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Employment Effects of Payroll Tax Subsidies

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Abstract

This paper exploits several reforms of wage subsidies in the framework of the German Minijob program to investigate substitution and complementarity relationships between subsidized and non-subsidized labor demand. We apply an instrumental variables approach and use administrative data on German establishments for the period 1999-2014. Particularly in small establishments (0-9 employees), subsidized Minijob employment comprises large shares of the work force, on average over 40 percent. For these establishments, robust evidence shows that increasing the subsidization of Minijob employment crowds out non-subsidized employment. Our results imply that Minijob employment in 2014 may have eliminated more than 0.5 million unsubsidized employment relationships just in small establishments.

JEL-Codes: J210, J230, J380, C260.

Keywords: wage subsidy, Minijob, labor demand, substitution effect, crowding out effect, displacement effect, employment, payroll tax.

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1. Introduction

One in six German workers takes advantage of subsidized "Minijob" employment (BA 2017a). Introduced decades ago, the original purpose of the payroll tax subsidy was to reduce bureaucratic burden and to facilitate flexible minor employment relationships. At the same time, however, subsidized employment might crowd out demand for unsubsidized labor. With more than 7.4 million Minijob employment relationships (as of July 2017), their impact on unsubsidized employment could potentially be substantial. Even though subsidized employment relationships exist in many national labor markets, the relationship between subsidized and unsubsidized employment and the potential crowding out and displacement effects, surprisingly, have hardly been investigated, so far. This study exploits various reforms of subsidized employment in Germany to identify the causal effect of payroll tax subsidies on the demand for unsubsidized labor.

Minijobs (*geringfügige Beschäftigung*) are employment relationships which pay less than a given amount of monthly earnings, currently 450 Euro; this corresponds to about 13 percent of gross average monthly earnings in unsubsidized employment.¹ Unsubsidized employment involves employer and employee contributions of about 20 percent each to the social insurance system and income tax obligations of employees. In contrast, Minijob employment is exempt from both employee social insurance contributions and income taxes. Instead, employers pay a fixed share (contribution rate) of Minijob earnings to cover, both, social insurance contributions and income taxes. This contribution rate changed in discrete steps from 22 percent in 1999 to 30 percent since 2006. We use these increases in employers' cost, i.e., the declines in

¹ STBA (2017, p.4) reports that gross average monthly earnings in unsubsidized full- and part-time employment amount to 3,415 Euro as of March 2017. Minijobs additionally comprise short-term employment relationships (*kurzfristige Beschäftigung*), which do not extend beyond (currently) 70 days per year, independent of earnings. We disregard this second category of Minijob employment, which is less prevalent and follows a strong seasonal pattern: BA (2010) reports between 270,000 employees in winter and about 450,000 in summer months. This compares to more than 7 million employment relationships under the 450 Euro limit.

subsidization to identify the causal effect of Minijob employment on the demand for unsubsidized labor.²

Our study connects to two different lines in the international literature, one discussing displacement effects of active labor market policies and the second discussing the employment effects of payroll taxes and payroll tax subsidies.

In their meta analyses, Card et al. (2010, 2017) pointed out that the impact of active labor market policies (ALMP) on those that do not participate is an important unsettled question and that the relevant literature is scarce. In an early contribution, Dahlberg and Forslund (2005) find large displacement effects of Swedish ALMP in particular of subsidized employment. More recently, Crépon et al. (2013) used randomized experiments to determine displacement effects ALMP in France. They conclude that standard program evaluation would have overstated the programs' effects by not considering the displacement effects.

In the literature on employment effects of payroll taxes, most studies find no employment effects in response to changes in effective payroll taxes. Saez et al. (2017, p.1) argue that payroll tax incidence falls on workers' net wages as "received wisdom." In an early study, Gruber (1997) shows that the reduction of payroll taxes levied on Chilean firms from 30 to 8.5 percent did not affect employment. Instead, it benefited workers via higher wages. Similarly, Anderson and Meyer (2000) evaluate the introduction of experience rating for firms' unemployment insurance contributions in the state of Washington and find no employment effects of changes in payroll taxes. This confirms earlier findings by Anderson and Meyer (1997). Korkeamäki and Uusitalo (2009) investigate the effect of regionally reduced payroll taxes in northern Finland and find no statistically significant employment responses. They conclude that wage increases took up a large share of the potential cost reductions. The same

² This differs from the recent New Tax Responsiveness literature which discusses the impact of social security contributions on income and labor cost (e.g., Adam et al. 2017, Saez et al. 2012).

holds for a regional payroll tax reduction in northern Sweden analyzed by Bennismarker et al. (2009). Even though they consider a change in tax rates twice as large as that used in the Finnish study, these authors find solely an increase in firm entry but no employment change for existing firms. Again, employees benefited from wage increases. Huttunen et al. (2013) focus on a payroll tax subsidy in Finland that targeted at low-skill older workers. The authors do not find any response in the target group's employment rate. Generally, the lack of employment effects might be explained by wage adjustments in combination with inelastic labor supply or demand (for a discussion see also Hamermesh 1993³).

A few contributions do document employment responses to payroll tax subsidies. For example, Kangasharju (2007) investigates a Finnish subsidy which supports firms that hired previously unemployed workers for at least 85 percent of full time hours for at most 10 months. Based on panel data, the author concludes that employment increased in subsidized firms without effects on the region or competing firms. Garsaa and Levratto (2015) study the effects of reductions in social security contributions on employment growth in French manufacturing firms and also find the expected effects. In both, the Finnish and the French case, the positive employment effect is found for large and successful firms. If such establishments benefit from (short-run) subsidies and differ in their technology from other firms, it is worthwhile to pay attention to effect heterogeneities.⁴ Most recently, Saez, Schoefer and Seim (2017) found that a major reduction in payroll taxes for the employment of younger workers in Sweden did not affect wages but increased employment. As this result contradicts most of the literature, the

³ Hamermesh (1993, p.169) shows that in a situation dominated by the low labor supply elasticity of adult male workers increases in payroll taxes should reduce wages; increases in payroll tax subsidies should generate at best small rises in equilibrium employment. As labor supply for Minijob employment differs and may be characterized by much higher labor supply elasticities, employment effects are plausible in our case.

⁴ Crépon and Desplat (2002) find positive employment effects of payroll tax reductions for French low wage workers in the 1990s. Cahuc et al. (2017) offer causal evaluations of temporary hiring subsidies for small French firms in 2008/09 and find positive employment effects. A separate literature evaluates the effect of subsidies on outcomes at the individual level, see, e.g., Murphy (2007) or Bingley and Lanot (2002).

authors argue that it may be related to the impact of fairness norms within firms which render wage discrimination impossible.

We contribute to the literature in several respects: first, we exploit exogenous changes in federal subsidies and apply an instrumental variables approach to identify the causal effects of institutional reforms. This allows us to determine the adjustment in unsubsidized employment in "complier" establishments, i.e., those adjusting their employment in response to changes in subsidization. Second, in contrast to wage subsidies evaluated in the previous literature, the Minijob subsidy is not tied to worker (e.g., age or skill level) or job characteristics (e.g., region or industry). Instead, the subsidy can universally benefit all. This renders the subsidy effects independent of specific characteristics of the work force, industry, or region and enhances the external validity of the findings.

Third, we extend prior research that commonly considers labor supply as a homogenous input and evaluates the effects on total overall employment. Instead, we focus on the impact of changes in subsidized Minijob employment on unsubsidized full-time and part-time employment and apprenticeship positions as potential substitutes. So far, such substitution effects between subsidized and unsubsidized labor have not been addressed in the literature on employment effects of wage subsidies. Instead, the literature on heterogeneous labor demand looks at the wage effects on employment using industry-level data.⁵ Our causal instrumental variables strategy expands on these contributions and studies employment effects with recent data and at the establishment level. Our analyses concentrate on small establishments with 0-9 workers as they use Minijob employment most intensely. This evidence on the crowding out

⁵ Two studies investigate the price elasticities of heterogeneous labor inputs specifically including marginal employment for Germany. Jacobi and Schaffner (2008) find a high own-wage elasticity of marginal employment and modest effects of changes in the wages of marginal employment on demand for alternative types of labor. In unpublished simulation exercises, Freier and Steiner (2010) conclude that an increase in the contribution rate for marginal employment would have a modest negative effect on overall labor demand (Freier and Steiner 2007).

and displacement of unsubsidized employment is of international interest both at the macro- and the micro-level.

We find that the statutory reductions of Minijob subsidization (i.e., increases of employer contribution rates) reduce demand for Minijob employment, which provides a relevant instrument. The corresponding instrumental variables estimations show that unsubsidized employment and subsidized Minijob employment are substitutes. Particularly in small establishments, unsubsidized employment increases if Minijob employment declines. The effect magnitude implies that one additional Minijob substitutes about 0.5 unsubsidized jobs in the small establishment sector (0-9 employees). Neglecting general equilibrium effects, this suggests that Minijob employment in 2014 in the small establishment sector alone crowded out more than 0.5 million unsubsidized employment relationships. Our results hold up in numerous robustness checks. We also demonstrate that our instrument is plausibly exogenous and the findings are not sensitive to a relaxation of the exclusion restriction by using the approach by Conley et al. (2012).

In the next section, we describe the institutional background and the reforms that we exploit in our analyses. We discuss our empirical approach and describe our data in section three. In section four, we present our results and robustness tests and section five concludes.

2. Institutional background

The Minijob program is one of the largest labor market programs in Germany. As of 2017, nearly 7.5 million individuals, i.e., one sixth of the labor force took advantage of this indirect wage subsidy (BA 2017a). Legally, individuals performing Minijobs are part-time employees subject to general labor laws. They benefit from sick pay, maternity leave benefits, and employment protection, and are entitled to paid vacation days, as well. At the same time, the Minijob program stipulates that individuals working in minor employment relationships are exempt from otherwise mandatory contributions to social insurances and from income taxes.

Instead, employers pay a fixed share of a worker's gross earnings to social insurance and tax authorities. The subsidy is currently available if monthly earnings do not exceed 450 Euro. Labor earnings above that amount are taxable and subject to social insurance contributions by workers and employers (for details see e.g. Eichhorst et al. 2012, Berthold and Coban 2013).

Similar subsidies existed since 1911 (*Reichsversicherungsordnung*). The rules were modified over time with varying objectives, e.g., to reduce bureaucratic burdens, to incentivize labor supply, to raise social insurance contributions, or to provide incentives for unsubsidized part-time employment. In 1999, a reform fixed the upper earnings limit at 325 Euro and set a limit of at most 15 working hours per week. Employees were exempt from social insurance contributions and income taxes, but employers paid lump-sum contributions of 22 percent of earnings.

A subsequent reform in 2003 aimed at reducing illicit employment and increasing employment opportunities for the unemployed through Minijobs as a stepping stone to the unsubsidized labor market.⁶ Further goals included enhancing firms' flexibility and reducing labor costs. The reform rendered Minijobs more attractive because it abolished the limit of 15 working hours per week, allowed to add a Minijob on top of an unsubsidized employment contract ("add-on Minijob"), raised the monthly earnings limit to 400 Euro, and fixed the employer contributions at 25 percent up from previously 22 percent.⁷ In the middle of 2006, employers' contributions to the social insurance system rose to 30 percent and in 2013, the

⁶ Freier and Steiner (2008) and Caliendo et al. (2016) evaluate the effectiveness of the Minijob program in facilitating re-entry to the labor market. Both teams focus on unemployed males and find, both, positive and negative effects of Minijob employment. Böheim and Weber (2011) evaluate a similar program in Austria and find negative effects of working in marginal employment when unemployed. About 10 percent of Minijob holders are registered as unemployed which allows up to 15 hours of paid employment (Körner et al. 2011, RWI 2012).

⁷ The 2003 reform also introduced an extended version of the program – Midijobs – for monthly earnings between 400 and 800 Euro. Employees in Midijobs pay social insurance contributions that increase on a sliding scale. The number of Midijobs increased from roughly 0.6 to 1.3 million between 2006 and 2010 (Eichhorst et al. 2012).

monthly earnings limits increased to 450 Euro.⁸ **Table 1** summarizes the relevant institutional changes of the Minijob program between 1999 and 2013. It documents also that all reform laws were passed shortly prior to the implementation dates leaving little room for anticipatory adjustments. The average tenure of Minijobs is around 900 days (information provided by Minijob-Zentrale) which indicates that these are rather stable employment relationships. Thus, it is unlikely that employers flexibly adjusted their labor demand in anticipation of regulatory changes.

In addition to employer contributions for social insurance and income tax obligations of Minijob employment, there exist three expense sharing schemes of lower salience: employers have to share the costs of sick pay (U1), maternity benefits (U2), and insolvency benefits (U3). These schemes exist since 2003 and demand small gross earnings shares between 0.1 (2005-2008) and 1.45 (2015) percent (for details see MZ 2017 and **Table A.3**). We disregard expenditures on these schemes in our main analyses and instead perform robustness tests to evaluate whether our results are sensitive to considering these additional costs.

Due to data availability, our identification strategy exploits contribution rate reforms as of April 1, 2003 and of July 1, 2006. Thus, it is important to pay attention to other potential changes in the institutional setting in this period. In 2003, two parallel changes occurred with respect to Minijob employment: the abolition of the hours limit and the introduction of add-on Minijobs. We control for these specific measures parametrically.

Overall, the Minijob reforms were introduced within an extensive package of labor market reforms (Hartz reform) in the early 2000s. The Hartz reform was implemented in four steps: Hartz I and II were passed in December of 2002 and mostly became effective on January 1, 2003. Hartz I deregulated temporary agency employment and modified the employment

⁸ For comparison, in 2016, unsubsidized employment was subject to total contribution rates close to 40 percent of earnings (DRV 2016a). Save for small exceptions, these contributions are split equally between employers and employees.

conditions for temporary agency workers. The law also introduced vouchers for vocational training. Hartz II modified marginal employment and introduced subsidies for the self-employment of previously unemployed workers. Hartz III and IV were passed in December of 2003. Hartz III (effective January 1, 2004) restructured the unemployment insurance itself. Finally, Hartz IV (effective January 1, 2005) reformed unemployment benefits and their interaction with means-tested social assistance.⁹

It appears plausible that many of the reforms, which occurred at different points in time, are orthogonal to the demand for subsidized Minijob employment (e.g., the internal organization of the unemployment insurance, the introduction of training vouchers, or changes in means-tested benefit system). Reforms that might interact with the demand for Minijob employment are the support of temporary agency work and of self-employment (i.e., elements of Harz I and II). Within a given firm, the use of temporary agency workers or self-employed workers might crowd out unsubsidized employment. If this reduced unsubsidized employment, both institutional changes would downward bias the estimates of the substitution effects of Minijob employment, which became more expensive after its reforms. Overall, the institutional framework should not be a problem for our analyses: for most changes, we do not expect direct effects on firms' production technology and the within-firm relationships between subsidized and unsubsidized employment. Those reforms that may affect labor demand should generate a conservative downward bias in the expected substitution relationship between Minijobs and unsubsidized employment. We extend the discussion of the identifying assumptions in the next section.

The utilization of Minijob employment is described by Hohendanner and Stegmaier (2012) or Bachmann et al. (2017). Recently, out of 7.4 million Minijobs about 2.6 million are

⁹ Causal evidence on the effectiveness of the Hartz reforms in their entirety are hard to come by. For a discussion and additional references see, e.g., Akyol, Neugart, and Pichler (2013).

"add-on Minijobs" leaving 4.8 million unsubsidized, exclusively-Minijob employment relationships. Particularly small establishments and the retail, general service, and restaurant sector use Minijob employment. More than 60 percent of Minijob employees are female. **Figure 1** shows a slight positive trend in Minijob employment since 2004 that appears to be driven by add-on Minijobs. After the 2003 reform, which abolished the weekly hours limit and allowed add-on Minijobs, the share of add-on Minijobs increased from about 17 percent in 2003 to above 30 percent in 2016. We control for this reform in our empirical model.

3. Methods and Data

3.1 Empirical Strategy

We exploit recent legislative changes to identify the causal effect of the Minijob subsidy on employers' demand for alternative non-subsidized types of employment. Specifically, we focus on the following outcome equation:

$$y_{it} = \beta_{0i} + \gamma \text{Mini}_{it} + f(\text{trend})' \beta_1 + X'_{it} \beta_2 + \varepsilon_{it} \quad (1)$$

where y_{it} represents the unsubsidized employment of establishment i in year t ; we consider several specific outcome measures y_{it} such as the number of employed individuals, the number of hours worked, and their respective logarithmic values. The vector β_{0i} includes establishment fixed effects. The explanatory variable of interest Mini_{it} measures the incidence of Minijob employment in establishment i in year t . Depending on the outcome measure, we use a corresponding specification for Mini_{it} such as the number of individuals, number of hours worked, or their logarithmic values. The polynomial $f(\text{trend})$ flexibly captures the time trend and X_{it} controls for potentially time-varying (institutional) characteristics. γ , β_1 , and β_2 are the parameters to be estimated, and ε_{it} is an error term. We expect a negative estimate for γ if Minijobs substitute unsubsidized employment.

Given the potential endogeneity of Minijob employment, which managers chose simultaneously with unsubsidized employment, we apply an instrumental variable (IV)

approach: we use the shifts in contribution rates occurring in reform years as an instrument for $Mini_{it}$ which identifies the local average treatment effect (LATE) for those responding to an increase in costs of Minijob employment. The corresponding first-stage equation is:

$$Mini_{it} = \alpha_{0i} + \delta Contribution_t + f(trend)' \alpha_1 + X'_{it} \alpha_2 + \epsilon_{it} \quad (2)$$

where $Contribution_t$ is the employer contribution rate for Minijobs in year t . Again, the parameters δ , α_1 , and α_2 are to be estimated within a fixed-effects framework and ϵ_{it} is an error term.¹⁰ To account for the potentially confounding effects of other regulations, we additionally control for the statutory limits on monthly earnings and working hours in Minijob employment in both equations (cf. **Table 1**).

The internal validity of our IV approach rests on the assumptions that the instrument is relevant and exogenous, and that its effects are monotonous. The relevance condition requires that changes in contribution rates affect employers' demand for Minijob employment. We test this condition and discuss the results in Section 4. The exogeneity condition requires that changes in contribution rates have no direct impact on demand for unsubsidized employment. This condition cannot be tested; however, we assume that conditional on Minijob employment the demand for unsubsidized employment does not vary independently with the cost of Minijobs, i.e., that any response in unsubsidized employment to changes in contribution rates occurs in connection with adjustments in Minijob employment. This would be the case if employers use a two-stage-decision process where they first determine demand for subsidized labor and then adjust demand for unsubsidized labor. The exogeneity condition also requires that the shift in the contribution rate itself is unrelated to labor demand structures. Such a correlation is implausible as the reforms were politically motivated, followed very different rationales over time, and affected the entire national labor market and all establishments.

¹⁰ The instrumental variables fixed effects estimator (we use Stata 14) applies the standard 2-stage-least squares estimator to within transformed variables; i.e., for each observation (i t) on each variable (x), we use $x_{i,t}^* = x_{i,t} - \text{mean}(x_{i,\cdot})$.

Nevertheless, in our baseline specification, we define *trend* as a third order polynomial in calendar years to flexibly control for any time trends in unsubsidized labor demand. In addition, we parametrically control for changes occurring in 2003 and offer a robustness test of the exogeneity assumption as suggested by Conley et al. (2012) below. Finally, the monotonicity condition assumes that there are no defiers, i.e., establishments that increase Minijob employment in response to its cost increases.

3.2 Data

We use administrative data from the Establishment History Panel (BHP) provided by the Research Data Centre of the German Federal Employment Agency at the Institute for Employment Research (IAB). The data build on the population of establishments in Germany with at least one employee subject to social security or in Minijob employment as of 30 June of any given year between 1975 and 2014 (Schmucker et al. 2016). The BHP provides a random 50 percent sample of these establishments, which comprises between 1.3 and 2.9 million establishments per year. Besides a large sample size, the BHP offers precise longitudinal information on stocks and flows of different types of employees. We focus on the period 1999-2014 because it provides information on Minijob employment.

In our sample, we omit observations on private households offering Minijob employment. We observe more than 3 million different establishments. Our sample comprises 20,241,824 establishment-year observations for the period 1999-2014. About 79 / 19 / 2 percent of establishment-year observations are in the small (0-9), middle (10-99), and large (100+ employees) establishment size categories, which is defined based on the number of employed individuals (head count) including unsubsidized and Minijob employment. On average, we

observe 9.7 unsubsidized and 2.3 Minijob employees per establishment and year.¹¹ Minijob employees in our data are unequally distributed across establishment sizes: We observe the highest Minijob employment share in the group of small establishments.¹²

Our dependent variable is an establishment's unsubsidized employment measured as the number of individuals or the total daily number of hours worked by these employees. Unsubsidized employment combines full-time and part-time workers as well as apprenticeship positions. Similarly, our key explanatory variable measures an establishment's Minijob employment in terms of the number of workers or hours worked per day.¹³ As the BHP lacks information on working hours, we draw on survey data from the German Socio-Economic Panel (SOEP), which is a longitudinal survey of over 11,000 private households conducted annually since 1984 (Wagner et al., 2007). The SOEP asks respondents detailed questions on labor market participation including the actual number of working hours per week and the type of employment. We use this information with cross-sectional weights to calculate annual averages of daily working hours by employment type (see **Table A.1**). To assure comparability with the BHP, we exclude employees in private households and those not being subject to social security contributions (civil servants and self-employed) from the SOEP while calculating the annual means. **Table A.1** reveals that the average daily working time remained relatively constant over time with nearly 9 hours in full-time, 5 hours in part-time, and around 2.5 hours in Minijob

¹¹ In small establishments, we observe on average 1.1 and 1.8, in medium sized establishments 4.9 and 18.8, and in large establishments 29.2 and 254.7 employees in the Minijob and unsubsidized employment categories, respectively.

¹² About 36 / 41 / 23 percent of all Minijob employees are employed in small / middle / large establishments, respectively. This differs from the distribution of unsubsidized employment where about 15 / 37 / 48 percent of all unsubsidized employees are employed in small / middle / large establishments. Thus, Minijob employment is concentrated in small establishments, which account for 15 percent of unsubsidized and 36 percent of subsidized employment relationships.

¹³ The BHP does not report add-on Minijob employments performed by individuals who simultaneously hold an unsubsidized employment in the same firm, which is rare. Thus, we observe the vast majority of add-on Minijobs as they are in a different firm than the unsubsidized employment. Another minor limitation of the BHP is the lack of data on the short-term category of Minijob employment, which is much less prevalent.

employment. We then combine the annual averages with the BHP data to calculate the volume of hours worked in unsubsidized and Minijob employment: for each establishment, we multiply its number of employees in a given employment type by the respective average working time in a particular year as gathered from **Table A.1**.¹⁴

Figures 2 and **3** describe the share of employees in Minijobs and share of hours worked by Minijob employees over time and by establishment size. Both figures show the uptake in Minijob employment around the 2003 reform and suggest a small decline after the 2006 reform. In addition, we see vast differences in the relevance of subsidized Minijob employment across establishment sizes. In small establishments, on average, two in five employees are on Minijobs and Minijob employees account for almost 20 percent of the hours worked. Given the vast differences across establishment sizes, we study labor demand relationships by establishment size and focus on small establishments with between 0 and 9 employees.¹⁵ The subsample of small establishments accounts for 16.1 of the overall 20.4 million establishment-year observations on about 2.6 million different establishments; in contrast, our data contain 3.9 and 0.37 million middle and large establishment-year observations.^{16, 17}

¹⁴ Given that apprentices spend some of their time in school, to approximate the labor input of this group, we use the average number of part-time employment hours.

¹⁵ We observe heterogeneous labor demand composition across establishment sizes: linear regressions show a positive correlation between Minijob and unsubsidized employment in middle and large establishments confirming the findings of Hohendanner and Stegmaier (2012). Also, first stage IV-regressions do not confirm the relevance of the instrument in the sample of large establishments (results not presented).

¹⁶ We investigated a potential correlation between the rate at which establishments drop out from the sample and the value of our instrument. We do not find such a correlation: the propensity to drop out from one year to the next slightly decreased between 2002 and 2003 and slightly increased between 2006 and 2007.

¹⁷ Generally, the distribution of small establishments across industries does not differ appreciably from that of larger establishments (see Appendix **Table A.2**). However, the share of establishments in agriculture is more than twice as high as that among larger establishments. In contrast, there are fewer establishments in manufacturing, particularly in food production, basic metal, and machinery. Not surprisingly the vast majority of about three quarters of all establishments is in the tertiary sector with a higher share of small than large establishments.

Our instrument corresponds to the sum of statutory contributions for Minijob employment paid by the employer (see last column of **Table 1**) and is expressed in percent of gross earnings. Further, we construct a dummy variable that indicates the period before April 1, 2003 when the working hour limit of 15 was abolished. Another institutional variable is the monthly earnings limit for Minijobs measured in Euro and the average social insurance contribution rate paid for unsubsidized employees (DRV 2016a¹⁸). **Table 2** shows descriptive statistics for the dependent and independent variables in our sample.

4. Results

4.1 Baseline Results

Table 3 presents the fixed effects estimation results for our main sample. Each column represents a different dependent variable describing the number of individuals in unsubsidized employment, its log value, the number of daily hours worked by unsubsidized workers, and its log value. Row 1 presents the estimate of γ without accounting for the potential endogeneity of labor demand shifts, row 2 shows the IV estimate. For completeness, we report the first-stage and reduced-form estimates in column 3 and 4, respectively. For the first stage, we show the estimated coefficient δ from equation 2. Column 4 displays the corresponding reduced-form effects of the instrument on the outcomes.

The linear regression estimates of equation (1) in row 1 yield that Minijob employment is significantly and negatively correlated with unsubsidized employment in small establishments. The first stage estimates of δ in row 3 as well as the F-test statistics presented in the bottom row of **Table 3** confirm that the instrument is relevant as the values of the F-

¹⁸ We disregard those elements of social insurance contributions that are collected only from employees and not from employers because these elements and their adjustments over time should not affect employer behavior.

statistics are far beyond the critical values: as expected, higher contribution rates are negatively correlated with Minijob employment.

The key results of interest are the 2SLS estimates of the causal effect of Minijob on unsubsidized employment presented in row 2 of **Table 3**. For all four outcomes, the point estimates are negative and highly statistically significant. This suggests that in small establishments, an increase in Minijob employment (due to reduced contribution rates) substitutes unsubsidized employment, i.e., the sum of full-time, part-time, and apprenticeship employment. The patterns are similar across all dependent variables and indicate that each Minijob employee and each hour worked by a Minijob worker substitutes about half an unsubsidized employee (column 1) and half an hour worked by an unsubsidized employee (column 3). Accordingly, based on our specification, on average, two Minijob workers replace one non-subsidized employee. The coefficient estimates in columns 2 and 4 suggest that the number of unsubsidized employees and their hours worked decline by about 0.35 percent if the respective Minijob employment increases by one percent.

We derive these IV estimates assuming that Minijob contribution rates do not have direct effects on the outcomes of interest, i.e., that the exclusion restrictions hold. Specifically, we assume that in

$$y_{it} = \beta_{0i} + \gamma \text{Mini}_{it} + f(\text{trend})' \beta_1 + X'_{it} \beta_2 + \pi \text{Contribution}_t + \varepsilon_{it} \quad (3)$$

π is valued zero. The methods developed by Conley et al. (2012) allow us to test the sensitivity of our results to violations of this assumption. For this purpose, we estimate the second stage coefficient (γ) and its confidence interval if the exclusion restriction is not exactly valid and π deviates from zero (for prior applications see, e.g., Wang 2013, Dimico 2017, Ager et al. 2017, or Nybom 2017). We apply the "union of confidence intervals" approach and assume that the coefficient of the instrument (π) in equation (3) is drawn from an asymmetric, positive support. The four panels in **Figure 4** depict the coefficient estimate of interest (γ) and its 95 percent confidence interval depending on the value of π , which under the exclusion restriction is zero.

The x-axes depict π as a share of the reduced form estimate of the contribution rate (see row 4 of **Table 3**). All four panels of **Figure 4** show that the estimates of γ remain statistically significant until π reaches about 80 or 90 percent of the reduced form estimate. Thus, a potential omitted variable, which is correlated with the contribution rate, would have to account for about 80 percent of the reduced form effect to render the 2SLS estimate insignificant, which is unlikely. Therefore, the results are not sensitive to the relaxation of the exogeneity assumption.

4.2 Robustness tests

In order to gauge the reliability of our results, we offer a broad set of robustness checks and describe the heterogeneity of effects. Specifically, we consider (i) changes in the specification of the time trend, (ii) changes in the definition of dependent variables and the instrument, (iii) heterogeneity checks, and (iv) changes in the sample composition.¹⁹ **Table 4** shows the alternative 2SLS results. For comparability, the first row repeats our baseline 2SLS estimates (see **Table 3**).

(i) Our data cover the period 1999-2014 on an annual basis. With such a relatively short period, estimation results might be sensitive to the representation of the time trend. As a robustness check, we use second or fourth order polynomials of the time trend instead of the third order polynomial in our baseline specification. The results in rows 1 and 2 confirm prior findings. The estimated causal effects remain very close to the baseline values and confirm the significant substitution effects.²⁰

¹⁹ In our setting, it is not possible to perform a valid placebo test. First, the period of observation does not contain enough years without changes in the instrument, which would allow us to evaluate placebo effects without covering periods affected by the actual reforms. In addition, the reforms affected all firms and all Minijobs, so that there are no placebo treatment groups.

²⁰ Additionally we estimated the model without time trend controls. The results remain statistically significant and negative with larger point estimates (results available upon request).

(ii) Next, we modify the dependent variables in two ways. First, we omit the group of apprentices from the count of unsubsidized workers because their development might be affected by alternative trends such as demographic shifts and changes in the educational system. Row 3 presents estimation results without this group. The effects are similar to those of the baseline estimation, with the exception of column 3 where the effect of Minijob employment increases in absolute magnitude from -0.559 to -0.660. This might suggest that apprentice employment dampens the effect of adjustments in Minijob employment.

Second, we modify the definition of the logarithmic values. In our baseline estimations, we added a constant of 0.001 prior to taking logarithms in columns 2 and 4 in order to be able to include the establishments with zero outcome values. Given that the sample contains a large share zero values for the dependent variables (25 percent), we investigate whether the choice of the added constant affects results. Row 4 in **Table 4** shows the estimates for columns 2 and 4 when, we alternatively add a value of 0.1 prior to calculating the logarithms. The estimates change slightly, but the difference is moderate and overall, the results are robust.

Third, we modify our instrument. So far, we only considered the salient changes in contribution rates consisting of tax and social insurance contributions. In addition, employers have to contribute to the expense sharing system (U1-U3). As these payments varied over time as well (see **Table A.3**), we investigate whether our estimates change when these expenses are included as an additional covariate.²¹ The baseline results are generally robust to this modification though the 2SLS estimates in row 5 increase slightly for the head-count outcomes and decrease for the hours outcomes compared to the baseline estimates. The results in columns 1 and 2 would suggest that employers do respond to the full cost and not just to the contribution

²¹ Note that we might consider only average values as presented in **Table A.3**. The U1 contributions actually work like an insurance and the rates depend on the chosen coverage. Employers in the public sector do not have to contribute to the U3 system because they are not at risk of insolvency. Information on the contribution rates for the period prior to 2003 is unavailable. While we use zero values for the pre 2003 years in the estimations, the results are robust to fixing the value at that observed for 2003, i.e., at 1.3 percent.

rate considered so far; however, this interpretation is not confirmed by the results in columns 3 and 4.

(iii) In row 6, we restrict the sample to the services sector only, which is the major user of the Minijobs employment. The sample size declines to 12.8 million observations. The causal effects are slightly smaller in magnitude than for the full sample, but overall the same patterns are very similar.

(iv) Next, we address four sampling issues. First, 25 percent of the establishment-year observations in our sample do not have unsubsidized employees. In their vast majority, these establishments employ only Minijob workers. As such establishments may respond most strongly to changes in the cost of Minijob employment, we include them in our baseline sample. In order to test the sensitivity of our findings with respect to the composition of small establishments, we reestimate our models after omitting two groups of establishments from the sample. Specifically, in row 7, we drop establishments which had no unsubsidized employment over a period of three consecutive years and establishments which were observed only once or twice in the sample and at that time had no unsubsidized employment. The remaining 11.7 million observations have unsubsidized employment at least once, which reduces the share of observations with a zero valued outcome from 25 to 6 percent of the overall sample. Row 7 in **Table 4** shows that our estimation results respond differently to this modification. The significant substitution effects are maintained throughout though with slightly increased effects in columns 1 and 3 (level outcomes) and smaller effects in columns 2 and 4 (log-value outcomes) compared to the baseline. Nevertheless, this allows us to conclude that our main results are not determined by establishments created only for the employment of Minijob workers.

In row 8, we omit observations collected after 2010 as the last change in the value of the instrument occurred in 2006 and this reform may not determine labor demand structures for

as long as 8 years afterwards. The reduced sample of 12.0 million observations yields slightly attenuated point estimates, but still confirms the finding of strong substitution effects.

Our sample considers all establishment-year observations with 0-9 employees. As the establishment size might be a response to the policy reform of interest the sample composition might be endogenous. Therefore, as a robustness test, we consider only those firms which had 0-9 employees at some point prior to the 2003 reform. We lose one quarter of the original sample but the point estimates continue to be significantly negative and in the same range.

Finally, we tested the sensitivity of our results to the definition of "small" establishments. Instead of using establishments with 0-9 employees, in row 10, we show results for establishments with up to 20 employees: the significant substitution effects are confirmed again.²²

5. Conclusions

We investigate the causal effect of subsidized employment on unsubsidized employment. Minijobs in Germany describe an employment relationship with reduced payroll taxes. This implicit subsidy has been modified several times since 1999. Our identification strategy exploits these law changes as exogenous shocks to the cost of subsidized Minijob employment. Specifically, we apply an instrumental variables (IV) approach within a panel data framework. We use vast administrative establishment level data for the years 1999-2014, which covers a fifty percent random sample of all establishments registered with the unemployment insurance.

We carefully describe the utilization of subsidized Minijob employment. Currently one in six employment relationships in the German labor market falls into this category (BA 2017a). We find that Minijob employment is predominantly used by small establishments, which make

²² We also estimated the models for the sample of establishments with 0-99 employees, which comprise more than 98 percent of the full sample. The results confirm negative and highly significant coefficient estimates at the first and second stage similar to those presented in **Table 3**.

up about 80 percent of all establishments and fill up to 40 percent of their positions and about 18 percent of their hours of work by Minijobs employment. Thus, our main analysis focusses on small establishments.

Our first-stage estimates yield that the instrumental variable, i.e., the rate at which employers contribute to social insurance and income taxes for Minijob employment, significantly affects the demand for Minijob employment. The main IV results suggest that Minijob employment crowds out unsubsidized employment. We find the substitution pattern for both the number of employees and of hours worked. Using their logarithmic transformations also leads to similar conclusions. We demonstrate that our results are not driven by a specific sample selection, the definition of the outcomes and instrumental variables, and the considered time trend specifications. Our estimates are also robust with regard to deviations from the exclusion restrictions based on tests along the methods suggested by Conley et al. (2012).

The estimated effect sizes imply that for small establishments, with currently about 1.2 million Minijob employment relationships, every additional Minijob employment about 0.5 unsubsidized jobs are lost. Thus, already among small establishments, more than half a million unsubsidized employment relationships are crowded out by subsidized Minijobs.

In addition, the subsidy generates fiscal costs. Minijob employment generates average monthly earnings of 300 Euro (DRV 2016b). The difference between the employer contribution rate for Minijobs and the contribution rates for unsubsidized employed amounts to about 12 percentage points of gross earnings.²³ Thus, the social insurance subsidy for average Minijob employment amounts to 36 Euro per month and 432 Euro per year. With about 7 million Minijob employees, this adds up to 3.02 billion Euro per year. If two Minijobs were substituted by one unsubsidized full-time, part-time or apprenticeship job, the unsubsidized employment

²³ Currently the sum of employer and employee contribution rates is at about 40 percent (DRV 2016a, p. 262). **Table 1** shows that currently employers contribute 28 percent of Minijob earnings to the social insurances.

would generate higher earnings (see DRV2016a) and would contribute to social insurances at higher rates. Social insurances were to gain substantially. Additionally, revenues from income tax payments would be collected from workers. One might argue that abolishing the subsidy would shift Minijob employment to the informal sector. However, this does not agree with our finding of a substitution relationship between Minijob and unsubsidized employment, which instead suggests that demand for unsubsidized employment increases if Minijob subsidies decline.

The analysis implicitly assumes that there are no substantial shifts in labor supply over the considered period; clearly, our interpretation that changes in employment reflect changes in labor demand strictly holds only to the extent that the nature and quantity of labor supply remained constant. The literature on causal employment effects of payroll tax subsidies generally maintains this assumption. With respect to the reform studied here, Caliendo and Wrohlich (2010) investigate the short-run labor supply responses to the 2003 Minijob reform; they find no significant effect on participation in marginal employment. This corroborates our interpretation. Given that over time the payroll tax subsidies were reduced, it is not plausible to expect an increasing inflow from the shadow economy into marginal employment.

Our findings suggest that employers respond rationally and replace subsidized by unsubsidized employment if the cost of subsidized employment increases. This pattern matches the recent evidence on the impact of minimum wages in Germany introduced in 2015. At that point, particularly low wage Minijob employment became more costly for employers. The demand for Minijob employment declined and about half of the related job losses was filled by unsubsidized employment (vom Berge et al. 2016). Despite the introduction of minimum wages, which rendered some Minijob employment less attractive, more than 7.4 million such jobs still exist and at least partly crowd out unsubsidized employment relationships.

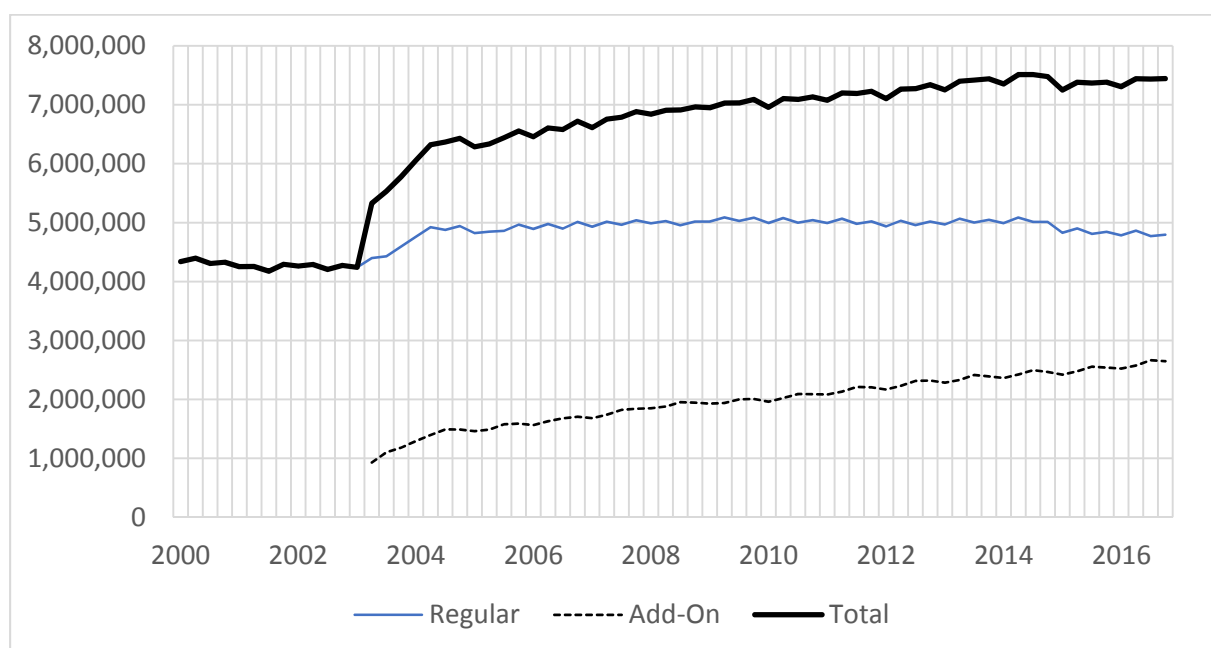
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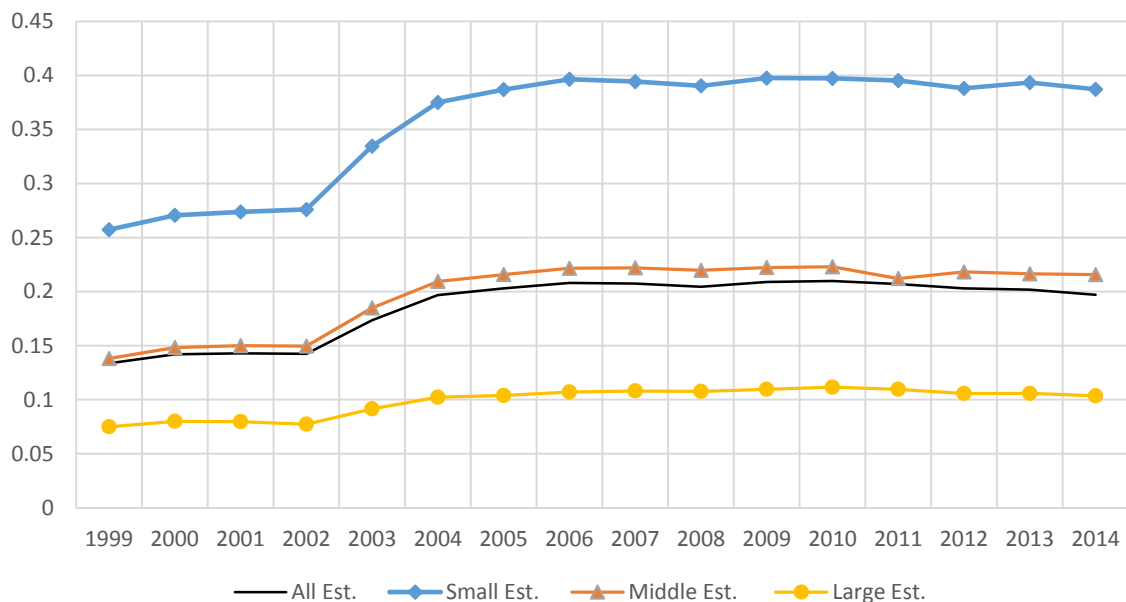
Figure 1 Total, Regular, and Add-On Minijob employment (Q1.2000 - Q4.2016)



Note: Minijob employment covers those employed in "450 Euro" Jobs (excluding short-term employment relationships). Regular Minijob employment counts employment relationships where the individual works only on the Minijob without an additional employment relationship registered at the same time. Since 2003, add-on Minijob are possible; here, Minijob employment is held in addition to other registered employment. The total number of the omitted short-term employment relationships never exceeded 350.000, in Dec. 2016 below 185.000.

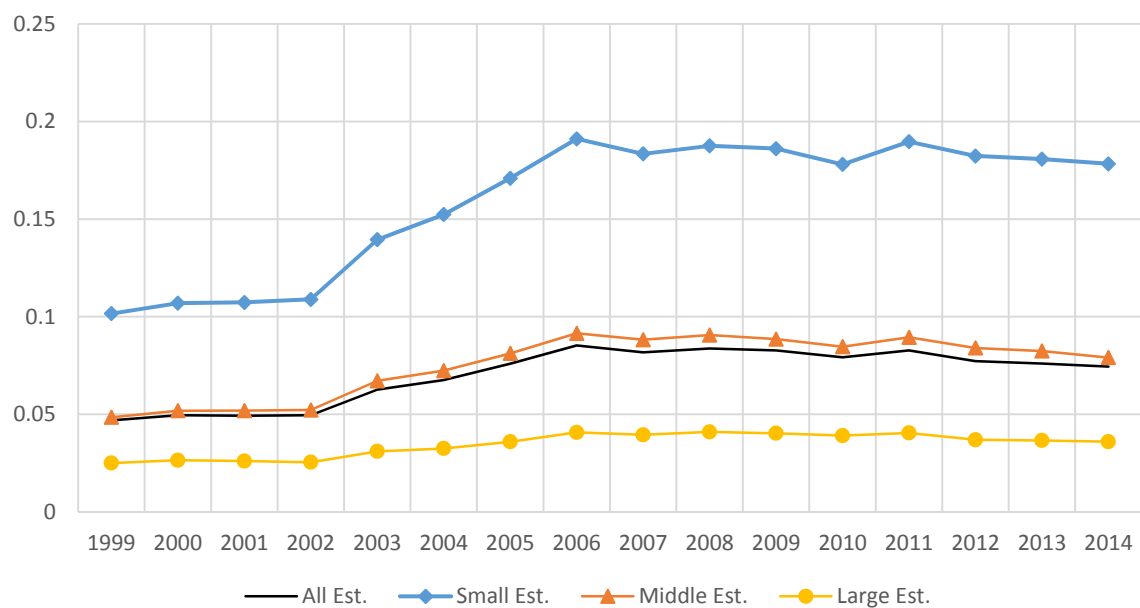
Source: Data received via personal email from Statistik der Bundesagentur für Arbeit.

Figure 2 Share of Minijob employees in all employees by establishment size



Source: BHP (1999-2014). "all employees" includes the unsubsidized full-time and part-time employed, apprentices, and Minijob employment.

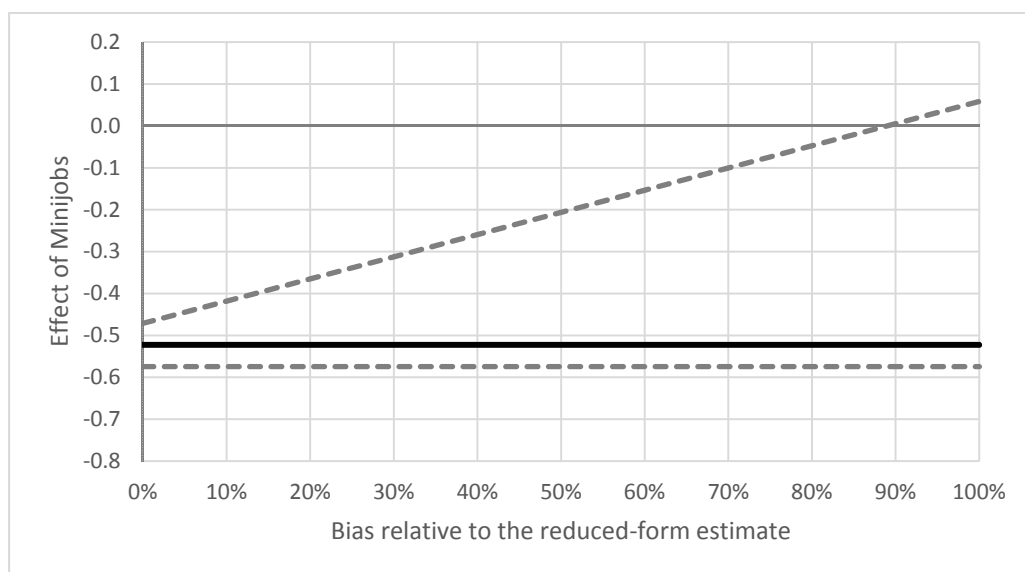
Figure 3 **Share of Minijob employment volume in full employment volume by establishment size**



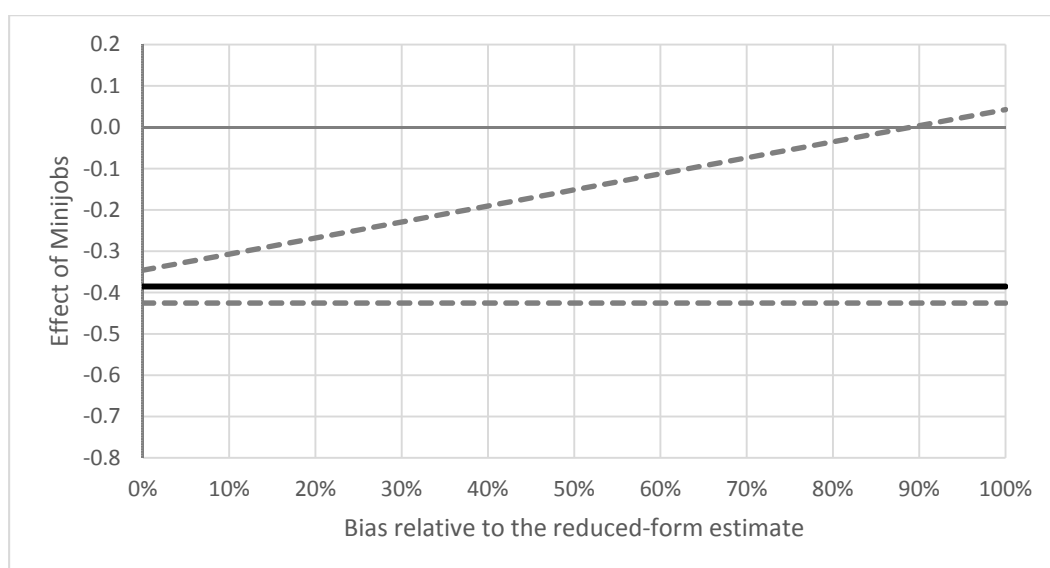
Source: BHP (1999-2014). "full employment volume" includes the hours worked by those in full-time and part-time unsubsidized employment, apprentices, and Minijob employment.

Figure 4 **Sensitivity of the IV estimates of the Minijob effect (γ) to violations of the exclusion restriction for four outcomes**

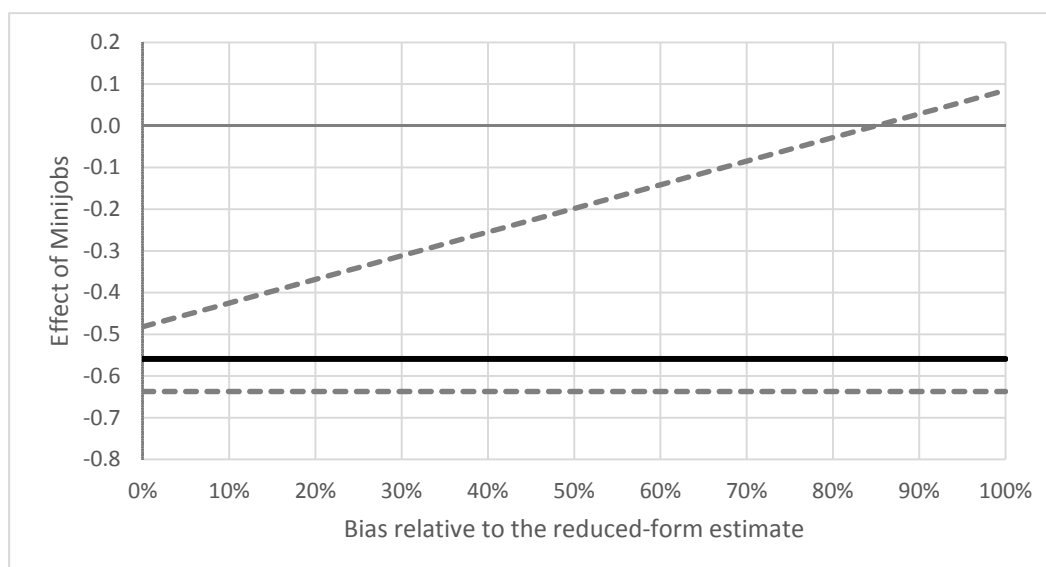
4.1 Outcome: number of employees



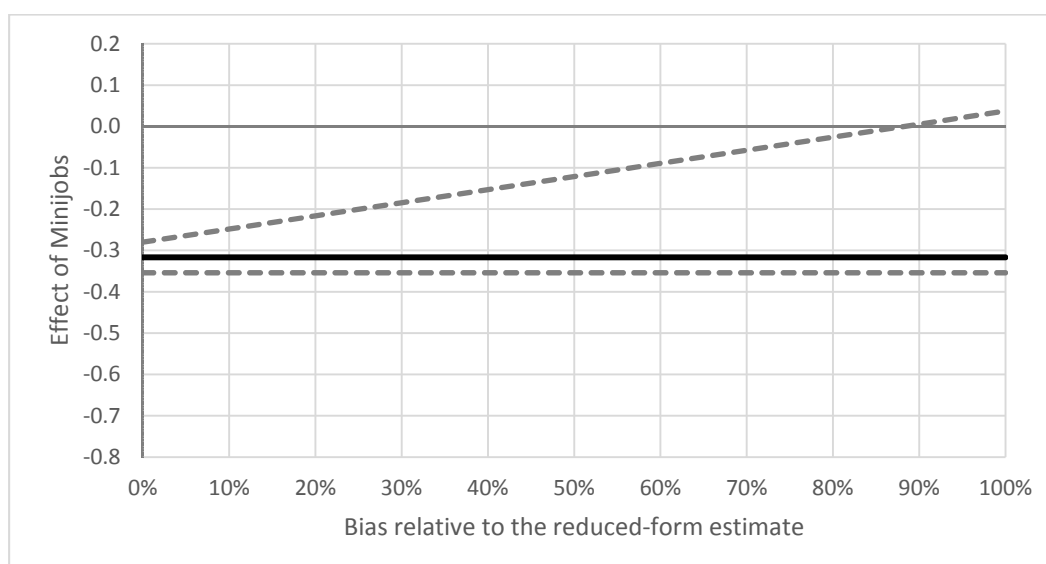
4.2 Outcome: log of number of employees



4.3 Outcome: number of hours worked



4.4 Outcome: log of number of hours worked



Note: The bold line represents the 2-SLS estimate of the Minijob effect (i.e., coefficient γ in equation 1); the dashed lines show the boundaries of its 95 percent confidence intervals. The 'Bias relative to the reduced-form estimate' on the x-axis reflects the deviation of the value of the instrument's coefficient in the second stage regression as a fraction of the reduced form coefficient estimate (see row 4 of **Table 3**). At 0 % bias, the estimate of the Minijob effect corresponds to row 2 of **Table 3**. The confidence intervals were determined following the "union of confidence intervals" approach as introduced by Conley et al. (2012).

Source: BHP (1999-2004)

Table 1. Legislative changes of the Minijobs program

Reform date (date of law change)	Monthly earnings limit (in Euro)	Working hours	Retirement insurance contribution (in %)	Health insurance contribution (in %)	Income tax (in %)	Sum of employer contributions (in %)
Apr 1, 1999 (Mar 24, 1999)	325	max. 15	12	10	0	22
Apr 1, 2003 (Dec 23, 2002)	400	no limit	12	11	2	25
July 1, 2006 (Jun 29, 2006)	400	no limit	15	13	2	30
Jan 1, 2013 (Dec 5, 2012)	450	no limit	15	13	2	30

Source: Own illustration based on Eichhorst et al. (2012) and Berthold and Coban (2013).

Table 2. Descriptive statistics of dependent and independent variables

	Mean	SD	Min	Max
Dependent Variables - Second Stage: Regular Employment				
Number of employees	1.973	2.013	0	9
log (no. of employees + 0.001)	-1.190	3.365	-6.91	2.197
Number of hours worked	14.864	15.722	0	79.11
log (no. of hrs worked + 0.001)	0.297	4.223	-6.91	4.371
Dependent Variables - First Stage: Minijob Employment				
Number of employees	1.103	1.345	0	9
log (no. of employees + 0.001)	-2.511	3.626	-6.91	2.197
Number of hours worked	2.800	3.429	0	24.93
log (no. of hrs worked + 0.001)	-1.956	4.078	-6.91	3.216
Independent Variables				
Contribution rate - regular	40.09	1.285	38.55	42.01
Monthly earnings limit (in Euro)	388.341	39.176	325	450
Working hour limitation (0/1)	0.760	0.427	0	1
Contribution rate - Minijob	26.843	3.404	22	30
Calendar year (1999-2014)	8.620	4.592	1	16

Source: BHP (1999-2014) and own calculations. The sample of small establishments (0-9 registered employees) comprises 16,143,184 establishment-year observations.

Table 3. Baseline estimation results - four outcome measures

	Number of employees	log (0.001 + no. of employees)	Number of hours worked	log (0.001 + no. of hours worked)
	1	2	3	4
1: FE	-0.153*** (0.001)	-0.228*** (0.000)	-0.392*** (0.002)	-0.256*** (0.000)
2: 2SLS-FE	-0.523*** (0.026)	-0.385*** (0.020)	-0.559*** (0.040)	-0.317*** (0.019)
3: First Stage	-0.013*** (0.000)	-0.031*** (0.001)	-0.065*** (0.001)	-0.041*** (0.001)
4: RF	0.007*** (0.000)	0.012*** (0.001)	0.037*** (0.003)	0.013*** (0.001)
F-test FS	1994.5	1251.0	7109.86	1828.85

Note: The number of observations is 16,143,184 establishment years with 2,688,051 different establishments. Standard errors are robust and clustered at the level of establishments. Across the four columns, the variable describing Minijob employment (i.e., variable *Mini* in equations 1 and 2) is defined according to the dependent variable in the main model (equation 1) as the number of employees, its log value, the number of minijob hours worked or its log value. The rows labelled FE and 2SLS-FE show the estimates of γ in fixed effects (FE) and two-stage least squares with fixed effects (2SLS-FE) regressions, respectively. The row labelled First Stage presents the coefficient estimate of the instrument in the first stage regression (i.e., δ in equation 2). F-test FS presents the value of the F-statistic in the test of significance for the instrument in the first stage regression. The row labelled RF provides the reduced form estimates. All models control for establishment fixed effects, a time trend (third order polynomial), add-on/free working hours (0/1), monthly earnings limit (in Euro), contribution rate for unsubsidized employees (in percentage points). *** $p < 0.001$, ** $p < 0.01$, * $p < 0.5$.

Source: BHP (1999-2014).

Table 4. Estimation results - robustness tests

	Number of employees 1	log (0.001 + no. of employees) 2	Number of hours worked 3	log (0.001 + no. of hours worked) 4	Number of observations 5
Baseline (Table 3)	-0.523*** (0.026)	-0.385*** (0.020)	-0.559*** (0.040)	-0.317*** (0.019)	16,143,184
Time trend specification					
1: Time trend quadratic	-0.553*** (0.021)	-0.429*** (0.017)	-0.560*** (0.031)	-0.359*** (0.015)	16,143,184
2: Time trend quartic	-0.502*** (0.028)	-0.373*** (0.021)	-0.568*** (0.043)	-0.309*** (0.020)	16,143,184
Definition of dependent and instrumental variable					
3: Omit apprentices	-0.567*** (0.026)	-0.426*** (0.022)	-0.660*** (0.040)	-0.355*** (0.020)	16,143,184
4: Log constant 0.1	- -	-0.402*** (0.019)	- -	-0.277*** (0.017)	16,143,184
5: Modified instrument	-0.616*** (0.021)	-0.438*** (0.016)	-0.306*** (0.019)	-0.299*** (0.012)	16,143,184
Heterogeneity					
6: Only service sector	-0.430*** (0.026)	-0.367*** (0.021)	-0.343*** (0.039)	-0.290*** (0.021)	12,838,406
Sample composition					
7: Omit zero employment	-0.668*** (0.036)	-0.284*** (0.019)	-0.806*** (0.058)	-0.228*** (0.019)	11,729,258
8: Omit years after 2010	-0.341*** (0.026)	-0.266*** (0.019)	-0.176*** (0.026)	-0.191*** (0.015)	12,000,090
9: 0-9 employees prior 2003	-0.432*** (0.111)	-0.211*** (0.017)	-0.631*** (0.190)	-0.170*** (0.016)	11,977,258
10: 0-20 employees	-0.665*** (0.030)	-0.374*** (0.019)	-0.860*** (0.045)	-0.304*** (0.017)	18,423,463

Note: See Table 3.**Source:** BHP (1999-2014).

Table A.1. Average number of working hours per day by employment type

Year	Working hours			Observations		
	Full-time	Part-time	Minijob	Full-time	Part-time	Minijob
1999	8.57	4.70	2.46	4950	990	336
2000	8.62	4.75	2.44	8070	1936	571
2001	8.62	4.69	2.40	7280	1800	572
2002	8.59	4.58	2.40	7348	1925	605
2003	8.55	4.75	2.45	6872	1920	663
2004	8.56	4.82	2.28	6552	1826	664
2005	8.63	4.88	2.50	6143	1739	701
2006	8.69	4.95	2.77	6347	1927	767
2007	8.79	4.98	2.68	6050	1898	725
2008	8.71	4.98	2.77	5861	1754	711
2009	8.72	5.05	2.66	5405	1669	689
2010	8.63	4.98	2.49	7023	2586	1122
2011	8.75	5.06	2.66	8453	3349	1239
2012	8.69	5.01	2.50	8272	3358	1247
2013	8.68	5.16	2.48	9398	3795	1546
2014	8.60	5.13	2.47	8230	3319	1405

Source: SOEP 1984-2015; own calculations using cross-sectional weights. Employees in private households, civil servants, and self-employed are excluded from the calculations.

Table A.2. Distribution of establishments across industries by establishment size

	All	Small	Middle & large	Diff.: Small - not small
Primary sector				
Agriculture	3.00%	3.39%	1.53%	1.87%
Mining	0.12%	0.08%	0.24%	-0.15%
Total	3.12%	3.48%	1.77%	1.71%
Secondary sector				
Man Food	1.46%	1.10%	2.81%	-1.72%
Man Textiles / Leather	0.35%	0.30%	0.54%	-0.24%
Man Wood	0.50%	0.49%	0.57%	-0.09%
Man Paper	1.00%	0.82%	1.66%	-0.84%
Man Coke	0.01%	0.00%	0.02%	-0.02%
Man Chemical	0.19%	0.10%	0.53%	-0.42%
Man Rubber	0.34%	0.18%	0.91%	-0.73%
Man Non Metallic	0.42%	0.35%	0.69%	-0.34%
Man Basic Metal	1.88%	1.48%	3.40%	-1.92%
Man Machinery	0.85%	0.52%	2.08%	-1.56%
Man Electric	0.95%	0.71%	1.86%	-1.15%
Man Transport	0.17%	0.11%	0.41%	-0.30%
Man Other	0.93%	0.89%	1.09%	-0.20%
Electric	0.25%	0.20%	0.47%	-0.27%
Construction	9.63%	9.79%	9.05%	0.74%
Total	18.93%	17.03%	26.09%	-9.06%
Tertiary sector				
Ser Wholesale	20.88%	20.71%	21.52%	-0.82%
Ser Hotel	7.46%	7.64%	6.79%	0.85%
Ser Transport	4.45%	4.02%	6.08%	-2.05%
Ser Finance	2.70%	2.92%	1.89%	1.03%
Ser Real Estate	8.88%	10.35%	3.36%	6.99%
Ser Other Services	15.50%	16.38%	12.18%	4.20%
Ser Public	15.64%	15.40%	16.57%	-1.17%
Ser Education	2.43%	2.08%	3.76%	-1.68%
Total	77.95%	79.49%	72.14%	7.34%
Total	100.00%	100.00%	100.00%	

Note: The table shows the distribution of establishment year observations, where we consider 16 mio small (0-9) and 4.269 mio not small (10+) establishments.

Source: BHP (1999-2014).

Table A.3. Average contribution rates to the expense sharing systems U1-U3

Since	U1	U2	U3	Sum
01.04.2003	1.2	0.1	0	1.3
01.01.2005	0.1	0	0	0.1
01.01.2006	0.1	0	0	0.1
01.07.2006	0.1	0	0	0.1
01.01.2007	0.1	0	0	0.1
01.01.2009	0.6	0.07	0.1	0.77
01.01.2010	0.6	0.07	0.41	1.08
01.01.2011	0.6	0.14	0	0.74
01.01.2012	0.7	0.14	0.04	0.88
01.01.2013	0.7	0.14	0.15	0.99
01.01.2015	0.7	0.24	0.15	1.09

Source: Minijob-Zentrale, https://www.minijob-zentrale.de/DE/02_fuer_journalisten/03_entwicklung/node.html [last access Oct. 23, 2017].