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Essays on Social Interactions and Policy Evaluation

Dissertation

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Abstract

In the first part, I explore the start of daily smoking, which is often after the re-sorting of students between elementary and secondary education. I employ a novel identification strategy based on this re-sorting, in order to estimate peer effects in youth smoking. The reflection problem is addressed by peers' pre-secondary-school smoking, which is not influenced by the current interaction. The self-selection is minimized by one's own pre-secondary school behavior and the pre-existing smoking prevalence among older schoolmates. The empirical findings from the Czech Republic, where the prevalence of youth smoking has recently reached high levels, suggest that male youth smoking is affected by classmates, while female smoking is not.

In the second part, I estimate the effect of opposite-gender peer drinking on individual risky sexual behavior among Czech youth. The identification strategy relies on two main controls for individual and group-specific unobservables. First, younger schoolmates' sexual behavior is a control for school-specific attitudes toward sexual behavior. Second, pre-determined individual pre-secondary-school alcohol consumption is used to control for self-selection into schools of individuals with specific attitudes toward alcohol. As opposed to Waddell (2010), I find that female drinking affects the male propensity to have unprotected sex, while male drinking does not have such an effect on female behavior. This finding corresponds to the fact that females have usually older sexual partners than males.

In the last chapter, I investigate the impact of a change in the Czech early retirement scheme on the labor force participation of older male workers. Using the difference-in-differences method I find that a reduction in early retirement benefits by 2–3% leads to approximately the same decrease in the probability of being inactive. The finding implies high elasticity of older male workers' participation rate. The public policy implication is that a reduction in early retirement benefits can serve as a very effective tool to increase the participation of older men in the Czech labor market.

Abstrakt

V první části této disertace navrhuji novou identifikační strategii, která pomáhá odhadnou vliv vrstevníků na kouření mládeže. Tato strategie využívá institucionálního uspořádání českého středoškolského systému a informací o věku, kdy jednotlivec začal kouřit denně. V případě institucionálního uspořádání využívám k identifikaci přechodu studentů mezi základní a střední školou a tím způsobenou obměnu spolužáků. Výsledky ukazují, že spolužáci ovlivňují denní kouření u studentů na českých středních školách. Na základě současné literatury jsem testoval vliv vrstevníků u obou pohlaví zvlášť a našel jsem signifikantní vliv spolužáků u mužů a nikoliv u žen. Toto zjištění je v souladu se současnou literaturou, která ukazuje, že muži adolescenti se více angažují v organizovaných mimoškolních aktivitách.

V druhé části odhaduji vliv pití alkoholu vrstevníků opačného pohlaví na riskantní sexuální chování mládeže. Identifikační strategie je postavena na dvou hlavních kontrolních proměnných. První proměnná, která kontroluje pro nepozorované charakteristiky pro danou školu, je sexuální chování mladších spolužáků. Druhá důležitá proměnná je pití alkoholu jednotlivců před nástupem na střední školu, což kontroluje pro selekci jednotlivců s různým přístupem k alkoholu do různých škol. Na rozdíl od Waddella (2010) zjišťuji, že pití žen na středních školách ovlivňuje pravděpodobnost, že jejich spolužáci muži budou mít nechráněný sex, kdežto opačně tento vztah neplatí. Toto zjištění koresponduje s tím, že ženy mají obvykle starší sexuální partnery než jejich muži vrstevníci.

V třetí kapitole zkoumám dopad změny v systému předčasných důchodů v České republice na participaci starších mužů na trhu práce. Pomocí metody rozdílu v rozdílech bylo zjištěno, že redukce předčasných důchodů o 2-3% vede k podobně velkému poklesu pravděpodobnosti neaktivity na trhu práce. Tento nález implikuje vysokou elasticitu nabídky práce starších pracovníků. Hlavní závěr pro veřejnou politiku je, že redukce předčasných důchodů je efektivní nástroj pro zvyšování participaci starších mužů na trhu práce.

Introduction

This thesis consists of three essays. In the first two, I investigate the peer effects in smoking and risky sexual behavior among Czech youth. In the third essay, I estimate effects of changes in an early retirement scheme on the labor market participation of older male workers.

Peer effects are important from a public policy perspective because they determine the efficiency of government policies designed to affect youth behavior. If group members affect each other, a policy that attempts to influence their attitude towards health related behavior has two effects: direct and indirect. The direct effect decreases smoking or risky sexual behavior by shifting the norms of young people. The indirect effect decreases risky activities even further by the multiplication of behavior, as individuals are influenced by their peers and follow their activities. Peer effects thus amplify public policy interventions against behavior that has, for example, negative social and individual health consequences.

The estimation of peer effects is methodologically complicated because, as Manski (1995) points out, the observation of similar behavior in a group does not prove the existence of social interactions within the group. The important goal of the first paper, entitled "*Sorting into Secondary Education and Peer Effects in Youth Smoking*", is to propose a strategy which deals with the main identification issues of estimating the effect of peers on smoking uptake, and which is applicable in most countries where students are re-sorted between primary and secondary schools.

The results suggest that peers do affect individual smoking decisions at Czech secondary schools with a significant difference between male and female smoking behavior: Male students are significantly more affected by their peer smoking. These findings are in line with the current literature (e.g. Kremer and Levy, 2008), which finds male students to be more involved in fraternities. Therefore, anti-smoking policies targeted at youth (e.g., smoking bans or information campaigns) can rely on peer effects as a reinforcement mechanism among male students.

In the second paper, entitled "*Don't Drink and… Avoid Risky Sex of Your Peers: The Influence of Alcohol Consumption of Opposite-Gender Peers on Youth Risky Sexual Behavior*", I estimate the effect of opposite gender peer drinking on individual risky sexual behavior among Czech youth. There are two main identification issues with estimating the effect of peer drinking on own sexual behavior: the selection into the peer groups and the omitted variable problem. My identification strategy relies on including two main controls for individual and group specific unobservables. First, younger schoolmates' sexual behavior is a control for school specific attitudes toward sexual behavior. Second, pre-determined individual pre-secondary-school alcohol consumption is used to control for self-selection into schools of individuals with specific attitudes toward alcohol.

My findings suggest that male propensity to have unprotected sex increases with their female peers' drinking, while male drinking does not affect their female classmates' propensity to have unprotected sex. These findings are opposite to the results of Waddell

(2010) for the US, where male drinking is a significant predictor of female sexual behavior. My results are, however, in line with the observation of a higher average age of first sexual partner for females which suggests that females' first sexual partners most likely do not come from their class.

The third paper, which is a joint work with David Kocourek, is named "*The Impact of Early Retirement Incentives on Labor Market Participation: Evidence from a Parametric Change in the Czech Republic*". As policy makers face the commonly known problem of an aging society, the labor supply of older workers becomes more important. The Czech government has reacted to this development and has decreased the incentives to retire early created by the social security system. Our results confirm that the 2–3% cut in early retirement benefits due to the 2001 reform boosted the labor participation of males eligible for early retirement by approximately the same amount. The reform increased the probability of being employed in the three-year period before a worker reaches the statutory standard retirement age. These results show that the elasticity of the extensive margin of labor supply of older Czech workers is relatively high, although we are not able to calculate the exact value because we lack individual data on wages. Nevertheless, the policy change was not purely fiscal improving since some of the affected people did not continue to work, but rather switched to unemployment as a substitute to early retirement.

Sorting into Secondary Education and Peer Effects in Youth Smoking

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Abstract

The start of daily smoking is often after the re-sorting of students between elementary and secondary education. I employ a novel identification strategy based on this re-sorting, in order to estimate peer effects in youth smoking. The reflection problem is addressed by peers' pre-secondary-school smoking, which is not influenced by the current interaction. The self-selection is minimized by one's own pre-secondary school behavior and the pre-existing smoking prevalence among older schoolmates. The empirical findings from the Czech Republic, where the prevalence of youth smoking has recently reached high levels, suggest that male youth smoking is affected by classmates, while female smoking is not.

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1. Introduction

Smoking is a major cause of lung cancer, chronic bronchitis and other diseases (Chaloupka and Warner, 2000). The habit usually starts during secondary-school age, when youth underestimates the health consequences of smoking and the addictive nature of tobacco. The current sociological and economic literature suggests that many youth outcomes can be determined by social interactions (Glynn, 1981). In particular, student smoking can be affected by the smoking of classmates, who can affect the costs of obtaining cigarettes or who provide important information about smoking. Such peer effects can be especially important at a young age. This paper explores the smoking behavior of freshmen at secondary schools approximately seven months after enrollment. Specifically, I test the effect of the new peers on students' daily smoking uptake.

From a public policy perspective the existence of social interactions is important, because they determine the efficiency of government policies designed to affect youth behavior. If group members affect each other, a policy that attempts to influence their attitude towards smoking has two effects – direct and indirect. The direct effect decreases smoking by shifting the norms of smokers. The indirect effect decreases demand even further by the multiplication of behavior as individuals are influenced by their peers. Peer effects thus amplify public policy interventions against smoking.

However, the estimation of peer effects is methodologically complicated, because, as Manski (1995) points out, observed similar behavior in a group does not prove the existence of social interactions within the group. The goal of this paper is to provide an identification strategy that deals with the main identification issues of estimating the effect of peers on smoking uptake, and that is generally applicable in most countries where students are re-sorted between primary and secondary schools.

Manski (1995) defines three possible sources of similar behavior in a group: endogenous, contextual, and correlated effects. The endogenous effect is defined as the effect of peers' behavior on actual individual decisions, while the contextual and correlated effects are confounding factors that can also result in similar behavior within a group, but do not imply the existence of a social multiplier. Specifically, the contextual effect allows for the behavior of a member of a group to be directly influenced not by peers' behavior, but by their characteristics. For example, peers' parents can directly influence individual behavior through restrictions on smoking during a visit at their home. The correlated effect captures other factors that can result in similar behavior and are not related to social interactions with peers. Students may self-select into a group based on similar unobserved preferences toward smoking or their smoking can be affected by an unobserved school-specific anti-smoking policy.

Another important identification issue is the reflection problem (Manski, 1995), which stems from the nature of social interactions – group behavior is always the aggregation of individual behavior and it is difficult to distinguish who influences whom in a peer group. Not addressing the reflection problem causes an upward bias of the estimated peer effects, similarly to the case of self-selection.

I introduce an identification strategy that addresses these key identification issues by using the typical institutional setting of a secondary schooling system combined with information about the initiation of smoking on a daily basis. Specifically, the strategy relies on the re-sorting of peers (classmate groups) between elementary and secondary education in the Czech Republic and on the availability of a survey of smoking behavior of secondary school students.

An important feature of the Czech secondary education system, as in other countries,[1] is the enrollment of students into various secondary schools based on an admission process taking place in the 9th grade. Of course, enrollment is driven by individual choice and by entrance exams organized by schools, but to some extent such enrollment into secondary schools is a natural experiment that assigns students to new peer groups. In this paper, this strategy is applied in the Czech Republic using the ESPAD survey,[2] covers not only information on current and pre-secondary school smoking, but also the behavior of an older cohort at the given secondary school.

To reduce the bias caused by the correlated effect, I use several controls for individual pre-secondary school behavior (first cigarette use, consumption of alcohol and marijuana) to predict current daily smoking. In addition, the smoking experience of third-year schoolmates is used to proxy school specific anti-smoking policies or sentiment.

To alleviate the reflection problem as well as the contextual problem I use pre-secondary school classmates' smoking instead of the current smoking of peers as the key explanatory variable. Pre-secondary school smokers are those peers who affect non-daily smokers, who, in turn, make their decision about taking up daily smoking. Based on the re-

[1] Graph 2 shows evidence that pupils from primary schools tend to go to different types of secondary schools. The PISA 2003 questionnaire asks students in their 9th year what is their intention in applying for the type of secondary school into which they want to be enrolled. The graph shows that the vast majority of primary schools are heterogeneous in terms of preferences of pupils over types of secondary school. The data show no evidence that there would be some primary schools, from which pupils would prefer only one type of secondary school. PISA 2003 also asks first year students at secondary schools whether they study field that they originally wanted and 86% of students have positive answer. This means that the distribution of preferences is approximately in line with the final distribution of pupils across types of secondary schools.

[2] Re-sorting between elementary and secondary school is typical for a majority of European nations, except Great Britain and the Scandinavian countries.

sorting of classmates, one can define who influences whom in a peer group and hence solve the reflection problem. This approach also identifies the effect of other time constant characteristics of peers on individual smoking uptake, i.e. identify the contextual effects.

In what follows, a peer group is defined as a class at a secondary school. The key outcome variable is current daily smoking of an individual student. This approach follows Lundborg (2006), who defines a smoker as an individual reporting smoking daily or almost daily. The data also allow us to distinguish pre-secondary daily smokers from students who start to smoke daily only in secondary school.

The case of the Czech Republic is an interesting one to study. The proportion of 16-year-old high school students reporting daily smoking is 26% and those reporting having smoked more than 40 cigarettes in their life are 40%, which is among the highest rates in Europe (Figure 1). The high proportion of young smokers suggests that tobacco control policy will have to play an important role in public health policies, and it is hoped that this research will be useful in designing them. This research also provides the first evidence of peer effects among youth in secondary schools with early tracking of children. The previous literature has generally examined peer effects among college students (Kremer and Levy, 2008) or in secondary school systems that do not re-sort students into different schools. The stream of literature that examine peer effects among secondary schools student usually identifies peer effects by using within-school variation in peers' smoking and using a traditional instrumental variable approach to address the reflection problem. My approach is in this context a novel one, because it allows for identification of peer effects in the more traditional institutional set-up and with data that cover only one class in each cohort in a secondary school.

The results suggest that peers do affect individuals' daily smoking at Czech secondary schools with a significant difference between males' and females' smoking behavior: male students are significantly more affected by peers' smoking. These findings are in line with the current literature (e.g. Kremer and Levy, 2008), which finds male students to be more involved in fraternities. Therefore, anti-smoking policies targeted at youth (e.g., smoking bans or information campaigns) can rely on peer effects as a reinforcement mechanism among male students.

2. Literature Review and Basic Methodological Issues

The basic econometric specification used for estimating peer effects generally has the following form:

(1) $$\Pr(smoke_{i(g)}) = \alpha_0 + \alpha_1 \overline{peer}_{-i(g)} + \alpha_2 X_{i(g)} + \alpha_3 \overline{X}_{-i(g)} + \varepsilon_{i(g)}$$

where $\Pr(smoke_{i(g)})$ is the probability of an individual *i* in a group *g* to be a daily smoker, $\overline{peer}_{-i(g)}$ is the average daily smoking of his/her peers in the group (after excluding individual i), $X_{i(g)}$ is the vector of an individual's characteristics, $\overline{X}_{-i(g)}$ is the vector of average peers' characteristics, and $\varepsilon_{i(g)}$ is the disturbance.

The three most-often addressed problems encountered when estimating equation (1) are reflection, self-selection, and the omission of an antismoking sentiment. All three bias the estimate of the endogenous effect (α_1) upward. The reflection problem is connected to the problem of reverse causality between $\overline{peer}_{-i(g),t}$ and $\Pr(smoke_{i(g),t})$, because the researcher cannot observe who influences whom in a class or other peer group.

Finding a solution to the reflection problem is difficult. Kremer and Levy (2008) summarize the recent literature on peer effects among college students and suggest that students' outcomes should be regressed on the pre-college outcomes of their peers rather than on contemporaneous peers' behavior. The important point is that current peers could not affect each others' behavior before they were enrolled in college. Using lagged characteristics of peers, however, may not be appropriate when social interactions among peers occurred in the previous period and the lag is in fact chosen arbitrarily. Then, the model is not properly identified (Fletcher, 2009).

Another stream of the literature addresses the reflection problem using an instrumental variable approach (e.g., Powell et al., 2005; Gaviar et al., 2001; Fletcher, 2009). Finding a credible instrument predicting $\overline{peer}_{-i(g)}$ but excluded form the model (1) is difficult if researchers do not provide additional controls for self-selection. Authors typically assume that some of peers' characteristics do not directly affect an individual's decision to take up smoking and use these characteristics as instruments, making the assumption that the contextual effect does not exist (α_3 is equal to zero). The credibility of these excluded instruments can be, of course, questioned. Finally Kremer and Levy (2008) rely on a experiment that randomly assigns college students to their roommates to deal with identification problems.

In this paper, I use an approach similar to that of Kremer and Levy (2008). Although the Czech secondary schooling system does not provide a randomized experiment, it is realistic to assume that the majority of classmates cannot have influenced each other before their enrollment into a secondary school (details about the institutional setting are extensively explained in the next section) Apart from the reflection problem, it is also necessary to address the self-selection into schools and the potential presence of a school specific anti-smoking sentiment.

Self-selection problem arises when a peer group is created based on some common unobserved factors affecting the peers' smoking and the individual's decision to smoke. For example, children with a similar family background that affects their propensity to become a smoker can sort themselves into specific schools. In the context of equation (1) the selection issue is reflected in the correlation of the common part of the error term $\varepsilon_{i(g)}$ with smoking prevalence $\overline{peer}_{-i(g)}$.

The most credible solution to this problem is a direct randomized assignment of individuals into peer groups. Randomized experiments are, however, rarely available to study secondary school students as secondary school systems are inherently based on sorting of students into schools based in part on unobservable characteristics. To overcome this selection problem, many recent studies (e.g., Fletcher, 2009 or Lunborg, 2006) use school fixed effects that control for all unobserved characteristics of a school as well as average unobservables of the school's students. Thus, their estimation uses only within-school variation in peers' behavior, which is claimed to be random. This approach can also be questioned, however, because existing evidence shows that students can be non-randomly assigned into classes based on their abilities and other characteristics. If this is the case, the estimates are again biased upward. On the other hand, it is also known that fixed effect estimation in the presence of sorting can cause a downward bias of the estimates due to negative correlation between unobserved and observed characteristics of students within a school (Bayer and Ross, 2006). Thus the total bias of fixed effect analysis is unknown.

I propose an alternative solution, which directly controls for unobserved school-specific characteristics of students using a natural assumption about the choice of school, namely that applicants derive their expectations about the school's smoking attitude based on the smoking prevalence among current enrolled students. The regression analysis thus employs older students' smoking as a proxy for expectations and preferences of fresh students toward smoking. This approach respects the design of the enrollment process and is suitable for the data that are widely available for European countries.

The next section describes the institutional setting of Czech secondary schools and the identification strategy in detail.

3. Institutional Setting and Identification Strategy

The Czech secondary school system is characterized by early tracking of students (Brunello and Checchi, 2008). Individuals usually attend their neighborhood elementary school and the majority of Czech youth are enrolled into secondary schools based on their choice and an admission exam administered at the age of 15, after completion of the 9th grade at elementary school.

Secondary schools can be divided into three basic types: academic, vocational, and apprenticeship. Academic and vocational schools usually provide four-year secondary programs[3] and students take a school-leaving exam (the so-called 'Maturita') at the end of

[3] Some academic schools also provide an 8 years program, to which students are enrolled after their 5th year at an elementary school. According to the manager of the ESPAD survey in the Czech Republic, it cannot be ruled out that a few classes from the 8 year track schools were included. However, the analysis on the sample not containing the academic schools does not show any main differences in results.

these programs. The 'Maturita' is a prerequisite for tertiary education and obligatory for all students at vocational and academic schools (Jurajda, 2005). Apprenticeship programs do not lead to 'Maturita' and apprentices do not apply to colleges and universities, but usually become blue collar workers[4]. As Munich (2004) points out, apprenticeship programs usually draw pupils from the lower end of the ability distribution.

The main difference between academic and vocational schools is in their curriculum. Academic schools provide a general education that prepares graduates for college and university studies. Vocational schools provide an education focused on various fields: technical, business, pedagogical, and healthcare. Their graduates are expected to be ready to enter the labor market as well as colleges in their particular field.

The majority (approximately 80%) of Czech secondary schools are public and do not charge a tuition fee. All secondary schools typically organize their own written entrance exams, which play a crucial role in the admission process (GPA from primary school is also taken into account). Although information about the admission process is not public, I can employ the following assumptions. First, classes at primary schools are generally heterogeneous in skill distribution, and pupils from one class apply to different secondary schools, which is supported by the PISA 2003 and depicted in graph 2.

. Secondly, families do not move into new neighborhoods based on the quality of secondary schools. These assumptions are supported by the fact that primary schools are usually not directly linked with any particular secondary school, and the mobility of families is generally fairly low.

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[4] Apprenticeship programs correspond to the ISCED 2 level, according to the OECD ranking. Secondary schools with 'Maturita' correspond to the ISCED 3A level.

The admission process has been recently under reform. My data cover the period 1999 – 2003, when the admission process had the following form. Graduates from primary school send an application to two secondary schools that are of interest to them. These secondary schools then select applicants based on results of entrance exams (and previous GPA). If an applicant is not successful in the first round, he/she enters a second round[5].

This mechanism has the following implications for the proposed identification strategy. First, there is a low chance that peers from one class at the secondary school could have interacted with each other before they were enrolled into the school. The second implication is that students can choose their schools based on their observed and unobserved characteristics affecting their smoking.

The first implication helps us to solve the reflection problem employing a similar method to Kremer and Levy (2008): by using predetermined smoking instead of current smoking. Thus, the baseline specification has the following form:

$$\text{(2)} \qquad \Pr(smoke_{i(g),t}) = \alpha_0 + \alpha_1 \overline{peer_{-i(g),t-1}} + \alpha_2 Exp_{i(g),t-1} + \alpha_3 X_{i(g)} + \alpha_4 \overline{X}_{-i(g)} + \varepsilon_{i(g),t}$$

where $\overline{peer_{-i(g),t-1}}$ is the pre-secondary school smoking of peers, $\Pr(smoke_{i(g),t})$ is the probability of an individual i becoming a daily smoker in class g, $Exp_{i(g),t-1}$ is the past experience of individuals with smoking cigarettes and marijuana, drinking beer and drunkenness; the remaining controls are the same as in the previously discussed model (1) and are time invariant.

5 Secondary schools were obliged to leave a certain number of free slots for the second round, and a few schools enrolled students even after the two official rounds were over.

The crucial implication of model (2) is the non-existence of reverse causality between pre-secondary school daily smoking among peers[6] and the probability of becoming a daily smoker.[7] In other words, an increase in daily smoking at secondary school cannot cause previous experience of peers' smoking.

Next, the selection problem needs to be addressed. First, specification (2) already controls for individual pre-secondary school behavioral characteristics (experience with a first cigarette, beer, marijuana and drunkenness), which should diminish selection bias based on pre-secondary school experience with risky behavior. The data also allow to control for family characteristics that do not change over time (education of parents, completeness of family, and the smoking of older siblings). The survey does not sufficiently cover smoking and other risky behavior of parents; I assume that the parental effect is constant over time.

However, self-selection can still bias the results if students sort into schools and classes based on their specific unobserved factors. That is, students may choose the secondary school where peers are similar in some unobserved characteristics that are correlated with the potential start of daily smoking. Then the correlation between the probability of becoming a daily smoker and peers' smoking is spurious. Students just indirectly express their preferences toward smoking by their choice of school.

To overcome this problem, I assume that sorting into secondary schools is time invariant. This assumption is common in the current literature and usually results in a fixed-

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[6] The peers' pre-secondary school daily smoking most likely contains a recall measurement error that can bias the results toward zero. This problem cannot be solved in this paper. A potential solution lies in undertaking a longitudinal survey that tracks students over time.

[7] One can also consider a second option to avoid the reflection problem: the instrumental variable approach. Current daily smoking of peers can be instrumented by the pre-secondary school smoking of peers following the suggestion in Powell et al. (2005). The instrumental variable approach is performed as a robustness check and presented in section 5.

effect analysis (e.g., Lundborg, 2006). Here, however, I modify the fixed effect approach to the available data, which contain not only one class of first-year students, but also one class of third-year students in each school. Importantly, this approach is also directly derived from the decision-making process of applicants about their preferred schools. In particular, the smoking behavior of third-year students is used as a control for the first year students' expectation about their future classmates. The reason is the potentially important role of third-year students' characteristics associated with smoking for the decision of applicants about their secondary schools. To clarify this approach, I consider the following model of smoking decisions and school choices.

In the first stage, individuals gain experience with smoking and related activities (alcohol and marijuana). These experiences are directly included in specification (2) using the vector of pre-secondary school characteristics $Exp_{i(g),t-1}$. In the second stage, students choose a secondary school and go through the admission process. The final stage takes place at the secondary school – the decision to become a daily smoker can depend on actually revealed peers' smoking.

The second stage is crucial for the effects of selection bias. The choice of secondary school can depend on various factors: individual preferences for schools, regional supply of secondary schools, quality of secondary schools, individual budget constraints, admission process, etc. The self-selection causes a bias to the extent that applicants choose a secondary school based on their preferences toward smoking. This can also be expressed as a minimization of the difference (Akerlof, 1997) between individual characteristics related to smoking and expected characteristics of future peers. Applicants might choose a school *S* that satisfies the following condition:

$$\min\{smoke_i - E_i[peers_{-i(g),S}]\}$$

where $smoke_i$ is a probability measure characterizing the propensity of an applicant i to smoke. It includes all observed and unobserved characteristics related to the current and potential future smoking (e.g., attitude toward smoking). $E_i[peers_{-i(g),S}]$ is an individual expectation about future peers' characteristics associated with their smoking.

Therefore, if a student has unobserved positive preferences toward smoking (and is likely to become a daily smoker), he/she would prefer to enroll in a secondary school with peers who have similar characteristics associated with smoking, holding all other school characteristics constant. This implies that individuals who choose a particular secondary school have similar expectations about future peers, which are driven by their current smoking and by common unobserved characteristics related to initiation of smoking in the future. The individuals' expectations about future peers $E_i[peers_{-i(g),S}]$ are unobserved, but a possible source of expectations about future peers could be the behavior of older students at the particular secondary school.[8]

However, the final composition of a class is also influenced by many other factors. In particular, the entrance exams and school policy of assigning students to particular classrooms is out of the control of applicants. Thus, the final composition of peers in a class g (subset of a school *S*) has the following form:

[8] It might be costly or not possible for applicants to search among current peers and at the same time make a correct guess about their future classmates, because they may have different preferences for schools. On the other hand, it is much more efficient to search among already enrolled older students and obtain information about their types.

(3) $$\overline{peer}_{-i(g),S} = \overline{E_i(peer_{-i(g),S})} + \mu_{g,S}$$

where $\mu_{g,S}$ is an unexpected "prediction" of shock that affects the composition of classroom g and $\overline{E_i[peers_{-i(g),S}]}$ is the mean over all students' expectations, which are formed based on the older students' characteristics.

Older schoolmates' lagged smoking approximates expectations about future peers, which is an easily available source of information about future peers. The unexpected shock μ_g remains in the variation of peers' smoking to allow for bias-free estimation of α_1. The estimated coefficient α_2 corresponds to the effect of past individual expectations about peers on the current smoking decision. This can also be interpreted as a neighborhood or a parental effect, because individuals were influenced by them before enrollment into the school. The final model is this:

(4) $$\Pr(smoke_{i(g),S,t}) = \alpha_0 + \alpha_1 peer_{-i(g),S,t-1} + \alpha_2 Old_{S,t-1} + \alpha_3 Exp_{i(g),S,t-1} + \alpha_4 X_{i(g),S,t} + \alpha_5 \overline{X}_{-i(g),S,t} + \varepsilon_{i(g),S,t}$$

where $Old_{S,t-1}$ is older schoolmates' experience with daily smoking (approximated from time *t-1* when applicants made their enrollment decision) at secondary school *S*. The lagged experience with smoking helps not to confound self-selection with current social interactions between older and younger students. The main advantage of this approach is the ability to directly control for unobserved preferences of students in the secondary schools using available information about older schoolmates.

4. Data Description and Overview of Risky Behavior

The data come from the European School Survey of Alcohol and Other Drugs (ESPAD). This survey primarily consists of 16-year-old high school students from 26 European countries who were asked about their tobacco, alcohol, and drug consumption. The survey was performed in four waves: 1995, 1999, 2003 and 2007. The database includes information about current smoking, past smoking and start of daily smoking, consumption of alcohol and marijuana, education of parents, the existence of siblings, use of spare time and the type of school that the student attends, perceived riskiness of smoking, average GPA, measure of self-esteem, and number of family members.

For the purpose of the estimation, I pool data from 1999 and 2003. The sample from 1995 does not contain information about third-year students, which is crucial for my identification strategy, and data from 2007 are not yet available. The sample from 1995 also omits some important characteristics (for example, siblings' smoking) and the average age is different: 15.8 as compared to 16.2 in 1999 and 2003. For these reasons the data from 1995 are not used for this analysis of peer effects.[9] The main quantitative description of smoking behavior is summarized in tables 1-5 (in appendix). The general prevalence of smoking is

[9] A comparison of 1995 and 1999 statistics reveals high growth of smoking prevalence in all three types of secondary schools. The size of the increase is substantial even after controlling for age, which on average increased between 1995 and 1999 by 0.4 years. Female smoking increased more than male smoking and the level of smoking considerably differs across schools. The proportion of smokers stays approximately the same in 1999 and 2003, the proportion of daily smokers slightly decreased, but still remains high. I leave this phenomenon for future research.

quite high. Forty-four percent of the sample report having at least one cigarette during the previous 30 days. Daily smoking is reported by 30% of the sample.

The statistics show a high variation of smoking outcomes across types of schools, but low variation across year of collection and gender. For example, the level of females smoking at academic schools is about 30% of that in apprenticeship schools (tables 1 and 2). This suggests that a different selection mechanism and/or social interactions can exist across types of schools. This is reflected in the estimation by controlling for school-types fixed effects.

Generally, a high prevalence of smoking is accompanied by a high consumption of alcohol: 72% of females and 88% of males consumed beer in the last 30 days. The smoking of marijuana during the last 30 days reached 22% of males and 18% of females in 2003. The consumption of beer and marijuana in my specification is used to control for individual pre-secondary school behavior and preferences toward risky behavior.

Table 3 shows the self-reported start of a daily smoking habit. If a respondent reports having started daily smoking younger than 15, it is assumed to be predetermined smoking that occurred before enrollment into the secondary school. Daily smoking initiation that is reported to have begun at the age of 15 and 16 most likely happens at the time of secondary education. Based on this information the key explanatory variable is created: peers' pre-secondary school daily smoking. This variable is summarized in table 4 and displays how different types of schools draw students with different smoking histories.

Table 5 displays the descriptive statistics of third-year students' past experience with daily smoking. This is the key variable controlling for the selection of first-year students into the schools. As described earlier, past daily smoking of older students may correspond to the formation of the prospective students' expectations. A similar variation to first-year students' smoking can be observed across types of schools and years.

The descriptive statistics of all other variables and characteristics of first-year students are in table 6. The structure of samples from 1999 and 2003 is fairly similar in many aspects, including the number of observations and classes, means of all predetermined individual characteristics (for example, age and education of parents), numbers of students in all three types of secondary schools, and geographical structure. For the purpose of estimation I pool these two samples. The time fixed effects used in estimation capture all unobserved differences between first year students surveyed in 1999 and 2003.

Finally, two different samples are presented in table 6: with and without pre-secondary school daily smokers. In fact, there are no important differences in terms of regional structure and other non-behavioral characteristics between these two samples.

5. Results

The results are presented in tables 7 and 8. They are from linear OLS regressions determining the individual probability of being a current daily smoker in the first year at secondary school, and all standard errors are clustered at the class level. The first presented result is from "naïve" specifications not controlling for the reflection and self-selection problem; subsequently, the reflection and selection biases are accounted for. The sample is divided into two parts: females and males, and show that social interaction has a different strength for each gender. The results in table 7 employ the full sample including those who report pre-secondary school daily smoking; while table 8 presents results with a restricted sample that does not contain pre-secondary school daily smokers.

The first specification (1) in table 7 does not control for any pre-secondary school behavioral characteristics and the key explanatory variable is the peers' current daily smoking (individual smoking is always excluded from the peer variable). The other control variables are time fixed effects, current GPA, participation in sports on a daily basis[10], parental education, older siblings' smoking and school-type fixed effects. The effect of peers' daily smoking on individual daily smoking is significant and positive. The coefficient is bigger for males (0.350) than for females (0.273).

Next, I substitute current smoking with pre-secondary school smoking to alleviate the reflection problem. The cost is the measurement error that can potentially bias the results downward as was discussed above. The estimated peer effects decreased to 0.268 for males and 0.170 for females, respectively.

The next step addresses the selection problem. First, pre-secondary school behavioral characteristics (experience with smoking, marijuana and alcohol) are included in the regression. These controls should also capture the family effect that is directly controlled for only by the education of parents, completeness of family and the smoking of an older sibling.

The results presented in the third row of table 7 show an approximately 40% decrease in the estimate of peer effects: 0.153 as opposed to 0.268 in the previous case for males and insignificant results for females. This suggests that sorting of students into secondary schools based on pre-school experience with cigarettes, drugs and alcohol strongly biases the peer

[10] In this approach *participation in sports on a daily basis* is not influenced by new peers at secondary school and is predetermined by his/her activity at an elementary school. In the Czech Republic, sport clubs are not associated with high schools, and children usually become members at an early age in elementary school and optionally stay there longer during their high school.

effects' estimate. As described above, this does not have to capture all the bias, because classmates may have other common characteristics that could influence the individual smoking decision (contextual effect). Therefore, additional controls for other peers' characteristics (with an individual's own level excluded) are included in specification (4): average level of parental education, family completeness, participation in sports on a daily basis, and siblings' smoking. The estimated effect of peers is lower, but the size of the decrease is smaller than in the previous cases. The peer effects for females remain insignificant.

In the final step, the potential sorting of students into schools based on their unobserved attitude toward smoking is considered using the variable *% third year daily smokers.* It is meant to capture those unobserved factors that are related to the choice of school and smoking decision. The pre-secondary school smoking of peers is the key explanatory variable and it can be observed that, by controlling for older students' smoking, the estimated effect of peers slightly decreases (specification (5) in table 7).

The effect of sorting is estimated to be positive and significant, which suggests that the sorting of students into secondary schools can exist. Intuitively, one can also interpret the impact of older students' past smoking as the influence of other school and neighborhood factors that might affect an individual's decision. The estimated effect of peers, however, does not change significantly. It decreases from 0.126 to 0.114 and standard errors are approximately the same.

A robustness check is done using fixed effect analysis. The caveat of the fixed effect analysis is in the limitation imposed by the data. The ESPAD contains only two classes in different grades (first and third year) for each secondary school. Therefore, I cannot use grade fixed effects, which might be important, because first and third-year students can differ in their unobserved characteristics. The result is found in row 6 of table 7. The estimated peer effects are similar to those that use only first year students. In the last row, I present the result from the sample that excludes all students from academic schools. The rationale for this is that some classes in the sample could be drawn from an eight-year track, which would invalidate the identification strategy dealing with the reflection and selection problem. The size of the estimated peer effects is similar to specification 5, and the standard errors increased from .05 to .06.

Table 8 presents results that use the restricted sample only with students who were not daily smokers before secondary school. By excluding all pre-secondary school daily smokers the self-selection is diminished, because smokers most likely sort into specific schools. The key explanatory variable is again the proportion of pre-secondary school smokers in classes. The analysis therefore estimates how pre-secondary school daily smokers influence those who did not smoke daily before their enrollment. The results presented in table 8 are similar to those estimated by the previous approach presented in table 7.

Another robustness check is the instrumental variable approach that uses the pre-secondary school smoking of peers as an instrument for current peers' smoking (tables 9 and 10). This approach should diminish the reflection problem, similarly to Powell et al. (2005). The first-stage regression suggests that this instrument has a very strong predictive power and is significant at the 1% level. The results for female and male students are the same as those in tables 7 and 8. The estimate of male peer effects together with standard error increases after applying the IV approach; a possible explanation may lie in measurement errors in the pre-secondary school smoking variable, or in the endogeneity of the instrument.

The hypothesis often tested in the literature is whether peers' smoking has a non-linear impact on an individual's decision. In order to test it, I use a similar methodology to that of Clark and Loheac (2007) and create dummy variables for each quartile of peers' pre-secondary daily smoking. The results are presented in table 11. The estimates are not significantly different from zero or from each other. Thus the hypothesis that peer effects are linear cannot be rejected.

The next hypothesis I test whether those students who report trying marijuana before being enrolled in secondary school are more likely to be affected by peers' smoking or not. For that purpose, I create a new variable - the interaction of previous experience with marijuana and peers' pre-secondary daily smoking. Although the estimate is positive and relatively high (0.15) for both females and males, it is not statistically significant from zero at the 10% level.

Comparing the results with the current literature is difficult due to different institutional setting, but I can claim that the results are rather on the lower end for estimated peer effects. For example, Lundborg (2006) estimated that increasing the number of peers' smoking by 25% increases the probability of smoking by 12 percentage points. Similar results to Lundborg's are presented in Powell et al. (2005) and Fletcher (2009). The magnitude of peer effects estimated in this paper is similar to the one in Clark and Loheac (2007), who also use lagged peers' smoking, but without any appropriate experiment that would assign students into new peer groups. They estimate that the impact of an increase in peers' smoking by 25% on individual smoking is 2.2 percentage points, while the result for Czech male youth is approximately 3 percentage points.

6. Conclusion and Policy Implications

In this paper, I use a social interaction framework to determine whether the daily smoking of classmates influences smoking decisions. Several estimation issues are addressed including the endogeneity of school choice, which might be related to the smoking decision.

The main results suggest that smoking decisions are affected by peers' smoking. There are significant endogenous peer effects mainly for male students. This finding has several important implications. Firstly, the decision and enrollment process into secondary schools has not just human capital consequences, but also important health implications. Secondly, public policies that attempt to influence youth smoking in Czech secondary schools can rely on the existence of a social multiplier for male students.

This analysis also has certain limitations. First, the peer group is arbitrarily defined as a class, which may be too narrow. For example, female students might spend time with mates not enrolled in the school. A social multiplier for females thus might exist, but not within a class. Second, the analysis omits several characteristics that might play an important role, namely an individual budget constraint. Although these variables would improve the analysis, they would most likely not change the estimated difference between males and females.

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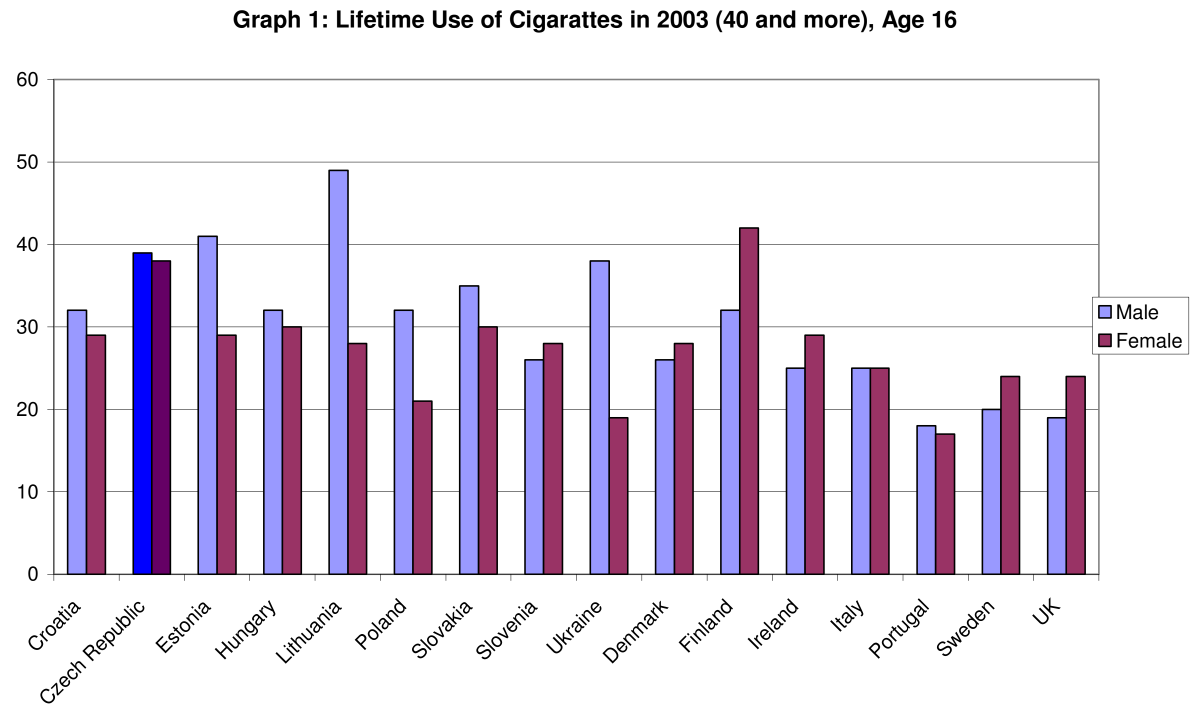
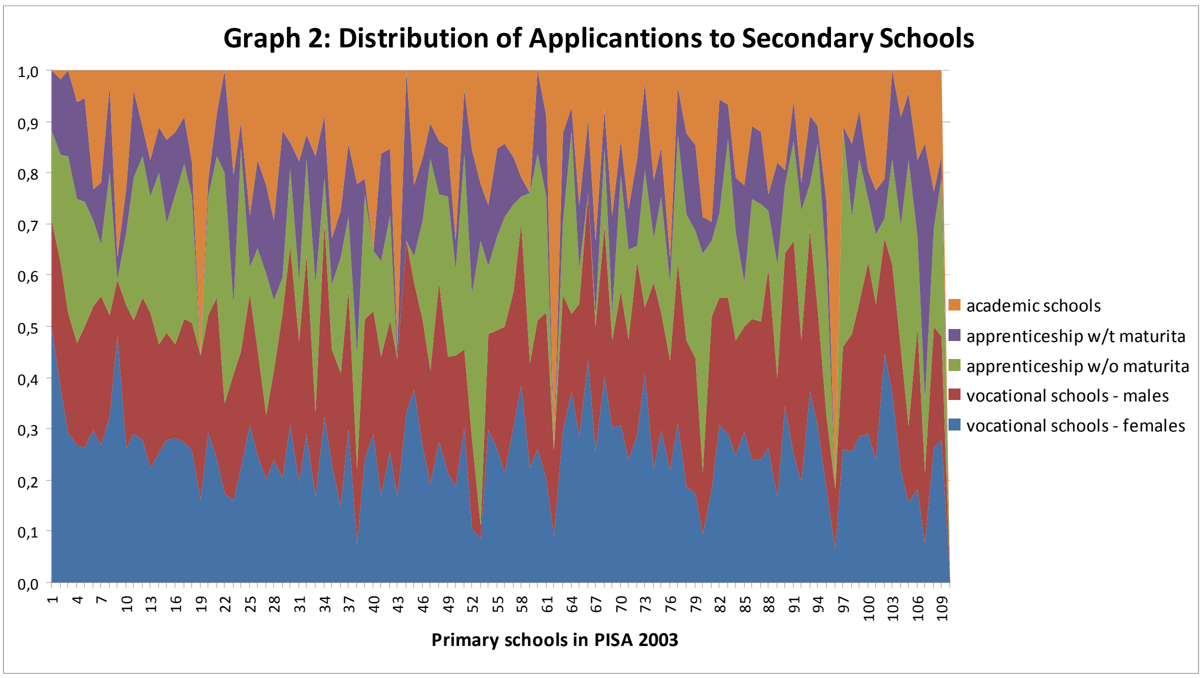
Appendix



Source: European School Survey of Alcohol and Other Drugs, 2003



Source: PISA 2003, own computations

Table 1: Trying a cigarette within the last 30 days (share, st.dev.)

	1999		2003	
School Type	Male	Female	Male	Female
Academic	.38 (.49)	.32 (.47)	.22 (.41)	.25 (.43)
Vocational	.40 (.49)	.43 (.49)	.42 (.49)	.42 (.49)
Apprenticeship	.55 (.50)	.56 (.49)	.58 (.49)	.69 (.46)
Total	.45 (.50)	.44 (.50)	.44 (.49)	.44 (.49)

Table 2: Current daily smoking (share, st.dev)

	1999		2003	
School Type	Male	Female	Male	Female
Academic	.21 (.41)	.19 (.39)	.08 (.27)	.09 (.28)
Vocational	.28 (.45)	.27 (.45)	.23 (.42)	.20 (.40)
Apprenticeship	.43 (.49)	.45 (.49)	.38 (.48)	.46 (.49)
Total	.32 (.27)	.30 (.46)	.27 (.44)	.24 (.42)

Source: ESPAD data, own computation

Table 3: Start of daily smoking, first year students (in %)

	1999		2003	
Age	Male	Female	Male	Female
Never	61.5	61.15	62.9	65.3
11 and earlier	2.8	0.8	2.8	1.0
12	3.3	2.8	3.5	2.8
13	5.1	5.2	5.5	5.3
14	10.2	9.3	9.2	9.1
15	11.6	12.7	11.4	12.4
16 and later	5.5	6.4	4.6	4.2
avg. age	16.2	16.2	16.1	16.1

Note: Self-reported start of daily smoking, individuals are enrolled in secondary school approximately at age 15.
Source: ESPAD data, own computation

Table 4: Peers' daily smoking before enrollment into secondary school, share (st.dev)

School Type	1999 Male	1999 Female	2003 Male	2003 Female
Academic	.15 (.11)	.13 (.10)	.09 (.07)	.09 (.06)
Vocational	.19 (.10)	.15 (.09)	.22 (.10)	.19 (.09)
Apprenticeship	.25 (.13)	.24 (.12)	.32 (.12)	.35 (.12)
Total	.20 (.12)	.17 (.11)	.23 (.14)	.20 (.13)

Source: ESPAD data, own computation

Table 5: Smoking experience of third year students, share (st.dev)

School Type	1999 Male	1999 Female	2003 Male	2003 Female
Academic	.11 (.32)	.14 (.35)	.12 (.33)	.13 (.34)
Vocational	.24 (.42)	.23 (.42)	.22 (.41)	.26 (.44)
Apprenticeship	.33 (.47)	.32 (.46)	.33 (.47)	.37 (.48)
Total	.24 (.43)	.23 (.42)	.25 (.43)	.25 (.43)

Source: ESPAD data, own computation

Table 6: Descriptive statistics

Variables	1999 (1) Mean (st.dev.)	1999 (2) Mean (st.dev)	2003 (3) Mean (st.dev)	2003 (4) Mean (st.dev)
Daily smoker	.31 (.46)	.22 (.42)	.26 (.44)	.16 (.37)
Daily smoker before sec. sch.	.19 (.39)	n.a.	.22 (.41)	n.a.
Try cig. before sec. sch.	.63 (.48)	.55 (.50)	.66 (.47)	.57 (.50)
Drunk before sec. sch.	.33 (.47)	.25 (.43)	.37 (.48)	.27 (.45)
Try marijuana bef. sec. sch.	.26 (.44)	.19 (.39)	.35 (.48)	.26 (.44)
Drink beer before sec. sch.	.70 (.46)	.66 (.47)	.75 (.43)	.70 (.46)
Complete family (1- yes)	.78 (.41)	.79 (.40)	.76 (.42)	.79 (.41)
Sport on daily basis	.29 (.46)	.30 (.46)	.29 (.46)	.31 (.46)
Father's college degree	.21 (.41)	.21 (.41)	.24 (.43)	.26 (.44)
Father's hs degree	.26 (.44)	.27 (.44)	.27 (.44)	.27 (.44)
Age	16.21 (.40)	16.22 (.40)	16.19 (.41)	16.19 (.41)
Female	.52 (.50)	.53 (.50)	.53 (.50)	.53 (.50)
GPA12	.43 (.50)	.47 (.50)	.40 (.49)	.44 (.50)
GPA34	.46 (.50)	.44 (.50)	.45 (.50)	.43 (.49)
GPA56	.09 (.29)	.08 (.27)	.11 (.31)	.08 (.28)
Older siblings smoker	.31 (.46)	.32 (.47)	.30 (.46)	.32 (.47)
Vocational school	.45 (.50)	.46 (.50)	.40 (.49)	.41 (.49)
Academic school	.22 (.42)	.24 (.43)	.26 (.44)	.30 (.46)
Apprenticeship	.33 (.47)	.30 (.46)	.34 (.48)	.29 (.46)
Regions:				
Prague	.11 (.31)	.10 (.30)	.10 (.30)	.10 (.30)
Central	.11 (.31)	.11 (.31)	.10 (.30)	.10 (.30)
South	.07 (.25)	.07 (.25)	.06 (.25)	.06 (.24)
West	.07 (.26)	.07 (.26)	.08 (.28)	.08 (.27)
North	.13 (.33)	.12 (.33)	.13 (.34)	.13 (.34)
East	.13 (.33)	.13 (.34)	.15 (.36)	.15 (.35)
Southeast	.19 (.40)	.19 (.39)	.17 (.38)	.17 (.38)
Northeast	.20 (.40)	.20 (.40)	.20 (.40)	.20 (.40)
Total number of observations	4676	3787	4622	3612
Number of classes	224	224	180	180

Note: samples (1) and (3) are full, samples (2) and (4) do not involve pre-secondary school daily smokers

Source: ESPAD data, own computation

Table 7: The estimation of peer effects (full sample)

	Peers' smoking	Controls for selection	Female	Male
(1)	% current daily smokers		.273***	.350***
			(.052)	(.049)
(2)	% pre-school daily smokers		.170***	.268***
			(.056)	(.054)
(3)	% pre-school daily smokers	Individual pre-school behavior	.019	.153***
			(.050)	(.053)
(4)	% pre-school daily smokers	(3) + peers' characteristics	.016	.126**
			(.50)	(.046)
(5)	% pre-school daily smokers	(4) + % older students' lagged smoking	.024	.114**
			(.054)	(.047)
(6)	% pre-school daily smokers	(4) + school fixed effect	.062	.142***
			(.045)	(.043)
(7)	% pre-school daily smokers	(5) no academic sch.	.032	.117*
			(.70)	(.061)

Note: Results come from OLS regressions. All specifications control for GPA, parental education, family completeness, school type, time and regional dummies. Standard errors are clustered on class level (in fixed effect estimation on school level)

Table 8: The estimation of peer effects (no pre-secondary school daily smokers)

	Peers' smoking	Controls for selection	Female	Male
(1)	% current daily smokers		0.242*** (0.053)	0.310*** (0.053)
(2)	% pre-school daily smokers		0.128* (0.069)	0.227*** (0.066)
(3)	% pre-school daily smokers	Individual pre-school behavior	0.063 (0.066)	0.198*** (0.063)
(4)	% pre-school daily smokers	(3) + peers' characteristics	0.050 (0.067)	0.173*** (0.061)
(5)	% pre-school daily smokers	(4) + % older students' lagged smoking	0.062 (0.067)	0.157** (0.061)
(6)	% pre-school daily smokers	School fixed effect	0.024 (0.066)	0.114* (0.0675)
(7)	% pre-school daily smokers	(5) no academic sch.	.056 (0.782)	.146** (.071)

Note: Results are from linear probability OLS regressions. All specifications control for GPA, parental education, family completeness, school type, time and regional dummies. Standard errors are clustered on class level (in fixed effect estimation on school level)

Table 9: Instrumental variable estimation

	Naive (female)	IV (female)	Naive (male)	IV (male)
	(3)	(4)	(5)	(6)
% current daily smokers	0.151***	0.060	0.218***	0.281***
	(0.048)	(0.094)	(0.053)	(0.074)
% older students lagged smokers	0.069	-0.026	0.112***	0.102***
	(0.043)	(0.046)	(0.034)	(0.032)
Individual pre-school behavior	X	X	X	X
GPA and Parental Education	X	X	X	X
School type, time and regional fixed effects	X	X	X	X
Peers' characteristics	X	X	X	X
Observations	4514	4514	4079	4079
R-squared	0.35	0.35	0.23	0.23

Note: The instrument is peers' pre-secondary daily smoking. The Instrumented variable is *% current daily smokers*. I control for selection using various pre-secondary school individual behavioral characteristics, current peers' characteristics and older schoolmates lagged behavior (as a proxy for expectations).

Table 10: Instrumental variable – first stage

	Female	Male
	(2)	`(3)
% pre-school daily smokers	0.571***	0.558***
	(0.015)	(0.016)
Individual pre-school behavior	X	X
GPA and Parental Education	X	X
School type, time, reg. fixed effects	X	X
Peers' characteristics	X	X
Observations	4515	4081
R-squared	0.62	0.48

Note: Explained variable is the current share of daily smokers.

Table 11: Testing for non-linearity of peer effects

	Male	Female
Peers' smoking:		
2. quartile	0.017	0.004
	(0.017)	(0.022)
3. quartile	0.006	0.033
	(0.018)	(0.023)
4. quartile	0.020	0.033
	(0.023)	(0.026)
Individual pre-school behavior	X	X
GPA and Parental Education	X	X
School type, time, reg. fixed effects	X	X
Peers’ characteristics	X	X
Observations	3648	3206
R-squared	0.24	0.17

Table 12: Testing for peer effects for smokers of marijuana

	Female	Male
Try marijuana* % pre-school daily smokers	0.150	0.153
	(0.137)	(0.121)
Try marijuana before sec. school	0.271***	0.208***
	(0.038)	(0.032)
% pre-school daily smokers	0.014	0.10*
	(0.059)	(0.058)
Individual pre-school behavior	X	X
GPA and Parental Education	X	X
School type, time, reg. fixed effects	X	X
Peers’ characteristics	X	X
Observations	3644	3195
R-squared	0.24	0.17

Don’t Drink and… Avoid Risky Sex of Your Peers: The Influence of Alcohol Consumption of Opposite-Gender Peers on Youth Risky Sexual Behavior

Filip Pertold

CERGE-EI*

I estimate the effect of opposite-gender peer drinking on individual risky sexual behavior among Czech youth. The identification strategy relies on two main controls for individual and group-specific unobservables. First, younger schoolmates’ sexual behavior is a control for school-specific attitudes toward sexual behavior. Second, pre-determined individual pre-secondary-school alcohol consumption is used to control for self-selection into schools of individuals with specific attitudes toward alcohol. As opposed to Waddell (2010), I find that female drinking affects the male propensity to have unprotected sex, while male drinking does not have such an effect on female behavior. This finding corresponds to the fact that females have usually older sexual partners than males.

V tomto článku odhaduji efekt pití alkoholu vrstevníků opačného pohlaví na riskantní sexuální chování českých středoškoláků. Identifikační strategie v tomto článku je závislá na dvou hlavních proměnných kontrolující pro individuální a skupinové nepozorované charakteristiky. První je průměrné sexuální chování mladších spolužáků ze stejné školy, což má především kontrolovat pro nepozorovatelný přístup k sexuálnímu chování specifický pro různé školy. Druhá hlavní kontrolní proměnná je spotřeba alkoholu před vstupem na střední školu. Ta má kontrolovat především pro selekci studentů do jednotlivých středních škol. Na rozdíl od Waddella (2010) jsem zjistil, že pití žen zvyšuje pravděpobnost jejich spolužáků mít nechráněný sex, kdežto pití mužů tento efekt na ženy nemá. Toto zjištění koresponduje s tím, že ženy mají obvykle starší první sexuální partnery.

***JEL Classification*:** J13; I12
Keywords: Peer effects, Sexual behavior, Drinking

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1. Introduction

Risky sexual behavior leads to many negative social and health consequences, especially among the youth. Teenage pregnancy, for example, might affect educational attainment of young parents (Heckman and Masterov, 2004) and sexually transmitted diseases have long-term health consequences. These are the reasons behind public policies aiming at the reduction of risky sexual behavior among teenagers. Focusing on youth alcohol consumption is sometimes one of the means of these policies, as it is believed to be one of the triggers of risky sexual behavior (Cooper, 2006).

A vast amount of literature thus attempts to quantify the causal link between alcohol consumption and risky sexual behavior. There is a long debate over the proper strategy that should be used to identify the effect (Lucraz et al., 2009), and in fact, no consensus has been established. The difficulty in estimating the causal relationship between alcohol consumption and risky sexual behavior at the individual level reflects the complex nature of the underlying mechanism between these two activities. The main problem is individual sexual activity and alcohol consumption might both vary with some common unobserved attributes, for example, risk aversion or family background. Finding a proper instrument, which would break this simultaneity is extremely difficult, i.e. predict only individual alcohol consumption but not have a direct effect on sexual activity (Rashad and Kastner, 2004). However, without establishing the existence of this relationship and understanding the underlying mechanism, it remains difficult to form proper policy.

A new insight into the potential underlying mechanism is provided by Waddell (2010). He suggests that it may not just be one´s own drinking that influences one’s own sexual behavior, but also opposite-gender peer drinking would also do so. As the sexual intercourse is of a bilateral nature, as Waddell points out, the role of opposite-gender peers can be very important. Except for Waddell (2010), there is not much written about any potential mechanisms that would link peer drinking and individual sexual behavior. However, development psychology has a few insights into the problem of peer drinking and sexual behavior. For example, Ascolese (2005) points out that binge drinking and other socializing are related to individual risky sexual behavior. As binge drinking obviously involves interactions with peers, peer drinking can increase the probability that an individual will ultimately have sexual intercourse. This means that drinking in a group does increase the individual's probability to drink and, at the same time, the individual's probability to have unprotected sex. If others did not drink, an individual would not engage in binge drinking and would not have risky sex. This can be considered as one potential explanation for the mechanism that links peer drinking to individual sexual behavior.

In the case of this relationship, there is less doubt about the way the causality goes. Peer drinking and one’s own sexual behavior do not suffer from the simultaneity stemming from one’s own unobservables, as in the case with own sexual behavior and drinking. However, there are two other identification issues with estimating the effect of peers’ drinking on own sexual behavior: the selection into the peer groups and the omitted variable problem (Kremer and Levy, 2008). The selection problem appears when individuals choose their own peers based on some unobserved characteristics, for example the attitude toward risky behavior. The omitted variable problem stems from the existence of other uncontrolled factors that might affect youth behavior, for example, school-specific policies toward alcohol consumption and sexual behavior.

Apart from these identification issues, one also needs to acknowledge that in the case of sexual behavior, peer effects might work differently for males and females. For example, Waddell documents that motivation for sexual activity and the general perception of sex differ substantially between females and males. Furthermore, Crochard et al. (2009) show that the average age of a first sexual partner differs substantially between genders: Females have much older partners than males. Thus, it can be expected that the relationship between peer drinking and one's own sexual behavior might be gender-specific, and the underlying mechanisms could also be different.

This paper explores the role of opposite-gender peer drinking in risky sexual behavior among close to 18-year-old Czech secondary school students, where peers are defined as classmates. The Czech Republic is generally an important case to study mainly because of the low proportion of condoms used during first sexual intercourse. Only 58.1 % of males use a condom compare to 88 % in France (Crochard et al., 2009).

The identification strategy employed in my paper builds on Waddell (2010), and on the large literature, that uses school and grade fixed effects to capture the source of selection bias that results from sorting into schools and classes. In my analysis, the selection problem is mitigated by two main controls. The first one is younger schoolmate sexual behavior, which should capture the school-specific level of risky sexual behavior. In particular, two years younger, same gender schoolmates are used in order to avoid the endogeneity driven by possible current social interactions between schoolmates. The second key control is individual, pre-secondary school drinking (up to 15 years of age), which should capture pre-determined unobservables related to drinking.[11]

My findings suggest that the male propensity to have unprotected sex increases with their female peers' drinking, while male drinking does not affect their female classmates' propensity to have unprotected sex. In the baseline specification, drinking is defined as reporting getting drunk in the last 30 days. These findings are opposite to those Waddell (2010) reports for the US, where male drinking is a significant predictor of female sexual behavior. My results are, however, in line with the higher average age of a first sexual partner for females such that these partners most likely do not come from the females' class. My

[11] A similar approach is used in Pertold (2009).

findings are also supported by several robustness checks. First, I estimate an instrumental variable model, in which the current drinking of opposite gender peers is instrumented by pre-secondary school peer drinking. Second, I use an alternative definition of alcohol drinking. As opposed to reporting getting drunk in the last 30 days, the alternative specification employs the definition of alcohol drinking as at least 3 times in the last 30 days. All the alternative specifications confirm the original results: Female drinking affects their classmates' sexual activity.

2. Literature Review and Estimation Issues

The recent literature on peer effects studies many youth outcomes: mainly on educational achievement (Kremer and Lavy, 2008), smoking and alcohol consumption (Lundborg, 2006), and also sexual behavior (Duncan et al., 2005 and Jaccarr et al., 2005). Peer effects in risky sexual behavior have been, however, estimated only in a framework where peers' sexual behavior affects individual propensity to have unprotected sex with no direct link to alcohol consumption.[12] The relationship between peers' alcohol consumption and own sexual behavior has been examined only in Waddell (2010).

All papers that estimate peer effects, deal with three key identification issues: group selection, omitted variable problems, and the reflection problem. The first problem arises when the conditions, under which a peer group is created, are not random and individuals self-select into a group based on their unobserved characteristics. The omitted variable problem appears when other uncontrolled parallel events affect both the left- and right-hand-side variable. The reflection problem arises when peer and individual behavior can affect each

[12] The consumption of alcohol is relatively easy to target by public policy; thus, I do not consider peer sexual activity as the key variable of interest. To estimate peer effects in sexual behavior one needs to employ an identification strategy that deals with the reflection problem as described below. Pertold (2009) estimates smoking classmate peer effects and solves the reflection problem using information about pre-secondary school smoking and the re-sorting of students from primary to secondary schools. Such a strategy is, however, not feasible in the case of sexual activity as I do not observe when exactly individuals have had unprotected sex.

other, and peer behavior is an aggregation of individual behavior.[13] Similarly to Waddell (2010), this problem is not directly addressed in this paper as it is improbable that opposite-gender sexual activity predicts individual risky drinking.[14]

To study the selection and omitted variable problems in an econometric setting, the following specification leads to the estimation of the effect of peers' drinking on individual risky behavior:

$$(1) \qquad sex_{ics} = \alpha_0 + \alpha_1 \overline{peerdrink_{cs}} + \alpha_2 \overline{X}_{cs} + \alpha_3 X_{ics} + \varepsilon_{ics}$$

Where sex_{ics} is a latent variable that is linked to the binary outcome of an individual i in class c and school s having unprotected sex, and $\overline{peerdrink_{cs}}$ refers to opposite-gender peer drinking. To estimate α_1, one needs to control not just for individual characteristics (X_{ics}) that drive individual sexual behavior, but also for the average of other peer characteristics ($\overline{X}_{cs}$). The effect of other peer characteristics, referred to Manski (1995) as the contextual effect, can be characterized as a vector of peer pre-determined characteristics that may affect individual behavior. For example, an individual may be affected by the knowledge of her peers about health consequences of risky sexual behavior.

The most important assumption behind the unbiased estimation of specification (1) is the error term is uncorrelated with the key explanatory variable $\overline{peerdrink_{cs}}$ and there is no

[13] The reflection problem is in fact an application of simultaneity, when a researcher does not observe who influences whom in a group and peer behavior is simultaneously changing with individual behavior (Manski, 1995).

[14] The problem of simultaneity can arise if individual sexual behavior predetermines peer drinking. For example, having unprotected sex can lead to peers' drinking due to psychological problems. To avoid this problem, one needs to find an instrument that predicts current drinking but is not directly correlated with individual sexual behavior. I propose pre-secondary school drinking as an instrument for current drinking. As it is described in Pertold (2009), Czech students at the age of 15 are re-sorted from primary schools, located usually in their neighborhood, into many different secondary schools and classes within a school. Therefore, classmates have usually little chance to affect each others' behavior before the enrollment into secondary schools. Pre-secondary school peer drinking should be properly excluded from the baseline regression that is presented below.

reverse causality between peer drinking and sexual behavior. Similarly to Waddell (2010), I assume the latter problem is of little importance; however, I provide results from an instrumental variable that should capture this estimation as a robustness check. The problem of group selection on unobserved characteristics requires more attention. If the make up of a peer group is not fully randomized, the self-selection of peers on unobserved preferences toward drinking and risky sexual behavior is possible, and the estimated effect would consequently be biased. The omitted variable problem can arise, for example, when schools have different approaches toward teaching students about the use of contraceptives. Not controlling for these factors also leads to a correlation between the error term and the key explanatory variable ($\overline{peerdrink_{cs}}$).

The literature provides no any ideal solution to these problems. The most reliable approach is to randomize the assignment of individuals to their peers. Kremer and Levy (2008) summarize results from various experiments that were organized usually at US colleges, where freshmen were randomly assigned to their roommates. This type of experiment is not usable for my research question. First, many secondary schooling systems, including the Czech one, are usually organized using an admission process that necessarily includes some type of selection. Second, the environment of college dormitories does not allow for an examination of the effect of opposite-gender behavior on one's own as roommates are of the same gender.

The papers that examine student behavior at secondary schools therefore usually employ school and grade-fixed effects to capture school-specific unobservables. The remaining variation in peers is thus supposed to be random (Lundbork, 2006; Waddell, 2010). However, even this approach does not necessarily lead to unbiased estimates. There is evidence that students within a cohort might be non-randomly assigned even to classes (Urquiola and Verhoogen, 2007).

I employ an identification strategy that is in a similar spirit to the fixed effects approach, but which reflects the limitation of the data and the nature of the Czech schooling system. The details are provided in the next section.

Another important issue in the estimation of peer effects is the actual definition of a peer group. The development psychology literature, for example, relies mostly on self-reported friends as the relevant peer group (Jaccard et al., 2005). While self-reported friends are probably the most relevant peer group, there are problems with this approach. Most importantly, it is very likely that even after controlling for individual time-constant characteristics, the selection problem is still an issue as the creation of a peer group can be based on an unobserved expectation about future behavior. It is also well known that teenagers often project their own behavior on their peers, which can cause a measurement bias.

Another stream of literature uses a class or a cohort at one school as a peer group (Lundbork, 2008). This approach might suffer from an imprecise definition of a peer group, which does not necessarily reflect reality, as students might be affected by other friends outside school. On the other hand, students often cannot fully control who is in their class, which diminishes the problem of group self-selection. The second advantage of this definition is that policy interventions can target a class or a school as a unit. Understanding the mechanism of peer effects within a class provides good background for designing such policies, and I therefore adopt the class peer-group approach.

3. Identification Strategy and the Econometric Specification

The solution I propose for the self-selection and omitted variable problems reflects the nature of the Czech secondary schooling system and the available information in my data. Students in the sample are in the third year of secondary school (aged 17.8 on average).

However, the available data also contains first year students (aged 16.2) for each school. Since it is extremely unlikely for third-year and first-year students of the same gender to have sexual intercourse, the first-year same gender students' sexual behavior can be employed to control for school-specific, risky-sex attitudes. This variable should capture the selection problem of some schools potentially attracting students with specific pre-secondary school experiences with risky sex. In order not to confound the selection effect with any potential interaction between first-year and third-year students, I employ the risky sexual behavior of the same gender. Thus regressions that estimate the effect of male drinking on female sexual behavior control for the first-year-female sexual behavior in the same school.

The second approach I employ to mitigate the selection problem is to control for the available information about pre-secondary-school drinking. According to the official statistics, Czech youth have their first experience with drinking at a very early age, most of them at a primary school, i.e. before the age of 15 (ESPAD, 2003). The data make it possible to track the self-reported histories of alcohol use, so they allow me to control for the selection of students into secondary schools based on pre-secondary school drinking.

The final econometric specification (2) also contains individual's current drinking, similar to Waddell (2010). Controlling for this variable allows me to interpret the peer drinking coefficient as corresponding to the effect of a peer's alcohol use on individual sexual behavior in addition to that of one's own drinking. I therefore estimate the following specification:

$$(2)\quad femsex_{ics} = \alpha_0 + \alpha_1 \overline{maledrink_{cs}} + \alpha_2 drink_{ics} + \alpha_4 \overline{youngfemsex_s} + \alpha_5 \overline{X}_{cs} + \alpha_5 X_{ics} + \varepsilon_{ics}$$

where $\overline{maledrink_{cs}}$ is the share of male classmates that report drunkenness in the last 30 days, $drink_{ics}$ stands for current and pre-secondary school drinking, $\overline{youngfemsex_s}$ represents the school-specific risky-sex attitudes approximated by the prevalence of risky sexual behavior among younger females at a given school *s*, and $\overline{X}_{cs}$ is a vector of peer variables that indicate the level of human capital and the share of complete families in a given class *c*. Finally, X_{ics} is a vector of individual-specific variables including family background, human capital, and the self-reported perception of the riskiness of smoking, taking as a proxy for the general perception of riskiness.

A similar specification can be formulated for males. The only difference is the key explanatory variable: female drinking behavior in a class.

(3) $$malesex_{ics} = \alpha_0 + \alpha_1 \overline{femdrink}_{cs} + \alpha_2 drink_{ics} + \alpha_4 \overline{youngmalesex}_s + \alpha_5 \overline{X}_{cs} + \alpha_5 X_{ics} + \varepsilon_{ics}$$

4. Data Description and Risky Sexual Behavior of the Czech Youth

The data come from the European School Survey of Alcohol and Other Drugs (ESPAD). This survey primarily consists of 16-year-old secondary school students very often from the first grade of secondary schools in 26 European countries who were asked about their tobacco, alcohol, and drug consumption and also about their sexual life. The key sex-life questions are whether the respondent had unprotected sex and whether s/he had sex that they eventually regretted. The survey was collected in four waves: 1995, 1999, 2003, and 2007. The Czech sample also records similar information for third-year students. Unfortunately, it is not possible to track at which age students had a particular sexual experience. The database also includes information about the education of parents, the existence of siblings, the use of spare time, the type of school, the perceived riskiness of smoking, the average GPA, a measure of self-esteem, and the number of family members.

For the purpose of the estimation, I pool data from 1999 and 2003. The sample from 1995 does not contain information about third-year students, which is used in the estimation, and data from 2007 are not available yet. The sample employed for the estimation consists of 1,851 third-year male students and 2,807 female students from 208 classes with at least a 10% share of each gender, covering altogether 208 schools in 1999 and 2003. In each school, I also observe one class of first-year students. The average age of the third-year students is 17.8 for males and females, which corresponds to the median age at which young people usually start with their sexual life (Crochard et al., 2009). The mean age of the first-year students' age is 16.2.

The two questions regarding sexual risky behavior contain information about unprotected sex and regretted sex. The first question is more relevant from a policy perspective because unprotected sex can have many negative social and individual consequences. Thirty percent of the sample answer that they had unprotected sex and, as shown in Table 2, one can observe significant differences across the three main types of Czech secondary schools: academic, vocational and apprenticeship.[15] Experiencing unprotected sex is reported by 17% of the male students in 1999 from academic schools, which is less than half of the share among apprentices. The share of students reporting unprotected sex is on average twice as high for third-year students than for first-year students.

[15] Academic and vocational schools usually provide four-year secondary programs, and students take a school-leaving exam (the 'Maturita') at the end of these programs. The 'Maturita' is a pre-requisite for tertiary education and obligatory for all students at vocational and academic schools (Jurajda, 2005). Apprenticeship programs do not lead to 'Maturita', and apprentices do not apply to colleges and universities but usually become blue-collar workers. As Munich (2004) points out, apprenticeship programs usually draw pupils from the lower end of the ability distribution. The main difference between academic and vocational schools is in their curriculum. Academic schools provide a general education that prepares graduates for college and university studies. Vocational schools provide an education focused on various fields: technical, business, pedagogical, and healthcare. Their graduates are expected to be ready to enter the labor market as well as colleges in their particular field.

On the other hand, one can observe nearly no differences across males and females within a school type and practically no change over time.

Significant differences across types of schools suggest that selection into schools plays an important role. As described in the previous section, the main variables that I use to control for school-specific sexual behavior are first-year student sexual behavior and the third-year students' own pre-determined individual drinking behavior prior to joining their current secondary school.

The ESPAD data do not contain complete information about the sexual life of students. In particular, there is no information about the time they had their first unprotected sex, which would be important for identifying the proportion of students that experienced first sex before their enrollment into a secondary school. Crochard et al. (2009) present recent statistics about the age at sexual debut. Table 1 shows the age of sexual debut as closely corresponding to the median age of the third year students in the ESPAD data (17.9). This means that approximately 50% of the sample most likely had their first sexual intercourse at the time of the survey and less than 25% had intercourse before enrollment into a secondary school.

The second important finding presented in Crochard et al. (2009) is the gender difference in the median age of the first sexual partner. Females report having a first sexual partner 2 years older than their own age (17) or the age of the males' first partner (17), which can be considered as a shortcoming of my definition of a peer group. As the ESPAD sample consists of classes with the same mean age of females and males, males have a higher chance to have a first sexual partner of the same age or younger. Females prefer an older partner for first sexual intercourse, who, given the age composition of the class, is less likely to come from their class. As the 25th percentile of the female partners' age (17) is approximately equal to the median age of the males' first sexual partner (17), it is possible to expect that the effect of classmate female drinking on the males' risky sexual behavior should be twice as stronger as the same effect of females on males. The relevant peer group of potential sexual partners for females is thus more likely older than their classmates.

Descriptive statistics of all variables are presented in Table 4. The generally lower proportion of apprenticeships in the sample is given by the exclusion of the highly gender-segregated classes, which are typical for these types of schools. Males and females do not differ strongly in terms of their average age, perceived riskiness of smoking one cigarette, parental education, and the completeness of family. However, a significant difference between males and females appears in the prevalence of drinking. About 57% of males reported drunkenness during the last 30 days, while females report drunkenness in 41% of the cases. This suggests that heavy alcohol consumption, while more present among young men, is a common activity for both genders. A similar gender difference also exists for pre-secondary-school drinking, and gender differences also exist in the reported GPA and sport activity.

5. Results

The estimated results based on OLS regressions are summarized in Table 5, which show a marginal effect of opposite-gender peer group drinking on the probability of having unprotected sex for the third-year students (α_1 from equations 2 and 3). Standard errors are clustered at the class level. The table compares the results for males and females, and each line refers to a single specification with control variables specified in the first column.

The first line thus presents the effect from a regression with only one explanatory variable: opposite-gender peer drinking. The estimates are positive and statistically significant, and they are larger for males. The first set of additional controls introduced in row (2) of Table 5 captures individual risk averseness and risk attitude as reflected in one's own smoking and school type dummies. The risk attitude is approximated by the perception of smoking and whether an individual is actually a current smoker. Controlling for these characteristics causes a drop in the estimated coefficient from 0.265 to 0.131 for males and from 0.157 to 0.101 for females. In row (3), I further control for individual human capital characteristics, age, and family characteristics including the completeness of family and parental education. The estimated effect drops further for females and males by approximately the same rate.

Specification (4) contains additional variables that characterize present and past individual drinking. Adding these covariates is the key step in identifying the effect of peer drinking on individual sexual behavior. If the association between peer drinking and individual sexual activity is driven mainly by a selection of drinkers into secondary schools, controlling for pre-secondary school drinking should mitigate the resulting biases in the estimation of α_1. The estimates of peer effects, however, remain significant and decrease by about ten percent for both females and males.

The results in row 5 are from a regression controlling for other peer characteristics: the completeness of family, smoking of siblings, education of parents and the type of school. All of these variables are supposed to capture other confounding factors related to unobserved factors affecting prevalence of drinking and risky sexual behavior within a class. The estimated coefficient drops even further to 0.112 for males and 0.074 for females.

The last control that is added into the analysis is younger schoolmate sexual behavior within the same gender. The idea behind this covariate is to control for the school-specific level of risky sexual attitudes. The estimated effects of male drinking on female sexual behavior become insignificant, while female drinking is still statistically significantly affecting male behavior. The estimated coefficient, 0.112, is approximately twice as large as that estimated by Waddell (2010). An increase in female peer drinking by one standard deviation causes an 11 percentage point increase in an individual's propensity to have unprotected sex.

The presented findings rely on a single definition of drinking: reporting drunkenness within the last 30 days. However, this measure of drinking is not the only possible one. Waddall (2010) uses for example the number of occasions during which alcohol was used. I provide several robustness checks that are presented in Table 6.

The first line presents the results from the original specification shown as specification (6) of Table 5. The first alternative specification uses pre-secondary school peer drinking as an instrument for the current drinking of peer. This specification is supposed to capture the potential simultaneity between peers drinking and sexual behavior, as the first sexual experience comes usually after the enrollment into secondary schools. The main result is fairly similar to that based on the original approach: Female drinking predicts male sexual behavior. However, the estimated coefficient on male drinking is now larger, albeit with corresponding larger standard errors. The third line contains results from a regression in which pre-secondary school drinking is used to proxy individual attitude toward drinking. This alternative definition estimates the effect of pre-determined peer alcohol drinking that is not affected by current social interactions. On the other hand, it can be contaminated by measurement error. The last alternative definition of drinking is to consume alcohol in the last 30 days at least 3 times. The estimated effect is again significant for the effect of female peer drinking on male sexual behavior, while male peer drinking remains insignificant as a predictor of female sexual behavior.

My finding is in line with the general description of sexual behavior of Czech youth provided in Crochard et al. (2009), who point out that female young adults have on average an older first sexual partner compare to males. The results are in line with the probability that males have twice as much of a higher probability to have a partner among their classmates than females. Thus, it is less likely that female sexual behavior would be affected by their classmates drinking.

6. Conclusions

In this paper, I estimate the effect of opposite-gender peer drinking on individual risky sexual behavior of Czech secondary-school students. The main finding is that female drinking within the same class significantly affects the male probability to have unprotected sex, while male drinking does not affect female risky sexual behavior. The size of the estimated effect means that an increase in female peers drinking by one standard deviation causes an 11 percentage point increase in an individual's propensity to have unprotected sex..

The policy implication of this analysis is that by reducing alcohol consumption among 18-year-old females, there would also be a substantial reduction in their male classmates' probability to have unprotected sex. On the other hand, female sexual behavior is less likely to be affected by male classmates consuming alcohol, even thought it could be affected by drinking of older males who are not observable. Results in Table 4 are consistent with possibly same peer effects for males and females, considering the fact that females have twice as much larger probability to have an older partner. It might be therefore important to target anti-drinking policies on females at a younger age. It is also necessary to highlight that the type of secondary school the student attends is a very important determinant of risky sexual behavior.

The identification strategy I employ deals with the selection problem and the omitted variable biases using various controls including the individual pre-determined pre-secondary school consumption of alcohol and the sexual behavior of younger schoolmates. The pre-secondary school consumption of alcohol mainly captures individual pre-determined unobservables related to the consumption of alcohol. Younger schoolmate sexual behavior serves as a control for school-specific attitudes toward risky sexual behavior. This identification strategy is different from that of Waddell (2010) and of many other papers that use school- and grade-fixed effects to deal with the problems of self-selection and omitted variables. The advantage of my approach is that it allows for a peer effect estimation in the absence of multiple-class information using data that contain only classes from different cohorts of students. Moreover while the fixed effects approach relied on within-school variation in peer drinking, my identification employs the part of variation in peer drinking across schools that is not driven by group selection.

My results contradict the findings in Waddell (2010), who implies that male drinking affects female sexual behavior based on employing the school-fixed-effect strategy. One explanation why my findings might differ from those in Waddell (2010) is that female teenagers in the US are more likely to have a sexual partner from their own class or cohort compared to the Czech females. There is, however, little of such evidence. In the US, 73% females between 17 and 19 have a sexual partner in the same age group or up to 3 years older (Kaiser Family Foundation, 2005). More precise statistics are available for African Americans. The mean age difference between sexual partners among those teenagers is approximately two years (Bauermeister, 2009), which is the same difference as in the case of Czech female teenagers. There is also no direct evidence that the choice of the first sexual partner is different or that the general attitude toward sex is different. The difference in the findings may be related to the different sources of variation employed in the estimation and, thus, remains a topic for further research.

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Appendix

Table 1: The Age of sexual debut and of a first sexual partner

	Age at sexual debut		Age of first sexual partner	
	Median	25^{th} – 75^{th} perc.	Median	25^{th} – 75^{th} perc.
Male	17	16-18	17	16-18
Female	17	16-18	19	17-20

Note: The different age for of the first sexual partner means that females usually have a more experienced first partner, whereas partners of males are on average the same age.
Source: Crochard et al. (2009)

Table 2: Unprotected sex and drinking behavior among third- and first-year students

	Unprotected sex*				Drinking**			
	1999		2003		1999		2003	
	Male	Female	Male	Female	Male	Female	Male	Female
Third-year students (mean age 17.8)								
Academic s.	17	14	20	21	54	33	53	37
Vocational s.	30	24	36	30	54	37	58	39
Apprenticeship	47	34	43	37	63	53	62	53
First-year students (mean age 16.2)								
Academic s.	11	8	6	8	40	27	31	25
Vocational s.	15	17	14	18	45	31	44	36
Apprenticeship	23	28	24	35	55	41	54	53

Note: * The share (in %) of those who report that they ever had unprotected sex.
** The share (in %) of those who report that they had been drunk in the last 30 days.

Table 3: Pre-secondary school drinking of third-year students (in %)

	1999		2003	
	Male	Female	Male	Female
Academic	21	17	27	18
Vocational	26	19	29	22
Apprenticeship	34	24	36	23

Note: A drinker is defined as someone reporting drunkenness prior to joining a secondary school.

Table 4: Descriptive statistics for third-year students, pooled 1999 and 2003 data

Variable	Male mean	Male st.dev.	Female mean	Female st.dev.
Having unprotected sex	0.27	0.45	0.30	0.46
Current drinking	0.57	0.49	0.41	0.49
Share of pre-sec. school drinkers	0.29	0.45	0.20	0.40
Share of females in class	0.52	0.20	0.66	0.17
Quality of family relationship (1low-5high)*	2.03	0.91	2.22	1.03
Smokers	0.46	0.50	0.46	0.50
Year dummy (2003)	0.58	0.49	0.62	0.49
Academic school	0.35	0.48	0.39	0.49
Vocational schools	0.37	0.48	0.38	0.48
Apprenticeship	0.28	0.45	0.24	0.42
GPA12	0.31	0.46	0.48	0.50
GPA34	0.54	0.50	0.43	0.50
GPA56	0.13	0.33	0.07	0.25
GPA78	0.01	0.08	0.00	0.04
Completeness of family	0.80	0.40	0.78	0.41
Parents - college degree	0.30	0.46	0.25	0.43
Parents - high school	0.28	0.45	0.25	0.43
Age	17.87	0.47	17.88	0.45
Perceived riskiness of 1 cigarette(1 low-5 high)*	2.09	0.88	2.03	0.78
Daily sport	0.38	0.49	0.20	0.40
Observations		1851		2807
Classes		208		208

*The regression analysis includes dummies for each level of the perceived riskiness of cigarettes and quality of the family relationship.

Table 5: The effects of female and male peer drinking on individual sexual risky behavior (third-year students)

		Female	Male
	Controls	Male peer drinking	Female peer drinking
(1)		0.157***	0.265***
		(0.052)	(0.062)
(2)	(1) + perception of risk, cig. smoking, school type	0.101***	0.131***
		(0.043)	(0.055)
(3)	(2) + human capital, family characteristics, age	0.095***	0.111***
		(0.039)	(0.055)
(4)	(3) + own present and past drinking	0.080***	0.103***
		(0.038)	(0.053)
(5)	(4) + other peers characteristics	0.074**	0.112**
		(0.036)	(0.056)
(6)	(5) + % younger students' risky sex. behavior	0.058	0.118**
		(0.041)	(0.056)
	Observations	2807	1851
	Classes	208	208

Note: Results come from LPM, all errors are clustered on class level.
The Sample contains only classes with more than 10% of opposite-gender peers. Peer drinking is defined as experiencing drunkenness in the last 30 days

Table 6: The alternative definitions of opposite gender peer drinking (results presented only from the final model)

		Females	Males
	Alternative specification of peer drinking	Male peer drinking	Female peer drinking
(1)	Drunkenness in the last 30 days (original specification)	0.058 (0.041)	0.118** (0.056)
(2)	Drunkenness in the last 30 days instrumented by pre-secondary school drinking	0.115 (0.11)	0.119* (0.062)
(3)	Pre-secondary school experience with drunkenness	0.062 (0.042)	0.083* (0.045)
(4)	Current drinking defined as 5 times in last 30 days	-0.031 (0.039)	0.080* (0.049)

Note: The results come from LPM, all errors are clustered on class level.
The sample contains only classes with more than 10% of opposite gender peers.
All specifications contain controls that are included in model 6 in Table 5.

Table 7: Full results of the effects of female peer drinking on the individual sexual riskiness of males
(third-year students)

Specification	1	2	3	4	5	6
% of female drinkers	0.265***	0.131**	0.113*	0.103*	0.112**	0.118**
	(0.063)	(0.057)	(0.058)	(0.058)	(0.056)	(0.056)
Current smoker		0.183***	0.176***	0.114***	0.102***	0.116***
		(0.0202)	(0.0211)	(0.0225)	(0.0224)	(0.0225)
Academic school		-0.102***	-0.107***	-0.107***	-0.0578	-0.0634
		(0.0317)	(0.0333)	(0.0328)	(0.0471)	(0.0485)
Vocational school		-0.0141	-0.0246	-0.0227	0.0114	-0.00157
		(0.0323)	(0.0329)	(0.0330)	(0.0350)	(0.0363)
Perception of riskiness of smoking=2		-0.0170	-0.0148	-0.00743	-0.007	-0.00145
		(0.0379)	(0.0379)	(0.0379)	(0.039)	(0.0397)
Perception of riskiness of smoking=3		0.000484	0.0241	0.0272	0.023	0.0270
		(0.0382)	(0.0400)	(0.0399)	(0.040)	(0.0407)
Perception of riskiness of smoking=4		-0.0552	-0.00726	-0.00328	-0.0008	0.000471
		(0.0360)	(0.0433)	(0.0436)	(0.0442)	(0.0439)
Perception of riskiness of smoking=5		-0.0354	-0.0382	-0.0254	-0.0102	-0.0184
		(0.0724)	(0.0743)	(0.0741)	(0.0763)	(0.0771)
Year dummy (2003)			0.0219	0.0177	0.0114	0.00819
			(0.0317)	(0.0318)	(0.0309)	(0.0312)
GPA 34			0.0462**	0.0407**	0.0393*	0.0416**
			(0.0210)	(0.0207)	(0.0209)	(0.0209)
GPA 56			0.0976**	0.0850**	0.0886**	0.103***
			(0.0382)	(0.0365)	(0.0365)	(0.0376)
GPA 78			-0.0694	-0.0846	-0.123	-0.106
			(0.0989)	(0.0914)	(0.104)	(0.111)
Parents-college degree			0.0485*	0.0466*	0.0532**	0.0529**
			(0.0259)	(0.0256)	(0.0253)	(0.0259)
Parents-high school			0.0153	0.0138	0.0203	0.0138
			(0.0224)	(0.0225)	(0.0224)	(0.0221)
Age			0.0552**	0.0545**	0.0548**	0.0569**
			(0.0260)	(0.0253)	(0.0264)	(0.0264)
Current drinker				0.110***	0.110***	0.131***
				(0.0197)	(0.0195)	(0.0191)
Pre-secondary school drinking				0.111***	0.106***	0.106***
				(0.0259)	(0.026)	(0.026)
Share of females in class					0.00909	-0.0101
					(0.0610)	(0.0615)
Quality of family relationship					0.0213**	0.0216**
					(0.00985)	(0.00981)
Completeness of family					-0.0536**	-0.0587**
					(0.0248)	(0.0251)
% smokers among siblings					-0.0895	-0.0617
					(0.0944)	(0.0963)
% sport daily					-0.0387	-0.0290
					(0.0994)	(0.102)
% parental college degree					-0.0611	-0.0533
					(0.0744)	(0.0749)

% complete family					-0.219**	-0.245**
					(0.0970)	(0.103)
% unprotected sex of younger sch.						-0.0181
						(0.0907)
Constant	0.167***	0.196***	-0.866*	-0.913**	-0.726	-0.726
	(0.0251)	(0.0523)	(0.470)	(0.461)	(0.484)	(0.484)
Observations	1851	1851	1851	1851	1851	1851
R-squared	0.014	0.075	0.086	0.117	0.127	0.117

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Tabel 7: Full results of the effects of male peer drinking on the individual sexual riskiness of females (third year students)

Specification	1	2	3	4	5	6
% of male drinkers	0.157***	0.101**	0.0951**	0.0803**	0.0741**	0.0584
	(0.052)	(0.0393)	(0.0373)	(0.0358)	(0.0364)	(0.0409)
Current smoker		0.216***	0.210***	0.151***	0.142***	0.147***
		(0.0171)	(0.0168)	(0.0171)	(0.0171)	(0.0174)
Academic school		-0.178***	-0.144***	-0.142***	-0.099***	-0.095***
		(0.0245)	(0.0260)	(0.0253)	(0.0331)	(0.0360)
Vocational school		-0.0517**	-0.0379	-0.0356	-0.00533	-0.00861
		(0.0259)	(0.0243)	(0.0239)	(0.0252)	(0.0270)
Perception of riskiness of smoking=2		0.0486**	0.0535**	0.0616***	0.0646***	0.0671***
		(0.0226)	(0.0223)	(0.0223)	(0.0217)	(0.0217)
Perception of riskiness of smoking=3		0.0610**	0.0847***	0.0821**	0.0763**	0.0794**
		(0.0290)	(0.0315)	(0.0316)	(0.0301)	(0.0306)
Perception of riskiness of smoking=4		-0.000	0.052	0.0682*	0.0638*	0.0660*
		(0.0270)	(0.037)	(0.036)	(0.035)	(0.035)
Perception of riskiness of smoking=5		0.0808	0.0984	0.116	0.115	0.0948
		(0.0717)	(0.0743)	(0.0760)	(0.0767)	(0.0760)
Year dummy (2003)			-0.00175	-0.00400	-0.00478	-0.00389
			(0.0316)	(0.0305)	(0.0295)	(0.0300)
GPA 34			0.0445**	0.0389**	0.0328*	0.0290*
			(0.0173)	(0.0170)	(0.0169)	(0.0172)
GPA 56			0.0930**	0.0791**	0.0584	0.0515
			(0.0382)	(0.0374)	(0.0368)	(0.0371)
GPA 78			-0.137	-0.0982	-0.118	-0.126
			(0.222)	(0.223)	(0.221)	(0.214)
Parents-college degree			-0.0313	-0.0380*	-0.0346	-0.0297
			(0.0212)	(0.0210)	(0.0217)	(0.0222)
Parents-high school			-0.0109	-0.00767	-0.00488	-0.00373
			(0.0216)	(0.0214)	(0.0219)	(0.0224)
Age			0.0918***	0.0983***	0.0944***	0.0959***
			(0.0228)	(0.0224)	(0.0223)	(0.0227)
Current drinker				0.0630***	0.0596***	0.0577***
				(0.0181)	(0.0180)	(0.0182)
Pre-secondary school drinking				0.184***	0.178***	0.175***
				(0.0226)	(0.0230)	(0.0235)
Share of females in class					-0.0860	-0.0862
					(0.0602)	(0.0613)
Quality of family relationship					0.0297***	0.0300***
					(0.00832)	(0.00849)
Completeness of family					-0.0571**	-0.0579**
					(0.0225)	(0.0229)
% smokers among siblings					-0.0707	-0.0318
					(0.0815)	(0.0799)
% sport daily					-0.164**	-0.182**
					(0.0757)	(0.0764)
% parental college degree					-0.0125	-0.0213
					(0.0582)	(0.0585)

% complete family					-0.0992	-0.124
					(0.0913)	(0.0909)
% unprotected sex of younger sch.						0.0356
						(0.0546)
Constant	0.210***	0.204***	-1.483***	-1.631***	-1.395***	-1.406***
	(0.0280)	(0.0366)	(0.404)	(0.396)	(0.393)	(0.399)
Observations	2807	2807	2807	2807	2807	2807
R-squared	0.006	0.104	0.114	0.145	0.156	0.157

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The Impact of Early Retirement Incentives on Labor Market Participation: Evidence from a Parametric Change in the Czech Republic

David Kocourek and Filip Pertold*

Abstract

We investigate the impact of a change in the Czech early retirement scheme on the labor force participation of older male workers. Using the difference-in-differences method we find that a reduction in early retirement benefits by 2–3% leads to approximately the same decrease in the probability of being inactive. Our finding implies high elasticity of older male workers' participation rate. The public policy implication is that a reduction in early retirement benefits can serve as a very effective tool to increase the participation of older men in the Czech labor market.

JEL classification: J21, J26
Keywords: early retirement, labor market participation, Czech Republic

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1. Introduction

As policy makers face the commonly known problem of an aging society, the labor supply of older workers becomes more important. The labor market decisions of older workers influence government expenditure on various social programs. For example, the way incentives to retire are formed is a crucial issue in keeping the pension system sustainable while the population is aging. Governments thus attempt to change the design of social security systems in order to respect demographic changes.

The Czech Republic is an example of an aging society.[16] The Czech government has reacted to this development and has decreased the incentives to retire early created by the social security system. Policy makers expect this step to reduce the number of people who receive retirement benefits and at the same time increase the number of contributors to the pension system. These unambiguous advantages make this policy step popular also among many other governments facing the issue of aging.[17]

The policy relevance of this topic is reflected in the current empirical literature. But it doesn’t exist clear answer about the causal impact of retirement incentives on the labor supply of older workers.

Cross-country comparisons show a strong negative relationship between early retirement incentives and labor force participation (Gruber and Wise, 1999, and Börsch-Supan, 2000). Papers examining changes in national policies suggest that the introduction of early retirement benefits as a specific form of retirement incentive decreases labor force participation (e.g. Brinch et al., 2001).

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[16] According to the projection of the Czech Statistical Office, the share of people aged 60 years and over will double in the next 30 years. Babecký and Dybczak (2009) try to model this aging scenario using an OLG model.

[17] It needs to be emphasized that the overall fiscal balance is improved unless retirees are proportionally compensated for longer service and unless employees leave the labor market and become unemployed or accept disability social assistance and/or become recipients of support from other social programs.

By contrast, other studies do not find clear evidence about the sensitivity of the labor supply of older workers to changes in the early retirement scheme. For example, Baker and Benjamin (1999) provide evidence from the USA and Canada which shows a relatively modest or non-existent reaction of the labor supply to changes in the early retirement scheme. Similarly, Moffitt (1987) finds relatively small effects of social security law on the labor supply of older workers in the USA.

There are only a few papers about the labor supply of Czech workers. Direct evidence concerning the labor supply of older workers is provided in Galuščák (2002) and Bičáková et al. (2008). Galuščák (2002) shows that the introduction of an earnings test, which imposed a benefit eligibility constraint on working pensioners, led to a significant and substantial decrease in the participation rate of workers who had reached statutory retirement age, whereas Bičáková et al. (2008) estimated the effect of tax changes on the labor supply of average Czech workers as being relatively modest. There is no direct evidence about the causal impact of early retirement incentives and the participation of older workers.

Retirement incentives can take various forms: explicit and implicit taxation and/or legal rules that restrict full-time work at a certain age. In our case we investigate the effect of reducing early retirement benefits, which are offered as non-labor income for individuals three years before the statutory retirement age. The policy change became effective in July 2001 and cut early retirement benefits by approximately 3% for new claimants. To illustrate this we also compare several incentive measures before and after the reform.

The social security statistics show that one year after the policy change, the number of new early retirees had decreased by half. This suggests that the direct impact of this policy step was strong. However, as we describe in the next section, older workers face several options regarding how to become non-employed (retire early[18], become unemployed, or enter

[18] The exact preconditions for early retirement are described in Act No. 155/1995 Coll.

disability retirement[19]). The positive causal effect of the policy change on the labor supply of older workers is under question.

In order to find the causal impact of the policy step, we use the difference-in-differences estimation method. The treatment group includes workers who are eligible for early retirement benefits (at most three years before the statutory retirement age). The control group contains workers who are just about to enter the eligibility age for early retirement, six to three years before the statutory retirement age to be more specific. The eligibility age for entering early retirement starts three years before the statutory retirement age. In particular, a marginal probit model is used for testing whether the policy change affects the participation rate of individuals who are eligible for early retirement, controlling for other characteristics of the individuals.

Our analysis shows that this policy increased the probability of a male participating in the labor market by 2–3% for those eligible for early retirement. This paper is organized as follows. The next section provides a detailed insight into the social security system in the Czech Republic. The official statistics and simulations of the policy change on individuals are described in section 3. Section 4 covers the data description of the treatment and control group. A graphical overview is presented in section 5, the econometric methodology is explained in section 6, and the results are described in section 7. Section 8 concludes.

2. Institutional Setting

The Czech retirement scheme is a standard pay-as-you-go (PAYG) system with mandatory participation for all employees and the self-employed as well. The basic features of the Czech pension system were inherited from the system run under the communist regime. A few legislative changes were implemented in the years after the fall of communism, but the

[19] To enter disability retirement certain health criteria have to be met. Hence, it is not a free choice of the individual.

basic features remained unchanged. The statutory retirement age is different for male and female workers; the retirement age of the latter depends on the number of children raised. Beside this differentiation the retirement age has been prolonged by two months for males and four months for females per year after 1996 to the year the male or female was supposed to retire under the former conditions. The retirement age for males in 1996 was set at 60 years.[20] The retirement age for females without children was 57 and each child raised reduces the retirement age by one year. At the time of the policy change the average retirement age was approximately 61.

Pension benefits are computed based on a formula that has an individual specific part (a percentage-based assessment) and a part, which is the same for everybody (the basic amount). The basic amount is the amount of money – laid down by law – that is received by everybody who is an old-age pension recipient. It can be understood as the minimum pension. The individual part reflects individual-specific characteristics, such as the earning history since 1986 and number of years in service. The wage history is discounted to the current value and then modified by reduction limits and reduction percentages to a calculation base (CB). The calculation CB represents the crucial step in the Czech pension formula and causes a high degree of redistribution in the system. Amount that is lower than the first reduction limit is fully included. But 30 % of the amount between the first and second reduction limit is included and only 10 % of the reminder, which is above the second reduction limit. The number of years in service proportionately increases (1.5 % per year) the size of the adjustment percentage (AP) and therefore the size of the percentage of the CB which will be counted as the percentage-based assessment (PA) in the pension formula. The longer an

[20] After that there is no single retirement age for the male population in a given year. The exact formulation is that the retirement age is prolonged by two months for each initiated age-year after December 31, 1995 before the individual reaches the age of 60. In practice this means that if a worker is 60 in February 2000, then his retirement age is 60 plus ten months. Therefore, the men from this example will retire in January 2001.

individual is in service, the higher the PA and therefore the higher the pension benefit will be. The exact formula can be found in Annex 1.

This formula is applied to every kind of retirement benefits, including early retirement benefits.[21] The early retirement benefits are lower than the standard ones, because they are reduced by an adjustment coefficient (rPYI), which was subject to the policy change. In particular, the "penalty" for early retirement before the reform was 0.6% and 0.3%[22] per each 90 days remaining to the standard retirement age before the policy was introduced. The policy step changed the degree of penalization for early retirement. In fact, both rates that adjust early retirement benefits (0.6% and 0.3%) were increased to 0.9%. For example, considering an individual who retires one year before her retirement age (a 0.6% reduction applied before the reform), the adjustment percentage of her benefit decreased by 3.6%after the reform instead of by 2.4% which would apply before the policy change – lower by 1.2 percentage points.

This decrease in the adjustment percentage proportionally decreases the pension benefit and hence has an influence on the motivation of workers to stay active on the Czech labor market until the statutory retirement age.

Table 1 shows the drop in officially newly granted early retirement benefits. The fall was approximately 10 percentage points of regular pension benefits. This observed change is most likely caused by two effects. The first one is driven by the change in early retirement

[21] The Czech social security scheme recognizes two types of early retirement. One is with permanently cut benefits, which allows individuals to retire at most three years before the eligibility age and the individual is not allowed to work after retiring. The decreased pension benefits are collected for the rest of the individual's life. The second is early retirement with temporarily cut benefits, which allows the individual to retire at most two years before the eligibility age and is tied to unemployment status for half of the year at least. The decreased pension benefits are recalculated when the eligibility age is reached and increased to the level as if one had retired at the eligibility age. Apart from that, two more ways of escaping employment status are available: becoming unemployed and becoming disabled. However, social support for disabled people is strictly tied to the health situation of the individual and hence cannot be regarded as a fully free choice of the individual, though the individual exerting pressure on the doctor who makes the decision about the disability pension can influence it.

[22] This applies for the case when an individual who applies for early retirement benefits and is aged 60 or more. For all other cases the permanent penalty is then just 0.6% per each 90 days before the standard retirement age.

benefits. The second one is driven by a change in the characteristics of workers who applied for early retirement before and after the policy step.

Tab. 1: Newly granted pensions (in CZK)

	1999	2000	2001	2002	2003	2004	2005
(1) all pensions	5,991	6,106	6,399	7,055	7,224	7,760	8,391
(2) at retirement age	6,222	6,485	6,823	7,226	7,512	7,968	8,693
(3) after retirement age	7,272	7,485	7,916	8,621	9,157	9,410	10,306
(4) early retirement – temporarily cut	5,370	5,513	5,838	5,917	6,224	6,404	6,836
(5) early retirement – permanently cut	**5,593**	**5,659**	**5,844**	**5,667**	**5,996**	**6,261**	**6,984**
(5)/(2) (in %)	90	87	86	78	80	79	80

Source: MLSA (2006), own computation of averages

The comparison of newly granted early retirement benefits before and after the reform does not provide a clear picture about the effect of the policy on benefits. It is probable that workers who applied for early retirement after the reform had stronger preferences toward leisure than workers who applied before the reform, and they might also have had different working histories[23], which determine their benefits. Therefore, we attempt to isolate the pure policy change effect from the sorting effect. For that purpose we create several typical individuals with different wage histories, which serve – together with length of service – as a major input for the computation of benefits.

We also compute the early retirement benefits before and after the change for individuals with virtually the same characteristics. The only parameter that changes is the degree of penalization, which was subject to the policy change. Our computations show that the net decrease in early retirement benefits was approximately 2–3% (CZK 120–250 per month in absolute terms). The cut corresponds approximately to 1–2.5% of the average net wage for male workers in the economy.

[23] Different wage histories and number of years in service, etc.

Tab. 2: Changes in early retirement benefits due to the policy change

	Years before eligible age T	Absolute decrease before/after (in CZK/month)	Relative decrease in early retirement benefit before/after (in %)	Change in terms of net wage (in percentage points)
70% of avg. wage	T-3	191	-3	-2.4
	T-2	133	-2	-1.6
	T-1	131	-2	-1.1
Avg. Wage	T-3	218	-3	-1.9
	T-2	149	-2	-1.3
	T-1	152	-2	-1.3
150% of avg. wage	T-3	237	-3	-1.3
	T-2	162	-2	-0.9
	T-1	166	-2	-0.9

Source: Own computation based on the official formula published in MLSA (2002).
Note: Benefits are computed for 46 years of service. The net wage is CZK 11,324 in 2001. Three income groups were chosen arbitrarily. 70% of the average wage reflects approximately the group of workers with the median wage and 150% of the average wage represents managers and high-paid workers in the Czech economy.

The ratio of the net wage to early retirement benefits (the net replacement rate) decreased by 0.9–2.4 percentage points. Generally, the highest decrease applied to those who wanted to enter early retirement three years before the eligibility age. Lower-income workers were penalized relatively more than upper-income groups. This is a result of the pension formula: benefits are relatively higher for low-income than for high-income workers. This implies that the policy change affected more strongly individuals who face a relatively disadvantaged position on the labor market.

Another way to assess the effect of this policy change is suggested in Börsch-Supan (2000). The author stresses the importance of the time dimension – how much it is worth to give up one year of retirement in terms of net benefit or social security wealth (SSW) computed as the difference between the expected discounted stream of all future benefits and social security taxes paid, which are computed as a percentage of gross earnings. The SSW formula, which states how to compute the social security wealth for an individual at age S planning to retire at age R, is

$$SSW_S(R) = \sum_{t=R}^{E} \pi(t \mid S) \cdot \delta^{t-S} \cdot B_t(R) - \sum_{t=S}^{R-1} \pi(t \mid S) \cdot \delta^{t-S} \cdot c \cdot W_t,$$

with:

SSW – social security wealth,
S – planning age,
R – planned retirement age,
E – expected age of death at age S,
$\pi(t \mid S)$ – probability of being alive at age t conditional on being alive at age S,
$B_t(R)$ – pension at age t for retirement at age R,
W_t – wage at age t,
δ – discount factor,
c – social security contribution rate.

SSW is very sensitive to many assumptions.[24] We employ the values for the discount factor and wage growth[25] from Coile and Gruber (2007) to keep the analysis consistent with the analysis of peak value (Coile and Gruber, 2007) and option value (Stock and Wise, 1990). In our computation of SSW we do not assume any indexation. The process of indexation in the Czech Republic depends very much on government discretion, as described in Dušek (2007) and Dušek and Kopecsni (2008).

Table 3 shows the basic computations of retirement incentives employing the lifetime budget constraint for an average earner.

Tab. 3: Monetary incentives before and after the reform (average earner)

Last age of work	Replacement rate – before	Replacement rate – after	SSW – before	SSW – after	Accrual rate – before	Accrual rate – after
58	0.837	0.828	699,347	690,703	-0.007	-0.007
59	0.870	0.864	650,158	644,474	-0.076	-0.072
60	0.906	0.903	598,921	595,727	-0.086	-0.082
61	0.936	0.936	545,586	544,716	-0.098	-0.094
62	0.964	0.964	489,416	488,365	-0.115	-0.115
63	1.012	1.012	445,006	443,768	-0.100	-0.100
64	1.037	1.037	389,143	387,718	-0.145	-0.145
65	1.105	1.105	352,270	350,657	-0.105	-0.106

Note: SSW – social security wealth – is defined as the sum of all discounted pension benefits and social security contributions. The accrual rate is defined as the relative year-to-year change in SSW.

[24] Assumptions regarding the individual discount rate, the future indexation of benefits under PAYG, the interest rate path, wage growth, etc.
[25] For simplicity we assume the same wage growth for all income groups.

Each row corresponds to the age at which a worker enters retirement. In this exercise we assume for the sake of simplicity that the statutory retirement age is 61. This means that everybody who enters retirement before the age of 61 is in early retirement regime and the worker is eligible for early retirement benefits at 58.

Comparing SSW before and after the reform, one can see a decrease in SSW for those who enter early retirement. SSW before and after the reform are highest at 58. The higher pension after longer time contributing to the social system cannot compensate for the social security contribution and hence SSW steadily decreases and therefore it is the best decision to retire as soon as possible since it maximizes the SSW.

A forward-looking approach to assessing the incentives created by the pension system can be studied using peak value and option value. Peak value (Coile and Gruber, 2007) is defined as all discounted benefits from entering retirement. In fact, it is maximized when SSW reaches its maximum. We performed this analysis and it obviously supports the preceding analysis that the reform has increased the incentives for the average earner to stay on the labor market. The second approach to assessing financial incentives is the option value model (Stock and Wise, 1990). The option value attempts to evaluate the optimal retirement age in utility terms and involves calculating the forgone earnings that could have been earned on the labor market. It is defined as the change in utility that results from working to the optimal age, which is determined by maximizing the lifetime utility over consumption and leisure. The problem of this approach is that one needs to employ certain assumptions about wage profile in the final career stage.

We employ the standard assumption of a linear wage profile, which is not necessarily a realistic assumption. Our results are summarized in Annex 2 and suggest that both according to the peak value and option value the optimal retirement age was not changed by the reform and is at the age of 58 in the case of option value and at 56 in the case of peak value.

However, there is one small exception of high earner whose option value reacts to the policy change and the optimal retirement age is moved by one year from 59 to 60.

One of the questions that this reform raised is what margin of the labor supply is affected, and in particular whether the reform affected the extensive or intensive margin of the labor supply of older workers. The extensive margin is affected only since the labor code restricts early retirement benefits: people who retire earlier (claim early retirement benefits) are not allowed to work at all.

3. Data Description and Treatment and Control Group

For the purposes of our research we use Czech Labor Force Survey data from 1998–2005 containing detailed information about the labor market status of a representative sample of 60,000 individuals and their households. On a rotating panel base, individuals and their households are surveyed during five consecutive quarters. Therefore, one fifth of the sample is replaced every quarter. We choose the subsample of males who are in the age window of six to zero years until the statutory standard retirement age. Hence, our sample includes 50,152 observations for 11,843 individuals. Summary statistics for the treatment and control groups can be found in Annex 4.

We divide this sample into four time periods – one period before the reform and three periods after the reform. Participation in the survey is restricted to up to five quarters. Within this period, we do not observe a sufficient number of changes in labor market status, thus we treat our sample as repeated cross-sectional data. The reason we choose only one period before the policy change is the low stability of the social security system: the legal system was stable for only two years before the policy change and approximately four years after the policy change. Our time span also reflects the comparability of the data. We define four consecutive periods, each 1.5 years long. The first is before the policy change (1Q2000–

2Q2001), the second is immediately after the policy change (3Q2001–4Q2002), the third is from 1Q2003 to 2Q2004, and the fourth covers 3Q2004–4Q2005. We also try alternative time spans, but this does not change our results significantly (see Annex 6). This division of the total time span into four periods covers the most institutionally stable period before and after the reform. On top of that, the results for several time periods after the reform confirm that the impact of the policy change is the same over time.

The important problem is the actual eligibility age, since the statutory retirement age has been lengthening by two months per year and gives additional noise to our data. To diminish this problem we calculate the individual statutory retirement age as defined by law. For that purpose we have to approximate the actual age of the respondents in the Labor Force Survey, because the survey per se does not provide information about the exact actual age (the accuracy is yearly frequency). Thus, we use only those individuals for which we observe a change in age during the period they were surveyed (Galuščák, 2002). Using these individuals we approximate the exact individual age at an accuracy of one quarter and calculate the actual individual statutory retirement age and simultaneously the eligibility age for the early-retirement. Based on this approximation we can also calculate the number of years to retirement. This makes our analysis more accurate and allows us to disentangle the effect of the early retirement change from the prolonging of the retirement age.

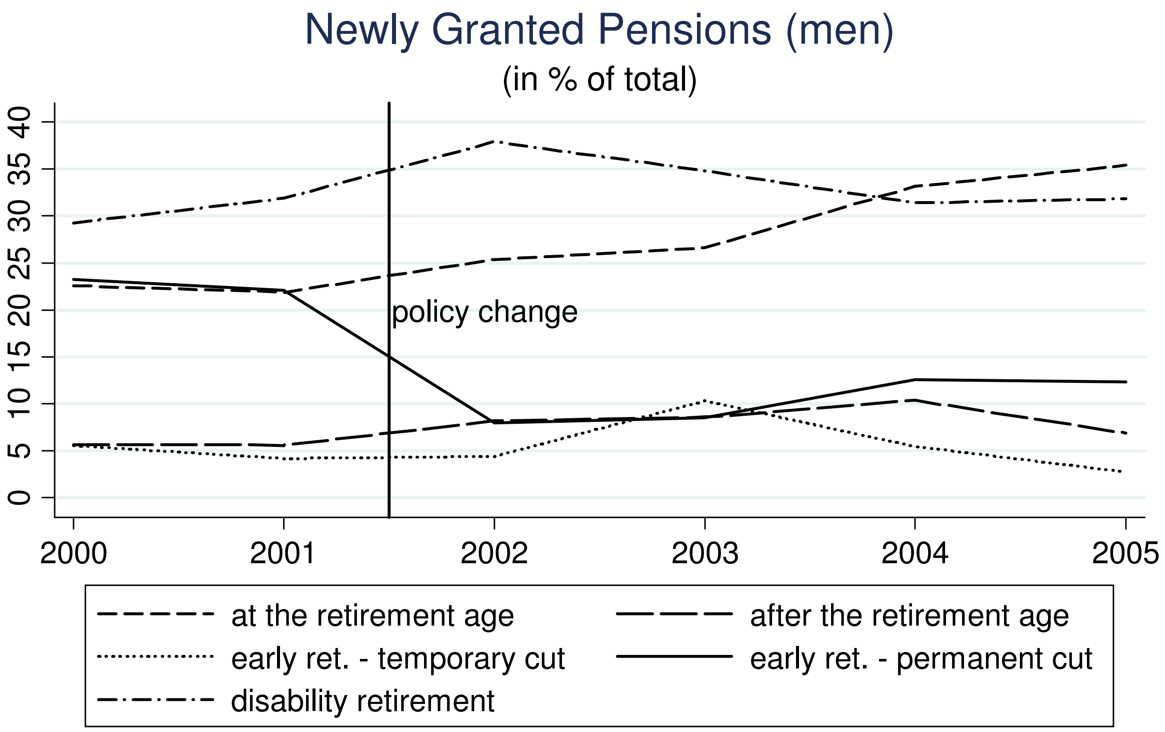
Using the number of years to the statutory retirement age we define the treatment and control groups. The treatment group contains people who are eligible for early retirement: up to three years before their standard retirement age. The younger individuals (more than three years before the eligibility age) are in the control group, because they were not directly affected by the policy. The relatively broad definition of the treatment group allows us to capture all individuals who were eligible for early retirement and could make the decision during the entire period of three years before reaching the statutory retirement age. The disadvantage is that in the period after the policy change the treatment group consists of two types of retirees: men who entered early retirement in the old system and those who entered in new system. This is reflected in our analyses and we interpret the results with respect to this fact.

The LFS data contain information about individual characteristics that are important for our analysis. For the purposes of our analysis we used the following characteristics: education, family status, number of persons in the household, and geographical location. The data do not include any information about wages or retirement benefits.

4. Graphical Overview

As we described above, the change in the early retirement scheme increases the incentive to stay in the labor market. As a preview of our results we present the official statistics of newly granted pensions (Fig. 1). The share of newly granted pensions for this particular pension scheme dropped significantly (the solid line).

Figure 1

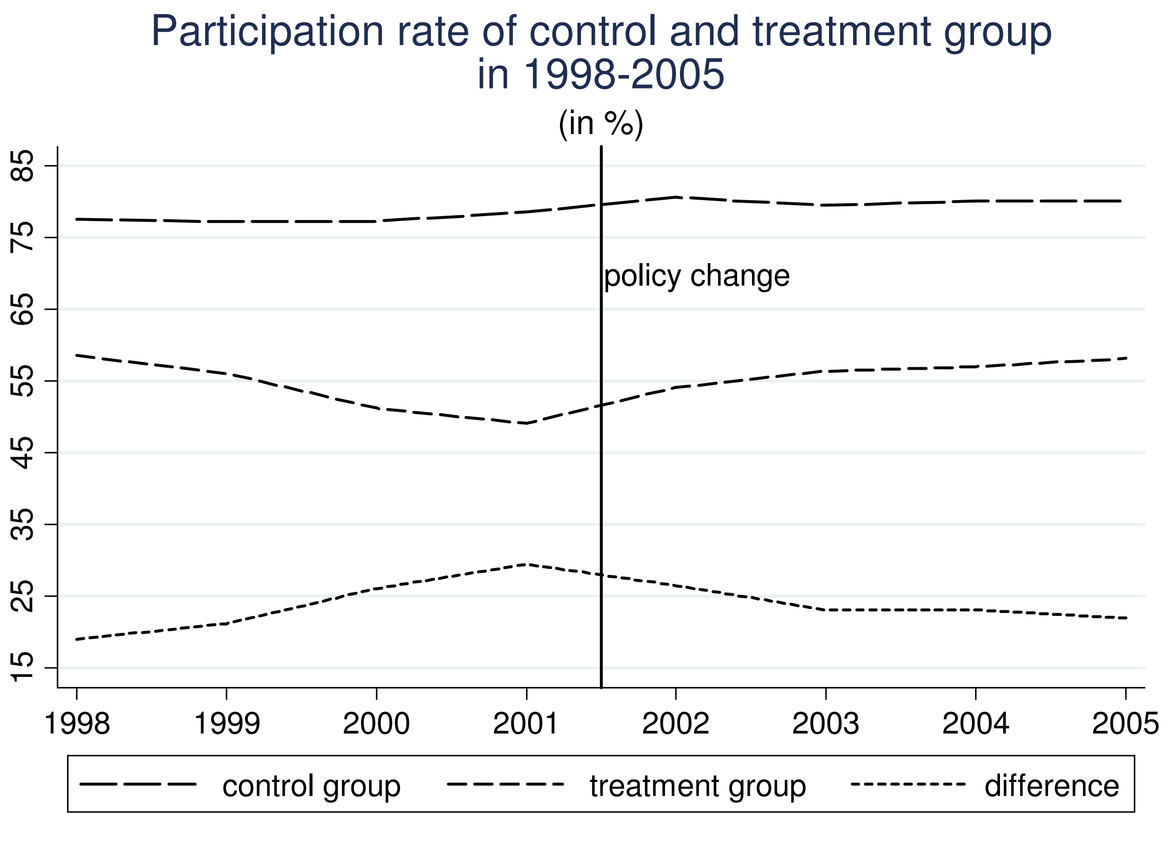


Source: Czech Social Security Administration, own calculation
Note: The short time span before the actual policy change is given by the limitation of official statistics.
The remainder to 100% are e.g. widower's and orphan's pensions.

This suggests that this reform could have a strong impact on the labor market decision. However, the total impact on the participation rate can be questioned, because the share of the other options for early exit could be used, as can be seen in Figure 1.

Further, we present the behavior of individuals using the Labor Force Survey data described above. Figure 2 depicts the participation rate of the control and treatment groups during 1998–2005. The participation rate of the treatment group increased by around ten percentage points between 2001 and 2004. The participation rate also increased in comparison with the control group. This suggests that our treatment group was subject to a specific shock that did not affect the control group. One can observe that this increase continued at a lower rate even during the period from the second half of 2003 to almost the end of 2004. It also contains the effect of the policy change, because in the first period after the policy change, the treatment group still contains older cohorts that entered early retirement before the policy change and remain in the treatment group. Due to data limitations and the institutional set-up, we cannot define the treatment group more precisely than 0–3 years before retirement.

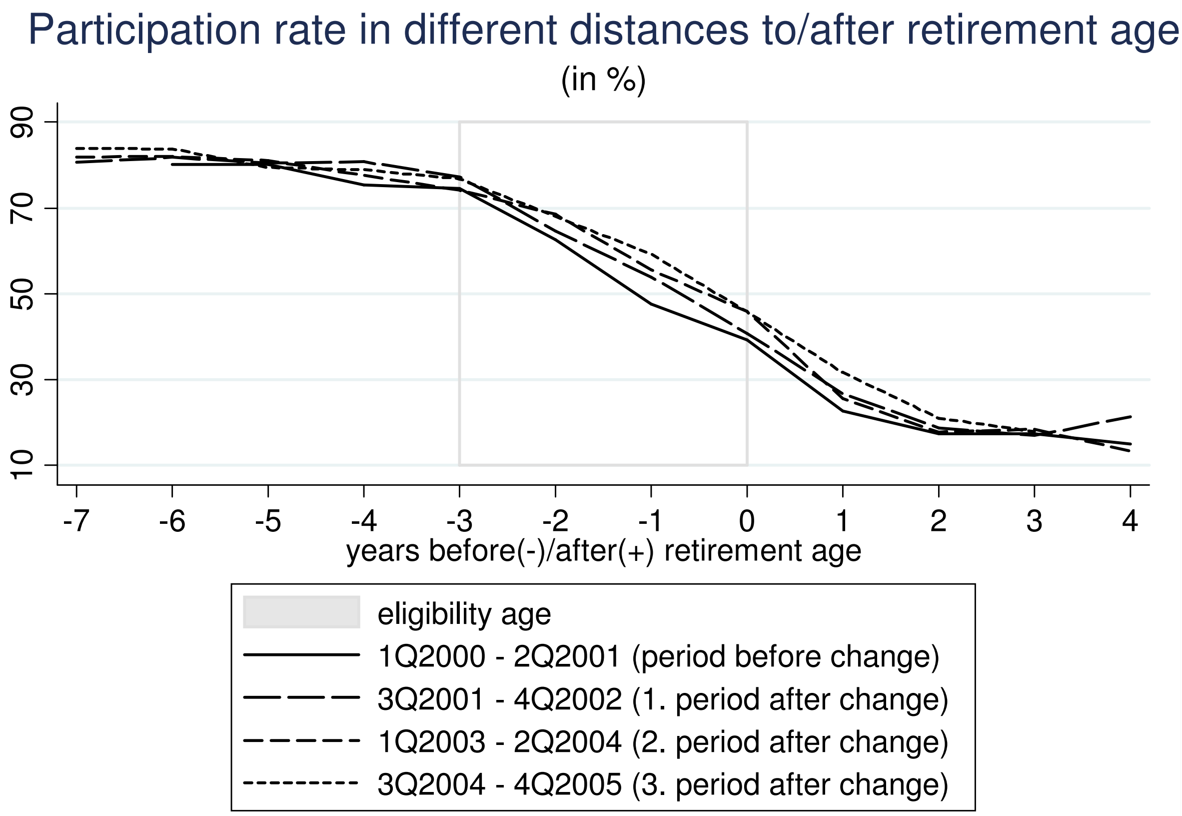
Figure 2



Source: Labor Force Survey, own calculation

In Figure 3 we can see how the participation rate changes over time in different years to/after retirement age. This quasi-cohort approach shows that the participation rate during the early retirement window (between -3 and 0) is the lowest in the period before the reform was introduced. Moreover, the trend that we observe in Figure 3 is clearly increasing. The difference between the pre-reform period and the last period studied at one year before the statutory retirement age is 12 percentage points.

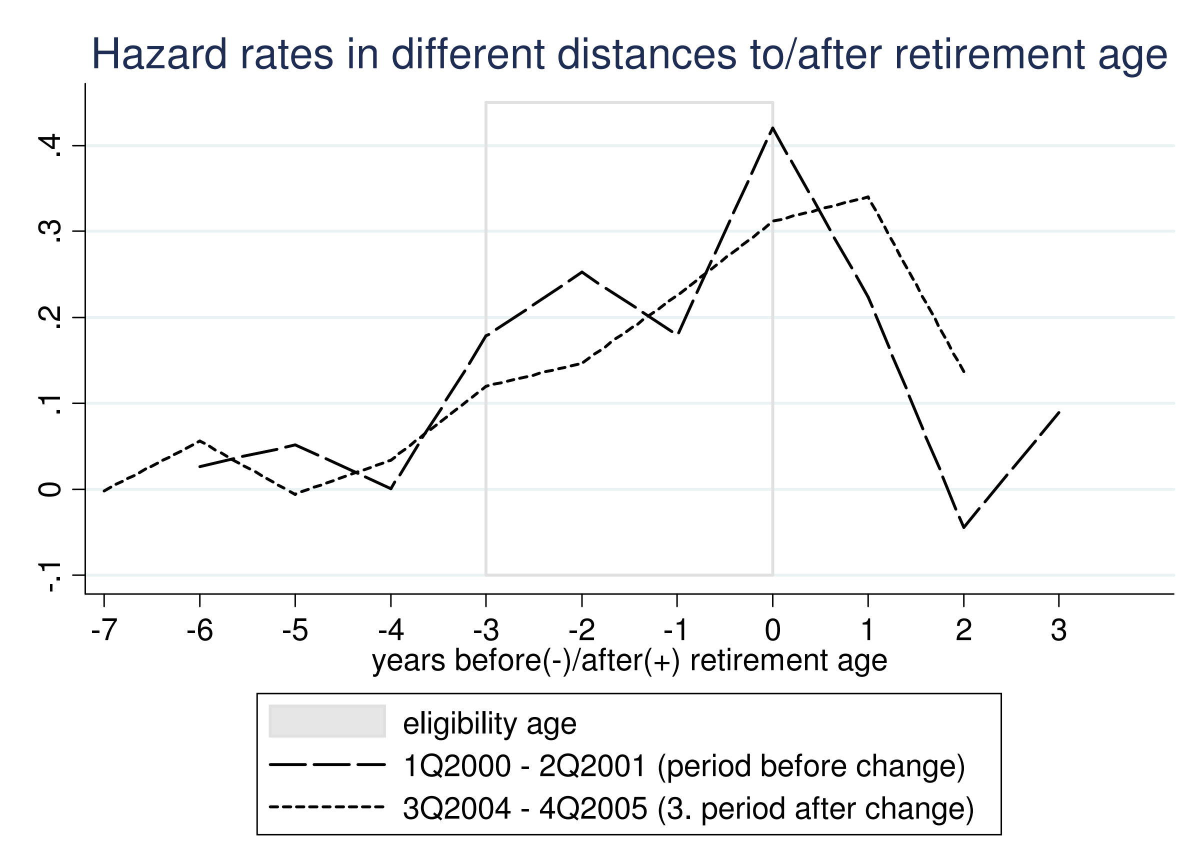
Figure 3



Source: Labor Force Survey, own calculation

We also present an alternative indicator – the hazard rate – representing the probability of labor force withdrawal due to retirement. Figure 4 depicts the hazard rates for two periods: before and 3 - 4.5 years after the policy change. In the cross-sectional setting, the definition of the hazard rate is one minus the retention rate, which is the participation rate of workers at age t divided by the participation rate of workers aged t-1 in the given year (Hurt, 1996).

Figure 4

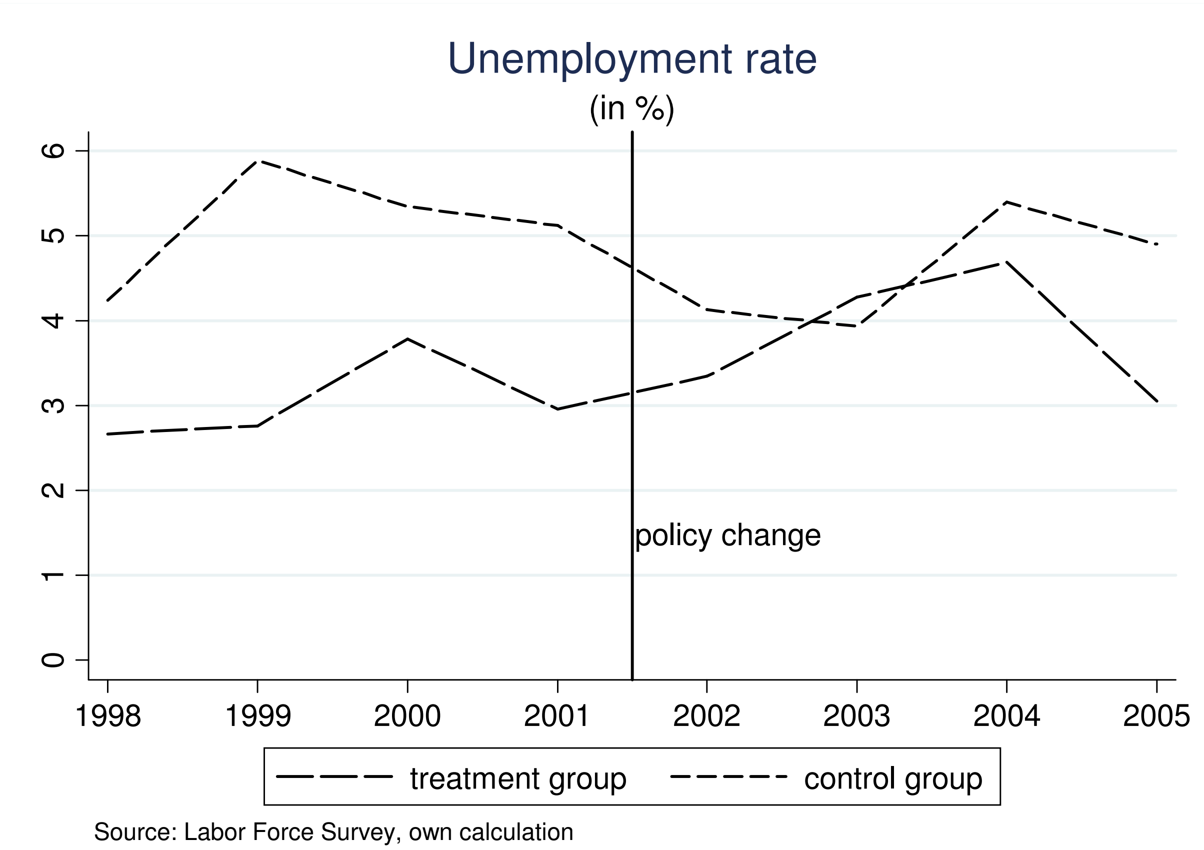


Source: Labor Force Survey, own calculation

The line representing the period before the policy change has two peaks: the first one (around -2, two to three years before the statutory retirement age) reflects entering early retirement before the policy change, while the second (around 0) represents entering standard retirement. The line for the period three years after the policy change shows a substantial change in the behavior of retirees. One can see the hazard rate smoothed over the number of years before/after retirement. Although early retirement frequently occurs, one cannot observe any particular peak before the statutory retirement age in the period starting with the third quarter of 2004. This is most probably an effect of the treatment we study. One can also see that it is also more common to retire after the statutory retirement age. This is in line with the hypothesis that workers generally stay longer in their jobs.

We also consider the problem of unemployment, which can potentially change over time and therefore raise questions about our results. Figure 5 shows the development of the unemployment rate over time. Unemployment rate is defined for each group separately so that we can control for the changes in labor force in particular group. The trend in unemployment is not clear, despite an upward movement of unemployment in the treatment group right after the policy change. However, one needs to be aware that the number of unemployed individuals in our sample is relatively small and this change is most probably not statistically significant. Moreover, the dynamics of the increase is slower when we calculate the unemployment rate using the labor force across the groups.

Figure 5



Source: Labor Force Survey, own calculation

This graphical overview suggests that our treatment group was hit by an external shock around the year 2001 which influenced its participation in the labor market. We believe that this shock was with high probability the change in the early retirement setting. This is, of course, not a rigorous analysis, because we cannot say whether the shift in participation in the labor market is statistically significant. The next sections thus provide a formal econometric analysis and computation of the increase in the probability of staying in the labor force.

5. Methodology of Econometric Analysis

As an identification strategy we use difference-in-differences (Baker and Benjamin, 1999). The treatment group includes workers who are eligible for early retirement benefits (at most three years before the actual statutory retirement age). The control group contains workers between 6-3 years to the statutory retirement age. The time periods chosen for the estimation are the following: 1.5 years before the policy change and 4.5 years after the policy change, divided into three periods of equal length. The increase in the total number of early retirement benefits was dramatic in the late 1990s. We do not want to mix the previous changes in the social security system into our analysis, so we use only one period before the policy as a benchmark for our analysis. The basic specification is the following:

$$
\begin{aligned}
y_{it} &= \alpha_i + \beta_1 OLD_{it} + \beta_2 AFTER1_{it} + \beta_3 AFTER2_{it} + \beta_4 AFTER3_{it} + \beta_5 (OLD_{it} * AFTER1_{it}) \\
&+ \beta_6 (OLD_{it} * AFTER2_{it}) + \beta_7 (OLD_{it} * AFTER3_{it}) + X_{it}\beta_8 + \varepsilon_{it},
\end{aligned}
$$

where y_{it} is one if an individual *i* is inactive (out of the labor force) at time *t* and zero when an individual is active in the same period. OLD_{it} is a dummy for the treatment group. $AFTER1_{it}$, $AFTER2_{it}$ and $AFTER3_{it}$ are dummy variables for the three consecutive periods (1.5 years long) after the policy change. The period before the policy change is defined as 1.5 years before the policy change became effective. X_{it} is the vector of observable individual characteristics (basic demographic characteristics: education, number of people in the household, marital status, geographical location) and ε_{it} is the error term. This model is estimated by a probit model with the standard maximum likelihood estimation technique.

The estimated coefficient β_1 captures all differences between the treatment and control groups that are unrelated to the policy change. β_2, β_3 and β_4 capture all the period-specific changes that influence the probability of being employed for the control and treatment groups. β_5, β_6 and β_7 are the coefficients of interest. They reflect the impact of the policy change on the inactivity of the treatment group relative to the control group. The vector of coefficients β_8 captures the influence of major demographic characteristics.

6. Results

Our final sample contains 50,152 observations, 26,735 from the treatment group and 23,417 from the control group. The estimated coefficients indicate that the treatment significantly increased the labor supply of the treatment group. The coefficients have the expected sign; however, the first period after the change does not have a significant impact on the labor supply. The reason is that our treatment group also contains people who entered early retirement under the previous system. Therefore, the pass-through to the participation rate of the treatment group is lagged and becomes visible only in periods $AFTER2$ and $AFTER3$. β_5 is not significant in our specification, and β_6 together with β_7 are negative and significant. After controlling for other observable characteristics, the results change mainly in the significance of the coefficients. The other controls are significant with the expected signs: higher education decreases the probability of being inactive. The number of household members has the same effect. We do not include the labor market status of spouses, because the labor market activity of spouses can also potentially be affected by the reform and thus it is an endogenous variable. To reveal the magnitude of the estimated effects – the impact on the probability – the marginal effects are presented in Table 4.

Tab. 4: Estimated coefficients from the probit model in three different specifications

Model	(1)	(2)	(3)
OLD*AFTER1	**-0.0159**	**-0.0108**	**-0.0096**
	(0.0180)	(0.0182)	(0.0182)
OLD*AFTER2	**-0.0509*** **	**-0.0340***	**-0.0318***
	(0.0179)	(0.0184)	(0.0184)
OLD*AFTER3	**-0.0457****	**-0.0354***	**-0.0317**
	(0.0187)	(0.0189)	(0.0191)
Personal characteristics		X	X
District dummies			X
N	50,152	50,152	50,152
Pseudo R-squared	0.07	0.10	0.14

Note: Coefficients are recalculated into the probability measure (min 0, max 1). The excluded variables are dummies for: control group, one period before policy change, interaction of control group and all periods. Full results are presented in Annex 5. Standard errors are in parentheses. We also performed linear probability estimation with OLS and it does not change the significance of the results.
*** p<0.01, ** p<0.05, * p<0.1

We estimated three different specifications. The most extended version contains individual characteristics and 76 dummies for districts. In all models this effect remains negative. The marginal effect of the reform on the probability of being inactive is close to -0.03, which can be interpreted as a 3% drop in the probability of being inactive for workers who are at most three years before the statutory retirement age. These results show that inactivity significantly decreased in the treatment group during 2003–2005 relative to the control group and the period before. Our results also show that there is no significant effect of the policy change in the period immediately after the policy change. This is probably due to the fact that the left-hand-side variable is a stock (the probability of being inactive) and thus the treatment group in the first period after the policy change contains a lot of individuals who entered early retirement before the policy change.

We are also aware of the problem with expectations, which might have influenced the behavior of people right before the reform became effective. In our case it would mean that people entered early retirement earlier just because the policy change occurred. This fact would bias our results. We cannot fully account for this phenomenon owing to data limitations. Thus, we did a robustness check and skipped the first half of 2001, since the law introducing the reform was passed in the Czech parliament at the beginning of 2001 and became effective in July 2001. We thus shorten the baseline period to one year. The results are summarized in Table 5 and suggest that even in this setting the reform decreased the inactivity rate among older workers. The size of this effect is, however, smaller and in specifications (2) and (3) the significance has vanished. However, the result for specification (1) could be considered as the lower bound of the estimated effect, because those people who reacted purely to the announcement of the reform would probably have entered early retirement later on if they behave rationally.

Tab. 5: Estimated coefficients from the probit model in three different specifications without the first half of 2001

Model	(1)	(2)	(3)
OLD*AFTER1	**-0.00**	**0.003**	**0.003**
	(0.0209)	(0.0211)	(.02104)
OLD*AFTER2	**-0.036***	**-0.020**	**-0.019**
	(0.0196)	(0.0201)	(0.0201)
OLD*AFTER3	**-0.030**	**-0.021**	**-0.019**
	(0.0204)	(0.0206)	(0.0207)
Personal characteristics		X	X
District dummies			X
N	46,127	46,127	46,127
Pseudo R-squared	0.06	0.11	0.13

Note: Coefficients are recalculated into the probability measure (min 0, max 1). The excluded variables are dummies for: control group, one period before policy change, interaction of control group and all periods. Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

The dummies that represent geographical location show high variation in labor market behavior across different regions in the Czech Republic. For example, individuals from the

Karvina region (border region to Poland strongly affected by the structural changes after fall of socialism) have a 40% higher chance of being inactive compared to individuals from Prague, even after controlling for all possible observable characteristics.

Our results show that the probability of being inactive (out of the labor force) has decreased since the reform came into force. This means that people have not started to leave the labor force by using other social programs (e.g. disability pensions), but this leaves the possibility of becoming unemployed and so this policy change might still have a negative impact on the fiscal position. Therefore, we decided to run the same probit specification but with the indicator variable of being employed. The results, available in Annex 7, are quite similar to those obtained earlier.

The Annex 8 presents additional robustness check and further extension of our analysis. We divided the control and treatment into the three smaller fractions of the length of one year. Further we also explore other labor statuses - employed and unemployed. The control group is considered only those who are 3-4 years before retirement. The results show that the reform was really efficient for increasing activity on the labor market. However, we also see that unemployment was also temporally increased. The employment is increased, but results are not significant. This will be explored further in the next version of this paper.

One more approach to check the robustness of our results is reported in Annex 9. This Annex reports results for the multinomial logit in which we compare the relative risks between the three basic statuses on the labor market – employed, unemployed and inactive. We find that for the treatment group in second and third period after the policy change the risk to be employed is higher than inactive. However we find the temporal effect for the unemployment as well which is even robust for all three equation specification.

We also attempted to use an explanatory variable that indicates change in labor market status. However, as we mentioned earlier, we face a problem with the lack of observations for people who change status during the period they were surveyed (i.e., four or five quarters). We divided our time span into two periods: two years before the reform and two years after the reform. We observed only a few changes in labor market status for the treatment group: 172 out of 2,541 individuals for the two years before the policy change and 113 out of 2,587 after the policy change. We can conclude that these numbers are in line with our hypothesis that the reduction in early retirement benefits caused fewer workers to enter early retirement. However, the number of observations in our sample does not allow any formal econometric analysis in this setting.

7. Conclusions and Policy Implications

Our results confirm that the 2–3% cut in early retirement benefits due to the 2001 reform boosted the labor participation of males eligible for early retirement by approximately the same amount. The reform increased the probability of being employed in the three-year period before a worker reaches the statutory standard retirement age. These results show that the elasticity of the extensive margin of labor supply of older Czech workers is relatively high, although we are not able to calculate the exact value because we lack individual data on wages. Nevertheless the policy change was not purely fiscal improving since some of the affected people did not continue to work, but rather switched to unemployment as a substitute to early retirement.

Our findings are generally in line with those, for example, from Germany, where Börsch-Supan (2000) found a high sensitivity of older workers' employment to the social security system design. Our results also correspond with Galuščák (2002), who found a substantially high sensitivity of the participation rate to change in the earnings test for workers older than the statutory retirement age. In this respect, our results are not fully comparable, because we examine older workers who are eligible for early retirement and have not reached the statutory retirement age.

In our approach, we assume that the difference in the labor supply between older and younger cohorts was not affected by any other shock than the policy change. This is the only possible way of empirically testing a public policy intervention affecting the whole population of one country.

The extent of our analyses is also limited by data availability. The dataset contains important characteristics about the retirement of males and – on top of that – it does not contain wages. Therefore, our analysis does not cover the labor supply of females and we do not directly estimate the elasticity of the labor supply to the individual budget constraint. Our results also indicate high differences of labor supply behavior across males with different characteristics (education, geographic location). This could be the subject of additional research.

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Annex 1: **Social security formulae**

$$P = BA + PA$$

$$PA = CB \cdot AP$$

$$CB = PAB \cdot rp_1 + \max(0, PAB - rl_1) \cdot (rp_1 - rp_2) + \max(0, PAB - rl_2) \cdot (rp_2 - rp_3)$$

$$AP = \mathrm{int}((IP_1 + IP_2 \cdot 0{,}8)/365) \cdot (PYI - 90per \cdot rPYI)$$

$$PAB = \frac{\sum_{y=Y-1-\min(30,Y-1-1986)}^{Y-1} AAB_y}{\min(30, Y-1-1986) - EP/365}$$

$$AAB_y = AB_y \cdot CGGAB_y$$

$$CGGAB_y = \frac{GAB_{2004} \cdot CvC_{2004}}{GAB_y}$$

P – pension benefit
BA – basic amount
PA – percentage-based assessment
CB – calculation base
AP – adjustment percentage
PAB – personal assessment base
rp_1 = 100%, **rp_2** = 30%, **rp_3** = 10% – reduction percentage
rl_j = first and second reduction limit in yearly terms
IP_j, j = 1, 2 – insured period (j = 1) and compensatory insured period (j = 2) counted as 80% of the length before reaching the age of 18 (only whole 365 days are included)
PYI – percentage for each year of insurance (1.5%)
90per – number of 90-day periods
rPYI – reduced percentage for each 90-day period of early retirement **(subject of policy change)**
AAB – annual assessment base
EP – excluded period
AB – assessment base
CGGAB – coefficient of the growth of the general assessment base
GAB – general assessment base
CvC – conversion coefficient
Y – year

Annex 2:

Monetary incentives before and after the reform (low wage earner)

Last age of work	Replacement rate – before	Replacement rate – after	SSW – before	SSW – after	Accrual rate – before	Accrual rate – after
58	1.074	1.062	648,662	640,970	-0.005	-0.005
59	1.118	1.110	606,802	601,781	-0.069	-0.065
60	1.165	1.160	561,769	559,087	-0.080	-0.076
61	1.203	1.203	515,824	515,214	-0.089	-0.085
62	1.247	1.247	469,394	468,659	-0.099	-0.099
63	1.304	1.304	428,142	427,275	-0.096	-0.097
64	1.336	1.336	377,901	376,903	-0.133	-0.134
65	1.429	1.429	347,119	345,990	-0.089	-0.089

Note: SSW – social security wealth – is defined as the sum of all discounted pension benefits and social security contributions. The accrual rate is defined as the relative year-to-year change in SSW.

Monetary incentives before and after the reform (high wage earner)

Last age of work	Replacement rate – before	Replacement rate – after	SSW – before	SSW – after	Accrual rate – before	Accrual rate – after
58	0.658	0.650	783,855	773,719	-0.005	-0.005
59	0.682	0.676	722,477	715,687	-0.069	-0.065
60	0.710	0.707	660,765	656,923	-0.080	-0.076
61	0.731	0.731	595,214	593,909	-0.077	-0.074
62	0.746	0.746	522,766	521,190	-0.089	-0.089
63	0.788	0.788	473,132	471,275	-0.087	-0.088
64	0.809	0.809	407,865	405,728	-0.123	-0.124
65	0.858	0.858	360,896	358,476	-0.085	-0.086

Note: SSW – social security wealth – is defined as the sum of all discounted pension benefits and social security contributions. The accrual rate is defined as the relative year-to-year change in SSW.

Annex 3:

Forward-looking social security incentives – Option Value

Ret. age	Before the change				After the change			
	low wage	avg. wage	high wage	SD	low wage	avg. wage	high wage	SD
56	10,840	14,364	20,048	3,794	10,837	14,342	20,571	4,025
57	4,491	6,136	8,970	1,850	4,490	6,126	9,503	2,087
58	0	0	343	162	0	0	883	416
59	1,777	912	0	726	1,462	566	156	545
60	4,117	2,366	178	1,612	3,533	1,734	0	1,442
61	6,752	4,334	1,190	2,277	5,932	3,441	728	2,125
62	9,615	6,862	3,295	2,587	8,795	5,969	2,834	2,435
63	12,107	8,449	3,538	3,511	11,287	7,556	3,076	3,357
64	15,670	11,426	5,680	4,094	14,850	10,532	5,218	3,939
65	17,407	12,743	6,410	4,507	16,587	11,850	5,948	4,352

Note: SD stands for standard deviation.

Forward-looking social security incentives – Peak Value

Ret. age	Before the change				After the change			
	low wage	avg. wage	high wage	SD	low wage	avg. wage	high wage	SD
56	-3,313	-4,652	-6,978	1,514	-3,432	-4,917	-7,327	1,605
57	-3,174	-4,574	-6,909	1,541	-3,389	-4,935	-7,356	1,633
58	-41,860	-49,189	-61,378	8,050	-39,189	-46,229	-58,032	7,774
59	-45,032	-51,238	-61,712	6,883	-42,694	-48,747	-58,765	6,627
60	-45,945	-53,334	-65,551	8,084	-43,872	-51,011	-63,014	7,898
61	-46,429	-56,170	-72,448	10,733	-46,556	-56,351	-72,719	10,793
62	-41,252	-44,409	-49,634	3,456	-41,383	-44,597	-49,915	3,518
63	-50,241	-55,864	-65,267	6,199	-50,372	-56,050	-65,547	6,260
64	-30,782	-36,873	-46,970	6,676	-30,914	-37,061	-47,252	6,738
65	-40,928	-46,833	-56,750	6,528	-41,061	-47,024	-57,036	6,591

Note: SD stands for standard deviation.

Annex 4:
Descriptive statistics

variable	control group				treatment group			
	Mean	Std. Dev.	Min.	Max.	Mean	Std. Dev.	Min.	Max.
inactivity status	0.17	0.38	0	1	0.42	0.49	0	1
elementary educ.	0.09	0.29	0	1	0.12	0.32	0	1
apprenticeship	0.54	0.50	0	1	0.50	0.50	0	1
high school educ.	0.24	0.43	0	1	0.25	0.43	0	1
lower tertiary educ.	0.01	0.10	0	1	0.01	0.09	0	1
upper tertiary educ.	0.11	0.32	0	1	0.12	0.32	0	1
unmarried	0.04	0.21	0	1	0.04	0.20	0	1
married	0.84	0.37	0	1	0.84	0.37	0	1
widowed	0.04	0.20	0	1	0.05	0.22	0	1
divorced	0.07	0.26	0	1	0.07	0.26	0	1
before the policy change	0.22	0.42	0	1	0.25	0.43	0	1
1-1.5 year after the policy change	0.24	0.43	0	1	0.26	0.44	0	1
1.5 - 3 years after the policy change	0.28	0.45	0	1	0.26	0.44	0	1
3 - 4.5 years after the policy change	0.26	0.44	0	1	0.23	0.42	0	1
number of household members	2.60	1.07	1	11	2.41	0.97	1	10
age	56.90	0.94	55.0	58.8	59.72	0.78	58.25	62.25

Annex 5:

Econometric results of the full baseline model

	Model 1	Model 2	Model 3
eligible age (old)	0.281***	0.275***	0.274***
	(0.0145)	(0.0146)	(0.0147)
1-1.5 year after the policy change (after1)	-0.0234*	-0.0180	-0.0205
	(0.0135)	(0.0136)	(0.0136)
1.5 - 3 years after the policy change (after2)	-0.0135	-0.0110	-0.0106
	(0.0143)	(0.0144)	(0.0144)
3 - 4.5 years after the policy change (after3)	-0.0223	-0.0193	-0.0223
	(0.0146)	(0.0147)	(0.0146)
interaction variable (oldxafter1)	-0.0159	-0.0108	-0.00922
	(0.0180)	(0.0182)	(0.0182)
interaction variable (oldxafter2)	-0.0509***	-0.0340*	-0.0318*
	(0.0179)	(0.0184)	(0.0184)
interaction variable (oldxafter3)	-0.0457**	-0.0354*	-0.0317
	(0.0187)	(0.0189)	(0.0191)
apprenticeship		-0.125***	-0.131***
		(0.0130)	(0.0131)
high school educ.		-0.191***	-0.188***
		(0.0108)	(0.0109)
lower tertiary educ.		-0.162***	-0.161***
		(0.0237)	(0.0224)
upper tertiary educ.		-0.250***	-0.243***
		(0.0076)	(0.0077)
unmarried		0.109***	0.118***
		(0.0228)	(0.0231)
widowed		0.0454**	0.0479**
		(0.0199)	(0.0199)
divorced		0.0377**	0.0369**
		(0.0171)	(0.0172)
number of household members		-0.0157***	-0.0161***
		(0.0045)	(0.0046)
76 districts (not reported)			X
Observations	50,152	50,152	50,152
Pseudo R-squared	0.07	0.11	0.14

Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Annex 6:

Estimated coefficients from the probit model in three different specifications (yearly periods)

Model	(1)	(2)	(3)
OLD*AFTER1	**-0.006**	**-0.003**	**-0.001**
	(0.019)	(0.019)	(0.020)
OLD*AFTER2	**-0.069***	**-0.057***	**-0.056***
	(0.020)	(0.020)	(0.020)
OLD*AFTER3	**-0.062***	**-0.043**	**-0.036*
	(0.019)	(0.020)	(0.002)
Personal characteristics		X	X
District dummies			X
N	33,842	33,842	33,842
Pseudo R-squared	0.07	0.11	0.14

Note: Coefficients are recalculated into the probability measure (min 0, max 1). The excluded variables are dummies for: control group, one period before policy change, interaction of control group and all periods.. Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Annex 7:

Estimated coefficients from the probit model in three different specifications (dependent variable – being employed)

Model	(1)	(2)	(3)
OLD*AFTER1	**0.011**	**0.005**	**0.004**
	(0.018)	(0.019)	(0.0193)
OLD*AFTER2	**0.042****	**0.022**	**0.019**
	(0.0192)	(0.0198)	(0.0199)
OLD*AFTER3	**0.046****	**0.035***	**0.031**
	(0.019)	(0.020)	(0.020)
Personal characteristics		X	X
District dummies			X
N	50,152	50,152	50,152
Pseudo R-squared	0.05	0.10	0.13

Note: Coefficients are recalculated into the probability measure (min 0, max 1). The excluded variables are dummies for: control group, one period before policy change, interaction of control group and all periods. Standard errors are in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Annex 8:

Alternative specification with narrow treatment and control group (one-year age window)

Marginal effects probit

Variables	Active	Employed	Unemployed
OLD1_after1	**-0.000**	**0.011**	**0.009**
	(0.028)	(0.030)	(0.014)
OLD1_after2	**0.072*** **	**0.045**	**0.038** **
	(0.023)	(0.027)	(0.020)
OLD1_after3	**0.053** **	**0.040**	**0.013**
	(0.024)	(0.027)	(0.015)
OLD2_after1	**0.030**	**0.024**	**0.002**
	(0.028)	(0.030)	(0.015)
OLD2_after2	**0.079*** **	**0.062** **	**0.034***
	(0.025)	(0.029)	(0.025)
OLD2_after3	**0.081*** **	**0.078** **	**0.008**
	(0.026)	(0.029)	(0.018)
OLD3_after1	**0.010**	**-0.019**	**0.003**
	(0.031)	(0.033)	(0.019)
OLD3_ after2	**0.069** **	**0.051**	**0.042**
	(0.027)	(0.031)	(0.035)
OLD3_ after3	**0.041**	**0.031**	**0.034**
	(0.031)	(0.033)	(0.035)

Note: Coefficients are recalculated into the probability measure (min 0, max 1). Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Linear regression

Policy variables	Inactive	Employed	Unemployed
OLD1_after1	**-0.007**	**-0.005**	**0.014**
	(0.027)	(0.028)	(0.015)
OLD1_after2	**-0.080*** **	**0.047***	**0.038** **
	(0.0260)	(0.0272)	(0.015)
OLD1_after3	**-0.060***	**0.045***	**0.015**
	(0.026)	(0.027)	(0.016)
OLD2_after1	**-0.044**	**0.035**	**0.006**
	(0.029)	(0.030)	(0.015)
OLD2_after2	**-0.098*** **	**0.074** **	**0.030***
	(0.032)	(0.032)	(0.016)
OLD2_after3	**-0.1056*** **	**0.0938*** **	**0.010**
	(0.0315)	(0.0323)	(0.016)
OLD3_after1	**0.007**	**-0.015**	**0.011**
	(0.030)	(0.0305)	(0.017)
OLD3_ after2	**-0.081** **	**0.057***	**0.034***
	(0.032)	(0.0331)	(0.019)
OLD3_ after3	**-0.051**	**0.037**	**0.025**
	(0.0336)	(0.034)	(0.020)

Note: *** p<0.01, ** p<0.05, * p<0.1 Control group is defined by the distance 3-4 years before retirement. The other control variables are year, control and treatment group fixed effects are family status, education and districts.

Annex 9:

Estimated coefficients from the multinomial logit

Model	(1)	(2)	(3)
employed			
OLD*AFTER1	**1.060**	**1.028**	**1.022**
	(0.103)	(0.104)	(0.104)
OLD*AFTER2	**1.266****	**1.153**	**1.135**
	(0.131)	(0.122)	(0.122)
OLD*AFTER3	**1.243****	**1.187**	**1.154**
	(0.133)	(0.130)	(0.129)
unemployed			
OLD*AFTER1	**1.199**	**1.192**	**1.194**
	(0.307)	(0.3062)	(0.3096)
OLD*AFTER2	**1.962*****	**1.915****	**1.937*****
	(0.499)	(0.4888)	(0.4978)
OLD*AFTER3	**1.354**	**1.3369**	**1.327**
	(0.372)	(0.367)	(0.367)
Personal characteristics		X	X
District dummies			X
N	50,152	50,152	50,152
Pseudo R-squared	0.06	0.10	0.13

Note: Coefficients are presented in relative risks. The base outcome is being inactive. Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1