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# Does job loss make you smoke and gain weight?

Jan Marcus<sup>1</sup> (DIW Berlin)

This paper estimates the effect of involuntary job loss on smoking behavior and body weight using German Socio-Economic Panel Study data. Baseline nonsmokers are more likely to start smoking due to job loss, while smokers do not intensify smoking. In particular, single individuals as well as those with lower health or socioeconomic status prior to job loss exhibit high rates of smoking initiation. Job loss increases body weight slightly, but significantly. The applied regression-adjusted semiparametric difference-in-difference matching strategy is robust against selection on observables and time-invariant unobservables. This paper provides an indirect test that the identifying assumption is not violated.

**JEL:** I12, J65

**Keywords:** Job loss, smoking, health behavior, difference-in-difference, propensity score matching

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# 1 Introduction

The loss of employment has a deep impact on the individual affected. It not only reduces income in both the short and long run ([Jacobson et al. 1993](#)), it also results in a plethora of other negative issues, such as decreased life-satisfaction ([Knabe & Rätzel 2011](#)), increased risk of divorce ([Charles & Stephens 2004](#)) and further job losses ([Stevens 1997](#)) as well as negative consequences for the children of those affected ([Lindo 2011](#)). This paper contributes to the literature on the effects of job loss by looking at the impact of involuntary job loss on smoking behavior and body weight. Knowing more about the overall consequences of job loss is crucial for policy-makers comparing the costs and benefits of labor market policies to prevent job losses. Furthermore, analyzing the effects of job loss on health behaviors may help us to better understand two scientific puzzles: Why job loss increases mortality at the individual level ([Eliason & Storrie 2009](#); [Sullivan & von Wachter 2009](#)) and why - at an aggregated level - health behaviors improve when the unemployment rate is high ([Ruhm & Black 2002](#); [Ruhm 2005](#)). The topic is also relevant from a public health perspective as smoking and excess body weight are among the major causes of preventable deaths.

There is ample evidence that unemployed individuals engage in unhealthy behaviors more often than those in employment (see [Henkel 2011](#); [Roelfs et al. 2011](#)). However, it is unclear whether this represents the causal effect of unemployment, the reverse causal effect or a spurious correlation. Most studies on the effect of job loss on health behaviors suffer from various shortcomings. The studies focus on correlations, concentrate on case-studies, do not deal with the endogeneity of job loss, or do not draw on longitudinal data (see [Roelfs et al. 2011](#)). This paper addresses these shortcomings by reverting to population-representative survey data, by looking at involuntary job loss, and by applying a regression-adjusted semiparametric difference-in-difference matching strategy. This strategy is not only robust against selection on observables (like conventional matching estimators), but also against selection on unobservables with time invariant effects (e.g., ability, childhood conditions). To interpret the estimated effects as causal, the estimation strategy assumes that no unobservable variables exist that simultaneously influence the probability of job loss and *changes* in health behaviors. This paper provides an indirect test to show that this identifying assumption is not violated in the present case.

Empirical evidence on the effects of job loss on health behaviors is mixed, although most studies find that job loss changes health behaviors for the worse (see [Henkel 2011](#);

Roelfs et al. 2011). It seems likely that these contradictory findings are at least partially due to methodological weaknesses. Only a few studies rely on longitudinal data and simultaneously do not concentrate on single plants. Among these studies, Morris et al. (1992) find job loss-related increases in body weight but no increases in either smoking or drinking. Another British study (Montgomery et al. 1998) finds an increased risk of smoking and *low* body weight among male participants with experience of unemployment - even when controlling for health behaviors at age 16.

Two studies using the U.S. Health and Retirement Survey (HRS) provide the most sophisticated strategies for the identification of causal effects, as they rely on longitudinal data and focus on involuntary job loss. Falba et al. (2005) show that job loss increases the probability of smoking relapse and increases the daily number of cigarettes smoked by existing smokers. Deb et al. (2011) find an increase in drinking and in the probability of being overweight, but only for individuals who already had poor health behaviors prior to job loss. This paper supplements these two studies by applying a different estimation technique, and by looking at the effect of job loss in a different welfare regime. While Falba et al. (2005) apply logit/OLS regressions and Deb et al. (2011) finite mixture models, this paper resorts to matching, an intuitive approach that does not rely as much on linearity assumptions. Due to the HRS sampling design these two studies can only look at individuals over 50, while the data of the German Socio-Economic Panel Study (SOEP) allows for an analysis of the entire range of working-age individuals. By focusing on Germany this paper looks at a country with more generous unemployment assistance than the U.S.

Using SOEP data for 2002-2010, this paper finds that job loss increases the probability of smoking initiation by 3 percentage points (55 percent) on average. However, there is little evidence that baseline smokers, i.e. individuals who smoked before the job loss, intensify their smoking or are less likely to stop smoking due to job loss. Job loss increases body weight slightly (by around  $0.1 \text{ kg/m}^2$ ), but significantly. These findings emerge regardless as to whether only individuals who lost their jobs due to plant closure are considered, or all individuals experiencing job loss due either to plant closure or dismissal. The results are robust over various matching specifications and different choices of the conditioning variables. Further analyses indicate treatment effect heterogeneity. In particular singles, younger individuals and individuals with lower health or socioeconomic status prior to job loss exhibit high rates of smoking initiation. The increase in body weight is larger for women and overweight individuals, but still well below  $0.5 \text{ kg/m}^2$ . In general, the effects on smoking and body weight are smaller than comparable findings for the U.S., which might be attributable to the more generous unemployment

assistance in Germany.

## 2 Related Literature

Initially the economic literature on the impact of job loss focused on lost earnings (see e.g. [Jacobson et al. 1993](#)). Research in the field developed and started looking at the implications of job loss beyond mere financial consequences to acquire a more exhaustive picture of job loss. This is of particular importance for policy-makers comparing the costs and benefits of labor market policies that prevent job losses; especially in the financial and economic crisis that enveloped the world starting in 2008. During this crisis the rate of job loss increased or is expected to rise in many western countries, e.g. in the U.S. the rate of job loss is on a record high ([Farber 2011](#)).

Previous research finds that the loss of employment strongly reduces life satisfaction ([Kassenboehmer & Haisken-DeNew 2009](#); [Knabe & Rätzl 2011](#)), increases the risk of divorce ([Charles & Stephens 2004](#)) and decreases fertility ([Del Bono et al. 2012](#)). Other studies analyze the health consequences of job loss. Unemployed individuals experience higher mortality (see [Roelfs et al. 2011](#)) and several studies provide evidence that this is (partially) due to the loss of employment and not merely due to poorer health status or other observable factors prior to job loss ([Eliaison & Storrie 2009](#); [Sullivan & von Wachter 2009](#)).<sup>2</sup>

At the macro-level, however, increases in the unemployment rate are found to *reduce* mortality ([Ruhm 2000](#)). While infant health also improves at aggregated levels when unemployment is high ([Dehejia & Lleras-Muney 2004](#)), at the individual level the loss of employment is found to reduce the birth weight of own children ([Lindo 2011](#)). Furthermore, physical inactivity and body weight as well as tobacco and alcohol consumption decrease during economic downturns ([Ruhm & Black 2002](#); [Ruhm 2005](#)). However, it is not clear whether the improvement in health behavior is driven by the unemployed or by those individuals who are still in employment but working less.

The literature discusses several arguments as to why job loss might affect smoking and body weight. From a theoretical point of view it is not clear whether job loss improves

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<sup>2</sup>However, there are other studies with sophisticated identification strategies that do not find significant adverse health effects of job loss ([Browning et al. 2006](#); [Böckerman & Ilmakunnas 2009](#); [Salm 2009](#); [Schmitz 2011](#)). These studies look at morbidity and health status indicators rather than at mortality. [Kuhn et al. \(2009\)](#) find increases in public health costs, resulting from job loss, predominantly for the treatment of mental health problems.

or worsens health behaviors.<sup>3</sup> One of the most relevant mechanisms might be stress. Medical studies find that stress increases both eating ([Adam & Epel 2007](#)) and smoking ([Kassel et al. 2003](#)). Hence, to cope with the stress associated with job loss, individuals might increase calorie and nicotine intake as a form of self-medication. Job loss-related stress might result from reduced income, the fear of not finding a new job and the loss of the non-financial benefits of work, such as respect of others, adhering to social norms and, therefore, identity ([Akerlof & Kranton 2000](#)).

Job loss might also affect health behaviors through an income effect. The loss in income might reduce the number of cigarettes smoked. The income effect on body weight is ambiguous (consumption of less food vs. cheaper, less healthy food). However, unemployment benefits are comparably generous in Germany. Recipients get 60% of their previous income (or 67 % if they have a child) for up to 24 months, depending on the duration of their own contributions to unemployment insurance. Moreover, the unemployed can receive tax-financed unemployment assistance, if unemployment insurance payments are below a certain threshold or if the payment period expires (see [Caliendo et al. 2009](#)). Other arguments as to why job loss might affect smoking and body weight include increased leisure time (with ambiguous effects on health behaviors) and a shift in the individual's time preference towards the present ([Schunck & Rogge 2010](#)).

Research on the impact of job loss on health behaviors contributes to the literature in several ways. Firstly, it investigates one of the potential mechanisms as to why job loss increases mortality. Job loss might have negative health consequences if health behaviors deteriorate. Secondly, analyzing the effect of job loss on smoking and body weight deepens our understanding of why aggregated health behaviors improve during economic downturns. Thirdly, the effect of job loss on health behavior is also relevant from a public health perspective. Smoking and excess body weight are first and third among the major causes of preventable deaths in industrialized countries according to the [World Health Organization \(2009\)](#). Policies aimed at reducing the prevalence of smoking and being overweight may be more effective if they consider the vulnerability of specific groups, such as individuals who have lost their employment.

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<sup>3</sup>In a strict sense, body weight is a state rather than a behavior. Yet, for the purpose of this study, body weight is considered to be a health behavior as it actually combines two health behaviors, eating (calories in) and exercising (calories out).

### 3 Empirical Strategy

To investigate the causal effect of job loss on body weight, smoking status and the daily number of cigarettes smoked, this paper applies a regression-adjusted difference-in-difference (DiD) matching strategy, similar to the one proposed by [Heckman et al. \(1997\)](#). The general idea of this estimator is to compare individuals who have lost their jobs with (nearly identical) individuals who have not lost their jobs and to see how the health behaviors of these two groups changed. This study focuses on the average treatment effect on the treated (ATT), i.e. changes in health behaviors brought about by the job loss of those who actually lost their employment. The identifying assumption for this ATT is that no unobserved variables exist that determine job loss and simultaneously influence a *change* in health behaviors, i.e. in the absence of treatment (job loss) the health behaviors of treated (individuals experiencing job loss) and matched controls (similar individuals not experiencing job loss in the period) follows the same trend. In formal notation the identifying assumption is given as:

$$E_{P|D=1}E[Y_0^a - Y_0^b|P, D = 1] = E_{P|D=1}E[Y_0^a - Y_0^b|P, D = 0], \quad (1)$$

where  $\Delta Y_0 = Y_0^a - Y_0^b$  refers to the change in health behaviors from before ( $b$ ) to after ( $a$ ) the treatment in the absence of treatment and  $D$  denotes the treatment group indicator.  $P$ , the propensity score, is the conditional probability of job loss, i.e.  $P = P(D = 1|S^b)$ , where  $S^b$  is a set of conditioning variables obtained in the pre-treatment period.

In general, DiD matching estimators of the ATT can be written as ([Heckman et al. 1997](#); [Smith & Todd 2005](#)):

$$\widehat{ATT} = \frac{1}{n_1} \sum_{i \in I_1} \left[ \Delta Y_{1i} - \sum_{j \in I_0} \omega(i, j) \cdot \Delta Y_{0j} \right], \quad (2)$$

where  $n_1$  is the number of cases in the treatment group  $I_1$ .  $I_0$  indicates the control group observations and  $\omega(i, j)$  is a matching procedure specific weight. For instance, for  $k$  nearest neighbor matching the weights  $\omega(i, j)$  take on the value  $1/k$  for the  $k$  nearest control neighbors of each treated  $i$  and 0 for all other non-treated. The ATT can be also expressed in matrix notation as:

$$\hat{\beta} = (\mathbf{X}'\mathbf{W}\mathbf{X})^{-1}\mathbf{X}'\Delta y, \quad (3)$$

where  $\Delta y$  is the vector of health behavior changes and  $\mathbf{W}$  a diagonal matrix with 1

as diagonal element for individuals of the treatment group and  $\omega(j) = \sum_{i \in I_1} \omega(i, j)$  for control group members. In the case of matching without regression adjustment,  $\mathbf{X}$  is a  $n$ -by-2 matrix, in which the first column consists of a vector of 1s and the second column of the values of  $D$  for each individual. With regression adjustment  $\mathbf{X}$  contains an additional column for each adjustment variable.<sup>4</sup>

I apply a three-step procedure to estimate the ATT. First, I estimate the propensity score, then I compute the weighting matrix of equation (3) and finally I perform a weighted regression to compute the ATT. In the first step, I estimate the propensity score by Probit regressions on the pooled sample. I do not revert to the propensity score directly but instead use the linear index of the propensity score, which is particularly effective at improving the balance between treated and controls (Rosenbaum & Rubin 1985a). In order to prevent the comparison of treated and untreated that are not comparable, I restrict the analysis to the region of common support. The analysis excludes treated observations whose linear propensity score exceeds the maximum of the linear propensity score in the control group or falls below its minimum. For kernel matching, the Epanechnikov kernel works as an additional common support condition since it matches only control observations within a specific interval around each treated observation.

In the second step, I compute the weights within cells defined by baseline smoking status (yes/no) and survey year according to the distance in the linear propensity score.<sup>5</sup> This is equivalent to exact matching on survey year and smoking status. As equations (2) and (3) show matching procedures basically differ with respect to the weights.<sup>6</sup> Asymptotically, all matching procedures produce the same results because they reduce to exact matching in infinite samples (Caliendo & Kopeinig 2008). For finite control

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<sup>4</sup>Morgan & Harding (2006) use a similar notation for introducing propensity score weighting (PSW). PSW differs notationally only with respect to the construction of  $\mathbf{W}$ . For each control group member  $j$  the diagonal element of  $\mathbf{W}$  equals  $\omega(j) = \widehat{P}_j / (1 - \widehat{P}_j)$ . Hence,  $\omega(j)$  is purely a function of the estimated propensity score,  $\widehat{P}_j$ , and not of the distance to any treatment observation. PSW *asymptotically* yields the same estimate as the mean difference between treated and matched controls. The matching estimator presented here, however, yields (in the absence of regression adjustment) numerically *identical* ATT estimates as the mean difference between treated and matched controls because this weighting method is basically a rewriting exercise. PSW is sensitive to misspecification of the propensity score equation because the propensity score enters the weighting function directly (Caliendo & Kopeinig 2008; Stuart 2010). The present estimator does not suffer from this shortcoming as it is just a rewriting of matching estimators and the propensity score does not enter the weighting directly.

<sup>5</sup>I use the program “psmatch2” (Leuven & Sianesi 2003) in Stata 11.2 to compute  $\omega(j)$ .

<sup>6</sup>See Smith & Todd (2005), Caliendo & Kopeinig (2008) and Stuart (2010) for excellent descriptions of the various matching procedures.



groups, there is no one best matching procedure for all situations (Caliendo & Kopeinig 2008). Yet, Heckman et al. (1997) and Smith & Todd (2005) argue for the use of kernel matching, which is the method primarily used in this study. To test the sensitivity of the results with respect to different matching procedures, I also apply 5-to-1 nearest neighbor caliper matching as a robustness check. For kernel matching the weights take on the form

$$\omega(i, j) = \frac{K \left[ \frac{P_j - P_i}{b_n} \right]}{\sum_j K \left[ \frac{P_j - P_i}{b_n} \right]}, \quad (4)$$

where  $P$  is the linear index of the propensity score,  $K[\cdot]$  is a specific kernel function and  $b_n$  is a bandwidth parameter. There is a general agreement that the choice of the kernel is less crucial than the choice of the bandwidth. I use the Epanechnikov kernel due to its slight superiority in terms of efficiency. The chosen bandwidth is 0.06 as applied by Heckman et al. (1997). Although Smith & Todd (2005) find their results to be robust against different bandwidth choices, in the section on robustness checks I apply a different bandwidth. The third step is the regression step, where I compute the ATT according to equation (3). I include all conditioning variables  $S$  from the propensity score equation in  $\mathbf{X}$ . While the calculation of the weights is performed within cells, the ATT is computed for the pooled sample.

The applied regression-adjusted difference-in-difference matching estimator improves conventional matching estimators in several ways. First, the DiD matching estimator eliminates the time-invariant outcome difference between individuals in the treatment and control group. The pure cross-sectional matching estimator assumes that all relevant variables that determine job loss and influence the *level* of health behaviors are included in the data set - and incorporated into the model. This is a much stronger assumption. Second, the regression-adjusted matching estimator remains consistent if either the propensity score equation or the regression equation is correctly specified. Therefore, the researcher has two opportunities to deal correctly with the selection bias and, hence, the model can be regarded as doubly robust (Bang & Robins 2005). The inclusion of all variables from the propensity score estimation reduces the effect of prevailing covariate imbalances after matching and, thus, decreases the small sample and asymptotic bias of the matching estimator (Abadie & Imbens 2006). Furthermore, including relevant variables in the regression step decreases unexplained variance in the outcome and, hence, the standard errors of the treatment effect estimates. This is similar to including control variables in randomized experiments. Third, I combine propensity score matching with exact matching on survey year and baseline smoking status, which I

regard as crucial variables for the purpose of this study. Smoking initiation and smoking cessation are different decisions and the distinction between these two decisions is useful (DeCicca et al. 2008). Therefore, I display smoking results separately for smokers and nonsmokers and match exactly on the smoking status to compare only like and like. Performing inexact matching on survey year would imply comparing individuals from different time periods. Additionally, matching on the survey year allows to control for general macroeconomic trends (like cigarette taxes, which are the same everywhere in Germany at a given point in time). Fourth, the matching and analysis steps are clearly separated. Therefore, the quality of the matching procedure is evaluated *prior* to the computation of the ATT.

To highlight some benefits of the applied matching procedure, the presentation of the results in table 3 starts with a simple specification without pre-treatment health behavior information and without exact matching. The other specifications gradually incorporate the more sophisticated procedures.

There is a debate in the literature as to how to estimate the variance of propensity score-based matching estimators (see Caliendo & Kopeinig 2008; Stuart 2010). The crux is that the variance estimation should take into account uncertainty in the propensity score estimation. Although there is evidence that standard errors are *overestimated* if the estimated propensity score is used instead of the true propensity score (see Stuart 2010). Hence, if the uncertainty in the propensity score model is not considered, erring might be on the conservative side. Usually bootstrapping constitutes a popular way of estimating standard errors when they are difficult to compute analytically or when the theoretical distribution of the relevant statistic is unknown. However, there is no formal justification for the application of the bootstrap in the case of matching and Abadie & Imbens (2008) show that bootstrapping fails in the case of nearest neighbor matching. Therefore, this study does not rely primarily on bootstrapped standard errors.<sup>7</sup> Instead I use robust standard errors from the weighted regressions. Robustness tests in the full working paper show that the applied standard errors are slightly more conservative than comparable bootstrapped standard errors and standard errors computed according to the formula suggested by Lechner (1999).

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<sup>7</sup>Although some argue that bootstrapping might be valid in the case of kernel matching, which does not rely on a fixed number of matches and is thus smoother (Todd 2008).

## 4 Data and Variables

This study uses data from the 2002-2010 waves of the German Socio-Economic Panel Study (SOEP), which is one of the largest and longest running survey panels in the world. Annually it conducts interviews with more than 20,000 individuals in over 10,000 households in Germany (Wagner et al. 2007). The SOEP provides a wide range of information at the individual and the household level, e.g. about working and living conditions. In even number years, starting in 2002, the SOEP includes a detailed health module, which I use to construct the outcome measures.

### 4.1 Outcome Variables

This study analyzes two different types of health behavior, smoking and body weight. I use information about smoking behavior and body weight from the 2002, 2004, 2006, 2008 and 2010 waves. The SOEP does not elicit this information in the odd numbered years. I look at two different smoking measures: the daily number of cigarettes smoked and a dummy variable indicating whether or not an individual currently smokes. For the current smoking status, I distinguish between smoking initiation (the decision of nonsmokers to start smoking),<sup>8</sup> smoking continuation (the decision of smokers to remain smokers) and smoking participation (the smoking decisions of smokers and nonsmokers combined) following DeCicca et al. (2008). I exclude observations of those who solely smoke pipes or cigars.

I use the body mass index (BMI) as weight outcome, which is defined as the individual's body weight divided by the square of the individual's height and measured in kg/m<sup>2</sup>. The study does not consider overweight status (BMI > 25) or obesity (BMI > 30) directly because few individuals in the sample change between these (arbitrary) weight categories. A gain in body weight is not necessarily bad for health, especially for underweight people. Yet, more than half of the treated individuals are overweight and more than 15 % of them are obese. Section 8 looks at whether there are differences in job loss induced body weight changes between overweight and normal weight individuals.

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<sup>8</sup>This includes both, individuals who smoke for the first time and individuals who restart smoking. Section 8 differentiates between these two groups.

## 4.2 Treatment and Control Group

Treatment and control group consist of individuals between 18 and 62 years of age,<sup>9</sup> who were neither self-employed nor civil servants and who were working either full or part-time during the pre-treatment period (denoted by  $t - 1$ ). I only include individuals with non-missing smoking and BMI information before and after treatment. This drops the sample size by about 2 %. The treatment group comprises individuals who lost their job due to plant closure or dismissal between two survey rounds with information about the individuals' health behaviors, i.e. between  $t - 1$  and  $t + 1$ . For the control group, I only select those individuals without job change between  $t - 1$  and  $t + 1$ . Note that the SOEP includes the information on health behaviors only every two years. Hence, between  $t - 1$  and  $t + 1$  a survey round without questions on health behaviors takes place (at time  $t$ ). Since the question about job loss in the SOEP incorporates all job losses occurring during the survey year and the previous year, the information about job loss is derived from either the interview in  $t$  or  $t + 1$ . All other data originate from  $t - 1$ , the last wave with information about the individuals' health behaviors before the treatment. On average, this last interview took place eleven months before the job loss. The time between job loss and the following interview is, on average, thirteen months.

Table 1: Overview of the conditioning variables

<b>Demographic</b>	
Female	0=male, 1=female
Age	in years; third order polynomial
Migrant	1=individual or parents moved to Germany, 0 else
Non-German	0=German, 1=foreign citizenship
Home owner	0=tenant, 1=home owner
Spouse	1=married or unmarried spouse in the household, 0 else
Children	1=children under 18 in household, 0 else
Survey year	4 categories (2002, 2004, 2006, 2008)
<b>Labor market</b>	
Labor earnings	logarithm of annual earnings in Euro
Never unemployed	0=ever unemployed, 1=never unemployed
Years unemployed	years with months in decimal form
Tenure	in years
Blue collar	0=white collar, 1=blue collar worker
Perceived job security <sup>+</sup>	3 categories (big worries, some worries, no worries)
Company size <sup>+</sup>	3 categories (< 20, 20-200, > 200 employees)
Industry sector	10 categories
<b>Educational</b>	
University	0=no university degree, 1=university degree

<sup>9</sup>The estimated effects are very similar when the sample is restricted to smaller age ranges (e.g. individuals between 25 and 55 or between 30 and 50; see the online appendix).

Table 1: Overview of the conditioning variables

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Vocational training	0=no vocational training, 1=vocational training
Secondary schooling <sup>+</sup>	4 categories (no degree/basic school, intermediate/other school academic school track (Abitur), technical school)
<b>Regional</b>	
Residential district	4 cat. (< 2000, 2000-20 000, 20 000-100 000, > 100 000 inhabitants)
Federal state	14 categories <sup>10</sup>
Regional unemployment	yearly information on the state level

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<sup>10</sup>I group Bremen with Lower Saxony and Hamburg with Schleswig-Holstein due to few cases.

Table 1: Overview of the conditioning variables

<b>Health</b>	
Self-rated health	3 categories (very good/good, satisfactory, poor/bad)
Private health insurance <sup>+</sup>	0=public, 1= private health insurance
Mental health <sup>+</sup>	based on SF12 questionnaire (see <a href="#">Andersen et al. 2007</a> ), cardinal
Physical health <sup>+</sup>	based on SF12 questionnaire, cardinal measure
Baseline smoker	0=baseline nonsmoker, 1=smoker before treatment
Number of cigarettes	daily number of cigarettes
Ever-smoker <sup>+</sup>	0=never smoked; 1=ex-smoker(asked only in 2002 survey)
Heavy smoker	0= < 20 cigarettes/day, 1= $\geq$ 20 cigarettes a day
Partner smokes <sup>+</sup>	0=partner is nonsmoker, 1=partner smokes
Overweight	0=BMI $\leq$ 25, 1=BMI > 25
Body weight	in kg
BMI	body mass index; in kg/m <sup>2</sup>
Height	in centimeters
Drinking behavior <sup>+</sup>	4 cat. (regular, moderate, rare drinker, abstainer) <sup>11</sup>
Variables with an additional category for missing values marked with <sup>+</sup> .	

The study only looks at involuntary job loss and, therefore, does not include individuals who voluntarily terminated their employment (including, for example, resignation or mutual agreement), because selection issues might arise. Further, it is not clear from a theoretical point of view, why this should lead to a change in health behaviors. It might be argued that individuals are dismissed *because* of a *change* in smoking behavior or body weight. Dismissals due to a high *level* of smoking or weight should not distort the results since the levels are taken into account in the matching procedure. To show that there is no reverse causality, I also perform analyses in which the treatment group only consists of individuals who lost their jobs due to plant closure, for which exogeneity is stricter. In the whole observation period there are 1,768 incidences of involuntary job loss. Since only 520 occurred due to plant closure, I do not solely rely on these cases. The potential control group consists of more than 22,500 observations.

### 4.3 Conditioning Variables

The identification strategy builds on the assumption that the model includes all variables that simultaneously influence the probability of job loss and changes in health behaviors. Therefore, the choice of the conditioning variables  $S$  is crucial. I reviewed

<sup>11</sup>Coding according to [Ziebarth & Grabka \(2009\)](#). Since the data on drinking behavior are only available since 2006 they are highly affected by missing values. Excluding these variables does not alter the results (see the online appendix).

variables used in other studies analyzing health effects of job loss<sup>12</sup> and include as many of these variables as possible. Since I rely on rich survey data, I can not only include more conditioning variables than any one of these other studies but I can also use almost all conditioning variables from these studies. It is even possible to include perceived job security, which was found to be a good predictor of subsequent job loss also when controlling for other characteristics (Stephens 2004). With respect to the outcome variables, the data allow to condition not only on smoking behavior and BMI prior to job loss but also on the smoking history, i.e. whether individuals ever smoked 100 cigarettes in life.

Matching variables can be roughly divided into demographic, labor market-related, educational, regional and health-related (see table 1). In order to not lose treatment observations with missing values on some conditioning variables, I set missing values to 0 and include binary variables indicating a missing value. The affected variables are marked with “+” in table 1. Hence, I treat missing values as just another category of these variables. Matching is therefore not only on the observed values but also on the missing data pattern (Stuart 2010). The ATT estimates are very similar when observations with missing values are excluded, the standard errors, however, slightly increase (results are not shown, but are available upon request). In total, the study conditions on 79 non-collinear variables.<sup>13</sup>

## 5 Matching Quality

The quality of matching can be assessed by comparing the means of the conditioning variables for treated and untreated after matching. If the means of treated and matched controls deviate very much, matching on the linear propensity score did not work well and rather different observations are compared. To determine whether a mean difference is large, I look at the standardized bias. The standardized bias displays for each conditioning variable  $S$  the difference between the means of treated ( $s_1$ ) and matched controls ( $s_0$ ) as a percentage of the square root of the average of the variances ( $\sigma_s^2$ ) in

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<sup>12</sup>These are Browning et al. (2006), Böckerman & Ilmakunnas (2009), Eliason & Storrie (2009), Falba et al. (2005), Salm (2009), Schmitz (2011) and Sullivan & von Wachter (2009). Additionally, I looked at the variables highlighted by Wichert & Wilke (2012) as determinants of job loss in Germany.

<sup>13</sup>The online appendix provides marginal effects for the conditioning variables in the propensity score equation.

Table 2: Means of treated, controls and matched controls - before treatment

Variable	Treated	Controls		Standard. unmatched	Bias (%) matched
		unmatched	matched		
<b>Demographic</b>					
Female <sup>+</sup>	41.33	47.11	42.21	-11.64	-1.78
Age	40.87	42.71	41.02	-18.06	-1.42
Migrant <sup>+</sup>	19.63	15.36	19.74	11.27	-0.28
Non-German <sup>+</sup>	15.38	11.59	15.68	11.11	-0.82
Home owner <sup>+</sup>	40.70	54.27	40.79	-27.41	-0.19
Spouse <sup>+</sup>	73.59	78.76	74.43	-12.15	-1.92
Children <sup>+</sup>	39.95	41.12	39.55	-2.38	0.83
<b>Labor market</b>					
Labor earnings	9.66	10.13	9.66	-38.07	-0.36
Never unemployed <sup>+</sup>	44.37	68.39	45.74	-49.91	-2.75
Years unemployed	1.07	0.44	1.05	40.04	0.87
Tenure	6.59	11.67	6.81	-58.05	-3.07
Blue collar <sup>+</sup>	49.02	32.90	48.35	33.24	1.35
Small company <sup>+</sup>	39.44	20.79	38.84	41.51	1.22
Medium company <sup>+</sup>	32.78	29.75	33.30	6.54	-1.11
Large company <sup>+</sup>	25.60	46.95	25.56	-45.52	0.10
No company info <sup>+</sup>	2.18	2.52	2.30	-2.23	-0.78
Job worries <sup>+</sup>	33.98	15.32	34.62	44.36	-1.35
No job worries <sup>+</sup>	20.44	38.85	20.18	-41.16	0.64
No info <sup>+</sup>	1.66	1.44	1.99	1.83	-2.46
<b>Education</b>					
University <sup>+</sup>	14.64	22.07	14.52	-19.29	0.32
Vocational training <sup>+</sup>	76.18	75.54	75.91	1.48	0.63
Basic school <sup>+</sup>	32.72	28.28	32.59	9.66	0.28
Intermediate school <sup>+</sup>	47.30	43.85	47.41	6.93	-0.22
Technical college <sup>+</sup>	3.96	6.52	4.08	-11.49	-0.58
Highest secondary <sup>+</sup>	13.43	19.66	13.35	-16.81	0.25
No schooling info <sup>+</sup>	2.58	1.70	2.58	6.13	0.04
<b>Regional</b>					
Village <sup>+</sup>	11.31	8.74	11.61	8.54	-0.94
Small town <sup>+</sup>	34.10	35.47	33.76	-2.87	0.71
Small city <sup>+</sup>	26.69	27.16	26.57	-1.06	0.29
Big city <sup>+</sup>	27.90	28.62	28.07	-1.61	-0.37
Regional unemployment	11.06	10.11	11.03	20.71	0.60



Table 2: Means of treated, controls and matched controls - Continued

Variable	Treated	Controls		Standard. Bias (%)	
		unmatched	matched	unmatched	matched
<b>Health</b>					
Bad health <sup>+</sup>	11.77	9.60	12.16	7.02	-1.21
Good health <sup>+</sup>	53.90	58.84	53.48	-9.96	0.84
Private health insurance <sup>+</sup>	3.27	6.71	3.40	-15.85	-0.71
No insurance info <sup>+</sup>	0.23	0.20	0.42	0.73	-3.36
Mental health	48.12	49.28	48.01	-9.97	0.89
Physical health	50.72	51.38	50.61	-6.11	1.01
No health info <sup>+</sup>	2.18	1.96	2.21	1.53	-0.19
Baseline smoker <sup>+</sup>	42.71	33.60	42.71	18.84	0.00
Number of cigarettes	7.29	5.49	7.44	18.27	-1.37
Ever-smoker <sup>+</sup>	63.66	57.34	63.70	12.96	-0.08
No Ever-smoker info <sup>+</sup>	5.17	5.77	5.12	-2.67	0.20
Heavy smoker <sup>+</sup>	19.52	14.73	19.70	12.74	-0.46
Partner smokes <sup>+</sup>	26.52	23.12	26.73	7.89	-0.46
No partner-smoke info <sup>+</sup>	29.79	25.45	28.84	9.73	2.10
Overweight <sup>+</sup>	53.21	51.10	53.67	4.23	-0.92
Body weight	77.72	77.00	77.77	4.50	-0.36
BMI	25.80	25.67	25.86	2.96	-1.25
Height	173.19	172.82	173.09	4.11	1.15
Abstainer <sup>+</sup>	4.54	3.71	4.45	4.16	0.42
Rare drinker <sup>+</sup>	10.91	13.01	11.09	-6.47	-0.58
Moderate drinker <sup>+</sup>	16.70	20.67	16.71	-10.18	-0.01
Regular drinker <sup>+</sup>	6.89	8.35	6.91	-5.50	-0.07
No alcohol info <sup>+</sup>	60.96	54.27	60.85	13.58	0.24

Note: Summary statistics for treated, all controls and matched controls. The first three columns present the means of selected variables before treatment for treated, controls and matched controls, respectively. The last two columns display the standardized bias in % before and after matching. <sup>+</sup> indicates that the mean represents a percentage share.

the two groups (Rosenbaum & Rubin 1985b):

$$SB_s = 100 \cdot \frac{\bar{s}_1 - \bar{s}_0}{\sqrt{\frac{1}{2}(\sigma_{s1}^2 + \sigma_{s0}^2)}} \quad (5)$$

When the standardized bias exceeds 5 %, the mean difference is regarded to be large (Caliendo & Kopeinig 2008), and balancing did not work very well.

Table 2 presents means and standardized biases for the pooled sample before and

after matching.<sup>14</sup> Before matching, those who lost their jobs differ. For instance, they are more likely to have a migration background, have lower levels of income, have more previous unemployment times, have less tenure, are more likely to be blue collar workers, work more often in small companies, are more concerned about the security of their jobs and have lower levels of secondary education. With respect to the health behaviours analyzed, the treated are more likely to smoke (43 % vs. 34 %), have a slightly higher BMI on average and are more likely to be overweight (53 % vs. 51 %) before job loss. Matching strongly reduces these disparities in health behaviors as well as in other conditioning variables. Therefore, matching seems to function properly. Small remaining differences between treated and matched controls are mitigated by the regression-adjustment.

After imposing the common support conditions, the sample comprises 1,742 treated observations (513 with plant closure). This reduces the sample size by about 1.5 %. The standardized bias is much smaller after matching and never exceeds the critical value of 5 %. The median standardized bias over all 79 conditioning variables is 10.07 before matching and 0.69 after matching; the mean standardized bias amounts to 0.86. In general, the median standardized bias is rather low compared to other studies (e.g. [Lechner 2002](#); [Kuhn et al. 2009](#)). Also based on this criterion the matching procedure succeeds.

## 6 Results

Table 3 presents the estimates of the effect of job loss on body weight and smoking (ATT) for several specifications (i) for all individuals and separately for (ii) baseline smokers and (iii) baseline nonsmokers. The table starts with the simplest specification in the first column. The other specifications gradually incorporate more sophisticated procedures. Specification (4) is the preferred specification. For each matching specification, table 3 also presents the share of treated observations off the common support, i.e. treated individuals without adequate match, and the median of the standardized bias.

Specification (1) does not use any information about the pre-treatment health behaviors in the estimation of the linear propensity score, the outcome variable or for regression adjustment.<sup>15</sup> Hence, I do not present results separately for baseline smokers

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<sup>14</sup>Matching is also on the square and cubic of age as well as on sets of binary variables for federal state, survey year and industry (see section 4). Table 2 excludes these variables due to space limitations. Also for these variables the standardized bias falls below the critical value of 5 % after matching. The calculation of the median standardized bias in tables 3 and 4 considers these variables.

<sup>15</sup>Variables concerning smoking status, smoking intensity and smoking history as well as the BMI are

and nonsmokers. This specification most closely resembles a pure cross-sectional matching estimator. Matching is *not* exact on survey year and baseline smoking status in this specification. In this specification, job loss increases smoking participation by 4.7 percentage points, the daily number of cigarettes smoked by 0.6 cigarettes and the BMI by 0.14 kg/m<sup>2</sup> - although the effect on BMI is not significant at common significance levels.

Table 3: The effect of job loss on smoking behavior and body weight

Outcome	cross-section	longitudinal		
	(1)	not in cells (2)	in cells (3)	w. Reg.-adj. (4)
<b>All individuals</b>				
Smoking participation	0.047*** (0.013)	0.023*** (0.008)	0.025*** (0.008)	0.024*** (0.008)
Number of cigarettes	0.626 * * (0.266)	0.176 (0.175)	0.358 (0.267)	0.234 (0.163)
Body mass index (BMI)	0.140 (0.121)	0.100* (0.053)	0.095* (0.053)	0.097* (0.052)
<b>Baseline nonsmokers</b>				
Smoking initiation		0.030*** (0.009)	0.028*** (0.009)	0.030*** (0.009)
<b>Baseline smokers</b>				
Smoking continuation		0.017 (0.013)	0.020 (0.014)	0.016 (0.013)
Number of cigarettes		0.046 (0.363)	0.462 (0.586)	0.086 (0.338)
Off common support (%)	0.11	0.17	1.47	1.47
Median std. bias	1.00	0.93	0.69	0.69

Note: The table presents the effect of job loss on smoking and body weight. Each cell displays the ATT from a separate regression and its robust standard error. Row names indicate the outcome. Specification (1) does not take into account baseline health behaviors, while specifications (2)-(4) use the change in health behavior as outcome. Specification (3) performs matching within cells defined by survey year and smoking status, which specification (2) does not. Specification (4) additionally includes the conditioning variables in the outcome equation. The row “Median std. bias” displays the median of the standardized bias over all matching variables. “Off common support” indicates the share of treated individuals (from a base of  $N = 1,768$ ) who are not considered in the estimation due to inappropriate matches. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

excluded in this specification.

Specification (2) is the difference-in-difference matching estimator where the outcome is the *change* in health behavior and where the propensity score estimation takes into account  $Y^b$ , the pre-treatment health behaviors. For the pooled sample, job loss increases the probability of smoking by 2.3 percentage points and increases the BMI by around  $0.1 \text{ kg}/m^2$ . This is in addition to the usual weight gain between two observation periods, which is about  $0.34 \text{ kg}/m^2$  for the matched controls. Therefore, the total body weight increase amounts to  $0.44 \text{ kg}/m^2$  for the treated. The effect on the current smoking status is principally driven by baseline nonsmokers starting to smoke. There is no significant increase in the probability of baseline smokers remaining smokers. Smokers also do not smoke significantly more cigarettes, on average, after their job loss. Compared to specification (1), the effect of job loss on health behaviors is considerably smaller. If there was no selection on unobservable characteristics with time-invariant effects, the estimates of the ATT should be very similar. The large differences might be taken as an indication that the cross-sectional matching estimator of specification (1) is biased upwardly.

While for specification (2) matching is on the linear index of the propensity score of the pooled sample, specification (3) additionally performs exact matching on survey year and smoking status. The median standardized bias is slightly smaller, while the share of treated observations outside the common support increases. The results resemble the results of the previous specification.

In specification (4) there is additional regression adjustment for all conditioning variables used in the estimation of the propensity score. As expected, regression adjustment slightly decreases the standard errors compared to specification (3). Some estimated effects decrease somewhat in magnitude. Baseline nonsmokers are around 3.0 percentage points more likely to smoke due to job loss. This implies an increase in the probability of starting smoking of more than 55 percent as less than 5.5 percent of the baseline nonsmokers start smoking. The  $0.1 \text{ kg}/m^2$  increase in BMI due to job loss denotes an increase of about 0.4 percent (0.3 kg) in total body weight and of about 30 percent in body weight change.

The average increase in BMI differs from findings of [Deb et al. \(2011\)](#), who only observe significant increases for one latent group in their finite mixture models. This group consists of individuals who already engage in unhealthy behaviors before job loss. For this group, [Deb et al. \(2011\)](#) estimate a job loss-related increase in BMI by more than one unit. Section 8 performs separate analyses for different at-risk groups and shows that for overweight individuals the effect is larger but still considerably below 0.5

BMI units.

The effect on smoking initiation is somewhat smaller than the effect in [Falba et al. \(2005\)](#), who estimate that job loss more than doubles the probability of smoking initiation. However, [Falba et al. \(2005\)](#) not only apply a different estimation strategy and analyze U.S. individuals aged over 50, but they also focus solely on ex-smokers and, therefore, on smoking relapse. In section 8, I perform separate analyses for ex-smokers. I find that for ex-smokers job loss increases the probability of (re)starting smoking by about 50 percent (5.6 percentage points), which is, again, smaller than the effect in [Falba et al. \(2005\)](#).

To gauge whether the statistically significant effects in specification (4) are meaningful, the findings of this study are compared with other effects on smoking and BMI reported in the literature. [Ruhm \(2005\)](#) estimates that a one point drop in the employment rate in the U.S. *reduces* the estimated smoking participation rate by 0.13 percentage points on the aggregate level. In contrast, I find individual job loss to *increase* smoking participation by 2.4 percentage points. This effect is almost twenty times as large. If healthy living really improves during economic downturns and smoking participation of individuals experiencing job loss increases, there must be some groups (e.g. employed individuals with reduced working hours) for which smoking rates decrease even more than the average estimated by [Ruhm \(2005\)](#). Exploiting large cigarette tax increases in the U.S., [DeCicca & McLeod \(2008\)](#) find that a \$1 increase in the cigarette excise tax reduces smoking participation by 1.0-1.5 percentage points. For BMI, job loss resulted in an estimated increase in BMI of about 0.1 units in table 3. This is considerably less than the reduction in BMI of 0.64 kg/m<sup>2</sup> resulting from a one point reduction in the employment rate ([Ruhm 2005](#)). In light of these other findings, the statistically significant increase in BMI seems to be of minor economic significance, while the increase in smoking participation seems to be rather high.

## 7 Robustness

This section conducts robustness checks and analyzes the plausibility of the identifying assumption. Further robustness tests can be found in the full working paper version of the paper.

For the treatment group in specification (5) I only consider individuals who lost their job due to plant closure since dismissals might be endogenous. This reduces the number of treated to less than one third of the original sample (520 observations). While

plant closure is largely exogenous to the individual, frequently it does not come without warning. Those experiencing a plant closure might be a very selective group as these individuals did not leave the plant earlier (Kletzer 1998). Table 4 shows that the two main effects, the effect on BMI and the effect on the current smoking status for non-smokers, are of similar magnitude to the base specification. However, the effect on BMI is no longer significant. For smokers, the effects on smoking status and the number of cigarettes remain insignificant but become negative. Therefore, the effect on smoking participation turns insignificant in the pooled sample.

To identify causal effects, all matching procedures assume that the conditioning variables,  $S$ , include all variables simultaneously influencing the probability of job loss and changes in health behaviors. There is no direct test for this assumption. However, I perform placebo regressions to add additional credibility to this assumption.

In this placebo regressions I check whether the job loss influences changes in health behaviors *before* the job loss. Applying a similar three-step-procedure as before, specification (6) shows that the placebo job loss does not influence changes in smoking behavior and BMI. All estimated effects are insignificant and close to zero. This specification adds plausibility to the assumption that there are no unobservables that influence both treatment and outcome in the preferred specification. The evolution of health behaviors of treated and matched controls is similar before the actual job loss.<sup>16</sup>

In the full working paper version, I show that the results are robust to applying different bandwidths in the construction of the kernel weights, to only including variables in the set of conditioning variables that are significant in stepwise probit regressions, to using different methods of standard error computation (formula suggested by Lechner (1999), bootstrapping), and to applying different matching procedures.<sup>17</sup>

Nevertheless, the ATT estimates might be biased. Several arguments suggest that the bias would be rather a downward-bias. First, individuals who lost their job might misreport their true adverse health behaviors. This does not produce a bias in the DiD estimation if they misreport in the same way before job loss, or if they change the degree of misreporting in the same way as the matched controls. However, if job loss makes

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<sup>16</sup>The working paper version shows that a placebo regression which does not take into account any information about health behaviors before the (placebo) treatment violates the unconfoundedness assumption. This indicates that the matching difference-in-difference estimator clearly outperforms the cross-sectional estimator.

<sup>17</sup>Also fixed effects regressions and OLS regressions, where I regress changes in health behaviors on the treatment indicator and the same set of covariates, yield very similar results (see the online appendix).

Table 4: The effect of job loss on smoking behavior and body weight - robustness checks

Outcome	Main (4)	Plant closure (5)	Placebo (6)
<b>All individuals</b>			
Smoking participation	0.024*** (0.008)	0.009 (0.014)	0.001 (0.010)
Number of cigarettes	0.234 (0.163)	-0.187 (0.256)	-0.190 (0.204)
Body mass index	0.097* (0.052)	0.099 (0.077)	-0.075 (0.063)
<b>Baseline nonsmokers</b>			
Smoking initiation	0.030*** (0.009)	0.029 * * (0.014)	0.015 (0.011)
<b>Baseline smokers</b>			
Smoking continuation	0.016 (0.013)	-0.021 (0.024)	-0.021 (0.018)
Number of cigarettes	0.086 (0.338)	-0.732 (0.556)	-0.591 (0.428)
Off common support (%)	1.47	1.35	0.53
Median std. bias	0.69	0.69	0.51

Note: The table presents the effect of job loss on smoking and body weight. Each cell displays the ATT from a separate regression and its robust standard error. Row names indicate the outcome. Specification (4) is the same as in table 3 ( $N = 1,768$ ). The treatment group in specification (5) consists only of individuals that lost their job due to plant closure (base  $N = 520$ ). Specification (6) is a placebo regression that tests whether the job loss influences changes in health behaviors *before* the job loss. The row “Median std. bias” displays the median of the standardized bias over all matching variables. “Off common support” indicates the share of treated individuals who are not considered in the estimation due to inappropriate matches. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

them a stronger underreporter, then the DiD estimation underestimates the true effect of job loss. It seems possible that the treated increase the degree of underreporting in order to avoid fulfilling prejudices against the unemployed. Though, it is also possible that they overreport their adverse health behaviors. This would lead to an upward-bias of the treatment effect. Second, those who experience a greater negative impact from their job loss (e.g. with respect to stress, finances or identity) might be more likely to drop out of the sample.<sup>18</sup> These individuals might also be the most likely to cope with the job loss by increasing body weight and nicotine consumption. Third, often job losses do not come without warning. If the period before the actual job loss was already stressful and poor health choices were how individuals coped with this increased stress, the pre-treatment outcome might incorporate the higher level of adverse health behaviors. There is some empirical evidence that the fear of unemployment harms mental health ([Knabe & Rätzel 2011](#); [Reichert & Tauchmann 2011](#)). This argument also suggests a downward-bias. However, the placebo regression in specification (6) does not indicate an impact of job loss on changes in health behavior before the job loss.

## 8 Heterogeneity Analysis

While the two previous sections focus on *average* treatment effects, this section analyzes subgroup-specific treatment effects. Average effects might hide that some subgroups show no reaction to job loss while others show particularly strong reactions. Therefore, the analysis of treatment effect heterogeneity is crucial for acquiring a better understanding of the consequences of job loss on smoking behavior and body weight.

To facilitate a comparison of only like and like, in addition to exact matching on survey year and smoking status, I perform exact matching on the particular grouping variable. I apply the same three-step-procedure as before, but compute the weights in step 2 separately for each combination of year, smoking status and the grouping variable. This resembles specification (4) in table 3 with the exception that matching is exact on the grouping variable. Since I impose the same common support conditions as before, sample sizes might vary. Furthermore, these analyses drop observations with missing information on the grouping variable. I do not perform exact matching on all grouping variables simultaneously; the procedure outlined is repeated for each grouping variable.<sup>19</sup>

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<sup>18</sup>For instance, [Dorsett \(2010\)](#) finds unemployment to be related to panel attrition when comparing survey and register data.

<sup>19</sup>The online appendix shows that OLS regressions provide also similar results for the heterogeneity



Grouping variables in table 5 include gender, age, partnership status, socioeconomic status, overweight status, health and unemployment status.

Table 5 shows the ATT for various subgroups and outcome variables.<sup>20</sup> The first column displays the number of treated observations (smokers and nonsmokers together) in the region of common support for the subgroup defined by the row name. Two similar studies (Falba et al. 2005; Deb et al. 2011) analyze only individuals over the age of 50. In order to make it easier to compare the results with these studies, I look at individuals who are aged 50 or older and contrast them with individuals younger than 50. The first panel of table 5 shows that the effect on BMI differs significantly by age groups. Only the BMI of young individuals increases due to job loss. Young nonsmokers are also significantly more likely to start smoking than their older counterparts. If this pattern is similar in the U.S., the probability of smoking initiation might be even higher in the general U.S. population than the results of Falba et al. (2005) indicate. Older baseline smokers are, however, more likely to continue smoking due to job loss (5.8 vs. 1.0 percentage points).

It is often argued that work is more crucial for the identity of men and, hence, their reaction can be expected to be stronger. Roelfs et al. (2011) cite some studies that find stronger effects of job loss and unemployment on various outcomes for men. The second panel of table 5 shows that the effect of job loss on smoking initiation is stronger for men (4.1 vs. 1.6 percentage points). Although the difference is not statistically significant from zero. The only significant gender difference is with respect to the weight gain, which is considerably larger for *women* (0.28 vs -0.01 kg/m<sup>2</sup>). Overall, there appears to be no clear evidence for stronger effects of job loss on health behaviors of men.

The next panel compares individuals with and without cohabiting spouse. Spouses can give financial and emotional support, thus mitigating stress and financial hardship associated with job loss. Furthermore, spouses can work as control authority preventing individuals from increasing smoking and body weight after job loss. The data support these theoretical expectations for smoking behavior. Among baseline nonsmokers, single individuals are the group showing the highest increase in the probability of starting smoking (6.7 percentage points). As a result of the loss of employment they are significantly more likely to start smoking than individuals with a spouse in the household.

Clark (2003) finds the effect of unemployment on mental health to be smaller in

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analysis.

<sup>20</sup>For reasons of brevity this table does not display the effects on the number of cigarettes for baseline smokers and nonsmokers combined but only for baseline smokers.

Table 5: Treatment effects by subgroups

	All individuals			Nonsmokers	Smokers	
	N	Smoking Particip.	BMI	Smoking Initiation	Smoking Contin.	No. of Cigarettes
Over 50	416	0.019	−0.067	0.002	0.058 * *	−0.499
Under 50	1318	0.026***	0.142 * *	0.039***	0.010	0.155
<i>Difference</i>		0.007	0.208*	0.037 * *	−0.049	0.653
Male	1018	0.027 * *	−0.014	0.041***	0.015	−0.390
Female	717	0.017	0.278***	0.016	0.014	0.671
<i>Difference</i>		−0.010	0.292***	−0.025	−0.001	1.061
Single	448	0.029*	0.095	0.067***	−0.011	0.372
Partner	1277	0.022***	0.111*	0.021 * *	0.023	−0.178
<i>Difference</i>		−0.006	0.016	−0.046*	0.034	−0.550
Lower unempl.	889	0.037***	0.116	0.048***	0.030	0.182
Higher unempl.	847	0.011	0.126*	0.016	0.004	0.235
<i>Difference</i>		−0.026*	0.010	−0.032*	−0.026	0.053
Lower educ.	658	0.035***	0.116	0.052***	0.025	−0.789
Higher educ.	1031	0.015	0.097	0.021 * *	0.001	0.269
<i>Difference</i>		−0.020	−0.020	−0.031	−0.023	1.058
Normal	803	0.033***	0.059	0.038***	0.022	0.165
Overweight	924	0.018*	0.134*	0.021*	0.012	0.043
<i>Difference</i>		−0.014	0.075	−0.017	−0.010	−0.122
Worse health	800	0.046***	0.052	0.048***	0.048***	0.468
Better health	923	0.007	0.172 * *	0.020*	−0.016	−0.625
<i>Difference</i>		−0.040***	0.120	−0.028	−0.064 * *	−1.093
Job found	1054	0.024 * *	0.088	0.026 * *	0.017	−0.211
Unemployed	688	0.025 * *	0.112	0.036***	0.014	0.521
<i>Difference</i>		−0.001	−0.024	−0.010	0.003	−0.732
No unempl.	545	0.028 * *	−0.060	0.026*	0.024	−0.121
Some unempl.	1223	0.022 * *	0.169***	0.032***	0.012	0.165
<i>Difference</i>		0.006	−0.229***	−0.006	0.012	−0.286

Note: The table presents the effect of job loss on smoking and body weight for various subgroups defined by the row variables. Each cell stands for a separate ATT. Super columns indicate the smoking status before job loss, columns indicate the outcome. The first column displays the number of all treated observations in the region of common support for the various subgroups. Estimation resembles the procedure of specification (4) in table 3 with the difference that matching is additionally exact on the grouping variable. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

regions with *higher* unemployment. In order to test whether the loss of employment has similar heterogeneous effects on health behaviors, I compare the effects in regions with high and low unemployment.<sup>21</sup> The fourth panel of table 5 shows that the effects of job loss on smoking initiation and participation are significantly higher in regions with lower unemployment. This finding suggests that job loss is less harmful when unemployment is more common in the region, and, hence, less of a social stigma.

Individuals with less education might react particularly strongly to job loss as they might have less savings to compensate foregone earnings. They might also be less aware of the dangers of adverse health behaviors and have smaller social networks to provide emotional support (Cutler & Lleras-Muney 2010). The results in the fifth panel support these considerations. Less educated individuals<sup>22</sup> are more prone to initiate smoking due to job loss. This difference is substantial (3.1 percentage points) but not significant. A similar picture emerges when the results are grouped according to other indicators of socioeconomic status, e.g. above vs. below median labor income, blue collar vs. white collar (results not shown). Socioeconomic gradients in the coping with job loss are found by other researchers as well (see Roelfs et al. (2011) for a discussion). For changes in BMI, though, no substantial differences are found between socioeconomic groups.

Deb et al. (2011) observe job loss-related increases in adverse health behaviors for individuals who have poor health behaviors prior to job loss. I can not confirm this finding for smoking, but the effects on BMI go in the same direction as the findings in Deb et al. (2011). Heavy smokers (smoking at least 20 cigarettes a day) and light smokers do not differ significantly in their reactions (results not shown). Yet, overweight individuals (BMI > 25) are more prone to increase their body weight as a consequence of job loss (see sixth panel of table 5). However, this difference is not statistically significant and also for overweight individuals the increase in BMI resulting from job loss amounts to clearly less than 0.5 BMI units. The next panel shows that individuals in the lowest self-rated health categories (satisfactory, poor or bad) are significantly more likely to smoke due to job loss. This might further increase existing health differences, since smoking increases morbidity and mortality. There are no significant differences in the effects on BMI.

For the last two grouping variables there is no need to recalculate the weights as the

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<sup>21</sup>I distinguish regions with high and low unemployment according to whether the unemployment in the federal state is below or above the annual median.

<sup>22</sup>Less educated means being in the lowest category of the CASMIN classification (inadequately completed, general elementary school or basic vocational qualification).

groupings rather reflect differences in the treatment. The second last panel differentiates between whether the individual had found a job at the time of the following interview or was still unemployed. Although those in unemployment are slightly more likely to have started smoking (3.6 vs. 2.6 percentage points), there are no statistically significant differences between the two groups. This underlines that the job loss event is inherently stressful. The last panel differentiates between individuals who directly found a new job without intervening unemployment and individuals with some unemployment after the job loss. There are no significant differences between the two groups with respect to effects on the smoking behavior, again pointing to the stressfulness of the job loss itself. However, individuals moving directly to a new job do not show any increases in BMI due to the job loss. Only individuals with some unemployment after the job loss significantly gain weight. This might indicate that it is the lack of physical work on the job that is driving the BMI results.

I analyzed differences in the effects of job loss for further subgroups.<sup>23</sup> There is no indication of a time trend in the effects of job loss: Individuals who lost their job in the first half of the observation period (i.e. before the 2006 survey) do not differ from individuals experiencing job loss in the second half (results not shown). However, among baseline nonsmokers individuals with a smoking history (i.e. individuals who had ever smoked at least 100 cigarettes in their life) have a substantially and significantly higher chance of (re-)starting smoking; although also individuals who never smoked exhibit a positive (but insignificant) probability of starting smoking due to the loss of employment.

Looking at the effects for all groups of table 5 together, it emerges that for each subgroup job loss increases the probability of starting smoking. This effect is significant for all groups, except for individuals over the age of 50 and females. The increase in smoking initiation emerges as the most robust finding affecting almost all groups in society. While there is no evidence of an overall effect of job loss on the probability of continuing smoking, for some subgroups (over age 50, worse health) the probability of continuing smoking increases significantly. However, job loss does not cause a significant increase in smoking intensity among baseline smokers for a single subgroup. For most groups I estimate a job loss-related increase in BMI, which is, however, only significant for some subgroups.

None of the analyzed groups experiences a strong increase in BMI due to job loss. However, it might be that job loss has no strong effect on BMI on average (even within

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<sup>23</sup>These results are not displayed in table 5 for the sake of clarity but available on request.

Table 6: The effect of job loss on body weight - extreme weight changes

main specification	
Outcome	(4)
BMI change < -3	-0.005 (0.004)
BMI change < -2	0.003 (0.006)
BMI change < -1	0.011 (0.010)
BMI change > 0	0.006 (0.013)
BMI change > 1	0.041*** (0.012)
BMI change > 2	0.020 * * (0.009)
BMI change > 3	0.016 * * (0.007)

Note: See table 3. Row names indicate the outcome. All outcomes are binary variables that take on the value “1” if the respective condition is met. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

the groups), but only increases the chance to experience more extreme weight changes.<sup>24</sup> Therefore, I look at whether job loss affects more extreme weight changes. Table 6 shows that job loss has no significant effect on whether the individual experiences any positive weight gain or a larger weight loss. Yet, job loss raises the chance that the individual’s weight increases by more than 1, 2 or 3 BMI units. This indicates that job loss might strongly increase the BMI for some individuals, but only marginally on average.

## 9 Conclusion

This paper investigates the causal effect of job loss on smoking behavior and body weight. Using data from the German SOEP, this paper finds that job loss increases the probability of smoking initiation by 3 percentage points. Job loss increases the body mass index (BMI) slightly (by about 0.1 kg/m<sup>2</sup> or 0.3 kg on average), but significantly. However, there is little evidence that baseline smokers intensify smoking or are less likely to stop smoking due to job loss. These findings emerge whether only those individuals

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<sup>24</sup>I would like to thank one of the anonymous referees for pointing this out.

who lost their jobs due to plant closure are considered, or all individuals experiencing job loss due either to plant closure or dismissal. The estimated causal effects of job loss can partly explain the positive association between unemployment and adverse health behaviors.

Further analyses indicate treatment effect heterogeneity. In particular singles, younger individuals and individuals with lower health or socioeconomic status prior to job loss exhibit high rates of smoking initiation. The increase in body weight is larger for women and overweight individuals, but still well below  $0.5 \text{ kg/m}^2$ . There is also some indication that job loss raises the chance to experience a larger BMI increase. Compared to other effects on smoking and body weight (see e.g. [Ruhm 2005](#); [DeCicca & McLeod 2008](#)), the increase in smoking seems considerable, while the average increase in BMI is rather small. In general, the effects on smoking and BMI fall below comparable findings for the U.S. ([Falba et al. 2005](#); [Deb et al. 2011](#)), which might be attributable to the more generous unemployment assistance in Germany.

This study facilitates a better understanding of the implications of job loss, which is not only of particular importance in times of financial and economic crises. While previous studies find detrimental health effects of job loss ([Eliason & Storrie 2009](#); [Sullivan & von Wachter 2009](#)), this study emphasizes worsening health behaviors as potential mechanism for these findings. While other economic studies find that health behaviors improve at aggregated levels when unemployment is high ([Ruhm & Black 2002](#); [Ruhm 2005](#)), the present results indicate that simply transferring these findings to the individual level might lead to an ecological fallacy.<sup>25</sup> The improvement of health behaviors during economic downturns appears not to be driven by newly unemployed (but possibly by employed individuals with reduced working hours). This micro-macro difference resembles other findings on the link between job loss/unemployment and health: while mortality ([Ruhm 2000](#)) and infant health ([Dehejia & Lleras-Muney 2004](#)) improve at aggregated levels when the unemployment rate is high, at the individual level job loss increases mortality ([Eliason & Storrie 2009](#); [Sullivan & von Wachter 2009](#)) and decreases infant health ([Lindo 2011](#)). The findings of this paper also emerge to be important from a public health perspective by highlighting that job loss is a crucial life event with a rather strong impact on smoking, one major cause of preventable deaths.

Policies aimed at preventing smoking initiation might be more effective if they consider

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<sup>25</sup>Although this study basically focus on job loss and not directly on unemployment, the results in section 8 demonstrate that the effects on health behaviors are similar when analyzing the entry into unemployment.

the vulnerability of specific groups, such as individuals who lost their employment. In general, the findings emphasize that labor market policies to prevent job losses are more beneficial than previously thought. Policy-makers should take the worsening of health behaviors into account when comparing the costs and benefits of such labor market policies.

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