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The magic of the new: How job changes affect job satisfaction

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**The Magic of the New:
How Job Changes Affect Job Satisfaction**

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The Magic of the New: How Job Changes Affect Job Satisfaction

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Abstract

We investigate a crucial event for job satisfaction: changing the workplace. For representative German panel data, we show that the reason why the previous employment ended is strongly linked to the satisfaction with the new job. When workers initiate a change of employer, they experience relatively high job satisfaction, though only in the short-term. To test causality, we exploit plant closure as exogenous trigger of job switching and find no causal effect of job changes on job satisfaction. Our findings concern research on workers' well-being as well as labor market and human resource policies.

Research highlights

- Starting a job at a different employer is, on average, linked to very high job satisfaction.
- The huge new job effect revealed is driven by the majority of voluntary job changes.
- Causality is not given, as the effect disappears in cases of exogenously triggered switches.
- For voluntary job changes, satisfaction with the new job declines strongly over time.

JEL Classification: I31; J28; J63; M50

Keywords: job satisfaction; job changes; new job; honeymoon-hangover effect; plant closure

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*Your heart must always be ready to leave,
and ready to begin again,
must form new bonds
with courage and without regret.
Every beginning offers a magic power
that protects us and helps us to endure.¹*

1. Job Changes and Job Satisfaction

In *Stages*, one of his most famous and probably most frequently cited poems, Hermann Hesse describes each end of a stage in life as part of a new beginning. The statement “every beginning offers a magic power” is expressive of the hope that each farewell, however sad it may be, will yield a promising start. The present study analyzes job changes as a transition from one stage to another. A large body of research documents that ending an employment is often accompanied by distress and sorrow: Satisfaction with a job decreases substantially before workers switch the workplace.² In contrast, we focus on the fresh start by analyzing the impact of job changes on job satisfaction.

Freeman (1978) already suggests that job satisfaction might play a double role for job changes, as predictor and result. Since then, the second role has drawn much less attention than the first one, which we consider an important shortcoming. Job satisfaction is very relevant to labor market research and policy as well as to human resource management, as it is linked to performance (e.g. Harter et al. 2010), absenteeism (e.g. Wegge et al. 2007), job search intentions (e.g. Card et al. 2012) and turnover costs (e.g. Swider et al. 2011). Switching the workplace can change all occupational aspects, so that we expect it to be a major source of (dis-)satisfaction. We therefore provide the first comprehensive analysis on this issue, including the investigation of causality. By distinguishing job changes in voluntarily (endogenously) and involuntarily (exogenously) triggered switches, we can answer the question if every parting is indeed followed by a promising start.

Besides the lack of research, our own findings of a previous study motivate the present analysis. Studying the effects of fixed-term contracts on workers’ job satisfaction (Chadi and Hetschko 2013), we show that a striking new job effect biases the job satisfaction of temporary compared to permanent workers upwards, since the former are observed much

¹ *Source:* Fischer (2011, part V, poem 16, ll. 5-10.). Extract from “Stages” (original title “Stufen”), written by Hermann Hesse in 1941.

² E.g. Clark et al. (1998), Clark (2001), Shields and Wheatley Price (2002), Delfgaauw (2007), Lévy-Garboua et al. (2007), Sousa-Poza and Sousa-Poza (2007), Böckerman and Ilmakunnas (2009), Green (2010).

more often in a happy period directly after a job change. Actually, a clear relationship between fixed-term contracts and job satisfaction appears only when controlling for being new in a job. This example shows the importance of considering job switching when empirically analyzing job satisfaction. The new job effect of job changes is able to bias relations between job satisfaction and its covariates, in particular when the variable of interest is correlated with previous switching.

In their study on job changes and job satisfaction of several hundred US-high-level managers, Boswell et al. (2005) reveal a pattern which they term the ‘honeymoon-hangover effect’. In line with the literature on voluntary quits, the managers express dissatisfaction with their work prior to job switching. After changing the job, however, there is a remarkable peak in satisfaction levels. The magic of the new then gradually disappears. At least two issues arise that we address in the present study. We answer whether these findings are valid for workers in general and whether they hold true for exogenously triggered job changes as well.

To the best of our knowledge, Akerlof et al. (1988) provide the only broad investigation of the relationship between job changes and job satisfaction that presents some evidence on workers’ satisfaction after changing the job and is published in an economics journal. Similar to the above-mentioned literature, their main concern is to analyze job switching as a consequence of dissatisfaction among workers using early National Longitudinal Survey data. In recent time, a few researchers have occasionally come in contact with the honeymoon-hangover effect, while focusing primarily on other research issues (e.g. Georgellis and Tabvuma 2010, Gielen 2013).³

Research so far does not clarify whether job changes causally affect job satisfaction. As long as employees decide voluntarily to leave their initial jobs, the drop in well-being beforehand and the honeymoon afterwards may simply reflect rational choices. People switch because they expect increases in their utility. But if involuntarily triggered switches also improve well-being, the link appears to be causal. One explanation could be that workers’ motivation, performance, and, in consequence, rewards are generally above-average when employees find themselves in a new environment.

³ Using data of the British Household Panel Study, both studies find job satisfaction peaks in a new job, followed by adaptation. Georgellis and Tabvuma (2010) use job satisfaction as an indicator for public service motivation and argue that adaptation to a new job is weaker when workers switch into the public sector. Gielen (2013) investigates the effects of quits on job match quality, for which she uses job satisfaction as a proxy.

From the perspective of economics, a finding according to which not only voluntary but also involuntary job changes strengthen workers' welfare would contradict the rationality of choices that microeconomic theory mostly adopts. Potential policy implications, however, are straightforward. Just as dissatisfaction from increased flexibilisation of the labor market can be used to argue against such policy measures, positive effects on workers' satisfaction can be seen as an argument in favor of it. A concrete example is the trade-off between low and high employment protection. The latter hinders firms from both hiring and firing (e.g. Nickell 1997, Cahuc and Postel-Vinay 2002) as well as employees from resigning (Gielen and Tatsiramos 2012). Labor turnover is, thus, the higher the more flexible the labor market. High flexibility of firing people leads to more involuntary workplace changes but also creates vacancies for voluntary switches. Regardless of the other advantages and disadvantages, flexibilisation policies are more promising from a welfare perspective when both types of job changes increase well-being.

The causality issue of the job change–job satisfaction relationship concerns personnel economics and human resource management as well. As mentioned above, work-related well-being is linked to several important organizational factors, such as performance and turnover. Also in the context of newcomer adjustment, job satisfaction is one of the key outcome variables (Bauer et al. 2007). Hence, when people who switched recently are very happy in general, but only at the beginning, this phenomenon may correspond to other important outcomes as well.

The present study investigates how strongly job satisfaction is affected by starting a new job. As we expect the relationship between job changes and job satisfaction to be highly endogenous, we are particularly concerned about the role of selectivity in this context. Exploiting the opportunities of our data, we can investigate the causality question by distinguishing between endogenously and exogenously triggered moves. To make this distinction independent of the complex issue that is mobility within firms, we focus on job changes between employers.⁴ Similar to research on involuntary and voluntary entries into unemployment (Kassenboehmer and Haisken-DeNew 2009), we use German Socio-Economic Panel data (Wagner et al. 2007) to separate between four reasons of job changes from the employee's point of view: quit, mutual agreement about employment termination,

⁴ In line with other studies, we use the terms job changes, changes of workplace and also employer changes interchangeably.

dismissal and plant closure. The last trigger is of special importance for our empirical identification strategy. Plant closures are exogenous events that are not determined by individual characteristics or workers' actions. This enables us to investigate the causal effect of job changes on job satisfaction in a natural labor market scenario.

We proceed as follows. The following section describes our database and methodology. As we use exogenous variation in job switching, the mean analysis in the third chapter already enables us to express preliminary results. Afterwards, we present multiple regression analyses and sensitivity analyses (Sections 4 and 5) confirming our main findings, which we finally conclude and discuss in Section 6.

2. Empirical Framework

2.1 Data

We use data from the German Socio-Economic Panel (SOEP) study, for which the same persons are interviewed year after year. This survey enables us to take a dynamic perspective on job mobility and includes multi-faceted information about job characteristics as well as other life circumstances. The SOEP is a very large survey with members of more than 10,000 households participating each year. It gives us the opportunity to investigate the impact of job changes on job satisfaction as well as the whole honeymoon-hangover pattern for data that is de facto representative for the German working population.

Job satisfaction is obtained in the SOEP via this question: "How satisfied are you with your job? Please answer on a scale ranging from 0 ('completely dissatisfied') to 10 ('completely satisfied')". Job changes and job terminations are inquired for the time since December 31 of the year before last. Because this date predates the previously conducted SOEP interview, employees sometimes report twice on the same event. We identify these cases using the available monthly information on job changes and date of interviews, so that our analysis includes only changes that took place between two SOEP interviews. With respect to the new job, we require that the new position has to be with a different employer, which excludes intra-firm mobility.

Regarding the termination of the previous job, we distinguish between four triggers of job changes. Firstly, we identify switches due to plant closures as involuntary and thus as exogenous. Dismissals seem to be similar as they are determined by the employer and probably not intended by the employee. In a few cases, however, workers who want to leave

the company may not quit but provoke being fired e.g. to receive severance payments. We can also investigate the more common case of voluntary job switching. Mutually agreed contract terminations are often considered a good option for both sides if employer and employee wish to separate at the same time. They can negotiate the conditions of their split-off to bypass employment protection legislation which prevents both from terminating the employment relationship unilaterally. Quitting is considered the most self-determined kind of job termination. Workers are not necessarily free from external pressure when they resign but they can fully decide on the timing. Quits that are followed by a new job may mostly happen because workers assume to improve their situation. This leads to the following order of reasons for job changes depending on the degree of voluntariness (from high to no): (1) quit, (2) mutually agreed termination, (3) dismissal, and (4) plant closure.

As the four triggers of job switching are consistently observed between 2001 and 2011, we restrict the analysis of employer changes to these years. Additional restrictions of the data concern employees doing apprenticeships, self-employed people and workers older or younger than the usual working age in Germany (that is from 18 to 65). Furthermore, we lose observations due to missing values of the variables used in our analysis. Table A1 in the Appendix provides descriptive statistics for our main sample. Realizing that group sizes shrink substantially the more we consider information about the time before and after job changes, we are careful with further restrictions. In our dynamic analyses, we examine two years in the old job before the change and two years in the new job. By conditioning on two different jobs within this time window, we mitigate the role of repeated job switching.

As described in detail in the following subsection, we exploit the available information on job characteristics and life circumstances in the econometric analysis. To gain further insights about the reasons and outcomes of job changes, we employ information that is available until 2007 to provide further descriptive evidence on changers' comparisons between initial and new job (Subsection 3.2).

2.2 Empirical Strategy and Methodology

Our empirical analysis covers three steps. Firstly, we apply a mean analysis describing how the four job change triggers lead to different job satisfaction trajectories around job changes (Section 3). This adds to the descriptive analysis of the just mentioned comparisons between the new and the former job.

In a second step, we make use of the SOEP's panel structure and implement multiple regression analyses with individual (σ_i) and time (τ_t) fixed effects (Section 4). In so doing, we control for time-invariant personal characteristics (e.g. personality) which determine a large proportion of inter-personal variation in happiness data (Lykken and Tellegen 1996). Like in previous studies, using lags and leads in satisfaction data (e.g. Clark et al. 2008, Powdthavee 2011, Georgellis et al. 2012), our outcome variable is assumed as cardinal to ease the interpretation of the results, which may be uncritical from a methodological point of view (Ferrer-i-Carbonell and Frijters 2004). Nevertheless, we address this issue in the course of our sensitivity analysis.

We consider job switching by binary variables representing the four triggers: *quit*, *agreed*, *dismissal*, *closure*. Each type can be distinguished by four points in time before and after the switch (covered by vector L). $t=1$ and $t=2$ denote the last SOEP interviews before the job change and $t=3$ and $t=4$ the first two interviews afterwards. For instance, *closure* _{$t=3$} is the binary variable for the first interview in the new job after the previous employment relationship was terminated due to plant closure. The differences between these points in time cover approximately one year.

Voluntary job changes may be accompanied by other major life events that also affect job satisfaction levels (e.g. Georgellis et al. 2012). However, it is often unclear whether such events are triggers or effects of job changes or neither of the two. We thus present estimation specifications including as well as excluding divorce, child birth, separation, marriage, death of spouse, becoming home owner, relocation, moving together with partner and recently living with someone in need of care in the household (vector of shocks S_{it}). These shocks may affect well-being not only at the time they happen, but also in the long-run. Hence, we take several life circumstances (vector C_{it}) into account. These are the number of children in the household, having a partner, someone in need of care living in the household and home ownership. Thereby, we also consider the variables age squared, overnight stays in hospital in the last year as well as being disabled. Finally, we test the impact of job characteristics (vector X_{it}) which might explain both decreasing satisfaction before and increasing satisfaction after a change of workplace. The features considered are earnings, overtime hours, full-time, company size, sectors, occupation, and autonomy in occupational actions. This leads to the following model:

$$JS_{it} = L_{it}'\beta + S_{it}'\chi + C_{it}'\delta + X_{it}'\phi + \sigma_i + \tau_t + \varepsilon_{it}$$

As our special focus is on plant closures, we extend the model by two further variables that might explain differences between people switching because of this trigger or others: job security and unemployment experience. Before we conduct lags and leads analyses based on the empirical model, we run regressions with only one binary variable for the first year in the new job, which allows us to make use of the full data sample. Moreover, we investigate the hangover effect, for which we examine the further development of employee well-being in the new job by considering tenure.

In the third and final step of the empirical analysis, we test the sensitivity of our results (Section 5). Thereby, we run the regressions based on various subsamples. In so doing, we investigate whether specific groups in the labor market drive our findings. We also address selection issues, using a matching-based approach, and discuss, among other things, some technical issues such as the role of attrition.

2.3 Reference Points

A key issue for interpreting the results of the present study is finding the satisfaction level that constitutes the best reference point for the identification of a causal honeymoon effect through switching between employers. An obvious but unsuitable solution would be to use the information from directly before the switch. At this time, the work-related well-being may already be influenced by the event that causes the transition. Concretely, a future plant closure can severely affect employees in their satisfaction via multiple ways that are unobservable to some extent and thus cannot be controlled in the econometric analysis. Comparing the job satisfaction level after the switch with the level directly before the event would therefore lead to a stronger increase in satisfaction the more distressful the reason for job termination was, independent of the new job in which our study is actually interested in. Hence, to measure the impact of a job change that is not biased by the type of trigger, one should go back another year by using the satisfaction level in $t=1$ as reference point. However, this shifting of the time point may mitigate but not fully solve the problem.

Alternatively, we interpret the $t=3$ coefficients of our fixed-effects estimates as new job effects. In so doing, we consider the individual mean well-being over time as the reference point. This strategy is well suited if adaptation occurs after job changes, as it does with respect to many life events (e.g. Frederick and Loewenstein 1999, Clark et al. 2008). Since we indeed find that changes in well-being after job switches are not sustainable in the long run

(hangover effect), individual mean job satisfaction constitutes an appropriate reference point. We discuss the issue of the reference point further in Section 6.

3. Mean Analysis

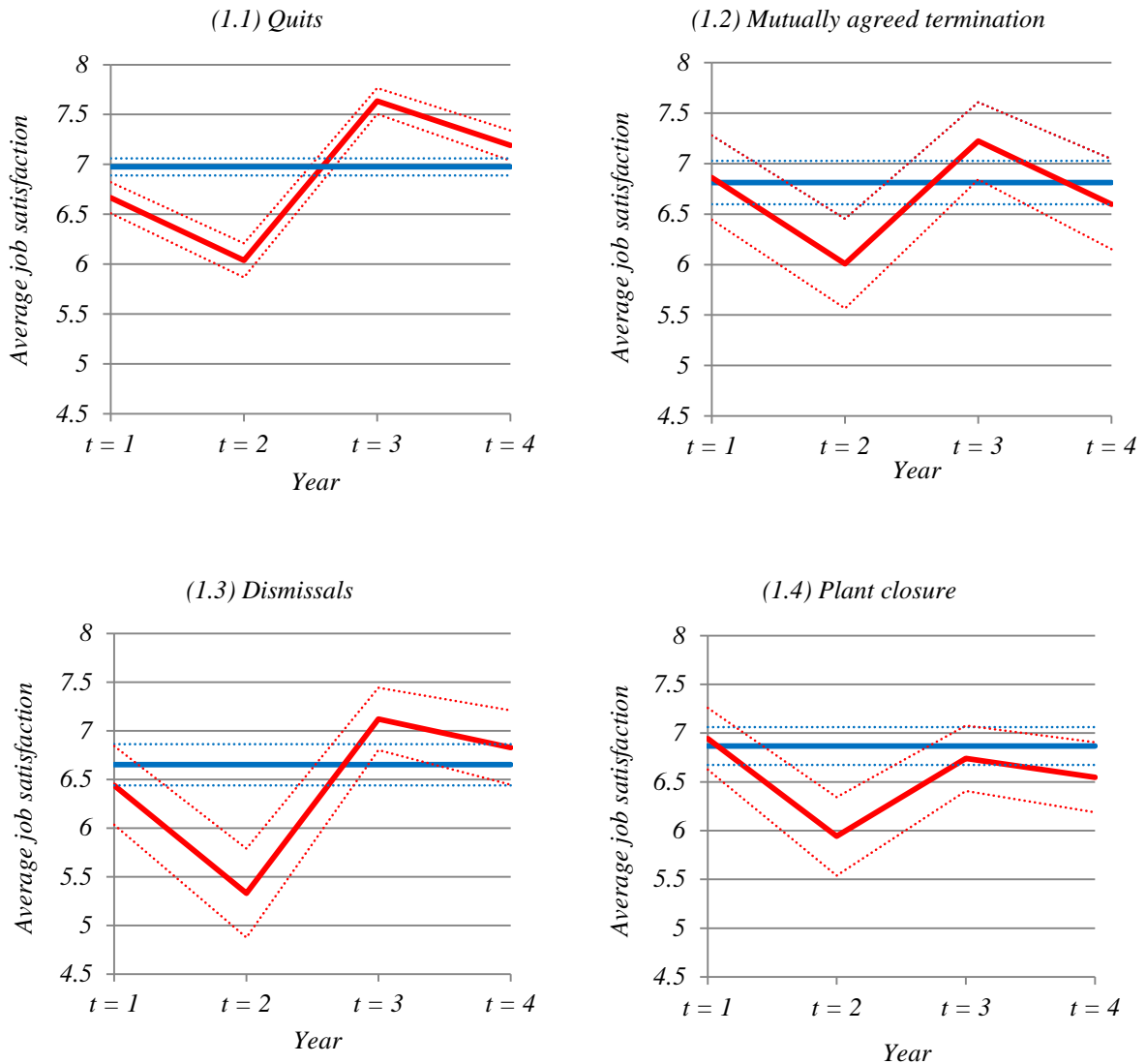
3.1 Graphical Analysis

We start our investigation by examining job satisfaction trajectories around job changes, distinguished between the four triggers of switches.⁵ Referring to the previous discussion on reference points, we consider group-specific averages in job satisfaction of each subsample, for which we use all available data. Figure 1 shows the outcomes of this first step of the empirical analysis and demonstrates that job satisfaction varies substantially before and after employer changes. In all cases, we observe extreme declines in the former job (from $t=1$ to $t=2$) before workers switch to a new employment. Afterwards, however, only three of the four groups show the honeymoon-hangover pattern as described by Boswell et al. (2005).

The job satisfaction of people who resign (Figure 1.1), agree to terminate the employment relation (Figure 1.2) or are dismissed (Figure 1.3) increases and surpasses both the group-specific average and the level in $t=1$ significantly. Resigning people show the highest mark-up compared to their mean satisfaction (about 0.7 points). After $t=3$, the three groups substantially lose satisfaction and end up slightly above (quits) or within the confidence intervals of their group-specific mean. Compared to $t=1$, only those who resigned are significantly better off in $t=4$. In general, the hangover costs almost the whole honeymoon within one year. In contrast to the patterns described so far in this section, the job satisfaction trajectory of people who change their jobs because of plant closures does not lead to a honeymoon (Figure 1.4). Using the job satisfaction in $t=1$ as an alternative reference point leads to the same conclusion.

⁵ Applying this dynamic perspective goes along with a loss of observations, as mentioned above. For instance, using lags and leads in job satisfaction does not allow us to examine job changes at the beginning or at the end of our investigation period. Still, we have more than hundred cases for each type of job change.

Figure 1. Mean Job Satisfaction and the Voluntariness of Job Changes

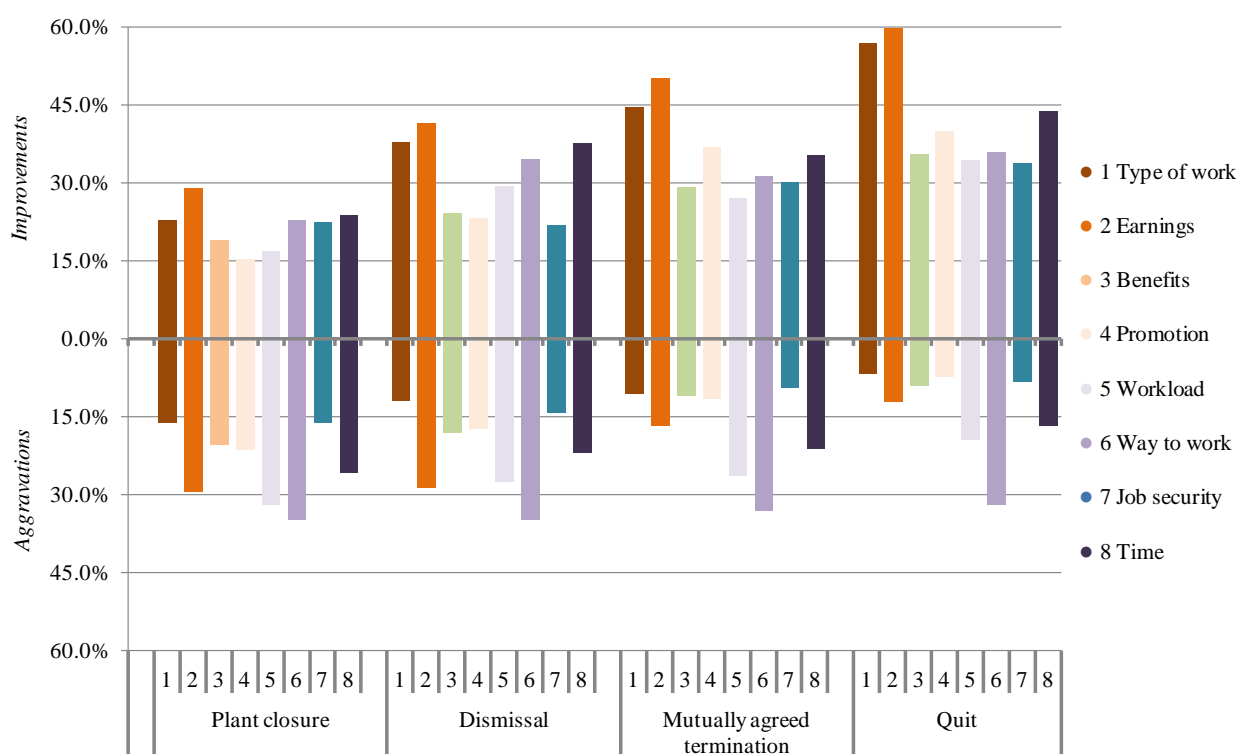


Notes: Points in time ($t=1,2,3,4$) mark time lags of approximately one year. Job changes take place between $t=2$ and $t=3$. Red lines denote switching because of the trigger mentioned in the respective diagram title. Blue lines represent individual job satisfaction means of each group. Dashed lines denote 95% confidence intervals.

3.2 Improvements and Aggravations in Job Characteristics

The information on how people view their new job in comparison to the previous one sheds some light on the reasons for voluntary job changes on the one hand, and it helps to understand why involuntary job changers do not experience honeymoon effects on the other hand. Figure 2 displays the shares of changers for each trigger who report improvements and aggravations in job characteristics.

Figure 2. Job Changes and Job Characteristics



Note. The remaining shares of changers neither improve nor deteriorate in the respective characteristic.

The pattern for workers who change because of plant closures is ambiguous. Job security and the type of work show significantly more improvements than aggravations, while the reverse applies to promotion probabilities, workload and length of the way to work. Earnings, fringe benefits and working time are neither dominated by improvements nor by aggravations. In all cases, the largest shares of people report no change.

Other job change triggers lead to more beneficial outcomes. People who were fired report significantly more often improvements than aggravations with respect to most of the features. No tendency is only shown in the cases of workload as well as length of the way to work and aggravations never dominate. Although this picture is mostly positive for fired workers, mutually agreed terminations and quits appear to be even more beneficial. In these cases, much more people gain than lose with respect to each characteristic, whereby vast majorities of workers report improvements in earnings and the type of work. These results indicate that

workers who resign, agree to terminate their employment and, though less pronounced, are dismissed improve their situation when they switch in contrast to those who switch their jobs due to plant closures. If voluntary job changes are at least to some extent motivated by the job characteristics analyzed, the differences between the four groups strengthen our expected order of triggers, as measured by voluntariness.

4. Regression Analyses

4.1 The New Job Effect

We first describe results on the effect of having a new job by implementing a model without neither lag nor lead variables. The corresponding outcomes in Table 1 are based on the main data sample. Table A2 in the Appendix presents the complete results. Beyond our main findings, the analysis reveals some general job satisfaction predictors among job characteristics. For instance, work-related well-being increases in earnings and autonomy in occupational actions. We do not further comment on these control variables as they by and large show the expected effects in the regressions.

The basic new job effect in workers' satisfaction levels appears to be enormous. The binary variable reflecting a recent switch to a different employer indicates a very strong increase in job satisfaction, far beyond the individual mean. Throughout the different specifications, the magnitude remains almost unchanged and hence independent of influences from observable aspects of the job. The positive impact is about half a point on the satisfaction scale, which is stronger than most of the effects revealed when analyzing the determinants of job satisfaction (see Appendix Table A2).

The key question of whether the remarkable honeymoon effect of a new job is particularly strong because of the large role of voluntary job switching can be addressed directly via our research design. In the last column of Table 1, we differentiate between the reasons why workers switch the employer and find that the new job effect is clearly driven by the large group of voluntary job changes. The more involuntary a change of workplace, the lower is the impact on job satisfaction. Amazingly, the positive new job effect completely disappears when workers were exogenously forced to switch due to plant closure. This shows that in the labor market at hand, we have enormous honeymoon effects in workers' new jobs but the phenomenon is not causally linked to the job change itself.

Table 1. The New Job Effect

Specification	1	2	3	4	5
Job change	0.502*** (0.044)	0.500*** (0.044)	0.502*** (0.044)	0.509*** (0.044)	
Job change due to quit					0.663*** (0.053)
Job change due to mutual agreement					0.438*** (0.133)
Job change due to dismissal					0.352*** (0.111)
Job change due to plant closure					-0.007 (0.126)
Year dummies	yes	yes	yes	yes	yes
Shocks		yes	yes	yes	yes
Life circumstances			yes	yes	yes
Job characteristics				yes	yes
Observations	72,428	72,428	72,428	72,428	72,428
Number of persons	15,205	15,205	15,205	15,205	15,205
Adjusted R-squared	0.016	0.016	0.016	0.021	0.021

Notes: * denotes significance at the 10% level, ** at the 5% level and *** at the 1% level. Robust standard errors are in parentheses. The dependent variable is job satisfaction. Regressions consider individual fixed effects. Complete results are presented in Table A2 in the Appendix.

4.2 Dynamic Analysis

We continue the analysis by expanding our empirical model with lags and leads variables, which reduces sample size. The goal is to learn more about the development of employees' satisfaction in the course of job switching by applying a dynamic perspective. We start without differentiating between the reasons for previous job termination and hence display the overall pattern for all changes of employer.

In line with the honeymoon-hangover pattern, the results in Table 2 show a remarkable job satisfaction peak in the first year of a new job ($t=3$), while in the second year the effect is still significantly positive ($t=4$) but much smaller. Beforehand, the employees are dissatisfied in their former job ($t=1$), which becomes worse in the final year ($t=2$). As expected, the experience of unemployment and overall job security both play a role for job satisfaction levels. Yet, the job change effects remain strong and significant even when we include variables capturing such variation. To find out what drives this honeymoon-hangover, we implement the full empirical model with lags and leads for each of the four job move triggers.

Table 2. Dynamic Analysis of the Job Change Pattern

Specification	1	2	3	4	5
Job change t=1	-0.374*** (0.049)	-0.377*** (0.049)	-0.367*** (0.049)	-0.351*** (0.049)	-0.312*** (0.048)
Job change t=2	-0.790*** (0.063)	-0.789*** (0.063)	-0.776*** (0.063)	-0.769*** (0.063)	-0.704*** (0.061)
Job change t=3	0.489*** (0.074)	0.487*** (0.074)	0.483*** (0.074)	0.474*** (0.074)	0.457*** (0.074)
Job change t=4	0.168*** (0.036)	0.170*** (0.037)	0.173*** (0.037)	0.159*** (0.037)	0.161*** (0.036)
Unemployment experience				0.351*** (0.122)	0.297** (0.121)
Job security, lowest level 1					-0.524*** (0.033)
Job security, highest level 3					0.292*** (0.023)
Year dummies	yes	yes	Yes	yes	yes
Shocks and life circumstances		yes	Yes	yes	yes
Job characteristics			Yes	yes	yes
Observations	42,097	42,097	42,097	42,097	42,097
Number of persons	10,389	10,389	10,389	10,389	10,389
Adjusted R-squared	0.026	0.027	0.030	0.030	0.049

Notes: * denotes significance at the 10% level, ** at the 5% level and *** at the 1% level. Robust standard errors are in parentheses. The dependent variable is job satisfaction. Regressions consider individual fixed effects. The first year in the new job is t=3. Job changes are restricted to cases of one previous job (in t=1 and t=2) and one new job (in t=3 and t=4).

The results in Table 3 demonstrate clearly that the general picture for all job changes revealed in Table 2 is driven by voluntary switches of employer. They lead to significantly higher job satisfaction in t=3 compared to the individual mean, which is particularly true for quits but also for mutually agreed terminations. The positive effect of having changed the workplace voluntarily lasts until t=4 although well-being strongly declines in both cases. In contrast, dismissed workers also experience higher job satisfaction in the new job, but the effects are not statistically significant. This difference to the results presented in Table 1 may appear due to the reduced sample size. In contrast, the results for the case of plant closure are clear-cut and robust. There is no honeymoon-hangover effect in any respect when we look at exogenously triggered job changes: Neither the difference to the individual mean nor the difference to t=1 is statistically significant.

Table 3. Dynamic Analysis of Different Types of Job Changes

Specification	1	2	3	4	5
Quit t=1	-0.360*** (0.081)	-0.361*** (0.081)	-0.348*** (0.081)	-0.345*** (0.081)	-0.320*** (0.081)
Quit t=2	-0.949*** (0.101)	-0.945*** (0.101)	-0.931*** (0.101)	-0.927*** (0.101)	-0.918*** (0.099)
Quit t=3	0.715*** (0.094)	0.710*** (0.094)	0.699*** (0.094)	0.699*** (0.094)	0.656*** (0.093)
Quit t=4	0.365*** (0.071)	0.363*** (0.071)	0.353*** (0.070)	0.353*** (0.070)	0.334*** (0.069)
Agreed t=1	-0.325*** (0.115)	-0.323*** (0.115)	-0.317*** (0.115)	-0.306*** (0.115)	-0.251** (0.113)
Agreed t=2	-0.741*** (0.161)	-0.729*** (0.160)	-0.700*** (0.160)	-0.680*** (0.160)	-0.618*** (0.159)
Agreed t=3	0.539** (0.212)	0.548** (0.213)	0.565*** (0.213)	0.566*** (0.213)	0.556*** (0.204)
Agreed t=4	0.258* (0.137)	0.265* (0.137)	0.268** (0.136)	0.270** (0.136)	0.280** (0.136)
Dismissal t=1	-0.296*** (0.085)	-0.296*** (0.085)	-0.296*** (0.086)	-0.273*** (0.087)	-0.239*** (0.086)
Dismissal t=2	-1.196*** (0.127)	-1.190*** (0.127)	-1.182*** (0.126)	-1.148*** (0.128)	-1.022*** (0.125)
Dismissal t=3	0.128 (0.207)	0.128 (0.207)	0.133 (0.207)	0.127 (0.207)	0.180 (0.206)
Dismissal t=4	0.211 (0.144)	0.210 (0.144)	0.214 (0.144)	0.202 (0.144)	0.185 (0.144)
Closure t=1	0.017 (0.108)	0.014 (0.108)	0.012 (0.108)	0.029 (0.107)	0.069 (0.106)
Closure t=2	-0.805*** (0.152)	-0.808*** (0.152)	-0.807*** (0.152)	-0.775*** (0.152)	-0.593*** (0.147)
Closure t=3	-0.163 (0.193)	-0.159 (0.194)	-0.171 (0.193)	-0.167 (0.193)	-0.183 (0.190)
Closure t=4	-0.036 (0.147)	-0.037 (0.148)	-0.032 (0.148)	-0.029 (0.147)	0.001 (0.143)
Year dummies	yes	yes	yes	yes	yes
Shocks and life circumstances		yes	yes	yes	yes
Job characteristics			yes	yes	yes
Unemployment experience				yes	yes
Job security					yes
Observations	42,097	42,097	42,097	42,097	42,097
Number of persons	10,389	10,389	10,389	10,389	10,389
Adjusted R-squared	0.026	0.027	0.030	0.030	0.049

Notes: * denotes significance at the 10% level, ** at the 5% level and *** at the 1% level. Robust standard errors are in parentheses. Regressions consider individual fixed effects. The first year in the new job is in t=3. Job changes are restricted to cases of one previous job (in t=1 and t=2) and one new job (in t=3 and t=4).

The comparison of different specifications reveals that neither life events nor life circumstances or job characteristics can fully explain the job satisfaction patterns observed.

The coefficients of lags and leads stay nearly the same in size and significance while variables are added to the model. A more profound discussion of whether these factors play a role in the job-satisfaction-job-change relationship is hence redundant. Even the experience of unemployment between the two jobs cannot explain why a voluntary change of workplace leads to a honeymoon period and involuntary changes do not. Only holding self-assessed job security constant seems to weaken the effect sizes in $t=2$ prior to dismissals and plant closures.

4.3 Adaptation to the New Job

The regression models implemented so far do not include tenure, as this would obviously bias the illustration of the new job effect. In the following, we include tenure as the variable of interest, which, thanks to our research design with its focus on employer changes, helps us in investigating the hangover phenomenon. The idea is to analyze a sample with both employees who continue working for their employers and newcomers to the organization. According to our distinction between the different reasons for a job change, we can separate the newcomers into four subgroups and implement interactions with tenure.

Table 4 shows that job satisfaction generally decreases in tenure, which is in line with recent findings (e.g. Theodossiou and Zangelidis 2009, Georgellis et al. 2012). As we are primarily concerned with group differences in this trend, we employ models using one linear tenure variable, reflecting the overall trend, and interactions with newcomer indicator variables. For the aggregated group of all newcomers combined, we observe a significant interaction effect in the first specification. This implies that the decline in job satisfaction is significantly stronger for newcomers, in line with the expectation of a hangover effect. The subsequent specifications show strong differences between subgroups of newcomers that confirm our previous findings. As there is no honeymoon effect in job satisfaction levels for those who are forced to switch the workplace, there is also no hangover. For employees who agreed to terminate the initial job, a slightly significant interaction effect appears, suggesting a hangover. Only for the largest group of workers who resigned, can we find a strong and significant decrease in satisfaction, far beyond the overall decline.⁶ As quits precede the overwhelming majority of job changes, this group drives the overall pattern in Specification 1.

⁶ If we drop workers with high tenure to increase the comparability between newcomers and other employees, the finding of a stronger decline in satisfaction among voluntary job changers stays the same.

Table 4. The Hangover Effect of the New Job

Specification	1	2	3	4	5
Reason for changing the workplace	all	only quit	only mutual agreement	only dismissal	only plant closure
Tenure	-0.043*** (0.007)	-0.043*** (0.007)	-0.043*** (0.008)	-0.043*** (0.008)	-0.043*** (0.008)
Interaction: Newcomer x Tenure	-0.063*** (0.013)	-0.076*** (0.016)	-0.054* (0.032)	-0.041 (0.036)	-0.011 (0.043)
Year dummies	yes	yes	yes	yes	yes
Shocks and life circumstances	yes	yes	yes	yes	yes
Job characteristics	yes	yes	yes	yes	yes
Observations	54,692	50,910	50,928	50,936	53,202
Number of persons	7,652	7,059	7,065	7,058	7,413
Adjusted R-squared	0.026	0.023	0.024	0.024	0.026

Notes: * denotes significance at the 10% level, ** at the 5% level and *** at the 1% level. Robust standard errors are in parentheses. The dependent variable is job satisfaction. Regressions consider individual fixed effects. The full sample (column 1) is restricted to only one type of job change each in columns 2 to 5.

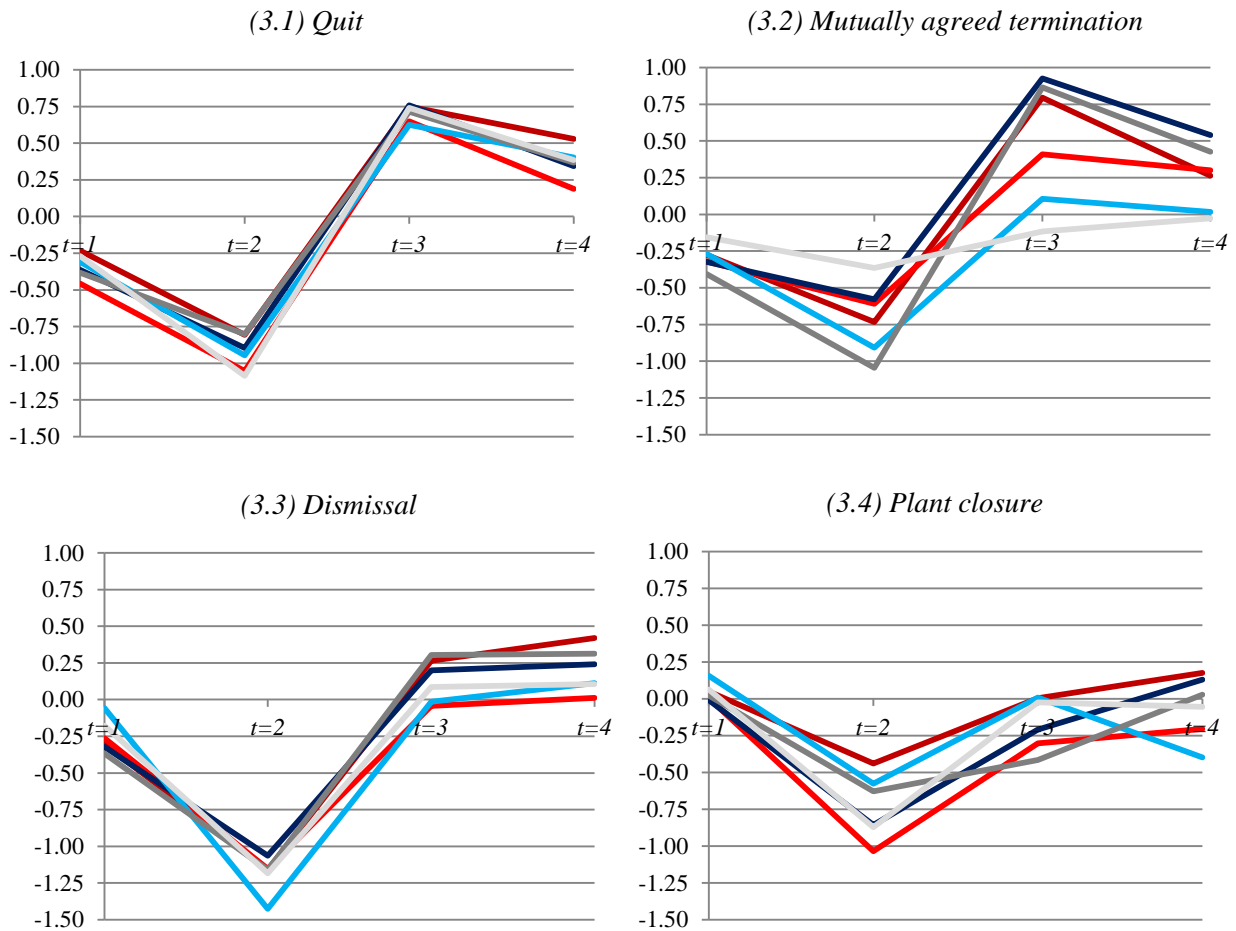
There is a multitude of interesting facets of the job change patterns revealed in our job satisfaction data. Yet, we cannot delve into deeper analyses of every particular aspect that may be of interest to the researcher, the labor policy-maker or the personnel manager. As we try to address the most pressing issues, we limit the following discussion to sensitivity analyses of potential selection effects and more technical aspects regarding methodology and data validity. Beforehand, we examine the replicability of our results for various subgroups.

5. Sensitivity Analyses

5.1 Subsamples

We differentiate the four job change types by gender (women/men), education (at most/more than 12 years) and age (at most/more than 40 years) to investigate whether our main results are driven by specific subgroups. The tests apply individual fixed effects regressions to our main model. We present the results in Figure 3 which draws the coefficients of the job-change-type-point-in-time variables such as $closure_{t=3}$. It is important to keep in mind that these values are predicted on the basis of a model that considers the impacts of various life events, job characteristics etc. on job satisfaction.

Figure 3. Job Changes and Job Satisfaction by Gender, Age and Education



Notes: The red lines denote different gender groups (bright: male, dark: female). The blue lines denote different years of education groups (bright: more than 12, dark: at most 12). The grey lines denote different age groups (bright: above 40 years, dark: 40 and below years). The values are coefficients predicted by group-specific regressions considering individual fixed effects.

The job satisfaction of the six subgroups around job changes develops most similarly in case of quits (Figure 3.1). The six trajectories run parallel and close together from $t=1$ to $t=4$ while they reveal considerable honeymoon-hangover patterns: The subgroups' job satisfaction levels in $t=3$ and $t=4$ are significantly higher than on average, as well as than in $t=1$, and decrease between $t=3$ and $t=4$. As Figure 3.2 shows, people in all of the six subgroups start at nearly the same well-being level that is below-average (significant among men and less educated people) in $t=1$ before they agree with their employers to split up. All subgroups experience declining job satisfaction until $t=2$, arriving at a significantly negative level compared to mean well-being. In contrast to their counterparts, women ($t=3$), less educated

($t=3$ and $t=4$) and younger workers ($t=3$ and $t=4$) report job satisfaction significantly above their mean levels. The well-being of women, men, less educated and younger workers is significantly higher in $t=3$ and $t=4$ compared to $t=1$.

Except in the case of older workers (40 years and more) and the better educated (12 years and more), subgroups report satisfaction levels that are significantly below average in $t=1$ when they are dismissed between $t=2$ and $t=3$ (Figure 3.3). The loss of job satisfaction from $t=1$ to $t=2$ is substantial and does not differ between the groups. In consequence, their job satisfaction deviates negatively at least in the amount of one point from the respective mean levels in $t=2$. In $t=3$ and $t=4$, only women reach a significant above-average satisfaction level in. As Figure 3.4 reveals, the different genders, age groups and education groups that are affected by plant closure start at very similar job satisfaction levels in $t=1$. At this point in time, they are not different from mean satisfaction on a statistically significant level. While in the year before the job change, $t=2$, job satisfaction is below average for all groups, satisfaction with the new job does not surpass the mean level or the level of $t=1$ significantly. Thus, our finding that involuntary job changes are not able to trigger honeymoon effects is robust across all six subgroups.

5.2 Potential Selectivity

We identify large and highly significant differences in job satisfaction with respect to our variables of interest. The key finding that the very huge positive impact found for the standard case of voluntary job changes completely disappears in case of an exogenous trigger may, however, be affected by some kind of selectivity. Recall that the idea of using exogenous variation in the data relates to the problem of endogeneity in most of the switches observed. Accordingly, the group of voluntary job changers constitutes a selective sample of more fortunate cases with regard to the outcomes of switching. This obviously drives the strong positive effect in workers' satisfaction and is the reason why we need an exogenous trigger of job changes to investigate causality. Nevertheless, selection effects within the group of those affected by plant closures might also play a role. First of all, not every former employee finds a new job within a certain period of time. We address this form of attrition via some descriptive information, while we comment on overall panel attrition below.

Many individuals become unemployed after a plant closure and search work for more than just a short period of time. Unemployment is a potential consequence of every type of job termination, even in cases of quits. Table B1 in the Appendix shows descriptive information

on individuals who are either employed in a new job or who are out of labor force after ending previous employment.⁷ We shed some light on the issue of selection with respect to both involuntarily (plant closure) and voluntarily (quit) triggered switches. For the former, the differences revealed are intuitive and point to a selection of more unfortunate cases into non-employment. Yet, the comparison overall suggests no severe differences between those who end up in our sample and those who left the labor force following plant closure. The latter are older, more often disabled and a little less educated. Looking at the situation two years prior to the plant closure, the impression of a mild selection of potentially weaker performers into non-employment is further substantiated. Compared to employees with a job later on, those without it are more likely to be blue collar workers with lower earnings and less autonomy in their previous job, which expresses lower hierarchical rank. With regards to quits, individual differences between those who take up a new job and those who leave the workforce are more severe than in case of plant closures. The selection here might be driven by women who first resign but then neither continue working in paid employment nor register themselves as unemployed.

We conclude that the type of selection into non-employment among those affected by plant closures poses no threat to our key finding. Even if the selection of more fortunate cases into jobs after plant closure would be more severe than we argue it is, it cannot alter our main finding qualitatively. The more relevant such kind of positive selection bias, the more negative is the actual effect of exogenously triggered changes. A problem with this result can only emerge if the insignificant new job effect is biased by a selection of negative cases. Yet, while the attrition out of the labor force does not confirm such notion, there could be another form of selectivity that we therefore investigate more deeply. Accordingly, there may be some well-performing employees who are induced to change the employer due to future plant closure who have foreknowledge and, thus, foresee the event. These workers may still be attractive to other employers, despite the negative development of their current firm. If these individuals accurately anticipate the plant closure and have the opportunity to quit voluntarily they may end up in a different subgroup according to our differentiation into job change types. If this is a relevant argument, the new job effect found for our exogenous trigger of job changes may be too low due to negative selection.

⁷ We restrict these samples by requiring individuals to be observed two years earlier in a job (with no job change in the year between). This allows comparing the same persons two years earlier in the previous job and in the situation directly after job termination.

We implement a matching-based strategy to address the potential issue of negative selection of workers switching after plant closures. The idea is to perceive the latter as treatment group to which we construct a control group that is as comparable as possible. If matching is successful, we can tackle the selection issue to ensure unbiased outcomes. In this context, we can exploit data on future expectations that SOEP respondents are asked about biennially. This automatically reduces observation numbers by about half, but we still have almost a hundred cases of workers switching after plant closures. This is similar to a study by Marcus (2013) to which this part of our robustness analysis comes very close. As in his comparison of individual health prior and after plant closure, we also use standard and novel matching tools. While the former exploits *propensity scores* (for a review, see Caliendo and Kopeinig 2008), the latter is the reweighting technique *entropy balancing* (Hainmueller 2012). The parallel use ensures that our results are not dependent on a specific way of matching. Table B2 gives descriptive information on the matching quality by showing means for the conditioning variables before and after applying matching.

Table 5 shows job satisfaction effects of employer changes using a common difference-in-difference (DiD) approach. Accordingly, we determine differences in job satisfaction levels between $t=1$ and $t=3$ and compare treatment and control groups with respect to these differences. Whether we apply reweighting via entropy balancing (Specification 2) or propensity score weighting (Specification 3), the effect of a job change triggered by plant closures is the same as in the case of a simple regression with covariates (Specification 1) and always insignificant. The same holds true for treatment effects that we measure when conducting standard kernel matching, for which we also vary the bandwidth in the last specification of the table. Instead of the difference in job satisfaction compared to $t=1$, we can also calculate the difference to the individual mean using matching techniques, which yields the same finding as before.⁸ We conclude that selection of negative cases into our treatment group is not a significant problem.

⁸ The only exception to this comes from analyses based on a larger data sample, which we can do easily by exclusion of the biennially observed variables on people's future expectations. Thereby, we increase the number of observed plant closure instances in our treatment group. The results do change slightly and mostly become negative, albeit in all cases insignificantly from zero. Since the selection argument relates to the anticipation of future events, we prefer the analysis based on the smaller data set, while noting that the (insignificant) effects shown in Table 5 are probably 'too' positive.

Table 5. The Impact of Job Changes on Job Satisfaction (DiD Analysis)

	(1) Standard Regression	(2) Entropy Balancing	(3) Propensity Score Weighting	(4) Kernel Matching ($b=0.06$)	(5) Kernel Matching ($b=0.03$)
Job change	0.004 (0.245)	0.012 (0.200)	0.010 (0.246)	0.038 (0.250)	0.032 (0.251)

Notes: The standard DiD regression (1) includes control variables according to Table B2. The same holds for Specifications 2 and 3, which display regression results after reweighting via entropy balancing and propensity score weighting. For the latter, the observations are weighted by 1 divided by 1 minus the propensity score. Specifications 4 and 5 present average treatment effects for the treated from propensity score matching (i.e. Epanechnikov kernel matching) using the standard bandwidth (b) of 0.06 respectively 0.03 as an alternative. In contrast to the robust standard errors in the first three columns, the standard errors in parentheses result from bootstrapping (based on 150 replications). The sample includes 23,358 observations with 90 individuals in the treatment group.

5.3 Further Robustness Checks

We conduct additional analyses, on which we report rather briefly in the following. The role of unemployment between the old and the new job is considered via an additional control variable in the above regressions. The investigation of this aspect can of course be carried out more extensively, for instance, using interaction terms, but this does not change our findings. Neither the large positive new job effect of voluntary job changes nor the insignificant outcome for involuntary switches is driven by this aspect.⁹ Furthermore, we have so far not dropped out observations from people who work in atypical forms of employment such as in case of agency workers. If we do so, the findings remain the same, even though observation numbers go down. In the context of fixed-term contracts, we refer to our separate study on this issue (Chadi and Hetschko 2013). To fully ensure the exogeneity of the plant closure treatment, one may also consider dropping smaller firm sizes. The smallest category is organizations with less than twenty employees. Restricting the data to firm sizes with at least 20 employees changes two results: The honeymoon-hangover pattern following mutual agreements becomes less pronounced and the job satisfaction effects for those affected by plant closure become generally more negative.

⁹ The only new insight from these analyses is that a more positive new job effect seems to emerge for employees after a dismissal if they have been unemployed in the meantime.

Repeated job switching is a phenomenon that may occur more often in case of involuntarily enforced job changes. Hence, we restrict the data to employees with no more than one job change in the entire period of investigation which does not affect our findings. Recall that in the dynamic analyses above we already require job changes to be observed for at least two years in the new job, while the previous job is observed for the last and year before last year. If we relax this point, so that case numbers go up again, the findings are also the same.¹⁰

Panel attrition might influence the results of our analysis if particularly satisfied or dissatisfied individuals are more likely to leave the survey in the course of job changes. In case of plant closures, it could be that very dissatisfied individuals refuse continuing in the survey, which would lead to a loss of unhappy persons. As discussed above, such kind of negative attrition and a possible upwards bias in the job satisfaction of remaining individuals cannot alter our findings concerning plant closures qualitatively. Yet, the extraordinarily high levels of satisfaction after voluntary job changes may be overestimated when selective attrition is at hand. While selectivity is present in this group anyway, we are still interested in looking deeper into these technical questions, especially as economists have become aware of potential survey artefacts in recent time. Referring to the idea of Heffetz and Rabin (2013), we find that those who change the workplace are somewhat harder to reach by the interviewers. On that note, the probability of subsequent attrition after a switch is generally higher, which is not unexpected. However, we can find neither significant differences between the job change triggers nor do we observe within any of the four groups that those who leave the panel are more or less satisfied with work than those who continue to participate.

People have a desire of portraying themselves more positively when other persons are present during the interview (e.g. Conti and Pudney 2011, Chadi 2013a). Such social desirability bias may play a particular role in the context of recent job changes. Using interaction terms between job change dummies and interviewer presence, we find that job satisfaction is much higher in case of the latter but there are no robust interaction effects that indicate biased findings on the impact of job switching. The same holds for potential measurement effects due to changes of interviewers or varying panel experience (Chadi 2013b). One could expect the latter to play a role for the analysis of tenure effects, as self-reported job satisfaction appears to decline strongly in the first years of panel participation,

¹⁰ Yet, the new job effect after quits becomes smaller when we allow repeated job switching. For the first year, the coefficient goes down from more than 0.6 to somewhat higher than 0.5.

but the negative hangover effect in satisfaction with the new job is robust against differences in respondents' panel experience.

Finally, the estimation method is often subject to debate among empirical researchers of well-being. Apart from the linear regression method which is easy to implement, other options are available to address the ordinal nature of the dependent variable. Using the conditional logit estimator by Chamberlain (1980), which requires collapsing the 11-score satisfaction data into a binary satisfied/dissatisfied variable, leads to the same findings.

6. Conclusion and Discussion

6.1 Findings

Job satisfaction varies enormously before and after job changes. While it is known that dissatisfaction with work often precedes the switch to a different employer, much less research exists on satisfaction with the new job as an outcome of the change. In our broad investigation of this question, we find that the term honeymoon-hangover effect (Boswell et al. 2005) describes the general job satisfaction pattern very well. Employees are extremely satisfied in their first year of a new job, far beyond their individual satisfaction mean. The magnitude of this effect appears to be, on average, about half a point on the job satisfaction scale. It turns out that the enormous satisfaction among organizational newcomers cannot be sustained, so that adaptation takes place in the following years of an employee-employer relationship.

Our study is one of the first revealing the honeymoon-hangover pattern in representative data from a large labor market. An important novelty is the analysis of the causal effect of job switching. Undisputedly, the enormous satisfaction among recent job changers is driven by selectivity. Those who expect something positive tend to switch, those who do not tend to stick with their employer. To get rid of the endogeneity problem, we require an exogenous trigger of job moves, for which we exploit information on the reasons why employees terminate a job before switching to a different employer. Four types of job change triggers can be distinguished consistently in our data set: quits, mutually agreed terminations, dismissals and plant closures.

The findings from this distinction are remarkably consistent and confirm the major role of voluntariness. The honeymoon-hangover pattern becomes even stronger if we examine only quits, as the most voluntary type. The new job effects are also quite robust, but somewhat

smaller after a mutually agreed termination between employer and employee, indicating a lower level of voluntariness. As people usually do not intend to get fired, job changes after dismissals are mostly of an involuntary kind, which goes along with less often observed honeymoon experiences at the new workplace.

Most importantly, the positive new job effect in job satisfaction disappears completely as soon as we focus on job changes after plant closures. In all of our attempts to detect effects from job switching, the satisfaction outcomes we find in this case are neither significantly different from individual mean levels nor shift well-being beyond its starting point at the beginning of our investigation period (that is approximately two years before the change of workplace). We conclude that there is no causal effect of job changes on job satisfaction. The same applies to the hangover effect that is only experienced by voluntary changers.

Further empirical evidence supports our approach and the conclusions drawn. In direct comparisons between the new and the former job, employee evaluations show a picture that strongly complies with our classification of job changes according to their voluntariness. In fact, the more voluntary the switching, the more likely newcomers report improvements in job attributes (e.g. earnings, promotion prospects). Meanwhile, the outcomes in such a comparison are much weaker for job changes after dismissals, and even more so after plant closures. In order to make sure that the poor results on exogenously triggered switches are not driven by some form of selectivity, we carry out several sensitivity analyses, including the application of matching-based estimation strategies. We also separate our sample into subgroups according to gender, age, and education level, which reveals only few differences and thus confirms the generalizability of our results. Furthermore, other potentially relevant job-related factors, such as unemployment experiences and job security, are neither capable of fully explaining the enormous honeymoon effects nor the large differences in job satisfaction between the switch types investigated.

6.2 Potential Directions of Future Research

An important question for the analysis and assessment of job changes is the choice of the appropriate reference level of job satisfaction. Boswell et al. (2009) only observe the satisfaction directly before the switch, in our terms $t=2$, and discuss their findings with respect to this point in time. They do not find significant differences between voluntary and involuntary changers, which might be due to the fact that there are very few cases of the latter in their sample, but may also be the consequence of the reference point chosen. We argue that

the job satisfaction in $t=2$ is strongly affected by the circumstances leading to the job change and, hence, is not suitable as reference point. The empirical evidence on the extreme and largely unexplained dissatisfaction of workers at the end of their tenure supports this view and thus our choice to apply both job satisfaction in $t=1$ and, first and foremost, the individual mean over time. A direction of future research might be to disentangle harbingers of the events that lead to the termination of a job and to study effects on workers' well-being before it happens.

A first explanation why the enormous new job effects we find have not been recognized in job satisfaction data so far refers to our definition of job switches, which is restricted to changes of employer. This makes the identification clear and consistent, as e.g. changes within firms can be interpreted differently and are sometimes reported as a new job but sometimes not. An aggregate variable that is used in all studies without specific job change distinction leads to other outcomes. With regard to internal mobility, we would expect honeymoons to appear mainly in case of promotions, but we leave this topic to future investigations. A second explanation relates to tenure, which most empirical researchers of job satisfaction routinely consider as a control variable. This might only partially address the honeymoon-hangover pattern. In our data, the magnitude of the overall new job effect is so large that it remains significant even if we control for tenure. When researchers are interested in variables being correlated with the probability of being new in a job, the effect on job satisfaction might be biased and considering tenure might not solve the issue sufficiently. A third relevant aspect for our findings is the labor market itself. We have data from a well-regulated labor market, which implies that workers are rather difficult to dismiss and voluntariness in job switching is the norm in Germany. It would be very promising to have more studies on job satisfaction effects from job changes and to compare countries with different levels of employment protection and flexibilisation.

Analyzing job satisfaction is of special importance, as it is strongly linked to many organizational outcomes. Therefore, we focus on the impact of job switching on this measure of well-being, while other outcomes may be relevant as well. Since our identification strategy cannot be transferred directly to alternative indicators, such analyses lie beyond the scope of our investigation, leaving room for further studies. Besides many other aspects, it would be interesting to investigate whether emotions at the workplace rather than cognitive evaluations of employment drive honeymoon and hangover. A way to answer this question is to analyze

positive and negative affect together with job satisfaction. Moreover, job changes do not necessarily concern working life only. This speaks in favor of investigating the impact of job switching on satisfaction with other domains of life, and possibly also on life satisfaction as a global indicator, to complement our findings.

6.3 Policy Implications

With respect to labor market policy, the enormous increases in job satisfaction that are possible via job changes seem to be an argument for more flexibilisation. However, the overwhelming majority of cases observed in the data are beneficial for the individuals because they are voluntary and display rational decisions. The positive experience of a new job is not a given fact, as we find that the causal impact of a job change on job satisfaction is insignificant. When employment protection legislation is reduced, so that more people change involuntarily, their work-related welfare does not necessarily benefit from that. However, fostering flexibility may create more vacancies which help switch-willing employees to improve their situation. Hence, when flexible labor markets allow workers to change jobs voluntarily, they may help them to maximize welfare. Yet, the question is whether such voluntariness as observed here is present in a labor market that is rendered more flexible via political measures. Besides, as the honeymoon of the new job is usually followed by a strong hangover effect, the benefits of job changes are not necessarily sustainable.

Our findings are important with regard to human resource management, as job satisfaction is also in this context regarded as an important policy goal. Personnel managers thus need to take into account the circumstances under which workers moved to a firm. Those who changed voluntarily may be very motivated and unlikely to leave the firm in the first years, in contrast to employees who lost their previous jobs involuntarily. As the former may already have a strong commitment to the new challenge, which certainly fosters successful integration, management is well advised to take special care of the latter. The strong hangover that most employees experience after the honeymoon period also deserves special attention, as risk of turnover and drop in performance may increase in tenure. In this respect, our results clearly show that the happier the honeymoon is, the stronger the hangover will be. Beyond the scope of our research, however, lies the question of how to deal with this phenomenon, or, to refer again to Hermann Hesse, how to maintain this special magic of the new.

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Appendix

Table A1. Descriptive Overview

	No new Job	Quit	Mutually agreed termination	Dismissal	Plant closure
Number of observations	69,815	1,594	311	435	273
Job satisfaction (mean, scale 0-10)	6.98	7.47	7.25	6.99	6.73
Shock: marriage	2.0%	3.3%	2.9%	2.8%	2.6%
Shock: divorce	0.7%	1.2%	1.0%	0.5%	0%
Shock: separation	1.8%	3.9%	2.3%	5.1%	2.9%
Shock: death of spouse	0.2%	0.1%	0%	0.2%	0%
Shock: child birth	2.2%	3.1%	4.2%	1.8%	2.6%
Shock: moving together with partner	2.0%	5.6%	5.1%	4.6%	2.9%
Shock: someone in household needs care	0.4%	0.2%	0%	0.2%	0%
Shock: home ownership	2.3%	3.0%	4.2%	3.2%	1.1%
Shock: recent relocation	8.7%	23.7%	17.0%	18.6%	12.5%
Female	47.0%	51.6%	48.2%	43.4%	41.0%
Age in years (mean)	43.71	36.18	36.68	37.86	41.45
Having a partner	86.6%	81.7%	86.8%	80.5%	88.6%
Number of children in household (mean)	0.63	0.67	0.78	0.58	0.71
Disabled	6.4%	2.4%	2.6%	3.4%	5.1%
Nights in hospital last year (mean)	0.74	0.48	0.66	1.72	0.48
Someone in household needs care	1.7%	0.9%	0.3%	1.4%	1.8%
Home ownership	55.0%	39.5%	46.3%	37.2%	45.1%
Education years (mean)	12.57	12.76	13.22	11.98	11.88
Sector: agriculture, energy, mining	2.5 %	1.8%	1.9%	2.8%	2.6%
Sector: manufacturing	19.6%	16.8%	18.0%	18.6%	27.8%
Sector: construction	12.7%	11.7%	12.2%	17.9%	17.2%
Sector: trade	13.0%	18.3%	13.2%	17.2%	19.8%
Sector: transport	5.1%	6.0%	4.5 %	3.9%	4.8%
Sector: banking and finance	4.5%	4.4%	2.6%	2.8%	2.2%
Sector: public administration	10.9%	3.7%	5.8%	2.8%	1.5%
Sector: education	8.2%	5.3%	6.8%	3.4%	0.4%
Sector: health and social work	12.0%	15.9%	19.0%	10.8%	7.0%
Sector: other services	11.5%	16.3%	16.1%	19.8%	16.8%
Occupation: civil servant	9.4%	2.0%	1.3%	0.5%	1.1%
Occupation: white collar worker	59.9%	69.1%	67.8%	52.2%	57.5%
Occupation: blue collar worker	30.6%	28.9%	30.9%	47.4%	41.4%
Firm size: below 20 employees	20.4%	30.4%	28.3%	39.3%	34.8%
Firm size: 20 to 200 employees	30.4%	31.4%	33.1%	35.4%	35.2%
Firm size: 200 to 2,000 employees	23.8%	20.9%	25.4%	14.0%	18.7%
Firm size: at least 2,000 employees	25.5%	17.3%	13.2%	11.3%	11.4%
Full-time employment	77.1%	77.0%	75.6%	79.8%	79.9%
Autonomy in occupational actions (mean, scale 1-5)	2.78	2.67	2.75	2.30	2.50
Overtime hours (mean)	3.83	4.32	4.72	4.19	3.94
Monthly net wage in Euro (mean)	1674.26	1454.58	1504.96	1205.79	1431.53
Unemployment experience in years (mean)	0.42	0.55	0.65	1.16	0.58
Job security (mean, scale 1-3)	2.31	2.36	2.27	1.91	2.07

Table A2. Multiple Regression Analysis with Individual Fixed Effects (Complete)

Specification	1	2	3	4	5
Year 2002	0.145*** (0.024)	-0.145*** (0.024)	-0.142*** (0.024)	-0.155*** (0.024)	-0.153*** (0.024)
Year 2003	0.155*** (0.025)	-0.153*** (0.025)	-0.147*** (0.025)	-0.170*** (0.025)	-0.168*** (0.025)
Year 2004	0.268*** (0.026)	-0.266*** (0.026)	-0.257*** (0.026)	-0.290*** (0.026)	-0.287*** (0.026)
Year 2005	0.317*** (0.028)	-0.316*** (0.028)	-0.304*** (0.028)	-0.346*** (0.028)	-0.342*** (0.028)
Year 2006	0.387*** (0.029)	-0.385*** (0.029)	-0.370*** (0.029)	-0.417*** (0.030)	-0.414*** (0.030)
Year 2007	0.420*** (0.028)	-0.418*** (0.028)	-0.400*** (0.030)	-0.455*** (0.030)	-0.452*** (0.030)
Year 2008	0.459*** (0.030)	-0.456*** (0.030)	-0.434*** (0.032)	-0.503*** (0.033)	-0.500*** (0.033)
Year 2009	0.518*** (0.032)	-0.515*** (0.032)	-0.489*** (0.034)	-0.570*** (0.035)	-0.567*** (0.035)
Year 2010	0.524*** (0.032)	-0.520*** (0.032)	-0.491*** (0.035)	-0.587*** (0.037)	-0.583*** (0.037)
Year 2011	0.517*** (0.033)	-0.513*** (0.033)	-0.479*** (0.036)	-0.586*** (0.039)	-0.582*** (0.039)
Shock: marriage		0.040 (0.048)	0.047 (0.048)	0.038 (0.048)	0.039 (0.048)
Shock: divorce		-0.048 (0.082)	-0.063 (0.082)	-0.065 (0.082)	-0.069 (0.082)
Shock: separation		0.183*** (0.052)	0.149*** (0.055)	0.146*** (0.055)	0.148*** (0.055)
Shock: death of spouse		0.115 (0.162)	0.065 (0.161)	0.056 (0.160)	0.057 (0.160)
Shock: child birth		0.015 (0.044)	0.022 (0.045)	0.014 (0.045)	0.013 (0.045)
Shock: moving together with partner		-0.037 (0.052)	-0.019 (0.052)	-0.020 (0.052)	-0.022 (0.052)
Shock: someone in household needs care		-0.219* (0.115)	-0.217 (0.148)	-0.214 (0.148)	-0.217 (0.148)
Shock: home ownership		-0.023 (0.042)	-0.028 (0.047)	-0.032 (0.047)	-0.031 (0.047)
Shock: recent relocation		0.033 (0.027)	0.034 (0.027)	0.032 (0.027)	0.029 (0.027)
Age ²			-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Having a partner			-0.085** (0.039)	-0.089** (0.039)	-0.088** (0.039)
No children in household			-0.017 (0.043)	-0.027 (0.043)	-0.028 (0.043)
One child in household			-0.018 (0.035)	-0.025 (0.034)	-0.026 (0.034)
At least three children in household			-0.027 (0.066)	-0.027 (0.065)	-0.026 (0.066)

To be continued on the next page!

<i>Specification</i>	1	2	3	4	5
Disabled			-0.193 ^{***} (0.062)	-0.187 ^{***} (0.061)	-0.187 ^{***} (0.061)
Nights in hospital last year			-0.004 ^{***} (0.002)	-0.004 ^{***} (0.002)	-0.004 ^{***} (0.002)
Someone in household needs care			-0.000 (0.123)	0.006 (0.124)	0.009 (0.124)
Home ownership			0.010 (0.039)	0.012 (0.039)	0.012 (0.039)
Education years			-0.007 (0.039)	-0.061 (0.039)	-0.062 (0.039)
Sector: manufacturing				-0.172 (0.107)	-0.177 [*] (0.107)
Sector: construction				-0.234 ^{**} (0.109)	-0.241 ^{**} (0.109)
Sector: trade				-0.280 ^{**} (0.113)	-0.284 ^{**} (0.113)
Sector: transport				-0.170 (0.128)	-0.182 (0.128)
Sector: banking and finance				-0.056 (0.151)	-0.067 (0.151)
Sector: public administration				-0.066 (0.115)	-0.071 (0.115)
Sector: education				-0.082 (0.127)	-0.088 (0.127)
Sector: health and social work				-0.165 (0.122)	-0.168 (0.122)
Sector: other services				-0.183 [*] (0.105)	-0.185 [*] (0.105)
Occupation: white collar worker				0.008 (0.120)	0.007 (0.121)
Occupation: blue collar worker				-0.131 (0.128)	-0.131 (0.129)
Firm size: 20 to 200 employees				0.122 ^{***} (0.042)	0.122 ^{***} (0.042)
Firm size: 200 to 2,000 employees				0.142 ^{***} (0.049)	0.143 ^{***} (0.049)
Firm size: at least 2,000 employees				0.201 ^{***} (0.050)	0.202 ^{***} (0.050)
Full-time employment				-0.066 (0.045)	-0.065 (0.045)
Autonomy in occupational actions, level 2				0.119 ^{**} (0.047)	0.119 ^{**} (0.047)
Autonomy in occupational actions, level 3				0.231 ^{***} (0.056)	0.232 ^{***} (0.056)
Autonomy in occupational actions, level 4				0.338 ^{***} (0.063)	0.338 ^{***} (0.063)
Autonomy in occupational actions, level 5				0.510 ^{***} (0.086)	0.510 ^{***} (0.086)
Log overtime hours				-0.033 ^{***} (0.011)	-0.033 ^{***} (0.011)

To be continued on the next page!

<i>Specification</i>	1	2	3	4	5
Log monthly net wage in Euro				0.298 ^{***}	0.295 ^{***}
				(0.040)	(0.040)
Job change	0.502 ^{***}	0.500 ^{***}	0.502 ^{***}	0.509 ^{***}	
	(0.044)	(0.044)	(0.044)	(0.044)	
Job change due to quit					0.663 ^{***}
					(0.053)
Job change due to mutual agreement					0.438 ^{***}
					(0.133)
Job change due to dismissal					0.352 ^{***}
					(0.111)
Job change due to plant closure					-0.007
					(0.126)
Constant	7.297 ^{***}	7.290 ^{***}	7.494 ^{***}	8.067 ^{***}	8.088 ^{***}
	(0.020)	(0.020)	(0.488)	(0.522)	(0.522)
Observations	72,428	72,428	72,428	72,428	42,097
Number of persons	15,205	15,205	15,205	15,205	10,389
Adjusted R-squared	0.016	0.016	0.016	0.021	0.021

*Notes: * denotes significance at the 10% level, ** at the 5% level and *** at the 1% level. Robust standard errors are in parentheses. The dependent variable is job satisfaction.*

Table B1. Selection Out of the Labor Force

Now	Plant closure			Quit		
	In	Out	difference	In	Out	difference
Number of observations	167	212		675	237	
Shock: marriage (share)	0.01	0.00	-0.01	0.03	0.03	0.00
Shock: divorce (share)	0.00	0.00	0.00	0.01	0.00	-0.01*
Shock: separation (share)	0.02	0.01	-0.01	0.03	0.02	-0.01
Shock: death of spouse (share)	0.00	0.00	0.00	0.00	0.00	0.00*
Shock: child birth (share)	0.02	0.03	0.01	0.03	0.06	0.03**
Shock: moving together with partner (share)	0.02	0.02	0.00	0.06	0.03	-0.03
Shock: someone needs care (share)	0.00	0.00	0.00	0.00	0.01	0.01
Shock: home ownership (share)	0.02	0.01	-0.00	0.03	0.01	-0.02*
Shock: recent relocation (share)	0.11	0.09	-0.01	0.23	0.15	-0.08***
Female (share)	0.41	0.41	0.00	0.48	0.73	0.25***
Age in years (mean)	41.83	47.17	5.35***	37.95	43.17	5.22***
Having a partner (share)	0.89	0.85	-0.04	0.83	0.86	0.03
Number of children in household (mean)	0.69	0.56	-0.14	0.67	0.66	-0.01
Disabled (share)	0.04	0.12	0.08***	0.03	0.11	0.07***
Nights in hospital last year (mean)	0.57	1.21	0.64	0.42	2.89	2.47***
Someone in household needs care (share)	0.01	0.01	0.00	0.01	0.01	0.00
Home ownership (share)	0.47	0.47	0.00	0.42	0.41	-0.01
Education years (mean)	12.01	11.33	-0.68***	13.06	11.99	-1.07***
Retirement (share)	0.00	0.01	0.01	0.00	0.11	0.11***
Registered as unemployed (share)	0.00	0.84	0.84***	0.00	0.43	0.43***
In two years	Plant closure			Quit		
Number of observations	167	212		675	237	
Age in years (mean)	39.83	45.17	5.35***	35.95	41.17	5.22***
Disabled (share)	0.05	0.07	0.02	0.03	0.08	0.04***
Nights in hospital last year (mean)	0.6	0.42	-0.17	0.51	0.67	0.16
Sector: agriculture, energy, mining (share)	0.01	0.01	0.00	0.02	0.03	0.02
Sector: manufacturing (share)	0.26	0.27	0.01	0.19	0.13	-0.06**
Sector: construction (share)	0.22	0.24	0.02	0.11	0.11	-0.01
Sector: trade (share)	0.2	0.28	0.07*	0.2	0.28	0.08**
Sector: transport (share)	0.08	0.02	-0.05**	0.05	0.04	-0.01
Sector: banking and finance (share)	0.03	0.02	-0.01	0.04	0.02	-0.02
Sector: public administration (share)	0.02	0	-0.02	0.03	0.02	-0.01
Sector: education (share)	0.01	0.01	0.01	0.04	0.08	0.04**
Sector: health and social work (share)	0.04	0.03	0.00	0.16	0.13	-0.03
Sector: other services (share)	0.13	0.1	-0.03	0.17	0.17	0.00
Occupation: civil servant (share)	0.02	0.00	-0.02*	0.02	0.01	-0.01
Occupation: white collar worker (share)	0.58	0.48	-0.10*	0.69	0.59	-0.09**
Occupation: blue collar worker (share)	0.40	0.52	0.12**	0.30	0.40	0.10***
Firm size: below 20 employees (share)	0.30	0.34	0.04	0.32	0.42	0.10***
Firm size: 20 to 200 employees (share)	0.40	0.41	0.02	0.33	0.32	-0.01
Firm size: 200 to 2,000 employees (share)	0.16	0.15	-0.02	0.18	0.13	-0.06**
Firm size: at least 2,000 employees (share)	0.14	0.10	-0.04	0.16	0.13	-0.03
Full-time employment (share)	0.83	0.77	-0.06	0.78	0.46	-0.32***
Part-time employment (share)	0.17	0.23	0.06	0.22	0.54	0.32***
Autonomy in occupational actions (mean)	2.62	2.24	-0.38***	2.84	2.24	-0.59***
Overtime Hours (mean)	4.03	3.34	-0.18*	4.97	2.96	-2.02***
Monthly net wage in Euro (mean)	1493.67	1305.04	-188.63**	1437.4	882.26	-555.14***
Tenure (mean)	9.11	11.46	2.34**	4.96	6.73	1.77***
Job security (mean)	1.97	1.88	-0.09	2.25	2.4	0.15***

Notes: * denotes significance at the 10% level, ** at the 5% level and *** at the 1% level. The significance levels in columns 3 and 6 result from t-tests on potential mean differences between employees who after job termination (due to plant closure or quit) move into new jobs (column 1 and 4) or leave the labor force (columns 2 and 5).

Table B2. Descriptive Statistics Before and After Reweighting

Variable	Means treated		Means controls			Standardized bias (%)		
	Closure	Raw	EB	PS	Raw	EB	PS	
Age in years	40.16	43.06	40.16	40.14	-30.56	-0.01	0.20	
Nights in hospital last year	0.71	0.70	0.71	0.71	0.29	0.00	0.03	
Education years	11.81	12.56	11.81	11.80	-33.35	-0.04	0.08	
Overtime hours	4.28	3.80	4.28	4.28	9.14	0.00	0.16	
Monthly net wage in Euro	1493.13	1659.72	1493.31	1483.59	-19.90	-0.02	1.14	
Unemployment experience in years	0.32	0.40	0.32	0.32	-8.16	0.01	-0.16	
Female (from here on:% shares)	0.42	0.46	0.42	0.43	-8.10	-0.01	-0.77	
Married	0.63	0.69	0.63	0.62	-11.65	0.00	2.69	
Divorced	0.04	0.08	0.04	0.05	-15.31	0.00	-0.92	
Separated	0.02	0.02	0.02	0.02	1.71	0.00	2.53	
Widowed	0.04	0.01	0.04	0.04	19.61	0.00	-0.24	
Single	0.26	0.20	0.26	0.27	13.71	0.01	-3.29	
Recent relocation	0.11	0.09	0.11	0.11	6.10	0.00	-0.51	
Having a partner	0.84	0.87	0.84	0.84	-6.77	0.00	2.11	
No children in household	0.46	0.58	0.46	0.47	-24.67	0.00	-3.54	
One child in household	0.26	0.21	0.26	0.25	11.36	0.00	2.16	
Two children in household	0.24	0.17	0.24	0.24	18.36	0.00	1.41	
At least three children in household	0.04	0.04	0.04	0.04	0.19	0.00	1.33	
Disabled	0.03	0.06	0.03	0.04	-11.26	0.00	-0.86	
Someone in household needs care	0.01	0.02	0.01	0.01	-3.80	0.00	-0.66	
Home ownership	0.49	0.56	0.49	0.48	-13.38	-0.01	1.29	
Sector: agriculture, energy, mining	0.01	0.02	0.01	0.01	-10.25	0.00	0.36	
Sector: manufacturing	0.29	0.20	0.29	0.29	20.25	0.02	-1.29	
Sector: construction	0.22	0.13	0.22	0.21	25.58	0.02	1.98	
Sector: trade	0.24	0.12	0.24	0.24	32.12	0.02	0.99	
Sector: transport	0.06	0.05	0.06	0.06	2.00	-0.01	-0.46	
Sector: banking and finance	0.02	0.05	0.02	0.02	-13.67	0.00	-0.78	
Sector: public administration	0.02	0.12	0.02	0.02	-37.51	-0.05	-0.54	
Sector: education	0.01	0.08	0.01	0.01	-33.58	-0.07	0.48	
Sector: health and social work	0.04	0.12	0.04	0.05	-27.90	0.00	-0.71	
Sector: other services	0.08	0.11	0.08	0.08	-11.45	0.01	-0.48	
Occupation: civil servant	0.01	0.09	0.01	0.01	-37.97	-0.11	0.09	
Occupation: white collar worker	0.61	0.60	0.61	0.61	1.81	0.02	0.56	
Occupation: blue collar worker	0.38	0.30	0.38	0.38	15.76	0.03	-0.62	
Firm size: below 20 employees	0.30	0.19	0.30	0.30	26.25	0.01	-0.33	
Firm size: 20 to 200 employees	0.41	0.30	0.41	0.40	22.21	0.00	1.29	
Firm size: 200 to 2,000 employees	0.14	0.25	0.14	0.15	-25.91	0.00	-1.56	
Firm size: at least 2,000 employees	0.14	0.26	0.14	0.14	-29.15	-0.02	0.35	
Full-time employment	0.81	0.78	0.81	0.82	7.52	0.01	-1.24	
Autonomy: level 1 of 5	0.14	0.12	0.14	0.15	6.31	0.01	-0.78	
Autonomy: level 2 of 5	0.34	0.27	0.34	0.35	17.25	0.02	-0.60	
Autonomy: level 3 of 5	0.31	0.35	0.31	0.31	-7.24	0.00	-0.42	
Autonomy: level 4 of 5	0.18	0.23	0.18	0.17	-13.16	-0.01	1.71	
Autonomy: level 5 of 5	0.02	0.04	0.02	0.02	-8.15	-0.04	0.29	
Job security: level 1 of 3	0.28	0.44	0.28	0.28	-34.42	-0.03	-0.08	
Job security: level 2 of 3	0.44	0.43	0.44	0.44	3.32	0.02	0.80	
Job security: level 3 of 3	0.28	0.13	0.28	0.28	36.85	0.02	-0.92	
Expectation: take up other job	0.12	0.08	0.12	0.12	22.15	0.01	-2.11	
Expectation: stop current job	0.07	0.05	0.07	0.07	8.10	0.00	-0.76	
Expectation: search for a job	0.26	0.16	0.26	0.26	35.98	0.01	-0.02	
Expectation: lose current job	0.34	0.19	0.34	0.34	65.34	0.03	-0.86	
Year: 2001	0.22	0.22	0.22	0.21	1.26	0.00	2.57	
Year: 2003	0.27	0.22	0.27	0.28	10.87	0.00	-2.80	
Year: 2005	0.18	0.20	0.18	0.17	-4.89	0.00	1.38	
Year: 2007	0.21	0.20	0.21	0.22	3.79	0.00	-1.45	
Year: 2009	0.12	0.17	0.12	0.12	-13.61	0.00	0.52	

Notes: Columns 3 and 4 show means for the control group after reweighting the raw data (column 2) according to entropy balancing (EB) and propensity scores (PS). The standardized bias compares the differences between treatment (column 1) and (weighted) control group and thereby evaluates the matching quality.

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