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Smoking and Education in France*

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Abstract: The educational expansion experienced by cohorts born after World War II in France is used to estimate the effect of schooling on the decisions to start and quit smoking. Educational expansion was driven by several reforms of the school system, and generated important changes in the distribution of schooling, especially for cohorts born between the 40s and the 70s. However, the share of those individuals who graduated with top university degrees and from the "Grandes Ecoles" remained stable throughout most of the period. We compare changes in smoking between these individuals (the control group) and the rest of the population (the treatment group), and between cohorts, to identify the effect of schooling on smoking. This difference-in-difference strategy provides evidence that, for women born between 1945 and 1965, education has had a negative effect on the decision to start smoking, and a negative impact on smoking duration. Results for men from the same birth cohorts are imprecise, but also suggest negative effects of education on smoking, albeit smaller than those observed for women.

Keywords: Smoking, Education, Duration Analysis, France.

JEL codes : I12, I18, I11

* We are grateful to Marc Gurgand for helpful discussion of the paper and to seminar participants at University of Lausanne.

SMOKING AND EDUCATION IN FRANCE.*

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Abstract

The educational expansion experienced by cohorts born after World War II in France is used to estimate the effect of schooling on the decisions to start and quit smoking. Educational expansion was driven by several reforms of the school system, and generated important changes in the distribution of schooling, especially for cohorts born between the 40s and the 70s. However, the share of those individuals who graduated with top university degrees and from the "Grandes Ecoles" remained stable throughout most of the period. We compare changes in smoking between these individuals (the control group) and the rest of the population (the treatment group), and between cohorts, to identify the effect of schooling on smoking. This difference-in-difference strategy provides evidence that, for women born between 1945 and 1965, education has had a negative effect on the decision to start smoking, and a negative impact on smoking duration. Results for men from the same birth cohorts are imprecise, but also suggest negative effects of education on smoking, albeit smaller than those observed for women.

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1 Introduction

Historically, smoking was not inversely related to education (Nourrisson, 1999; Hughes, 2003). The education-smoking gradient has become negative in most affluent countries only over the past forty years or so (see, Kenkel and Liu, 2007). This reversal in the education-smoking gradient coincides with the emergence of firm scientific evidence on the health hazard of smoking, and the development of tobacco control policies. For instance, between 1980 and 2000, cigarette prices doubled in France as a consequence of strong tax hikes and information about the harmful effects of smoking was publicised widely by media campaigns and medical institutions. Over the same period, tobacco expenditures of French households in the three poorest deciles of the income distribution did not vary, while expenditures of the richest households decreased strongly (Godefroy, 2003).¹ This paper addresses the question whether there has been a *causal* negative effect of education on smoking in cohorts that grew up in times of mass smoking: *the baby-boomer generation* born between 1945 and 1965.² It focuses on two outcomes that characterise smoking histories and that help to determine the health risks of smoking: the age of smoking onset and smoking duration.

There are at least two routes through which education may have a causal negative impact on smoking. First, the smoking-education gradient may be accounted for by a "value of life" argument. Smoking not only increases the probability of illness, but also the losses incurred, which depend on education through the discounted wage stream one can expect to receive over one's lifecycle. If more educated individuals have higher wages, they face *ceteris paribus* higher losses from dying prematurely: education is positively correlated with the opportunity costs of smoking. This is the opportunity cost explanation. Second, Grossman (1972, 2000) hypothesises that the more educated are more efficient at producing health from inputs such as health care or lifestyle behaviours - this is termed productive efficiency - and because they make better choice of health behaviours - this is allocative efficiency. They would allocate less resources to smoking (and other health-damaging but pleasurable behaviours) because they are more capable to process new information about the risks of smoking-induced health events.³ Both the opportunity costs and the efficiency arguments predict that education should have a negative impact on smoking. This prediction was challenged by Farrell and Fuchs (1982), who remarked that the education-smoking correlation is spurious if a third factor - for instance a low discount rate or high innate cognitive abilities - has simultaneously a positive impact on the demand for education and a negative effect on smoking. Since then, assessing the causal impact of education on smoking has become an

important issue in health economics. Most studies address it by applying instrumental variable (IV) techniques, as do we here.

Our empirical strategy to identify the causal effect of education on smoking exploits the educational expansion that took place in France after World War II. Between 1945 and the 1990s, levels of schooling increased from one generation to another. This expansion was favoured by several state reforms, which raised the compulsory minimum school leaving age, programmed school construction, and lowered financial hurdles and selectivity for access to secondary education tracks and the lower university levels. Applying an idea originally developed by Gurgand and Maurin (2007) to estimate earning returns to education, we use those individuals with the best schooling attainments – defined here as a Baccalaureat plus 5 years of completed studies - as a “control group”. The share of this group remained constant – around 10% - in birth cohorts born between 1945 and 1965, which means that selectivity into this education level was left unaffected by the educational expansion. We therefore assume that the difference between the unobservable characteristics of the control group and those of the rest of the population did not vary across cohorts. However, education in the rest of the population increased across cohorts, as barriers were progressively lowered for the access to other levels of education (defined as the “treatment group”). Comparing the education and smoking gaps between the control and the treatment groups and between cohorts provides a difference-in-difference strategy to estimate the impact of schooling on smoking. The corresponding IV strategy is to instrument schooling by a set of interaction dummies that are constructed by crossing dummies for birth cohorts with a dummy for membership of the treatment group. This set of instruments captures the fact that only individuals in the treatment group were affected by changes in the supply of education, and that the intensity of the treatment varied from one cohort to another.

Using individual retrospective data on smoking histories drawn from two nationally representative surveys, we estimate, for men and women separately, discrete time duration models for the decisions to start and to quit smoking. Simple OLS estimates of linear probability models show negative correlations of education with starting, and positive correlations with quitting: the more educated starts later in their life and have lower smoking durations. The IV estimates do not significantly differ from standard OLS estimates. They have the same signs, are slightly higher in magnitude, but are much more imprecise. A statistically significant effect is found for smoking duration among women only: an additional completed year of schooling increases the hazard of quitting by about 0.3 percentage points. When we also control for interaction effects between calendar time or cigarette price and education, the latter has a negative effect on women’s hazards of starting and quitting: less 0.3 percentage points per additional completed year of education for the former, and plus 0.6 percentage points for the latter. We interpret these results as evidence that

the education-smoking relationship is causal for women. For men, the returns to education on smoking are negative, but the IV estimates are too imprecise: it is not possible to reject or confirm the causal hypothesis. Last, the IV point estimates are higher than the OLS correlations, probably because school policies have had more impact on the education decisions of persons with low education levels, who have higher marginal health return to schooling.

The paper is organized as follows. Section 2 surveys the theory and the evidence on the education-smoking/health relationship. Section 3 describes changes in the educational system over the Twentieth Century in France, and explains the identification strategy. Section 4 presents the data, discusses the reliability of the retrospective data on smoking histories, and outlines the empirical specification. Section 5 presents and discusses the results. Section 6 analyses the complementarity between education and anti-smoking policies. Section 7 concludes.

2 Theory and existing evidence

This section proposes a simple framework to formalise the opportunity cost and the efficiency arguments. It shows that they not only imply that smoking should be negatively correlated with education, but also that the effect of smoking policies varies with levels of education. It outlines then the identification issues raised in the empirical literature.

2.1 A model of education, smoking and anti-smoking policies

We consider a consumer in a two-period, two-goods economy. To keep things simple, there is no saving. Let c_t and y_t be, respectively, the consumption of tobacco and a numeraire good in period t . The utility function of the consumer is $U(c_t, y_t) = u(c_t) + y_t$, where $u(\cdot)$ is an increasing concave function. At each period, they earn a wage $w(E)$, where E is their education level. The wage increases in E ($w_E \geq 0$) and the budget constraint is $w(E) = y_t + pc_t$, where p is the relative price of cigarettes. We also introduce a survival probability for the second period, $\pi(c_1, K)$, which depends on the first-period tobacco consumption and knowledge K . $\pi(\cdot)$ is decreasing and concave in c , decreases in K , and $\pi(0, K) = 1$. The efficiency hypothesis implies, first, that those who have more knowledge perceive higher marginal risk from smoking, so that the cross-derivative π_{cK} is negative. Second, knowledge K is produced using education and a set of information Ω , such that the more educated are more efficient: $K = h(E, \Omega)$ with $h_E, h_\Omega, h_{E\Omega} \geq 0$ and $h_{EE}, h_{\Omega\Omega} \leq 0$. The maximisation problem is:

$$\begin{aligned} & \text{Max}_{c_1, c_2, y_1, y_2} \{u(c_1) + y_1 + \beta(E)\pi(c_1, K)[u(c_2) + y_2]\} \\ & w(E) = y_t + pc_t, c_t \geq 0, y_t > 0, \quad t = 1, 2 \end{aligned} \quad (1)$$

where β is a discount factor, which is non-decreasing in E (Becker and Mulligan, 1997).⁴ Since there is no saving, the optimal solution can be found by solving the optimisation problem for the second period only. $V(E, p)$, the second-period indirect utility function, represents the education-related opportunity cost of smoking. Then, the individual smokes if:⁵

$$\lambda(E, p, \Omega) = u_c(0) - p + \beta(E)\pi_c(0, K)V(E, p) > 0 \quad (2)$$

Equation (2) states that the individual smokes if the current marginal utility at $c=0$ is greater than the full price of smoking. The latter is the sum of the market price, p , and the expected marginal cost of smoking, $-\beta\pi_c V(E, p)$, which is positive since π_c is negative.

What is the marginal effect of education on smoking? A positive shock on E decreases the probability of participation because:

$$\frac{d\lambda}{dE} = +\beta_E \pi_c V + \beta \pi_{cK} h_E V + \beta \pi_c V_E \leq 0 \quad (3)$$

First, the more educated are more efficient ($h_E \geq 0$ and $\pi_{cK} \leq 0$) so that they perceive a higher marginal risk of not receiving the second period benefit V . Second, the positive effect of education on the discount factor ($\beta_E \geq 0$) reinforces the education-smoking gradient. Third, the marginal loss of utility is higher for the more educated if they do not survive in the first period: they face higher education-related opportunity costs because V_E is positive.⁶ Education has a direct negative causal impact on smoking: this is the first prediction of the model.

What is the marginal impact of a change in available information Ω ? Differentiating $\lambda(E, p, \Omega)$ with respect to Ω yields:

$$\frac{d\lambda}{d\Omega} = \pi_{cK} h_{\Omega} \beta V \leq 0 \quad (4)$$

Hence, smoking participation should fall because information renders opportunity costs more salient. If one assumes as a second-order approximation that π_{cc} and π_{cK} do not vary with education, then the decrease in consumption is more important for the more educated: information and education are complementary in the production of smoking abstinence.

$$\frac{d\left(\frac{d\lambda}{d\Omega}\right)}{dE} = \pi_{cK} \left[h_{\Omega} \frac{d(\beta V)}{dE} + h_{E\Omega} \beta V \right] \leq 0 \quad (5)$$

which is negative because the more educated are more efficient ($h_{E\Omega} > 0$), and they face higher discounted opportunity costs (βV increases with E). The more educated should be more affected by the release of health warnings. This is the second prediction of the model.

Consider now a permanent price increase. This has a direct negative effect on participation because it increases the immediate cost of smoking. However, it also depreciates the expected value of life for the second period, since V is decreasing in p . We expect this indirect effect to be lower than the direct effect, *i.e.* the price elasticity should be negative, although no clear prediction can be drawn at this stage:

$$\frac{d\lambda}{dp} = -1 + \beta\pi_c V_p \quad (6)$$

However, while the information elasticity is higher (in absolute value) for the more educated, this is the opposite for the price elasticity, since:

$$\frac{d\left(\frac{d\lambda}{dp}\right)}{dE} = \beta_E \pi_c V_p + \beta \pi_{cK} h_E V_p \geq 0 \quad (7)$$

This is the third prediction of the model, which holds because V_p does not vary with E . While the more educated benefit more from information policies, the less educated are more sensitive to price variations, so that a price increase should imply a larger gain in terms of life expectancy for them than for the more educated. In this sense, price policies and education policies are likely to have different distributional implications for the production of smoking abstinence, and more generally public health.

2.2 Empirical identification of the schooling-smoking relationship

The existence of a causal effect of schooling on health outcomes and behaviours was first seriously challenged by Farrell and Fuchs (1982). They emphasise that “the schooling-health connection may result from the action of some underlying differences among individuals that affect both schooling and health behaviours, such as family socialization, mental ability, or internal rate of time preference” (p. 218). Since then, a number of studies have used the method of instrumental variables (IV) to identify the causal effect of schooling on smoking (or, more generally, health).

IV estimation requires that a set of variable – the instruments - be significantly correlated with schooling and can be excluded from the smoking equation. If the first condition fails, then the instruments are said to be weak, and IV estimates are biased in the same direction as estimates obtained without instrumentation (Bound, Jaeger and Baker, 1995). The second condition - the

exclusion restriction – must have some theoretical justification.⁷ For instance, Grimard and Parent (2007) and de Walque (2007) instrument education by either a gender-cohort dummy for being a male in the cohorts at risk for induction into the Vietnam war (Grimard and Parent) or by a continuous measure of the induction risk (de Walque).⁸ As college attendance was a way to avoid the draft, cohorts of US men that were at risk of induction had good reason to enroll in college after the high school even if it would not have been their choice in normal times. Here, the IV strategy can be interpreted in a difference-in-differences (DiD) framework. These papers compare implicitly trends in smoking across cohorts between men and women, the latter being the "control group". Both studies present evidence that education has a negative impact on smoking, and increases the likelihood of quitting for smokers.

The literature also use instruments related to educational policies and schooling policies, because policy variables that describe variations in the supply of education or in the institutional constraints on schooling decisions arguably do not affect health outcomes and behaviours other than through their indirect effect on the demand for education. Along these lines, Kenkel *et al.* (2006) instrument schooling in their smoking and obesity equations by US state-specific educational policy variables, such as compulsory schooling laws.⁹ As the timing of changes in compulsory schooling differed from one state to another, schooling decisions vary by birth cohorts and state. Hence, the comparison of smoking prevalence and schooling levels between states and between cohorts identifies, in a DiD set-up, the impact of education on smoking, if cohort-state trends do not affect smoking.

Schooling reforms have also been used to infer the effect of education on smoking or health when only variations between cohorts are available. Arendt (2005) uses Danish school reforms in 1958 and 1975, which modified the supply of education and increased the compulsory school leaving age (CSLA), to compare schooling and smoking between those cohorts that were affected by these reforms and those cohorts that were not. In the same vein, using French data, Albouy and Lequien (2008) exploit two reforms that extended the CSLA from 13 to 14 and 14 to 16 for those cohorts born between 1923 and 1952 and after 1952 respectively, to estimate the impact of the school leaving age (their measure of education) on survival at age 80. In both studies, the instrument is merely a dummy that indicates whether the individual belongs to a birth cohort that was treated by the reform. Identification here relies on a regression discontinuity design (RDD) that relates differences in smoking to differences in schooling between the first birth cohorts that have been affected by a reform and cohorts born one to three years earlier. In both studies, the effect estimated using the RDD is higher in magnitude, of same sign, and standard errors are so large that neither the exogeneity of schooling, nor the null of no schooling effect can be rejected. As outlined by Grossman (2004), "treatment effects" estimates are often larger than simple OLS because they

rely on school policies that affect mainly the distribution of the lowest education levels, *i.e.* those individuals who potentially have the highest marginal health returns to education.¹⁰

Our paper tests the first prediction of our model: the causal negative effect of education on smoking. As the best available evidence is provided by studies that use schooling reforms to identify the causal effect of education in a DiD framework, we also follow this approach. Taken together, the second and third predictions of the model imply that smoking policies, which are a mix of information campaigns and tax hikes, must have had a negative marginal effect on smoking. The way education interacts with smoking policies is however *a priori* ambiguous. Yet, smoking policies may affect the slope of education-smoking relationship. Section 6 proposes some suggestive empirical evidence on this issue. Section 3 now presents our identification strategy.

3 Educational expansion and the identification strategy

3.1 Education reforms and educational expansion in 20th Century France

Figures A1 and A2 in Appendix A show how the distribution of education levels has evolved for birth cohorts born between 1919 and 1970. Given historical changes in the nomenclature of degrees, we here use four broad education levels to measure the highest national degree achieved by an individual: no qualification and primary education (*NOQUAL/CEP*), short secondary general and vocational/technical or professional education under the Baccalaureat (*BEPC/CAP*), the Baccalaureat (*BAC*), degrees after the Baccalaureat (*BAC+2/BAC+3/BAC+4*). Table A1 defines these education levels more precisely, gives some equivalence between French, UK and US educational programmes according to the ISCED-97 classification, as well as the equivalence scale in terms of completed years of schooling (OECD, 1999).

There has been a clear educational expansion for both women and men throughout the 20th Century, which was made easier by series of reforms. Lower secondary education became free at the turn of the 1930s. The Zay reform, in 1936, extended the CSLA from 13 to 14 for cohorts born after 1923. Needs for skilled workers strongly increased after World War II as well as the demand for education so that, in 1959, the Berthoin reform of the educational system was designed to favour access for children of all social classes to secondary education.¹¹ The CSLA rose from 14 to 16 and new educational tracks were created for individuals born after 1952. As a consequence, secondary school and university attendance as well as post-Baccalaureat programmes developed vigorously, especially after Fouchet's reforms in 1963 and 1966, and Faure's law in 1968. A last hurdle toward mass education was crossed with the Haby reform, adopted in 1976, which unified educational tracks in the first cycle of secondary education for individuals born after 1965. Before 1976, pupils could be oriented into vocational tracks after the second (completed) year of

secondary school (the “5ème”), while orientation is now usually determined after four years (Prost, 1981). In 1984, the Ministry of Education decided 80% of each cohort should achieve the Baccalaureat level, and a new "vocational" Baccalaureat was created in 1985/1986. Nowadays, about 60% of individuals, born since the end of the 1970's, have the Baccalaureat, whilst this figure was only 20% for those born in the 1950's.

In comparison with Albouy and Lequien (2008), the main covariate of interest in the current paper is schooling level, which will be measured in the regressions on a convenient continuous metric: the number of completed years of schooling according to the ISCED equivalence scale. To identify the effect of schooling on mortality, Albouy and Lequien use instead the number of years spent at school, which may not reflect actual achievements.¹² Their RDD identification strategy uses the clear and discontinuous increase in the proportion of individuals leaving school after age 16 in cohorts born just after 1952. However, Figures A1 and A2 display evidence that reforms did not induce any significant discontinuity in the distribution of schooling levels between cohorts born before and after 1952. As a consequence, it is not possible to use here such a cohort-based RDD.

We now provide a more detailed picture of the evolution of schooling attainment in higher education in the dataset that is used for the estimation (a full description of the data is provided in Section 4). The dataset was constructed by appending individual records from two cross-sectional surveys of the national statistics office (INSEE): “Enquête sur les Conditions de Vie des Ménages 2001” (*EPCV2001*) and “Enquête santé 2002-2003” (*ES2003*). Figures A3 and A4 focus on cohorts born between 1942 and 1976, and decompose the distribution of schooling achievements by cohort and gender in six levels: *Grandes Ecoles* (“*GE*”), Five years after the Baccalaureat but non *Grandes Ecoles* (“*BAC+5, non-GE*”), between two and four years after the Baccalaureat (“*BAC+2/BAC+3/BAC+4*”), *BAC*, *BEPC/CAP* and *NOQUAL/CEP*. These figures highlight one important empirical fact: the share of those individuals with a *BAC+5* level does not appear to have increased before cohorts born in the mid-1960s. For males, it is even clear that the share of graduates from the *Grandes Ecoles* was fairly stable across cohorts born before the mid-1970s. Gurgand and Maurin (2007, hereafter *GM*) observe further that the social composition of this group of individuals were left unaffected by the educational expansion.¹³ Assuming that, conditional on observable variables (here reduced to social background), individuals are selected on unobservables (such as ability or time preference) into education levels, the mean value of unobserved heterogeneities in the *Grandes Ecoles* graduates should have remained constant. If this group of individuals has experienced no systematic compositional change in unobservable factors over cohorts born between the 1940s and the 1970s, it forms a legitimate “control group”. Then, by looking at changes between cohorts in the smoking gap between this control group and the rest of the population, it is possible to assess the causal effect of smoking on education. We now present

more formally this DiD strategy. Section 4 will adapt it to the particular characteristics of the dataset.

3.2 Identification strategy

We now present Gurgand and Maurin's (2007) identification strategy, assuming that we have for a panel of individuals i , measures of smoking behaviour O_{ia} at age a . They have finished their schooling, with level e , which is an ordered time-invariant variable that takes integer values from 1 to S . c index cohorts and t calendar year. We start from the following linear model of the schooling-smoking relationship:

$$\mathbf{E}(O_{ia} | a = a_i, t, c_i, e_i = s, u_i) = \theta_{c_i} + \gamma_{a_i} + \beta_s + u_i \quad (8)$$

where $\mathbf{E}(\cdot)$ is the conditional expectation operator, and the θ_c , γ_a and β_s are dummies corresponding respectively to the cohort, the age and the schooling level of individual i . The cohort and age dummies absorb the effect of calendar time. u_i represents the time-invariant effect of unobserved characteristics (such as mental ability or time preferences). The identifying restriction proposed by GM is:

$$\forall c, c': \mathbf{E}(u | e = S, c) - \mathbf{E}(u | e < S, c) = \mathbf{E}(u | e = S, c') - \mathbf{E}(u | e < S, c') \quad (9)$$

where the index i has been dropped for convenience. It amounts to supposing that the difference in observables between the top-level individuals and the rest of the population remains constant across cohorts. Individuals at the top education level ($e=S$) belong to the control group, while the rest of the population ($e<S$) forms the treatment group. Further, in cohort c 's top-level, average smoking is:

$$\mathbf{E}(O | e = S, c, a) = \theta_c + \gamma_a + \beta_S + \mathbf{E}(u | e = S, c, a) \quad (10)$$

while for the rest of the population it is:

$$\mathbf{E}(O | e < S, c, a) = \theta_c + \gamma_a + \sum_{s=1}^{S-1} \beta_s \Pr(e = s | e < S, c) + \mathbf{E}(u | e < S, c) \quad (11)$$

where $\Pr(e=s|e<S, c)$ is the probability to reach level s for individuals in cohort c 's treatment group. The smoking gap at age a between the treatment group and the control group in cohort c is:

$$\Delta(c, a) = \mathbf{E}(O | e < S, c, a) - \mathbf{E}(O | e = S, c, a) \quad (12)$$

Using equations (9) to (12), the change in smoking gap at age a between cohorts c and c' identifies:

$$\forall c, c': \Delta(c', a) - \Delta(c, a) = \sum_{s=1}^{S-1} \beta_s [\Pr(e = s | e < S, c') - \Pr(e = s | e < S, c)] \quad (13)$$

Hence, if the “true” model is given by (8), and assumption (9) holds, a DiD strategy based on (13) will identify a weighted average of the causal effects β_s . What does this estimate represent?

Note that $\Pr(e = 1 | e < S, c') = 1 - \sum_{s=2}^{S-1} \Pr(e = s | e < S, c')$, and equation (13) becomes:

$$\forall c, c': \Delta(c', a) - \Delta(c, a) = \sum_{s=2}^{S-1} (\beta_s - \beta_1) [\Pr(e = s | e < S, c') - \Pr(e = s | e < S, c)] \quad (14)$$

If there are negative returns to education due to smoking, then $\forall s > 1, \beta_s < \beta_1$. Note also, that shares of education levels are generally higher in younger cohorts, except for the upper and the lower levels (see Figures A3 and A4). Hence, $\forall s > 1, \forall c, c'/c' > c, \Pr(e = s | e < S, c') - \Pr(e = s | e < S, c) \geq 0$. As a consequence, $\forall c, c'/c' > c, \Delta(c', a) < \Delta(c, a)$ and the smoking gap must decrease with year of birth. Equation (14) also shows that the estimator will give more weight to schooling levels whose share changed a lot across cohorts. To illustrate this point, Figure A5 represents a hypothetical evolution of shares for four levels of schooling. The fourth level constitutes the control group, as its share (at the top) remains constant. The y-axis gives the cumulated percentages of shares, which can be interpreted as a measure of u (more precisely, $u = F(v|X)$, where F is the c.d.f. of some unobserved trait v affecting selection, conditional on age and a set of observed characteristics X). In this set-up, the DiD strategy essentially compares the smoking gap between individuals in CG (the “control group”) and individuals in T1 or T2, to the smoking gap between CG’ and T1’ or T2’. Hence, the identification relies on those individuals whose education level would be different, given their unobserved characteristics, if they belonged to a different cohort, e.g. those in T1 with $e=2$ who would have been in T1’ with $e=3$, had they belonged to cohort c' . Since the size of T2 is bigger than the size of T1, this identification strategy will tend to reflect the health returns to education at education levels where changes have been more important. As shown in Figures A3 and A4, for cohorts born before 1970, educational expansion essentially affected the lower part of the schooling distribution, where returns are expected to be higher. In the end, the DiD strategy will give more weight to changes in smoking behaviour between cohorts amongst the less educated.

A straightforward IV equivalent procedure is to estimate the following model (Duflo, 2001; Gurgand and Maurin, 2007):

$$\begin{aligned} O_{ia} &= \theta_{c_i} + \gamma_{a_i} + \alpha 1\{e_i = S\} + rE_i + \varepsilon_{ia} \\ \varepsilon_{ia} &= u_i + \eta_{ia}, \mathbf{E}(\eta_{ia} | c_i, a = a_i, e_i, E_i) = 0 \end{aligned} \quad (15)$$

where E_i – the number of completed years of schooling – is instrumented by a set of dummies constructed by interacting membership to the “treatment” group and cohort dummies, i.e.

$\{\forall c, \theta_c \times 1\{e_i < S\}\}$. The intuition is that these interaction dummies are correlated with changes in education policies (such as school construction or institutional reforms of rules of access to education levels), but that these changes did not affect the control group, i.e. those individuals who would have anyway managed to get the highest schooling attainments given their observable and unobservable characteristics. Identification requires that there be no difference in cohort trends in smoking between the top level and the rest of the population, so that the instruments can be safely excluded from equation (15). Hence, identification is parametric in essence, but the exclusion restriction can be tested. This paper estimates variants of equation (15). In this perspective, the next section presents the data, defines the estimation sample and the outcomes of interest, and adapts the empirical strategy to the peculiarities of the data.

4 Data and empirical model

This paper studies smoking behaviour through the decisions to start and quit smoking. Retrospective data on smoking histories are drawn from two data sets: *Enquête Permanente sur les Conditions de Vie des Ménages* [Permanent Survey on the Conditions of Living of French Households] (EPCV2001); *Enquête Santé* 2002-2003 [Health Survey 2003-2003] (ES2003). These are nationally representative surveys of households and individuals conducted by the French national institute for statistics and economic studies (INSEE). About 5,200 households participated to EPCV2001 in March 2001. A core section gives detailed information on individual characteristics. Of the 12,653 individuals surveyed, one individual aged over 15 in each household was randomly drawn to answer a special health section. There are a total of 5,194 observations for which sociodemographic and health variables are available. More than 16,800 households participated to ES2003, between October 2002 and September 2003, representing a total of 32,165 individuals aged over 15. Health and sociodemographic information were collected for all households members. Note that, compared to other French surveys that also provide information on smoking careers, education is measured here using a very precise nomenclature, in particular for post-baccalaureat education levels. Both surveys provide information about smoking careers, i.e. whether the individual has been a casual smoker, the age of starting, and the age of quitting if they are a former smoker.

4.1 Sample selection

A smoker or a former smoker is defined as someone who smokes or used to smoke at least one cigarette a day. With respect to this definition, should we classify occasional smokers, i.e. individuals who do not smoke every day at the time of the survey, as smokers or former smokers?

In ES2003, occasional smokers that used to be daily smokers are treated as current smokers, in the sense that we know the age of smoking onset, but not the age at which they stopped smoking everyday. Occasional smokers who have never smoked everyday are treated as non-smokers. In EPCV2001, no distinction is possible between occasional smokers and non-smokers. Both can be former smokers, if they have smoked every day at some point in their lives. Given that occasional smokers represent only 5% of smokers in ES2003, we choose to keep them. Section 5 explores the sensitivity of the results to this choice.

Figure A6 in Appendix A reports the prevalence of lifetime smoking by year of birth for all individuals born after 1910 in the intermediary sample, obtained by dropping also those observations for which information about smoking careers was missing. The proportion of lifetime smokers is concave in the year of birth, with a maximum for cohorts born around 1970. This pattern reflects a mortality bias: lifetime smokers are less likely to be represented in older cohorts, since they tend to die earlier. The mortality bias may have important consequences for the estimation of education and price effects, because the elderly surveyed in 2001 are, on average, less educated and less prone to have started smoking.¹⁴ For this reason, and following other authors (de Walque, 2007), we restrict the sample to individuals aged under 60 at the time of the survey.

We also leave out those individuals who were still studying or under 24 years-old, to avoid measurement errors on education, except those who already reached the “Grandes Ecoles” level. This leaves us with 16,575 individuals, for which information about education, year of birth and smoking career is complete.

The main assumption underlying our identification strategy is that the share of those individuals with the top education level (the control group) remained constant across cohorts. Hence, we have to define the top level, and to choose a cohort-window over which its share remains constant. GM use individuals in *Grandes Ecoles* (GE) as the control group, because its share remained constant for men born between 1946 and 1966. As a consequence, they restrict their attention to this group. The main difference in the current paper is that we have fewer observations. Hence, there are not many individuals with a GE diploma in each cohort, and this would be a control group of very small size. Choosing GE as the control group would also rule out the possibility of studying smoking among women. We therefore use another control group: all individuals with at least five completed years of schooling after the Baccalaureat (BAC+5). Tables A2 and A3 report changes in the distribution of education levels across cohorts for individuals born between 1942 and 1976. It seems that the share of individuals with a BAC+5 degree increased for women for cohorts born after 1960, and for male cohorts born after the end of the 1960s. We checked this observation by running a probit regression of BAC+5 on the year of birth, and testing whether the slope was significantly different from zero, for different cohort-windows, and for men and women separately

(SLOPE TEST). We also ran probit regressions of BAC+5 on all cohort dummies and tested the equality of the coefficients for each pair of cohorts (DUMMIES TEST).¹⁵ It appears that the share of BAC+5 graduates was significantly higher for cohorts born in 1966 and after for men and women. This is not surprising, since these cohorts were affected by the Haby reform, which favoured their access to higher education. Hence, this paper focuses on the 21 cohorts born between 1945 and 1965, the “baby-boomers”.

The p-value of the slope in the SLOPE TEST is 0.059 for women, and 0.365 for men. The low p-value for women is essentially due to the 1963 and 1964 cohorts. Had we focused on a smaller cohort-window, 1945-1963, the p-value in the SLOPE TEST would have been 0.373. For women, the DUMMIES TEST reveals that shares of graduates at BAC+5 are significantly different in 12 pairwise comparisons of cohorts out of 210 (21 cohorts*20/2). In particular, the share of BAC+5 in the cohort born in 1947 is often smaller compared to other cohorts, while it is higher in cohorts born in 1964 and 1965 (which explains the p-value in the SLOPE TEST). For men, there are 37 significant differences out of 210 comparisons, which essentially results from higher shares of BAC+5 in cohorts born in 1953, 1956, 1964 and 1965. These tests reveal that the upper bound might have been 1963 instead of 1965. In Section 5, we explore the robustness of the results to this choice. Note that in all cases the identifying assumption can only hold approximately: had we exhaustive records of education levels per cohorts, all shares would have been significantly different.¹⁶ The choice of the cohort window leaves us with 5,673 women and 4,878 men.

4.2 Outcomes

Starting smoking

We first analyse the decision to start via the age of smoking onset. In EPCV2001, it is directly given by answers to the question “How old were you when you started to smoke daily?”, which was asked to current and former smokers. In ES2003, age of starting for current smokers (occasional or daily) can be constructed by using answers to the question “since how many years do you smoke?”. For former smokers, answers to two questions have to be used: “How long ago did you quit (in months or years)?” and “since how many years did you smoke?”. Given the date of birth, the date of interview, and information about quitting, it is possible to bound the age of onset, within intervals of one to two years, with lower and upper limits that are not necessary integer. We chose to measure age of onset by the lowest integer age in the interval (choosing the highest one does not impact the results).¹⁷ An important feature of the data set is that many individuals have not started smoking at the time of the survey. For them, age of onset i is their age at the time of the

survey, minus one year since they could start smoking before their next birthday. These observations are interpreted as right-censored spells in standard duration analysis.

Individuals are supposed to be at risk of starting to smoke when they are 10-years old. A panel of individual-age observations is then constructed by expanding each individual by the number of years at risk, i.e. for those who smoked at some point in their life: age of onset-10. The dependent variable O_{ia} of specification (15) is then a dummy, which equals 1 only for the last year at risk (the age of onset) of smokers and former smokers, and 0 for other years and for never smokers.

Quitting smoking

To analyse the decision to quit smoking, we use the sub-sample of smokers and former smokers for whom we know the time since the most recent quit ("How long ago did you quit smoking?"). The dependent variable is a dummy variable which indicates, for each year since age of smoking onset, if the individual quit during that year. Smoking duration is computed as the date at the time of the survey minus the date of starting minus, for former smokers only, the time since quitting. It can be bounded within intervals of one to two years. Using information about the birth date and the date of interview, we can then bound age of quitting in intervals of one (for current smokers and most former smokers) to three years (for a few former smokers). As for starting, we chose the lower integer age in the intervals to define age of quitting. We then construct a panel by expanding each individual observation by age of quitting minus age of starting.

In then end, we have a panel, which indicates for each individual i , at each age a , if they were a smoker or not. This panel can be matched with aggregate time-series data on the basis of calendar year.¹⁸

Recall errors

Since the data set has been constructed on the basis of retrospective information about smoking careers, voluntary or involuntary recall errors may threaten the empirical inference. This would be the case in particular, if errors are correlated with the instruments, i.e. if they vary both with cohorts and education. To check whether recall errors are a potential threat, we confront smoking prevalence by gender and education computed from our pseudo-panel for 1980/1981 and 1991/1992, with the prevalence provided by the "Enquête Santé 1980-1981" (ES1981) and "Enquête Santé 1991-1992" (ES1992). Table A4 reports by gender and education level, and for three groups of cohorts (1945-1951, 1952-1958, 1959-1965), the smoking prevalence (in %) in ES1981 and ES1992, with the associated 95% confidence interval. Under each number, it also shows the difference (in percentage points) with the prevalence computed in our pseudo-panel.

The pseudo-panel provides fairly accurate estimates of smoking prevalence in 1991/1992, for both men and women and all cohorts. It can be seen that these fall in general within the 95% confidence interval computed from ES1992. Moreover, the difference between the smoking prevalence computed from the pseudo-panel and the prevalence given by ES1992 does not exhibit systematic patterns of variation with either birth cohort or education level. There are more differences with prevalence computed for 1980/1981. Table A4 reveals that smoking status in 1980/1981 is likely to be under-reported 20 years later, and that underreporting increases with education, especially in younger cohorts (with a difference of about 20 percentage points for men and women born after 1952). This might be explained by stronger anti-smoking norms among the well-educated, as smoking preferences of the latter were more influenced by public information campaigns. We have no a priori predictions about the sign and the magnitude of the bias, but problems with measurement errors will be detected by testing over-identification by the instruments.

4.3 Empirical modelling

Statistical model

Throughout the paper, we use a linear probability model (LPM) to model the decisions to start and quit smoking.¹⁹ Following equation (15), the statistical model that will be estimated by gender is:

$$O_{ia} = \alpha X_{ia} + r \text{Education}_a + \varepsilon_{ia} \quad (16)$$

Where X_{ia} is a set of characteristics at age a , r is the coefficient of interest – the average return of education on smoking -, and Education_i is the numbers of completed years of schooling. It is constructed according to the ISCED equivalence scale in Table A1.

For analysing the starting decision, inference uses all periods over which individuals are at risk of starting smoking. O_{ia} is a dummy which takes the value 0 if the individual has not yet started to smoke at age a , and 1 if he decided to start at a . Individuals who have already started are no longer at risk, and are removed from the sample. This is equivalent to assuming a discrete time hazard model, and working on the hazard of starting smoking during period t . Individuals are supposed to become at risk when they are aged over 10.

In the analysis of the quitting decision, inference uses the sub-sample of smokers and former smokers only. In this sub-sample, each individual observation in the data set has been expanded by the smoking duration. O_{it} equals 1 if quitting takes place at age a (it is then the last observation for an individual), and 0 otherwise.

Control variables

X_{ia} is a set of control variables that includes: a fixed effect for membership of the control group – 1{Education level=BAC+5}; a set of dummies to control for birth cohort effects (set of control $n^{\circ}1$) or, alternatively, a polynomial function of *year of birth* (set of control $n^{\circ}2$); a polynomial function of calendar time; a set of dummies to control for duration dependence. It is possible to include the latter, which is equivalent to controlling for an age effect, because of the panel nature of the data set and, in models for starting, because calendar time effects are controlled by polynomial trends and not a saturated set of dummies. Interactions between calendar time trends and cohort trends are also included.

Duration dependence is an important issue in the literature on smoking careers (Douglas and Hariharan, 1994; Forster and Jones, 2001). Figures A7 and A8 in Appendix display non-parametric estimates of hazards of starting and quitting. Clearly, the more educated (BAC and more”), start less and quit more at any given age or smoking duration, except the well-educated women born between 1945 and 1955, who have higher starting hazards than the less educated ones. The hazard of starting is increasing, peaks between age 18 and 20, and then decreases. It is almost zero after 35. The hazard of quitting is globally increasing but is irregular. This is due to rounding or heaping effects, whereby individuals tend to declare smoking durations that are multiple of 5. An appropriate set of dummies will control for duration dependence in a flexible manner.²⁰

The common trend assumption

Figures A9 and A10 report for men and women differences in propensity to start (“the smoking gap”) and differences in the average number of completed years of schooling (“the education gap”) between the treatment group (BAC+5) and the control group (under BAC+5). The education gap is negative, and decreases in magnitude with cohorts. The smoking gap is also negative for older cohorts – the BAC+5 graduates are more likely to start -, and becomes positive only for cohorts born after 1955/1957 (for men and women). Following Section 3.2.’s insights, the broad picture of a positive relationship between schooling and smoking appears, *i.e.* more schooling for the treated group implies more smoking relatively to the control group. Figures A11 and A12 offer a similar conclusion regarding the decision to quit.

Nonparametric identification in a DiD framework requires however that trends in outcomes be the same before the beginning of the treatment in the control group and the treatment group. This is the common trend assumption (*e.g.*, Blundell and Costa-Dias, 2002). In the context of the current paper, replacing the “time arrow” by a “cohort arrow”, it means that for any cohort c born between 1945 and 1965, the smoking gap must have been constant for all cohorts born before c , *i.e.* for all

cohorts! Since this contradicts the data, we introduce as control variable a linear cohort trend and a linear time trend that are specific to the “control group”.²¹

5 Results

Tables A5 and A6 present the estimates of returns to education on the decision to start and the decision to quit smoking, in male and female cohorts born between 1945 and 1965, using BAC+5 graduates as a control group. In each table, for women and men successively, four columns of OLS and IV results, corresponding to four different specifications, are presented. The OLS results are reported in line 3. The IV estimates appear in line 7. The bottom part (lines 10 and 11) presents two specification tests, so that the reader can assess the robustness of the IV procedure. Line 10 reports the F-statistics associated with the first stage model – the instrumentation of years of schooling on the set of dummies constructed by interacting cohort dummies and a dummy for not being a BAC+5 graduates. The instruments are relevant if the F-statistics is higher than 10. Otherwise, they are said to be weak, and the IV estimates are likely to be strongly biased. Line 11 shows the p-value of a Hansen test of over-identifying restrictions. The instruments can be safely excluded from the smoking-schooling equation if they are orthogonal to the residuals, which is indicated by a p-value higher than 0.10.

In each table, Column 1 (for women) and Column 5 (for men) start by estimating the effect of education with a full set of dummies to control for cohort and calendar time effects (set of control $n^{\circ}1$ as shown in lines 5 and 9). Columns 2 and 6 add a linear cohort trend specific to the BAC+5 graduates, as indicated in lines 4 and 8, in order to adjust for differential underlying trends in smoking behaviours between the treatment and the control group. Columns 3 and 4 for women, and 7 and 8 for men, replicate these estimations, with a more parsimonious specification for the trends (set of control $n^{\circ}2$): the cohort and calendar time dummies are replaced by quadratic trends.

5.1 Main results

Regarding the decision to start, for women, the OLS coefficient associated with years of schooling is 0.006 and not significant, while for men it is -0.051 and significant (Table A5, Columns 1 and 5). These coefficients are to be interpreted as variations in % points in the hazard of starting: an increase of one year in completed years of schooling decreases significantly the hazard of starting for men by 0.051 percentage points. The IV estimates in Columns 1 and 5 are not significant, because the instruments are weak in this specification: the F-statistics are very low (2.21 and 3.98), while the p-values are very high (higher than 0.9). Adding a cohort trend specific to BAC+5 does change neither the OLS nor the IV results (Columns 2 and 6). When a more

Commentaire [a1] : Are you sure about this interpretation of %/hazard ratios? It applies to proportional hazard/exponential models but not to a linear specification like the one you are using?

parsimonious set of controls is used (n^2), the IV procedure produces unbiased results: the F-statistics of the first-stage regression in the bottom of Table A5 are higher than 10, while the p-value of the over-identification test decrease, but are still higher than 0.10. The OLS results are unaffected by changing the specification for the cohort and calendar time effects. In Columns 4 and 8, the IV estimates are negative for both women (-0.178) and men (-0.143), higher in magnitude than the OLS estimates for men, but not significant. For men in particular, the absolute t-statistics, which was about 4.55 in the OLS regressions, falls to 0.96: the IV procedure produces a significant loss of efficiency, which renders the estimates imprecise. This loss of efficiency is higher when the cohort trend specific to BAC+5 is not included, in Columns 3 and 7 (the t-statistics for men is 0.21). In addition, for men, the p-value in Column 7, line 11 falls closer to the rejection threshold of 0.10, so that omitting the cohort trend specific to BAC+5 clearly threatens the inference. The specification in Columns 4 and 8 is therefore our preferred model, in terms of robustness.

Regarding the decision to quit, the comparison of specifications leads to the same conclusion: more robust results are obtained with a parsimonious set of controls (Columns 3 and 4 for women, 7 and 8 for men). Nevertheless, controlling for a cohort trend specific to BAC+5 does not impact either the IV estimates nor the IV specification tests. This is not surprising, since the smoking gap in Figures A11 and A12 remains fairly constant: the common trend assumption is more likely to hold. The OLS estimates of returns to one year more of schooling are similar for men and women, around +0.06 percentage points in the hazard of quitting. But the IV returns, in columns 4 and 8, are strikingly different: there are higher and significant at the level of 5% for women (+0.31), and similar to the OLS returns but not significant for men (+.06 with a t-statistics of 0.64). For women, the effect seems modest, but one should bear in mind that the yearly probability of quitting is also very low (1.40% for women). Increasing the probability of quitting by 0.3 percentage points corresponds to increasing the odds of quitting by about 20%!

Commentaire [a2] : Same comment as above

To sum up the main results, we find evidence of a causal effect of education on smoking for the decision to quit among women but not among men. Regarding whether more educated people are more likely to start smoking, or whether more educated men are more likely to quit smoking, a causal effect may exist but the instruments do not create enough variation to pick it up.

5.2 Sensitivity analysis and marginal effects

We now investigate the sensitivity of the results to the choice of the estimation technique. Econometric studies of smoking careers generally use Split-Population Duration Models (SPDM)

to investigate smoking initiation, and Weibull, Generalised Gamma or Proportional Hazard Cox duration models to analyse smoking duration (Douglas and Hariharan, 1994; Forster and Jones, 2001).

As many individuals never start smoking (around 60%), it has been argued that specific heterogeneity factors differentiate a type of individual who will never start, whatever the environment (social norms, prices etc.), and a type who will start at some point in life. Absent a conceptual definition of types and a structural framework for measuring them, individuals are supposed to fall in latent classes of risk, whose membership is modelled according to a discrete random process. Conditional on being at risk, age of starting follows a log-logistic or a log-normal distribution. This is the SPDM, actually a degenerate case of a more general mixture model of continuous distributions for time-at-risk, wherein the individual random effect would be discretely distributed over several mass points, and one mass point would attract individuals who will never start. The SPDM can capture potential misspecification of the hazard function when a parsimonious data generating process for modelling duration has been assumed, such as the log-normal or log-logistic density functions. Appendix B details more formally this model. In the current paper, we rely on linear probability models (LPM), and duration dependence is modelled in a rather flexible manner, by using a set of dummies for times at risk. Table A7, in Appendix A, compares estimation results from the LPM and the SPDM. For each gender, there are four columns of coefficients. The first column displays estimates from the LPM when education is not instrumented. In the second column, results from the SPDM model are presented. The third column instruments schooling in the LPM, as in Table A5. Last, the fourth column reports estimation results from the SPDM, when endogeneity of schooling is controlled by a control function approach, as recommended by Terza *et al.* (2008). In a first step, the schooling equation is estimated using the covariates and the instruments. Then, the residuals are computed and, in a second-step, they are included in the list of control variables in the estimation of the SPDM model. This is a two stages residual inclusion (2SRI) procedure. As it was not possible to achieve convergence in the maximisation of the SPDM likelihood with the set of covariates used previously, we dropped calendar time variables from the set of control variables in all regressions and used instead, as a convenient proxy measure of calendar time effects, the yearly price of tobacco. Comparing the LPM results in Table A5 (Columns 2, 4, 6 and 8) and Table A7 (Columns 1, 3, 5 and 7) reveals that this does not impact the estimates. To ease comparisons between the LPM and the SPDM in Table A7, the bottom line presents the elasticity of smoking prevalence at age 18 to a one-year increase in schooling level. Given the size of the confidence intervals, results from the SPDM and the LPM are not significantly different. However, the negative, causal, relationship between education and smoking initiation becomes significant for men, when the

SPDM is used instead of the LPM. Figures A13 and A14 present, for women and men, plots of the empirical survival function (in grey) and the survival functions predicted by the LPM (the continuous black line) and the SPDM (the dotted black line). The empirical survival function reaches a limit at 50% for men and 70% for women, corresponding to the proportion of the sample who had never smoked at the time of the surveys. The proportion of eventual starters predicted by the LPM fits well the actual proportion, for men and women. Moreover, the LPM survival curve has the same shape as the empirical survival function, while this is not the case for the survival curve derived from the SPDM: the latter tends to under-predict smoking initiation after age 18. In the end, the LPM has arguably a better fit than the SPDM. On this basis, and as a conservative choice, we can conclude that the causal impact of education on smoking initiation is negative but not significant.

Regarding the decision to quit, Table A8 compares for women and men results from the LPM and a Weibull Duration Model (WDM).²² In the latter, the shape of duration dependence depends on one parameter only. Results from the LPM and WDM are similar for women. But, for men, WDM provides higher, albeit not significant, estimates of the causal returns to education (see the last column of results): one more year of education increases the mean probability of quitting by 0.2 percentage points.²³ Figures A15 and A16 show that the LPM model (the bold black line) fits the empirical survival curve (the grey line) as well as the WDM (the thin black line).

Table A9 presents alternative estimation results, using our preferred specification. Columns 1 and 2, and 5 and 6, replicate the estimates for men and women in a sample from which occasional smokers (as defined in ES2003) have been dropped. The estimates are almost unaffected. Columns 3 and 4, and 7 and 8, show results for starting and quitting, for women and men respectively, but with a smaller cohort window. Cohorts born in 1964 and 1965 were dropped, following our observation in Section 4.1 that the share of BAC+5 graduates may not be constant (especially for women) when these cohorts are included. This does not impact the regression results. Columns 5 and 6 of Table A7 display the results for men, when the control group is defined as in Gurgand and Maurin (2007): the “Grandes Ecoles” graduates born between 1946 and 1964. Here again, the estimates are quite similar to those obtained in Tables A5 and A6, although the returns to schooling are slightly higher for the decision to quit.

6 Anti-smoking policies and the education-smoking gradient

According to the model in Section 2, price policies are likely to have less impact on the more educated, while information policies are likely to have more effect on the more educated. As anti-smoking policies in France are often a mix of information campaigns and tax hikes, the way they

interact with education policies in the production of smoking abstinence, and more generally public health, is ambiguous. We now investigate this issue.

6.1 Anti-smoking policies in France

Selling and even producing tobacco has long been a state-monopoly in France, except between 1717 and 1789 when the monopoly was conceded to the colonial estate "Compagnie des Indes". Until the beginning of the 1970s, tobacco-related illness was not a pressing concern for health authorities in France, albeit the first custom tax on tobacco products was set-up by King Louis XIII in 1629 on the "official" ground that tobacco affected health (Nourrisson, 1999). Since 1674 French governments have considered the monopoly as a secure source of tax revenues and relied on tobacco excise taxes to support the public finances. Even though the first report on nicotine toxicity date back to Dr. Bailly's experiments with dogs in 1693 and that the first anti-smoking league was set up in 1877, laws to ban smoking in public places and pro-smoking advertising, and to set up prevention policies were adopted much later on, in 1976 (Veil's law) and 1991 (Evin's law) (Nourrisson, 1999; Berlivet, 2000).

To examine trends in sales and price over the 20th century, yearly sales data were obtained from the tobacco industry documentation centre (Centre de Documentation et d'Information sur le Tabac). We aggregated cigarette and loose tobacco (for hand-made cigarettes, pipes and chewing) sales, with a conversion rate of 1g per cigarette.²⁴ A relative price index was constructed using INSEE data for the period (1949-2002). Figure A17 shows that yearly sales per capita continuously increased from 1949 until the Veil law, with a parallel drop in prices. Then both prices and aggregate sales did not move for almost 15 years. From the early 1990s, there have been strong price increases. Indeed, Evin's law allowed the Government to remove tobacco prices from the computation of the Consumer Price Index, as used in the process of European Monetary Union. Then, strong tax increases were coupled with even more vigorous information-based anti-smoking campaigns (Berlivet, 2000).

Clearly, as there is no spatial variations in these public policies, the effect of smoking policies cannot be disentangled from the effect of other time-varying factors, especially changes in social norms.

6.2 Interactions between policy and the education-smoking gradient

While the effect of education on smoking (or health) has been widely investigated, there is much less empirical evidence on how education interacts with anti-smoking policies. Farrell and Fuchs (1982) show that, in the U.S., the schooling-smoking correlation became significantly

negative in cohorts born after 1953 only, *i.e.* for individuals who were exposed during their adolescence to explicit information about the health consequences of smoking. They also find that the difference in the probability of smoking between individuals with differing years of schooling does not change between ages 17 and 24. If one is willing to accept that trends in smoking prevalence up to age 17 did not differ according to eventual schooling attainments, this is DiD-style evidence that a third factor drove the negative schooling-smoking gradient among those individuals who went to school at least until age 17. Taken together, their findings imply that this third factor would impact smoking behaviour through interactions with public policies. Then information campaigns would be regressive, in the sense that they are more efficient to curb smoking trends among the more educated, because the latter are more able to understand risks, have lower discount rates and/or higher opportunity costs.

Kenkel's (1991) study on the relationship between health behaviour and health knowledge supports this result. He finds that the negative correlation between health knowledge and smoking increases in magnitude with schooling: health knowledge matters more when education is higher, and information policies are likely to be regressive. He also notes that differences in health knowledge explain no more than 20% of the education-smoking gradient, which suggests that the opportunity cost explanation may play a more important role than suggested in the economics literature. Kenkel and Liu (2007) provide evidence that, in the US, the negative education-smoking gradient appeared in the 1960s, coinciding with the emergence of a positive gradient between schooling and knowledge about smoking risks. But in the 1980s, the latter became flat, as information percolated down, while the education-smoking gradient still persists. One interpretation is that the opportunity costs of smoking now explain most of the gradient (Grossman, 2006).

In the context of the current paper, one way to look at interactions between smoking policies (or correlates such as social norms) and education is to test whether the effect of education on smoking behaviours was different before the anti-smoking laws, in 1976 and 1991, and after. This is done by constructing two dummies, VEIL and EVIN, which equal 1 if calendar year at age a is (on average) higher than 1976 and 1991 respectively. These dummies are then introduced in the regressions, as well as their interactions with the number of completed years of schooling. The latter are instrumented by interactions between cohort dummies, EVIN or VEIL and an indicator for not being a BAC+5 graduate. The explanatory power of the excluded instruments is tested via the Cragg-Donald F statistics (Stock and Yogo, 2001). We also control for policy effects specific to the control group, by interacting VEIL and EVIN with BAC+5. Last, VEIL and EVIN are also interacted with a linear calendar time trend. One may interpret this specification in a Difference-in-Difference-in-Difference (DDD) framework, whereby DiD estimates of the schooling-smoking

relationship inferred from individuals observed before the Veil or Evin law are compared to DiD estimates inferred from individuals observed after the Veil or Evin law.

6.3 Results.

The results are displayed in Table A10. For each sex, and each decision (start or quit), two sets of results are reported. Benchmark estimates from a specification without interactions between VEIL and EVIN and years of schooling are first reported. Then the interaction terms are introduced. OLS estimates reveal that the negative schooling-smoking relationship has been reinforced after 1991 for men, and after 1976 for women (see the coefficients on the interactions with EVIN and VEIL respectively). However, IV estimates do not confirm this fact, except for the decision to quit by women. After 1976, more educated women were more likely to quit than before 1976, and one year of education increases the hazard of quitting by 0.468 percentage points, in addition to the effect of 0.087 percentage points that is observed before 1976. However, one must be cautious about this result as the F-statistics is quite low, around 5.5, which indicates that the IV bias relatively to the OLS bias is around 20% (Stock and Yogo, 2001). Regarding the decision to start by women, the schooling coefficient is significant, but the exogeneity of the instrument set is rejected by Hansen's test (p-value = 0.059). Other specifications including interactions between years of schooling and polynomial time trends were also tested. They produce similar results.

Commentaire [a3] : Same issue

Last, we replaced the time trends, and the VEIL and EVIN dummies, by a single variable, the logarithm of real relative price of cigarettes (presented in Figure A17). Variations in the latter can be interpreted as synthetic measure of variations in the intensity of anti-smoking policies. Their effect on smoking is therefore likely to be composite, reflecting simultaneously a pure price effect, but also the impact of information campaigns; changes in social norms; and greater availability of nicotine replacement therapy. Table A11 shows the results. In general, the coefficient on the price is negative for the decision to start, and positive for the decision to quit. This suggests that anti-smoking policies, or correlates of these policies (i.e. social norms) have had a negative impact on smoking as expected. Regarding the coefficients on schooling and its interaction with price, the IV procedure yields insignificant estimates for men. For women, there is an important change: education has a significant negative impact on their hazard of starting (-0.312 percentage points), as well as education interacted with price (-0.931 percentage points). Women's hazard of quitting is still significantly affected by education – one year of education implies an increase of 0.58 percentage points -, and the interaction between education and price has also a positive effect. The coefficient on price is, perhaps surprisingly, associated with a positive impact on the hazard of starting and a negative impact on the hazard of quitting (+7.55 and -7.95 points of percentage respectively). However, given the magnitude of the interaction effect and that 77.5% of women

have at least 9 years of schooling, the effect of price on smoking is negative for most women, except those with low levels of education (NOQUAL/CEP). This specification was re-estimated with additional controls for calendar time trends (a linear trend, VEIL, EVIN and their interactions with the trend). The results are very similar regarding the effect of schooling and its interaction with price, with even better first-stage statistics.²⁵ The sign of the coefficient on price is reversed in OLS estimates, because the price variable and the calendar time variables are highly collinear – this shows indeed that the identification of price effects by non-linearities with calendar time effects, as in Forster and Jones (2001), is not possible in our data.

Overall, it appears that education has a causal negative effect on smoking among women, as it decreases their hazard of starting and increases their hazard of quitting, especially after the emergence of anti-smoking policies. The empirical evidence presented in this section suggests that anti-smoking policies or correlates of these policies may have reinforced the education gradient in women's smoking. These results are consistent with the nonparametric estimates of the hazard of starting presented in Figure A7, which clearly show that in older cohorts (1945-1955) more educated women were more likely to start smoking, while this was the converse in younger cohorts (1955-1965).

7 Conclusion

In spite of the substantial increase in levels of education that occurred in the last century, the education-smoking gradient persists and is even stronger among younger cohorts (Aliaga, 2001). This paper has proposed a cohort-based strategy to identify the causal effect of education on smoking in France. We find evidence that education has had a causal negative effect on women's smoking, and an indication that education policies and smoking policies (or correlates of smoking policies) are complementary in the production of smoking abstinence. For men, estimates are too imprecise to confirm or reject a causal explanation of the education-smoking link.

Education effects are more significant for women than for men: this may be due to gender differences in the benefits of educational expansion and in the impact of information policies on risk perceptions. Educational expansion was followed by an increase in women's labour market participation between the end of the 60s and the beginning of the 80s: the rate of women's labour market participation increased from 36.1% in 1968 to 44.8% in 1982.²⁶ Over the same period, men's labour market participation remained fairly stable. Hence, in baby-boom cohorts, the increase in opportunity costs of smoking associated to educational expansion was relatively higher for women than for men. The education differential in women smoking was reinforced since the mid-70s (the Veil law) by the promotion of healthy, smoke-free behaviour, and the diffusion of

information regarding the specificity of smoking dangers for women (interactions with oral contraceptive, risks during pregnancy etc. see Amos, 1996): smoking has progressively lost its glamour, and has become a threat both for working woman - it makes look ugly and, as for men, is associated to monetary losses -, and for mothers. Nowadays, well-educated French women have still higher quitting rates during pregnancy, and lower postpartum relapse rates (Lelong *et al.*, 2001).²⁷ Accordingly, we find evidence that the effect of education on women's smoking initiation has become negative over time, in interaction with anti-smoking policies. Last, educational expansion occurred at key moments of the diffusion of cigarette smoking in French women. Available historical data on smoking trends in France reveals that the majority of French men, whatever their social background, used to smoke before World War II (WWII). At that time, tobacco use was regarded as an inappropriate behaviour for women. Hence, smoking was gendered, and women took up cigarette smoking as a widespread habit after WWII only. The initial increase in smoking prevalence, as in all countries, was socially differentiated. Because smoking was seen as an innovative behaviour, it took up first among women with some relative advantage in terms of educational level and upward mobility (Graham, 1996; Pampel, 2003; Hughes, 2003). Accordingly, in women born between 1945 and 1955, starting was positively correlated with education (see Figure A7). Males from these cohorts were already somewhere between the peak stage of the epidemic and the downward phase. In the latter, following the theory of diffusion, more educated individuals are more likely to show the first declines in consumption. Men had already initiated the downward phase of the diffusion process before the emergence of anti-smoking policies in the end of the 70s.²⁸ Hence, men transitions in an out smoking were less likely to be affected differentially (according to the education level) by information campaigns. The trend away from smoking, after the 70s, in educated women is more likely to be a consequence of anti-smoking policies than it is for men. Thanks to these policies, women have covered the four stages of the cigarette smoking epidemic – innovation, adoption, peak, decline – in a few decades, while the same process took almost one century and several generations for French men. To sum-up, quicker changes between cohorts in education-related opportunity costs and in perceptions of smoking risks explain that more significant results are found for women than for men.

Our results however leave open the following question: is the current education-smoking gradient explained mainly by differences in education-related opportunity costs, or by differences in efficiency.²⁹ The opportunity costs argument seems more relevant, since almost all individuals are informed about the dangers of smoking.³⁰ It is also consistent with the evolution of returns to education over the past 30 years (Magnac and Thesmar, 2002). If the social gradient in health behaviour is mainly explained by differences in opportunity costs, it would be interesting to test

whether redistributive and labour market policies have an indirect effect on social heterogeneity in health behaviours by reducing the variation in opportunity costs. This is left for future research.

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Appendix A. Figures and Tables

Table A.1. Definition of qualifications with international equivalents

Variable's name	Educational programmes (French name)	Description with some U.K. and/or U.S. equivalents	ISCED-97 level	Equivalence in formal years of schooling
<i>NOQUAL</i>	No qualifications	No qualifications	0	0
<i>CEP</i>	Certificat d'Etudes Elémentaires (C.E.P.), Diplôme de fin d'étude obligatoire.	No equivalent certification for adult literacy and numeracy	1	5
<i>BEPC</i>	Brevet des collèges (B.E.P.C.), Brevet d'Enseignement Primaire Supérieur, Brevet Elémentaire.	Certification for having completed the first stage of secondary school: <i>cf.</i> grade 9 in the U.S.A., Certificates of Secondary Education grades 1-5 in the U.K....	2A	9
<i>CAP</i>	Certificat d'Aptitude Professionnelle (CAP), Brevet d'Etudes professionnelles.	Vocational Qualifications: <i>cf.</i> GNVQ Foudation and Intermediate levels in U.K.	3C	11
<i>BAC</i>	Baccalauréat (general or technical), Baccalauréat (first part), Certificat de fin d'études secondaires, Brevets professionnels, Brevet supérieur.	National Diplomas which certify High School vocational, professional or general studies: <i>cf.</i> GCE A– and S-level or GNVQ A-level in U.K., High School Diploma in the U.S.A..	3A, 3B, (3C)	12, except 11 for those with only the first part of the baccalaureat and 13 for those with a vocational baccalaureat
<i>BAC+2</i>	Bac+1 and Bac+2	Programmes in 1 or 2 years after the Baccalaureat: <i>cf.</i> Vocational Certificate or Academic Associates's Degree Programme in the U.S.A., Higher National Diploma etc. in U.K..	4A, 4C, 5B, (5A with first two years of university successfully completed).	14
<i>BAC+3</i> <i>BAC+4</i>	Bac+3 and Bac+4	Programmes in 3 or 4 years after the baccalaureat	5A with at least three years at the university completed, first cycle only	16
<i>BAC+5</i>	DEA, DESS, 3ème cycle, “Grandes Ecoles”	Master	5A, second cycle	18
<i>GE</i>	“Grandes Ecoles”	Unknown		18

Figure A1: Distribution of Qualifications by cohorts, females

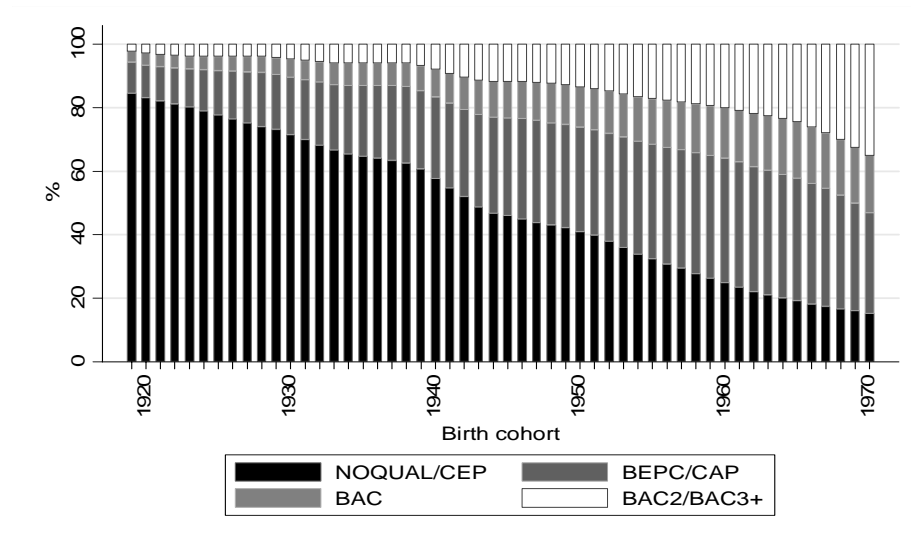
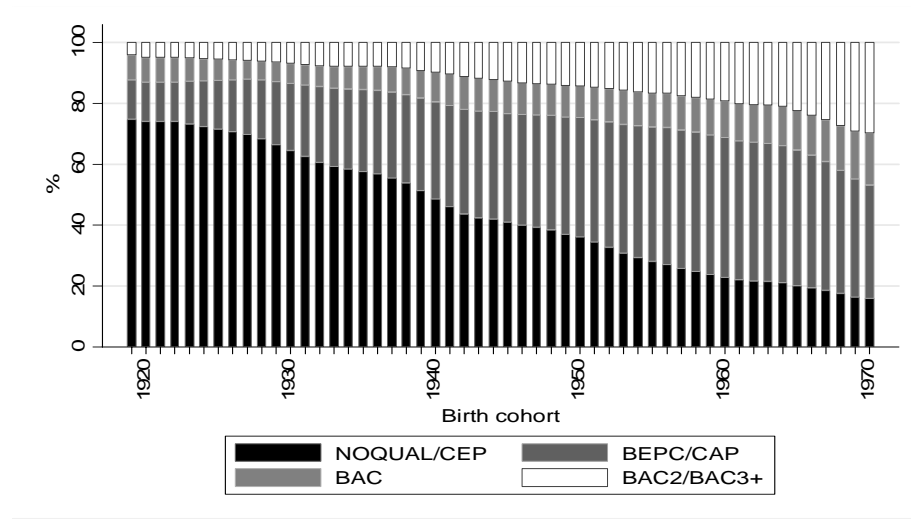


Figure A2: Distribution of Qualifications by cohorts, males



Notes: these distributions of qualifications by gender and cohort were computed using **aggregate results** from five cross-sectional surveys that were nationally representative of the population living in France: the surveys "Formation et Qualification Professionnelle" (1970, 1977, 1985, 1993) and the 1999 National Census.

Figure A3: Distribution of Qualifications by cohorts, females (ES2003+EPCV2001, N=10891).

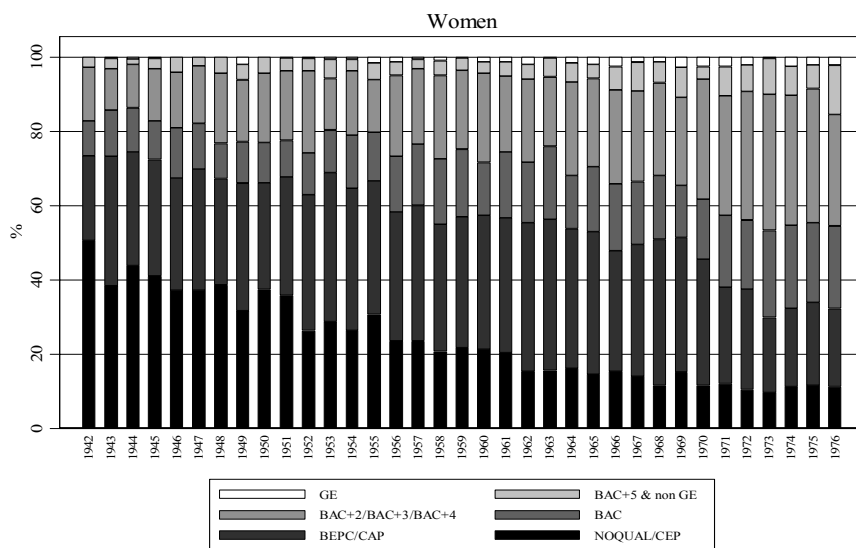
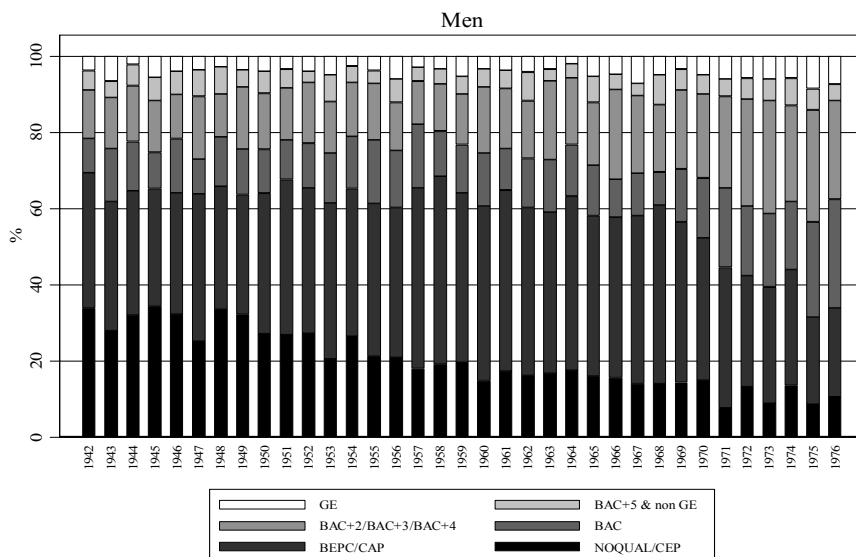


Figure A4: Distribution of Qualifications by cohorts, males (ES2003+EPCV2001, N=9808).



Notes: these distributions of qualifications by gender and cohort were computed using **individual** data from the “Enquête sur les Conditions de Vie des Ménages 2001” (EPCV2001) et “Enquête santé 2002-2003” (ES2003).

Figure A5: Identification.

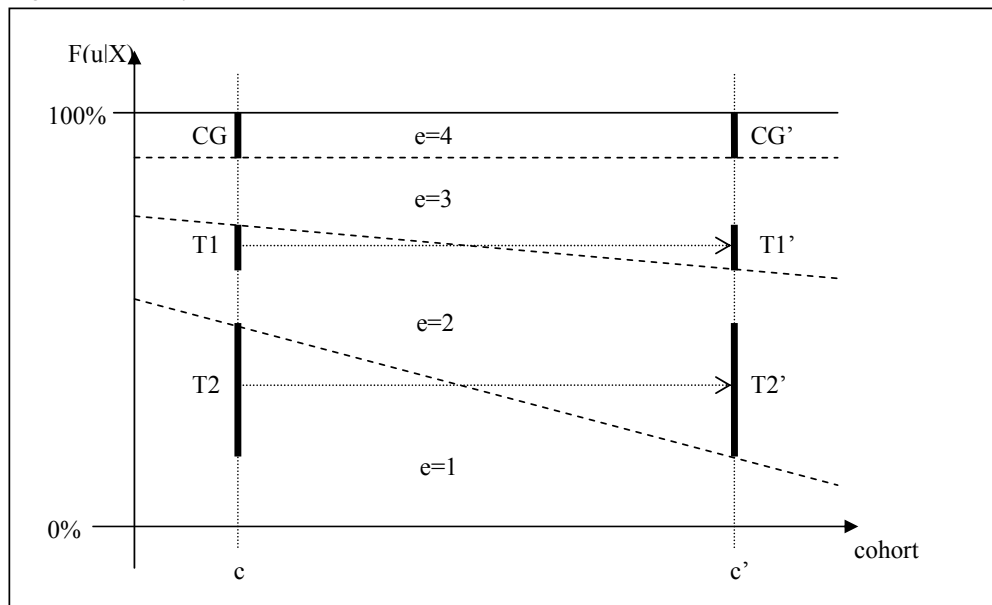


Figure A6. Sample proportion of lifetime smokers by year of birth.

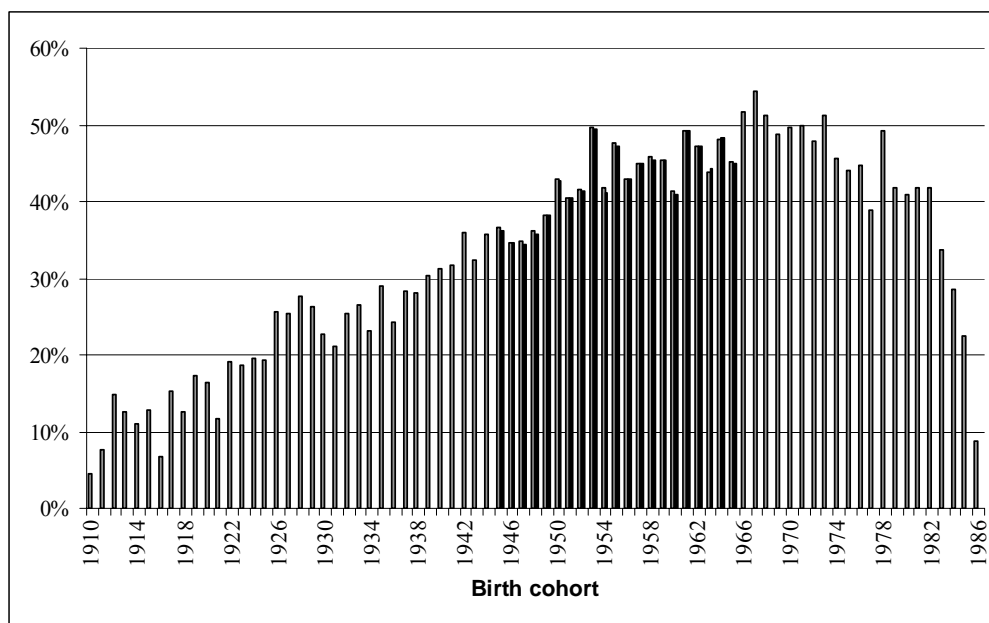


Table A2: Shares of education levels by cohorts – ES2003 + EPCV2001 – females (N=10107)

Cohort	NOQUAL/CEP	BEPC/CAP	BAC	BAC+2 to BAC+4	BAC+5 (inc. GE)	GE only
1944	43.4%	30.7%	16.1%	12.2%	2.0%	0.5%
1948	38.3%	28.7%	17.5%	18.8%	4.3%	0.0%
1952	26.2%	36.7%	21.7%	22.1%	3.7%	0.4%
1956	23.3%	35.1%	22.3%	21.6%	5.1%	1.4%
1960	21.3%	35.8%	25.3%	24.3%	4.4%	1.4%
1964	15.9%	37.5%	28.0%	25.0%	7.3%	1.5%
1968	11.2%	39.3%	29.4%	25.1%	7.3%	1.3%
1972	10.1%	26.9%	37.4%	35.0%	9.4%	2.1%
1976	10.6%	19.7%	39.4%	32.3%	16.2%	2.0%
Average 1942-1976	23.1%	33.0%	25.4%	22.5%	6.1%	1.2%

Table A3: Shares of education levels by cohorts – ES2003 + EPCV2001 – males (N=8985)

Cohort	NOQUAL/CEP	BEPC/CAP	BAC	BAC+2 to BAC+4	BAC+5 (inc. GE)	GE only
1944	32.4%	32.4%	20.1%	14.5%	7.8%	2.2%
1948	33.3%	32.3%	20.1%	11.6%	9.9%	2.7%
1952	27.0%	37.8%	21.6%	16.9%	6.8%	4.0%
1956	20.7%	39.5%	23.0%	12.9%	12.1%	5.9%
1960	14.6%	45.8%	26.3%	17.5%	8.1%	3.2%
1964	17.4%	45.3%	24.5%	17.8%	6.0%	2.0%
1968	13.7%	46.4%	22.2%	18.1%	12.5%	4.8%
1972	12.7%	28.0%	39.2%	28.4%	11.9%	5.6%
1976	10.1%	22.1%	46.7%	27.6%	11.6%	7.0%
Average 1942-1976	20.0%	38.4%	25.0%	17.8%	9.8%	4.4%

Table A4: Comparisons of smoking prevalence - Males

		Women			Men		
	<i>Cohorts</i>	<i>1945-1951</i>	<i>1952-1958</i>	<i>1959-1965</i>	<i>1945-1951</i>	<i>1952-1958</i>	<i>1959-1965</i>
Smoking prevalence in 1980-1981							
NOQUAL/CEP	CI95 in ES1981	17.7%±3.6%	28.7%±4.8%	27.2%±4.2%	57.5%±5.1%	63.3%±5.8%	35.4%±3.9%
	Δ(ES1981-our data)	-1.4%	-4.8%	-8.3%	-6.4%	-13.4%	-3.0%
BEPC/CAP	CI95 in ES1981	22.1%±4.5%	29.4%±4.8%	25.4%±3.8%	52.2%±4.9%	59.1%±4.7%	35.4%±4.2%
	Δ(ES1981-our data)	-5.1%	-2.8%	-7.7%	-4.8%	-8.0%	-6.4%
BAC	CI95 in ES1981	21.4%±8.0%	37.1%±7.8%	28.8%±7.1%	46.2%±10.3%	50.9%±9.3%	32.2%±8.7%
	Δ(ES1981-our data)	1.9%	-11.9%	-15.0%	-1.5%	-12.6%	-9.6%
> BAC	CI95 in ES1981	29.6%±7.1%	30.6%±6.9%	32.3%±17.4%	36.1%±7.4%	43.8%±8.4%	30.4%±20.3%
	Δ(ES1981-our data)	-2.4%	-7.3%	-21.9%	5.5%	-7.8%	-16.9%
Smoking prevalence in 1991-1992							
NOQUAL/CEP	CI95 in ES1992	16.5%±3.4%	28.2%±4.9%	40.9%±5.8%	49.1%±5.1%	59.5%±5.8%	62.6%±6.5%
	Δ(ES1981-our data)	1.5%	2.1%	-4.3%	-1.2%	-6.7%	-6.5%
BEPC/CAP	CI95 in ES1992	17.7%±3.9%	33.4%±4.4%	36.3%±4.4%	41.4%±4.7%	49.0%±4.5%	55.0%±4.4%
	Δ(ES1981-our data)	0.3%	-2.1%	3.5%	-1.2%	-0.8%	-2.9%
BAC	CI95 in ES1992	23.7%±7.9%	28.4±7.3%	29.4%±6.6%	42.7±9.4%	32.5%±8.3%	41.8%±9.4%
	Δ(ES1981-our data)	-0.5%	-1.0%	3.2%	-4.2%	5.2%	1.3%
> BAC	CI95 in ES1992	25.4%±6.1%	29.7%±5.9%	27.3%±5.8%	37.1%±6.7%	35.2%±6.1%	35.5%±7.0%
	Δ(ES1981-our data)	-0.4%	-3.0%	3.3%	-2.1%	-0.7%	-3.1%
BAC+5	CI95 in ES1992	18.2%±11.9%	22.0%±13.2%	24.5%±12.5%	31.0%±10.1%	35.2%±10.0%	26.3%±10.1%
	Δ(ES1981-our data)	4.5%	3.0%	7.2%	2.1%	-5.9%	0.1%

Note: this table should be read as follows: according to ES1981, the smoking prevalence in women born between 1945 and 1951 was 17.7% in 1980-1981, and the corresponding confidence interval at the level of 5% was [17.7%-3.6%,17.7%+3.6%]=[14.1%, 21.3%]. The smoking prevalence for these women in our data set was 1.4 points of percentage lower, hence 17.7%-1.4%=16.3%.

Figure A7. Empirical hazard of starting smoking by gender, education and cohorts.

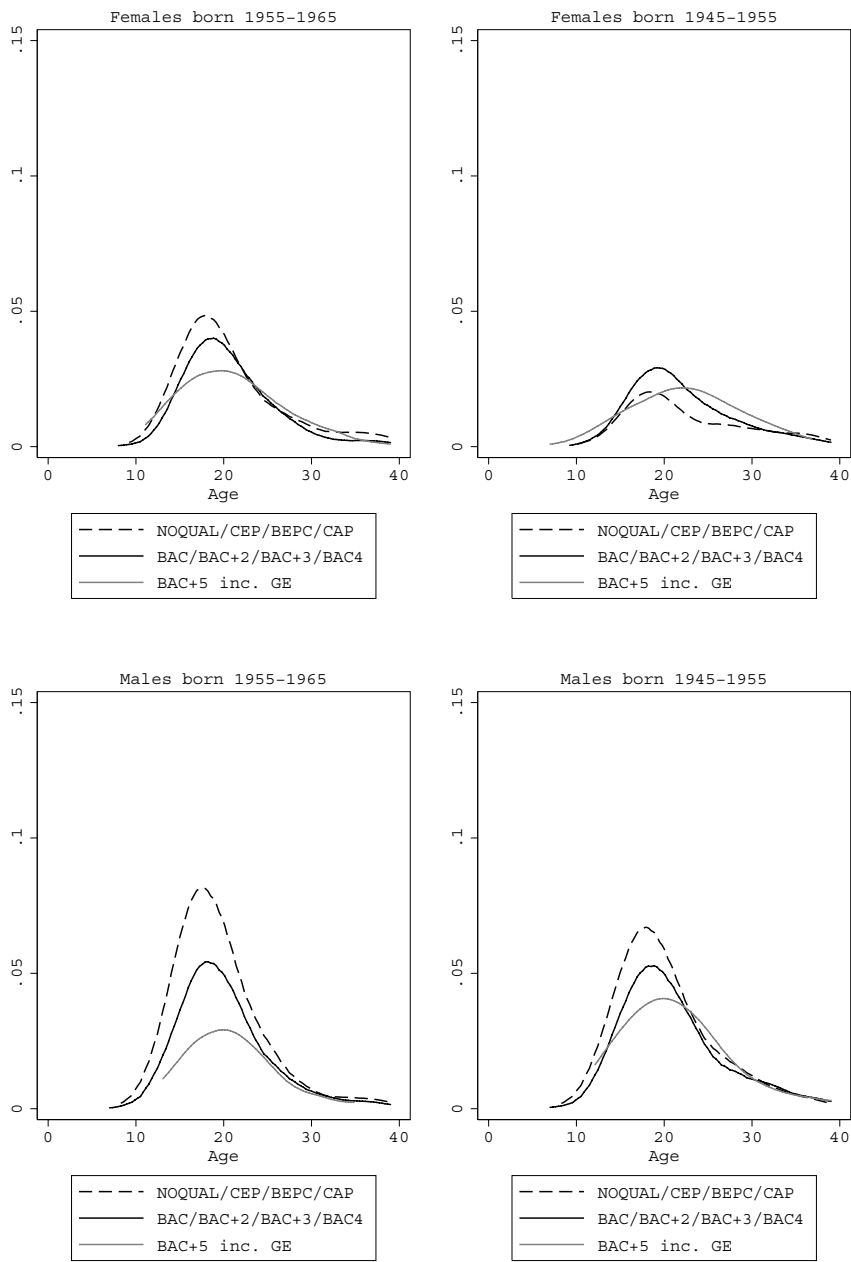


Figure A8. Empirical hazard of quitting smoking by gender, education and cohorts.

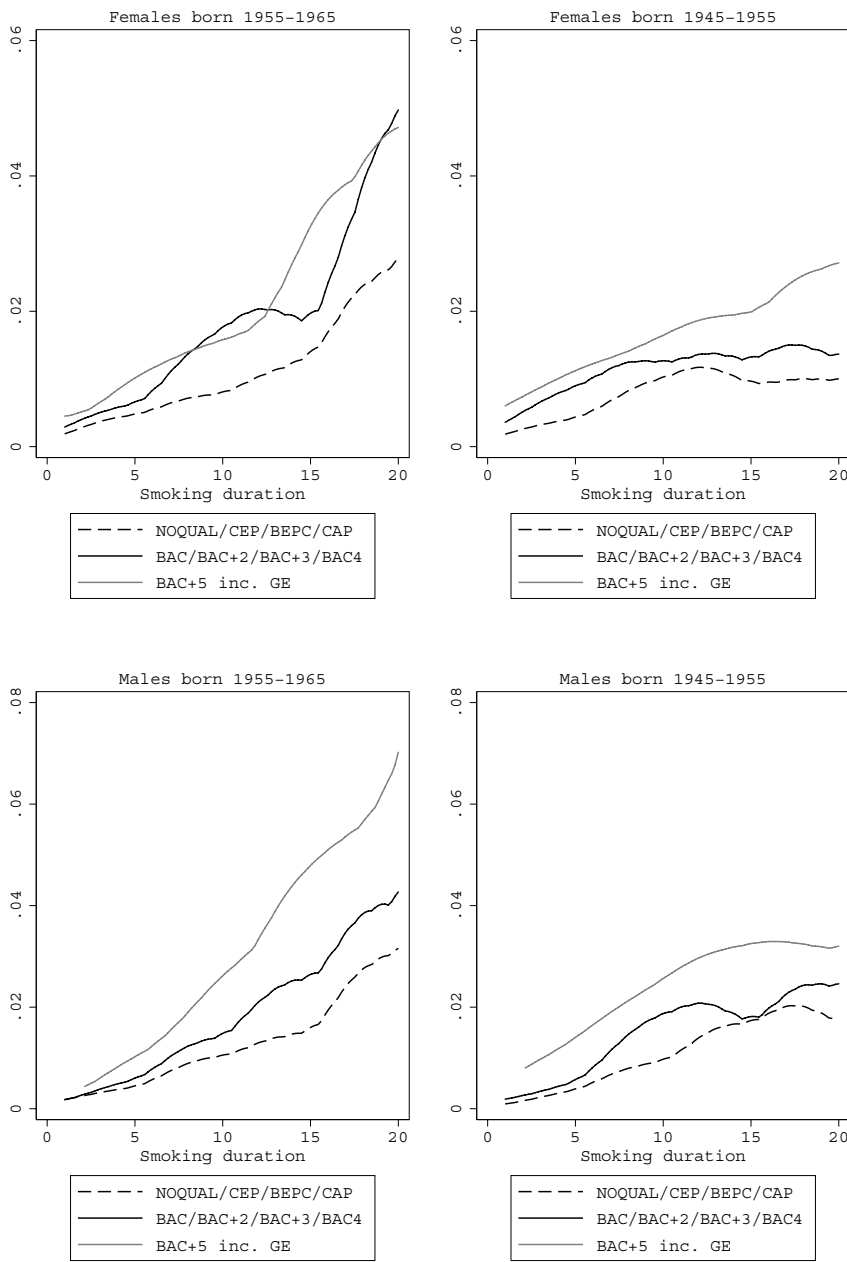


Figure A9. Smoking gap and schooling gap – women - decision to start

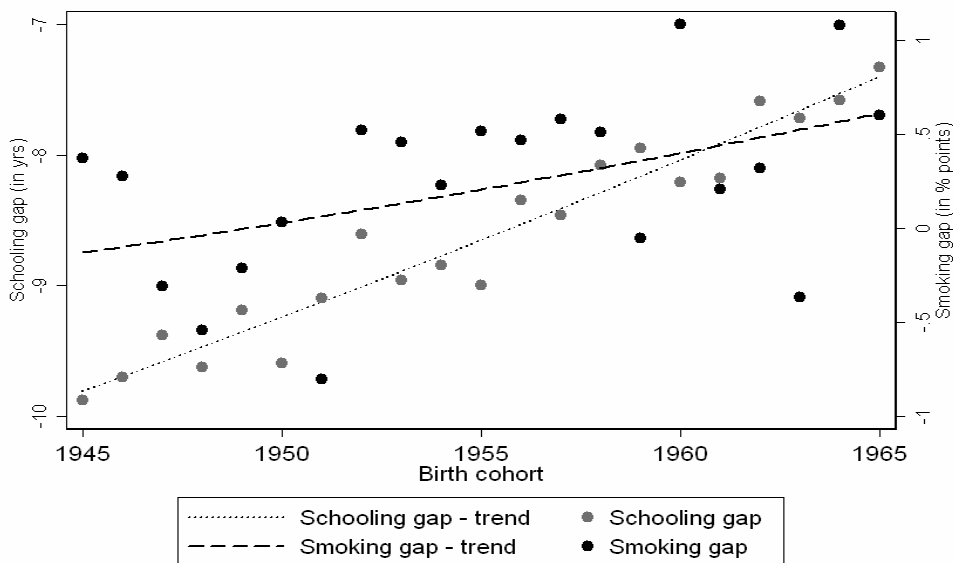


Figure A10. Smoking gap and schooling gap – men - decision to start

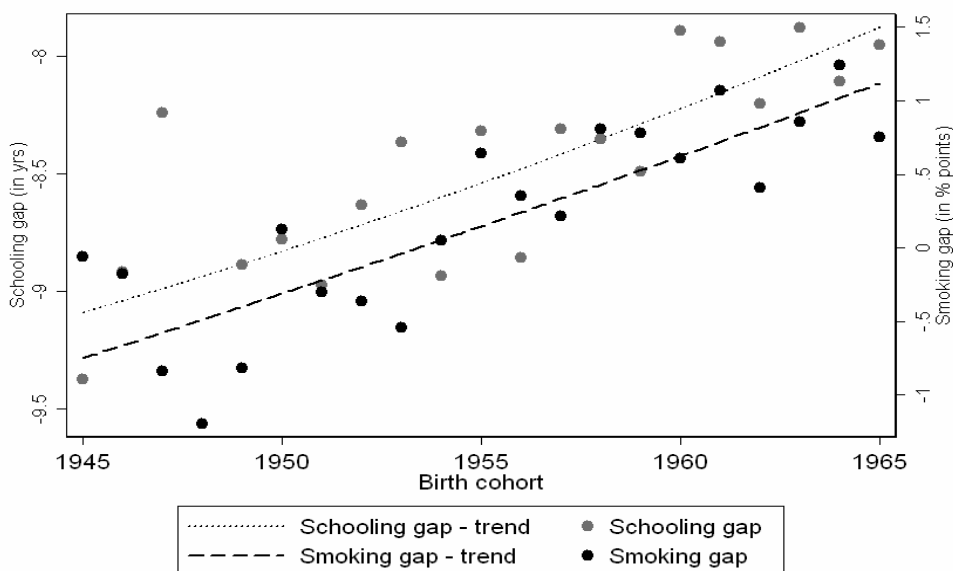


Figure A11. Smoking gap and schooling gap – women - decision to quit

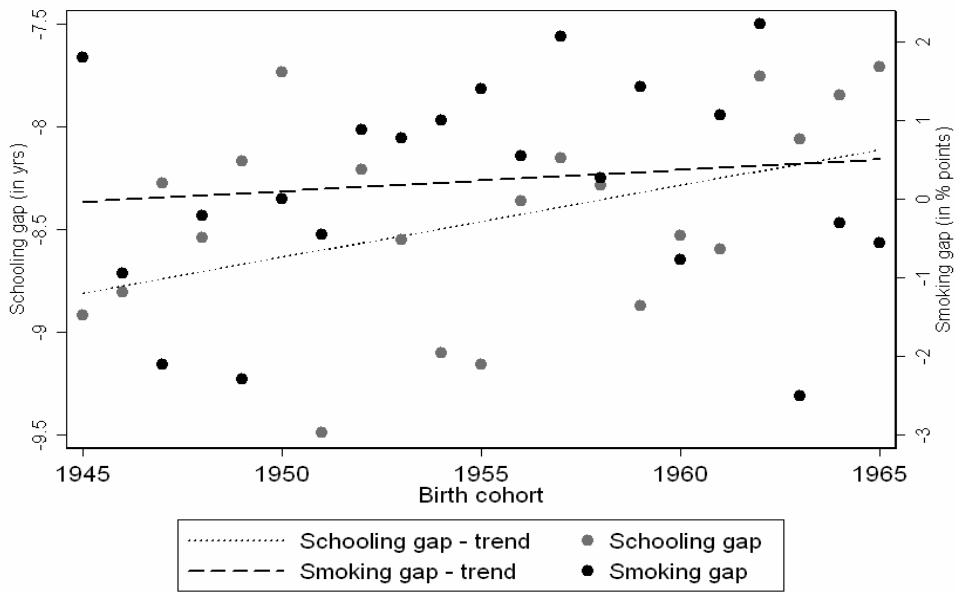


Figure A12. Smoking gap and schooling gap – men - decision to quit

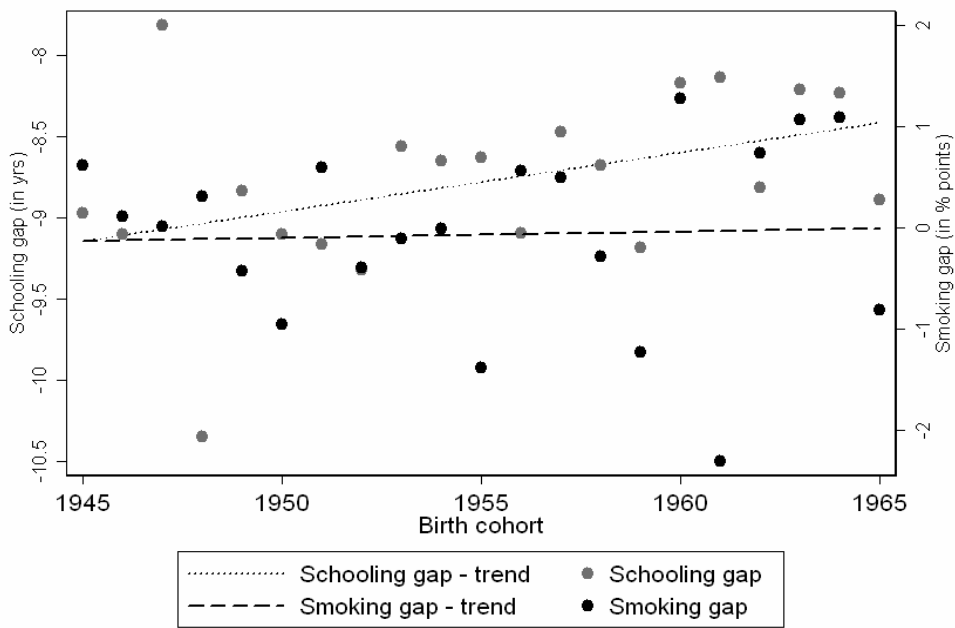


Table A5. Estimation results - cohorts born between 1945 and 1965 – dependent variable = decision to start

Gender Sample characteristics	Women (162063 observation, 5673 individuals)				Men (108367 observations, 4878 individuals)			
	<i>Technique</i>	<i>Ordinary Least Squares</i>						
Years of schooling	0.006 (1.07)	0.006 (1.06)	0.006 (1.02)	0.006 (1.02)	-0.051** (4.53)	-0.051** (4.54)	-0.050** (4.53)	-0.051** (4.58)
Trend: (Year of birth) *Bac5	No	Yes	No	Yes	No	Yes	No	Yes
Other controls	(1)	(1)	(2)	(2)	(1)	(1)	(2)	(2)
<i>Technique</i>	<i>Instrumental Variables</i>							
Years of schooling	0.046 (0.13)	-0.172 (0.26)	-0.133 (1.09)	-0.178 (1.27)	0.007 (0.02)	-0.249 (0.55)	-0.030 (0.21)	-0.143 (0.96)
Trend: (Year of birth) *Bac5	No	Yes	No	Yes	No	Yes	No	Yes
Other controls	(1)	(1)	(2)	(2)	(1)	(1)	(2)	(2)
F-statistics, excluded instruments first stage	2.21	0.65	17.62	13.97	3.98	3.09	29.37	27.92
Hansen p-value	0.92	0.90	0.36	0.33	0.99	0.99	0.12	0.32

Note: The reported effects are changes in the probability of starting in **% points**. Absolute t statistics in parenthesis; ** = significant at the level of 1%; Set of controls (1): cohort fixed effects, a *quadratic* calendar time trend, a linear calendar time trend interacted with cohort fixed effects, a linear calendar time trend interacted with BAC+5, a dummy for Non-BAC+5, duration fixed effects and their interactions with Bac5; Set of controls (2): a quadratic cohort trend, a *quadratic* calendar time trend, a dummy for Non-Bac5, interaction between a linear cohort trend and a linear calendar time trend, duration fixed effects and their interactions with Bac5.

Table A6. Estimation results - cohorts born between 1945 and 1965 – dependent variable = decision to quit

Gender Sample characteristics	Women (42187 observation, 1911 individuals)				Men (62595 observations, 2600 individuals)			
	<i>Technique</i>	<i>Ordinary Least Squares</i>						
Years of schooling	0.059** (4.85)	0.059** (4.85)	0.061** (5.10)	0.061** (5.09)	0.056** (5.44)	0.056** (5.44)	0.056** (5.49)	0.056** (5.49)
Trend: (Year of birth) *Bac5	No	Yes	No	Yes	No	Yes	No	Yes
Other controls	(1)	(1)	(2)	(2)	(1)	(1)	(2)	(2)
<i>Technique</i>	<i>Instrumental Variables</i>							
Years of schooling	-0.621 (1.06)	-0.764 (1.25)	0.318* (2.27)	0.313* (2.24)	-0.010 (0.03)	-0.056 (0.17)	0.065 (0.67)	0.062 (0.64)
Trend: (Year of birth) *Bac5	No	Yes	No	Yes	No	Yes	No	Yes
Other controls	(1)	(1)	(2)	(2)	(1)	(1)	(2)	(2)
F-statistics, excluded instruments first stage	1.04	1.03	17.26	18.10	3.49	3.60	42.01	44.16
Hansen p-value	0.33	0.36	0.32	0.28	0.80	0.81	0.44	0.42

Note: The reported effects are changes in the probability of starting in % **points**. Absolute t statistics in parenthesis; * = significant at the level of 5%, ** = at the level of 1%; Set of controls (1): cohort fixed effects, a *quadratic* calendar time trend, a linear calendar time trend interacted with cohort fixed effects, a linear calendar time trend interacted with BAC+5, a dummy for Non-BAC+5, duration fixed effects and their interactions with Bac5; Set of controls (2): a quadratic cohort trend, a *quadratic* calendar time trend, a linear cohort trend interacted with BAC+5, a dummy for Non-Bac5, interaction between a linear cohort trend and a linear calendar time trend, duration fixed effects and their interactions with Bac5.

Table A7 - Decision to start – Split-population Duration Model vs. Linear Probability Model

Gender Sample characteristics	Women (162063 observation, 5673 individuals)				Men (108367 observations, 4878 individuals)			
	OLS-LPM	SPDM	IV-LPM	2SRI-SPDM	OLS-LPM	SPDM	IV-LPM	2SRI-SPDM
Technique								
SPDM: conditional duration (elasticity of age of onset conditional on being at risk))	–	-0.001 (0.29)	–	0.043 (0.91)	–	0.005** (2.76)	–	0.097** (3.83)
SPDM: probability of being at risk for starting (coefficient in the probit equation)	–	0.013** (2.67)	–	-0.125 (1.36)	–	-0.016** (3.07)	–	-0.028 (0.47)
LPM: change in the probability of starting (in % points)	0.005 (0.94)	–	-0.179 (1.27)	–	-0.052** (4.96)		-0.142 (0.95)	
F-statistics, excluded instruments first stage (LPM)	–	–	13.96	–	–	–	27.93	–
Hansen p-value (LPM)	–	–	0.303	–	–	–	0.278	–
Elasticity of smoking prevalence at age 18 to a one-year increase in schooling	-0.0004 (0.94)	0.0012 (1.20)	-0.0143 (1.27)	-0.0314 (1.17)	-0.0041** (4.96)	-0.0063** (3.75)	-0.0114 (0.95)	-0.0543* (2.51)

Note: Absolute t statistics in parenthesis; ** = significant at the level of 1%, * = at the level of 5%; LPM = “Linear Probability Model”; SPDM = “Split-population duration model”. Set of controls: a quadratic cohort trend, a dummy for Non-Bac+5, a linear cohort trend interacted with Bac+5. The polynomial calendar time trend is replaced by the yearly price of tobacco (in log), as there was too much collinearity to achieve convergence in the estimation of the SPDM model. For the LPM model, the reported elasticity is approximately computed by multiplying the coefficient of years of schooling by eight (the number of period at risk between age 10 and age 18). For the SPDM model, the reported elasticity is computed by averaging the elasticities computed for all individuals in the estimation sample, and the t-statistics is computed using 1000 bootstrap draws of the model’s coefficients in their estimated law (i.e. a multivariate normal).

Figure A13. Predicted and actual survival rates for starting - Women

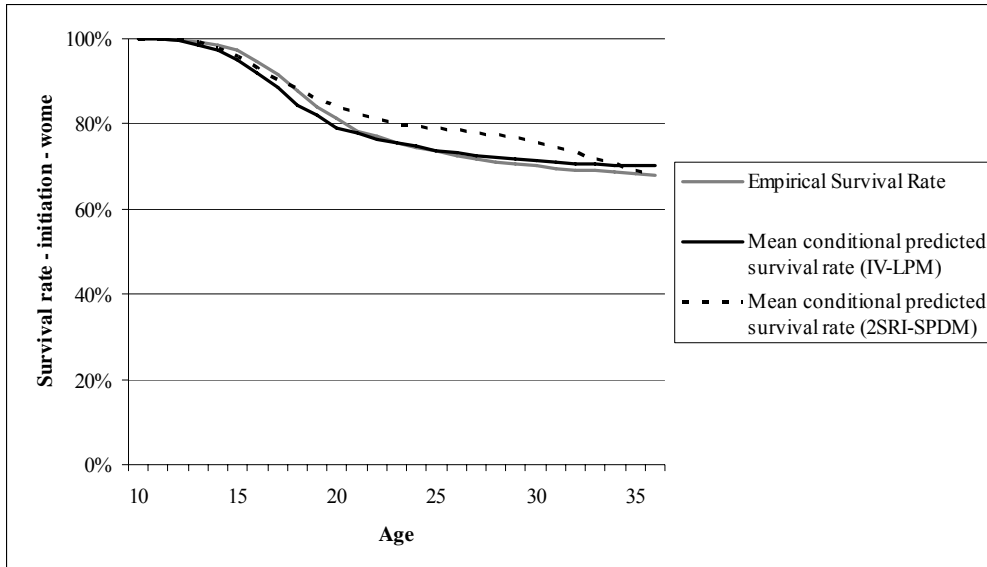


Figure A14. Predicted and actual survival rates for starting - Men

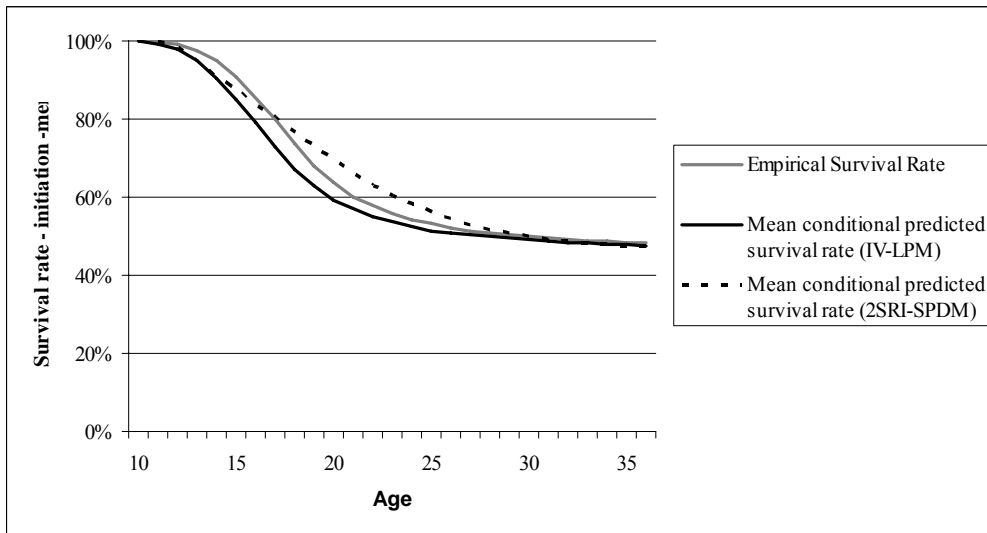


Table A8. Estimation results – Decision to quit – Weibull Duration Model vs. Linear Probability Model

Gender Sample characteristics	Women (42187 observation, 1911 individuals)				Men (62595 observations, 2600 individuals)			
	OLS-LPM	Weibull	IV-LPM	2SRI- Weibull	OLS-LPM	Weibull	IV-LPM	2SRI – Weibull
Technique								
Weibull: elasticity of smoking duration	–	-0.021 (4.32)	–	-0.116** (2.67)	–	-0.017** (5.46)	–	-0.038 (1.70)
Weibull: corresponding average change in the probability of quitting (in % points) LPM: change in the probability of quitting (in % points)	0.061** (5.09)	0.066 (4.10)	0.313* (2.24)	0.421** (2.93)	0.056** (5.49)	0.066** (5.27)	0.062 (0.64)	0.181 (1.31)
F-statistics, excluded instruments first stage (LPM)	–	–	18.10	–	–	–	44.16	–
Hansen p-value (LPM)	–	–	0.279	–	–	–	0.416	–

Note: absolute t statistics in parenthesis; ** = significant at the level of 1%, * = at the level of 5%; LPM = “Linear Probability Model”; 2SRI = “Two-stage residual inclusion”. Set of controls: a quadratic cohort trend, a dummy for Non-Bac+5, a linear cohort trend interacted with Bac+5, a quadratic time-trend, a linear time-trend interacted with Bac+5 (, a linear time-trend interacted with a linear cohort trend. The effect of one more year of schooling on the probability of quitting in the Weibull model is computed by averaging, over all observations, the difference between the probability of survival predicted given the actual schooling level E, and the probability of survival predicted for E+1. The t-statistics in the 2SRI-Weibull specification are computed by bootstrapping over the two steps.

Figure A15. Marginal effect of a one year increase in schooling on the survival rate for smoking duration – treatment group only – Women

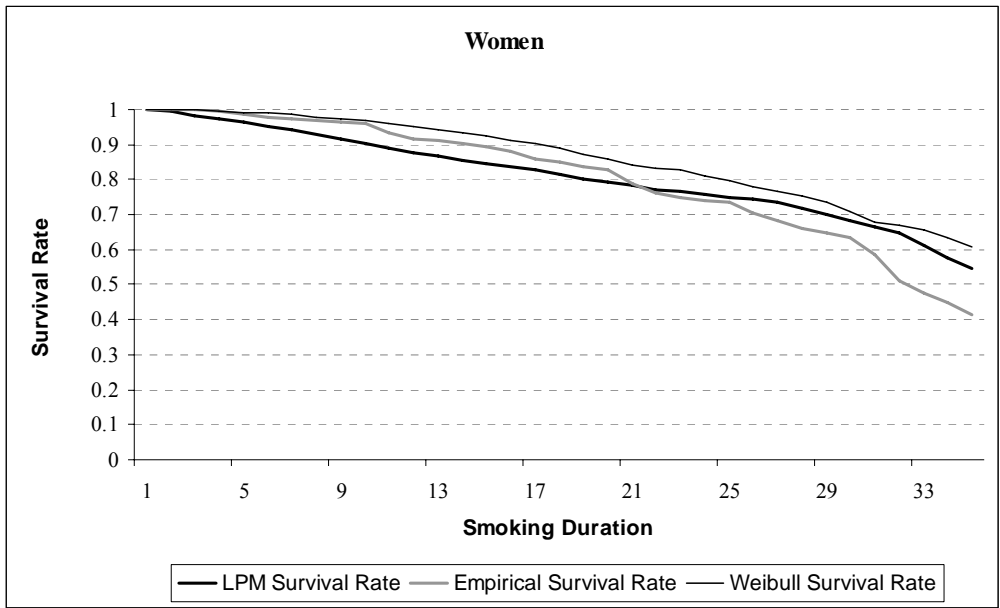


Figure A16. Marginal effect of a one year increase in schooling on the survival rate for smoking duration – treatment group only – Men

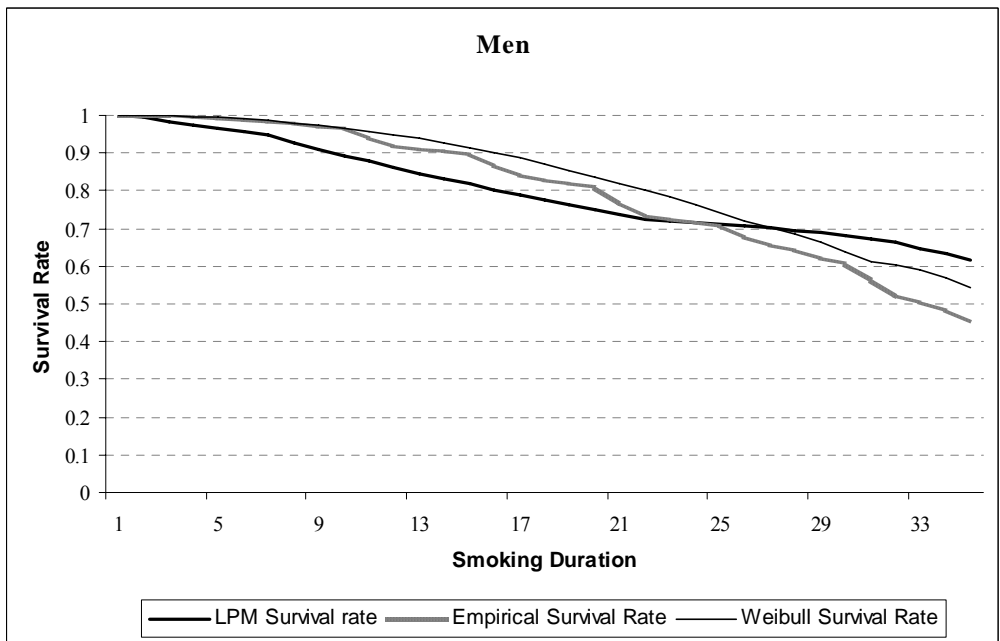


Table A9. Estimation results – alternative specifications

Gender	Women				Men					
	BAC+5				BAC+5				GE	
Control group	BAC+5				BAC+5				GE	
Cohort window	1945-1965		1945-1963		1945-1965		1945-1963		1946-1964	
Other selection rule	No occasional smokers				No occasional smokers					
Decision	Start	Quit	Start	Quit	Start	Quit	Start	Quit	Start	Quit
Number of observations	155119	41377	150651	38134	102036	61322	99974	57771	99796	57765
Number of individuals	5453	1866	5103	1676	4668	2542	4392	2342	4485	2400
<i>Technique</i>	<i>Ordinary Least Squares</i>									
Years of schooling	0.007 (1.27)	0.063** (5.19)	0.008 (1.43)	0.061** (4.97)	-0.055** (4.69)	0.057** (5.48)	-0.045** (4.01)	0.062** (5.84)	-0.057** (5.45)	0.064** (6.34)
Trend: (Year of birth) *Bac5	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	(2)	(2)	(2)	(2)	(2)	(2)	(2)	(2)	(2)	(2)
<i>Technique</i>	<i>Instrumental Variables</i>									
Years of schooling	-0.202 (1.54)	0.287** (2.14)	-0.090 (0.59)	0.308* (2.21)	-0.109 (0.64)	0.049 (0.49)	-0.177 (1.29)	0.096 (0.95)	-0.171 (1.03)	0.111 (1.16)
Trend: (Year of birth) *Bac5	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	(2)	(2)	(2)	(2)	(2)	(2)	(2)	(2)	(2)	(2)
F-statistics, excluded instruments first stage	15.57	20.26	28.79	19.83	22.09	42.24	15.31	46.90	20.79	44.65
Hansen p-value	0.34	0.32	0.42	0.44	0.23	0.40	0.16	0.40	0.13	0.39

Note: The reported effects are changes in the probability of starting in % points. Absolute t statistics in parenthesis; ** = significant at the level of 1%, * = at the level of 5%; Set of controls (1): cohort fixed effects, a *quadratic* calendar time trend, a linear calendar time trend interacted with cohort fixed effects, a linear calendar time trend interacted with BAC+5, a dummy for Non-BAC+5, duration fixed effects and their interactions with Bac5; Set of controls (2): a quadratic cohort trend, a *quadratic* calendar time trend, a dummy for Non-Bac5, interaction between a linear cohort trend and a linear calendar time trend, duration fixed effects and their interactions with Bac5.

Figure A17. Trends in price and sales.

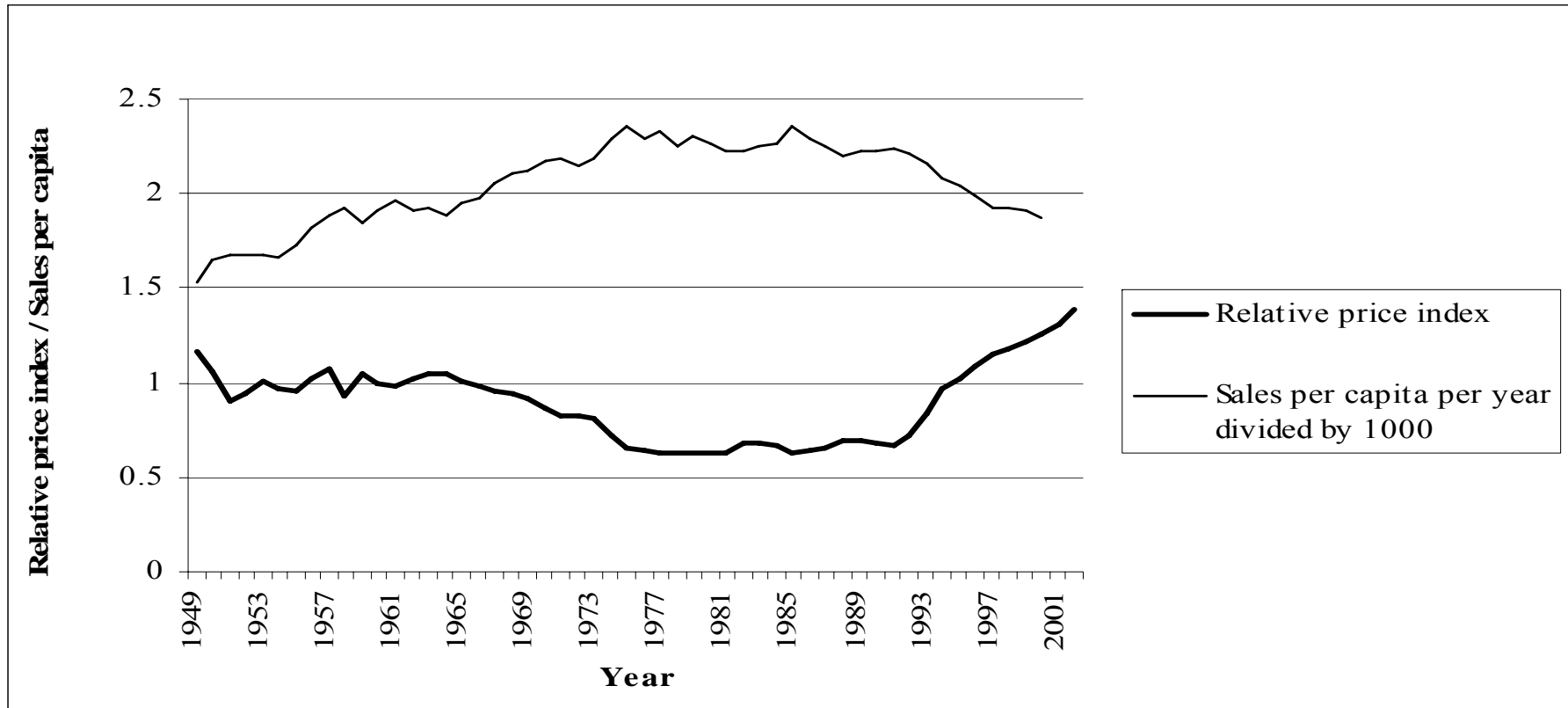


Table 10. Smoking policies and education - cohorts born between 1945 and 1965

Gender	Women				Men			
Decision	Start		Quit		Start		Quit	
Number of observations	162063		42187		108367		62595	
Number of individuals	5673		1911		4878		2600	
<i>Technique</i>	<i>Ordinary Least Squares</i>							
Years of schooling	0.006 (1.02)	0.020*** (2.10)	0.060*** (5.01)	0.034** (2.02)	-0.051*** (4.57)	-0.069*** (3.69)	0.055*** (5.39)	0.027** (2.57)
Years of schooling*VEIL		-0.026* (1.85)		-0.001 (0.07)		0.010 (0.69)		0.039** (2.37)
Years of schooling*EVIN		0.000 (0.01)		0.069** (2.40)		0.055*** (2.59)		-0.006 (0.23)
Other controls	(3)							
<i>Technique</i>	<i>Instrumental Variables</i>							
Years of schooling	-0.177 (1.26)	-0.430*** (2.59)	0.313** (2.23)	0.087 (0.41)	-0.144 (0.97)	-0.324 (1.60)	0.064 (0.67)	0.017 (0.11)
Years of schooling*VEIL		0.455*** (3.40)		0.051 (0.17)		0.326 (1.29)		0.109 (0.50)
Years of schooling*EVIN		0.029 (0.23)		0.468* (1.75)		0.033 (0.13)		-0.123 (0.58)
Other controls	(3)							
Cragg-Donald F-statistics, first stage	13.96	4.842	18.08	5.52	27.93	9.345	44.16	13.54
Hansen p-value	0.30	0.06	0.29	0.82	0.35	0.78	0.45	0.35

Note: The reported effects are changes in the probability of starting in % **points**. Absolute t statistics in parenthesis; * = significant at the level of 10%, ** = at the level of 5%, *** = at the level of 1%; Set of controls (3): VEIL, EVIN, a linear cohort trend interacted with BAC+5 and a quadratic cohort trend; a *linear* calendar time trend, its interaction with a linear cohort trend, its interaction with BAC+5, its interactions with VEIL and EVIN, a dummy for Non-BAC+5; an interaction dummy between BAC+5 and VEIL, an interaction dummy between BAC+5 and EVIN; duration fixed effects and their interactions with Bac5.

Table A11. Smoking policies and education - cohorts born between 1945 and 1965

Gender	Women				Men			
	Start		Quit		Start		Quit	
Number of observations	162063		42187		108367		62595	
Number of individuals	5673		1911		4878		2600	
<i>Technique</i>	<i>Ordinary Least Squares</i>							
Years of schooling	0.005 (0.97)	0.007 (1.56)	0.061*** (5.07)	0.087*** (3.81)	-0.051*** (4.59)	-0.044*** (3.62)	0.060*** (5.01)	0.066*** (3.26)
Years of schooling*Log(PRICE)		-0.013 (0.64)		0.123* (1.91)		0.035 (0.88)		-0.027 (0.47)
Log(PRICE)	-1.079*** (11.74)	-1.196 (5.98)	2.968*** (9.28)	1.800*** (2.77)	-0.518*** (3.13)	-0.859** (1.98)	2.821*** (9.82)	2.576*** (4.53)
Other controls	(4)							
<i>Technique</i>	<i>Instrumental Variables</i>							
Years of schooling	-0.178 (1.27)	-0.312** (2.22)	0.326** (2.33)	0.584*** (3.15)	-0.141 (0.95)	-0.112 (0.51)	0.063 (0.65)	-0.061 (0.43)
Years of schooling*LOG(PRICE)		-0.931*** (3.94)		1.161** (2.05)		0.278 (0.65)		-0.348 (0.71)
LOG(PRICE)	-1.047*** (8.30)	7.558*** (3.45)	3.027*** (10.24)	-7.954 (1.48)	-0.507** (2.22)	-3.204 (0.59)	2.820*** (10.60)	6.014 (1.35)
Other controls	(4)							
Cragg-Donald F-statistics, first stage	13.96	7.08	18.16	8.32	27.92	11.774	44.21	13.94
Hansen p-value	0.30	0.49	0.27	0.14	0.30	0.65	0.57	0.027

Note: The reported effects are changes in the probability of starting in % points. Absolute t statistics in parenthesis; * = significant at the level of 10%, ** = at the level of 5%, *** = at the level of 1%; Set of controls (4): a linear cohort trend interacted with BAC+5 and a quadratic cohort trend; a dummy for Non-BAC+5; duration fixed effects and their interactions with Bac5.

Appendix B. Econometrics of the Split-Population Duration Model

The split-population duration model assumes that only a fraction of the population will eventually start smoking. The probability of being at risk for starting ($R^*=1$) is specified as follows:

$$\begin{cases} \Pr(\text{eventually start smoking}) = \Pr(R^* = 1) = \Phi(Z_i \alpha) \\ \Pr(\text{never start smoking}) = \Pr(R^* = 0) = 1 - \Phi(Z_i \alpha) \end{cases} \quad (\text{B1})$$

where Z_i is a vector of time-invariant covariates, Φ is the standard normal c.d.f. and α is a parameter vector. Conditional on being at risk, the p.d.f. $f(\cdot)$ and the survival function $S(\cdot)$ of the age of starting T are defined as:

$$\begin{cases} f(T | R^* = 1; x(\tau), \tau = 1 \dots T) = S(T | R^* = 1; x(\tau), \tau = 1 \dots T) \frac{\lambda^{1/\gamma} T^{1/\gamma - 1}}{\gamma \{1 + (\lambda T)^{1/\gamma}\}} \\ S(T | R^* = 1; x(\tau), \tau = 1 \dots T) = \prod_{\tau=1}^T \frac{S(\tau | x(\tau))}{S(\tau - 1 | x(\tau))} \end{cases} \quad (\text{B2})$$

$$\text{with } S(\tau_1 | x(\tau_2)) = \frac{1}{\{1 + (\lambda \tau_1)^{1/\gamma}\}} \text{ and } \lambda = \exp(-x(\tau_2) \beta)$$

When γ is lower than 1, the hazard is increasing and then decreasing in τ . We are interested in the identification of the joint distribution of T and C . For lifetime smokers, we necessarily have $R^* = 1$ and $C = 1$. Hence, their contribution to the likelihood is simply:

$$\begin{aligned} \Pr(T, C = 1) &= \Pr(T, C = 1 | R^* = 1) \Pr(R^* = 1) \\ &= f(T | R^* = 1; x(\tau), \tau = 1 \dots T) \Phi(Z \alpha) \end{aligned} \quad (\text{B3})$$

Those who are observed as not starting are either not at risk ($R^* = 0$) or at risk but the risk event did not occur ($R^* = 1$ but $C = 0$). Their contribution to the likelihood is:

$$\begin{aligned} \Pr(T, C = 0) &= \Pr(R^* = 0) \underbrace{\Pr(T, C = 0 | R^* = 0)}_{=1} + \Pr(T, C = 0 | R^* = 1) \\ &= (1 - \Phi(Z \alpha)) + \Phi(Z \alpha) S(T | R^* = 1; x(\tau), \tau = 1 \dots T) \end{aligned} \quad (\text{B4})$$

The distribution of T is then parametrically identified by the maximisation of the log-likelihood function for which individual i 's contribution is:

$$\text{LnL}(T_i, C_i | x_i(\tau), \tau = 1 \dots T_i) = C_i \ln \{ \Pr(T_i, C_i = 1) \} + (1 - C_i) \ln \{ \Pr(T_i, C_i = 0) \} \quad (\text{B5})$$

Since the SPDM is a degenerate mixture model, we estimate it by E-M. algorithms, and compute the variance by the inverse of the hessian matrices.

ENDNOTES

¹ In 1979, cigarette taxes represented 3.08% of the total income of the households in the lowest income decile, against 0.28% for the households in the highest income decile. In 2000, these figures were respectively 5.25% and 0.48% (Godefroy, 2003).

² On the transformation of tobacco use over time, and its medicalisation in the US during the 1960s, see Hughes (2003, chap. 3). One difference between France and the U.S. is that the beginning of anti-smoking campaigns dates back to the end of the 1970s only in France. Actually, from World War II to the 1980s, smoking remained a strong symbol of freedom of choice (especially for women), and a mark of sociability.

³ Productive efficiency plays a minor role in the education-smoking relationship, although one may imagine, for instance, that the more educated smoke less intensively each cigarette, leading them to be less addicted to nicotine and therefore more able to quit.

⁴ As noted by Grossman (2006), the Becker and Mulligan approach to time preferences is still controversial. All the predictions of our simple model indeed hold when education has no causal effect on the discount factor, which is then an unobserved third factor.

⁵ Since there is no saving, the optimal solution can be found by solving the optimisation problem for the second period only. Denote by $V(E)$ the second-period indirect utility function, we have: $V(E) = \text{Max}_{c_2, y_2} \{u(c_2) + y_2\}$ and $w(E) = y_2 + pc_2, c_2 \geq 0, y_2 > 0$

⁶ Using the envelope theorem, the effects of education on the second period well-being is positive, while the price effect is negative: $V_E = w_E \geq 0; V_p = -c_2 \leq 0$

⁷ This was not always the case in earlier studies. For instance, Sander (1995) uses parents' schooling to instrument education in a smoking equation. As pointed out by Kenkel *et al.* (2006), more educated parents may simultaneously transmit stronger anti-smoking norms to their children and invest more in their education.

⁸ de Walque (2007) also use the risk of being killing in action as an instrument.

⁹ These instruments are also employed by Adams (2002) and Lleras-Muney (2005), who study the impact of education on self-assessed health and adult mortality respectively.

¹⁰ This point appears clearly in the work of Auld and Sidhu (2005). They find that an additional year of schooling may have some positive effect on health only for those individuals at low levels of education and with low cognitive abilities.

¹¹ The minimum school leaving age rose from 14 to 16 and new educational tracks were created for individuals born after 1952. As a consequence, Secondary School and University attendance as well as post-Baccalaureat programmes developed vigorously, especially after the Fouchet's reforms in 1963 and 1966, and the Faure's law in 1968.

¹² Indeed, years of schooling *per se* are not an appropriate indicator of health knowledge. As stated by Chevalier *et al.* (2004), "knowledge may [...] come in indivisible "lumps" and it make sense for these to be associated with credentials".

¹³ Pierre Bourdieu has largely illustrated this point in his work on social reproduction in France (see, in particular, Bourdieu, 1979).

¹⁴ A well-known epidemiological study of male British doctors monitored over 50 years shows that survival probabilities of smokers and non-smokers begin to diverge around age 50 (Doll *et al.*, 2004). Such selection bias can go either way, depending on whether smokers' mortality risk increases or not with education. We find no education-smoking gradient in these data for the elderly, which could be interpreted as evidence that the more educated smokers have a lower risk of mortality.

¹⁵ In SLOPE TEST, the equation $BAC+5 = \text{probit}(\alpha * (\text{YEAR OF BIRTH}) + \beta)$ is estimated on the set of observations included in the cohort window, and the hypothesis $H_0: \alpha = 0$ is tested. If it is not rejected, then shares of BAC+5 can be considered as fairly stable over the cohort window under consideration. In DUMMIE TEST, the equation $BAC+5 = \text{probit}(\sum_c \alpha_c I\{\text{YEAR OF BIRTH} = c\} + \beta)$ is estimated on all cohorts, and for all pairs of cohorts (c, c') , the hypothesis $H_0: \alpha_c = \alpha_{c'}$ can be tested. The full results are available upon request from the authors.

¹⁶ This is of course one weakness of this cohort-based strategy: even if there is a numerus clausus for access to the top educational level, the share of a cohort which gets this level change with the size of the cohort.

¹⁷ To illustrate, in ES2003, we observe for instance a smoker interviewed the 15th of January 2003, who smokes since 23 years, and is born the 8th of December 1965. Given that smoking duration is provided with an uncertainty of ± 6 months, he started between the 15/06/1979 and the 15/06/1980, hence between age 14.58 and age 15.42. In this case age of onset smoking=15.

¹⁸ For instance, if an individual is observed at risk for starting at age 18, and is born May 1963, then the calendar year with which observation at age 18 is matched is May 1981, i.e. $1981+5/12$. Consequently, if the data set is matched with *yearly* price series, the price observed for this observation will be $8/12$ of the 1981 price plus $4/12$ of the 1982 price.

¹⁹ Recent works on the education-smoking relationship also use OLS to analyse smoking status (Grimard and Parent, 2007; de Walque, 2007).

²⁰ In the model for starting, we introduce 16 dummies for each year from 10-years old to 25-years old, all longer duration being in the reference group. In our analysis of quitting, we introduce 8 dummies only. Given that they are heaping and rounding effects in the self-reporting of smoking duration, the dummies indicate whether the duration is in one of the following intervals $[0,3]$, $[4,7]$, $[8,12]$, $[13,17]$, $[18,22]$, $[23,27]$, $[28,32]$, $[33,100]$. Allowing for one dummy per year has almost no impact on the results.

²¹ One may argue that identification is therefore nonparametric, but this is small price to pay for identification, and controlling for differential trends between the control and the treatment groups is quite common, at least in studies using cohort-based identification strategies (see, *inter alia*, Grimard and Parent, 2007; de Walque, 2007).

²² Allowing for more flexibility by the use of a semi-parametric Cox model does not affect significantly the results, although the fit is slightly better. Note also that, for consistency between the first and second steps of the estimation procedure, schooling is instrumented on the covariates, the instruments, and the logarithm of time instead of a set of dummies for each period-at-risk, to mimic the pattern of duration dependence in the Weibull model.

²³ Alternative results were also obtained using a logit model instead of the linear probability model, and a logit model with individual random effects, as omission of the latter may generate a bias if duration dependence and education are correlated. The results do not qualitatively change. For women, for instance, when education is instrumented, the LPM yields an estimate of the (average) hazard ratio of 1.207, as against 1.284 and 1.286 respectively in the logit specifications without and with fixed effects.

²⁴ To obtain sales per capita, sales were divided by INSEE yearly figures for the total population aged over 15 in France.

²⁵ For women, regarding the decision to quit, the coefficient on education is 0.510 (2.73) and 1.000 (1.73) for the interaction with price, with a F-statistic of 7.98 (about 15% of relative bias) and a p-value of 0.33 for the Hansen test. The corresponding number for women's decision to start are respectively -0.296 (2.02) for education, -0.862 (3.16) for the interaction effect, 7.086 for F-statistics and 0.49 for the p-value.

²⁶ Source: INSEE, Enquêtes Emploi.

²⁷ Berlivet (2000) for France, Hughes (2003) for the U.S. and Berridge and Loughlin (2005) for the U.K. document the use of mass advertising by the governments in their anti-smoking policies, and in particular the focus on women, especially pregnant women.

²⁸ It is actually because many educated men were already convinced that smoking is dangerous for health that anti-smoking policies were implemented (Berlivet, 2000, on the birth of the Veil law).

²⁹ In France, recent population surveys show that almost 90% of the population and 85% of smokers recognize that smoking is addictive and harmful (HCSP, 1998). Less educated individuals understand the major dangers from smoking, so that the scope for diffusion of information about the health consequences of smoking is now limited (Viscusi, 1992). However, knowledge about the health risks of smoking is often incomplete. The less educated may not have the general knowledge that would allow them to fully comprehend some specific components of this risk, such as everyday disabilities resulting from a chronic obstructive pulmonary disease (Kenkel, 1991; Sloan *et al.*, 2003).

³⁰ Along this line of argument, Fuchs (2004) notes that "To explain the education-health connection, some researchers have proposed that those with more schooling are quicker to act on new health information or take advantage of improvements in medical technology. This seems reasonable, but is it important? The persistence of the negative gradient between education and cigarette smoking many decades after information about the harmful effects of smoking became widespread raises questions about the robustness of this explanation."

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