DISCUSSION PAPER SERIES

IZA DP No. 8803

# Do Changes in Regulation Affect Temporary Agency Workers' Job Satisfaction?

Henna Busk
Elke J. Jahn
Christine Singer

January 2015

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

# Do Changes in Regulation Affect Temporary Agency Workers' Job Satisfaction?

**Henna Busk**
*University of Jyväskylä*

**Elke J. Jahn**
*Institute for Employment Research (IAB), University of Bayreuth and IZA*

**Christine Singer**
*Institute for Employment Research (IAB) and University of Erlangen-Nuremberg*

Discussion Paper No. 8803
January 2015

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 8803
January 2015

# ABSTRACT

## Do Changes in Regulation Affect Temporary Agency Workers' Job Satisfaction?*

This paper evaluates the impact on temporary agency workers' job satisfaction of a reform that considerably changed regulations covering the temporary help service sector in Germany. We isolate the causal effect of this reform by combining a difference-in-difference and matching approach and using rich survey data. We find that the change of the law substantially decreased agency workers' job satisfaction while regular workers' job satisfaction remained unchanged. Further analysis reveals that the negative effect on agency workers' job satisfaction can be attributed to a decrease in wages and an increase in perceived job insecurity. These results are also robust to the use of different specifications and placebo tests.

JEL Classification: J28, J41, J88

Keywords: temporary agency employment, deregulation, job satisfaction

Corresponding author:

Christine Singer
Institute for Employment Research (IAB)
Regensburger Strasse 104
D-90478 Nuremberg
Germany
E-mail: christine.singer@iab.de

* We would like to thank Petri Böckerman, Kristian Koerselman, Michael Oberfichtner, and Gesine Stephan for their helpful suggestions. We further appreciate comments received from participants at the 2014 EEA meeting, the 2014 EALE meeting and research seminars at the University of Jyväskylä.

# 1 Introduction

In most European countries, the use of temporary agency employment has been eased over the past two decades, while regulations of permanent contracts were essentially left unchanged (OECD 2013).[1] It therefore comes as no surprise that the temporary help service sector has shown impressive growth throughout Europe. This is one reason why agency employment has become central to the policy debate about increasing labor market flexibility. Specifically, there are concerns that dual labor markets might emerge due to the spot market nature and the poor working conditions of agency jobs (e.g., Boeri 2011, Jahn et al. 2012). Indeed, there is ample evidence that the average wage of agency workers usually lags behind that of permanent staff (e.g., Böheim and Cardoso 2009, Hamersma et al. 2014, Jahn 2010, Segal and Sullivan 1998), they face higher unemployment risks (Antoni and Jahn 2009, Houseman et al. 2009, Autor and Houseman 2010), they have less access to training (Nienhüser and Matiaske 2006), and they are in more work-related accidents (Garcia-Serrano et al. 2011).

To judge whether flexible employment forms in general are favorable compared with other contractual arrangements, the literature increasingly relies on job satisfaction as an aggregate measure for how workers value various job characteristics. The advantage of this measure is that it not only reflects satisfaction with objective working conditions such as job stability or wages, which are common to all contractual arrangements, but it also contains assessments on unobservable or unmeasurable job characteristics such as the importance of inclusion in the work environment (Hamermesh 2001, de Graaf-Zijl 2012, Clark 2001).

The contribution of our paper is to combine two strands of the literature on temporary agency employment. We first draw upon studies investigating the job satisfaction of agency workers, and second, we follow the literature that aims to evaluate the effects of regulations of flexible employment forms. More precisely, we analyze the influence on male agency workers' job satisfaction of a German reform in 2003 that considerably changed regulations regarding the use of temporary agency workers.

The relationship between job satisfaction and flexible job arrangements in general has been the subject of numerous previous studies (for a meta study, see Wilkin 2013). Flexible job arrangements cover a broad set of employment forms, including contingent employment, temporary agency employment, on-call work, fixed-term employment, seasonal jobs, and casual work. Overall, the

[1] To ease readability, the terms 'temp job' and 'agency employment' are used as synonyms for 'temporary agency employment', 'temps' or 'agency workers' are used instead of 'temporary agency workers', and 'perms' is used instead of 'regular workers'.

literature shows that workers in flexible job arrangements are less satisfied than workers employed in regular jobs, although there is some heterogeneity depending on the type of contractual arrangement. For example, recent studies have shown that workers hired on fixed-term contracts seem to be as satisfied with their jobs as regular workers (e.g., D'Addio et al. 2007, Bardasi and Francesconi 2004, Boeri and Garibaldi 2009, Chadi and Hetschko 2013, Origo and Pagani 2009).

So far, only few studies focus solely on the job satisfaction of temporary agency workers. Those studies consistently find that agency workers experience lower job satisfaction compared with regular workers (Buddelmeyer et al. 2013, de Graaf-Zijl 2012, Grund et al. 2014, Green and Heywood 2011, Wooden and Warren 2004). Much of this difference can be attributed to the content of the job, working non-standard hours, and job insecurity. Investigating the source of job insecurity is of particular importance and requires further investigation because recent evidence shows that perceived job security rather than contractual job insecurity is the driving factor behind this result (Jahn 2013).

Knowledge about how the regulation of the temporary agency sector affects temp workers' job satisfaction and their working conditions is particularly important as national governments increasingly begin to re-regulate this employment form. So far, studies analyzing the consequences of changing the regulations of temporary agency employment have focused on objective outcomes such as employment duration (Antoni and Jahn 2009), the demand for temp workers (Jahn and Bentzen 2012), and the pay gap for temps (Jahn 2010).

To date, there is no study available, which analyzes how changes in regulations of the law covering the temporary help service sector affects temp workers' job satisfaction. However, there are two studies investigating how the strictness of regulations of temporary contracts affects the job satisfaction of workers hired on a fixed-term basis. Using a cross-country sample of fixed-term workers from the European Community Household Panel, Salvatori (2010) analyses the impact of the strictness of regulations of temporary contracts on fixed-term workers' job satisfaction. The author finds that fewer regulations of these jobs are positively correlated with the job satisfaction of fixed-term and permanent workers. That flexible workers' job satisfaction is not only determined by the level of regulation of the contractual type has been shown by Origo and Pagani (2009) using the Eurobarometer. In countries with generous unemployment insurance systems, fixed-term workers are not significantly less satisfied with their jobs. However, if unemployment insurance systems only provide basic insurance against unemployment, fixed-term workers are more dissatisfied.

To investigate how changes in regulations affect temporary agency workers' job satisfaction, this study follows a different route and exploits longitudinal data for Germany for the period from 2002 to 2006. Using survey data from the German Socio-Economic Panel (SOEP) and combining a difference-in-difference (DID) approach with propensity score (PS) matching, we exploit a quasi-experimental reform in 2003 that only affected agency workers. Thus, we compare the job satisfaction of temps and perms while controlling for selection into temporary agency work based on a rich set of control variables.

We find that agency workers' job satisfaction decreased considerably after the reform. We also show that the negative effect on agency worker's job satisfaction can be partially attributed to a drop in wages and an increase in perceived job insecurity. Our results might also be of interest for other countries because Germany is one of the biggest markets for temporary agency work worldwide. In 2012, the share of agency workers among the total population was approximately 2.2 percent, which is well above the European average of 1.6 percent. For comparison, in 2012, the share of agency workers was 3.8 percent in the UK, 2.7 percent in the Netherlands, 2.0 percent in France, 1.4 percent in Japan, and 2.0 percent in the US (CIETT 2014).

The remainder of the paper is organized as follows. In Section 2, we describe the institutional background. Section 3 explains our estimation strategy, while in Section 4, we describe the data set used and provide descriptive statistics for the dependent and independent variables. Section 5 presents the estimation results. The presentation of the baseline results is followed by a number of robustness checks. Finally, we investigate the specific working conditions that might drive the negative effect on job satisfaction. Section 6 summarizes and concludes the paper.

## 2 Institutional Background

During the past decade, the temporary help service sector has grown substantially in Germany; the share of temp workers increased from approximately 1.3 percent of the wage and salary workforce in 2001 to approximately 3 percent in 2013 (Figure 1). In 2013, on average 840,000 workers were employed in this sector. When investigating labor market flows, however, it becomes evident that the temporary help service industry is even larger than the stock figure suggests. For example, in 2013, approximately 950,000 new temp jobs were concluded, and 1.1 million temp jobs were dissolved.

Figure 1 also documents that the agency sector has particularly grown after the reform in 2003, which is described in detail below. Firms mainly use agency employment as a buffer over the business cycle: During the recent economic crisis, the number of agency workers dropped quite

dramatically. Approximately 70 percent of the total job loss during the Great Recession was due to mass lay-offs in the temporary help service sector (Federal Employment Agency, 2014). Figure 1 also shows that the sector recovered immediately after the crisis.

[Figure 1 about here.]

In Germany, temporary agency employment is regulated by the Labor Placement Act, which governs the sector with specific regulations. One key element of this law is to regulate the tripartite employment relationship between a temporary work agency, an agency worker and the user firm. Workers are employed by the temporary work agency, which hires them out to a user firm. The wages and terms of employment are fixed in the contract between temp worker and the agency; the user firm then supervises and assigns tasks to the temp. Standard labor law applies to all workers in Germany, which implies that agency workers are entitled to health insurance, pension benefits, paid vacation, unemployment benefits and, after a trial period of six months, employment protection.

Since its first implementation in 1972, the Labour Placement Act has been modified several times (for details, see Antoni and Jahn 2009; Burda and Kvasnicka 2006). For the purpose of our study, the reforms in 2002 and 2003 are important. Until 2002, there were three main regulations affecting employment contracts between agencies and temp workers. First, the maximum period of assignment limited the assignment period at the same user firm to 12 months without interruption. After this period, the agency was required to replace the worker with another worker to perform the same task. If the agency could not find a follow-up assignment at another user firm, it was required to dismiss the worker. Second, the synchronization ban stated that the length of the employment contract between a temp agency and a temp worker had to exceed the length of an assignment to a user firm by at least 25 percent, even when no follow-up assignment was available. An exception was possible for the first assignment. The aim of this regulation was to create an incentive for agencies to bridge periods of non-assignment if there was no immediate follow-up assignment at hand. Third, the re-employment ban only permitted a one-time termination of an employment contract and a worker's subsequent re-employment.

Starting on January 1, 2002, the maximum period of assignment was extended to 24 months. Moreover, after being assigned for more than 12 months at the same user firm, the principle of equal pay applied, i.e., agency workers were entitled to the same remuneration as workers hired directly by the user firm. Both components of the reform should have increased temp workers' job satisfaction because a longer maximum period of assignment could potentially prolong the employment duration with the agency and thus increase job security. Because temps in Germany are

paid approximately 25 percent less than comparable regular workers (Jahn 2010), we would also expect that the introduction of the principle of equal treatment after 12 months of assignment should have had a positive effect on temps' job satisfaction.

The reform we exploit in this paper came into effect on January 1, 2003, and had a transition period of one year until all changes became legally binding on January 1, 2004. Starting on January 1, 2003, the maximum period of assignment, the synchronization ban and the re-employment ban were eliminated. Moreover, agencies that used a sectoral collective agreement could deviate from the wages and working conditions of the user firm that the temp worker was assigned to. Agencies that did not sign a collective agreement were required to apply the principle of equal pay from the first day of an assignment. At the beginning of 2003, there were barely any collective agreements in the temporary help service sector. For this reason, the law guaranteed a transition period of one year to provide agencies time to negotiate collective agreements. Thus, this regulation set a high incentive for agencies to implement a collective agreement. Otherwise, wage costs would have increased considerably. Therefore, it comes as no surprise that the first (major) collective agreement in this sector was concluded in May 2003. By the end of 2003, approximately 97 percent of all agencies had signed a collective agreement (Jahn 2010). Consequently, the principle of equal treatment had lost any importance by the beginning of 2004.

We argue that the reform in 2003 might have had important implications for agency workers' job satisfaction. First, when the synchronization and the re-employment ban were eliminated at the beginning of 2003, agencies and user firms gained considerable flexibility. An agency could not only dismiss temp workers as soon as an assignment ended at a user firm but also re-hire them when needed rather than providing a bridge during a period of non-assignment. This reform likely corresponded to a decrease in employment stability and job security for agency workers. Therefore, we expect that the elimination of both the re-employment ban and the synchronization ban negatively affected job satisfaction. At the same time, the introduction of an unlimited assignment period could have had a positive effect on employment duration with the agency, on job security and thus on job satisfaction.

Regarding the introduction of collective agreements in the temp sector at the beginning of 2004, one would at first sight expect that this affects temp workers' job satisfaction positively. A higher representation by unions should come with better working conditions like the introduction of working time accounts, the regulation of the maximum working time per day, and higher wages.[2]

[2] Nevertheless, wage gaps between temps and the staff of user firms remained possible if the wages in the user firm's collective agreement were higher than the wages in the agency's collective agreement (Jahn 2010).

However, a drop in wages is also possible because first, workers with long assignment periods lost the right for equal pay. Second, overtime compensation is rather high in Germany and temps often have to work overtime. If overtime is balanced by working time accounts instead by overtime compensation temp workers' overall remuneration might have decreased. We would thus expect that this part of the reform negatively affected temps' job satisfaction.

To sum up, it is ambiguous whether these changes affected the job satisfaction of temporary agency workers positively or negatively.

## 3 Empirical Strategy

The difficulty when evaluating the effects of the reform is that the contractual regulations came into effect in January 2003, while the regulation concerning remuneration only became legally binding in January 2004. This Section describes how we evaluate the impact of the reform and how we address the transition period.

To identify the causal effect of the reform we exploit the fact that the reform only affected agency workers and not workers in regular employment. Assuming that job satisfaction is a summary indicator for satisfaction with the entire set of working conditions, the effect on temps' job satisfaction can be evaluated using a DID design. The most crucial assumption when applying a DID approach is the parallel trends assumption. The validity of the parallel trends assumption is necessary to obtain a counterfactual outcome value for the treated group after the reform because for this group, the non-treatment outcome is not observable. In our case, this assumption requires that the job satisfaction of temporary agency workers (the treatment group) and regular workers (the control group) follows a parallel trend over time (parallel change of absolute job satisfaction). However, temps and perms differ in many characteristics, including job satisfaction and its evolution over time. For this reason, we follow, e.g., Halla and Zweimüller (2013), and additionally apply propensity score (PS) matching. Ideally, after matching, the control and treatment group should be comparable in terms of decisive socio-economic characteristics, and trends should be similar after having established a well-balanced sample. We then compare job satisfaction between both groups and over time obtaining a DID estimate.

Moreover, the reform of the law covering the temporary help service sector took place during the Hartz Reforms, which came into effect between 2002 and 2006. In addition to restructuring the Public Employment Service, these reforms changed the eligibility criteria for unemployment benefits. Although the Hartz Reforms did not affect regulations for employed workers, they might have still indirectly affected workers who were at risk of becoming unemployed. This is the second reason for

combining PS matching and DID. Using employment biographies to match workers, we can rule out that these changes might have affected the treatment and control groups differently.

Finally, PS matching allows us to control for a potential compositional bias of temp workers before and after the reform. This bias may occur because we rely on an unbalanced panel due to the limited number of temporary agency workers in the SOEP (see Section 4). Consequently, we cannot follow the same group of perms and temps before and after the reform, which implies that the composition of both the temp and perm workers might change by observation year. To take into account that the temp workers in our sample differ in characteristics before and after the reform, we match to each temp worker a weighted control group for each year separately.[3] We implement the PS matching approach by performing nearest-neighbor propensity score matching with 20 neighbors and replacements based on a logit model and the full sample.[4] To meet the conditional independence assumption, we match on 'pre-treatment characteristics', i.e., characteristics that are themselves not influenced by the status of being a temp worker (Caliendo and Kopeinig 2009). Section 4 discusses the variables that we regard as essential for this assumption to hold.

Using the sample of the matched treatment and control group, in a second step, we can apply the DID estimator to obtain the reform effect. The baseline DID model is estimated using pooled ordinary least squares (OLS):

$$JS_{it} = \beta_0 + \beta_1 temp_{it} + \beta_2 reform + \beta_3 (temp_{it} * reform) + \theta_t + \gamma X_{it} + \varepsilon_{it}. \quad (1)$$

where $JS_{it}$ denotes the dependent variable (overall job satisfaction) of worker *i* ($i = 1, \dots, N$) in year $t$ ($t = 2002, \dots, 2006$). $temp_{it}$ is equal to 1 if an individual reports as an agency worker in a given year and thus belongs to the treatment group and is 0 otherwise. The variable $reform$ captures the introduction of the reform and is equal to 1 in the years after the reform and 0 before. In addition, we include $\theta_t$, absolute time-fixed effects (calendar year dummies), and a vector $X_{it}$ of controls as described in Section 4. Finally, $\beta_3$, the coefficient of the interaction term ($temp_{it} * reform$), is the parameter of primary interest because it captures the effect of the reform on temps' job satisfaction, provided that $\beta_1$ and $\beta_2$ describe the counterfactual change in job satisfaction in the absence of treatment.

[3] We further address this problem by comparing selected characteristics of the (matched) temps before and after the reform. We find that they do not differ (see Table A.1).

[4] Caliendo and Kopeinig (2008) provide a comprehensive discussion of the propensity score approach.

Equation (1) might be restrictive in two ways. First, pre- and post-treatment dynamics ($\theta_t$) are assumed to be the same for perms and temps. Second, we assume that the treatment effect occurs only in the first period after treatment and that this shift of $\beta_3$ remains permanent over time.

To test whether the estimates are correctly identified, we follow Mora and Reggio (2012) and define two alternative DID models that allow for more flexibility. First, Equation (2) additionally includes the group-specific linear trends $trend_t$ and $(trend_t * temp_{it})$, which capture differences in group dynamics both before and after treatment. The treatment effect is still identified by $\beta_3$, provided that the counterfactual for the average change in job satisfaction of the treated group now corresponds to $\beta_2, \beta_4,$ and $\beta_5$ because including the trends changes the identifying assumption by assuming parallel growth. While the parallel trends assumption requires that average changes in job satisfaction are comparable between perms and temps in the absence of treatment, the parallel growths assumption requires that the growth paths (absolute change plus acceleration) in job satisfaction are comparable. Insignificant differences in parallel growth paths between the treated and the control group ($\beta_5$) are considered to be a test for the compliance of parallel growth and parallel paths (for details, see Mora and Reggio 2012).

$$JS_{it} = \beta_0 + \beta_1 temp_{it} + \beta_2 reform + \beta_3 (temp_{it} * reform) +$$

$$\beta_4\, trend_t + \beta_5 (trend_t * temp_{it}) + \theta_t + \gamma X_{it} + \varepsilon_{it}. \quad (2)$$

Second, we apply a DID model that provides full flexibility by allowing the causal treatment effect to vary over time. Assuming common pre-treatment trends of temps and perms, the reform effect in this model is identified by the coefficient of $(\theta_t * temp_{it})$ in the corresponding post-treatment years.[5]

$$JS_{it} = \beta_0 + \beta_1 temp_{it} + \theta_t + (\theta_t * temp_{it}) + \gamma X_{it} + \varepsilon_{it}. \quad (3)$$

As discussed in Section 2, the maximum period of assignment, the synchronization ban and the re-employment ban were eliminated on January 1, 2003, while there was a transition period for applying collective agreements until January 1, 2004. We therefore offer two specifications for all of the estimations that differ regarding the timing of the reform. In our preferred specification, we consider 2004 as the first post-reform year because only by then had all of the changes become effective. To obtain the full effect of the reform rather than a spurious effect, we exclude from this specification observations for 2003. In our second specification, we consider 2003 as the first post-

[5] We may state here that we do not find any pseudo-effects in the placebo analyses (Section 5.2). This result supports the validity of the assumptions of parallel growth and parallel trends.

reform year to investigate whether the reform caused changes in job satisfaction before it became legally binding.

## 4 Data

We take yearly observations of temporary agency workers and permanent workers from the SOEP (for further information on the SOEP, see Wagner et al. 2007). The treatment group consists of workers who reported to be employed by a temporary work agency. The comparison group consists of workers who reported to be permanently employed outside the sector.

We limit the baseline analysis to the observation period 2002 to 2006 for two reasons: First, the question about being a temp worker was asked for the first time in 2001, thus creating a higher risk of measurement error. Second, as explained in Section 2, a less significant reform affecting the temporary help service sector was enacted in 2002 that extended the maximum assignment period from 12 to 24 months and introduced the principle of equal treatment after 12 months of assignment at the same user firm. This reform likely improved working conditions for agency workers with long assignments, and as Figure 2 shows, it might have caused the increase in agency workers' job satisfaction in 2002. Thus, including 2001 in our observation period will likely bias the effects of the reform that we are interested in.

Furthermore, we restrict our analysis to male workers because approximately 70 percent of agency workers in Germany are men (Federal Employment Agency 2014). Moreover, it is well documented that the job satisfaction of flexible workers varies considerably between men and women, which is likely due to women's more pronounced preferences to work flexibly or to combine family responsibilities with labor force participation (e.g., Booth 2002, Clark 1997, D'Addio et al. 2007, Sousa-Poza and Sousa-Poza 2003). We also restrict our sample to workers aged 18 to 60 years, and we exclude apprentices, self-employed workers, civil servants, and participants in programs of active labor market policy.[6] Due to the small number of agency workers, we are not able to exploit the panel structure of the SOEP, and we therefore use the unbalanced panel. After applying the matching procedure, the resulting full sample consists of 230 observations (124 persons) for temps and 2,883 (weighted) observations (1,905 persons) for perms.

Compared with register data, the SOEP has the advantage of providing a broad range of information, allowing us to control to some extent for personality traits, which are usually unobserved.

[6] Following Schäfer (2012), we also drop workers who reported a change from regular to agency employment during the past year without switching the employer because such a contractual change is not plausible.

Furthermore, we know the self-reported job satisfaction levels of workers in their current jobs, which enables us to use job satisfaction as our main dependent variable. Job satisfaction is derived from the question, 'How satisfied are you with your job?' The response options are measured on a Likert scale and vary from 0 (totally unsatisfied) to 10 (totally satisfied).

Figure 2 displays the mean weighted job satisfaction of matched temps and perms by year. As expected, agency workers' job satisfaction increased from approximately 6.5 to 7 Likert points between 2001 and 2002. This increase is likely due to the introduction of equal pay for temps assigned for longer than 12 months at the same user firm and is one of the reasons why we dropped 2001 from our observation period. In 2003, agency workers' job satisfaction considerably decreased. As outlined in Section 2, the law allowed a transition period of one year for the introduction of collective agreements. Therefore, the decrease to approximately 6 Likert points in 2003 likely does not capture the entire effect of the reform. Indeed, job satisfaction further decreased in 2004 to approximately 5.7 Likert points. We take the decrease in these two years as initial evidence for a reform effect on temp workers' job satisfaction.

[Figure 2 about here.]

The overall trends in job satisfaction of temps and perms appear quite similar, particularly after the reform. Job satisfaction for temps is lower than for perms over the entire observation period, with a common slight downward trend for both groups.

[Table 1 about here.]

Job satisfaction depends on socio-economic characteristics, which might drive selection into agency work. As described in Section 3, we control for selection into agency employment by applying a matching procedure. To do so, we identify three different groups of confounders: general labor market conditions, factors that impact the hiring decisions of temporary work agencies, and factors that influence the employment decisions by workers. Including the regional unemployment rate at the federal state level in the regression, we account for general variations in labor supply and demand and structural regional differences.

Temporary work agencies hire workers based on their potential productivity. We capture this aspect by matching on personal characteristics such as age (linear and squared), education (three categories), job position (dummy for blue collar worker), health status (dummy for being sick for at least six weeks during the past year), foreign nationality (dummy), marital status (dummy), and the presence of children (dummy). The degree of labor market attachment is captured by the cumulative duration of unemployment experience in the past (three categories). We consider previous

unemployment experience to be the driving factor that explains a worker's decision to accept a temp job, which also might impact job satisfaction. Longer unemployment periods in the past indicate lower productivity and reservation wages and might increase the willingness of workers to accept temporary agency work (Kvasnicka 2009). Finally, we include the variable 'SOEP frequency' which indicates how many times the worker has previously answered the questionnaire.

Table 1 compares the average socio-economic characteristics of the treatment and control group over all person-year observations. The descriptive statistics before matching suggest that workers indeed select into agency employment because there are significant differences in the average characteristics between both groups. Temps are generally younger and less educated than perms. In addition, temps are less likely to be married and less likely to have children; these findings are likely related to their lower age. Moreover, the proportion of blue collar workers and foreigners is higher among temp workers. Finally, temps have less stable employment careers. The proportion of temps ever unemployed is considerably higher; if unemployed, they are also unemployed for a longer period. Moreover, temp workers live in regions with higher unemployment rates.

Table 1 also provides sample statistics and t-tests for mean differences for the matched sample used for the final estimations. After matching, temps and perms do not differ significantly in any socio-economic characteristic. In addition to the comparison of observations aggregated over years, as displayed in Table 1, we also conducted *t*-tests for differences in characteristics in the yearly observations. None of the null hypotheses were rejected. Moreover, we ensured that the mean standardized bias for the control variables did not exceed the 5 percent benchmark, as recommended by Caliendo and Hujer (2006). These tables are available upon request.

# 5 Estimation Results

## 5.1 Baseline Results

In this Section, we discuss the effects of the reform on male temp workers' job satisfaction. The observation period used for the analysis covers 2002 to 2006, i.e., two years before and three years after the reform. As described in Section 2, we offer two specifications: In Panel A of Table 2, we present estimation results assuming that the reform came into effect on January 1, 2004, when all of the legal changes became binding. To capture the full effect of the reform, we exclude observations for 2003. In Panel B, we retain all of the observations and present results assuming that the reform had already affected workers interviewed in 2003, i.e., after the introduction of the change but before the regulation of remuneration became legally binding. For brevity, we only report the reform and trend coefficients. The full regressions are available upon request.

The estimation reported in Column (1) in Panel A only includes a dummy for being an agency worker. The result confirms the expectation and descriptive statistics that temp workers were on average approximately 1.2 points less satisfied with their job than regular workers. In the next step, we employ the DID approach, as described in Equation (1). Column (2) shows that temp workers' job satisfaction decreased on average by approximately 1.3 Likert points after the reform, eliminating general job satisfaction differences between both groups. This result implies that in 2002, when the principle of equal pay after 12 months on assignment was originally introduced, the job satisfaction of agency workers matched that of regular workers. After agencies signed collective bargaining agreements, temps were no longer eligible for equal pay, resulting in a permanent downward shift in temp workers' job satisfaction. These results confirm the descriptive evidence from Figure 2. Note also that the coefficient for the reform dummy is not significant. This result confirms our expectation that the other Hartz Reforms did not affect the job satisfaction of the employed.

The estimation presented in column (3) introduces group-specific trends, assuming common growth in job satisfaction. Compared with the baseline DID, the coefficient measuring the reform effect remains almost the same. There seems to be a small downward trend in job satisfaction for both groups, with no difference between the treatment and control group. The absence of group-specific trends confirms the equivalence of common trends and common growth and further indicates that the common trends assumption holds. Finally, the estimation reported in column (4) allows for flexible dynamics of the treatment effect. Again, we find support that the reform decreased job satisfaction on average by 1.4 Likert points in 2004. In summary, all of the specifications suggest that the deregulation of the law covering the temporary help service sector negatively affected temp workers' job satisfaction.

The results reported in Panel B, which assumes that the legal changes had already affected job satisfaction in 2003, support these findings. We find negative effects of the reform that range on average between 1.2 and 1.3 Likert points, which is only somewhat smaller than the estimates reported in Panel A. The similarity of the coefficients in Panels A and B clearly shows that the largest impact of the reform on temps' job satisfaction had already occurred in 2003, and thus during the transition period and not only after the changes became legally binding in 2004. This result indicates that workers expected that agencies would circumvent the principal of equal pay by signing collective agreements. Moreover, the similarity of the coefficients over all of the models and specifications supports our research design.

[Table 2 about here.]

## 5.2 Sensitivity Analysis

To test whether the underlying assumptions are met, we conduct a series of robustness checks. First, by estimating OLS, so far we have treated job satisfaction as a continuous variable rather than an ordinal variable, facilitating the interpretation of the coefficient. To test whether the results change when we take the ordinal nature of the variable into account, we follow Van Praag and Ferrer-i-Carbonell (2006) and implement Probit OLS (POLS). After computing the conditional expectation for the latent variable, we subsequently apply OLS again. The estimates reported in column (1) of Table 3 confirm the significant negative effect of the reform on job satisfaction.

[Table 3 about here.]

In the next step, we test whether unobserved heterogeneity or the individual baseline satisfaction levels of temps are biasing the estimations. In the worst case, inherently unsatisfied workers select into agency employment, which might drive the lower average job satisfaction of temp workers (Green and Heywood 2011). By using a fixed effects (FE) model, we are able to eliminate mean differences in job satisfaction and time invariant unobserved heterogeneity. A drawback of the FE model is that we can only identify the change in job satisfaction ($\text{temp}_{it} * reform$) for those individuals who switched from a perm job before the reform to a temp job after the reform. However, the number of individuals in our sample who changed contract types during the reform period is small. Moreover, when estimating an FE model, we cannot use the matching weights. Therefore, the FE estimation can only be used as a rough guidepost to check whether unobserved heterogeneity might have biased the results. Despite these drawbacks, the results of the FE estimation support the findings so far that temps' job satisfaction considerably decreased after the reform. Not surprisingly, due to the small number of observations and the large increase in standard errors, the reform effect is not significant.[7]

To investigate whether we are able to establish parallel trends and growth by matching perms and temps on the propensity score, we estimate the effect of a pseudo-reform in the next step. For that purpose, we assume for the moment that the reform was introduced one year earlier, i.e., in 2002. To do so, we re-include observations for 2001. Column (3) in Table 3 shows that we no longer find any reform effect but only average differences in job satisfaction between the treatment and control group of approximately 0.9 points. This result strongly supports the validity of the parallel trends and growth assumption.

[7] The fixed effects model could also be estimated by using an ordered logit model with fixed effects, as suggested by Ferrer-i-Carbonell and Frijters (2004), where the ordered data are collapsed into binary data with individual-specific thresholds. However, this approach is extremely data consuming, and we therefore cannot follow it.

Prior studies have explained the main differences in job satisfaction between perms and temps based on working conditions such as job insecurity, working non-standard hours, or a job's content. Including other working conditions, these factors accumulate to the summary indicator job satisfaction (Buddelmeyer et al. 2013, de Graaf-Zijl 2012, de Cuyper et al. 2009, Hamermesh 2001, Wooden and Warren 2004). Because workplace characteristics belong to the contractual type we are interested in, they cannot serve as pre-treatment variables and are therefore not included in the propensity score matching. However, in the regressions on the already matched sample, we are able to include those workplace characteristics that we consider exogenous and potentially important. Our objective is to determine whether the decrease in job satisfaction after the reform was driven by those variables. Following Kalleberg et al. (2000), we add to the model log real hourly wages, job tenure in years, firm size (four categories), commuting distance to the workplace in km, a dummy for whether a worker received compensation for overtime, and a subjective indicator for job insecurity.[8] However, we are missing information on workplace characteristics such as integration into the work environment and social contacts within the firm, which might explain part of the job satisfaction gap. After including additional controls for workplace characteristics, the reform effect (Table 3, Column 4) decreases to 1 Likert point. This result implies that changes in the described workplace characteristics explain some of the job satisfaction gap but are clearly not the sole reasons why temps and perms experience different levels of job satisfaction. We will elaborate on the role of certain working conditions more closely in the next Section.

As a final robustness check, Column 5 presents the results using the full sample of temp and perm workers instead of the matched sample. Although the matched sample increases the balancing of workers in the treatment and control group, matching might restrict the analysis to a certain selection of permanent workers. Moreover, estimating the reform effect using the full sample would correspond to an average treatment effect rather than an average treatment effect on the treated. Column (5) shows that the results are robust to the estimates from the matched sample. Thus, the negative impact of the deregulation holds not only for the matched sample but also appears to be of a more general nature.

We repeated all of the robustness checks for specification B of Table 2, assuming that the reform took place in 2003. The results are almost identical, which implies that the liberalization of the law had already reduced temp workers' job satisfaction during the transition period and therefore immediately after its introduction.

[8] Job insecurity is derived from the question, 'How concerned are you about the following issues?' and the subsequent question, 'Your job security (if employed)'. Based on the response options of 'very concerned', concerned', 'somewhat concerned' or 'not concerned at all', we consider workers who chose the first option to be insecure about their jobs.

### 5.3 Explaining the Changes in Job Satisfaction

The results in Table 3, Column 4 show that changes in specific workplace characteristics might at least partly explain the lower job satisfaction of temporary workers after the reform. As described in Section 2, we expect that this negative effect is mainly driven via two channels: First, by temps' lower job security, as agencies were free to dissolve the contracts any time and re-employ the temp again. Second, the negative effect may be driven by lower wages because the principal of equal pay was abandoned. To test these hypotheses, we investigate whether these two working conditions changed after the reform.[9] Specifically, we now use the (log) wage, perceived job insecurity, and job tenure as dependent variables and include in the regressions the temp dummies, the interaction term measuring the effect of the reform, and the same controls as in Table 2. We only report the results using the sample from our preferred specification reported in Panel A of Table 2.

[Table 4 about here.]

The results shown in Table 4 indicate that wages for agency workers indeed decreased considerably after the reform. The effect on job insecurity was quite pronounced as well. For temp workers, perceived job insecurity increased by 17 percentage points after the reform, although the effect is only significant at the ten-percent level (*p-value:* 0.068). Nevertheless, the results imply that changes in wages and job insecurity might be likely candidates to explain the reduction in job satisfaction after the reform.

To check whether the reform also affected actual employment durations, we examine changes in job tenure as a final outcome. Table 4 shows that job tenure for temps was approximately 4.6 years lower than the job tenure for perms. However, job tenure did not decrease for temps due to the reform. In summary, we find that in addition to a considerable drop in wages, one likely second reason for the decrease in temps' job satisfaction was the fear that they could more easily be dismissed rather than actually being fired. This finding is in line with the results found by Jahn (2013), who shows that perceived rather than formal job security matters most for job satisfaction.

## 6 Conclusion

During the last decades, the temporary help service sector expanded considerably in most European countries, increasing the economic importance of this industry worldwide. Due to this development, the working conditions of agency workers increasingly gained political and scientific attention. This study investigates how the changes of the law covering the temporary help service sector in

[9] We also experimented with the remaining working conditions outlined in Section 5.2. In all cases we did not find a significant reform effect.

Germany in 2003 affected temp workers' job satisfaction. For this purpose, we combine propensity score matching with a difference-in-difference approach using the SOEP, which contains a rich set of control variables.

We find significantly negative effects of the reform on temps' job satisfaction across the two specifications we examined, with qualitatively and quantitatively very similar effects. This result indicates that the change in the law had already affected temp workers' job satisfaction at the beginning of the transition period. Moreover, we find that the reform negatively affected temp workers' wages and positively affected temp workers' perceived job insecurity. However, we could not confirm that actual job tenure decreased after the reform. This result is in line with the finding that subjective rather than actual job security matters for job satisfaction (Jahn 2013). Concerning the validity of our results, we conducted numerous robustness checks. The negative reform effect remains consistently significant over all specifications, which emphasizes the validity of our research design.

Because the working conditions that we controlled for do not explain the entire reform effect, one must consider further potential confounders that we could not account for. Recent studies emphasize that the feeling of being socially excluded or poorly integrated into the labor market negatively affects agency workers' well-being (Sende and Vitera 2013, Gundert and Hohendanner 2013). This result implies that temps are emotionally less attached to their user firm and most likely suffer from weak integration at the workplace due to assignments that are too short. Thus, temp workers might not be able to establish satisfactory social relationships. Because perceived job insecurity decreased after the reform, the feeling of social exclusion might have increased for temps, thereby further decreasing their job satisfaction levels. To establish the link between job satisfaction and social inclusion seems to be a promising avenue for future research.

.

# Table and Figures

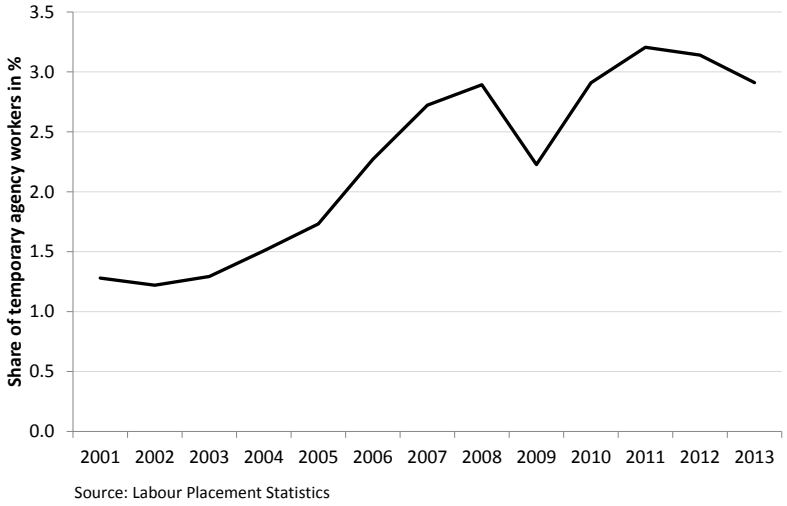


**Figure 1: Share of temporary agency workers in Germany, 2001–2012**

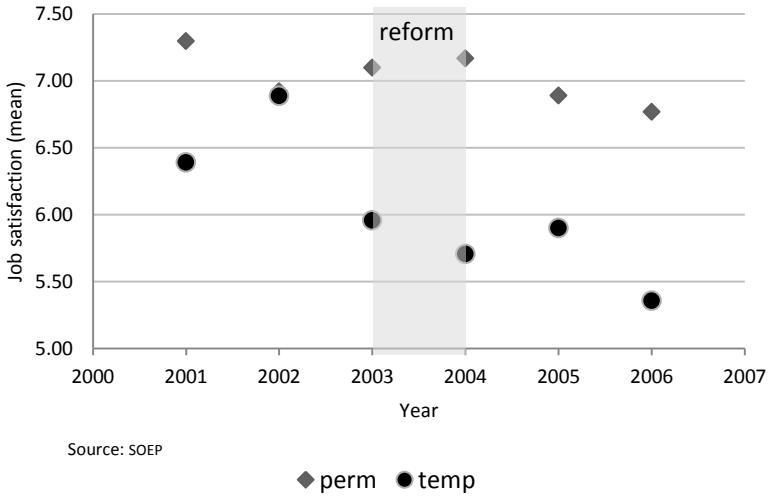


**Figure 2: Job satisfaction of temp workers and matched perm workers**

**Table 1: Sample statistics by treatment status before and after matching**

| | *Full Sample* | | | *Matched Sample* | | |
|---|---|---|---|---|---|---|
| | Control group | Treatment group | Signif. of diff. | Control group | Treatment group | Signif. of diff. |
| | ( N=15138) | (N=232) | | ( N=2750) | (N=231) | |
| Age | 41.916 | 35.569 | *** | 35.959 | 35.623 | |
| Age sqrt. | 184.604 | 138.244 | *** | 140.913 | 138.613 | |
| Education | | | | | | |
| Low | 0.082 | 0.129 | *** | 0.113 | 0.126 | |
| Middle | 0.592 | 0.694 | *** | 0.705 | 0.697 | |
| High | 0.326 | 0.177 | *** | 0.182 | 0.177 | |
| Foreign | 0.090 | 0.121 | | 0.114 | 0.121 | |
| Child in household | 0.448 | 0.319 | *** | 0.321 | 0.320 | |
| Married | 0.711 | 0.422 | *** | 0.431 | 0.424 | |
| Sick for six weeks in the past year | 0.044 | 0.052 | | 0.051 | 0.052 | |
| Blue collar worker | 0.467 | 0.776 | *** | 0.769 | 0.775 | |
| Total unemployment experience | | | | | | |
| No unemployment | 0.690 | 0.272 | *** | 0.267 | 0.273 | |
| > 0 & <= 0.5 years | 0.147 | 0.241 | *** | 0.243 | 0.242 | |
| > 0.5 years | 0.162 | 0.487 | *** | 0.490 | 0.485 | |
| SOEP frequency | 10.012 | 8.435 | *** | 8.519 | 8.446 | |
| Regional unemployment rate | 11.838 | 13.789 | *** | 13.918 | 13.768 | |

*Notes:* The dataset used is the SOEP. Average socio-economic characteristics of all individuals are measured by treatment status and calendar year in the full sample and in the matched sample. We use nearest neighbor propensity matching with 20 neighbors and replacement separately by calendar year for 2002 to 2006. Observations for the control group in the matched sample are weighted. Matching variables are as reported in this Table, except for matching in 2006, where age and age sqrt are replaced by age only. *, **, and *** indicate statistical significance of the difference in the average characteristics at the 10-percent level, 5-percent level, and 1-percent level, respectively.

**Table 2: The effect of the reform on temp workers' job satisfaction**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | OLS | DID Baseline | DID Group-specific trend | DID Full flexible |
| **Panel A: Excluding 2003** | | | | |
| Temp | -1.165** | -0.063 | -0.092 | -0.062 |
| | (0.206) | (0.358) | (0.419) | (0.359) |
| Reform 2004 | | 0.047 | 0.217 | |
| | | (0.221) | (0.224) | |
| Temp*reform 2004 | | -1.292** | -1.350* | -1.448** |
| | | (0.429) | (0.644) | (0.518) |
| Trend | | | -0.155* | |
| | | | (0.071) | |
| Trend*temp | | | 0.028 | |
| | | | (0.223) | |
| Observations | 2,381 | 2,381 | 2,381 | 2,381 |
| Number of persons | 1,682 | 1,682 | 1,682 | 1,682 |
| R-squared | 0.131 | 0.141 | 0.141 | 0.142 |
| **Panel B: 2002-2006** | | | | |
| Temp | -1.169** | -0.038 | -0.003 | -0.037 |
| | (0.190) | (0.375) | (0.410) | (0.376) |
| Reform 2003 | | 0.118 | -0.205 | |
| | | (0.229) | (0.230) | |
| Temp*reform 2003 | | -1.282** | -1.192* | -1.151* |
| | | (0.424) | (0.547) | (0.496) |
| Trend | | | -0.114* | |
| | | | (0.056) | |
| Trend*temp | | | -0.035 | |
| | | | (0.144) | |
| Observations | 2,981 | 2,981 | 2,981 | 2,981 |
| Number of persons | 1,930 | 1,930 | 1,930 | 1,930 |
| R-squared | 0.113 | 0.121 | 0.121 | 0.122 |

*Notes:* The dataset used is the SOEP. The dependent variable is job satisfaction, which is measured on a Likert scale ranging from 0 (totally unsatisfied) to 10 (fully satisfied). Estimates are based on a matched sample of treated and control units where weights are used. All of the models include two age dummies, two education dummies, citizenship, children in the household (dummy), marital status (dummy), one indicator for being sick for more than 6 weeks, blue collar worker (dummy), two dummies for the duration of unemployment experience, a dummy for West Germany, the yearly regional unemployment rate, year dummies, and a wave dummy indicating how many times the worker previously answered the questionnaire. Standard errors clustered at the individual level are given in parentheses. **, * and + indicate significance at the 1%, 5% and 10% levels, respectively.

**Table 3: Robustness checks**

| | (1) POLS | (2) FE | (3) OLS Placebo | (4) OLS Workplace controls | (5) OLS Full Sample |
|---|---|---|---|---|---|
| **Panel A: Excluding 2003** | | | | | |
| Temp | -0.076 | 0.684 | -0.918** | -0.109 | -0.065 |
| | (0.168) | (0.929) | (0.306) | (0.341) | (0.353) |
| Reform 2004 | 0.011 | 0.529* | | 0.078 | -0.034 |
| | (0.103) | (0.243) | | (0.210) | (0.041) |
| Placebo reform (2002) | | | 0.157 | | |
| | | | (0.226) | | |
| Temp*reform 2004 | -0.513* | -0.830 | | -1.022* | -1.283** |
| | (0.200) | (0.721) | | (0.406) | (0.422) |
| Temp*placebo (2002) | | | -0.245 | | |
| | | | (0.365) | | |
| Observations | 2,381 | 2,381 | 3,251 | 2,381 | 12,206 |
| Number of persons | 1,682 | 1,682 | 2,017 | 1,682 | 4,746 |
| R-squared | 0.134 | 0.062 | 0.116 | 0.203 | 0.026 |
| **Panel B: 2002-2006** | | | | | |
| Temp | -0.063 | 0.032 | -0.924** | -0.054 | -0.066 |
| | (0.178) | (0.831) | (0.310) | (0.347) | (0.354) |
| Reform 2003 | 0.019 | 0.310+ | | 0.039 | 0.001 |
| | (0.109) | (0.170) | | (0.219) | (0.036) |
| Placebo reform (2002) | | | 0.167 | | |
| | | | (0.222) | | |
| Temp*reform 2003 | -0.515** | -0.780 | | -1.066** | -1.235** |
| | (0.199) | (0.718) | | (0.395) | (0.402) |
| Temp*placebo (2002) | | | -0.244 | | |
| | | | (0.355) | | |
| Observations | 2,981 | 2,981 | 3,851 | 2,981 | 15,370 |
| Number of persons | 1,930 | 1,930 | 2,379 | 1,930 | 4,883 |
| R-squared | 0.112 | 0.068 | 0.101 | 0.176 | 0.026 |

*Notes:* The dataset used is the SOEP. The same controls are used as described in Table 2. The dependent variable is job satisfaction, measured on a Likert scale ranging from 0 (totally unsatisfied) to 10 (fully satisfied). Estimates are based on a matched sample of treated and control units where weights are used. Workplace controls are firm size (3 dummies), log wage, job tenure, financial compensation for overtime (dummy), commuting distance to workplace (2 dummies), and perceived job insecurity. Standard errors clustered at the individual level are given in parentheses. **, * and + indicate significance at the 1%, 5% and 10% levels, respectively.

**Table 4: The effect of the reform on workplace characteristics (Specification A)**

| | Log wage | Job insecurity | Job tenure |
|---|---|---|---|
| Temp | 0.055 | 0.071 | -4.824** |
| | (0.082) | (0.087) | (0.716) |
| Reform 2004 | -0.006 | 0.026 | 0.100 |
| | (0.033) | (0.044) | (0.427) |
| Temp*reform 2004 | -0.283** | 0.171+ | 0.755 |
| | (0.083) | (0.094) | (0.767) |
| Observations | 2,381 | 2,381 | 2,381 |
| Number of persons | 1,682 | 1,682 | 1,682 |
| R-squared | 0.374 | 0.125 | 0.355 |

*Notes:* The dataset used is the SOEP. The same controls are used as described in Table 3. Standard errors clustered at the individual level are given in parentheses. **, * and + indicate significance at the 1%, 5% and 10% levels, respectively.

# Appendix

**Table A1: Selected descriptive statistics by contract type before and after the reform**

| | *Temp workers* | | | *Permanent workers* | | |
|---|---|---|---|---|---|---|
| | 2002 | 2006 | Signif. of diff. | 2002 | 2006 | Signif. of diff. |
| | N=27 | N=56 | | N=378 | N=612 | |
| Age | 38.000 | 35.518 | | 38.181 | 35.801 | |
| Age sqrt. | 156.504 | 134.934 | | 158.873 | 136.770 | |
| Education | | | | | | |
| Low | 0.111 | 0.036 | | 0.096 | 0.036 | |
| Middle | 0.667 | 0.804 | | 0.681 | 0.794 | |
| High | 0.222 | 0.161 | | 0.222 | 0.171 | |
| Foreign | 0.111 | 0.125 | | 0.098 | 0.127 | |
| Child in household | 0.370 | 0.286 | | 0.380 | 0.301 | |
| Married | 0.481 | 0.357 | | 0.506 | 0.372 | |
| Sick for six weeks in the past year | 0.111 | 0.036 | | 0.085 | 0.035 | |
| Blue collar worker | 0.778 | 0.804 | | 0.783 | 0.802 | |
| Total unemployment experience | | | | | | |
| No unemployment | 0.296 | 0.214 | | 0.322 | 0.211 | |
| > 0 & <= 0.5 years | 0.185 | 0.179 | | 0.146 | 0.185 | |
| > 0.5 years | 0.519 | 0.607 | | 0.531 | 0.604 | |
| SOEP frequency | 7.667 | 9.232 | | 7.428 | 9.103 | |
| Regional unemployment rate | 15.100 | 12.779 | ** | 14.841 | 12.794 | * |

Notes: The average characteristics in the matched sample of individuals by treatment status before and after the reform are shown. We use nearest neighbor propensity matching with 20 neighbors and separately by calendar year for 2002 to 2006. Observations for the control group in the matched sample are weighted. Matching variables are as reported in this Table, except for matching in 2006, where age and age sqrt are replaced by age only. *, **, and *** indicate statistical significance of the difference in the average characteristics at the 10-percent level, 5-percent level, and 1-percent level, respectively.

## References


Antoni, M. and Jahn, E. (2009). Do changes in regulation affect employment duration in temporary help agencies? *Industrial and Labor Relations Review* 62, 226-251.

Autor, D. and Houseman, S. (2010). Do temporary help jobs improve labor market outcomes for low-skilled workers? Evidence from "Work First". *American Economic Journal: Applied Economics* 2, 96–128.

Bardasi, E. and Francesconi, M. (2004). The impact of atypical employment on individual well-being: Evidence from a panel of British workers. *Social Science and Medicine* 58, 1671-1688.

Boeri, T. (2011). Institutional reforms and dualism in European labor markets, In I.O. Ashenfelter and D. Card (eds.). Handbook of Labor Economics, Vol. 4b, Chapter 13, Amsterdam: Elsevier, 1173-1236.

Boeri, T. and Garibaldi, P. (2009). Beyond Eurosclerosis. *Economic Policy*, 409–461.

Böheim, R. and Cardoso, A. (2009). Temporary help services employment in Portugal. 1995-2000. In David Autor (ed.). Studies in Labor Market Intermediation. Chicago: The University of Chicago Press, 1-23.

Booth, A., Francesconi, M. and Frank, J. (2002). Temporary jobs: stepping stones or dead ends? *The Economic Journal* 112, 189-213.

Buddelmeyer, H., McVicar, D., and Wooden, M. (2013). Non-standard 'contingent' employment and job satisfaction: A panel data analysis. IZA Discussion Paper No. 7590, Bonn.

Burda, M. and Kvasnicka, M. (2006). Zeitarbeit in Deutschland: Trends und Perspektiven. *Perspektiven der Wirtschaftspolitik* 7, 195-225.

Caliendo, M., Hujer, R. (2006). The microeconometric estimation of treatment effects – An overview. *Allgemeines Statistisches Archiv* 90, 197-212.

Caliendo, M., Kopeinig, S. (2008). Some practical guidance for the implementation of propensity score matching. *Journal of Economic Surveys* 22, 31-72.

Chadi, A. and Hetschko, C. (2013). Flexibilisation without hesistation? Temporary contracts and workers' satisfaction, IAAEU Discussion Paper Series No. 4/2013, Trier.

CIETT (2014). Economic Report, Brussels. Online available: http://www.ciett.org/fileadmin/templates/ciett/docs/Stats/Economic_report_2014/CIETT_ER2013.pdf

Clark, A. (1997). Job satisfaction and gender: Why are women so happy at work? *Labour Economics* 4, 341-372

Clark, A. (2001). What really matters in a job? Hedonic measurement using quit data. *Labour Economics* 8, 223-42.

D'Addio, A., Eriksson, T. and Frijters, P. (2007). An analysis of the determinants of job satisfaction when individual's baseline satisfaction levels may differ. *Applied Economics* 39, 2413-2423.

De Cuyper, N., Notelaers, G. and De Witte, H. (2009). Job insecurity and employability in fixed-term contractors, agency workers, and permanent workers: Association with job satisfaction and affective organizational commitment. *Journal of Occupational Health Psychology* 14, 193-205.

De Graaf-Zijl, M. (2012). Job satisfaction and contingent employment. *De Economist* 160, 197-218.

Federal Employment Agency (2014). Arbeitsmarktberichterstattung: Der Arbeitsmarkt in Deutschland, Zeitarbeit in Deutschland – Aktuelle Entwicklungen, Nürnberg.

Ferrer-i-Carbonell, A. and Frijters, P. (2004). How important is methodology for the estimates of the determinants of happiness? *Economic Journal* 114, 641-659.

Garcia-Serrano, C., Hernanz, V., and Tohria, L. (2011). Mind the gap, please! The effect of temporary help agencies on the consequences of work accidents. *Journal of Labor Research* 31, 162-182.

Green, C. and Heywood, J. (2011). Flexible contracts and subjective well-being. *Economic Inquiry* 49, 716-729.

Grund, C., Martin, J., and Minten, A. (2014). Beschäftigungsstruktur und Zufriedenheit von Zeitarbeitnehmern in Deutschland. SOEPpapers on Multidisciplinary Panel Data Research 677, The German Institute for Economic Research (DIW), Berlin.

Gundert, S. and Hohendanner, C. (2013). Do fixed-term and temporary agency workers feel socially excluded? Labour market integration and social well-being in Germany. *Acta Sociologica* online publication DOI: 10.1177/0001699313496588.

Halla, M. and Zweimüller, M. (2013). The effect of health on income: Quasi-experimental evidence from commuting accidents. *Labour Economics* 24, 23-38.

Hamermesh D. (2001). The changing distribution of job satisfaction. *Journal of Human Resources* 36, 1-30.

Hamersma, S, Heinrich, C., Mueser, P. (2014). Temporary help work: Compensating differentials and multiple job-holding. *Industrial Relations* 53, 72-100.

Houseman, S., Kalleberg, A., and Erickcek, G. (2003). The role of temporary agency employment in tight labor markets. *Industrial and Labor Relations Review* 57, 105-127.

Jahn, E. (2010). Reassessing the pay gap for temps in Germany. *Journal of Economics and Statistics* 230, 208-233.

Jahn, E. (2013). Don't worry, be flexible? Job satisfaction among flexible workers. LASER Discussion Papers - Paper No. 71.

Jahn, E. and Bentzen, J. (2012). What drives the demand for temporary agency workers? *Labour* 26, 341-355.

Jahn, E., Riphahn, R., and Schnabel, C. (2012). Feature: Flexible forms of employment: Boon and bane. *The Economic Journal* 122, F115-F124.

Kalleberg, A.L., Reskin, B.F., and Hudson, K. (2000). Bad jobs in America: Standard and nonstandard employment relations and job quality in the United States. *American Sociological Review* 65, 256-278.

Kvasnicka, M. (2009). Does temporary agency work provide a stepping stone to regular employment? In Autor, D. (ed.). Studies of Labor Market Intermediation. Chicago: The University of Chicago Press, 335-372.

Mora, R. and Reggio, I. (2012). Treatment effect identification using alternative parallel assumptions. Working Paper 12-33, Economic Series (48).

Nienhüser, W. and Matiaske, W. (2006). Effects of the 'principle of non-discrimination' on temporary agency work: Compensation and working conditions of temporary agency workers in 15 European countries, *Industrial Relations Journal* 37, 64-77.

OECD (2013), Employment Outlook, Paris.

Origo, F. and Pagani, L. (2009). Flexicurity and job satisfaction in Europe: The importance of perceived and actual job stability for well-being at work. *Labour Economics* 16, 547-555.

Salvatori, A. (2010). Labour contract regulations and workers' wellbeing: International longitudinal evidence. *Labour Economics* 17, 667-678.

Schäfer, H. (2012). Temporary agency work in the SOEP: Coping with data quality problems. SOEPpapers on Multidisciplinary Panel Data Research 454, The German Institute for Economic Research (DIW), Berlin.

Segal, L. and Sullivan, D. (1998). Wage differentials for temporary services work: Evidence from administration data. Federal Reserve Bank of Chicago. Working Paper Series. No. WP-98-23. Chicago.

Sende, C. and Vietera, J. (2013). Commitment und Arbeitszufriedenheit bei Zeitarbeitnehmern und Stammbeschäftigten. In Bornewasser, M. and Zülch, G. (eds.). Arbeitszeit Zeitarbeit. Wiesbaden: Springer Gabler, 281-304.

Sousa-Poza, A. and Sousa-Poza, A.A. (2003). Gender differences in job satisfaction in Britain, 1991-2000: Permanent or transitory? *Applied Economic Letters* 10, 691-694.

Van Praag, B. and Ferrer-i-Carbonell, A. (2006). An almost integration-free approach to ordered response models. Tinbergen Institute Discussion Paper 2006-047/3, Tinbergen.

Wagner, G.G., Frick, J.R. and Schupp, J. (2007). The German Socio-Economic Panel Study (SOEP) – Scope, evolution and enhancements. *Schmollers Jahrbuch* 127(1), 139-169.

Wilkin, C.L. (2013). I can‘t get no job satisfaction: Meta-analysis comparing permanent and contingent workers. *Journal of Organizational Behavior* 34, 47-64.

Wooden, M. and Warren, D. (2004). Non-standard employment and job satisfaction: Evidence from the HILDA Survey. *The Journal of Industrial Relations* 46, 275-297.