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Income Support, Employment Transitions and Well-Being

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Abstract

Using specific panel data of German welfare benefit recipients, we investigate the non-pecuniary life satisfaction effects of in-work benefits. Our empirical strategy combines difference-in-difference designs with synthetic control groups to analyse transitions of workers between unemployment, regular employment and employment accompanied by welfare receipt. Working makes people generally better off than being unemployed but employed welfare recipients do not reach the life satisfaction level of regular employees. This implies that welfare receipt entails non-compliance with the norm to make one's own living. Our findings allow us to draw cautious conclusions on employment subsidies paid as welfare benefits.

JEL Classification Codes: I31, I38, J60, J68

Keywords: life satisfaction, subsidized employment, unemployment, income support, in-work benefits, social identity, social norms

1. Introduction

Income increases people's consumption opportunities, irrespective of its source, and thus raises individual welfare. We question this implicit assumption made in economics and argue that the source of income does matter for individual well-being, at least for those employed workers who receive supplementary income support, i.e. in-work benefits. They may suffer from being dependent on public income support rather than being able to make their own living.

Recently, the debates about winners and losers from globalization, digitalization and other economic shocks such as the one following the Coronavirus outbreak have revitalized the interest in redistribution policies and in-work benefit programs. The main instruments of these policies cushion the consequences of job loss and help the unemployed in overcoming joblessness. Previous insights on the well-being effects of unemployment shed light on the potential benefits of such measures, but likewise point to its possible drawbacks. Being jobless reduces life satisfaction far beyond the misery caused by the income loss (see e.g. Winkelmann and Winkelmann 1998, Blanchflower and Oswald 2004, Knabe and Rätzel 2011). It worsens perceived social status, causes people to violate the social norm to work and leads to a deterioration in self-esteem, or identity utility (cf. Schöb 2013). Even a very generous passive labour market policy that compensated unemployed workers fully for the income loss would thus fail to restore the well-being of the unemployed.

Active labour market policy aimed at fostering reemployment therefore seems, at first glance, to be a more promising tool to alleviate unemployment-induced well-being losses. Employment subsidization schemes, such as the US 'Earned Income Tax Credit', the French 'La prime pour l'emploi', the British 'Working Tax Credit' and the German 'Ergänzendes Arbeitslosengeld II' (supplementary unemployment benefits II, UB II in the following) are prominent examples. They increase the income of low-paid workers to raise their labour supply while keeping firms' labour costs low and labour demand high (Saez 2002, Brewer et al. 2006, Chetty, Friedman, and Saez 2013). Additional jobs for unemployed workers, created in this way, *inter alia*, help them to restore their social identity as they allow them to comply with the norm to work. These policies should thus raise the life satisfaction of the beneficiaries. It is an unsettled question, however, whether such policy tools allow subsidized workers to fully recover from unemployment. This will only be the case if the subsidy does not affect subjective

well-being beyond its income effect. When workers' social identity not only depends on being employed, but also on adhering to the norm of making a living by one's own efforts (Elster 1989), which we term the non-dependency norm, an employment subsidy shaped as income support will not suffice to completely remove the misery of the unemployed. Workers who receive public income support may realize that they live off public assistance instead of making their own living. As a result, they may be less satisfied with their lives than non-subsidized (in the following also 'regularly') employed workers.

In this study, we assess the role of such a non-dependency norm for employees' well-being by analysing the well-being effects of subsidized employment in the form of in-work benefits compared to unemployment and regular employment. We examine the well-being of working UB II recipients in Germany, using data of the *Panel Arbeitsmarkt und soziale Sicherung* ('Panel Study Labour Market and Social Security', in the following: PASS). The panel structure of PASS and its special focus on welfare recipients allow us to follow workers out of subsidized employment and into it, entering or coming from either regular employment or unemployment, and to observe the within-person changes in life satisfaction accompanying these transitions. Our identification strategy addresses selection due to both time-invariant unobservable and time-variant observable characteristics by utilizing difference-in-differences designs and constructing synthetic control groups. Multiple regression analyses enable us to disentangle the monetary effect of transitions into or out of the receipt of in-work benefits from the respective non-monetary effect. In the process, we exploit the richness of our dataset to account for reasons leading people into and out of labour market states that might have own life satisfaction effects (e.g. changes in income and wealth, health and household size). As people are not assigned exogenously to the three labour market states 'unemployment', 'subsidized employment' and 'regular employment', we rely on the assumption that all reasons that simultaneously explain switching labour market states and changing life satisfaction are observable and considered.

We find that people become better off when they leave unemployment and start to work while continuing to receive welfare, and that this cannot be solely explained by the associated rise in available income. Employed people, however, who leave income support and become regularly employed also experience an increase in life satisfaction, even when controlling for income changes. As these people enjoy all the benefits of employment before and after the change, such as adhering to the work norm, this second finding points to a negative impact of receiving income support. This result is in line with the hypothesis that in-work benefits prevent the beneficiaries from complying with the non-dependency norm.

We proceed as follows: Section 2 reviews the previous literature on in-work benefits and relates our contribution to it. In Section 3, we derive hypotheses for our empirical analysis based on the notion that an individual's identity utility depends on social norms related to work and welfare dependency. Data and sampling are described in Section 4, while our empirical identification strategy is elaborated in Section 5. Section 6 presents our main results. In Section 7, we conduct consistency and robustness checks before we conclude with Section 8.

2. Previous research and contribution

Our analysis adds to the literature on the effects of in-work benefit programs. They are mostly analysed regarding their impact on the decision to provide work (Blundell 2000, Blundell and Hoynes 2004; see Van der Linden 2016 for an extensive account), and, more recently, with respect to their long-run effectiveness in reducing poverty (Bitler, Hoynes, and Kuka 2017, Hoynes and Patel 2018). Only very few studies also examined well-being effects. For instance, Gregg, Harkness, and Smith (2009) show that the introduction of a wide range of different programs of income support, including in-work benefits, improved the mental health of lone parents in the UK. Likewise, Boyd-Swan et al. (2016) find that an extension of the US Earned Income Tax Credit increases the mental health and subjective well-being of single mothers. In contrast to these studies, we analyse the *non-monetary* well-being effects of an in-work benefit program. Conducting a field experiment, this has also been done by Dorsett and Oswald (2014). They show that a combined program of cash transfers, job coaching and training, granted in addition to the existing British income support measures, deteriorated single mothers' non-pecuniary well-being in the long run. However, they do not disentangle the potential effect of the in-work benefit part from the impacts of the other measures, as well as from the interplay of measures.

We complement the literature by focusing on the entire population of a country's low-paid employees. As single parents may respond in a particular way, given their circumstances of living and the high opportunity cost of working, analyses of that group may not reveal the general well-being effect of receiving in-work benefits. Furthermore, we make use of especially well-suited observational data that allow us to come up with a comprehensive identification strategy to estimate the well-being effects of receiving the German supplementary UB II. In the process, we embed our analysis in the theory of social identity to explain non-monetary effects on worker well-being.

By looking at Germany, we examine a country that employs only one general income support measure for both the non-working and the working poor. Analysing local in-work benefit schemes preceding the introduction of supplementary UB II (e.g. the 'Mainz Model'), Krug (2009) shows that welfare recipients are less satisfied with their wages than non-recipients. This result might already point to non-monetary utility losses from receiving income support, although a broader measure of subjective well-being would certainly be more revealing in this respect. A caveat of Krug's (2009) results emerges as the receipt of the subsidies under investigation may depend on many unobserved individual characteristics reflecting the selection of participants, such as personality traits affecting the general motivation to work and the general perception of a given amount of income as fair. Our panel data allow us to resolve these issues by estimating changes of life satisfaction within the same person, i.e. at given time-stable traits and attitudes.

Similarly, methodological enhancements distinguish our study from that of Chadi (2012). He provides first descriptive evidence consistent with the notion that receiving supplementary UB II reduces life satisfaction. However, his study compares the well-being of people on regular jobs to a variety of subsidized jobs, including marginal and unsteady employment, which differ from regular jobs in many respects. In contrast, we exclude any of these kinds of minor employment modes and use synthetic control group designs to separately compare subsidized workers to regular employees and unemployed workers in comparable circumstances of (working) life. Finally, we consider much more information on individual's financial situation (wages, savings) as well as changes in life accompanying the start or termination of in-work benefit receipt (e.g. job mobility, being single). Overall, our study exploits a much richer data set that allows us to account for many more potential issues of selection. We are thus much better able to identify the non-monetary effects of in-work benefits on life satisfaction and provide novel reasons for such effects.

3. Hypotheses

Losing work influences subjective well-being in different ways. It has a strong impact on life satisfaction. Thereby, it affects the cognitive evaluation of one's whole life, but has hardly any impact on affective well-being in the course of a day (Knabe et al. 2010). The theory of social identity offers a suitable concept to explain such effects on cognitive evaluation of one's life (see e.g., Tajfel and Turner 1986). People form an ideal self-concept that is partly individualistic but also depends on the way they consider themselves part of a shared social category. This

shared social identity emerges from cognitive criteria, such as shared fate, values, situations, or attributes (see Turner and Reynolds 2010, p. 20). Belonging to a social group requires the fulfilment of shared norms. Identity utility is reduced if one does not adhere to these shared norms.

The idea that involuntary unemployment leads to well-being losses through the violation of social norms is well-established in the literature. People who have finished their education and are below retirement age may consider themselves as belonging to a group with a shared social identity as 'working-age people', which comes along with a specific set of social norms. A major norm for them is the social norm to work. It obligates a working-age person to work, i.e. social identity constitutes the expectation to contribute to societal welfare by one's own work (Clark 2003, Shields and Wheatley Price 2005, Powdthavee 2007, Shields, Wheatley Price, and Wooden 2009). Retirement, for instance, increases unemployed workers' life satisfaction, presumably because it allows them to leave the social category 'working age', whose norm to work they violate, and enter the social category 'retirement age', which does not prescribe being employed (Hetschko, Knabe, and Schöb 2014).

Social identity thus affects 'identity utility' (Akerlof and Kranton 2000), but it is important to note that the abstract notion of a loss in identity utility may materialize in many different ways (Schöb 2013). Unemployed people may lose self-esteem not meeting their ideal selves. Seeking unsuccessfully for a job might make them feel less in control of their own lives as they cannot escape the deviation from their ideal self. Actions of others might also contribute to losses in identity utility in the form of stigmatization. Accordingly, we formulate

$$\text{Hypothesis 1:} \quad LS(Unemployed, \bullet) < LS(Subsidized\ employed, \bullet),$$

where *LS* denotes life satisfaction.

Besides the work norm, compliance with the norm to make a living by one's own efforts may also be an important means to generate identity utility while identifying with the group of working-age people (cf. Stutzer and Lalive 2004, Chadi 2014). In line with this notion, stigmatization, by oneself or by others, has in particular been discussed as a source of disutility from the fact that one receives income from the welfare state (Moffitt 1983, Besley and Coate 1992, Stuber and Schlesinger 2006, Kassenboehmer and Haisken-DeNew 2009). Indeed, this so-called 'welfare stigma' is often considered as a major reason why many people do not apply for welfare although they are eligible (Riphahn 2001, Whelan 2010, Bruckmeier et al. 2013). Non-take-up behaviour, however, might as well originate from lacking knowledge about one's

eligibility for welfare. In these cases, being dependent on welfare itself does not need to reduce utility. It is therefore worthwhile to analyse life satisfaction in order to directly identify the effect of welfare receipt on workers' well-being.

We conjecture that becoming regularly employed restores job seekers' adherence to both the work norm and the non-dependency norm. In contrast, becoming reemployed while receiving in-work benefits conditional on household neediness ensures adherence to the work norm only. Employed workers, subsidized in this way, are still welfare-dependent. This could reduce identity utility, and hence life satisfaction, due to the deviation from the non-dependency norm:

$$\text{Hypothesis 2: } LS(Subsidized\ employed, \bullet) < LS(Regularly\ employed, \bullet).$$

4. Institutional background, data and samples

The *Sozialgesetzbuch II* (Social Code II) regulates income support in Germany. Welfare benefits are granted at the household level (*Bedarfsgemeinschaft*) if the household is unable to generate a well-defined socio-economic subsistence level of income and cannot rely on savings.[1] Up to that level, 'unemployment benefits II' (*Arbeitslosengeld II*, UB II) supplements the household's income. The specific amount of welfare benefits depends on the earnings of the household members, the numbers of adults and children living in the household and regional costs for housing and heating. As a result, even workers with the same earnings may be entitled to UB II or not, dependent on the other criteria. Employees who live in households with low income are thus eligible to receive supplementary UB II. As long as the monthly gross labour income is less than 100 euros, UB II entitlement is not reduced at all. However, each euro of additionally earned labour income increases the disposable household income by only 20 cents up to a threshold of 1,000 euros per month. The remaining 80 cents are used to pay for social security contributions and to reduce UB II transfers. From 1,000 to 1,200 euros, the disposable household income increases by only 10 cents per euro earned labour income (1,500 euros if the worker has dependent children). Beyond that, any additional net labour income is completely used to reduce the remaining UB II transfers. As a result of this income support scheme, people

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[1] In 2018, the final year of our investigation period, the level of normal requirements changes on a yearly basis and was 416 euros monthly for the first adult in the household and 374 euros for the spouse. Children younger than 6 years give rise to an entitlement of 240 euros, 6-13 year old children 296 euros, 14-17 year old children 316 euros, and 18-25 year old dependent adults 332 euros. Accommodation and heating are paid separately and are set at the city/county level. Household receiving UB II are not allowed to have savings worth more than 9,750-10,050 euros per adult (dependent on the year of birth) and 3,100 euros per child.

can receive welfare benefits even though their total net income exceeds the socio-economic subsistence level. This is intended to encourage workers to accept low-paid jobs.

We make use of PASS data covering about 15,000 individuals living in 10,000 households in Germany who have been surveyed annually since the turn of the year from 2006 to 2007. The panel structure enables us to exploit within-person variation. For the first wave, two similarly large samples of households were drawn ('dual sampling'). The first part is representative for the German population whereas the second part was drawn from register data of households that receive UB II. Both samples are regularly refreshed, such that the respective numbers of surveyed households remain similar over time. As a result, PASS surveys considerably more in-work benefit recipients per year than comparable household surveys such as the German Socio-Economic Panel (SOEP). The two parts of the survey do not vary regarding the information included. The data cover subjective well-being, employment biographies and other relevant characteristics. Furthermore, PASS contains many UB II specific questions (see Trappmann et al. 2010, 2013). We utilize all annual waves starting from the second wave onwards (2007/08, 2008/09, 2010-2018). Due to fundamental changes in the questionnaire design, we do not make use of the first PASS wave (2006/07). In doing so, we also substantially reduce the potential problem that people tend to report higher life satisfaction when surveyed for the first time (e.g. Frijters und Beatton 2012).[2]

We distinguish three distinct individual labour market states: regularly employed, subsidized employed and unemployed. For the purpose of our analysis, we define a person as employed if she reports any employment spell (including self-employment) at the time of the interview. Besides this information, we condition being employed on a working time from 15 hours to a maximum of 80 hours a week. Employees who do not receive income support are considered regularly employed, while employees who live in a household receiving UB II are considered subsidized employed. Unemployed workers are not employed, are registered as unemployed and do not report any working hours at the time of the interview. We restrict our samples to persons of working age (25-59 years) and exclude pupils, students, workers on parental leave, (early) retirees and public servants. Given these restrictions, the PASS waves we use include 43,635 observations of regularly employed workers, 4,508 observations of employees receiving income support and 27,125 observations of unemployed people.

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[2] We also checked if controls for time-in-panel and sample origin (the two initial samples, refreshments) affect our results, which they do not (see Supplementary material, table S2).

Subjective well-being is measured using a general question on people's life satisfaction, '*In general, how satisfied are you currently with your life on the whole?*', which respondents answer on an eleven-point scale from '0 = very dissatisfied' to '10 = very satisfied'. In addition, we make use of data on the inflation-adjusted disposable equalized monthly household income, which approximates individual consumption opportunities by accounting for the number of household members and economies of scale in housing. People indicate the exact amount of household income in euros. Following the OECD equivalence scale, household income is then divided by a weighted number of the persons living in the same household. While the first person gets a weight of 1, any additional person older than 14 years gets a weight of 0.5, children up to the age of 14 years get a weight of 0.3. As proxies for wealth, we employ categories of the stock of household savings (from savings accounts, shares, building society deposits, and life insurances, but not real estate): less than 1,000 euros, 1,000-2,500, 2,500-5,000, 5,000-10,000, 10,000-20,000, 20,000-50,000, 50,000+.

Socio-demographic characteristics are gender, age, number of adults in household, numbers of children inside and outside the household, giving care to friends or relatives, marital status, migration background, years of schooling, living in former East or West Germany, and the overall number of distinct unemployment episodes. For unemployed workers, we distinguish between first-tier (unemployment insurance benefits) and second-tier benefits (means-tested UB II).[3] Data on social relations outside the household are included as well (*'How many close friends or family members, with whom you have a close relationship, do you have outside your household?'*). Current health status is represented by being registered as disabled or not, filing for disability or not (as current disability shock), the number of visits to a doctor within the last three months and the report of zero/one or more hospital stays within the last twelve months.

For employed individuals, we include information about job characteristics, such as gross labour earnings as a proxy for individual productivity, actual weekly working hours, tenure, blue collar versus white collar, self-employed versus fulltime paid job versus part-time paid job, fixed-term contract versus permanent contract. Work strain is considered in a novel way by merging a rich work strain index (*Arbeitsbelastungsindex*, see Kroll 2011) with our data. The scale is generated from 39 items of a job questionnaire and aggregates ergonomic burden,

[3] When workers become unemployed, they receive unemployment insurance benefits (Arbeitslosengeld I, UB I in the following), which amounts to 60% of the former net labor income (67% in exceptional cases). As long as UB I is not as high as the socio-economic subsistence level, workers can receive supplementary UB II. After a certain period of time, the entitlement for UB I expires (between 6 and 24 months, depending on the age of the recipient and the time the person has contributed to the insurance). Henceforth, unemployed workers are only eligible for means-tested UB II.

psychological strain, social strain, environmental burden and temporal burden of the current occupation. Based on the International Standard Classification of Occupations (ISCO-88), we assign the resulting work strain value ('1 = lowest strain level' to '10 = highest strain level') to all employees in the sample.[4] This detailed information allows us to capture occupation-specific job characteristics on a very detailed level. For instance, the strain from being a waiter or barkeeper (ISCO-88 code 5123) is '9', whereas the work strain for a restaurant manager (ISCO-88 code 1315) is '8'. By doing so, we also capture occupation-specific job insecurity and job-related health risks. To shed light on the mechanisms for well-being effects we find, we make use of data on the number of workers' recent personal contacts to the 'Jobcenter'[5] in charge as well as on employees' views of how important it is to make a contribution to society through work (not important at all / less important / quite important / very important).

5. Empirical strategy

Ideally, we would rely on purely exogenous variation in labour market status to identify life satisfaction effects of subsidized employment compared to unemployment and regular employment. However, this is not feasible based on the data at hand. We therefore address endogeneity by combining difference-in-differences (DiD) designs with matching. The DiD design tackles potential selection-into-treatment issues (here: selection into transitions between labour market states) originating from unobserved heterogeneity such as individual ability (Subsection 5.1). Matching addresses observable heterogeneity, originating for instance from age (Subsection 5.2). Finally (Subsection 5.3), based on the matched sample, we perform regressions of the DiD that control for changes in life to disentangle the well-being effects of being subsidized employed from the well-being effects of the reasons for being subsidized employed (e.g. changes in income, wealth, household size, health). Our identifying assumption is, hence, that unconsidered changes in life with influence on life satisfaction do not concern the control group differently from the transition group. That is, conditional on the variables taken into account, the control groups must display the trend in life satisfaction the transition groups would experience if they did not change labour market states ('parallel trend assumption'). We examine the plausibility of this assumption by several sensitivity analyses.

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[4] 81% of merged work strain scores are ISCO-88 4-digit level, for 16% of occupations we have information on a 3-digit level and 3% of our work strain information were merged on a 2-digit ISCO-88 code level.
[5] 'Jobcenter' is the official *German* term. This local merger of social security office and employment agency administers UB II for both employed and unemployed people. In addition, it is supposed to help and incentivize them to overcome welfare dependency.

For instance, we document that our transition and control groups follow common pre-trends. Moreover, we conduct an additional analysis on one-adult households only (one-person households or single-parents) where the number of children does not change. This ensures that most influential events in family life (e.g. birth of a child, spousal unemployment) cannot simultaneously explain a change in well-being and switching between labour market states.

5.1 Difference-in-differences design

We conduct four separate difference-in-differences approaches (DiD). Each DiD approach examines within-worker variations in life satisfaction (*LS*) accompanying the transitions from subsidized employment to either unemployment or regular employment and *vice versa*. By doing so, we rule out that time-invariant unobserved heterogeneity, which simultaneously affects people's labour market status and well-being, confounds the genuine life satisfaction effects of the labour market states. In addition, this strategy allows us to examine whether the respective direction of the transition matters, by analysing transitions separately instead of applying a framework that combines all transitions in one approach, such as an individual-fixed effects estimation. Hence, it is also tested whether, for instance, transitions from unemployment into subsidized employment and out of subsidized employment into unemployment yield qualitatively and quantitatively corresponding well-being changes, or not.

Considering transitions separately also allows us to assign transition-specific synthetic control groups to each transition group. In doing so, we take into account that, for instance, regular employees and unemployed workers who become subsidized employees differ from each other in many respects. This is also the main reason why we deliberately did not choose a combined fixed-effects panel data estimation of the effects of all four changes between labour market states (which is, however, presented as a robustness check). Such an approach would rely on the assumption of comparability of one control group to four different transition groups of status switchers. We would be limited in improving the comparability of control group and transition groups and it would not be possible to account for differences in job characteristics between people who transition from subsidized to regular employment and people who remain in the initial labour market status. By definition, these characteristics are missing for unemployed workers and should thus not be controlled for in a fixed-effects regression including all of the three labour market states. Since we do not have a joint pre-transition point in time for all transitions, it is also not possible to account for job characteristics by matching transition and control groups according to their job characteristics at that point in time.

We group our DiD approaches according to the hypotheses to be examined (see Section 2). To begin with, we focus on transitions from unemployment to subsidized employment (test I.1), or *vice versa* (test I.2). Subsequently, we track life satisfaction changes of regularly employed individuals who change to subsidized employment (test II.1) and subsidized employed persons switching to regular employment (test II.2). The transitions always take place between two PASS interviews, which generally encompass a period of approximately one year. The respective control groups always stay in the initial labour market status. They might switch jobs between the two PASS interviews (for any reason) but end up in the same status again.[6] Including the control groups is necessary to disentangle the change in *LS* from counterfactual changes in *LS* (e.g. time trends). The DiD is thus the difference in the changes in *LS* from one PASS interview to the next between the respective transition and control groups. Figure 1 summarizes the four DiD approaches and assigns the numbers of observations. As a consistency check, we also examine transitions between unemployment and regular employment.

An important requirement for estimating unbiased effects is that people do not anticipate switching labour market states at the pre-transition reference point in time. This assumption might in particular be violated when it comes to transitions into unemployment, as employers have to observe periods of notice in Germany which depend on a worker's tenure. For 81% of the transition group of workers who enter unemployment (test I.2), this period covers three months or less, unless the employer has agreed on a longer period. Hence, unemployed workers could know that they will lose work before the actual event, which is why their satisfaction may already reduce, as we expect a negative effect of unemployment in line with previous studies (Section 2). To tackle anticipation bias, we exclude observations of people who become unemployed if their transition takes place three months or less after the pre-transition PASS interview. For the reason of consistency, we do the same for the reverse transition, i.e. from unemployment to subsidized employment. In fact, expecting reemployment in the near future also improves well-being in advance (Knabe and Rätzel 2010). Table S3 in the Supplementary material shows that this restriction affects only people who are about to lose work. The effect of the transition seems to increase with the time that passes by between the last PASS interview when employed and the entry into unemployment.

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[6] In tests II.1 and II.2, we control for having switched jobs from the previous PASS interview to the current interview. As we show later on (Section 7.3, table A7 in the Appendix), it does not matter for our results if we include people who change jobs in the control groups or the transition groups of tests II.1 and II.2.

Figure 1: The different DiD designs

Test I

Pre-transition employment status

unemployment   subsidized employment   regular employment

I.1 treat (*n* = 411)
I.2 treat (*n* = 188)
II.2 treat (*n* = 681)
II.1 treat (*n* = 345)

I.1 control (*n* = 12,103)
I.2 control (*n* = 1,501)
II.2 control (*n* = 1,482)
II.1 control (*n* = 22,767)

Post-transition employment status

unemployment   subsidized employment   regular employment

Test II
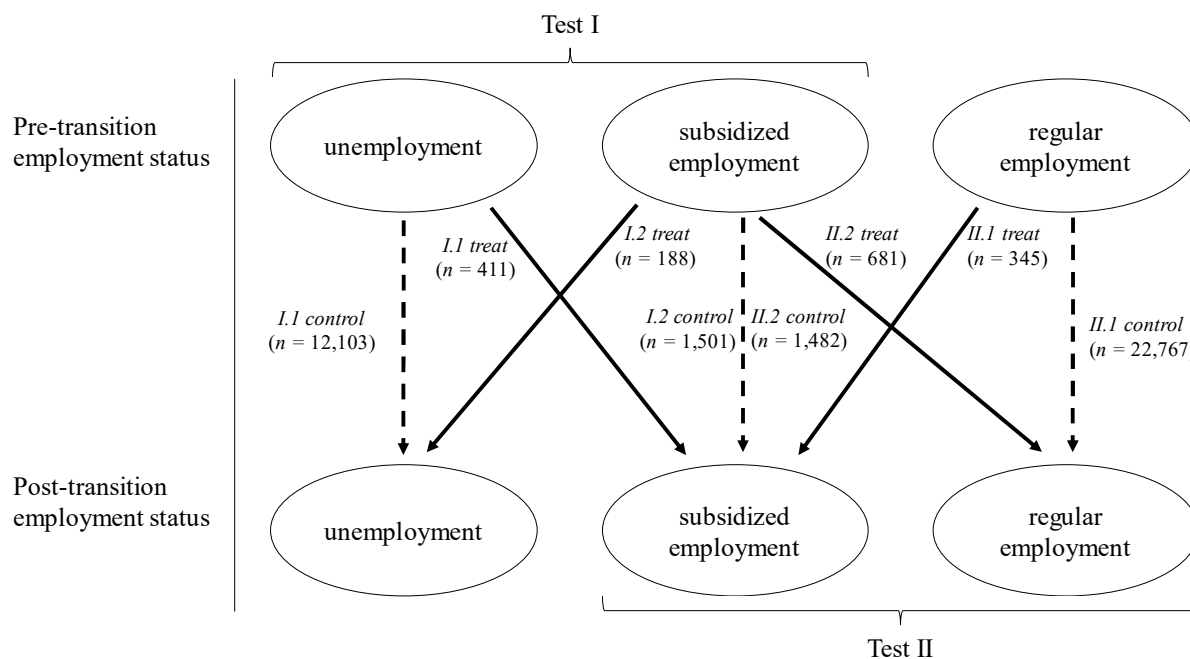
Note. The figure depicts the different transition and control groups and their respective numbers of observations. The sizes of control groups I.2 and II.2 (people who remain subsidized employed) slightly vary since the different sets of control variables cause missing values to a different extent. For our consistency checks on direct transitions between unemployment and regular employment, we rely on 589 transitions from regular employment to unemployment (control group of 22,696 regularly employed people) and on 1,032 observations of unemployed people who become regularly employed (control group of 12,024 unemployed people).

5.2 Matching

As mentioned above, the parallel trend assumption requires the control groups to experience the trend in life satisfaction the transition groups would show if they were not switching labour market states. The probability of switching from one labour market status to another between two PASS interviews may vary with observable characteristics such as age, marital status, income or stock of savings. To meet the parallel trend assumption, we thus use fitted sets of observable variables to equalize the propensities to switch labour market states between transition and control groups for each test before the transition, as explained in detail in this subsection. Since changes in life accompanying the transition may also confound the well-being effects of switching between labour market states, we control for these changes, as explained in the next subsection.

Considering transitions separately in the DiD design is a first step to making the respective transition and control groups more comparable, since they each start from the same labour market status. To further increase similarity, we exploit the richness of the PASS data set and take an extensive number of characteristics into account in order to reweight the observations

of the transition-specific control groups such that, ideally, they face the same *ex ante* propensity to transition as the transition group. Meeting UB II eligibility criteria affects this propensity. We therefore condition on equivalent income, savings, marital status and, separately, for the number of adults and children in a household. The propensities to switch between subsidized employment, unemployment and regular employment may also depend on gender, age, children living in a different household, having at least one child of preschool age, educational attainment, migration background, social contacts, region (East vs. West Germany), previous unemployment experience (number of episodes), health status (disability status, overnight stays in hospital, visits to doctor), and being a caregiver, which are thus considered as well. For people starting the transition from either subsidized or regular employment, we can also account for job characteristics that may differ regarding the probability of prospective transitions (differences in earnings, work strain, working hours and tenure, blue-collar, white-collar, or self-employment, part-time job and temporary contract).[7]

The reweighting technique entropy balancing (EB) is used to generate individual weights for all observations of the control groups such that the statistical moments of the given sets of observable characteristics, and thus ideally the propensity of switching labour market states, equalize between the transition and the control group (cf. Hainmueller 2012). For this purpose, a loss function minimizes the entropy distance of control group individuals' base weights (1/N) and EB weights upon the condition that the set of control group covariate moments are as similar as possible to the transition group moments.[8] The data allow us to balance on the first and second moment for tests I.1, II.1 and II.2 as well as on the first moment for test I.2. For test I.2, the high number of balancing variables in combination with a rather low number of observations prevents the balancing algorithm for the second moment from converging. Cardinal and nominal covariates are balanced on mean and variance, whereas categorical variables need only be balanced on the first moment.

In contrast to propensity score based matching methods, this condition guarantees high matching quality as the covariate distributions of the transition and control groups will definitely equalize. EB thus also obviates the complex and somewhat arbitrary process of choosing covariates in order to achieve balanced covariate distributions. The set of covariates originates from theoretical considerations only. In addition, EB does not need to rely on the

[7] There are different views about matching on pre-treatment outcomes (O'Neill et al. 2016, Chabé-Ferret 2017). We do not condition on pre-transition satisfaction, as doing so may bias the transition effects, and as not doing so is the more modest approach given our results, as shown by table S2 in the Supplementary material.
[8] We have implemented EB using the Stata package ebalance written by Hainmueller and Xu (2013).

assumptions of propensity score estimations.[9] Although we prefer EB matching for our analysis, we obtain the same findings using propensity score reweighting (see Supplementary material, table S2). Tables A1-A4 in the Appendix describe the *LS* as well as conditioning variables of the transition and control groups of each DiD approach before and after reweighting. These tables document that, before reweighting, the transition and control groups differ in many respects. In particular, people who switch between different states of employment differ from the respective controls regarding labour earnings, tenure and hours of work. EB eliminates the observed differences between transition and control groups.

Based on the reweighted sample, we analyse if transition and control groups follow parallel trends up to the pre-transition point in time. We therefore extend the investigation period to the third-last (named '$t = -3$') and the second-last pre-transition year ($t = -2$). Figures A1.I and A1.II in the Appendix show that all of the control groups follow the similar trends in life satisfaction as the transition groups until the pre-transition PASS interview ($t = -1$). Note that the groups are smaller than the samples underlying our main analysis, since there is attrition from $t = -3$ to $t = -1$.[10] Moreover, household size and equivalent income evolve similarly between the respective transition and control groups. Accordingly, placebo tests for the changes in life satisfaction, income and household size from the third-last ($t = -3$) to the second-last PASS interview ($t = -2$) before the transition group switches labour market states as well as from the second-last to the last PASS interview suggest no effects of the respective transition that takes place after $t = -1$ (Appendix, table A5.1-A5.4). Only in the case of the transition from regular employment to subsidized employment are the changes in income and household size from $t = -2$ to $t = -1$ statistically significantly different between transition group and control group. But the parallel change in life satisfaction is negligible. We conclude that, as far as it is possible to shed light on the parallel trend assumption, we largely find no evidence for its violation. Still, we should keep in mind that the results of test II.1 need to be interpreted more cautiously than the findings from all other tests.

5.3 Regression analyses

Many reasons that explain why people are in labour market states are already covered by DiD design and matching (e.g. health). However, since the change of labour market states takes

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[9] These reasons explain why EB is increasingly popular. For previous applications see, for instance, Marcus (2013, 2014), Freier, Schumann and Siedler (2015), Neuenkirch and Tillmann (2016).

[10] This restricts the validity of the descriptive analysis of pre-trends, as the groups underlying figure A1 do not necessarily represent the samples underlying our main results. For instance, attrition causes that we do not see an effect of the transition from a subsidized job to unemployment on life satisfaction in figure A1 anymore.

place after the pre-treatment reference point in time, later changes in time-variant characteristics might explain both switching states and varying well-being (e.g. a health shock). This could in particular be relevant for identifying well-being effects of switches between subsidized employment and regular employment (test II), where the trigger of the event is not restricted to a small set of events as in the cases of unemployment and job finding (test I). As UB eligibility is determined at the household level, transitions between a subsidized job and a paid job can originate from events in the life of the persons themselves, or the life of their partners (e.g. pay rise, job change, birth of a child, spousal unemployment).

To trigger UB II receipt, or its expiration, each of these reasons must lead to a change of household income, wealth, or size.[11] We conduct multiple regression analyses for each test based on the EB-reweighted control groups to eliminate such sources of bias. The basic econometric model underlying each of our tests explains the individual i's change in life satisfaction between the pre-transition PASS and the post-transition PASS interviews ($\Delta LS = LS_{post} - LS_{pre}$) by being part of the transition group (dummy $TRANS$). Wave dummies ($WAVE$) account for time effects, such as cyclically driven uncertainty about future employment stability. The basic version of the model finally includes the average change in LS of the reference group (α) and an individual error term ε:

$$(1) \quad \begin{aligned} \Delta LS_i &= \alpha + \beta TRANS_i + \gamma \Delta \ln(Y_i) + \delta \Delta WEALTH_i + \chi \Delta SIZE_i + \eta \Delta \ln(DOCVIS_i) \\ &\quad + \phi CARE_i + \mathbf{WAVE_i'}\xi + \varepsilon_i. \end{aligned}$$

As mentioned above, any event leading into or out of subsidized employment must be reflected by an increase or decrease in household income, savings or household size, because these characteristics determine the eligibility for income support. Our econometric model therefore considers the changes in log-point household income ($\Delta \ln(Y) = \ln(Y_{post}) - \ln(Y_{pre})$), whereby we account for decreasing marginal utility of income. Considering $\Delta \ln(Y)$ also allows us to disentangle the non-monetary effect of switching labour market states (e.g. norm effects) from the change of income accompanying these transitions.

In order to control for the income change, the income distributions of the three labour market states must overlap. This might be questioned as UB II receipt requires income to fall short of the minimum subsistence level. However, as described in more detail at the beginning of Section 4, being entitled to UB II depends on the household composition and regional

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[11] The institutional setting is part of the general German welfare system and did not change during our period of investigation, so the same entitlement criteria apply to all subjects.

differences in housing costs so that even two workers with the same individual earnings are not necessarily both entitled to UB II. A single household may well be regularly employed although its equivalent income falls short of the equivalent income of subsidized employed persons that have children. As figure A2 in the Appendix shows, there is indeed substantial overlap between the income distributions of unemployed workers, regular employees and subsidized employees.

We also control for changes between the seven ascending savings categories to proxy changes in financial wealth, i.e. $\Delta WEALTH = WEALTH_{post} – WEALTH_{pre}$. The categories are represented by their lower bound as the highest category is top-coded. They broaden with the amount of savings so that decreasing marginal utility is considered. In the process, we also account for permanent income (Knabe and Rätzel 2011), since changes of future income expectations should trigger changes of savings. We moreover control for a change in a household's size ($\Delta SIZE$) and changes in the number of visits to a doctor ($\Delta \ln(DOCVIS) = \ln(DOCVIS_{post}) – \ln(DOCVIS_{pre})$) to account for health shocks. Here, we also assume that the marginal well-being effect of an additional visit is decreasing. Finally, we include a binary variable indicating the change in the number of people the i is responsible for as a caregiver ($\Delta CARE$).

$\Delta SIZE$ may not perfectly control for life satisfaction effects of events that produce changes in household size. For instance, a person who moves out of their parents' home might experience the decrease of household size differently from the parents whose child moves out rendering their household smaller (e.g. Piper and Jackson 2017). We will therefore conduct robustness checks for all tests based only on single-adult households (pre- and post-transition) where fewer parallel events in family life take place. Most importantly, there are no family life events that change household size and therefore eligibility for UB II. In the process, we also rule out that events in the lives of family members, such as spousal unemployment, explain why people take up income support and their life satisfaction changes (Clark 2003, Knabe, Schöb, and Weimann 2016, Nikolova and Ayhan 2019). Alternatively, we could try to control for all family-related life events, but it would never be possible to consider each conceivable event. A subgroup analysis on single-adult households allows us to minimize the impact of family-related life events, too.

Tests II.1 and II.2 are based on samples of workers who are employed at both the pre-transition and the post-transition PASS interview. Here, we add further controls concerning occupational changes between the two points in time. In particular, job mobility (new job: *NEWJOB*, see Chadi and Hetschko 2020) and changes in working hours ($\Delta HOURS$, see Rätzel

2012, Wunder and Heineck 2013) might alter well-being and could thus confound the genuine effects of switching between subsidized and regular employment. We also control for changes in work strain due to a change of occupation, $\Delta STRAIN$. Recall that we merge a very detailed work strain index with our data. This index aggregates the strain originating from manifold working conditions, such as the mental burden of occupation-specific uncertainty about employment stability and the physical burden of manual work (see Section 4). This control variable hence adds up many characteristics of work and thus allows us to address them in the sense of a compensating differential. The modified model is

$$
\begin{aligned}
(2) \quad \Delta LS_i = \alpha &+ \beta TRANS_i + \gamma \Delta \ln Y_i + \delta \Delta WEALTH_i + \chi \Delta SIZE_i + \eta \Delta \ln DOCVIS_i \\
&+ \phi \Delta CARE_i + \theta NEWJOB_i + \kappa \Delta STRAIN_i + \lambda \Delta HOURS_i + \mathbf{WAVE_i'} \xi + \varepsilon_i.
\end{aligned}
$$

The relevance of well-being changes accompanying transitions between labour market states depends, inter alia, on how long these changes last. The previous literature finds adaptation to almost every life event (e.g. Frijters, Johnston, and Shields 2011), but not to unemployment (e.g. Clark et al. 2008). We estimate specifications of (1) and (2) that distinguish the transition groups according to the time that has elapsed since the transition has taken place. To account for nonlinearities, we use binary variables identifying three groups of people, those who switched states at most 3 months ago, between 4 and 6 months ago, or more than 6 months ago. Once people enter a new labour market status, they might switch again. People staying a relatively long time in the same status are thus potentially selected. We therefore examine only the time that elapsed from the transition to the first PASS interview thereafter, which in most of the cases encompasses less than one year.

6. Results

6.1 Do the unemployed benefit from subsidized employment?
We track individuals who experience a switch from unemployment to subsidized employment (test I.1) or vice versa (test I.2). Figure 2 depicts the average life satisfaction levels of both the transition group before and after the switch and the balanced control group of people who continue to stay in the respective initial labour market status.

Workers who transition from unemployment to subsidized employment experience a strong increase in life satisfaction. This change significantly exceeds the respective change in life satisfaction of the balanced control group by 0.493 points ($p < 0.01$; without balancing, this DiD amounts to 0.484, $p < 0.01$). The opposite transition (test I.2) yields a corresponding

pattern. Subsidized workers who become unemployed experience a drop in life satisfaction whereas the well-being level of the control group remains quite stable. The difference in the life satisfaction change between transition and control group is −0.425 ($p < 0.01$; without balancing, this DiD would be −0.488, $p < 0.01$). In sum, our mean analyses suggest that a job restores workers' life satisfaction even though the new job is subsidized. Vice versa, losing a subsidized job reduces life satisfaction so that a consistent pattern of subsidized employment being superior to being unemployed emerges.

Notably, the pre-transition levels of satisfaction differ significantly between the groups. Unobserved characteristics and anticipation of the switch may explain these differences. The former issue would not affect our results due to the difference-in-difference design. Anticipation of the transitions would mean that the effects we measure are lower bounds of the true effects. To shed light on this conjecture, we correct for anticipation in the sensitivity check presented in table S3 in the Supplementary Material, by excluding people whose pre-transition interview took place close to the switch (< 6 months, < 9 months). This does not yield substantially larger effect sizes.

Figure 2: Average changes in life satisfaction between subsidized jobs and unemployment
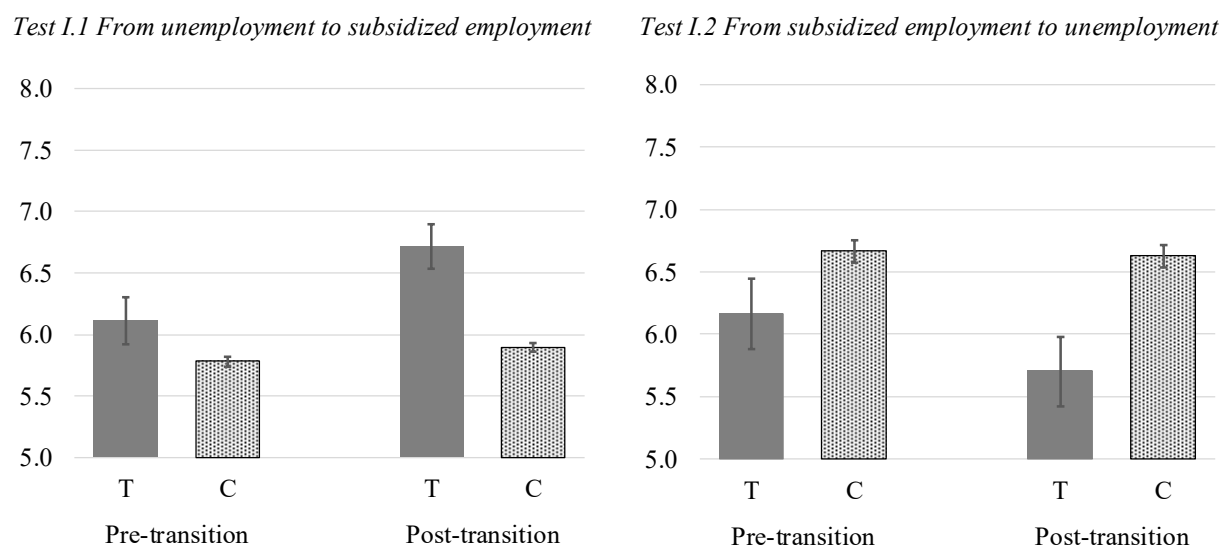
Test I.1 From unemployment to subsidized employment

Test I.2 From subsidized employment to unemployment

Source. PASS 2007-2018.
Note: Bars illustrate the average life satisfaction level of the respective transition groups (T) and control groups (C). Whiskers denote 95% confidence intervals. Control group balanced on first and second moment for test I.1 and on first moment for test I.2.

As explained in Section 5.3, multiple OLS regressions allow us to disentangle the monetary component of the well-being effect of transitions between labour market states from non-monetary aspects. In addition, the genuine life satisfaction effect of the transition will be better

approached if well-being effects of coincident changes in the UB II eligibility criteria are controlled for. According to columns 1 and 2 of table 1, changes in disposable household income, savings, household size, visits to a doctor and caregiving in the empirical model hardly affect the DiD estimate of switching from unemployment to subsidized employment. The effect remains sizeable and statistically highly significant.

Table 1: DiD unemployment and subsidized employment

Dependent variable: ΔLS	Test I.1: Unemployment → Subsidized employment					Test I.2: Subsidized employment → Unemployment				
	1	2	3	4	5	6	7	8	9	10
Transition	0.498***	0.442***	0.435***	-0.003		-0.440***	-0.393***	-0.416***	-0.333	
	(0.089)	(0.091)	(0.090)	(0.323)		(0.155)	(0.152)	(0.151)	(0.544)	
× Work is contribution				0.449					-0.513	
				(0.353)					(0.598)	
Work is contribution				-0.000					0.059	
				(0.094)					(0.221)	
Transition, ≤ 3 months ago					0.639***					-0.446**
					(0.158)					(0.216)
Transition, 4-6 months ago					0.484***					-0.600*
					(0.157)					(0.327)
Transition, > 6 months ago					0.261*					0.089
					(0.136)					(0.268)
Δ disposable income (ln)		0.318***	0.313***	0.242	0.314***		0.477**	0.469**	0.207	0.611**
		(0.120)	(0.121)	(0.170)	(0.120)		(0.220)	(0.214)	(0.262)	(0.264)
Δ savings (classes)		0.019	0.020	0.049	0.019		0.189**	0.182**	0.288***	0.178**
		(0.056)	(0.056)	(0.082)	(0.055)		(0.074)	(0.074)	(0.105)	(0.080)
Δ household members		0.005	0.008	-0.032	0.016		-0.266	-0.273	-0.059	-0.239
		(0.115)	(0.115)	(0.162)	(0.116)		(0.236)	(0.236)	(0.409)	(0.262)
Δ doctor (ln)		-0.016	-0.016	0.015	-0.013		-0.057	-0.052	0.006	-0.075*
		(0.025)	(0.024)	(0.035)	(0.024)		(0.043)	(0.043)	(0.075)	(0.043)
Δ caregiving		-0.035	-0.038	-0.070	-0.028		0.025	0.032	0.547	-0.370
		(0.140)	(0.140)	(0.236)	(0.137)		(0.297)	(0.304)	(0.762)	(0.274)
Δ network (ln)			0.043	0.015	0.044			0.137*	0.141	0.251***
			(0.042)	(0.053)	(0.040)			(0.078)	(0.188)	(0.079)
Wave controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Constant	0.346***	0.365***	0.375***	0.228	0.394***	-0.131	-0.142	-0.101	1.859***	-0.033
	(0.117)	(0.118)	(0.118)	(0.242)	(0.119)	(0.238)	(0.225)	(0.223)	(0.418)	(0.263)
Number of observations	12,514	12,514	12,514	2,458	12,513	1,689	1,689	1,689	886	1,650
R^2	0.025	0.031	0.032	0.036	0.036	0.020	0.045	0.053	0.194	0.100

Source. PASS 2007-2018.
Note: *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Standard errors clustered at household level. The control group consists of individuals who are still unemployed (columns 1 to 5) and of individuals who remain in subsidized employment (columns 6 to 10). Both control groups have been reweighted by pre-transition characteristics (see tables A1 and A2 in the Appendix). The constant (columns 3 and 8) represents a change in life satisfaction of a control group individual without any change in disposable household income, savings, household size, visits to a doctor, caregiving or close contacts outside the household.

We suspect the deviation from the social norm to work and the consequential loss of identity utility to be the main driver of the observed difference-in-difference effect (cf. Section 2). Psychological research emphasizes the importance of close contacts outside the nuclear family

as a main non-monetary benefit of employment (e.g. Jahoda 1981). This may hence serve as an alternative explanation to the positive effect of switching from unemployment to subsidized employment. To test this conjecture, we control for the change in the log number of close contacts outside the household in the estimation presented in column 3. The modification barely alters the positive effect of switching labour market states. This is in line with the notion that unemployment yields no lasting changes in affective experience over the course of the day, to which social interactions should mainly contribute (Knabe et al. 2010).

These results produce *prima facie* evidence in favour of the idea that a gain in identity utility explains the positive effect of taking up a subsidized job. To shed more direct light on this conjecture, we make use of a survey question on how important people think it is to make a contribution to society by working. We assume that the social identity of workers who strongly agree to this statement particularly depends on complying with the social norm to work. As a result, people who agree to the statement will gain more satisfaction from employment than people who disagree, if identity utility plays a part in the well-being effect of leaving joblessness.

The information on workers' take on work as a social contribution is only available for all people who have been employed in 2016, 2017 or 2018. Here, 77% of employees agree or agree strongly to the statement. Assuming temporal stability of this attitude, we can assign this information to the same workers when unemployed in the waves prior to 2016.[12] There is nevertheless a substantial loss of 80% of observations compared to the initial sample of test I.1. Entropy balancing was repeated for this truncated sample. We distinguish between two categories, 'agree' and 'strongly agree' to work being a social contribution on the one hand side and 'disagree' or 'strongly disagree' on the other hand. The resulting binary variable is interacted in a modified version of model (1) with the transition dummy (column 4). It turns out that workers who disagree with the statement do not seem to benefit from switching labour market states beyond the channels that are controlled for (level effect of transition). In sharp contrast, those who at least agree improve life satisfaction, although not at a statistically significant level. This lends some support to our conjecture that working-age people generate identity utility in working, even if their job is a subsidized one, and that this a major reason why they benefit from becoming subsidized employed after a period of joblessness.[13]

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[12] Between two-years, workers' attitude towards work as a means to make a contribution is 0.52. A large part of the variation seems to be random, however, as the three-year correlation is still 0.48.

[13] Controlling for 'work as contribution' we also tackle possible self-selection of workers into employment who consider work as an important social contribution.

Next, we examine the persistence of the well-being change accompanying the transition from unemployment to subsidized employment (column 5).[14] We find evidence for adaptation. The estimated coefficient is the smallest for those who have already been subsidized employed for at least seven months at the time of the interview. The difference to people who are subsidized employed for less than three months is significant at the 10% level. In addition, a simple linear interaction of the transition group indicator with the time since the transition in months turns out to be -0.044 ($p < 0.05$), predicting that the immediate surge in satisfaction (0.705, $p < 0.01$) disappears over the course of 16 months (results for linear interaction not shown in table).

We repeat these regressions for test I.2, which estimates the effect of becoming unemployed after having been subsidized employed. Columns 6 to 8 show that this transition reduces life satisfaction compared to remaining in subsidized employment, while controlling for changes in income, savings, household size, contacts outside the household, visits to doctors and caregiving barely affects this result. In column 9, we integrate people's agreement with the view of work as a social contribution in test I.2. Again, entropy balancing was repeated for a reduced sample allowing for this analysis. The effect is not clearly negative anymore for people who disagree with the statement that work is a means to make a societal contribution. In contrast, those who agree to the statement suffer clearly from the transition, although the difference to people who disagree is statistically insignificant (interaction effect). The final column shows that people who have already stayed unemployed for more than six months do not show a negative effect anymore, which is indicative of adaptation. The difference to those who have spent three to six months unemployed is significant at the 10% level.

Overall, tests I.1 and I.2 support our first hypothesis. Unemployment is accompanied by lower life satisfaction than subsidized employment. The benefits of working, such as complying with the social norm to work, seem to render a subsidized job more satisfying than having no job at all, although subsidized employees do not adhere to the norm of making one's own living. Our results thus suggest in-work benefits to be a suitable instrument for restoring the well-being of the unemployed if it fosters their reemployment opportunities. Besides, we find that increasing income benefits life satisfaction. Only for test I.2, we find that savings improve life satisfaction on a statistically significant level.

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[14] Note that we cannot identify the time in months for the whole of the transition groups, which is why the sample sizes reduce slightly in these specifications and why entropy balancing needed to be rerun. This also applies to column 10 as well as table 2, columns 6 and 12.

6.2 Is subsidized employment equivalent to regular employment?

Next, we follow workers from regular employment into subsidized employment (test II.1), and from subsidized employment to regular employment (test II.2). To begin with, figure 3 allows us to derive descriptive results from mean comparisons. On average, transitioning from regular employment to subsidized employment reduces satisfaction with life whereas staying regularly employed slightly increases well-being. The DiD amounts to −0.244 points ($p < 0.05$; without balancing it would be −0.153, $p = 0.11$). Becoming a regular worker after having been subsidized employed increases well-being. The difference to the change in life satisfaction of people who stay subsidized employed is 0.330 ($p < 0.01$; without balancing: 0.300, $p < 0.01$).

The difference-in-differences shown in figure 3 are smaller than those presented in figure 2. This might imply that having any kind of job brings about many benefits such as income, a structure in daily life, social contacts as well as the compliance with the norm to work (Jahoda 1981), whereas subsidized employment and regular employment differ in fewer ways.

Figure 3: Average changes in life satisfaction between subsidized and regular jobs
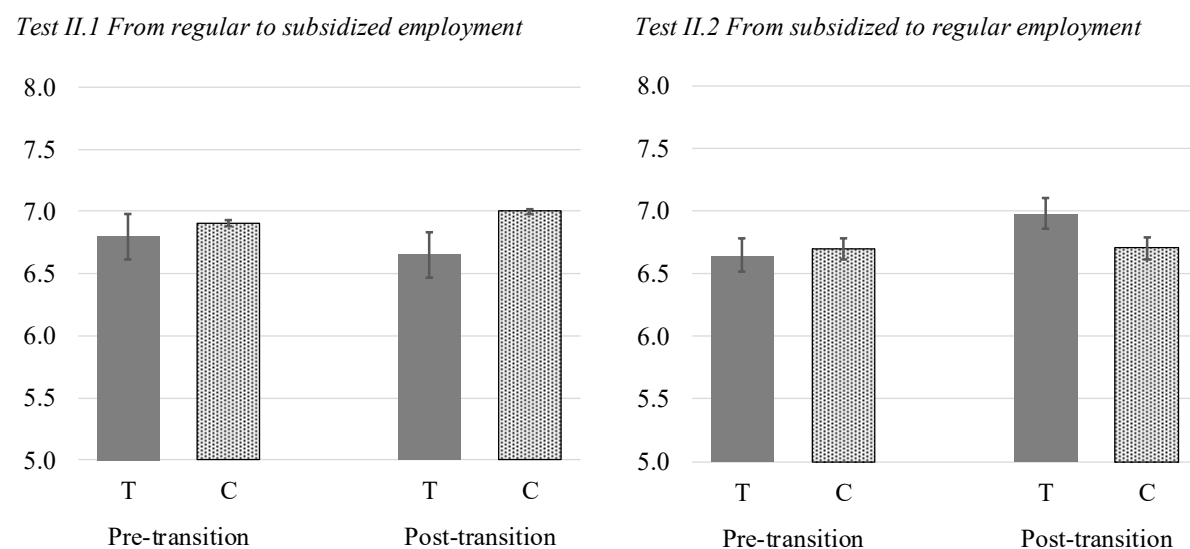
Test II.1 From regular to subsidized employment *Test II.2 From subsidized to regular employment*

Source. PASS 2007-2018.
Note: Bars illustrate the average life satisfaction level of the respective transition groups (T) and control groups (C). Whiskers denote 95% confidence intervals. Control group balanced on first and second moment.

Table 2 reports the results of multiple regression analyses considering changes in household size, disposable income, savings, health, caregiving and occupational characteristics. For test II.1, adding these controls hardly alters the negative effect of becoming subsidized employed after a regular job (columns 1 and 2). Consistent with tests I.1 and I.2, we complement the model by change in log contacts outside the household, although there is no such clear notion

for this step here as in the case of transitions between subsidized employment and unemployment. Considering this control leaves our results unchanged (column 3).

With the extended regression analysis presented in column 4, we address one particular reason why people suffer from becoming subsidized employed if they were on regular jobs before. German 'Jobcenters' serve as both social security office and employment agency. Case managers try to incentivize transfer recipients to overcome welfare dependency by making frequent appointments, sending requests and calling recipients by phone. Thus, frequent contacts with the Jobcenter render the social grouping and the non-compliance with the respective non-dependency norm very salient to subsidized workers. They might feel particularly stigmatized as being unable to make their own living due to such treatment, i.e. they suffer from the treatment stigma as described by Stuber and Schlesinger (2006).

Many people living in households receiving UB II are not in contact with their local Jobcenter. For instance, the Jobcenter may decide not to target those who contribute a relatively high share of the overall household income. Rather, it may incentivize other household members to find employment or improve earnings. Jobcenter contacts might make the individual's responsibility for the neediness of the household more salient and thus the individual's violation of the social norm to make a living. Besides the treatment stigma, an 'identity stigma' might therefore be another reason why being in contact with the Jobcenter lowers well-being.

However, Jobcenter contacts are not only a proxy of a salient deviation from the non-dependency norm, as they might also be perceived as social support (Gundert and Hohendanner 2015). To shed light on these considerations, we distinguish between transitions of people with and without Jobcenter contacts in column 4. Note that the underlying data are not available for all PASS waves such that this part of our investigation relies on a smaller sample for which entropy balancing was repeated. There is no level variable of Jobcenter contacts since the control group of regular employees is by definition not in contact with the Jobcenter. The estimation suggests that people who report these contacts lose life satisfaction in the transition unlike workers who do not.

As a next step, we repeat the analysis of people's attitude to work. Perhaps surprisingly at first glance, people who think that work is essential to make a contribution to society do not seem to suffer in particular from moving from regular employment to subsidized employment. On the contrary, if anything, high work attitude employees show less of a difference in satisfaction between regular work and subsidized work compared to others (not at a statistically

significant level, though). They might be content as long as they are employed in some way and thus do not consider it as important as others whether that work is subsidized or not.

Table 2: DiD regular employment and subsidized employment

(2.1) Test II.1: Regular employment → Subsidized employment

Dependent variable: Δ LS	1	2	3	4	5	6
Transition	-0.239** (0.099)	-0.220** (0.105)	-0.228** (0.103)	0.223 (0.225)	-0.508** (0.202)	
× Jobcenter contact afterwards = yes				-0.578** (0.249)		
× Work is contribution					0.272 (0.247)	
Work is contribution					0.211* (0.112)	
Transition, ≤ 3 months ago						-0.235 (0.243)
Transition, 4-6 months ago						-0.676*** (0.212)
Transition, > 6 months ago						-0.201 (0.150)
Δ disposable income (ln)		0.087 (0.117)	0.069 (0.122)	0.180 (0.122)	-0.008 (0.126)	-0.024 (0.131)
Δ savings (classes)		-0.012 (0.037)	-0.014 (0.036)	0.016 (0.036)	0.025 (0.040)	0.000 (0.041)
Δ household members		0.021 (0.128)	0.034 (0.125)	-0.059 (0.133)	0.040 (0.134)	0.056 (0.143)
Δ doctor (ln)		-0.067*** (0.025)	-0.067*** (0.025)	-0.082*** (0.028)	-0.075** (0.030)	-0.052* (0.027)
Δ caregiving		-0.155 (0.163)	-0.154 (0.164)	-0.180 (0.177)	-0.026 (0.234)	-0.108 (0.185)
New job		0.123 (0.128)	0.126 (0.127)	0.251* (0.143)	0.381*** (0.138)	0.050 (0.134)
Δ work strain		-0.045 (0.054)	-0.045 (0.054)	-0.031 (0.058)	0.031 (0.051)	-0.021 (0.050)
Δ working hours per week		0.005 (0.006)	0.006 (0.006)	0.003 (0.007)	0.006 (0.007)	0.006 (0.007)
Δ network (ln)			0.087 (0.060)	0.099* (0.060)	0.042 (0.063)	0.125* (0.070)
Wave controls	yes	yes	yes	yes	yes	yes
Constant	0.170 (0.154)	0.146 (0.154)	0.145 (0.155)	0.094 (0.159)	-0.031 (0.304)	0.080 (0.144)
Number of observations	23,112	23,112	23,112	20,457	15,661	23,046
R^2	0.017	0.026	0.030	0.050	0.055	0.034

(2.2) Test II.2: Subsidized employment $\rightarrow$ Regular employment

Dependent variable: Δ LS	7	8	9	10	11	12
Transition	0.318***	0.251***	0.242***	0.228	0.400	
	(0.083)	(0.083)	(0.082)	(0.186)	(0.253)	
× Jobcenter contact				0.096		
				(0.204)		
× Work is contribution					-0.241	
					(0.278)	
Jobcenter contact				-0.116		
				(0.121)		
Work is contribution					0.136	
					(0.143)	
Transition, $\leq$ 3 months ago						0.329**
						(0.138)
Transition, 4-6 months ago						0.321**
						(0.150)
Transition, > 6 months ago						0.220*
						(0.113)
Δ disposable income (ln)		0.281**	0.272**	0.237*	0.046	0.266**
		(0.115)	(0.114)	(0.121)	(0.154)	(0.122)
Δ savings (classes)		0.008	0.007	0.019	0.020	0.005
		(0.039)	(0.038)	(0.041)	(0.044)	(0.041)
Δ household members		-0.049	-0.026	0.033	-0.009	0.023
		(0.111)	(0.108)	(0.133)	(0.157)	(0.121)
Δ doctor (ln)		-0.084***	-0.086***	-0.069**	-0.109***	-0.086***
		(0.027)	(0.027)	(0.029)	(0.027)	(0.028)
Δ caregiving		-0.000	0.014	-0.086	0.077	-0.049
		(0.118)	(0.117)	(0.154)	(0.144)	(0.123)
New job		0.363***	0.356***	0.363***	0.373***	0.386***
		(0.114)	(0.110)	(0.125)	(0.135)	(0.114)
Δ work strain		-0.080	-0.080	-0.067	-0.123*	-0.062
		(0.052)	(0.050)	(0.051)	(0.068)	(0.054)
Δ working hours per week		0.003	0.002	0.006	0.001	0.000
		(0.006)	(0.006)	(0.007)	(0.007)	(0.006)
Δ network (ln)			0.142***	0.137**	0.071	0.152***
			(0.048)	(0.059)	(0.056)	(0.051)
Wave controls	yes	yes	yes	yes	yes	yes
Constant	-0.177	-0.243	-0.166	-0.119	-0.095	-0.164
	(0.197)	(0.192)	(0.177)	(0.205)	(0.298)	(0.179)
Number of observations	2,163	2,163	2,163	1,850	1,242	2,065
R^2	0.019	0.042	0.053	0.053	0.057	0.056

Source. PASS 2007-2018.

Note: *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Standard errors clustered at household level. The control group consists of individuals that remain regularly employed (columns 1-5) and individuals that remain in subsidized employment (columns 7-12). The control groups have been reweighted by pre-transition (for details see Appendix tables A3 and A4). The constant (columns 3 and 9) states a change in life satisfaction of a control group individual that does not experience any change in disposable household income, savings, household size, visits to doctor, caregiving, close contacts outside the household, work strain or working hours and has not recently switched jobs.

We also examine the persistence of the negative change in life satisfaction after becoming subsidized employed in table 2. As column 6 reveals, those who have already been in this status for three to six months show the strongest negative effect, being indicative of a non-linear adaptation path. Afterwards, the effect size significantly reduces ($p < 0.05$), beforehand, it is insignificantly smaller.

Non-take-up might be an issue in test II.1. People remain part of the reference group of regularly employed workers who could have taken up welfare, but eventually did not. Whether this affects our results can be tested by excluding people in the bottom decile (or quintile) of the distribution of household income in the control group after the transition. These people are the most likely to be eligible for in-work benefits. We also exclude people in the transition group below the same income threshold (i.e. even a larger fraction as they earn less) as otherwise the transition indicator and the income control would be colinear to a substantial extent. Based on a specification with the full set of controls (cf. column 3 in table 2), the transition effects are -0.246 ($p = 0.034$, bottom decile) and -0.280 ($p = 0.054$, bottom quintile), respectively (results not shown in table).

Considering the controls of test II.1 in test II.2 (panel (2.2), columns 7-9) hardly reduces the positive mean difference-in-difference of leaving in-work benefit receipt reported above. Interacting the transition with being in contact with the local Jobcenter before the transition (column 10) reveals that its effect increases if not only welfare receipt but also Jobcenter contacts end, although the single interaction term is not statistically significant. As for test II.1, the Jobcenter analysis relies on a smaller sample for which the balancing was repeated. Again, we find that people who think that work is an important contribution do not necessarily benefit more from regular employment (column 11).

According to the final column 12, the life satisfaction change accompanying the transition to regular employment slightly reduces over time. A linear specification of an interaction term of the transition group and the time since the transition appears to be -0.027 ($p < 0.1$). The immediate gain of life satisfaction of 0.460 ($p < 0.01$) is predicted to be eaten up over the course of 17 months (results for linear interaction not shown in table).

In sum, the empirical analyses on transitions between subsidized employment and regular employment provide evidence in support of hypothesis 2. The fact that subsidized employees do not adhere to the non-dependency norm could explain why they enjoy lower well-being than regular workers.

In addition, we find evidence for positive roles of income and enlarging social networks in workers' well-being, as well as for a honeymoon effect of starting a new job (Chadi and Hetschko 2020). Visits to a doctor decrease well-being in either test.

6.3 Regular employment, unemployment and the non-dependency norm

So far, our results indicate LS (*Unemployed, •*) $<$ LS (*Subsidized employed, •*) and LS (*Subsidized employed, •*) $<$ LS (*Regularly employed, •*). For the reason of consistency, we should expect LS (*Unemployed, •*) $<$ LS (*Regularly employed, •*) from a similar analysis of transitions between regular employment and unemployment. We therefore repeat our identification for the transition from regular employment to unemployment as well as the reverse transition. Based on the full set of controls, i.e. corresponding to columns 3 and 8 in tables 1 as well as 3 and 9 in table 2, respectively, the signs of the effects of these changes of labour market states are consistent with our previous results. Becoming unemployed yields a reduction of -0.469 ($p < 0.01$) points in life satisfaction, whereas taking up a regular job substantially improves well-being by 0.798 points ($p < 0.01$, see also Supplementary material, table S1). Both effects are thus in line with what we would expect from adding up the effects of the separate transitions with subsidized employment as an intermediate stage in tables 1 and 2.

Unlike in tests I and II before, but in line with the literature above on unemployment and life satisfaction, we do not find evidence for adaptation to unemployment (see table S1). Hence, it is probably not the different dataset or methodology we use in contrast to previous studies that produce some indication of adaptation in tests I and II.

Taken together, the estimated difference between having a regular job and being unemployed is roughly in line with previous research using other German datasets with the same satisfaction scale. For instance, Clark et al. (2008) as well as Kassenboehmer and Haisken De-New (2009) estimate satisfaction differences between employment and unemployment of −0.2 to −0.8, depending on gender.

7. Heterogeneous effects and sensitivity checks

7.1 Family life events

We need to address the impact of changes in household composition carefully as it could confound transition effects (see Section 5). A related threat to our identification strategy could come from the spill-over effects of changes in the lives of other household members. To

eliminate any issue arising from the fact that other people living in the same household affect UB II receipt, we estimate tests I and II separately for one-adult households only (one-person households or single-parents). Both groups of people, those who change labour market states and those who do not, continue to be single-adult households from the pre-transition PASS interview to the post-transition PASS interview. We also ensure that the number of children living in the household does not change.

Table 3 displays the results for the four main tests based on the single-adult household subsamples and allows for a comparison to the previous results, which are based on the full sample. Entropy balancing is performed on the first moment only, due to the smaller sample sizes. The effects of transitioning between labour market states are qualitatively the same for all tests, which makes a causal interpretation of those more credible.

Table 3: Subgroup analysis for single-adult households

	Test I.1	Test I.2	Test II.1	Test II.2
Transition effects, full sample:				
Wave controls (1st / 6th specification)	0.498***	-0.440***	-0.239**	0.318***
	(0.089)	(0.155)	(0.099)	(0.083)
Full set of controls (3rd / 8th specification)	0.435***	-0.416***	-0.228**	0.242***
	(0.090)	(0.151)	(0.103)	(0.082)
Number of observations	12,514	1,689	23,112	2,163
Number of 'treated' observations	411	188	345	681
Transition effects, single-household sample:				
Wave controls (1st / 6th specification)	0.372***	-0.517**	-0.340**	0.253**
	(0.124)	(0.218)	(0.141)	(0.122)
Full set of controls (3rd / 8th specification)	0.286**	-0.434*	-0.261*	0.213*
	(0.127)	(0.223)	(0.145)	(0.128)
Number of observations	7,226	782	6,285	962
Number of 'treated' observations	210	86	143	274

Source: PASS 2007-2018.
Note: *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Robust standard errors in parentheses. For the upper panel see the notes of tables 1 and 2. The lower panel reports transition effects of tests I.1, I.2, II.1, II.2 estimated for single-adult households only, i.e. one single adult or single parent with the same number of dependent children at both reference points in time. Entropy balancing was applied on the first moment. The conditioning variables are the same as for the full sample, except for variables that do not vary across single households (number of adults in household, marital status). Controls (wave, full set) refer to respective columns of tables 1 and 2. Unlike the main results, the full set of controls does not include the changes in household size.

7.2 Socio-demographic background and employment situation

In this subsection, we investigate effect heterogeneity regarding socio-demographic background and the characteristics of employment. Methodologically, we always use interaction terms with the transition variables to examine whether a certain characteristic modifies the effects of switching between labour market states. The corresponding interaction effects are documented in table A6 in the Appendix (level effects included but not shown). We consider findings particularly important if they hold for both transitions within a test (I or II).

We start with gender, age, and education. Men are expected to show stronger effects than women, since fulfilling the norms to work and to be self-sufficient is rather part of traditional male identity than of traditional female identity. However, we do not find more pronounced transition effects for men than for women. We do not have a prior about the role of age in the well-being evolution around switches between labour market states. If anything, age increases the satisfaction difference between subsidized employment and unemployment but decreases the satisfaction difference between subsidized and regular employment (interaction effects are not significant). A similar point could be made concerning years of education that might inflate the satisfaction gap between regular employment and subsidised employment, but also not at a statistically significant level.

East Germans receive lower wages and, therefore, are more likely to rely on in-work benefits. Hence, the intensity of the non-dependency norm may be weaker in the Eastern part such that non-compliance with this norm is less harmful to workers' well-being compared to the Western part (Chadi 2014). However, tests II.1 and II.2 do not clearly point in this direction. Similar to the East-West divide, the strength of the social norm to work and not to depend on public assistance might be weaker in people with a migrant background. Compared to natives, they may have a somewhat different set of norms for cultural reasons and are more likely to be unemployed, as compared to subsidized or regular employment, and to receive in-work benefits when employed. According to our analyses, unemployment is not less harmful for people with migrant background than for others. However, we find weak evidence that people with migrant background perceive receiving welfare benefits when employed less harmful than people without migrant background. The interaction effects are substantial and symmetric for tests II.1 and II.2, but statistically insignificant.

To differentiate the strength of the norms among different groups of migrants, we additionally interact the migrant subgroup with having German citizenship. Here, we assume that migrants who are German citizens identify more with the native population, where the

strengths of the social norms to work and not to depend on public assistance are stronger. Hence, we suspect that migrants with German citizenship try harder to adhere to the corresponding norms than migrants with foreign citizenship. In line with this notion, migrants with foreign citizenship do not benefit from becoming subsidized employed after unemployment and seem to suffer less from unemployment after subsidized employment than migrants with German citizenship.

We also tested whether the satisfaction of parents and, in particular, single parents responds differently to employment transitions from that of the average worker, but do not find consistent patterns. The same applies to interactions with previous unemployment experience. Furthermore, except for test I.1, our samples consist of employed people at the pre-transition point in time, so we can distinguish them dependent on their previous employment situation. To this end, the transition variables are interacted with (i) part-time employment (15 to 35 hours a week as opposed to at least 35 hours) and (ii) temporary employment (i.e. a fixed-term contract as opposed to a permanent one). We find weak evidence that losing a temporary subsidized job is less harmful for life satisfaction than losing a permanent subsidized job (in line with, e.g., Chadi and Hetschko 2016).

7.3 Income specifications, job characteristics and responsibility for in-work-benefit receipt
It is obviously important to accurately control the income change accompanying a transition of labor market states. We therefore vary the way of accounting for the income change in table A7 in the Appendix. In the process, we use the absolute change and the relative change alternatively to the change in logs and repeat these specifications for the household income as an alternative to the equivalent household income. None of these sensitivity checks produce estimates of satisfaction changes different from before.

Another question is whether further job characteristics that cannot be accounted for in the empirical model may change and affect satisfaction when people switch from subsidized employment to regular employment, or vice versa. To the extent that these changes are reflected in equalizing earnings differentials, i.e. they are compensated by earnings changes, they lead to an underestimation of the satisfaction effects. Therefore, they do not affect our results qualitatively. In addition, we can test whether people who do not switch jobs and thus may be unlikely to experience a change of job characteristics show the same satisfaction effects as the whole transition group. This holds true, as tables A7.3 and A7.4 in the Appendix show.

The group of workers who do not switch jobs while entering or leaving subsidized employment is also of interest for another reason. As their jobs do not change, they may be less likely than the average 'treated' individual to be personally responsible for the take-up or termination of in-work benefit receipt. The fact that they show practically the same satisfaction effects as the entire transition groups indicates that it does not matter for these effects who in a household is ultimately responsible for the receipt of welfare benefits. The analysis of single-adult households in Section 7.1 can be interpreted in the same way. Here, it is always the surveyed person who bears that responsibility. Still, the differences in satisfaction changes compared to the entire transition groups are negligible.

As another check presented in table A7, we vary the way of considering occupational characteristics. First, we test whether our results continue to hold if we leave out the work strain index in the matching and also the change in work strain in the empirical model. Second, we replace the work strain index by matching on the ten major ISCO-88 occupation groups and by controlling for any change in ISCO-88 four-digit occupations in the empirical model. None of these changes affect our findings.

7.4 Individual-fixed effects estimation and the role of income support in unemployment
In this section, we present the results of a panel estimation of life satisfaction dependent on labour market status. This is for two reasons. First, the sizes of the transition groups in our main empirical approach are relatively small which raises the issue of external validity. A higher number of observations can be obtained if observations of all transitions are pooled and observations beyond the first year after the respective transition are included. The individual fixed effects estimation relies on such a sample and hence higher numbers of observations: 2,556 observations of subsidized employed workers, 13,064 observations of unemployed workers and 27,339 observations of regularly employed workers.

Second, the panel estimation with individual fixed effects allows for examining the receipt of UB II as violation of the non-dependency norm in unemployed people. Up to here, we have argued that the findings of test II imply that income support reduces life satisfaction when employed for that reason. As a check of consistency for this interpretation, we compare unemployed workers dependent on whether they rely on UB II. Some of them not, as they rely on their partner's labour income or receive unemployment insurance benefits. These people may feel that they violate the non-dependency norm to a lesser extent than if they received UB II. As mentioned above, we deliberately did not choose an individual fixed effects approach to

be able to control for job characteristics and to build specific synthetic control groups for each of the transitions. In this case, however, our previous approach would not work. The control group of unemployed workers who stay unemployed for more than one year and continue to receive no UB II would be small and selective. The main reason is that the entitlement to unemployment insurance benefits expires once unemployment lasts more than one year, except for elderly workers who can receive the payment up to two years.

The individual-fixed effect estimation presented in table A8 supports our previous results. While regular employment serves as reference labour market status, we find that subsidized employees are less satisfied with their lives (column 1). Unemployment reduces well-being by another 0.6 points (according to H_o: $\beta - \gamma = 0$, $p < 0.01$). Thus, the satisfaction difference from subsidized employment to regular employment is, again, smaller than that to unemployment. Splitting the unemployed into different types of transfers (column 2) shows that those who depend on UB II report the lowest satisfaction levels. The difference between the two effects (γ_1 vs γ_2) is statistically significant (H_o: $\gamma_1 - \gamma_2 = 0$, $p < 0.05$). Hence, the receipt of means-tested welfare benefits yields consistent satisfaction effects on both employed (test II) and unemployed workers (fixed effects results). At the same time, the fixed-effects estimates also point to the special importance of being employed, since subsidized workers are significantly better off than unemployed workers, even if the latter do not rely on public income support (H_o: $\beta - \gamma_1 = 0$, $p < 0.01$).

8. Concluding discussion

In-work benefits help workers to overcome the extraordinary misery of unemployment. However, workers may still feel dependent on public support as they remain unable to make their own living. Our results support both views. The transition from unemployment into subsidized employment increases life satisfaction by more than what is explainable by the associated change in income. Bringing people back to work thus allows them to regain the non-monetary benefits of working, such as complying with the social norm to work. The fact that the transition from subsidized employment into regular employment also yields an improvement in life satisfaction beyond the income effect implies that subsidized employment does not fully remove the loss of well-being caused by an unemployment experience that is associated with welfare dependency. Being employed but having to rely on income support leaves people dependent on public transfers and thus does not allow them to adhere to the non-dependency norm.

Our results can explain why some eligible workers do not apply for welfare (Bruckmeier et al. 2013). The subsidy may not compensate for the well-being loss caused by non-compliance with the norm to make a living by one's own effort. Closely related to this, such a norm might also make it socially undesirable to receive UB II. This could explain why some people misreport not to receive the benefits (Kreuter, Müller, and Trappmann 2010, Bruckmeier, Müller, and Riphahn 2014). These workers probably suffer from receiving UB II the most as they are even willing to hide this circumstance from an anonymous survey. As these people are not part of the group of subsidized workers in our samples, although they actually belong to this group, our estimations should be interpreted as a lower bound of the true well-being difference between regular employment and subsidized employment.

Our findings imply that employment subsidies are beneficial if they bring people back into employment. They offset, at least partly, the harm done by involuntary unemployment but fail to make workers as well off as those who are regularly employed. This might not be a disadvantage. In-work benefits might work as a stepping stone to bring involuntarily unemployed people back into regular work in the long run. Starting a low-paid job makes it easier for poorly educated workers to get well-paid in the future (Knabe and Plum 2013). They may accumulate human capital on the job and signal their motivation to work and thus encourage employers to hire them rather than unemployed workers. Besides the monetary incentive to receive a higher wage, our analysis suggests that the non-monetary incentive to overcome welfare dependency may also motivate them to use subsidized employment as a stepping stone to regular employment.

If, however, there is no or little upward mobility, in-work benefits will permanently fund a persistent group of working poor that are unable to find a regular job. Then, in-work benefits would create permanent non-negligible well-being cost for the 'beneficiaries', as they are not as well off as the regularly employed. In this case, a cautious policy recommendation is to replace individually means-tested income support by a general income redistribution scheme that does not entail a violation of the non-dependency norm. For instance, payroll subsidies, such as reduced social security contributions at the lower end of the wage distribution, bring down gross labour costs without necessarily reducing net wages paid out to workers (e.g. Phelps 1994). This can boost labour demand and thus provide more regular job opportunities. It also diminishes the individually perceived dependence on the welfare state and thus the negative stigma effects from norm violation.

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Appendix

Figure A1.I: Time trends in life satisfaction, household size and income – test I

Test I.1: From unemployment ($t = -1$) to subsidized employment ($t = 0$)
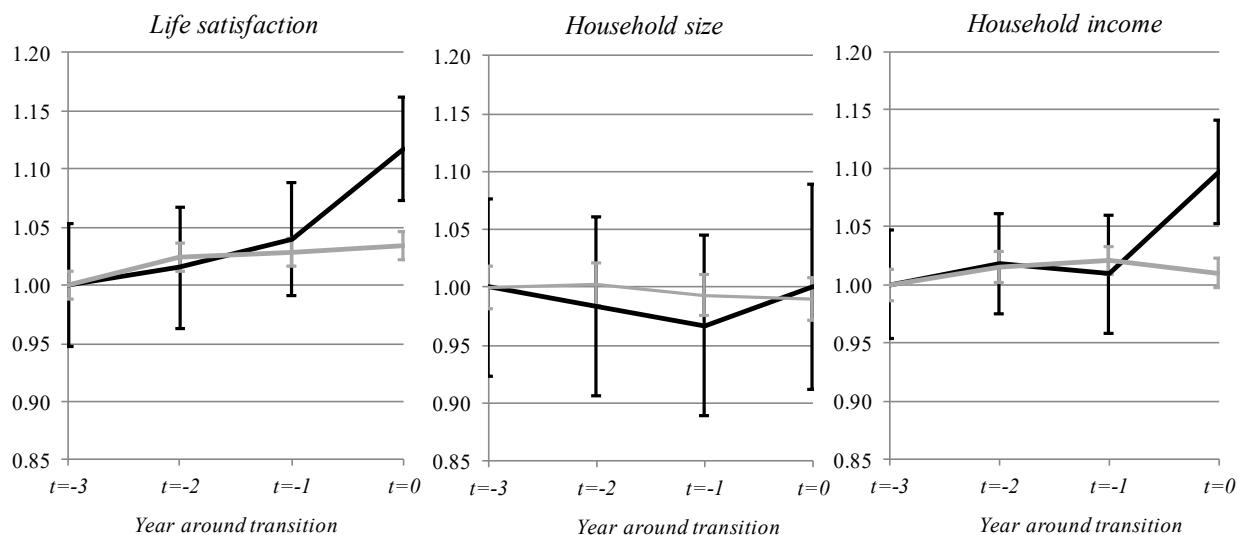
Test I.2: From subsidized employment ($t = -1$) to unemployment ($t = 0$)
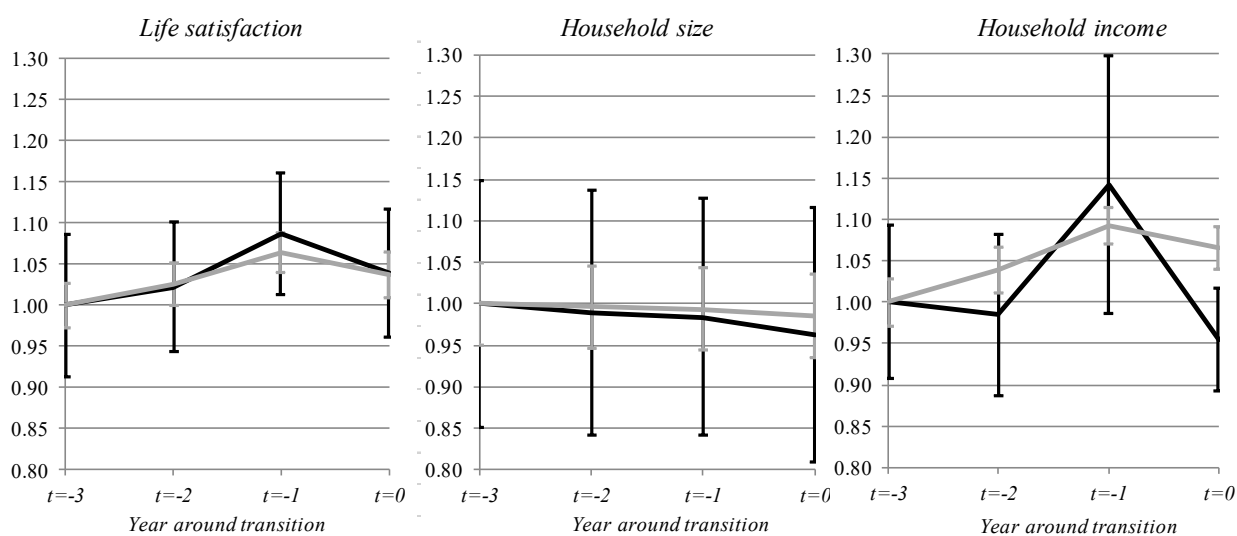
Source: PASS 2007-2018.
Note: Black lines denote transition group trends, grey lines denote trends of the synthetic control groups (i.e. after entropy balancing). Whiskers display 95% confidence intervals. Transition groups change labour market status from t=-1 to t=0, which denote two subsequent PASS waves with a time lag of approximately one year At time t=-2 (t=-3), transition groups are interviewed for the second-last (third-last) time before switching. All mean levels are normalized relative to the level at t=-3.

Figure A1.II: Trends in life satisfaction, household size and income – test II

Test II.1: From regular employment (*t* = −1) to subsidized employment (*t* = 0)
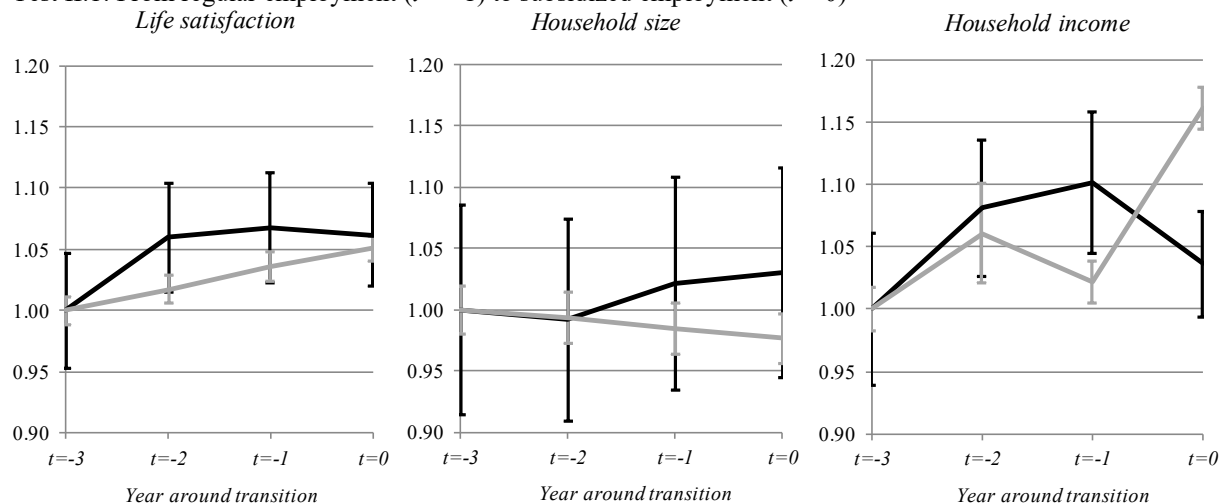
Test II.2: From subsidized employment (*t* = −1) to regular employment (*t* = 0)
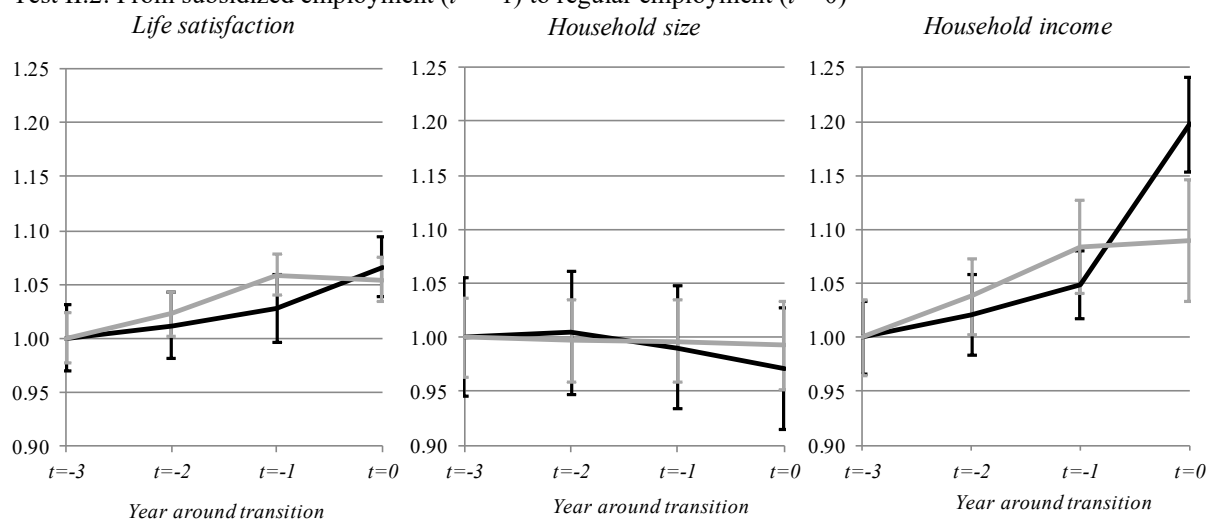
Source: PASS 2007-2018.
Note: Black lines denote transition group trends, grey lines denote trends of the synthetic control groups (i.e. after entropy balancing). Whiskers display 95% confidence intervals. transition groups change labour market status from t=-1 to t=0, which denote two subsequent PASS waves with a time lag of approximately one year. At time t=-2 (t=-3), transition groups are interviewed for the second-last (third-last) time before switching. All mean levels are normalized relative to the level at t=-3.

Figure A2: Overlap of income distributions by labour market status

■ regularly employed (n=43,056)   ▦ subsidized employed (n=4,495)   ▧ unemployed (n=26,866)
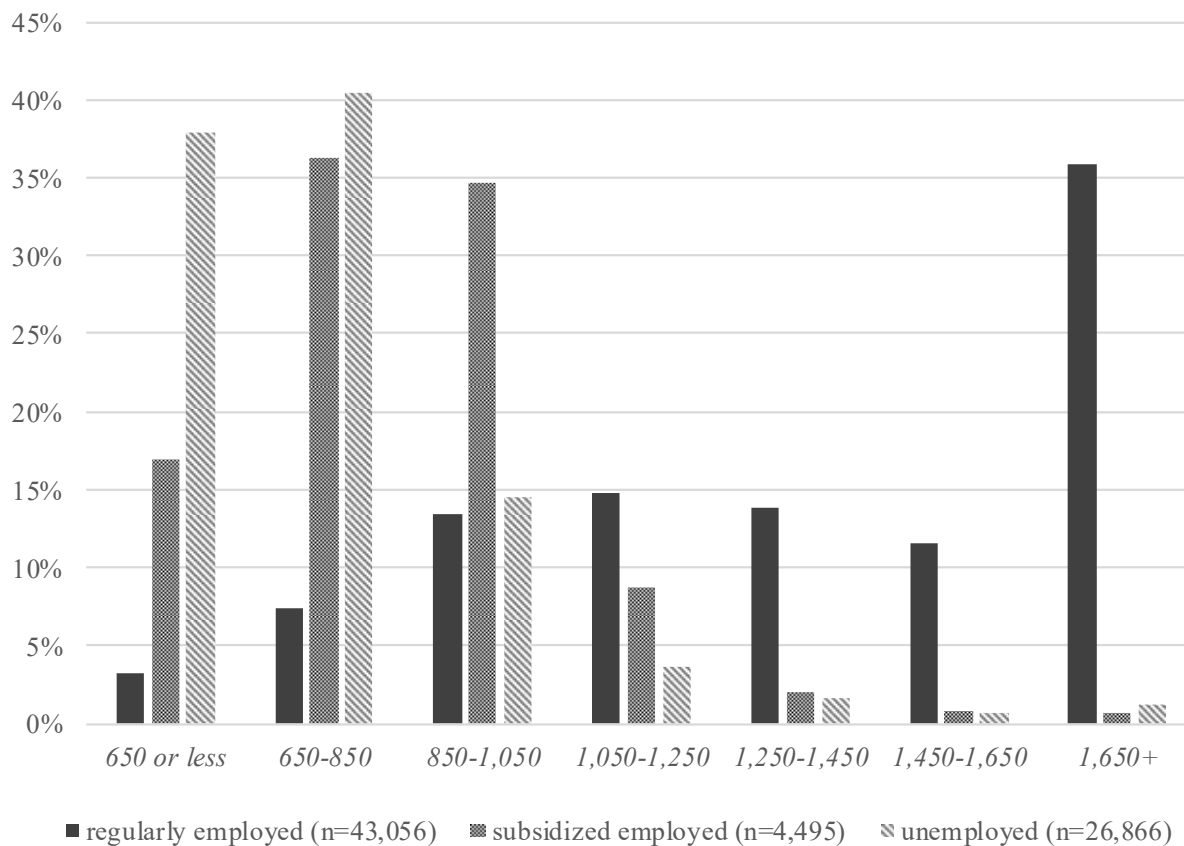
Source: PASS 2007-2018.
Note: Following the OECD equivalence scale, inflation-adjusted household income is divided by a weighted number of the persons living in the same household. While the first person gets a weight of 1, any additional person older than 14 years gets a weight of 0.5, children up to the age of 14 years get a weight of 0.3.

Table A1: Summary statistics test I.1

	Range	Transition		Control		Δ	Balanced control	
		Mean / share	*Std. dev. (means)*	*Mean / share*	*Std. dev. (means)*	*Control-Trans*	*Mean / share*	*Std. dev. (means)*
Number of observations		411		12,103			12,103	
Subjective well-being:								
Life satisfaction	0-10	6.11	1.95	5.58	2.19	-0.53***	5.78	2.12
Income and Wealth:								
Equivalent income, in euros	≤ 7,991	747.47	243.14	707.17	275.61	-40.30***	747.22	243.14
Savings, class	0-7	0.65	0.94	0.65	1.07	0.00	0.65	0.94
Socio-demographic characteristics:								
Age, in years	25-59	41.23	8.79	44.29	9.71	3.07***	41.21	8.78
Adults in household	1-9	1.45	0.55	1.47	0.65	0.01	1.45	0.55
Children in household	0-7	1.08	1.11	0.77	1.17	-0.31***	1.08	1.11
Children outside household	0-10	0.56	1.02	0.74	1.15	0.18***	0.56	1.02
Child younger 7 years in household	share	0.26		0.16		-0.10***	0.26	
Close contacts outside household	1-99	6.75	8.50	6.46	7.81	-0.29	6.75	8.50
Years of schooling	7-21	11.46	2.31	11.10	2.46	-0.35***	11.45	2.31
Unemployment benefit receipt	share	0.07		0.05		-0.02**	0.07	
Unemployment benefit II receipt	share	0.95		0.91		-0.04***	0.95	
Gender: male	share	0.41		0.49		0.09***	0.41	
Marital status: single	share	0.35		0.40		0.05*	0.35	
Marital status: married	share	0.30		0.26		-0.04*	0.30	
Marital st.: separated/divorced/widowed	share	0.34		0.34		0.00	0.34	
Immigrant (1st – 3rd generation)	share	0.36		0.32		-0.04*	0.36	
Region: West-Germany	share	0.65		0.64		-0.01	0.65	
Number of unemployment episodes	0-99	0.86	2.23	0.76	2.27	-0.10	0.86	2.23
Health:								
Disability, officially registered	share	0.10		0.17		0.07***	0.10	
Disability, currently applying	share	0.02		0.05		0.02**	0.02	
Overnight stay in hospital (last year)	share	0.12		0.20		0.07***	0.12	
Number of visits to a doctor (last quarter)	0-99	2.93	5.44	3.64	6.19	0.71**	2.93	5.44
Care giving to relatives or friends	share	0.08		0.09		0.01	0.08	

Source: PASS 2007-2018.
*Note: All variables relate to pre-transition PASS interviews; *denotes significance at the 10%
level, **at the 5% level and ***at the 1% level. Control group balanced on first and second moment.*

Table A2: Summary statistics test I.2

	Range	Transition		Control		Δ	Balanced control	
		Mean / share	*Std. dev. (means)*	*Mean / share*	*Std. dev. (means)*	*Control-Trans*	*Mean / share*	*Std. dev. (means)*
Number of observations		188		1,501			1,501	
Subjective well-being:								
Life satisfaction	0-10	6.16	1.96	6.62	1.75	0.46***	6.66	1.78
Income and Wealth:								
Equivalent income, in euros	≤ 8,398	870.92	651.72	835.97	208.27	-34.94	870.92	202.27
Savings, class	0-6	0.65	0.92	0.82	1.05	0.17**	0.65	0.89
Socio-demographic characteristics:								
Age, in years	25-59	41.98	8.86	43.79	8.59	1.82***	41.98	8.57
Adults in household	1-9	1.62	0.90	1.53	0.56	-0.09	1.62	0.77
Children in household	0-7	1.24	1.31	1.13	1.10	-0.11	1.24	1.12
Children outside household	0-7	0.62	0.99	0.64	0.99	0.01	0.62	1.04
Child younger 7 years in household	share	0.31		0.23		-0.08**	0.31	
Close contacts outside household	1-99	6.49	7.74	6.59	6.95	0.10	6.49	6.31
Years of schooling	7-21	11.00	2.20	11.63	2.30	0.63***	11.00	1.95
Gender: male	share	0.41		0.33		-0.08**	0.41	
Marital status: single	share	0.29		0.29		0.00	0.29	
Marital status: married	share	0.34		0.37		0.04	0.34	
Marital st.: separated/divorced/widowed	share	0.38		0.34		-0.04	0.38	
Immigrant (1st – 3rd generation)	share	0.29		0.33		0.04	0.29	
Region: West-Germany	share	0.61		0.63		0.02	0.61	
Number of unemployment episodes	0-30	0.85	2.35	0.48	1.89	-0.37**	0.85	3.16
Health:								
Disability, officially registered	share	0.08		0.09		0.01	0.08	
Disability, currently applying	share	0.02		0.02		0.01	0.02	
Overnight stay in hospital (last year)	share	0.13		0.12		-0.01	0.13	
Number of visits to a doctor (last quarter)	0-90	3.03	3.63	2.30	3.96	-0.74**	3.03	7.62
Care giving to relatives or friends	share	0.04		0.06		0.02	0.04	
Job characteristics:								
Monthly gross earnings, in euros	≤ 3,300	971.73	542.78	945.25	453.55	-26.48	971.73	464.98
Employment Type: Blue-collar	share	0.40		0.31		-0.09**	0.40	
Employment Type: White-collar	share	0.51		0.54		0.04	0.51	
Employment Type: Self-employed	share	0.09		0.14		0.05*	0.09	
Temporary contract	share	0.37		0.17		-0.19***	0.37	
Part-time employed	share	0.52		0.61		0.09**	0.52	
Work strain index	1-10	7.07	2.09	6.72	2.32	-0.35**	7.07	2.20
Actual working hours per week	15-80	34.28	12.25	31.67	11.65	-2.61***	34.28	12.55
Tenure, in months	≤ 499	26.99	43.21	57.81	75.02	30.82***	27.04	34.56

Source: PASS 2007-2018.

Note: All variables relate to pre-transition PASS interviews; *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Control group balanced on first moment.

Table A3: Summary statistics test II.1

	Range	Transition		Control		Δ	Balanced control	
		Mean / share	*Std. dev. (means)*	*Mean / share*	*Std. dev. (means)*	*Control-Trans*	*Mean / share*	*Std. dev. (means)*
Number of observations		345		22,767			22,767	
Subjective well-being:								
Life satisfaction	0-10	6.80	1.74	7.42	1.42	0.62***	6.91	1.73
Income and Wealth:								
Equivalent income, in euros	≤ 101,113	931.51	339.80	1,651.71	1,354.16	720.20***	930.69	340.23
Savings, class	0-7	1.07	1.23	3.10	2.24	2.04***	1.07	1.23
Socio-demographic characteristics:								
Age, in years	25-59	41.99	9.39	43.19	9.19	1.20**	41.95	9.38
Adults in household	1-11	1.62	0.63	1.78	0.61	0.16***	1.62	0.63
Children in household	0-8	0.97	1.05	0.91	1.02	-0.07	0.97	1.05
Children outside household	0-9	0.68	1.04	0.50	0.86	-0.19***	0.68	1.04
Child younger 7 years in household	share	0.23		0.17		-0.06***	0.23	
Close contacts outside household	1-99	7.37	8.80	7.46	7.11	0.09	7.37	8.79
Years of schooling	7-21	11.44	2.23	12.83	2.73	1.39***	11.43	2.23
Gender: male	share	0.44		0.51		0.07**	0.44	
Marital status: single	share	0.31		0.30		-0.01	0.31	
Marital status: married	share	0.35		0.52		0.17***	0.35	
Marital st.: separated/divorced/widowed	share	0.34		0.17		-0.17***	0.34	
Immigrant (1st – 3rd generation)	share	0.30		0.22		-0.08***	0.30	
Region: West-Germany	share	0.61		0.71		0.10***	0.61	
Number of unemployment episodes	0-42	0.59	2.20	0.24	0.99	-0.35***	0.59	2.20
Health:								
Disability, officially registered	share	0.07		0.07		0.01	0.07	
Disability, currently applying	share	0.01		0.01		0.00	0.01	
Overnight stay in hospital (last year)	share	0.11		0.10		-0.01	0.11	
Number of visits to a doctor (last quarter)	0-70	1.96	2.93	2.06	3.32	0.10	1.96	2.93
Care giving to relatives or friends	share	0.08		0.06		-0.02		
Job characteristics:								
Monthly gross earnings, in euros	≤ 106,300	1,291.70	889.53	2,602.72	2628.72	1,311.02***	1,290.56	889.14
Employment Type: Blue-collar	share	0.33		0.22		-0.11***		0.47
Employment Type: White-collar	share	0.56		0.69		0.13***		0.50
Employment Type: Self-employed	share	0.11		0.08		-0.03*		0.31
Temporary contract	share	0.19		0.11		-0.08***		0.39
Part-time employed	share	0.46		0.23		-0.22***		0.50
Work strain index	1-10	6.66	2.34	5.60	2.80	-1.06***	6.66	2.34
Actual working hours per week	15-80	35.33	11.71	40.09	10.61	4.76***	35.30	11.70
Tenure, in months	≤ 539	48.72	70.17	92.23	100.48	43.51***	48.68	70.14

Source: PASS 2007-2018.

Note: All variables relate to pre-transition PASS interviews; *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Control group balanced on first and second moment.

Table A4: Summary statistics test II.2

	Range	Transition		Control		Δ	Balanced control	
		Mean / share	*Std. dev. (means)*	*Mean / share*	*Std. dev. (means)*	*Control-Trans*	*Mean / share*	*Std. dev. (means)*
Number of observations		681		1,482			1,482	
Subjective well-being:								
Life satisfaction	0-10	6.65	1.80	6.63	1.75	-0.02	6.70	1.63
Income and Wealth:								
Equivalent income, in euros	≤ 2,713	870.95	235.86	836.08	208.39	-34.87***	870.73	235.83
Savings, class	0-6	1.01	1.18	0.82	1.05	-0.18***	1.01	1.18
Socio-demographic characteristics:								
Age, in years	25-59	41.98	8.81	43.80	8.57	1.82***	41.97	8.81
Adults in household	1-7	1.59	0.60	1.53	0.56	-0.06**	1.59	0.60
Children in household	0-6	1.09	1.02	1.13	1.09	0.04	1.09	1.02
Children outside household	0-7	0.54	0.92	0.64	0.99	0.10**	0.54	0.92
Child younger 7 years in household	share	0.24		0.23		0.00	0.24	
Close contacts outside household	1-99	6.64	5.98	6.59	6.96	-0.05	6.64	5.98
Years of schooling	7-21	11.64	2.14	11.62	2.30	-0.02	11.64	2.14
Gender: male	share	0.38		0.33		-0.05**	0.38	
Marital status: single	share	0.29		0.28		-0.01	0.29	
Marital status: married	share	0.37		0.37		0.00	0.37	
Marital st.: separated/divorced/widowed	share	0.33		0.34		0.02	0.33	
Immigrant (1st – 3rd generation)	share	0.28		0.33		0.05**	0.28	
Region: West-Germany	share	0.62		0.63		0.01	0.62	
Number of unemployment episodes	0-30	0.63	1.58	0.48	1.89	-0.15*	0.63	1.58
Health:								
Disability, officially registered	share	0.06		0.09		0.03**	0.06	
Disability, currently applying	share	0.02		0.02		0.00	0.02	
Overnight stay in hospital (last year)	share	0.09		0.12		0.02	0.09	
Number of visits to a doctor (last quarter)	0-90	2.31	5.82	2.30	3.98	-0.01	2.30	5.82
Care giving to relatives or friends	share	0.05		0.06		0.01	0.05	
Job characteristics:								
Monthly gross earnings, in euros	≤ 4,200	1,156.95	546.56	944.39	453.17	-212.56***	1,156.66	546.49
Employment Type: Blue-collar	share	0.31		0.31		0.00	0.31	
Employment Type: White-collar	share	0.58		0.55		-0.03	0.58	
Employment Type: Self-employed	share	0.11		0.14		0.03*	0.11	
Temporary contract	share	0.19		0.17		-0.02	0.19	
Part-time employed	share	0.49		0.61		0.12***	0.49	
Work strain index	1-10	6.55	2.40	6.72	2.31	0.17	6.55	2.40
Actual working hours per week	15-80	33.96	11.30	31.59	11.59	-2.37***	33.95	11.30
Tenure, in months	≤ 499	49.57	71.21	58.03	75.05	8.46**	49.56	71.20

Source: PASS 2007-2018.
Note: All variables relate to pre-transition PASS interviews; *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Control group balanced on first and second moment.

Table A5: Placebo tests

	Changes in life satisfaction			Changes in income			Changes in household size		
Test I.1	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*
T: UE → SE	-0.014	-0.061	0.498***	7.127	-25.207*	130.472***	-0.040*	-0.019	0.039
	(0.135)	(0.110)	(0.089)	(16.490)	(15.166)	(28.270)	(0.024)	(0.024)	(0.026)
Wave	yes	yes	yes	yes	yes	yes	yes	yes	yes
Constant	0.413***	0.471***	0.346***	3.240	23.411	-43.163*	0.012	0.015	-0.041
	(0.155)	(0.149)	(0.117)	(22.938)	(22.890)	(22.680)	(0.014)	(0.020)	(0.025)
No. of obs.	5,774	8,577	12,514	5,769	8,570	12,514	5,787	8,587	12,514
R²	0.014	0.015	0.025	0.008	0.010	0.028	0.024	0.018	0.010

	Changes in life satisfaction			Changes in income			Changes in household size		
Test I.2	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*
T: SE → UE	-0.069	0.083	-0.440***	-40.290	82.741	-111.450*	-0.030	-0.026	0.002
	(0.244)	(0.176)	(0.155)	(37.169)	(90.954)	(62.898)	(0.043)	(0.038)	(0.037)
Wave	yes	yes	yes	yes	yes	yes	yes	yes	yes
Constant	0.377	0.220	-0.131	9.796	-39.159	65.795	0.008	0.059	0.079
	(0.249)	(0.260)	(0.238)	(40.842)	(61.406)	(61.351)	(0.024)	(0.039)	(0.050)
No. of obs.	921	1,260	1,689	919	1,260	1,689	924	1,262	1,689
R²	0.016	0.013	0.020	0.021	0.030	0.033	0.016	0.017	0.027

	Changes in life satisfaction			Changes in income			Changes in household size		
Test II.1	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*
T: RE → SE	0.200	-0.007	-0.239**	-0.754	82.297***	-200.623***	-0.019	0.069***	0.078***
	(0.128)	(0.113)	(0.099)	(29.380)	(26.156)	(18.388)	(0.045)	(0.026)	(0.028)
Wave	yes	yes	yes	yes	yes	yes	yes	yes	yes
Constant	0.119	0.130	0.170	209.863**	-62.178*	164.272***	0.063*	-0.053**	-0.027
	(0.165)	(0.183)	(0.154)	(107.039)	(34.514)	(35.314)	(0.038)	(0.027)	(0.027)
No. of obs.	14,443	18,312	23,112	14,355	18,244	23,112	14,449	18,318	23,112
R²	0.023	0.008	0.017	0.004	0.004	0.057	0.010	0.017	0.013

	Changes in life satisfaction			Changes in income			Changes in household size		
Test II.2	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*	*t=-3 to t=-2*	*t=-2 to t=1*	*t=-1 to t=0*
T: SE → RE	-0.097	-0.056	0.318***	-10.585	-5.922	121.557***	0.019	-0.018	-0.034
	(0.113)	(0.098)	(0.083)	(21.162)	(17.203)	(16.053)	(0.028)	(0.023)	(0.022)
Wave	yes	yes	yes	yes	yes	yes	yes	yes	yes
Constant	0.116	0.371*	-0.177	32.258	56.488***	12.919	0.042	-0.004	-0.005
	(0.181)	(0.211)	(0.197)	(25.940)	(18.423)	(28.167)	(0.057)	(0.033)	(0.039)
No. of obs.	1,157	1,574	2,163	1,154	1,574	2,163	1,161	1,578	2,163
R²	0.013	0.014	0.019	0.011	0.010	0.046	0.007	0.007	0.007

Source: PASS 2007-2018.

Note: t=-3 (t=-2, t=-1) represents the third-last interview (second-last interview, last interview) before the respective transition (T) between unemployment (UE), subsidized employment (SE), regular employment (RE). *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Control groups balanced on the first and the second moment (tests I.1, II.1, II.2), or on the first moment only (test I.2).

Table A6: Interaction effects

(A6.1) From unemployment to subsidized employment

	Baseline	Gender	Age	Education	West	Migrant backgr.	Migrant+ foreigner	Parent	Single parent	Jobless in past
Transition I.1	0.435***	0.422***	0.268	1.077**	0.441***	0.469***	0.470***	0.344*	0.425***	0.493***
	(0.090)	(0.109)	(0.446)	(0.488)	(0.159)	(0.113)	(0.112)	(0.199)	(0.100)	(0.093)
× male		0.033								
		(0.183)								
× age			0.004							
			(0.011)							
× years of education				-0.056						
				(0.041)						
× West-German origin					-0.008					
					(0.190)					
× migrant background						-0.096	0.143			
						(0.186)	(0.221)			
× migrant background × not a German citizen							-0.507*			
							(0.297)			
× parent								0.116		
								(0.222)		
× single parent									0.073	
									(0.226)	
× unemployment episodes										-0.066*
										(0.034)
Controls: Table 1, Sp. (3)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of observations	12,514	12,514	12,514	12,514	12,514	12,514	12,514	12,514	12,514	12,514
R²	0.032	0.032	0.032	0.034	0.032	0.033	0.039	0.032	0.032	0.035

(A6.2) From subsidized employment to unemployment

	Baseline	Gender	Age	Education	West	Migrant backgr.	Migrant+ foreigner	Parent	Single parent	Jobless in past	Part-time job	Temp. job
Transition I.2	-0.416***	-0.417**	-0.277	-0.460	-0.170	-0.355**	-0.354**	-0.166	-0.387**	-0.421***	-0.421*	-0.579***
	(0.151)	(0.179)	(0.715)	(0.724)	(0.231)	(0.176)	(0.176)	(0.364)	(0.168)	(0.160)	(0.244)	(0.203)
× male		0.009										
		(0.300)										
× age			-0.003									
			(0.016)									
× years of education				0.004								
				(0.063)								
× West-German origin					-0.399							
					(0.299)							
× migrant background						-0.212	-0.389					
						(0.306)	(0.357)					
× migrant background × not a German citizen							0.396					
							(0.516)					
× parent								-0.300				
								(0.399)				
× single parent									-0.153			
									(0.401)			
× unemployment episodes										0.007		
										(0.043)		
× part-time job											0.009	
											(0.308)	
× temporary job												0.435
												(0.273)
Controls: Table 1, Sp. (8)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of observations	1,689	1,689	1,689	1,689	1,689	1,689	1,689	1,689	1,689	1,689	1,689	1,689
R²	0.053	0.057	0.054	0.054	0.060	0.054	0.055	0.055	0.054	0.053	0.053	0.057

(A6.3) From regular employment to subsidized employment

	Baseline	Gender	Age	Education	West	Migrant backgr.	Migrant+ foreigner	Parent	Single parent	Jobless in past	Part-time job	Temp. job
Transition II.1	-0.228**	-0.306**	-0.603	-0.054	-0.089	-0.312**	-0.311**	-0.086	-0.214*	-0.206**	-0.269**	-0.227**
	(0.103)	(0.123)	(0.454)	(0.582)	(0.172)	(0.123)	(0.123)	(0.233)	(0.112)	(0.104)	(0.136)	(0.110)
× male		0.176										
		(0.191)										
× age			0.009									
			(0.010)									
× years of education				-0.015								
				(0.048)								
× West-German origin					-0.228							
					(0.213)							
× migrant background						0.275	0.237					
						(0.214)	(0.245)					
× migrant background × not a German citizen							0.101					
							(0.394)					
× parent								-0.179				
								(0.255)				
× single parent									-0.100			
									(0.267)			
× unemployment episodes										-0.032		
										(0.072)		
× part-time job											0.076	
											(0.198)	
× temporary job												-0.008
												(0.278)
Controls: Table 2, Sp. (3)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of observations	23,112	23,112	23,112	23,112	23,112	23,112	23,112	23,112	23,112	23,112	23,112	23,112
R²	0.030	0.030	0.031	0.030	0.031	0.032	0.032	0.031	0.031	0.024	0.030	0.030

(A6.4) From subsidized employment to regular employment

	Baseline	Gender	Age	Education	West	Migrant backgr.	Migrant+ foreigner	Parent	Single parent	Jobless in past	Part-time job	Temp. job
Transition II.2	0.242***	0.227**	0.456	0.073	0.310**	0.291***	0.292***	0.368*	0.256***	0.157*	0.234*	0.257***
	(0.082)	(0.097)	(0.391)	(0.447)	(0.139)	(0.100)	(0.101)	(0.202)	(0.091)	(0.091)	(0.125)	(0.091)
× male		0.041										
		(0.179)										
× age			-0.005									
			(0.009)									
× years of education				0.015								
				(0.037)								
× West-German origin					-0.111							
					(0.169)							
× migrant background						-0.175	-0.189					
						(0.158)	(0.179)					
× migrant background × not a German citizen							0.035					
							(0.271)					
× parent								-0.154				
								(0.222)				
× single parent									-0.075			
									(0.203)			
× unemployment episodes										0.131		
										(0.083)		
× part-time job											0.025	
											(0.169)	
× temporary job												-0.077
												(0.201)
Controls: Table 2, Sp. (9)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of observations	2,163	2,163	2,163	2,163	2,163	2,163	2,163	2,163	2,163	2,163	2,163	2,163
R²	0.053	0.055	0.054	0.054	0.054	0.054	0.054	0.054	0.054	0.058	0.054	0.053

Source: PASS 2007-2018.

*Note: *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Standard errors clustered at household level. The tables show interaction effect of switching labour market states for tests I.1 (A6.1), I.2 (A6.2), II.1 (A6.3), and II.2 (A6.4). The corresponding level effects are included, but not shown. Control groups are balanced on variables listed in tables A1 to A4.*

Table A7: Alternative ways of controlling for income and job characteristics

Table A7.1. Unemployment → Subsidized employment (Test I.1)

Dependent variable: ΔLS	Equivalent income (OECD scale)			Household income		
Specification	change of logs†	simple change / 1000	relative change	change of logs†	simple change / 1000	relative change
Transition	0.435***	0.427***	0.478***	0.437***	0.430***	0.479***
	(0.090)	(0.090)	(0.091)	(0.091)	(0.091)	(0.091)
Change of income	0.313***	0.415***	0.005***	0.297**	0.281***	0.003***
	(0.121)	(0.147)	(0.001)	(0.118)	(0.105)	(0.000)
Controls: table 1, 3rd column	yes	yes	yes	yes	yes	yes
Number of observations	12,514	12,514	12,514	12,514	12,514	12,514
Size of the transition group	411	411	411	411	411	411
R^2	0.032	0.037	0.028	0.032	0.036	0.028

Table A7.2. Subsidized employment → Unemployment (Test I.2)

Dependent variable: ΔLS	Equivalent income (OECD scale)			Household income			Occupation	
Specification	change of logs†	simple Δ / 1000	relative change	change of logs†	simple Δ / 1000	relative change	Strain left out	Major ISCO‡
Transition	-0.416***	-0.441***	-0.434***	-0.422***	-0.438***	-0.437***	-0.416***	-0.406***
	(0.151)	(0.153)	(0.150)	(0.151)	(0.152)	(0.150)	(0.151)	(0.152)
Change of income	0.469**	0.071	0.332***	0.413**	0.082	0.319***	0.469**	0.455**
	(0.214)	(0.141)	(0.088)	(0.209)	(0.115)	(0.085)	(0.213)	(0.212)
Controls: table 1, 8th column	yes	yes	yes	yes	yes	yes	yes¥	yes¥
Number of observations	1,689	1,689	1,689	1,689	1,689	1,689	1,689	1,689
Size of the transition group	188	188	188	188	188	188	188	188
R^2	0.053	0.045	0.054	0.051	0.046	0.054	0.053	0.052

Table A7.3. Regular employment → Subsidized employment (Test II.1)

Dependent variable: ΔLS	Equivalent income (OECD scale)			Household income			Occupation		
Specification	change of logs†	simple Δ / 1000	relative change	change of logs†	simple Δ / 1000	relative change	no job changers	Strain left out	ISCO controls‡
Transition	-0.228**	-0.218**	-0.245**	-0.228**	-0.228**	-0.242**	-0.218*	-0.232**	-0.229**
	(0.103)	(0.103)	(0.103)	(0.103)	(0.103)	(0.103)	(0.118)	(0.103)	(0.103)
Change of income	0.069	0.129	-0.016	0.071	0.044	-0.005	0.076	0.067	0.053
	(0.122)	(0.114)	(0.033)	(0.124)	(0.069)	(0.031)	(0.142)	(0.122)	(0.125)
Any change in ISCO88									-0.168
									(0.210)
Controls: table 2, 3th column	yes	yes	yes	yes	yes	yes	yes	yes¥	yes¥
Number of observations	22,767	22,767	22,767	22,767	22,767	22,767	20,751	22,767	22,767
Size of the transition group	345	345	345	345	345	345	276	345	345
R^2	0.030	0.030	0.029	0.030	0.030	0.029	0.016	0.030	0.029

Table A7.4. Subsidized employment → Regular employment (Test II.2)

Dependent variable: ΔLS	Equivalent income (OECD scale)			Household income			Occupation		
Specification	change of logs†	simple Δ / 1000	relative change	change of logs†	simple Δ / 1000	relative change	no job changers	Strain left out	ISCO controls‡
Transition	0.242***	0.236***	0.264***	0.242***	0.244***	0.265***	0.244***	0.251***	0.266***
	(0.082)	(0.083)	(0.081)	(0.082)	(0.083)	(0.081)	(0.091)	(0.083)	(0.084)
Change of income	0.272**	0.310*	0.056	0.269**	0.143	0.047	0.204	0.271**	0.282**
	(0.114)	(0.162)	(0.040)	(0.113)	(0.090)	(0.040)	(0.127)	(0.114)	(0.116)
Any change in ISCO88									0.068
									(0.188)
Controls: table 2, 8th column	yes	yes	yes	yes	yes	yes	yes	yes¥	yes¥
Number of observations	2,163	2,163	2,163	2,163	2,163	2,163	1,861	2,163	2,163
Size of the transition group	681	681	681	681	681	681	538	681	681
R^2	0.053	0.052	0.050	0.053	0.051	0.050	0.055	0.053	0.053

Source: PASS 2007-2018.

Note: Standard errors clustered at household level, ***p<0.01, **p<0.05, *p<0.1, †baseline specification, ‡matching on the ISCO major group (0-9) plus controlling for a binary variable indicating any change in ISCO-88 (up to four digits) in the regression (concerns tests II.1 and II.2 only), ¥no matching on work strain, no control for change in work strain (concerns tests II.1 and II.2 only). Control groups balanced on the first and the second moment (tests I.1, II.1, II.2), or on the first moment only (test I.2). For a full list of matching variables see tables A1 to A4.

Table A8: Individual fixed-effects estimation

Depended variable: Life Satisfaction	(1) OLS FE	(2) OLS FE different transfers
Labour market status (ref. regularly employed)		
Subsidized employed ('β')	-0.219***	-0.230***
	(0.049)	(0.050)
Unemployed ('γ')	-0.782***	
	(0.054)	
Unemployed: UB I / no transfers ('γ1')		-0.649***
		(0.080)
Unemployed: UB II / UB I + UB II ('γ2')		-0.814***
		(0.056)
Log equivalent income	0.215***	0.214***
	(0.029)	(0.029)
Savings class	0.021***	0.020***
	(0.007)	(0.007)
Log number of visits to a doctor, last three months	-0.049***	-0.049***
	(0.005)	(0.005)
Log number of close contacts outside household	0.085***	0.085***
	(0.010)	(0.010)
Further controls	yes	yes
Constant	4.740***	4.760***
	(1.109)	(1.110)
R^2	0.039	0.040
Number of persons	13,260	13,260
Number of observations	42,959	42,959

Source: PASS 2007-2018.

Note: *denotes significance at the 10% level, **at the 5% level and ***at the 1% level. Standard errors clustered at household level. Further controls include age, age², marital status, numbers of adults in household, care giving to friends/relatives, children in household / below seven years in household / in different household, stays in hospital last year (yes), disability status.