

Do changes in regulation affect job satisfaction of temporary agency workers? ⁺

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Abstract: This paper evaluates the impact of a reform, which considerably relaxed regulations covering the temporary help service sector on temporary agency workers' job satisfaction. We isolate the causal effect of this reform introduced in Germany in 2003 using rich survey data and combining a difference-in-difference and matching approach. We find that the change of the law substantially decreased job satisfaction of agency workers while job satisfaction of regular workers 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 after the reform. These results are also robust to the use of different specifications and placebo tests.

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1 Introduction

In most European countries, the use of temporary agency employment has been stepwise eased over the past two decades while regulations for permanent contracts were essentially left unchanged (OECD 2013).¹ It comes therefore as no surprise that there has been a European wide impressive growth of the temporary help service sector. It is the spot market nature along with the poor working conditions of agency jobs, why agency employment has become the heart of the policy debate about increasing labour market flexibility (e.g. Boeri 2011, Jahn et al. 2012).

And indeed, there is ample evidence that the average wage of agency workers usually lags behind that of the permanent staff (e.g. Böheim and Cardoso 2009, Hamersma et al. 2014, Jahn 2010, Segal and Sullivan 1998), that they face higher unemployment risks (Antoni and Jahn 2009, Houseman et al. 2009, Autor and Houseman 2010), have less access to training (Nienhüser and Matiaske 2006), or suffer more often from work related accidents (Garcia-Serrano et al. 2011).

To judge if flexible employment forms in general are favourable compared to 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 like job stability or wages, which are common to all contractual arrangements, but also contains assessments on usually unobservable or unmeasurable job characteristics like 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 job satisfaction of agency workers and second, follow the literature that aims at evaluating the effects of the regulation of flexible employment forms. More precisely, we analyse the influence of a German reform in 2003 that considerably abandoned restrictions on the use of temporary agency workers, on male agency workers' job satisfaction.

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

¹ For simplicity, 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.

example, recent studies have shown that workers hired on direct-hire fixed-term contracts seem to be equally 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, there are only few studies available focusing solely on job satisfaction of temporary agency workers. Those studies consistently find that agency workers experience lower job satisfaction compared to regular workers (Buddelmeyer et al. 2013, de Graaf-Zijl 2012, 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 needs further investigation, as recent evidence shows that perceived job security rather than contractual job insecurity is the driving factor behind this result (Jahn 2013).

The knowledge about how the deregulation of the temporary agency sector affects job satisfaction of temp workers and thus working conditions is particularly important, as national governments increasingly start to re-regulate this employment form. So far, studies analysing the consequences of easing the use of temporary agency workers focused on objective outcomes like 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 investigates how regulations of the law covering the temporary help service sector affects temp workers' job satisfaction. However, there are two studies available investigating how regulations for temporary contracts affect 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 restrictions on fixed-term workers' job satisfaction. This study finds that fewer regulations for these jobs are positively correlated with job satisfaction of fixed-term and permanent workers. That job satisfaction of flexible workers 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.

In order to investigate how changes in regulation affect job satisfaction of temporary agency workers this study follows a different route and exploits longitudinal data for Germany for the period 2002-2006. 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. In this setup, we compare 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 decreases considerably after the reform. We also show that the negative effect on agency worker's job-satisfaction can be partially attributed to a decrease in wages and an increase in perceived job-insecurity after the reform. Our results should be of interest also for other countries as Germany is one of the biggest markets for temporary agency work worldwide. In 2012 the share of agency workers among the total population was about 2.2 percent. This is well above the European average with a share of 1.6 percent. For comparison, in 2012 the share of agency workers in UK was 3.8 percent, in the Netherlands 2.7 percent, in France 2.0 percent, in Japan 1.4 percent and in the US 2.0 percent (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 we describe in section 4 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 which working conditions 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 about 3 percent in 2013 (Figure 1). In 2013 on average about 840.000 workers were employed in this sector. When investigating the labour market flows, it becomes evident that the temporary help service industry is even larger than the stock figure suggests. For example in 2013 about 950.000 new temp jobs were concluded and 1.1 million temp jobs were dissolved.

Figure 1 also documents that the agency sector has particular grown after the reform in 2003, which will be explained in detail below. Firms use agency employment mainly as a buffer over the business cycle: During the recent economic crisis the number of agency workers dropped quite dramatically. Around 70 percent of the total job loss during the Great Recession was due to the 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 Labour Placement Act which governs the sector with specific regulations. One key element of this law is to regulate the tripartite employment relationship between temporary work agency, agency worker and the user firm. Workers are employed by the temporary work agency which hires them out to a user firm. Wages and terms of employment are fixed in the contract between temp worker and agency; the user firm then supervises and assigns tasks to the temp. Standard labour law applies to all workers in Germany. This implies that also agency workers are entitled for health insurance, pension benefits, paid vacation, unemployment benefits and, after a trial period of six months, for 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 of importance. Until 2002, there were three main regulations affecting the employment contract between agency and temp worker. First, the maximum period of assignment limited the assignment period at the same user firm without interruption to 12 months. After this period, the agency had 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 had to dismiss the worker. Second, the synchronization ban required that the length of an employment contract between temp agency and temp worker exceeded the length of the 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 the employment contract and a worker's re-employment.

From January 1, 2002 onwards, the maximum period of assignment was extended to 24 months. Moreover, after being assigned 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, as a longer maximum period of assignment could potentially prolong employment duration with the agency and thus increase job security. As temps in Germany usually are paid about 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 exploited 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. From January 1, 2003

onwards, the maximum period of assignment, the synchronization ban and the re-employment ban were abandoned. Moreover, agencies which applied a sectoral collective agreement could deviate from wages and working conditions provided by the user firm the temp worker was assigned to. Agencies not signing a collective agreement had 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 give agencies time to bargain collective agreements. It is straightforward that the latter regulation set a high incentive for agencies to apply a collective agreement. Otherwise wage costs would have increased tremendously due to the huge pay gap. Therefore, it comes as no surprise that the first (major) collective agreement in this sector was already concluded in May 2003. By the end of 2003, about 97 percent of all agencies had signed a collective agreement (Jahn 2010). Consequently, the principle of equal treatment had lost any importance at the beginning of 2004.

We argue that the reform in 2003 might have had important implications for agency workers' job satisfaction. First of all, when the synchronisation and the re-employment ban were abandoned at the outset of 2003, agencies and user firms gained considerable flexibility. The agency could not only dismiss temp workers as soon as the assignment ended at the user firm, but also re-hire them when needed again rather than to bridge periods without assignment. This likely corresponds to a decrease in employment stability and in job security for agency workers. Therefore, we expect that the elimination of both the re-employment 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 of employment duration with the agency and thus job security and job satisfaction.

At first sight, one would expect that the introduction of collective agreements in the temp sector at the beginning of 2004 might have had a positive effect on wages and thus on temp workers' job satisfaction. However, wage gaps between temps and the staff of the user firm remained still possible if wages in a user firm's collective agreement were higher than wages in the agency's collective agreement (Jahn 2010). Moreover, as agencies could also pay wages according to collective agreements for workers assigned more than 12 months, workers with long assignment periods lost the right for equal pay. Consequently we would expect that the reform affected job satisfaction of temps negatively.²

The difficulty when evaluating the reform effect is that the contractual regulations came into effect in January 2003 while the regulation concerning the remuneration only became legally binding in January 2004. The next section describes how we handle this transition period.

² Jahn (2010) shows that the 2004 policy change did increase the size of the temporary agency wage gap.

3 Empirical Strategy

In this paper, we exploit the fact that the reform only affected agency workers, but not workers in regular employment. Provided that job satisfaction is a summary indicator for the satisfaction with the entire set of working conditions, we expect a negative effect on temps' job satisfaction which 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 after the reform, as for this group the non-treatment outcome is not observable. In our case, this assumption requires that 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. This is the reason why we follow e.g. Halla and Zweimüller (2013) and apply additionally (propensity score) PS matching. Ideally, after matching 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. Besides restructuring the Public Employment Service these reforms changed eligibility criteria for unemployment benefits. Although the Hartz-Reforms did not affect regulations for employed workers, they might have had still indirectly affected workers being at risk to become unemployed. This is the second reason for combining PS matching and DID. Using the employment biography to match workers, we can rule out that these changes might have affected treatment and control group differently.

Finally, this 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 temp and perm workers might change by observation year.

To take into account that 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 neighbour propensity score matching

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

with 20 neighbours and replacement 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 which are themselves not influenced by the status of being a temp worker (Caliendo and Kopeinig 2009). Section 4 discusses the variables that we regard essential for this assumption to hold.

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

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

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 to be 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 θ_t , absolute time-fixed effects (calendar year dummies), and a vector X_{it} of controls as described in section 4. Finally, β_3 , the coefficient of the interaction term ($temp_{it} * reform$) is the parameter of primary interest as it captures the reform effect on temps’ job satisfaction, provided that β_1 and β_2 describe the counterfactual change of job satisfaction in the absence of treatment.

Equation (1) might be restrictive in two ways. First, pre- and post-treatment dynamics (θ_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 β_3 remains permanent over time.

To test if the hitherto estimates are correctly identified, we follow Mora and Reggio (2012) and define two alternative DID-models, which allow for more flexibility. First, equation (2) additionally includes 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 β_3 , provided that the counterfactual for the average change of job satisfaction of the treated now corresponds to β_2, β_4 , and β_5 , because including trends changes the identifying assumption by assuming parallel growths. 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 growth paths (absolute change plus acceleration) in job satisfaction are comparable. Insignificant differences in parallel growth paths between treated and

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

controls (β_5) is considered to be a test for the compliance of parallel growths 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 synchronisation ban and the re-employment ban were abandoned 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 estimations, which differ regarding the timing of the reform. In our preferred specification, we consider the year 2004 as the first post-reform year as only by then all changes came into effect. In order to obtain the full effect of the reform rather than a spurious effect, we exclude in this specification observations for the year 2003. In our second specification we consider 2003 as the first post-reform year to investigate whether the reform caused changes in job satisfaction also before it became legally binding.

4 Data

In order to put our approach into practice, 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 on being a temp worker was asked for the first time in 2001, thus holding a higher risk of measurement error. Second, as explained in section 2, there was already a small reform affecting the temporary help service sector in 2002, which extended the maximum assignment period from 12 to 24 months and introduced the principle of equal treatment after 12 months of assignment at the

⁵ We may already say here, that we do not find any pseudo-effects in the placebo analyses (section 5.2). This supports the validity of the assumptions of parallel growth and parallel trends.

same user firm. This reform likely improved working conditions for agency workers with long assignments, and as Figure 2 shows, might have caused the increase in agency workers' job satisfaction in 2002. Thus, including the year 2001 into our observation period will likely bias the effects of the reform we are interested in.

Furthermore, we restrict our analysis to male workers as about 70 percent of agency workers in Germany are men (Federal Employment Agency 2014). Moreover, it is well documented that job satisfaction of flexible workers varies between men and women considerably, likely due to women's more pronounced preferences to work flexibly or to combine family responsibilities with labour 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 exclude apprentices, self-employed, civil servants, and participants in programs of active labour market policy measures.⁶ Due to the small number of agency workers we are not able to exploit the panel structure of the SOEP and use therefore the unbalanced panel. The resulting full sample after applying the matching procedure consists of 230 observations (124 persons) for temps and 2.883 (weighted) observations (1.905 persons) for perms.

Compared to 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. Furthermore, we know the self-reported job satisfaction levels of workers in their current jobs, which enables us to use it 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, job satisfaction of agency workers increased from about 6.5 to 7 Likert points between 2001 and 2002. This rise is likely due to the introduction of equal pay for temps assigned longer than 12 months at the same user firm, and is one of the reasons why we dropped the year 2001 from our observation period. In 2003, agency workers' job satisfaction decreased considerable. As outlined in section 2, the law allowed a transition period of one year for the introduction of collective agreements. Therefore, the drop to about 6 points in 2003 likely does not capture the entire reform effect. And indeed, job satisfaction further decreased in 2004 to about 5.7 Likert points. We take the drop in these two years as first evidence for a reform effect on temp workers' job satisfaction.

[Figure 2 about here.]

⁶ Following Schäfer (2012) we also dropped workers who report a change from regular to agency employment during the past year without switching the employer as such a contractual change is not plausible.

However, the overall trends in job satisfaction of temps and perms appear quite common, in particular 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 moreover depends on socio-economic characteristics, which might also drive the selection into agency work. As outlined 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 labour market conditions, factors that impact the hiring decisions by temporary work agencies, and factors that influence the employment decisions by workers. Including the regional unemployment rate at the federal state level into the regression, we account for general varying labour supply and demand and structural regional differences.

Temporary work agencies hire workers based on their potential productivity. We capture this 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 labour market attachment is captured by the cumulated 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 pick up temporary agency work (Kvasnicka 2009). Finally, we include a variable - SOEP frequency - which indicates how often the worker has already 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 as there are significant differences in the average characteristics between both groups. Temps are generally younger and less educated than perms. Probably related to the lower age of temps is the finding that they are less often married and have less often children. Moreover, the share of blue collar workers and foreigners is higher among temp workers. Finally, temps have less stable employment careers. The share of temps ever unemployed is considerably higher; if unemployed they are also longer unemployed. 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 of 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 the years 2002 to 2006, i.e. two years before and three years after the reform. As outlined 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 legal changes became binding. In order to capture the full reform effect, we exclude observations for the year 2003. In Panel B we keep all observations and present results assuming that the reform already affected workers interviewed in 2003, i.e. after the introduction of the change, but before the regulation of the remuneration became legally binding. For brevity we only report 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 are on average about 1.2 points less satisfied with their job than regular workers. In a next step, we employ the DID approach as described in equation (1). Column (2) reveals that temp workers' job satisfaction decreases on average by about 1.3 Likert points after the reform, removing general job satisfaction differences between both groups. This implies that in 2002, when the principle of equal pay after 12 months on assignment was originally introduced, job satisfaction of agency workers matched the one of regular workers. After agencies signed collective agreements, temps were no longer eligible for equal pay, resulting in a permanent downward shift of temp workers' job satisfaction. These results confirm the descriptive evidence of Figure 2. Note also, that the coefficient for the reform dummy is not significant. This confirms our expectation that the other Hartz-reforms did not affect job-satisfaction of the employed.

The estimation presented in column (3) introduces group specific trends, assuming common growth of job satisfaction. Compared to 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, without any difference between treatment and control group. The absence of group specific

trends confirms the equivalence of common trends and common growth, and furthermore 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 proof that the reform decreased job satisfaction on average by 1.4 Likert points in 2004. Summing up, all specifications suggest that the deregulation of the law covering the temporary help service sector negatively affected job satisfaction of temp workers.

The results reported in Panel B, which assumes that the legal changes already had an effect on job satisfaction in 2003, support these findings. We find negative reform effects on average ranging between 1.2. and 1.3 Likert points which are only somewhat smaller than those estimates reported in Panel A. The similarity of coefficients in panel A and B makes clear that the reform had its biggest impact on temps' job satisfaction already in 2003, and thus during the transition period and not only after the changes became legally binding in 2004. This indicates that workers were expecting that agencies would circumvent the principal of equal pay by signing collective agreements. Moreover, the similarity of the coefficients over all models and specifications supports our research design.

[Table 2 about here.]

5.2 Sensitivity analysis

In order to test whether the underlying assumptions are met, we conduct a series of robustness checks. First, by estimating OLS, we so far treated job satisfaction as a continuous variable rather than an ordinal variable, facilitating the interpretation of the coefficient. To test if 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 reform effect on job satisfaction.

[Table 3 about here.]

In a next step we test if unobserved heterogeneity or individual baseline satisfaction levels of temps are biasing the hitherto 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 draw-back of the FE model is that we can only identify the change in job satisfaction ($temp_{it} * reform$) 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 a FE model we must go without the matching weights. Therefore, the FE estimation can only be a rough guidepost to see if unobserved heterogeneity might bias the results. Despite these drawbacks, the results of the FE estimation support the finding so far that temps' job satisfaction drops after the reform considerably. Not surprisingly, due to the small number of observations and the large increase in standard errors the reform effect is not significant.⁷

In order 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 a next step. For that purpose we assume for the moment that the reform was introduced already one year earlier, i.e. in 2002. To do so, we re-include observations for the year 2001. Column (3) in Table 3 shows that we do no longer find any reform effect, but only average differences in job satisfaction between treatment and control group of about 0.9 points. This result strongly supports the validity of the parallel trends and growth assumption.

The literature so far explained main differences in job satisfaction between perms and temps accounting for working conditions like job insecurity or the content of a job. Among other working conditions, these factors accumulate to the summary indicator job satisfaction (Hamermesh 2001, de Graaf-Zijl 2012, de Cuyper et al. 2009). As workplace characteristics belong to the contractual type we are interested in, they cannot serve as pre-treatment variables and are thus 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 find out if the drop in job satisfaction after the reform is 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), the 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 like 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 implies that changes in the described workplace characteristics explain some of the job satisfaction gap, but are by far 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.

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 to binary data with individual-specific thresholds. However, this approach is extremely data consuming which is the reason we cannot follow these routes.

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 "very concerned", "somewhat concerned" or "not concerned at all", we consider those workers as insecure about their jobs, who chose the first option.

As a final robustness check, column 5 presents the results using the full sample of temp and perm workers instead of the matched sample. Even though the matched sample increases the balancing of workers in 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 seems to be of a more general nature.

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

5.3 Reform-induced changes linked to job satisfaction

The results in Table 3, column 4, have shown that changes in specific workplace characteristics might at least partly explain lower job satisfaction of temporary workers after the reform. As outlined in section 2, we expect that the negative effect is mainly driven via two channels: First, by temps' lower job security, as agencies were free to dissolve the contract any time and re-employ them any time again. Second, by lower wages, because the principal of equal pay was abandoned. In order to test these surmises we investigate whether these two working conditions changed after the reform. Specifically, we now use the (log) wage, perceived job insecurity, and job tenure as dependent variables and include in the regressions besides the temp dummies, and the interaction term measuring the reform effect, 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 in Table 4 indicate that wages for agency workers indeed decreased considerably after the reform. The effect on job insecurity is quite pronounced as well. For temp workers, perceived job insecurity increased by 17 percentage points after the reform, even though 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 explaining the reduction in job satisfaction after the reform.

In order to check if the reform also affected actual employment durations, we look at changes in job tenure as a final outcome. Table 4 shows that job tenure for temps is about 4.6 years lower than the

job tenure for perms. However, job tenure did not decrease for temps due to the reform. Summing up, we find that besides a considerable drop in wages one likely second reason for the decrease of temps' job satisfaction is the fear of being more easily dismissed, rather than being actually fired. This finding is in line with the results of Jahn (2013) who shows that perceived rather than formal job security matters 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 liberalization of the temporary help service sector in Germany in 2004 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. As the reform eased the use of agency workers and also affected their remuneration, we expect that agency workers' wages and job security decreased after the reform, inducing a decrease in job satisfaction.

We find significantly negative effects of the reform on temps' job satisfaction across the two specifications we provide with qualitatively and quantitatively very similar effects. This indicates that the change of the law affected temp workers' job satisfaction already at the beginning of the transition period. Moreover, we find that the reform affected temp workers' wages negatively and perceived job insecurity positively. 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 conduct numerous robustness checks. The negative reform effect remains consistently significant over all specifications emphasizing the validity of our research design.

As the working conditions we controlled for do not explain the entire reform effect, one has to consider further potential confounders that we could not account for. A recent literature emphasizes that the feeling of being socially excluded or poorly integrated into the labour market affects agency workers' well-being negatively (Sende and Vitera 2013, Gundert and Hohendanner 2013). This implies that temps are emotionally less attached to the user firm and most likely suffer from a weak integration at the workplace due to too short assignments. Thus, they might not be able to establish satisfactory social relationships. As perceived job insecurity decreased after the reform, the feeling of social exclusion might have increased for temps, thereby decreasing further their job satisfaction levels. As we lack such information, it is up to future research to take this into account.

Table and Figures

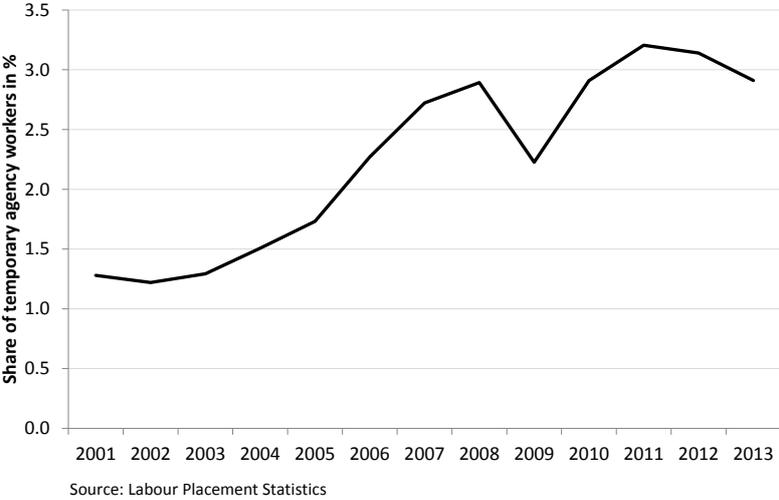


Figure 1: Share of temporary agency workers in Germany, 2001–2012

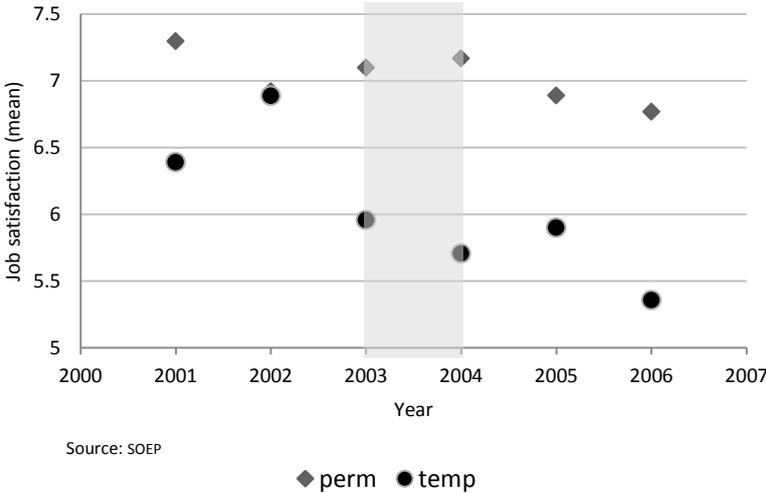


Figure 2: Job-satisfaction of temp and matched perm workers

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

	<i>Full Sample</i>			<i>Matched Sample</i>		
	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 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 data set used is the SOEP. Average socio-economic characteristics of all individuals by treatment status measured by calendar year in the full sample and in the matched sample. Nearest neighbor propensity matching with 20 neighbors and replacement of treatment and control units 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 the table above, except for matching in the year 2006, where age and age sqrt are replaced by three age groups (19-25, 26-40, 41-60). *, **, 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 job satisfaction of temp workers

	(1)	(2)	(3)	(4)
	OLS	DID Baseline	DID Group-specific trend	DID Full flexible
Panel A: Excluding 2003				
Temp	-1.163** (0.206)	-0.062 (0.358)	-0.088 (0.419)	-0.061 (0.359)
Reform 2004		0.047 (0.021)	-0.213 (0.224)	
Temp*reform 2004		-1.292** (0.429)	-1.345* (0.644)	-1.445** (0.519)
Trend			-0.153* (0.071)	
Trend*temp			0.026 (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	-1.168** (0.190)	-0.036 (0.375)	-0.001 (0.410)	-0.035 (0.376)
Reform 2004		0.120 (0.229)	-0.207 (0.230)	
Temp*reform 2003		-1.283** (0.424)	-1.192* (0.547)	-1.154* (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	0.112	0.120	0.121	0.122

Notes: The data set used is the SOEP. 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. All models include two age dummies, two education dummies, citizenship, children in the household (dummy), marital status (dummy), one indicator for being sick 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 often the worker has 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)	(2)	(3)	(4)	(5)
	POLS	FE	OLS Placebo	OLS Workplace controls	OLS Full Sample
Panel A: Excluding 2003					
Temp	-0.074 (0.168)	0.684 (0.929)	-0.917** (0.306)	-0.109 (0.341)	-0.065 (0.353)
Reform 2004	0.012 (0.103)	0.529* (0.243)		0.078 (0.210)	-0.034 (0.365)
Placebo reform (2002)			0.156 (0.226)		
Temp*reform 2004	-0.514* (0.200)	-0.830 (0.721)		-1.022* (0.406)	-1.283** (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	0.133	0.062	0.116	0.203	0.026
Panel B: 2002-2006					
Temp	-0.061 (0.178)	0.032 (0.831)	-0.923** (0.310)	-0.054 (0.347)	-0.066 (0.354)
Reform 2003	0.021 (0.108)	0.310 ⁺ (0.170)		0.041 (0.219)	0.001 (0.036)
Placebo reform (2002)			0.166 (0.222)		
Temp*reform 2003	-0.517** (0.199)	-0.780 (0.718)		-1.067** (0.395)	-1.235** (0.402)
Temp*placebo (2002)			-0.243 (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 data set 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 (0.082)	0.071 (0.087)	-4.833** (0.716)
Reform 2004	-0.007 (0.033)	0.026 (0.044)	0.084 (0.428)
Temp*reform 2004	-0.283** (0.083)	0.171 ⁺ (0.094)	0.769 (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 data set 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

	<i>Temp workers</i>			<i>Permanent workers</i>		
	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 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: This table provides the average characteristics in the matched sample of individuals by treatment status before and after the reform. Nearest neighbor propensity matching with 20 neighbors and replacement of treatment and control units separated by calendar year for 2002 to 2006. Observations for the control group in the matched sample are weighted. Matching variables are as reported in the table above, except for matching in the year 2006, where age and age sqrt are replaced by three age groups (19-25, 26-40, 41-60). *, **, and *** indicate statistical significance of the difference in the average characteristics at the 10-percent level, 5-percent level, and 1-percent level, respectively.

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