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**Taking lemons for a trial run: Does type of job exit affect the risk of entering fixed-term employment in Germany?**

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**Keywords:** job exit, fixed-term employment, screening, Germany

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## **Abstract**

A central argument for the deregulation of employment contracts is that fixed-term contracts boost employment of job seekers with uncertain productivity by giving employers a tool to screen such applicants over a longer period of time before permanent hire. We test this proposition by comparing the risk of entering fixed-term employment for individually laid off workers with that for individuals who have left their previous job for other reasons. This strategy is based on the assumption that in the German context individual layoffs create uncertainty about job seekers' productivity. We use data on work exits and subsequent labour market re-entry of the prime-age workforce in Germany from waves 2000-2013 of the Socio-Economic Panel. Our results show that the risk of fixed-term employment is substantively smaller after voluntary job exits but reveal only a small and statistically insignificant risk difference between individual layoffs and workplace closures after adjusting for differences in socio-economic background and characteristics of the previous job. These findings challenge the view that employers use fixed-term contracts as an instrument to screen specific groups of workers whose productivity is highly uncertain, at least with regard to recent career disruptions.

**Keywords:** job exit, fixed-term employment, screening, Germany

## Introduction

This study investigates the allocation of previously employed job seekers to fixed-term contracts. Specifically, we compare the risk of entering temporary employment after different types of job exits to test if employers use fixed-term contracts to screen job seekers with uncertain productivity before employing them long-term.

Since the 1980s there has been a noticeable expansion of fixed-term employment in many OECD countries usually justified as a measure to battle unemployment (e.g. Hipp, Bernhardt and Allmendinger, 2015; Kalleberg, 2000; Kalleberg, 2011; OECD, 2002).<sup>1</sup> Fixed-term contracts increase the flexibility of firms. For workers, however, temporary employment is linked with diminished job security and lower income levels. Previous research also found negative associations with welfare security, health, and psychological well-being (Emmenegger *et al.*, 2012; Gash, Mertens and Gordo, 2007; Kalleberg, 2009; Kalleberg, 2011; Mertens, Gash and McGinnity, 2007; Virtanen *et al.*, 2005). Whereas there is no clear evidence regarding the ability of fixed-term employment to lower unemployment levels, the rise in fixed-term contracts came with increased inequality in the workforce (Barbieri, 2009; Barbieri and Cutuli, 2016; DiPrete *et al.*, 2006; Emmenegger *et al.*, 2012; Gebel and Giesecke, 2016; Giesecke and Groß, 2003). Exacerbating the issue, fixed-term contracts are found predominantly among typically disadvantaged groups, that is, recent entrants to the labour market, migrants, women, elderly and unskilled workers, or previously unemployed individuals (Amuedo-Dorantes, 2000; Dieckhoff, 2011; Gebel and Giesecke, 2011; Gebel and Giesecke, 2016; Giesecke and Groß, 2003; OECD, 2002). The distribution of fixed-term contracts follows a stratification logic according to which bad jobs are assigned to presumably unproductive workers. Thus, the increased use of fixed-term employment might further

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<sup>1</sup> We use the terms temporary and fixed-term interchangeably. Thus, our definition of temporary employment does not entail being employed by a temporary work agency (cf. Kalleberg, 2000).

entrench existing stratification in the labour market with regard to job quality (Barbieri and Cutuli, 2016; Biegert, 2014; Emmenegger *et al.*, 2012; Fervers and Schwander, 2015; Gash and McGinnity, 2007).

Nevertheless, scholars and policy-makers alike have hailed fixed-term contracts as an opportunity to improve the labour market (re)integration of disadvantaged workers. Fixed-term contracts may enable employers to screen newly hired employees for a prolonged period of time (Baranowska, Gebel and Kotowska, 2011; Faccini, 2014; Portugal and Varejão, 2010). The possibility of learning in detail about match quality before making a decision about long-term employment might lead to employment for workers who otherwise would not have received a job offer at all. If fixed-term contracts are used to screen workers that signal uncertain productivity, the targeted allocation to fixed-term contracts might actually reduce existing labour market inequality in terms of employment rates.

It is difficult to disentangle whether a worker is allocated a fixed-term contract because of a match between bad job and presumably unproductive worker or in order to screen a worker because of uncertain productivity. In the present article, we advance the literature by investigating whether employers use fixed-term contracts as a screening device for job seekers with uncertain productivity. Similar to Gibbons and Katz' (1991) study on "Layoffs and Lemons", we shift the focus to specific career disruptions (see also Brand, 2003; Gibbons and Katz, 1991; Grund, 1999; Mincer, 1986; Schmelzer, 2012). Specifically, we contrast the effect of different job-exit types on subsequently entering fixed-term employment. This provides leverage for disentangling the use of temporary jobs as a screening device from mere productivity-to-job matches. In the context of German employment protection legislation, we stipulate that different types of job exits imply distinct levels of uncertainty for employers (Brand, 2006; Gibbons and Katz, 1991). If employers targeted fixed-term contracts at hires with uncertain productivity, this should be reflected in

differences in the likelihood of entering fixed-term employment after job-loss through individual layoff compared to other job exit types.

For our analysis, we use data on work exits and subsequent re-entry to the labour market by German employees from the Socio-Economic Panel (SOEP). Germany is an ideal test case for the hypothesis that job exit types affect the likelihood of subsequent fixed-term employment. Strict employment protection in Germany makes fixed-term contracts particularly valuable to employers because firing unproductive workers is very costly (Canziani and Petrongolo, 2001; Fervers and Schwander, 2015). Moreover, the admissible reasons to justify individual layoffs render this job exit type particularly ambiguous as a productivity signal. We use inverse probability of treatment and censoring weighting, a semi-parametric method for the estimation of causal effects, because it allows us to simultaneously address both selection into different job exit types and into subsequent reemployment.

## **Background**

### *The allocation of fixed-term contracts*

Fixed-term contracts increase the operational flexibility of a company because they enable the reduction of labour regardless of employment protection for permanent contracts. It is thus in employers' interest to maintain a steady stock of workers on fixed-term contracts who can be readily laid off when there is a need to adjust to economic downturns (Blanchard and Landier, 2002; Golden and Appelbaum, 1992). Because the use of fixed-term contracts is restricted by labour market regulation and employers have an interest to attract certain workers by offering better jobs, potential employees have to be selected into fixed-term contracts.

There is ample evidence for social stratification in the distribution of fixed-term contracts. This distribution follows typical signals of individual productivity (Akerlof, 1976; Akerlof, 1970; Spence, 1973). In a variety of countries, temporary employment is

disproportionately found among labour market entrants, immigrants, the low educated (and in some cases the very high educated), and women (Amuedo-Dorantes, 2000; Gebel, 2010; Gebel and Giesecke, 2011; Gebel and Giesecke, 2016; Giesecke and Groß, 2003; McGinnity, Mertens and Gundert, 2005). Additionally, the incidence of temporary employment varies according to previous employment record as well as occupation, sector, and firm size (Dieckhoff, 2011; Gebel, 2010; Giesecke and Groß, 2003). These findings imply that because temporary jobs are considered bad jobs with less security and lower pay, employers will offer them to workers who signal low productivity. Employers have no interest in tying less productive workers to their company. Therefore, they increase their ability to lay off these workers instead of those with higher productivity when the economic situation demands. This indicates that employers turn contract type into an aspect of overall job quality. As is the case with low wage or part-time jobs, bad jobs are matched to workers with low productivity.

However, scholars regularly point out that temporary contracts differ from other bad or atypical jobs because of their fixed-term property. This property, they argue, enables circumvention of employment protection regulations, effectively rendering fixed-term contracts potential devices for screening workers after hire (Baranowska *et al.*, 2011; Faccini, 2014; Portugal and Varejão, 2010). Used as prolonged probation periods, temporary contracts enable employers to collect first-hand information on worker performance, thus lowering their risk of permanently hiring unproductive workers (Wang and Weiss, 1998). This explanation of the allocation of fixed-term contracts to workers emphasizes signals that induce uncertainty in addition to traditional signals of worker productivity (Akerlof, 1976; Akerlof, 1970; Spence, 1973). Employers might use fixed-term contracts to obtain a more complete picture of workers for whom the accuracy of traditional productivity indicators such as educational credentials or employment history is compromised. One implication is that fixed-term contracts may particularly benefit jobseekers with traits that signal a relatively high degree of

uncertainty regarding their productivity and who, without the possibility of fixed-term contracts, would struggle to find work.

Although several studies discuss this screening hypothesis, they usually do not take the uncertainty dimension into account. Rather they either expect a need for screening for individuals with low productivity or interpret fixed-term employment as a screening phase when workers subsequently transition into permanent employment (Baranowska *et al.*, 2011; Faccini, 2014; Portugal and Varejão, 2010). Despite delivering numerous insights about labour market processes, these studies cannot distinguish screening from mere productivity-to-job matches, because the need to screen should only differ across groups of workers if they induce distinct levels of uncertainty about their productivity. If, for instance, an employee signals low productivity with a high certainty he/she might well be matched to a bad job – maybe a fixed-term contract – but there would be no need for **extended** screening. To distinguish screening from simple allocation of workers with low productivity to unattractive jobs, a framework is needed in which an additional signal casts doubt on the actual productivity of a worker and thus might lead to differences in otherwise comparable individuals. In the following, we propose a framework in which different types of job exits help us disentangle uncertain productivity from (presumed) low productivity.

#### *Layoffs, lemons, and fixed-term contracts in the German context*

The literature on scars of unemployment establishes job-loss as an event able to explain different career trajectories of otherwise comparable individuals (e.g. Arulampalam, 2001; Brand, 2006; Brand, 2015; Gangl, 2004; Gangl, 2006; Ruhm, 1991). Within the setting of unemployment scarring, Gibbons and Katz (1991) use job exit types to test hypotheses about signalling effects on income. Referring to Akerlof's (1970) famous study on used car markets, they propose that job exit type might provide a signal that helps employers sort out potential "lemons" (see also Brand, 2003; Brand, 2006). Specifically, they argue that layoffs



send a signal of lower productivity as they suggest slack performance or faulty work. By contrast, they suggest that future employers do not blame workers for losing their jobs in the case of workplace closure.

In the present study, we make use of the distinction between individual layoffs and workplace closure to test if employers use temporary jobs for screening in Germany. Due to the rather strict German employment protection legislation for permanent contracts, fixed-term contracts are particularly valuable to employers as a tool to maintain flexibility and – potentially – to screen workers with uncertain productivity. Similar to most other OECD countries temporary employment increased notably in Germany (Eichhorst, Marx and Tobsch, 2015; OECD, 2002; OECD, 2014). In 2011, fixed term contracts made up almost 15% of all German work contracts.<sup>2</sup> Unlike in the US where the low degree of employment protection renders individual layoffs a signal for low productivity (Gibbons and Katz, 1991), individual layoffs are to some extent decoupled from productivity in the German context. After a trial period of usually six months, individual layoffs are permitted only for three reasons: misconduct (e.g., theft, frequent absenteeism, or slack work), indisposition (e.g., chronic illness, disability), and redundancy. In case of the latter, dismissals need to be socially tenable, judged on grounds of seniority, age, and family obligations of individual employees (Jahn, 2009). In addition, work councils are able to veto ambiguous layoffs and thereby can delay their enforcement. Because only some individual layoffs in Germany are therefore

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<sup>2</sup> Legislation eased the use of fixed-term contracts steadily over the last 3 or so decades. Until 1985, when short fixed-term contracts became generally available in an effort to battle widespread unemployment, temporary employment in Germany was an exception for specific, legally defined circumstances (e.g., replacements of workers on leave, temporary projects). Further legislation in 1996 and 1999 restricted fixed-term contracts to a maximum duration of two years for workers who had not been previously hired by the same employer, with the possibility of prolonging initially shorter contracts up to three times. Longer durations remained feasible only in justified situations such as temporally limited project work or academia. These regulations were relaxed again in the early 2000s Hartz reforms. For instance, newly established firms were allowed to use fixed-term contracts for up to 4 years without having to provide a valid reason.

indicative of low productivity, this job exit type entails a high degree of uncertainty (see Figure 1). By contrast, the expected productivity distribution among workers who lost their job due to workplace closure should be less dispersed and more positive on average. Comparing two otherwise similar workers (in terms of their traditional productivity signals), the signal of individual layoff should create doubt about the actual productivity in future employers.<sup>3</sup>

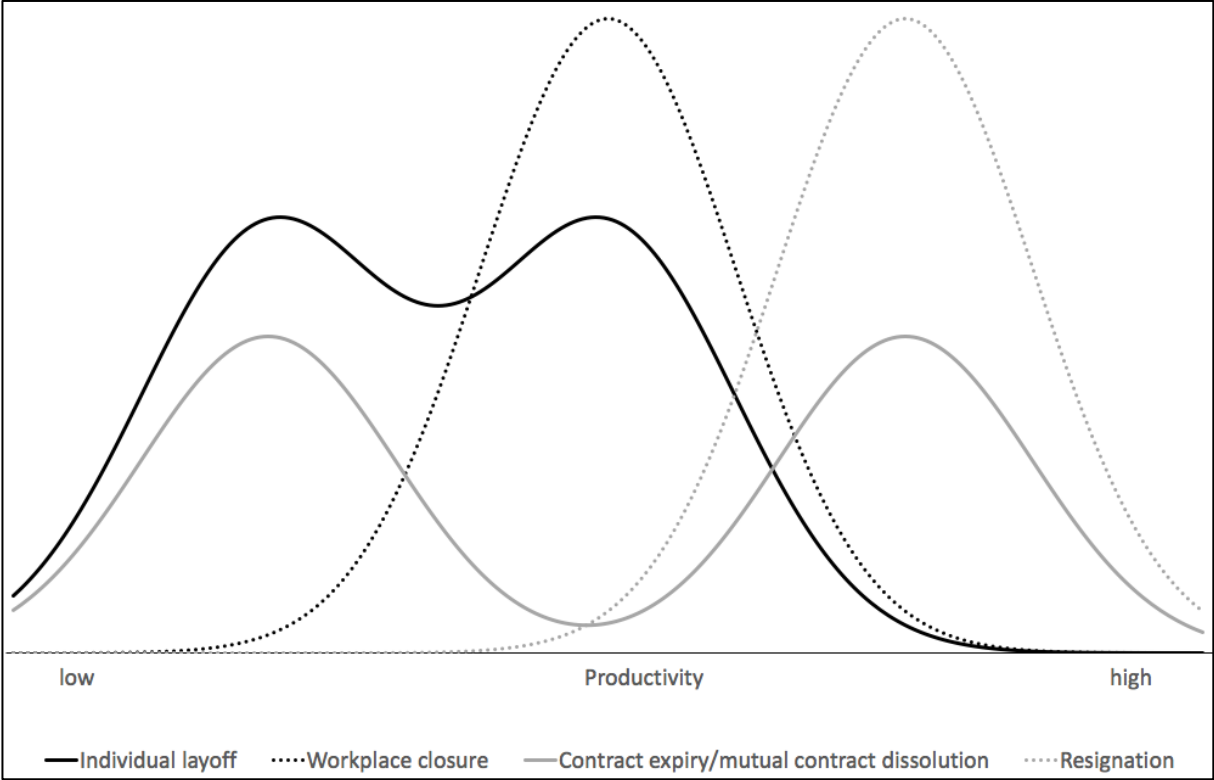
Thus, our central hypothesis is: *Individual layoffs increase the probability of subsequently entering fixed-term employment relative to workplace closures for otherwise comparable individuals.*

For a more complete perspective on the relationship between job exit type and allocation to temporary employment, we also looked at voluntary job exits by employees and two more ambiguous job exit types, namely, mutual termination of the job and contract expiry. Existing research shows that voluntary job exits are associated with better subsequent job quality, most likely because of already having better job prospects lined up at the time of exit (Mincer, 1986; Schmelzer, 2012). In line with that reasoning and under the assumption that temporary jobs are widely regarded as being unattractive, we expect that individuals who leave current employment voluntarily have a relatively high productivity and therefore a lower risk of entering temporary employment. In addition, other job exit types are more often associated with a subsequent period of unemployment further decreasing job prospects

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<sup>3</sup> A growing literature discusses the role of labour market intermediaries in the matching process of jobs and workers (e.g. Autor, 2009). The increased importance of third party actors such as Public Employment Offices and temporary help agencies in so-called triadic relationships has not been matched by evidence on how they might moderate the matching process, however (Bills *et al.* 2017). In the present study, the efforts of actors to whom worker searches have been outsourced might help reduce uncertainty about worker productivity and thus mitigate adverse selection between workers who were laid off and those who lost their job due to workplace closure. On the other hand, intermediaries might anticipate employers' preferences and minimize risk and thus exacerbate effects of uncertainty as has been shown for gendered hiring effects (Fernandez-Mateo and King, 2011). As we do not have information on the role of intermediaries in the job matching process, we have to defer to future studies to investigate these issues.

relative to voluntary exits (Edin and Gustavsson, 2008). Similarly, mutual contract dissolution might indicate that an employee already has good job prospects. However, the employers' willingness to let an employee go might also signal low productivity or a bad fit. Along the same lines, the expiry of a contract sends mixed signals. On the one hand, it might indicate a less productive worker since the employer does not renew the contract. On the other hand, it might be the choice of the employee to let the contract run out and take a better job opportunity. The perceived necessity to screen these workers should thus be higher than workplace closures yet below layoffs.



**Figure 1. Hypothesized productivity distributions for different job exit types**

## **Method**

### *Data and sample*

Our empirical analyses are based on the SOEP v30 (Wagner, Frick and Schupp, 2007), an annual survey of representative German households carried out since 1984 by the SOEP Group located at the German Institute for Economic Research, Berlin. The vast majority of respondents completed personal paper and pencil or computer-assisted interviews with trained survey personnel, telephone interviews and mail-in questionnaires being an exception. Besides a host of socio-economic and demographic covariates, these data include information on working respondents' contract type and the reason for job exits.

Because consistent information on reasons for job exit is only available from 2001 onwards, we used covariate information from waves 2000 to 2013. We focused on respondents aged 25 to 54 at the time of job exit who left their job following resignation, mutual contract termination, contract expiry, individual layoff, or workplace closure between 2001 and 2010, yielding 4,965 job exits from 3,732 respondents. From this original sample, 246 respondents (7%) and 351 job exits (7%) were dropped due to missing values on covariates. We used the remaining sample to analyse censoring, that is, not being observed in dependent employment and with valid information on contract type within the three waves following job exit. Finally, there were 3,307 uncensored job exits from 2,526 respondents, for which we could estimate the association between type of job exit and subsequent employment contract. Of these job exits, 1,415 were resignations, 334 were mutual terminations, 498 were contract expiries, 749 were individual layoffs, and 311 were workplace closures.

### *Measures*

The main dependent variable of this study is an indicator for holding a fixed-term as opposed to a permanent employment contract in the first job following a job exit. This information was recorded annually through respondents' self-reports. To determine the date

of reemployment, we used monthly activity calendars, with which we were able to match subsequent contract type to each job exit. We also generated a censoring indicator to record whether information on employment contract was unavailable, either because respondents had not re-entered dependent employment within three years after job exit or because contract type upon labour market re-entry was missing.

The main independent variable of this study is type of job exit, which we also derived from annual self-reports by respondents. We used a variable based on annual information regarding the reason of the most recent job exit in the year preceding an interview as well as the current calendar year up until the interview date. Respondents could choose, besides other types of job exit that we excluded from the present analyses, from the following categories: giving notice, mutual contract termination, contract expiry, individual layoff, and workplace closure.<sup>4</sup>

To model selection into the different job exits and being censored, we used a number of variables measured at the interview before job exit that capture respondents' demographic background, self-rated health, household structure and resources, qualifications as well as characteristics of the previous job. Demographic background variables included age, sex, an indicator whether a respondent herself or any of her parents immigrated to Germany, and an indicator whether a respondent resided in a federal state belonging to West Germany as opposed to East Germany. Self-rated health was available on a five-point-scale, which we turned into an indicator for unsatisfactory or bad health. Marital status ('single', 'married', 'separated/divorced/widowed'), number of children in the household ('one child', 'two children', 'three or more children'), and annual household income were used to measure household structure and resources. Measures for qualifications were indicators for education based on the International Standard Classification of Education ('inadequate and elementary',

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<sup>4</sup> Excluded job exit types are retirement, maternity leave, and going out of business after self-employment.

‘intermediate or Abitur with vocational training’, ‘higher vocational’, ‘tertiary’) as well as years of experience in full-time and part-time employment. Measured characteristics of the previous job included duration of tenure, public sector vs. private sector employment, firm size (‘less than 20 employees’, ‘20 to 100 employees’, ‘201 to 2000 employees’, ‘2001 and more employees’), occupation based on the two-level International Standard Classification of Occupations-88, and industry with both occupation and industry including a category for missing values. To adjust for aggregate conditions that may change over time and affect both job exit types and trends in temporary employment, we included indicators for the survey year.<sup>5</sup>

### *Analytic strategy*

Estimating the effect of job exit type on entering temporary employment faces two key challenges: differences between job exits other than type of exit that affect entering temporary employment (i.e., confounding) and non-random selection into the sample with observed contract type (i.e., systematic censoring). For example, employees at an earlier career stage with little work experience may be more likely to be laid off and have a higher risk of entering temporary employment thus creating a spurious association between individual layoff and subsequent contract type. But a spurious association may also result if censoring (and thus observation of contract type) were related to job exit type and entering temporary employment (Elwert and Winship, 2014; Winship and Mare, 1992). This would be the case if job exit type affected the probability of being censored such that only the most productive individuals who were laid off would be observed with valid information on contract type (see Online Supplement for a graphical depiction of our causal model).

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<sup>5</sup> We also run separate robustness checks for the period before 2007 and since in order to assess whether the reforms in the early 2000s or the economic crisis moderate our results (see Table S3 in the Online Supplement). We do not find substantive differences for the two time periods.

We address these challenges by using inverse probability of treatment and censoring weighting (Hernán and Robins, 2006; Hernán and Robins, forthcoming). Instead of explicitly controlling for measured covariates to correct for confounding and non-random censoring, this technique regresses contract type after reemployment on type of job exit in a weighted pseudo-population in which job exit type is independent of covariates and censoring is independent of covariates and type of job exit. The treatment weight  $tw$  is formally defined as the ratio of the unconditional probability that a respondent  $i$  experiences the observed job exit type  $e$  and the same probability conditional on covariates  $Z$  measured before the job exit,

$$tw_i = \frac{P(E_i=e_i)}{P(E_i=e_i|Z_i)}. \quad (1)$$

Applying this weight creates a pseudo-population, in which respondents with covariate values overrepresented in the observed job exit type are given less weight, whereas respondents with less frequent covariate values receive a higher weight. After weighting, confounders are thus equally distributed among all types of job exit.

Reweighting with censoring weights,

$$cw_i = \frac{P(C_i=0)}{P(C_i=0|E_i, Z_i)}, \quad (2)$$

corrects for non-random censoring based on job exit type and covariates measured before job exit. Equivalent to treatment weights, using the weights  $cw_i$  creates a pseudo-population that would have been observed had censoring been random with regard to type of job exit and covariates. Using the product of both weights for reweighting the uncensored sample thus effectively corrects for confounding by the measured covariates and non-random censoring based on job exit type and measured covariates. Because all probabilities in Equations 1 and 2

are unknown, we estimated them using multinomial logistic regression and logistic regression, respectively (see Table S1 in the Online Supplement for the models estimating both denominators).

Under the assumptions of no unmeasured confounding and systematic censoring, positivity, and correct parametric specification of the weight models, simple risk differences in the weighted pseudo-populations yield consistent estimators for the average causal effect of job exit type on entering temporary employment. No unmeasured confounding and systematic censoring is a theoretical assumption that cannot be tested empirically. Its plausibility depends on whether the measured covariates include relevant confounders and determinants of censoring. Positivity demands a nonzero probability of each job exit type for any combination of covariate values to ensure a comparison of “like with like”. Violations of positivity as well as misspecifications of the weight models lead to estimated weights with mean values far from one or large standard deviations (Cole and Hernán, 2008). Table S2 in the Online Supplement shows that neither was the case for our weights. The above assumptions are the same assumptions as for conventional regression. An important advantage of inverse probability weighting is, however, that covariates are not included in the final outcome model. Therefore, misspecification bias through exclusion of interaction effects between covariates and job exit types can be avoided and the coefficients for job exit types directly correspond to average differences in the probability to enter temporary employment (and in the probability to be uncensored) (Morgan and Todd, 2008; Winship and Elwert, 2010).

## **Results**

We present our findings in three steps. First, we show mean differences in covariates by type of job exit. Second, we display how censored and uncensored job exits differ with regard to job exit type and covariates. Finally, we present the estimated differences between



job exit type in the probability of being uncensored and the probability of entering temporary employment. In both cases, we report unadjusted differences along with estimates after covariate adjustment through IPT weighting. For differences in entering temporary employment, we also adjust for systematic censoring based on job exit type and measured covariates.

*Covariate differences by job exit type and censoring status*

Table 1 depicts covariate differences by job exit type for the full analytic sample based on means for continuous variables and on percentages for categorical variables. Comparing individual layoffs to workplace closure, there are no large differences on these measured characteristics, with the exception that marriage is less frequent among the former. The contrast between individual layoffs and resignations is, as one would expect, much larger. Individuals who experienced layoff were more likely to be male, more often reported to be not in good health and to reside in East Germany, had lower education and household income, and less often worked in the public sector and larger firms than individuals who resigned voluntarily. In terms of education, household income, family status, and age, resignations were quite similar to mutual contract dissolutions. But the latter also had the highest percentage of individuals who reported being not in good health. Contract expiries were unique in their high percentage of public sector employment and being single as well as comparatively little work experience.

**Table 1. Means and percentages for covariates before job exit by type of job exit, full sample**

	Individual layoff	Resignation	Mutual termination	Contract expiry	Workplace closed
Age	39.0	37.3	38.7	37.0	40.5
Male	56.5	50.0	53.2	45.7	58.5
Migration background	23.2	20.7	18.0	18.9	21.5
Poor or bad health	13.1	8.8	15.0	9.8	10.8
Family status					
Married	54.2	54.7	56.6	41.5	66.8
Single	31.8	34.5	29.7	44.9	23.7

Separated/divorced/widowed	14.0	10.8	13.8	13.6	9.5
Number of children in HH					
None	53.6	53.6	50.4	58.2	50.5
One	25.3	22.8	27.3	24.0	25.5
Two	15.8	17.5	18.9	13.9	19.8
Three or more	5.3	6.1	3.4	3.9	4.2
Residence in West Germany	61.4	83.3	71.6	58.8	69.9
Education					
Inadequate and elementary	13.6	8.5	8.5	9.2	13.8
Middle and Abi vocational	65.7	57.0	52.5	53.1	65.5
Higher Vocational	7.4	9.4	10.6	6.2	7.3
Higher Education	13.3	25.2	28.4	31.5	13.4
Full-time work experience	12.7	10.1	11.7	8.7	14.4
Part-time work experience	1.7	2.4	2.2	2.0	2.1
Tenure with employer	3.7	3.9	6.0	1.4	6.7
Annual HH income in 1,000€	33.8	42.2	42.2	34.2	38.3
Public sector employment	7.4	13.7	21.0	37.9	6.6
Firm size					
< 20 employees	45.0	34.1	28.6	24.9	39.8
20 – 200 employees	33.9	31.2	29.2	38.0	35.2
200 – 2000 employees	13.8	17.9	21.2	19.0	14.3
2000 + employees	7.3	16.8	21.0	18.1	10.8
Occupation					
Legisl./sen. officials/manag.	4.1	5.9	5.5	3.0	4.2
Professionals	6.9	16.6	21.4	29.4	9.9
Technicians	16.2	22.8	25.8	17.7	17.4
Clerks	11.0	11.4	12.7	8.8	12.7
Service & sales	12.5	13.9	10.0	8.6	10.8
Crafts/skilled agricult./fish.	28.1	15.0	11.0	15.6	25.3
Operators/elementary occ.	20.5	13.3	12.9	14.6	19.3
Unknown	0.7	1.2	0.6	2.4	0.4
Industry					
Agricult./forestry/fish.	3.0	0.8	1.3	2.6	0.2
Manufact./mining/constr.	41.6	28.0	28.2	22.0	46.4
Electricity/gas/water	0.2	0.6	1.3	0.9	0.2
Trade	16.6	17.1	10.4	7.7	18.9
Hotels/restaurants	3.9	4.4	3.0	3.4	3.7
Transport/communication	5.0	6.1	6.1	3.1	7.9
Finance/insurance	1.7	3.9	3.6	0.7	2.0
Real estate	11.4	13.0	12.1	9.7	9.0
Administration	0.9	1.5	4.9	8.2	1.5
Education	2.4	4.8	5.3	17.0	1.3
Health/social work	7.7	13.9	16.7	14.3	3.3
Other service	3.2	3.8	5.3	8.0	2.4
Unknown	2.4	1.9	1.9	2.6	3.1
N (persons)	1,071	1,486	452	654	434
N (job exits)	1,232	1,710	473	744	455

*Note.* Statistics pertain to full sample including observations without valid information on subsequent contract type

Turning to a comparison of those who were observed with valid information on contract type within three years after job exit and those who were not, Table 2 shows that individual layoffs were more frequent among the censored and that resignations were more frequent among the uncensored. The other job exit types were distributed much more equally.

Moreover, censored observations generally were more likely to report not being in good health, more likely to reside in East Germany, and had lower education than uncensored observations. They also were less likely to be a professional or to be a technician and more likely to have previously worked as an operator or in an elementary occupation

**Table 2. Means and percentages for covariates by censoring status**

	Uncensored	Censored
Job exit type		
Individual layoff	22.6	37.0
Resignation	42.8	22.6
Mutual termination	10.1	10.6
Contract expiry	15.1	18.8
Workplace closed	9.4	11.0
Age	37.6	39.4
Male	52.9	50.2
Migration background	19.8	23.6
Poor or bad health	9.4	15.1
Family status		
Married	53.0	55.9
Single	35.8	29.2
Separated/divorced/widowed	11.2	15.0
Number of children in HH		
None	54.9	50.7
One	23.9	25.7
Two	16.8	17.0
Three or more	4.4	6.6
Residence in West Germany	72.3	67.5
Education		
Inadequate and elementary	8.6	15.3
Middle and Abi vocational	58.4	60.7
Higher Vocational	8.6	7.3
Higher Education	24.4	16.6
Full-time work experience	10.9	11.8
Part-time work experience	2.0	2.2
Tenure with employer	3.9	4.2
Annual HH income in 1,000€	39.1	36.1
Public sector employment	16.4	14.8
Firm size		
< 20 employees	33.9	39.6
20 – 200 employees	33.6	32.2
200 – 2000 employees	17.6	15.4
2000 + employees	14.9	12.8
Occupation		
Legisl./sen. officials/manag.	5.1	3.6
Professionals	17.3	12.5
Technicians	21.5	16.1
Clerks	10.9	11.8
Service & sales	11.6	12.9
Crafts/skilled agricult./fish.	18.3	21.5
Operators/elementary occ.	14.2	20.6
Unknown	1.1	1.1
Industry		

Agricult./forestry/fish.	1.4	2.3
Manufact./mining/constr.	32.3	32.8
Electricity/gas/water	0.7	0.3
Trade	14.5	15.9
Hotels/restaurants	3.7	4.3
Transport/communication	5.6	5.4
Finance/insurance	2.7	2.2
Real estate	11.9	10.9
Administration	2.5	3.4
Education	6.4	4.5
Health/social work	12.0	10.4
Other service	4.0	5.1
Unknown	2.2	2.5
N (persons)	2,526	1,254
N (job exits)	3,307	1,307

*Note.* Uncensored = observed with valid information on contract type within three years after job exit.

### *Job exit type, censoring, and temporary employment*

Table 3 summarizes the results regarding differences in the probability to be uncensored and the risk to enter temporary employment after the different job exit types. The first line displays the respective risk after individual layoff, which serves as the reference to which the other job exit types are compared. These comparisons on the risk difference scale are shown in the subsequent four lines. Finally, Table 3 shows the results of testing whether the null hypothesis of equality between the different risk differences can be rejected, which provides information on statistically significant differences in the probability of being uncensored and the probability of entering temporary employment between all other job exit types. The first two columns show the results without taking into account differences on measured covariates. In Columns 3 to 4, the estimates were adjusted for measured covariate differences using IPT weighting. The final column reports the estimates for the risk of entering temporary employment that were additionally adjusted for differences in censoring based on job exit type and covariates.

Beginning with the unadjusted estimates for being uncensored, Table 3 shows that for only roughly 61% of job exits through individual layoff valid information on contract type was observed within three years after job exit. For all other job exit types a significantly

higher probability of being uncensored was estimated, with by far the largest advantage found for resignations with 22 points. After resignations, the probability of being uncensored was also significantly higher than for all other job exit types. In contrast, there were no significant differences between workplace closure, mutual contract dissolution, and contract expiry.

The risk of entering temporary employment after individual layoff was estimated at roughly 35% (conditional on being uncensored). While the risk difference for resignation was -10 points, there were no significant risk differences between individual layoff and workplace closure. At an estimated -1 point, the difference was also substantively small. After mutual contract termination and, particularly, contract expiry the risk of entering temporary employment was 6 points and 25 points higher relative to individual dismissal, respectively.

To evaluate whether these observed risk differences were driven by existing heterogeneity between the job exit types on measured covariates and censoring we used inverse probability weighting. The results, however, remained virtually the same. Individual layoff still was associated with a sizably lower probability of being uncensored compared to all other job exit types except contract expiry. There was no penalty for individual layoff relative to workplace closure with regard to entering temporary employment upon re-entry to the labour market, the main focus of this analysis. Measured covariates, therefore can explain neither the similarity of workplace closures and individual layoffs in terms of entering temporary employment nor the large differences between individual layoffs and voluntary job exits. At first, the significantly higher likelihood to enter fixed-term employment after contract expiry seems striking. Yet, by definition, contract expiry can only occur at the end of a fixed-term contract. It is plausible to assume that what we observe are individuals who repeatedly enter temporary employment, perhaps due to their own preferences or specific careers such as academia.

In sum, our results do not conform to our main hypothesis that individual layoffs are associated with an increased risk of entering temporary employment compared to workplace closure. We find that job exit type is informative with regard to observing valid information on contract type within three years after job exit, a large part of which is based on prolonged time out of the labour market. Individuals who were laid off are notably less successful in finding employment in the observed period. Yet, conditional on being observed in employment within three years of job exit, they do not show a higher likelihood of fixed-term employment. Thus, the argument that employers target fixed-term contracts at job seekers with particularly uncertain productivity was not supported.

**Table 3. Estimated Differences in Probability to be Uncensored and to Enter Temporary Employment by Type of Job Exit**

	Unadjusted		IPT-Weighted		IPT&C-Weighted
	Uncensored	Temporary Employment	Uncensored	Temporary Employment	Temporary Employment
Reference probability					
Individual layoff	.608*** (.014)	.348*** (.018)	.619*** (.016)	.375*** (.021)	.378*** (.021)
Risk differences					
(1) Resignation	.220*** (.017)	-.103*** (.021)	.188*** (.020)	-.120*** (.025)	-.119*** (.025)
(2) Mutual termination	.098*** (.025)	.059 (.033)	.107*** (.029)	.057 (.039)	.055 (.040)
(3) Contract expiry	.061** (.022)	.248*** (.029)	.032 (.030)	.171*** (.039)	.171*** (.040)
(4) Workplace closure	.076** (.026)	-.011 (.032)	.084** (.032)	-.011 (.044)	-.007 (.044)
Test of equality of risk differences					
(1) vs. (2)	***	***	**	***	***
(1) vs. (3)	***	***	***	***	***
(1) vs. (4)	***	**	**	**	**
(2) vs. (3)	n.s.	***	*	*	*
(2) vs. (4)	n.s.	n.s.	n.s.	n.s.	n.s.
(3) vs. (4)	n.s.	***	n.s.	***	**

N (persons)	3,486	2,526	3,486	2,526	2,526
N (job exits)	4,614	3,307	4,614	3,307	3,307

*Note.* \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses. Uncensored = observed with valid information on employment contract within three years after job exit.

## Discussion

In this study, we tested the hypothesis that allocation into temporary employment is (in part) based on signals of uncertain productivity, because employers want to screen specific groups of workers, whose permanent hire would be too risky. Whereas the existing literature delivers clear evidence for a socially stratified distribution of contract types across typical signals of worker productivity, there are no tests of fixed-term contracts being used as a screening device to reduce uncertainty. We argued that comparing different types of job exits might prove more informative regarding the screening hypothesis because they imply distinct levels of uncertainty on top of traditional productivity signals. Differences between job exit types would provide a clearer indication of employers' willingness to use fixed-term contracts to screen workers with uncertain productivity and to avoid inadvertently employing "lemons" on permanent contracts. We compared the effect of individual layoff on entering temporary employment with the effects of workplace closure and other types of job exits using panel data on the German prime-age workforce (SOEP 2000-2013). In the context of the German employment protection legislation, we expected that individual layoffs induce a particularly high level of uncertainty and thus should increase the probability of fixed-term employment. To account for various sources of confounding and sample selection that may produce a spurious association between job exit type and subsequent employment contract, we used inverse probability of treatment and censoring weights in our models. Altogether, the study contributed to the literature by shedding light on the question whether employers use temporary contracts to screen risky hires by using job exit types to disentangle the allocation process of contracts types from mere job-to-skill matches.

Contrary to the seminal study by Gibbons and Katz (1991) but consistent with an earlier study on Germany regarding wage mobility after different types of work breaks (Grund, 1999), we found no meaningful differences between layoffs and workplace closure in the probability to enter temporary employment. Similar to Schmelzer (2012), we found that only voluntary resignation is advantageous with regard to avoiding fixed-term contracts. We found little to no evidence that the allocation of temporary contracts depends on the degree of uncertainty that marks workers' productivity as indicated by their job exit type. Adjusting for measured covariates such as education, work experience, or prior occupation did not change this result, because, on average, differences on these characteristics were only minor between these two job exit types.<sup>6</sup> Therefore, unmeasured characteristics such as personality may play an important role for individual layoffs. But although we cannot account for potential confounders like personality, the results suggest that, if at all, they only affect hiring decisions, not the type of contract as individual layoffs substantively lower the probability of being observed in employment (i.e., being uncensored) within three years after job exit but not with the type of contract. In other words, it seems like employers take layoff (or its unmeasured determinants like personality) into account for hiring but not for the decision about contract type.

These results speak against the stipulation that employers use temporary employment as a tool to selectively screen workers with uncertain productivity. Our results do not exclude the possibility that employers use temporary contracts for screening workers on the job independent of uncertainty, eventually turning temporary contracts into stepping-stones to better and stable employment opportunities (although the existing evidence for the stepping-stone function of fixed-term contracts is mixed, see e.g. Baranowska *et al.*, 2011; Booth, Francesconi and Frank, 2002; de Graaf-Zijl, van den Berg and Heyma, 2011; Faccini, 2014;

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<sup>6</sup> This may indicate that most of the individual layoffs are due to redundancy, which in the German context, is decoupled from productivity to some degree.



Gebel and Giesecke, 2016; Portugal and Varejão, 2010). Additionally, there might be other sources of uncertainty besides job exit type which prompt employers to offer workers fixed-term contracts in order to screen them. For the case of job exit types, however, our findings challenge the view that employers target them at specific workers with relatively uncertain productivity. Therefore, the analysis raises doubt that the expansion of temporary employment has particularly improved the job prospects of individuals whose productivity is uncertain to employers, by giving them the opportunity for prolonged probation. As temporary employment is generally more likely after an involuntary job exit, our findings also indicate that employers allocate fixed-term contracts to applicants with lower bargaining power. In light of the existing evidence on the distribution of fixed-term contracts, this implies that employers might use them as a general insurance against hiring unproductive workers, perhaps in order to be flexible in economic downturns (Blanchard and Landier, 2002; Golden and Appelbaum, 1992). In this perspective, the benefits for workers or job seekers are less clear-cut. Rather, our findings support the view that contract type is part of the overall quality of a position, and that temporary contracts (which are less desirable for a number of reasons) are offered to less productive workers. The increased use of fixed-term contracts might thus further entrench labour market inequality between social groups (Barbieri and Cutuli, 2016; Biegert, 2014; Emmenegger *et al.*, 2012; Gash and McGinnity, 2007).

Our analysis had a number of limitations, which might provide starting points for further empirical research. The similarity of workplace closure and individual layoff with regard to entering temporary employment may be caused by regional depressions in the level of job quality following the closure of large firms, thereby masking the true differences between laid off workers and those displaced by workplace closures. Further analyses may address this issue by regionally stratified estimation. Furthermore, workplace closure might

be foreseeable in some instances, which might lead to pre-emptive layoffs and mutual contract dissolutions, which might distort the signalling power of different job exits. There is also evidence for recall bias in the reporting of reasons for job exit (Song, 2007). Because the SOEP collects this information with a time lag of only 12 months, on average, we are confident that this is not a major issue for our analysis. In the context of German employment protection legislation, there is a six months probation period even for permanent contracts. In some cases, this period might be considered enough to screen workers with uncertain productivity. If so, however, the argument for increasing the use of fixed-term contracts to enable additional screening would be invalid. Finally, the analysis focuses on one specific form of career disruption that may signal uncertainty regarding productivity. Additional insights on the selective use of fixed-term contracts may be gained by extending the analysis to other factors such as health issues or incarceration, that are less dependent on the overall economic context.

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## Online Supplement

**Table S1. Summary of models for estimating denominators of treatment weight (multinomial logistic regression) and censoring weight (logistic regression)**

	Denominator treatment weight			Workplace closure	Denominator Censoring weight
	Individual layoff	Mutual termination	Contract expiry		
Job exit type (Ref.: Individual layoff)					
Resignation					-.972*** (.094)
Mutual termination					-.321** (.124)
Contract expiry					-.116 (.113)
Company closed					-.382** (.124)
Age	.047 (.069)	-.123 (.094)	-.205* (.083)	.039 (.099)	-.039 (.056)
Age <sup>2</sup>	-.000 (.001)	.002 (.001)	.003*** (.001)	.000 (.001)	.001 (.001)
Migration background	.304** (.107)	.013 (.153)	.157 (.141)	.146 (.148)	.120 (.093)
Occupation (Ref.: Legislators, senior officials and managers)					
Professionals	-.308 (.241)	.227 (.274)	.328 (.283)	.456 (.345)	.124 (.210)
Technicians	.010 (.216)	.172 (.253)	-.206 (.291)	.340 (.304)	-.039 (.195)
Clerks	.183 (.234)	.251 (.279)	.069 (.315)	.387 (.325)	.178 (.203)
Service & sales	.046 (.235)	-.187 (.302)	-.347 (.314)	.019 (.321)	.081 (.211)
Crafts/skilled agriculture/fishing	.228 (.224)	-.509 (.301)	.050 (.301)	.195 (.316)	.245 (.198)
Operators/Elementary	.267 (.227)	-.099 (.291)	.012 (.297)	.296 (.314)	.356 (.197)
Unknown	-.208 (.495)	-.641 (.666)	-.521 (.519)	-.414 (.718)	.130 (.405)
Industry (Ref.: agricult./ forestry/fishing)					
Manufacturing/mining /construction	-.475 (.365)	-.410 (.530)	-1.115** (.399)	2.164* (1.049)	-.228 (.272)
Electricity/gas/water	-1.779* (.874)	.132 (.720)	-.807 (.681)	.793 (1.500)	-.873 (.535)
Trade	-.816* (.378)	-.921 (.548)	-1.610*** (.421)	1.880 (1.057)	-.061 (.283)
Hotels/restaurants	-1.225** (.425)	-.654 (.621)	-1.290** (.468)	1.597 (1.099)	-.059 (.341)
Transp./communic.	-1.092** (.409)	-.578 (.566)	-1.948*** (.461)	2.070 (1.075)	-.209 (.299)



Finance/insurance	-1.195** (.449)	-.808 (.598)	-2.681*** (.648)	1.261 (1.114)	.032 (.364)
Real estate	-.852* (.379)	-.520 (.545)	-1.391*** (.419)	1.500 (1.067)	-.200 (.291)
Administration	-1.068* (.512)	.306 (.615)	-.072 (.474)	1.663 (1.178)	.092 (.349)
Education	-1.127** (.437)	-.504 (.584)	-.296 (.444)	.600 (1.143)	-.339 (.323)
Health/social work	-1.181** (.389)	-.306 (.550)	-1.221** (.419)	.362 (1.081)	-.135 (.296)
Other service	-1.095* (.429)	-.136 (.579)	-.631 (.432)	1.156 (1.103)	-.008 (.315)
Unknown	-.845 (.450)	-.353 (.640)	-1.104* (.492)	2.144 (1.099)	-.126 (.345)
Public sector employee	-.108 (.163)	.110 (.178)	.807*** (.161)	.013 (.266)	-.130 (.129)
Family status (Ref.: Married)					
Never-married	-.017 (.171)	-.175 (.210)	.054 (.202)	-.148 (.254)	-.262 (.147)
Separated/divorced/ widowed	-.197 (.177)	.294 (.221)	.056 (.216)	-.371 (.256)	-.367* (.145)
Male	-.231 (.185)	.288 (.225)	-.081 (.233)	-.069 (.247)	-.606*** (.157)
Never-married x male	.094 (.217)	.063 (.273)	.123 (.263)	-.127 (.313)	.503** (.185)
Separated/divorced/ widowed x male	.485 (.278)	-.048 (.358)	.100 (.349)	-.135 (.393)	1.121*** (.224)
Tenure with previous employer	-.023 (.020)	.039 (.024)	-.348*** (.034)	.063** (.024)	.021 (.016)
(Tenure with previous employer) <sup>2</sup>	.001 (.001)	.001 (.001)	.008*** (.002)	-.000 (.001)	.000 (.001)
Education (Ref.: Inadequate and elementary)					
Middle vocational and Abitur with vocational	-.391** (.139)	-.218 (.205)	-.096 (.190)	-.462* (.192)	-.386*** (.117)
Higher vocational	-.658*** (.195)	-.203 (.271)	-.361 (.273)	-.842** (.269)	-.455** (.176)
Higher education	-.751*** (.184)	-.245 (.246)	-.272 (.240)	-1.028*** (.272)	-.629*** (.163)
Firm size (Ref.: < 20 employees)					
20 – 199 employees	-.160 (.097)	-.017 (.143)	.523*** (.129)	-.115 (.134)	-.155 (.085)
200 – 1999 employees	-.324* (.127)	.122 (.171)	.485** (.160)	-.471** (.179)	-.134 (.109)
2000 + employees	-.697*** (.151)	.252 (.168)	.596*** (.171)	-.553** (.196)	-.044 (.119)
Full-time work experience	-.003 (.024)	-.004 (.033)	-.050 (.029)	.040 (.035)	-.052** (.020)
(Full-time work experience) <sup>2</sup>	-.000 (.001)	-.000 (.001)	.000 (.001)	-.001 (.001)	.001 (.001)
Part-time work experience	-.037	.000	-.091*	-.053	-.030

	(.034)	(.045)	(.042)	(.044)	(.027)
(Part-time work experience) <sup>2</sup>	-.001	-.001	.001	.003	.000
	(.002)	(.003)	(.002)	(.002)	(.001)
Number of children in household (Ref.: None)					
One	-.093	.428*	.212	.114	-.001
	(.161)	(.198)	(.191)	(.219)	(.132)
Two	.045	.119	.270	-.101	-.048
	(.187)	(.258)	(.228)	(.269)	(.155)
Three or more	-.379	-.471	-.220	-.714	.454
	(.293)	(.465)	(.340)	(.543)	(.247)
One child in household x male	.103	-.383	-.267	-.206	.309
	(.222)	(.283)	(.276)	(.297)	(.187)
Two children in household x male	-.205	.155	-.283	.237	.552*
	(.260)	(.338)	(.334)	(.342)	(.218)
Three or more children in household x male	.481	.045	.198	.365	.294
	(.386)	(.625)	(.496)	(.649)	(.320)
Log(household income)	-.452***	-.111	-.585***	-.327*	-.217**
	(.084)	(.113)	(.113)	(.129)	(.074)
Poor health	.357*	.606***	.183	.065	.372***
	(.139)	(.162)	(.173)	(.186)	(.104)
Resides in West Germany	-1.067***	-.719***	-1.154***	-.730***	-.093
	(.103)	(.138)	(.124)	(.143)	(.086)
Year of job exit (Ref.: 2001)					
2002	.239	-.238	.248	.443*	.225
	(.157)	(.204)	(.202)	(.213)	(.134)
2003	.741***	.089	.575**	.871***	.368**
	(.161)	(.201)	(.202)	(.214)	(.136)
2004	.847***	.149	.806***	.851***	.322*
	(.171)	(.219)	(.202)	(.233)	(.141)
2005	1.035***	.370	.917***	.987***	.281
	(.174)	(.216)	(.218)	(.240)	(.144)
2006	.956***	.091	.838***	.672*	.107
	(.189)	(.254)	(.246)	(.261)	(.165)
2007	.633***	-.288	.283	.187	-.407*
	(.179)	(.242)	(.230)	(.266)	(.166)
2008	.301	-.142	.591**	.018	-.313*
	(.177)	(.215)	(.208)	(.258)	(.155)
2009	.201	-.287	.539**	-.130	-.083
	(.175)	(.227)	(.208)	(.264)	(.157)
2010	.766***	.138	.474	.812**	-.196
	(.186)	(.233)	(.244)	(.252)	(.164)
Intercept	1.395	2.069	6.073***	-3.229	1.072
	(1.386)	(1.882)	(1.666)	(2.208)	(1.130)
N (persons)			2,526		3,486
N (job exits)			3,307		4,614

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

**Table S2. Descriptive statistics for inverse probability weights**

	<i>M</i>	<i>sd</i>	Percentiles			
			<i>1st</i>	<i>25th</i>	<i>75th</i>	<i>99th</i>
Treatment weight (TW)	0.96	0.65	0.21	0.58	1.12	4.36
Censoring weight (CW)	1.00	0.23	0.76	0.85	1.08	1.86
TW x CW	0.95	0.70	0.21	0.54	1.12	3.93

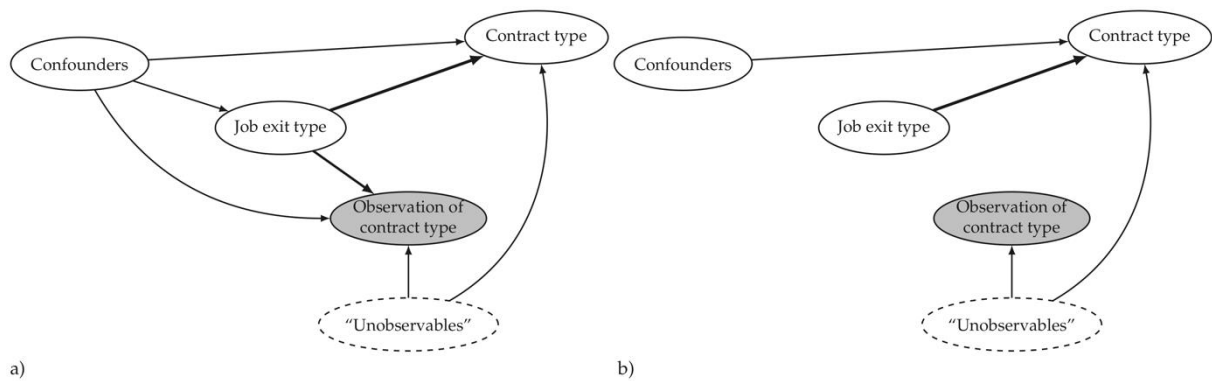
*Note.* Statistics pertain to uncensored sample.

**Table S3. Estimated Differences in Probability to be Uncensored and to Enter Temporary Employment by Type of Job Exit and Time Period (IPW Estimates)**

	2000–2006		2007–2013	
	Uncensored	Temporary Employment	Uncensored	Temporary Employment
Reference probability				
Individual layoff	.595 <sup>***</sup>	.344 <sup>***</sup>	.665 <sup>***</sup>	.442 <sup>***</sup>
	(.020)	(.025)	(.027)	(.037)
Risk differences				
(1) Resignation	.175 <sup>***</sup>	-.092 <sup>**</sup>	.209 <sup>***</sup>	-.172 <sup>***</sup>
	(.025)	(.031)	(.031)	(.043)
(2) Mutual termination	.092 <sup>*</sup>	.047	.140 <sup>**</sup>	.073
	(.037)	(.048)	(.045)	(.071)
(3) Contract expiry	-.006	.163 <sup>**</sup>	.102 <sup>*</sup>	.177 <sup>**</sup>
	(.039)	(.052)	(.045)	(.059)
(4) Workplace closure	.087 <sup>*</sup>	-.000	.079	-.019
	(.039)	(.051)	(.056)	(.082)
N (persons)	2,509	1,722	1,384	1,070
N (job exits)	3,016	2,067	1,598	1,240

*Note.* <sup>\*</sup>  $p < 0.05$ , <sup>\*\*</sup>  $p < 0.01$ , <sup>\*\*\*</sup>  $p < 0.001$ . Robust standard errors in parentheses.

Uncensored = observed with valid information on employment contract within three years after job exit.



**Figure S1.** Hypothesized causal relations between job exit type and contract type (in the first job after job exit) in population (a) and weighted pseudo-population (b)

In order to eliminate spurious association between job exit type and contract type we need to adjust for the common causes (i.e., confounders) of both variables as captured by the set of covariates included in our analyses, such as education, occupation, and sex. Additional spurious association may result from the fact that in order to analyze the effect of interest implicitly condition on having observed the contract type within three years after job exit (i.e., uncensored observations). This variable may be a collider on a path between job exit type and contract type, conditioning on which induces spurious association between the two variables.

Inverse probability of treatment and censoring weighting creates a pseudo-population in which job exit type is independent of the (measured) confounders and observing the contract type is independent of job exit type and the (measured) confounders. In this pseudo-population, the association between job exit type and contract type identifies the causal effect of interest under the assumption that there are no unmeasured common causes of job exit type and contract type (see Figure S1b).