifo Working Paper, No. 11508

**Provided in Cooperation with:**

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

*Suggested Citation:* Collischon, Matthias; Cygan-Rehm, Kamila; Riphahn, Regina T. (2024) : Subsidized Small Jobs and Maternal Labor Market Outcomes in the Long Run, CESifo Working Paper, No. 11508, CESifo GmbH, Munich

This Version is available at:

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# Subsidized Small Jobs and Maternal Labor Market Outcomes in the Long Run

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## **Impressum:**

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

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Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

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# Subsidized Small Jobs and Maternal Labor Market Outcomes in the Long Run

## 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.

JEL-Codes: J220, J130, J180.

Keywords: motherhood penalty, small job subsidies, Minijobs, maternal employment, labor market policy, propensity score matching.

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November 17, 2024

We gratefully acknowledge funding by grant CY82/1-1 of the German Research Foundation. We are grateful for comments by participants of the 2023 Academy of Sociology conference, the annual EALE Conference 2020, the IAB Conference on Labour Market Transitions: Challenges for Public Policies and Research 2020, the 2021 conference of the Society of Labor Economists, the 2021 ESPE and EEA-ESEM conferences, and seminars at ZEW Mannheim, Free University Berlin, and Leibniz University Hannover.

## 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; contributions in the sociological literature on the 'welfare state paradox' even argue that family-friendly policies might be determinants of maternal disadvantages (Mandel and Semyonov 2006 or Pettit and Hook 2009). 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 in 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.<sup>1</sup> 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.<sup>2</sup>

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<sup>1</sup> 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 4.3), 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.

<sup>2</sup> 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.

Eventually, we compare the development of long-run labor market outcomes of the matched groups using an event-study approach.

We confirm staggering and persistent motherhood penalties for 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. At the same time, the differences between the Minijobbers and the group of non-employed mothers are generally much smaller, and 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 four contributions to several literatures. First, we add to the broad literature studying various explanations for persistent motherhood penalties. Extensive research focuses on the relevance of 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.<sup>3</sup>

Second, our study is most closely related to the literature evaluating the effects of governmental policies on maternal labor market outcomes.<sup>4</sup> 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, especially, if they affect the “experience capital” accumulated in work over time (Costa Dias et al. 2020).

Third, our findings add a new perspective to the discussion of the welfare-state-paradox and the trade-off implicit in the promotion of gender equality by progressive welfare states: Mandel and Semyonov (2005, 2006) pointed out that developed welfare states with generous family policies facilitate women's access to the labor force. Yet, exactly these policies generate

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<sup>3</sup> 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.

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



occupational segregation and restricted opportunities for women. Gangl and Ziefle (2009, 2015) showed that German mothers benefit from welfare state programs and then take more time off, enter part-time jobs, and pursue typically female jobs. They also argue that employers pass the economic cost of family policy on mothers through statistical discrimination. Some more recent contributions are skeptical regarding the welfare-state-paradox hypothesis: Brady et al. (2020) investigate the relationship between family policies and labor market outcomes across 21 countries and find no support for the tradeoffs. Similarly, Mun and Jung (2018) exploit family policy reforms in Japan to examine whether employers respond by increasing discrimination against female workers. The authors do not find evidence for such a response and conclude that if a welfare-state-paradox exists, it must be explained by labor supply mechanisms. Our analysis contributes to these discussions and broadens their perspective by studying a labor market policy that is not targeted at mothers or parents. Just as family policies can have unintended side effects, so can other public policies designed to support employees in small jobs. It is important to scrutinize the full range of labor market policies to understand the determinants of gender-based labor market inequalities.

Fourth, 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 and wage growth in the long run.<sup>5</sup>

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

Our findings are of broad interest for three reasons. First, 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.<sup>6</sup> Messenger and Wallot (2015) show in detailed case studies that in several countries (Australia, Austria, Germany, Ireland, The Netherlands, UK), more than 10 percent of women in dependent employment work in such non-standard arrangements with low working hours and poor job characteristics. The setting is attractive for employers to meet fluctuating demand while the impact on workers can be adverse. Thus, our results are relevant for various national labor market settings. Second, we point to the importance of scrutinizing labor market and fiscal policies to better understand their gendered relevance. There might be relevant tradeoffs to consider between supporting small-job employees and avoiding gender-based labor market effects. Finally, numerous countries promote part-time employment for recent mothers without attention to the long-run effects of such policies (e.g., Joseph et al. 2013, De Quinto and Gonzalez 2024, and Bärtsch and Sandner 2024).

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**

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<sup>6</sup> 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.

The Minijob program is one of the largest labor market programs in Germany and subsidizes small jobs, i.e., employments with earnings below the Minijob earnings limit. 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, 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<sup>7</sup>. 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

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

market.<sup>8</sup> 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  $T(\cdot)$  yields that  $2 \cdot T(0.5 \cdot y_C) \leq T(y_M) + T(y_F)$ . 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.<sup>9</sup> 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. These tax incentives apply to the vast majority of mothers, as during the period we study, approximately 80% of births in Germany occurred within marriage (DESTATIS 2020).

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<sup>8</sup> 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).

<sup>9</sup> 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.

## 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.<sup>10</sup> 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 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.<sup>11</sup> 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.<sup>12</sup> 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.

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<sup>10</sup> Alternatively, families could choose payments of 450 Euros for up to 12 months.

<sup>11</sup> 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.

<sup>12</sup> 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).

Parental leave regulations changed substantially in 2007; the means test was abolished, payouts could reach higher amounts, daddy-months were added and the typical payout period was 14 months (for details, see, Bergemann and Riphahn 2023, Frodermann et al. 2023). To ensure a homogenous institutional setting for our analysis, we focus on births before this reform. Nevertheless, we show in the online Appendix C that our main conclusions hold for births after the reform.

## 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, 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.<sup>13</sup> In particular, our main analysis compares mothers in subsidized Minijob employment and two alternative control groups: all regularly (full-time or part-time) employed mothers and those in low-paying regular employment (lowest earnings quartile in month 13). In section 4.4 we also consider mothers who initially do not participate in the labor market as a further comparison group. We view the first two comparison groups as more appropriate because both mothers in Minijobs and mothers in (low-paid) regular employment decide in favor of participation in the first place whereas mothers in non-employment decide against it. Thus, given the shared choice at the extensive margin, we believe that Minijobbers and the regularly employed mothers are likely to share (unobserved) attitudes toward labor market participation and career preferences than mothers who initially stay out of employment.

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

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<sup>13</sup> 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 4.3, we offer a robustness test with respect to an alternative choice of the cutoff of 13 months after birth.

experience, Minijob experience (all measured in years<sup>14</sup>), education (5 indicators), establishment size (7 indicators), and federal state main effects and interacted with dummies for a child's 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 4.3 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 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

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<sup>14</sup> 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.



before and 10 years after birth. 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, calendar-time, and age fixed effects. Such a specification non-parametrically captures the underlying life-cycle patterns and developments in outcomes over time such as wage inflation, business cycles, or institutional changes. The approach provides consistent estimates under the main identifying assumption that conditional on covariates, the event, i.e., the first birth, is not determined by the outcome variables (smoothness around the event). Intuitively, it implies that conditional on maternal age and calendar time, first birth generates changes in the outcomes that are arguably orthogonal to their unobserved determinants. Kleven et al. (2019b, Appendix B) caution that for a long event time horizon, the estimated child penalties might be susceptible to unobserved changes in non-child-related earnings components over time, which requires stronger assumptions than smoothness. Nevertheless, they argue that allowing for non-parametric age and calendar time controls (as we do in our main model specification) alleviates this concern. They provide further validity checks that strongly support the plausibility of the identifying assumptions.

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  $Y_{ist}$  is the outcome of interest for mother  $i$  from group  $g$  at calendar time  $s$  and at event time  $t$ , which we measure in 6-month steps. Thus,  $t$  runs in 30 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 ( $\alpha_j$ ) 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 point estimates for the post-event periods flexibly capture the average treatment effect of the birth over time net of age and time-specific effects given that we include a full set of calendar-time ( $s$ ) indicators in 6-month steps and dummies for maternal age at calendar time  $s$  (or event time  $t$ ). The effect of interest is dynamic but assumed to be homogenous across observations (Borusyak and Jaravel 2018, Freyaldenhoven et al. 2019, Schmidheiny and Siegloch 2019). 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. However, regular employment with low earnings captures mostly part-time employment and can thus serve as a proxy. 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  $\alpha_t$  over the predicted outcome at time  $t$  when omitting the contribution of the event indicator.<sup>15</sup> 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

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<sup>15</sup> The value in the denominator is calculated by first predicting the outcome for each panel observation, then deducting the coefficient  $\alpha_t$  and, finally, averaging across the observations in the relevant group and event time cell.

We use administrative data from the Sample of Integrated Labour Market Biographies (SIAB) (Antoni et al., 2019).<sup>16</sup> 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. Apart from the large sample size, the main advantage of the SIAB over self-reported survey data is the likely negligible measurement error in the employment and earnings information and the availability of detailed labor market biographies, which we exploit in our matching procedure.

As our data report Minijob employment only since 1999, we consider West German mothers who gave birth between 1999 and 2006.<sup>17</sup> 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.<sup>18</sup> 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

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<sup>16</sup> 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

<sup>17</sup> 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). When observations of East German mothers are added to the sample the results are robust.

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

highest earnings.<sup>19</sup> 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.<sup>20</sup>

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 split all mothers into comparison groups (Minijobbers versus alternative control groups). The results are robust to alternative choices of the point of time after birth determining the treatment status (see, Section 4.3, for a robustness check). 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).

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.<sup>21</sup> **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

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<sup>19</sup> In a robustness test we validated that using the total earnings from all employment spells in a given period yields the same results.

<sup>20</sup> 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).

<sup>21</sup> 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.

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 4.4. 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 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 focus on married couples and 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.<sup>22</sup> Following Kleven et

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<sup>22</sup> Our data do not provide information on net earnings. However, by focusing on gross earnings, we follow related literature (e.g., Kleven, Landais, Posch, Steinhauer, and Zweimüller

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.<sup>23</sup> 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 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.

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2019, Kleven, Landais, and Sogaard 2019, Angelov, Johansson, and Lindahl 2016). Moreover, the interpretability of results for net earnings would be limited because the applicable income tax rates and relevant social insurance contributions in Germany are determined by a broad set of issues such as the number of children, place of residence, partner tax code choices, and incomes from other sources. It is also the gross earnings that determine future pension claims.

<sup>23</sup> Prior to month 6 before birth, some women were not (yet) in full-time employment, which is why the share increases over time.

**Table 1** describes the relevant subsamples and the matching quality. Overall, when we compare the pre-birth characteristics across the samples, mothers who took up Minijobs after birth are 'negatively' selected in terms of, for example, their younger age at first birth, lower pre-birth earnings, lower probability of having higher education, and a smaller pre-birth establishment size compared to our two main control groups (regular employees, low-paid regular employees) and the non-employed mothers. On the other hand, the Minijobbers seem to be 'positively' selected in terms of having a longer tenure with their pre-birth employer, less unemployment experience, and higher rates of vocational training compared to the other groups. The comparison across all characteristics included in **Table 1** points to similar differences across all control groups in the raw data. If anything, most of the highly significant differences in the unbalanced samples are concentrated in the last column (i.e., for the comparison with mothers who initially stay non-employed after birth). Importantly, all of the significant differences disappear after we apply the propensity score matching. In section 4.3, 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.6**). In each of the comparison groups, our matching procedure finds appropriate matching partners for nearly all Minijob observations. **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 51.3 percent. The difference between the two groups after 10 years is thus 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 of being 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, including past earnings and employment status, we observe identical developments for the Minijobbers and the control group until childbirth. Thus, the two groups have overlapping pre-trends by construction. 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.<sup>24</sup> 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

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<sup>24</sup> The control group status is assigned based on activities in month 13 after birth whereas **Figure 4.1** 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 4.1**.



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 employment. The results are similar to those in **Figure 4.1**: mothers starting out in low-paid regular employment after birth have a significantly 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 calendar time 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. In the right panel, we consider mothers who started out with a low-paid regular job after birth as a more conservative comparison group. Although their penalties in earnings significantly differ from those of Minijobbers only up to six years after birth (as shown in **Figure 4.2**), these mid-term differences still add up to a substantial loss in cumulative earnings for the Minijobbers over time as illustrated in **Figure 5**.

Overall, we find that taking up Minijob employment after birth causes long-run disadvantages in earnings. 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. Unfortunately, we cannot determine the separate relevance of changes in hours worked vs. hourly wages as our data do not include this information.

## **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 Appendix **Figures A.4.1** and **A.4.2** 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 A.4.1** 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.<sup>25</sup> This disadvantage emerges shortly after birth and increases over time. **Figure A.4.2** 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.<sup>26</sup>

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 these issues in **Figures A.5.1** and **A.5.2** using event study designs.

The outcome in **Figure A.5.1** 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

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<sup>25</sup> Given the low incidence of unskilled jobs in our data, in **Figure A.4.1** we extend the earnings threshold for the definition of low-paid regular employment from the bottom quartile to 40<sup>th</sup> 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 A.4.1**. For comparability, we also use the 40<sup>th</sup> percentile when defining low-paid employment in **Figure A.4.2** on experience in complex jobs. Nevertheless, **Figure A.4.2** looks nearly identical if we use the 25<sup>th</sup> percentile, which makes us confident that the sensitivity of the results for unskilled jobs (**Figure A.4.1**) is solely due to limited outcome variation in the data.

<sup>26</sup> Heterogeneity analyses yield that mothers with higher pre-birth tenure and thus potentially higher firm-specific human capital suffer more from taking up Minijob vs. regular employment than mothers with low pre-birth tenure.

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 A.5.2** 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.

In addition, due to tax incentives, the Minijob subsidies can have more severe consequences for mothers facing high-income tax rates once they exceed the Minijob earnings threshold. In **Appendix B**, we take advantage of additional data (described in detail in section 4.3) to test for such heterogeneous responses based on an interaction between the income tax regime and the Minijob subsidy. We find that 10 years after the birth the motherhood penalties in employment and earnings for mothers initially employed in Minijobs are larger in the top than in the bottom tercile of husband earnings. This is consistent with the tax incentives trapping especially mothers with high-income partners in the Minijob subsidy.

### 4.3 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**.<sup>27</sup>

Panel B 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.<sup>28</sup> In panel C, 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 D, 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 E, 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.<sup>29</sup> In panel F, 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

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<sup>27</sup> **Table A.1** in the appendix presents the robustness tests when mothers in Minijobs are compared to the group of mothers low-paid regular employments.

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

<sup>29</sup> 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.6** (panel h).

penalties with Minijob vs. regular employment are confirmed in this sample as well; we exploit newer data on births in the years 2007-2011 in Appendix C which confirms these results. 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 marital status, 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.6** shows that this is not the case.<sup>30</sup> 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 available dataset that links the social security records of married

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<sup>30</sup> Given the small sample size, we use an unbalanced panel for **Figure A.6** 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.

couples. The identification of married partners is based on shared addresses and last names and conditional on 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 main data and thus re-run our analysis using the sample of married couples instead. The data start in 2001. Otherwise, we apply similar sample restrictions as in our main analysis. The alternative sample allows us to additionally match on partner characteristics.<sup>31</sup> Panel G in **Table 2** shows the results. Despite some differences in the sample composition, the estimates confirm our baseline results and yield even larger Minijob penalties after ten years. We also carefully investigate whether the estimation results differ with and without controls for partner characteristics in the matching procedure. **Figure A.7** 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.<sup>32</sup>

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 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

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<sup>31</sup> 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.

<sup>32</sup> 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).

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.

#### **4.4 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.8** in the Appendix depicts the baseline results for this control group.<sup>33</sup> We find that over time, Minijob employees are statistically significantly more likely to subsequently take up regular employment than mothers who are not employed in month 13 after birth (top panels). However, compared to our main results the effects are much smaller in magnitude. 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 of the labor force for longer than Minijobbers, they do not suffer significantly larger earnings losses. These results remain robust in alternative specifications and sample restrictions (see **Table A.2** which follows the robustness tests described in section 4.3).

### **5. Conclusions**

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

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<sup>33</sup> **Figure A.9** shows the distribution of the corresponding propensity scores.



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. While a broad literature discusses the potential unintended consequences of progressive welfare state policies on maternal labor market outcomes (Brady et al. 2020 and sources cited therein), this discussion has not been extended beyond family policies. Surprisingly, so far, there is also 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. Even if we use only low-paid regularly employed mothers as a more conservative comparison group we find that mothers in Minijob employment face an adverse impact in the early years of motherhood. 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. Since pension claims are determined based on lifetime earnings, this has considerable implications even after retirement. 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 labor market experience in jobs that are less valuable in terms of skill requirements compared to those who return to regular employment after birth. In the early years after childbirth, Minijobbers have also significantly more volatile employment histories because they more frequently switch employers. However, in the long run, this difference is no longer statistically significant. In

addition to mechanisms operating through reduced hourly wages, reduced daily earnings may also be partly due to reductions in the number of hours worked, which we cannot investigate.

A remaining question is whether the long-term effects of Minijob employment after childbirth are determined by labor supply or labor demand factors. On the one hand, tax incentives can lead to the 'Minijob trap' that keeps working mothers in Minijob employment for long periods. On the other hand, there is strong evidence of the role of labor demand in determining responses to Minijob regulations. For example, Tazhitdinova (2020) exploits the large notch generated by Minijobs to study labor supply elasticities. Her findings of suboptimal labor supply adjustments in response to Minijob tax incentives point to the importance of labor demand. Similarly, Gudgeon and Trenkle (2023) study the speed of Minijob earnings responses to changes in the Minijob earnings threshold. Focusing on married women in West Germany, they find substantial delays in the adjustment of labor supply to tax changes and argue that labor demand plays a quantitatively meaningful role in this adjustment process. In contrast to these two contributions, we evaluate the long-run effects of labor market choices after birth. While labor demand may delay aggregate responses to Minijob reforms, it seems less plausible that the 'Minijob trap' is due to frictions imposed by employer behavior.

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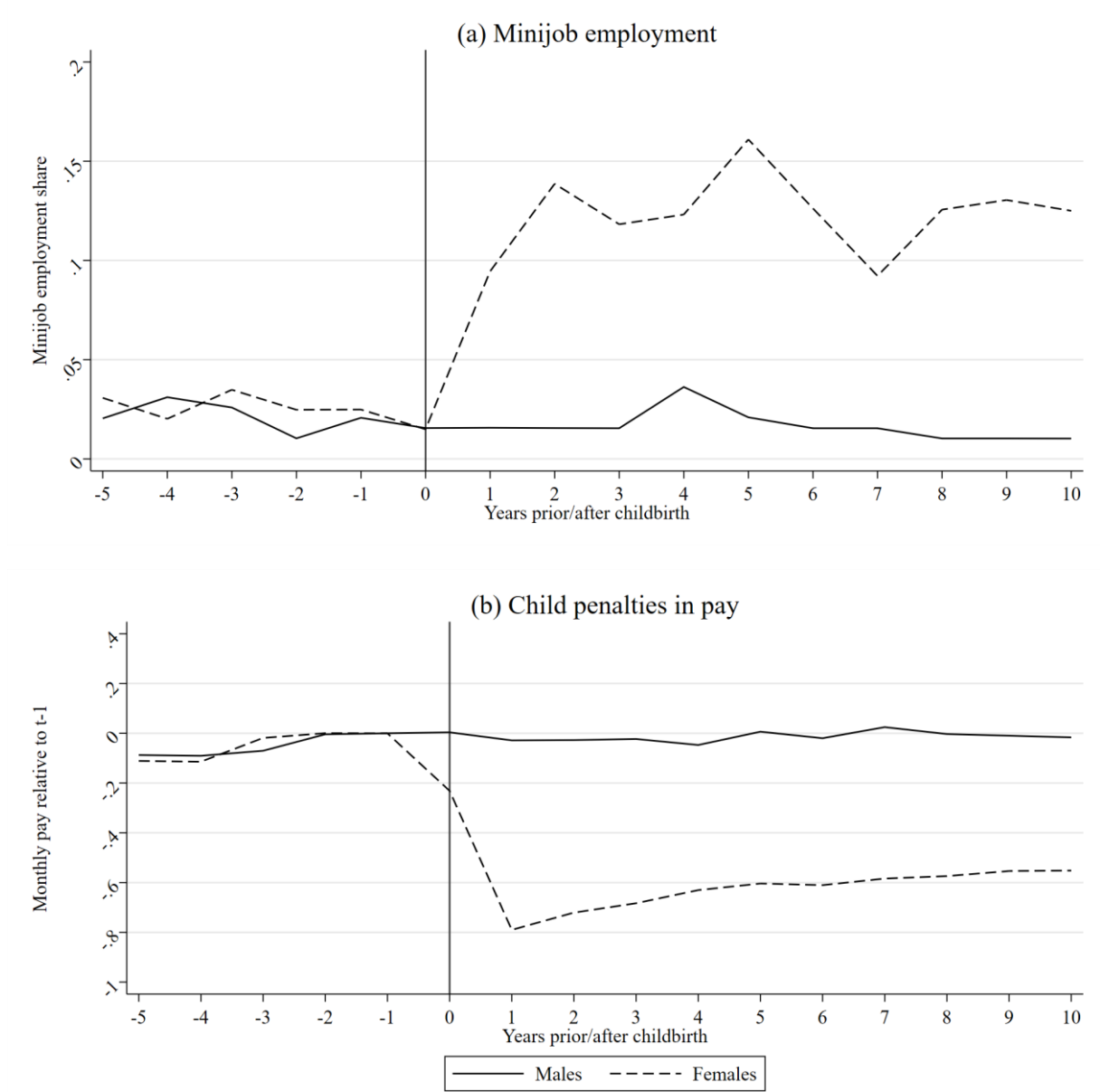
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**Figure 1** Average Minijob employment around birth and the motherhood earnings penalty

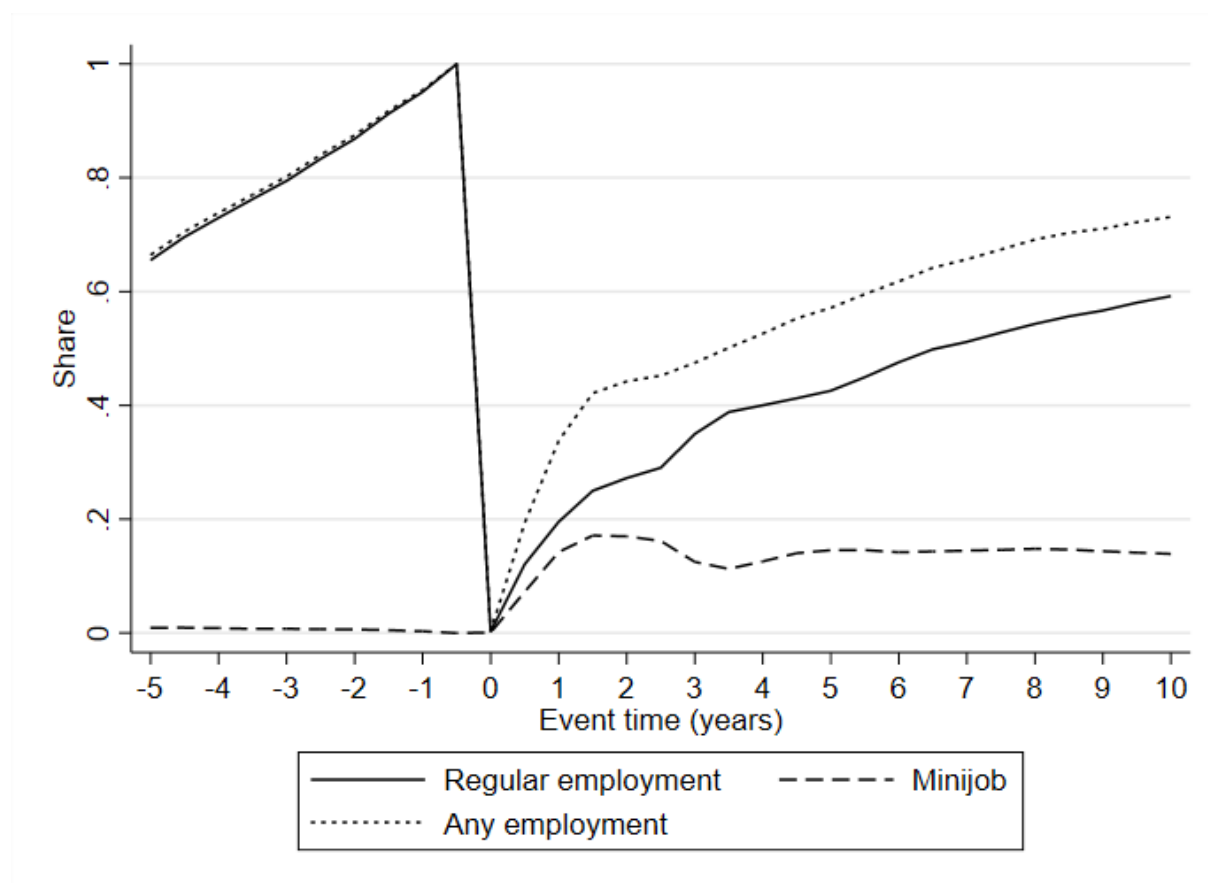


**Note:** The Minijob employment share in panel (a) describes the share of Minijob employees among all mothers and fathers. Panel (b) shows the estimated child penalties using the methodology by Kleven et al. (2019b). Thus, it replicates the results for Germany presented in Figure 3 in Kleven et al.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



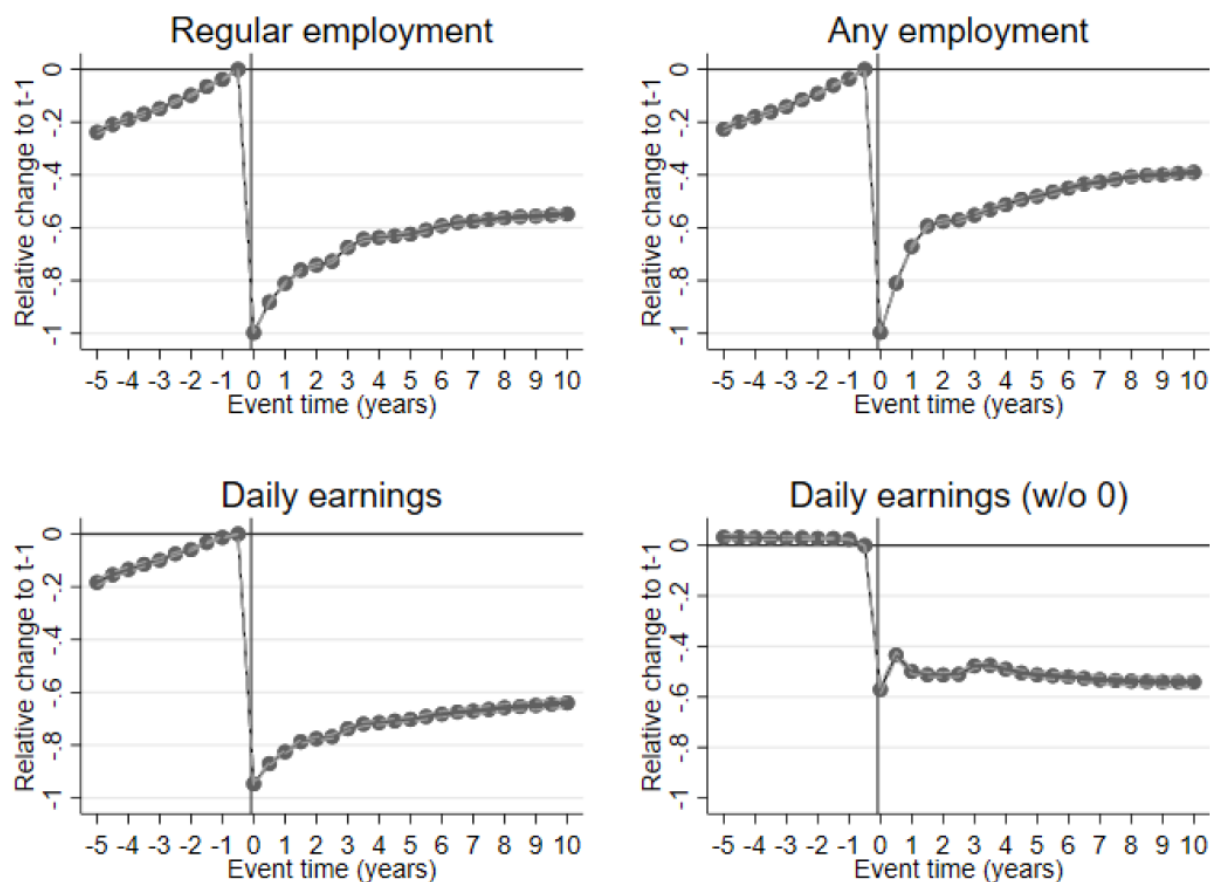
**Figure 2** Development of average labor force status of sampled mothers over time



**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.

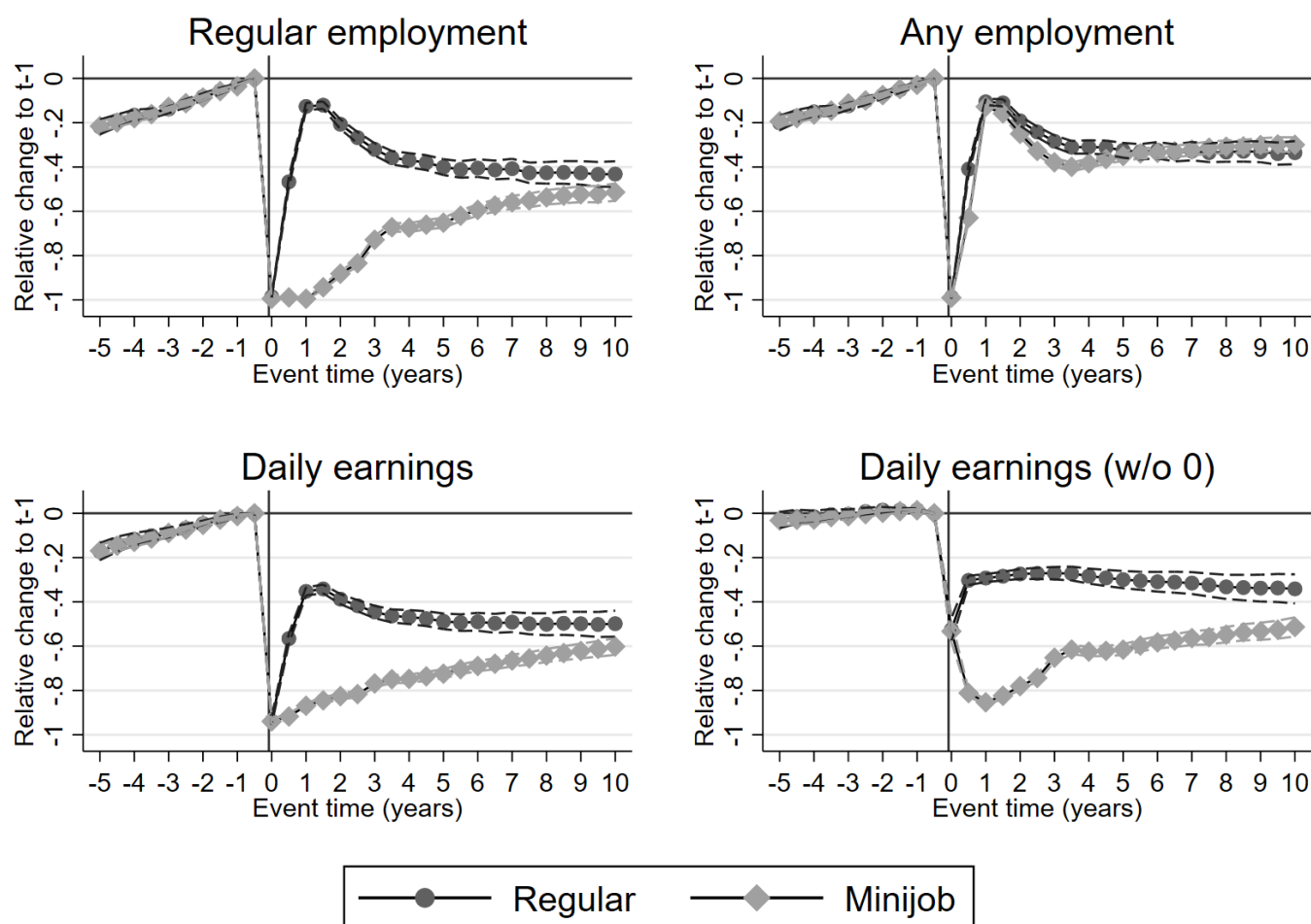
**Figure 3** Development of outcome measures for the full sample



**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.

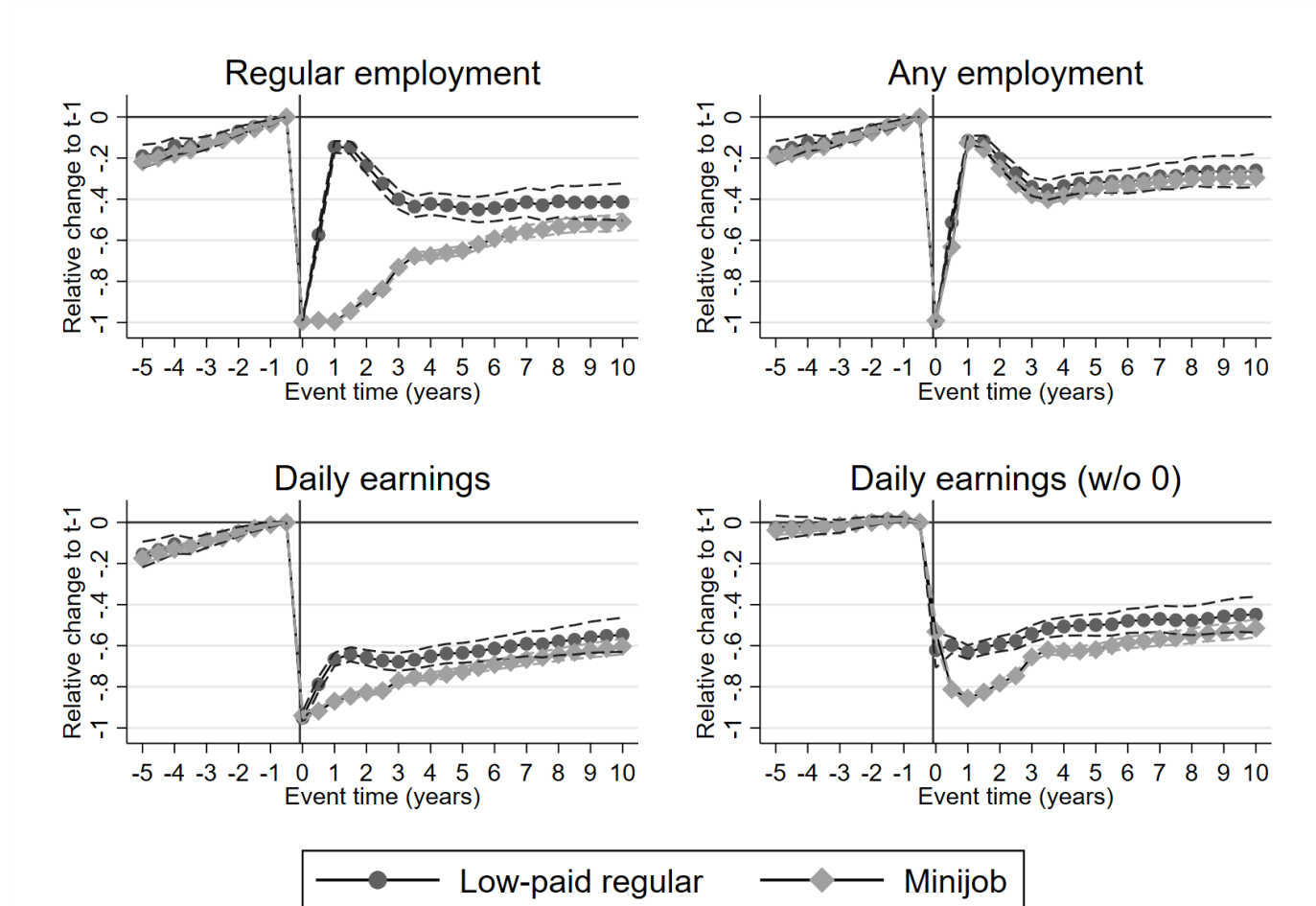
**Figure 4.1** Motherhood penalties for mothers starting out in Minijob versus in regular employment after birth (control) for four outcomes



**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.

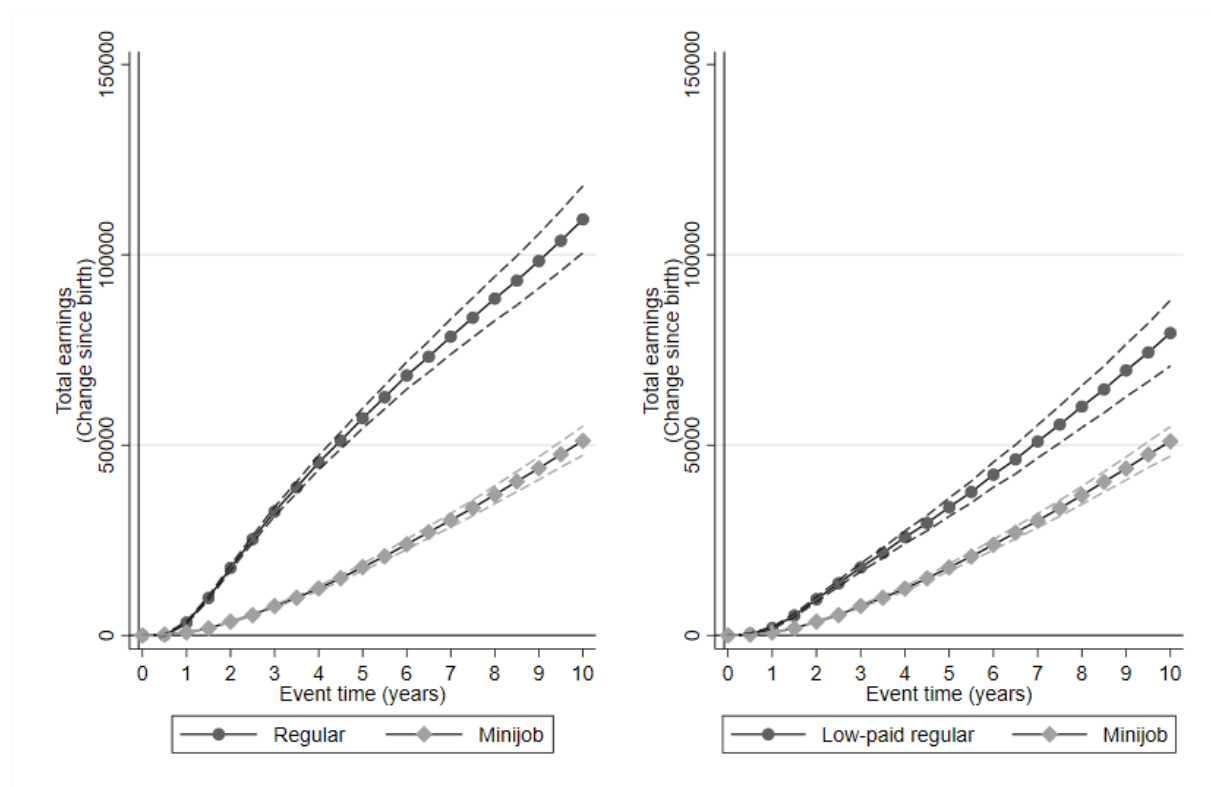
**Figure 4.2** Motherhood penalties for mothers starting out in Minijob versus low-paid regular employment after birth (control) for four outcomes



**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.

Table 1		Balancing			of			covariates			
			Minijob Mean	Low-paid regular Mean	Diff	Minijob Mean	Regular employee Mean	Diff	Minijob Mean	Not employed 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
<b>(A) Baseline</b>							
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>(B) 1 Nearest Neighbor matching, caliper 0.001</b>							
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%	
<b>(C) Drop bottom and top 5% of matching weights</b>							
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%	
<b>(D) Matching 18 months after birth</b>							
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
<b>(E) Dropping mothers after identified second birth</b>							
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%	
<b>(F) Births 2007-2012</b>							
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%		-	
<b>(G) Alternative sample: Matching with partner characteristics, births 2001-2006</b>							
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 Difference	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 Difference	0.392***	(0.014)	0.202***	(0.026)	0.138***	(0.049)
	Relative Diff.	-92%		-39%		-24%	

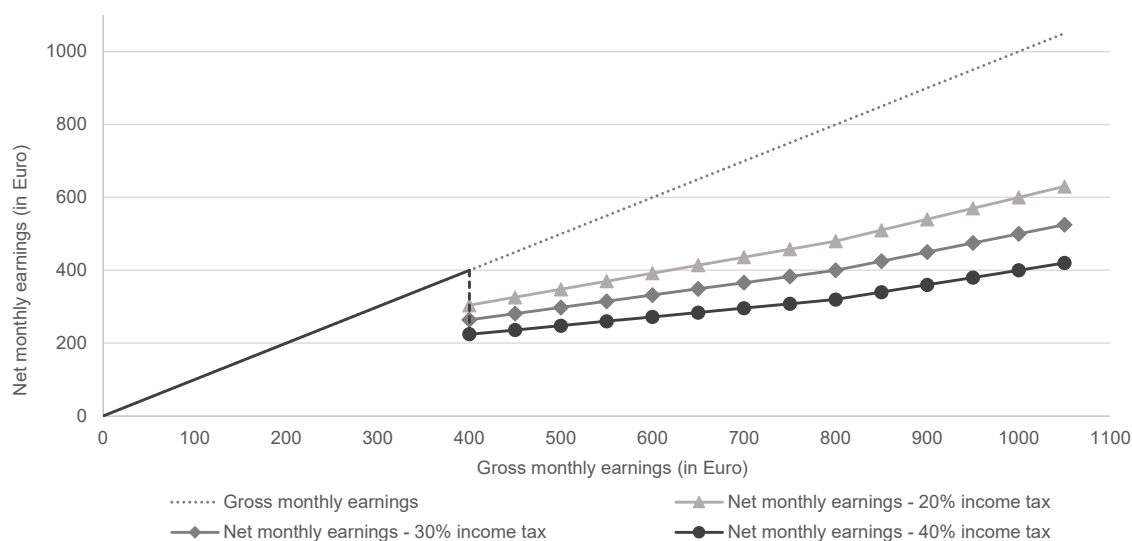
**Note:** In each panel, the first two rows show the estimated coefficients on selected event time dummies (i.e., 2, 5, and 10 years after birth). Each row comes from a separate linear estimation of equation 1 in the matched samples of mothers who returned to regular employment after birth and those 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 F is based on an auxiliary dataset (see text for details).

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

## **Appendix**

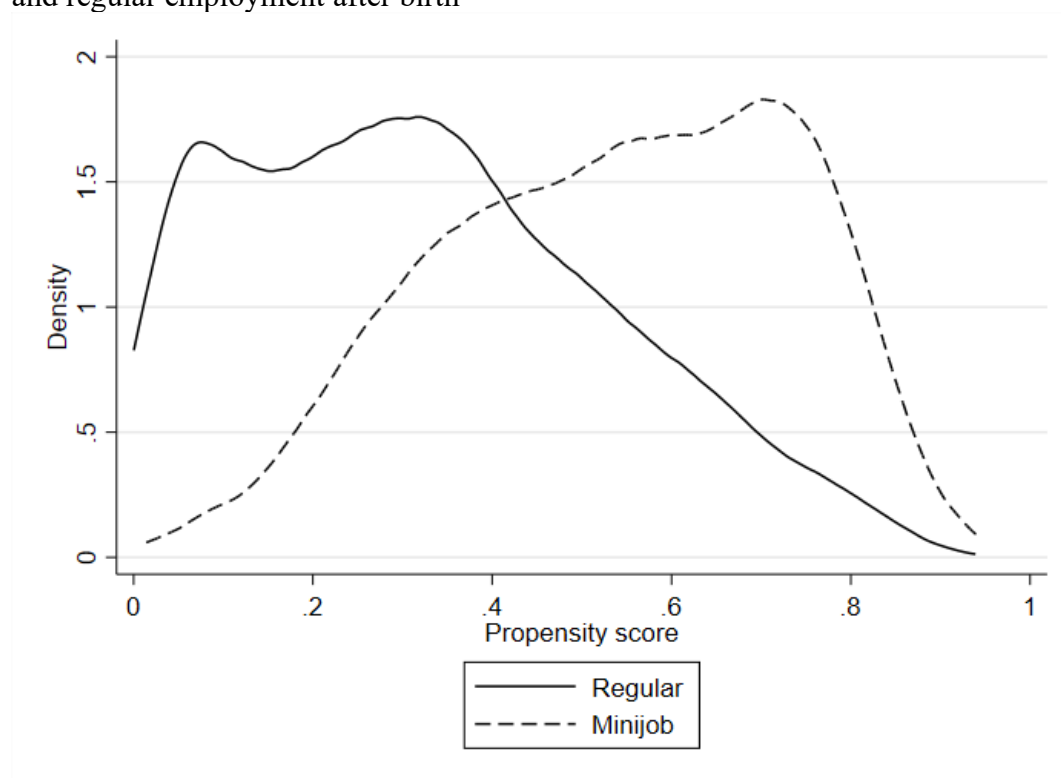
## Appendix A: Additional Figures and Tables

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

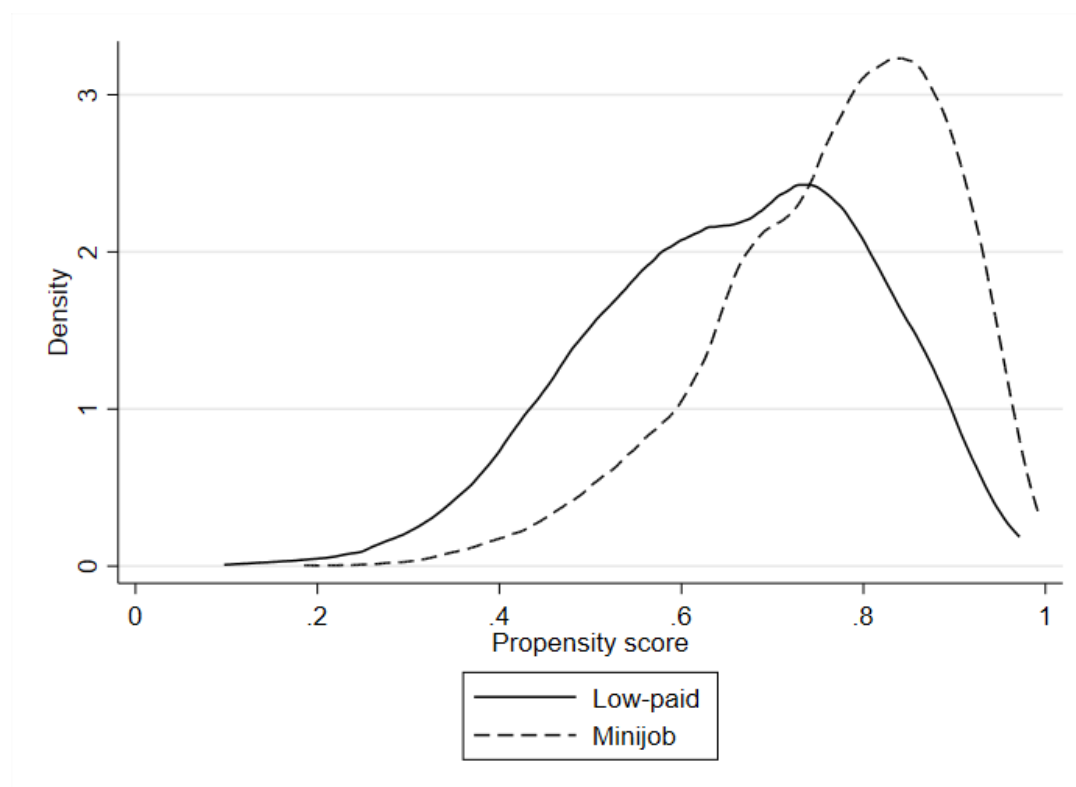


**Note:** Own presentation based on actual total social insurance contributions of 20 percent and assumed average tax rates of 20, 30, and 40 percent. In the gross earnings range between 400 and 800 Euros social insurance contributions which are deducted from gross earnings in addition to income taxes are subsidized on a sliding scale, i.e., at declining rates.

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

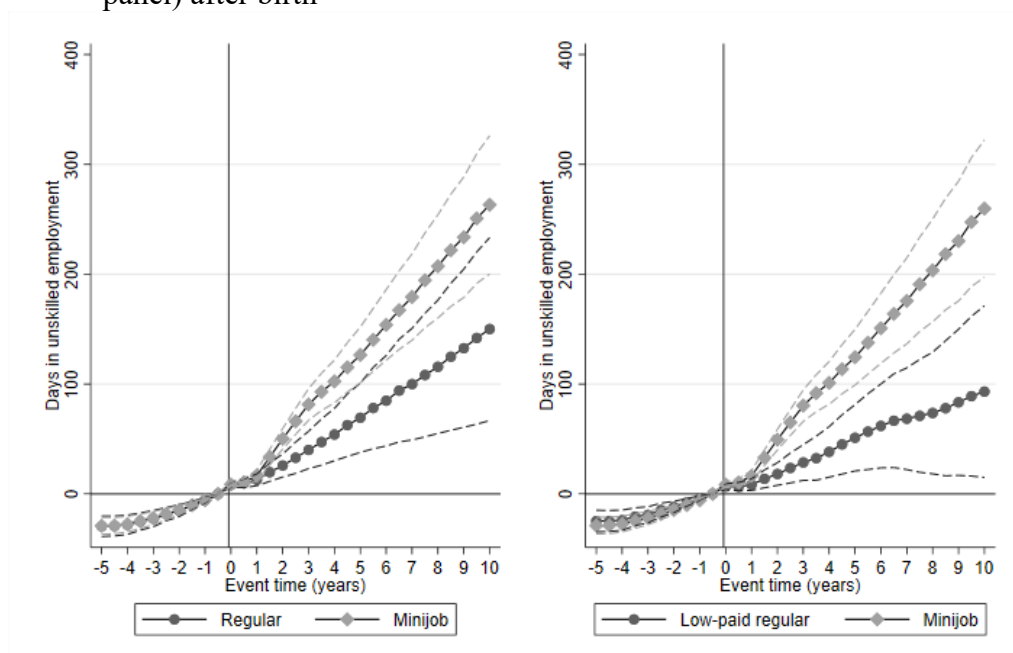


**Figure A.3** Propensity score distributions when matching mothers starting out in Minijob and low-paid regular employment after birth

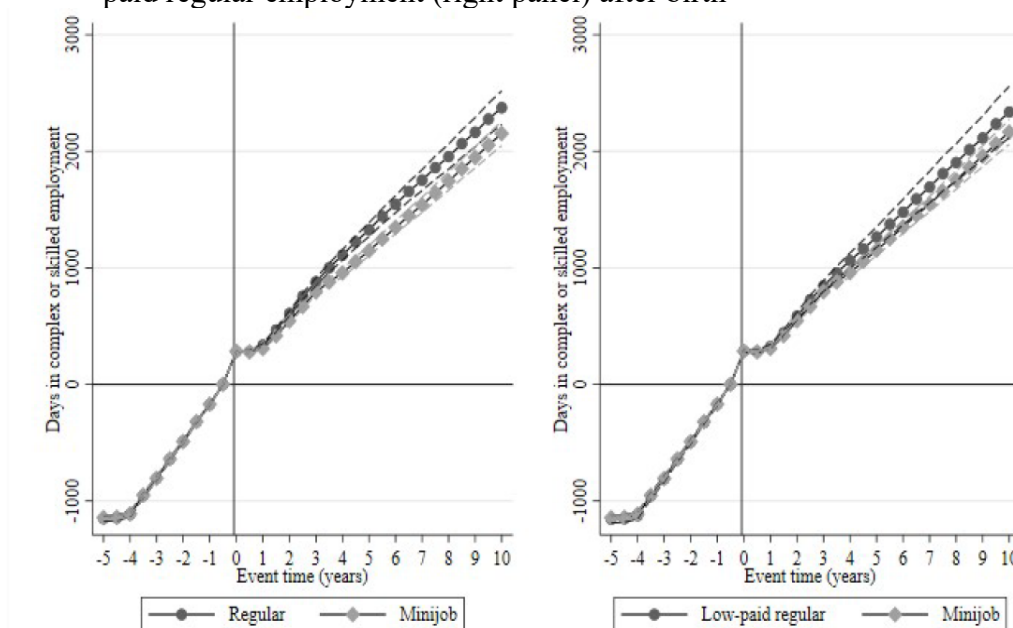


**Source:** SIAB (1999-2017) and own calculations.

**Figure A.4.1** 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



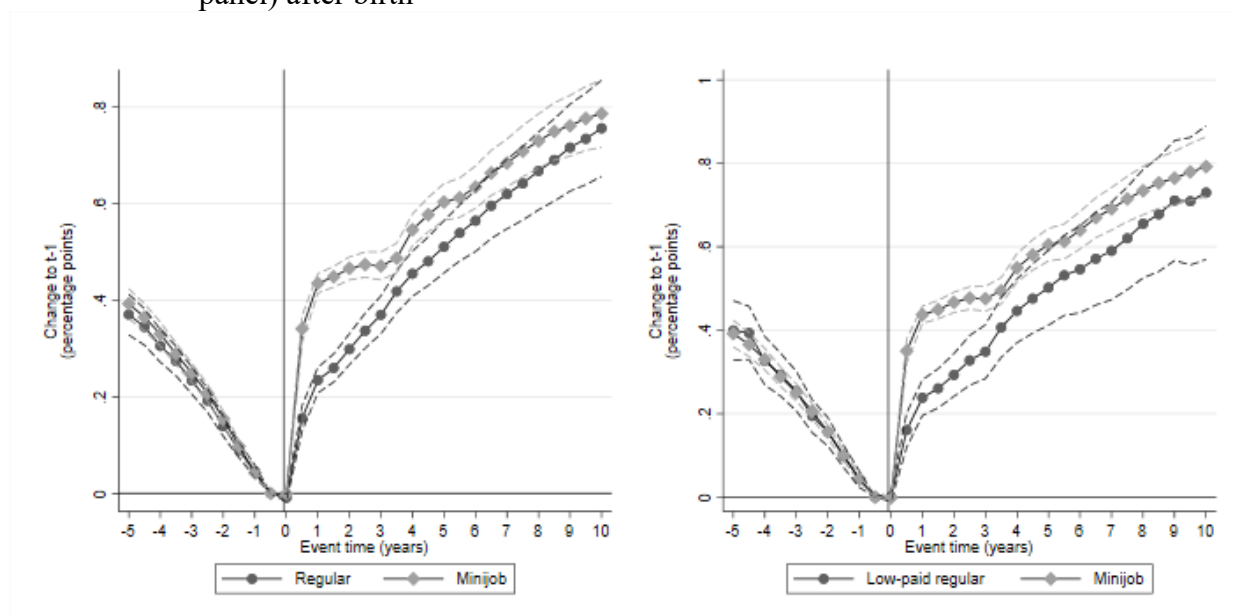
**Figure A.4.2** Labor market experience (in days) in skilled and complex jobs for mothers starting out in Minijob versus those in regular employment (left panel) and low-paid regular employment (right panel) after birth



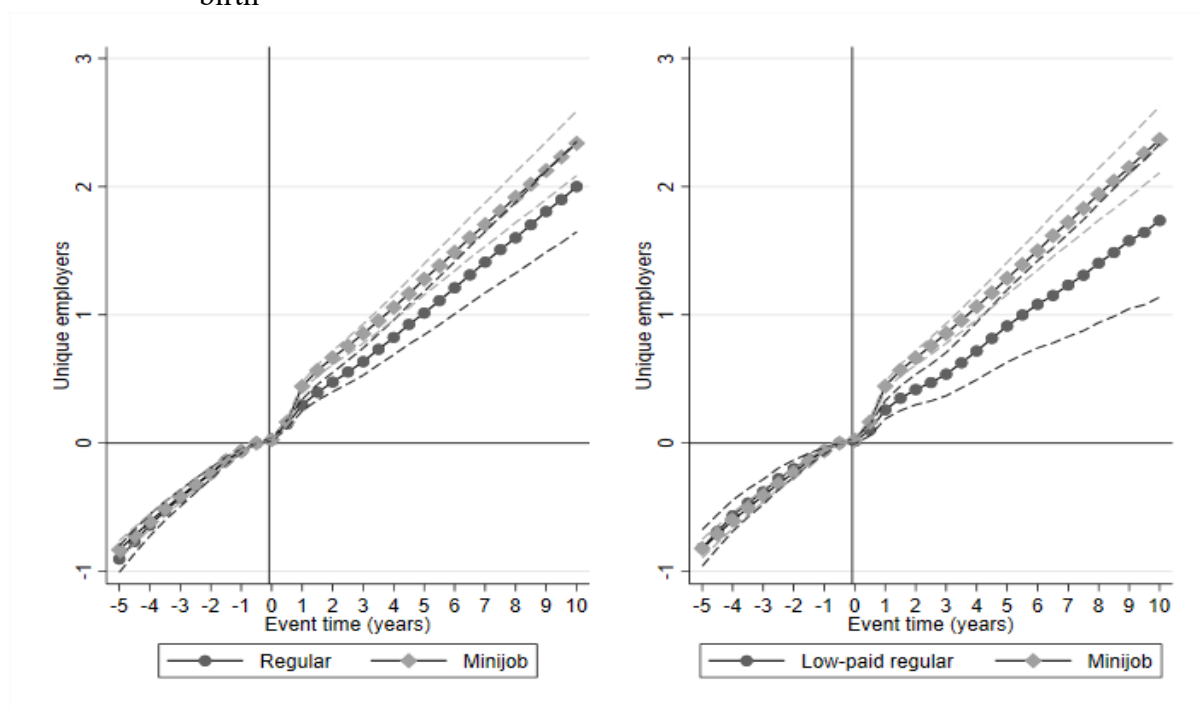
**Note:** The x-axis is indexed to 0 in the month of birth. The figures show maternal labor market experience by job complexity at child's age  $t$ . The estimation samples are a result of a matching of the Minijob group to the two alternative groups. The figures show 95 percent confidence intervals 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 A.4.1 and A.4.2.

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

**Figure A.5.1** 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



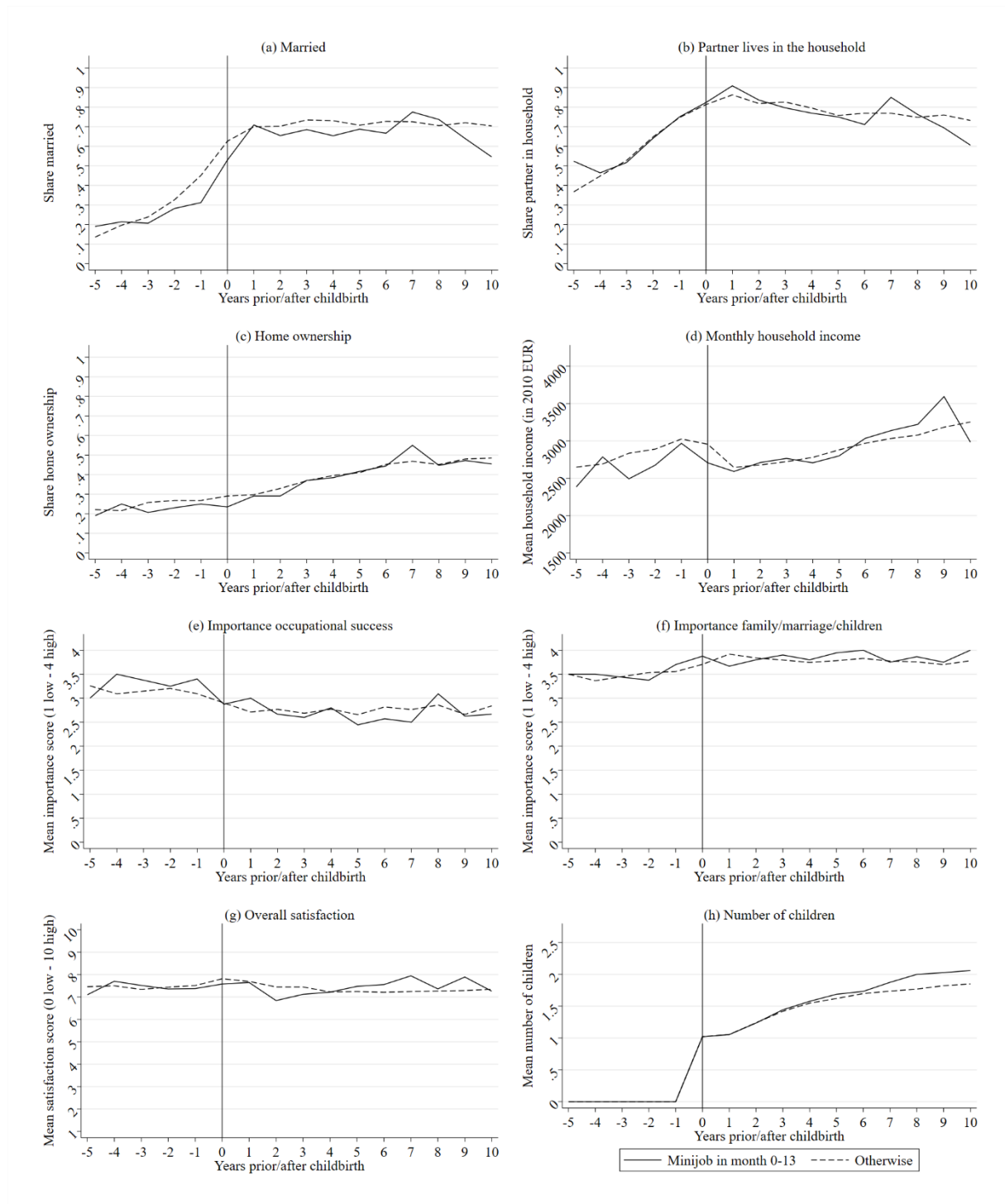
**Figure A.5.2** Number of employers for mothers starting out in Minijob versus those in regular employment (left panel) and low-paid regular employment (right panel) after birth



**Note:** The x-axis is indexed to 0 in the month of birth. **Figure A.5.1** 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 A.5.2** 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.

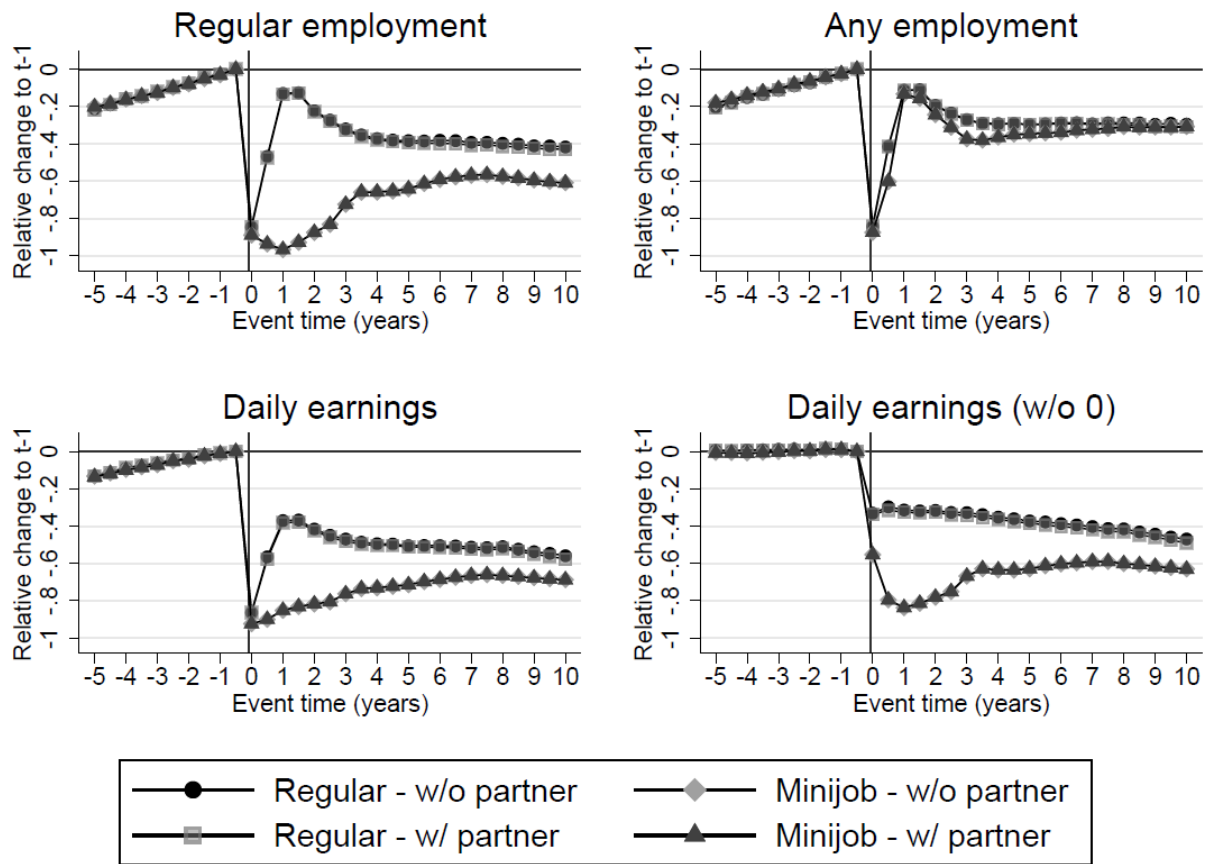
**Figure A.6** Household characteristics and attitudes around birth for mothers starting out in Minijob employment after birth and remaining mothers



**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. The question on the importance of occupational success or family/marriage/children was “Different things are important to different people. How important are the following things to you?”

**Source:** SOEP (1984-2018, v36, [10.5684/soep.core.v36eu](https://www.diw.de/en/data-soep-core-v36)).

**Figure A.7** 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

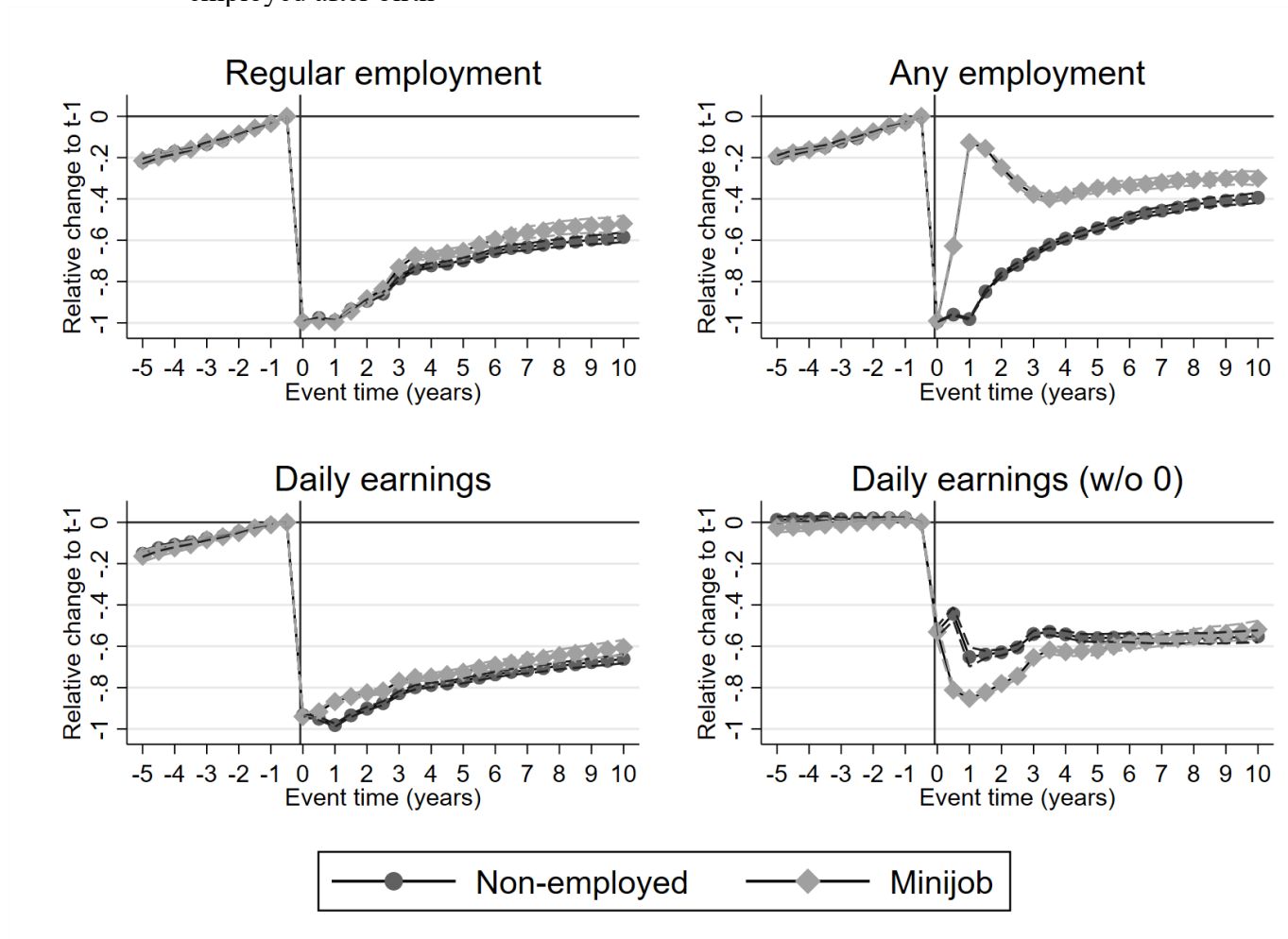


**Note:** The x-axis is indexed to 0 in the month of birth. The figures show the employment status and pay at child's age  $t$  relative to the respective outcome 6 months prior to birth. The estimation samples are a result of a matching of the Minijob group to the control group of regularly employed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

**Source:** IEB (2001-2017) and own calculations.



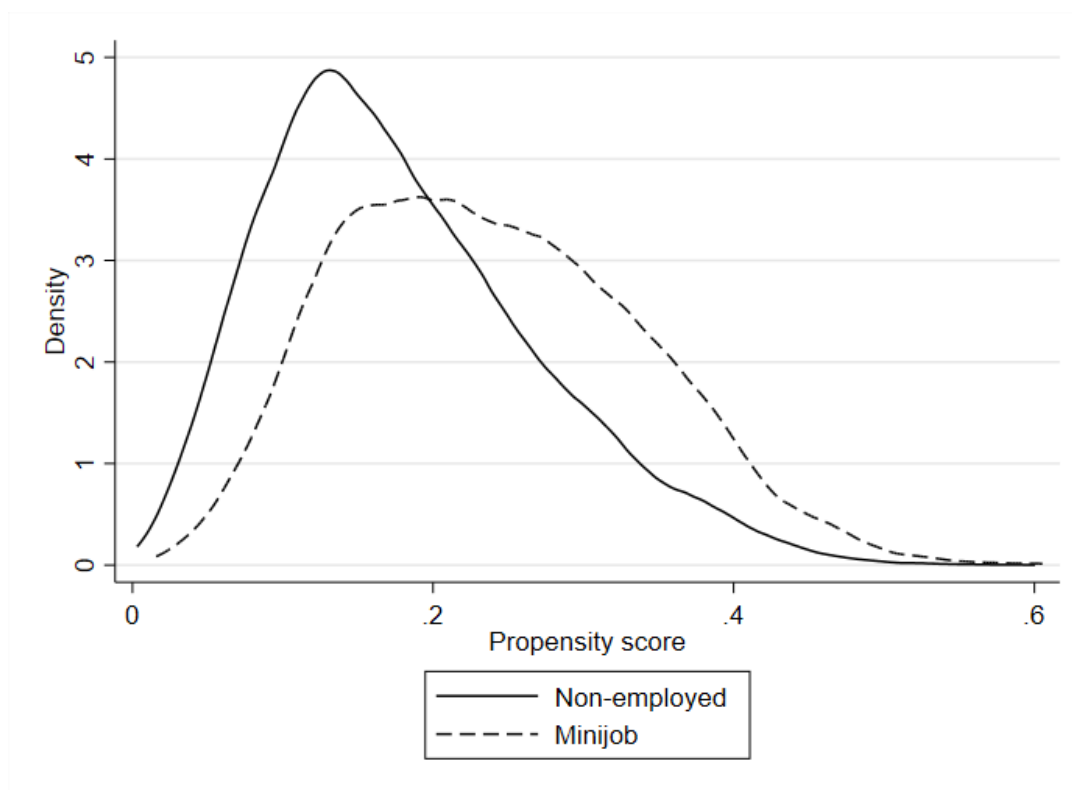
**Figure A.8** Motherhood penalties for mothers starting out in Minijob versus those not employed after birth



**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.9** Propensity score distributions when matching mothers starting out in Minijob and non-employment after birth



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

**Table A.1** Robustness tests comparing Minijobbers to mothers in low-paid regular employment

		2 Years after birth		5 Years after birth		10 Years after birth	
		b	se	b	se	b	se
<b>(A) Baseline</b>							
Regular employment	Not employed	-0.234***	(0.019)	-0.443***	(0.030)	-0.409***	(0.046)
	Minijob	-0.884***	(0.007)	-0.651***	(0.012)	-0.509***	(0.021)
	Absolute Diff.	0.650***	(0.020)	0.208***	(0.032)	0.100***	(0.051)
	Relative Diff.	-278%		-47%		-24%	
Daily Earnings (incl. 0)	Not employed	-0.658***	(0.020)	-0.634***	(0.025)	-0.542***	(0.044)
	Minijob	-0.827***	(0.007)	-0.726***	(0.011)	-0.602***	(0.019)
	Absolute Diff.	0.169***	(0.021)	0.092***	(0.027)	0.060***	(0.048)
	Relative Diff.	-26%		-15%		-11%	
<b>(B) 1 Nearest Neighbor matching, caliper 0.001</b>							
Regular Employment	Not employed	-0.238***	(0.022)	-0.443***	(0.034)	-0.414***	(0.054)
	Minijob	-0.884***	(0.007)	-0.651***	(0.012)	-0.509***	(0.021)
	Absolute Diff.	0.646***	(0.023)	0.209***	(0.036)	0.094***	(0.057)
	Relative Diff.	-271%		-47%		-23%	
Daily Earnings (incl. 0)	Not employed	-0.664***	0.022	-0.639***	(0.030)	-0.541***	(0.052)
	Minijob	-0.827***	0.007	-0.726***	(0.011)	-0.602***	(0.019)
	Absolute Diff.	0.163***	(0.023)	0.087*	(0.032)	0.06093496	(0.056)
	Relative Diff.	-24%		-14%		-11%	
<b>(C) Drop bottom and top 5% of matching weights</b>							
Regular Employment	Not employed	-0.234***	(0.020)	-0.443***	(0.029)	-0.409***	(0.046)
	Minijob	-0.884***	(0.007)	-0.651***	(0.012)	-0.509***	(0.021)
	Absolute Diff.	0.650***	(0.021)	0.209***	(0.031)	0.099***	(0.051)
	Relative Diff.	-277%		-47%		-24%	
Daily Earnings (incl. 0)	Not employed	-0.658***	(0.020)	-0.634***	(0.025)	-0.542***	(0.044)
	Minijob	-0.827***	(0.007)	-0.726***	(0.011)	-0.602***	(0.019)
	Absolute Diff.	0.169***	(0.021)	0.093***	(0.028)	0.061***	(0.048)
	Relative Diff.	-26%		-15%		-11%	
<b>(D) Matching 18 months after birth</b>							
Regular Employment	Not employed	-0.205***	(0.017)	-0.459***	(0.026)	-0.429***	(0.041)
	Minijob	-0.929***	(0.005)	-0.672***	(0.010)	-0.528***	(0.018)
	Absolute Diff.	0.724***	(0.018)	0.213***	(0.028)	0.099***	(0.045)
	Relative Diff.	-353%		-46%		-23%	
Daily Earnings (incl. 0)	Not employed	-0.654***	(0.016)	-0.643***	(0.021)	-0.543***	(0.034)
	Minijob	-0.845***	(0.007)	-0.734***	(0.009)	-0.611***	(0.016)
	Absolute Diff.	0.191***	(0.017)	0.091***	(0.023)	0.068***	(0.038)
	Relative Diff.	-29%		-14%		-13%	
<b>(E) Dropping mothers after identified second birth</b>							
Regular Employment	Not employed	-0.181***	(0.019)	-0.310***	(0.035)	-0.303***	(0.065)
	Minijob	-0.885***	(0.007)	-0.605***	(0.014)	-0.515***	(0.025)
	Absolute Diff.	0.704***	(0.021)	0.295***	(0.038)	0.212***	(0.069)
	Relative Diff.	-388%		-95%		-70%	
Daily Earnings (incl. 0)	Not employed	-0.632***	(0.019)	-0.532***	(0.030)	-0.446***	(0.064)
	Minijob	-0.829***	(0.008)	-0.696***	(0.012)	-0.593***	(0.023)
	Absolute Diff.	0.197***	(0.021)	0.164***	(0.033)	0.147***	(0.068)
	Relative Diff.	-31%		-31%		-33%	

**Note:** See Table 2.

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

**Table A.2** Robustness tests comparing Minijobbers to initially nonemployed mothers

		2 Years after birth		5 Years after birth		10 Years after birth	
		b	se	b	se	b	se
<b>(A) Baseline</b>							
Regular employment	Not employed	-0.893***	(0.003)	-0.699***	(0.006)	-0.585***	(0.012)
	Minijob	-0.882***	(0.007)	-0.653***	(0.011)	-0.519***	(0.019)
	Absolute Diff.	-0.011***	(0.007)	-0.046***	(0.013)	-0.066***	(0.022)
	Relative Diff.	1%		7%		11%	
Daily Earnings (incl. 0)	Not employed	-0.902***	(0.006)	-0.767***	(0.006)	-0.661***	(0.011)
	Minijob	-0.825***	(0.007)	-0.725***	(0.011)	-0.605***	(0.019)
	Absolute Diff.	-0.077***	(0.009)	-0.042***	(0.013)	-0.056***	(0.022)
	Relative Diff.	9%		5%		8%	
<b>(B) 1 Nearest Neighbor matching, caliper 0.001</b>							
Regular Employment	Not employed	-0.890***	(0.008)	-0.684***	(0.014)	-0.577***	(0.026)
	Minijob	-0.883***	(0.006)	-0.653***	(0.011)	-0.519***	(0.019)
	Absolute Diff.	-0.007	(0.010)	-0.031	(0.018)	-0.058	(0.032)
	Relative Diff.	1%		5%		10%	
Daily Earnings (incl. 0)	Not employed	-0.897***	(0.010)	-0.760***	(0.015)	-0.657***	(0.026)
	Minijob	-0.825***	(0.007)	-0.725***	(0.011)	-0.605***	(0.019)
	Absolute Diff.	-0.072***	(0.012)	-0.035*	(0.019)	-0.052	(0.032)
	Relative Diff.	8%		5%		8%	
<b>(C) Drop bottom and top 5% of matching weights</b>							
Regular Employment	Not employed	-0.889***	(0.003)	-0.694***	(0.006)	-0.581***	(0.011)
	Minijob	-0.883***	(0.006)	-0.653***	(0.110)	-0.519***	(0.019)
	Absolute Diff.	-0.006***	(0.007)	-0.041***	(0.110)	-0.062***	(0.022)
	Relative Diff.	1%		6%		11%	
Daily Earnings (incl. 0)	Not employed	-0.903***	(0.004)	-0.766***	(0.006)	-0.662***	(0.010)
	Minijob	-0.825***	(0.007)	-0.725***	(0.011)	-0.605***	(0.019)
	Absolute Diff.	-0.078***	(0.008)	-0.041***	(0.013)	-0.057***	(0.021)
	Relative Diff.	9%		5%		9%	
<b>(D) Matching 18 months after birth</b>							
Regular Employment	Not employed	-0.935***	(0.003)	-0.710***	(0.006)	-0.589***	(0.012)
	Minijob	-0.927***	(0.005)	-0.700***	(0.010)	-0.529***	(0.018)
	Absolute Diff.	-0.008***	(0.006)	-0.010***	(0.012)	-0.060***	(0.022)
	Relative Diff.	1%		1%		10%	
Daily Earnings (incl. 0)	Not employed	-0.939***	(0.005)	-0.781***	(0.006)	-0.669***	(0.011)
	Minijob	-0.843***	(0.007)	-0.733***	(0.010)	-0.610***	(0.016)
	Absolute Diff.	-0.096***	(0.009)	-0.048***	(0.012)	-0.059***	(0.019)
	Relative Diff.	10%		6%		9%	
<b>(E) Dropping mothers after identified second birth</b>							
Regular Employment	Not employed	-0.898***	(0.004)	-0.672***	(0.007)	-0.591***	(0.014)
	Minijob	-0.884***	(0.007)	-0.609***	(0.013)	-0.529***	(0.023)
	Absolute Diff.	-0.014***	(0.008)	-0.063***	(0.015)	-0.062***	(0.027)
	Relative Diff.	2%		9%		10%	
Daily Earnings (incl. 0)	Not employed	-0.906***	(0.007)	-0.750***	(0.006)	-0.660***	(0.012)
	Minijob	-0.826***	(0.007)	-0.697***	(0.011)	-0.599***	(0.021)
	Absolute Diff.	-0.080***	(0.010)	-0.053***	(0.013)	-0.061***	(0.024)
	Relative Diff.	9%		7%		9%	

**Note:** See Table 2.**Source:** SIAB (1999-2017) and own calculations.

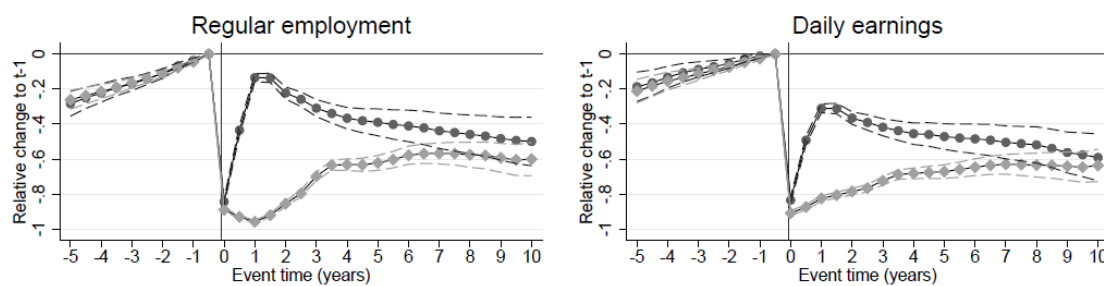
## **Appendix B Testing for the relevance of an additional mechanism**

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 threshold 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.

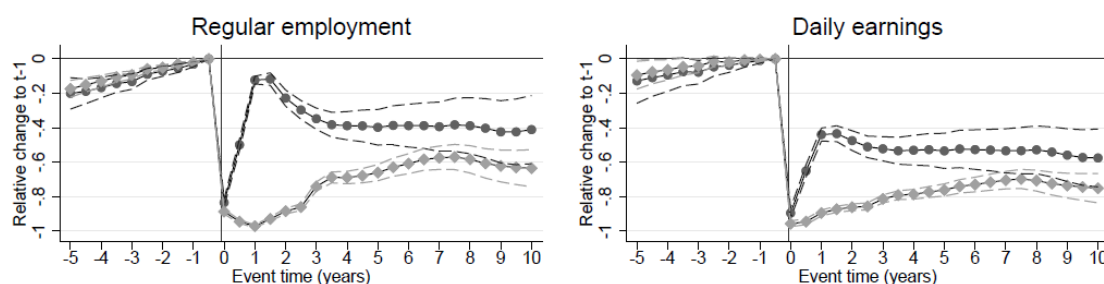
To validate this incentive mechanism, we test whether the Minijob motherhood penalty varies with spousal income. For this purpose, we use the sample of observations with matched partner information (see section 4.3) and group mothers in three terciles depending on the relative earnings of their spouse 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 (i.e., in the third than in the first partner wage tercile). This pattern is consistent with the tax incentives trapping especially mothers with high-income partners 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

Partner wage: first tercile



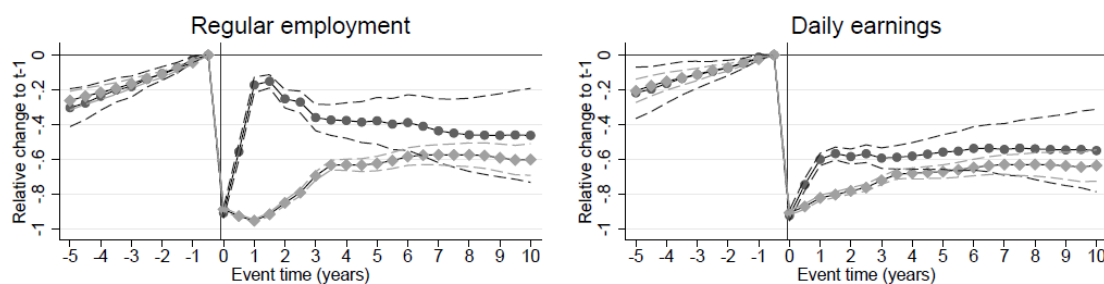
Partner wage: third tercile



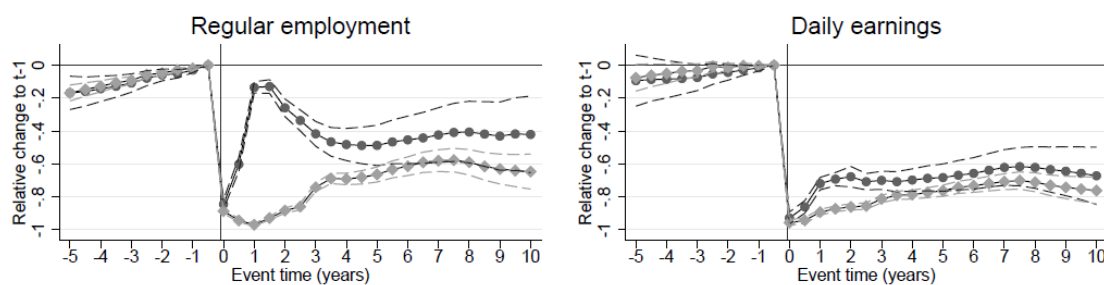
—●— Regular —◆— Minijob

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

Partner wage: first tercile



Partner wage: third tercile



—●— Low-paid regular —◆— Minijob

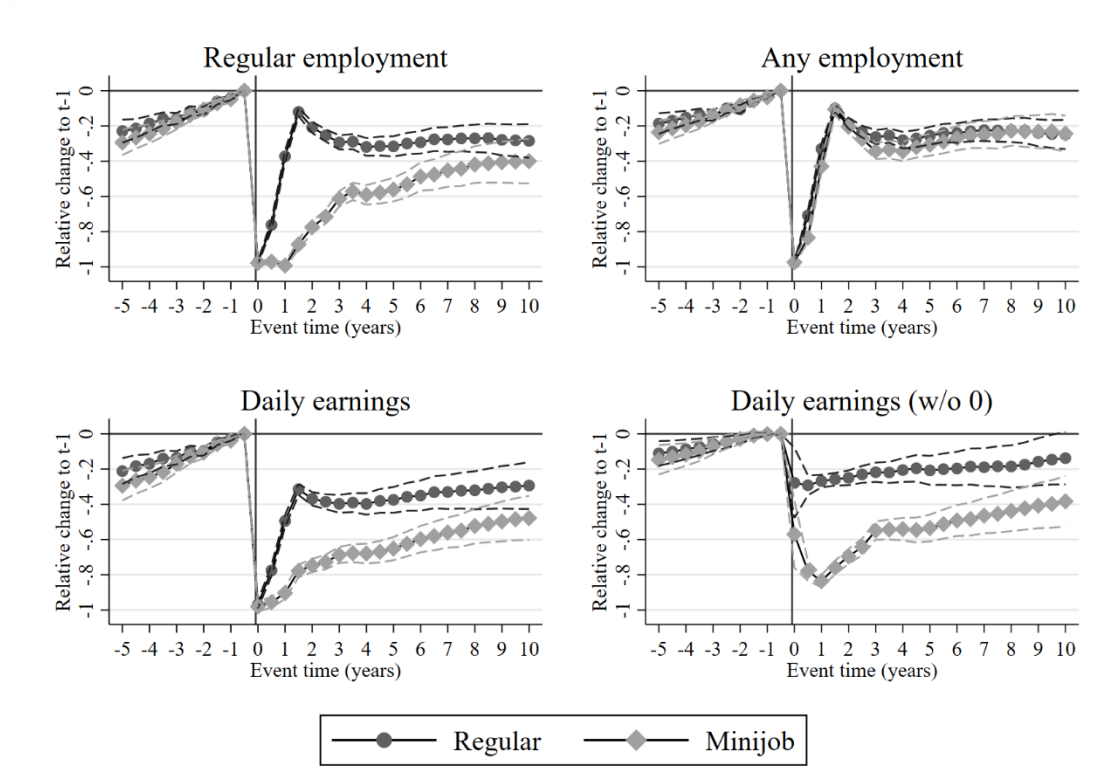
## Appendix C Outcomes for more recent periods

The main analysis of the paper studies maternal outcomes up to 10 years after births that took place between 1999 and 2006. We chose the 2006 threshold because it marked the end of the institution of *Erziehungsgeld* (see section 2.3). For births that occurred on and after January 1, 2007, the *Erziehungsgeld* policy was replaced by *Elterngeld*. *Elterngeld* is a paid parental leave scheme which is no longer means tested (for details see e.g., Bergemann and Riphahn, 2023).

In this Appendix, we present the results of analyses on maternal labor market outcomes after births that took place between 2007 and 2011, i.e., under the new institutional setting. To be able to use a 10-year observation post-birth window we had to use a more recent version of the administrative data than in the main analysis (SIAB 7521 instead of SIAB 7517). Except for the later timing of births and the different dataset all other analysis steps were maintained.

Between 2007 and 2011 we observe 8,882 birth outcomes. 871 mothers entered a Minijob by month 13 after birth, and 2,943 mothers entered regular employment. **Figure C.1** presents the long-run outcomes for these more recent births. While the confidence intervals in the earnings outcomes are somewhat wider than in **Figure 4.1** the results are very similar. **Table C.1** Panel B presents the baseline results comparable to those in **Table 2** Panel A which are copied in Panel A of **Table C.1**. The results in terms of the absolute differences in the outcomes for treatment and control group observations over time are very similar across both panels.

**Figure C.1** Motherhood penalties for mothers starting out in Minijob versus in regular employment after birth



**Note:** See **Figure 4.1**

**Source:** SIAB (1999-2021) and own calculations.

**Table C.1** Estimation Results

		2 Years after birth		5 Years after birth		10 Years after birth	
		b	se	b	se	b	se
<b>(A) Births 1999-2006 (SIAB 7517)</b>							
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>(B) Births 2007-2011 (SIAB 7521)</b>							
Regular Employment	Regular	-0.208***	(0.016)	-0.316***	(0.029)	-0.285***	(0.065)
	Minijob	-0.774***	(0.017)	-0.561***	(0.034)	-0.400***	(0.049)
	Absolute Diff.	0.566***	(0.023)	0.245***	(0.045)	0.115***	(0.081)
	Relative Diff.	-272%		-78%		-40%	
Daily Earnings (incl. 0)	Regular	-0.368***	(0.019)	-0.375***	(0.037)	-0.293***	(0.068)
	Minijob	-0.747***	(0.191)	-0.651***	(0.034)	-0.478***	(0.064)
	Absolute Diff.	0.379***	(0.192)	0.276***	(0.050)	0.185***	(0.093)
	Relative Diff.	-103%		-74%		-63%	

**Note:** Panel A is reproduced from Table 2, Panel B shows new results.

**Source:** for Panel B: SIAB (1999-2021) and own calculations.