

Caliendo, Marco; Wrohlich, Katharina

Working Paper

Evaluating the German 'Mini-Job' Reform Using a True Natural Experiment

DIW Discussion Papers, No. 569

Provided in Cooperation with:

German Institute for Economic Research (DIW Berlin)

Suggested Citation: Caliendo, Marco; Wrohlich, Katharina (2006) : Evaluating the German 'Mini-Job' Reform Using a True Natural Experiment, DIW Discussion Papers, No. 569, Deutsches Institut für Wirtschaftsforschung (DIW), Berlin

This Version is available at:

<https://hdl.handle.net/10419/18462>

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.

Discussion Papers

569

Marco Caliendo
Katharina Wrohlich

Evaluating the German "Mini-Job" Reform Using a
True Natural Experiment

Berlin, March 2006



DIW Berlin

German Institute
for Economic Research

Opinions expressed in this paper are those of the author and do not necessarily reflect views of the Institute.

IMPRESSUM
© DIW Berlin, 2006
DIW Berlin
German Institute for Economic Research
Königin-Luise-Str. 5
14195 Berlin
Tel. +49 (30) 897 89-0
Fax +49 (30) 897 89-200
www.diw.de

ISSN print edition 1433-0210
ISSN electronic edition 1619-4535

All rights reserved.
Reproduction and distribution
in any form, also in parts,
requires the express written
permission of DIW Berlin.

Evaluating the German “Mini-Job” Reform Using a True Natural Experiment*

Marco Caliendo[†]

Katharina Wrohlich[‡]

DIW BERLIN

DIW BERLIN

Working Paper

This draft: March 20, 2006

Abstract

Increasing work incentives for people with low incomes is a common topic in the policy debate across European countries. The “Mini-Job” reform in Germany - introduced on April 1, 2003 - can be seen in line with these policies, exempting labour income below a certain threshold from taxes and employees’ social security contributions. We carry out an ex-post evaluation to identify the short-run effects of this reform. Our identification strategy uses an exogenous variation in the interview months in the German Socio-Economic Panel, that allows us to distinguish groups that are (or are not) affected by the reform. To account for seasonal effects we additionally use a difference-in-differences strategy. The results show that the short-run effects of the reform are limited. We find no significant short-run effects for marginal employment. However, there is evidence that single men who are already employed react immediately and increase secondary job holding.

Keywords: Evaluation, Natural Experiment, Difference-in-Differences, Marginal Employment.

JEL Classification: C25, H31, J68

*The authors thank Peter Haan, Benjamin Price, Jürgen Schupp, Viktor Steiner, Arne Uhlendorff and Johannes Ziemendorff as well as seminar participants at DIW for valuable comments. All remaining errors are our own. Financial support of the German Science Foundation (DFG) under the research program “Flexibilisierungspotenziale bei heterogenen Arbeitsmärkten” (project STE 681/5-1) is gratefully acknowledged.

[†]Marco Caliendo is Senior Research Associate at the German Institute for Economic Research (DIW) in Berlin and Research Fellow of the IZA, Bonn and the IAB, Nuremberg, e-mail: mcaliendo@diw.de.

[‡]Katharina Wrohlich is Research Associate at the German Institute for Economic Research (DIW) in Berlin and Research Affiliate of the IZA, Bonn, e-mail: kwrohlich@diw.de.

1 Introduction

As a response to persistently high unemployment rates, especially of low-skilled people, wage subsidies have been intensively discussed in European countries. Following the example of the Earned Income Tax Credit (EITC) introduced in the 70s in the US (see, e.g., Scholz, 1996), several European countries have introduced in-work benefits, tax credits, or subsidies to social security contributions (SSC) for working individuals. Examples are the Working Family Tax Credit (WFTC) in the UK (see, e.g., Blundell, Duncan, McCrae, and Meghir, 2000) and the French *Prime Pour l'Emploi* (see, e.g., Stancanelli, 2005).¹ The “Mini-Job” reform introduced in Germany in 2003 can also be seen in line with these policies. The main objective of this reform is to provide positive work incentives for people with low earnings potential by subsidising social security contributions. The government expected to achieve that goal by exempting labour income up to 400 euros from employees’ SSC and introducing a degressive subsidy for earnings between 401 and 800 euro. To be specific, this reform included three major changes from pre-reform regulations. First, the maximum amount for earnings exempted from SSC was increased from 325 to 400 euros. Jobs with earnings less than this threshold are called mini-jobs. Second, the previous maximum hours restrictions (15 hours per week) was abolished. Third, income up to 400 euros per month from a mini-job hold as a secondary job is now exempted from SSC and income tax.²

The theoretically-expected employment effects of wage subsidies depend on the design of the policy instrument and on various other institutional and economic factors (see, e.g., Blundell, 2000, or Moffit, 2003). The expectation of unambiguously positive effects on labour force participation is based on two conditions. First, the subsidies have to be targeted at individual income rather than household income, and second, the reform has to change the incentives to take up work for recipients of unemployment benefits or other social transfers. The subsidies under the German “Mini-Job” reform are indeed targeted at the individual level; however, the budget constraint for recipients of social transfers hardly changes due to strict withdrawal of earnings, as is shown in Steiner and Wrohlich (2005).

Theoretical predictions about the hours worked, on the other hand, are not so

¹For a detailed overview of recent European “Making Work Pay” policy reforms, see Orsini (2006).

²See Steiner and Wrohlich (2005) for a more detailed description of the reform.

straightforward: for individuals with earnings within the subsidised range it might be optimal to increase working hours (if the substitution effect dominates the income effect), whereas for individuals with earnings slightly above 400 euros, it might be profitable to reduce working hours under the subsidy.³ The total effect on working hours for the population will therefore depend on the distribution of households along the working-hours/income distribution and thus has to be evaluated empirically.

A series of papers has estimated the effects of the 2003 “Mini-Job” reform based on ex-ante simulations with behavioural microsimulation models. They suggest only very moderate participation effects and even negative effects on working hours.⁴ This is in strong contrast to numbers published by official statistics suggesting an additional number of 930,000 jobs created already one month after introduction of the reform.⁵ Hence, an ex-post evaluation is called for. This is an especially difficult task here, since from 2004 onwards various other legal changes have been introduced which might affect labour supply decisions of individuals. Therefore it should be obvious that a comparison of the mini-jobs realised in 2004 with pre-reform numbers will not reveal the true effect of the reform. Furthermore, in contrast to other evaluation studies of labour market policies, the distinction between control and treatment groups is not initially clear, since the reform is relevant for the whole population. A thorough evaluation has to take these points into consideration and should be based on a credible identification strategy. We will do so by using the exogenous variation in the interview date of the German Socio-Economic Panel (SOEP). The interviews are conducted between January and October in each year. Since the reform was introduced on April 1, 2003, we observe some people who are interviewed before the reform and others interviewed after the new legislation was implemented. This allows us to estimate the immediate short-run effect of the reform. To account for seasonal variation, we additionally use a difference-in-differences approach.

We will explain our identification strategy in more detail in Section 2, where we will also describe the data used for the analysis. Section 3 contains the estimation results, before Section 4 concludes.

³The same holds for people just above the 800 euro threshold, who are not analysed here.

⁴See e.g. Steiner and Wrohlich (2005), Arntz, Feil, and Spermann (2003) or Bargain, Caliendo, Haan, and Orsini (2006).

⁵See press-release of the “Mini-Job-Zentrale” from July 18th, 2003: “930,000 neue Jobs durch geringfügig Beschäftigte”.

2 Data and Evaluation Strategy

2.1 Evaluation Design

Our empirical analysis is based on the German Socio-Economic Panel (SOEP), a sample gathering socio-demographic and financial information about 12,000 representative households each year. We will use the waves for the years 2002 and 2003. The individuals are interviewed in person from January until October each year.⁶ Our identification of the treatment effect of the reform will be based on this exogenous variation in the interview month.

As already mentioned we want to evaluate the effects of the reform on some outcome Y , for example, the probability of beginning a mini-job for certain groups of the population. In the usual microeconomic evaluation framework (the “potential outcome approach”, most commonly called the Roy (1951)-Rubin (1974) model), the treatment effect Δ is given by a comparison of the treatment outcome (Y^1) with a hypothetical situation where the same individual does not receive treatment (Y^0), i.e.: $\Delta = Y^1 - Y^0$. The fundamental evaluation problem arises because we can never observe both potential outcomes for the same individual at the same time. A simple comparison between outcomes of treated and untreated individuals is not possible if they are selective groups, that is when the condition $E(Y^0 | D = 1) = E(Y^0 | D = 0)$ does not hold, where D is a binary treatment indicator. Let us transfer this general framework to our evaluation question, before we present our identification strategy.

The “Mini-Job” reform was introduced on April 1, 2003, and applies to the whole population. Hence, we have no direct treatment group which has received the treatment and whose outcome we could compare with a control group who did not receive the treatment. The whole population before April 1, 2003, was not affected by the reform, while the whole population after April 1, 2003, was affected by it. It should also be noted that the whole population was (not) affected by the reform in 2004 (2002). Comparing the outcomes between these two years ($Y_{2004}^1 - Y_{2002}^0$) will not give us the actual treatment effect, since other regulations were also changed. Most significant of these were changes in the income tax as part of the German Tax Reform. From 2003 to 2004, the basic allowance was increased from 7,235 to 7,664 euros per year, the tax

⁶For a detailed description of the data, see Haisken De-New and Frick (2003).

rate of the first tax bracket was reduced from 19.9 to 16.0 percent, and the top tax rate was reduced from 48.5 to 45.0 percent. Clearly, this reform also affected labour supply decisions of individuals with low earnings.⁷

However, the timing of the SOEP interviews gives us an opportunity to identify the true treatment effect. As mentioned above, the SOEP interviews are conducted between January and October of each year. We argue that the random variation of the interviews mimics a true natural experiment, where we can compare the effects for the group of participants, i.e. the people who were interviewed when the reform was already implemented in t_{2003} , with the group of controls, i.e. the people who were interviewed before the reform was implemented in t_{2003} ⁸:

$$\Delta = Y_{2003}^1 - Y_{2003'}^0. \quad (1)$$

Most of the interviews are accomplished within the first quarter. In fact, by default households are contacted by the interviewers in the first quarter of each year. If this contact is not successful, whether because no one is at home or the household has moved to another address, households are contacted again in the second quarter of the year and so on. Since on average most of the post-reform interviews are completed by May 2003⁹, it should also be clear that we are only able to estimate the immediate short-run effects of the reform.

A problem which might arise with this approach are potential differences in unobserved characteristics (UC) between individuals interviewed before April and those interviewed after April as well as seasonal employment effects (SEE). If employment in the mini-job sector varies heavily within a year or if the two groups differ in unobserved characteristics the above-mentioned approach becomes invalid since

$$Y_{2003}^1 - Y_{2003'}^0 = \Delta + SEE + UC. \quad (2)$$

To account for these potential sources of bias, we apply a control mechanism based on the difference-in-differences (DID) approach¹⁰, using the seasonal variation and

⁷For a detailed description and an estimation of labour supply reactions to this tax reform see Haan and Steiner (2005).

⁸The superscript ' behind year information indicates the first quarter of the year, year information without superscript indicates quarters 2-4.

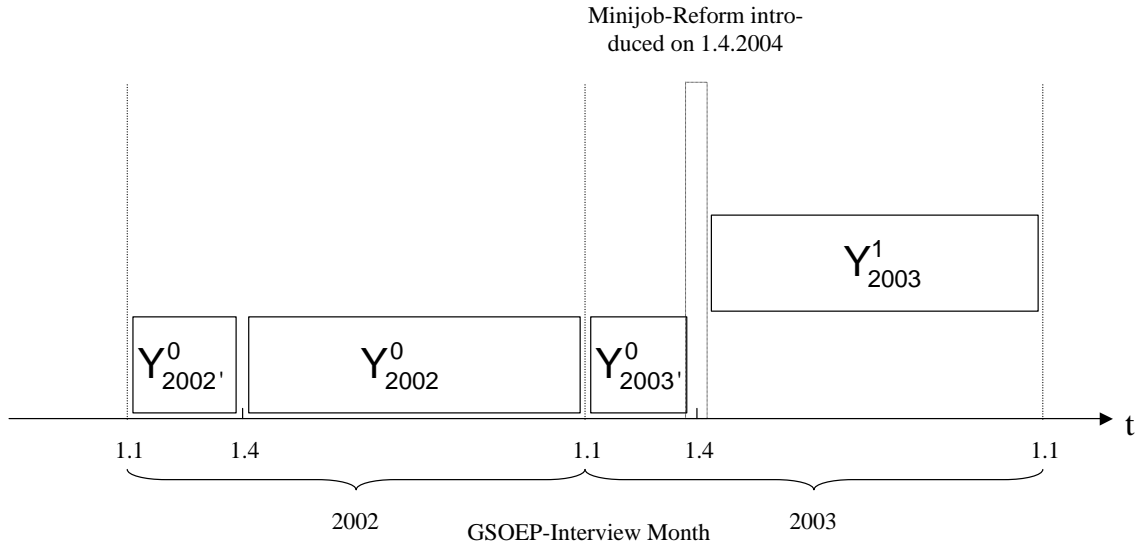
⁹In 2003 80% of interviews were conducted in the first quarter, 8% in April, 5% in May, 4% in June and the rest (3%) between July and October.

¹⁰See, for example, Heckman, LaLonde, and Smith (1999) for an overview.

unobserved differences in the year 2002 to account for the seasonal variation and unobserved differences in 2003. Clearly, this assumption is only valid if both patterns have not changed over the two years, such that $SEE_{2003} = SEE_{2002}$ and $UC_{2003} = UC_{2002}$. The treatment effect is then given by (see also Figure 1):

$$\Delta = (Y_{2003}^1 - Y_{2003'}^0) - (Y_{2002}^0 - Y_{2002'}^0). \quad (3)$$

Figure 1: Definition of the Subsamples According to the SOEP Interview Month



Since we are using cross-sectional information from two waves of the SOEP, the populations in 2002 and 2003 as well as the populations in the first and subsequent quarters might not be the same. To account for variations in observable characteristics, we specify the outcome variable Y in a parametric way and estimate the effect on the whole sample with interaction effects.

The equation we will estimate can be specified as

$$y_i^* = \beta_1 * d2003_i + \beta_2 * after_i + \beta_3 * d2003 \times after_i + \gamma' X_i + \varepsilon_i, \quad (4)$$

where y_i^* is a latent variable such as the propensity to be marginally employed or to hold a secondary job (the outcome variables will be specified in more detail in Section 2.2), $d2003_i$ is a dummy variable indicating whether individual i is observed in 2003, $after$ is a dummy variable indicating whether individual i is observed after the first quarter of a year, and $d2003 \times after$ is an interaction term of these two variables.

Vector X_i summarises control variables such as age, education, family status, number of children, health status, etc., and ε_i is an unobserved error term. The β s and the vector γ include the respective coefficients. We are particularly interested in β_3 , which yields the causal effect of the reform. Since we do not observe the latent variable y_i^* , but the binary outcome variables y_i , we will estimate equation (4) using a probit model. The marginal effect corresponding to the coefficient β_3 can thus be interpreted as change in the probability of the outcome variable (e.g. marginal employment) due to the reform.

2.2 Definition of Outcome Variables and Subgroups

We are interested in two outcome variables, namely the probability of being in marginal employment (“geringfügige Beschäftigung”) and the probability of having a secondary job (“Nebenerwerbstätigkeit”), since the incentives to take up these two types of jobs have been changed by the “Mini-Job” reform. This is an extension of previous ex-ante evaluation studies that did not analyse the effects on secondary job holding. However, there is evidence that the strong increase in mini-jobs after the introduction of the reform were in fact not jobs taken up by individuals who were previously not employed, but secondary jobs of people who had already been working.¹¹ Thus, in addition to the analysis of marginal employment, we are particularly interested in the effect of the reform on secondary job holding.

In the SOEP data, there are several questions containing information about employment status, working hours, earnings, and job characteristics. One drawback of the interview-based SOEP, as with any kind of self-reported data, is that there are some inconsistencies in answers to these questions. For example, some individuals state that they are part-time employed (instead of marginally employed) but have earnings less than 325 or 400 euro, which would classify them as marginally employed. Other individuals report not working when asked for their employment status, but state later that they receive earnings from a secondary job. Therefore, we decided on the following definition of “being marginally employed”. An individual is defined to be in marginal employment if

- the answer to employment status is “marginally employed”, or

¹¹For details see Bundesagentur für Arbeit (2003).

- the answer to employment status is “part-time employed” and gross monthly earnings are reported to be less than 400 euro, or
- the answer to job characteristics is “this job is a 325 euro/400 euro Job”, or
- the answer to employment status is “not working” and the individual reports having a secondary job with gross monthly earnings less than 400 euro.

Note that we use the post-reform threshold of 400 euro for all individuals (interviewed before and after the reform), since we are not interested in redefinitions of already existing jobs.

Similar problems arise with respect to the definition of secondary job holding. As explained above, we observe individuals with a secondary job but classifying themselves as non-working at the same time. Our definition of having a secondary job is as follows. An individual has a secondary job if

- the answer to employment status is “full-time employed” or “part-time employed” and the individual reports gross monthly earnings from regular or irregular secondary jobs less than 400 euro.

Table 1 shows the total number of observations in the four subsamples, interviewed before and after April 1st in 2002 and 2003, respectively. To analyse the changes with respect to marginal employment we look at the whole population, whereas we focus on individuals who are full- or part-time employed to analyse secondary job-holding. For both analyses we focus on individuals aged between 16 and 70 years.

Results of several ex-ante evaluation studies (see Steiner and Wrohlich, 2005, or Bargain, Caliendo, Haan, and, Orsini, 2005) have shown that the reaction to the reform differs between population subgroups. For example, women in couple households have been shown to adjust their labour supply more than men. Therefore, we differentiate between several subgroups in our analysis. In particular, we run separate estimations for men and women. Furthermore, we analyse the effect on the labour supply of students, which has not been studied in earlier evaluation studies.

Table 1 also shows the total number of observations marginally employed or holding a secondary job. We see that marginal employment is more prevalent among

Table 1: Number of Observations, Marginal and Secondary Employment in the Subsamples

Group	Subsample (Interviewed in...)			Marginal Employment			Secondary Employment		
				Obs.	Abs.	Share	Obs.	abs.	Share
Men	2002	before	April 1st	7007	267	0.0381	4372	160	0.0366
		after		2240	91	0.0406	1530	59	0.0386
	2003	before	April 1st	7102	313	0.0441	4367	168	0.0385
		after		1805	99	0.0548	1199	53	0.0442
Women	2002	before	April 1st	7366	872	0.1184	3422	119	0.0348
		after		2405	352	0.1464	1178	43	0.0365
	2003	before	April 1st	7527	1020	0.1355	3494	118	0.0338
		after		1917	307	0.1601	956	35	0.0366

Note: The high income sample of the SOEP is not included, since this entire group was interviewed after April in the 2003 wave. Numbers for marginal employment refer to the whole population between 16 and 70 years old. Numbers for secondary employment refer to the whole population holding a full- or part-time job (between 16 and 70 years old).

Source: SOEP, waves 2002 and 2003.

women than men, whereas the opposite is true for secondary job holding. Additionally, marginal employment is higher in the second and third quarter of both years when compared to the first quarter. Furthermore, it is interesting to note that we observe an increase in marginal employment from the first quarter of 2002 to the first quarter of 2003.

Before we turn to the estimation results, we look at some descriptors of the covariates used in the estimations. Thereby we differentiate the four subsamples under consideration. Note that - as already discussed - even if the “after” groups differ systematically from the “before” groups with respect to unobservable characteristics, this does not flaw our results as long as we assume that the differences are the same in 2002 and 2003. The only assumption that needs to be valid for our study is that the interview month of the 2003 wave is independent of the introduction of the “Mini-Job” reform on April 1, 2003.

As can be seen from Table 1 above, there are far fewer observations in the two subsamples “interviewed after April 1st”, which is due to the interview routine of the SOEP already described. Therefore, the group of individuals interviewed after April 1st might be systematically different from those interviewed in the first three months. However, Table 2 shows that the subsamples do not differ with respect to most observable characteristics. One exception is the regional origin of the individuals.

Table 2: Some Descriptive Statistics - Differentiated by Interview Date

	2002		2003	
	Before	After	Before	After
	April 1st	April 1st	April 1st	April 1st
Age	43.62	41.22	43.64	41.61
Female	0.512	0.518	0.515	0.515
No education	0.017	0.026	0.018	0.024
High School Degree	0.201	0.240	0.207	0.243
Vocational Training	0.658	0.618	0.657	0.615
Academic	0.170	0.191	0.172	0.186
Disabled	0.096	0.074	0.095	0.078
Married	0.640	0.613	0.623	0.625
Single	0.170	0.183	0.175	0.178
German	0.919	0.864	0.923	0.862
Living in East Germany	0.285	0.139	0.290	0.105
Children under 15	0.329	0.357	0.318	0.361

Source: SOEP, waves 2002 and 2003.

East Germans are far less represented in the “after” samples (less than 15 percent of all households) than in the “before” samples (approximately 30 percent).¹² Moreover, there are also small differences in age and education, individuals in the “after” sample being younger and better educated. What is important for our set-up, however, is that the differences between the “after” and “before” samples are very similar for the two years.

3 Results

We run separate estimations on different subgroups and outcome variables. Since we expect different effects for men and women, we perform separate analyses for these two groups. In addition, we investigate whether the reform has impacts on the labour supply of students. This group was even more affected by the reform, because the SSC-exemption threshold is at the same time the exemption limit of earned income for recipients of student aid.

For men and women, we run estimations for two outcome variables, namely marginal employment (Section 3.1) and secondary job holding (Section 3.2). For students we combine these two outcome variables into one due to the limited number of observa-

¹²Given the differences in this variable, we have also performed the analysis separately for East and West Germany, which did not yield different results as the ones we will present in the next section.

tions (3.3). For all subgroups and outcome variables, we run three probit estimations, respectively. The first estimation is only for the year 2003, and includes a dummy indicating “interviewed after April 1st” as a single explanatory variable. This corresponds to the “raw” effect of the reform, without controlling for potential seasonal effects or possible differences in observable or unobservable characteristics between individuals interviewed in and after the first quarter of 2003. In the second estimation, we control for differences in observable characteristics by including a set of control variables such as age, educational variables, regional variables, marital status, and number of children. Finally, in the third estimation we pool data from 2002 and 2003, and include two more variables, a dummy indicating the year 2003 and an interaction term between this year dummy and the “interviewed after April 1st” dummy (see equation 4). Note that this variable measures the effect that the “Mini-Job” reform had on the outcome variable, controlling for seasonal effects, observable and unobservable characteristics that differ between the groups interviewed in and after the first quarter of each year.

3.1 Effects on Marginal Employment

Table 3 shows a short summary of the estimation results for marginal employment, where we have displayed marginal effects. Full estimation results, including the coefficients and standard errors of the control variables, can be found in Table A.1 in the Appendix. For men, the first model indicates that the “raw” effect of the reform is positive, i.e. in the second and third quarter of 2003 we observe more men in marginal employment than in the first quarter of 2003. This is still true if we control for differences in observable characteristics, as can be seen from column 2. The third column shows the estimation of the pooled sample of 2002 and 2003 including a dummy variable indicating the year 2003, and an interaction term of this dummy with the dummy indicating “interviewed after the reform”. We further interacted this variable with the “single” dummy, because ex-ante studies have shown different reactions to the reform by singles and individuals living with a partner. Doing so allows us to calculate marginal effects for singles and couples separately.¹³ As our results show, as

¹³The marginal effect for individuals living in couples is computed as $\Phi(\widehat{\beta}_{after} + \widehat{\beta}_{2003} + \widehat{\beta}_{after2003} + \widehat{\gamma}'\bar{X}) - \Phi(\widehat{\beta}_{after} + \widehat{\gamma}'\bar{X}) - \Phi(\widehat{\beta}_{2003} + \widehat{\gamma}'\bar{X}) + \Phi(\widehat{\gamma}'\bar{X})$ where Φ is the cdf of the normal distribution. For singles, the marginal effect corresponds to $\Phi(\widehat{\beta}_{after} + \widehat{\beta}_{2003} + \widehat{\beta}_{single} + \widehat{\beta}_{after2003} + \widehat{\beta}_{after2003single} + \widehat{\gamma}'\bar{X}) - \Phi(\widehat{\beta}_{after} + \widehat{\beta}_{single} + \widehat{\gamma}'\bar{X}) - \Phi(\widehat{\beta}_{2003} + \widehat{\beta}_{single} + \widehat{\gamma}'\bar{X}) + \Phi(\widehat{\beta}_{single} + \widehat{\gamma}'\bar{X})$. The corresponding standard errors are calculated using the Delta method.

far as the probability of being marginally employed is concerned, neither single men nor men living in couples react to the reform.

Table 3: Estimation Results - Marginal Employment

Variable	Men			Women		
	Model 1 Marg.Eff.	Model 2 Marg.Eff.	Model 3 Marg.Eff.	Model 1 Marg.Eff.	Model 2 Marg.Eff.	Model 3 Marg.Eff.
after	0.0108* (0.0059)	0.0132** (0.0055)	0.0036 (0.0045)	0.0246*** (0.0093)	0.0062 (0.0087)	0.0171** (0.0080)
d2003			0.0045* (0.0024)			0.0195*** (0.0041)
after×2003 (Couples)			0.0090 (0.0067)			-0.0092 (0.0108)
after×2003 (Singles)			0.0145 (0.0135)			-0.0158 (0.0247)
Controlled for Covariates	no	yes	yes	no	yes	yes
Log-Likelihood	-1666.813	-1529.5	-2919.3	-3829.5	-3664.7	-7183.4
Observations	8,907	8,907	18,154	9,444	9,444	19,215

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) correct for correlation across repeated observations of individuals.

Covariates include: age, age², no education, high school degree, vocational training, academic, disabled, married, single, german, number of children in different age classes, and a dummy for living in East Germany. See also Table A.1.

Source: Estimations based on SOEP, waves 2002 and 2003.

Similar to what we observe for men, we find a positive and significant “raw” effect of the reform for women (see column 4). This effect disappears, however, if we control for socio-demographics (column 5) and for differences in seasonal employment effects and unobservable characteristics between the “before” and “after” samples (column 6). Note that in the full model presented in column 6, the coefficients of the variables *after* and *d2003* are positive and significant, indicating that for women the probability of being marginally employed is higher in the second and third quarter of each year, and that this probability is also higher in 2003. However, there is no causal effect of the reform, which would be caught by the effect of the variables *after* × 2003 and *after* × 2003 × *single* (see Table A.1 in the Appendix).

Thus, our first conclusion is that in the short-run (defined as about two months after the reform), there has not been a significant change in marginal employment that could be causally related to the legislation introduced on April 1st, 2003. However, at least for women, marginal employment seems to be higher in the summer months than in winter and higher in 2003 than in 2002.

3.2 Effects on Secondary Job Holding

Let us now turn to the analysis of the probability of holding a secondary job. As already explained above, for these estimations we focus on the sample of full-time or part-time employed individuals only. As Table 4 (column 1) shows, there seems to be no significant “raw” effect of the reform for men, as the share of men holding a secondary job does not differ between the first and the subsequent quarters of 2003.¹⁴ This is also true if we control for differences in observed characteristics (column 2) and for differences in seasonal employment effects and unobserved characteristics (column 3).

Table 4: Estimation Results - Secondary Employment

Variable	Men			Women		
	Model 1 Marg.Eff.	Model 2 Marg.Eff.	Model 3 Marg.Eff.	Model 1 Marg.Eff.	Model 2 Marg.Eff.	Model 3 Marg.Eff.
after	0.0058 (0.0066)	0.0051 (0.0062)	0.0012 (0.0054)	0.0030 (0.0068)	0.0004 (0.0062)	0.0001 (0.0057)
d2003			0.0017 (0.0030)			-0.0009 (0.0033)
after×2003 (Couples)			-0.0015 (0.0072)			0.0017 (0.0087)
after×2003 (Singles)			0.0311 (0.0198)			-0.0023 (0.0207)
Controlled for Covariates	no	yes	yes	no	yes	yes
Log-Likelihood	-929.1	-891.6	-1794.2	-665.8	-644.4	-1324.8
Observations	5,564	5,564	11,466	4,447	4,447	9,047

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) correct for correlation across repeated observations of individuals.

Covariates include: age, age², no education, high school degree, vocational training, academic, disabled, married, single, german, number of children in different age classes, a dummy for living in East Germany, industry class, full-time employment dummy, and overtime. See also Table A.2.

Source: Estimations based on SOEP, waves 2002 and 2003.

However, as the results of this estimation show (see Table A.2 in the Appendix), we do find a positive effect for single men that is significant at the 10 percent level. The marginal effect corresponding to this coefficient amounts to 0.031. This implies that for single men, the probability of having a secondary job increases by 3.1 percentage points. Since the probability of holding a secondary job before the reform for single men is 3.7 percent, this effect almost implies a doubling of secondary employment in this group. However, the standard error of the marginal effect amounts to 0.0198.¹⁵

¹⁴Full estimation results can be found in Table A.2 in the Appendix.

¹⁵Statistical (non)significance of the estimated coefficient of the interaction term does not neces-

The marginal effect is thus not significant at the 10 percent level, the empirical significance level amounting to 11.5%. Given the economic significance of the effect and the relatively limited number of observations, we would not conclude from the standard error that the reform did not affect this group, but rather that there is evidence for a positive effect on secondary employment of single men. As columns 4 to 6 of Table 4 show, we do not find a corresponding effect for women.

3.3 Effects for Students

The estimation results for students can be found in Table 5.¹⁶

Table 5: Estimation Results - Marginal and/or Secondary Employment for Students

Variable	Students		
	Model 1 Marg.Eff.	Model 2 Marg.Eff.	Model 3 Marg.Eff.
after	0.0639*** (0.0220)	0.0500** (0.0220)	0.0465** (0.0193)
d2003			0.0379*** (0.0112)
after×2003			0.0042 (0.0267)
Controlled for Covariates	no	yes	yes
Log-Likelihood	-1161.7	-1134.1	-2197.5
Observations	2,295	2,295	4,703

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) correct for correlation across repeated observations of individuals.

Covariates include: age, age², no education, high school degree, vocational training, academic, disabled, married, single, german, number of children in different age classes, and a dummy for living in East Germany. See also Table A.4.

Source: Estimations based on SOEP, waves 2002 and 2003.

Similar to what we found for women with respect to marginal employment, for students there is a positive and significant “raw” effect of the reform, in that students in the second and third quarter of 2003 are more likely to be observed in marginal employment or holding a secondary job than in the first quarter of 2003. This is sarily imply (non)significance of the marginal effect of this variable in non-linear models (see Ai and Norton, 2003).

¹⁶Table A.3 in the Appendix contains the total number of observations for this group as well as the numbers on being marginal employed and/or holding a secondary job. Due to the limited number of observations we pooled male and female observations and included a control variable for gender. Full estimation results can be found in Table A.4 in the Appendix.

still true once we control for socio-demographic characteristics. The difference-in-differences model, however, shows that there is no causal effect of the reform, even though the probability of being marginally employed or holding a secondary job is higher in 2003 and in the second and third quarter of each year.¹⁷

To sum up, we find that in the short run, there is evidence that the reform had a causal effect for single men, whose probability of having a secondary job increases by about three percent. According to our estimation results, the reform had no causal effect on marginal employment in any of the subgroups.

4 Conclusions

The aim of this paper was to evaluate the causal effect of the German “Mini-Job” reform from 2003 on the probabilities of being in marginal employment or of having a secondary job. Based on our identification strategy, we were able to identify the short-run effects of the reform. We could not find a significant effect on the probability of being marginally employed for any subgroup. However, we found evidence that the probability of having a secondary job increases for single men.

All ex-ante evaluation studies using behavioural microsimulation models predict similar effects from the “Mini-Job” reform. They find a small yet significant effect on the labour force participation of women living in couple households. As described above, we do not find a significant effect on the participation in marginal employment in the short run. However, since the effects that are calculated with ex-ante microsimulation techniques correspond to long-term effects, our results need not necessarily be a contradiction to this literature. The effect of the “Mini-Job” reform on students, as well as on secondary job holding has not been analysed so far. However, as we show, secondary job holding is the only outcome variable for which we find any short-run effect, at least for the group of single men.

The numbers published by official sources portrayed the “Mini-Job” reform as quite successful in generating new employment. The Federal Ministry of Health and Social Affairs stated in July 2003 that three months after the reform, 930,000 new jobs had been created. These numbers were corrected by the Federal Employment Agency in

¹⁷We also ran the same model including the variable $after2003 \times single$, which did not change the results.

November 2003, who stated that one month after the reform, there was an increase in marginal employment of as high as 79,000 individuals and an increase of secondary jobs by 580,000. As stated above, we did not find any significant effects on marginal employment. This could be due to various reasons. First, while we showed that for women and students marginal employment is higher in the summer months and in 2003, our results show that there is no significant *causal* effect of the reform. Thus, our conclusion is that the immediate increase in marginal employment of 79,000 jobs cannot be causally related to the reform. As far as secondary jobs are concerned, our results differ to a much larger extent from the numbers published by the Federal Employment Agency. We only find evidence for a positive reaction to the reform among single men, whose probability of having a secondary job increases. However, this can not explain the total increase of 580,000 secondary jobs stated above. We believe that a large fraction of these “new” jobs are actually redefinitions of previously fake self-employment. This effect cannot be identified with the SOEP data. The same is true for turning illegal jobs into legal employment (see also Schupp and Birkner, 2004).

Thus, we conclude that in the short-run, the reform had a very limited *causal* impact on the labour supply in Germany. The high numbers that circulated in the press in the first months after the reform were to a great extent referring to (i) additional jobs that have been created not due to the reform but to seasonal employment effects and the general trend of increasing marginal employment and (ii) to redefinitions of already existing jobs or the turning of illegal jobs into legal employment.

References

- AI, C., AND E. NORTON (2003): "Interaction terms in logit and probit models," *Economics Letters*, 80, 123–129.
- ARNTZ, M., M. FEIL, AND A. SPERMANN (2003): "Die Arbeitsangebotseffekte der neuen Mini- und Midijobs - eine Ex-Ante Evaluation," *Mitteilungen aus der Arbeitsmarkt- und Berufsforschung*, 3/2003.
- BARGAIN, O., M. CALIENDO, P. HAAN, AND K. ORSINI (2006): "Making Work Pay' in a Rationed Labour Market," Discussion Paper No. 2033, IZA Bonn.
- BLUNDELL, R. (2000): "Work Initiatives and 'in-work' Benefit Reforms: A Review," *Oxford Review of Economic Policy*, 16, 27–44.
- BLUNDELL, R., A. DUNCAN, J. MCCRAE, AND C. MEGHIR (2000): "The Labour Market impact of the Working Families Tax Credit," *Fiscal Studies*, 21(1), 75–104.
- BUNDESAGENTUR FÜR ARBEIT (2003): "Erste statistische Ergebnisse ueber Minijobs nach der gesetzlichen Neuregelung zum 1. April 2003," Press release november 2003, Bundesanstalt fuer Arbeit.
- HAAN, P., AND V. STEINER (2005): "Distributional Effects of the German Tax Reform 2000 - A Behavioral Microsimulation Analysis," *Journal of Applied Social Science Studies*, 125, 39–49.
- HAIKEN DE-NEW, J., AND J. FRICK (2003): *Desktop Compendium to The German Socio-Economic Panel Study (SOEP)*. DIW, Berlin.
- HECKMAN, J., R. LALONDE, AND J. SMITH (1999): "The Economics and Econometrics of Active Labor Market Programs," in *Handbook of Labor Economics Vol.III*, ed. by O. Ashenfelter, and D. Card, pp. 1865–2097. Elsevier, Amsterdam.
- MOFFITT, R. (2003): "Welfare Program and Labor Supply," Working Paper 9168, NBER.
- ORSINI, K. (2006): "Tax-Benefit Reform and the Labor Market: Evidence from Belgium and other EU countries," Mimeo.
- ROY, A. (1951): "Some Thoughts on the Distribution of Earnings," *Oxford Economic Papers*, 3(2), 135–145.
- RUBIN, D. (1974): "Estimating Causal Effects to Treatments in Randomised and Nonrandomised Studies," *Journal of Educational Psychology*, 66, 688–701.
- SCHOLZ, J. K. (1996): "In-Work Benefits in the United States: The Earned Income Tax Credit," *The Economic Journal*, 106, 156–169.
- SCHUPP, J., AND E. BIRKNER (2004): "Kleine Beschäftigungsverhältnisse: Kein Jobwunder," Wochenbericht No. 34/2004, DIW, Berlin.
- STANCANELLI, E. (2005): "Evaluating the Impact of the French Tax Credit Programme, "La Prime Pour L'Emploi": A Difference in Difference Model," Working Paper, OFCE.
- STEINER, V., AND K. WROHLICH (2005): "Work Incentives and Labour Supply Effects of the Mini-Jobs Reform in Germany," *Empirica*, 32, 91–116.

Appendix - Tables

Table A.1: Estimation Results - Marginal Employment - Full Model ¹

Variable	Men			Women		
	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.
after	0.106*	0.148**	0.05	0.107***	0.029	0.083**
	(0.055)	(0.059)	(0.058)	(0.039)	(0.04)	(0.038)
d2003			0.062*			0.097***
			(0.032)			(0.020)
after×2003			0.095			-0.05
			(0.081)			(0.051)
after×2003×single			0.029			-0.021
			(0.132)			(0.099)
age		-0.121***	-0.119***		0.025***	0.018**
		(0.011)	(0.009)		(0.009)	(0.007)
age squared		0.001***	0.001***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
no education		-0.293	-0.225		-0.202	-0.215*
		(0.228)	(0.163)		(0.131)	(0.120)
high-school degree		0.392***	0.349***		0.067	0.021
		(0.065)	(0.057)		(0.048)	(0.043)
vocational training		-0.026	-0.057		-0.156***	-0.163***
		(0.062)	(0.049)		(0.039)	(0.034)
academic		-0.221***	-0.258***		-0.358***	-0.364***
		(0.080)	(0.075)		(0.058)	(0.054)
disabled		-0.029	0.041		-0.307***	-0.234***
		(0.084)	(0.073)		(0.075)	(0.06)
single		-0.006	-0.022		-0.013	0.01
		(0.073)	(0.062)		(0.055)	(0.047)
married		-0.222***	-0.206***		0.195***	0.220***
		(0.079)	(0.063)		(0.053)	(0.043)
german		-0.033	0.014		0.184***	0.219***
		(0.093)	(0.077)		(0.062)	(0.055)
children under 15		-0.033	-0.056			
		(0.065)	(0.052)			
children under 1					-0.234**	-0.199***
					(0.099)	(0.074)
children under 7					0.069	0.058
					(0.048)	(0.039)
children between 8-15					0.185***	0.202***
					(0.040)	(0.033)
east german		0.039	0.032		-0.309***	-0.344***
		(0.057)	(0.049)		(0.043)	(0.037)
constant	-1.705***	0.598***	0.486***	-1.101***	-1.493***	-1.495***
	(0.026)	(0.218)	(0.182)	(0.018)	(0.168)	(0.139)
Log-Likelihood	-1666.8	-1529.5	-2919.3	-3829.5	-3664.7	-7183.4
Observations	8,907	8,907	18,154	9,444	9,444	19,215

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) are corrected for correlation across repeated observations of individuals.

All variables except age and age squared are dummy variables, taking the value 1 if the condition is fulfilled.

Source: Estimations based on SOEP, waves 2002 and 2003.

Table A.2: Estimation Results - Secondary Employment - Full Model ¹

Variable	Men			Women		
	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.
after	0.066 (0.072)	0.065 (0.076)	0.015 (0.071)	0.038 (0.086)	0.006 (0.090)	0.001 (0.081)
d2003			0.023 (0.039)			-0.012 (0.047)
after×2003			-0.02 (0.096)			0.024 (0.122)
after×2003×single			0.320* (0.172)			-0.041 (0.179)
age		0.02 (0.025)	0.043** (0.021)		0.001 (0.027)	0.003 (0.020)
age squared		-0.000 (0.000)	-0.001** (0.000)		-0.000 (0.000)	-0.000 (0.000)
no education		-0.058 (0.415)	-0.404 (0.387)		0.506* (0.294)	0.38 (0.251)
vocational training		0.234*** (0.087)	0.113 (0.07)		-0.013 (0.092)	-0.059 (0.074)
academic		0.108 (0.084)	0.091 (0.07)		0.185* (0.099)	0.195** (0.081)
disabled		0.067 (0.144)	0.072 (0.116)		-0.154 (0.203)	0.046 (0.140)
married		0.131 (0.098)	0.077 (0.082)		-0.124 (0.107)	-0.038 (0.085)
single		0.117 (0.103)	-0.028 (0.096)		0.163 (0.107)	0.242*** (0.083)
german		0.164 (0.133)	0.166 (0.112)		-0.096 (0.143)	0.027 (0.120)
children under 15		-0.08 (0.079)	-0.059 (0.065)			
children under 1					-0.072 (0.123)	-0.100 (0.097)
children between 8-15					-0.137 (0.098)	-0.108 (0.075)
east german		-0.121 (0.082)	-0.142** (0.070)		-0.156 (0.097)	-0.122 (0.076)
civil servant		0.119 (0.116)	0.084 (0.099)		-0.320* (0.180)	-0.320** (0.134)
self-employed		-0.377*** (0.138)	-0.293*** (0.110)		-0.18 (0.184)	-0.255 (0.163)
industry class. 2		-0.107 (0.108)	-0.057 (0.093)		0.046 (0.176)	-0.066 (0.166)
industry class. 3		0.031 (0.122)	0.124 (0.099)		-0.101 (0.142)	-0.148 (0.112)
industry class. 4		-0.181 (0.160)	-0.165 (0.126)		-0.326 (0.296)	-0.264 (0.248)
industry class. 5		0.184** (0.093)	0.231*** (0.079)		0.169 (0.107)	0.134 (0.090)
industry class. 6		0.109 (0.119)	0.196** (0.099)		-0.112 (0.155)	-0.020 (0.116)
industry class. 7		0.16 (0.139)	0.139 (0.115)		0.275* (0.151)	0.257** (0.124)

Continued on next page.

Table A.2 continued.

Variable	Men			Women		
	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.
overtime ($< 3h$)		-0.056 (0.087)	0.027 (0.067)		0.105 (0.087)	0.123* (0.064)
overtime ($\geq 3h$)		0.147** (0.075)	0.163*** (0.059)		-0.024 (0.109)	0.007 (0.077)
full-time employed		-0.507*** (0.140)	-0.560*** (0.109)		-0.069 (0.085)	-0.148** (0.068)
constant	-1.769*** (0.035)	-1.930*** (0.502)	-2.348*** (0.402)	-1.828*** (0.041)	-1.693*** (0.532)	-1.797*** (0.397)
Log-Likelihood	-929.1	-891.6	-1794.2	-665.8	-644.4	-1324.8
Observations	5564	5564	11466	4447	4447	9047

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) are corrected for correlation across repeated observations of individuals.

All variables except age and age squared are dummy variables, taking the value 1 if the condition is fulfilled.

Source: Estimations based on SOEP, waves 2002 and 2003.

Table A.3: Number of Observations, Marginal and/or Secondary Employment for Students in the Subsamples¹

Subsample			Marg. or Secon. Employment		
			Obs.	abs.	in %
2002	before	April 1st	1778	274	0.0574
	after		630	132	0.0873
2003	before	April 1st	1819	350	0.0808
	after		476	122	0.1324

Note: High income sample of the SOEP is not included, since this entire group was interviewed after April in the 2003 wave. Numbers refer to the population in “Ausbildung”.

Source: SOEP, waves 2002 and 2003.

Table A.4: Estimation Results - Marginal Employment - Full Model ¹

Variable	Students			Pensioner		
	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.	Model 1 Coef.	Model 2 Coef.	Model 3 Coef.
after	0.214*** (0.071)	0.160** (0.073)	0.159** (0.068)	0.206** (0.093)	0.142 (0.096)	-0.041 (0.098)
d2003			0.145*** (0.043)			0.034 (0.042)
after×2003			-0.005 (0.093)			0.179 (0.123)
age		0.060*** (0.021)	0.048*** (0.018)		0.001 (0.047)	0.023 (0.045)
female		0.204*** (0.061)	0.229*** (0.049)		-0.151* (0.078)	-0.149** (0.067)
age squared		-0.001** (0.000)	-0.001** (0.000)		-0.000 (0.000)	-0.000 (0.000)
no education					-0.168 (0.281)	-0.204 (0.263)
high-school degree					0.007 (0.137)	0.000 (0.119)
vocational training		-0.239** (0.096)	-0.247*** (0.077)		0.168* (0.092)	0.148* (0.076)
academic					0.222* (0.122)	0.131 (0.107)
disabled		-0.514 (0.314)	-0.331 (0.207)		-0.232*** (0.086)	-0.217*** (0.074)
single		0.033 (0.075)	0.083 (0.060)		-0.159 (0.153)	-0.011 (0.138)
married		-0.388*** (0.136)	-0.351*** (0.105)		-0.148 (0.141)	-0.028 (0.131)
german		0.283** (0.126)	0.178* (0.102)		0.145 (0.179)	0.257* (0.155)
children under 15		-0.134* (0.071)	-0.06 (0.055)		-0.275 (0.270)	-0.144 (0.171)
east german		-0.224*** (0.071)	-0.246*** (0.058)		-0.279*** (0.088)	-0.314*** (0.075)
constant	-0.869*** (0.034)	-2.025*** (0.342)	-1.889*** (0.285)	-1.523*** (0.040)	-0.818 (1.283)	-1.519 (1.264)
Log-Likelihood	-1161.7	-1134.1	-2197.5	-701.6	-683.9	-1319.9
Observations	2,295	2,295	4,703	2,821	2,821	5,671

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) are corrected for correlation across repeated observations of individuals.

All variables except age and age squared are dummy variables, taking the value 1 if the condition is fulfilled.

Source: Estimations based on SOEP, waves 2002 and 2003.