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Abstract

Although unemployment likely entails various externalities, research examining its spillover effects on spouses is scarce. This is the first paper to estimate effects of unemployment on the smoking behavior of both spouses. Using German Socio-Economic Panel data, we combine matching and difference-in-differences estimation, employing the post-double-selection method for control-variable selection via Lasso regressions. One spouse's unemployment increases both spouses' smoking probability and intensity. Smoking relapses and decreased smoking cessation drive the effects. Effects are stronger if the partner already smokes and if the male partner becomes unemployed. Of several mechanisms discussed, we identify smoking to cope with stress as relevant.

Keywords: smoking, risky health behaviors, unemployment, job loss, spillover effects, post-double-selection method

JEL classification: I12, J63, J65, C23

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1 Introduction

When an individual becomes unemployed, it likely also affects their spouse in various ways. For instance, unemployment decreases income (Jacobson et al. 1993; Eliason and Storrie 2006; Hijzen et al. 2010) and can result in social isolation (Kunze and Suppa 2017a), poorer mental health (Marcus 2013; Schaller and Stevens 2015), decreased life satisfaction (Kassenboehmer and Haisken-DeNew 2009), and changed health behaviors (Gallo et al. 2001; Deb et al. 2011; Marcus 2014; Golden and Perreira 2015) – all of which may affect their spouse.

However, surprisingly little is known about how spouses are affected by unemployment. Some evidence suggests that spousal life satisfaction decreases (Winkelmann and Winkelmann 1995; Luhmann et al. 2014), divorce rates increase (Charles and Stephens 2004), spousal labor force participation rates increase (Stephens 2002), and spousal social activities decrease (Kunze and Suppa 2017b). Further, spousal mental health decreases (Clark 2003; Marcus 2013; Mendolia 2014; Bubonya et al. 2017). However, to our knowledge, no study examines the spousal spillover effects of unemployment on risky health behaviors, in general, and cigarette smoking, in particular, one of the leading causes of preventable deaths (World Health Organization 2012).

This study examines the causal effect of unemployment on spousal smoking behavior. For this purpose, we focus on arguably exogenous entries into unemployment (including plant closures and lay-offs), combining difference-in-differences (DiD) estimation with a matching strategy based on entropy balancing. For selecting control variables, we complement our econometric approach with the post-double-selection method, a machine learning tool based on Lasso regressions (Belloni et al. 2014a; 2014b). This data-informed technique allows us to also consider interactions and higher order polynomials of the control variables.

Using rich German Socio-Economic Panel (SOEP) data, we look at married and unmarried cohabiting couples approximately one year after the job loss. Similar to own unemployment, spousal unemployment increases the probability and intensity of smoking, both increasing the daily number of cigarettes smoked by 8 percent. Further, the probability of smoking increases 2-4 percentage points with either own or spousal unemployment. These estimates translate into an increase in the smoking prevalence of approximately 7-11 percent. The effects of spousal unemployment are generally slightly smaller than that of own unemployment. While smoking increases among both men and women when they enter unemployment themselves, the spousal spillover effects are driven by male unemployment. The smoking effects of own and spousal unemployment are mostly driven by individuals whose partner smoked prior to unemployment. The results further highlight that increased smoking initiation is mainly driven by smoking relapses among former smokers.

Our study contributes to two branches of health economics literature, namely that of intra-household spillover effects of major life events and that on unemployment consequences. The former literature shows that specific events in the life of one individual also impact their spouse.[1]

[1] Other studies examine spillover effects on children (e.g., Lindo 2011; Black et al. 2016).

Most prominently, an individual's death strongly affects spousal health as it increases the risk of depression (Lindeboom et al. 2002; Siflinger 2017), leads to longer hospital stays (Tseng et al. 2018), and decreases life expectancy (van den Berg et al. 2011). Other studies investigate the health consequences of spousal retirement (Bertoni and Brunello 2017; Müller and Shaikh 2018). Several studies document that job loss and unemployment decrease spousal mental health (Clark 2003; Marcus 2013; Mendolia 2014; Bubonya et al. 2017) and that the fear of unemployment also reduces spousal mental health (Bünnings et al. 2017). All these studies highlight the importance of understanding spillover effects to understand the full health consequences of specific life events is highlighted. We contribute to this literature by analyzing the spillover effects of unemployment on spousal smoking behavior, which has not been investigated previously.

Evidence on the health consequences of own unemployment is mixed.[2] While some studies find no evidence of job loss impacting various health measures (Browning et al. 2006; Böckerman and Ilmakunnas 2009; Salm 2009; Schmitz 2011), others find that job loss increases hospitalizations and mortality (Eliason and Storrie 2009a; Eliason and Storrie 2009b; Sullivan and von Wachter 2009; Browning and Heinesen 2012), negatively affects self-reported health measures (Brand et al. 2008; Strully 2009; Marcus 2013; Schröder 2013; Schaller and Stevens 2015; Schiele and Schmitz 2016), and blood-based biomarkers (Michaud et al. 2016). Concerning risky health behaviors, evidence for the effects of own job loss on weight gain is also mixed (Deb et al. 2011; Jónsdóttir and Ásgeirsdóttir 2014; Marcus 2014). Alcohol consumption increases for specific subgroups following job loss (Gallo et al. 2001; Deb et al. 2011). However, several studies suggest that own job loss increases the probability and intensity of smoking (Falba et al. 2005; Marcus 2014; Black et al. 2015; Golden and Perreira 2015). We complement these studies by examining whether spousal smoking behavior is also affected.

While there are at least five reasons suggesting that smoking behavior might change due to spousal unemployment, the direction of the effect is theoretically ambiguous. First, smoking is often seen as a way to reduce stress (Kassel et al. 2003; Golden and Perreira 2015) and previous studies highlight that job loss and unemployment induce stress. Likewise, unemployment might also increase spousal stress levels, thereby increasing their smoking. Second, there might be an income effect, meaning that unemployment reduces household income available for purchasing cigarettes. This might decrease smoking rates of both spouses following one spouse's unemployment. Third, there might be a constraint effect (Manski 2000), meaning that increased smoking of one spouse effectively tightens the household budget constraint, which might lead to the other spouse smoking less. Fourth, there is evidence that changes in smoking behavior are positively related within couples (Falba and Sindelar 2008; Fletcher and Marksteiner 2017). This mechanism predicts an increase in smoking due to spousal unemployment if own unemployment increases smoking. Fifth,

[2] Apart from studies focusing on exogenous entries into unemployment, there are numerous studies examining associations between unemployment, on the one hand, and health and health behaviors, on the other. Henkel (2011) and Roelfs et al. (2011) provide systematic reviews of these studies.

unemployment might increase the time available for smoking or to invest in health (such as anti-addiction courses). This mechanism predicts an ambiguous effect for own unemployment and is less relevant for spousal unemployment. In summary, there might either be a positive, a negative, or no spousal spillover effect at all, depending on which mechanism dominates. Furthermore, the effects of own and spousal unemployment on smoking behavior are likely partially interdependent. In the end, it is an empirical question.

The paper is structured as follows. Section 2 introduces the data. Section 3 outlines the empirical strategy, while Section 4 presents the results. Finally, Section 5 concludes.

2 Data

We use data from the German Socio-Economic Panel (SOEP, version 33), which currently surveys approximately 30,000 individuals in around 11,000 households every year (Goebel et al. 2018). The SOEP has several advantages for our study. First, its panel structure allows for observing smoking behavior before and after the treatment. Second, all adult household members are surveyed individually and can be linked, thus enabling us to follow both spouses over time, even following household dissolutions. Third, unlike register data, SOEP contains individual information on current smoking status and smoking intensity. Fourth, for the construction of control variables, we can rely on a large set of labor market, health, and socio-economic information at the individual and household levels.

2.1 Outcome variables

Questions concerning whether an individual currently smokes and the number of cigarettes smoked per day are asked every two years since 2002. We therefore mainly use data collected in the eight even years between 2002 and 2016. Our outcome variables are the changes in the spousal spouses' smoking status and intensity between two survey waves containing smoking information. We measure the smoking intensity by the log number of cigarettes smoked per day.[3]

2.2 Treatment indicator

We construct the binary treatment indicator at the couple level. The treatment group consists of couples in which one spouse enters unemployment due to involuntary job loss between two survey waves with the smoking questions.[4] We refer to this spouse as the directly affected spouse and to the other as the indirectly affected spouse. The control group consists of couples in which the (potentially) directly affected spouse is continuously employed between two relevant survey waves.[5]

[3]We construct the log transformation as ln(cigarettes+1) to avoid undefined values for non-smokers and quitters. In the robustness section, we also consider changes in the number of cigarettes (in levels) as outcome.

[4]The Online Appendix outlines the institutional background, i.e. the German unemployment insurance system.

[5]We make this restriction as job changes affect well-being (Chadi and Hetschko 2018), thus potentially impacting smoking.

In our main specification, we consider unemployment due to plant closures, redundancies, and layoffs. In an alternative specification, we consider only unemployment resulting from plant closures, which has the advantage that plant closures are usually not the result of individual behavior, while it is sometimes argued that layoffs are potentially endogenous. However, looking only at plant closures is not our preferred specification for several reasons. First, individuals who enter unemployment due to plant closure might be a selective group as they did not leave the company earlier. Browning and Heinesen (2012) report that in Denmark, in those plants that eventually close, more than 90 percent of displaced workers leave within the two years before the actual plant closure. Second, as plant closures happen rather rarely in countries like Germany, it is an atypical reason for entering unemployment.[6] If unemployment has different consequences for individuals who experience a plant closure, this might limit the generalizability of our results. Employing a broader treatment definition, we also implicitly investigate the effects of unemployment on couples affected by any downsizing preceding the plant closure. In the robustness section, we show that the results are insensitive to considering only unemployment due to plant closures. In this context, we also analyze the effects of unemployment for any reason (including own resignation, mutual agreement, and sabbatical) and of experiencing a job loss, irrespective of whether the individual was subsequently unemployed or not.

2.3 Sample selection

We restrict our sample to married and unmarried heterosexual couples in which the (potentially) directly affected spouse is working full-time or part-time in the private sector and is between 18 and 60 years old pre-treatment. Couples are generally included in the sample regardless of the indirectly affected spouse's working status and age. We only exclude couples in which both spouses involuntarily enter unemployment in the same period.[7] We consider couples living together in the same household at baseline, i.e., in the last pre-treatment wave containing smoking information. However, we do not impose the restriction that couples must live together following treatment. We drop couples with missing values in the treatment indicator or the smoking measures in the two even years before and the first even year after unemployment. The final pooled sample consists of 15,507 couples: 283 couples in the treatment group and 15,224 couples in the control group.

3 Empirical strategy

To estimate causal treatment effects given non-random assignment of unemployment, we follow a standard approach and rely on the conditional independence assumption (CIA, Rosenbaum and

[6] Plant closures account for approximately five percent of German unemployment (Schmitz 2011).

[7] In addition, we observe seven directly affected spouses with two unemployment experiences over time. These couples appear twice in the treatment group; results are robust if these couples are excluded.

Rubin 1983):

$$(Y(1), Y(0)) \perp\!\!\!\perp D|C, \tag{1}$$

where $Y(1)$ and $Y(0)$ denote potential outcome values, D is the treatment, and C is a set of control variables. However, we face the typical challenge of not knowing the correct set of control variables. On the one hand, if relevant control variables are omitted from C, the estimated effects of the treatment D are biased. On the other hand, overcautiously selecting unnecessary variables may lead to variance inflation.

Our empirical strategy employs post-double selection, a supervised machine learning method that helps to choose variables relevant for the identification assumption. It also helps to avoid imposing strong functional form assumptions or selecting unnecessary control variables. Moreover, to increase the plausibility of the CIA, our empirical strategy focuses on arguably exogenous entries into unemployment and combines difference-in-differences (DiD) estimation with entropy balancing, a matching strategy that balances pre-treatment covariates more effectively than common propensity score methods. The matching procedure addresses bias due to selection-on-observables, while the DiD approach rules out selection on time-invariant unobservables that might affect both treatment and outcome (e.g., time-invariant unobserved personality traits).

We proceed in five steps:

1. Choose the pool of potential control variables based on previous studies, including a set of interaction terms and higher order polynomials of these variables.
2. From this pool of potential control variables, select variables predicting treatment status (via Lasso regression), C_D.
3. Apply entropy balancing based on C_D ("matching step").
4. Select variables from the pool of potential control variables that predict the outcome variables via Lasso regression. This step is performed separately for our four outcome variables (smoking status and intensity of both spouses), $C_{Yk}, k = 1, ..., 4$.
5. Estimate the effect of unemployment on the smoking behavior of couples ("regression analysis step"), based on weights from the matching step and the union of control variables selected in the steps 2 and 4, as $C = \{C_{Y1}, C_{Y2}, C_{Y3}, C_{Y4}, C_D\}$.

The following describes each step in more detail.

3.1 The pool of potential control variables

Given the vast range of topics covered by the SOEP, we are able to include almost all variables from related studies (see Table A.1 in the appendix).[8] The control variables originate from the

[8]Specifically, we consider Falba et al. (2005), Browning et al. (2006), Böckerman and Ilmakunnas (2009), Eliason and Storrie (2009), Kuhn et al. (2009), Salm (2009), Sullivan and von Wachter (2009), Schmitz (2011), Browning and

baseline wave. We also include the smoking variables from the last two pre-treatment periods, thus capturing unobserved, time-invariant differences between the treatment and control groups.

The final pool of potential control variables additionally includes a set of interaction terms, logarithmic transformations, as well as second and third order polynomials of these control variables.[9] This relaxes functional form assumptions regarding the relationship between the control variables with treatment and outcomes. Altogether, the pool of potential control variables encompasses 4,188 variables.

3.2 Selection of variables that predict treatment status

To select control variables, we use Belloni et al.'s (2014a; 2014b) post-double-selection method based on Lasso regressions (hereafter, "double-Lasso").

The Lasso estimator is expressed as:

$$\hat{\boldsymbol{\beta}}^{\boldsymbol{L}} = \underset{\boldsymbol{\beta} \in \mathbb{R}^p}{\arg\min} \frac{1}{n} \left(\sum_{i=1}^{n} [d_i - \boldsymbol{v_i'}\boldsymbol{\beta}]^2 + \lambda \sum_{j=1}^{p} |\beta_j| \right), \tag{2}$$

where d_i is the treatment indicator. $\boldsymbol{v_i}$ denotes the vector of potential control variables (from step 1), p the number of potential control variables, and n the sample size. λ is the penalty factor.[10] $\hat{\boldsymbol{\beta}}^{\boldsymbol{L}}$ denotes the vector of coefficients solving equation (2) and is chosen to minimize the sum of squared residuals as well as a penalty term considering the sum of the absolute values of the coefficients. Lasso is particularly well-suited for control variable selection due to its kink at zero, meaning that many of the coefficients of the vector $\hat{\boldsymbol{\beta}}^{\boldsymbol{L}}$ are set to zero. This step chooses all variables relevant for the treatment from the pool of potential control variables, i.e. all variables with nonzero estimated coefficients based on equation (2), C_D.

3.3 Entropy balancing

To make the treatment and control groups more similar with respect to the variables that predict treatment status, we employ entropy balancing, a multivariate reweighting method that focuses directly on achieving covariate balance (Hainmueller 2012). Unless explicitly stated otherwise, we compute gender-specific balancing weights, which is similar to exact matching on gender. The entropy balancing scheme assigns a scalar weight to observations in the control group such that the control group's distributions of all selected covariates match the treatment group's covariate

Heinesen (2012), Marcus (2013), Marcus (2014), Schaller and Stevens (2015), Schiele and Schmitz (2016), Bünnings et al. (2017), and Cygan-Rehm et al. (2017).

[9] Specifically, for each binary variable, we include interactions with all other (binary and continuous) variables, while for each continuous variable, we also include log, squared, and cubic terms.

[10] The least absolute shrinkage and selection operator (Lasso) is a regularized regression method originally designed for prediction (Tibshirani 1996). We use Stata's user-written program "rlasso" (Ahrens et al. 2018) and construct the penalty factor λ using the estimation parameters, as recommended by Belloni et al. (2012). Specifically, we use $\lambda = 2c\sqrt{n}\Phi^{-1}(1 - \gamma/(log(n)2p))$, where $c = 1.1, \gamma = 0.1$.

distributions on the first and second moment.[11] This produces a sample in which the means and variances of all C_D variables are the same in the treatment and control group. Of all the possible weighting schemes that fulfill these balancing requirements, entropy balancing chooses the one where all weights are non-negative and deviate the least from uniform weights.

3.4 Selection of variables that predict the outcomes

This step selects variables from the pool of potential control variables that predict the outcomes. Based on equation (2), this step performs separate Lasso regressions for each of our four outcome variables (smoking status and intensity of each spouse), $C_{Yk}, k = 1, ..., 4$. That is, d_i now refers to the final outcome variables. Additionally, to increasing the plausibility of the identification assumption, this step increases precision by selecting variables that explain further residual variation in the respective outcome (Hainmueller 2012; Belloni et al. 2014a). The double-Lasso derives its name from the two separate Lasso regressions (steps 2 and 4).

3.5 Regression analysis step

We then regress *changes* in smoking behavior (Y) of spouse S on the treatment indicator (D), controlling for C, the set of double-Lasso control variables. The resulting DiD estimation equation is written as:

$$\Delta Y^S = \alpha^S + \delta^S \cdot D + C'\gamma^S + I'\eta^S + \varepsilon^S, \tag{3}$$

which is estimated by weighted least squares using the weights from entropy balancing.[12] I denotes fixed effects for states, industry sectors, and years, to address general differences in outcomes and treatment across regions, industry sectors, and time. The final set of control variables C is formed by the union of variables selected in steps 2 and 4, as $C = \{C_{Y1}, C_{Y2}, C_{Y3}, C_{Y4}, C_D\}$. That is, we disregard all variables from the pool of potential control variables, for which $\beta_j = 0$ in equation (2) for *all* four outcome variables *and* for the treatment indicator. Apart from the construction of the weights, the estimator for the average treatment effect on the treated (δ) resembles Heckman et al.'s (1997) regression-adjusted semi-parametric DiD matching strategy. ε denotes the error term clustered at the household level.

Our empirical strategy is double robust (Bang and Robins 2005) in the sense that we get

[11] We perform entropy balancing using Stata's user-written program "ebalance" (Hainmueller and Xu 2013) and applying the default tolerance level.

[12] To see that equation (3) constitutes a DiD equation, consider the following DiD-style equation that allows for a differential effect of the pre-determined control variables in the two periods:

$$\widehat{Y} = \beta + \alpha POST + \nu D + \delta(POST \cdot D) + C'\omega + C'\gamma \cdot POST + I'\theta + I'\eta \cdot POST,$$

where $POST$ is an indicator variable for the period after the job loss. For the post- and pre-period, respectively, we have

$$\widehat{Y^{POST}} = \beta + \alpha + (\nu + \delta) \cdot D + C'(\omega + \gamma) + I'(\theta + \eta)$$
$$\widehat{Y^{PRE}} = \beta + \nu D + C'\omega + I'\eta.$$

Subtracting the two equations, yields equation (3), with $\Delta Y = Y^{POST} - Y^{PRE}$.

unbiased estimates if either the set of variables that predicts treatment, C_D, is correct or the set of variables that predicts changes in the outcome, C_{Yk}. The two steps of the double-Lasso procedure highlight our strategy's two chances to get it right.

3.6 Summary statistics

The double-Lasso procedure selects 37 out of the 4,188 variables. Specifically, 12 variables are selected as predictors of the treatment status and 25 variables as predictors for at least one of the four outcome variables. Table 1 provides summary statistics for these variables. After entropy balancing, the standardized difference in means of all covariates used in our matching strategy (i.e., the C_D variables selected in step 2) is below five percent, the criterion for successful matching proposed by Caliendo and Kopeinig (2008). After entropy balancing, even most covariates that are used only in the treatment effect estimation step and not in the matching step (i.e., the C_{Yk} variables selected in step 4) satisfy this condition. Remaining imbalances are addressed in the regressions step.

Before matching, the treated couples tend to be slightly older, in worse health, and less educated than their control-group counterparts. Treated couples are also more likely to smoke at baseline (38.2 vs. 29.2 percent of directly affected spouses; 32.9 vs. 27.4 percent of indirectly affected spouses).[13]

4 Results

Table 2 starts with a simple DiD model that looks at differential changes in the outcomes between treatment and control group without control variables, except the lagged dependent variable from the last pre-treatment observation.[14] The results in column (1) of panel A suggest that when one spouse enters unemployment, the probability of smoking increases by 5.5 percentage points for directly affected spouses and by 2.7 percentage points for indirectly affected spouses, on average. The effects are very similar in column (2), which considers the conventional control variables in the matching and regression step. Column (3) shows the results for our preferred specification, the double-Lasso regression-adjusted DiD matching estimator. These effects are also very similar: Unemployment increases the probability of smoking by 4.2 percentage points for directly affected spouses and by 2.5 percentage points for indirectly affected spouses. The displayed p-values show that the two point estimates are not significantly different from one another, suggesting that the effect of unemployment on smoking status is similar for the directly and indirectly affected spouses.

The direct effect is similar for unemployment of males and females (panels B and C). However,

[13] Appendix Table A.1 provides the standardized differences based on propensity score weighting (control group weights constructed as $1/(1 - PS(C_c))$, where $PS(C_c)$ is the propensity score) for the conventional set of control variables. While propensity score weighting works well in balancing the control variables, entropy balancing generally produces better balancing. Propensity score weighting even increases the standardized difference for some variables.

[14] As, our estimations do not include individual fixed effects, Nickell (1981) Bias issues do not apply.

there is a clear gender difference regarding the spillover effect: Among indirectly affected spouses, the effect is 4.2 percentage points when the male becomes unemployed and precisely zero when the female becomes unemployed. This suggests that men do not change their smoking behavior when their female partner enters unemployment. However, when men enter unemployment, both spouses increase smoking to a similar degree. One explanation might be that, in our sample, the male is typically the main breadwinner, with the unemployment of the main breadwinner causing more stress. We empirically investigate this mechanisms in Section 4.2. A total of 38.2 and 32.9 percent of directly and indirectly affected spouses smoke at baseline, respectively (see Table A.1). Hence, unemployment increases the prevalence of smoking by 11.0 percent for the directly affected and by 7.6 percent for indirectly affected spouses.

The effects on smoking intensity exhibit a similar pattern (columns 4-6). The results for our preferred specification suggest that unemployment increases the daily number of cigarettes smoked by 8.5 percent for directly affected individuals and by 7.8 percent for indirectly affected spouses (column 6). Again, the spillover effect is driven by male unemployment. In these couples, the increase in spousal smoking intensity is even more pronounced than the increase in own smoking intensity (11.4 vs. 9.4 percent, respectively, although the difference is not statistically significant). Additionally, the direct effect of female unemployment on own smoking intensity is similar to the overall effect (9.6 percent).

Results in Table 2 highlight that own and spousal unemployment similarly affect individual smoking status and intensity. All three specifications produce similar results with respect to effect direction, size, and statistical significance. Yet, the results suggest that treatment effects are most precisely estimated with our preferred specification relative to the two alternative models.

4.1 Treatment effect heterogeneity by baseline smoking status

Next, we examine whether the overall effects mask effect differences between individuals with different smoking histories. We differentiate between never smokers (i.e., individuals who have never smoked), former smokers (i.e., individuals who have smoked before but do not smoke at baseline), and baseline smokers (i.e., individuals who smoke at baseline). These three individual smoking histories combined could lead to nine different groups at the couple level and, hence, rather small subgroups. We therefore first analyze only individual smoking histories (see Table 3). Subsequently, we look at smoking histories at the couple level (see Table 4), only differentiating between baseline smokers and baseline non-smokers (grouping never and former smokers together initially, later looking specifically at former smokers).[15]

Table 3, column (1) shows that neither own nor spousal unemployment increases the probability

[15] In all columns in Tables 3 and 4, we show results for a simple, unmatched difference-in-differences model including the lagged dependent variable from the last pre-treatment wave without further regression adjustment. This is the specification from column (1) in Table 2, which produces rather similar results as our preferred specification. Due to small sample sizes in specific subgroups of Tables 3 and 4, entropy balancing does not always converge. However, using entropy balancing with relaxed balancing constraints (specifically the tolerance level) provides very similar results to the presented DiD results without matching. Results are available upon request.

of smoking initiation or smoking intensity for never smokers. The spousal spillover effects of unemployment on smoking in panel B are significantly negative, but very small and economically not meaningful.[16] The results for former smokers in Column (2) shows that own unemployment increases the probability of smoking relapse and smoking intensity by approximately 10 percentage points and 20 percent, respectively. The effects of spousal unemployment on former smokers presented in panel B are of similar magnitude to our main results in Table 2, but are too imprecisely estimated to be statistically significant. Column (3), displaying the results for baseline smokers, shows that spousal unemployment increases the probability of smoking continuation and smoking intensity by approximately 7 percentage points and 17 percent, respectively. Panel B shows the intensive margin effects conditional on smoking at baseline, while our main specification shows the unconditional intensive margin results. The magnitude of own unemployment effects on smoking at the extensive and intensive margin is about 5 percentage points and 10 percent, respectively; although the latter effect is not statistically significant.

The results presented in Table 3 suggest no increases in smoking initiation among adult never smokers, which is consistent with findings that habit formation predominantly occurs in adolescence (Glynn et al. 1993; Nonnemaker and Farrelly 2011). Our results emphasize that unemployment triggers smoking relapses among former smokers and decreases smoking cessation among smokers. Moreover, this pattern of spousal spillover effects is consistent with findings by Müller and Shaikh (2018) and Fletcher and Marksteiner (2017).

Table 4 considers the baseline smoking status of both spouses jointly, that is, at the couple level. Column (1) shows results for couples in which both spouses are non-smokers at baseline and shows that own unemployment significantly increases smoking probability and intensity. Column (2) investigates couples where only the indirectly affected spouse is a smoker. For this group, all estimated coefficients are clearly larger compared to non-smoker couples (column 1), hinting at the importance of the indirectly affected spouse's smoking status. In contrast, if only the directly affected spouse is a smoker at baseline (column 3), the estimated coefficients for own unemployment are statistically insignificant and even negative. However, if the directly and indirectly affected spouses are both smokers at baseline (column 4), the magnitude of the effect of own unemployment is substantially larger for smoking status (9.3 vs. -0.7 percentage points) and smoking intensity (22.0 vs. -4.4 percent). Thus, having a non-smoking partner appears to have a protective effect for the directly affected spouse.

Moreover, there is evidence that a non-smoking partner mitigates the consequences of unemployment for the indirectly affected spouse: For indirectly affected spouses who smoke at baseline (columns 2 and 4), the effect of spousal unemployment on smoking probability and intensity is clearly smaller if the directly affected spouse is a non-smoker (3.6 vs. 8.2 percentage points and

[16] We do not want place too much weight on this statistically significant negative effect; it is significant because, among never smokers, no indirectly affected spouse in the treatment groups starts smoking. When applying conventional standard errors, the effect is no longer statistically significant.

4.7 vs. 23.7 percent, respectively). Similarly, for indirectly affected spouses who are non-smokers at baseline (columns 1 and 3), the effects of spousal unemployment on smoking are smaller if their directly affected partner is a non-smoker at baseline. Generally, we obtain the largest effects if both spouses smoke at baseline (column 4). Thus, having a non-smoking partner appears to have a protective effect for both directly and indirectly affected individuals.

As combining never and former smokers might mask heterogeneous treatment effects, we further analyze the effect of having a non-smoking partner among couples consisting only of smokers and former smokers at baseline (i.e. excluding never smokers). Despite decreased precision, having a non-smoking partner also seems beneficial for directly and indirectly affected spouses in this specific subsample (columns 4-7).

4.2 Mechanisms

This section sheds some more light on which mechanism(s) drive(s) our results (income, constraint, partner's behavior, time, or stress effect). Our findings provide little evidence for an income effect, as it would suggest that cigarette consumption of both spouses decreases due to reduced household income. Similarly, there is no evidence for the constraint effect (one spouse's increased smoking decreases the other spouse's smoking due to household budget constraints), as it would imply that the effects go in opposite directions for own and spousal unemployment. However, our findings are consistent with the partner effect, postulating that changes in smoking behavior are positively related within couples. Furthermore, the time mechanism mainly predicts changes in the smoking behavior for own unemployment, but cannot explain why we find similar smoking changes for own and spousal unemployment. We additionally examine whether unemployment affects leisure time satisfaction in Table 5.[17] Own unemployment increases satisfaction with leisure time, whereas spousal unemployment has no significant effect. This pattern does not support the time effect as the main mechanism since we find increased smoking due to own and spousal unemployment.

Our results are consistent with the stress effect, which suggests that smoking is a way to reduce stress (Kassel et al. 2003; Golden and Perreira 2015). Table 5 investigates this mechanism, showing that male unemployment increases financial stress for both spouses, while female unemployment only increases financial stress for the females themselves.[18] This is a noteworthy pattern, as it perfectly matches the pattern for the smoking effects. The last four columns provide additional evidence that financial stress could be an important mechanism. In these specifications, we differentiate couples according to the indirectly affected spouse's employment status at baseline. The comparison of columns (3) and (4) shows that the effects of own unemployment on smoking status are more pronounced when the indirectly affected spouse does not work full-time; i.e. when the directly affected spouse is the main bread-winner. This pattern is similar for smoking intensity

[17] As measured on a scale from 0 (not satisfied) to 10 (very satisfied). Results in the first two columns of Table 5 rely on our main estimation strategy but use the variable provided in the column header as outcome.

[18] This outcome relates to the question of concerns about own economic situation, measured on a scale from 1 (not at all) to 3 (very concerned).

(columns 5 and 6), irrespective of the directly affected spouse's gender (see panels B and C). Spousal unemployment patterns are more mixed.

4.3 Robustness tests

Table 6 shows robustness tests for the pooled sample, while Appendix Tables A.2 and A.3 provide the gender-specific results. We first run placebo regressions to assess the plausibility of our identifying assumption. Here the treatment variable takes the value of one, two years before the actual treatment.[19] The placebo effects of own and spousal unemployment are small and statistically insignificant (column 2), suggesting that there is no anticipation of the imminent unemployment event resulting in changes in smoking behavior. Trends in smoking behavior do not differ between the treatment and control groups before treatment.

Columns (3) to (6) examine various estimation issues. Given SOEP's two-stage sampling design, column (3) clusters the standard errors at the SOEP's primary sampling unit level (Abadie et al. 2017), electoral units. Column (4) employs propensity score weighting, column (5) constructs the entropy balancing weights using the control variables selected in step 2 *and* step 4 of the double-Lasso procedure, and column (6) does not perform exact matching on gender. Following Knabe and Rätzel (2011), column (7) restricts the sample to couples with directly affected spouses aged between 22 and 55 years at baseline. Column (8) considers the number of cigarettes for constructing the outcome variable (and not its log). Our results are robust regarding all these estimation issues.

Columns (9) to (11) present results for alternative treatment definitions. When only considering unemployment spells resulting from plant closures (column 9), smoking intensity and the probability of smoking increase for directly and indirectly affected spouses. The results are generally very similar to our main specification concerning direction, size, and statistical significance, although they are less precisely estimated given the smaller sample size. Considering unemployment spells for any reason (including resignation, mutual agreement, and sabbatical), the effects are again similar to our main specification (column 10). Column (11) considers all couples affected by involuntary job loss, irrespective of whether it resulted in unemployment or not. The effects are similar to before, yet slightly smaller, suggesting that job loss with subsequent unemployment is more severe. This specification also addresses concerns that becoming and staying unemployed after job loss might be endogenous.

[19] Based on this placebo treatment indicator, we repeat the matching and regression step, except this time all baseline characteristics are from two years prior to the actual baseline. Thus, we use the lags of all baseline variables as described in Table 1, but not the second lags due to sample size considerations.

5 Conclusion

This is the first study examining the effect of unemployment on spousal smoking behavior. Using German panel data, we show that one spouse's unemployment increases the smoking probability of both spouses by 2 to 4 percentage points. Moreover, the number of cigarettes smoked per day also increases an average of 8 percent. This reflects an increase in smoking prevalence of approximately 11 and 7 percent among spouses directly and indirectly affected by unemployment, respectively. While smoking increases among both men and women when they enter unemployment themselves, spillover effects are mainly driven by male unemployment. Individuals both directly and indirectly affected by unemployment are even more likely to smoke and to smoke more cigarettes per day if their partner is a smoker at baseline. The effects are more pronounced among individuals who smoke themselves. Exploring potential mechanisms for the effects, we find that stress is a key driving factor, as unemployment increases stress and smoking is a strategy of coping with stress. Our results are also consistent with a partner effect, meaning that changes in one spouse's smoking behavior affect the other spouse's smoking behavior.

Our findings highlight that the extent to which own and spousal unemployment affect individuals also depends on the characteristics and behaviors of their spouse. As smoking strongly increases the risks for a wide variety of cancers and cardiovascular diseases, it is important to consider the spillover effects on spouses and intra-household interactions of behaviors in order to determine the full health consequences of unemployment. This is particularly important for studies examining the public health costs of unemployment (e.g., Kuhn et al. 2009). The findings of increased smoking initiation and decreased smoking cessation due to own and spousal unemployment likely translate directly into substantial health losses with respect to mortality (Taylor et al. 2002; Doll et al. 2004) and morbidity (Østbye and Taylor 2004; Timmermans et al. 2018).

Moreover, the findings emphasize that unemployment triggers smoking relapses. This is especially relevant for policies in countries that have increased smoking cessation rates. Our results further show that unemployed individuals and their spouses are a high-risk group with respect to smoking, particularly if their partner is already a smoker. Policies and interventions aimed at reducing smoking rates might focus on these high-risk groups. Generally, our findings highlight the relevance of intra-household spillover effects of major life events, even with respect to health behaviors.

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Tables

Table 1
Summary statistics for double-Lasso selected control variables.

Variable		Means treated	Means controls		Std. difference (%)	
			Raw	EB	Raw	EB
Predictor variables: treatment						
Log tenure		1.7	2.4	1.7	−69.6	0.4
Log labor earnings		9.6	10.3	9.6	−48.3	1.5
Interaction terms						
(age)	(major job worries)	20.0	6.3	19.9	66.7	0.2
(basic schooling)	(regional unemployment)	3.8	2.0	3.8	40.4	0.2
(basic schooling)	(major job worries)$^+$	0.2	0.0	0.2	44.6	0.0
(ever smoker)	(major job worries)$^+$	31.4	8.9	31.4	58.4	0.1
(blue collar worker)	(small company)$^+$	22.6	6.4	22.6	47.3	0.1
(blue collar worker)	(major job worries)$^+$	26.1	6.5	26.1	55.0	0.1
(poor health)	(spouse non-German)$^+$	5.7	1.4	5.7	23.4	0.0
(spouse not working)	(spouse overweight or obese)$^+$	23.0	11.0	23.0	32.2	0.0
(small company)	(major job worries)$^+$	13.8	2.5	13.8	42.4	0.0
(small company)	(spouse not working)$^+$	12.0	3.3	12.0	33.4	0.0
Union of predictor variables: all outcomes						
Baseline smoker$^+$		38.2	29.2	37.4	19.0	1.6
Lagged baseline smoker$^+$		42.4	30.5	39.0	24.9	7.0
Ever smoker$^+$		69.3	61.5	68.3	16.3	2.1
Log no. of cigarettes/day^a		1.0	0.8	1.0	20.0	0.9
Lagged log no. of cigarettes/day^a		1.2	0.8	1.1	26.1	5.5
Lagged squared log no. of cigarettes/day^a		3.4	2.3	3.2	26.2	4.5
Spouse baseline smoker$^+$		32.9	27.4	34.3	11.9	−3.1
Spouse lagged baseline smoker$^+$		33.6	28.7	35.2	10.6	−3.5
Spouse ever smoker$^+$		62.5	60.3	63.8	4.7	−2.6
Spouse log no. of cigarettes/day^a		0.9	0.7	0.9	15.0	−0.1
Spouse lagged log no. of cigarettes/day^a		0.9	0.8	1.0	13.2	−2.0
Spouse lagged squared log no. of cigarettes/day^a		2.7	2.1	2.7	14.5	−0.6
Interaction terms						
(years full-time)	(spouse baseline smoker)	6.3	5.2	6.5	11.1	−1.3
(blue collar worker)	(spouse ever smoker)$^+$	33.2	19.8	35.3	30.8	−4.3
(vocational training)	(spouse ever smoker)$^+$	48.1	47.2	50.5	1.7	−4.8
(physical health)	(spouse ever smoker)	30.5	30.9	32.1	−1.7	−6.2
(baseline smoker)	(never unemployed)$^+$	11.3	17.9	15.2	−18.8	−11.4
(ever smoker)	(blue collar worker)$^+$	38.9	22.4	40.1	36.3	−2.4
(ever smoker)	(children)$^+$	30.0	28.4	30.1	3.7	−0.1
(ever smoker)	(spouse no. of cigarettes/day)^a	5.0	3.4	4.6	19.9	4.7
(ever smoker)	(spouse ever smoker)$^+$	50.2	42.8	48.6	14.9	3.2
(spouse baseline smoker)	(spouse works full-time)$^+$	13.1	15.4	16.5	−6.5	−9.6
(spouse baseline smoker)	(spouse priv. health insur.)$^+$	2.5	2.7	2.0	−1.6	2.8
(spouse baseline smoker)	(spouse never unemployed)$^+$	11.3	14.5	15.0	−9.6	−11.0
(children)	(spouse ever smoker)$^+$	26.9	26.8	28.0	0.2	−2.5
N		283	15224			

Note: The pre-treatment means of variables for the treatment and control groups are in the first and second columns, respectively. Lagged variables refer to observations from the first pre-treatment period. The means of the reweighted control group using entropy balancing (EB) weights are in the third column. The last two columns comprise the standardized difference in means, a matching quality indicator. The standardized difference in means for each control variable s is defined as $SD_s = 100 \cdot (\bar{s_1} - \bar{s_0})/\sqrt{0.5 \cdot (\sigma^2_{s1} + \sigma^2_{s0})}$, where $\bar{s_1}$ and $\bar{s_0}$ are the means of treated and controls, respectively, and σ^2_{s1} and σ^2_{s0} are the corresponding variances.
(variable a) (variable b): Interaction term of variable a and variable b.
$^+$ The mean represents a percentage share, ^{a} Includes non-smokers.

Table 2
Main results: effect of unemployment on smoking behavior.

	Smoking status			Smoking intensity		
	simple DiD	matched DiD	double-Lasso matched DiD	simple DiD	matched DiD	double-Lasso matched DiD
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: pooled sample						
Own unemployment	0.055***	0.042***	0.042***	0.114***	0.082**	0.085**
	(0.018)	(0.016)	(0.015)	(0.041)	(0.040)	(0.039)
Spousal unemployment	0.027**	0.037***	0.025*	0.083**	0.113***	0.078**
	(0.014)	(0.014)	(0.014)	(0.038)	(0.038)	(0.036)
p-value of difference	0.211	0.818	0.394	0.563	0.534	0.891
$N_{Treated}$	283	283	283	283	283	283
N	15507	15507	15507	15507	15507	15507
Panel B: unemployment of males						
Own unemployment	0.052**	0.043**	0.051**	0.094	0.077	0.094*
	(0.026)	(0.021)	(0.021)	(0.061)	(0.055)	(0.054)
Spousal unemployment	0.046**	0.056***	0.042**	0.127**	0.151***	0.114**
	(0.019)	(0.019)	(0.019)	(0.050)	(0.050)	(0.048)
p-value of difference	0.827	0.627	0.756	0.662	0.259	0.761
$N_{Treated}$	169	169	169	169	169	169
N	8574	8574	8574	8574	8574	8574
Panel C: unemployment of females						
Own unemployment	0.059***	0.037**	0.039***	0.144***	0.086**	0.096***
	(0.023)	(0.018)	(0.015)	(0.049)	(0.037)	(0.035)
Spousal unemployment	0.003	0.008	-0.004	0.022	0.055	0.005
	(0.019)	(0.014)	(0.016)	(0.056)	(0.040)	(0.046)
p-value of difference	0.052	0.202	0.051	0.056	0.549	0.110
$N_{Treated}$	114	114	114	114	114	114
N	6933	6933	6933	6933	6933	6933
Set of control variables		CC	DL		CC	DL
Matching		EB	EB		EB	EB

Note: The table displays the effect of own and spousal unemployment on smoking behavior. Standard errors clustered at the household level are in parentheses. Regressions in columns (1) and (4) are estimated without control variables other than the lagged dependent variable from the last pre-treatment observation. Regressions in columns (2) and (5) are estimated using the conventional control variables (CC), including state, industry, and year fixed effects. Columns (3) and (6) are estimated using the union of control variables identified by the double-Lasso (DL), including state, industry, and year fixed effects. Regressions are weighted by entropy balancing (EB) weights as indicated. p-values indicate whether the effects of own and spousal unemployment are different.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3
Heterogeneous treatment effect by individual baseline smoking status.

	Never smokers (1)	Former smokers (2)	Smokers (3)
Panel A: effect on smoking status			
Own unemployment	0.008	0.104**	0.047*
	(0.011)	(0.041)	(0.028)
$N_{Treated}$	87	88	108
N	5944	5008	4555
Spousal unemployment	-0.005***	0.027	0.066**
	(0.001)	(0.034)	(0.028)
$N_{Treated}$	106	84	93
N	6156	5083	4268
Panel B: Effect on smoking intensity			
Own unemployment	0.025	0.197**	0.103
	(0.033)	(0.089)	(0.072)
$N_{Treated}$	87	88	108
N	5944	5008	4555
Spousal unemployment	-0.010***	0.101	0.170**
	(0.002)	(0.088)	(0.079)
$N_{Treated}$	106	84	93
N	6156	5083	4268

Note: The table displays the effect of own and spousal unemployment on the smoking behavior of individuals based on smoking history. Standard errors clustered at the household level are in parentheses. All regressions are unweighted and include the lagged dependent variable from the last pre-treatment observation without further regression adjustment.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4
Heterogeneous treatment effects by baseline smoking status of both spouses, with and without never smokers.

	Pooled sample				Without never smokers		
	Both non-smokers	Mixed smoker couples		Both smokers	Both former smokers	Mixed smoker couples	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: effect on smoking status							
Own unemployment	0.046**	0.111*	-0.007	0.093***	0.058	0.147*	0.030
	(0.023)	(0.064)	(0.053)	(0.025)	(0.060)	(0.089)	(0.066)
Spousal unemployment	0.003	0.036	0.015	0.082***	0.040	-0.021	0.044
	(0.016)	(0.054)	(0.039)	(0.030)	(0.054)	(0.076)	(0.075)
p-value of difference	0.085	0.389	0.747	0.787	0.776	0.153	0.895
Panel B: effect on smoking intensity							
Own unemployment	0.095*	0.207	-0.044	0.220***	0.101	0.268	-0.061
	(0.050)	(0.141)	(0.128)	(0.072)	(0.129)	(0.198)	(0.152)
Spousal unemployment	0.030	0.047	0.039	0.237***	0.166	-0.086	0.106
	(0.044)	(0.143)	(0.091)	(0.091)	(0.157)	(0.202)	(0.175)
p-value of difference	0.270	0.395	0.594	0.881	0.688	0.159	0.456
$N_{Treated}$	140	35	50	58	35	24	25
N	9011	1941	2228	2327	2075	1079	1173
Directly affected spouse: smoker	no	no	yes	yes	no	no	yes
Indirectly affected spouse: smoker	no	yes	no	yes	no	yes	no
$N_{Treated}$	140	35	50	58	35	24	25
N	9011	1941	2228	2327	2075	1079	1173

Note: The table displays the effect of own and spousal unemployment on smoking behavior according to the smoking history of both spouses. In columns (5) to (7), couples with at least one never smoker are excluded from the analysis. Standard errors clustered at the household level are in parentheses. All regressions are unweighted and include the lagged dependent variable from the last pre-treatment observation without further regression adjustment. p-values indicate whether the effects of own and spousal unemployment are different.

Table 5
Analysis of mechanisms.

	Time	Stress				
	Satisfaction with leisure time	Financial stress	Smoking status		Smoking intensity	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: pooled sample						
Own unemployment	0.658***	0.286***	0.019	0.065***	0.034	0.125**
	(0.114)	(0.031)	(0.020)	(0.021)	(0.048)	(0.057)
Spousal unemployment	0.103	0.112***	0.011	0.021	0.066	0.045
	(0.120)	(0.031)	(0.017)	(0.019)	(0.043)	(0.051)
p-value of difference	0.000	0.000	0.765	0.087	0.638	0.225
$N_{Treated}$	279	278	124	159	124	159
N	15301	15415	8117	7390	8117	7390
Panel B: unemployment of males						
Own unemployment	0.782***	0.351***	0.019	0.049*	-0.034	0.096
	(0.146)	(0.035)	(0.030)	(0.027)	(0.076)	(0.069)
Spousal unemployment	0.079	0.218***	0.050	0.038*	0.155**	0.096*
	(0.159)	(0.039)	(0.033)	(0.022)	(0.079)	(0.056)
p-value of difference	0.000	0.002	0.518	0.711	0.101	0.995
$N_{Treated}$	168	164	49	120	49	120
N	8502	8520	2515	6059	2515	6059
Panel C: unemployment of females						
Own unemployment	0.486***	0.187***	0.030*	0.086***	0.097**	0.168***
	(0.149)	(0.042)	(0.017)	(0.019)	(0.040)	(0.048)
Spousal unemployment	0.151	-0.023	0.002	-0.055*	0.032	-0.184*
	(0.154)	(0.042)	(0.016)	(0.031)	(0.043)	(0.095)
p-value of difference	0.065	0.000	0.257	0.000	0.259	0.000
$N_{Treated}$	111	114	75	39	75	39
N	6799	6895	5602	1331	5602	1331
Indirectly affected spouse: works full-time			yes	no	yes	no

Note: The table displays the effect of own and spousal unemployment on selected alternative outcomes and on smoking behavior according to the employment status of the indirectly affected spouse at baseline. Reported outcomes in columns (1) and (2) are satisfaction with leisure time and stress measured as financial worries, respectively. All regressions are estimated using a union of control variables identified by the double-Lasso and include state, industry, and year fixed effects. Regressions are weighted by entropy balancing weights. Standard errors clustered at the household level are in parentheses. *p*-values indicate whether the effects of own and spousal unemployment are different.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6
Sensitivity analyses.

	Main	Placebo	Estimation issues				Alternative	Alternative	Alternative treatment		
	specification	regression	s.e.	PS	All Lasso	Not exact	sample	outcome	Plant closure	All reasons	Job loss
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: effect on smoking status											
Own unemployment	0.042***	-0.015	0.042***	0.046***	0.038**	0.042***	0.039**		0.027	0.024**	0.024**
	(0.015)	(0.013)	(0.015)	(0.015)	(0.016)	(0.015)	(0.017)		(0.017)	(0.012)	(0.010)
Spousal unemployment	0.025*	0.009	0.025*	0.024*	0.027*	0.026**	0.038**		0.044**	0.027**	0.024***
	(0.014)	(0.015)	(0.014)	(0.013)	(0.014)	(0.013)	(0.015)		(0.017)	(0.012)	(0.009)
p-value of difference	0.394	0.194	0.394	0.270	0.593	0.413	0.964		0.381	0.868	0.974
Panel B: effect on smoking intensity											
Own unemployment	0.085**	-0.031	0.085**	0.097***	0.067*	0.085**	0.091**	0.205	0.102**	0.046	0.038
	(0.039)	(0.037)	(0.038)	(0.037)	(0.040)	(0.037)	(0.044)	(0.281)	(0.042)	(0.030)	(0.025)
Spousal unemployment	0.078**	0.023	0.078**	0.082**	0.087**	0.084**	0.125***	0.670***	0.122***	0.094***	0.061***
	(0.036)	(0.038)	(0.037)	(0.034)	(0.037)	(0.034)	(0.040)	(0.249)	(0.045)	(0.032)	(0.022)
p-value of difference	0.891	0.250	0.889	0.762	0.693	0.987	0.558	0.200	0.702	0.238	0.464
Matching	EB	EB	EB	PS	EB	EB	EB	EB	EB	EB	EB
Clustered SE	HH	HH	PSU	HH	HH	HH	HH	HH	HH	HH	HH
Age in years	18-60	18-60	18-60	18-60	18-60	18-60	22-55	18-60	18-60	18-60	18-60
$N_{Treated}$	283	275	283	283	283	283	227	283	67	555	845
N	15507	15830	15503	14839	15507	15507	13468	15507	15291	15779	16069

Note: All regressions are estimated using the double-Lasso specification including entropy balancing (EB) or propensity score (PS) weights as indicated. Entropy balancing weights constructed using all double-Lasso covariates are applied in column (5). Matching is not exact on gender in column (6). The sample restricted to couples with directly affected spouses aged 22–55 years at baseline is in column (7). In column (8), smoking intensity is measured as the number of cigarettes smoked per day. Matching is on the first moment (mean) only in column (9). p-values indicate whether the effects of own and spousal unemployment are different. All regressions include state, industry, and year fixed effects. Standard errors clustered at the household (HH) or primary sampling unit (PSU) level are in parentheses, as indicated.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix

Table A.1
Summary statistics: means and standardized difference.

Variable	Means treated	Means controls			Std. difference (%)		
		Raw	EB	PS	Raw	EB	PS
Directly affected spouse - *Demographic*							
Age	46.3	45.6	46.1	46.4	7.7	1.8	−1.9
Female+	40.3	44.8	40.3	39.9	−9.1	0.0	0.8
Migrant+	22.6	15.1	22.6	22.4	19.3	0.1	0.6
Non-German+	18.4	11.2	18.3	18.5	20.2	0.1	−0.3
Labor							
Tenure	8.1	13.5	8.1	8.1	−57.8	0.3	0.5
Log labor earnings	9.6	10.3	9.6	9.5	−48.3	2.2	6.4
Never unemployed+	39.2	66.0	39.1	38.6	−55.7	0.3	1.2
Blue collar worker+	54.8	31.6	54.6	55.3	48.1	0.4	−1.0
No blue collar worker info+	0.4	0.1	0.4	0.1	6.7	0.0	5.8
Small company+	37.1	19.1	37.0	37.6	40.8	0.2	−1.0
Small to medium company+	34.3	29.8	34.2	34.9	9.7	0.2	−1.2
Medium company+	14.1	24.9	14.1	12.7	−27.3	0.0	4.1
Large company+	11.7	24.8	11.7	11.9	−34.4	0.0	−0.7
Major job worries+	42.4	13.9	42.3	43.7	66.7	0.3	−2.6
Some job worries+	41.3	44.3	41.2	40.7	−5.9	0.3	1.3
No job worries+	16.3	41.8	16.5	15.6	−58.7	−0.7	1.7
Years full-time	19.6	18.9	19.5	19.4	6.2	0.6	1.2
Basic schooling+	41.7	26.1	41.6	43.5	33.3	0.3	−3.7
Intermediate schooling+	45.2	45.4	45.1	43.7	−0.4	0.3	3.1
Technical college+	2.5	6.9	2.5	3.1	−21.1	0.0	−3.7
Highest secondary+	8.8	20.6	8.8	8.2	−33.5	0.0	2.2
University+	13.4	23.7	13.4	12.3	−26.7	0.0	3.2
Vocational training+	78.1	77.2	77.9	78.0	2.0	0.6	0.2
Health							
Physical health	49.2	51.8	49.0	49.1	−28.7	1.8	1.0
Mental health	48.6	50.6	48.4	48.2	−22.9	1.9	4.2
Poor health+	19.4	9.9	19.4	20.3	27.1	0.1	−2.2
Medium health+	36.7	33.5	36.6	37.7	6.7	0.2	−2.0
Good health+	43.8	56.5	44.0	42.0	−25.6	−0.3	3.7
Height in centimeters	172.4	173.1	171.8	172.5	−7.5	6.7	−0.5
Body mass index	26.9	26.3	26.8	26.8	12.3	1.9	1.8
Underweight+	1.8	1.0	1.8	2.0	7.0	0.0	−1.9
Overweight or obese+	64.0	57.9	63.7	63.7	12.4	0.5	0.6
Heavy smoker+	7.4	4.4	7.4	8.4	12.9	0.0	−3.5
Ever smoker+	69.3	61.5	69.0	69.2	16.3	0.5	0.1
No ever smoker info+	2.8	6.7	2.8	2.6	−18.2	0.0	1.7
Baseline smoker+	38.2	29.2	38.0	36.9	19.0	0.2	2.7
Log no. of cigarettes/day[a]	1.0	0.8	1.0	1.0	20.0	0.3	1.1
Lagged baseline smoker+	42.4	30.5	42.3	42.2	24.9	0.3	0.5
Lagged log no. of cigarettes/day[a]	1.2	0.8	1.2	1.2	26.1	0.3	−0.3
N	283	15224					

Note: The pre-treatment means of the variables for the treatment and control groups are in the first and second columns, respectively. Lagged variables refer to observations from the first pre-treatment period. The means of the reweighted control group using entropy balancing (EB) and propensity score (PS) weights are in the third and fourth columns, respectively. The last three columns comprise the standardized difference in means, a matching quality indicator. The standardized difference in means for each control variable s is defined as $SD_s = 100 \cdot (\bar{s_1} - \bar{s_0})/\sqrt{0.5 \cdot (\sigma_{s1}^2 + \sigma_{s0}^2)}$, where $\bar{s_1}$ and $\bar{s_0}$ are the means of treated and controls, respectively, and σ_{s1}^2 and σ_{s0}^2 are the corresponding variances.

Table A.1
Continued.

Variable	Means treated	Means controls			Std. difference (%)		
		Raw	EB	PS	Raw	EB	PS
Indirectly affected spouse - *Demographic*							
Age	46.2	45.8	46.0	46.4	4.5	1.6	−2.0
Migrant+	19.8	15.4	19.8	19.7	11.4	0.1	0.2
Non-German+	16.6	11.6	16.6	16.5	14.5	0.1	0.2
Labor							
Log labor earnings	7.4	8.4	7.3	7.4	−26.9	0.6	−1.6
Never unemployed+	45.2	61.1	45.1	44.1	−32.2	0.3	2.2
Blue collar worker+	20.8	21.6	20.8	21.8	−1.7	0.1	−2.3
No blue collar worker info+	0.4	0.5	0.4	0.4	−1.8	0.0	0.0
Not working+	33.2	20.4	33.1	33.1	29.1	0.2	0.2
Works full-time+	43.8	52.5	43.8	44.0	−17.4	0.0	−0.4
Works part-time+	23.0	27.0	23.1	22.9	−9.4	−0.2	0.3
Basic schooling+	30.0	26.0	30.0	32.2	9.0	0.2	−4.6
Intermediate schooling+	49.8	45.9	49.6	48.4	7.8	0.3	2.8
Technical college+	4.6	6.1	4.6	4.4	−6.8	0.0	0.9
Highest secondary+	13.1	20.9	13.1	12.5	−20.8	0.0	1.7
University+	17.0	23.5	16.9	15.6	−16.2	0.0	3.7
Vocational training+	75.3	76.2	75.0	75.6	−2.2	0.6	−0.7
Health							
Physical health	49.1	51.0	48.9	49.2	−20.7	1.8	−1.0
Mental health	48.9	50.4	48.7	49.1	−16.4	1.9	−1.9
Poor health+	14.8	12.3	14.8	15.0	7.4	0.0	−0.4
Medium health+	38.5	32.8	38.4	38.1	12.0	0.2	0.9
Good health+	46.6	54.9	46.8	46.9	−16.6	−0.3	−0.6
Height in centimeters	170.7	171.6	170.0	170.6	−10.6	6.9	0.5
Body mass index	27.1	26.2	27.0	27.1	20.2	2.1	1.6
Underweight+	1.1	1.3	1.1	1.4	−2.1	0.0	−2.8
Overweight or obese+	63.6	55.6	63.4	62.9	16.4	0.5	1.5
Heavy smoker+	7.4	3.7	7.4	7.4	16.3	0.0	−0.1
Ever smoker+	62.5	60.3	62.3	63.0	4.7	0.5	−0.8
No ever smoker info+	4.2	6.8	4.2	3.9	−11.2	0.0	1.7
Baseline smoker+	32.9	27.4	32.8	32.7	11.9	0.2	0.3
Log number of cigarettes/day[a]	0.9	0.7	0.9	0.9	15.0	0.3	0.2
Lagged baseline smoker+	33.6	28.7	33.5	33.4	10.6	0.2	0.3
Lagged log number of cigarettes/day[a]	0.9	0.8	0.9	0.9	13.2	0.3	−0.3
Couple information							
Children+	44.9	48.0	44.7	45.4	−6.3	0.3	−1.1
Married+	83.7	89.0	83.5	83.9	−15.3	0.6	−0.5
Home owner+	48.4	65.0	48.2	47.9	−33.9	0.3	1.0
Lives in urban area+	67.1	64.4	66.9	65.9	5.8	0.5	2.5
Regional unemployment	9.8	9.0	9.8	9.7	19.9	0.8	3.4
Year 2004+	35.3	21.4	35.2	36.3	31.2	0.2	−2.0
Year 2006+	13.8	19.0	13.9	12.9	−14.1	−0.2	2.6
Year 2008+	27.9	18.1	27.9	29.0	23.4	0.1	−2.5
Year 2010+	9.9	14.7	9.9	10.1	−14.7	0.0	−0.7
Year 2012+	8.5	12.4	8.6	7.5	−12.8	−0.3	3.8
Year 2014+	4.6	14.4	4.6	4.2	−33.8	0.0	1.8
N	283	15224					

Note (continued): Descriptive statistics for the ten industry sector dummies are not shown due to space limitations.
+ Mean represents a percentage share. [a] Includes non-smokers.

Table A.2
Sensitivity analyses: unemployment of males.

	Main	Placebo	Estimation issues				Alternative	Alternative	Alternative treatment		
	specification	regression	s.e.	PS	All Lasso	Not exact	sample	outcome	Plant closure	All reasons	Job loss
	(1)	(2)	(3)	(4)	(5)	(30)	(7)	(8)	(9)	(10)	(11)
Panel A: effect on smoking status											
Own unemployment	0.051**	-0.029	0.051**	0.056***	0.040*	0.056***	0.055**		0.044*	0.030**	0.017
	(0.021)	(0.018)	(0.021)	(0.021)	(0.021)	(0.021)	(0.024)		(0.023)	(0.015)	(0.013)
Spousal unemployment	0.042**	0.015	0.042**	0.038**	0.047**	0.035*	0.058***		0.068***	0.032**	0.028**
	(0.019)	(0.017)	(0.020)	(0.019)	(0.019)	(0.019)	(0.021)		(0.026)	(0.015)	(0.012)
p-value of difference	0.756	0.044	0.753	0.478	0.801	0.432	0.924		0.392	0.928	0.499
Panel B: effect on smoking intensity											
Own unemployment	0.094*	-0.044	0.094*	0.111**	0.062	0.103**	0.114*	0.063	0.074	0.050	0.016
	(0.054)	(0.051)	(0.054)	(0.052)	(0.055)	(0.052)	(0.062)	(0.394)	(0.055)	(0.039)	(0.034)
Spousal unemployment	0.114**	0.039	0.114**	0.105**	0.133***	0.100**	0.167***	0.775***	0.162**	0.110***	0.067**
	(0.048)	(0.041)	(0.049)	(0.046)	(0.048)	(0.047)	(0.053)	(0.292)	(0.063)	(0.040)	(0.029)
p-value of difference	0.761	0.130	0.758	0.929	0.288	0.959	0.493	0.132	0.215	0.248	0.215
Matching	EB	EB	EB	PS	EB	EB	EB	EB	EB	EB	EB
Clustered SE	HH	HH	PSU	HH	HH	HH	HH	HH	HH	HH	HH
Age in years	18-60	18-60	18-60	18-60	18-60	18-60	22-55	18-60	18-60	18-60	18-60
$N_{Treated}$	169	170	169	169	169	169	138	169	35	306	516
N	8574	8859	8574	8310	8574	8574	7411	8574	8440	8711	8921

Note: All regressions are estimated using the double-Lasso specification including entropy balancing (EB) or propensity score (PS) weights as indicated. Entropy balancing weights constructed using all double-Lasso covariates are applied in column (5). Matching is not exact on gender in column (6). The sample restricted to couples with directly affected spouses aged 22–55 years at baseline is in column (7). In column (8), smoking intensity is measured as the number of cigarettes smoked per day. Matching is on the first moment (mean) only in column (9). p-values indicate whether the effects of own and spousal unemployment are different. All regressions include state, industry, and year fixed effects. Standard errors clustered at the household (HH) or primary sampling unit (PSU) level are in parentheses, as indicated.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3
Sensitivity analyses: unemployment of females.

	Main	Placebo	Estimation issues				Alternative	Alternative	Alternative treatment		
	specification	regression	s.e.	PS	All Lasso	Not exact	sample	outcome	Plant closure	All reasons	Job loss
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: effect on smoking status											
Own unemployment	0.039***	-0.007	0.039***	0.039***	0.036**	0.033**	0.018		0.007	0.018	0.037***
	(0.015)	(0.017)	(0.014)	(0.014)	(0.017)	(0.014)	(0.014)		(0.011)	(0.017)	(0.014)
Spousal unemployment	-0.004	-0.006	-0.004	0.001	-0.003	0.006	0.006		0.010	0.013	0.018
	(0.016)	(0.023)	(0.016)	(0.013)	(0.016)	(0.014)	(0.015)		(0.008)	(0.015)	(0.012)
p-value of difference	0.051	0.962	0.045	0.061	0.098	0.197	0.542		0.780	0.800	0.253
Panel B: effect on smoking intensity											
Own unemployment	0.096***	-0.045	0.096***	0.099***	0.076**	0.087***	0.064*	0.561**	0.095***	0.035	0.077**
	(0.035)	(0.047)	(0.035)	(0.032)	(0.039)	(0.033)	(0.036)	(0.231)	(0.033)	(0.039)	(0.032)
Spousal unemployment	0.005	-0.018	0.004	0.026	0.017	0.040	0.044	0.359	0.053*	0.057	0.050
	(0.046)	(0.064)	(0.046)	(0.039)	(0.047)	(0.043)	(0.045)	(0.374)	(0.030)	(0.043)	(0.033)
p-value of difference	0.110	0.723	0.108	0.157	0.306	0.386	0.705	0.627	0.236	0.705	0.521
Matching	EB	EB	EB	PS	EB	EB	EB	EB	EB	EB	EB
Clustered SE	HH	HH	PSU	HH	HH	HH	HH	HH	HH	HH	HH
Age in years	18-60	18-60	18-60	18-60	18-60	18-60	22-55	18-60	18-60	18-60	18-60
$N_{Treated}$	114	105	114	114	114	114	89	114	32	249	329
N	6933	6971	6929	6529	6933	6933	6057	6933	6851	7068	7148

Note: All regressions are estimated using the double-Lasso specification including entropy balancing (EB) or propensity score (PS) weights as indicated. Entropy balancing weights constructed using all double-Lasso covariates are applied in column (5). Matching is not exact on gender in column (6). The sample restricted to couples with directly affected spouses aged 22–55 years at baseline is in column (7). In column (8), smoking intensity is measured as the number of cigarettes smoked per day. Matching is on the first moment (mean) only in column (9). p-values indicate whether the effects of own and spousal unemployment are different. All regressions include state, industry, and year fixed effects. Standard errors clustered at the household (HH) or primary sampling unit (PSU) level are in parentheses, as indicated.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Online Appendix

Institutional background

Germany's rather generous unemployment benefit system consists of two main components, *Arbeitslosengeld 1* and *Arbeitslosengeld 2*.

Arbeitslosengeld 1 mainly covers the transition from employment into involuntary unemployment. Its financial benefits amount to 60 percent of the average net income in the last twelve months and to 67 percent for individuals with children, or the maximum amount defined by the social security ceiling (2,964 Euro per month in 2012, for instance). In general, individuals are entitled to benefits for half the length they have paid contributions or a maximum of one year. In order to be eligible individuals must have paid contributions for at least 12 of the last 24 months. In recognition of increased difficulties of findings employment in older age, the maximum duration gradually extends to up to two years for individuals aged 58 years and older.

Subsequently to receiving *Arbeitslosengeld 1*, individuals with ongoing unemployment may transfer to *Arbeitslosengeld 2*, which aims to secure the individual's subsistence level and can be renewed on a yearly basis.

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