



The impact of employment transitions on health in Germany. A difference-in-differences propensity score matching approach



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ABSTRACT

This article investigates the effects of transitions between employment and unemployment on health. It also addresses the question of whether or not the widespread use of temporary employment has altered the positive health effects of employment. Drawing on data from the German Socio-Economic Panel for the period 1995–2010, we apply difference-in-differences propensity score matching to identify the direct causal effects of unemployment and reemployment on psychological and physical health. This combination of two approaches towards causal inference controls for both unobserved fixed effects and observable differences in a flexible semi-parametric specification. Our sample includes persons between the ages of 16–54 who have at least experienced one respective employment transition (treatment groups) or are continuously employed or unemployed (control groups). The results show that only psychological but not physical health is causally affected by the respective employment transitions. Specifically, the effects of unemployment and reemployment are of similar size, highlighting the importance of reemployment in compensating unemployment's negative impact on psychological health. In contrast, health selection and confounding seem to be important determinants of the cross-sectional association between unemployment and physical health. Carrying out separate analyses for permanent and temporary workers, we shed new light on the health effects of temporary employment. It has been argued that the rise of temporary employment has introduced a new inequality in the world of work, blurring the line between employment and unemployment. However, contrary to our expectations we find that both employment transitions have effects of a similar size for permanent and temporary workers. In sum, our results highlight two points. First, longitudinal research is needed to properly evaluate the health effects of unemployment, reemployment, and temporary employment. Second, compared to temporary employment, unemployment is still the greater threat to individuals' psychological health.

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1. Introduction

Employment is of primary importance for meeting the socially defined needs of individuals in Western societies (Nordenmark and Strandh, 1999). It not only provides economic resources (Strandh, 2000) but also a major social role and identity (Jahoda, 1982). Consequently, unemployment is one of the most important threats to individuals' psychological and physical health. Job loss brings along economic deprivation (Gallie et al., 2003), disrupts valued identities and self-esteem, and reduces individuals' agency over their life (Strandh, 2000). Accordingly, many studies have

documented the negative health effects of unemployment (see McKee-Ryan et al., 2005; Paul and Moser, 2009 for meta-analyses; see Wanberg, 2012 for a review). However, there are fewer studies available on the health effects of reemployment (e.g., Kessler et al., 1989; Strully, 2009), although it may be considered one of the most important interventions in offsetting the negative effects of unemployment. Thus, the following study, first, contributes to the existing literature by examining both the psychological and physical health consequences of losing and finding a job. Instead of pooling transitions into and out of unemployment to estimate a single effect, we propose separate estimations in order to choose the appropriate control groups and allow for an asymmetry in the effects of unemployment and reemployment (Young, 2012).

A second contribution of this paper is that we will address the widespread use of temporary employment that is said to have led to increased work inequalities (Barbieri, 2009). This predominantly

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applies to countries such as Germany, where the core workforce is still sheltered by strong employment protection whereas the work periphery and labour market outsiders such as unemployed workers are increasingly confronted with insecure jobs. These changes in the organisation of work and the employment relationship (Benach et al., 2000) are said to have blurred the line between employment and unemployment and thus reduced the positive health effects of employment (Gash et al., 2007). A central research question is whether temporary jobs have integrative potential by improving the health of unemployed workers or whether they are just precarious jobs that are detrimental to one's health implying similar health risks as unemployment. In this respect this paper provides a detailed investigation of whether the impacts of unemployment and reemployment are moderated by the quality of work, focussing on the labour market segmentation between temporary and permanent work contracts.

Third, this paper makes a methodological contribution to the existing literature. Issues of health selection and other confounding factors will be addressed (Burgard et al., 2007). Whereas meta-analyses and most longitudinal research suggest that unemployment causally impairs and reemployment improves health (Burgard et al., 2007; Huber et al., 2011; Strully, 2009) some recent research has found that the association between unemployment and health is spurious (Böckerman and Ilmakunnas, 2009; Browning et al., 2006). Using longitudinal data from the German Socio-Economic Panel (SOEP) 1995–2010 we combine the strengths of two approaches toward causal inference (Heckman et al., 1997). Propensity score matching (PSM) tries to get rid of selection bias by conditioning on confounding variables and past health status using a flexible semi-parametric specification (Kim et al., 2008; Quesnel-Vallée et al., 2010). This is combined with a difference-in-differences (DID) estimator that removes unobserved fixed effects via within-person comparisons over time as well as common period and ageing effects by comparing the trends of a treatment and control group.

2. Theory and research hypotheses

Based on previous theoretical research Nordenmark and Strandh (1999) have identified two main rewards of employment: economic and psychosocial. Although welfare receipt and household income provide financial support to unemployed individuals, employment is still the primary source of income in Western societies (Strandh, 2000). In particular, the downsizing of welfare states in times of rising unemployment is thought to have increased the risk of economic hardship caused by job loss. It necessitates workers to adjust their living standards (Korpi, 2001) and use their savings to smooth consumption. In addition, the financial insecurity restricts individuals' control over their own situation and, thereby, their ability to plan for the future (Strandh, 2000). In the end, the financial worries accompanying unemployment may only be resolved by reemployment. Therefore, it is expected that employment transitions cause changes in both psychological and physical health.

For example, economic resources are the prerequisite of planning for the future; having financial troubles renders long-term decisions impossible and, thus, causes distress. The economic consequences of unemployment may also transfer into social and personal problems. For instance, the need to relocate may at the same time imply leaving behind family and friends. Unemployment may also decrease health-promoting behaviours (e.g., exercising) and increase health-damaging behaviours (e.g., smoking). Besides, physical symptoms may arise from psychological distress and in the long-run accumulate into illness. As changes in physical health need time to manifest, somewhat weaker effects may be expected

in the short-run. Reemployment seems to be important in resolving these issues and restoring individuals' health.

Aside from employment's manifest function, it is thought to bring along some latent benefits (Jahoda, 1982). For many it provides a major social role, which defines their social standing and affects their self-conception and social identity. In addition, employment provides social contacts which lend emotional and social support. These benefits seem particularly important for individuals' psychological health suggesting that physical health may be affected to a smaller extent. Furthermore, job loss permanently disrupts career paths. It not only deprives persons of income and wealth, but also of the power and prestige attached to their jobs, affecting their self-esteem.

Lastly, unemployment may stigmatise people as nonuseful and permanently reduce their confidence (Young, 2012). If the unemployed anticipate that future jobs will not be as rewarding as their last job, this may cause psychological problems and in the long-run even manifest into physical illnesses. Therefore, we expect that unemployment negatively affects psychological and physical health, whereas reemployment improves unemployed persons' health by restoring financial security and providing psychosocial benefits. This leads us to our first two hypotheses:

Hypothesis 1. On average, unemployment decreases individuals' psychological and physical health.

Hypothesis 2. On average, reemployment increases individuals' psychological and physical health.

However, as the rewards of employment vary with the quality of work (e.g., Kalleberg et al., 2000), we expect that permanent workers are harmed more by job loss than temporary workers. Respectively, finding permanent work should be more beneficial to psychological and physical health than finding temporary employment. In contrast to permanent work, temporary jobs offer lower wages, less prestige and power, limited autonomy, less pleasant working conditions, fewer career prospects, and higher job insecurity (e.g., Scherer, 2009).

While permanent workers may experience a greater need to adjust their living standards than temporary workers, because they have "farther to fall" (Berchick et al., 2012; p. 1893), the latter may face more difficulties cushioning the economic consequences of job loss through family income and savings. However, the restriction that financial insecurity puts on individuals' agency – compared to their previous situation – is still expected to be more harmful to permanent workers' health. While job insecurity is a characteristic of temporary work, permanent workers experience increased insecurity due to job loss (Strandh, 2000). Similarly, job loss is unforeseeable to most permanent workers, whereas fixed-term contracts are terminated on a specific date. Given this suddenness, losing a permanent job is expected to be more harmful to a worker's psychological and physical health. In return, these arguments suggest that taking up work via a fixed-term contract may not be sufficient in resolving the issues of unemployment and in restoring individuals' health.

Besides these economic characteristics, permanent and temporary work differs with respect to the psychosocial rewards of employment. Permanent work guarantees high job security and offers structured career ladders giving access to jobs of high prestige and power (Scherer, 2009). Such jobs may provide a major social role and identity as well as social contacts and support, boosting individuals' self-esteem and health. In contrast, temporary workers are often considered a "buffer workforce" who can be laid off in times of low demand (Booth et al., 2002). The low autonomy, the unpleasant working conditions, and limited career prospects temporary workers face are likely to result in an instrumental

attitude towards work. Moreover, the nature of fixed-term contracts makes it less likely that the job provides further social and personal resources. From this it follows, that losing one's job may be more harmful to permanent workers, who identify with their job. In fact, in the case of unbearable working conditions losing a temporary job could be a relief. In consequence, finding a temporary job may not resolve the insecurities of being unemployed and may not provide much psychosocial rewards. Although in the end any job may be better than unemployment (Hypothesis 2), the quality of work is decisive with respect to workers psychological and physical health. Accordingly, this leads us to the following hypotheses:

Hypothesis 3. On average, becoming unemployed is worse for the health of individuals in permanent work than those in temporary work.

Hypothesis 4. On average, finding temporary work is less beneficial to health than finding permanent work.

3. Research design

We draw on data from the German Socio-Economic Panel (SOEP) for the period 1995–2010. The SOEP is designed to be nationally representative of German households and surveys up to 20,000 persons each year. It offers yearly panel data on health and job characteristics as well as retrospective monthly data on activity status (Wagner et al., 2007). We supplement the yearly data with monthly calendar data to examine the effects of employment transitions on psychological and physical health. The key information comes from the yearly interviews. Person-spells ranging from a yearly interview in t to $t + 1$ enter the analyses if a transition from employment to unemployment (or the reverse) takes place or if continuous employment or unemployment is present. In order to focus on the dynamics between employment and unemployment person-spells are excluded if a person is in education, inactive, or has missing information in t or $t + 1$. Whereas the majority of previous studies have used yearly information alone to define employment transitions, we supplement the yearly data with information on a person's monthly activity status. In few cases inconsistencies between the yearly and monthly activity status have been overwritten by using the more reliable yearly information. Based on the monthly data we can define “proper” treatment and control groups:

For the treatment “becoming unemployed” (employed at t and unemployed at $t + 1$) we only use persons who report one single change from the status of employment to unemployment within the period $[t; t + 1]$ and no other kind of monthly activities such as inactivity or education. We compare those persons to the control group of persons who are continuously employed in each month of the period $[t; t + 1]$.

For the treatment “reemployment” (unemployed at t and employed at $t + 1$) we only use persons who report one single change from the status of unemployment to employment within the period $[t; t + 1]$ and no other kind of monthly activities such as inactivity or education. We compare those persons to the control group of persons who are continuously unemployed in each month of the period $[t; t + 1]$.

Thus, using the monthly data we can exclude problematic person-spells. For example, based on the yearly data alone, a person enters the first control group by being employed at t and $t + 1$, although this person may have experienced several months of unemployment in between. Obviously, such a person does not represent a good control case if the treatment itself is unemployment. Because the SOEP calendar allows for parallel monthly

activity statuses, a state space was defined with unemployment as the first priority, followed by employment, inactivity, and other activities. By using this state space the registered unemployed, who may legally hold marginal part-time jobs in Germany, are defined as unemployed. The analyses are restricted to individuals between the ages of 16–54 in order to fade out issues of the retirement process.

As described above our analyses are based on person-spells meaning that some persons may contribute with more than one person-spell. Overall, we observe 2594 transitions into unemployment (treatment “job loss”) (based on 578 persons) and 79,784 person-spells of continuous employment (respective control group) (based on 13,497 persons). In addition, we observe 2470 transitions out of unemployment (treatment “reemployment”) (based on 750 persons) and 4753 person-spells of continuous unemployment (respective control group) (based on 1025 persons). Figs. 1 and 2 illustrate some basic descriptive statistics of these person-spells. The median length of our observation window $[t; t + 1]$ is 13 months, but it may vary across individuals from 3 to 22 months. Figs. 1 and 2 also show that the median unemployment duration is 5 months in case of job loss, whereas the median employment duration is 10 months in case of reemployment. This difference in the exposure time to the respective treatments suggests that separate analyses are required.

In the analyses on whether or not the impacts of unemployment and reemployment are moderated by the quality of work, we focus on the labour market segmentation between temporary and permanent contracts. The information about the type of contract comes from the yearly interviews. In contrast to permanent employment that is defined by open-ended contracts, temporary employment involves contracts of a predefined limited duration. For example, it includes standard fixed-term contracts and – if not based on open-ended contracts – casual or seasonal work, temporary agency work, and work with a contract for a specific task. Following previous studies we define apprenticeships as education and not as temporary work (McGinnity et al., 2005).

We use two outcome variables to measure individuals' health at the yearly interviews in t and $t + 1$: *psychological health* is measured by a life satisfaction scale ranging from 0 (completely dissatisfied) to 10 (completely satisfied). *Physical health* is measured by a health satisfaction scale ranging from 0 (completely dissatisfied) to 10 (completely satisfied). The corresponding questions are: “How satisfied are you with your life, all things considered?” and “How satisfied are you with your health?”. Unfortunately, the valid and reliable SF-12 measures of physical and psychological health (Ware et al., 1996) are only available biannually since 2002. However, a supplementary analysis shows moderate to strong correlations (0.46 and 0.67 respectively) between our measures of life

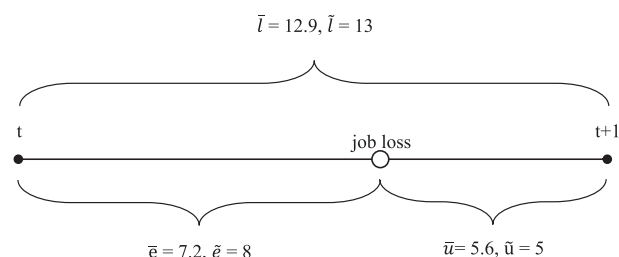


Fig. 1. Job loss and unemployment between t and $t + 1$. Notes: \bar{l} and \bar{l} describe the average and median lengths in months of the person-spells (computed over the treatment and control group). \bar{e} and \bar{e} and \bar{u} and \bar{u} represent the average and median lengths of months in employment and unemployment (computed over the treatment group).

Source: SOEP 1995–2010 (v28), own calculations.

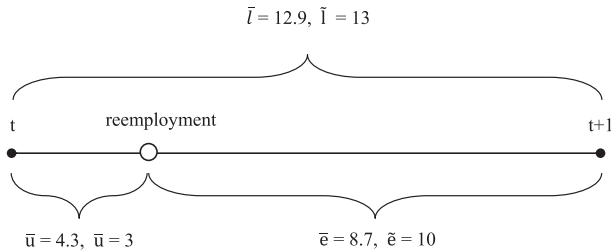


Fig. 2. Reemployment between t and $t + 1$. Notes: \bar{l} and \bar{l} describe the average and median lengths in months of the person-spells (computed over the treatment and control group). \bar{e} and \bar{e} and \bar{u} and \bar{u} represent the average and median lengths of months in employment and unemployment (computed over the treatment group). Source: SOEP 1995–2010 (v28), own calculations.

satisfaction and health satisfaction and the SF-12 measures of psychological and physical health. This also confirms the findings of Headey et al. (1993), illustrating that life satisfaction represents an important dimension of mental health. Besides, global life satisfaction scales have been shown to be valid, reliable, and sensitive to change such that they are well-suited for analyses on the effects of employment transitions on health (Diener et al., 2013). In addition, we find that our measure of health satisfaction is highly correlated (0.77 using the SOEP) with a widely used measure of subjective health (“How would you evaluate your present health?”, 1-very good to 5-bad”) that has been shown to be a good predictor of mortality (e.g., Idler and Benyamini, 1997).

We apply a difference-in-differences propensity score matching (DID-PSM) approach (Heckman et al., 1997). Drawing on the potential outcome framework we distinguish a treatment group ($D = 1$) who experiences the respective employment transition and a control group ($D = 0$) who does not. For both groups two potential outcomes at each time point are defined (Y^0, Y^1) but only one outcome is observed, whereas the other outcome remains an unobserved counterfactual. According to the difference-in-differences approach the effect of an employment transition is identified by comparing the change in health $E(Y_{t+1}^1 - Y_t^0 | D = 1)$ of the treatment group between period t and $t + 1$ to the counterfactual trend in health $E(Y_{t+1}^0 - Y_t^0 | D = 1)$ they would have experienced in the absence of the treatment. This counterfactual trend is approximated by the actual change in health $E(Y_{t+1}^0 - Y_t^0 | D = 0)$ of the control group according to the crucial “common trend assumption”

$$E(Y_{t+1}^0 - Y_t^0 | D = 1) = E(Y_{t+1}^0 - Y_t^0 | D = 0).$$

These within-person comparisons allow us to eliminate unobserved individual fixed effects. In contrast to a simple fixed-effects estimator, the between-comparison with the trend of a control group additionally removes any common period effects that affect the treatment and control group in identical ways as well as any ageing effects. For example, a simple fixed-effects estimator would underestimate the health effects of reemployment if the health of those continuously unemployed further declines over time. Whereas previous studies estimate a single effect for both transitions, we choose different control groups (see our description above).

In order to make the common trend assumption more plausible the respective control groups are based on the similarity in propensity scores in the DID-PSM design. The one-dimensional propensity score $P(D = 1|X)$ measures the probability of making the respective employment transition versus not, conditional on a high dimensional vector of control variables X . The propensity score estimation represents the first step of PSM and it coincides with a

logistic regression explaining the determinants of employment transitions. An additional common support condition guarantees that only persons with suitable control cases are considered. In the second step of PSM, algorithms form “statistical twins” that have similar propensity scores. All PSM analyses were performed using the Stata ado psmatch2 (Leuven and Sianesi, 2012). Comparing several algorithms we found rather consistent results, yet we decided for a 5-nearest neighbour (NN) matching (with replacement) because it showed the best balancing properties of control variables (Caliendo and Kopeinig, 2008). Although some persons may contribute with more than one person-spell this does not affect the composition of the 5-nearest neighbour control spells. With the exception of a very small share of treatment spells, the information for the respective treatment and control spells comes from different persons. The changes in the health outcomes of the treated and matched controls are then compared to estimate the average treatment effect on the treated (ATT), that is, the health effect of making the employment transition for those who actually experience the transition:

$$ATT^{DID-PSM} = \frac{1}{N_{D_1}} \sum_{i \in D_1 \cap S} \left[(Y_{i,t+1}^1 - Y_{i,t}^0) - \sum_{j \in D_0 \cap S} w_{ij} (Y_{j,t+1}^0 - Y_{j,t}^0) \right]$$

where D_1 (D_0) represents the treatment (control) group, w_{ij} the nearest neighbour matching weights, and S the area of common covariate support. PSM makes the standard DID assumption more plausible by forming statistical twin pairs before performing the DID estimator. Compared to the alternative of controlling linearly for the X variables in a DID regression, the semi-parametric DID-PSM, first, has the advantage that it is more flexible in avoiding misspecification errors. Second, PSM guarantees a more appropriate weighting of covariates. Third, linear regressions would extrapolate beyond the region of common support, making comparisons of non-comparable persons.

We use a rich set of control variables X that are expected to influence both changes in health and employment transitions. These variables are measured before the treatment in order to avoid endogeneity problems. Specifically, we control for previous cumulated work and unemployment experiences including terms for the squared durations. Moreover, we capture differences in human capital in terms of years of education attended, which is calculated based on information of the highest degree received (including apprenticeships and vocational degrees) and the standard duration of degree programs. We also account for socio-demographic variables such as gender, citizenship, a flexible cubic specification for real equalised disposable household income as well as dummy variables for the presence of a spouse, a partner and/or children in the household. We control for age in a quadratic specification in order to eliminate ageing effects in the difference-in-differences (DID) design. We also condition on the spell length as the length of the period between interviews in t and $t + 1$ differs between individuals. Period dummies (5-year intervals) and regional unemployment rate (calculated for each German federal state on a yearly basis) will proxy for overall time trends and local labour market conditions. Furthermore, we include a dummy variable for disability status (i.e., share of legally attested disability of 30% and more) and the pre-treatment level of physical health for analyses on psychological health and vice versa. A sensitivity analysis shows that excluding these related baseline health measures does not change our results substantially. We do not condition on the pre-treatment outcome itself, because Lechner (2010) states that this can induce a correlation with the treatment and, thus, violate the common trend assumption. Because in our

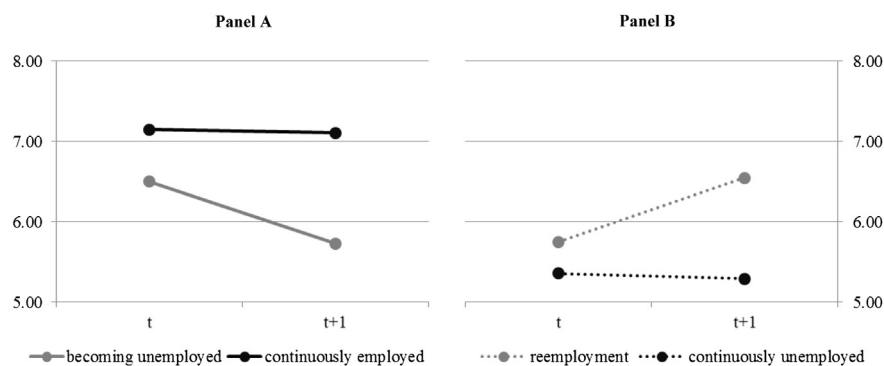


Fig. 3. Psychological health trajectories of treatment and control groups (unadjusted). Source: SOEP 1995–2010 (v28), own calculations.

application the common trend assumption is plausible without conditioning on the pre-treatment outcome (see sensitivity analyses below), we refrain from conditioning in order to avoid spurious treatment effects.

Our study design of choosing different appropriate control groups for each employment transition additionally reduces the sample heterogeneity making the conditional common trend assumption more plausible.

4. Empirical results

In order to assess the impact of employment transitions on individuals' health, descriptive analyses offer a good starting point. Figs. 3 and 4 display the changes in psychological and physical health among the respective treatment and control groups. Panel A (left-part) shows the health trajectories of workers who lose their job between t and $t+1$ and those who are continuously employed. Panel B (right-part) shows the respective health trajectories of unemployed individuals who find a job and those who are continuously unemployed.

Figs. 3 and 4 reveal that workers who lose their job between t and $t+1$ are already in worse psychological and physical health than those who are continuously employed (Panel A). For example, the baseline difference is about 0.7 points for psychological health and 0.4 points for physical health. Similarly, unemployed persons who find a job between t and $t+1$ have a 0.4 points better score for psychological health as well as a 0.8 points better score for physical health at the baseline compared to those who are continuously unemployed. These findings suggest that selection contributes to the cross-sectional association between unemployment and psychological and physical health and highlights the importance of a

longitudinal research design taking into account health selection and other confounding factors (Burgard et al., 2007). This is confirmed by comparing the physical and psychological health of the continuously employed and continuously unemployed across Panel A and B. The former group reports a 1.8 points better score for psychological health and a 1.1 points better score for physical health.

Examining the psychological health trajectories between t and $t+1$, we find relevant changes for the respective treatment groups and only small changes for the control groups. Considering the difference in change between these groups unemployment and reemployment seem to have a substantial effect on psychological health. However, Fig. 4 shows that the treatment and control groups experience almost no changes in physical health suggesting that it is not altered by the respective employment transitions. In order to confirm the descriptive results, in the next step, we take into account further characteristics that are expected to differ across the respective treatment and control groups. A DID-PSM approach allows us to not only to control for period and age effects, but also to balance the treatment and control groups with respect to the observed characteristics X making the common trend assumption more plausible.

Examining the balance before and after PSM (Table 1) illustrates that the unmatched treatment and control groups differ substantially in terms of important confounding factors. Workers becoming unemployed are overrepresented among youths, non-German citizens, unmarried persons, and persons with low household income compared to continuously employed workers. Reemployment occurs more often among young persons, males, and unemployed persons who live in more affluent households compared to the group of people who remain unemployed. Job losses occur more often in

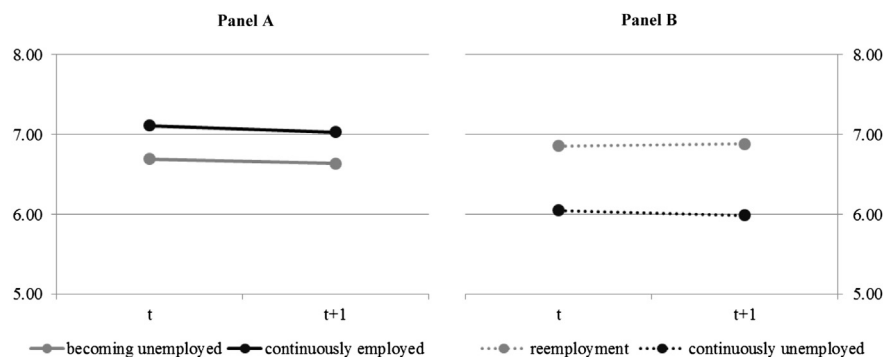


Fig. 4. Physical health trajectories of treatment and control groups (unadjusted). Source: SOEP 1995–2010 (v28), own calculations.

Table 1

Covariate balancing: mean differences before and after matching.

		Psychological health						Physical health					
		Transition into unemployment			Transition out of unemployment			Transition into unemployment			Transition out of unemployment		
		Treated	Control	% Bias	Treated	Control	% Bias	Treated	Control	% Bias	Treated	Control	% Bias
Spell length	Before	12.85	12.87	−1.3	13.07	12.80	16.1	12.85	12.87	−1.3	13.07	12.80	16.1
	After	12.85	12.85	0.1	13.06	13.04	1.3	12.85	12.84	0.5	13.06	13.01	2.7
Socio-demographics													
Age in years (linear)	Before	37.23	39.15	−21.1	35.95	39.86	−41.2	37.23	39.15	−21.3	35.95	39.86	−41.2
	After	37.23	37.32	−1.1	35.97	36.05	−0.8	37.22	37.28	−0.6	35.97	36.02	−0.6
Age in years (quadratic)	Before	1476.10	1606.00	−19.0	1385.50	1676.50	−40.9	1476.10	1606.00	−19.0	1385.50	1676.50	−40.9
	After	1476.60	1482.90	−0.9	1387.70	1395.80	−1.1	1476.00	1477.80	−0.3	1387.00	1395.10	−1.1
Female (Ref.: male)	Before	0.46	0.46	0.3	0.48	0.55	−15.1	0.46	0.46	0.3	0.48	0.55	−15.1
	After	0.46	0.47	−1.7	0.48	0.48	0.9	0.46	0.47	−2.5	0.48	0.47	1.1
German (Ref.: non-German)	Before	0.87	0.91	−13.9	0.89	0.87	5.6	0.87	0.91	−13.9	0.89	0.87	5.6
	After	0.87	0.86	2.1	0.89	0.87	4.0	0.87	0.87	−1.7	0.89	0.87	3.5
Partner in household (Ref.: no partner in household)													
Spouse in household	Before	0.54	0.65	−24.0	0.50	0.46	8.3	0.54	0.65	−24.0	0.50	0.46	8.3
	After	0.54	0.55	−2.8	0.50	0.50	0.1	0.54	0.55	−2.1	0.50	0.50	0.3
Life Partner in household	Before	0.15	0.13	5.8	0.17	0.15	3.4	0.15	0.13	5.8	0.17	0.15	3.4
	After	0.15	0.15	0.9	0.17	0.16	2.5	0.15	0.15	0.1	0.17	0.15	3.6
Number of children in household	Before	0.65	0.62	3.0	0.62	0.66	−4.1	0.65	0.62	3.0	0.62	0.66	−4.1
	After	0.65	0.64	0.3	0.62	0.62	0.7	0.65	0.64	0.5	0.62	0.62	0.4
Real equivalised disposable household income (linear)	Before	16,282	21,670	−52.6	14,654	11,942	41.2	16,282	21,670	−52.6	14,654	11,942	41.2
	After	16,292	16,496	−2.0	14,606	14,695	−1.4	16,291	26,435	−1.4	14,608	14,831	−3.4
Real equivalised disposable household income (quadratic)	Before	3.4*10 ⁸	6.1*10 ⁸	−6.7	2.6*10 ⁸	1.8*10 ⁸	25.3	3.4*10 ⁸	6.1*10 ⁸	−6.7	2.6*10 ⁸	1.8*10 ⁸	25.3
	After	3.4*10 ⁸	3.5*10 ⁸	−0.3	2.6*10 ⁸	2.7*10 ⁸	−1.8	3.4*10 ⁸	3.5*10 ⁸	−0.3	2.6*10 ⁸	2.7*10 ⁸	−2.4
Real equivalised disposable household income (cubic)	Before	1.1*10 ¹³	4.9*10 ¹³	−0.8	6.0*10 ¹²	3.8*10 ¹²	10.4	1.1*10 ¹³	4.9*10 ¹³	−0.8	6.0*10 ¹²	3.8*10 ¹²	10.4
	After	1.1*10 ¹³	1.2*10 ¹³	−0.0	5.9*10 ¹²	6.1*10 ¹²	−1.0	1.1*10 ¹³	1.3*10 ¹³	−0.0	5.9*10 ¹²	6.1*10 ¹²	−1.0
Context characteristics													
Regional unemployment rate in %	Before	13.29	11.47	38.0	13.74	14.32	−11.9	13.29	11.47	38.0	13.74	14.32	−11.9
	After	13.28	13.11	3.5	13.75	13.52	4.7	13.28	13.05	4.8	13.75	13.68	1.5
Period (Ref.: 1995–1999)													
2000–2004	Before	0.43	0.39	6.8	0.34	0.41	−13.7	0.43	0.39	6.8	0.34	0.41	−13.7
	After	0.43	0.41	2.8	0.34	0.32	4.6	0.43	0.43	0.4	0.34	0.33	2.5
2005–2010	Before	0.24	0.33	−19.5	0.30	0.36	−12.5	0.24	0.33	−19.5	0.30	0.36	−12.5
	After	0.24	0.26	−3.6	0.30	0.29	1.9	0.24	0.25	−2.7	0.30	0.30	0.9
Education and employment biography													
Education in years	Before	11.49	12.46	−39.9	11.60	10.88	34.1	11.49	12.46	−39.9	11.60	10.88	34.1
	After	11.49	11.55	−2.3	11.57	11.63	−2.9	11.49	11.52	−1.0	11.58	11.66	−3.9
Employment experience in years (linear)	Before	13.89	16.36	−27.1	12.08	13.50	−15.4	13.89	16.36	−27.1	12.08	13.50	−15.4
	After	13.90	13.84	0.7	12.11	12.06	0.5	13.89	13.80	1.0	12.10	12.07	0.3
Employment experience in years (quadratic)	Before	280.42	345.77	−21.3	230.52	268.28	−13.4	280.42	345.77	−21.3	230.52	268.28	−13.4
	After	280.63	278.82	0.8	231.41	229.72	0.6	280.42	276.10	1.4	231.01	230.78	0.1
Unemployment experience in years (linear)	Before	1.53	0.39	67.0	2.10	4.45	−75.6	1.53	0.39	67.0	2.10	4.45	−75.6
	After	1.53	1.60	−4.2	2.11	2.14	−1.1	1.53	1.60	−4.1	2.11	2.17	−1.9
Unemployment experience in years (quadratic)	Before	7.10	1.19	38.4	10.33	33.14	−54.8	7.10	1.19	38.4	10.33	33.14	−54.8
	After	7.09	7.52	−2.8	10.39	10.69	−0.7	7.07	7.61	−3.5	10.38	11.17	−1.9
Disability and health status													
Disability	Before	0.04	0.04	2.1	0.04	0.08	−17.5	0.04	0.04	2.1	0.04	0.08	−17.5
	After	0.04	0.04	0.4	0.04	0.05	−3.2	0.04	0.04	0.7	0.04	0.05	−2.7
Physical health	Before	6.69	7.11	−20.7	6.86	6.05	35.9	—	—	—	—	—	—
	After	6.69	6.66	1.7	6.85	6.85	−0.1	—	—	—	—	—	—
Psychological health	Before	—	—	—	—	—	—	6.50	7.15	−38.2	5.75	5.36	19.4
	After	—	—	—	—	—	—	6.50	6.45	2.9	5.75	5.78	−1.4

Notes: All covariates are measured before the treatment; % bias denotes the standardised percentage bias (Caliendo and Kopeinig, 2008). "Ref." = reference group.

Source: SOEP 1995–2010 (version v28), own calculations.

times of high regional unemployment while reemployment chances are lower. In addition, job loss more often affects low educated persons with less employment experience and persons who already experienced unemployment in the past. Specifically, compared to people in continuous employment, persons who lose a job have on average almost one year less of education, 2.5 years less of employment experience, and almost 1 year more of previous unemployment experience. Previous unemployment experience is much higher for the group of reemployed persons (2.1 years on average) and the highest among those who remain unemployed (4.5 years on

average). Groups also significantly differ in terms of their baseline health status. It is lower among persons who lose a job as compared to continuously employed workers. It is higher among those who become reemployed as compared to continuously unemployed workers. The comparison of the mean standardised bias before and after PSM shows that matching significantly reduces the mean standardised bias for each covariate far below the standard threshold of 5% (Caliendo and Kopeinig, 2008). Thus, the PSM succeeds in balancing the covariates. Imposing the common support condition only leads to the exclusion of a few treatment observations.

Consequently, we can expect that the common trend assumption is more plausible than before. Although there exists no way to determine if the assumption is true, its plausibility can be visually assessed as well as through so-called placebo-treatment tests (Heckman and Hotz, 1989). Plotting the conditional health trajectories of the treatment and control group for the pre-treatment periods $[t - 2; t - 1]$ and $[t - 1; t]$ reveals no substantial differences across the respective groups (graphs not shown). Giving placebo-treatments in the pre-treatment periods confirms this visual impression. The differences in the trends across the treatment and control groups are neither substantially important nor statistically significant at the conventional levels.

After the successful PSM, we can now assess the ATT in order to contrast the results with our hypotheses (Table 2). Considering the health effects of losing a job we find that, on average, psychological health is reduced by an amount of about 0.78 points on an eleven-point scale (standard error (S.E.) = 0.05; effect size (E.S.) = -0.45). Using the standardised mean difference as a measure of effect size (with the pooled pre-treatment standard deviation to scale the estimates; see Morris, 2008), this effect can be regarded to be of medium size. This finding is also in line with previous research (see the effect sizes reported in the meta-analysis of Paul and Moser, 2009).

In contrast, physical health remains unaltered by job loss (ATT = 0.01, S.E. = 0.05). Both results are in line with the descriptive analyses, though the effect of unemployment on psychological health is somewhat stronger using the DID-PSM. Consequently, our first research hypothesis can only be confirmed with respect to psychological health. In contrast, the results suggest that the cross-sectional association between unemployment and physical health is to some extent due to health selection and other confounding factors. However, to examine to what extent health selection may cause employment transitions another research design would be required.

The analyses on reemployment yield similar treatment effects regarding psychological and physical health. Finding a job, on average, increases psychological health by 0.96 points (S.E. = 0.08, E.S. = 0.48). Again, this is affirmative of the descriptive analyses. By contrast, becoming employed does not have any effect on physical health (ATT = 0.00, S.E. = 0.08). This result further supports the view that health selection and confounding contribute to the cross-sectional association between unemployment and physical health. Correspondingly, Hypothesis 2 is also only partly confirmed. In sum, the analyses reveal that psychological health is substantially affected by employment transitions (i.e., effect sizes of about 0.50), whereas at the same time we find no differences in the change in physical health between the respective treatment and control groups. However, given the theoretical discussion it is possible, that the average treatment effects hide significant effect heterogeneity, meaning that only particular groups of workers and unemployed persons are sensitive to the respective employment transitions. Although we do not find any unconditional treatment effects with

Table 3

Conditional average treatment effects on the treated (CATT).

	Psychological health			Physical health		
	ATT	S.E.	N_t/N_c	ATT	S.E.	N_t/N_c
<i>Transition into unemployment</i>						
Fixed-term contract at t	-0.72	0.11	750/ 2875	0.09	0.12	772/ 2875
Permanent contract at t	-0.81	0.06	1817/ 73,125	-0.05	0.06	1816/ 73,125
<i>Transition out of unemployment</i>						
Fixed-term contract at $t + 1$	0.96	0.09	990/ 4573	0.09	0.09	990/ 4573
Permanent contract at $t + 1$	1.05	0.09	1466/ 4573	0.02	0.09	1465/ 4573

Notes: Bootstrapped standard errors (S.E.) (200 repetitions). N_t = number of treated, N_c = number of controls. Numbers refer to person-spells.

Source: SOEP 1995–2010 (v28), own calculations.

respect to physical health, the subgroups of permanent and temporary workers may be affected to varying degrees.

In order to assess, whether or not temporary employment carries along similar health risks as unemployment, we additionally perform separate analyses by employment contract. For these subsamples we still achieve a satisfying balancing of covariates (the standardised median bias ranges from 0.4 to 4.8%). Once again, imposing the common support just leads to the exclusion of a few treated individuals. Table 3 displays the conditional average treatment effects (CATT) on the treated. Similar to the unconditional analyses we find that, in total, physical health is hardly affected by the respective employment transitions. Only unemployment has a slightly negative effect on the physical health of permanent workers. However, the effect is neither substantially important nor statistically significant. With respect to psychological health, we find substantial effects for both transitions and subgroups. In line with our third and fourth hypothesis, losing a job is less harmful to temporary workers (CATT = -0.72, S.E. = 0.11, E.S. = -0.42) than to permanent workers (CATT = -0.81, S.E. = 0.06, E.S. = -0.48), whereas finding a job is more beneficial to the latter. However, the differences are very small in size (0.09 for both employment transitions) and, therefore, should not be overemphasised. Thus, the results rather reject the expectation that temporary employment has blurred the line between employment and unemployment. On the contrary the results show that losing a temporary job matters to individuals' health and that reentering the labour market via a fixed-term contract can in the short-run resolve health issues associated with unemployment. In terms of health, temporary work seems to be closer to permanent work than unemployment, though the results do not necessarily generalise into the long-run.

5. Conclusions

This article sought to reveal the causal effects of unemployment and reemployment on individuals' health. In addition, we addressed the question of whether or not the widespread use of temporary employment has altered the positive health effects of employment by carrying out separate analyses for permanent and temporary workers. Using the SOEP (1995–2010), we analysed carefully constructed employment transitions (annual and monthly data) by combining the strength of two approaches towards causal inference: difference-in-differences and propensity score matching. Our analyses are, thus, robust to selection on observables and unobserved fixed characteristics.

The empirical results illustrate that employment transitions affect individuals' psychological but not physical health. With respect to psychological health, we find substantially significant

Table 2

Average treatment effects on the treated (ATT).

	Psychological health			Physical health		
	ATT	S.E.	N_t/N_c	ATT	S.E.	N_t/N_c
Transition into unemployment	-0.78	0.05	2592/ 79,784	0.01	0.05	2592/ 79,784
Transition out of unemployment	0.96	0.08	2457/ 4573	0.00	0.08	2459/ 4573

Notes: Bootstrapped standard errors (S.E.) (200 repetitions). N_t = number of treated, N_c = number of controls. Numbers refer to person-spells.

Source: SOEP 1995–2010 (v28), own calculations.

effects which are of similar size for unemployment and reemployment. These findings highlight the importance of reemployment as an intervention to improve unemployed individuals' psychological health. However, comparing the effects does not allow answering the question of whether or not unemployment scars health beyond reemployment (e.g., Young, 2012). In contrast, health selection and confounding seem to be important determinants of the cross-sectional association between unemployment and physical health, although some alternative explanations cannot be ruled out (e.g., physical health is only affected in the long-run). Nevertheless, our findings suggest that individuals' employment chances are affected by their physical health, implying that policy makers need to realise that promoting health is part of a successful activation policy.

The most interesting findings, however, relate to the question of whether or not the rise of temporary employment has altered the positive health effects of employment. Our results suggest that, on average, temporary work is associated with similar health benefits as permanent work and suggest that unemployment is the most important threat to individuals' health. Thus, fixed-term contracts are not only stepping stones supporting labour market integration they also bring along similar health benefits as permanent jobs. Using a longitudinal design to assess the health impact of temporary employment (e.g., Gash et al., 2007) allows for both a downward comparison to unemployment and an upward comparison to permanent employment. In contrast to cross-sectional analyses that compare the health of permanent and temporary workers (see Virtanen et al., 2005 for a review), this addresses the issues of health selection and confounding and provides a more complete picture facilitating an evaluation of temporary employment.

A limitation of our methodological approach is that health selection may still affect our results. Although our sensitivity test of the common trend assumption provides no indication of selection on long-term trends in health, selection based on short-term health shocks may still bias our results. However, the time between our pre-treatment measure and the treatment is rather short (median: about 8 months) and a sensitivity analysis that further reduces this time interval shows consistent results. Moreover, the German institutional setting of rigid employment protection and additional regulations on firing sick workers makes it difficult for employers to immediately lay off workers who suffer from a health shock. In addition, if sick employees are fired or voluntarily quit their job they are likely to become inactive as registering unemployed requires workers to be (physically and mentally) capable to work. As we cannot completely rule out health selection we refer the reader to more specific research on this issue (e.g., Chandola et al., 2003; Cardano et al., 2004).

Another methodological problem of our study may be measurement error and justification bias (Huber et al., 2011; pp. 489–490). Time-constant measurement error should be eliminated by our DID design. Moreover, in Germany there are no strong incentives to underreport health because working capability is a requirement for benefit receipt. The problem of measurement error could, however, be better solved using objective health measures and structural equation modelling (e.g., Chandola et al., 2003).

Future research would do well in examining the long-term health consequences of unemployment and temporary employment. Although we find that finding temporary and permanent work affects unemployed individuals' psychological health to a similar degree, it may still be that fixed-term workers are worse off in the long-run. Likewise, a more differentiated analysis of the effects of the duration of unemployment, particularly the effects of long-term unemployment, is required. Furthermore, different forms of non-standard work in general and temporary employment in particular (e.g., agency workers; see Quesnel-Vallée et al., 2010)

need to be considered to highlight potential heterogeneity among these work arrangements. While our global and subjective outcome measures are well-suited to reveal health inequalities, only specific measures can identify which aspects of health are particularly affected. Lastly, follow-up studies should address the question of whether or not unemployment negatively affects health even beyond reemployment. Although we have estimated the causal effects of both employment transitions, a comparison of the respective effects provides only tentative answers. Using a similar design and method, future research needs to follow individuals passing through the sequence of employment, unemployment, and reemployment. In line with this research, such a study would help to draw a more complete picture of the social costs of unemployment and temporary employment.

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