Phasing out payroll tax subsidies

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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 'Minijob 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 Minijobs, they are effective only for a narrow treatment group.

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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 'Minijob 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 Minijobs, they are effective only for a narrow treatment group.

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Key words: Midijobs, Minijobs, payroll tax subsidy, causal effects, difference-in-differences, regression discontinuity, SOEP, SIAB

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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.[1] 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 evaluate the labor supply effects of these programs.[2] 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 in 2003 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. This addresses 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 and Trenkle 2023, 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

[1] See, e.g., Immervoll et al. (2007) or Jara et al. (2020).

[2] 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).

number of jobs across the earnings distribution. Midijobs were designed to offer a way out of this 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.1**).[3] 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), 1,600 (in 2022), and even 2,000 Euro in 2023, and the lack of prior evidence, the Minijob trap addressed in this paper is important.

We use two complementary empirical strategies to investigate whether the introduction of Midijobs succeeded in reducing the Minijob trap. 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 discontinuity design to administrative data and investigate whether there is a jump in the transition probability. Second, we exploit 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. This attenuates the kink in the net earnings schedule, particularly for individuals who pay low or no income taxes (e.g., single individuals). In 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

[3] 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.1** clearly shows that more employees were covered after the Midijob earnings ceiling increased on July 1, 2019 from 850 to 1,300 Euro per month.

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 (RDD) 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 complement the results of Tazhitdinova (2020) and Gudgeon and Trenkle (2023), the two contributions closest to ours. 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 (2023) 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 out of their need for support by expanding their labor supply.[4] Only few contributions address the role of benefit phase-out. Eissa and Liebman (1996) study benefit phase-out in the EITC, where higher marginal tax rates were expected to reduce labor supply. Interestingly, they find single mothers to be rather unresponsive to increasing marginal tax rates in the phase-out region of the program.[5] 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 (2014) 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

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

[5] 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).

is "received wisdom" that the payroll tax incidence falls on workers' net wages.[6] We add to this literature by focusing on the intensive margin of labor supply: we study whether payroll tax subsidies increase workers' propensity to expand labor supply and earnings beyond Minijobs.

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.[7]

The literature on Midijobs is limited and largely descriptive. Most studies explain the Midijob instrument and its utilization.[8] 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, 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

[6] 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 Bennmarker et al. (2009) who provide evidence from Chile, the United States, Finland, and Sweden, respectively.

[7] 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).

[8] 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).

could constitute a useful policy for other (national) labor markets, as well.[9] 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 section 3 and our difference-in-differences analyses in section 4. In section 5 we conclude.

2. Institutional Background

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).[10] This regulation exists since the early days of the German social insurance system (1893) to limit the bureaucratic burden for small jobs (BMAS 2018, p.110). Minijob regulations were modified over time with varying objectives, e.g., to raise social insurance contributions or to provide incentives for regular employment.

The wage subsidy inherent in Minijob employment generates a large, discontinuous change in tax and social insurance liabilities at the Minijob earnings ceiling. 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 **Figure 2**). While social insurance contributions are relevant for all employees, income taxes differ for

[9] 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.

[10] 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.

joint and individual filers; in section 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 may be affected by sizeable tax rates on their entire earnings as soon as they exceed the Minijob earnings threshold. If individuals are, e.g., subject to a 30 percent average income tax rate, (pre-reform) net earnings would drop from 325 to about 163 Euros as gross earnings exceeded 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 (see **Figure 1**) and causes substantial bunching in the earnings distribution (Gudgeon and Trenkle 2023, 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.[11] 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

[11] 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).

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.

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. At the end of 2022, 4.3 and 3.5 million individuals held these jobs as their main employment (in a labor force of about 45 million), respectively.

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 existing subsidized Minijob employment and entering higher-earning employment.

Utilization patterns of Mini- and Midijobs are characterized by Oschmiansky and Berthold (2020) or Tazhitdinova (2020). Generally, females and workers with low formal education have a relatively high propensity to work in Minijobs. 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) which account for 15 percent of regular employment but 36 percent of Minijobs (Collischon et al. 2021). In our survey (administrative) data no more than 4 (1) percent of all Minijobs are in private households. 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. Less than 20 percent of new Midijob employments originate in Minijobs. 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 (for details see Herget and Riphahn 2022).

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 determines the immediate change in transitions out of Minijobs upon the introduction of Midijob subsidies on April 1, 2003. Our outcome of interest (*Y*) reflects whether an individual *i* leaves a Minijob for higher-earning employment in the next month *t+1*. The reform date of April 1, 2003 provides a sharp discontinuity in the regulatory setting. We exploit it to determine whether transition behaviors change for Minijob employees observed shortly before and shortly after the reform date: prior to April 1, 2003 Minijobbers faced higher deductions after leaving the Minijob earnings range than after April 1, 2003.[12]

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 α_2 provides the local average treatment effect of the reform, i.e., the reform effect on the transition

[12] Hausman and Rapson (2018) label regression discontinuity designs (RDD) with time as the running variable 'regression discontinuity in time' (RDiT). They discuss conceptual differences between conventional RDD and RDiT, additional challenges in RDiT settings, and further robustness checks which we offer below.

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

$$Y_{it+1} = \alpha_0 + \alpha_1\, time_{it} + \alpha_2\, after_{it} + \alpha_3\, (after_{it} \cdot time_{it}) + \beta_1\, X_{it} + \varepsilon_{it}\,. \qquad (1)$$

ε represents a random error term. We will augment equation 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[13] and apply three alternative specifications of the covariate vector X as robustness checks: we start out without controls, then consider a set of basic demographics (age, gender, German citizenship, East German residence), and finally offer controls for an extended specification (education, tenure, occupation, industry, firm size).

A causal interpretation of α_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 effects.[14] 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 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

[13] In particular, we calculated calendar month-specific average transition rates and deducted their difference from the average of 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.2** shows the detailed monthly deviations from calendar month-specific means over time.
[14] To the extent that adjustments in labor contracts take more time, we underestimate the true impact of the reform. Gudgeon and Trenkle (2023) show delayed responses to the 2003 reform.

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. 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, this would 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 (4,239,948) and March 31, 2003 (4,263,180).[15] Therefore, the effect cannot be large.

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, without starts after April 1, 2003) and, second, were started after the reform legislation was passed (Sample C, without starts after December 31, 2002). 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

[15] Figures received from Statistik der Bundesagentur für Arbeit via personal email.

realized without leaving a Minijob. Therefore, the estimate of α_2 represents the combined effect of introducing Midijobs and expanding the Minijob earnings ceiling. To gauge the relevance of a threshold shift for transition rate adjustments we measure the response in transition rates to the 2013 reform which changed the Minijob earnings threshold from 400 to 450 Euros without relevant changes to the Midijob regime.

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).[16] 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. Minijobs are used frequently by students and by retirees; in 2003 (2019), 19 (18) percent of Minijobbers were below age 25 and 30 (33) percent above age 54 (BA 2004, 2020). We select those aged 30-59 to exclude students and retirees who may be subject to additional regulations. Similarly, unemployed individuals may hold Minijobs subject to specific restrictions in addition to unemployment benefits. Therefore, we follow Tazhitdinova (2020) and Gudgeon and Trenkle (2023) and omit Minijobbers who at the same time receive unemployment benefits.[17] We generate a 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).

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

[17] Dropping observations with parallel unemployment benefit receipt reduces the final sample by 175,418 monthly Minijob observations.

In our analyses, we also consider two alternative samples: Sample B drops Minijob observations from employments that started after the reform date of April 1, 2003; this reduces the sample size to 751,217. Sample C, omits all Minijob observations from employments that started after 31.12.2002; this reduces the sample size to 715,313 (see Appendix **Table A.2** for a summary of the three samples).

Our outcome indicates whether an individual 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, which includes all Minijobs in our observation period, 0.87 percent of all monthly observations leave the Minijob for higher-paying employment (0.86 percent before the reform).

Table 1 shows descriptive statistics for the basic set of control variables in Sample A. The vast majority of Minijobbers is female (89 percent). Many Minijobbers are observed in the 35-44 age window; most female Minijobbers are aged 35-44 (43 percent vs. 29 percent of males), while most male Minijobbers are aged 50-59 (40 percent vs. 26 percent of females, not shown in **Table 1**). Only 6 percent of Minijobs are held by East Germans which is much below their population share of about 20 percent. The 8.5 percent share of foreign citizens approximately is close to the 2003 foreign workforce share of 6 percent. Appendix **Table A.3** shows descriptive statistics on the extended set of covariates for Sample A. About 70.8 percent of Minijobbers have completed a vocational education, 18.7 percent hold no formal degree (19.3 percent of females and 14.1 percent of males), about 4 percent hold a tertiary degree (7.8 percent of male and 3.4 percent of female observations), and this information is missing for 6.6 percent of the sample. About half of all monthly Minijob observations hold their job for already more than 2 years. About two thirds of all Minijob occupations work on simple manual, service or administrative tasks, and mostly in smaller firms. About 80 percent of all Minijobs are found in just three industry groups (hospitality, traffic and logistics, and education).

Tables 1 and **A.3** also describe the mean monthly transition rates from Minijobs to employments with higher earnings for different groups. We 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 men is higher than that of females (0.014 vs. 0.008), and rates of East Germans and foreign citizens surpass those of West Germans and German nationals substantially. The likelihood of leaving Minijobs for higher-paying jobs declines with age. We observe higher transition rates among those with higher education, in more qualified occupations, and in larger firms. Monthly transition probabilities are lower for individuals with higher Minijob tenure.

3.3 Baseline results of discontinuity analyses

Figure 3 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 (**Figure 3.1**). With sample B we find no shift at the reform date and declining transition rates throughout (**Figure 3.2**), and with sample C we obtain a downward shift in transition rates at the reform date (**Figure 3.3**). Appendix **Figure A.3** presents these graphs by gender and finds large positive discontinuities for men and no changes for women.

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 α_2, i.e., the shift in transition rates at the reform date as shown in equation (1). Column 1 provides the α_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 (Appendix **Table A.4** shows the complete results of the linear specifications for Sample A for illustration). The coefficient estimate is small. It indicates that the propensity to leave a Minijob for higher-earning employment increased on the reform date by .05 percent points or 6.5 percent of the pre-reform mean transition probability (see column entitled RE); however, the coefficient is imprecisely estimated. Subsequent rows present the results of alternative model specifications: 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), row 4 allows for quadratic time trends before and after the reform without covariates, and rows 5 and 6 add basic and extended controls to the quadratic model. The estimates of α_2 are slightly larger and statistically significant in the quadratic specifications. Given the strong correlation of tenure with transition rates, it is not surprising that the estimates are sensitive to accounting for the potentially endogenous tenure indicators in the extended specification. Overall, we do not find large reform effects at the discontinuity.[18]

Next, we consider the estimates for Sample B which omits Minijobs that started after the reform (see column 2). The estimates for α_2 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 after omitting Minijobs that were initiated after the reform law was passed (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 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 (Appendix **Table A.5** shows descriptive statistics for the 2013 sample). Our sample here considers individuals who worked in Minijobs between Jan. 1, 2012, and Dec. 31, 2014. It comprises more than one million observations with a relatively

[18] We also estimated models with cubic time trends. Again, we obtain positive and significant estimates of α_2 which are slightly larger than in the quadratic specification.

high average transition rate of 1.6 percent. In this situation, the estimated α_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 where transition rates declined on average by less than 9 percent of the pre-reform mean. 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. This suggests that we may underestimate the impact of the introduction of Midijobs in 2003.

While classical regression discontinuity strategies offer tests for sorting in the running variable (McCrary 2008), this is not useful when time is the running variable (see Hausman and Rapson 2018). We offer tests for sorting in covariates in **Table A.6**. 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. Section 3.4 offers an extensive discussion of the heterogeneity between male and female subsamples. For the covariates of the extended specification, we obtain a few significant effects. We do not see clear patterns and do not expect these results to affect our main findings.[19]

3.4 Regression discontinuity results - heterogeneity and robustness

Given the heterogeneity in the Minijob utilization of men and women both in terms of intensity and correlations (see **Tables 1** and **A.3**), we separately analyze the two subsamples. We observe

[19] Even the statistically significant coefficient estimates do not show consistent signs or magnitudes (see, e.g., tenure). It seems unlikely that small changes in the industry composition – in particular in the missing category – reflect important discontinuities affecting our results. Overall, the estimation results in **Table 2** do not differ between specifications that do and that do not control for the extended specification.

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). **Table 3** shows the estimation results for α_2 based on the full Sample A as in **Table 2** for both subsamples. The estimates in columns (1) and (2) indicate substantial differences between genders. While the estimates for males are all statistically highly 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 .6 to .9 percentage points or 50-80 percent of the pre-reform mean; the effects for women are close to zero. We show the relevant 2013 estimation results in columns 3 and 4; interestingly, the gender difference in transition rates out of Minijobs continues to be large in 2013. The 2013 reform hardly affected male transitions. This suggests that for men the 2003 estimate is not likely to be biased downward by the change in the Minijob earnings limit. In contrast, the negative estimates for females around the 2013 reform may indicate that without the change in Minijob earnings limit in 2003 females' transition propensities would have increased by more than estimated. However, the gender-specific heterogeneity in responses to an increase in the Minijob earnings ceiling does not balance the large gender differences in columns 1 and 2.

So, while we hardly find a change in average transition rates when the Midijob subsidy was introduced in 2003, we find substantial gender heterogeneities: for men, the propensity to leave Minijob employment for a higher-paying job increased discontinuously and by more than 50 percent with the introduction of Midijobs. There is no comparable effect for women.

This leaves two open questions: first, are these findings robust and second, what explains the effect heterogeneity by gender? So far, we presented six different empirical specifications for three versions of the full sample, which cover different sampling periods of Minijob employment. In addition, we estimated all specifications by gender which confirmed the findings in **Table 3** (see **Table A.7**).

As a first additional robustness check, we consider an inflow sample: instead of using all monthly Minijob observations available around the reform date we only considered those individuals who entered Minijobs on or after April 1, 2002, i.e., one year prior to the reform. This implicitly excludes very long running Minijob spells. Appendix **Table A.8** presents the inflow sample estimation results for men and women in the familiar framework in columns (1) and (2). The estimates confirm large, statistically significant positive estimates for men and smaller insignificant estimates for women. Additionally, we then limited the inflow sample and dropped those Minijob spells that started after the reform date of April 1, 2003. Columns (3) and (4) of Appendix **Table A.8** present the estimation results which confirm prior findings.

As a second robustness test, we varied the observation window around the reform. Instead of using 12 months before and after, we evaluated the results 9 and 15 months around the reform date. Appendix **Table A.9** presents the estimation results which again confirm prior findings. In a third test, we performed a "donut-estimation" where we omit observations at the cutoff (March 2003) in order to determine whether they determine the results. Appendix **Table A.10** presents the estimation results which once again confirm prior findings.[20]

So, given that the findings appear to be robust we discuss the second question, i.e., the potential mechanisms behind gender differences in transition rates and in the responses to the introduction of the Midijob subsidy. We start out by investigating possible differences in response timing: an explanation of the observed gender differences might be that only males respond immediately at the reform date and females' behavioral adjustments take more time possibly due to frictions and sluggish labor market responses (see Tazhitdinova 2020 for corresponding gender differences in elasticities). The discontinuity strategy identifies the local treatment effect, i.e., the discontinuous increase in the transition rate at the reform date. For an

[20] In addition, our results are robust to adding younger individuals to our sample (e.g., using age 25-59 instead of 30-59 as in our baseline sample).

alternative perspective, we offer the estimates of a difference-in-differences estimation. It compares the average change in transition rates for males and females over the period covered by the sample, i.e., 12 months before and after the reform (see Appendix **Table A.11**). The results confirm that the change in transition rates was significantly smaller for females than males even in a longer run perspective. This result is robust across specifications, varies only slightly across observation windows, and confirms the gender difference found before.

One interesting difference between male and female Minijobbers is their past tenure on the job (see Appendix **Table A.3**). Men are more likely to be observed with short job tenure whereas more than fifty percent of all female Minijob observations have accumulated at least 24 months of tenure in the given employment. If there is negative duration dependence with lower exit rates for longer Minijobs, this might generate a Minijob trap that also could be distributed differently across genders. To investigate the association of tenure with the propensity to respond to the 2003 reform we estimate our models separately for Minijob observations with short and long tenure, i.e., below and above 24 months on the job. Appendix **Table A.12** shows the results. We focus on sample A and the specifications without additional control variables. Overall, the transition probability declines with tenure. We generally find larger coefficient estimates and relative effect sizes for short than for long tenure employment. Thus, the reform particularly increased transition rates to unsubsidized regular employment for those who had not been employed in Minijobs for too long before.[21] The mechanisms behind these patterns may relate to skill depreciation connected to low-skill Minijob employment (for similar patterns see Collischon et al. 2022).

Clearly, numerous alternative and additional mechanisms may affect gender differences; the surveys on the motivation of Minijob use that we reported on in section 2 above provide clear evidence on the difference in Minijob rationales by gender (see Bachmann et al.

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[21] We do not find these estimates when we split samples by age instead of tenure.

2017).[22] One important gender-specificity is related to the heterogeneity of income tax rates between secondary earners in marriages and single individuals. Women often use Minijob employments when they are secondary earners in married couples. The next section exploits this heterogeneity to construct treatment and control groups for the Midijob introduction. If the control group of (mostly female) secondary earners in marriages responds less to the reform than the treatment group of non-married (mostly male) Minijobbers this mechanism may interact with the effect heterogeneity described in this section.

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.

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,

[22] Additionally, we compared the two genders' propensity to change employers when leaving a Minijob and the distribution of Minijobbers across occupations and industries. We find only minor differences: females are more likely to work in the hospitality industry and males in transportation and construction. Employment in private households is rare in our data. Women are more likely to hold clerical positions in office jobs and men are more likely to work on manual jobs and simple services.

we hypothesize that individuals with low income tax rates (e.g., single 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.

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 (*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).[23] 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 μ be a random error. We estimate the coefficient vectors α and β in this model:

$$Y_{it+1} = \alpha_0 + \alpha_1\, after_{it} + \alpha_2\, treat_{it} + \alpha_3\, (after_{it} \cdot treat_{it}) + \beta_1\, X_{it} + \mu_{it} \quad . \qquad (2)$$

Our effect of interest is the estimate of α_3. It indicates whether individuals with low income tax burdens (*treat* = 1) changed their propensity to transition out of the Minijob earnings range after

[23] Gudgeon and Trenkle (2023) 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.

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, α_3 partly reflects heterogeneous responses 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 composition of the workforce in response to the treatment. First, we examined whether individuals might switch between treatment and control group 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.13** presents the p-values for tests of the hypothesis that the mean values of treatment and control group characteristics changed over time. We observe some minor adjustments but no major changes in characteristics.[24]

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 (see our discussion in section 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.

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).[25] 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.[26]

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[24] 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.

[25] We use SOEP v35 (1984-2018), DOI:10.5684/soep-core.v35.

[26] 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.

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.[27] 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.13**).[28]

[27] 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.

[28] 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.

Our dependent variable indicates whether a person held a Minijob in period t and in period t+1 transited 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 (treat = 0) have a much lower average transition rate than non-married individuals (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 4 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.[29]

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 α_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 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

[29] 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 a full vector of year indicators and their interactions with the treatment indicator but without additional controls. **Figure A.4** 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.

citizenship) and then add an extended set of controls (see table notes and Appendix **Table A.14** 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.[30]

Table 6 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.[31] 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. Omitting observations with East German residence as in column 5 reveals somewhat larger effects in West than in East Germany.[32]

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

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[30] We use sampling weights in the analyses of SOEP data. The results in **Table 5** are sensitive to this choice.

[31] 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.

[32] 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.

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.14** and **A.15** for descriptive statistics).[33] **Table 7** 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 α_3. Overall, the transition rates 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 **6** 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 7** 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.

5. Conclusions

Many countries subsidize low-income employment or small jobs. The phasing out of such subsidies can affect labor supply incentives (Hoynes 2007, Eissa and Hoynes 2006). 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

[33] We drop 32 individuals for whom information on marital status, our treatment indicator, is missing in the data.

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 (i.e., 7.5 out of 45.3 million individuals). 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, control group) to that of those more strongly affected by the reform (non-married individuals, treatment group) in a difference-in-differences strategy. Based on survey data we find that those 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.

As a rough quantitative assessment, we find that the Midijob introduction increased the annual number of transitions from Minijob to regular employment by about 71,300.[34] At the same time, the Midijob subsidy reduces social insurance revenues annually by about 324 million Euro.[35] This yields an average fiscal cost of 4,544 Euro for each additional transition, which may represent a reasonable investment if the new, higher-earning employment is stable.

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) .Apparently, the adjustment of social insurance contributions is not sufficient to flatten the net earnings distribution at the Minijob earnings ceiling.[36] 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.

Politically, it has been attractive to increase the upper ceiling of the Midijob subsidy. It rose from 800 Euro per month in 2003, to 850 Euro in 2013, to 1.300 Euro in 2019, to 1.600 Euro in 2022 and 2.000 Euro in 2023. While low-income earners benefit from reduced payroll taxes we did not find convincing evidence that Midijobs abolish the Minijob trap in Germany.

[34] We observe about 4.6 million Minijobs in 2003, of which 10 percent are held by non-married individuals. The transition rate for this group increased by about 15.5 percentage points due to the reform, which yields (0.155*460,000=) 71,300 additional transitions.

[35] We observe about 1 million Midijobs in 2004 with mean earnings of 630 Euro per month or 7,560 Euro per year (own calculations). At the mean, the Midijob subsidy reduces social insurance contributions from about 126 to 99 Euro per month. Compared to unsubsidized contributions the annual payment to social insurances thus declines by about 324 Euro per year for each Midijobber.

[36] The recent literature also discusses the relevance of labor demand responses to such reforms (see Gudgeon and Trenkle 2023, Tazhitdinova 2020, and Haywood and Neumann 2021).

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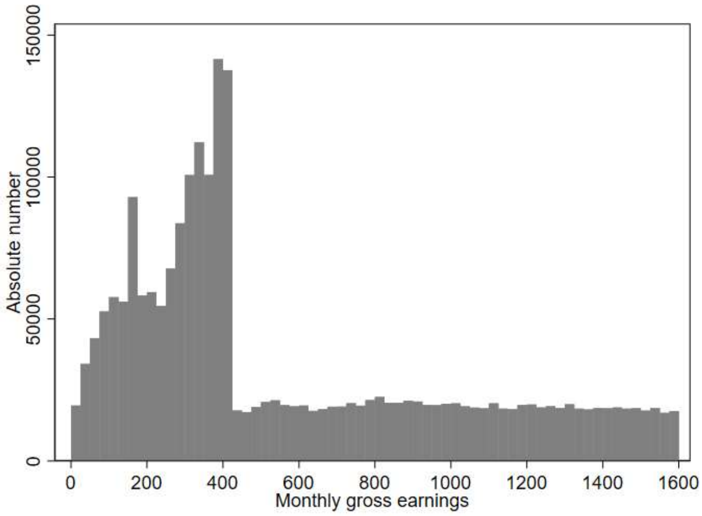
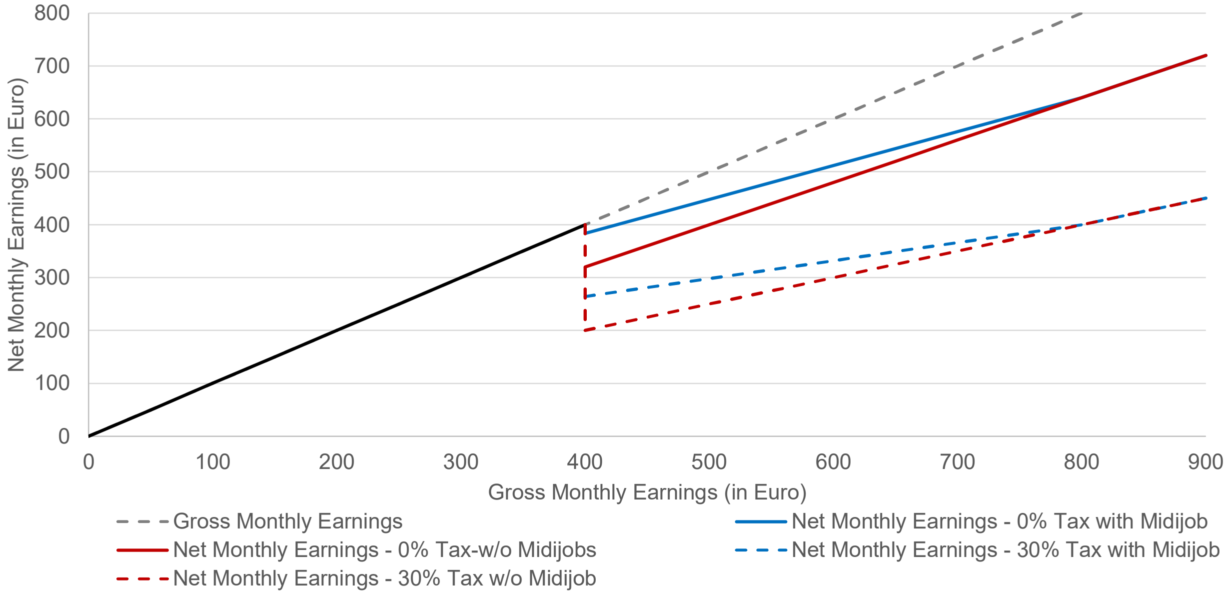
Figure 1 Distribution of gross monthly earnings (2006)



Note: The graph represents the bottom part of the gross earnings distribution for all employed individuals registered with the unemployment insurance in 2006 and shows the number of employees per 50 Euro bin of monthly gross earnings.
Source: SIAB, own calculations.

Figure 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 a zero income taxes.
Source: Own illustration.

Figure 3 Monthly transition rate from Minijob Employment (April 2002-March 2004)

Figure 3.1 Sample A (all Minijob observations in the observation window)

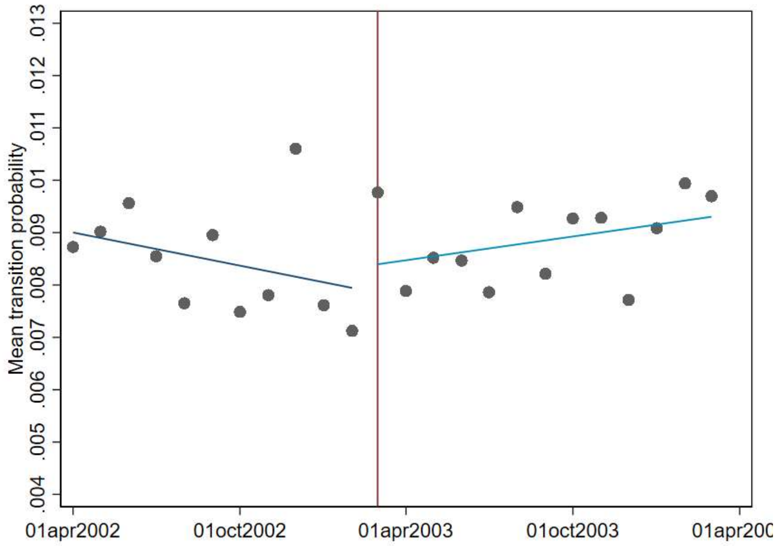


Figure 3.2 Sample B (Sample A without Minijobs started after April 1, 2003)

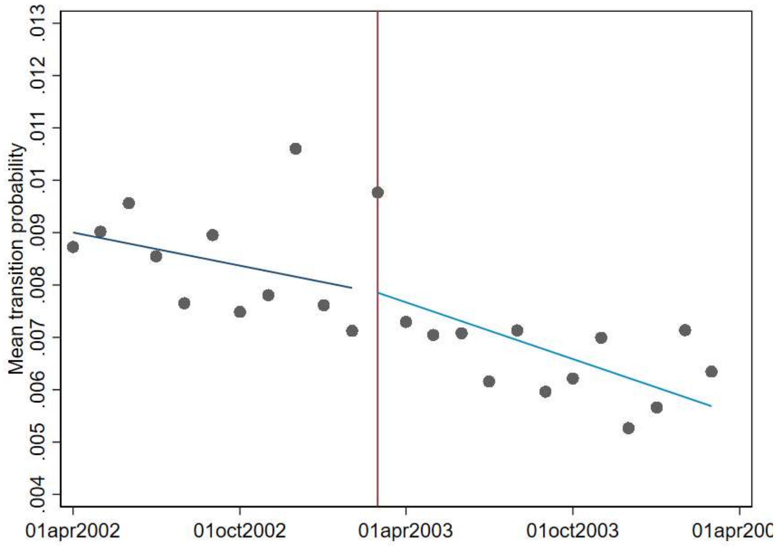
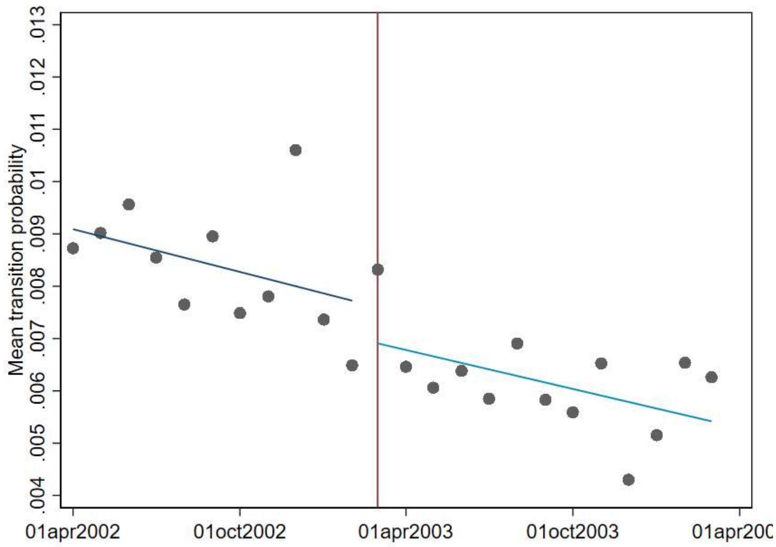
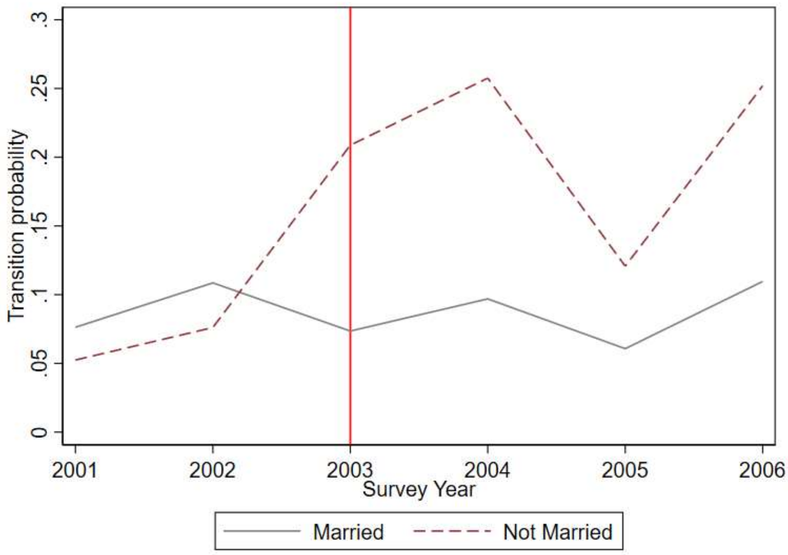


Figure 3.3 Sample C (Sample B without Minijobs started after Dec. 31, 2002)



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.3**.
Source: SIAB (2017) and own calculations.

Figure 4 Annual transition rate from Minijob employment over time by treatment status (SOEP sample)



Note: The red vertical indicates the reform date of April 1, 2003. The transition rate indicates the share of Minijobs in the survey year (t) that will be left for higher-earning employment by the next survey year (t+1).
Source: SOEP wave 35 and own calculations (weighted data).

Table 1 Descriptive Statistics - Basic Controls (SIAB Sample)

	Full Sample		Mean Transition Rate when variable has		Mean by gender	
Variable	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.
Source: SIAB (2017) and own calculations.

Table 2 Estimation Results - SIAB

	Sample A - 2003		Sample B - 2003		Sample C - 2003		Sample A - 2013	
	(1)		(2)		(3)		(4)	
Specification	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 α_2 in equation (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 ratio of the coefficient estimate of α_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.
Source: SIAB (2017) and own calculations.

Table 3 Estimation Results - SIAB - by gender

	Sample A - 2003				Sample A - 2013			
	(1) Men		(2) Women		(3) Men		(4) Women	
Specification	Coeff	RE	Coeff	RE	Coeff	RE	Coeff	RE
1 Linear no controls	0.0083 ***	73.6%	-0.0004	-5.2%	0.0018	7.6%	-0.0009	-5.9%
2 Linear basic controls	0.0078 ***	69.0%	-0.0005	-6.1%	0.0014	5.8%	-0.0008	-5.5%
3 Linear ext. controls	0.0061 ***	54.0%	-0.0007	-8.5%	0.0011	4.6%	-0.0010 *	-6.8%
4 Quadratic no controls	0.0063 ***	55.7%	0.0003	3.3%	-0.0007	-3.0%	-0.0015 *	-10.6%
5 Quadr. basic controls	0.0092 ***	81.4%	0.0002	2.4%	-0.0009	-3.8%	-0.0015 *	-10.3%
6 Quadr. ext. controls	0.0071 ***	62.8%	0.0001	1.2%	-0.0009	-3.8%	-0.0016 **	-11.0%
N	95,277		757,964		222,556		840,336	
Pre-reform mean Y	0.0113		0.0082		0.0240		0.0146	

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

Table 4 Descriptive Statistics - Basic Controls: 2003 Reform Sample (SOEP)

	Descriptives		Mean transition rate when variable has	
Variable	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	0.1559	0.1078
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.
Source: SOEP wave 35 and own calculations.

Table 5 Estimation Results - 2003 reform baseline results (SOEP)

	Full Sample (1)	Full Sample (2)	Full Sample (3)	Full Sample (4)
post	0.004	0.004	-0.000	-
	(0.015)	(0.015)	(0.015)	
treat	0.002	-0.022	-0.038	-0.0041
	(0.029)	(0.031)	(0.032)	(0.033)
post * treat	0.139***	0.123**	0.131***	0.134***
	(0.052)	(0.051)	(0.050)	(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.
Source: SOEP wave 35 and own calculations.

Table 6 Estimation Results - 2003 reform robustness and heterogeneity (SOEP)

	No problematic obs. (1)	2001-2004 (2)	2001-2007 (3)	Only Women (4)	Only West Germany (5)
post	-0.008	0.013	0.006	0.003	0.005
	(0.020)	(0.021)	(0.015)	(0.015)	(0.016)
treat	-0.065*	-0.020	-0.019	0.006	-0.026
	(0.039)	(0.031)	(0.031)	(0.032)	(0.029)
post*treat	0.157***	0.156**	0.124**	0.091*	0.162***
	(0.056)	(0.078)	(0.048)	(0.054)	(0.056)
Controls - basic	yes	yes	yes	yes	yes
Controls - extended	no	no	no	no	no
Year fixed effects	no	no	no	no	no
Number of obs.	2,160	1,767	3,199	2,583	2,529

Note: See note below **Table 5**.
Source: SOEP wave 35 and own calculations.

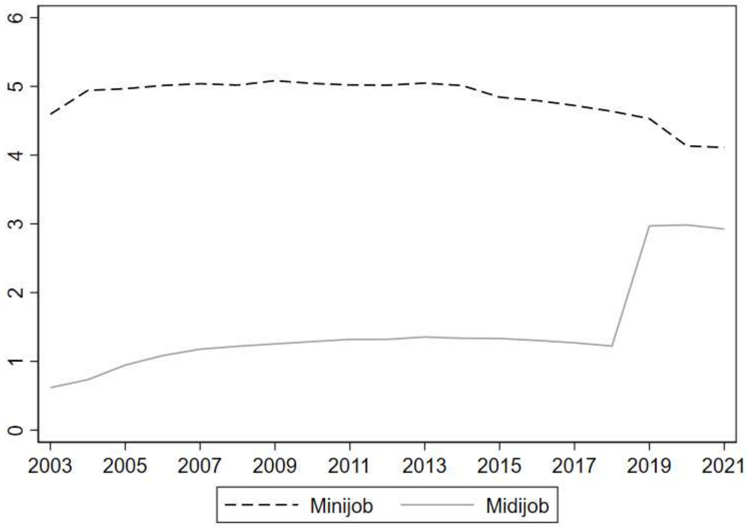
Table 7 Estimation Results - effects of the 2013 reform (SOEP)

	Full Sample (1)	Full Sample (2)	Full Sample (3)	Full Sample (4)
post	0.022	0.022	0.023	-
	(0.017)	(0.017)	(0.017)	
treat	0.068*	0.067*	0.065*	0.062*
	(0.039)	(0.038)	(0.036)	(0.036)
post * treat	-0.026	-0.033	-0.025	-0.023
	(0.049)	(0.048)	(0.045)	(0.045)
Controls - basic	no	yes	yes	yes
Controls - extended	no	no	yes	yes
Year fixed effects	no	no	no	yes

Note: The estimations use 5,326 observations covering the years 2011-2016 (for descriptive statistics see **Table A.12**). See note below **Table 5**.
Source: SOEP wave 35 and own calculations.

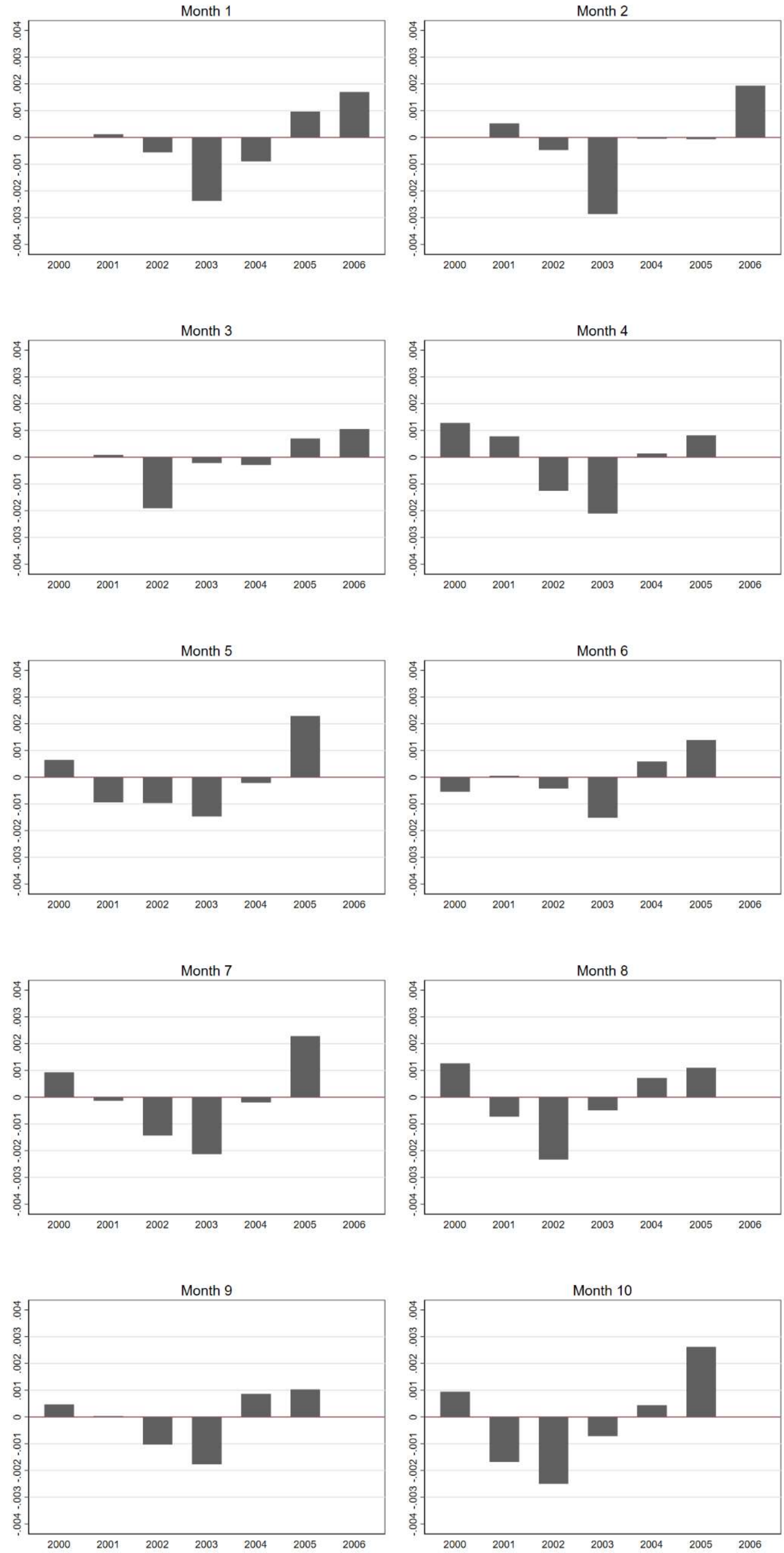
Online Appendix

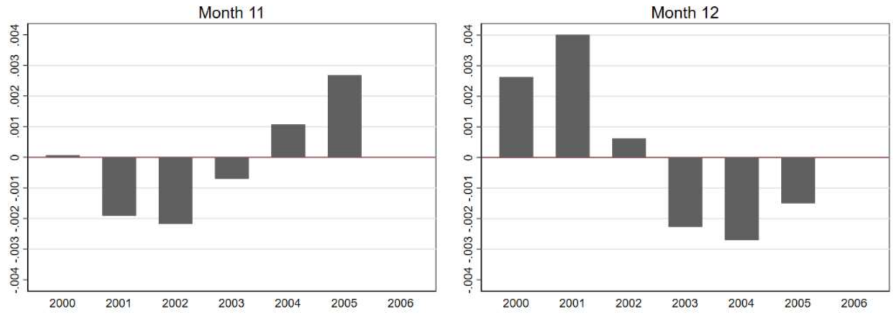
Figure A.1 Number of Mini- and Midijobs over time (per end of year, in millions)



Note: On July 1, 2019 the Midijob earnings ceiling increased from 850 to 1,300 Euro per month, thus covering more employees. The Minijob number reflects only those who hold a Minijob as a main employment; as of 2021 an additional 3 million individuals hold Minijobs as a secondary job.
Source: Statistik der Bundesagentur für Arbeit.

Figure A.2 Annual deviation of calendar-month specific mean transition rates





Note: Each column characterizes the difference in the calendar month specific mean transition rate relative to the overall calendar-month specific average transition rate over all six observation years.
Source: SIAB (2017) and own calculations.

Figure A.3 Monthly transition rate from Minijob employment by gender (male left panel, female right panel)

Figure A.3.1 Sample A (all Minijob observations in the observation window)

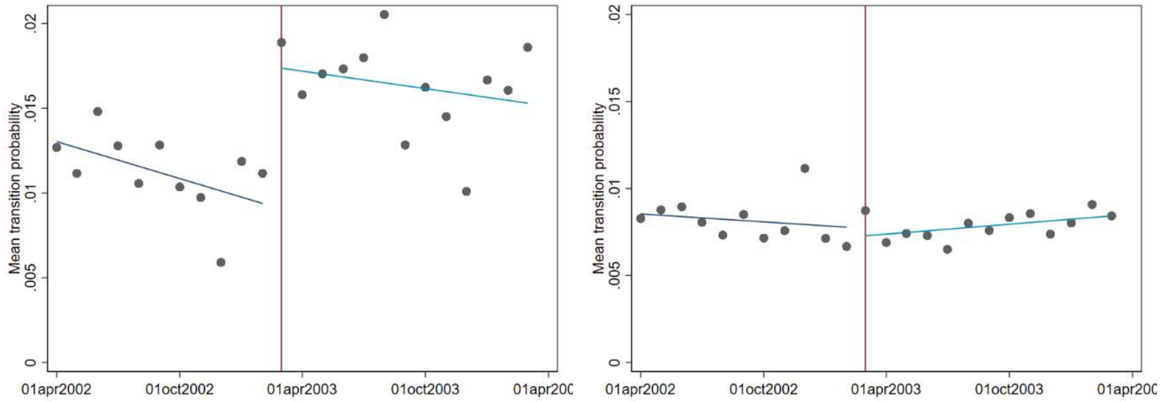


Figure A.3.2 Sample B (Sample A without Minijobs started after April 1, 2003)

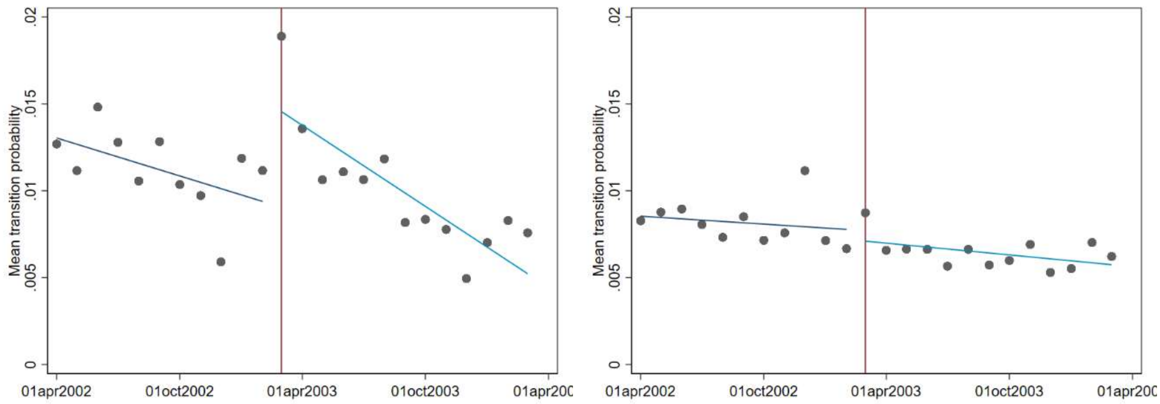
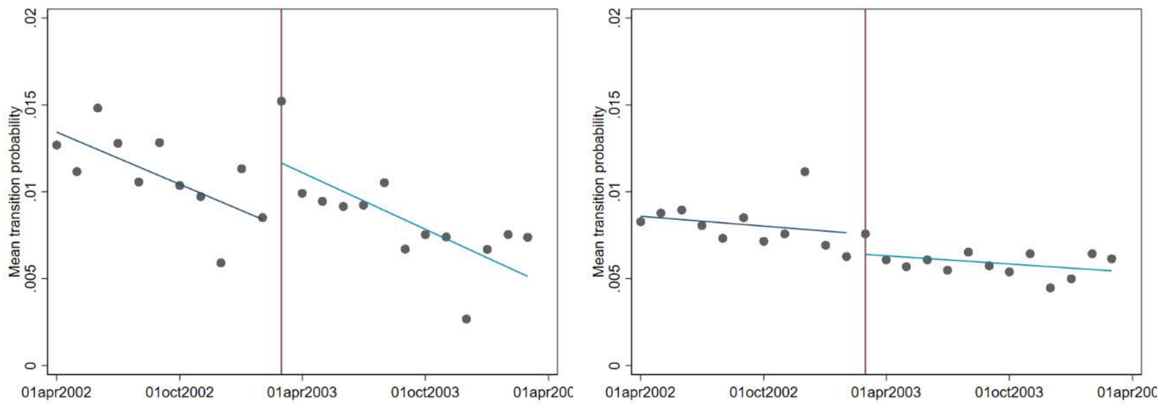


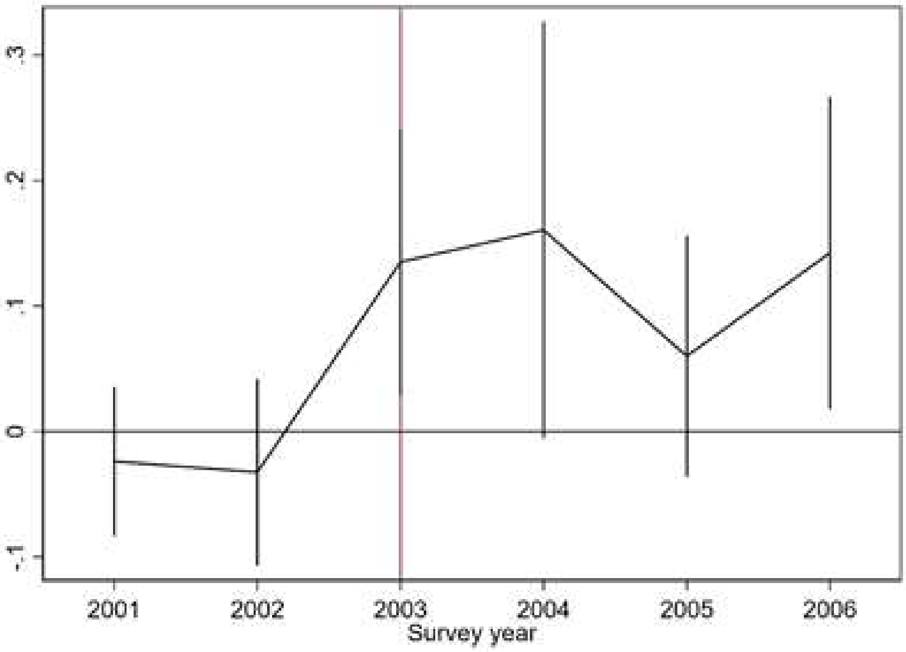
Figure A.3.3 Sample C (Sample B without Minijobs started after Dec. 31, 2002)



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.

Source: SIAB (2017) and own calculations.

Figure A.4 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 a full set of year indicators and its 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.

Table A.1 Monthly earnings range of Mini- and Midijobs

Reform date (date of law change)	Minijobs	Midijobs
Apr 1, 1999 (Mar 24, 1999)	0 - 325 Euro	-
Apr 1, 2003 (Dec 23, 2002)	0 - 400 Euro	400.01 - 800 Euro
Jan 1, 2013 (Dec 5, 2012)	0 - 450 Euro	450.01 - 850 Euro
July 1, 2019 (Nov 8, 2018)	0 - 450 Euro	450.01 - 1,300 Euro
Oct 1, 2022 (June 28, 2022)	0 - 520 Euro	520.01 - 1,600 Euro
Jan 1, 2023 (Oct 7, 2022)	0 - 520 Euro	520.01 - 2,000 Euro

Source: Own illustration.

Table A.2 Summary of SIAB Samples

	Sample A	Sample B	Sample C
Number of observations	853,241	751,217	715,313
First observation	April 1, 2002	April 1, 2002	April 1, 2002
Last observation	March 31, 2004	March 31, 2004	March 31, 2004
Minijob started prior to	April 1, 2004	April 1, 2003	January 1, 2003

Source: Own illustration.

Table A.3 Descriptive Statistics - Extended Controls (SIAB)

	Full Sample		Full Sample	Means by Gender	
			Mean Y		
Variable	Mean	Std. Dev.	when valued 1	Men	Women
Educ: No vocational degree (0/1)	0.1866	0.3896	0.0077	0.1407	0.1929
Educ: Vocational degree (0/1)	0.7078	0.4547	0.0093	0.6979	0.7093
Educ: Tertiary education (0/1)	0.0398	0.1956	0.0123	0.0781	0.0340
Educ: Missing (0/1)	0.0657	0.2478	0.0035	0.0833	0.0637
Tenure: 0-3 months (0/1)	0.1078	0.3101	0.0202	0.1786	0.0989
Tenure: 4-6 months (0/1)	0.0825	0.2751	0.0132	0.1145	0.0785
Tenure: 7-12 months (0/1)	0.1263	0.3322	0.0110	0.1483	0.1236
Tenure: 13-18 months (0/1)	0.0933	0.2909	0.0085	0.0911	0.0936
Tenure: 19-24 months (0/1)	0.0815	0.2737	0.0076	0.0762	0.0822
Tenure: 25-36 months (0/1)	0.2747	0.4464	0.0060	0.2208	0.2815
Tenure: 37-48 months (0/1)	0.2018	0.4013	0.0044	0.1480	0.2085
Tenure: 49+ months (0/1)	0.0320	0.1761	0.0030	0.0226	0.0332
Occup.: Agriculture and other (0/1)	0.0365	0.1875	0.0072	0.0552	0.0337
Occup.: Simple manual tasks (0/1)	0.0777	0.2676	0.0082	0.1081	0.0715
Occup.: Qualified manual tasks (0/1)	0.0435	0.2040	0.0115	0.0851	0.0364
Occup.: Engineer, techn., semi-prof. (0/1)	0.0464	0.2102	0.0145	0.0509	0.0464
Occup.: Simple service (0/1)	0.3873	0.4871	0.0084	0.4515	0.3779
Occup.: Qualified service (0/1)	0.0701	0.2553	0.0087	0.0332	0.0778
Occup.: Professional and manager (0/1)	0.0137	0.1164	0.0114	0.0408	0.0103
Occup.: Simple administration (0/1)	0.1910	0.3931	0.0075	0.0825	0.2046
Occup.: Qualified administration (0/1)	0.1340	0.3406	0.0089	0.0926	0.1415
Firm-size: 0-19 (0/1)	0.5941	0.4911	0.0077	0.5702	0.5990
Firm-size: 20-99 (0/1)	0.2177	0.4127	0.0095	0.2321	0.2143
Firm-size: 100-199 (0/1)	0.0655	0.2474	0.0101	0.0721	0.0649
Firm-size: 200-299 (0/1)	0.0315	0.1746	0.0114	0.0331	0.0314
Firm-size: 300+ (0/1)	0.0912	0.2879	0.0115	0.0925	0.0905
Industry: Agriculture (0/1)	0.0210	0.1434	0.0073	0.0310	0.0199
Industry: Production of food (0/1)	0.0370	0.1887	0.0070	0.0147	0.0411
Industry: Production of cons. goods (0/1)	0.0264	0.1603	0.0067	0.0292	0.0254
Industry: Prod. of commercial goods (0/1)	0.0313	0.1743	0.0080	0.0319	0.0305
Industry: Prod. of investment goods (0/1)	0.0224	0.1481	0.0075	0.0220	0.0223
Industry: Construction (0/1)	0.0325	0.1772	0.0100	0.0527	0.0284
Industry: Hospitality (0/1)	0.3635	0.4810	0.0083	0.2974	0.3700
Industry: Traffic, logistics, storage (0/1)	0.2532	0.4348	0.0096	0.3293	0.2418
Industry: Education (0/1)	0.1885	0.3911	0.0089	0.1689	0.1984
Industry: Missing (0/1)	0.0242	0.1536	0.0111	0.0230	0.0222

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.
Source: SIAB (2017) and own calculations.

Table A.4 Full estimation results - linear specifications Sample A **Table 2** (SIAB)

	Linear no controls (1)		Linear basic controls (2)		Linear extend. controls (3)	
	Coeff.	Std.Err.	Coeff.	Std.Err.	Coeff.	Std.Err.
After (0/1)	0,00055	0,00041	0,00044	0,00041	0,000097	0,00042
Time	-0,00003	0,00002 **	-0,00003	0,00002 **	0,00002	0,00002
After*Time	0,00006	0,00002 ***	0,00005	0,00002 ***	0,00005	0,00002 **
Female (0/1)	-	-	-0,00666	0,00041 ***	-0,00510	0,00041 ***
Age: 30-34 (0/1) (ref.)	-	-	-	-	-	-
Age: 35-39 (0/1)	-	-	-0,0043	0,0004 ***	-0,0036	0,0004 ***
Age: 40-44 (0/1)	-	-	-0,0066	0,0004 ***	-0,0053	0,0004 ***
Age: 45-49 (0/1)	-	-	-0,0092	0,0004 ***	-0,0076	0,0004 ***
Age: 50-54 (0/1)	-	-	-0,0119	0,0004 ***	-0,0097	0,0004 ***
Age: 55-59 (0/1)	-	-	-0,0139	0,0004 ***	-0,0111	0,0004 ***
East Germany (0/1)	-	-	0,0062	0,0006 ***	0,0052	0,0006 ***
Foreign Nationality (0/1)	-	-	-0,0027	0,0004 ***	-0,0029	0,0004 ***
Educ: No vocational degree (0/1)	-	-	-	-	0,0026	0,0003 ***
Educ: Vocational degree (0/1)	-	-	-	-	0,0044	0,0003 ***
Educ: Tertiary education (0/1)	-	-	-	-	0,0052	0,0007 ***
Educ: Missing (0/1) (ref.)	-	-	-	-	-	-
Tenure: 0-3 months (0/1) (ref.)	-	-	-	-	-	-
Tenure: 4-6 months (0/1)	-	-	-	-	-0,0069	0,0006 ***
Tenure: 7-12 months (0/1)	-	-	-	-	-0,0089	0,0006 ***
Tenure: 13-18 months (0/1)	-	-	-	-	-0,0109	0,0006 ***
Tenure: 19-24 months (0/1)	-	-	-	-	-0,0118	0,0006 ***
Tenure: 25-36 months (0/1)	-	-	-	-	-0,0122	0,0005 ***
Tenure: 37-48 months (0/1)	-	-	-	-	-0,0130	0,0005 ***
Tenure: 49+ months (0/1)	-	-	-	-	-0,0139	0,0006 ***
Occup.: Agriculture and other (0/1) (ref.)	-	-	-	-	-	-
Occup.: Simple manual tasks (0/1)	-	-	-	-	0,0005	0,0006
Occup.: Qualified manual tasks (0/1)	-	-	-	-	0,0035	0,0008 ***
Occup.: Engineer, techn., semi-prof. (0/1)	-	-	-	-	0,0060	0,0008 ***
Occup.: Simple service (0/1)	-	-	-	-	0,0011	0,0005 *
Occup.: Qualified service (0/1)	-	-	-	-	0,0022	0,0007 ***
Occup.: Professional and manager (0/1)	-	-	-	-	0,0016	0,0011
Occup.: Simple administration (0/1)	-	-	-	-	0,0014	0,0006 **
Occup.: Qualified administration (0/1)	-	-	-	-	0,0025	0,0006 ***
Firm-size: 0-19 (0/1) (ref.)	-	-	-	-	-	-
Firm-size: 20-99 (0/1)	-	-	-	-	0,0016	0,0003 ***
Firm-size: 100-199 (0/1)	-	-	-	-	0,0022	0,0005 ***
Firm-size: 200-299 (0/1)	-	-	-	-	0,0033	0,0007 ***
Firm-size: 300+ (0/1)	-	-	-	-	0,0035	0,0004 ***
Industry: Agriculture (0/1) (ref.)	-	-	-	-	-	-
Industry: Production of food (0/1)	-	-	-	-	-0,0003	0,0008
Industry: Production of cons. goods (0/1)	-	-	-	-	-0,0015	0,0009 *
Industry: Prod. of commercial goods (0/1)	-	-	-	-	-0,0001	0,0009
Industry: Prod. of investment goods (0/1)	-	-	-	-	-0,0009	0,0009
Industry: Construction (0/1)	-	-	-	-	0,0018	0,0009 *
Industry: Hospitality (0/1)	-	-	-	-	0,0005	0,0007
Industry: Traffic, logistics, storage (0/1)	-	-	-	-	0,0006	0,0007
Industry: Education (0/1)	-	-	-	-	0,0002	0,0007
Industry: Missing (0/1)	-	-	-	-	0,0008	0,0010
Constant	0,0078	0,0003 ***	0,0232	0,0007 ***	0,0242	0,0011 ***

Source: SIAB (2017) and own calculations.

Table A.5 Descriptive Statistics - Basic Controls 2013 Sample (SIAB)

	Full Sample		Mean Transition Rate when variable has	
Variable	Mean	Std. Dev.	value 0	value 1
Transition (0/1)	0.0162	0.1261	0.0000	1.0000
After (0/1)	0.4939	0.4999	0.0164	0.0159
Time (in days)	12.518	210.86	-	-
Female (0/1)	0.7906	0.4069	0.0233	0.0143
Age: 30-34 (0/1)	0.1214	0.3266	-	0.0271
Age: 35-39 (0/1)	0.1299	0.3361	-	0.0240
Age: 40-44 (0/1)	0.1718	0.3772	-	0.0185
Age: 45-49 (0/1)	0.2021	0.4016	-	0.0147
Age: 50-54 (0/1)	0.1999	0.3999	-	0.0113
Age: 55-59 (0/1)	0.1749	0.3799	-	0.0076
East Germany (0/1)	0.1247	0.3304	0.0157	0.0196
Foreign Nationality (0/1)	0.1288	0.3349	0.0153	0.0217

Source: SIAB (2017) and own calculations.

Table A.6 Test for covariate discontinuity

	Sample A		Sample B		Sample C	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
Basic Specification:						
Female (0/1)	-0.0058	0.0022 ***	-0.0020	0.0022	-0.0004	0.0022
Age (Linear)	-0.0632	0.0583	-0.0477	0.0589	0.0228	0.0610
East Germany (0/1)	0.0006	0.0017	0.0011	0.0017	-0.0002	0.0017
Foreign Nationality (0/1)	-0.0015	0.0020	-0.0010	0.0020	0.0002	0.0021
Educ: No vocational degree (0/1)	0.0006	0.0018	0.0002	0.0018	0.0005	0.0019
Educ: Vocational degree (0/1)	-0.0010	0.0028	-0.0006	0.0028	-0.0016	0.0029
Educ: Tertiary education (0/1)	-0.0005	0.0033	0.0004	0.0033	0.0012	0.0034
Educ: Missing (0/1)	0.0009	0.0013	-0.00002	0.0014	-0.0001	0.0014
Extended Specification:						
Tenure (Linear)	-0.5858	0.0980 ***	0.1866	0.0969 *	0.5113	0.0928 ***
Occup.: Agriculture and other (0/1)	0.0012	0.0013	0.0006	0.0013	0.0011	0.0014
Occup.: Simple manual tasks (0/1)	0.0010	0.0019	0.0015	0.0019	0.0007	0.0019
Occup.: Qualified manual tasks (0/1)	0.0017	0.0014	0.0011	0.0014	0.0008	0.0014
Occup.: Engineer, techn., semi-prof. (0/1)	0.0002	0.0015	0.0003	0.0015	0.0001	0.0002
Occup.: Simple service (0/1)	-0.0015	0.0035	-0.0019	0.0035	-0.0033	0.0036
Occup.: Qualified service (0/1)	-0.0020	0.0019	-0.0013	0.0019	-0.0008	0.0020
Occup.: Professional and manager (0/1)	0.0002	0.0008	-0.0002	0.0008	-0.0001	0.0009
Occup.: Simple administration (0/1)	-0.0042	0.0028	-0.0026	0.0029	-0.0025	0.0030
Occup.: Qualified administration (0/1)	0.0035	0.0024	0.0024	0.0024	0.0039	0.0026
Firm-size: 0-19 (0/1)	-0.0005	0.0035	-0.0029	0.0035	0.0011	0.0037
Firm-size: 20-99 (0/1)	0.0049	0.0029 *	0.0049	0.0030 *	0.0035	0.0031
Firm-size: 100-199 (0/1)	-0.0002	0.0018	0.0003	0.0018	-0.0009	0.0019
Firm-size: 200-299 (0/1)	-0.0009	0.0013	-0.0006	0.0013	-0.0004	0.0013
Firm-size: 300+ (0/1)	-0.0032	0.0021	-0.0017	0.0021	-0.0033	0.0022
Industry: Agriculture (0/1)	0.0025	0.0010 **	0.0019	0.0010 *	0.0019	0.0011 *
Industry: Production of food (0/1)	-0.0007	0.0014	-0.0002	0.0014	-0.0005	0.0015
Industry: Production of cons. goods (0/1)	0.0004	0.0011	0.0003	0.0012	0.0005	0.0012
Industry: Prod. of commercial goods (0/1)	0.0013	0.0012	0.0015	0.0012	0.0015	0.0013
Industry: Prod. of investment goods (0/1)	0.0004	0.0010	0.0002	0.0011	0.0002	0.0011
Industry: Construction (0/1)	0.0010	0.0012	0.0008	0.0012	0.0014	0.0013
Industry: Hospitality (0/1)	0.0125	0.0034 ***	0.0088	0.0035 **	0.0080	0.0036 **
Industry: Traffic, logistics, storage (0/1)	0.0055	0.0031 *	0.0032	0.0031	0.0019	0.0032
Industry: Education (0/1)	-0.0022	0.0028	-0.0017	0.0029	-0.0002	0.0030
Industry: Missing (0/1)	-0.0206	0.0011 ***	-0.0149	0.0011 ***	-0.0146	0.0011 ***

Note: The table presents tests for covariate continuity at the reform date. A linear regression of the discontinuity model as in equation (1) was estimated with each explanatory variable being used as the dependent variable. The table presents the coefficient (α_2) of the discontinuity indicator (*after*) and its standard error using the quadratic specification for time. Standard errors are clustered at the individual level; * p<0.10, ** p<0.05, ***p<0.01.
Source: SIAB (2017) and own calculations.

Table A.7 Estimation Results – SIAB – by gender – samples B and C

	Sample B - 2003						Sample C - 2003					
	(1) Men			(2) Women			(3) Men			(4) Women		
Specification	Coeff		RE	Coeff		RE	Coeff		RE	Coeff		RE
1 Linear no controls	0.0055	***	48.7%	-0.0006		48.7%	0.0037	**	33.6%	-0.0011	***	-13.6%
2 Linear basic controls	0.0054	***	47.8%	-0.0006		47.8%	0.0038	**	34.5%	-0.0011	***	-13.6%
3 Linear ext. controls	0.0053	***	46.9%	-0.0006		46.9%	0.0037	**	33.6%	-0.0010	**	-12.3%
4 Quadratic no controls	0.0071	***	62.8%	0.0004		62.8%	0.0062	**	56.4%	-0.0002		-2.5%
5 Quadr. basic controls	0.0071	***	62.8%	0.0003		62.8%	0.0063	***	57.3%	-0.0002		-2.5%
6 Quadr. ext. controls	0.0070	***	61.9%	0.0004		61.9%	0.0061	**	55.5%	-0.0001		-1.2%
N	75,333			675,884			70,929			644,384		
Pre-reform mean Y	0.0113			0.0082			0.0110			0.0081		

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

Table A.8 Robustness test - inflow sample (SIAB)

	Inflow A - 2003 (1) Men			Inflow A - 2003 (2) Women		Inflow B - 2003 (3) Men			Inflow B - 2003 (4) Women		
Specification	Coeff		RE	Coeff	RE	Coeff		RE	Coeff		RE
1 Linear no controls	0.0097	**	42.2%	0.0011	7.9%	0.0075	*	32.6%	0.0013		9.4%
2 Linear basic controls	0.0101	***	43.9%	0.0011	7.9%	0.0077	*	33.5%	0.0012		8.6%
3 Linear ext. controls	0.0100	***	43.5%	0.0013	9.4%	0.0081	*	35.2%	0.0018		12.9%
4 Quadratic no controls	0.0151	**	65.7%	0.0027	19.4%	0.0122	*	53.0%	0.0038	**	27.3%
5 Quadr. basic controls	0.0152	**	66.1%	0.0027	19.4%	0.0121	*	52.6%	0.0037	**	26.6%
6 Quadr. ext. controls	0.0150	**	65.2%	0.0027	19.4%	0.0125	*	54.3%	0.0038	**	27.3%
N	38,744			205,548		18,900			123,488		
Pre-reform mean Y	0.0230			0.0139		0.0230			0.0139		

Source: SIAB (2017) and own calculations.

Table A.9 Robustness test - changed observation window (SIAB)

	Sample A - 2003 + / - 9 months around reform date								
	(1) All			(2) Women			(3) Men		
Specification	Coeff		RE	Coeff		RE	Coeff		RE
1 Linear no controls	0.0069		61.1%	-0.0005		-6.1%	0.0100	***	41.7%
2 Linear basic controls	0.0006		5.3%	-0.0005		-6.1%	0.0096	***	40.0%
3 Linear ext. controls	0.0002		1.8%	-0.0007		-8.5%	0.0074	***	30.8%
4 Quadratic no controls	0.0016	**	14.2%	0.0012	*	14.6%	0.0047		19.6%
5 Quadr. basic controls	0.0015	**	13.3%	0.0011		13.4%	0.0049	*	20.4%
6 Quadr. ext. controls	0.0013	*	11.5%	0.0010		12.2%	0.0034		14.2%
N	631,829			561,749			70,080		
Pre-reform mean Y	0.0113			0.0082			0.0240		

	Sample A - 2003 + / - 15 months around reform date								
	(1) All			(2) Women			(3) Men		
Specification	Coeff		RE	Coeff		RE	Coeff		RE
1 Linear no controls	0.0003		2.7%	-0.0005		-6.1%	0.0067	***	27.9%
2 Linear basic controls	0.0002		1.8%	-0.0005		-6.1%	0.0062	***	25.8%
3 Linear ext. controls	-0.0002		-1.8%	-0.0008	**	-9.8%	0.0042	***	17.5%
4 Quadratic no controls	0.0010	*	8.8%	-0.0001		-1.2%	0.0103	***	42.9%
5 Quadr. basic controls	0.0009		8.0%	-0.0002		-2.4%	0.0100	***	41.7%
6 Quadr. ext. controls	0.0006		5.3%	-0.0004		-4.9%	0.0083	***	34.6%
N	1,077,118			956,042			121,076		
Pre-reform mean Y	0.0113			0.0082			0.0240		

Source: SIAB (2017) and own calculations.

Table A.10 Robustness test - donut estimation for 2003 Sample A (SIAB)

	All		Men		Women	
	(1)		(2)		(3)	
Specification	Coeff	RE	Coeff	RE	Coeff	RE
1 Linear no controls	0.0001	1.2%	0.0079 ***	69.9%	-0.0009 **	9.8%
2 Linear basic controls	-0.0001	-1.2%	0.0074 ***	65.5%	-0.0010 **	10.9%
3 Linear ext. controls	-0.0006	-7.1%	0.0050 ***	44.2%	-0.0013 ***	14.1%
4 Quadratic no controls	0.0005	5.9%	0.0094 ***	83.2%	-0.0007	7.6%
5 Quadr. basic controls	0.0004	4.7%	0.0091 ***	80.5%	-0.0007	7.6%
6 Quadr. ext. controls	-0.0003	-3.5%	0.0071 ***	62.8%	0.0001	1.1%
N	821,864		92,089		729,775	
Pre-reform mean Y	0.0085		0.0113		0.0092	

Note: The estimations use the original Sample A data for 2003. They omit Minijob observations in the month of March 2003, which would be the first affected by the reform.
Source: SIAB (2017) and own calculations.

Table A.11 Changes in transition rates by gender (SIAB)

	+ / - 1 Year	+ / - 2 Years	+ / - 3 Years
	(1)	(2)	(3)
Specification	Coeff	Coeff	Coeff
1 No controls	-0.0053 ***	-0.0056 ***	-0.0058 ***
2 Basic controls	-0.0049 ***	-0.0050 ***	-0.0051 ***
3 Extended controls	-0.0042 ***	-0.0042 ***	-0.0042 ***
N	853,241	1,779,354	2,779,069
Mean Y	0.0087	0.0094	0.0100

Note: The table shows the coefficient of the interaction of the indicator variables *female* times *post reform* in a difference in difference setting where *female* and post reform main effects are controlled in all three specifications. The estimations use a sample of all observed person months in Minijob employment (comparable to Sample A) over alternative observation windows. Column (1) considers all observations between April 2002 and March 2004, column (2) considers all observations between April 2001 and March 2005, and column (3) considers all observations between April 2000 and March 2006. The rows differ in terms of control variables and use the specifications as in **Table 2**. Standard errors are clustered at the person level.
Source: SIAB (2017) and own calculations.

Table A.12 Heterogeneity by Minijob Tenure (SIAB)

	Sample A (1) All		Sample A (2) Men		Sample A (3) Women	
	Coeff	RE	Coeff	RE	Coeff	RE
Panel A: Tenure < 24 months						
1 Linear no controls	0.0011	9.5%	0.0120 ***	73.2%	-0.0006	-5.5%
2 Quadratic no controls	0.0019 *	16.4%	0.0119 ***	72.6%	0.0004	3.7%
N	419,364		57,991		361,373	
Pre-reform mean Y	0.0116		0.0164		0.0109	
Panel B: Tenure >= 24 months						
1 Linear no controls	-0.0002	-3.6%	0.0019	44.2%	-0.0004	-7.0%
2 Quadratic no controls	0.0005	9.1%	0.0050 **	116.3%	0.0001	1.8%
N	433,877		37,286		396,591	
Pre-reform mean Y	0.0055		0.0043		0.0057	

Source: SIAB (2017) and own calculations.

Table A.13 Test of the equality of mean characteristics of treatment and control group before and after the reform

Variable	Treated p Value	Controls p Value
Basic Specification		
Female (0/1)	0.3084	** 0.0103
Age: 30-34 (0/1)	0.3189	** 0.0161
Age: 35-39 (0/1)	0.6409	*** 0.0046
Age: 40-44 (0/1)	0.6688	*** 0.0022
Age: 45-49 (0/1)	0.6537	0.4712
Age: 50-54 (0/1)	0.2847	0.8293
Age: 55-59 (0/1)	0.4355	* 0.0997
East Germany (0/1)	** 0.0156	0.4940
Foreign Nationality (0/1)	0.7863	0.9587
Extended Specification		
Educ: No vocational degree (0/1)	0.2460	0.6939
Educ: Vocational degree (0/1)	0.1199	0.1513
Educ: Tertiary education (0/1)	0.3683	*** 0.0094
Educ: Missing, dropout, in school (0/1)	0.9413	0.6671
Tenure: 0-3 months (0/1)	0.2700	0.7721
Tenure: 4-6 months (0/1)	0.9939	0.7952
Tenure: 7-12 months (0/1)	0.8106	0.5044
Tenure: 13-18 months (0/1)	0.1937	0.2577
Tenure: 19-24 months (0/1)	0.1478	0.1010
Tenure: 25-36 months (0/1)	0.6206	0.1123
Tenure: 37-48 months (0/1)	0.6521	0.7441
Tenure: 49+ months (0/1)	0.7293	0.8910
Occup.: Agriculture and other (0/1)	* 0.0773	0.3011
Occup.: Simple manual tasks (0/1)	0.3036	* 0.0997
Occup.: Qualified manual tasks (0/1)	0.5875	0.1774
Occup.: Engineer, technician, semi-prof. (0/1)	0.4331	0.3802
Occup.: Simple service (0/1)	0.1597	0.9368
Occup.: Qualified service (0/1)	0.6792	0.7174
Occup.: Professional and manager (0/1)	0.7362	* 0.0834
Occup.: Simple administration (0/1)	0.7963	0.5363
Occup.: Qualified administration (0/1)	0.9827	0.8985
Firm-size: 0-19 (0/1)	** 0.0232	0.8473
Firm-size: 20-199 (0/1)	** 0.0410	0.2873
Firm-size: 200+ (0/1)	** 0.0337	0.9197
Firm-size: Self-employed (0/1)	0.6325	0.9739
Firm-size: Missing (0/1)	0.3008	0.3232
Industry: Agriculture and mining (0/1)	0.8499	0.1738
Industry: Retail, repair, maintenance (0/1)	0.3489	0.1186
Industry: Hospitality (0/1)	0.8093	0.3127
Industry: Traffic, logistics, telecom. (0/1)	0.4956	0.5497
Industry: Banking, real estate (0/1)	0.1039	* 0.0719
Industry: Public Admin, Educ., Military (0/1)	0.2230	0.3631
Industry: Health and social system (0/1)	0.9408	0.1172
Industry: Other services (0/1)	0.2020	0.6736
Industry: Private household (0/1)	0.8131	0.2246
Industry: Missing (0/1)	0.2102	*** 0.0095
N	274	2,460

Note: The table shows p-values of two-sided hypothesis tests of equality of means. The data are not weighted.
Source: SOEP wave 35 and own calculations.

Table A.14 Descriptive Statistics - Extended Controls: 2003 and 2013 Samples (SOEP)

	Sample 2003		Sample 2013	
	Mean	Std. dev.	Mean	Std. dev.
Educ: No vocational degree (0/1)	0.1579	0.3647	0.1861	0.3892
Educ: Vocational degree (0/1)	0.7438	0.4366	0.6660	0.4717
Educ: Tertiary education (0/1)	0.0815	0.2737	0.1018	0.3024
Educ: Missing, dropout, in school (0/1)	0.0168	0.1286	0.0462	0.2099
Tenure: 0-3 months (0/1)	0.0596	0.2367	0.0719	0.2584
Tenure: 4-6 months (0/1)	0.0779	0.2680	0.0943	0.2922
Tenure: 7-12 months (0/1)	0.1809	0.3850	0.1733	0.3785
Tenure: 13-18 months (0/1)	0.1060	0.3079	0.1050	0.3065
Tenure: 19-24 months (0/1)	0.0599	0.2374	0.0601	0.2377
Tenure: 25-36 months (0/1)	0.0961	0.2948	0.0913	0.2880
Tenure: 37-48 months (0/1)	0.0705	0.2561	0.0775	0.2675
Tenure: 49+ months (0/1)	0.3490	0.4768	0.3266	0.4691
Occup.: Agriculture and other (0/1)	0.1133	0.3170	0.0749	0.2633
Occup.: Simple manual tasks (0/1)	0.0194	0.1379	0.0222	0.1472
Occup.: Qualified manual tasks (0/1)	0.0497	0.2174	0.0708	0.2565
Occup.: Engineer, technician, semi-prof. (0/1)	0.0947	0.2928	0.1110	0.3141
Occup.: Simple service (0/1)	0.2917	0.4546	0.3708	0.4831
Occup.: Qualified service (0/1)	0.0906	0.2872	0.0759	0.2648
Occup.: Professional and manager (0/1)	0.0208	0.1429	0.0180	0.1331
Occup.: Simple administration (0/1)	0.1495	0.3566	0.1211	0.3263
Occup.: Qualified administration (0/1)	0.1703	0.3760	0.1354	0.3422
Firm-size: 0-19 (0/1)	0.5830	0.4932	0.5548	0.4970
Firm-size: 20-199 (0/1)	0.1846	0.3880	0.2028	0.4021
Firm-size: 200+ (0/1)	0.1107	0.3139	0.1725	0.3779
Firm-size: Self-employed (0/1)	0.0223	0.1477	0.0199	0.1397
Firm-size: Missing (0/1)	0.0994	0.2993	0.0499	0.2178
Industry: Agriculture and mining (0/1)	0.1257	0.3316	0.1339	0.3405
Industry: Retail, repair, maintenance (0/1)	0.2255	0.4180	0.1870	0.2900
Industry: Hospitality (0/1)	0.0508	0.2196	0.0834	0.2765
Industry: Traffic, logistics, telecom. (0/1)	0.0227	0.1488	0.0454	0.2083
Industry: Banking, real estate (0/1)	0.1363	0.3432	0.1772	0.3819
Industry: Public Admin, Educ., Military (0/1)	0.0512	0.2204	0.0811	0.2730
Industry: Health and social system (0/1)	0.1312	0.3377	0.1373	0.3441
Industry: Other services (0/1)	0.0724	0.2591	0.0529	0.2240
Industry: Private household (0/1)	0.0424	0.2015	0.0407	0.1977
Industry: Missing (0/1)	0.1418	0.3489	0.0610	0.2394
N	2,736		5,326	

Note: The data are not weighted.
Source: SOEP wave 35 and own calculations.

Table A.15 Descriptive Statistics - Basic Controls: 2013 Reform Sample (SOEP)

	Descriptives		Mean transition rate when variable has	
Variable	Mean	Std. Dev.	value 0	value 1
Transition (0/1)	0.1508	0.3579	0.0000	1.0000
Post (0/1)	0.6530	0.4761	0.1201	0.1394
Treat (0/1)	0.1906	0.3928	0.0995	0.1809
Female (0/1)	0.8945	0.3073	0.1559	0.1078
Age: 30-34 (0/1)	0.1607	0.3673	-	0.1537
Age: 35-39 (0/1)	0.2047	0.4035	-	0.1347
Age: 40-44 (0/1)	0.1977	0.3983	-	0.1116
Age: 45-49 (0/1)	0.1827	0.3864	-	0.1037
Age: 50-54 (0/1)	0.1384	0.3453	-	0.1108
Age: 55-59 (0/1)	0.1158	0.3201	-	0.0678
East Germany (0/1)	0.1066	0.3087	0.1085	0.1667
Foreign Nationality (0/1)	0.1825	0.3863	0.1148	0.1068

Note: The descriptive statistics describe the sample of 5,326 person-year observations. The data are not weighted.
Source: SOEP wave 35 and own calculations.