

Herget, Anna; Riphahn, Regina T.

Working Paper

Phasing Out Payroll Tax Subsidies

CESifo Working Paper, No. 11605

Provided in Cooperation with:

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

Suggested Citation: Herget, Anna; Riphahn, Regina T. (2024) : Phasing Out Payroll Tax Subsidies, CESifo Working Paper, No. 11605, CESifo GmbH, Munich

This Version is available at:

<https://hdl.handle.net/10419/312115>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Phasing Out Payroll Tax Subsidies

Anna Herget, Regina T. Riphahn

Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

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

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

Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

Editor: Clemens Fuest

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

An electronic version of the paper may be downloaded

- from the SSRN website: www.SSRN.com
- from the RePEc website: www.RePEc.org
- from the CESifo website: <https://www.cesifo.org/en/wp>

Phasing Out Payroll Tax Subsidies

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.

JEL-Codes: J210, J380, H240.

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

Anna Herget
FAU Erlangen-Nürnberg / Germany
anna.herget@fau.de

*Regina T. Riphahn**
FAU Erlangen-Nürnberg / Germany
regina.riphahn@fau.de

*corresponding author

December 19, 2024

We thank two anonymous referees and the editor for insightful comments that helped us improve the paper. We are grateful to the German Research Foundation (DFG) for funding project RI 856/10-1 and 10-2. The paper benefited from discussions with Alisa Tazhitdinova and comments from Matthias Collischon, Kamila Cygan-Rehm, Markus Nagler, and Amelie Schiprowski. We thank participants of the Ausschuss für Bevölkerungsökonomik (2023), the BGPE research workshop (2023), Economics Department seminar at FAU (2023), European Economic Association meetings (2022), the UC Santa Barbara seminar (2022), annual meetings of the Verein für Socialpolitik (2021), the European Association of Labour Economists (2021), a Brown Bag Seminar at the University of Würzburg (2021), and the Applied Microeconomics Workshop at FAU Erlangen-Nürnberg (2021) for helpful comments and suggestions.

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 to incentivize labor supply and earnings increases for workers with very low earnings. This is a challenge in many labor markets.

The Midijob program was implemented on April 1, 2003. It reduces employee social insurance contributions for jobs within the Midijob earnings range, originally 400 to 800 Euro per month. It was introduced to address a labor supply disincentive generated by Minijobs; Minijobs are low-income employments paying up to 400 Euro per month at the time. They cause substantial bunching at the Minijob earnings limit (Gudgeon 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**

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).

illustrates the uneven number of jobs across the earnings distribution. Midijobs were designed to address the Minijob trap and we investigate whether the Midijob program succeeds in this respect.

Both programs, Minijobs and Midijobs are used extensively. At the end of 2021, 4.1 million individuals used Minijobs as their main employment and about 3 million workers were employed in Midijobs (see Appendix **Figure A.2**).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. First, we exploit the introduction of Midijobs on April 1, 2003, to evaluate their immediate effects on the propensity to transition from a Minijob to higher-earning, regular employment. For this purpose, we apply a regression discontinuity in time (RDiT) design to administrative data and investigate whether there is a jump in the transition probability at the cutoff date. However, this approach may suffer from shortcomings, e.g., by capturing only local effects. Our second approach exploits a unique feature of income tax regulations. Specifically, Midijobs reduced workers' social insurance contribution rates in the earnings range just above the Minijob earnings limit leaving income tax rates untouched. This attenuates the kink in the net earnings schedule, particularly for individuals who pay low or no income taxes (e.g., single individuals). In 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

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.2** 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.

the reform whereas the latter group is our control group. We use survey data from the German Socioeconomic Panel (SOEP) which offers information on marital status and apply a difference-in-differences (DID) analysis to study the propensity to transition from a Minijob to higher-earning, regular employment. The DID analysis yields an average treatment effect on the treated which captures more sluggish labor supply responses than the discontinuity design.

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

These findings complement the results of Tazhitdinova (2020) and Gudgeon and Trenkle (2023). 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. 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.

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).

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

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

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).

programs. If the Midijob program effectively supports transitions to extended labor supply it could constitute a useful policy for other (national) labor markets, as well.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 offer a policy discussion and conclude.

2. Background

2.1 Institutions and Reforms

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

At the Minijob earnings ceiling, the sudden fade-out of the wage subsidy inherent in Minijob employment generates a large, discontinuous change in tax and social insurance liabilities. When they earn below the Minijob earnings ceiling employees pay neither social insurance contributions nor income taxes. When they earn above the Minijob earnings ceiling, income taxes on total earnings plus social insurance contributions of about 20 percent become

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.

payable (see **Figure 2**). This leads to a (large) reduction in net monthly earnings. While social insurance contributions are relevant for all employees, income taxes differ for joint and individual filers; in 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, i.e., secondary earners in marriages, may be affected by sizeable tax rates on their entire earnings as soon as they exceed the Minijob earnings threshold. Thus, the Minijob earnings threshold generates a discontinuity in the level (a notch) and the slope (a kink) of the net earnings schedule (Kleven 2016). This "Minijob trap" bars increases in labor supply and earnings and causes substantial bunching in the earnings distribution (see **Figure 1**) (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

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).

insurance contribution rates of 20 percent, the Midijob rates increased on a sliding scale starting at about 4 percent for monthly earnings of 400 Euro and reaching the unsubsidized level of 20 percent for monthly earnings of 800 Euro. At the same time, Midijob employees are fully liable for income taxes on their total earnings (see **Figure 2**).¹²

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

The introduction of the Midijob subsidy of social insurance contribution rates was intended to attenuate the notch in the net earnings distribution, phase out the Minijob subsidy, and incentivize labor supply and earnings beyond the Minijob earnings ceiling. We investigate whether Midijobs effectively reduced the barriers to exiting subsidized Minijob employment and entering higher-earning employment. A priori, as the Midijob subsidy only covers social insurance contributions but not income taxes, the policy left a substantial tax notch for secondary earners. Therefore, we should not expect the Midijob reform to affect transitions from Minijobs for secondary earners much.

2.2 Utilization Patterns and Changes in the Earnings Distribution

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

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

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

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

The reform of April 1, 2003 not only introduced the Midijob subsidy for social insurance contributions. It also shifted the threshold of the Minijob earnings limit from 325 to 400 Euros per month. We present the distribution of monthly gross earnings after the reform (2003-2012)

in **Figure 1. Figure 3** shows the change in the distribution of monthly gross earnings between 2002 and 2004. The frequency of jobs earning 325-400 Euro increased, that of jobs earning 250-325 Euro declined. Interestingly, the frequency of employments in the Midijob earnings range between 400 and 800 Euro did not increase.

3. Discontinuous Increase in Transitions out of Minijobs?

3.1 Empirical Strategy: Regression Discontinuity Design

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

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 rate. To allow for a change in overall time trends in transitions out of Minijobs after the reform date we consider interaction effects of *time* and *after*. The baseline specification for individual i observed in a Minijob in period t is

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

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

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

the dependent variable at the monthly level¹⁴ and apply three alternative specifications of the covariate vector X as robustness checks.

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 effect.¹⁵ 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 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.¹⁶

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

¹⁵ 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.

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

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

Another important aspect is that the Minijob earnings threshold increased on April 1, 2003 from 325 to 400 Euros at the same time as Midijobs were introduced. This may attenuate transitions out of Minijob employment: after April 1, 2003, small earnings increases could be realized without leaving a Minijob. Therefore, the estimate of α_2 represents the combined effect of introducing Midijobs and expanding the Minijob earnings ceiling.

3.2 Administrative data for RDD analysis (SIAB)

For the analysis of changes in transitions out of Minijobs, we use administrative data. The Sample of Integrated Labour Market Biographies (SIAB) data offer a 2 percent random sample of all individuals registered with the unemployment insurance between 1975 and 2017 (Antoni et al., 2019).¹⁷ 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, 2003.¹⁸ We select those aged 30-59 to exclude students and retirees who may be subject to additional regulations. We omit

¹⁷ 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

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

Minijobbers who at the same time receive unemployment benefits. 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). Dropping Minijob observations from employments that started after the reform date of April 1, 2003 reduces the sample size to 751,217 (Sample B). Omitting all Minijob observations from employments that started after 31.12.2002 reduces the sample size to 715,313 (Sample C). Appendix **Table A.2** summarizes the characteristics of the three samples.

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

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

3.3 Baseline results of discontinuity analyses

Figure 4 presents graphic evidence of the development of the de-seasonalized transition rates with linear time trends for the three subsamples. We obtain mixed results: for the full sample A we find an upward shift in transition rates at the reform date and a change in slopes (**Figure 4.1**). With sample B we find no shift and declining transition rates throughout (**Figure 4.2**), and with sample C we obtain a downward shift in transition rates (**Figure 4.3**).¹⁹

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

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

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 (see **Table A.4** for the full results). The coefficient estimate is small and statistically insignificant. It indicates that the propensity to leave a Minijob for higher-earning employment increased on the reform date by .05 percent points or 6.5 percent of the pre-reform mean transition probability (see column entitled RE). Row 2 adds basic controls to the linear model (gender, age groups, citizenship, East vs. West German residence), row 3 considers additional control variables (education, tenure, occupation, industry, firm-size), rows 4-6 allow for quadratic time trends before and after the reform. Overall, we do not find large reform effects at the discontinuity.²⁰

The estimates for α_2 of Sample B do not differ substantively from those in column 1. The relative effect sizes (see the column entitled RE) based on the quadratic specifications are again slightly larger than those in rows 1-3. In column 3 we repeat our analyses for Sample C. The estimate in row 1 turns significantly negative while all other specifications again yield small positive and statistically insignificant estimates.

On April 1, 2003 two changes were implemented: Midijob subsidies were introduced and the Minijob earnings threshold increased from 325 to 400 Euro. The shift of the Minijob earnings ceiling may have attenuated any positive Midijob effect because prior Minijob employees could realize higher earnings without leaving the Minijob framework. We apply two approaches to determine whether the change in the Minijob earnings ceiling affected our results. First, we exploit the increase of the Minijob earnings ceiling from 400 to 450 Euro per month on January 1, 2013 to study the potential impact of a change in the Minijob earnings ceiling on transition rates. Column 4 of **Table 2** presents the estimation results that obtain when we apply the procedure used in column 1 to the reform of 2013 (see **Table A.5** for descriptive statistics).

²⁰ 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.

Our sample here considers individuals who worked in Minijobs between Jan. 1, 2012, and Dec. 31, 2014. 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. Overall, these results suggest that the extension of the Minijob earnings ceiling in 2013 did not affect transition rates out of Minijobs in important ways. If the same pattern had held ten years earlier for the 2003 reform, then the positive effects observed in columns 1-3 would have been larger without the change in thresholds. Thus, we may underestimate the impact of the introduction of Midijobs in 2003. In a second approach, we redefine the outcome measure in the pre-reform period to indicate transitions to regular employments earning beyond 400 Euro per month rather than merely an exit from Minijob employment. This eliminates the change in the earnings threshold that occurred on the actual reform date. **Table A.6** in the appendix shows that the main results are hardly affected by this adjustment.²¹

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

3.4 Regression discontinuity results - heterogeneity and robustness

Given the heterogeneity in the Minijob utilization of men and women, we separately analyze the two subsamples. We observe only 95,277 male but 757,964 female Minijob months with much higher average monthly transition rates for men (1.4 percent) than for women (0.8 percent). **Figure 5.1** confirms this level difference and suggests that while males responded to

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

the reform, females did not. Columns 1 and 2 of **Table 3** show the estimation results for α_2 based on Sample A as in **Table 2** by gender. The estimates indicate substantial differences: While the estimates for males are statistically significant, positive, and sizable those for females are insignificant, small, and partly negative. The effects for men indicate an increase in the mean transition rate out of Minijobs at the point of the reform of about .6 to .9 percentage points or 50-80 percent of the pre-reform mean; the effects for women are close to zero.²² So, while we hardly find a change in average transition rates when the Midijob subsidy was introduced in 2003, we find substantial gender heterogeneities. This means that the overall effect in **Table 2** is mainly driven by males, i.e., 10 percent of the observations in our sample.

An important gender difference relates to the heterogeneity of income tax rates between secondary earners in marriages – typically females – and single individuals. In contrast to non-married individuals, secondary earners in married couples are mostly affected by high income tax burdens. Women often use Minijob employment when they are secondary earners in married couples. For them, Midijobs hardly increase the incentives to expand the labor earnings beyond the Minijob cutoff (see **Figure 2**). Section 4 (below) exploits this heterogeneity and offers detail on the institutional background. Here, we compare the two groups' responses to the reform. Based on correlation patterns of secondary earner status as observed in survey data we predicted secondary earner status based on the SIAB administrative data. Then, we separated two groups in Sample A based on predicted single vs. secondary earner status (for details see **Appendix B**). **Figure 5.2** depicts transition patterns over time based on predicted secondary earner status: while single earners responded to the reform, those predicted to be secondary earners did not. In columns 3 and 4 of **Table 3**, we describe the heterogeneity in discontinuity results for the two groups. The results indicate substantial differences between the two subsamples. While the transition rates to higher-earning regular employment among Minijobbers who are likely to be

²² We show the relevant 2013 estimation results for males and females in Appendix **Table A.8**.

married do not change after the reform we find positive significant effects among non-married individuals.

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

4. Heterogeneous Treatment Effects by Tax Status?

4.1 Empirical Strategy: Difference-in-Differences Estimation

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

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

to the reform than those with high income tax rates (e.g., secondary earners in marriages) because the reform made a relevant difference for the former but not for the latter.

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:

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

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 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.

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 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 the composition of the workforce in response to the treatment. First, we examined whether individuals might switch between treatment and control groups in response to the reform. We found that individuals changed their marital status in both directions in similar magnitudes before and after the reform. Second, we evaluated whether there were compositional changes with respect to observable characteristics within the treatment or control group over time. Such changes might indicate differential selection into the two groups in response to the reform. **Table A.10** presents the p-values for tests of the hypothesis that the mean values of treatment

and control 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 behavior (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

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.11**).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 $t+1$ transitioned to regular employment paying social insurance contributions and earning above the Minijob earnings limit; we evaluate transitions between 2001 and 2006. The average annual transition rate is 10.3 percent. The last two columns of **Table 4** describe the mean transition rates for different groups. We observe higher rates after than before the reform date. As expected, married individuals ($\text{treat} = 0$) have a much lower average transition rate than non-married individuals ($\text{treat} = 1$). The transition rate of men is higher than that of women (16 vs. 11 percent). As in the SIAB data, the likelihood of leaving Minijobs declines with age.

Figure A.7 shows the development of transition rates separately for our treatment and control groups using weighted data. Unfortunately, our data provide only two annual observations prior to the reform, i.e., 2001 and 2002. However, in these years the development of transition rates out of Minijobs is similar for treatment and control groups which suggests parallel paths prior to the reform.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 rate of about 10 percent this is a rather large unconditional effect. The result is confirmed in columns 2 and 3, where we first consider controls for basic demographics (gender, age group, East German residence, and foreign

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 year indicators and their interactions with the treatment indicator but without additional controls. **Figure 6** shows the differences between treatment and control groups. Again, outcome differences prior to the reform are insignificant, and transition rates for the non-married increase faster after the reform than those for married Minijobbers.

citizenship) and then add an extended set of controls (see table notes and Appendix **Table A.11** for descriptive statistics). Column 4 shows that the estimation results are robust when we replace the overall 'post' effect with a set of calendar year fixed effects.³⁰

Table A.12 presents the results of additional tests and describes effect heterogeneities. Column 1 shows the results after omitting those observations for which we cannot be sure whether a potential transition happened before or after the reform on April 1, 2003.³¹ 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, confirming the results of our RDIT analyses. 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

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.

control (the married) groups responded differently to this change this may bias our finding. In order to gauge the overall relevance of the ceiling shift we consider the reform of January 1, 2013 when the Minijob earnings ceiling was increased again, this time from 400 to 450 Euro per month. We evaluate the impact of this reform on changes in transition propensities. We use the same sample and treatment definitions as before just shifting the observation period to 10 years later (see **Tables A.11** and **A.13** for descriptive statistics).³³ **Table A.14** shows the results on Minijob transitions for the period 2011 to 2016. We evaluate the reform heterogeneity for treatment and control groups around the increase in the Minijob earnings ceiling from 400 to 450 Euros on January 1, 2013. In this case, the results yield a negative and statistically insignificant estimate of 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 **A.12** are not likely to be biased by the change in Minijob earnings ceiling that happened simultaneously with the introduction of the Midijob subsidy. The findings in **Table A.14** corroborate our finding of a significant and large increase in transitions out of Minijobs after the 2003 reform for our treatment group, the non-married.

4.4 Difference in Differences Analyses using Administrative Data

Compared to the survey data, our administrative data offer larger samples, more precise information, and are observed at monthly instead of annual frequency. Even though the administrative data lack information on marital status, we can replicate the difference-in-differences analyses using this larger database. In this section, we show two approaches to

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

approximate the survey-based analyses. In the first approach, we replace the non-married treatment indicator from section 4.3 with an indicator of male gender; in the survey data, the share of males among the non-married (22.1 percent) is six times as high as the share of males among the married (3.7 percent). Therefore, this might be a reasonable first approximation. In the second approach, we estimate a prediction equation for being non-married with the survey data and use it to predict non-married status in the administrative data. The predicted non-married status is then used as a treatment indicator in the difference-in-differences analysis (for details see **Appendix B**).

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

Table 6 shows the results of the difference-in-differences analyses. In columns 1-3 the treatment indicator reflects male gender and in columns 4-6 the treatment indicator reflects predicted non-married status. In both, we show the results of the three different specifications as in columns 1-3 of **Table 5**. In contrast to the estimations using survey data the results in **Table 6** are much more precise and confirm previously observed patterns. Across all columns, we find significantly larger increases in transition probabilities from Minijobs to regular employment among males and individuals predicted to be non-married relative to females and married individuals (i.e., likely secondary earners), respectively. The treatment effect among males is about .4 percentage points (column 3) and that among the non-married is .32 percentage points (column 6) above that of the control group. These are large effects compared to the mean monthly transition rate of 1.2 percent. While this relative effect size is smaller than that observed with the survey data based on annual observations, the patterns confirm the survey-based evidence.

4.5 Difference in Differences Analyses using Separate Outcomes

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

5. Conclusions

Many countries subsidize low-income employment or small jobs. The phasing out of such subsidies can affect labor supply incentives (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 phasing-out of Minijobs. We are the first to investigate whether introducing Midijobs effectively reduced the Minijob trap.

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

We use two complementary identification strategies to investigate the effect of the introduction of Midijobs on the propensity to exit Minijobs for regular employment. Our first empirical strategy exploits the discontinuity in the regulation over time and uses a large

administrative dataset for a regression discontinuity-type approach. We evaluate the jump in transition rates that is observed at the moment of the reform. We find small significant increases in transitions out of Minijob employment starting April 1, 2003. Heterogeneity analyses of this local effect yield that male Minijobbers strongly responded to the Midijob reform whereas the Midijob introduction hardly affected females' transitions out of Minijob employment.

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

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

Given the institutional setting, it is unsurprising that the policy reform effectively only treated a few workers. Since the reform did not address the notch generated by income taxes it structurally excluded a vast majority of Minijobbers from the reform benefits, i.e., secondary earners in married couples who are mostly women. In contrast, the Midijob benefits did accrue to non-married workers, who responded by substantially increasing their transition rates from Minijobs to regular higher-paying jobs. To abolish the Minijob trap for all Minijobbers the income tax-induced notch in the earnings distribution at the Minijob earnings threshold must be smoothed.

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

The problems resulting from phasing out small jobs subsidies essentially derive from their combination with the income tax splitting system for married couples. Our results suggest that in countries with individual income tax regimes policy instruments such as Midijobs can effectively phase out small jobs subsidies and generate incentives to transition from Minijobs to regular, higher-paying employment. Merely adjusting social insurance contributions is insufficient to abolish the Minijob trap.³⁵ 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.

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

³⁵ The literature also discusses the relevance of labor demand responses to such reforms (see Gudgeon and Trenkle 2023, Tazhitdinova 2020, Haywood and Neumann 2021 and Chetty et al. 2011).

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

Bibliography

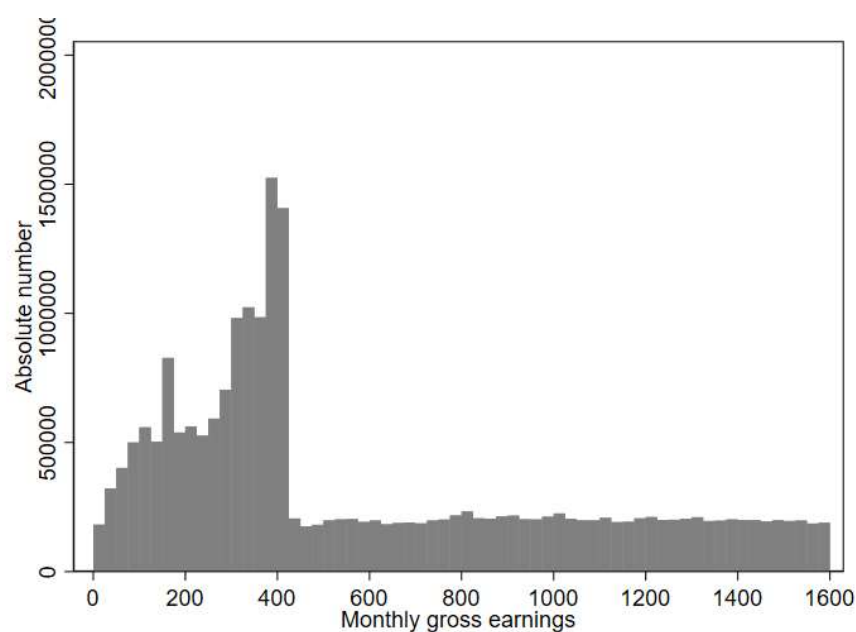
- Anderson, Patricia A. and Bruce D. Meyer (1997). The effects of firm specific taxes and government mandates with an application to the U.S. unemployment insurance program, *Journal of Public Economics* 65, 119-145.
- Anderson, Patricia A. and Bruce D. Meyer (2000). The effects of the unemployment insurance payroll tax on wages, employment, claims and denials, *Journal of Public Economics* 78, 81-106.
- Antoni, Manfred, Schmucker, Alexandra, Seth, Stefan and Philipp vom Berge (2019). Sample of Integrated Labour Market Biographies (SIAB) 1975-2017. FDZ data report, 02/2019 (en), Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB), Nuremberg.
- Azmat, Ghazala (2014) Evaluating the effectiveness of in-work tax credits, *Empirical Economics* 46, 397-425.
- BA (Bundesagentur für Arbeit) (2014). Arbeitsmarkt in Zahlen – Beschäftigungsstatistik: Geringfügig entlohnte Beschäftigte nach ausgewählten Merkmalen (ab Stichtag 30. Juni 1999). Nürnberg. BA (Bundesagentur für Arbeit) (2020), Statistik der Bundesagentur für Arbeit, Tabellen: Länderreport über Beschäftigte (Quartalszahlen) Deutschland, Stichtag: 31.12.2019, Oktober 2020, Nürnberg.
- Bach, Stefan, Hermann Buslei and Michelle Harnisch (2018a). Midijob-Reform entlastet Geringverdienende, vor allem teilzeiterwerbstätige Frauen, *DIW aktuell* Nr. 16, 28. August 2018, Berlin.
- Bach, Stefan, Peter Haan and Michelle Harnisch (2018b). Progressive Sozialbeiträge können niedrige Einkommen entlasten, *DIW Wochenbericht* 13+14, Berlin.
- Bachmann, Ronald, Ellen von den Driesch, Christoph Ehlert, Regina Flake, Hanna Frings, Sandra Schaffner, and Markus Scheuer (2012). Studie zur Analyse der geringfügigen Beschäftigungsverhältnisse: Forschungsvorhaben im Auftrag des Ministeriums für Arbeit, Integration und Soziales des Landes Nordrhein-Westfalen, RWI Projektberichte, Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI), Essen. <http://hdl.handle.net/10419/72618> [last access: 27 Nov., 2022].
- Bargain, Olivier and Kristian Orsini (2006). In-work policies in Europe: Killing two birds with one stone?, *Labour Economics* 13(6), 667-697.
- Bargain, Olivier, Marco Caliendo, Peter Haan, and Kristian Orsini (2010). "Making Work Pay" in a rationed labour market, *Journal of Population Economics* 23, 323-351.
- Bastian, Jacob (2020). The rise of working mothers and the 1975 Earned Income Tax Credit, *American Economic Journal: Economic Policy* 12(3), 44-75.
- Bastian, Jacob and Lance Lochner (2022). The Earned Income Tax Credit and Maternal Time Use. More Time Working and Less Time with Kids? *Journal of Labor Economics* 40(3), 573-612.
- Beckmann, Fabian (2020). Perceived Job Quality in German Minijobs: A Multidimensional Analysis of Work in Marginal Part-Time Employment Relationships, *Management-Revue* 31(2), 116-144.
- Bennmarker, Helge, Erik Mellander, and Björn Öckert (2009). Do regional payroll tax reductions boost employment? *Labour Economics* 16, 480-489.
- Berthold, Norbert and Mustafa Coban (2013). Mini- und Midijobs in Deutschland: Lohnsubventionierung ohne Beschäftigungseffekte? *ORDO: Jahrbuch für die Ordnung von Wirtschaft und Gesellschaft* 64, 289-323.
- Berthold, Norbert and Mustafa Coban (2014). Kombilöhne gegen Erwerbsarmut: Warum die USA erfolgreicher sind als Deutschland, *Wirtschaftsdienst* 2/2014, 118-124.

- Blömer, Maximilian, Przemyslaw Brandt, and Andreas Peichl (2021). *Raus aus der Zweitverdienerinnenfalle Reformvorschläge zum Abbau von Fehlanreizen im deutschen Steuer- und Sozialversicherungssystem*, Bertelsmann Stiftung, Gütersloh.
- Blundell, Richard (2000). The Labour Market Impact of the Working Families' Tax Credit, *Fiscal Studies* 21(1), 75-103.
- BMAS (Bundesministerium für Arbeit und Soziales) (2018). *Übersicht über das Sozialrecht*, BW Bildung und Wissen, Nürnberg.
- Brandt, Torsten (2005). Mini- und Midijobs im Kontext aktivierender Arbeitsmarkt- und Sozialpolitik: Konsequenzen für Arbeitnehmerrechte, den Arbeitsmarkt und den Zusammenhang von Wohlfahrt und Beschäftigung, *WSI Diskussionspapier* No. 142 Düsseldorf: Wirtschafts- und Sozialwissenschaftliches Institut in der Hans-Böckler-Stiftung.
- Brandt, Torsten (2006). Bilanz der Minijobs und Reformperspektiven, *WSI-Mitteilungen* 8, 446-452.
- Browning, Edgar K. (1995). Effects of the Earned Income Tax Credit on Income and Welfare, *National Tax Journal* 48(1), 23-43.
- Bruckmeier, Kerstin, Torsten Lietzmann, Jannek Mühlhan, Jens Stegmaier (2018), Geringfügige Beschäftigung aus der Perspektive von Beschäftigten und Betrieben sowie Verteilungs- und Arbeitsmarktwirkungen einer Ausweitung, *IAB-Stellungnahme* 16/2018, Nürnberg.
- Caliendo, Marco, Steffen Künn, and Arne Uhlenhorff (2016). Earnings exemptions for unemployed workers: The relationship between marginal employment, unemployment duration and job quality, *Labour Economics* 42, 177-193.
- Carrillo-Tudela, Carlos, Andrey Launov, and Jean-Marc Robin (2021), The fall in German unemployment: A flow analysis, *European Economic Review* 132, 103658.
- Chetty, Raj, John N. Friedman, Tore Olsen, and Luigi Pistaferri (2011). Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records, *Quarterly Journal of Economics* 126(2), 749-804.
- Chetty, Raj, John N. Friedman, and Emmanuel Saez (2013). Using Differences in Knowledge across Neighborhoods to Uncover the Impacts of the EITC on Earnings, *American Economic Review* 103(7), 2683-2721.
- Chetty, Raj and Emmanuel Saez (2013). Teaching the Tax Code: Earnings Responses to an Experiment with EITC Recipients, *American Economic Journal: Applied Economics* 5(1), 1-31.
- Collischon, Matthias, Kamila Cygan-Rehm, and Regina T. Riphahn (2021). Employment Effects of Payroll Tax Subsidies, *Small Business Economics* 57(3), 1201-1219.
- Dahl, Molly, Thomas DeLeire, and Jonathan Schwabisch (2009). Stepping Stone or Dead End? The Effect of the EITC on Earnings Growth, *National Tax Journal* 62(2), 329-346.
- Di Porto, Edoardo, Pietro Garibaldi, Giovanni Mastrobuoni, and Paolo Naticcioni (2022). The Perverse Effect of Flexible Work Arrangements on Informality, *IZA Discussion Paper* No. 15794, Institute of Labor Economics, Bonn.
- Dolado, Juan J., Etienne Lalé, and Hélène Turon (2021). Zero-Hours Contracts in a Frictional Labor Market, *IZA Discussion Paper* No. 14979, Institute of Labor Economics, Bonn.
- Dundler, Agnes, Thomas Frank, and Christopher Grimm (2019). Grundlagen: Methodenbericht - Beschäftigte mit geringen Entgelten, Bundesagentur für Arbeit, Nürnberg.
- Eichhorst, Werner, Tina Hinz, Paul Marx, Andreas Peichl, Nico Pestel, Sebastian Siegloch, Erich Thode, and Verena Tobsch (2012). Geringfügige Beschäftigung: Situation und Gestaltungsoptionen. *IZA Research Report* No. 47, Institute of Labor Economics, Bonn.
- Eissa, Nada and Jeffery Liebman (1996). Labor supply responses to the Earned Income Tax Credit, *Quarterly Journal of Economics* 111(2), 605-637.

- Eissa, Nada, and Hilary Williamson Hoynes (2004). Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit, *Journal of Public Economics* 88(9-10), 1931-1958.
- Eissa, Nada and Hilary Hoynes (2006). The Hours of Work Response of Married Couples: Taxes and the Earned Income Tax Credit. In: Agell, Jonas and Peter Birch Sørensen (eds.) *Tax Policy and Labor Market Performance*, MIT Press, Chapter 5.
- Fertig, Michael, Jochen Kluve and Markus Scheuer (2005). *Was hat die Reform der Minijobs bewirkt? Erfahrungen nach einem Jahr*. RWI Schriften 77, Duncker & Humblot, Berlin.
- Fertig, Michael and Jochen Kluve (2006). Alternative Beschäftigungsformen in Deutschland: Effekte der Neuregelung von Zeitarbeit, Minijobs und Midijobs, *Vierteljahreshefte zur Wirtschaftsforschung* 75, 97-117.
- Fichtl, Anita (2015). Mini- and Midi-Jobs in Germany – Effects of Marginal Employment on Unemployment, *FORES Policy Paper* 2015:3, Stockholm.
- Francesconi, Marco and Wilbert van der Klaauw (2007). The socioeconomic consequences of "in-work" benefit reform for British lone mothers, *Journal of Human Resources* 42(1), 1-31.
- Freier, Ronny and Viktor Steiner (2008). 'Marginal employment': stepping stone or dead end? Evaluating the German experience, *Zeitschrift für Arbeitsmarktforschung* 41(2/3), 223-243.
- Goebel, Jan, Markus M. Grabka, Stefan Liebig, Martin Kroh, David Richter, and Carsten Schroeder (2019). The German Socio-economic Panel (SOEP), *Jahrbücher für Nationalökonomie und Statistik* 239(2), 345-360.
- Grogger, Jeffrey (2003). The effects of time limits, the EITC, and other policy changes on welfare use, work, and income among female-headed families, *Review of Economics and Statistics* 85(2), 394-408.
- Gruber, Jonathan (1997). The Incidence of Payroll Taxation: Evidence from Chile, *Journal of Labor Economics* 15(3, pt.2), S72-S101.
- Gudgeon, Matthew and Simon Trenkle (2023). The Speed of Earnings Responses to Taxation and the Role of Firm Labor Demand, forthcoming: *Journal of Labor Economics*.
- Haywood, Luke and Michael Neuman (2021). Equilibrium Effects of Tax Exemptions for Low Pay, *Labour Economics* 71, 101976.
- Hausman, Catherine and David S. Rapson (2018). Regression Discontinuity in Time: Considerations for Empirical Applications, *Annual Review of Resource Economics* 10, 533-552.
- Herget, Anna and Regina T. Riphahn (2022). The untold story of Midijobs, *Journal of Economics and Statistics (Jahrbücher für Nationalökonomie und Statistik)* 242(3), 309-341.
- Herzog-Stein, Alexander and Werner Sesselmeier (2012). Alternativen zu Mini- und Midijobs? Die Beispiele Frankreich und Vereinigtes Königreich, *WSI Mitteilungen* 2012-01, 41-49.
- Hoynes, Hilary (2007). The EITC Disincentive: A Reply to Paul Trampe, *Econ Journal Watch* 4(3), 321-325.
- Hoynes, Hilary and Ankur Patel (2018). Effective Policy for reducing poverty and inequality? The Earned Income Tax Credit and the distribution of income, *Journal of Human Resources* 53(4), 859-890.
- Huttunen, Kristina, Jukka Pirttilä, and Roope Uusitalo (2013). The employment effects of low-wage subsidies, *Journal of Public Economics* 97, 49-60.
- Immervoll, Herwig, Henrik Jacob Kleven, Claus Thustrup Kreiner, and Emmanuel Saez (2007). Welfare reform in European Countries: A Microsimulation Analysis, *Economic Journal* 117, 1-44.

- Jara, H. Xavier, Katrin Gasior, and Mattia Makovec (2020). Work Incentives at the Extensive and Intensive Margin in Europe: The Role of Taxes, Benefits and Population Characteristics, *Social Indicators Research* 152, 705-778.
- Keller, Berndt and Hartmut Seifert (2015). Atypical forms of employment in the public sector - are there any? *WSI-Diskussionspapier* No. 199, Düsseldorf: Wirtschafts- und Sozialwissenschaftliches Institut in der Hans-Böckler-Stiftung.
- Keller, Berndt, Toralf Pusch, and Hartmut Seifert (2021). Midijobs: Die unbekannte Variante der atypischen Beschäftigung, *WSI Mitteilungen* 14(2), 160-170.
- Kleven, Henrik, (2016). Bunching, *Annual Review of Economics* 8, 435-464.
- Korkeamäki, Ossi and Roope Uusitalo (2009). Employment and wage effects of a payroll-tax cut - evidence from a regional experiment, *International Tax and Public Finance* 16, 753-772.
- LaLumia, Sara (2009). The Earned Income Tax Credit and Reported Self-Employment Income, *National Tax Journal* 62(2), 191-217.
- Leigh, Andrew (2007). Earned Income Tax Credits and Labor Supply: New Evidence from a British Natural Experiment, *National Tax Journal* 60(2), 205-224.
- Lietzmann, Torsten, Paul Schmelzer, Jürgen Wiemers (2017). Marginal employment for welfare recipients: stepping stone or obstacle? *Labour* 31(4), 394-414.
- Meyer, B. D. (2002). Labor supply at the extensive and intensive margins: The EITC, welfare, and hours worked. *American Economic Review* 92(2), 373-379.
- Neumark, David and Peter Shirley (2020), The long-run effects of the Earned Income Tax Credit on Women's Earnings, NBER Working Paper No. 24114, Cambridge, Mass.
- Oschmiansky, Frank and Julia Berthold (2020), Minijobs und Midijobs, available at <https://www.bpb.de/themen/arbeit/arbeitsmarktpolitik/317249/minijobs-und-midijobs/> (last access: August 9, 2022).
- Saez, Emmanuel, Benjamin Schoefer, and David Seim (2019). Payroll taxes, firm behavior, and rent sharing: Evidence from a young workers' tax cut in Sweden, *American Economic Review* 109(5), 1717-63.
- Scarfe, Rachel (2021). Flexibility or certainty? The aggregate effects of casual jobs on labour markets, *Edinburgh School of Economics Discussion Paper Series* No. 294, University of Edinburgh.
- Seifert, Hartmut (2017). Wie lassen sich Entwicklung und Strukturen atypischer Beschäftigungsverhältnisse erklären? *WSI-Mitteilungen* 2017-01, 5-15.
- Tazhitdinova, Alisa (2020). Do Only Tax Incentives Matter? Labor Supply and Demand Responses to an Unusually Large and Salient Tax Break, *Journal of Public Economics* 184 (April), 104162.
- Tazhitdinova, Alisa (2022). Increasing Hours Worked: Moonlighting Responses to a Large Tax Reform, *American Economic Journal: Economic Policy* 14(1), 473-500.
- Trampe, Paul (2007). The EITC Disincentive: The Effects on Hours Worked from the Phase-out of the Earned Income Tax Credit, *Econ Journal Watch* 4(3), 308-320.
- Trampe, Paul (2008). The EITC Disincentive: A Reply to Dr. Hilary Hoynes, *Econ Journal Watch* 5(1), 59-65.

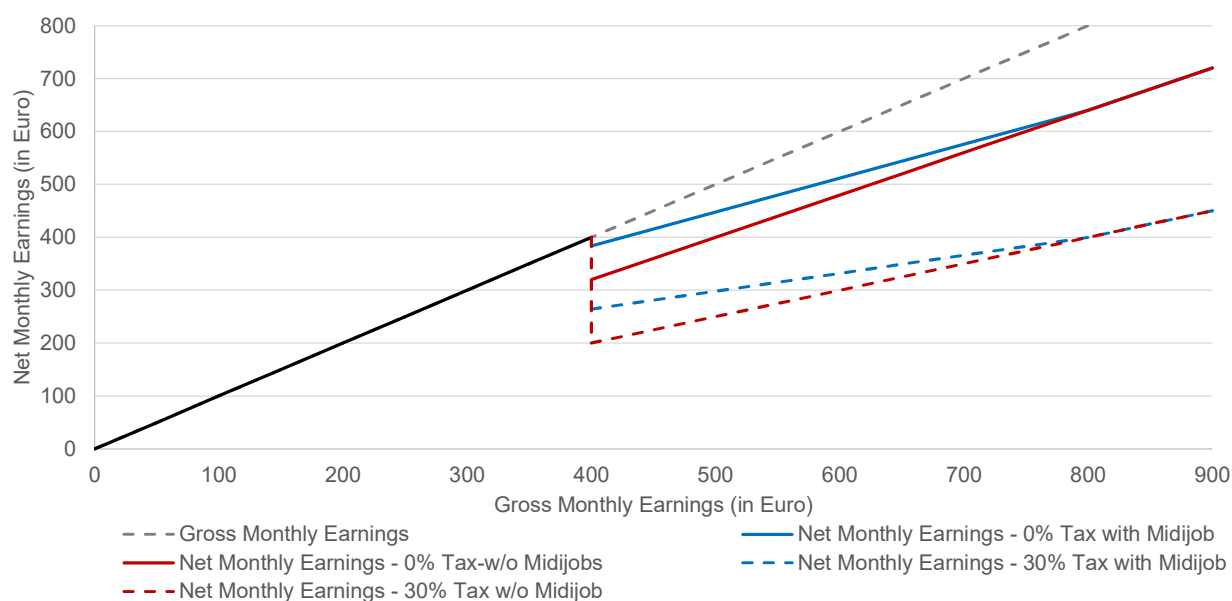
Figure 1 Distribution of gross monthly earnings (April 2003 – December 2012)



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

Source: SIAB, own calculations.

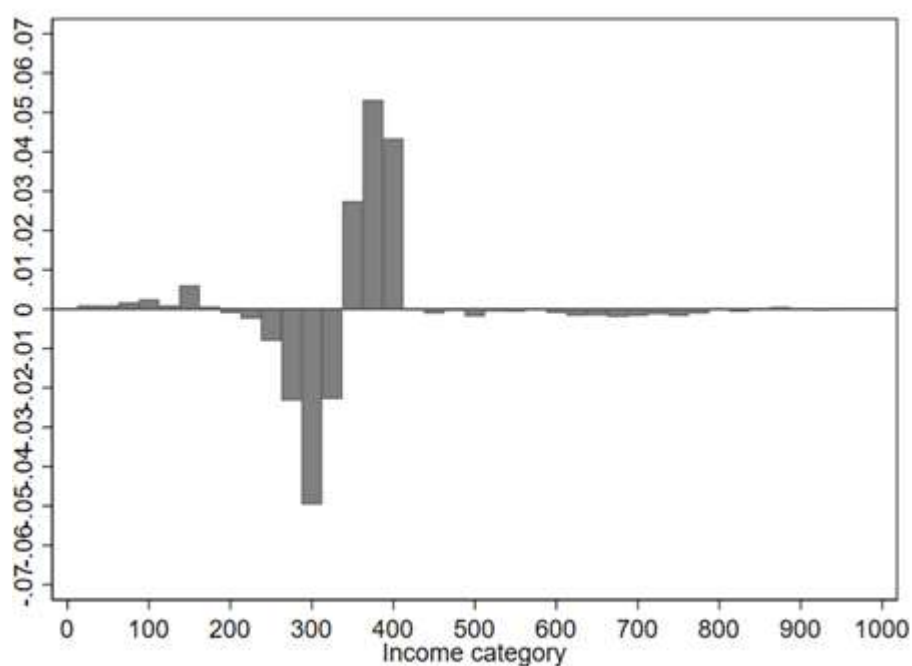
Figure 2 Net earnings with and without Midijob subsidy by income tax burden (2003)



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 zero income taxes.

Source: Own illustration.

Figure 3 Change in the distribution of gross monthly earnings 2002-2004



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

Source: SIAB (2017), own calculations.

Figure 4 Monthly transition rate from Minijob Employment (April 2002-March 2004)
Figure 4.1 Sample A (all Minijob observations in the observation window)

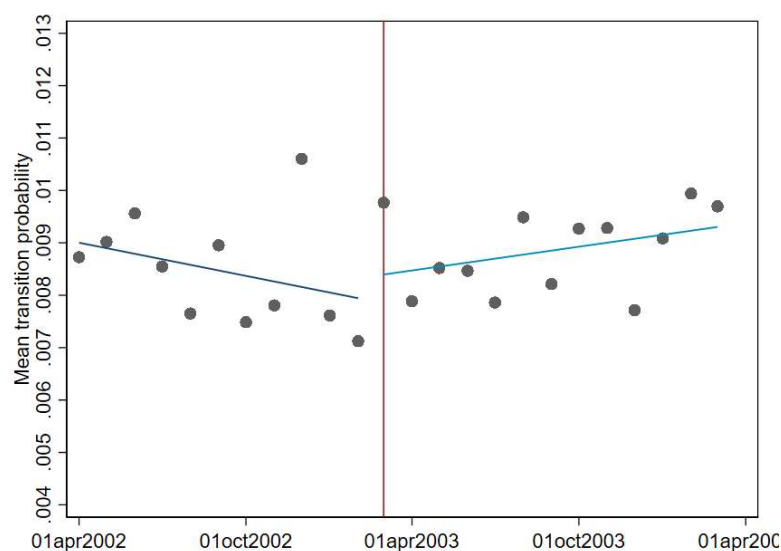


Figure 4.2 Sample B (Sample A without Minijobs started after April 1, 2003)

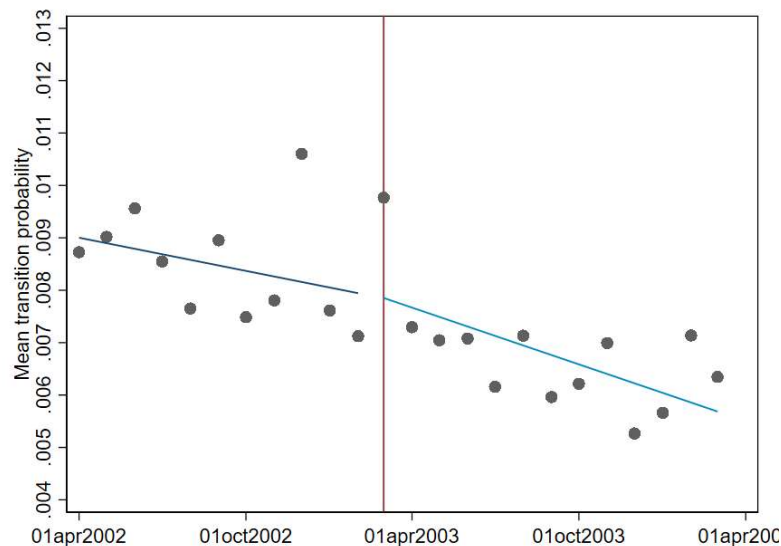
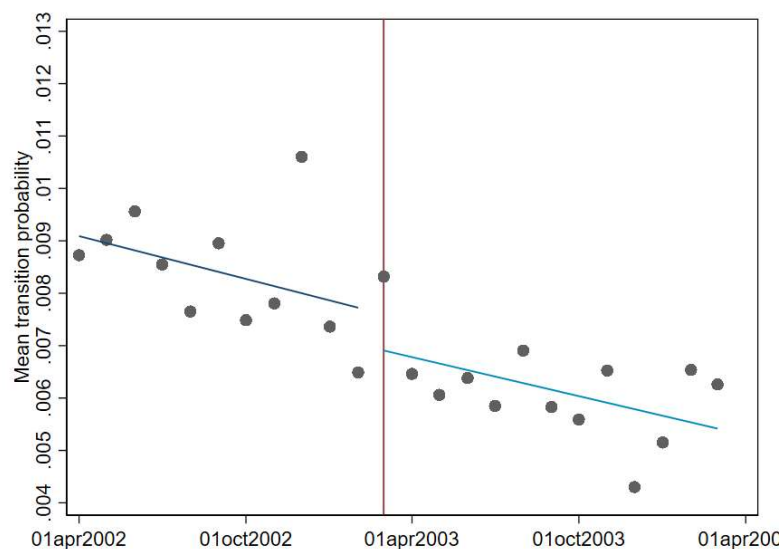


Figure 4.3 Sample C (Sample B without Minijobs started after Dec. 31, 2002)



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.6**.
Source: SIAB (2017) and own calculations.

Figure 5 Monthly transition rate from Minijob Employment (April 2002-March 2004)
Figure 5.1 By gender (males left panel, females right panel)

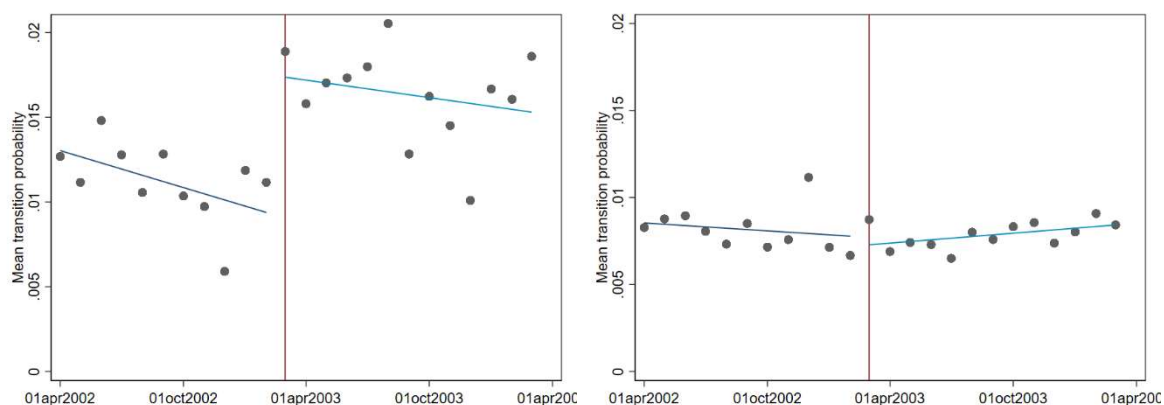
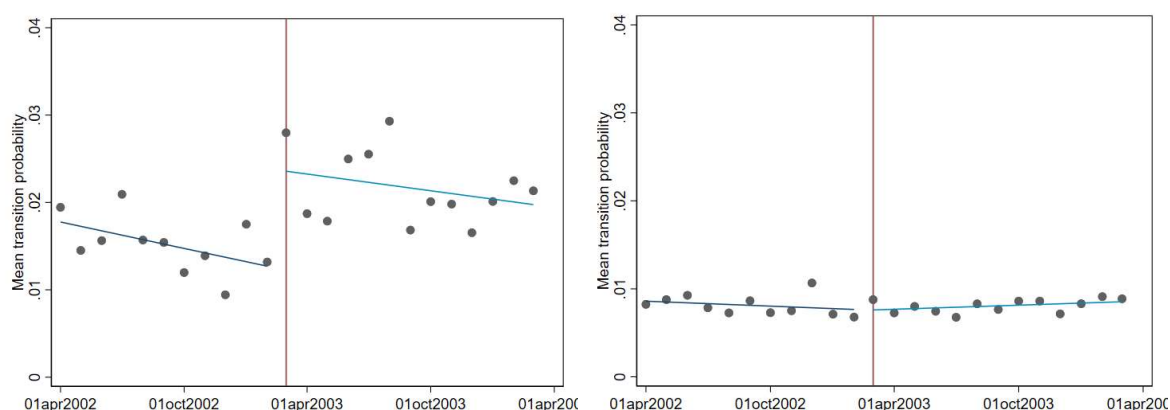


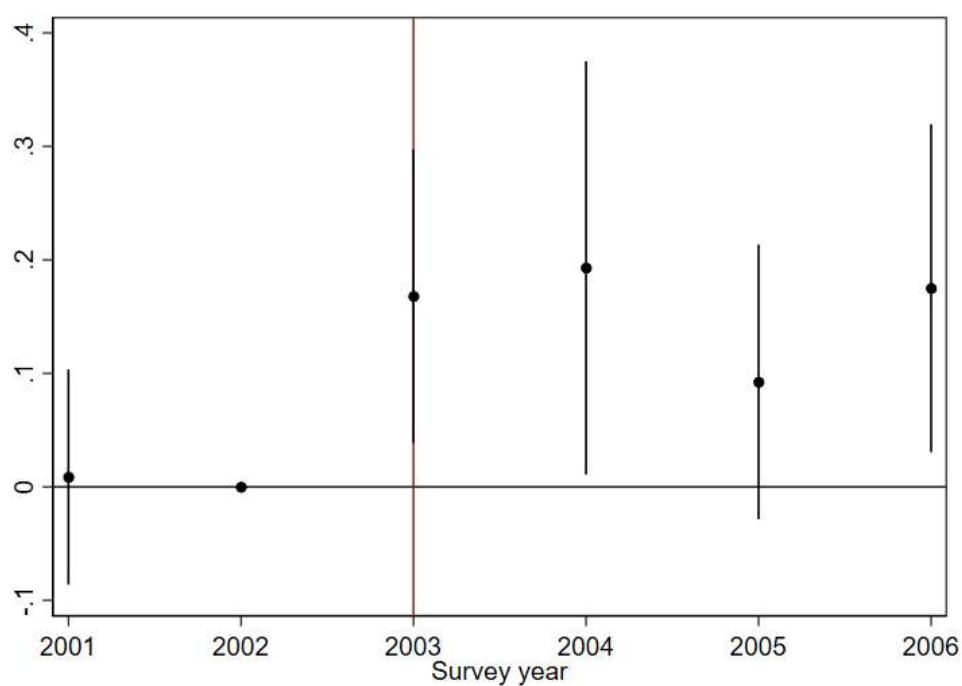
Figure 5.2 By predicted single earner status (single earners left panel, secondary earners right panel)



Note: The graphs represent the development of monthly average transition rates from Minijob to regular employment. The sample includes all who hold a Minijob as their main employment without being registered unemployed in a given month. The monthly transition rates are de-seasonalized. Both figures use subsamples from Sample A. Please note that the y-axis of Panel 5.2 is scaled different from Panel 5.1

Source: SIAB (2017) and own calculations.

Figure 6 Event study analysis of parallel trends (SOEP)



Note: The figure depicts estimated coefficients from linear regressions and their confidence intervals. The outcome of transitions from Minijobs to regular employment is regressed on year indicators and their interaction with the treatment indicator. We depict 90 percent confidence intervals of the interaction term coefficients using standard errors that are clustered at the individual level.

Source: SOEP wave 35 and own calculations.

Table 1 Descriptive Statistics - Basic Controls (SIAB Sample)

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

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

Source: SIAB (2017) and own calculations.

Table 2 RDiT Estimates of Transitions out of Minijobs - SIAB

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

Note: Linear regressions, standard errors clustered at the individual level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The columns entitled "Coeff" provide the estimate of 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 relative effect, i.e., 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 RDiT Estimates - SIAB - by gender and predicted secondary earner status

Specification	Sample A - 2003				Sample A - 2003			
	(1) Men		(2) Women		(3) Pred. Non-Married		(4) Pred. Married	
	Coeff	RE	Coeff	RE	Coeff	RE	Coeff	RE
1 Linear no controls	0.0083 ***	73.6%	-0.0004	-5.2%	0.0152 ***	85.9%	-0.0001	-0.9%
2 Linear basic controls	0.0078 ***	69.0%	-0.0005	-6.1%	0.0149 ***	84.2%	-0.0002	-2.2%
3 Linear ext. controls	0.0061 ***	54.0%	-0.0007	-8.5%	0.0129 ***	72.9%	-0.0005	-5.5%
4 Quadratic no controls	0.0063 ***	55.7%	0.0003	3.3%	0.0135 ***	76.3%	0.0005	5.4%
5 Quadr. basic controls	0.0092 ***	81.4%	0.0002	2.4%	0.0136 ***	76.8%	0.0005	5.0%
6 Quadr. ext. controls	0.0071 ***	62.8%	0.0001	1.2%	0.0118 ***	66.7%	0.0002	2.1%
N	95,277		757,964		54,183		890,793	
Pre-reform mean Y	0.0113		0.0082		0.0177		0.0092	

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

Source: SIAB (2017) and own calculations.

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

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

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

Source: SOEP wave 35 and own calculations.

Table 5 DID Estimates - 2003 reform baseline results (SOEP)

	Full Sample (1)	Full Sample (2)	Full Sample (3)	Full Sample (4)
post	0.004 (0.015)	0.004 (0.015)	-0.000 (0.015)	-
treat	0.002 (0.029)	-0.022 (0.031)	-0.038 (0.032)	-0.0041 (0.033)
post * treat	0.139*** (0.052)	0.123** (0.051)	0.131*** (0.050)	0.134*** (0.050)
Controls - basic	no	yes	yes	yes
Controls - extended	no	no	yes	yes
Year fixed effects	no	no	no	yes

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

Source: SOEP wave 35 and own calculations.

Table 6 DID Estimates - 2003 reform results for two treatment groups (SIAB)

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

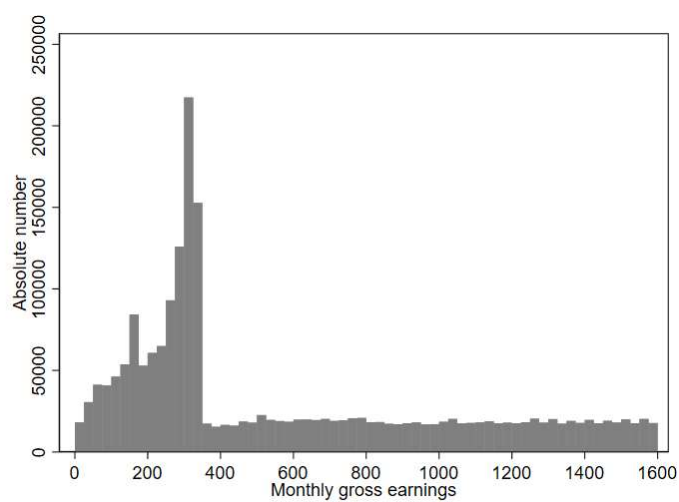
Note: All estimations use 2,831,554 person-month observations. The estimations are based on linear regressions. Standard errors clustered at the individual level are in parentheses. In columns 1-3 the treatment indicator reflects male gender and in columns 4-6 the treatment indicator reflects a predicted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

Source: SIAB (2017) and own calculations.

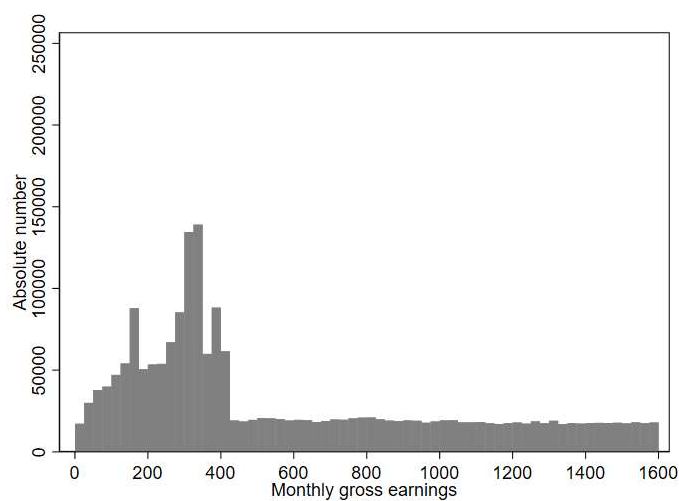
Online Appendix

Figure A.1 Distribution of gross monthly earnings (selected years)

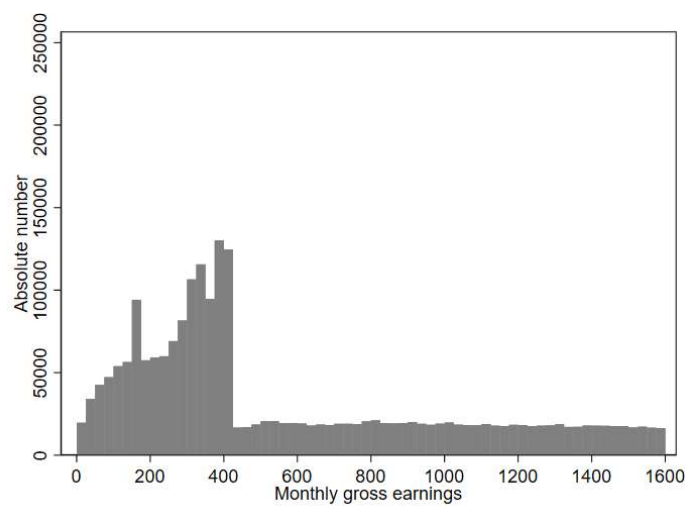
2001



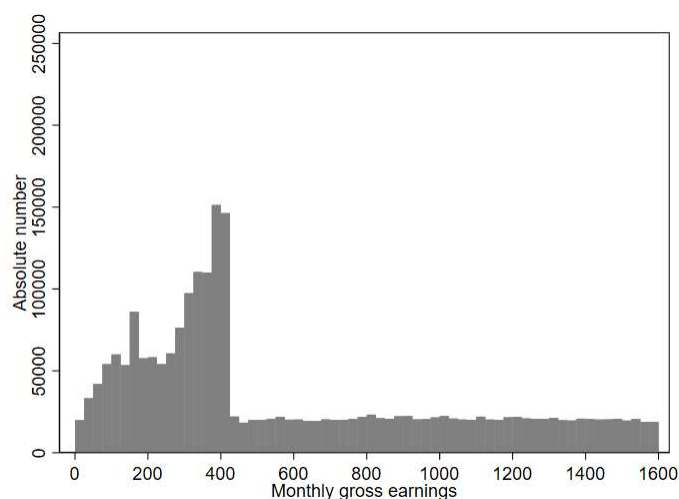
2003



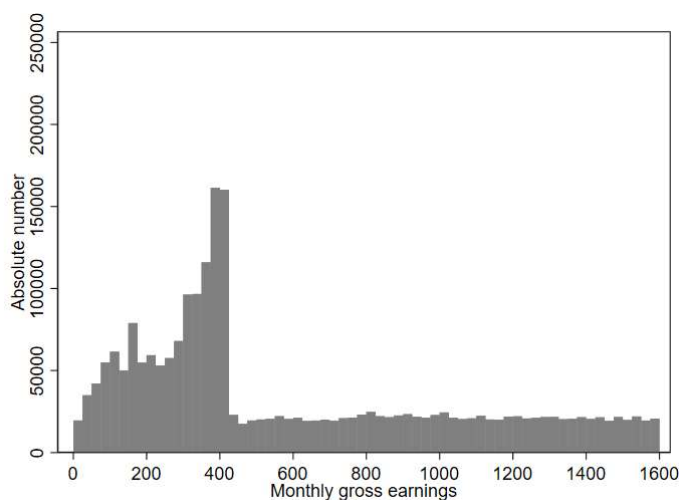
2005



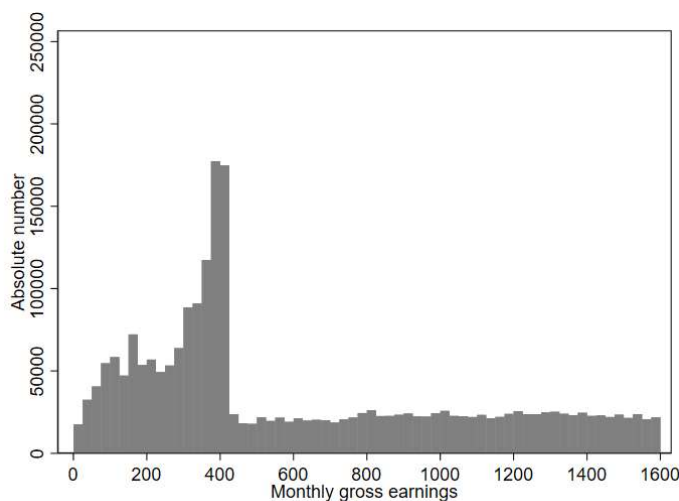
2007



2009



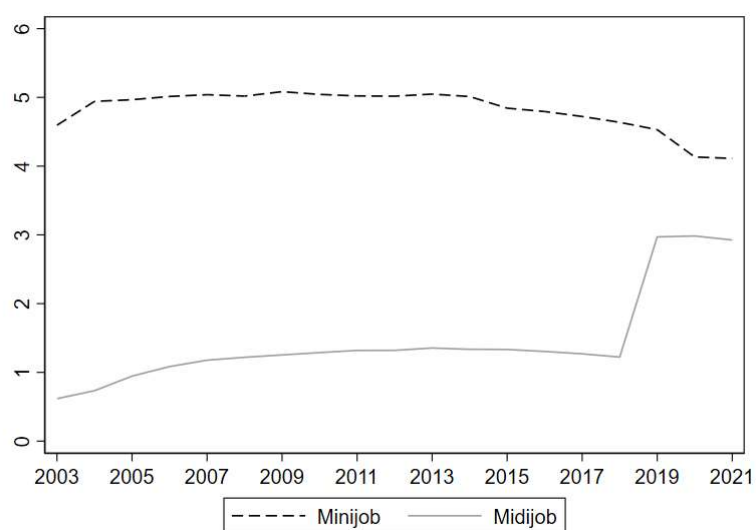
2011



Note: The graph represents the frequency of monthly earnings in the bottom part of the gross earnings distribution for all employed individuals registered with the unemployment insurance. It depicts all monthly earnings observed in different calendar years using 50 Euro bins of monthly gross earnings.

Source: SIAB, own calculations.

Figure A.2 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.3 Minijob / Midijob vs. EITC subsidies

Figure A.3.1 Minijob / Midijob subsidies before April 1, 2003 by income tax rates (in Euro)

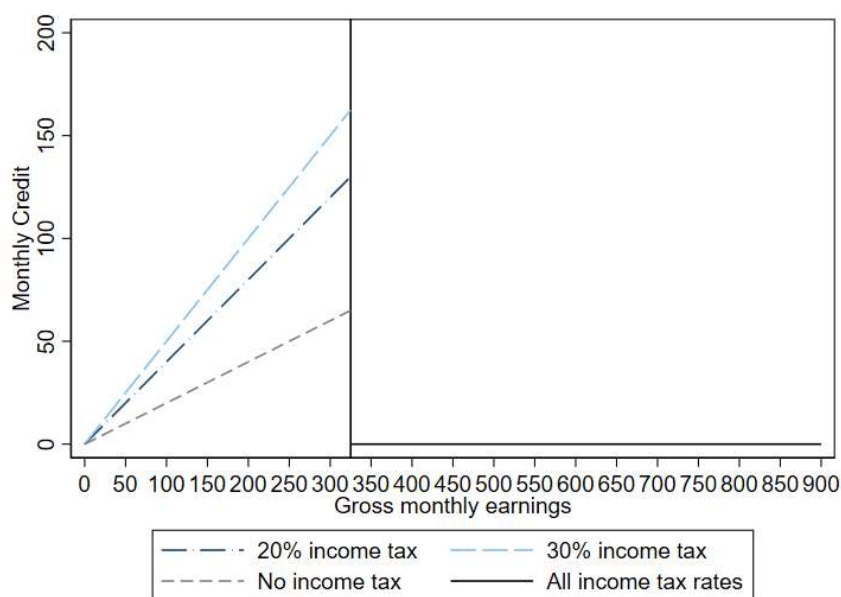


Figure A.3.2 Minijob / Midijob subsidies after April 1, 2003 by income tax rates (in Euro)

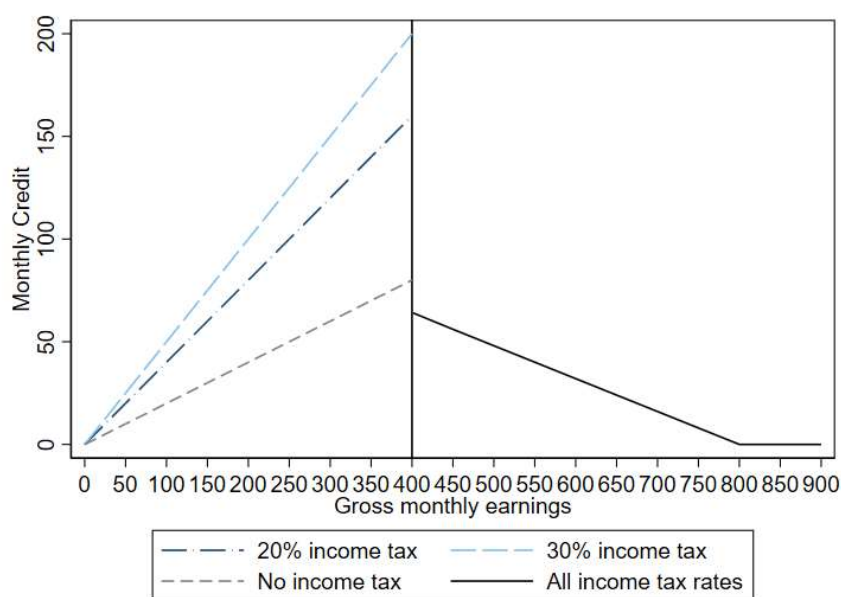
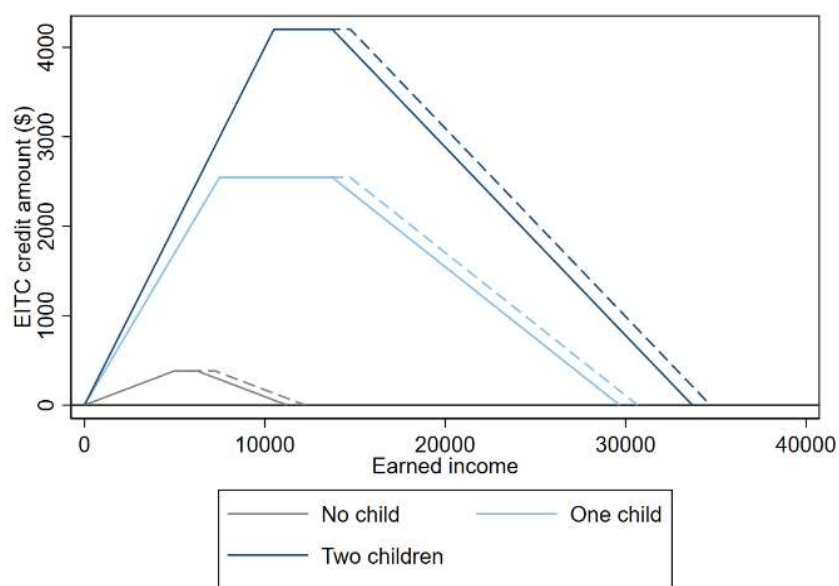


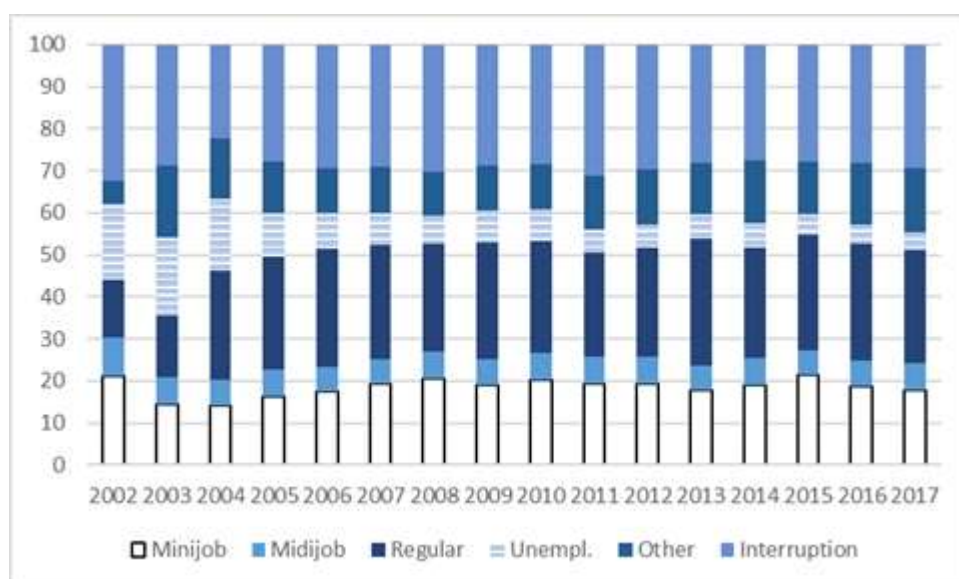
Figure A.3.3 EITC subsidies (in USD)



Note: Figure A.3.3 depicts the EITC subsidy as of 2003. The solid lines represent annual credits for single individuals and the dashed lines indicate annual credits for married couples.

Source: Figures A.3.1 and 3.2 own illustrations, Figure A.3.3 based on information available for 2003 in https://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/pdf/historical_eitc_parameters.pdf (last access March 15, 2024)

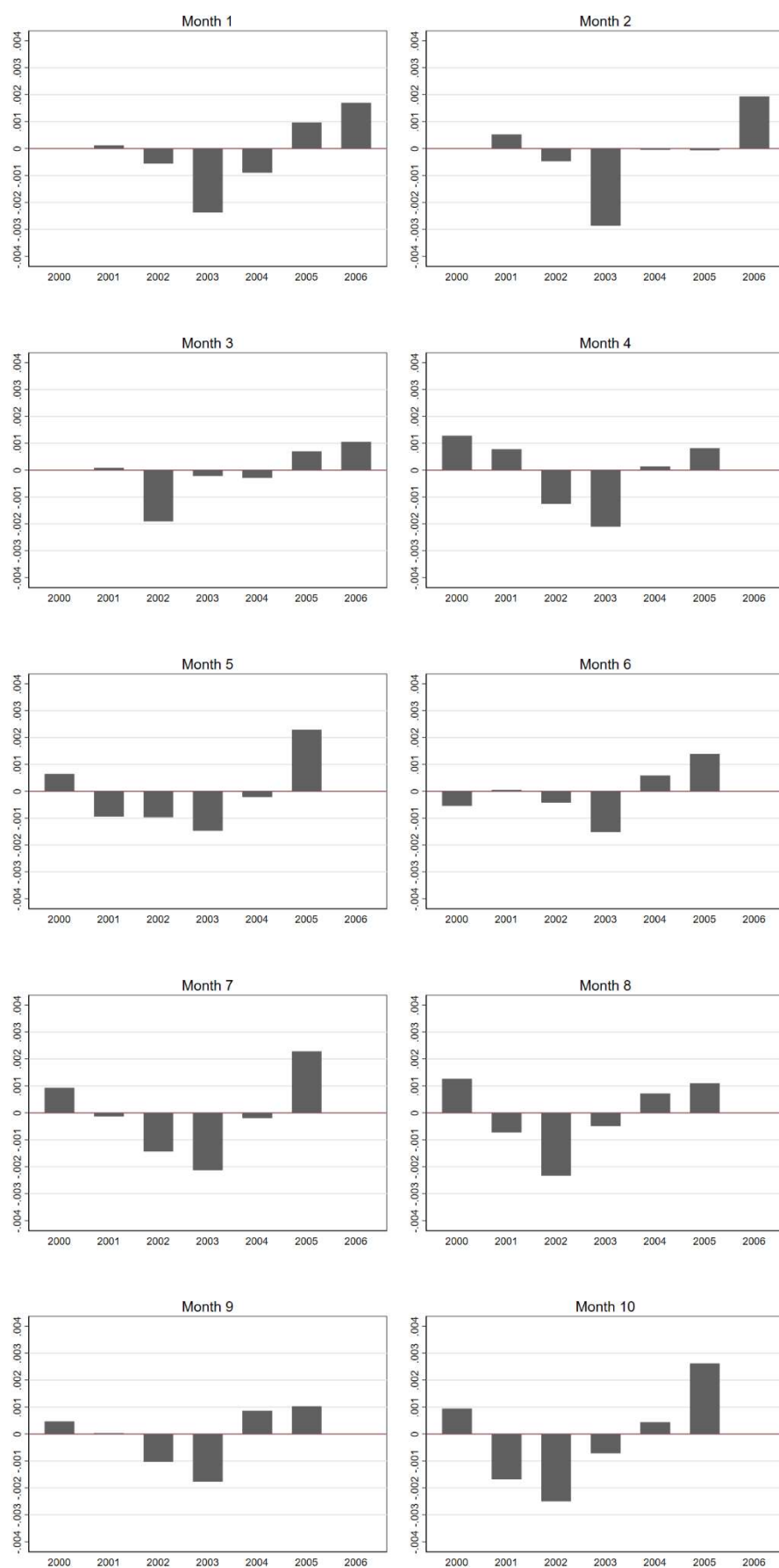
Figure A.4 Labor force transitions into new Midijobs over time: States of origin

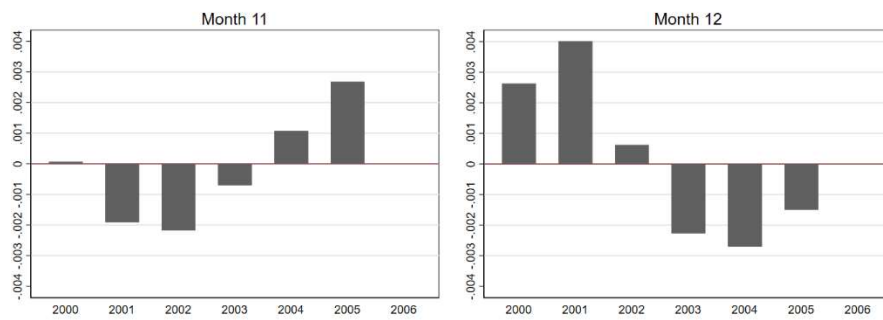


Note: In 2002 and 2003 before the reform Midijobs are coded as employments earning between 400 and 800 Euro per month. "Regular" represents employments earnings more than the Midijob earnings range. "Other" positions are, e.g., sick spells, motherhood related out of the labor force states, sabbaticals, student traineeships, or internships. The "Interruption" state is coded if the time since the last recorded labor market state exceeds 31 days; this can include out-of-the-labor force states or self-employment. Individuals entering at the extensive margin from out of the labor force states would be observed in the "interruptions" category.

Source: Own calculations based on SIAB, 1975–2017.

Figure A.5 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.6 Monthly transition rate from Minijob employment by gender
(male left panel, female right panel)

Figure A.6.1 Sample A (all Minijob observations in the observation window)

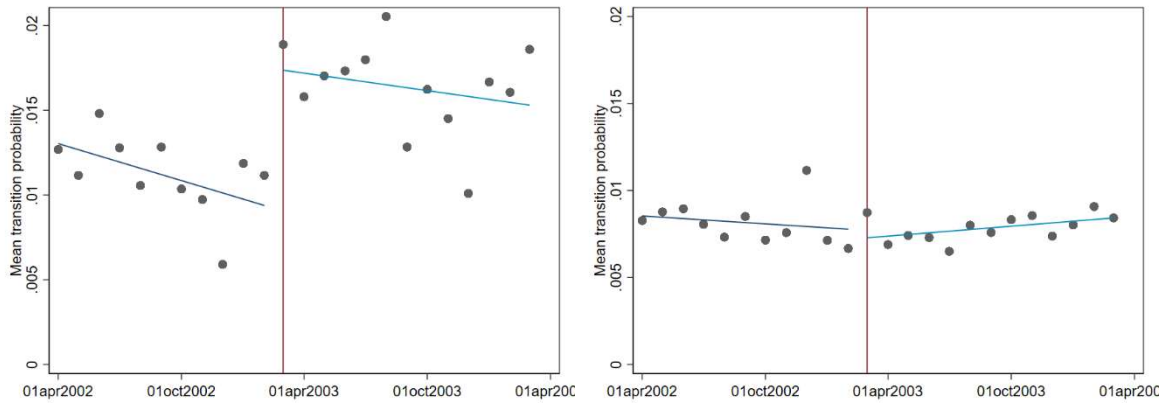


Figure A.6.2 Sample B (Sample A without Minijobs started after April 1, 2003)

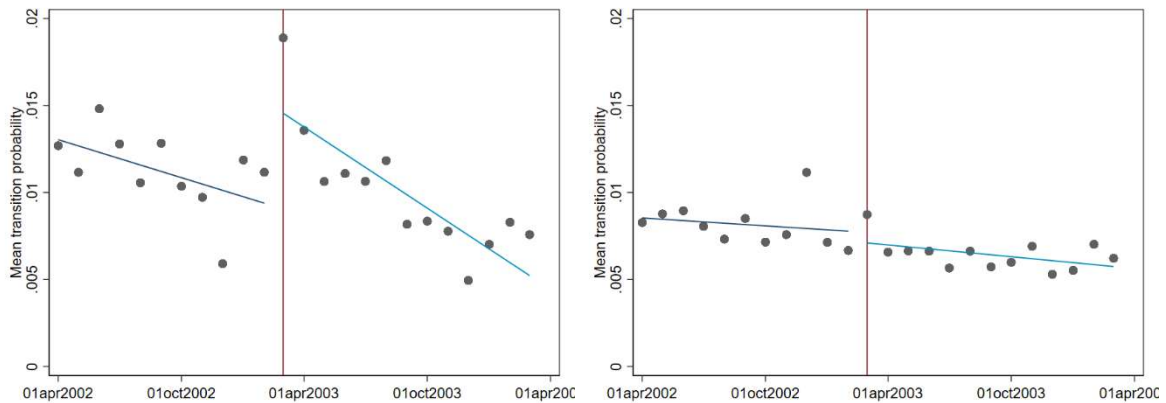
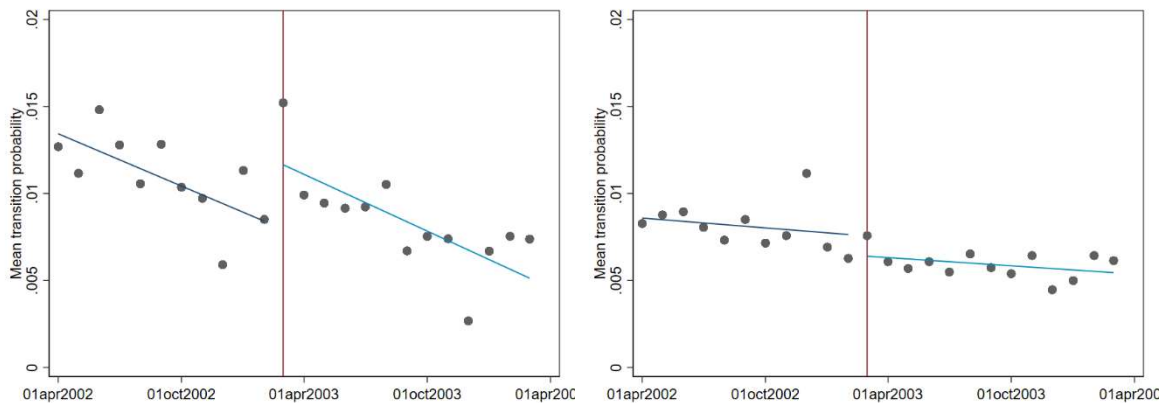


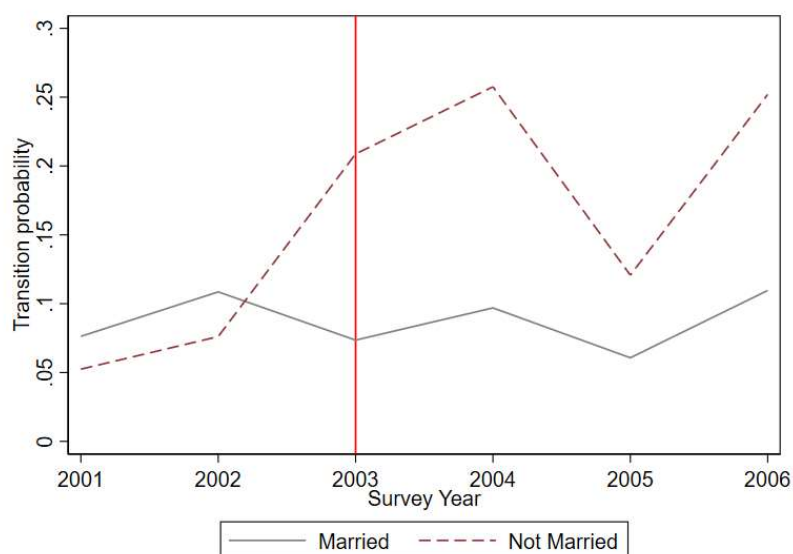
Figure A.6.3 Sample C (Sample B without Minijobs started after Dec. 31, 2002)



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.7 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 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)

Variable	Full Sample		Full Sample Mean Y when valued 1	Means by Gender	
	Mean	Std. Dev.		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 RDiT Full estimation results - linear specifications Sample A **Table 2** (SIAB)

	Linear no controls		Linear basic controls		Linear extend. controls	
	(1)		(2)		(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)

Variable	Full Sample		Mean Transition Rate when variable has	
	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 RDiT estimation robustness to a changed definition of pre-reform transitions

Specification	Sample A - 2003 Outcome Transitions beyond 400 Euro					
	(1) All		(2) Women		(3) Men	
	Coeff	RE	Coeff	RE	Coeff	RE
1 Linear no controls	0.0003	4.3%	-0.0005	-7.5%	0.0063 ***	65.7%
2 Linear basic controls	0.0002	2.9%	-0.0005	-7.5%	0.0059 ***	61.5%
3 Linear ext. controls	-0.0001	-1.4%	-0.0007 **	-10.4%	0.0044 ***	45.9%
4 Quadratic no controls	0.0006	8.6%	-0.0002	-3.0%	0.0070 ***	73.0%
5 Quadr. basic controls	0.0005	7.1%	-0.0002	-3.0%	0.0069 ***	71.9%
6 Quadr. ext. controls	0.0003	4.3%	-0.0003	-4.5%	0.0049 **	51.1%
N	853,241		756,964		95,277	
Pre-reform mean Y	0.0070		0.0067		0.0096	

Note: See **Table 2**; the dependent variable in the pre- and the post-reform period was coded 1 if a transition occurred to regular employment earning more than 400 Euro. In the main analyses (i.e., **Tables 2** and **3**), the dependent variable in the pre-reform period was coded 1 for any transition out of Minijob employment including to regular jobs earning 325-400 Euro.

Source: SIAB (2017) and own calculations.

Table A.7 RDiT Analysis: 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.8 RDit Estimates of Transition out of Minijob – SIAB – by gender Sample A 2013

Specification	Sample A - 2013			
	(1) Men		(2) Women	
	Coeff	RE	Coeff	RE
1 Linear no controls	0.0018	7.6%	-0.0009	-5.9%
2 Linear basic controls	0.0014	5.8%	-0.0008	-5.5%
3 Linear ext. controls	0.0011	4.6%	-0.0010 *	-6.8%
4 Quadratic no controls	-0.0007	-3.0%	-0.0015 *	-10.6%
5 Quadr. basic controls	-0.0009	-3.8%	-0.0015 *	-10.3%
6 Quadr. ext. controls	-0.0009	-3.8%	-0.0016 *	-11.0%
N	222,556		840,336	
Pre-reform mean Y	0.0240		0.0146	

Note: see **Table 2**.

Source: SIAB (2017) and own calculations.

Table A.9 RDit Estimates of Transition out of Minijob – SIAB – By Transition outcome

Specification	Sample A - 2003 - Men				Sample A - 2003 - Pred. Non-Married			
	(1) In Midijob		(2) Beyond Midjob		(3) In Midijob		(2) Beyond Midjob	
	Coeff	RE	Coeff	RE	Coeff	RE	Coeff	RE
1 Linear no controls	0.0018 *	54.5%	0.0054 ***	65.9%	0.0026	61.9%	0.0071 **	74.0%
2 Linear basic controls	0.0017 *	51.5%	0.0051 ***	62.2%	0.0026	61.9%	0.0069 **	71.9%
3 Linear ext. controls	0.0014	42.4%	0.0038 **	46.3%	0.0022	52.4%	0.0055 **	57.3%
4 Quadratic no controls	0.0024	72.7%	0.0054 **	65.9%	0.0015	35.7%	0.0073 *	76.0%
5 Quadr. basic controls	0.0024	72.7%	0.0053 **	64.6%	0.0015	35.7%	0.0073 *	76.0%
6 Quadr. ext. controls	0.0020	60.6%	0.0038 *	46.3%	0.0012	28.6%	0.0060	62.5%
N	39,127		95,277		17,986		46,180	
Pre-reform mean Y	0.0033		0.0082		0.0042		0.0096	

Note: See **Table 3**.

Source: SIAB (2017) and own calculations.

Table A.10 DID Analysis: 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.11 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.12 DID Estimates - 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 (0.020)	0.013 (0.021)	0.006 (0.015)	0.003 (0.015)	0.005 (0.016)
treat	-0.065* (0.039)	-0.020 (0.031)	-0.019 (0.031)	0.006 (0.032)	-0.026 (0.029)
post*treat	0.157*** (0.056)	0.156** (0.078)	0.124** (0.048)	0.091* (0.054)	0.162*** (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 A.13 Descriptive Statistics - Basic Controls: 2013 Reform Sample (SOEP)

Variable	Descriptives		Mean transition rate when variable has	
	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.

Table A.14 DID Estimates - effects of the 2013 reform (SOEP)

	Full Sample (1)	Full Sample (2)	Full Sample (3)	Full Sample (4)
post	0.022 (0.017)	0.022 (0.017)	0.023 (0.017)	-
treat	0.068* (0.039)	0.067* (0.038)	0.065* (0.036)	0.062* (0.036)
post * treat	-0.026 (0.049)	-0.033 (0.048)	-0.025 (0.045)	-0.023 (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.

Table A.15 DID Estimates – SOEP – By Transition Outcome

	Transition from Mini- to Midijob				Transition from Mini- beyond Midijob			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	0.007 (0.010)	0.007 (0.011)	0.006 (0.010)	-	-0.003 (0.012)	-0.003 (0.012)	-0.006 (0.012)	-
treat	0.012 (0.022)	0.002 (0.022)	-0.011 (0.024)	-0.011 (0.024)	-0.009 (0.019)	-0.024 (0.021)	-0.026 (0.022)	-0.030 (0.022)
post * treat	0.035 (0.034)	0.025 (0.035)	0.031 (0.034)	0.029* (0.034)	0.104** (0.044)	0.098** (0.041)	0.100** (0.040)	0.105*** (0.040)
Controls - basic	no	yes	yes	yes	no	yes	yes	yes
Controls - extended	no	no	yes	yes	no	no	yes	yes
Year fixed effects	no	no	no	yes	no	no	no	yes

Note: See **Table 5**. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP wave 35 and own calculations.

Table A.16 DID Estimates – SIAB – By Transition Outcome

	Transition from Mini- to Midijob			Transition to beyond Midijob		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat = male						
post	0.0004*** (0.0001)	0.0004*** (0.0003)	0.0006*** (0.0001)	-0.0004*** (0.0001)	-0.0003** (0.0001)	-0.0003** (0.0001)
treat	0.0002 (0.0002)	0.0007** (0.0002)	0.0003 (0.0002)	0.0023 (0.0003)	0.0029*** (0.0003)	0.0024*** (0.0003)
post * treat	0.0005* (0.0003)	0.0002 (0.0003)	-0.0000 (0.0003)	0.0021*** (0.0003)	0.0016** (0.0003)	0.0015*** (0.0003)
Treat = predicted non-married						
post	0.0004*** (0.0001)	0.0005*** (0.0001)	0.0006*** (0.0001)	-0.0002** (0.0001)	-0.0002* (0.0001)	-0.0002 (0.0001)
treat	0.0013*** (0.0003)	0.0002 (0.0004)	0.0002 (0.0004)	0.0042*** (0.0005)	0.0006 (0.0005)	0.0006 (0.0005)
post * treat	-0.0002 (0.0004)	0.0002 (0.0004)	-0.0004 (0.0004)	0.0005 (0.0005)	0.0007 (0.0005)	0.0006 (0.0005)
Controls - basic	no	yes	yes	no	yes	yes
Controls - extended	no	no	yes	no	no	yes

Note: See Table 6. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SIAB (2017) and own calculations.

Appendix B – Prediction of Single Earner Status in the Administrative Data

The incentives to increase labor earnings beyond Minijobs can be affected by income taxes (see **Figure 2**). For secondary earners in married couples who often face high income tax rates, the introduction of Midijobs did not change the incentives to increase labor earnings to a large extent. It is interesting to test hypotheses concerning the heterogeneous treatment effects for single and secondary earners in married couples with administrative data.

Unfortunately, the administrative data do not contain single vs. secondary earner status information. Therefore, we predicted single earner status based on the SOEP survey data. As described in Section 4 of the manuscript, in the survey data we consider individuals aged 30-59 whom we observe in any of the years 2001-2006. We have 2,736 person-year observations on 1,255 different individuals. In this sample, 10.1 percent of the observations are single earners (39.9 percent of male and 8.3 percent of female observations).

In the prediction model, we considered (categories of) the following explanatory variables and their interaction terms: gender, foreign nationality, residence in East Germany, educational degree, year of birth, age, occupation, labor market experience, and unemployment experience. Based on the estimation equation as presented in **Table B.1**, a linear regression model generated an R^2 value of 0.198 and an adjusted R^2 of 0.184.

We applied the estimated coefficient to the SIAB data and generated individual predicted probabilities of being single earners. In our analyses, we dichotomized the continuous predicted treatment indicator such that the top 10 percent of predicted single earners are coded 1 and the others 0; this matches the observed share of about 10 percent single earners in the SOEP data. Our model predicts that 54 percent of male and 2 percent of female observations are single earners. This implies a cutoff for the predicted probability of being single of about 33 percent. Had we applied the more standard 50 percent cutoff we would have obtained a very small sample of only 5.6 percent single individuals in the SIAB sample.

The predicted single earner status is used to investigate heterogeneous treatment effects in the RDiT framework in Section 3.4 (for sample A) and in the DID approach in Section 4.4. For the analyses discussed in Section 4.4 we use all monthly Minijob observations between April of 2001 and March of 2006 for individuals aged 30-39 who do not simultaneously receive unemployment benefits. This yields a sample of $N=2,831,554$ monthly observations.

Table B.1 Prediction Equation for Single Earner Status

Variable	Coef.	Std. err.
Female (0/1)	-0.1556	0.1954
Foreign Nationality (0/1)	-0.3179	0.0810
East Germany (0/1)	0.0412	0.1740
Educ: No vocational degree	Reference	
Educ: Vocational degree	0.1410	0.0682
Educ: Tertiary degree	0.0479	0.0958
Educ: Missing or in school	-0.2958	0.1590
Birth year: 1942-1946	Reference	
Birth year: 1947-1951	-0.1405	0.0873
Birth year: 1952-1956	-0.1272	0.0998
Birth year: 1957-1961	-0.0990	0.1669
Birth year: 1962-1966	-0.2236	0.1555
Birth year: 1967-1971	0.1590	0.1905
Birth year: 1972-1976	0.4395	0.1959
Age grouped: 30-39	Reference	
Age grouped: 40-49	0.1726	0.1141
Age grouped: 50-59	-0.2565	0.1682
Occupation: Agriculture and other	Reference	
Occupation: Simple and Qualified manual task	-0.0478	0.0258
Occupation: Simple and Qualified service	-0.0170	0.0181
Occupation: Engineer, techn., semi-prof.	-0.0465	0.0235
Occupation: Professional and manager	-0.0602	0.0403
Occupation: Simple and Qualified administration	-0.0493	0.0185
Labor market experience (in years)	0.0000	0.0009
Unemployment experience (in years)	0.0405	0.0054
Female * Foreign Nationality	0.2330	0.0806
Female * East Germany	-0.1190	0.0630
Female * Voc. Education	-0.1605	0.0698
Female * Tertiary Education	-0.0392	0.0984
Female * Missing	0.2407	0.1656
Female * 1947-1951	0.1243	0.0917
Female * 1952-1956	0.0840	0.1034
Female * 1957-1961	0.0651	0.1693
Female * 1962-1966	0.1814	0.1589
Female * 1967-1971	-0.1755	0.1938
Female * 1972-1976	-0.3909	0.1995
Female * 40-49	-0.1657	0.1153
Female * 50-59	0.2470	0.1696
East Germany * 1947-1951	0.0973	0.1018
East Germany * 1952-1956	0.2406	0.1029
East Germany * 1957-1961	0.0277	0.1325
East Germany * 1962-1966	0.1415	0.1414
East Germany * 1967-1971	0.1519	0.1740
East Germany * 1972-1976	0.1145	0.1847
East Germany * 40-49	0.0685	0.1136
East Germany * 50-59	0.0595	0.1435
Foreign Nationality * 40-49	0.0483	0.0407
Foreign Nationality * 50-59	0.1383	0.0441
Age 40-49 * Unemployment experience	-0.0029	0.0067
Age 50-59 * Unemployment experience	-0.0093	0.0068
East Germany * Unemployment experience	-0.0182	0.0066
Constant	0.2814	0.1938

Source: SOEP (2001-2006).