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Does screening participation affect cigarette smokers' decision to quit? A long-horizon panel data analysis

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ABSTRACT

BACKGROUND – Despite decades of intensive anti-tobacco initiatives, millions of people are still smoking. The health authorities are seeking new tools and extended knowledge. Screening programs may, in addition to the potential health benefits from early detection of smoking related diseases, also increase smoking cessation among participants. This study examines the effect of screening participation by comparing the smokers' cessation hazard in screening years to non-screening years. METHODS – All smokers (n=10,471) participated in a three-wave cardiovascular screening and were followed up over a maximum of 14 years. The panel was merged with administrative registers. We used a flexible discrete-time duration model to investigate the effect of the screening program while simultaneously accounting for the possible influence of personal characteristics, addiction indicators, economic factors, health status and health changes. Specifically, we examined and compared long-term smokers (LT; smoked ≥25 years) with short-term (ST; smoked ≤ 5 years) and medium-term (MT; smoked 10-20 years) smokers. RESULTS – We found that 29% of LT smokers quitted smoking during the follow-up whereas 32% of MT and 48% of ST smokers reported the same. The screening participation years stood out as especially important for all groups. The impact of the first screening was particularly high, and for the first two screenings, the effect was higher for long-term smokers than for the smokers with shorter smoking careers. Receiving an abnormal test result was not associated with a significant increase in cessation hazard for any group of smokers. CONCLUSIONS – The substantial effect of being invited to and participating in a screening appears robust, and may prove useful when discussing future policies for smoking cessation. This paper suggests that further initiatives for consultations with health personnel, in this case through a screening program, could increase the quitting hazard.

KEYWORDS – cigarette cessation, duration model, quitting hazard, screening, long-term smokers, health status and shocks, policy intervention

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Introduction

Measured by the substantial drop in the prevalence of daily cigarette smokers, the western anti-tobacco policy has been relatively successful. Still, further reduction is an important target for health authorities. Expansion of many well-known and frequently used means for reducing cigarette consumption (e.g. increased tobacco taxes)

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may have limited political appeal or prove inefficient. Many countries have already implemented an extensive set of smoking regulations, and further use of traditional anti-smoking campaigns on current smokers may have less effect as the harmfulness of smoking is common knowledge. Policy makers and others working for an improved "health of the nation" therefore seek new tools as well as improved knowledge about the efficiency of traditional means to combat cigarette smoking (Zhu, Lee, Zhuang, Gamst, & Wolfson, 2012; Warner & Mendez, 2010).

One suggestion for increasing the cessation rate is more direct and extended contact between the health care sector and the smokers, here exemplified by the use of screening programs. Screening programs may contribute to increased public health by early detection of diseases, like for instance suggested by the results of the National Lung Screening Test with respect to participants' reduced lung cancer mortality (Aberle et al., 2011). In addition, screening programs may also influence participants' smoking habit. By simply being invited to and participating in such a program, smokers are reminded of, and confronted with, their individual health risk. For smokers who have already considered to reduce or cease smoking, the screening participation may be sufficient to actually take action. Screening programs might, however, also have the opposite effect if smokers with a negative (normal) screening outcome think they are "safe" and therefore continue their cigarette consumption. Thus, the screening's effect on smoking behaviour is not a priori given.

Empirical investigations have so far not yielded conclusive results. Observational studies have found higher quitting rates among screening participants compared to the general population of smokers (see e.g. Taylor et al., 2007; Ostroff, Buckshee, Mancuso, Yankelevitz, & Henschke, 2001). Generalizing from this finding is difficult, however, as participants may deviate from other smokers on outcome-related characteristics. A few randomized control trials (RCT) have been conducted: Ashraf et al. (2009) tested whether the treatment group (participated in a lung screening) had a higher cessation rate than the comparison group (received no CT screening) among a group of smokers. They found similar quit rates for both groups at the 1-year follow-up. A Dutch-Belgian RCT found a slightly lower smoking abstinence rate among the group receiving lung screening than among the control group (van der Aalst, van den Bergh, Willemsen, de Koning, & van Klaveren, 2010). The difference was no longer significant when an intention-to-treat analysis was conducted. Shi & Iguchi (2011) found no difference in quitting rates between a group receiving lung screening every 4-6 months and a group receiving annual screenings. Barry et al. (2012) reported similar smoking cessation rates in the trial arm and the controls among a group of current smokers (n=6,807). The studies confirmed, however, that trial participants were more inclined to stop smoking than the general population of smokers.

We contribute to this literature by examining the possible effect of screening from a new angle, namely by comparing cessation rates in screening years to cessation rates in non-screening years for a group of smokers who all participated in an extensive three-wave cardiovascular screening program in Norway. The Norwegian trend in smoking prevalence is in line with those of other Western countries (WHO, 2013; Lopez, Collishaw, & Piha, 1994). By international standards our panel data set was fairly rich, as it had a long observation window (up to 14 years), covered a large number of participants (n=10,471) and offered four categories of controls: personal characteristics; health status and health shock variables; indicators of addiction status; economic factors. A substantial part of the data set stems from administrative registers, which may have reduced problems like recall bias and imprecise reporting. The panel design is distinctive in that records from a three-wave panel with a distance of roughly five years between the waves are `superimposed' on annual register information.

We further contribute by examining whether the screening effect differs among three groups of smokers. As, by general belief, people who have smoked for decades are more "immune" to anti-smoking interventions than less habitual smokers, we distinguished smokers according to the length of their pre-sample smoking career: Short-term smokers (ST-smokers) with careers up to five years, Medium-term smokers (MT-smokers) with careers between 10 and 20 years, and Long-term smokers (LT-smokers) with careers of at least 25 years. Using the same model setup we contrasted the results for LT-smokers to the results of smokers with shorter careers, a priori allowing for group differences in the coefficient values.

Previous studies have also examined whether participants' quitting rates are influenced by the screening outcome. Although many have found that those receiving a negative test result are less likely to quit smoking, the results are somewhat mixed (see e.g. van der Aalst, van Klaveren, van den Bergh, Willemsen, & de Koning, 2011; Anderson et al., 2009; Styn et al., 2009; Townsend et al., 2005; Ostroff et al., 2001). In addition to examining the possible effect of screening outcome we also examine whether participants' initial health and changes in health status over time impact the cessation rate, i.e. we are able to take into account an extensive set of self-reported and objective health measures.

Data and sample description

Our main body of data is extracted from a comprehensive cardiovascular screening program conducted by the National Health Screening Service (presently: the Norwegian Institute of Public Health). The program involved three screenings, in three among the nineteen counties over the 1974–1988 period. For practical and institutional reasons the screenings were not synchronized across the counties, taking place approximately every fifth year[1]. In the first screening all inhabitants aged 35–49 years, and a 10% random sample of persons between 20 and 34 years old, were invited to participate. The target groups for the second and third screenings combined previous participants and new cohorts. The three screening dates will be denoted as R1, R2, and R3.

Altogether 65,624 subjects were invited to the first screening in the three counties, and 88% participated (Bjartveit, Foss, Gjervig, & Lund-Larsen, 1979). Of all those invited, the attendance rates for the second and third screenings were 88% and 84%, respectively. Participants were asked to fill out a questionnaire at home and bring it to the screening station. Information on any history of cardiovascular diseases, diabetes, use of anti-hypertensives, symptoms of cardiovascular diseases, physical activity during leisure time and at work, smoking habits, stress factors in social life, and family history of coronary heart diseases were recorded. An additional questionnaire was handed out at the screening station and the participants were asked to complete it at home and return it by mail. A simple health examination was carried out at the screening station. Height and weight, systolic and diastolic blood pressure were measured according to a standard protocol and a non-fasting blood sample was drawn and analysed for serum total cholesterol and triglycerides. At R1 and R2 a mass miniature chest x-ray was taken. In the statistical analyses presented in section 3, results from these medical tests are included along with responses from the questionnaires.

The individuals included in the current sample satisfied three criteria: they were screened for the first time in 1975–1978, stated that they were daily cigarette smokers then, and participated in both follow-ups[2]. The number of participants was 12,499. As smoking history is assumed to influence on the likelihood of quitting, we accounted for the variation in participants' smoking careers prior to R1. We thus defined three groups according to their reported number of years as a smoker prior to entering the study: 1) Short-term smokers (ST-smokers), having smoked up to 5 years at R1, n=905; 2) Medium-term smokers (MT-smokers), having smoked 10–20 years at R1, n=7,641; and 3) Long-term smokers (LT-smokers), having 25 years or more at R1, n=1,925. To increase the probability of detecting any group differences we excluded some career lengths when defining the groups, i.e. we randomly excluded those with 6-9 years and 21–24 years of smoking prior to R1. This reduced the final sample size to 10,471, as some of the participants did not belong to either of the ST, MT and LT smoker groups thus defined.

The data from the screening were merged with information for each of the years 1974-1988 from administrative registers (Statistics Norway) on the individual's income, education, marital status, and household size. Annual cigarette prices were obtained from the same source. Thus, the contiguous, unbalanced panel dataset had information on more variables in the screening years than in the non-screening years, providing a composite of a 3-wave and a 14-year panel.

Descriptive statistics for the four categories of controls are given in Table 1 and further details are provided in tables A1 and A2, see Appendix.

To assess the effect of the screening participation on smoking cessation we included dummies for the screening years (screening1-3), which, because the screening periods were not synchronized across individuals, are (individual, time)-subscripted. We assume that if a test result affected an individual's smoking cessation, it would do so in the same year as the message was received, and therefore created dummies with value equal to one for the respective screening years if an unfavourable test result (as determined by medical experts) was revealed to the participants (badreport1-3). As the possible effect of

Table 1. Summary statistics, subgroups and full sample

Variable	Full sample Mean	Full sample St.dev	Long term smokers (smoked ≥25 years) Mean	Long term smokers (smoked ≥25 years) St.dev	Medium term smokers (smoked 10–20 years) Mean	Medium term smokers (smoked 10–20 years) St.dev	Short term smokers (Smoked up to 5 years) Mean	Short term smokers (Smoked up to 5 years) St.dev
Demographics (dummies)								
Male	0.550	0.497	0.798	0.401	0.507	0.500	0.303	0.460
Age	38.87	5.845	44.31	2.722	38.49	4.387	32.66	8.841
Educ1	0.513	0.500	0.609	0.488	0.509	0.500	0.382	0.486
Educ2	0.426	0.494	0.355	0.479	0.425	0.494	0.535	0.499
Educ3	0.049	0.216	0.024	0.153	0.052	0.222	0.072	0.258
Educ4	0.008	0.086	0.006	0.079	0.009	0.094	0.004	0.066
Children	0.764	0.424	0.684	0.465	0.800	0.400	0.688	0.463
New baby	0.018	0.133	0.003	0.057	0.011	0.104	0.030	0.171
Single	0.109	0.312	0.091	0.288	0.087	0.282	0.190	0.391
Married	0.843	0.364	0.845	0.362	0.858	0.348	0.737	0.440
Div-wid	0.063	0.243	0.065	0.247	0.033	0.180	0.070	0.256
Health (dummies)								
Symptoms (R1)	0.026	0.159	0.030	0.170	0.023	0.148	0.025	0.157
Illness (R1)	0.043	0.203	0.089	0.285	0.034	0.181	0.024	0.154
Bmihigh (R1)	0.362	0.481	0.477	0.500	0.340	0.474	0.331	0.471
Actwork (R1)	0.384	0.487	0.460	0.499	0.375	0.484	0.304	0.460
Exercise (R1)	0.212	0.409	0.205	0.404	0.218	0.413	0.170	0.376
Disabled (R1)	0.016	0.124	0.029	0.168	0.012	0.110	0.017	0.128
Sympshock12	0.008	0.091	0.013	0.115	0.007	0.081	0.011	0.104
Sympshock23	0.008	0.087	0.008	0.091	0.007	0.083	0.007	0.085
Illshock12	0.017	0.130	0.035	0.183	0.014	0.117	0.014	0.116
Illshock23	0.022	0.147	0.037	0.190	0.019	0.137	0.013	0.113
Badreport1	0.017	0.130	0.024	0.155	0.016	0.125	0.013	0.113
Badreport2	0.016	0.127	0.023	0.150	0.015	0.123	0.012	0.111
Badreport3	0.008	0.102	0.009	0.118	0.007	0.097	0.006	0.102
Addiction								
Debut_age	21.68	5.664	17.78	2.567	22.06	4.495	29.09	9.120
Number_cig	13.42	6.619	15.23	7.084	13.18	6.324	9.71	5.685
Economic								
Cigprice (ln)	1.032	5.629	1.073	5.618	1.037	5.633	0.863	5.595
Income (ln)	5.914	0.891	5.941	0.846	5.793	0.896	5.623	1.135
Dep. Var. (dummy)								
Quit smoking	0.034	0.180	0.029	0.167	0.033	0.178	0.056	0.231
No of obs.	122,974		19,638		75,248		7,628	
No of persons	12,499		1,925		7,641		905	

the test results on smoking participation would only be registered at the next screening, the badreport3 variable at R3 was left out of the analyses.

Demographic variables: Table 1 shows that there were roughly equal proportions of men and women in the full sample (mean of dummy *male*=0.55), with large differences across the three subgroups, e.g., 80% of the LT-smokers were men (mean of *LT-male* dummy=0.798). Mean age at the start of the survey was 39 years (*age*), which increased from 33 to 44 years across subgroups with increasing length of the smoking career. More than half of the respondents had left school after the mandatory minimum schooling (≤ 9 years). Three out of four had children younger than 16 years at the start of the study (mean of dummy *children* = 0.764) and 2% had a new baby in that year (mean of dummy *new baby* = 0.018). The overall share of unmarried was 11% (mean of dummy *single*=0.109), while 6% became separated, divorced or widowed during the study period (mean of dummy *div-wid* =0.063).

Indicators of addiction: Two tobacco addiction indicators were included: the starting age of smoking (*debut_age*) and the log of the maximum number of cigarettes smoked per day (*number_cig*). Mean starting age was 21 years, but declined significantly with the number of years as a smoker (29 years for ST- and 18 years for LT-smokers). Also, the mean number of daily cigarettes varied with smoking experience; LT-smokers reported to smoke 15 cigarettes per day compared to 13 and 10 cigarettes for MT- and ST-smokers, respectively.

Price and income indicators: We let the annual log-increase of the CPI-deflated price of a 20-pack of Marlboro represent the general price development for cigarettes ($year_t$ - $year_{t-1}$), as prices of the various cigarette brands in Norway tend to move in parallel. Since current smokers can be said to have already "absorbed" the previous year's price level, we assumed that it is the relative price increase that could potentially impact the quitting hazard. Family income data (based on assessments for tax purposes) was deflated by the CPI and normalized by family size (i.e., divided by the square root of family numbers, including children below 16 years of age). Considering the high proportion of married respondents and female home-makers with unpaid employment, family income was preferred to individual income. For a small subsample, income data were missing or reported as zero in certain years. Instead of deleting these units, at the risk of introducing selection bias, we replaced the missing values with the individual's mean income from the other years.

Health status and health shock indicators. At the start of the study period, respondents had a certain health status and a stock of information regarding their status. At R1, R2, and R3 some new information was provided, which was supplemented with results from the medical tests announced after the screening. This information - which to some respondents could have emerged as health shocks - may have affected the decision to continue or to quit smoking. We have incorporated these ideas by including: (i) health status variables at R1 (self-reported and registered by health personnel), (ii) dummies indicating worsened health status from R1 to R2, or from R2 to R3; and (iii) test results from blood tests and x-rays for the corresponding screening year.

A significantly higher proportion of LT-smokers than smokers in the other groups had experienced symptoms of a cardiovascular illness at R1 (indicated by the dummy *symptoms*), had a cardiovascular disease or diabetes (dummy *illness*) or had a body mass index (BMI) above 25 (dummy *bmihigh*) as presented in Table 1. Long-term smokers also reported a higher frequency of having a physically demanding job (dummy *actwork)* and a higher frequency of exercise (dummy *exercise*) than the group with a smoking career less than 6 years. More LT-smokers received disability pension (dummy *disabled*) than both of the other groups.

Negative health shocks, recorded as dummies at R2 and R3, may have started to influence smoking behaviour prior to that date (e.g., the individuals may have experienced symptoms indicating lung problems soon after the previous screening). As described in Table A2, this is taken into account by the creation of the *sympshock12-23* and *illshock12-23* variables. Long-term smokers reported a higher prevalence of symptoms or illnesses at R2 and R3 than the other two groups.

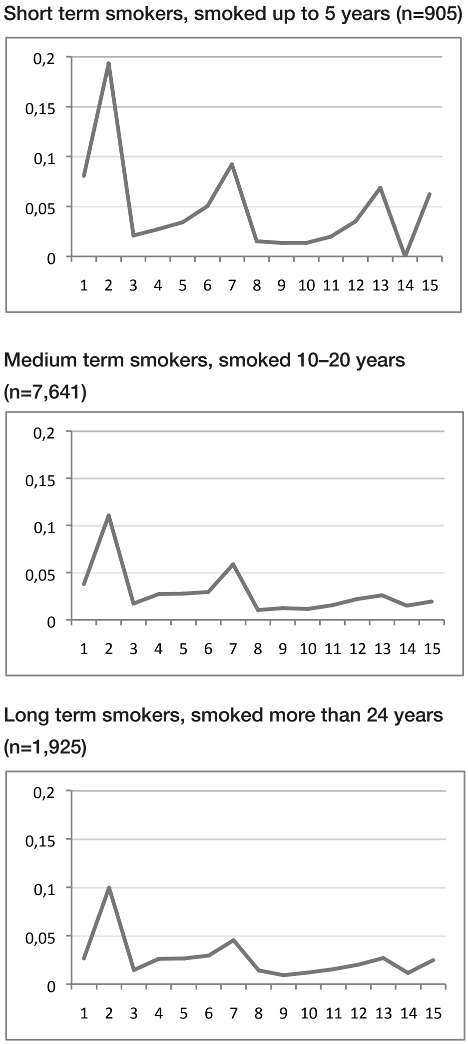
Dependent variable: Over the study period, 29% of the LT-smokers reported to cease smoking compared to 32% and 48% among the MT- and ST- smokers. The quitters were asked whether they had terminated the habit less than 3 months, 3-12 months, 1-5 years or more than 5 years prior to the date of recording. From these entries we constructed a binary quitting variable based on the year the individuals ceased smoking (if they quit smoking at all). For the 1–5 years category the quitting year is constructed from the information the respondents gave regarding the number of years as a smoker at each screening[3].

Econometric model

Studies examining cigarette quitting either employ a discrete choice framework or duration models (Forster and Jones, 2001). Many logistic and probit models of quitting are found in the literature, see e.g. Hyland et al. (2004), Ross, Powell, Tauras and Chaloupka (2005), and DeCicca, Kenkel and Mathios (2008), while time-series analyses using the smoking participation rate as the dependent variable have proved less useful. This is because changes in such rates cannot distinguish between changes in the starting and the quitting rate, and factors influencing the two rates may differ (Douglas 1998).

We employed a discrete-time duration model (Jenkins, 2005). A duration model focuses on the risk of transition (hazard) from one state to another (e.g., from smoking to non-smoking) while taking into account control variables and duration dependence. Compared to the well-known Cox regression model in survival analysis, the current model has two advantages: i) the duration dependence is specified and ii) we can easily account for time varying covariates like prices, income etc. We modelled the duration dependence flexibly using a piecewise constant specification based on year dummies and allowed for stepwise changes in the coefficient vector by splitting the sample in three groups according to the length of pre-sampling the smoking careers. In modelling the quitting hazard, we kept attention on two time variables: time in process as a smoker and time in the screening process. This is be-

Figure 1. Hazard rates of cigarette quitting*



*The x-axis represents the number of time periods (years) since the start of the study

cause a respondent's inclination to cease smoking is likely to depend on how long he/she has been addicted to smoking and on how long he/she has been scrutinized by health authorities and has accumulated health screening information.

In addition to the set of covariates, we also accounted for unobserved heterogeneity. Several ways of modelling heterogeneity in survival processes were considered; the Akaike and the Bayesian information criteria (AIC and BIC) suggested that the gamma specification was superior to the normal and the discrete multinomial distribution, so the results shown in Table 2 are based on a cloglog model with gamma distributed heterogeneity.

The model and the method are described in more details in the Appendix. All data analyses were completed using Stata version 12.1.

Results

The non-parametric hazard rates for ST-, MT- and LT-smokers are presented in Figure 1, where the x-axis represents the time elapsed since the start of the study. For all groups there was a downward sloping trend indicating that the quitting rate declined over time. The trends each had three peaks, which may well reflect the three rounds of screening but alternative explanations are possible. For all smokers, period 2 represents R1 (the year of the first screening), R2 occurred in period 7 for the majority of the respondents but varied somewhat, as did the period when the screening was terminated at R3.

Examining the peaks more closely, we found that among the 1,151 smokers who ceased smoking in the first screening year, 39% reported to have done so within the 3 month period before visiting the screening station, 21% quit 3 to 12 months before the screening while 40% quit smoking after the screening that year. The corresponding numbers for the second screening year are 37%, 30%, and 33%, respectively. As the third screening year marks the end of the follow-up period, only those who ceased

Table 2. Regression results for frailty model with gamma distribution
Dependent variable is the hazard of cigarette quitting

	Long term smokers (smoked ≥25 years) No of obs.=19,638			Medium term smokers (smoked 10–20 years) No of obs.=75,248			Short term smokers (smoked up to 5 years) No of obs.= 7,628		
	Exp(b)	95% confidence interval		Exp(b)	95% confidence interval		Exp(b)	95% confidence interval	
Interventions									
Screening1	5.861***	3.785	9.076	3.744***	3.149	4.452	3.944***	2.394	6.497
Screening2	3.619***	1.855	7.059	2.406***	1.663	3.482	2.209*	0.887	5.498
Screening3	1.504	0.711	3.181	1.932***	1.276	2.924	3.179**	1.007	10.03
Demographics									
Male	3.145***	1.766	5.602	2.367***	1.993	2.812	2.028***	1.224	3.358
Age	1.062	0.986	1.143	0.973***	0.956	0.990	0.845***	0.743	0.961
Educ2	1.239	0.917	1.673	1.234***	1.102	1.382	1.557**	1.083	2.237
Educ3	1.482	0.525	3.134	1.920***	1.510	2.441	1.922**	1.037	3.560
Educ4	13.10**	1.859	92.36	2.344***	1.397	3.933	11.88**	1.314	107.3
Children	1.008	0.771	1.316	1.223	1.074	1.391	0.831	0.611	1.129
New baby	1.255	0.339	4.639	1.079	0.792	1.469	1.889**	1.133	3.151
Single	0.613*	0.357	1.054	0.802*	0.641	1.003	0.567**	0.366	0.878
Div-wid	0.615	0.338	1.117	0.875	0.688	1.113	0.638	0.321	1.267
Health									
Symptoms (R1)	3.293***	1.507	7.194	0.905	0.624	1.311	1.401	0.527	3.727
Illness (R1)	0.913	0.550	1.517	1.434**	1.082	1.902	0.512	0.170	1.537
Bmi_high (R1)	1.610***	1.154	2.246	1.262***	1.113	1.430	1.301	0.867	1.952
Actwork(R1)	0.770	0.556	1.066	0.888*	0.786	1.004	0.698*	0.459	1.062
Exercise (R1)	0.906	0.635	1.293	1.138*	0.999	1.297	1.132	0.736	1.741
Disabled (R1)	0.401**	0.162	0.994	0.628*	0.362	1.090	1.701	0.381	7.604
Sympshock12	1.107	0.429	2.858	1.167	0.615	2.216	0.846	0.229	3.132
Sympshock23	0.838	0.171	4.104	0.740	0.266	2.059	0.870	0.097	7.827
Illshock12	1.107	0.634	1.934	1.720***	1.206	2.452	1.286	0.408	4.057
Illshock23	2.033**	1.111	3.720	3.572***	2.566	4.972	1.594	0.323	7.876
Badreport1	0.879	0.595	1.300	0.997	0.816	1.219	0.845	0.477	1.498
Badreport2	1.349	0.815	2.231	1.000	0.764	1.309	0.882	0.401	1.939
Addiction									
Debut_age	1.023	0.942	1.110	1.079***	1.057	1.101	1.187***	1.046	1.347
No.cig	1.032	0.747	1.424	0.686***	0.604	0.780	0.645**	0.449	0.927
Economic									
Cigprice	0.995	0.978	1.013	1.012***	1.004	1.020	1.009	0.987	1.031
Income	0.834**	0.717	0.969	1.026	0.963	1.093	1.203**	1.037	1.395
Gamma varience	3.948***	1.899	8.206	1.635***	0.934	2.863	2.100**	0.808	5.454
LR-test statistic	12.984 (p>0.000)			16.178 (P>0.000)			5.513 (P>0.009)		
Log likelihood	-2356.324			-9867.170			-1435.063		

[i] Please note: The constant term and the coefficients of the time dummies are suppressed but are available on request.
* p<0.1; **p<0.05; ***p<0.01

smoking before that date were registered as quitters.

The following analysis, based on the model and methods described in more details in the Appendix, examine whether the hazard pattern presented in Figure 1 is spurious or could be interpreted as a distinct effect of the screening participation and outcome. Table 2 presents the main estimation results block-wise.

The first block of results shows that, even after taking account of several controls, including health status and health shock indicators, the intervention (screening 1, 2 and 3) substantially influenced the quitting hazard for all groups. The impact of the first screening was particularly high for all groups, and for the first two screenings the effect was higher for LT-smokers than for the smokers with shorter smoking careers. For the latter screening, however, the coefficient of screening 3 was statistically insignificant for LT-smokers.

In all three "smoking-career categories", males had a 2-3 times higher risk of quitting, while age seems to be of importance for only ST- and MT-smokers, i.e. younger people were more likely to quit than older ones. Increased education was associated with increased probability of quitting. Our indicators of addiction did not come out as significant for the smoking cessation of LT-smokers. For ST- and MT-smokers the later the smoking debut and the smaller the number of daily cigarettes consumed, the higher the quitting hazard. Finally, changes in the deflated cigarette price affected MT-smokers only.

Health status at the start of the observation period affected the quitting hazard for MT- and LT-smokers only. In particular, having a high BMI tended to increase quitting, while being disabled reduced it. Having experienced symptoms at R1 substantially affected LT-smokers only while MT-smokers were affected by experienced illness. Illnesses occurring between two adjacent screenings (*illshock12, illshock23*) significantly increased cessation for those who at R1 had smoked more than 10 years. On the other hand, receiving a bad report for the blood tests or the x-ray exams (*badreport1 and 2*) at R1 or R2 did not significantly influence cessation for any group of smokers.

For every group of smokers and for all the parameterizations of latent heterogeneity considered, the null hypothesis of no random heterogeneity is rejected (see the bottom block of Table 2).

Sensitivity analyses

Redefining "true quitters": Since many cigarette quitters are known to start again at some later point, the results above are potentially biased. Even though the individual time-series in our sample are "cut off" when the respondents reported to have ceased smoking, the panel format of the data allows us to examine to what extent a relapse occurs. Doing this, we found that 27% of the smokers who quit in the first screening year reported to be daily cigarette smokers again at R2, and 25% of quitters in the R1-R2 period had started again at R3. To assess the magnitude of the potential bias, we re-ran the estimations after having excluded all observations from the quitters for whom a relapse to smoking is known to have occurred at some later point. Relative to the results in Table 2, the impact of the first screening program was then somewhat diminished for all groups, the impact of the second screening increased for MT- and LT-smokers and became statistically insignificant for ST ($p<0.17$), while the effect of the third screening was virtually unchanged for MT- and LT-smokers and reduced for ST-smokers. Still, the main results appear fairly robust; being invited to and participating in the program was important for the overall impetus to quit among all groups of smokers in the sample.

Omitting screening dummies: Given the importance of the screening dummies on the quitting hazards we also wanted to examine whether excluding them would substantially affect the coefficients of the remaining covariates. The results, however, were very similar to those in Table 2, that is, the same covariates were statistically significant and of basically the same magnitude. Not unexpectedly, the coefficients for the time dummies changed somewhat more, in particular the dummies for periods 2 and 7 (corresponding to R1 and R2 for many respondents). They switched from being small and non-significant to becoming larger and significant.

Concluding remarks and policy implications

The strong and significant impact of the screening intervention on the quitting hazard is interesting and suggests that the screening itself could explain the peaked pattern in Figure 1. The first screening had the largest effect but the influence of subsequent screenings also seems considerable. As mentioned above, many of those who ceased cigarette smoking in the screening years reported to have done so during the three months immediately *preceding* the participation date. One interpretation of this finding is that the letter of invitation reminded and alerted the smokers of the negative health effects of their cigarette consumption and raised a fear of what the screening could possibly reveal. Thus, for a fraction of the smokers who may have already considered giving up the habit, the reminder seems to have been sufficient to take action. Also, the quitting hazard shortly after participation was high, which is in line with results reported of the effect of CT screenings on smoking cessation (Styn et al., 2009; Ostroff et al., 2001).

This screening effect may seem to stand in some contrast to the results reported in the RCT studies mentioned above (Ashraf et al., 2009; van der Aalst et al., 2010; Shi & Iguchi, 2011; Barry et al., 2012). These RCT studies did not find an effect on smoking behaviour when comparing those who received lung cancer screening to their control groups. They all found, however, that screening participants had higher cessation rates than the general population of non-participants. Our study differs in two important aspects; Firstly, our study participants constitute their own control groups, i.e. we compare the quitting hazard in screening years to that of non-screening years for the same individuals. We find that the risk of cigarette quitting is higher in screening years than in non-screening years for the smokers that constitute our study group. Secondly, in our study, *all* inhabitants in certain age groups were invited to participate in the screening and the response rate was remarkably high (88%). This probably implies that the problem of self-selection into screening participation is less pronounced in this study than in studies with a more restricted population from which the participants were invited. Thus, one may expect that the difference between the current sample and the general population is smaller here than in the above cited studies.

Our coefficient estimates for the LT-smokers, in particular the finding of an increased quitting hazard for this group, is undoubtedly important since the health gains for giving up smoking are substantial even for smokers with a long-standing career (Taylor, Hasselblad, Henley, Thun, & Sloan, 2002; Ostbye and Taylor, 2004). Our results suggest that extended use of targeted screening programs or other consultations with health care providers may be particularly effective for this group of experienced smokers. Irrespective of whether or not there has been a "hardening" of remaining smokers in recent years (Lund, Lund, & Kvaavik, 2011; Docherty and McNeill, 2012), any cessation measures that seem to affect LT-smokers in particular should be of interest for policy makers and others concerned with promoting improved health in the population.

Further, the finding that adverse health outcomes recorded at the start of the intervention period and declining health status recorded during the study period seems to influence cessation more strongly for smokers with longer careers could suggest that LT-smokers with health issues may be particularly responsive to anti-tobacco initiatives. In contrast, neither having a cardiovascular disease or diabetes – or symptoms of such – nor being disabled, having a high BMI or exercising regularly are associated with an increased quitting hazard for ST-smokers.

No group of smokers seemed to significantly increase their quitting hazard in response to an unfavourable test result, supporting findings of e.g. van der Aalst et al. (2011), Cox et al. (2003), and Anderson et al. (2009), and suggesting that merely an indication of a negative health development is not sufficient for reducing their smoking habits. Ashraf et al. (2009) and Styn (2009) on the other hand, reported higher cessation rates after abnormal test results or referral to a physician.

Increases in the cigarette tax/price did not seem very effective in influencing the overall cessation rate. While it is generally assumed that young smokers are more price-sensitive (Farrelly, Bray, Pechacek, & Woollery, 2001), it is still a matter of dispute whether adults' quitting behaviour is influenced by price increases (DeCicca and McLeod, 2008). Representing cigarette prices by a relative price increase variable, we found that only MT-smokers are responsive to price changes. However, increased cigarette taxes could still affect smoking intensity (Chaloupka and Warner, 2000; Gallet and List, 2003) and may thus indirectly increase the quitting hazard rate.

Our dataset, although being far from perfect, had a rather long observation window, many participants, and combined personal characteristics, indicators of addiction status, economic factors, health status and health shock variables (subjective and objective), and governmental interventions. This suggests that problems related to omitted variable bias, spurious effects interactions, etc., could be less pronounced than in similar studies based on shorter data vectors. Also, the presence of data from administrative registers may to some extent reduce measurement problems (recall bias, etc.). The very high participation rate suggests that the study sample is fairly representative of the general population and that the risk of selection bias is reduced. Our use of self-reported smoking cessation as the outcome variable, and not any measures of intention-to-quit, may be viewed as a further advantage of the study (IARC, 2008).

The validity of self-reported smoking behaviour can be questioned, however, and it has been claimed that smokers are inclined to underestimate the amount smoked or to deny their smoking all together (Patrick et al., 1994). This reporting bias may be more pronounced for the number of cigarettes smoked per day than for whether or not they smoke. Many studies have examined the validity of self-reported smoking behaviour by comparing survey results to biological markers, and like for instance Wong, Shields, Leatherdale, Malaison and Hammond (2012), they generally suggest that self-reports provide accurate estimates of cigarette smoking prevalence. Adolescents and expecting mothers seem be somewhat more imprecise in their reporting (Patrick et al., 1994; Shipton et al., 2009), but as there were no teenagers included and less than one per cent of the sample were pregnant at the time of interview, this reporting bias should not influence our results to any large extent. Further, since smoking was less stigmatized at the time of the data collection, the risk of people underreporting their actual smoking behaviour is possibly reduced.

One limitation is that the records are from only a three-wave panel within a fourteen year period, and thus provide less information about the two intervening periods. The problem of "heaping" (i.e. the tendency that people report "round" numbers, see e.g. Bar and Lilliard, 2012) could in our case relate both to the number of reported years since they started to smoke and to the number of years since the quitters gave up their habit. As we have split the smokers into groups depending on their pre-sampling smoking career and have set the categories so that they comprise the "round" numbers (5, 10, 15, and 20) as well as the numbers nearby, the potential effect of the first type of "heaping" should be substantially reduced. Further, "heaping" with respect to the number of years since quitting is probably less problematic here than in other datasets, as the relevant retrospective period at each screening interview was relatively short (less than 6 years for most quitters).

The data may, of course, be criticised for being somewhat old and to some extent outdated. Although important smoking related factors have changed since the start of the study period (new cessation products have become available, the knowledge and focus of health-damaging effects of smoking has increased, new restrictions have been introduced, etc.) it seems likely that the structural findings still apply. Participation in a screening program may constitute a "teachable moment" for smokers as it may "motivate individuals to spountanously adopt risk-reducing health behaviours" (McBride, Emmons, & Lipkus, 2003; Taylor et al., 2007).

Despite intensified actions for reducing tobacco consumption there are still millions of daily cigarette smokers. Given that further extensions of many traditional interventions have little political appeal, alternative approaches to reduce smoking are much wanted by the health authorities. The significant effect of being invited to and participating in a screening appears robust, and may prove useful when discussing future policies to promote smoking cessation. This paper suggests that further initiatives for consultations with health personnel, in this case through a screening program, could increase the quitting hazard. That the effect was substantial also for long-term smokers is interesting and could be potentially important in planning future smoking cessation programs.

Declaration of interest None.

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NOTES

1 The counties and the screening periods were: "Oppland" 1976–1978, 1981–1983, 1986–1988; "Sogn og Fjordane" 1975–1976, 1980–1981, 1985–1986, and "Finnmark" 1974–1975, 1977–1978, 1987–1988.

2 Technically, since participants provided smoking information that encompassed their smoking status one year prior to the screening, we started the panel in 1974 for those screened in 1975, in 1975 for those screened in 1976, etc.

3 For a fraction of the respondents this information could not be used due to obvious measurement errors. To avoid possible selection bias, these subjects (8.8%) were assigned a randomly picked year of smoking cessation within the 1–5 year interval. To test the sensitivity of the assignment we re-ran the estimations for the three groups excluding this subgroup of quitters. The estimates remained roughly unchanged and the hazard ratios of screening were still highly significant and their value increased for all groups.

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Appendix: Model and method

We employ a discrete-time hazard model, with τ denoting the first observation year and t the running calendar year, index the individual smokers by i ($i=1, 2,\dots, n$) and analyse the stock of persons conditional on already being a smoker (see Lancaster, 1990, p. 91 and Verbeek, 2004, p. 247). The observation period for individual i extends from period $t=\tau$ till period $t=\tau+s_i$. Its length is i-dependent both because a person was dropped from further follow-up from the year he/she ceased smoking (uncensored cases, $\delta_i=1$) and because the the study design implied that the follow-up period differed among those who continued smoking (censored cases, $\delta_i=0$).

Letting B_i and T_i ($B_i < \tau < T_i$) represent the calendar periods in which individual i begins and ends smoking, respectively, the hazard rate for year t, i.e., the probability that smoker i quits in year t, conditional on having started in period B_i and having smoked until period t, is

(1) $h_{i,t} = P(T_i = t|B_i, T_i > t-1), \mathrm{t} = \tau, \dots, \tau + s_i$.

Since the probability that smoker i did not cease smoking in period t is (1- $h_{i,t}$), the conditional probability of observing the event history in the case of continued smoking throughout the observation window [τ, τ+s_i] is:

(2) $q_{i,\tau+s_i} = P(T_i > \tau + s_i|B_i, T_i > \tau - 1) = \prod_{t=\tau}^{\tau+s_i}(1 - h_{i,t})$.

Using (1) and (2), the probability that individual i quits smoking during the study interval is

(3) $P(T_i = \tau + s_i|B_i, T_i > \tau - 1) = h_{i,\tau+s_i}q_{i,\tau+s_i-1} = h_{i,\tau+s_i}\prod_{t=\tau}^{\tau+s_i-1}(1 - h_{i,t})$.

Combining (2) and (3), the log-likelihood can be expressed as

$$\log(L) = \sum_{i=1}^{n}\{ \delta_i \log[\frac{h_{i,\tau+s_i}}{(1 - h_{i,\tau+s_i})}] + \sum_{t=\tau}^{\tau+s_i}\log(1 - h_{i,t})\}.$$

Defining $y_{it} = 1$ if $t=\tau+s_i$ & $\delta_i=1$, and $y_{it} = 0$ otherwise, we can rewrite the latter expression more conveniently as (see Jenkins 1995, section II):

$$(4)\ \log(L) = \sum_{i=1}^{n} \sum_{t=\tau}^{\tau+s_i} \{y_{i,t} \log[\frac{h_{i,t}}{(1-h_{i,t})}] + \log(1-h_{it})\}.$$

To parameterize h_{it} we chose the cloglog function, which implies that the complementary, continue smoking, probability is

$$1 - h_{it} = \exp[-\exp\{-z_{it}\}] \iff -\log[-\log(1-h_{it})] = z_{it}.$$

Here z_{it} is a linear function of observed covariates x_{it}, and the duration dependence, $\Theta(t)$. In modelling h_{it} two time variables are involved: time in process as a smoker and time in the screening process, because a respondent's inclination to cease smoking is likely to depend on how long he/she (i) has been addicted to smoking and (ii) has been scrutinized by health authorities and thereby has accumulated health screening information. We model $\Theta(t)$ flexibly, using a piecewise constant function based on year dummies, allowing for stepwise changes in the coefficient vector by splitting the sample according to the length of the smoking career (SC) before observation starts, *i.e.,* $SC_i = \tau - B_i$.

Denoting the coefficient vectors of x_{it} and $\Theta(t)$ by, respectively, β_j and μ_j, if the length of the pre-sample smoking career belongs to the *j'th* interval I_j *(ST-, MT- and LT-smokers,* respectively*)*, letting ε_i, represent unobserved heterogeneity, we have

$$(5)\ z_{it} = -\log[-\log(1-h_{it})] = \beta_j' x_{it} + \mu_j' \theta(t) + \varepsilon_i \ for\ SC_i \in I_j.$$

Since the Akaike and the Bayesian information criteria (AIC and BIC) suggested that in modelling heterogeneity, the gamma distribution was superior to the normal and the discrete multinomial distribution, the results presented in section 3 are based on a cloglog model with ε_i following the former distribution.

Appendix Table A1 Variable description

Variables	Operationalization	Type*	Data source**
interventions			
Screening1	Dummy; 1 in the time period of R1	6	H.P, screening
Screening2	Dummy; 1 in the time period of R2	7	H.P, screening
Screening3	Dummy; 1 in the time period of R3	8	H.P, screening
Demographics			
Male	Dummy;1 if male	2	H.P, screening
Age	Age at start of survey 1	Time invariant	S.R, screening
Educ1	Dummy; 1 if highest education is min. schooling (mandatory)	3	Statistics Norway
Educ2	Dummy; 1 if highest education is secondary school	3	Statistics Norway
Educ3	Dummy; 1 if between 12 and 15 years of schooling	3	Statistics Norway
Educ4	Dummy; 1 if highest education is university degree	3	Statistics Norway
Children	Dummy; 1 if having children under the age of 16	Time varying	Statistics Norway
New Baby	Dummy; 1 if having a new baby	Time varying	Statistics Norway
Single	Dummy; 1 if not registered with spouse or cohabitant	Time varying	Statistics Norway
Married	Dummy; 1 if married	Time varying	Statistics Norway
Div-wid	Dummy; 1 if divorced, separated or widowed	Time varying	Statistics Norway
Screening2	Dummy; 1 in the time period of R2	7	H.P, screening
Screening3	Dummy; 1 in the time period of R3	8	H.P, screening
Health			
Symptm (R1)	Dummy; 1 if symptoms of heart/lung illness (R1)	2	S.R, screening
Illness (R1)	Dummy; 1 if having heart/lung illness (R1)	2	S.R, screening
Bmihigh (R1)	Dummy; 1 if body mass index >25 (R1)	2	H.P, screening
Actwork(R1)	Dummy; 1 if having physical demanding work (R1)	2	S.R, screening
Exercise (R1)	Dummy; 1 if exercising at least 4 hours per week (R1)	2	S.R, screening
Disabled (R1)	Dummy; 1 if receiving disability benefit (R1)	2	S.R, screening
Sympchange12	Dummy; 1 if new symptoms are reported in R2	4	S.R, screening
Sympchange23	Dummy; 1 if new symptoms are reported in R3	5	S.R, screening
Illchange12	Dummy; 1 if new heart/lung illnesses are reported in R2	4	S.R, screening
Illchange23	Dummy; 1 if new heart/lung illnesses are reported in R3	5	S.R, screening
Badreport1	Dummy; 1 if score above cut-off, blood tests or x-ray, in R1	6	H.P, screening
Badreport2	Dummy; 1 if score above cut-off, blood tests or x-ray, in R2	7	H.P, screening
Addiction			
Smokeage	Age when started to smoke	2	S.R, screening
Number_cig	Ln of max reported cigarettes smoked per day	Time invariant	S.R, screening
Economic			
Cigprice	Ln of the difference of CPI adjusted price (P1975-P1974)	Time varying	Statistics Norway
Income	Ln of CPI adjusted family income	Time varying	Statistics Norway
Dependent var.			
Quit smoking	Dummy; 1 if quitting smoking	1	S.R, screening

*For dummy variables, see Table A2 for explanation
** S.R= self reported, H.P = registered by health personnel

Appendix Table A2 Types of dummy variables

Time period	Year	1[i]	2[ii]	3[iii]	4[iv]	5[iv]	6[v]	7[v]	8[v]
1	1974	0	1	0	0	0	0	0	0
2	1975, 1. screening	0	1	0	0	0	1	0	0
3	1976	0	1	0	1	0	0	0	0
4	1977	0	1	0	1	0	0	0	0
5	1978	0	1	0	1	0	0	0	0
6	1979	1	1	0	1	0	0	0	0
7	1980, 2. screening	.	1	0	1	0	0	1	0
8	1981	.	1	0	0	1	0	0	0
9	1982	.	1	0	0	1	0	0	0
10	1983	.	1	1	0	1	0	0	0
11	1984	.	1	1	0	1	0	0	0
12	1985	.	1	1	0	1	0	0	0
13	1986, 3. screening	.	1	1	0	1	0	0	1
14	1987	.	.	.	.	.	.	.	.
15	1988	.	.	.	.	.	.	.	.

[i] The vector exemplifies an individual who quits smoking in 1979.
[ii] The vector exemplifies a dummy that is time-invariant for the whole period (e.g. male, started to smoke at an early age, etc.)
[iii] The vector exemplifies an individual who changes status in 1983 (e.g. become divorced this year)
[iv] The vectors exemplifies an individual who has changed health status between two screenings (*Sympchange 12, 23* and *Illchange 12, 23*)
[v] The vector exemplifies an individual who has been screened in 1975, 1980 and 1986.