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

Adrian Chadi¹ Clemens Hetschko²

¹Institute for Labour Law and Industrial Relations in the European Union (IAAEU), Universität Trier, Behringstraße 21, D-54296 Trier, Germany (Email: Chadi@iaaeu.de)

²School of Business and Economics, Freie Universität Berlin, Boltzmannstraße 20, D-14195 Berlin, Germany (Email: Clemens.Hetschko@fu-berlin.de)

Abstract

We investigate a crucial event for job satisfaction: changing one's workplace. For representative German panel data, we show that the reason why the previous employment ended is strongly linked to satisfaction with the new job. Workers initiating a change of employer experience extraordinarily high job satisfaction, though in the short term only. To investigate causality, we exploit the event of plant closure as an exogenous trigger of job switching. In this case, we find no significantly positive effect of job changes on job satisfaction. Our findings complement research on workers' well-being and concern labor market policies and human resource management.

*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 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 distress and sorrow often accompany the ending of an employment relationship, with satisfaction with a job decreasing substantially before workers switch their workplace.² We, in contrast, focus on the fresh start by analyzing how job changes affect job satisfaction after having moved to a new place of work.

Several reasons increasingly motivate labor economists to study occupational mobility. Even though the financial returns to tenure are still considerable, studies suggest that workers switch jobs more and more frequently, at least in some countries like the United States and France (Kambourov & Manovskii, 2008; Lalé, 2012). Moreover, trends in occupational mobility are closely related to trends in wage inequality, which themselves receive attention from labor researchers (Kambourov & Manovskii, 2009). In a recent study, Groes, Kircher, and Manovskii (2015) focus on the drivers of occupational mobility and document that workers at the lower and at the higher end of a firm's wage distribution tend to switch jobs particularly often. To allow

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for a broader picture on the implications of changing labor markets, economists increasingly make use of subjective data to inform both research and policymakers. In this context, job satisfaction is regarded as a measure of worker's well-being (e.g., Frey, 2008; Helliwell & Barrington-Leigh, 2010; Weimann, Knabe, & Schöb, 2015). Besides the utility derived from earnings, management research shows that job satisfaction covers the welfare people obtain from a variety of aspects related to their work, such as fulfillment, task characteristics, personal growth, promotion opportunities, managerial quality, organizational support, and social relations at work (see Spector, 1997 and Fisher, 2010 for reviews). Although there is an overlap of different disciplines interested in similar research questions at this point, labor economists in particular refer to the work by managerial researchers and applied psychologists on job satisfaction as a predictor of employees' choices, such as absence behavior (e.g., Diestel, Wegge, & Schmidt, 2014; Wegge, Schmidt, Parkes, & van Dick, 2007). Meta-analyses summarize a large amount of evidence documenting that job satisfaction correlates positively with employee performance (Iaffaldano & Muchinsky, 1985; Judge, Thoresen, Bono, & Patton, 2001; Petty, McGee, & Cavender, 1984), negatively with employee turnover (Griffeth, Hom, & Gaertner, 2000), and positively with aggregate productivity at the business-unit level (Harter, Schmidt, & Hayes, 2002).³ As a result, job satisfaction is considered to be important for understanding how job search activities translate into actual quits (Swider, Boswell, & Zimmerman, 2011) and allows firms to foster economically relevant outcomes, such as successful newcomer adjustment (Bauer, Bodner, Erdogan, Truxillo, & Tucker, 2007).

Despite this impressively large interdisciplinary body of studies, there is a remarkable lack of research on how job satisfaction is affected by job changes. Though Freeman (1978) already suggests in an early contribution that job satisfaction might play a double role for job mobility, as predictor and outcome, the second role has drawn much less attention than the first. The case study of Boswell, Boudreau, and Tichy (2005) analyzes the relationship of job changes and job satisfaction for U.S. managers and reveals a pattern that the authors term the "honeymoon-hangover effect." In line with the literature on voluntary quits, subjects express dissatisfaction with their work prior to job switching. The rather novel finding is the extraordinary peak in satisfaction levels that managers report in their new job as well as the fact that this "magic of the new" quickly disappears. Boswell, Shipp, Payne, and Culbertson (2009) obtain similar findings when interviewing a number of newcomers to a public sector service organization. These results raise the question of whether the honeymoon-hangover pattern describes job changes in general or whether it only holds for job switching in rather selective samples. Some studies do indeed document changes in job satisfaction similar to the honeymoon-hangover pattern, while they focus primarily on other research issues (e.g., Georgellis & Tabvuma, 2010; Gielen, 2013; Grund, 2001; Johnston & Lee, 2012, 2013). In their broad investigation of the relationship between job changes and job satisfaction, Akerlof, Rose, and Yellen (1988) also present some representative evidence on workers' satisfaction after the job change. As their main aim is to analyze job switching as a consequence of dissatisfaction among workers, we conclude from our review of the literature that a comprehensive discussion of job satisfaction in the new job is still lacking.

To fill this gap, we employ large-scale German panel data to document the honeymoon-hangover effect accompanying job changes in a representative sample of employees. Using information on various life circumstances, parallel life events, job characteristics, and comparisons of initial and new job, we investigate the robustness of the pattern and its potential drivers. Most importantly, we shed light on the question whether switching jobs itself affects job satisfaction, for instance, because newcomers' motivation, performance, and, in consequence, rewards are generally above-average. By distinguishing between endogenously and exogenously triggered job changes, we can answer the question of whether every parting is indeed followed by a promising start. To make this distinction independent of the complex issue of mobility within firms, we focus on changes of employers.⁴ Similar to research on entries into unemployment (Kassenboehmer & Haisken-DeNew, 2009), we use German Socio-Economic Panel (SOEP) data (Wagner, Frick, & Schupp, 2007) to distinguish between four reasons for job changes from the employee's point of view: quit, mutual agreement about employment termination, dismissal, and plant closure. The last event is of special importance as only an exogenous trigger helps us to investigate how switching jobs itself affects job satisfaction. We analyze and consider selection issues in this respect by individual fixed-effects estimations as well as a matching approach, exploiting the richness of our data set.

We proceed as follows: The next section describes our database and methodology. We present our main results in Section 3. Section 4 documents various sensitivity analyses, and Section 5 concludes.

2 FRAMEWORK

2.1 Data

We use data from the German 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 multifaceted 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.

Information on job satisfaction is obtained in the SOEP with the question: “How satisfied are you with your job? Please answer on a scale ranging from 0 (‘completely dissatisfied’) to 10 (‘completely satisfied’).” Details of job changes and job terminations are elicited for the time period beginning 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.

Regarding the termination of the previous job, we distinguish between four triggers of job changes. Switches after plant closures are considered as unintended and most likely exogenous. Dismissals seem to be similar as they are determined by the employer. In a few cases, however, workers who want to leave the company may not quit but provoke being fired, for instance, to receive severance payments. This may apply in particular to the German labor market in which a high level of employment protection renders dismissals almost impossible unless a substantial compensation is paid. Although a dismissal in our data will thus not always be involuntary, it is also certainly not exogenous to the quality of the job–worker match if, for example, bad job performance plays a role. In contrast to more or less involuntary job changes, we also investigate the more common case of voluntary job switching. A special phenomenon in this regard is mutually agreed contract terminations, which are often considered a good option for both employer and employee if either of them wishes to separate. Sometimes, however, employees who are at risk of being dismissed propose a mutually agreed termination to avoid sending out a negative signal to other potential employers. A mutually agreed termination is then a good option to circumvent a dismissal, although workers did not actually intend to leave the firm in the first place. Although the mutually agreed termination is mostly voluntary, based on the idea of a consensus between both sides, in some cases it is not. Finally, a worker-induced resignation (or “quit”) is the most self-determined kind of job termination. Here as well, workers are not necessarily free from external pressure when they resign, but they can at least fully decide upon the timing. Quits that are followed by a new job may generally occur because workers assume they can improve their situation. Hence, in the following analysis, we inspect four types of job changes, depending on the reason for the initial job termination: (1) quit, (2) mutually agreed termination, (3) dismissal, and (4) plant closure.⁵

As the four triggers of job switching are consistently observed between 2001 and 2011, we restrict our analysis of employer changes to these years.⁶ By focusing on those individuals who report to have started a new employment after the termination of the previous job, we exclude cases of prolonged joblessness. To shed light on the issue of sample selection, Table A1 in the Appendix describes the raw data of people who terminate an employment relationship for one of the four reasons within our investigation period. Looking at workers that are reemployed within five years (bottom half of Table A1) reveals plant closures to be accompanied by more months of unemployment on average than quits and mutually agreed terminations, whereas unemployment spells are the longest for dismissals.

We also compare people who are either considered job switchers in our analysis (“in”) or not (“out”) when the first interview after the termination of the initial job takes place. The figures in Table A1 show that more than half of workers who terminate jobs for the reason of a plant closure are reemployed by the next SOEP interview. The average length of unemployment of these people is only about one month. Data on gross hourly wage, education, and health in the previous job suggest a selection of more productive workers into the group of considered job changers. However, differences across groups are not especially large, suggesting the selection to be rather weak. Age and the fraction of females hardly differ, whereas workers in our sample even have more children on average than unconsidered workers, though children might be a potential obstacle for successful job searches.

Table A1 shows similar differences between observations selected for our study and those left out with respect to quits and mutually agreed terminations. Most of them are reemployed fairly quickly, in particular when they are in good health, earned relatively high wages beforehand, and have high levels of education. If they are considered in our sample, they have experienced only a few weeks of unemployment on average. In contrast, dismissed people are without work for a longer period of time and their reemployment prospects appear to be rather unrelated to education and previous wage.

In addition to excluding observations of prolonged joblessness, as described in Table A1, we imply further sampling restrictions for our analysis in the following. With respect to the new job, we require that the new position be with a different employer, thus excluding intrafirm mobility. We also do not consider data of employees doing apprenticeships, self-employed people, and workers older or younger than the usual working age in Germany (from 18 to 65 years). Table A2 in the Appendix provides descriptive statistics for our main sample, which results from these decisions. 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. Once

the sample that underlies a specific step of our analysis deviates from the main sample, we report numbers of observations for the job change types included.⁷

2.2 Identification

To identify the effect of job changes on job satisfaction, we deliberately focus on plant closures. Most importantly, this event provides us with the rare chance to circumvent the endogeneity in the implications of changes in people's working lives. As a plant closure is plausibly exogenous from the perspective of the worker, it allows inspecting the impact of subsequent job changes in the field where experimental setups in which people are randomly induced to switch jobs are generally hard to imagine. Hence, plant closures are of great interest for researchers and have been central to the empirical strategy of numerous studies in labor research. However, there are some important issues to be taken into account.

First, plant closures do not provide a clean natural experiment with randomized groups of affected and unaffected individuals. Descriptive statistics show that people who recently switched jobs differ on average from people who stay in the same job with regard to several aspects, such as gender, age, home ownership, occupation, or firm size (see Table A2). There are other characteristics, such as health, however, which do not vary significantly between the two groups. Because differences in observable characteristics could point to differences in unobserved characteristics, one aim of the empirical approach is to take both observed differences and unobserved time-invariant heterogeneity into account. By exploiting the panel structure of the data in our empirical model (see the next subsection), we can control for time-invariant personal characteristics (e.g., personality) which determine a large proportion of interpersonal variation in happiness data (Lykken & Tellegen, 1996). Furthermore, focusing on plant closures should also account for some time-variant heterogeneity. For instance, not one recent family life event (marriage, divorce, etc.) differs in its likelihood between people who switched the job after a plant closure and those who stayed in the same job. However, even if differences between the two groups were not severe and the empirical approach promised successful identification, the lacking randomness of the plant closure would limit the generalizability of our findings. Obvious in our context are sector differences, according to which plant closures happen more often in manufacturing, construction, and trade, for instance, which is something we need to consider when interpreting the findings.

We have to consider two further selection issues with the plant-closure identification, which we will empirically address in the course of our sensitivity analyses (Section 4). One is the potential selection of individuals into new jobs in contrast to workers who experience longer spells of unemployment after plant closures, which we started to discuss in the previous subsection. The second issue is the possible selection before the event, as some employees may have foreknowledge of a future plant closure. These individuals may still be attractive to another employer and have thus an opportunity to leave the firm before it fails. If such a selection is relevant, the effect of switching after a plant closure could be biased downwards due to a negative selection into treatment. A helpful facet of the SOEP in this regard is that it allows us to address this issue employing data on workers' expectations of future job mobility.⁸ These measures might not perfectly reflect people's uncertainties regarding the future, but plausibly capture some of the differences between workers who are laid-off and those who stay employed. We therefore use the data on expectations as part of a matching approach to balance the potential differences between both groups (Section 4).

2.3 Empirical model and methodology

Our empirical analysis consists of three steps. First, we apply a mean analysis describing how the four job change triggers lead to different job satisfaction trajectories around job changes. 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. Similar to previous studies using lags and leads in satisfaction data (e.g., Clark, Diener, Georgellis, & Lucas, 2008; Georgellis, Lange, & Tabvuma, 2012; Powdthavee, 2011), our outcome variable is assumed to be cardinal to ease the interpretation of the results, which may be uncritical from a methodological point of view (Ferrer-i-Carbonell and Frijters, 2004). We address this issue in the course of our sensitivity analysis.

We consider job switching by using binary variables representing the four triggers: *quit*, *agreed*, *dismissal*, and *closure*. Each type can be distinguished by means of four points in time before and after the switch (covered by vector L). $t = -2$ and $t = -1$ denote the last SOEP interviews before the job change and $t = 0$ and $t = 1$ the first two interviews afterwards. For instance, $closure_{t=0}$ 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 a homeowner, 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 homeownership. In the process, 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}) that 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 incorporating two further variables that might explain the differences between people switching because of this trigger and because of other triggers: 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. This allows us to make use of the full data sample. In addition, 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 4) by running regressions using 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 technical issues such as the role of attrition.

2.4 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 honeymoon effect of switching employers. An obvious but unsuitable solution would be to use the information from the time directly before the switch. At this time, work-related well-being may already be influenced by the event that causes the transition. 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. Hence, to measure the impact of a job change that is not biased by the type of trigger, one should at least go back another year and use the satisfaction level at $t = -2$ as reference point. Alternatively, we interpret the $t = 0$ 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., Clark et al., 2008; Frederick & Loewenstein, 1999). Because 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.

3 RESULTS

3.1 Graphical analysis

We start our investigation by examining the trajectories of mean job satisfaction around job changes as distinguished by the four job change triggers. The data come from our main sample (see Table A2). As we follow workers two SOEP interviews before the switch as well as up to the second interview afterwards, we cannot make use of observations that are not surveyed for these four interviews in a row around their job change (see the notes below Figure 1 where we report observation numbers of observations for all types of job changes). Bearing in mind the previous discussion on reference points, we consider group-specific averages in job satisfaction of each subsample, for which we use all the available data. The group-specific average represents the mean job satisfaction of all observations of workers who experience the respective job change at least once during the period investigated.

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 of the cases, we observe strong and significant declines in the former job (from $t = -2$ to $t = -1$) before workers switch to new employment, and the reverse once the change occurs. At $t = 0$, the job satisfaction of people who resign significantly surpasses both the group-specific average and the level at $t = -2$. This group loses well-being significantly between $t = 0$ and $t = 1$, but is nevertheless more satisfied at

FIGURE 1 Mean job satisfaction and the types of job change

Notes: Points in time ($t = -2, -1, 0, 1$) mark time lags of approximately one year. Job changes take place between $t = -1$ and $t = 0$. Black lines connect mean values of job satisfaction at different points in time around job changes for the reason mentioned in the respective diagram title. Gray lines represent individual job satisfaction means of each group. Whiskers denote 95% confidence intervals. The figures rely on observations of 638 quits, 107 mutually agreed terminations, 139 dismissals, and also 139 plant closures.

$t = 1$ than at $t = -2$. For cases of dismissals and mutually agreed terminations, there is evidence for honeymoon effects in the first year only. Switching does not seem to improve the well-being of workers who lost their jobs due to plant closures.

3.2 The new job effect

We first describe the results on the effect of having a new job by implementing a model without lag or lead variables. The corresponding outcomes in Table 1 are based on the main data sample. The basic new job effect in workers' satisfaction levels appears to be striking. 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 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. A comparison of the effects suggests that the more involuntary a change of workplace, the lower the impact on job satisfaction. The positive new job effect completely disappears when workers were exogenously forced to switch due to plant closure.⁹ Although extensive honeymoon effects accompany most new jobs, we cannot conclude that this phenomenon necessarily originates from the job change itself.

3.3 Dynamic analysis

We continue the analysis by extending our empirical model with lags and leads variables. 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.

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.

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 ($t = 0$), whereas in the second year the effect is still significantly positive ($t = 1$) but much smaller. Beforehand, the employees are dissatisfied in their former job ($t = -2$), which becomes worse in the final year ($t = -1$). 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.

The results in Table 3 demonstrate clearly that the general picture revealed in Table 2 for all job changes is driven by voluntary switches of employers. These lead to significantly higher job satisfaction at $t = 0$ 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 $t = 1$ although well-being strongly declines afterwards 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 be 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 $t = -2$ is statistically significant.

3.4 Potential mechanisms

The comparison of different specifications in Tables 2 and 3 reveals that neither life events nor life circumstances nor job characteristics can fully explain the job satisfaction patterns observed. Certainly, individuals may have many different reasons to switch jobs for which we cannot control. To get a better impression of the motives for taking up a new job, more detailed assessments of different job satisfaction domains were useful, as these have been analyzed in other data sets (e.g., Clark, 1997; Green & Heywood, 2011; Lange, Pacheco, & Shrotryia, 2010). The SOEP instead has a variable on the satisfaction with personal income that for some waves is part of the same question battery as job satisfaction is, and hence equally elicited. Replacing our main outcome variable in the same fixed-effects estimations with income satisfaction shows a honeymoon effect if the job change is triggered by resignation, whereas job switches following plant closures do not yield such effects. In addition, the SOEP also offers comparisons of new and previous job regarding a variety of job characteristics, which allows for a broader picture. People are asked whether the new job is better, equal, or worse than the former job with respect to each characteristic. We calculate the “net improvement” as the difference between the shares of people reporting an improvement and people reporting a deterioration of the respective characteristic.

TABLE 2 Dynamic analysis of the job change pattern

<i>Specification</i>	1	2	3	4	5
Job change $t = -2$	-0.374*** (0.049)	-0.377*** (0.049)	-0.367*** (0.049)	-0.351*** (0.049)	-0.312*** (0.048)
Job change $t = -1$	-0.790*** (0.063)	-0.789*** (0.063)	-0.776*** (0.063)	-0.769*** (0.063)	-0.704*** (0.061)
Job change $t = 0$	0.489*** (0.074)	0.487*** (0.074)	0.483*** (0.074)	0.474*** (0.074)	0.457*** (0.074)
Job change $t = 1$	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 <i>R</i> -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 $t = 0$. Job changes are restricted to cases of one previous job (at $t = -2$ and $t = -1$) and one new job (at $t = 0$ and $t = 1$). The sample includes 454 cases of job changes following quits, 75 following mutual agreements, 100 following dismissals, and 109 following plant closures.

Figure 2 displays these shares for each of the eight job characteristics in question, dependent on the respective trigger of switching. For illustration purposes, we always add a ninth bar reflecting the average of all net improvements. The picture drawn for quits sheds some light on potential reasons why workers initiate switches, suggesting that earnings are far from being the only motive.¹⁰ Workers who resign are better off after switching jobs than beforehand regarding all of the characteristics. Though less pronounced, workers who agree to terminate the employment relationship and even dismissed workers also report more improvements than deteriorations, except with respect to workload and way to work.

The pattern for workers who change because of plant closures is ambiguous. Job security and the type of work show significantly more improvements than deteriorations, whereas the reverse applies to the probability of promotion, workload, and way to work. Earnings, fringe benefits, and working time neither improve nor deteriorate. The largest shares of people report no change. In sum, these results indicate that switching employers because of a plant closure does not improve the situation of workers overall. Given that workers act rationally, this result strengthens the assumption that such switches are both involuntary and exogenous. If voluntary job changes are rational in the sense that they improve workers' situations, the differences between the four triggers of switching empirically support the idea of ordering these different cases according to the degree of voluntariness.

3.5 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. In accordance with 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 previous findings (e.g., Georgellis et al., 2012; Theodossiou & Zangelidis, 2009). 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

TABLE 3 Dynamic analysis of different types of job changes

<i>Specification</i>	1	2	3	4	5
Quit $t = -2$	-0.360*** (0.081)	-0.361*** (0.081)	-0.348*** (0.081)	-0.345*** (0.081)	-0.320*** (0.081)
Quit $t = -1$	-0.949*** (0.101)	-0.945*** (0.101)	-0.931*** (0.101)	-0.927*** (0.101)	-0.918*** (0.099)
Quit $t = 0$	0.715*** (0.094)	0.710*** (0.094)	0.699*** (0.094)	0.699*** (0.094)	0.656*** (0.093)
Quit $t = 1$	0.365*** (0.071)	0.363*** (0.071)	0.353*** (0.070)	0.353*** (0.070)	0.334*** (0.069)
Agreed $t = -2$	-0.325*** (0.115)	-0.323*** (0.115)	-0.317*** (0.115)	-0.306*** (0.115)	-0.251** (0.113)
Agreed $t = -1$	-0.741*** (0.161)	-0.729*** (0.160)	-0.700*** (0.160)	-0.680*** (0.160)	-0.618*** (0.159)
Agreed $t = 0$	0.539** (0.212)	0.548** (0.213)	0.565*** (0.213)	0.566*** (0.213)	0.556*** (0.204)
Agreed $t = 1$	0.258* (0.137)	0.265* (0.137)	0.268** (0.136)	0.270** (0.136)	0.280** (0.136)
Dismissal $t = -2$	-0.296*** (0.085)	-0.296*** (0.085)	-0.296*** (0.086)	-0.273*** (0.087)	-0.239*** (0.086)
Dismissal $t = -1$	-1.196*** (0.127)	-1.190*** (0.127)	-1.182*** (0.126)	-1.148*** (0.128)	-1.022*** (0.125)
Dismissal $t = 0$	0.128 (0.207)	0.128 (0.207)	0.133 (0.207)	0.127 (0.207)	0.180 (0.206)
Dismissal $t = 1$	0.211 (0.144)	0.210 (0.144)	0.214 (0.144)	0.202 (0.144)	0.185 (0.144)
Closure $t = -2$	0.017 (0.108)	0.014 (0.108)	0.012 (0.108)	0.029 (0.107)	0.069 (0.106)
Closure $t = -1$	-0.805*** (0.152)	-0.808*** (0.152)	-0.807*** (0.152)	-0.775*** (0.152)	-0.593*** (0.147)
Closure $t = 0$	-0.163 (0.193)	-0.159 (0.194)	-0.171 (0.193)	-0.167 (0.193)	-0.183 (0.190)
Closure $t = 1$	-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 <i>R</i> -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 at $t = 0$. Job changes are restricted to cases of one previous job (at $t = -2$ and $t = -1$) and one new job (at $t = 0$ and $t = 1$). The sample includes 454 cases of job changes following quits, 75 following mutual agreements, 100 following dismissals, and 109 following plant closures.

FIGURE 2 Job changes and job characteristics

Notes: As the data are only available from 2001 to 2007 in our investigation period, the observation numbers are 1,023 quits, 195 mutually agreed terminations, 275 dismissals, and 189 plant closures. Some comparisons rely on up to 2% lower numbers of observations because of a few missing values.

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 × 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	53,202	50,936	50,928	50,910
Number of persons	7,652	7,413	7,058	7,065	7,059
Adjusted R-squared	0.026	0.026	0.024	0.024	0.023

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 2–5. The sample includes 432 job changes following quits, 77 following mutual agreements, 84 following dismissals, and 78 following plant closures.

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, which is in line with the expectation of a hangover effect. The subsequent specifications show strong differences between subgroups of newcomers, confirming our previous findings. As there is no honeymoon effect in job satisfaction levels for those who are forced to switch their 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, those who resigned, can we find a strong and significant decrease in satisfaction far beyond the overall decline.¹¹ As quits precede the overwhelming majority of job changes, this group drives the overall pattern in Specification 1.

4 SENSITIVITY ANALYSIS

4.1 Subsamples

As a first sensitivity analysis, job changes after plant closures and quits are distinguished by gender (women/men), education (at most/more than 12 years), and age (at most/more than 40 years) in order 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 A1 in the Appendix, which depicts the coefficients of the lags and leads variables, such as $closure_t = 0$. It is important to keep in mind that these values are predicted on the basis of a model that controls for the impacts of various life events, job characteristics, and sociodemographic characteristics on job satisfaction.

In the case of quits, the analysis reveals extensive similarity in the development of the job satisfaction of the six subgroups around job changes (Figure A1.1). The finding that self-intended switches lead to a clear honeymoon-hangover pattern is not driven by any of the subgroups in particular. The different gender, age, and education groups also show similar progressions of job satisfaction when affected by plant closure between $t = -1$ and $t = 0$ (Figure A1.2). Most importantly, the job satisfaction level in the new job ($t = 0$) exceeds neither the individual average over time nor the level of $t = -2$. Thus, our finding that exogenously triggered job changes are not able to generate honeymoon effects is robust across all six subgroups.

4.2 Potential selectivity

We identify large and highly significant differences in job satisfaction with respect to our variables of interest. The key finding that the extensive honeymoon effect found for the most common type of job changes completely disappears in the case of an exogenous trigger may, however, be affected by some kind of selectivity. Recall that our idea to use exogenous variation in the data relates to the endogeneity in most of the switches observed. Accordingly, the group of voluntary job changers constitutes a selective sample of fortunate cases with regard to the outcomes of switching. Nevertheless, selection effects within the group of those affected by plant closures might also play a role, as discussed in Section 2.2. First of all, not every former employee finds a new job within a certain period of time. We address this form of attrition using descriptive information, continuing our discussion of Section 2.1, while we comment on overall panel attrition below.

As shown in Table A1, our samples are selective in the sense that we exclude individuals who become unemployed and seek 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 extends the information provided above on individuals who are either employed in a new job or who are out of the labor force after ending previous employment, focusing on involuntarily (plant closure) and voluntarily (quit) triggered switches.¹² For the former, the differences revealed are intuitive and point to a selection of more unfortunate cases into nonemployment. As before, the overall comparison 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 nonemployment is further substantiated. Compared to employees with a job later on, those without one are more likely to be blue-collar workers with lower earnings and less autonomy in their previous job, which expresses lower hierarchical rank. With regard to quits, individual differences between those who take up a new job and those who leave the workforce are more severe than in the 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 nonemployment 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 were more severe than we argue it is, it could not alter our main finding qualitatively. The more relevant such positive selection bias is, the more negative the actual effect of exogenously triggered changes is. A problem with this result can only emerge if the insignificant new job effect is biased by a selection of negative cases. Although the attrition of out of the labor force does not confirm such a notion, there could be another form of selectivity resulting from possible resignations of more productive workers prior to anticipated plant closure (see Section 2.2).

We implement a matching-based strategy that specifically aims to address the potential issue of negative selection of remaining workers switching after plant closures. The idea is to perceive these workers as a treatment group and to construct a control group that is as comparable as possible to this treatment group. If matching is successful, we can tackle the selection issue to ensure unbiased outcomes. We only consider workers who switch because of plant closures (treatment group) and workers who stay in the same job for at least three years (control group). Matching occurs at time $t = -2$, that is the second-last interview members

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 due to plant closure	0.004	0.012	0.010	0.038	0.032
	(0.245)	(0.200)	(0.246)	(0.250)	(0.251)

Notes: The DiD regression analysis explains the difference between the treatment and control group in the change in job satisfaction between $t = -2$ and $t = 0$. Specification 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 or 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 (switching because of plant closure). The control group stays in the same job.

of the treatment group give before switching and the first interview of three interviews in which members of the control group report no job change. Table B2 gives descriptive information on all the conditioning variables.

We apply a difference-in-differences (DiD) approach to identify the potential honeymoon effect of changes after plant closures. This estimates the difference between the treatment and control group in the change in job satisfaction between $t = -2$ and $t = 0$. In this process, we proceed in a very similar way to Marcus (2013). Similar to his comparison of individual health prior to and after plant closure, we also use standard and novel matching tools. While the former exploits propensity scores (for a review, see Caliendo & Kopeinig, 2008), the latter is the reweighting technique of entropy balancing (Hainmueller, 2012). The parallel use ensures that our results are not dependent on a specific method of matching. Table B2 also documents the matching quality by showing means before and after applying matching.

Table 5 shows the corresponding results. Whether we apply reweighting via entropy balancing (Column 2) or propensity score weighting (Column 3), the effect of a job change triggered by plant closures does not differ qualitatively from a simple regression with covariates (Column 1). 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 presented in the table. Instead of the difference in job satisfaction compared to $t = -2$, we can also calculate the difference to the individual mean using matching techniques, which yields the same finding as before.¹³ We conclude that selection of negative cases into our treatment group is not a significant problem.

4.3 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 by means of an additional control variable in the above regressions. The investigation of this aspect can of course be carried out more extensively, for instance, by using interaction terms or by excluding all cases with such unemployment spells. However, these attempts affect neither the large positive new job effect that we find for job changes in general nor the insignificant outcome when we focus on exogenously triggered job switches.¹⁴ Furthermore, we have so far not dropped the observations of people who work in forms of employment that are nonstandard in the German labor market, such as fixed-term employees or temporary agency workers. If we do so, the findings remain the same, even though observation numbers go down. This is also true if we exclude cases of atypical employment in the previous job, prior to the job change. To fully ensure the exogeneity of the plant closure, one may also consider dropping smaller firm sizes. The smallest category is organizations with less than 20 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.

Arguably, there are some individuals in our sample who never switch jobs and might be hardly comparable to our groups of workers who recently have changed employers. For another robustness check, we therefore repeat our fixed-effects estimations based on a sample that excludes workers who never report a job move at any point in time while participating in the SOEP survey. To increase comparability further, we also exclude civil servants who are known to switch jobs comparatively seldom in the German labor market. Although these steps shrink our sample, the results echo those reported so far with respect to all of the four types of job changes.

Repeated job switching is a phenomenon that may occur more often in the case of involuntary job changes. Hence, we restrict the data to employees with no more than one job change in the entire period of investigation. This 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, whereas the previous job is observed for the last year and the year before that. If we relax this point so that case numbers go up again, the findings are also the same.¹⁵

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 the case of plant closures, it could be that very dissatisfied individuals refuse to continue participating in the survey, which would lead to a loss of unhappy persons. As discussed above, this kind of negative attrition and a possible upward bias in the job satisfaction of remaining individuals cannot qualitatively alter our findings concerning plant closures. However, the extraordinarily high levels of satisfaction after voluntary job changes may be overestimated when selective attrition is at hand. Although 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 artifacts in recent times. Noting the findings of Heffetz and Rabin (2013), we find that it is somewhat harder for interviewers to reach those who change their workplace. For this reason, 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 to portray themselves more positively when other people are present during the interview (e.g., Chadi, 2013a; Conti & Pudney, 2011). 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 the 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 in the analysis of tenure effects, as self-reported job satisfaction appears to decline strongly in the first years of panel participation, 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. Use of the conditional logit estimator by Chamberlain (1980), which requires the 11-score satisfaction data to be collapsed into a binary satisfied/dissatisfied variable, leads to the same findings.

5 DISCUSSION AND CONCLUSION

5.1 Findings

Job satisfaction varies immensely before and after job changes. Although it is well 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 topic, 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 the first year of their 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 prodigious satisfaction among organizational newcomers cannot be sustained and adaptation takes place. We find this hangover phenomenon to be stronger than the usual job satisfaction decline in tenure.

Our study is the first that documents the honeymoon-hangover pattern comprehensively in representative data from a large labor market. An important novelty is the analysis of the direct effect of job switching. Undisputedly, the exceptional satisfaction among recent job changers is driven by selectivity. Those who expect something positive tend to switch, and those who do not stick with their employer. Addressing endogeneity, we exploit information on the reasons why employees terminate a job before switching to a different employer. The findings from this distinction are remarkably consistent and suggest that voluntariness plays a major role in this context. The honeymoon-hangover pattern becomes even stronger than on average if we examine only quits as the most likely voluntary trigger of job mobility. The new job effects are also quite robust, but somewhat smaller after a termination that is mutually agreed by employer and employee. As people usually do not intend to get fired, job changes after dismissals are mostly of an involuntary nature, which is accompanied by less frequently observed honeymoon experiences at the new workplace. Most importantly, the positive new job effect in job satisfaction vanishes as soon as we focus on job changes after plant closures. 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 (which is approximately two years before the change of workplace). Hence, the evidence suggests that there is no direct effect of job changes on job satisfaction. The same applies to the hangover effect, which is only experienced by voluntary changers.

To analyze in what way the results may be driven by some form of selectivity, we carry out matching-based sensitivity analyses, which help us to address selection into the group of people who potentially experience plant closures. The DiD approach ensures tackling individual-fixed characteristics. The very rich panel data allow us to address many relevant issues, such as anticipation of plant closures that in some cases may not come as a surprise to the employees. Although we take a large set of relevant and observable characteristics into account and control for as much as possible, some unobserved time-variant heterogeneity might still be relevant. In consequence, we cannot completely rule out that direct effects of switching jobs in work-related well-being do exist. Moreover, we have to mention limitations regarding the generalizability of the findings. Because plant closures rarely happen in sectors such as banking, education, or public administration, the evidence from cases where plant closures do occur might not fully apply to all other work environments.

5.2 Implications

A first explanation as to why the sizable new job effects we find have not been recognized in job satisfaction research so far might be our definition of job switches, which is restricted to changes of employer. This makes the identification clear and consistent, as, for instance, changes within firms can be interpreted differently and may sometimes be reported as a new job, but sometimes not. Possibly, an aggregate variable that is used in all studies without specific job change distinction leads to other outcomes. A second explanation relates to tenure, which most empirical researchers of job satisfaction routinely consider as a control variable. This, however, only partially addresses the honeymoon-hangover pattern. In our data, the overall new job effect remains large and significant even if we control for tenure. This leads to an important implication for future studies. When researchers are interested in variables being correlated with the probability of being new in a job, the effect on job satisfaction is probably biased and considering tenure might not sufficiently solve the issue. For example, our investigation into the potential side effects of fixed-term employment shows that it can be very important for researchers to fully take into account the honeymoon-hangover effect in job satisfaction (Chadi & Hetschko, 2016).

With respect to labor market policy, the notable increases in job satisfaction that are possible via job changes seem to be an argument for more flexible labor markets. 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 no evidence for any impact of exogenously triggered job changes on job satisfaction. Nevertheless, fostering flexibility may create more vacancies and this could help employees who wish to improve their situation. Hence, when flexible labor markets allow workers to change jobs, this may potentially increase welfare.

Our findings are important with regard to human resource management as job satisfaction is also regarded as an important policy goal in this context. 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 the risk of turnover and drop in performance may increase quickly with 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 the magic of the new.

NOTES

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

² For example, Clark, Georgellis, and Sanfey (1998), Clark (2001), Shields and Price (2002), Delfgaauw (2007), Lévy-Garboua, Montmarquette, and Simonnet (2007), Sousa-Poza and Sousa-Poza (2007), Böckerman and Ilmakunnas (2009), and Green (2010).

³ In addition, these empirical relationships have also been documented in many economic contributions, such as Böckerman and Ilmakunnas (2012), who analyze the effect of job satisfaction on productivity. Major differences to research by other disciplines are the focus on representative data and identification issues. Obviously, for all of these relationships the actual effects may exist in either direction, as also discussed in the management literature (see, e.g., Harter, Schmidt, Asplund, Killham, & Agrawal, 2002; Schneider, Hanges, Smith, & Salvaggio, 2003). For that reasons, researchers make use of laboratory experiments to analyze satisfaction at work, either as outcome (e.g., Azmat & Iriberry, 2016) or as determinant (e.g., Oswald, Proto, & Sgroi, 2015). To gather evidence that is both valid and generalizable, Card, Mas, Moretti, and Saez (2012) study job satisfaction in a field experiment.

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

⁵ In Section 3.4, we provide descriptive evidence on changers’ comparisons between the initial job and the new job, which help us to validate our interpretation regarding the voluntariness of the four triggers.

- ⁶ The SOEP includes further categories that typically do not lead to employer changes (e.g., maternity leave, retirement) and are therefore left out. Over the years, there is a variation in these questionnaire items, also due to legislative changes. In addition, we do not consider those who report “end of fixed-term contract” as it is fairly difficult to assess this reason’s degree of voluntariness from the employees’ point of view, which would be necessary for our analyses. We discuss this case and the role of fixed-term employment in general for employees’ satisfaction with the job in a previous study (Chadi & Hetschko, 2016).
- ⁷ For instance, 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.
- ⁸ SOEP respondents are asked biennially about their expectations regarding several job-related events that they expect to happen in the two years to follow. Of special interest are probabilities of expected job termination, job loss, job search, and start of a new job.
- ⁹ This pattern is robust to the consideration of control variables, which we can vary like in the first four columns. The coefficient for job change due to plant closure is close to zero in each case.
- ¹⁰ Grund (2013) provides a deeper discussion of this information in the SOEP focusing on job changes after quits. For further evidence on factors determining job choices, see, for example, Turban, Eyring, and Campion (1993) and Boswell, Roehling, LePine, and Moynihan (2003). See Konrad, Ritchie, Lieb, and Corrigan (2000) for a meta-analysis. The finding that other factors beyond income play a major role in people’s motives for their job choices is very common in this literature.
- ¹¹ If we dropped 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 would emerge as well.
- ¹² 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 us to compare the same persons two years earlier in the previous job and directly after job termination.
- ¹³ 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. In doing so, 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 differing from zero. Because 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.
- ¹⁴ Note that cases with unemployment experiences between the termination of the previous job and the start of the new one constitute a clear minority in our data. Most individuals are able to find the new job very soon, which is in line with previous research on job mobility (Bratberg, Salvanes, & Vaage, 2010).
- ¹⁵ 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. To get an impression of proportions, about 14% of all the people in our main sample experience at least one job change during the entire investigation period. About 2% switch jobs twice and below 0.5% more than twice.

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