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RESEARCH ARTICLE

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The effect of unemployment on the smoking behavior of couples

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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

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

JEL CLASSIFICATION

I12; J63; J65; C23

1 | INTRODUCTION

When an individual becomes unemployed, it likely also affects their spouse in various ways. For instance, unemployment decreases income (Eliason & Storrie, 2006; Hijzen et al., 2010; Jacobson et al., 1993) and can result in social isolation (Kunze & Suppa, 2017), poorer mental health (Marcus, 2013; Schaller & Stevens, 2015), decreased life satisfaction (Kassenboehmer & Haisken-DeNew, 2009), and changed health behaviors (Deb et al., 2011; Gallo et al., 2001; Golden & Perreira, 2015; Marcus, 2014)—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 (Luhmann et al., 2014; Winkelmann & Winkelmann, 1995), divorce rates increase (Charles & Stephens, 2004), spousal labor force participation rates increase (Stephens, 2002), and spousal social activities decrease (Kunze & Suppa, 2017). Further, spousal mental health decreases (Bubonya et al., 2017; Clark, 2003; Marcus, 2013; Mendolia, 2014). 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 involuntary entries into unemployment, combining difference-in-differences (DiD) estimation with a matching strategy based on entropy balancing. For selecting control variables, we complement our econometric approach with two different procedures for control variable selection: a conventional approach based on previous studies and economic intuition as well as a machine learning approach based on Lasso regressions, the post-double-selection method (Belloni et al., 2014a, 2014b).

Using rich German Socio-Economic Panel (SOEP) data, we look at married and unmarried cohabiting couples approximately 1 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 about 8%. 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%. The effects of spousal unemployment are generally slightly smaller than that of own unemployment. Although 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 on intrahousehold 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] 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 & Brunello, 2017; Müller & Shaikh, 2018). Several studies document that job loss and unemployment decrease spousal mental health (Bubonya et al., 2017; Clark, 2003; Marcus, 2013; Mendolia, 2014) 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. We contribute to this literature by analyzing the spillover effects of unemployment on spousal smoking behavior. This question is not yet investigated, despite its relevance. From a public health perspective, the question is relevant because it allows for better understanding those factors that contribute to engaging in smoking, one of the leading causes of preventable deaths. In particular, it contributes to our understanding of whether individuals might change their smoking behavior later in life due to specific events in their or their spouse's lives. From a labor economics perspective, it is important to understand the full costs of unemployment, including potential externalities for spouses.

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

Although 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 (Golden & Perreira, 2015; Kassel et al., 2003), 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 & Sindelar, 2008; Fletcher & Marksteiner, 2017). This mechanism predicts an increase in smoking due to spousal unemployment if own unemployment increases smoking. Fifth, unemployment might increase

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

[2] Apart from studies focusing on involuntary 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. Further, there is also a literature examining the relationship between macroeconomic conditions (including unemployment rates) and health and health behaviors (Ruhm, 2000).

the time available for smoking or to invest in health (such as antiaddiction courses). This mechanism predicts an ambiguous effect for own unemployment and is less relevant for spousal unemployment. In summary, there might 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, whereas 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 socioeconomic 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 2 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]

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, whereas 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% of displaced workers leave within the 2 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 Section 4.3, 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.

[3]We construct the log transformation as ln(cigarettes+1) to avoid undefined values for nonsmokers and quitters. In the robustness section, we also consider changes in the number of cigarettes (in levels) and the inverse hyperbolic sine transformation of the number of cigarettes as outcome.
[4]Appendix S1 outlines the institutional background, that is, the German unemployment insurance system.
[5]We make this restriction as job changes affect well-being (Chadi & Hetschko, 2018), thus potentially impacting smoking.
[6]Plant closures account for approximately 5% of German unemployment (Schmitz, 2011).

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, that is, 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 nonrandom assignment of unemployment, we follow a standard approach and rely on the conditional independence assumption (Rosenbaum & 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.

3.1 | Combining matching and DiD

To increase the plausibility of the conditional independence assumption, our empirical strategy focuses on involuntary entries into unemployment and combines 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, whereas the DiD approach rules out selection on time-invariant unobservables that might affect both treatment and outcome (e.g., time-invariant unobserved personality traits). Our identification strategy assumes that there are no unobserved variables that simultaneously affect *changes* in the smoking behavior and the probability to become involuntarily unemployed.

To make the treatment and control groups more similar with respect to the selected control variables, we employ entropy balancing, a multivariate reweighting method focusing directly on achieving covariate balance (Hainmueller, 2012). This is the "matching step." 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 distributions on the first and second moment.[8] This produces a sample in which the means and variances of all selected control variables are the same in the treatment and control groups. Of all the possible weighting schemes that fulfill these balancing requirements, entropy balancing chooses the one where all weights are nonnegative and deviate the least from uniform weights.

Our empirical strategy regresses *changes* in smoking behavior (Y) of spouse S on the treatment indicator (D), controlling for C, the selected control variables ("regression step"). The resulting DiD estimation equation is written as

$$\Delta Y^S = \alpha^S + \delta^S \cdot D + C'\gamma^S + I'\eta^S + \varepsilon^S, \tag{2}$$

which is estimated by weighted least squares using the weights from entropy balancing.[9] I denotes fixed effects for states, industry sectors, and years, to address general differences in outcomes and treatment across regions, industry sectors,

[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.
[8] We perform entropy balancing using Stata's user-written program "ebalance" (Hainmueller & Xu, 2013) and applying the default tolerance level of 0.015.
[9] To see that Equation (2) constitutes a DiD equation, consider the following DiD-style equation that allows for a differential effect of the predetermined control variables in the two periods:

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

and time. 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 semiparametric DiD matching strategy. ε denotes the error term clustered at the household level.

Our empirical strategy is double robust (Bang & Robins, 2005) in the sense that we obtain unbiased estimates if either the set of variables that predicts treatment is correct or the set of variables that predicts changes in the outcome. However, we face the typical challenge of not knowing the correct set of control variables in Equations (1) and (2). 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. We apply two different approaches for the selection of control variables, which we outline below.

3.2 | Conventional control variable selection

The first approach for control variable selection is guided by economic intuition and based on the control variables used in related studies. Given the vast range of topics covered by the SOEP, we are able to include almost all control variables from related studies (see Table A1).[10] The control variables originate from the baseline wave. These control variables take into account factors that might predict treatment status and factors that might predict smoking behavior. We consider age, gender, migration background, job worries, German nationality, place of residence at the federal state level, type of residential district, home ownership, whether children live in the household, and marital status. Although these variables relate to selection into treatment by employees, employer-driven selection might pose another threat to our identification strategy. In order to account for this as well, we consider educational attainment, tenure, work experience measured as years worked full-time, income, previous unemployment experiences, occupational position, and a set of health-related variables. The health-related variables also include the smoking variables from the last two pre-treatment periods, thus capturing unobserved, time-invariant differences between the treatment and control groups. As treatment status plausibly also depends on firm characteristics (Disney et al., 2003; Müller & Stegmaier, 2015), we further control for firm size. We also consider demographic, labor market and health-related variables of the indirectly affected spouse. All these control variables constitute the *conventional set of control variables*, C_{CC}, and they are presented in Table A1.

3.3 | Double-Lasso-based control variable selection

Our second empirical approach for control variable selection relaxes functional form assumptions regarding the relationship between the control variables with treatment and outcomes. Contrary to the conventional set of control variables, this approach considers interactions between different control variables and higher order polynomials of these control variables.[11] As the resulting pool of potential control variables is extremely large (it encompasses 4,188 variables), we select relevant control variables from this large pool via Belloni et al. (2014a, 2014b) post-double-selection method based on Lasso regressions (hereafter, "double-Lasso"), a supervised machine learning method.

In the first part of this approach, we select variables predicting treatment status (C_D) via Lasso regression, and in the second part, we select variables predicting the outcome variables (C_{Yk})—again via Lasso regression. The double-Lasso derives its name from these two separate Lasso regressions. The Lasso estimator is expressed as

$$\hat{\boldsymbol{\beta}}^{\boldsymbol{L}} = \underset{\boldsymbol{\beta}\in\mathbb{R}^p}{\arg\min}\,\frac{1}{n}\left(\sum_{i=1}^{n}[d_i - \boldsymbol{v}_{\boldsymbol{i}}'\boldsymbol{\beta}]^2 + \lambda\sum_{j=1}^{p}|\beta_j|\right), \tag{3}$$

where $POST$ is an indicator variable for the period after the job loss. For the postperiod and preperiod, 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 (2), with $\Delta Y = Y^{POST} - Y^{PRE}$.

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

[11]Specifically, for each binary variable of the conventional set, we include interactions with all other (binary and continuous) variables, whereas for each continuous variable, we also include log, squared, and cubic terms.

where d_i is the treatment indicator (in the first part). $\boldsymbol{v_i}$ denotes the vector of potential control variables, p the number of potential control variables, and n the sample size. λ is the penalty factor.[12] $\hat{\boldsymbol{\beta}}^{\boldsymbol{L}}$ denotes the vector of coefficients solving Equation (3) 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. In the first part, we choose all variables with nonzero estimated coefficients based on Equation (3), C_D.[13]

In the second part, we select variables from the pool of potential control variables that predict the outcomes. Based on Equation (3), this step performs separate Lasso regressions for each of our four outcome variables (smoking status and intensity of each spouse), $C_{Yk}, k = 1, \ldots, 4$. That is, d_i now refers to the final outcome variables. The final *double-Lasso set of control variables*, $C_{DL} = \{C_D, C_{Y1}, C_{Y2}, C_{Y3}, C_{Y4}\}$, encompasses 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.

3.4 | Comparison of the procedures for control variable selection

The first procedure for control variable selection is more of a standard procedure in the literature. It is well known and computationally easier to implement. However, it also has some shortcomings. First, it assumes that the researchers know the correct functional form of including the control variables. Second, it is not clear whether the selected variables are actually relevant from an econometric perspective and selecting unnecessary variables may lead to a higher variance of the estimator. The double-Lasso procedure tries to overcome these two shortcomings. However, it is econometrically more complex and computationally more intense. Additionally, the selection of control variables is less intuitive, and interaction terms might be selected but not the constituting main effects.[14]

It is important to highlight that the main difference between the two procedures is with respect to the selection of the set of control variables—given that the relevant control variables are observed. Both procedures fail if an unobserved variable exists that—even after the other control variables have been taken into account—simultaneously affects *changes* in the smoking behavior and the probability to become involuntarily unemployed.

Table 1 provides summary statistics for the variables selected by double-Lasso, whereas Table A1 presents summary statistics for the conventional set of control variables. These tables also highlight the differences between the procedures for control variable selection. After entropy balancing, the standardized difference in means of all covariates used in the double-Lasso matching strategy (i.e., the C_D variables selected in the first part of the double-Lasso) is below 5% (see column 7 of Table 1), 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 the second part of the double-Lasso) satisfy this condition. Remaining imbalances are addressed in the regressions step. Although the treatment and control groups are rather similar after matching, following both procedures for control variable selection, some differences emerge. First, the standardized difference of the interaction terms in the C_D variables is generally smaller for the double-Lasso procedure. Second, the standardized difference for the variables without interactions is generally smaller for the conventional set of controls (see also Table A1). Both points are not surprising, given that the double-Lasso procedure also considers interaction terms, whereas the conventional set considers *all* variables (without interactions). However, Tables 1 and Table A1 show that both procedures also substantially reduce the imbalance for variables that are not considered in the respective procedure (e.g., the double-Lasso procedure improves covariate balance also for variables that are not selected by double-Lasso).

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% of directly affected spouses and 32.9% vs. 27.4% of indirectly affected spouses).[15]

[12]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 $c = 1.1, \gamma = 0.1$.

[13]For double-Lasso, we include only this first set of variables, C_D, in the matching step to highlight the double-robust property of our strategy: We obtain unbiased estimates if either the matching step is correctly specified (i.e., we have the correct set of C_D variables) or the regression step (i.e., the correct set of C_{Yk} variables). The results are robust to including both sets of variables in matching and regression step.

[14]However, as in most matching applications, the interest is not in interpreting the coefficients of the control variables. The only purpose of the control variables is to make the treatment and control groups more similar.

[15]Table A1 provides the standardized differences based on propensity score weighting (control group weights constructed as $PS(C_c)/(1 - PS(C_c))$, where $PS(C_c)$ is the propensity score) for the conventional set of control variables. Although propensity score weighting works well in balancing the

TABLE 1 Summary statistics for double-Lasso selected control variables

Variable		Means treated (1)	Means controls: Raw (2)	Means controls: CC-EB (3)	Means controls: DL-EB (4)	Std. difference (%): Raw (5)	Std. difference (%): CC-EB (6)	Std. difference (%): DL-EB (7)
Predictor variables: Treatment								
Log tenure		1.7	2.4	1.8	1.7	−69.6	−2.1	0.4
Log labor earnings		9.6	10.3	9.6	9.6	−48.3	2.2	1.5
Interaction terms								
(age)	(major job worries)	20.0	6.3	19.8	19.9	66.7	0.5	0.2
(basic schooling)	(regional unemployment)	3.8	2.0	3.5	3.8	40.4	6.1	0.2
(basic schooling)	(major job worries)[a]	0.2	0.0	0.2	0.2	44.6	−0.2	0.0
(ever smoker)	(major job worries)[a]	31.4	8.9	29.9	31.4	58.4	3.4	0.1
(blue collar worker)	(small company)[a]	22.6	6.4	19.6	22.6	47.3	7.3	0.1
(blue collar worker)	(major job worries)[a]	26.1	6.5	27.5	26.1	55.0	−3.0	0.1
(poor health)	(spouse non-German)[a]	5.7	1.4	3.6	5.7	23.4	9.8	−0.0
(spouse not working)	(spouse overweight or obese)[a]	23.0	11.0	21.2	23.0	32.2	4.2	0.0
(small company)	(major job worries)[a]	13.8	2.5	14.0	13.8	42.4	−0.6	0.0
(small company)	(spouse not working)[a]	12.0	3.3	11.7	12.0	33.4	1.0	−0.0
Union of predictor variables: All outcomes								
Baseline smoker[a]		38.2	29.2	38.0	37.4	19.0	0.2	1.6
Lagged baseline smoker[a]		42.4	30.5	42.3	39.0	24.9	0.3	7.0
Ever smoker[a]		69.3	61.5	69.0	68.3	16.3	0.5	2.1
Log no. of cigarettes per day[b]		1.0	0.8	1.0	1.0	20.0	0.3	0.9
Lagged log no. of cigarettes per day[b]		1.2	0.8	1.2	1.1	26.1	0.3	5.5
Lagged squared log no. of cigarettes per day[b]		3.4	2.3	3.4	3.2	26.2	0.1	4.5
Spouse baseline smoker[a]		32.9	27.4	32.8	34.3	11.9	0.2	−3.1
Spouse lagged baseline smoker[a]		33.6	28.7	33.5	35.2	10.6	0.2	−3.5
Spouse ever smoker[a]		62.5	60.3	62.3	63.8	4.7	0.5	−2.6
Spouse log no. of cigarettes per day[b]		0.9	0.7	0.9	0.9	15.0	0.3	−0.1
Spouse lagged log no. of cigarettes per day[b]		0.9	0.8	0.9	1.0	13.2	0.3	−2.0
Spouse lagged squared log no. of cigarettes per day[b]		2.7	2.1	2.7	2.7	14.5	0.0	−0.6
Interaction terms								
(years full-time)	(spouse baseline smoker)	6.3	5.2	6.3	6.5	11.1	0.3	−1.3
(blue collar worker)	(spouse ever smoker)[a]	33.2	19.8	35.3	35.3	30.8	−4.3	−4.3
(vocational training)	(spouse ever smoker)[a]	48.1	47.2	48.5	50.5	1.7	−0.9	−4.8
(physical health)	(spouse ever smoker)	30.5	30.9	30.6	32.1	−1.7	−0.4	−6.2
(baseline smoker)	(never unemployed)[a]	11.3	17.9	13.0	15.2	−18.8	−5.0	−11.4
(ever smoker)	(blue collar worker)[a]	38.9	22.4	40.5	40.1	36.3	−3.3	−2.4
(ever smoker)	(children)[a]	30.0	28.4	29.2	30.1	3.7	1.8	−0.1
(ever smoker)	(spouse no. of cigarettes per day)[b]	5.0	3.4	4.9	4.6	19.9	1.3	4.7
(ever smoker)	(spouse ever smoker)[a]	50.2	42.8	48.4	48.6	14.9	3.6	3.2
(spouse baseline smoker)	(spouse works full-time)[a]	13.1	15.4	13.4	16.5	−6.5	−1.1	−9.6
(spouse baseline smoker)	(spouse priv. health insur.)[a]	2.5	2.7	1.9	2.0	−1.6	4.1	2.8
(spouse baseline smoker)	(spouse never unemployed)[a]	11.3	14.5	12.5	15.0	−9.6	−3.7	−11.0
(children)	(spouse ever smoker)[a]	26.9	26.8	25.5	28.0	0.2	3.1	−2.5
N		283	15,224					

Note. The pre-treatment means of variables for the treatment and control groups are in the first and second column, respectively. Lagged variables refer to observations from the first pre-treatment period. The means of the reweighted control group using entropy balancing weights (CC-EB: with the conventional set of control variables; DL-EB: with the double-Lasso set of control variables) are in the third and fourth column, 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. (variable a) (variable b): interaction term of variable a and variable b.

[a]The mean represents a percentage share.

[b]Includes nonsmokers.

TABLE 2 Main results: Effect of unemployment on smoking behavior

	Smoking status			Smoking intensity		
	Simple DiD (1)	Matched DiD (2)	Double-Lasso matched DiD (3)	Simple DiD (4)	Matched DiD (5)	Double-Lasso matched DiD (6)
(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	.211	.818	.394	.563	.534	.891
N_{Treated}	283	283	283	283	283	283
N	15,507	15,507	15,507	15,507	15,507	15,507
(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	.827	.627	.756	.662	.259	.761
N Treated	169	169	169	169	169	169
N	8,574	8,574	8,574	8,574	8,574	8,574
(c) Unemployment of females						
Own unemployment	0.059***	0.037**	0.039***	0.144***	.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	.052	.202	.051	.056	.549	.110
N_{Treated}	114	114	114	114	114	114
N	6,933	6,933	6,933	6,933	6,933	6,933
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* < .1.

***p* < .05.

****p* < .01.

4 | RESULTS

Table 2 starts with a simple DiD model that looks at differential changes in the outcomes between the treatment and control groups without control variables, except the lagged dependent variable from the last pre-treatment observation.[16] 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 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.

control variables, entropy balancing generally produces better balancing. Propensity score weighting even increases the standardized difference for some variables.

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

TABLE 3 Heterogeneous treatment effect by individual baseline smoking status

	Never smokers (1)	Former smokers (2)	Smokers (3)
(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	5,944	5,008	4,555
Spousal unemployment	−0.005***	0.027	0.066**
	(0.001)	(0.034)	(0.028)
N_{Treated}	106	84	93
N	6,156	5,083	4,268
(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	5,944	5,008	4,555
Spousal unemployment	−0.010***	0.101	0.170**
	(0.002)	(0.088)	(0.079)
N_{Treated}	106	84	93
N	6,156	5,083	4,268

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.
*$p < .1$.
**$p < .05$.
***$p < .01$.

The direct effect is similar for unemployment of males and females (panels b and c). However, there is a clear gender difference regarding the spillover effect: Among indirectly affected spouses, the effect is 5.6 to 4.2 percentage points when the male becomes unemployed (columns 2 and 3, respectively) 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% of directly and indirectly affected spouses smoke at baseline, respectively (see Table A1). Hence, unemployment increases the prevalence of smoking by 11.0% for the directly affected and by 7.6% to 11.2% for indirectly affected spouses.

The effects on smoking intensity exhibit a similar pattern (columns 4–6). The results from columns 5 and 6 suggest that unemployment increases the daily number of cigarettes smoked by 8.2% and 8.5% for directly affected individuals, respectively, and by 11.3% and 7.8% for indirectly affected spouses, respectively. 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 (15.1% vs. 7.7% in column 5 and 11.4% vs. 9.4% in column 6, 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.

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. Although the results suggest that treatment effects are most precisely estimated with the double-Lasso specification, according to the Bayesian information criterion, sometimes it is the specification with the conventional set of controls that is preferred and sometimes it is the specification with the double-Lasso-based controls.

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

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

	Pooled sample				Without never smokers		
	Both nonsmokers	Mixed smoker couples		Both smokers	Both former smokers	Mixed smoker couples	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(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	.085	.389	.747	.787	.776	.153	.895
(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	.270	.395	.594	.881	.688	.159	.456
N_{Treated}	140	35	50	58	35	24	25
N	9,011	1,941	2,228	2,327	2,075	1,079	1,173
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	9,011	1,941	2,228	2,327	2,075	1,079	1,173

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.

*$p < .1$.

**$p < .05$.

***$p < .01$.

TABLE 5 Analysis of mechanisms using double-Lasso

	Time	Stress				
	Satisfaction with leisure time	Financial stress	Smoking status		Smoking intensity	
	(1)	(2)	(3)	(4)	(5)	(6)
(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	.000	.000	.765	.087	.638	.225
N_{Treated}	279	278	124	159	124	159
N	15,301	15,415	8,117	7,390	8,117	7,390
(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	.000	.002	.518	.711	.101	.995
N_{Treated}	168	164	49	120	49	120
N	8,502	8,520	2,515	6,059	2,515	6,059
(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	.065	.000	.257	.000	.259	.000
N_{Treated}	111	114	75	39	75	39
N	6,799	6,895	5,602	1,331	5,602	1,331
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 the 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.

*$p < .1$.

**$p < .05$.

***$p < .01$.

smoking histories at the couple level (see Table 4), only differentiating between baseline smokers and baseline nonsmokers (grouping never and former smokers together initially, later looking specifically at former smokers).[17]

Table 3, column 1, shows that neither own nor spousal unemployment increases the probability 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.[18] The results for former smokers in column 2 show that own unemployment increases the probability of smoking relapse and smoking intensity by approximately 10 percentage points and 20%, 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%, respectively. Panel (b) shows

[17]In all columns in Tables 3 and 4, we show results for a simple, unmatched DiD 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 specifications based on entropy balancing. 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.

[18]We do not want to 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.

TABLE 6 Sensitivity analyses using double-Lasso

	Main specification	Placebo regression	Estimation issues				Alternative sample	Alternative outcome		Alternative treatment		
			SE	PS	All Lasso	Not exact		Raw	IHS	Plant closure	All reasons	Job loss
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(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	.394	.194	.394	.270	.593	.413	.964			.381	.868	.974
(b) Effect on smoking intensity												
Own unemployment	0.085^{**}	−0.031	0.085^{**}	0.097^{***}	0.067^{*}	0.085^{**}	0.091^{**}	0.205	0.104^{**}	0.102^{**}	0.046	0.038
	(0.039)	(0.037)	(0.038)	(0.037)	(0.040)	(0.037)	(0.044)	(0.281)	(0.048)	(0.042)	(0.030)	(0.025)
Spousal unemployment	0.078^{**}	0.023	0.078^{**}	0.082^{**}	0.087^{**}	0.084^{**}	0.125^{***}	0.670^{***}	0.094^{**}	0.122^{***}	0.094^{***}	0.061^{***}
	(0.036)	(0.038)	(0.037)	(0.034)	(0.037)	(0.034)	(0.040)	(0.249)	(0.044)	(0.045)	(0.032)	(0.022)
p value of difference	.891	.250	.889	.762	.693	.987	.558	.200	.877	.702	.238	.464
Matching	EB	EB	EB	PS	EB	EB	EB	EB	EB	EB	EB	EB
Clustered *SE*	HH	HH	PSU	HH	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	18–60
N_{Treated}	283	275	283	283	283	283	227	283	283	67	555	845
N	15,507	15,830	15,503	14,839	15,507	15,507	13,468	15,507	15,507	15,291	15,779	16,069

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 columns 8 and 9, smoking intensity is measured as the number of cigarettes smoked per day and as the inverse hyperbolic sine (IHS) transformation of the number of cigarettes smoked per day, respectively. Matching is on the first moment (mean) only in column 10. *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.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

the intensive margin effects conditional on smoking at baseline, whereas 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%, 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 & 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 nonsmokers 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 with nonsmoker 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%). Thus, having a nonsmoking partner appears to have a protective effect for the directly affected spouse.

Moreover, there is evidence that a nonsmoking 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 nonsmoker (3.6 vs. 8.2 percentage points and 4.7% vs. 23.7%, respectively). Similarly, for indirectly affected spouses who are nonsmokers at baseline (columns 1 and 3), the effects of spousal unemployment on smoking are smaller if their directly affected partner is a nonsmoker at baseline. Generally, we obtain the largest effects if both spouses smoke at baseline (column 4). Thus, having a nonsmoking 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 nonsmoking partner among couples consisting only of smokers and former smokers at baseline (i.e., excluding never smokers). Despite decreased precision, having a nonsmoking 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.[19] Analogous to the specification based on the double-Lasso set of controls in Table 5, Table A2 shows the results for the specification based on the conventional set of control variables. 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 because 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 (Golden & Perreira, 2015; Kassel et al., 2003). Tables 5 and A2 investigate this mechanism, showing that male unemployment increases financial stress for both spouses, whereas female unemployment only increases financial stress for the females themselves.[20] 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

[19]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.

[20]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*).

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, that is, when the directly affected spouse is the main breadwinner. This pattern is similar for smoking intensity (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, whereas Tables A3 and A4 provide the gender-specific results.[21] We first run placebo regressions to assess the plausibility of our identifying assumption. Here, the treatment variable takes the value of one, 2 years before the actual treatment.[22] 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 both parts of the double-Lasso procedure (i.e., C_D and C_{Yk}), 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. Columns 8 and 9 consider the number of cigarettes and the inverse hyperbolic sine transformation of the number of cigarettes, respectively, for constructing the outcome variable (and not its log). Our results are robust regarding all these estimation issues.

Columns 10 to 12 present results for alternative treatment definitions. When only considering unemployment spells resulting from plant closures (column 10), 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 11). Column 12 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.

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%. This reflects an increase in smoking prevalence of approximately 11% and 7% among spouses directly and indirectly affected by unemployment, respectively. Although 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. Although we apply two different procedures for control variable selection (conventional and machine learning based) that provide similar results, both procedures fail if an unobserved variable exists that simultaneously affects *changes* in the smoking behavior and the probability to become involuntarily unemployed.

[21] Although these tables are based on the double-Lasso set of controls, Table A5 as well as Tables O.1 and O.2 in the Supporting Information analogously show robustness tests based on the conventional set of control variables.
[22] Based on this placebo treatment indicator, we repeat the matching and regression step, except this time all baseline characteristics are from 2 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.

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 intrahousehold 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 (Doll et al., 2004; Taylor et al., 2002) and morbidity (Østbye & 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. Generally, our findings highlight the relevance of intrahousehold spillover effects of major life events, even with respect to health behaviors.

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CONFLICT OF INTEREST

None.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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