Stop Smoking, Your Paycheck Will Thank You!

Wage Effects from Smoking Cessation

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ABSTRACT

A growing body of literature has investigated the wage penalty attached to smoking. Little research, in contrast, has been done on the wage effects of smoking cessation. Using survey panel data from Germany, we study the relative earnings of smokers and former smokers over an extended period of time. We control for selection into smoking by imposing smoking initiation as the common initial condition and exclusively focusing on ever smokers, i.e. smokers and former smokers. Although we do not find evidence for an average wage premium of smoking cessation, our estimates point to heterogeneous causal effects. Individuals who did not smoke very long or who quit many years ago do appear to benefit in their earnings from quitting smoking. The prospect of a higher paycheck for short-time smokers and long-term quitters provides an additional incentive to smokers to quit smoking, an argument health authorities may want to utilize in public smoking cessation campaigns.

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1. INTRODUCTION

Smoking may have sizeable adverse effects beyond direct health damage. In particular, wages of smokers may suffer from their addiction to tobacco, creating additional costs to both individual and society. Studying the consequences of smoking for wages, a growing body of literature in economics has produced evidence for the existence and non-negligible magnitude of such a smoking wage penalty. Little research, however, has been devoted to smoking cessation and its consequences for wages. This is surprising. For smokers, smoking cessation and not initiation is the decision margin of direct interest, and its likely consequences for health and possibly pay hence a prime concern. Wage gains from quitting, if indeed materializing, could provide an additional incentive for smokers to quit. Policy makers and health authorities could also fruitfully employ the prospect of such gains as an argument in smoking cessation campaigns.

Existing estimates of the smoking wage penalty are of little use in inferring these consequences. Previous work has almost exclusively focused on the relative wages of current smokers and current non-smokers. The latter group, however, includes not only individuals who have never smoked, but also past smokers. Treating these alike is likely to bias causal inference for two reasons. First, former and never smokers may systematically differ in productivity-relevant characteristics, that is self-select into these two smoking states. And second, smoking may have adverse wage effects that persist even when individuals quit, as caused for instance by irreversible health damage or irreparable career setbacks. Treating both groups nevertheless alike blurs the very distinction between treatment and control group and makes it difficult to interpret in any meaningful sense wage differentials calculated in

this way. It is for these two reasons that existing estimates of the smoking wage penalty are likely to be biased.[1] And for the same reasons, they are also an inadequate measure from which to infer the wage effects of smoking cessation.

Given the lack of solid evidence on the earnings effects of smoking cessation, this paper investigates whether or not smokers on average benefit from quitting, what time it takes for any potential positive effects to materialize and how large any such benefits are in magnitude. Importantly, the decision to quit smoking requires by definition prior smoking initiation. Therefore, we impose smoking initiation as the common initial condition on our estimation sample and exclusively focus on ever smokers, i.e. smokers and former smokers. This allows us to abstract from selection into smoking, which may otherwise spur causal effects. The restriction on ever smokers has the additional benefit that the group of individuals we consider is already more homogeneous than the population at large which also includes never smokers. In particular, observable and unobservable characteristics that have determined smoking initiation need not be considered in the analysis, i.e. we do not have to model the starting decision of individuals.

To identify effects of smoking cessation we make use of several empirical strategies to control for common factors that may influence both quitting decisions and wages. In addition, we consider changes in smoking status by previous smoking duration and by the elapsed time since quitting (length of and time since treatment). Using panel data from the German Socio-Economic Panel Study (SOEP) we do not find robust evidence for a general wage premium attached to smoking cessation. Although initial evidence from pooled OLS regressions suggests significant wage gains from smoking cessation, random- and fixed-effects analyses show that such a

[1] Anger and Kvasnicka (2010) show that the smoking wage penalty is reduced by as much as a third, if past smoking of individuals is controlled for.

markup is due to a selection effect. This interpretation is corroborated by additional specifications which control for both unobserved time-invariant and time-variant characteristics. Despite this lack of evidence for an average return to quitting, wages of certain past smoker groups appear to be affected. Specifically, we find evidence of a positive effect of smoking cessation on wages for individuals who did not smoke very long or who have quit many years ago. Hence, making greater use of the information on individuals' smoking histories reveals that smoking and smoking cessation exert an influence on wages, not for all but for some durations.

Our paper contributes to the literature on smoking and earnings in several ways. To the best of our knowledge, our study is the first in-depth analysis of the wage effects of smoking cessation. While some studies on the smoking wage penalty provide also some tentative co-findings on wage differentials for past smokers, none of them investigates the wages effects of smoking cessation and its underlying mechanisms in great detail.

Second, and in contrast to the existing literature on smoking and earnings, we restrict our analysis to smokers and former smokers. By imposing a common initial condition (smoking initiation), we obtain a more homogeneous group of ever smokers in our estimation sample. This allows us to focus exclusively on the effects of smoking cessation and to avoid spurious effects from selection into smoking.

Third, we provide evidence on the short- and long-term wage effects of smoking and smoking cessation by considering both smoking duration and time elapsed since quitting as explanatory variables in the analysis.

And finally, we try several approaches to identify the causal channels.

2. BACKGROUND AND PREVIOUS LITERATUR

Several reasons are cited in the literature why smoking may adversely affect earnings, including reduced individual productivity of smokers due to higher rates of absenteeism and health problems, and potential discrimination of smokers by employers and co-workers (see, for example, the discussion in Levine et al., 1997).

All of these channels may have persistent effects on earnings. Health damage may be irreversible, career setbacks irreparable, and missing educational investments not to be regained. If so, then smoking harms both the current *and* future earnings capacity of individuals. Former smokers will be affected by their smoking history even after quitting, and wage penalties for smoking will show no tendency of decline even after years of smoking cessation. However, prospects might well be less dim. Adverse health effects may in part subside with time and career setbacks be compensated to some degree or altogether if individuals quit smoking. If so, then quitting smoking may well entail significant wage gains for individuals. Which of these two scenarios, the bright or the dim, describes the prospects of quitters more accurately can only be answered empirically.

Empirical inquiries into the wage effects of smoking cessation have to address a fundamental endogeneity problem: smoking cessation is unlikely to be random. Smoking is strongly addictive, and successful smoking cessation can therefore be very demanding in terms of drive and will power. These are personal attributes that are also of great importance in the labor market, as they may affect worker productivity and hence wages. Empirical studies on the wage effects of smoking cessation have to control thoroughly for this potential self-selection of more productive workers into quitting. Otherwise, wage differentials calculated between smokers and former smokers will suffer from selection bias. [Problem der

Endogenität und des selection bias müssen wir schon in der Einleitung erwähnen und sagen wie wir das Problem lösen!]

2.1 PREVIOUS LITERATURE

Empirical studies on the earnings effects of tobacco use have found significant wage penalties attached to smoking, ranging from 2% to 24% (Levine, Gustafson, and Velenchik, 1997; Heineck and Schwarze, 2003; van Ours, 2004; Auld, 2005). As noted, however, most existing analyses focus exclusively on the current smoking status of individuals and calculate the smoking wage penalty as the average difference in wages between smokers and current non-smokers.[2]

The group of past smokers has been long neglected as a distinct group in empirical analyses of smoking and wages, although it is the relevant comparison group for a smoker that considers quitting. In an early study, Levine et al. (1997) showed, albeit descriptively, that workers in the U.S. who quit smoking between 1984 and 1991 exhibit higher wage growth than workers who did not change their smoking status in this period (i.e. either stayed a smoker *or a non-smoker*). More recently, Anger and Kvasnicka (2010) have noted and analyzed this failure in the literature to control for the past smoking behavior of individuals. Using data for Germany, they showed that confounding past smokers and never smokers leads to biased estimates of the smoking wage penalty, Anger and Kvasnicka (2010) also provide first evidence for a smoking wage premium for past smokers. However, they do not focus exclusively on the effects of smoking cessation and do not control for unobserved heterogeneity in their analysis.

[2] A notable exception is the unpublished study by Lee (1999). However, Lee uses only very crude earnings information: the average earnings in the occupational category of an individual.

Existing studies on the smoking wage penalty mostly use cross-sectional data or only two to three years of panel data (e.g. Heineck and Schwarze 2003, Levine et al. 1997, Grafova and Stafford 2009). Two exceptions are Brune (2007) and Braakmann (2008) who use longer-running panel data from the British Household Panel Study (BHPS). Brune (2007) uses retrospective data on smoking and shows that the smoking wage penalty found in cross-sectional analyses is substantially reduced (from 13% to 2%) when using panel data methods that control for unobserved individual heterogeneity. Using OLS regressions, he also provides some indirect evidence on the wage premium for past smokers, showing that any wage differential between never smokers and quitters disappears for smokers that have stopped smoking more than two years ago. Braakman (2008) uses annual instead of retrospective smoking data from the BHPS and confirms the results by Brune (2007) that controlling for unobserved individual heterogeneity reduces the smoking wage penalty substantially. He also produces evidence that any returns from smoking initiation or cessation are small relative to not changing one's smoking status, i.e. remaining either a smoker or non-smoker. Unfortunately, Braakman (2008) does not fully exploit the smoking history of individuals. Also, he treats past smokers who stopped smoking before the sampling period as never smokers, which implies systematic misclassification of long-term quitters. Furthermore, and like Brune (2007), Braakman (2008) does not adjust his estimation sample to ever smokers when studying smoking cessation, but still considers never smokers in the analysis. This is clearly inappropriate. The relevant control group for studying the effects of smoking cessation are continuing smokers, not individuals that do not change their smoking status. Grafova and Stafford (2009) compare wages of persistent smokers, former smokers and never smokers based on the Panel Study of Income Dynamics

(PSID). They find a wage penalty for persistent smokers, which they attribute to some extent to adverse effects on health status, but mainly to "unobserved preferences and behaviour of persistent smokers." However, they only employ individual smoking histories for the years 1986, 1999, and 2001, and hence do not take advantage of the full smoking histories of individuals. Furthermore, the authors do not differentiate between smokers and past smokers according to the length of and time since exposure. By taking into account smoking and quitting durations, the present study provides an extensive analysis on wage effects of smoking cessation and makes an important contribution to the literature.

3. DATA AND SUMMARY STATISTICS

We use data from the German Socio-Economic Panel (SOEP), a representative longitudinal survey of individuals in Germany conducted annually since 1984 (see Haisken-DeNew and Frick, 2005, for a description of the dataset). In the 2002 wave of the SOEP, extensive information on smoking behavior of individuals was sampled, including whether or not individuals had smoked regularly in the past. Unlike many previous studies on smoking and wages, therefore, we may distinguish between three groups of workers in our data: current smokers, past smokers, and individuals that have never smoked. In addition, current and past smokers are asked at what age they had started to smoke, and past smokers are asked in what year they had quit smoking. This retrospective data allows us to calculate the yearly smoking status of individuals across the life course, the duration of smoking for all ever smokers as well as the quitting duration for past smokers. We dropped individuals who have never smoked on a regular basis because these "social

smokers" are not asked at which age they had started or in which year they had stopped smoking. Since the smoking histories are crucial to our study, we also dropped ever smokers without valid information on their starting age and past smokers without information on their quitting year from the sample.[3] By matching the retrospective information on smoking behavior sampled in 2002 to information on earnings and socio-economic characteristics from earlier waves, we generate a panel dataset for the period 1984 to 2002 that contains yearly information on individual earnings and productivity-related personal characteristics, as well as yearly information on smoking status, (past) smoking duration, and time elapsed since quitting.

We restrict the estimation sample to workers who have ever regularly smoked cigarettes in their life, i.e. to workers who are either current smokers or former smokers. As explained above, we impose this restriction (initial condition) on our estimation sample as the decision to quit, which is the focus of our analysis, requires by definition prior smoking initiation. By conditioning on ever smokers, we do not have to model the starting decision of individuals, as we implicitly control for observable and unobservable characteristics that have determined smoking initiation.

Furthermore, we select only male workers that are of German nationality, aged 27-60, work between 10 and 60 hours a week, earn a gross hourly wage of at least the equivalent of 4 Euros in 2002, and live in West Germany. These restrictions are imposed to ensure comparability with estimation samples used in the existing literature. Our conclusions, however, do not hinge on these restrictions.

[3] To re-construct individual smoking histories we made the conservative assumption that an individual had smoked during the whole year in which he started smoking and during the whole year in which a past smoker quit. We did not take into account information on the stopping month due to potential inaccurate measurement and due to missing information for more than 20% of all quitters. For a discussion on recall error in this retrospective data, see section 6. Robustness Checks.

Our dependent variable is the log of gross hourly wages (calculated from gross monthly earnings and actual weekly hours of work). Covariates included in all regressions are age, age squared, two sets of indicator variables for the respectively highest schooling and professional degree obtained, and annual indicators for the individual waves of the survey.

Summary statistics on workers in our estimation sample are provided in Table 1. As is evident, past smokers in our estimation sample differ markedly from current smokers in productivity-related characteristics. While there are no significant differences in educational degrees discernable, past smokers are on average older, and more have higher professional qualifications. Finally, note that average real hourly wages of past smokers exceed those of workers who still smoke.

– Table 1 about here –

4. ESTIMATION METHODS

We study the wage effects of smoking cessation using standard Mincer-type earnings regressions which we augment by measures of (past) smoking behavior. Denoting by y_{it} the log of an individual i's hourly wage at time t, by x_{it} a vector of individual characteristics at t that may affect her productivity at t, and by s_{it} her smoking status at time t, we estimate regressions of the following basic form:

$$y_{it} = x_{it}^{'}\beta + s_{it}\gamma + \alpha_i + u_{it},$$

where β is a parameter vector, γ our prime coefficient of interest, α_i an individual specific effect, and u_{it} an idiosyncratic error term.

Using the covariates already described, we estimate the wage effects of smoking cessation using three different models. First, and as a benchmark, we use a pooled OLS model (POLS). POLS regressions do not take into account unobserved worker heterogeneity, which is equivalent to assuming that the unobserved individual specific effect α_i is identical for all persons. If this assumption is violated, POLS estimates will be biased. We therefore also estimate random effects models (RE). The RE model assumes the individual specific effect α_i is randomly distributed across individuals and uncorrelated with the set of explanatory variables. Finally, and to allow also for correlation between α_i with the explanatory variables, we also estimate a fixed effects model (FE). Note that the individual specific effect α_i may not alone be correlated with the smoking variable but also the idiosyncratic error term, if earnings and smoking are simultaneously determined. All standard errors will be clustered at the individual level.

As mentioned above, one problem of the analysis is that smoking status may not be random. Previous studies have tried to solve the endogeneity problem of smoking by using instrumental variables for smoking status. For example, Lillard (2010) uses cigarette prices at the time when an individual was still young to model the starting decision of individuals during adolescence. However, this approach is not useful in our analysis, since we condition on smoking initiation by exclusively focusing on ever smokers. Furthermore, cigarette prices during adolescence are unlikely to be good predictors of smoking cessation, as ongoing smokers and past smokers in our sample do not differ in their average age at smoking initiation.[4] In addition, we cannot use current cigarette prices as instrument for smoking cessation, since tobacco prices do not vary between regions and hence are captured by the year

[4] The average age at the time of smoking initiation in our sample is 17.3 (SD 3.3) for ongoing smokers and 17.2 (SD 2.9) for past smokers.

dummies. Since other good instruments for (past) smoker status are extremely difficult if not impossible to find, we refrain from using two two-stage least-squares (2SLS) estimates in our study. Instead, we refer to the FE model, in which time-invariant personal attributes that may be important for successful quits, such as drive and will power, are controlled for.

Another potential problem besetting retrospective data on past smoking behavior (starting age, stopping year) is recall error, that is survey respondents may misstate the true timing of an event. Survey respondents may round up or down the age, calendar year, or time since the event occurred in a way that causes heaping in the reported dates (Lillard, Bar, and Wang, 2008). This recall error affects our independent variable (past smoker) and hence leads to classical measurement error, that is the increase in the signal-to-noise ratio causes attenuation bias. We should keep this in mind when evaluating the results, which we should interpret as a lower bound.

5. REGRESSION RESULTS

5.1 CHANGE IN SMOKING STATUS

Table 2 contains the wage regression results from the POLS model (Model 1), the RE model (Model 2) and the FE model (Model 3). In all three models, the human capital controls have expected signs and are, except of the education variables in the FE regressions, statistically significant at the 1% level. Age has a non-linear effect, showing a hump-shaped age-earnings profile in all three models. In the POLS, the returns to education rise with higher educational degrees, with a premium of around 30% for workers with a higher secondary degree relative to those without any

schooling degree. University graduates receive an additional 30% wage premium relative to those without any professional degree. The returns to education are reduced in size, when unobserved individual effects are controlled for in the RE model, and they lose statistical significance, when fixed effects are assumed.[5]

With respect to wage effects from smoking cessation, the simple POLS regression shows that past smokers receive on average a wage premium relative to current smokers (see column 1 of Table 2), which is in line with previous findings (Anger and Kvasnicka, 2010). The coefficient of s_{it} is highly statistically significant and implies a 5.4% higher wage for past smokers relative to current smokers.[6] This wage mark-up however more than halves in magnitude (to 2.4%) if we control for unobserved heterogeneity using a RE model (column 2). In the FE model (column 3), the coefficient on past smoking changes sign, but misses statistical significance and is virtually zero in size. The average wage gains from smoking cessation found in the POLS model hence disappear, when individual fixed effects are controlled for.

– Table 2 about here –

The question is now as to what can explain the disappearance of wage gains for quitters in the FE model. First, the result may be explainable solely by systematic selection of smokers into quitting based on (time-invariant) unobserved characteristics that are positively correlated with wages. Second, there could in fact exist positive wage effects of smoking cessation which may be invisible due to

[5] We tested these models against each other. The Breusch and Pagan multiplier test shows superiority of the RE model over the POLS model, and the Hausman specification test shows that the FE is preferred over the RE model

[6] The raw differentials with only year dummies as controls were 0.108 (robust SE: 0.017) in the POLS, 0.047 (robust SE: 0.011) in the RE model, and 0.005 (robust SE: 0.010) in the FE model.

composition effects. Undoubtedly, the group of individuals who quit smoking is heterogeneous in many respects, for example in terms of smoking durations, and some types of quitters could gain from smoking cessation, while others do not. Moreover, even if there is a smoking cessation premium, it is likely that wages take some time to show an effect. To distinguish between these explanations it is insufficient to consider only the past smoking status of an individual. Instead, there is need to investigate more thoroughly individual smoking histories and herewith potential heterogeneous effects. And third, any existing smoking wage premium could be disguised by pre-cessation wage dips.

To investigate whether the wage of quitters drops before smoking cessation, we restrict our sample to current smokers and re-estimate the POLS, RE, and FE models of Table 2, replacing only the indicator for smoking cessation with an indicator for future quitting (quitting next year). If there is a pre-cessation wage dip, the coefficient on future quitting should be significantly negative. In addition, this specification provides another test of the relationship between smoking cessation and earnings. If former smokers earn more simply because of non–random selection out of smoking, then these individuals should have the same positive unobserved characteristics, and hence also enjoy the same wage premium before they actually quit smoking. Hence, this specification allows us not only to control for unobserved time-invariant characteristics, as it is the case in FE model in Table 2, but also for time-variant characteristics, since we may capture individual pre-cessation trends.

Regression results are tabulated in Table 3. Consistent with our earlier findings, estimates from POLS (Model 1) show a wage premium even for future quitting (4.5%), and hence does not confirm the hypothesis of a pre-cessation wage

dip.[7] Although this mark-up is smaller and less significant than the average wage effect of smoking cessation estimated for past smokers who have already quit (Table 2), it is evident that wages of quitters do not drop before smoking cessation. Instead, (future) quitting status may be associated with unobserved individual characteristics that command a positive return in the labor market. The regression results for the RE and FE models (Models 2 and 3) support this interpretation. In the RE model, the coefficient of the future quitting indicator is reduced in size and becomes statistically insignificant in the FE model.

– Table 3 about here –

Additional evidence is provided in Figure 1, which shows both pre- and post-cessation trends in the relative wage of quitters. The graph plots the coefficients on 17 lead and 17 lag dummies from a FE model based on a sample of ongoing smokers and smokers that we observe quitting during the observation period. As is evident, there is no wage dip in the years before smoking cessation, after controlling for age, schooling/professional degrees, year dummies and unobserved fixed effects. The wage differential between (future) quitters and smokers slightly fluctuates around zero in the years before smoking cessation and goes on an upward trend one year after. Despite the large standard errors, which are due to the relatively small number of observations in each of the 35 cells, this graph provides a first hint for the existence of a smoking cessation wage premium which cannot be attributed to (time-invariant) unobserved characteristics that drive smokers into quitting. Furthermore, the graph points to positive wage effects, which however take some time to come

[7] This finding is robust to adding more pre-cessation indicators, e.g. quitting next two years.

into effect. This may be one of the potential explanations why there might be a wage premium for some smokers but not for others, and it seems to be worthwhile to examine heterogeneous effects of smoking cessation.

– Figure 1 about here –

5.2 HETEROGENEOUS EFFECTS BY SMOKING DURATIONS AND QUITTING DURATIONS

In the regressions above, we have estimated exclusively wage differentials between past smokers and smokers. By focusing solely on smoking status in our analysis, however, we disregarded any differences between workers in both smoking duration (length of exposure) and quitting duration (time since exposure). Smoking durations vary considerably across (past) smokers in our estimation sample (see the summary statistics in Table 1). Similarly, quitters show marked differences in elapsed quitting durations. Both of these dimensions might be important for wages, yet still average out in their effects when not taken explicitly into account as in our regression analysis so far. The earnings capacity of short-time smokers might be reduced by less than the earnings capacity of individuals who already smoke for decades. Likewise, past smokers who have quit many years ago might have had sufficient time to recover, at least partially, from any adverse effects of smoking on their health, career, and wages. In short, the wage effects of smoking and smoking cessation might depend on the length and time since exposure.

To check for a potential dependence of wages on individual smoking histories, we perform two types of analyses. In the first, we focus on the importance of smoking duration for both smokers and past smokers. In the second, we seek to

gauge the influence of time since exposure on the wages of quitters. For the first, we augment our regression models of Table 2 by adding main effects for smoking duration and interaction terms of smoking duration and smoking as regressors. As shown in Table 4, smoking duration indeed matters, even when controlling for (time-invariant) unobserved worker characteristics (see Columns 2 and 3 in Table 4). In the fixed effects specification, that is our preferred model, past smokers receive a wage premium that is reduced by an average of 1.4% for every year that an individual has smoked. In other words, past smokers who had smoked more than 11 years before they quit fail on average to improve their lot by ceasing to smoke, while quitters who smoked for less than this threshold duration do. For current smokers, in contrast, smoking duration does not appear to exert any influence on remuneration even when unobserved individual effects are not controlled for (see Column 1 in Table 4).

One possible explanation for this finding may be downward wage rigidity at the bottom wage distribution, where one can find considerably more smokers than past smokers.[8] However, we do not find evidence for this hypothesis in unreported regressions which we conduct separately for workers at different positions of the wage distribution. Another explanation may be that ongoing smokers with long smoking durations may be hindered from participating in the labor market in the first place, for example due to adverse health effects. This explanation is corroborated by results from unreported regressions where we use a linear probability model to estimate the employment probability of smokers and past smokers, including standard human capital variables and year dummies. Whereas for past smokers an additional year of smoking in the past increases the probability of employment by one percentage point (with a negative main effect of 12 percentage points for past

[8] In our sample, the bottom 10% of the wage distribution contains 80% smokers, whereas smokers are underrepresented at the top 10% of the wage distribution (55%).

smokers), the increase of the smoking duration by one year lowers the probability of being employed by one percentage point for ongoing smokers. These differences are robust to taking into account unobserved individual characteristics by applying a FE model. Hence, whereas long smoking durations appear as a barrier to job holding for current smokers, they seem to hit wages of past smokers.

– Table 4 about here –

We next focus on quitting duration. To investigate whether the wages of quitters are affected by the time that has elapsed since quitting, we augment our POLS, RE, and FE models of Table 2 by adding a term and squared term for quitting duration regressors. Instead of using the whole sample of ever smokers, we first restrict the analysis to workers who have ever quit within the observation period. Hence, the comparison group consists of past smokers before they quit, i.e. with quitting duration zero, which generates a stronger test for the measurement of any smoking cessation effects. By using this more homogeneous sample, we implicitly control for unobservable characteristics that may have determined smoking cessation, in addition to controlling for selection into smoking. Table 5 contains the main regression output. As is evident, the wage premium that past smokers receive increases in quitting duration. When assuming fixed effects, our preferred specification, both the magnitude and the statistical significance increase compared to the POLS and the RE model. The result from the FE model implies that the return to wages of an additional year of quitting is about 0.8%. For the average quitter in our data, this implies a wage premium of more than 9%.

– Table 5 about here –

An alternative and arguably more flexible way to study the apparent non-linear influence of quitting duration on wages is the use of indicators for different quitting durations. In contrast to the previous exercise (result in Table 5) we now calculate the "full" wage premium of past smokers according to different quitting durations, i.e. relative to their smoking counterparts in the labor market. Hence, we keep smokers as the base group in the estimation sample. We consider four different groups of quitters: quitters who stopped smoking (i) less than 5 years ago (about 20% of all former smokers), (ii) between 5 and 9 years ago (25% of former smokers), (iii) between 10 and to 20 years ago (37% of former smokers), and (iv) more than 20 years ago (18% of former smokers). The respective regression results from the POLS, RE, and FE models are tabulated in Table 6. In the POLS model (Column 1), recent quitters (< 5 years ago) receive a wage premium of 3.3% relative to smokers. However, this wage premium completely disappears once unobserved worker characteristics are controlled for (Columns 2 and 3). In contrast, long-time quitters enjoy a wage premium in all models. Moreover, as is evident, the magnitude of this premium increases in quitting duration. In the FE model, for instance, having quit for 5 to 9 years is associated with a wage gain of 2.6%. This premium increases in magnitude if smoking cessation occurred between 10 and 20 years ago (3.3%), and it is even larger if quitting took place more than 20 years ago (6%). Overall, therefore, our earlier findings are firmly corroborated by these results.

– Table 6 about here –

Summarizing the above, our results on the relationship between smoking status and wages suggest that the wage premium enjoyed by quitters in simple OLS regressions is due to a pure selection effect.

5.3 ROBUSTNESS CHECKS

To investigate whether any other factors than the ones considered might drive our results on the wage premium of smoking cessation, we carried out several robustness checks, which will be discussed in this section.[9]

Measurement error in retrospective data: One potential problem in the estimates above is inaccurate measurement of individual smoking status due to recall error. Lillard, Bar, and Wang (2008) provide an extensive overview of measurement errors in retrospective data, differentiating between age heaping, calendar heaping, and time heaping. They show that a mismatch between the true and a reported date causes attenuation bias. Figure 2 documents the distribution of ages at which smoking was initiated that were reported by all ever smokers in our sample. For the great majority of ever smokers, tobacco consumption began during adolescence. While most ever smokers report a starting age of 16 or 18, individuals who state to have started smoking later than at age 16 or 18 tend to report an age which is a multiple of five (20, 25, 30, 35). Age heaping, therefore, seems to be an issue in our sample, although the evidence is not very strong.[10] Likewise, Figure 3 shows that past smokers seem to round the calendar year they report to have quit smoking. The reported quitting years are "heaped" at multiples of 5, and even stronger at multiples

[9] All results are available from the authors upon request.
[10] 21 percent of all ever smokers report a starting age which is a multiple of five.

of 10, with clear peaks at calendar years 1970, 1980, 1990, and 2000.[11] To control for potential measurement error from heaping, we follow Lillard, Bar, and Wang (2008) in including parametric controls in our regressions to account for the presence of any heaping and for its particular type. Specifically, we include a set of dummy variables in our regressions which indicate whether a respondent might possibly have rounded her reported age of smoking initiation or year of quitting to units of 5 in age, time, or calendar year. Taking into account these heaping controls increases the size of the coefficient on smoking status (Table 2), which is evidence for the presence of attenuation bias, but the coefficient only changes very slightly. Likewise, controlling for heaping does barely affect the coefficients in the regressions of Table 3, 4, 5, and 6, and the regressions are estimated with roughly the same precision.[12] Hence, controlling explicitly for recall error does not change our major conclusions.

– Figure 2 about here –

– Figure 3 about here –

Individual job characteristics: Studies on wage determination often include a number of further individual characteristics or job attributes to capture worker productivity. Including variables such as occupation and industry should also to some extent control that workers who differ in smoking habits might self-select into

[11] 29 percent of all past smokers report a year that is a multiple of five as year when they gave up smoking, 18 percent report a year which is a multiple of ten. The year 2001 stands out as the year which is most frequently reported as the year of quitting. This is presumably due to the fact that retrospective smoking information was sampled only one year later, in 2002. Many past smokers may have reported to have stopped "last year", but most likely not all of them managed to quit definitely. Hence, recall error, is unlikely to be responsible for this abnormality.

[12] The heaping coefficients are mostly negative, but of very small size. None of them shows statistical significance at any conventional level in all of our models. Furthermore, an F-Test reveals that the heaping controls are not jointly significant. Note that the heaping variables for starting age, calendar year, and time are dropped in the FE model, as the date of smoking initiation is fixed for individuals over time.

specific jobs that either reward smoking cessation or do not punish smoking. We are aware that these controls are potentially endogenous and their inclusion will "over-control" and bias the coefficients of interest downward. However, as a sensitivity analysis, we re-estimated the above regressions adding controls, such as occupation, tenure, a dummy for public sector employment, firm size, region indicators, and industry dummies, which did not materially change our results.

Heterogeneous effects by starting age: Even if current smokers and past smokers, like in our sample, do not differ in their average age at smoking initiation, one may still argue that different wage effects from smoking (cessation) could arise for individuals who started smoking early and those who started later in their life. Previous research provides evidence that smokers with an early starting age find it more difficult to quit (Breslau and Peterson, 1996). The age at smoking initiation is an unobserved time-invariant individual characteristic, which we control for in the FE model above. However, to investigate whether there are heterogeneous effects by starting age, we split our sample into two halves: early starters (smoking initiation up to age 16) and late starters (smoking initiation from age 17). Again, the coefficient on smoking status (Table 2) did not change substantially, and the results of Table 3, 4, 5, and 6 were barely affected, which is unsurprising, since starting age was a fix effect….

Cohort effects: Some of the findings above may be driven by cohort effects, as there may be substantial differences in smoking behavior between individuals born in different cohorts. These could be explained by the changing awareness of tobacco-related health risks over time or by changes in the social acceptance of smoking. For example, the finding of a higher wage premium for past smokers with longer quitting durations may be driven by quitters of older cohorts who started

smoking at a time when it was not known to be very harmful, but quit when evidence on smoking-related health damage became increasingly available. These older cohorts have on average longer quitting durations, which hence could be spuriously correlated with higher wages. To investigate this confounding factor, we split our sample in older and younger cohorts and re-estimated the regressions above using a FE model for each group.[13] Although less precisely estimated, the regressions support the finding that the wage premium for quitters increases with longer quitting durations independently of the cohort. An additional reason why cohort effects are unlikely to drive the results above is that both ongoing smokers and quitters of older cohorts tend to have longer smoking durations, which are however *negatively* associated with wages, although older cohorts earn more in the labor market. Overall, we conclude that the smoking cessation wage premium is not caused by cohort effects.

Health status and productivity: Furthermore, we include self-rated health as in Grafova and Stafford (2009, p. 384) to account for the possibility that smoking affects wages through health-related lower productivity.marital status?

Lagged wage effects: So far: Wage effects of smoking cessation in different quitting categories. Now, we are focusing more on the time directly after smoking cessation. Table 7 shows lagged wage effects of smoking cessation from FE regressions based on the whole sample of ever smokers with 1- to 5-year lags. As is evident in the first three columns, there are no lagged wage effects for past smokers who have stopped last year, at least 2 years ago, or at least 3 years ago. However, having stopped at least 4 or 5 years ago seems to be related to a wage markup for

[13] We cannot estimate the whole sample using cohort dummies instead of separate regressions, since the cohort dummies cancel out in the FE model.

those quitters even when unobserved effects are controlled for. After this 4- or 5-year lag, past smokers seem to gain a 2.5% wage premium relative to smokers.

5.4 DISCUSSION

Two major conclusions may be drawn from the results of our analyses. First, smokers who quit do not on average benefit in their earnings from smoking cessation. Although initial evidence from pooled OLS regressions suggested otherwise, random- and fixed-effects analyses showed that such markup is most likely due to unobserved characteristics which are related to both smoking behaviour and wages. Additional explorations that control for future quitting instead of actual quitting corroborated such an interpretation. Second, and despite the lack of evidence for an average return to quitting, wages of workers appear to be affected if not mere changes in smoking status are considered, but changes in smoking duration and changes in the elapsed time since quitting (length of and time since treatment). Making greater use of the information on individuals' smoking histories available in the data along these two dimensions shows that smoking and smoking cessation do exert an influence on wages, not for all but for some durations (heterogeneous effects). To be more precise, only longer-time smokers appear to be harmed in their earnings potential. And similarly, only longer-time quitters appear to enjoy a wage markup from smoking cessation.

The findings in this paper, like those in the existing literature on smoking and wages, are naturally restricted to employed individuals who participate in the survey. If, for example, smokers are more likely to suffer health problems which hinder them to work, or if smokers are more strongly affected by sample attrition due to

unobserved individual characteristics, the overall wage premium attached to smoking cessation will be underestimated.[14] Therefore, we interpret our findings of wage effects of smoking cessation as a lower bound.

6. CONCLUSION

Smoking is bad for health. However, smoking may have sizeable adverse effects beyond direct health damage. In particular, wages of smokers may suffer from their addiction to tobacco, creating additional costs to both individual and society. Studying the consequences of smoking for wages, a growing body of literature in economics has produced evidence for the existence and non-negligible magnitude of this smoking wage penalty. Little research, however, has been devoted explicitly to smoking cessation and its consequences for wages. This is surprising. For smokers and past smokers, that is for a large part of the population, smoking cessation is the decision margin of immediate interest. Likewise, its consequences for health and possibly pay, should be outcomes of primary concern.

Existing estimates of the smoking wage penalty are of little use in inferring these consequences, as previous studies have almost exclusively compared wages of current smokers to those of all current non-smokers, which include both never and past smokers. Unless the causal relationship between smoking and earnings is entirely contemporaneous and the selection into and out of smoking identical in terms of individual characteristics that also affect worker productivity, wage penalties calculated only with reference to current smoking status will not reflect the true wage costs of smoking. For the same reasons, any wage effects of smoking

[14] Results from unreported regressions from a linear probability model, which includes standard human capital variables and year dummies, point to a 4 percentage point higher probability of employment for past smokers relative to current smokers.

cessation will not be the mere negative of the smoking wage penalty found in the literature.

Given the lack of solid evidence on the earnings effects of smoking cessation, this paper has investigated whether or not smokers on average benefit from quitting, what time it takes for any potential positive effects to materialize, and how large any such benefits are in magnitude.

Using data from the German Socio-Economic Panel Study (SOEP), we focused exclusively on ever smokers to study smoking cessation and its effects. To identify the latter, we made use of several empirical strategies, including random and fixed-effects models and the use of future smoking status to gauge the influence also of unobserved time-variant worker characteristics on quitting decisions and wages. A wage premium for quitting, found in pooled OLS regressions, completely disappears when controlling for worker fixed effects. However, additional analyses that control also for (past) smoking duration and time elapsed since quitting produce evidence for a positive effect of smoking cessation on wages for individuals who did not smoke very long or who have quit long ago, even in fixed-effects regressions.

Our findings of a causal effect of smoking cessation on wages for short-time smokers and long-term quitters provide an additional incentive for smokers to quit. The prospects of such potential wage gains may also be used by policy makers and health authorities as an additional argument in smoking cessation campaigns. The simple message to smokers should be to stop as soon as possible and to quit forever.

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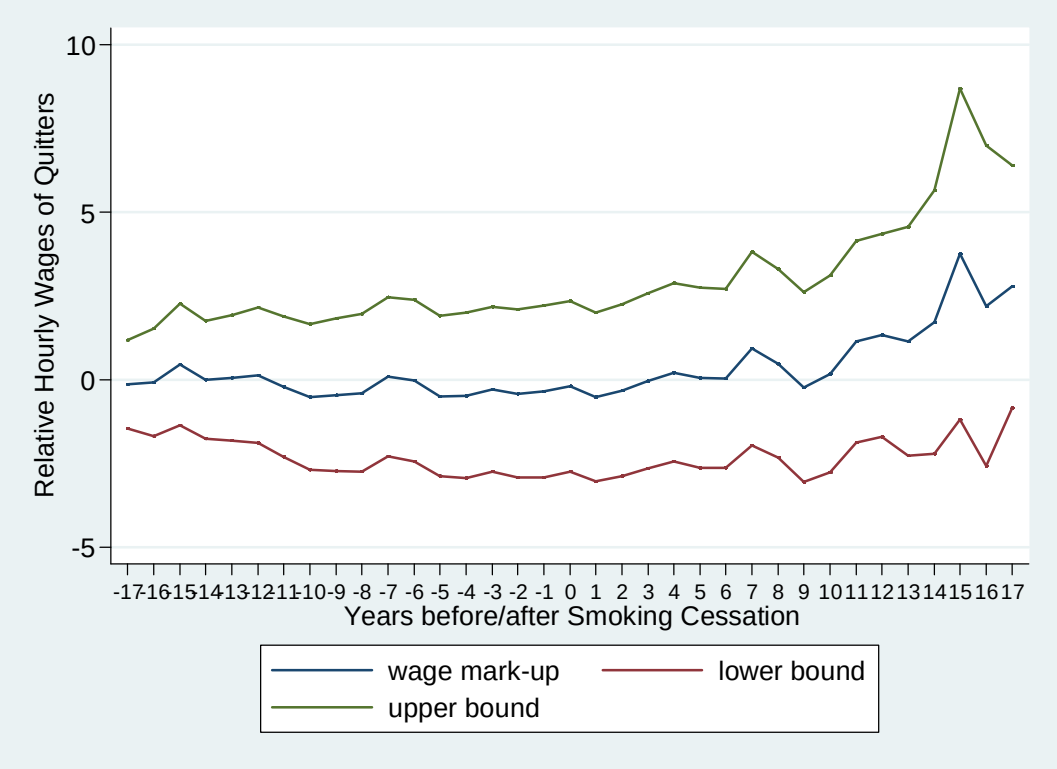
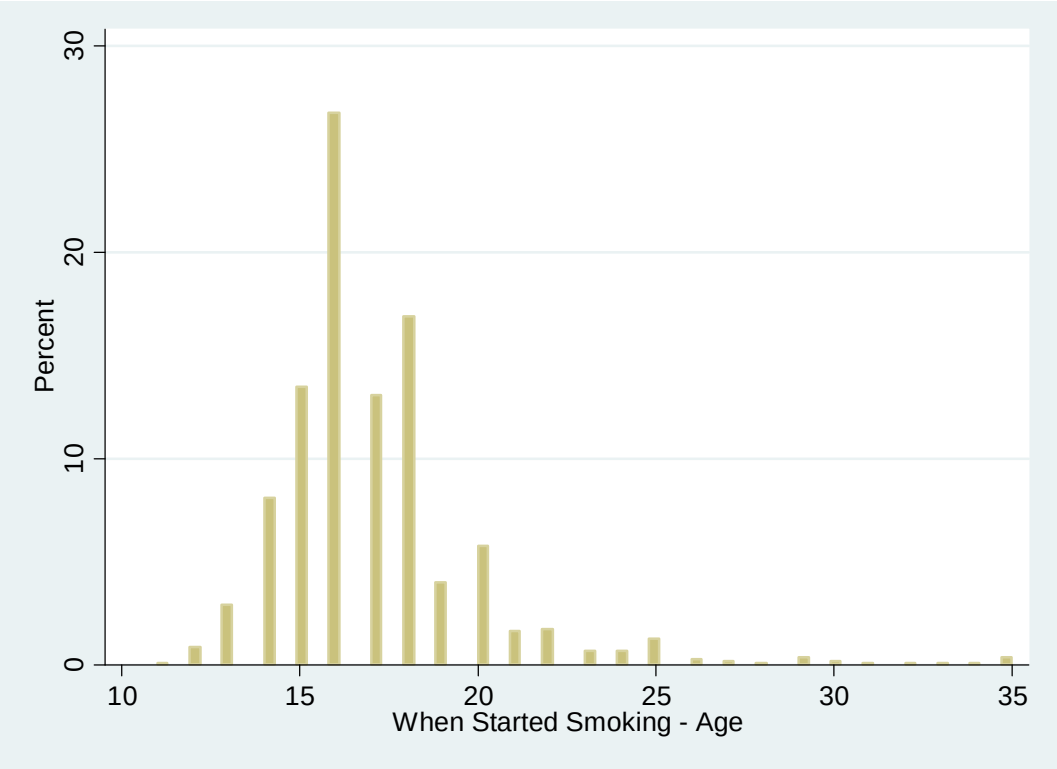
Figure 1: Wage Differentials Before and After Smoking Cessation



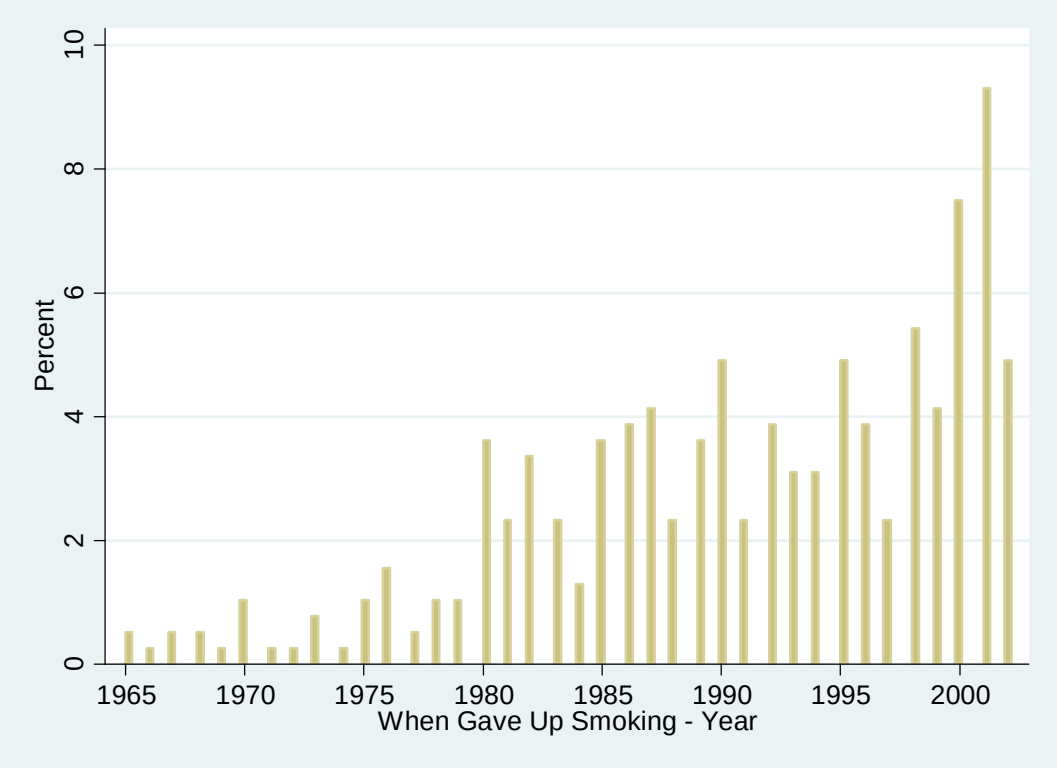
Notes: SOEP 1984-2002. West German male workers of German nationality, aged 27–60 years. Estimation sample includes only individuals who never quit and individuals who quit within the observation period (7,563 observations). Dependent variable: log gross hourly wage. The coefficients on the lead and lag dummies are estimated with a FE model with additional controls for age (cubic term), schooling/professional degrees, and year dummies. Standard errors are clustered at the individual level. The upper and lower bounds show 95% confidence intervals.

Figure 2: Age Heaping in Retrospectively Sampled Starting Age



Source: SOEP 2002

Figure 3: Calendar Year Heaping in Retrospectively Sampled Stopping Year



Source: SOEP 2002

Table 1: Summary Statistics on Workers Who Have Ever Smoked

	Current smokers	Past smokers
Dependent variable		
Real hourly gross wage (in €)	14.7 (5.3)	16.5 (7.4)*
Controls		
Age (in years)	39.7 (8.4)	45.8 (8.7)*
Education (share)		
Primary	0.55	0.55
Secondary	0.22	0.20*
Higher	0.20	0.24*
No school degree	0.02	0.01*
Professional degree (share)		
No Professional degree	0.11	0.07*
Vocational training	0.76	0.74
University	0.14	0.18*
Smoking history		
Smoking duration	23.4 (8.5)	17.8 (8.7)*
Quitting duration	-	11.7 (7.7)
N (Total = 9,766, Persons=1,872)	6,797	2,976

Notes: SOEP, 1984-2002. West German male workers of German nationality, aged 27–60 years. Means of variables (standard deviation in parentheses). * Difference statistically significant at the 1% level. Real hourly gross wages are expressed in terms of 2002 wages using the West German Consumer Price Index. Shares of categories may not sum to one due to rounding differences.

Table 2: Wage Effects from Smoking Cessation for Workers Who Have Ever Smoked [Micha, bitte Überschriften nochmal checken: ich bin damit noch nicht zufrieden, würde aber gerne die jeweiligen Gruppen dort schon erwähnen]

	POLS	RE	FE
Past smoker	0.054***	0.024	-0.002
	(0.020)	(0.015)	(0.019)
Age	0.049***	0.048***	0.078***
	(0.005)	(0.004)	(0.004)
Age squared * 100	-0.048***	-0.046***	-0.048***
	(0.006)	(0.005)	(0.005)
Lower secondary school	0.076**	0.050	-0.010
	(0.032)	(0.031)	(0.042)
Intermediate secondary school	0.193***	0.130***	-0.011
	(0.034)	(0.033)	(0.046)
Higher secondary school	0.261***	0.182***	-0.004
	(0.038)	(0.037)	(0.056)
Vocational training	0.073***	0.056***	0.036
	(0.020)	(0.017)	(0.023)
University	0.279***	0.221***	0.050
	(0.032)	(0.030)	(0.044)
Heaping Stop C5	-0.012	0.014	0.030
	(0.028)	(0.019)	(0.024)
Heaping Stop A5	0.004	-0.000	-0.002
	(0.027)	(0.024)	(0.030)
Heaping Stop T5	-0.032	-0.003	0.002
	(0.034)	(0.026)	(0.037)
Heaping Start C5	-0.007	-0.004	-
	(0.016)	(0.016)	-
Heaping Start A5	-0.002	-0.003	-
	(0.015)	(0.014)	-
Heaping Start T5	-0.009	-0.003	-
	(0.018)	(0.016)	-
Constant	0.826***	0.847***	0.126
	(0.110)	(0.088)	(0.104)
F-Test for joint significance/ Wald chi2	89.66	4037.41	129.97
Observations	9,773	9,773	9,773
Persons	1,872	1,872	1,872

Notes: SOEP, 1984-2002. Dependent variable: log gross hourly wage.
Standard errors are clustered at the individual level (in parentheses), *** p<0.01, ** p<0.05, * p<0.1. Breusch Pagan Lagrange Multiplier test for the presence of random effects: chi2=13389.43. Hausman test for systematic differences in coefficients: chi2= 171.01.
The reference group for the schooling/professional degrees is “no degree”. The heaping variables are dummies which equal one if an individual reports a multiple of 5 as age (A), calendar year (C), or time (T) since the event (start/stop smoking) occurred. Regressions also include year dummies.

Table 3: Wage Effects from Future Quitting for Smokers

	POLS	RE	FE
Quitting next year	0.046**	0.026**	0.015
	(0.019)	(0.013)	(0.014)
Age	0.048***	0.048***	0.075***
	(0.007)	(0.005)	(0.005)
Age squared * 100	-0.047***	-0.045***	-0.046***
	(0.008)	(0.006)	(0.007)
Schooling/professional degrees[1]	+	+	+
Heaping controls	+	+	+
Year dummies[2]	+	+	+
Observations	6,072	6,072	6,072
Persons	1,284	1,284	1,284

Notes: SOEP, 1984-2002. Estimation sample includes only individuals that smoke in a given year. Dependent variable: log gross hourly wage.
Standard errors are clustered at the individual level (in parentheses), *** p<0.01, ** p<0.05, * p<0.1. [1] jointly significant at the 1% level in the POLS and RE model. [2] jointly significant at the 1% level in all models.

Table 4: Wage Effects from Smoking Cessation by Smoking Duration for Workers Who Have Ever Smoked

	POLS	RE	FE
Past smoker	0.113	0.093*	0.153**
	(0.076)	(0.053)	(0.074)
Past smoker * smoking duration	-0.005	-0.007	-0.014**
	(0.007)	(0.005)	(0.006)
Past smoker * smoking duration squ * 100	0.005	0.011	0.026**
	(0.014)	(0.009)	(0.012)
Smoking duration	0.004	-0.000	-0.002
	(0.005)	(0.004)	(0.006)
Smoking duration squ x 100	-0.010	-0.006	-0.005
	(0.009)	(0.007)	(0.009)
Age	0.044***	0.050***	0.081***
	(0.008)	(0.006)	(0.008)
Age squared * 100	-0.041***	-0.045***	-0.048***
	(0.009)	(0.007)	(0.008)
Schooling/professional degrees[1]	+	+	+
Heaping controls	+	+	+
Year dummies[2]	+	+	+
Observations	9,773	9,773	9,773
Persons	1,872	1,872	1,872

Notes: SOEP, 1984-2002. Dependent variable: log gross hourly wage.
Standard errors are clustered at the individual level (in parentheses), *** p<0.01, ** p<0.05, * p<0.1. [1] jointly significant at the 1% level in the POLS and RE model. [2] jointly significant at the 1% level in all models.

Table 5: Wage Effects from Smoking Cessation by Quitting Duration for Workers Who Have Ever Quit

	POLS	RE	FE
Quitting duration	0.003	0.004	0.008**
	(0.003)	(0.003)	(0.004)
Quitting duration squ x 100	-0.002	0.001	0.001
	(0.011)	(0.009)	(0.010)
Age	0.047***	0.049***	0.080***
	(0.008)	(0.007)	(0.007)
Age squared * 100	-0.048***	-0.051***	-0.056***
	(0.009)	(0.008)	(0.009)
Schooling/professional degrees[1]	+	+	+
Heaping controls	+	+	+
Year dummies[2]	+	+	+
Observations	3,769	3,769	3,769
Persons	656	656	656

Notes: SOEP, 1984-2002. Estimation sample includes only individuals that quit within the observation period. Dependent variable: log gross hourly wage.
Standard errors are clustered at the individual level (in parentheses), *** p<0.01, ** p<0.05, * p<0.1. [1] jointly significant at the 1% level in the POLS and RE model.
[2] jointly significant at the 1% level in all models.

Table 6: Wage Effects from Smoking Cessation by Quitting Duration for Workers Who Have Ever Smoked

	POLS	RE	FE
Past smoker quitting less than 5 yrs ago	0.033	0.010	-0.007
	(0.021)	(0.015)	(0.019)
Past smoker quitting 5 to 9 yrs ago	0.003	0.027**	0.026**
	(0.018)	(0.012)	(0.013)
Past smoker quitting 10 to 20 yrs ago	0.037*	0.040**	0.033*
	(0.022)	(0.016)	(0.020)
Past smoker quitting > 20 yrs ago	0.039	0.063**	0.061**
	(0.031)	(0.025)	(0.031)
Age	0.049***	0.050***	0.079***
	(0.005)	(0.004)	(0.004)
Age squared * 100	-0.049***	-0.048***	-0.050***
	(0.006)	(0.005)	(0.005)
Schooling/professional degrees[1]	+	+	+
Heaping controls	+	+	+
Year dummies[2]	+	+	+
Observations	9,773	9,773	9,773
Persons	1,872	1,872	1,872

Notes: SOEP, 1984-2002. Dependent variable: log gross hourly wage.
Standard errors are clustered at the individual level (in parentheses), *** p<0.01, ** p<0.05, * p<0.1. [1] jointly significant at the 1% level in the POLS and RE model.
[2] jointly significant at the 1% level in all models.

Table 7: Lagged Wage Effects from Smoking Cessation

	FE	FE	FE	FE	FE
Having stopped last year or before	-0.002	-	-	-	-
	(0.019)	-	-	-	-
Having stopped at least 2 years ago	-	0.019	-	-	-
	-	(0.014)	-	-	-
Having stopped at least 3 years ago	-	-	0.020	-	-
	-	-	(0.013)	-	-
Having stopped at least 4 years ago	-	-	-	0.025*	-
	-	-	-	(0.014)	-
Having stopped at least 5 years ago	-	-	-	-	0.024*
	-	-	-	-	(0.013)
Age	0.078***	0.078***	0.078***	0.078***	0.078***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Age squared * 100	-0.048***	-0.048***	-0.048***	-0.048***	-0.048***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Schooling/professional degrees[1]	+	+	+	+	+
Heaping controls	+	+	+	+	+
Year dummies[2]	+	+	+	+	+
Observations	9,773	9,773	9,773	9,773	9,773
Persons	1,872	1,872	1,872	1,872	1,872

Notes: SOEP, 1984-2002. Dependent variable: log gross hourly wage.
Standard errors are clustered at the individual level (in parentheses), *** p<0.01, ** p<0.05, * p<0.1. [1] jointly significant at the 1% level in the POLS and RE model.
[2] jointly significant at the 1% level in all models.