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Biased by Success and Failure: How Unemployment Shapes Stated Locus of Control

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Abstract

Due to its extraordinary explanatory power for individual behavior, the interest in the concept of locus of control (LOC) has increased substantially within applied economic research. But, even though LOC has been found to affect economic behavior in many ways, the reliability of these findings is at risk as they commonly rely on the assumption that LOC is stable over the life course. While absolute stability has been generally rejected, the extent to which LOC and thus personality changes is, nonetheless, strongly debated. We contribute to this discussion by analyzing the effect of unemployment on LOC. Based on German panel data, we apply a difference-in-difference approach by using an involuntary job loss as trigger for unemployment. Overall, we find a significant shift in stated LOC due to unemployment. Because the effect is observable during unemployment only and not heterogeneous with respect to individual characteristics or unemployment duration, we conclude that only the stated LOC is biased during unemployment but the underlying personality trait itself is not affected.

Keywords: personality, locus of control, unemployment, measurement error
JEL codes: C83, J24, J64, J65

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1 Introduction

Individual beliefs regarding the causal relationship of one's own efforts and its consequences on life have been identified as tremendous explanatory factor for economic behavior. Believing to be the architect of one's own fortune and thus having, by the definition of Rotter (1966), an internal locus of control (LOC) has been shown to positively affect wages (Heineck and Anger, 2010) and human capital investments (Coleman and DeLeire, 2003). Conversely, making fate, bad luck or other people responsible, i.e. having an external LOC, reduces for example job search effort (Caliendo *et al.*, 2015b) and savings (Cobb-Clark *et al.*, 2016).[1] But, these findings share one Achilles' heel: the assumption that LOC is a stable trait.

In empirical economics, LOC and other personality traits are typically considered to be stable over the life course. Shaped by genes, childhood and adolescence, it is argued personality characteristics change only marginally during adulthood and are almost set into stone (McCrae and Costa Jr., 1994; Borghans *et al.*, 2008). Exogenously given, using them as explanatory factor is straightforward as the timing of measurement or endogeneity by reverse causality do not need to be discussed. From an economic perspective, this proposition has been analyzed, but not questioned. In case of LOC, Cobb-Clark and Schurer (2013) find small and economically irrelevant effects of a variety of positive and negative life events on LOC.

An event which has been analyzed by Cobb-Clark and Schurer (2013) only to a limited extent, but has shown to affect individuals in many, heterogeneous ways is unemployment. Depending on its cause, its duration and the affected individual, unemployment deteriorates not only short-term outcomes, but also leaves long-lasting scars (Arulampalam *et al.*, 2001). For this reason, it is crucial for all facets of the individual life-cycle (see Machin and Manning, 1999). In case of LOC, instability due to unemployment would have far-reaching technical and political implications. If unemployment interacts not only with cognitive, but also with non-cognitive skills, considerations about skill deterioration during unemployment need to be updated and findings on the effect of LOC on labor market outcomes would be biased by endogeneity (Cobb-Clark and Schurer, 2013). Additionally, as stated by Heckman (2011, p. 31), policy makers would gain a new '*avenue for intervention and policy*' as the psychological impact of unemployment has been underestimated so far. Gathering specific insights on LOC's stability after job-loss, during unemployment

[1] Additionally, studies have found an significant effect of LOC on entrepreneurial activity (Hansemark, 2003; Caliendo *et al.*, 2014), occupational attainment and advancement (Andrisani, 1977; Cobb-Clark and Tan, 2011; Ahn, 2015; Schnitzlein and Stephani, 2016), maternity leave (Berger and Haywood, 2016), internal migration (Caliendo *et al.*, 2015a), health behavior (Cobb-Clark *et al.*, 2014), parental investment (Lekfuangfu *et al.*, 2017), unemployment duration (Uhlendorff, 2004), selection into performance appraisal (Heywood *et al.*, 2017), job satisfaction (Ng *et al.*, 2006) and employment-related training (Caliendo *et al.*, 2016). See Cobb-Clark (2015) for a comprehensive overview on LOC and labor market outcomes.

and after entering a new job is therefore a critical assessment we discuss in the following study.

Analyzing trait stability, however, has considerable data limitations. Most representative panel datasets include personality questions on a three to six years routine only. Additionally, these questions are a selection of an extensive personality questionnaire only and thus potentially prone to measurement biases (Rammstedt *et al.*, 2010; Rammstedt and Kemper, 2011). Answers may change with individual perception and moods without actually reflecting the underlying personality trait. Falsifying the stability assumption on the basis of such noise would be an unjustified conjecture.

Based on these considerations, we apply a theoretical model which distinguishes between stated and actual LOC. Here, the 'stated locus of control' (SC) is composed by the actual, behavior driving locus of control and a context-specific component. If an event such as being laid off occurs, SC may then change for two reasons. The experience either causes a learning effect, which changes the underlying personality trait permanently, or comes along with a transitory effect on the context-specific component, affecting the measurement of locus of control during unemployment only. Here, state-dependent anchoring effects or coping behavior may put the responsibility of current unemployment into the hands of fate, bad luck or the power of others, without any systematic long-run effects on innate LOC.

In contrast to previous studies, we examine the stability of LOC with focus on these two different channels. For this purpose, we make use of the German Socio-Economic Panel (SOEP, 2016) and apply an empirical reduced-form analysis based on a difference-in-difference approach using involuntary job loss as trigger of unemployment. In order to control for any functional form of selection into dismissal, we rely on a matching procedure called 'Entropy Balancing' (see Hainmueller, 2012) which has several advantages to a linear estimation approach.

Our analysis provides evidence that stated LOC is strongly affected by unemployment. Independent from its cause, its duration, previous experiences and individual characteristics, unemployed individuals report a significant reduction in SC. Nevertheless, this effect vanishes as soon as re-employment is achieved. Because actual personality changes are expected to be persistent and heterogeneous, at least to a small extent, we conclude that these results are caused by a change in the context-specific component during unemployment.

After all, we can only speculate about the psychological nature behind those findings. But, independent from their origins, the results have important methodological implications future research has to be aware of, i.e. measurement of LOC is biased during unemployment. However, our results also make clear that rejecting the stability assumption in general might be an unjustified claim. Achilles' heel may not be as vulnerable as it has been argued.

The outline of the paper is as follows. Section 2 and 3 provide an overview of the relevant literature and introduce theoretical considerations to derive expectations on the following empirical analysis. Section 4 and 5 describe the data and our empirical strategy in detail. The main results are then presented in Section 6. Section 7 discusses the results' sensitivity. The paper concludes in Section 8.

2 Stability of Personality Traits

Arising from genetic disposition and experience from early childhood to adolescence (see e.g. Specht *et al.*, 2013; Dahmann and Anger, 2014; Soto and Tackett, 2015; Peter and Spiess, 2016), personality is defined as *"the relatively enduring patterns of thoughts, feelings, and behaviors that reflect the tendency to respond in certain ways under certain circumstances"* (Roberts, 2009, p.140). But, the extent to which these patterns change during adulthood is still discussed controversially.

Even though perfect consistency of personality has been broadly falsified, the extent to which it varies systematically is still vague.[2] A large strand of psychological literature opposes the assumption of stability by proposing a form of biological maturation, i.e. a development of personality with age (McCrae and Costa Jr, 2008). McCrae and Costa Jr. (1994) find that individuals reach a certain level within the Big Five[3] at the age of 30 and remain relatively stable afterwards. Roberts *et al.* (2006, 2008), Roberts and DelVecchio (2000) as well as Roberts and Mroczek (2008) provide evidence that a variety of personality traits and preferences, e.g. the Big Five, self-confidence and self-control, change with age. Specht *et al.* (2013) confirm an age dependency within LOC.

In addition, a second strand of psychological theories focuses more strongly on the role of environmental sources of change such as individual life events. Those theories assume that personality can be formed by experiences and changing social roles throughout the life course (Roberts *et al.*, 2008; Boyce *et al.*, 2015). In line with this, Cobb-Clark and Schurer (2011, 2013) provide evidence on the impact of a variety of positive and negative life events on LOC and the Big Five. Using Australian panel data, both studies find small effects

[2] As stated by Cobb-Clark and Schurer (2013) and Roberts and DelVecchio (2000), controversial findings in trait development are partly rooted in different concepts of consistency and stability. In general, one can distinguish between absolute and relative stability within a group or an individual (Roberts and Mroczek, 2008). The two relative concepts, *rank-order* and *ipsative* consistency, are typically not considered within empirical economics. The absolute concepts are mean-level and intra-individual consistency. The latter focuses on the personality trait's changes within one individual over time. If those changes can be observed systematically in a sample or group of individuals, also mean-level inconsistency is given as this concept depicts structural changes within the whole population. The following study uses mean-level consistency synonymously for the stability assumption as only structural mean-level changes imply endogeneity issues.

[3] The Five Factor Personality Inventory or short "Big Five" is a psychological concept for capturing individual's personality and is the most widely accepted taxonomy of traits in personality psychology. The concept is based on the assumption that all differences in individual personality can be ascribed to the five personality dimensions Openness, Conscientiousness, Extraversion, Agreeableness and Neuroticism. (Gerlitz and Schupp, 2005; Coleman, 2012)

for some events but conclude that the effect sizes are not sufficient to be economically or empirically relevant. This also holds for the events of being dismissed as well as experiencing unemployment for consecutive years. But, due to data limitations, their analysis is neither able to focus on transitions into (or out of) unemployment nor to look into any kind of heterogeneity. In fact, heterogeneity within unemployment duration may act as driver of personality changes as pointed out by Boyce *et al.* (2015). Using data from the SOEP, they find considerable effects of unemployment and its duration on three of the five considered traits, namely *agreeableness*, *conscientiousness*, and *openness*. However, using the same data, Anger *et al.* (2017) find small effects on *openness* only. Irrespective of different methods, Boyce *et al.* (2015) and Anger *et al.* (2017) diverge in two main aspects which may explain these differences. First, the latter relies on exogenous reasons of job loss, i.e. plant closures, only, allowing to control for potential selection issues. Second, in contrast to Anger *et al.* (2017) and Cobb-Clark and Schurer (2011), Boyce *et al.* (2015) explicitly consider the employment state *during* the personality interview. Our analysis will consider these differences and find the current employment state to be the key factor in changing reported personality.

More specific literature on the relationship between (un-)employment and LOC is rather inconclusive. Gottschalk (2005) reviews the effect of employment on LOC. Using US panel data on single parents, the author finds a significant positive effect of an exogenous increase in the hours worked on LOC. Using a survey of students, Winefield *et al.* (1991) find a significant loss in LOC for young adults when being unemployed or unsatisfied employed. By using the German reunification as exogenous event, Diewald (2007) shows that control beliefs are significantly affected by labor market transitions between a variety of occupational positions. The effect is especially strong in the case of becoming unemployed. Legerski *et al.* (2006) examine the effect of displacement on LOC in a case study of US steelworkers. They find an increase in *Internal Control* while the other two considered dimensions *Powerful Others* and *Chance* do not change significantly.[4] They explain those, what they argue, counterintuitive findings with the fact that the whole reference group is affected by the closure and not only single individuals. Nevertheless, all those studies are largely based on selective and small samples or focus on young individuals who are in general expected to be more volatile in their personality. Given this lack in external validity and representativeness, evidence on the direct impact of job loss and unemployment on LOC is still scarce.

Most recent and closely connected to our study, Infurna *et al.* (2016) present an analysis which focuses on the effect of unemployment shocks on LOC. Using SOEP data from 1994 to 1996, they find that LOC remained relatively stable during unemployment with exemption of women and the low educated. But, the study has limitations. Due to a small

[4]For their analysis, the authors use the three-dimensional LOC scale by Levenson (1981) which consists of three latent variables, namely *Powerful Others*, *Chance* and *Internal Control*.

sample size, Infurna *et al.* (2016) need to rely on voluntary and involuntary unemployment and can thus not unambiguously exclude potential self-selection into unemployment. Due to voluntary job quits, the effect could therefore be underestimated. Additionally, the sample is truncated to unemployment of up to 12 month. Heterogeneity in unemployment intensity, as underlined by Boyce *et al.* (2015), and the persistence of the effect after re-employment can thus not be analyzed. Moreover, due to the small time frame between two LOC interviews in their analysis, their results are at risk of being biased downwards by anticipation. Finally, the years under review have been critical for many employees, especially in East-Germany, since the reunification process caused considerable lay-offs and an all-time peak in unemployment during the '90s. The external validity of the analysis is therefore at stake because parallel events of reunification may have affected individuals in a counteracting way.

3 Theoretical Considerations

One important issue, which has been largely neglected in the debate about consistency in personality so far, is the underlying data used. For a variety of reasons, measurement issues within personality questions might arise. Borghans *et al.* (2008) emphasize that self-reported traits are in general only imperfect proxies for actual traits. Following Rammstedt *et al.* (2010) as well as Rammstedt and Kemper (2011), reported traits may be biased for the low educated in specific as their answers are prone to a variety of response biases, such as the tendency for acquiescence. Golsteyn and Schildberg-Hörisch (2017) discuss potential anchoring effects during personality interviews.

Based on these concerns and considerations of Borghans *et al.* (2008), we allow for a context- and situation-specific component in the measurement of personality traits. In the following, we disband the assumption that stated and actual LOC equal each other at any time. The revealed or 'stated locus of control' (SC) is composed by the actual, behavior driving locus of control (LOC) and a context-specific term ϵ. Following the previous literature, the first is a cumulative function of any past events. It is thus shaped by the *stock* of past experiences. The latter depends on the experiences in the most present period t only. Hence, it is only the *flow* of current events that affect ϵ. With X_t as vector of any experiences between $t-1$ and t and X_0 as inherited genes, the stated locus of control a survey participant reveals is then described by the term

$$SC_t = LOC\left(X_0, ..., X_t\right) + \epsilon\left(X_t\right)$$

with LOC and ϵ as functions of X_t and its predecessors.The effect of a current event X_t on SC can be generally described by the term's derivative

$$dSC_t = \left(\frac{\partial LOC}{\partial X_t} + \frac{\partial \epsilon}{\partial X_t}\right) dX_t.$$

It comprises of two effects. One is the long-lasting, i.e. permanent, effect on the innate LOC. The second effect is the temporary effect that only lasts as long as the experience persists.

As previously discussed, events or experiences in the childhood affect the personality. When adolescence is passed, the exogeneity assumption argues that no event can affect LOC. If $\bar{t}$ defines the end of adolescence, this assumption equates $\partial LOC/\partial X_{t>\bar{t}}$ with zero. Accordingly, the LOC-function simplifies to $LOC(X_0, ..., X_{\bar{t}})$. In contrast and in accordance with the reasoning of Cobb-Clark and Schurer (2013), this must not be the case. Throughout the life cycle, any event might actually lead to a long-lasting update in LOC. Then, $\partial LOC/\partial X_{t>\bar{t}} \neq 0$ can apply, the hypothesis of a stable personality trait has to be rejected and endogeneity concerns due to reverse causality as discussed by Cobb-Clark and Schurer (2013) put any estimation at risk.

Not every event needs to imply $\partial LOC/\partial X_{t>\bar{t}} \neq 0$. The event's impact must be sufficient to cause a learning process. For this reason, this study focuses on displacement and its ensuing unemployment, as it has been shown that these events affect multiple dimensions of the every day life. Besides life satisfaction (Clark and Oswald, 1994), health (Schmitz, 2011; Browning and Heinesen, 2012), fertility decisions (Huttunen and Kellokumpu, 2016), risk aversion (Hetschko and Preuss, 2015) and, strongly related to our study, the Big Five Factors (Boyce *et al.*, 2015) change with displacement and unemployment. Within this model, X_t then represents a set of experiences: an involuntary loss of employment, unemployment and, at least for some individuals, re-employment. All confront individuals with experiences about their ability to control their lives, potentially leading to an update in their own beliefs.

In accordance with the previous literature, we expect that a job loss and ensuing unemployment shift LOC towards externality. A non-intended and involuntary job loss is, by definition, not under control of an individual. Being dismissed is therefore an experience where the employer, not oneself, takes control over a central dimension of the everyday life. In addition, unemployment does not only reduce income. It inhabits unpleasant duties, like writing applications for unemployment aid or jobs and visiting the employment agency on a regular basis. These actions are not chosen at free will, but may be perceived as dictated by a third party, e.g. the government, the society or the family. Hence, unemployed have a reduced set of choices, which can be a challenging experience, giving individuals new insights on their ability to affect life's outcomes. In contrast, successful job search could enhance the belief in oneself and shift LOC in the opposite direction.

However, being unemployed may come along with $\partial \epsilon/\partial X_t \neq 0$. Following the theory of social identity, unemployment is a direct contradiction to the social norm of working, implying a loss in utility (Schöb, 2013; Hetschko *et al.*, 2014). Hence, blaming fate instead of blaming oneself 'outsources' the responsibility of unemployment and may im-

Table 1: LOC questionnaire in the SOEP

Question:	*The following statements apply to different attitudes towards life and the future. To what degree do you personally agree with the following statements?* *Please answer according to the following scale: 1 means disagree completely, and 7 means agree completely.*
Item No.	
I1.	How my life goes depends on me
I2.	Compared to other people, I have not achieved what I deserve
I3.	What a person achieves in life is above all a question of fate or luck
I4.	If a person is socially or politically active, he/she can have an effect on social conditions
I5.	I frequently have the experience that other people have a controlling influence over my life
I6.	One has to work hard in order to succeed
I7.	If I run up against difficulties in life, I often doubt my own abilities
I8.	Opportunities I have in life are determined by the social conditions
I9.	Inborn abilities are more important than any efforts one can make
I10.	I have little control over the things that happen in my life

Source: SOEP 1999, 2005, 2010, 2015.
Notes: SOEP 1999 does include the same questionnaire, but asked the survey participant to rate the statements on a scale from 1 'I agree completely' to 4 'disagree completely'.

prove current individual well-being. LOC is then not only an explaining factor for the ability to cope with negative events (Buddelmeyer and Powdthavee, 2016), but may act as coping channel itself. Adapting ones own belief is then an *active* strategy to manage unemployment and social desirability.

Alternatively, $\partial\epsilon/\partial X_t \neq 0$ can be subconscious and therefore a *passive* action. As the LOC questionnaire focuses in its semantic heavily on the perception of success and failure, being unemployed could be emphasized while filling out the questions (see Table 1). Being asked, for instance, whether '*one has control over the things that happen to oneself*' while being unsuccessful on the labor market, accentuates recent, as failure perceived events. Anchoring (see Furnham and Boo, 2011) may therefore affect stated locus of control.

But, in sharp contrast to $\partial LOC/\partial X_t$, as soon as unemployment is left, there is no reason for coping or anchoring anymore. ϵ falls back on its level before job loss and so should SC. dSC is therefore transitory only and limited to unemployment. Interpreting $dSC = (\partial\epsilon/\partial X_t)dX_t$ as long-lasting change in personality would be an unjustified deduction. Accordingly, endogeneity issues discussed by Cobb-Clark and Schurer (2013) do not resolve because the LOC, meant to be measured in empirical economics, does not change.

But, ϵ may have other, similarly harmful consequences on empirical economics. Two issues can resolve depending on ϵ's nature. First, it could act as survey error, meaning that people behave in correspondence to LOC, but report SC. Neglecting this error will then lead to biased estimates since SC is not comparable between employment states. Accordingly, one needs to measure SC at a point in time which is unaffected by $\epsilon(X_t)$ (e.g. during employment). Alternatively, individuals might always behave in correspondence to their stated locus of control. Then, estimations on decision making will be biased as soon as $\epsilon(X_t)$ during SC measurement deviates from $\epsilon(X_{t+1})$ during decision making. Accordingly, SC and decision making must be measured with the same X_t.

While the change in SC can empirically be identified, the specific explaining channel cannot. Because ϵ_{t+1} does not depend on X_t, effects due to $\partial\epsilon/\partial X_t$ are expected to vanish as soon as their reason, e.g. unemployment, disappears. Non-persistence is however not sufficient to identify $\partial\epsilon/\partial X_t$ as reason for dSC. What cannot be ruled out is the potential counteracting effect of re-employment on LOC, i.e.

$$\frac{\partial LOC}{\partial x_{dismissal}} + \frac{\partial LOC}{\partial x_{unemployment}} = -\frac{\partial LOC}{\partial x_{reemployment}}$$

with $x_{dismissal}$, $x_{unemployment}$ and $x_{reemployment}$ as elements of X_t. Hence, additional evidence is needed.

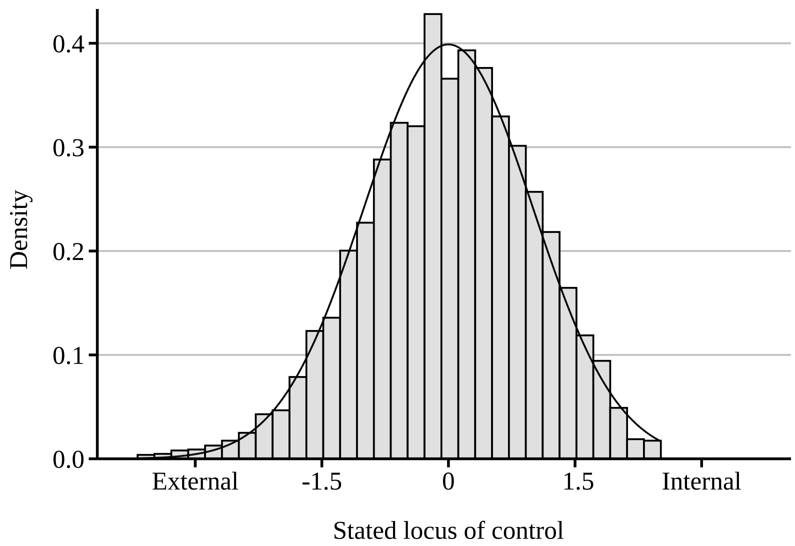
Following Cunha and Heckman (2007), the formation of skills of young individuals underlies strong heterogeneity. Previous experience and intensity of the input determine any changes in cognitive and non-cognitive skills due to complementary and positive, but decreasing effects. Job loss, unemployment and re-employment may cause similar heterogeneous learning for adults. Then, previous unemployment experiences, i.e. $(X_1, ..., X_{t-1})$, and unemployment severity should alter the magnitude of $\partial LOC/\partial X_t$. The first displacement or unemployment experience are expected to come along with the strongest update, while consecutive job losses or unemployment spells should not bring any additional information. Similar arguments hold for unemployment duration. A short unemployment spell should not affect SC to the same amount as a long-term spell. Changes in SC due to $\partial LOC/\partial X_t$ should thus be heterogeneous.

4 Data

For the empirical analysis, we use data from the German Socio-Economic Panel (SOEP). The SOEP is an annual representative household panel study of about 22,000 individuals living in 12,000 households in Germany (see Wagner *et al.*, 2007). Besides manifold information about the socio-economic conditions of individuals and households as well as monthly information about labor force status, the SOEP includes a questionnaire about the individual LOC on a regular basis and is thus ideally suited for our analysis. Our estimation sample covers the waves 1999 to 2015.

Stated Locus of Control In the SOEP waves of 1999, 2005, 2010 and 2015, survey participants have been asked to what degree the list of statements presented in Table 1 can be applied to their own attitudes towards own efforts and life's outcomes. Each individual rates these statements on a Likert scale ranging from 1 ('agree completely') to 4 ('disagree completely') in 1999 and on a 7-point scale ranging from 'disagree completely' (= 1) to 'agree completely' (= 7) in the years 2005, 2010 and 2015. In order to increase the sample

Figure 1: Stated locus of control distribution



Source: SOEP 1999-2015, own calculations.
Notes: Pooled distribution of stated locus of control based on annual factor analysis with one factor. Black line denotes standard normal distribution.

size, the scale of 1999 is harmonized by reversing and stretching individual responses.[5] This procedure preserves the standard deviation, while still allowing to evaluate individual changes.

In line with previous literature (see e.g. Cobb-Clark *et al.*, 2014; Caliendo *et al.*, 2015a), we create a continuous SC variable based on factor analysis loadings, since simple averaging may cause measurement error and attenuation bias (Piatek and Pinger, 2016). To derive robust loadings on one factor, two adjustments within the LOC items are executed. First, several items indicate external beliefs with increasing agreement (see Table 1). These items, i.e. Items 2, 3, 5, 8 and 10, are reversed prior to the factor analysis such that all scales indicate an internal LOC with increasing agreement. Second, we exclude Item 4 and 9 from the analysis. While the first does neither load on an external nor internal factor (see Figure A1a in the Appendix), Item 9's wording cannot be specified unambiguously as internal or external item. Reducing the set of items affects the within validity of our measure positively as Crombach's alpha increases from 0.61 to 0.67. The resulting factor follows a standard normal distribution and ranges from negative (external) to positive (internal) values. Figure 1 shows the resulting distribution of the computed SC variable.

To analyze SC's intra-individual consistency, the difference in SC between two LOC interviews is computed, i.e. $\Delta SC_i = SC_{it} - SC_{it-1}$. Here, $t-1$ represents the previous

[5]More precisely, the recoding from the 4-point-scale to the 7-point-scale is: 1 'agree completely' = 7 'agree completely'; 2 'partly agree' = 5; 3 'disagree partly' = 3; 4 'disagree completely' = 1 'disagree completely'. In 1999, each item on the questionnaire was labeled. From 2005 onwards, only the extremes were labeled.

and t the current LOC interview. As the timing in LOC questionnaires changed from six to five years after 2005, Δ represents a corresponding difference and is measured, due to the standardization, as share of one standard deviation (SD).

Unemployment by Displacement The identification of dismissed individuals is based on annual SOEP data between $t-1$ and t. By relying on the self-reported reason for a job loss, we restrict the analysis to involuntary unemployment, i.e. due to plant closure or displacement by employer. Other reasons which can be identified are potentially voluntary (i.e own resignation, mutual agreement, end of temporary contract, retirement or suspension). Estimations may then be biased by selectivity or reverse causality. Because plant closures are often considered as exogenous reason for a job loss and therefore suited best for the identification of causal effects (see e.g. Kassenboehmer and Haisken-DeNew, 2009; Schmitz, 2011; Marcus, 2013; Hetschko and Preuss, 2015; Anger *et al.*, 2017), the analysis will partly restrict the analysis to this specific group. The relatively infrequent occurrence of plant closures and resulting small sample sizes, however, do not allow for a detailed heterogeneity analysis.

The employment status in t is not restricted to any occupations. Instead, the analysis distinguishes between three employment states in t. First, those who report re-employment (EMP), i.e. part-time, full-time or self-employment, second, registered unemployed (UE), and third, a residuum group of any other states (OS), such as maternity leave, education, marginal employed or non-working. Including any states after dismissal prevents the sample from being biased by selectivity. However, due to its heterogeneous character, 'other states' is not meant to be interpreted. Overall, only the number of displacements is used as restriction, because consecutive job losses potentially correspond to an unusual environment. Individuals reporting more than three job losses between two LOC interviews are excluded from the analysis.

To evaluate shock persistence and heterogeneity by unemployment severity, we extract the time difference between the last job loss and t as well as the unemployment duration (of the last unemployment spell) on a monthly basis. Unfortunately, individuals changing into other states than unemployment or re-employment after job loss do not necessarily report a consistent unemployment duration. Hence, if analysis refers to the time spent in unemployment, individuals within the third group are excluded for precautionary reasons.

Control Group and Overall Sample Restrictions To pursue a difference-in-difference approach, we define a control group of individuals who did not involuntarily lose their job between $t-1$ and t. This group excludes individuals reporting retirement or voluntary unemployment spells of more than three month between $t-1$ and t.

Independent from the affiliation to the treated or controls, all individuals must be regularly employed in $t-1$. The comparison group is thus still employed in t. The restriction on $t-1$ also excludes any irregularly and marginally employed as well as trainees or civil servants from the analysis. Furthermore, the sample is restricted to 25 to 65 year old adults.

To control for selection processes into involuntary job loss, additional control variables are taken from the interview in $t-1$. These information include socio-demographic variables (age, education, martial status, unemployment experience, number of children, gender, region of residence) and job characteristics (wage, tenure, working hours, collar type, firm size, industry). Additionally, information on parallel life events (child birth, divorce, death of spouse, separation, marriage, flat/house move) are derived from all waves between $t-1$ and t. A full list of variables, their mean and standard deviation (separated by dismissed and non-dismissed) is presented in Table A1 and A2 in the Appendix.

Table A3 in the Appendix summarizes the data losses. Overall, 1,452 dismissed individuals and 9,152 non-dismissed are available for the analysis. Of these 1,452 dismissed individuals, 57% report re-employment in t, 25% unemployment and 17% another state.

5 Empirical Strategy

5.1 Estimation Approach

In order to attain average treatment effects on treated (ATT), we need to control for potential selection. For this purpose, OLS regression typically includes a variety of covariates and their squared or interactions terms. But in this case, estimations underlie the assumption of one specific functional form of selection on observables. In advance, Hainmueller (2012) suggests a more generalized approach, named Entropy Balancing (EB).[6] EB is a non-parametric weighting procedure, which allows us to expand the assumption from one specific to any functional form of selection. This is achieved by re-weighting the previously defined control group such that its distribution of observable characteristics matches those of the dismissed, i.e. a *synthetic* control group is build. It relies on those individuals in the control group in particular who resemble the dismissed best, while largely neglecting non-fitting ones. In contrast to other matching procedures, the EB algorithm does not rely on manually iterative estimated propensities. It is therefore less prone to mis-specifications within the choice of balancing covariates. We rely the re-weighting on an extensive list of socio-demographic and job characteristics measured in $t-1$, subsumed as Z_{it-1} (see Table A1).

By assuming that the re-weighted control group resembles the treated if the event of interest would not have occurred, the effect of job loss is estimated by the differences in means, i.e. $E\left(\Delta SC|JL=1, Z_{t-1}\right) - \widehat{E}\left(\Delta SC|JL=0, Z_{t-1}\right)$ with $JL=1$ if an individual experiences a job loss and 0 otherwise. Then, $\hat{E}(\cdot)$ represents the counterfactual change in SC.

[6]We make use of the 'ebalance' Stata package (Hainmueller and Xu, 2013).

When analyzing displacement, however, one needs to account for its ensuing events, i.e. unemployment and re-employment. As proposed by the theoretical consideration, additionally to the potential displacement shock, being unemployed in t may affect SC through a change in the context-specific component ϵ. For this reason, we need to control for the current employment state in t. To achieve this, we separate the group of displaced by their employment status during the interview in t, in the following summarized by S_t. We allow for the states *employed* (EMP), *unemployed* (UE) and *other states* (OS) in t. Then, the expected value of interest expands to $E\left(\Delta SC|JL = 1, S_t, Z_{t-1}\right)$.

Re-weighting makes the treated and control group comparable to one point in time, here the pre-event period $t-1$. Hence, any additional events that could affect SC after $t-1$ are not controlled for so far. For this reason, a simple mean analysis could not be sufficient to yield the ATT. Therefore, an OLS estimation is implemented, which accounts for differences after the time of balancing. We consider the model

$$\Delta SC_i = \alpha_1 + \beta_1' JL_i \times S_{it} + \gamma_1' Shocks_i + \delta_1' Year_i + u_{1i} \tag{1}$$

where $Year_i$ denotes a survey year vector and $Shocks_i$ a vector of parallel life events. Given the interaction of JL_i and S_{it}, β_1 identifies the effect of a job loss in combination with one specific labor market status in t. The average change in SC is denoted by α_1, u_{1i} is the error term. Since all information from Z_{it-1} are included in the estimation through the EB weights, it does not need to be included again. Nonetheless, we also present estimations including Z_{it-1} as covariates instead of weights to test the sensitivity of the EB procedure.

Analyzing the effect of the shock separately by employment status does, however, not rule out the possibility of a systematic difference between the unemployed and re-employed individuals. In a second step, we will therefore analyze the effect of re-employment within the subgroup of unemployed. For this reason, we change the left-hand side variable of our baseline Model (1) into the difference in SC from t to $t+1$. Similarly, ΔSC from $t-2$ to $t-1$ can be used to shed light on the common trend assumption of our difference-in-difference approach.

In order to distinguish persistent and state-dependent effects, a counteracting effect of re-employment has to be ruled out. Following the theoretical considerations, a learning effect on the true LOC is expected to be heterogeneous. For this reason, we analyze the interaction of the effect with a number of characteristics of the shock itself and the individual in order to identify the driving channel behind changes in SC indirectly.

First of all, a learning effect through unemployment may increase with its severity, e.g. duration of unemployment. But, simply interacting JL_i and unemployment duration in the estimation equation may blur the effect, because for unemployed the spell has not ended and may interact with SC differently. To avoid this, a threefold interaction term of JL, S_{it} and *month in unemployment* is added as an extension to the estimation.

Secondly, if the learning effect is driven by the shock (e.g. displacement or re-employment) itself, the effect could diminish over time. To identify this kind of relationship, we introduce another threefold interaction term, namely the interaction of JL_i, S_{it} and the *time since the last job loss shock*. For the employed, the *time since re-employment* can be used additionally.

If T_i is considered as unemployment duration, time since job loss or time since re-employment, Model (1) can be expanded to

$$\Delta SC_i = \alpha_2 + \beta_2' JL_i \times S_{it} + \nu' JL_i \times S_{it} \times T_i + \gamma_2' Shocks_i + \delta_2' Year_i + u_{2i} \qquad (2)$$

where ν describes the effect of one additional month of T_i on ΔSC_i for individual i which experiences a job loss and reports status S_i in t.

5.2 Descriptives and Balancing Results

Before the results of the main estimations are presented in Section 6, the underlying data is examined in more detail to assess the necessity and effectiveness of the balancing procedure. In line with expectations, dismissed individuals and individuals in the unweighted control group are different from each other in multiple aspects. Dismissed individuals are more often male, have spent more years in unemployment, have lower educational levels, report a lower net monthly income and work in smaller firms (see Table A1 in the Appendix). Additionally, dismissed individuals report more frequently parallel life events, such as a divorce or separation (see Table A2 in the Appendix), and the baseline stated control SC_{t-1} of dismissed individuals lies, on average, 0.186 SD below the unweighted control group. Treated individuals thus start off with a lower stated control from the beginning.

Given that the event of involuntary job loss is not randomly distributed, we apply the EB process to make the control group comparable with the treated. Table A1 in the Appendix illustrates one exemplary weighting procedure. Here, the control group is re-arranged such that its socio-demographic and job characteristics match those of the full group of dismissed. As a result, the differences in means between dismissed and non-dismissed individuals is zero for all considered variables after the re-weighting.

Despite re-weighting, SC_{t-1} still differs between the full group of dismissed and its synthetic control group. But, compared to the unweighted scenario, the difference reduces considerably from 0.186 SD to 0.074 SD. The difference in SC_{t-1} is therefore driven to a great extent by observable characteristics. The remaining difference could thus originate from anticipation as the treated might have reduced their SC in fear of a potential job loss early on. To rule out such anticipatory effects on SC_{t-1}, the treated can be restricted to those experiencing the dismissal not earlier than two, three or four years after $t-1$. This does, however, not reduce the differences in SC_{t-1}. The diverging levels may therefore reflect the correlation between LOC on labor market success discussed by the literature: those with a lower LOC are more likely to be dismissed. In principle, SC_{t-1} could also be included in the weighting process in order to reduce the difference in SC_{t-1} manually. But, endogeneity potentially resolves when the balancing is partly based on the dependent variable.[7]

Because each sub-group of $JL \times S_t$ can underlie its own group-specific selection process, the re-weighting process is not only applied for the full group of dismissed. Each sub-group gets its own synthetic control group. Table A4 in the Appendix presents the average SC_{t-1} of a variety of sub-groups and their own synthetic control group. Focusing on one employment state or plant closure as reason for displacement reduces the difference in average SC_{t-1} to a less significant or even insignificant level. Thus, the more specific the analyzed group or the reason of job loss, the better selection can be controlled for. However, focusing on plant closure comes at a cost: the more we address exogeneity, the less representative the results become as plant closure is a rare event, potentially lacking external validity. Dismissals by employer and plant closure are therefore complementary for the analysis.

6 Results

6.1 Mean Analysis

Table 2 summarizes the average ΔSC of the dismissed and its subgroups by labor force status, i.e employed (EMP), unemployed (UE) and other states (OS) as well as the average of the sub-group specific synthetic control group.

On average, dismissed individuals reduce their SC by 0.049 SD (see Column (1)), which is roughly the same magnitude found by Cobb-Clark and Schurer (2013). Controlling for the counterfactual change in SC, the effect of job loss on SC is the difference of average ΔSC between the sub-group of interest and its synthetic control group. For the full sample we observe a highly significant effect of -0.094 SD (see Column (1)). Nevertheless, those reporting employment or any other state in t after experiencing displacement do not adapt their SC differently from their counterpart.[8] On the contrary, unemployment comes along with an effect of -0.357 SD (see Column (3)), which is considerable. Following the estimation of Heineck and Anger (2010), 1.0 SD in SC comes along with an increase of 7.5% in wages. A plain adaptation on our case would therefore implicate that future wages

[7] Including SC_{t-1} in the weighting process (despite these concerns) does not affect the upcoming results. These and other discussed, but not explicitly presented results can be obtained from the authors upon request.

[8] The observed change of SC in the group of *other states* is very heterogeneous. Similar to unemployed, non-working individuals report a significant negative effect while other occupations are associated with no significant changes. However, sample sizes are to small for further analysis.

Table 2: Mean ΔSC of treated and their sub-group specific synthetic control group

	Displacement				Plant Closure only			
	All	EMP_t	UE_t	OS_t	All	EMP_t	UE_t	OS_t
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observations	1,452	841	370	241	496	321	100	75
Treated	-0.049	0.059	-0.307	-0.031	-0.057	0.002	-0.267	-0.029
	(1.099)	(1.033)	(1.145)	(1.184)	(1.082)	(1.021)	(1.162)	(1.201)
Synthetic Control	0.045	0.031	0.051	0.072	0.019	0.011	0.017	0.038
	(1.020)	(1.014)	(1.024)	(1.013)	(1.009)	(0.997)	(1.011)	(1.007)
Difference	-0.094***	0.028	-0.357***	-0.103	-0.076	-0.009	-0.284**	-0.067

Source: SOEP 1999-2015, own calculations.
*Notes: Standard deviation in parenthesis. * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$ denote significance level of the difference from zero. Mean from the synthetic control group results from re-weighting the full control group of 9152 observations such that the descriptives of Table A1 match those of the group specified in the header. The un-weighted mean (standard deviation) of ΔSC within the control group is 0.008 (0.982).*

decrease by 2.8% ceteris paribus due to unemployment's effect on LOC. Nevertheless, as we do not observe the effect after re-employment, this wage calculation is only an instrument for magnitude-interpretation and no likely implication of our identified effects.

The reason of job loss is neglectable for the results. Focusing on the exogenous reason of job loss, i.e. plant closure (see Columns (5) to (8)), confirms the previous observations, although the effect's magnitude slightly decreases. In fact, extending the event of interest from involuntary displacement to any kind of job change (e.g. own resignation, mutual agreement, end of temporary contract and suspension) comes along with equivalent results, too. Thus, the shock itself does not play a role for the effects. The main driver of the identified changes is the employment state during the second LOC interview.

6.2 Regression Analysis

Even though the EB approach controls for selection on observables, the effect of job loss and unemployment may actually originate in parallel life events which could have caused or result from the job loss. In this case, the previous mean analysis would underlie an omitted variable bias. To account for this, Model (1) considers a variety of other life events as additional covariates within a weighted OLS estimation. Additionally, we test whether the set of covariates used for the balancing has an effect on the results.

In the following, the synthetic control group is built for the whole group of dismissed. Selection into employment states in t is therefore neglected at this point. However, applying sub-group specific weights in separate estimations has no effect on the results (see Table A5 in the Appendix). Hence, for the sake of simplicity, Table 3 presents the corresponding estimation results using EB weights for the full group of dismissed.

Table 3: Weighted OLS regression results

	All displacements				Plant closure only	
	(1)	(2)	(3)	(4)	(5)	(6)
Displacement						
× EMP in t	0.054	0.048	0.029	0.025	-0.003	-0.005
	(0.037)	(0.038)	(0.039)	(0.039)	(0.058)	(0.058)
× UE in t	-0.309***	-0.306***	-0.327***	-0.325***	-0.266**	-0.266**
	(0.061)	(0.061)	(0.062)	(0.062)	(0.116)	(0.119)
× OS in t	-0.032	-0.034	-0.055	-0.063	-0.029	-0.030
	(0.077)	(0.077)	(0.078)	(0.078)	(0.138)	(0.135)
Parallel life events						
Child birth				-0.000		-0.035
				(0.051)		(0.078)
Death of spouse				0.217		0.359
				(0.183)		(0.247)
Separation				0.018		0.047
				(0.072)		(0.131)
Divorce				0.111		0.023
				(0.099)		(0.139)
Moved				0.002		-0.123
				(0.068)		(0.103)
Married				0.103*		0.130
				(0.056)		(0.096)
Constant	0.006	-0.012	0.006	-0.012	0.006	-0.048
	(0.019)	(0.026)	(0.028)	(0.030)	(0.019)	(0.042)
Years	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographics		EB	EB	EB		EB
Job characteristics			EB	EB		EB
Observations	10604	10604	10604	10604	9648	9648
Adj. R^2	0.005	0.014	0.013	0.014	0.001	0.010

Source: SOEP 1999-2015, own calculations.
*Notes: Robust standard errors in parentheses with * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Change in standardized control perception determined by factor analysis as dependent variable. Except for Column (1) and (5), where no weighting takes place, a synthetic control group is established by re-weighting the control group such that its descriptives of the with EB marked covariates match those of the group of all displaced (see Column (2), (3) and (4)) or those experiencing a plant closure (see Column (6)).*

In general, the set of covariates used for the balancing procedure does not affect the results. Including time fixed effects only (see Table 3 Column (1)) and adding socio-demographics (Column (2)), job-characteristics (Column (3)) or parallel life events (Column (4)) as controls indicate equivalent results, i.e. those reporting unemployment in t are the only affected. Relying on plant closures does not change the robustness of the results (see Column (5) and (6)). In accordance with Cobb-Clark and Schurer (2013), other parallel life events have only minimal effects on SC (see Column (4) and (6)). Concerns regarding omitted variable biases due to parallel life events can therefore be rejected.

The result's independence from the covariates in the matching procedure is, however, of crucial importance for our identification strategy. Because up to five years can pass between $t-1$ and the job loss event, it is questionable whether $t-1$ is actually suited to make the control group comparable with the treated. But, since the effects are robust and the chosen socio-demographic characteristics are fixed over time to a great extent, the point of measurement is irrelevant to our results. As additional control on this subject, alternative points of measurement have been implemented. Using averages of the covariates in the sense of a Mundlak-Chamberlain-Correction (Mundlak, 1978; Chamberlain, 1984) or shifting the point of measurement anywhere between $t-1$ and t, resolves in equivalent results.

For a general comparison between the EB approach and the linear model, Table A6 in the Appendix presents the estimation results of the standard linear OLS regression including all variables of the re-weighting directly into the model. However, despite the restrictive assumption on the functional form of selection, the linear approach yields equivalent coefficients as the EB approach.

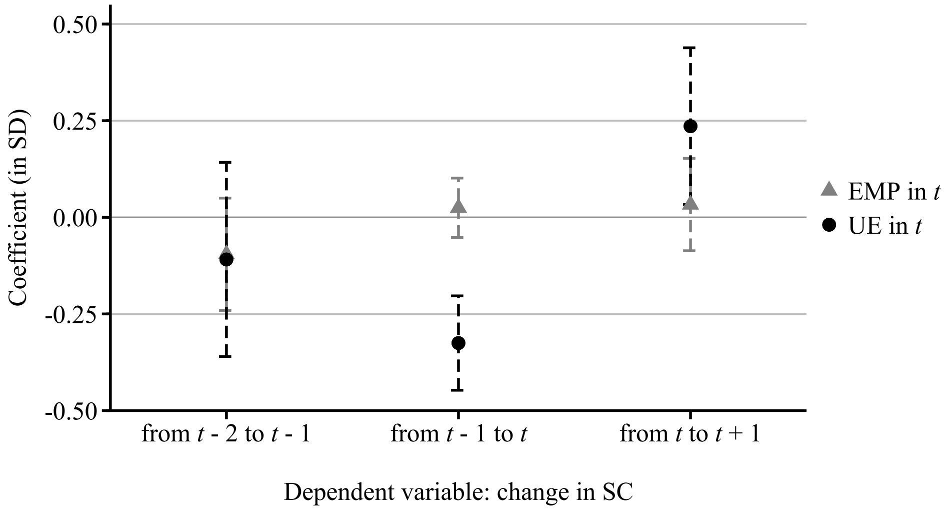
6.3 Re-Employment and Common Trend

Arguing that the effect is limited to unemployment is restricted to the assumption that unemployed individuals will fall back on their baseline SC level as soon as they achieve re-employment. Proof on this subject can only be obtained by analyzing the effect of re-employment within the subgroup of unemployed, as the previous results on the different effects for unemployed and re-employed individuals could also be driven by systematic differences between both groups. For this reason, we change the left-hand side variable of our baseline Model (1) into the difference in SC from t to $t+1$. Similarly, ΔSC from $t-2$ to $t-1$ can be used to shed light on the common trend assumption of our difference-in-difference approach.

The two additional estimations rely on those individuals in our sample, who participated either at the LOC interview before $(t-2)$ or after $(t+1)$ our baseline time frame. Despite restricting for regular employment in $t-2$ or $t+1$, respectively, we do not limit the sample any further. Treated individuals may therefore switch multiple times into unemployment and back again. Balancing is redone for each estimation separately and considers the full set of controls in $t-1$. Figure 2 condensates the estimations to the coefficient of interest and illustrates the results on those treated reporting employment and unemployment in t. For reference, results from the previous estimations are displayed again, i.e. effects of job loss by employment state on ΔSC from $t-1$ to t.

On average, neither the employed nor the unemployed treated diverge significantly from the synthetic control group concerning ΔSC between $t-2$ and $t-1$. Contradictions with the common trend do therefore not apply. As discussed before, SC does not change for the employed treated immediately after the job loss. Only the unemployed treated

Figure 2: Results on anticipation and re-employment



Source: SOEP 1999-2015, own calculations.
Note: Estimated coefficients for those experiencing a job loss between $t-1$ and t and the employment state 'employed in t' (grey) and 'unemployed in t' (black). Whiskers denote 95% confidence interval, based on robust standard errors. Dependent variable is the change in SC within the time frame denoted by x-axis. Estimations from t to $t+1$ (from $t-2$ to $t-1$) include individuals who have been regularly employed in $t+1$ ($t-2$) only. Those reporting unemployment in t are thus re-employed in $t+1$. See Table A7 in the Appendix for full results.

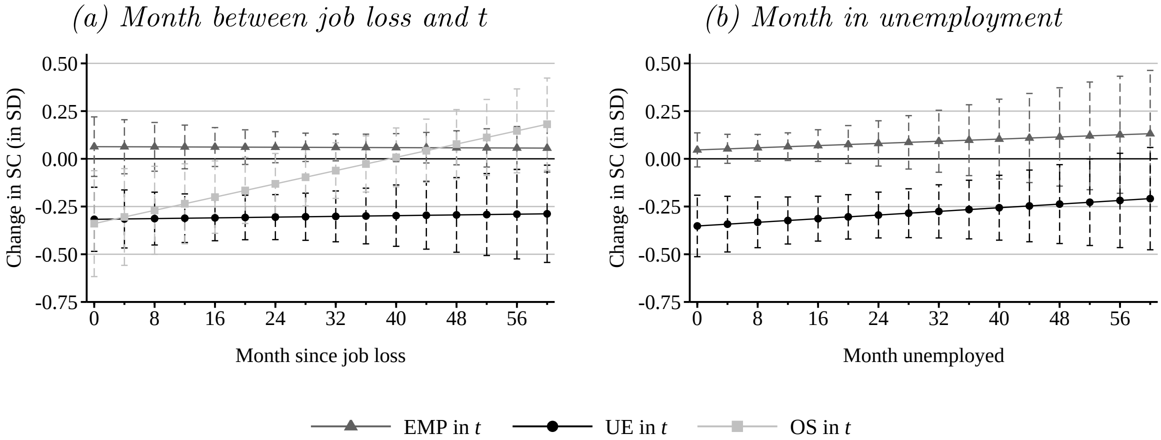
reduce their SC level from $t-1$ to t significantly. But, this effect turns around when re-employment is achieved in $t+1$. From t to $t+1$, SC increases approximately by the same amount it has decreased before. We can therefore conclude that the change in SC has a transitory character only.

6.4 Heterogeneity Analysis

Recalling the theoretical considerations, the change in SC is either rooted in an actual personality update ($\partial LOC/\partial X_t \neq 0$) due to a series of events (displacement, unemployment) or in a context-specific component ($\partial \epsilon/\partial X_t \neq 0$) during the state of unemployment. So far, evidence speaks in favor of the latter channel, as the effect is limited to unemployed. Nevertheless, the evidence is not sufficient, as a true personality change due to unemployment could be compensated by an additional, counteracting effect of re-employment. To shed further light on this, the following section examines heterogeneity within the shock to distinguish between the two potential channels.

Job Loss Timing If the change in SC is purely state-dependent and transitory, we would expect the effect of displacement to be independent from the time since job loss. If the effect of displacement however diminishes over time, the previous results originate from structurally different timing of the event. Displacement could affect the underlying LOC, but those who are already employed in t may have experienced the event earlier, the effect

Figure 3: Change in SC by timing



Source: SOEP 1999-2015, own calculations.
Notes: Whiskers denote 95% confidence interval, based on robust standard errors. All estimations use EB weights based on socio-demographics and job characteristics in $t-1$. Parallel live events and year fixed effects included. Time variable denoted by x-axis. Full results presented in Table A10 in the Appendix.

of the job loss may thus have already vanished. Differences between employment states are then spurious.

In the following, we use the samples variation in timing of dismissal to test whether this applies. Using detailed information about the job loss and the interview date, we introduce an interaction term of the displacement indicator and time since job loss measured in month as proposed by Model (2). Figure 3a displays the predicted change in SC for all considered employment groups in dependence of the time since job loss.

For the groups of unemployed and employed in t, the marginal effect of time since job loss is approximately zero and the overall effect of displacement in interaction with the employment state in t does not change compared to results of Model (1). Similar results are obtained when using polynomial time trends, categorical time variables or a non-weighted, linear estimation approach. The previous results do therefore not originate from a structural different timing of the event.

A different picture is observed within the group of 'other states' in t. Here, we find a significant effect of timing. The closer the job loss, the stronger the effect of displacement. But, the effect originates from the heterogeneity within the group. Similar to unemployed, non-working individuals report a significant negative effect, while individuals in education or marginal employment do not. Because a great share of those reporting 'non-working' inhabit this status only for a limited time after job loss (before transferring back into employment or an equivalent state), a significant interaction can be observed. Therefore, effects are not limited to those registered as unemployed, but also hold for any non-working individual.

Similarly to the variation in time since job loss, using the time since re-employment has no effect on the employed treated either. The effect is therefore detached from any timing in displacement or re-employment. Similar conclusions can be made if this analysis is restricted to those experiencing a plant closure as reason for job loss.

Unemployment Duration Another source of heterogeneity could lie within the shocks' severity. An update in innate personality through displacement and unemployment is expected to be more pronounced if unemployment hits individuals harder. In this case, differences between employment states could resolve from structurally different unemployment duration and not, as previously argued, from the current employment state. On the contrary, $\partial\epsilon/\partial X_t$ implies homogeneity with respect to severity.

The duration of the last unemployment spell is used as first indicator for individual unemployment affectedness. As before, the spell is included in the estimation as summarized by Model (2). Figure 3b presents the corresponding results.[9]

In summary, employed and unemployed in t do not show significant effects in correspondence to time spent in unemployment. Equivalent results can be obtained when the time variable is included as polynomial, in categories or the non-weighting, linear approach is used. Restricting the event of interest to plant closures again yields similar conclusions.

Alternatively to unemployment duration, severity can arise through variation in income loss. But, including the change in income on household level from $t-1$ to t as explanatory variable does not alter the coefficients nor has a significant marginal effect on the change in SC. Separating the sample into two, those with and those without any unemployment experience before, do not indicate differences in ΔSC either (see Table 4, Row 2). Similarly, using the number of unemployment spells for separating the sample into individuals with previous job loss experience does also not yield significant different results (see Table 4, Row 3). The expected heterogeneity of a learning effect (i.e. $\partial LOC/\partial X_t$) can therefore not be identified.

Subsample Analysis Heterogeneity, however, is not limited to timing aspects. It may also arise from the individual socio-economic background. Depending on individual characteristics, job loss and unemployment affects individuals differently, which could translate into varying effects on ΔSC. For this reason, we estimate Model (1) separately for several subsamples of interest. Table 4 presents the corresponding results. We focus on the labor force status 'employed' as well as 'unemployed' in t and consider any kind of displacement. EB weights are estimated for each subsample separately and included in the OLS estimation. Full estimation results are presented in Table A8 and A9 in the Appendix.

[9] Recall that the group of other state in t is omitted in the following, since not all included states allow for a correct identification of month spent in unemployment.

Table 4: Weighted OLS regression results by subsamples

		(1) Sample size EMP / UE / Control	(2) EMP in t		(3) UE in t	
(1)	Full sample	841 / 370 / 9152	0.025	(0.039)	-0.325***	(0.062)
(2.1)	Years unemployed = 0	400 / 162 / 6092	0.040	(0.056)	-0.298***	(0.093)
(2.2)	Years unemployed > 0	441 / 208 / 3060	0.002	(0.056)	-0.354***	(0.084)
(3.1)	Number of UE spells ≤ 2	453 / 140 / 8008	0.007	(0.049)	-0.220**	(0.101)
(3.2)	Number of UE spells > 2	388 / 230 / 1144	0.071	(0.069)	-0.359***	(0.086)
(4.1)	Women	309 / 152 / 4362	0.039	(0.061)	-0.220**	(0.088)
(4.2)	Men	532 / 218 / 4790	0.004	(0.052)	-0.409***	(0.086)
(5.1)	Occ. autonomy ≤ 3	682 / 330 / 6880	0.048	(0.044)	-0.318***	(0.067)
(5.2)	Occ. autonomy > 3	159 / 40 / 2272	-0.077	(0.082)	-0.438**	(0.180)
(6.1)	Age ≤ 46	503 / 169 / 4557	0.054	(0.052)	-0.391***	(0.092)
(6.2)	Age > 46	338 / 201 / 4595	-0.037	(0.062)	-0.274***	(0.085)
(7.1)	Years in educ. ≤ 11.6	477 / 248 / 4678	0.074	(0.054)	-0.324***	(0.082)
(7.2)	Years in educ. > 11.6	364 / 122 / 4474	-0.046	(0.056)	-0.347***	(0.090)
(8.1)	Earnings $\leq 1,400$ €	548 / 274 / 4403	0.029	(0.051)	-0.335***	(0.073)
(8.2)	Earnings $> 1,400$ €	293 / 96 / 4749	0.009	(0.060)	-0.308**	(0.125)
(9.1)	$SC_{it\text{-}1} \leq med(SC_{t\text{-}1})$	454 / 237 / 4469	0.034	(0.051)	-0.341***	(0.069)
(9.2)	$SC_{it\text{-}1} > med(SC_{t\text{-}1})$	387 / 133 / 4683	-0.015	(0.050)	-0.587***	(0.096)

Source: SOEP 1999-2015, own calculations.
*Notes: Robust standard errors in parentheses with * p < 0.1, ** p < 0.05 and *** p < 0.01. Each line presents estimation results of Model (1) by subsamples. Change in standardized stated control perception determined by factor analysis as dependent variable. Full sample estimation equivalent to Column (4) in Table 3. Subsample cutoffs set to sample median in $t-1$. Each estimation uses its own generated EB weights based variables listed in Table A1. For full results see Table A8 and A9 in the Appendix.*

Overall, we do not find any heterogeneity within employment states. Following Table 4, none of the listed subsamples change their level of SC when being re-employed in t, while all subsamples do when they report unemployment. On average, men experience a stronger reduction in SC when unemployed than women (see Row (4)). These differences are in line with previous studies which find stronger effects of unemployment on men. Especially because men experience a stronger identity loss from unemployment (see Hetschko *et al.*, 2014), this heterogeneity is in line with implications of the context-specific component. However, the effects are statistically not different from each other. Loss in identity might also be amplified when a high level of everyday autonomy is lost. But, separating the sample by individually assessed level of occupational autonomy indicates no significant heterogeneity (see Row (5)). Following the results on age dependency of personality traits (see Section 2), the younger are expected to be more volatile in their personality. Within our sample, we observe only small and statistically insignificant differences between age groups (see Row (6)). Almost no variation in effects is observed with respect to education (see Row (7)). Arguing the low educated have worse labor market prospects, a job loss is a greater obstacle to clear. Consequently, the challenge and therefore learning may be greater. However, this is obviously not the case. Similarly, we do not observe any heterogeneity with respect to monthly net earnings in $t-1$ (see Row (8)). Row (9) indicates that individuals with a relatively high SC in $t-1$ react slightly stronger than others. But again, the effects are not statistically different from each other. This effect is potentially caused by the standardization process and its resulting boundaries of SC.

Altogether, we cannot identify a specific group which causes the previous results in particular. The effect is observable for any individual reporting unemployment. Heterogeneity between and within employment states can therefore not be used as explanation for the results. All individuals react similarly to unemployment.

7 Sensitivity

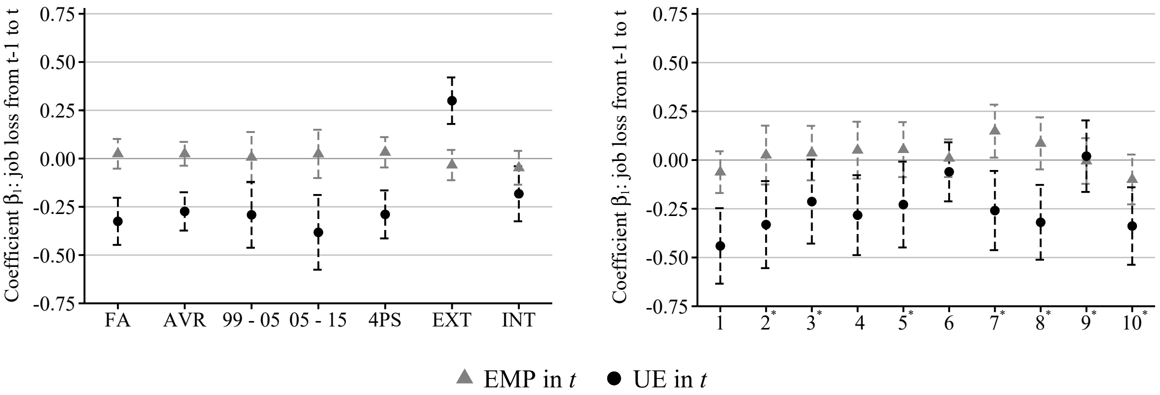
So far, the analysis did not reveal any evidence that SC changes permanently due to a job loss. Following the previous theoretical considerations, the results, thus, stand in favor of a state-specific control perception rather than an updated non-cognitive skill. Whether or not these results are sensitive to our data restrictions will be evaluated in the following section.

Alternative Measures of LOC To see whether the previous results are sensitive to our SC computations, Figure 4a displays a variety of estimations based on alternative definitions. All rely on separated estimations, using EB weights, controls for parallel life events and year fixed effects. Again, Figure 4a reduces the estimations to two coefficients, i.e. displacement in interaction with unemployment or employment in t. For comparison, the basic results from factor analysis (*FA*) are presented first. They correspond to the estimation results from Table 3 Column (4).

Even though factor analysis and standardization come along with several advantages, its process lacks transparency. Non-weighted averages over all questionnaire items are therefore a common alternative to estimate individual level of LOC (see for instance Caliendo *et al.*, 2015b). Using the change in average item response generally replicates our results though (see *AVR* in Figure 4a). Dropping observations from 2006 to 2015 or from 1999 to 2005 is not crucial either. The scale transformation we have implemented for the wave 1999 does not put the validity of the results at stake. Alternatively, instead of rescaling the questionnaire from 1999 upwards, we reduced the 7-point-scale to a 4-point-scale. As indicated by *4PS* in Figure 4a, the results do not change either.

When we focus the factor analysis on one dimension of LOC, i.e. internal or external items, we come to equal conclusions (see Figure 4a *EXT* and *INT*). Answers on external items (six items) increase, which is equivalent to a decrease in SC, while answers on

Figure 4: Estimation results by various LOC computation



Source: SOEP 1999-2015, own calculations.
Notes: Whiskers denote 95% confidence interval, based on robust standard errors. All estimations use EB weights based on socio-demographics and job characteristics in $t-1$*. Parallel life events and year fixed effects included. Dependent variable is the change in the variable denoted by x-axis. Legend Figure 4a: FA corresponds to estimations based on factor analysis (see Table 3 Column (4)). AVR defines SC as average of all LOC items.* $(99-05)$ *and* $(05-15)$ *restrict the sample to the six (1999 to 2005) or five year interval (2005 to 2010 and 2010 to 2015). 4PS transforms all 7-point-scales to a 4-point-scale. EXT and INT are averages of external and internal items. External items have not been reversed. Legend Figure 4b: Number 1 to 10 use change in one item as dependent variable. Underlying question listed in Table 1. * indicates items which are considered for the external dimension of LOC. They have been reversed prior to the analysis such that with increasing consent an internal LOC is represented.*

internal items (two items) decrease. Our general statement, unemployed are the only affected, is thus robust with regards to the SC computations.

Figure 4b goes one step further by using the change in one specific item as dependent variable. The listed item number corresponds to the question stated in Table 1. With few exemptions, dismissed individuals change their answering behavior according to previous results. Individuals who have experienced dismissal and report unemployment in t change their answering behavior in all used items at a 5% significance level, except in Item 6 '*one has to work hard in order to succeed*' and Item 9 '*Inborn abilities are more important than any efforts one can make*'. Altogether, the effect previously discussed is not limited to one specific dimension of the LOC questionnaire. It does manifest itself in almost any question.

The treated who are already employed in t report a significant decrease in one item only, namely '*If I run up against difficulties in life, I often doubt my own abilities*' (Item 7). Hence, a shock does not leave without a trace. Individuals seem to gain confidence by achieving re-employment. But, the change in Item 7 is not sufficient to cause any significant effect on SC in the end.

Placebo Outcomes Unemployment may be accompanied with general changes in mood, which affects the answering behavior overall. In this case, we would misinterpret the systematic change in answering behavior as specific change in SC. We test this hypothesis by looking at other, non-labor related subjective questions (placebo outcomes) and their changes. We make use of questions concerning the frequency of worries about crime, peace, environment and racism. But, we find no evidence that our sample of dismissed and unemployed in t answer non-related questions differently.

Questionnaires Reliability Given that the general answering behavior of unemployed does not change, the LOC questionnaire might not be suited for unemployed, specifically due to its focus on labor market success. Following Rammstedt *et al.* (2010), separated factor analysis can reveal whether questionnaires are generally answered differently between subsamples or not. Factor loadings will then differ between groups. Figure A1b in the Appendix presents the factor loadings of the dismissed (separated by status) and the control group.

All three factor analyses identify two dimensions within the LOC questionnaire, namely external and internal. Additionally, loadings in all three factor analyses load in the same direction and approximately to the same amount. Moreover, the internal validity of SC does not indicate any differences. Crombach's alpha is for the non-dismissed and employed treated around 0.67, while it lies around 0.64 for the treated unemployed. The results do therefore not stem from structurally different answering behavior with respect to the LOC questionnaire.

Interview Effects Individuals may feel obliged by their social role as unemployed to report a stronger external LOC to show that they are not responsible for their situation. In this case, one can expect a stronger change in SC when individuals need to report LOC in a personal interview instead of a self-completion questionnaire, since perceived social desirability may increase in a face-to-face situation (Conti and Pudney, 2011; Chadi, 2013). Within the SOEP, some individuals are still interviewed personally, while others fill out questionnaires by themselves. We use this variation for additional tests, but find no heterogeneity within our sample.

8 Conclusion

The discussion about the stability of personality traits puts any empirical strategy using LOC as explanatory variable in a fragile position. Considerations about labor market success and LOC need to be critically reviewed if the interrelation is not one-sided as commonly assumed.

In summary, we find stated locus of control to be strongly affected by unemployment. Independent from its cause, its duration, previous experience and individual characteristics, the unemployed, on average, perceive to have less control over their own life. But, as soon as re-employment is achieved, SC leaps back to its original level, leaving no trace of the event.

Following our theoretical considerations, this observed change in SC should not be interpreted as change in the underlying personality. It is more likely resulting from approximative personality measurement in surveys. In our case, social desirability or anchoring effects appear to bias answers in a structural manner. In consequence, measurement issues arise during unemployment, putting any estimation at risk which does not control for this issue. Reassuringly, general mean-level consistency is not rejected, i.e. stated locus of control is not affected by a job loss, *past* unemployment spells or re-employment. Considerations about permanent deterioration of non-cognitive skills and endogeneity issues due to reverse causality do thus not apply. However, Cobb-Clark and Schurer (2013) neglect a crucial part of the story by ignoring the current employment state. In fact, differentiating between past and current unemployment spells could explain controversial results on the stability of the Big Five (Boyce *et al.*, 2015; Anger *et al.*, 2017).

After all, we can only speculate about the origins of our findings. The effect is independent from the reason of unemployment, i.e. voluntary or involuntary job loss, and does also occur for non-employed individuals. The status of being registered as unemployed and receiving unemployment benefits is therefore not decisive. Furthermore, a face-to-face interview does not interact with the results either. Effects from social desirability are, however, expected to vary between individuals with respect to their social role and the amount of cognitive dissonance (Schöb, 2013; Hetschko *et al.*, 2014). Only marginally stronger effects for men point in this direction. Hence, altering ones own beliefs seems not to be an active coping strategy to shift the responsibility of unemployment to other sources, namely fate, bad luck or other people. The results are more likely to arise from passive actions. Not being employed while ranking the statements on locus of control potentially biases the answers towards the inability to make one's own living. Anchoring effects may therefore explain our findings best.

Unfortunately, we cannot answer the question whether the presented effects manifest themselves on individual behavior or not. Both cases have, however, contradictory implications on empirical economics. When the effect is a survey bias, it will not have any effects on actual behavior and SC is comparable between equal employment states only. On the contrary, when individuals act always in correspondence to their stated locus of control, decision making should only be analyzed using the corresponding state-specific SC. Analyzing the nature of the effects is, however, beneath the scope of this study as it is hardly feasible to extract changing behavior in independence of unemployment.

Overall, independent from their origins and behavioral implications, our findings have important methodological implications for any further research which uses locus of control as explanatory variable in economic decision-making models. It becomes clear that although underlying LOC might actually be stable, SC is only an insufficient proxy during unemployment or other periods of incisive life-time events. The personality questions do therefore not underlie a general instability problem but they imply a volatile component future research needs to be aware of.

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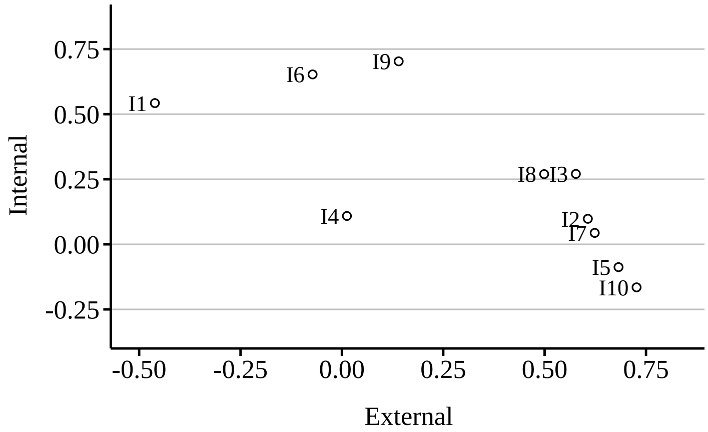
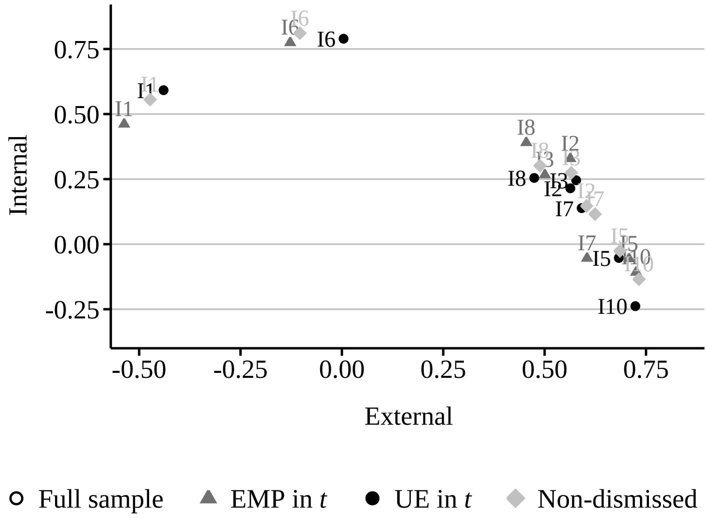
Appendix

Figure A1: Factor loadings

(a) Full sample in $t-1$



(b) By treatment in t



Source: SOEP 1999-2015, own calculations.
Notes: Factor analysis with two forced factors. Labels I1 to I10 correspond to Table 1. Principal-component factor method used. Rotation by 'orthogonal varimax'. Figure A1b does not include Item 4 and 9.

Table A1: Descriptive Statistics

	(A1.1) Dismissed mean/share	(A1.2) Non-dismissed mean/share un-weighted	Diff. (1) - (2)	(A1.3) Non-dismissed mean/share weighted	Diff. (1) - (3)
Socio-demographics at t-1					
Age	46.901	47.150	-0.250	46.917	-0.016
	(10.030)	(9.101)		(10.029)	
Years unemployed	0.959	0.448	0.511***	0.959	0.000
	(1.522)	(1.017)		(1.522)	
Number of children	0.653	0.647	0.006	0.653	0.000
	(0.909)	(0.909)		(0.909)	
Female	0.416	0.477	-0.061***	0.416	0.000
Married	0.615	0.643	-0.028**	0.615	0.000
East	0.369	0.271	0.099***	0.369	0.000
School degree					
No degree	0.028	0.015	0.013***	0.028	0.000
Sec. school degree	0.304	0.245	0.058***	0.304	0.000
Interm. school degree	0.423	0.395	0.028**	0.423	0.000
Upper sec. degree	0.176	0.293	-0.118***	0.176	0.000
Other degree	0.070	0.052	0.018**	0.070	0.000
Apprenticeship	0.577	0.511	0.066***	0.577	0.000
University degree	0.338	0.452	-0.114***	0.338	0.000
Job characteristics at t-1					
Net monthly wage	1249.512	1627.176	-377.663***	1249.230	0.283
	(697.287)	(1011.326)		(610.737)	
Tenure	6.944	10.782	-3.838***	6.946	-0.002
	(8.085)	(8.821)		(8.086)	
Full time	0.804	0.793	0.011	0.804	0.000
Part time	0.196	0.207	-0.011	0.196	0.000
Blue collar worker	0.493	0.309	0.184***	0.493	0.000
White collar worker	0.507	0.691	-0.184***	0.507	0.000
Small firm size	0.397	0.204	0.193***	0.397	0.000
Medium firm size	0.317	0.293	0.025*	0.318	0.000
Large firm size	0.237	0.466	-0.229***	0.237	0.000
Industry					
Manufacturing	0.114	0.129	-0.016*	0.114	0.000
Agriculture	0.024	0.010	0.014***	0.024	0.000
Mining	0.059	0.074	-0.016**	0.059	0.000
Chemicals	0.055	0.063	-0.008	0.055	0.000
Construction	0.134	0.052	0.081***	0.134	0.000
Textile	0.014	0.006	0.008***	0.014	0.000
Retail	0.171	0.114	0.057***	0.172	0.000
Transport	0.047	0.049	-0.002	0.047	0.000
Public Service	0.124	0.274	-0.150***	0.124	0.000
Private Service	0.116	0.118	-0.002	0.116	0.000
Other	0.143	0.110	0.033***	0.143	0.000
Observations	1452	9152		9152	

Source: SOEP 1999-2015, own calculations.
Notes: Significance level of the test on the difference from zero denoted by * $p < 0.1$, ** $p < 0.05$ *and* *** $p < 0.01$. *Standard deviation in parenthesis.*

Table A2: Descriptive Statistics - parallel life events

	(A2.1)	(A2.2)		(A2.3)	
	Dismissed	Non-dismissed		Non-dismissed	
	share	share un-weighted	Diff. (1) - (2)	share weighted	Diff. (1) - (3)
Child birth	0.121	0.123	-0.002	0.124	-0.002
Death of spouse	0.013	0.006	0.007**	0.004	0.009***
Separation	0.107	0.072	0.035***	0.069	0.037***
Divorce	0.039	0.028	0.011**	0.024	0.015***
Moved	0.088	0.072	0.016***	0.073	0.016*
Married	0.099	0.098	0.001	0.100	-0.001
Observations	1452	9152		9152	

Source: SOEP 1999-2015, own calculations.
Notes: See Table A1 for notes.

Table A3: Sample dropouts by groups and period

	By group		By period			
	Dismissed	Non-Dismissed	2005	2010	2015	Total
Observations reporting ΔSC_i	2,570	20,384	7,218	9,183	6,553	22,954
- Not employed in $t-1$	-673	-6,209	-2,198	-2,873	-1,811	-6,882
- Not regularly employed in $t-1$	-378	-3,618	-1,134	-1,630	-1,232	-3,996
- More than three month unemployed	0	-1,216	-462	-434	-320	-1,216
- Missing time information	-33	0	-8	-12	-13	-33
- More than three displacements	-10	0	-9	-1	0	-10
- Missing variables	-24	-189	-61	-71	-81	-213
Final sample	1,452	9,152	3,346	4,162	3,096	10,604

Source: SOEP 1999-2015.

Table A4: Mean SC in $t-1$ of treated and their sub-group specific synthetic control group

	Displacement				Plant Closure only			
	All (A4.1)	EMP_t (A4.2)	UE_t (A4.3)	OS_t (A4.4)	All (A4.5)	EMP_t (A4.6)	UE_t (A4.7)	OS_t (A4.8)
Observations	1,452	841	370	241	496	321	100	75
Treated	-0.161	-0.108	-0.251	-0.206	-0.098	-0.041	-0.193	-0.215
	(1.028)	(1.025)	(0.995)	(1.078)	(1.010)	(1.007)	(1.024)	(0.999)
Synthetic Control	-0.087	-0.032	-0.144	-0.171	-0.047	0.007	-0.138	-0.118
	(1.008)	(1.001)	(1.006)	(1.011)	(1.010)	(0.999)	(1.006)	(1.018)
Difference	-0.074**	-0.076*	-0.107*	-0.035	-0.051	-0.049	-0.055	-0.097

Source: SOEP 1999-2015, own calculations.
*Notes: Standard deviation in parenthesis. * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$ denote significance level of the difference from zero. Mean from the synthetic control group results from re-weighting the full control group of 9152 observations such that the descriptives of Table A1 match those of the group specified in the header. The un-weighted mean (standard deviation) of SC_{t-1} within the control group is 0.025 (0.993).*

Table A5: Weighted OLS regression results with sub-group specific weights

	All displacements			Plant closure only		
	(1)	(2)	(3)	(4)	(5)	(6)
Displacement						
× EMP in t	0.024			-0.011		
	(0.039)			(0.058)		
× UE in t		-0.327***			-0.270**	
		(0.065)			(0.120)	
× OS in t			-0.049			0.010
			(0.082)			(0.131)
Parallel life events						
Child birth	0.034	-0.017	-0.132	-0.007	0.067	-0.129
	(0.057)	(0.107)	(0.127)	(0.079)	(0.244)	(0.228)
Death of spouse	0.177	-0.209	0.512	0.342***	0.019	1.322***
	(0.198)	(0.318)	(0.375)	(0.124)	(0.286)	(0.333)
Separation	0.123	-0.066	-0.355**	0.169	-0.186	-0.056
	(0.081)	(0.160)	(0.180)	(0.136)	(0.370)	(0.398)
Divorce	0.087	-0.121	0.646***	-0.051	0.075	0.025
	(0.116)	(0.204)	(0.215)	(0.177)	(0.179)	(0.292)
Moved	0.005	0.037	-0.006	-0.033	-0.018	-0.611**
	(0.073)	(0.164)	(0.144)	(0.104)	(0.223)	(0.300)
Married	0.044	0.260**	0.190	0.048	0.426	0.456**
	(0.062)	(0.132)	(0.135)	(0.104)	(0.350)	(0.216)
Constant	-0.027	0.013	0.004	-0.052	0.015	-0.183
	(0.034)	(0.051)	(0.060)	(0.047)	(0.083)	(0.122)
Years	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographics	EB	EB	EB	EB	EB	EB
Job characteristics	EB	EB	EB	EB	EB	EB
Observations	9993	9522	9393	9473	9252	9227

Source: SOEP 1999-2015, own calculations.
*Notes: Robust standard errors in parentheses with * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Change in standardized control perception determined by factor analysis as dependent variable. A synthetic control group is established by re-weighting the control group such that its descriptives of the with EB marked covariates match those of the sub-group of displaced denoted by second row. First row denotes reason for job loss.*

Table A6: Non-weighted OLS estimations by labor force status in t

	(A6.1)		(A6.2)		(A6.3) Plant Closure	
Displacement						
$\times$ EMP in t	0.046	(0.038)	0.042	(0.038)	-0.009	(0.059)
$\times$ UE in t	-0.331***	(0.061)	-0.331***	(0.061)	-0.280**	(0.117)
$\times$ OS in t	-0.072	(0.077)	-0.072	(0.077)	-0.049	(0.140)
Socio-demographics in $t-1$						
Age	-0.039***	(0.011)	-0.042***	(0.012)	-0.033***	(0.012)
Age squared	0.000***	(0.000)	0.000***	(0.000)	0.000***	(0.000)
Years unemployed	0.018*	(0.010)	0.018*	(0.010)	0.018	(0.011)
Number of children	-0.000	(0.012)	-0.000	(0.012)	-0.002	(0.013)
Female	0.019	(0.020)	0.000	(0.026)	-0.007	(0.027)
Married	-0.034	(0.024)	-0.030	(0.025)	-0.028	(0.027)
East	-0.033	(0.023)	-0.030	(0.024)	-0.034	(0.025)
School degree (reference: intermediate)						
No degree	0.052	(0.093)	0.057	(0.094)	0.015	(0.099)
Sec. school degree	0.021	(0.028)	0.025	(0.029)	0.016	(0.030)
Other degree	0.011	(0.051)	0.011	(0.053)	0.018	(0.055)
Upper sec. degree	0.013	(0.025)	0.015	(0.026)	0.024	(0.027)
Apprenticeship	0.005	(0.028)	0.003	(0.029)	-0.005	(0.029)
University degree	-0.025	(0.030)	-0.024	(0.031)	-0.031	(0.033)
Job-characteristics in $t-1$						
Net monthly wage (euros)/1000			-0.015	(0.013)	-0.020	(0.014)
Tenure			0.000	(0.001)	-0.000	(0.001)
Part time			0.007	(0.031)	-0.013	(0.032)
White collar worker			0.020	(0.028)	0.013	(0.029)
Medium firm size			0.023	(0.028)	0.028	(0.029)
Large firm size			0.009	(0.027)	0.008	(0.028)
Industry (reference: manufacturing)						
Agriculture			-0.016	(0.097)	-0.016	(0.110)
Mining			-0.041	(0.045)	-0.042	(0.047)
Chemicals			0.023	(0.048)	0.023	(0.049)
Construction			0.001	(0.051)	-0.004	(0.055)
Textile			-0.012	(0.100)	0.049	(0.111)
Retail			-0.035	(0.042)	-0.030	(0.043)
Transport			-0.072	(0.050)	-0.050	(0.052)
Public Service			-0.027	(0.036)	-0.027	(0.037)
Private Service			0.017	(0.040)	0.031	(0.042)
Other			-0.097**	(0.041)	-0.083*	(0.043)
Parallel life events						
Child birth			-0.031	(0.033)	-0.037	(0.035)
Death of spouse			0.154	(0.119)	0.161	(0.128)
Separation			-0.024	(0.041)	-0.043	(0.043)
Divorce			0.099*	(0.056)	0.079	(0.058)
Moved			-0.003	(0.041)	0.005	(0.043)
Married			0.012	(0.037)	0.000	(0.039)
Constant	0.933***	(0.256)	1.041***	(0.268)	0.870***	(0.285)
Years	Yes		Yes		Yes	
Observations	10604		10604		9648	
Adj. R^2	0.007		0.007		0.002	

Source: SOEP 1999-2015, own calculations.
*Notes: Robust standard errors in parentheses with * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Change in SC determined by factor analysis as dependent variable. The reference group inhabits full time employment as blue collar worker in a small firm, has an intermediate school degree, is male, not married and is employed within the manufacturing sector.*

Table A7: Anticipation and reversion

Change in SC from	(A7.1) $t=-2$ to $t=-1$		(A7.2) $t=0$ to $t=1$	
Displacement between $t-1$ and t				
× EMP in t	-0.096	(0.074)	0.033	(0.061)
× UE in t	-0.109	(0.128)	0.236**	(0.103)
× OS in t	-0.227	(0.179)	0.104	(0.242)
Parallel life events				
Child birth	0.019	(0.093)	-0.096	(0.109)
Death of spouse	0.794***	(0.251)	-0.066	(0.169)
Separation	0.116	(0.118)	-0.049	(0.120)
Divorce	-0.023	(0.138)	0.242	(0.161)
Moved	0.163	(0.121)	-0.194	(0.141)
Married	-0.002	(0.099)	0.048	(0.144)
Constant	-0.058	(0.043)	-0.069*	(0.039)
Years	Yes		Yes	
Socio-demographics at $t-1$	EB		EB	
Job characteristics at $t-1$	EB		EB	
Observations	3804		3987	
Adj. R^2	0.009		0.009	

Source: SOEP 1999-2015, own calculations.
*Notes: Robust standard errors in parentheses with * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Difference in standardized control perception determined by factor analysis as dependent variable. Considered time frame defined by first row. Parallel life events during the same period as defined in first row. Specification (A7.1) and (A7.2) consider regular employment in $t = -2$ or $t = 1$, respectively.*

Table A8: *Weighted OLS estimations by subsamples*

	(A8.1) Years UE		(A8.2) #UE spells		(A8.3) Gender		(A8.4) Occ. Auton.	
	Low	High	Low	High	Female	Male	Low	High
Displacement								
× EMP in t	0.040	0.002	0.007	0.071	0.039	0.004	0.048	-0.077
	(0.056)	(0.056)	(0.049)	(0.069)	(0.061)	(0.052)	(0.044)	(0.082)
× UE in t	-0.298***	-0.354***	-0.220**	-0.359***	-0.220**	-0.409***	-0.318***	-0.438**
	(0.093)	(0.084)	(0.101)	(0.086)	(0.088)	(0.086)	(0.067)	(0.180)
× OS in t	0.037	-0.180	0.085	-0.214*	0.090	-0.278*	-0.088	0.077
	(0.102)	(0.117)	(0.102)	(0.123)	(0.085)	(0.142)	(0.083)	(0.230)
Parallel life events								
Child birth	-0.015	-0.003	-0.012	0.007	-0.082	0.034	-0.005	0.042
	(0.068)	(0.074)	(0.063)	(0.089)	(0.075)	(0.068)	(0.057)	(0.110)
Death of spouse	0.381*	0.149	0.385**	0.087	0.140	0.505	0.184	0.452***
	(0.208)	(0.263)	(0.185)	(0.298)	(0.176)	(0.517)	(0.209)	(0.149)
Separation	-0.005	0.046	0.130	-0.080	-0.000	0.020	-0.024	0.259
	(0.110)	(0.098)	(0.099)	(0.116)	(0.084)	(0.117)	(0.082)	(0.161)
Divorce	0.200	0.050	0.140	0.105	0.069	0.147	0.154	-0.179
	(0.156)	(0.122)	(0.147)	(0.145)	(0.120)	(0.158)	(0.106)	(0.241)
Moved	-0.052	0.027	-0.008	-0.045	-0.119	0.101	0.018	-0.060
	(0.093)	(0.099)	(0.080)	(0.120)	(0.086)	(0.101)	(0.076)	(0.139)
Married	0.002	0.195**	0.101	0.117	0.130*	0.083	0.140**	-0.132
	(0.077)	(0.082)	(0.071)	(0.100)	(0.075)	(0.080)	(0.063)	(0.103)
Constant	0.063	-0.078*	0.018	-0.059	0.010	-0.013	-0.005	-0.061
	(0.040)	(0.046)	(0.037)	(0.059)	(0.041)	(0.042)	(0.033)	(0.067)
Years	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographics	EB	EB	EB	EB	EB	EB	EB	EB
Job characteristics	EB	EB	EB	EB	EB	EB	EB	EB
Shocks	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6773	3831	8735	1869	4966	5638	8113	2491
Adj. R^2	0.014	0.024	0.010	0.023	0.008	0.025	0.016	0.024

Source: SOEP 1999-2015, own calculations.

Notes: Robust standard errors in parentheses with * $p < 0.1$, ** $p < 0.05$ *and* *** $p < 0.01$. *Change in standardized control perception as dependent variable. Cutoffs of the separated estimations set to the sample median in* $t-1$ *(low* $\leq$ *median): years unemployed (0 years), number of unemployment spells (2, median within treatment group) and occupation autonomy (level 3 of 5). For each estimation EB was conducted separately.*

Table A9: Weighted OLS estimations by subsamples (cont.)

	(A9.1) Age		(A9.2) Years in educ.		(A9.3) Month. earnings		(A9.4) SC_{t-1}	
	Low	High	Low	High	Low	High	Low	High
Displacement								
× EMP in t	0.054	-0.037	0.074	-0.046	0.029	0.009	0.034	-0.015
	(0.052)	(0.062)	(0.054)	(0.056)	(0.051)	(0.060)	(0.051)	(0.050)
× UE in t	-0.391***	-0.274***	-0.324***	-0.347***	-0.335***	-0.308**	-0.341***	-0.587***
	(0.092)	(0.085)	(0.082)	(0.090)	(0.073)	(0.125)	(0.069)	(0.096)
× OS in t	-0.239**	0.014	-0.069	-0.066	-0.083	-0.034	0.040	-0.354***
	(0.108)	(0.104)	(0.097)	(0.122)	(0.089)	(0.167)	(0.086)	(0.110)
Parallel life events								
Child birth	0.002	0.454	-0.006	-0.004	-0.003	0.029	-0.032	0.059
	(0.055)	(0.367)	(0.072)	(0.068)	(0.067)	(0.077)	(0.069)	(0.065)
Death of spouse	-0.063	0.295	0.407*	-0.260	0.275	0.083	0.010	0.630**
	(0.336)	(0.212)	(0.209)	(0.321)	(0.200)	(0.437)	(0.222)	(0.282)
Separation	0.046	-0.059	-0.074	0.126	0.016	0.029	0.096	-0.040
	(0.084)	(0.139)	(0.104)	(0.098)	(0.090)	(0.127)	(0.082)	(0.103)
Divorce	0.243**	-0.141	0.065	0.177	0.021	0.359**	-0.015	0.167
	(0.122)	(0.156)	(0.149)	(0.127)	(0.121)	(0.177)	(0.118)	(0.138)
Moved	-0.033	0.091	0.101	-0.118	-0.019	0.021	-0.010	0.060
	(0.075)	(0.168)	(0.099)	(0.085)	(0.083)	(0.118)	(0.091)	(0.087)
Married	0.071	0.233*	0.151*	0.049	0.135*	0.050	0.135*	0.149**
	(0.062)	(0.136)	(0.085)	(0.070)	(0.074)	(0.089)	(0.079)	(0.067)
Constant	-0.010	-0.013	-0.000	-0.033	-0.015	0.008	0.386***	-0.437***
	(0.042)	(0.046)	(0.040)	(0.043)	(0.038)	(0.052)	(0.038)	(0.040)
Years	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographics	EB	EB	EB	EB	EB	EB	EB	EB
Job characteristics	EB	EB	EB	EB	EB	EB	EB	EB
Shocks	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5313	5291	5579	5025	5413	5191	5302	5302
Adj. R^2	0.021	0.015	0.018	0.016	0.019	0.013	0.021	0.045

Source: SOEP 1999-2015, own calculations.
*Notes: Robust standard errors in parentheses with * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Change in standardized control perception as dependent variable. Cutoffs of the separated estimations set to the sample median in $t-1$ (low $\leq$ median): Age (46 years), years in education (11.5 years), monthly earnings (1,400 €) and perception to control (.047). For each estimation EB was conducted separately.*

Table A10: OLS estimations by time variation

	(A10.1) Time since job loss		(A10.2) Time in unemployment	
Displacement× EMP in t	0.029	(0.081)	0.012	(0.049)
× time	-0.000	(0.002)	0.001	(0.003)
Displacement× UE in t	-0.335***	(0.088)	-0.370***	(0.084)
× time	0.000	(0.003)	0.002	(0.003)
Displacement× OS in t	-0.371***	(0.142)		
× time	0.009**	(0.004)		
Parallel life events				
Child birth	-0.003	(0.051)	0.020	(0.052)
Death of spouse	0.210	(0.182)	0.040	(0.169)
Separation	0.020	(0.072)	0.064	(0.073)
Divorce	0.104	(0.098)	0.019	(0.100)
Moved	0.002	(0.068)	0.017	(0.070)
Married	0.107*	(0.056)	0.096*	(0.058)
Constant	-0.014	(0.030)	-0.016	(0.030)
Years	Yes		Yes	
Socio-demographics	EB		EB	
Job characteristics	EB		EB	
Observations	10604		10363	
Adj. R^2	0.016		0.015	

Source: SOEP 1999-2015, own calculations.

*Notes: Robust standard errors in parentheses with * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Change in standardized control perception determined by factor analysis as dependent variable.*