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# Phasing out payroll tax subsidies

Anna Herget<sup>1</sup> · Regina T. Riphahn<sup>1</sup>

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## Abstract

Many countries subsidize low-income employments or small jobs. These subsidies and their phasing out can generate labor market frictions and distort incentives. The German *Minijob* program subsidizes low-income jobs. It generates a ‘Mini-job trap’ with substantial bunching along the earnings distribution. Since 2003, the newly introduced *Midijob* subsidy aims to reduce the Minijob-induced notch in the net earnings distribution. Midijobs reduce payroll taxes for employments above the Minijob earnings ceiling. We investigate whether introducing Midijobs reduced the Minijob trap. We apply a regression discontinuity design using administrative data and a difference-in-differences estimation using survey data. While in both cases our results show a small positive overall effect of Midijobs on transitions out of Mini-jobs, they are effective only for a narrow treatment group.

**Keywords** Midijobs · Minijobs · Payroll tax subsidy · Causal effects · Difference-in-differences · Regression discontinuity · SOEP · SIAB

**JEL Classification** J21 · J38 · H24

## 1 Introduction

In recent decades, many countries have shifted minimum income protection from traditional means-tested transfers to make work pay policies to incentivize labor supply.<sup>1</sup> Make work pay policies condition on employment and either provide subsidies to employers or to employees via negative income taxes or payroll tax subsidies. Well-known examples of such policies are the U.S. Earned Income Tax Credit (EITC) and the U.K. Working Family Tax Credit (WFTC). Numerous contributions

<sup>1</sup> See, e.g., Immervoll et al. (2007) or Jara et al. (2020).

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evaluate the labor supply effects of these programs.<sup>2</sup> This paper contributes to the make-work pay literature: we study the effect of a new program (*Midijobs*) aimed at phasing out a payroll tax subsidy (*Minijobs*) in Germany. The Midijob program was introduced to incentivize labor supply and earnings increases for workers with very low earnings. This is a challenge in many labor markets.

The Midijob program was implemented on April 1, 2003. It reduces employee social insurance contributions for jobs within the Midijob earnings range, originally 400 to 800 Euro per month. It was introduced to address a labor supply disincentive generated by Minijobs; Minijobs are low-income employments paying up to 400 Euro per month at the time. They cause substantial bunching at the Minijob earnings limit (Gudgeon & Trenkle, 2024; Tazhitdinova, 2020) because Minijob employees pay neither income taxes nor social insurance contributions. Taxes and contributions fall due on total earnings when earnings exceed the Minijob earnings threshold; Tazhitdinova (2020) pointed out that an average woman's combined tax and social insurance liabilities when earning 1 Euro beyond the pre 2003 Minijob earnings threshold reached 45 percent of her total gross income. Therefore, workers rarely extend their earnings beyond this limit which generates the so-called 'Minijob trap'. Figure 1 illustrates the uneven number of jobs across the earnings distribution. Midijobs were designed to address the Minijob trap and we investigate whether the Midijob program succeeds in this respect.

Both programs, Minijobs and Midijobs are used extensively. At the end of 2021, 4.1 million individuals used Minijobs as their main employment and about 3 million workers were employed in Midijobs (see Appendix Figure A.2).<sup>3</sup> Thus, the programs jointly covered 15.3 percent or one-sixth of the German labor force. Given the magnitude of the Midijob program, its expansions to earnings limits of 1,300 (in 2019), 1600 (in 2022), and even 2000 Euro in 2023, and the lack of prior evidence, the Minijob trap addressed in this paper is important.

We use two complementary empirical strategies. First, we exploit the introduction of Midijobs on April 1, 2003, to evaluate their immediate effects on the propensity to transition from a Minijob to higher-earning, regular employment. For this purpose, we apply a regression discontinuity in time (RDiT) design to administrative data and investigate whether there is a jump in the transition probability at the cutoff date. However, this approach may suffer from shortcomings, e.g., by capturing only local effects. Our second approach exploits a unique feature of income tax regulations. Specifically, Midijobs reduced workers' social insurance contribution rates in the earnings range just above the Minijob earnings limit leaving income tax rates untouched. This attenuates the kink in the net earnings schedule, particularly for individuals who pay low or no income taxes (e.g., single individuals). In

<sup>2</sup> For a recent survey see Neumark and Shirley (2020), also Bastian and Lochner (2022), Bastian (2020), Hoynes and Patel (2018), Azmat (2014), Chetty and Saez (2013), Chetty et al. (2013), Dahl et al. (2009), Francesconi and van der Klaauw (2007), Eissa and Hoynes (2004), and Eissa and Liebman (1996).

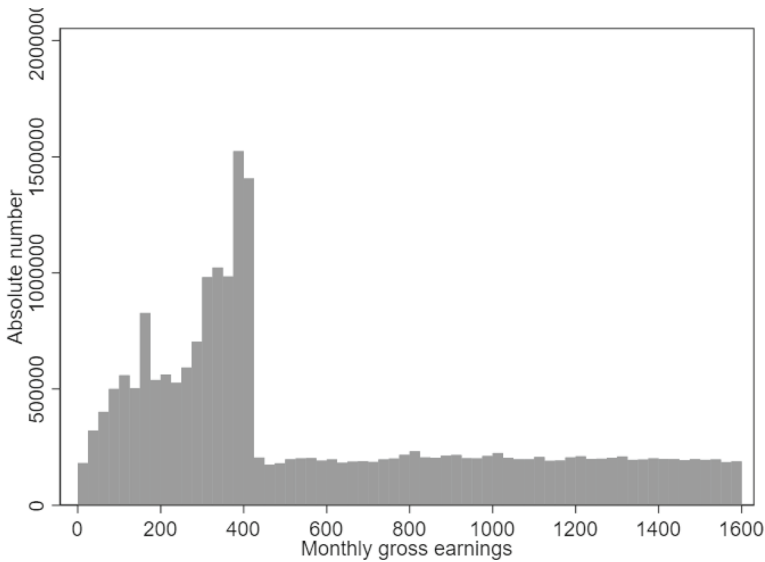
<sup>3</sup> Another 3.1 million individuals use Minijobs as secondary employment. As secondary job holding is not in the focus of the Midijob regulation we do not discuss it further (for a detailed analysis see, e.g., Tazhitdinova 2022). – The utilization of Minijobs declined slightly in 2015 with the introduction of mandatory minimum wages and again during the Covid pandemic. Figure A.2 clearly shows that more employees were covered after the Midijob earnings ceiling increased on July 1, 2019 from 850 to 1300 Euro per month.

contrast, Midijobs hardly affect the net earnings schedule of individuals who at the kink additionally become subject to high income tax rates (e.g., secondary earners in marriages) because for them income taxes dominate social insurance contributions. We consider the former group to be treated by the reform whereas the latter group is our control group. We use survey data from the German Socioeconomic Panel (SOEP) which offers information on marital status and apply a difference-in-differences (DID) analysis to study the propensity to transition from a Minijob to higher-earning, regular employment. The DID analysis yields an average treatment effect on the treated which captures more sluggish labor supply responses than the discontinuity design.

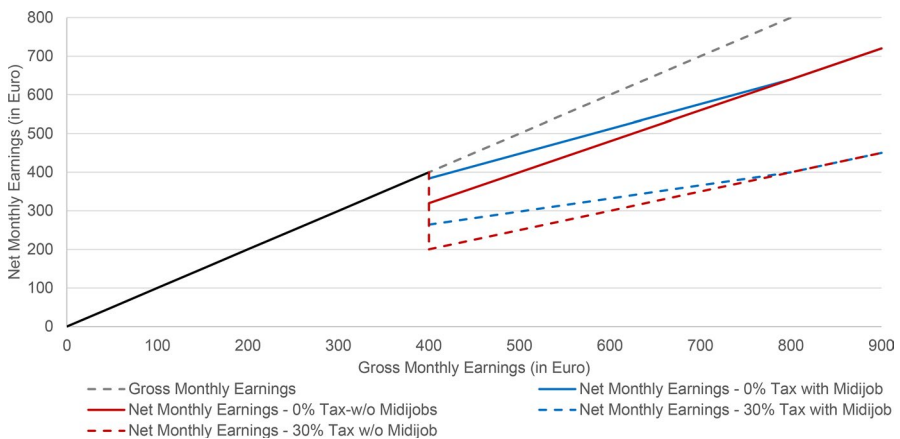
We find that while the program works on average, it does not work for all. The first strategy (RDiT) yields small increases of at most 15 percent in average monthly transition rates out of Minijobs into higher paying, regular employment at the time of the Midijob introduction. However, these changes are concentrated in the small group of male Minijob holders. The second strategy (DID) confirms these patterns: the reform effect on annual transitions out of Minijobs is significantly larger in the treatment than in the control group. Non-married individuals with low-income tax burdens respond more strongly to the reform than secondary earners in marriages—typically females—who are subject to high-income tax rates. The results of both identification strategies are robust to various specification changes and sample adjustments. Overall, Midijobs were introduced to phase out Minijob subsidies and to reduce bunching at the Minijob earnings threshold. This objective was missed for most Minijob employees. The Minijob trap did not disappear after the introduction of Midijobs. These findings have a clear takeaway: in countries with household taxation, phase-out policies from Minijob-type notches must account for the entire tax domain (including the income tax-induced portion of the notch) to be effective.

These findings complement the results of Tazhitdinova (2020) and Gudgeon and Trenkle (2024). Both papers focus on Minijob employment and study labor supply elasticities at the Minijob earnings ceiling. Tazhitdinova (2020) uses bunching approaches to determine annual labor supply elasticities between 1999 and 2010. She finds larger labor supply elasticities for men than for women and strong increases in labor supply elasticities for single individuals after 2003. Gudgeon and Trenkle (2024) focus on a sample of married women only and study the responsiveness of labor supply to shifts in the Minijob earnings ceilings in 2003 and 2013 over time. The authors find substantial delays in earnings responses and argue that labor demand frictions attenuate estimates of intensive margin labor supply elasticities. We add to these contributions by focusing on the effect of Midijobs; they may be a mechanism to phase out Minijob subsidies and to facilitate transitions to regular employment.

Our research connects to several strands of the literature. First, we contribute to the international literature on the effectiveness of making work pay policies such as the EITC in the United States or the United Kingdom tax credit programs. Numerous studies investigate whether the subsidy programs enable beneficiaries to grow



**Fig. 1** Distribution of gross monthly earnings (April 2003–December 2012). Note: The graph represents the bottom part of the gross earnings distribution for all employed individuals registered with the unemployment insurance. Using all jobs observed between April 2003 and December 2012 it shows the number of employees per 50 Euro bin of monthly gross earnings. For depictions of annual gross earnings distributions, see Appendix Fig. A.1. *Source:* SIAB, own calculations



**Fig. 2** Net earnings with and without Midijob subsidy by income tax burden (2003). Note: The graph sketches net monthly earnings along the development of monthly gross earnings. Up to gross earnings of 400 Euro per month Minijobs eliminate any difference between gross and net warnings. Beyond the Minijob earnings threshold the red (blue) lines indicate the situation before (after) the introduction of Midijobs. The dashed lines assume an average income tax rate of 30 percent whereas the straight lines assume zero income taxes. *Source:* Own illustration

out of their need for support by expanding their labor supply.<sup>4</sup> Only few contributions address the role of benefit phase-out. Eissa and Liebman (1996) study benefit phase-out in the EITC. Interestingly, they find single mothers to be rather unresponsive to increasing marginal tax rates in the phase-out region of the program.<sup>5</sup> Leigh (2007) investigates the role of phase-out rates using the 1999 reform of the United Kingdom's tax credit. He confirms that lower phase-out rates have positive impacts on labor supply. Comparing the effects of the U.S. EITC and the German Minijob program Berthold and Coban (2013) conclude that in contrast to the EITC, the German program was ineffective in supporting low-income earners. However, so far, it is still unresolved whether the phasing out of the Minijob subsidy by means of Midijobs succeeds in encouraging higher-earning employment. We are the first to address this issue.

Second, we contribute to international research on the employment effects of payroll tax subsidies. Most studies investigate the extensive margin of labor supply and find no employment effects in response to changes in payroll taxes. Saez et al., (2019, p.1) argue that it is "received wisdom" that the payroll tax incidence falls on workers' net wages.<sup>6</sup> We add to this literature by focusing on the intensive margin of labor supply.

A third branch of studies investigates the stepping stone character of Minijobs themselves. The 2003 reform rendered Minijob employment more attractive to incentivize the labor market entry of those previously not employed: lawmakers hoped for Mini- and Midijobs to become stepping stones into regular employment. Several studies evaluate the programs and conclude that it is unlikely that the programs act as a stepping stone.<sup>7</sup>

The literature on Midijobs is limited and largely descriptive. Most studies explain the Midijob instrument and its utilization.<sup>8</sup> Bach et al., (2018a, 2018b) conclude that the 2019 expansion of the Midijob earnings ceiling from 850 to 1,300 Euro per month may even worsen the part-time employment trap.

Our analyses contribute to the literature in several ways. First, while the literature on payroll tax subsidies focuses on the extensive margin of labor force participation,

<sup>4</sup> See, e.g., Bargain and Orsini (2006), Bargain et al. (2010), Blundell (2000), Francesconi and van der Klaauw (2007), or Grogger (2003).

<sup>5</sup> Browning (1995) calculates potential negative income effects induced by high marginal taxes in the phase-out region of the EITC. However, empirical studies did not support this rationale (see also Meyer 2002). LaLumia (2009) investigates the reporting behavior of self-employed in response to the incentives of the EITC program and confirms that the phase-out region generates less of a response than the phase-in region. Trampe (2007) summarizes the literature which hardly found negative effects of the EITC phase-out region. He finds small negative effects (however, for a discussion see Hoynes 2007 and Trampe 2008).

<sup>6</sup> This is broadly supported in the literature, see e.g., Gruber (1997), Anderson and Meyer (1997, 2000), Korkeamäki and Uusitalo (2009), Huttunen et al. (2013), and Bennismarker et al. (2009) who provide evidence from Chile, the United States, Finland, and Sweden, respectively.

<sup>7</sup> See e.g., Fertig et al. (2005), Fertig and Kluve (2006), and Freier and Steiner (2008) or more recently Caliendo et al. (2016), Lietzmann et al. (2017), and Carrillo-Tudela et al. (2021).

<sup>8</sup> See, e.g., Fertig and Kluve (2006), Brandt (2005, 2006), Herzog-Stein and Sesselmeier (2012), Berthold and Coban (2013), Fichtl (2015), Keller and Seifert (2015), Seifert (2017), Dundler et al. (2019), Keller et al. (2021), and Herget and Riphahn (2022).

we study the intensive margin of labor supply. Small jobs restrict overall labor supply, limit human capital investments and career prospects (Beckmann, 2020), and inhibit the accumulation of pension claims. Therefore, it is important to understand mechanisms that support transitions to regular employment. Second, the literatures on ‘making work pay’ and on the stepping stone character of small jobs have not yet addressed the relevance of the phasing out of payroll tax subsidy programs. If the Midijob program effectively supports transitions to extended labor supply it could constitute a useful policy for other (national) labor markets, as well.<sup>9</sup> Finally, we are the first to study the effect of the introduction of Midijobs on the propensity to leave Minijob employment, i.e., the effectiveness of Midijobs as a labor market policy.

The structure of this paper is as follows. In section two, we provide institutional detail on the Mini- and Midijob programs and their development over time. We describe our empirical analysis based on the regulatory discontinuity in Sect. 3 and our difference-in-differences analyses in Sect. 4. In Sect. 5 we offer a policy discussion and conclude.

## 2 Background

### 2.1 Institutions and reforms

Minijob employees, i.e., those earning no more than the Minijob earnings threshold are exempt from otherwise mandatory social insurance contributions and income taxes. Instead, their employers pay a fixed share of gross Minijob earnings to social insurance and tax authorities (for details see, e.g., Collischon et al., 2021).<sup>10</sup> This regulation has existed since the early days of the German social insurance system (1893) to limit the bureaucratic burden for small jobs (BMAS 2018, p.110). Over time, Minijob regulations were modified with varying objectives.

At the Minijob earnings ceiling, the sudden fade-out of the wage subsidy inherent in Minijob employment generates a large, discontinuous change in tax and social insurance liabilities. When they earn below the Minijob earnings ceiling employees pay neither social insurance contributions nor income taxes. When they earn above the Minijob earnings ceiling, income taxes on total earnings plus social insurance contributions of about 20 percent become payable (see Fig. 2). This leads to a (large) reduction in net monthly earnings. While social insurance contributions are relevant for all employees, income taxes differ for joint and individual filers; in Sect. 4, our identification strategy takes advantage of this heterogeneity. Those filing individually benefit from a sizable initial tax allowance and do not face income taxes immediately after exceeding the earnings threshold. In contrast, joint filers, i.e., secondary

<sup>9</sup> Di Porto et al. (2022), Dolado et al. (2021), and Scarfe (2021) recently studied zero-hours contracts and casual work in Italy, the United Kingdom, and Australia, respectively. Jobs based on these contracts are similar to German Minijobs.

<sup>10</sup> Minijobs also take the form of short-term employment relationships which do not extend beyond (currently) 70 days, independent of earnings. We disregard this second category of Minijob employment, which is much less prevalent.

earners in marriages, may be affected by sizeable tax rates on their entire earnings as soon as they exceed the Minijob earnings threshold. Thus, the Minijob earnings threshold generates a discontinuity in the level (a notch) and the slope (a kink) of the net earnings schedule (Kleven, 2016). This “Minijob trap” bars increases in labor supply and earnings and causes substantial bunching in the earnings distribution (see Fig. 1) (Gudgeon & Trenkle, 2024; Tazhitdinova, 2020).

The reforms implemented on April 1, 2003 raised the monthly Minijob earnings ceiling from 325 to 400 Euro, abolished a limit of 15 working hours per week, increased employer contribution rates from 22 to 25 percent of Minijob earnings, and—most interesting for us—newly introduced the Midijob program.<sup>11</sup> The intention of the 2003 reform was (a) to reduce illicit moonlighting by making legal small jobs more attractive and (b) to offer stepping-stone employment opportunities for the unemployed and opportunities for upward mobility for those in marginal employment (Eichhorst et al., 2012). The introduction of Midijobs did not affect employers who continued to pay regular social insurance contributions of about 20 percent on earnings beyond the Minijob earnings ceiling.

Midijobs were introduced to incentivize labor supply beyond the Minijob earnings ceiling; Midijobs entail payroll tax subsidies for employees earning between (then) 400 and 800 Euro per month. These subsidies phase out as earnings increase. Instead of full regular social insurance contribution rates of 20 percent, the Midijob rates increased on a sliding scale starting at about 4 percent for monthly earnings of 400 Euro and reaching the unsubsidized level of 20 percent for monthly earnings of 800 Euro. At the same time, Midijob employees are fully liable for income taxes on their total earnings (see Fig. 2).<sup>12</sup>

In 2013, the monthly Minijob and Midijob earnings limits were raised to 450 and 850 Euro, respectively (for later adjustments see Appendix Table A.1). After this reform, social insurance contribution rates for Midijobs commenced at about 10 percent for monthly earnings starting at 451 Euro and increased to 20 percent at monthly earnings of 850 Euro. Minijobs and Midijobs have been used intensely.

The introduction of the Midijob subsidy of social insurance contribution rates was intended to attenuate the notch in the net earnings distribution, phase out the Minijob subsidy, and incentivize labor supply and earnings beyond the Minijob earnings ceiling. We investigate whether Midijobs effectively reduced the barriers to exiting subsidized Minijob employment and entering higher-earning employment.

<sup>11</sup> The relevant legislation (*Zweites Gesetz für Moderne Dienstleistungen am Arbeitsmarkt, Hartz II*) was passed on December 23, 2002 as an early element of a bundle of labor market reforms. For a compact review of the reforms see, e.g., Carrillo-Tudela et al. (2021).

<sup>12</sup> Figure A.3 compares the implicit Minijob and Midijob subsidy to the patterns of the US EITC. Figure A.3.1 shows the situation before the 2003 reform: Minijob subsidies accrued in the 0–325 Euro earnings range. They increased with household tax rates and terminated abruptly at the Minijob earnings limit; depending on the tax regime they amount to about 50 percent of gross earnings. Figure A.3.2 shows the monthly credit after the 2003 reform. Here, the Midijob subsidy for earnings beyond the Minijob earnings limit (now 400 Euro) is introduced which is independent of tax rates. It starts at about 16 percent of gross earnings (about 64 Euro) and declines along the Midijob earnings range. Figure A.3.3 contrasts these patterns with the EITC regime of 2003 where subsidies are phased out without abrupt breakpoints as implied by the Minijob earnings threshold.



A priori, as the Midijob subsidy only covers social insurance contributions but not income taxes, the policy left a substantial tax notch for secondary earners. Therefore, we should not expect the Midijob reform to affect transitions from Minijobs for secondary earners much.

## 2.2 Utilization patterns and changes in the earnings distribution

While Mini- and Midijobs cover different earnings ranges and feature different utilization patterns, both are characterized by a prevalence of female workers (see, e.g., Oschmiansky and Berthold (2020), Tazhitdinova (2020), Herget and Riphahn (2022)). Based on weighted survey data and in the age group that we study (30–59) more than 90 percent of Minijobbers are female. Among these females, about 90 percent are married and thus generally secondary earners who face income taxes when earning above the Minijob earnings ceiling.

Typically, Minijobs pay low hourly wages. Classic employers of Minijobbers are in the hospitality industry (bars, restaurants), cleaning and building services, or retail. Minijob employment is concentrated in small establishments (0–9 employees) (Collischon et al., 2021). Minijobs are often informal with limited duration, no written contracts, irregular work hours, and on-call employment (Bruckmeier et al., 2018). Bachmann et al. (2012) asked Minijobbers why they use a Minijob (with multiple answers possible); almost 60 percent were motivated by additional earnings, 15 percent by gathering work experience, 14 percent indicated that this was the only job they could find, and 14 percent were motivated by the possibility to work flexible hours. For female Minijobbers being able to combine work and family life as well as flexible hours were substantially more important than for male Minijobbers (31 percent vs. 17 percent).

Midijobs are typically part-time positions. About 62 percent are held by females. Males use Midijobs typically when they are young. 46 (61) percent of female (male) Midijobbers are younger than 35 and 44 (29) percent are aged 35–54 (Herget & Riphahn, 2022). In Figure A.4, we describe for the calendar years 2002–2017 from which states of origin individuals enter Midijob employment; for the period before the Midijob introduction, we use the Midijob earnings range. Based on administrative data, no more than 20 percent of Midijobs originate from Minijob employment.

The reform of April 1, 2003 not only introduced the Midijob subsidy for social insurance contributions. It also shifted the threshold of the Minijob earnings limit from 325 to 400 Euros per month. We present the distribution of monthly gross earnings after the reform (2003–2012) in Fig. 1. Figure 3 shows the change in the distribution of monthly gross earnings between 2002 and 2004. The frequency of jobs earning 325–400 Euro increased, that of jobs earning 250–325 Euro declined. Interestingly, the frequency of employments in the Midijob earnings range between 400 and 800 Euro did not increase.

### 3 Discontinuous increase in transitions out of minijobs?

#### 3.1 Empirical strategy: regression discontinuity design

We are interested in the effect of the introduction of the Midijob subsidy on Minijobbers' propensity to expand earnings beyond the Minijob earnings ceiling. Our first empirical strategy exploits the sharp discontinuity in the regulatory setting on April 1, 2003, to determine whether transition behaviors change for Minijob employees observed shortly before and shortly after the reform date. Our outcome of interest ( $Y$ ) reflects whether an individual  $i$  leaves a Minijob for higher-earning employment in the next month  $t + 1$ .<sup>13</sup>

In our empirical model, the running variable *time* represents a time trend that is recentered on the reform date. The indicator *after* shows whether a potential transition outcome  $Y$  is observed after the reform date. If the identifying assumptions hold the coefficient  $\alpha_2$  provides the local average treatment effect of the reform, i.e., the reform effect on the transition rate. To allow for a change in overall time trends in transitions out of Minijobs after the reform date we consider interaction effects of *time* and *after*. The baseline specification for individual  $i$  observed in a Minijob in period  $t$  is.

$$Y_{it+1} = \alpha_0 + \alpha_1 \text{time}_{it} + \alpha_2 \text{after}_{it} + \alpha_3 (\text{after}_{it} \cdot \text{time}_{it}) + \beta_1 X_{it} + \varepsilon_{it}. \quad (1)$$

$\varepsilon$  represents a random error term. We will augment Eq. 1 by allowing for quadratic terms of the recentered *time* indicator and its interaction with the *after* indicator. We de-seasonalize the dependent variable at the monthly level<sup>14</sup> and apply three alternative specifications of the covariate vector  $X$  as robustness checks.

A causal interpretation of  $\alpha_2$  is plausible only if without the reform transition rates out of Minijobs would have developed continuously. Then, any discontinuous change in transition rates at the reform date can be interpreted as the immediate causal reform effect.<sup>15</sup> However, our identifying assumption could be violated. As the reform was passed in late December of 2002 and set the reform date to be April 1, 2003, there may have been different anticipatory responses. First, individuals in Minijobs might have postponed planned transitions to regular employment until after April 1. This would yield an upward bias in our estimated effect. Second, individuals who would have taken up regular employment after the reform might now have started regular employment even prior to the reform because it will become

<sup>13</sup> Hausman and Rapson (2018) label regression discontinuity designs (RDD) with time as the running variable 'regression discontinuity in time' (RDiT).

<sup>14</sup> In particular, we calculated calendar month-specific average transition rates and deducted their difference from the average of all calendar months from individual outcomes in the relevant months. To improve measurement, we calculated month-specific averages using observations from 6 years around the reform year (04/2000–03/2006). Transition rates are highest at the end of quarters, particularly at the end of the calendar year. Appendix Figure A.5 shows the detailed monthly deviations from calendar month-specific means over time.

<sup>15</sup> To the extent that adjustments in labor contracts take more time, we underestimate the true impact of the reform. Gudgeon and Trenkle (2024) show delayed responses to the 2003 reform.

more attractive starting April 1, 2003. This would downward bias our effect estimation. Finally, additional labor market entrants may have started Minijob employment prior to April 1, 2003, because they knew that it would become more attractive after April 1. This might cause an upward bias if pre-reform transition rates are attenuated. On balance, we do not expect these three mechanisms to be very effective.<sup>16</sup>

We will nevertheless consider alternative samples to gauge whether the reform affected the sample of Minijobbers over time. We omit Minijob employments that, first, were started after the reform was implemented (Sample B) and, second, were started after the reform legislation was passed (Sample C). If there were changes in the selection into Minijob employment after the reform, Sample B excludes such patterns from our data. If there were changes in the behavior of Minijobbers after the reform law was passed, Sample C eliminates these effects. If our results do not differ across the sampling strategies it appears unlikely that they are biased due to changed behaviors or selection mechanisms.

Another important aspect is that the Minijob earnings threshold increased on April 1, 2003 from 325 to 400 Euros at the same time as Midijobs were introduced. This may attenuate transitions out of Minijob employment: after April 1, 2003, small earnings increases could be realized without leaving a Minijob. Therefore, the estimate of  $\alpha_2$  represents the combined effect of introducing Midijobs and expanding the Minijob earnings ceiling.

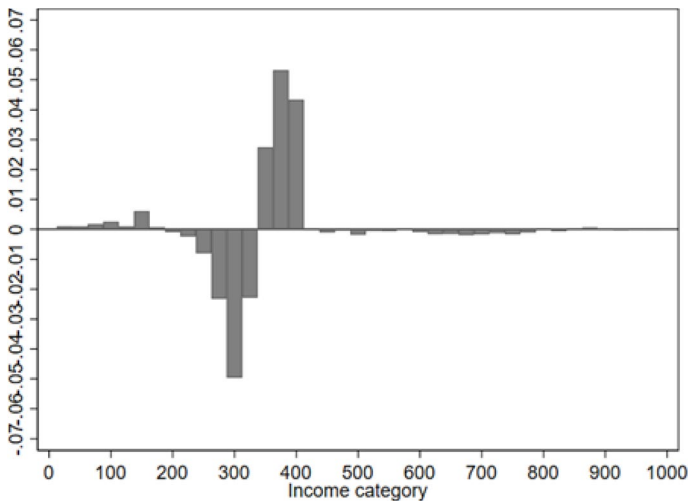
### 3.2 Administrative data for RDD analysis (SIAB)

For the analysis of changes in transitions out of Minijobs, we use administrative data. The Sample of Integrated Labour Market Biographies (SIAB) data offer a 2 percent random sample of all individuals registered with the unemployment insurance between 1975 and 2017 (Antoni et al., 2019).<sup>17</sup> The data provide precise information on the day-to-day employment status and job transitions. We consider individuals employed in a Minijob as their main employment in a time window of 12 months before and after the reform of April 1, 2003.<sup>18</sup> We select those aged 30–59 to exclude students and retirees who may be subject to additional regulations. We omit Minijobbers who at the same time receive unemployment benefits. We generate a

<sup>16</sup> If individuals planned a transition from a Minijob to regular employment in quarter 1 of 2003, the change in future conditions should not affect the current choices. In addition, they depend on employers to agree with the change in plans. Therefore, scenario one may not be used often. Second, if individuals make a transition to regular employment earlier than planned, i.e., prior to April 1, they are subject to high contribution rates. Only in rare circumstances would this be a rational choice. Finally, scenario three argues that there may be additional inflows to Minijobs in the first quarter of 2003 because they become more attractive later. Aggregate statistics indicate that the total number of Minijobbers is even slightly smaller on March 31, 2003 (4,135,827) than March 31, 2002 (4,147,453) (BA, 2014). Therefore, the effect cannot be large.

<sup>17</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. <https://doi.org/10.5164/IAB.FDZD.1902.en.v1>

<sup>18</sup> Minijobs are coded if the variable "erwerbsstatus" takes on values 109 or 209. Employments coded 101 are considered to be non-Minijob regular employments.



**Fig. 3** Change in the distribution of gross monthly earnings 2002–2004. Note: We grouped the number of monthly employment spells in the earnings range up to 1000 Euro in 25 Euro bins, defined as  $[x, x + 25[$  Euro. Then we calculated the frequency of each nominal earnings bin in 2002 and 2004. The figures show the difference in the relative frequencies of given earnings bins between the post-reform and the pre-reform periods. Positive (negative) values indicate an increase (decrease) in the relative frequency of employment in the relevant bin. Source: SIAB (2017), own calculations

monthly panel data set that comprises 853,241 monthly Minijob observations from 146,776 different Minijobs observed between April 1, 2002 and March 31, 2004 (Sample A). Dropping Minijob observations from employments that started after the reform date of April 1, 2003 reduces the sample size to 751,217 (Sample B). Omitting all Minijob observations from employments that started after 31.12.2002 reduces the sample size to 715,313 (Sample C). Appendix Table A.2 summarizes the characteristics of the three samples.

Our outcome indicates whether an individual  $i$  in a Minijob in month  $t$  left the Minijob by month  $t + 1$  for regular employment with earnings exceeding the Minijob earnings limit. In Sample A 0.87 percent of all monthly observations leave the Minijob for higher-paying employment.

Tables 1 (and A.3) show descriptive statistics and confirm that the vast majority of Minijobbers is female (89 percent). We describe mean outcomes for different groups and observe a very small increase in transition rates after the reform date, from 0.86 to 0.88 percent in the full sample. The transition rate of males is higher than that of females (0.014 vs. 0.008).

### 3.3 Baseline results of discontinuity analyses

Figure 4 presents graphic evidence of the development of the de-seasonalized transition rates with linear time trends for the three subsamples. We obtain mixed results: for the full sample A we find an upward shift in transition rates at the reform date and a change in slopes (Fig. 4a). With sample B we find no shift and declining

transition rates throughout (Fig. 4b), and with sample C we obtain a downward shift in transition rates (Fig. 4c).<sup>19</sup>

Columns 1–3 of Table 2 present estimation results for the three different samples using six different specifications each. The table describes the estimates of  $\alpha_2$ , i.e., the shift in transition rates at the reform date as shown in Eq. (1). Column 1 provides the  $\alpha_2$  estimates for Sample A. In row 1 we control for a linear time trend and allow for different slopes before and after the reform (see Table A.4 for the full results). The coefficient estimate is small and statistically insignificant. It indicates that the propensity to leave a Minijob for higher-earning employment increased on the reform date by 0.05 percent points or 6.5 percent of the pre-reform mean transition probability (see column entitled RE). Row 2 adds basic controls to the linear model (gender, age groups, citizenship, East vs. West German residence), row 3 considers additional control variables (education, tenure, occupation, industry, firm-size), rows 4–6 allow for quadratic time trends before and after the reform. Overall, we do not find large reform effects at the discontinuity.<sup>20</sup>

The estimates for  $\alpha_2$  of Sample B do not differ substantively from those in column 1. The relative effect sizes (see the column entitled RE) based on the quadratic specifications are again slightly larger than those in rows 1–3. In column 3 we repeat our analyses for Sample C. The estimate in row 1 turns significantly negative while all other specifications again yield small positive and statistically insignificant estimates.

On April 1, 2003 two changes were implemented: Midijob subsidies were introduced and the Minijob earnings threshold increased from 325 to 400 Euro. The shift of the Minijob earnings ceiling may have attenuated any positive Midijob effect because prior Minijob employees could realize higher earnings without leaving the Minijob framework. We apply two approaches to determine whether the change in the Minijob earnings ceiling affected our results. First, we exploit the increase of the Minijob earnings ceiling from 400 to 450 Euro per month on January 1, 2013 to study the potential impact of a change in the Minijob earnings ceiling on transition rates. Column 4 of Table 2 presents the estimation results that obtain when we apply the procedure used in column 1 to the reform of 2013 (see Table A.5 for descriptive statistics). Our sample here considers individuals who worked in Minijobs between Jan. 1, 2012, and Dec. 31, 2014. The estimated  $\alpha_2$  coefficients are all negative. This agrees with our expectation that after the range of Minijob earnings is extended the propensity to leave Minijobs should fall: labor supply expansions can be realized more easily within Minijobs. The relative effect sizes are small and statistically significant only for the quadratic specification. Overall, these results suggest that the extension of the Minijob earnings ceiling in 2013 did not affect transition rates out of Minijobs in important ways. If the same pattern had held ten years earlier for the 2003 reform, then the positive effects observed in columns 1–3 would have been larger without the change in thresholds. Thus, we may underestimate the impact of

<sup>19</sup> Appendix Figure A.6 presents these graphs by gender and finds large positive discontinuities for men and no changes for women.

<sup>20</sup> We also estimated models with cubic time trends. Again, we obtain positive and significant estimates of  $\alpha_2$  which are slightly larger than in the quadratic specification.

the introduction of Midijobs in 2003. In a second approach, we redefine the outcome measure in the pre-reform period to indicate transitions to regular employments earning beyond 400 Euro per month rather than merely an exit from Minijob employment. This eliminates the change in the earnings threshold that occurred on the actual reform date. Table A.6 in the appendix shows that the main results are hardly affected by this adjustment.<sup>21</sup>

We offer tests for sorting in covariates in Table A.7. It shows the results of using our covariates as outcomes in RD estimations. Except for gender in Sample A, we do not observe sorting for the covariates of the basic specification.

### 3.4 Regression discontinuity results—heterogeneity and robustness

Given the heterogeneity in the Minijob utilization of men and women, we separately analyze the two subsamples. We observe only 95,277 male but 757,964 female Minijob months with much higher average monthly transition rates for men (1.4 percent) than for women (0.8 percent). Figure 5a confirms this level difference and suggests that while males responded to the reform, females did not. Columns 1 and 2 of Table 3 show the estimation results for  $\alpha_2$  based on Sample A as in Table 2 by gender. The estimates indicate substantial differences: While the estimates for males are statistically significant, positive, and sizable those for females are insignificant, small, and partly negative. The effects for men indicate an increase in the mean transition rate out of Minijobs at the point of the reform of about 0.6 to 0.9 percentage points or 50–80 percent of the pre-reform mean; the effects for women are close to zero.<sup>22</sup> So, while we hardly find a change in average transition rates when the Midijob subsidy was introduced in 2003, we find substantial gender heterogeneities. This means that the overall effect in Table 2 is mainly driven by males, i.e., 10 percent of the observations in our sample.

An important gender difference relates to the heterogeneity of income tax rates between secondary earners in marriages—typically females—and single individuals. In contrast to non-married individuals, secondary earners in married couples are mostly affected by high income tax burdens. Women often use Minijob employment when they are secondary earners in married couples. For them, Midijobs hardly increase the incentives to expand the labor earnings beyond the Minijob cutoff (see Fig. 2). Section 4 (below) exploits this heterogeneity and offers detail on the institutional background. Here, we compare the two groups' responses to the reform. Based on correlation patterns of secondary earner status as observed in survey data we predicted secondary earner status based on the SIAB administrative data. Then, we separated two groups in Sample A based on predicted single vs. secondary earner status (for details see Appendix B). Figure 5b depicts transition patterns over time based on predicted secondary earner status: while single earners responded to

<sup>21</sup> In addition, it is noteworthy that the majority of exits from Minijobs to regular employment immediately transition to earnings beyond the Midijob threshold of 800 Euro. Fewer than 10 percent remain within the earnings range of up to 100 Euro above the Minijob earnings threshold suggesting a limited relevance of the change in the Minijob earnings threshold.

<sup>22</sup> We show the relevant 2013 estimation results for males and females in Appendix Table A.8.

**Table 1** Descriptive Statistics—Basic Controls (SIAB Sample). *Source:* SIAB (2017) and own calculations

Variable	Full sample		Mean Transition Rate when variable has		Mean by gender	
	Mean	Std. Dev	value 0	value 1	Men	Women
Transition (0/1)	0.0087	0.0929	0.0000	1.0000	0.0142	0.0080
After (0/1)	0.5194	0.4996	0.0086	0.0088	0.5559	0.5149
Time	23.009	213.55	—	—	38.1960	21.1000
Female (0/1)	0.8883	0.3150	0.0142	0.0080	0.0000	1.0000
Age: 30–34 (0/1)	0.1609	0.3675	—	0.0160	0.1629	0.1607
Age: 35–39 (0/1)	0.2138	0.4100	—	0.0112	0.1435	0.2227
Age: 40–44 (0/1)	0.1975	0.3981	—	0.0089	0.1448	0.2042
Age: 45–49 (0/1)	0.1552	0.3621	—	0.0066	0.1466	0.1563
Age: 50–54 (0/1)	0.1472	0.3543	—	0.0041	0.1807	0.1430
Age: 55–59 (0/1)	0.1253	0.3311	—	0.0026	0.2214	0.1132
East Germany (0/1)	0.0595	0.2365	0.0083	0.0146	0.1468	0.0485
Foreign nationality (0/1)	0.0854	0.2795	0.0084	0.0126	0.1094	0.0832

Note: The descriptive statistics describe the sample of 853,241 person-year observations, with 95,277 male and 757,964 female observations. The data are not weighted

the reform, those predicted to be secondary earners did not. In columns 3 and 4 of Table 3, we describe the heterogeneity in discontinuity results for the two groups. The results indicate substantial differences between the two subsamples. While the transition rates to higher-earning regular employment among Minijobbers who are likely to be married do not change after the reform we find positive significant effects among non-married individuals.

So far, we investigated shifts in transition rates from Minijobs to employments with higher earnings and found significant increases after the reform for the male and predicted non-married subsamples. In Table A.9, we separately inspect transitions from Minijobs to Midijobs and from Minijobs to higher-earning, i.e., regular employment beyond the Midijob earnings range for two subsamples. We observe positive effects for both types of transitions. However, the propensity to leave for regular employment appears to respond much stronger to the reform.

## 4 Heterogeneous treatment effects by tax status?

### 4.1 Empirical strategy: difference-in-differences estimation

Our second empirical strategy considers a difference-in-differences (DID) framework to study Midijobs' effects on the propensity to exit Minijobs for regular jobs. We exploit the heterogeneity of the Minijob-induced notch in the net earnings distribution that exists for individuals with different income tax rates.

**Table 2** RDiT Estimates of Transitions out of Minijobs–SIAB. *Source:* SIAB (2017) and own calculations

Specification	Sample A—2003		Sample B—2003		Sample C—2003		Sample A—2013	
	(1)		(2)		(3)		(4)	
	Coeff	RE	Coeff	RE	Coeff	RE	Coeff	RE
1 Linear no controls	0.0005	6.5%	0.0001	0.7%	−0.0007*	−8.2%	−0.0003	−1.9%
2 Linear basic controls	0.0004	4.7%	0.0001	1.1%	−0.0006	−7.1%	−0.0003	−1.7%
3 Linear ext. controls	0.0001	1.2%	0.0003	3.5%	0.0001	1.2%	−0.0005	−3.3%
4 Quadratic no controls	0.0013**	14.8%	0.0011*	13.1%	0.0004	5.3%	−0.0014*	−8.4%
5 Quadr. basic controls	0.0012*	14.1%	0.0011*	13.0%	0.0005	5.5%	−0.0014*	−8.4%
6 Quadr. ext. controls	0.0009	10.6%	0.0013**	15.3%	0.0007	8.6%	−0.0015**	−8.9%
N	853,241		751,217		715,313		1,062,892	
Pre-reform mean Y	0.0085		0.0085		0.0084		0.0167	

Note: Linear regressions, standard errors clustered at the individual level; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\* $p < 0.01$ . The columns entitled “Coeff” provide the estimate of  $\alpha_2$  in Eq. (1), “N” and “Pre-reform mean Y” provide the number of observations and pre-reform mean of the outcome for each sample. “RE” is the relative effect, i.e., the ratio of the coefficient estimate of  $\alpha_2$  and the pre-reform mean of the dependent variable and characterizes the relative effect size. The models without added controls (in rows 1 and 4) control for ‘after’ indicator, the re-centered time trend (linear or quadratic), their interaction(s), and a constant term. The vector of basic control variables (in rows 2 and 5) additionally account for gender, 5 age group indicators, East German residence, and German citizenship. The vector of extended controls (in rows 3 and 6) additionally accounts for 3 indicators of educational attainment, 7 indicators of tenure, 8 indicators of occupation, 4 indicators of firm size, and 9 indicators of industry. Sample A considers all Minijob-months observed between April 1, 2002 and March 31, 2004. Sample B drops those Minijobs that were started after April 1, 2003 and Sample C drops those Minijobs that were started after Dec. 31, 2002. Sample A-2013 replicates Sample A-2003 around the reform date of January 1, 2013

Figure 2 shows the relationship between gross and net income. The solid lines describe the situation for a person with no income tax obligation, and the dashed lines describe the situation for a person with relatively high income tax rates, both before and after the reform. For both groups, net income falls at the Minijob earnings ceiling. The decline is larger for the individual with income tax obligations. The notch characterizes the Minijob employment trap. In both cases, the reform (blue lines) attenuates the drop in net earnings and reduces the disincentives to expand labor supply. After the reform, the notch almost disappears for individuals without income tax obligations but not for those subject to income taxes. Therefore and based on the results of the discontinuity analyses in Table 3 and Fig. 5, we hypothesize that individuals with low income tax rates (e.g., non-married individuals) respond more strongly to the reform than those with high income tax rates (e.g., secondary earners in marriages) because the reform made a relevant difference for the former but not for the latter (Fig. 6).

To test this hypothesis our outcome of interest ( $Y$ ) again is a dichotomous indicator of whether an individual  $i$  in a Minijob in period  $t$  leaves the Minijob between period  $t$  and  $t+1$  and transits to employment with earnings above the Minijob threshold. We distinguish the situation before and after the reform of April 1, 2003



**Fig. 4** Monthly transition rate from Minijob Employment (April 2002–March 2004). **a** Sample A (all Minijob observations in the observation window). **b** Sample B (Sample A without Minijobs started after April 1, 2003). **c** Sample C (Sample B without Minijobs started after Dec. 31, 2002). Note: The graphs represent the development of monthly average transition rates from Minijob to regular employment. The sample includes all who hold a Minijob as their main employment without being registered unemployed in a given month. The monthly transition rates are de-seasonalized. For a representation of these graphs by gender see Appendix Figure A.6. *Source:* SIAB (2017) and own calculations

(after) and we differentiate groups that were affected to different extents: secondary earners in married couples are subject to high income tax rates; we consider them as our control group observations ( $treat=0$ ).<sup>23</sup> For them, the Midijob subsidy of social insurance contributions hardly reduces the relevant notch in the earnings distribution. In contrast, non-married individuals with or without a partner in the household enjoy an individual tax allowance that exempts annual earnings of up to about 9,000 Euro from income tax payments (*Grundfreibetrag*). For these individuals marginal and average income tax rates at the Minijob earnings threshold are low and—depending on other sources of income—may even be zero. Therefore, the introduction of the Midijob subsidy constitutes a relevant reduction in the notch in their net earnings distribution. We consider them as the treatment group of the reform ( $treat=1$ ).

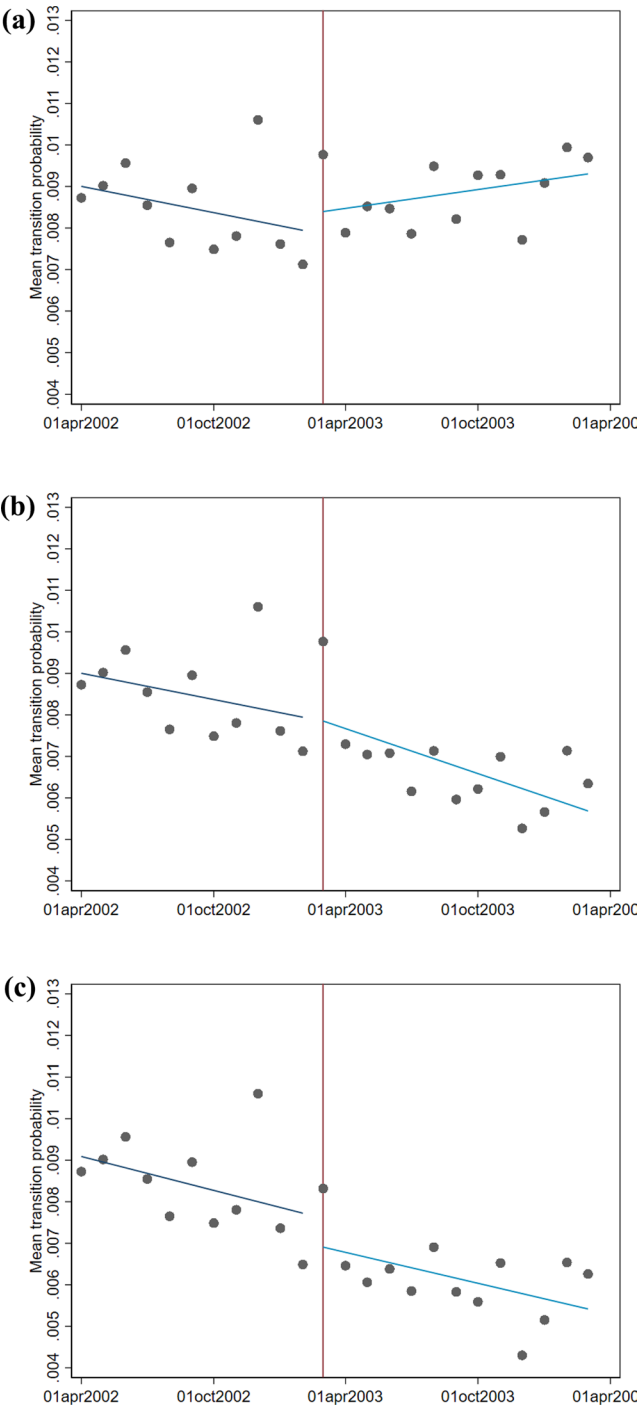
We use a standard DID model and consider covariates ( $X$ ) to reduce the residual variance. Let  $\mu$  be a random error. We estimate the coefficient vectors  $\alpha$  and  $\beta$  in this model:

$$Y_{it+1} = \alpha_0 + \alpha_1 \text{after}_{it} + \alpha_2 \text{treat}_{it} + \alpha_3 (\text{after}_{it} \cdot \text{treat}_{it}) + \beta_1 X_{it} + \mu_{it} \quad (2)$$

Our effect of interest is the estimate of  $\alpha_3$ . It indicates whether individuals with low income tax burdens ( $treat=1$ ) changed their propensity to transition out of the Minijob earnings range after the reform more than individuals with high income tax burdens ( $treat=0$ ). If such a difference exists it suggests that the reform reduced the Minijob trap at least for part of the population.

The DID estimate represents a causal reform effect if several conditions are met: first, without the reform, the time trend in the propensity to leave a Minijob for higher-earning employment should have developed in parallel for individuals in the treatment and the control group. We inspect the evidence on pre-reform trends in the next section. Second, the reform should not affect treatment or control groups in ways other than through the introduction of the Midijob subsidy. This requirement is violated as the reform not only introduced the Midijob payroll tax subsidy for earnings above the Minijob earnings threshold but also increased the Minijob earnings threshold itself from 325 to 400 Euro per month. However, as the increased earnings threshold affected treatment as well as control group observations, it will bias the estimate of the effect of the Midijob introduction only if the two groups respond differently to the threshold shift. In that case,  $\alpha_3$  partly reflects heterogeneous responses

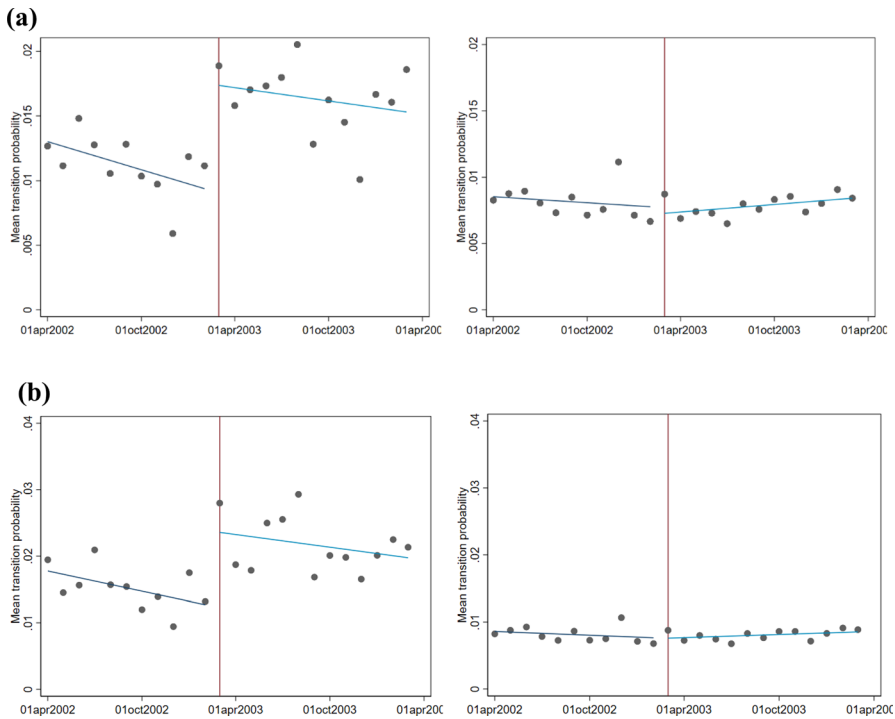
<sup>23</sup> Gudgeon and Trenkle (2024) focus their analysis of Minijob reforms on married women exactly because they are subject to a large notch in their potential earnings. Unfortunately, these authors' administrative data is not available to us.



**Table 3** RDIT Estimates—SIAB—by gender and predicted secondary earner status. *Source:* SIAB (2017) and own calculations

Specification	Sample A—2003			Sample A—2003		
	(1) Men		(2) Women		(3) Pred. Non-Married	
	Coeff	RE	Coeff	RE	Coeff	RE
1 Linear no controls	0.0083	***	−0.0004	−5.2%	0.0152	***
2 Linear basic controls	0.0078	***	−0.0005	−6.1%	0.0149	***
3 Linear ext. controls	0.0061	***	−0.0007	−8.5%	0.0129	***
4 Quadratic no controls	0.0063	***	0.0003	3.3%	0.0135	***
5 Quadr. basic controls	0.0092	***	0.0002	2.4%	0.0136	***
6 Quadr. ext. controls	0.0071	***	0.0001	1.2%	0.0118	***
N	95,277		757,964		54,183	
Pre-reform mean Y	0.0113		0.0082		0.0177	
					890,793	
					0.0092	

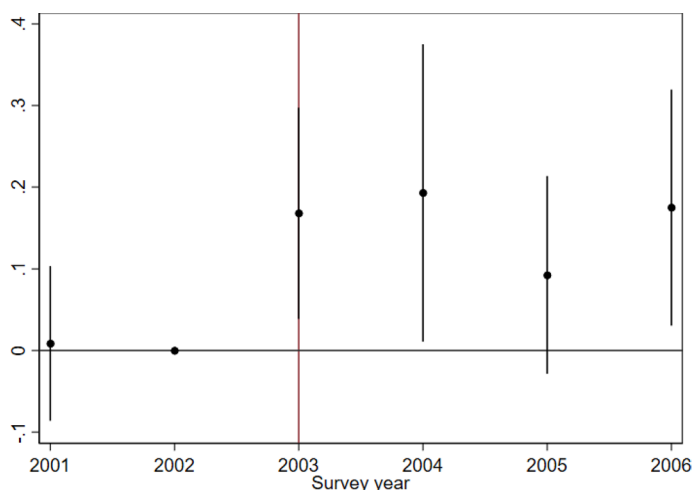
Note: See Table 2. Columns 3 and 4 split the sample based on the predicted probability of being non-married, i.e., single earner vs. married, i.e., likely secondary earner, which was based on the linear regression estimated on SOEP data (see Appendix B)



**Fig. 5** Monthly transition rate from Minijob Employment (April 2002–March 2004). **a** By gender (males left panel, females right panel). **b** By predicted single earner status (single earners left panel, secondary earners right panel). Note: The graphs represent the development of monthly average transition rates from Minijob to regular employment. The sample includes all who hold a Minijob as their main employment without being registered unemployed in a given month. The monthly transition rates are de-seasonalized. Both figures use subsamples from Sample A. Please note that the y-axis of Panel 5b is scaled different from Panel 5a. Source: SIAB (2017) and own calculations

to the change in the threshold. To gauge the relevance of this shift in the Minijob earnings ceiling for Minijob exits we again exploit a later adjustment in the Minijob earnings threshold: on January 1, 2013, the monthly Minijob/Midijob earnings thresholds increased from 400/800 to 450/850. We test this reform's effect on transitions out of Minijobs to approximate the impact of the 2003 change in the earnings ceiling from 325 to 400 Euros.

A third identification requirement is that there are no systematic changes in the composition of the workforce in response to the treatment. First, we examined whether individuals might switch between treatment and control groups in response to the reform. We found that individuals changed their marital status in both directions in similar magnitudes before and after the reform. Second, we evaluated whether there were compositional changes with respect to observable characteristics within the treatment or control group over time. Such changes might indicate differential selection into the two groups in response to the reform. Table A.10 presents the p-values for tests of the hypothesis that the mean values of treatment and control



**Fig. 6** Event study analysis of parallel trends (SOEP). Note: The figure depicts estimated coefficients from linear regressions and their confidence intervals. The outcome of transitions from Minijobs to regular employment is regressed on year indicators and their interaction with the treatment indicator. We depict 90 percent confidence intervals of the interaction term coefficients using standard errors that are clustered at the individual level. Source: SOEP wave 35 and own calculations

group characteristics changed over time. We observe some minor adjustments but no major changes in characteristics.<sup>24</sup>

The final identification requirement is the absence of anticipation effects. If in response to the reform, Minijobbers postponed their transition to higher-earning employment or changed the take up of Minijobs altogether this biases our estimates if it affects treatment and control group observations differently. The reform was passed into law on December 23, 2002, and became effective on April 1, 2003, which does allow for potential anticipatory behavior (see our discussion in Sect. 3.1). Even though there is no rationale as to why treatment and control group observations might differ in their response, we nevertheless inspect whether our estimates are sensitive to the time window of our sample.

This identification strategy exploits potential heterogeneity in treatment effects. Comparing the behavioral responses of more (treatment group) and less (control group) strongly affected individuals does not indicate the overall average effect of the Midijob introduction. However, it can offer evidence as to whether there is an effect at all if those most affected respond differently from those least affected.

<sup>24</sup> In the control group, the share of female observations drops from 97 to 95 percent and the share with tertiary education increases from 7 to 9 percent. In the treatment group, the share of observations in East Germany increases and the grouping of Minijobbers in the smaller firm size categories is shifted somewhat.

## 4.2 Survey data for difference-in-differences analysis (SOEP)

Our survey data are taken from the German Socio-Economic Panel Study (SOEP), an annual household panel survey collected since 1984 (Goebel et al., 2019).<sup>25</sup> We use data covering the years 2001–2006 to evaluate the 2003 reform. Again, we restrict our sample to individuals aged 30–59 to omit students and retirees.<sup>26</sup>

We are interested in whether the reform affected the propensity to transition from Minijob employment to regular employment differently for more and less affected individuals. Our sample considers individuals employed in a Minijob as their main employment at the time of the annual survey. Since 2001, the survey asks directly about Minijob employment. We use this self-reported information and consider only those individuals to be in a Minijob who additionally indicate to earn no more than the Minijob earnings threshold.<sup>27</sup> This leaves us with 2,736 person-year observations of 1,255 different individuals in Minijob employment in the years 2001–2006. The 1,255 individuals are observed in 1,604 different Minijob employment relationships over time. Unfortunately, the sample size is much smaller than in the administrative data. However, as the administrative data do not provide information on marital status this analysis requires survey data. Table 4 provides descriptive statistics on our sample. The vast majority of our Minijobbers is female (94.4 percent) with the largest group aged 35–44 (mean age is 43). Relative to aggregate population shares Minijobs are used relatively more intensely in West than in East Germany and more by German citizens than by non-citizens. In our sample, about 90 percent of the observations are married and thus in our control group. The treatment group comprises those who are single (including those in cohabiting couples), divorced, or widowed. We observe 173 (276) and 1,082 (2,460) different individuals (person-year observations) in the treatment vs. control groups, respectively (for descriptive statistics on additional controls see Appendix Table A.11).<sup>28</sup>

Our dependent variable indicates whether a person held a Minijob in period  $t$  and in period  $t + 1$  transitioned to regular employment paying social insurance contributions and earning above the Minijob earnings limit; we evaluate transitions between 2001 and 2006. The average annual transition rate is 10.3 percent. The last two columns of Table 4 describe the mean transition rates for different groups. We observe higher rates after than before the reform date. As expected, married individuals ( $\text{treat} = 0$ )

<sup>25</sup> We use SOEP v35 (1984–2018), DOI <https://doi.org/10.5684/soep-core.v35>.

<sup>26</sup> We account for oversampling and non-response in the data by applying the cross-sectional sample weights provided with the SOEP data. In contrast to our analysis of register data we do not omit Minijobbers who are unemployed here due to the resulting small sample sizes.

<sup>27</sup> Due to the second restriction, we lose 663 of 3,399 observations or 19.5 percent of those who indicated to work in a Minijob. We also drop two individuals for whom information on marital status, our treatment indicator, is missing. We do not use information from earlier survey years because they applied a different survey question to collect employment status information.

<sup>28</sup> We use a time varying treatment assignment where individuals enter the control group upon marriage. Ideally, the group assignment would be fixed prior to the treatment, e.g., based on marital status in 2002 or 2001. However, this reduces our sample size by about half. The estimates are robust in terms of their signs but have large standard errors. We consider it implausible that changes in marital status are connected to the introduction of Midijobs.

have a much lower average transition rate than non-married individuals ( $\text{treat}=1$ ). The transition rate of men is higher than that of women (16 vs. 11 percent). As in the SIAB data, the likelihood of leaving Minijobs declines with age.

Figure A.7 shows the development of transition rates separately for our treatment and control groups using weighted data. Unfortunately, our data provide only two annual observations prior to the reform, i.e., 2001 and 2002. However, in these years the development of transition rates out of Minijobs is similar for treatment and control groups which suggests parallel paths prior to the reform.<sup>29</sup>

### 4.3 Results: difference-in-differences analyses

Table 5 shows our first set of DID results. Column 1 offers results without control variables and shows that the estimate of  $\alpha_3$  is statistically significant and positive. It suggests that after the reform the treatment group of non-married individuals increased their propensity to leave a Minijob for regular employment by about 14 percentage points more than the control group of married persons; relative to a mean transition rate of about 10 percent this is a rather large unconditional effect. The result is confirmed in columns 2 and 3, where we first consider controls for basic demographics (gender, age group, East German residence, and foreign citizenship) and then add an extended set of controls (see table notes and Appendix Table A.11 for descriptive statistics). Column 4 shows that the estimation results are robust when we replace the overall ‘post’ effect with a set of calendar year fixed effects.<sup>30</sup>

Table A.12 presents the results of additional tests and describes effect heterogeneities. Column 1 shows the results after omitting those observations for which we cannot be sure whether a potential transition happened before or after the reform on April 1, 2003.<sup>31</sup> The estimates on the thus reduced sample confirm the significant positive treatment effect. In columns 2 and 3 we evaluate the sensitivity of the results to the considered time window of observations. First, we omit two years of post-reform observations (column 2) and then we add an additional post-reform observation year (column 3): our main result hardly changes. In column 4 we omit male observations; the result shows that women respond substantially less to the reform than men, confirming the results of our RDIT analyses. Omitting observations with

<sup>29</sup> In a separate analysis, we estimated an event study to describe the pre- and post-treatment differences between treatment and control groups. We regressed the outcome on year indicators and their interactions with the treatment indicator but without additional controls. Figure 6 shows the differences between treatment and control groups. Again, outcome differences prior to the reform are insignificant, and transition rates for the non-married increase faster after the reform than those for married Minijobbers.

<sup>30</sup> We use sampling weights in the analyses of SOEP data. The results in Table 5 are sensitive to this choice.

<sup>31</sup> The uncertainty is due to the annual interview which informs only about the status at the time of the interview but not about when a status change occurred. We omit two groups of observations: those for whom we know the Minijob status in 2002 but do not know whether the transition to the 2003 status took place before or after the reform date of April 1, 2003 and those for whom we know their Minijob status prior to April 1, 2003 but do not know whether their transition to the 2004 status took place before or after the reform date of April 1, 2003.

**Table 4** Descriptive Statistics—Basic Controls: 2003 Reform Sample (SOEP). *Source:* SOEP wave 35 and own calculations

Variable	Descriptives	Mean transition rate when variable has		
	Mean	Std. Dev	Value 0	Value 1
Transition (0/1)	0.1031	0.3041	0.0000	1.0000
Post (0/1)	0.5808	0.4935	0.0850	0.1179
Treat (0/1)	0.1009	0.3012	0.0995	0.1809
Female (0/1)	0.9441	0.2298	0.0952	0.2353
Age: 30–34 (0/1)	0.1648	0.3711	–	0.1537
Age: 35–39 (0/1)	0.2288	0.4201	–	0.1347
Age: 40–44 (0/1)	0.1988	0.3992	–	0.1116
Age: 45–49 (0/1)	0.1659	0.3721	–	0.1037
Age: 50–54 (0/1)	0.1411	0.3482	–	0.1108
Age: 55–59 (0/1)	0.1005	0.3007	–	0.0678
East Germany (0/1)	0.0757	0.2645	0.1085	0.1667
Foreign Nationality (0/1)	0.1137	0.3175	0.1148	0.1068

Note: The descriptive statistics describe the sample of 2,736 person-year observations. The data are not weighted

East German residence as in column 5 reveals somewhat larger effects in West than in East Germany.<sup>32</sup>

Finally, we need to account for the fact that the reform of April 1, 2003 shifted the Minijob earnings ceiling from 325 to 400 Euro per month. If treatment (the non-married) and control (the married) groups responded differently to this change this may bias our finding. In order to gauge the overall relevance of the ceiling shift we consider the reform of January 1, 2013 when the Minijob earnings ceiling was increased again, this time from 400 to 450 Euro per month. We evaluate the impact of this reform on changes in transition propensities. We use the same sample and treatment definitions as before just shifting the observation period to 10 years later (see Tables A.11 and A.13 for descriptive statistics).<sup>33</sup> Table A.14 shows the results on Minijob transitions for the period 2011 to 2016. We evaluate the reform heterogeneity for treatment and control groups around the increase in the Minijob earnings ceiling from 400 to 450 Euros on January 1, 2013. In this case, the results yield a negative and statistically insignificant estimate of  $\alpha_3$ . Overall, the transition rates

<sup>32</sup> Marital status may be a weak proxy for the income tax burden. Therefore, we investigated whether it might represent alternative mechanisms, instead. We replaced our treatment indicator of not married (T) vs. married (C) individuals by several alternatives. First, for the sample of married persons ( $N=2,460$ ) we used an indicator of whether a person has children (C) or not (T). Second, we considered non-married individuals with (C) vs. without stable partners (T). In neither case did we obtain statistically significant treatment effects. Third, we compared non-married (T) individuals only to married individuals without children (C) to safeguard against effects of childcare. Here, we continue to find significant positive treatment effects, supporting our main results.

<sup>33</sup> We drop 32 individuals for whom information on marital status, our treatment indicator, is missing in the data.



**Table 5** DID Estimates—2003 reform baseline results (SOEP). *Source:* SOEP wave 35 and own calculations

	Full sample (1)	Full sample (2)	Full sample (3)	Full sample (4)
Post	0.004 (0.015)	0.004 (0.015)	−0.000 (0.015)	—
Treat	0.002 (0.029)	−0.022 (0.031)	−0.038 (0.032)	−0.0041 (0.033)
Post * treat	0.139*** (0.052)	0.123** (0.051)	0.131*** (0.050)	0.134*** (0.050)
Controls—basic	No	Yes	Yes	Yes
Controls—extended	No	No	Yes	Yes
Year fixed effects	No	No	No	Yes

Note: All estimations use 2,736 person-year observations. Linear regressions, standard errors clustered at the individual level are in parentheses. The vector of basic controls accounts for an indicator of gender, 5 indicators of age group, an indicator of East German residence, and an indicator of non-German citizenship. The vector of extended controls accounts for 5 education indicators, 7 tenure indicators, 4 firm size indicators, 9 industry indicators, and 8 occupation indicators. Column 4 replaces the post indicator with a set of calendar year fixed effects. The estimations use cross-sectional sample weights to account for non-response and oversampling. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

increased slightly for the control group of married individuals after the reform (see row 1) and transition rates of the treatment group are generally significantly higher (row 2). Importantly, the reform did not affect the relative transition rates of the two groups. If these patterns similarly held in 2003 our findings of a significant increase in transition rates after the 2003 reform as reported in Tables 5 and A.12 are not likely to be biased by the change in Minijob earnings ceiling that happened simultaneously with the introduction of the Midijob subsidy. The findings in Table A.14 corroborate our finding of a significant and large increase in transitions out of Minijobs after the 2003 reform for our treatment group, the non-married.

#### 4.4 Difference in differences analyses using administrative data

Compared to the survey data, our administrative data offer larger samples, more precise information, and are observed at monthly instead of annual frequency. Even though the administrative data lack information on marital status, we can replicate the difference-in-differences analyses using this larger database. In this section, we show two approaches to approximate the survey-based analyses. In the first approach, we replace the non-married treatment indicator from Sect. 4.3 with an indicator of male gender; in the survey data, the share of males among the non-married (22.1 percent) is six times as high as the share of males among the married (3.7 percent). Therefore, this might be a reasonable first approximation. In the second approach, we estimate a prediction equation for being non-married with the survey data and use it to predict non-married status in the administrative data. The

predicted non-married status is then used as a treatment indicator in the difference-in-differences analysis (for details see Appendix B).

Similar to the survey data, the SIAB sample now considers all individuals aged 30–59 observed in Minijob employment between April 1, 2001 and March 31, 2006. As in Sect. 3, we omit unemployed Minijobbers because they face different incentives. This yields 2,831,554 monthly observations. The dependent variable describes the transition to regular higher-paying employment. Its mean is at 1.2 percent of the monthly observations.

Table 6 shows the results of the difference-in-differences analyses. In columns 1–3 the treatment indicator reflects male gender and in columns 4–6 the treatment indicator reflects predicted non-married status. In both, we show the results of the three different specifications as in columns 1–3 of Table 5. In contrast to the estimations using survey data the results in Table 6 are much more precise and confirm previously observed patterns. Across all columns, we find significantly larger increases in transition probabilities from Minijobs to regular employment among males and individuals predicted to be non-married relative to females and married individuals (i.e., likely secondary earners), respectively. The treatment effect among males is about 0.4 percentage points (column 3) and that among the non-married is 0.32 percentage points (column 6) above that of the control group. These are large effects compared to the mean monthly transition rate of 1.2 percent. While this relative effect size is smaller than that observed with the survey data based on annual observations, the patterns confirm the survey-based evidence.

#### 4.5 Difference in differences analyses using separate outcomes

So far, we investigated reform effects on transitions from Minijobs to employments with higher earnings and found significant treatment effects. We confirmed the results of the survey-based analysis when using proxy indicators of independent tax status from administrative data. In Tables A.15 and A.16, we separately inspect transitions from Minijobs to Midijobs and from Minijobs to higher-earning employment. Surprisingly—and confirming the results presented in Table A.9—we find that the propensity to leave a Minijob for regular employment appears to respond stronger to the reform than leaving for Midijobs. This pattern does not agree with the predictions of a bunching model. We leave it for future research to study this development.

## 5 Conclusions

Many countries subsidize low-income employment or small jobs. The phasing out of such subsidies can affect labor supply incentives (Eissa & Hoynes, 2006; Hoynes, 2007). We study the German *Minijob* program which subsidizes low-income jobs and generates a ‘Minijob trap’ with substantial bunching along the earnings distribution. In 2003, the *Midijob* subsidy was introduced to reduce the Minijob-induced notch in the net earnings distribution and to ease the phasing-out of Minijobs. We

are the first to investigate whether introducing Midijobs effectively reduced the Minijob trap.

The reform that we study here is of substantial relevance for the German labor market. By 2019, about 16 percent of the total German employed labor force was employed in Mini- or Midijob employments. Also, Midijob coverage was recently expanded vastly to cover earnings up to 2,000 Euro per month—without any evidence of its effectiveness.

We use two complementary identification strategies to investigate the effect of the introduction of Midijobs on the propensity to exit Minijobs for regular employment. Our first empirical strategy exploits the discontinuity in the regulation over time and uses a large administrative dataset for a regression discontinuity-type approach. We evaluate the jump in transition rates that is observed at the moment of the reform. We find small significant increases in transitions out of Minijob employment starting April 1, 2003. Heterogeneity analyses of this local effect yield that male Minijobbers strongly responded to the Midijob reform whereas the Midijob introduction hardly affected females' transitions out of Minijob employment.

Our second empirical strategy exploits the heterogeneity in the Midijob effect for individuals with different income tax obligations. We compare the response of those hardly affected by the reform (secondary earners in marriages) to that of those more strongly affected by the reform (non-married individuals) in a difference-in-differences strategy. Based on survey data we find that individuals for whom the reform effectively reduced the notch in the net earnings distribution indeed increased their transition rate out of Minijob employment significantly stronger than the control group of married individuals for whom we do not observe a change in transition rates after the reform. This suggests that the reform may have been effective in reducing the Minijob trap for some employees. Our analyses show the combined effect of the introduction of Midijobs and an increase in the Minijob earnings threshold. We can show that the relevance of the latter is limited by analyzing a later increase in the Minijob earnings threshold.

Both empirical strategies yield that the reform was effective on average. However, based on the first strategy that conclusion does not hold for females who make up about 90 percent of our sample. Based on the second strategy the conclusion does not hold for married individuals who, also, account for about 90 percent of our sample. To the extent that the original objective of the Midijob subsidy was to reduce the notch in the net earnings distribution, it was ineffective as a phasing-out tool for most Minijobbers (see also Carrillo-Tudela et al., 2021). Interestingly, even those individuals who responded to the introduction of Midijobs with increased transition rates out of Minijobs were more likely to directly transition to regular employment as opposed to taking advantage of the Midijob subsidy.

Given the institutional setting, it is unsurprising that the policy reform effectively only treated a few workers. Since the reform did not address the notch generated by income taxes it structurally excluded a vast majority of Minijobbers from the reform benefits, i.e., secondary earners in married couples who are mostly women. In contrast, the Midijob benefits did accrue to non-married workers, who responded by substantially increasing their transition rates from Minijobs to regular higher-paying

**Table 6** DID Estimates—2003 reform results for two treatment groups (SIAB). *Source:* SIAB (2017) and own calculations.

	Treat = male			Treat = non-married		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0012*** (0.0002)	0.0009*** (0.0002)	0.0009*** (0.0002)	0.0014*** (0.0002)
Treat	0.0040*** (0.0005)	0.0051*** (0.0005)	0.0036*** (0.0005)	0.0083*** (0.0009)	0.0007 (0.0009)	0.0007 (0.0009)
Post * treat	0.0062*** (0.0006)	0.0050*** (0.0006)	0.0041*** (0.0006)	0.0039*** (0.0010)	0.0039*** (0.0010)	0.0032*** (0.0010)
Controls—basic	No	Yes	Yes	No	Yes	Yes
Controls—extended	No	No	Yes	No	No	Yes

Note: All estimations use 2,831,554 person-month observations. The estimations are based on linear regressions. Standard errors clustered at the individual level are in parentheses. In columns 1-3 the treatment indicator reflects male gender and in columns 4-6 the treatment indicator reflects a predicted high probability of being not married. The vector of basic controls accounts for an indicator of gender (not in columns 1-3), 5 indicators of age group, an indicator of East German residence, and an indicator of non-German citizenship. The vector of extended controls accounts for 3 education indicators, 7 tenure indicators, 4 firm size indicators, 9 industry indicators, and 12 occupation indicators. The pre-reform mean transition probability is 0.918 percent per month. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

jobs. To abolish the Minijob trap for all Minijobbers the income tax-induced notch in the earnings distribution at the Minijob earnings threshold must be smoothed.

The design of small jobs subsidies in the German setting can be improved in different ways. One option is to generally abolish the tax exemption of Minijobs. Another option is to modify the income tax splitting system by reducing marginal income taxes for secondary earners or by offering basic tax exemptions for their earnings. A third alternative could be to expand the Midijob subsidy to cover a phase-in of income taxes plus social insurance contributions.<sup>34</sup>

The problems resulting from phasing out small jobs subsidies essentially derive from their combination with the income tax splitting system for married couples. Our results suggest that in countries with individual income tax regimes policy instruments such as Midijobs can effectively phase out small jobs subsidies and generate incentives to transition from Minijobs to regular, higher-paying employment. Merely adjusting social insurance contributions is insufficient to abolish the Minijob trap.<sup>35</sup> Instead, it appears to be more promising to address the income tax system which generates high marginal and average tax rates at the Minijob earnings ceiling—particularly for secondary earners in marriages.

<sup>34</sup> For detailed simulation studies of alternative reform options see Eichhorst et al. (2012), Bach et al. (2018b), or Blömer et al. (2021).

<sup>35</sup> The literature also discusses the relevance of labor demand responses to such reforms (see Gudgeon and Trenkle 2024, Tazhitdinova 2020, Haywood and Neumann, 2021 and Chetty et al., 2011).

Politically, it has been attractive to increase the upper ceiling of the Midijob subsidy. While low-income earners benefit from reduced payroll taxes, we did not find convincing evidence that Midijobs abolished the Minijob trap in Germany.

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**Data availability** The data that support the findings of this study are available from two sources: (1) Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) in Nuremberg. The SIAB 1975–2017 can be used after signing a contract. <https://doi.org/10.5164/IAB.FDZD.1902.en.v1>. (2) Research Data Centre (FDZ) of the Socio-Economic Panel. The SOEP Survey can be used after signing a contract. <https://doi.org/10.5684/soep-core.v35>. We will be happy to make our codes available for both parts of the analysis.

## Declarations

**Conflict of interest** The authors declare no competing interests.

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