

Referee Report
“Essays on Social Interactions and Policy Evaluation”
by Filip Pertold

Paper 1: “Sorting into Secondary Education and Peer Effects in Youth Smoking”

This paper investigates whether the smoking behavior secondary school students is affected by peers behaviors. The author propose a new identification strategy based on the re-sorting of peers between elementary and secondary education in the Czech Republic. The research is motivated by the fact that “from a public policy perspective the existence of social interactions is important, because the determine the efficiency of government policies designed to affect youth behavior”.

The literature so far has rely on within-school variation in peers smoking and on IV approach. Several contributions have tried to deal with what Manski defines endogenous, contextual and correlated effects. I think that this contribution makes an advance and tries to propose an alternative solution to the identification of peers effects in risky behavior. However, the author misses to provide convincing evidence about the goodness of his identification strategy, thus I think that the paper would benefit from a better discussion of the empirical strategy.

My suggestions follow.

- 1) **Re-sorting of students between elementary and secondary school:** the re-sorting of students between elementary and secondary school represents the crucial aspect for identification purposes. In fact, the author defining the enrollment into secondary schools as a natural experiment that assigns students to new peer groups exploits this characteristic of the Czech school system in order to solve the reflection problem and address the identification issues. The identification strategy is very robust, but the main weakness derives from the absence of a clear evidence about the randomness in the assignment in the secondary school. Even if I am sympathetic with the approach used by the author, I would appreciate a more detailed discussion about the selection process and some compelling evidence in favor of author's statement. The crucial point in peers effects identification is that current peers could not affect each others' behavior before they were enrolled in secondary school. The author suggests that “it is realistic to assume that the majority of classmates cannot have influenced each other before their enrollment into a secondary school” and in Section 3 provides a description of the Czech schooling system. Unfortunately, his discussion does not provide any evidence in support of his analysis. In fact, he states that: i) classes at primary schools are generally heterogenous in skill distribution and pupils from one class apply to different secondary schools; ii) secondly, families do not move into new neighborhoods based on the quality of secondary schools...These assumption 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. Is the author able to present any figure to support his conjecture? It exists a wide literature on the value parents place on school quality (i.e. Black, QJE, 1999). Moreover, the

presumption that pupils from one class apply to different secondary schools is not proved. Under the assumption that families mobility is low, we should expect that elementary students will apply in secondary school located in the same neighborhood where they live or in very close neighborhoods. Moreover, it is very reasonable to assume that elementary schools are located in the same neighborhood where they live and thus we should expect it is very likely that elementary school students may end up to enrolled into the same secondary school. In many European Countries elementary students may ask to belong to the same secondary class of their elementary classmates. For these reasons I would expect the author to provide enough evidence to rule out the possibility that these channels are at work in the Czech Republic.

- 2) **Males vs Female:** the author proposes a separate analysis by gender. I tend to be very skeptical about this approach. In fact, I would prefer to see the full-sample estimates and then estimates by gender testing for eventual differences in males and females behavior. I think this approach cleaner and does not impose any a priori hypothesis on males and females behavior.
- 3) **IV approach:** I personally prefer the IV approach to the first part of the paper. I think that the IV approach allows to better contextualize the contribution of the paper. IV estimates show that the bias goes in opposite direction for males and females. Are there any reasons to expect this difference? I would suggest the author to present F-statistic for weak instrument.
- 4) **Conclusions:** The paper is very weak in the concluding part which is basically a summary and doesn't contain real conclusions. In terms of policy conclusions the paper would require a lot of work.

Minor Comments

- 1) Table must be self-explaining
- 2) Check authors' surname: for example, Gaviria and not Gaviar.

Paper 2: “Don't Drink and...Avoid Risky Sex of Your Peers: The Influence of Alcohol Consumption of Opposite-Gender Peers on Youth Risky Sexual Behavior”

This paper investigates the role of opposite-gender peers' drinking in the risky sexual behavior among nearly 18 years old Czech secondary school students, where peers are defined as classmates. The research is motivated by the fact that “the Czech Republic is generally an important case to study mainly because of the low proportion of condoms used during first sexual intercourse”. The author

This paper contributes to the increasing literature on risky behaviors and in particular, on the causal effect of alcohol consumption on risky sexual behavior. The author presents and discusses many previous contributions, but he ignores two relevant papers by Jeff DeSimone: "Binge Drinking and Risky Sex among College Students," (2010) NBER Working Papers and "Binge Drinking & Sex in High School," (2010) NBER Working Papers 16132.

The paper devotes great attention to the empirical and methodological part, but it is very weak and inadequate in his motivation and theoretical part. The author does not provide any theoretical motivation why we should expect that opposite-gender peers' drinking may have an effect on risky sexual behavior. He cites Waddell (2010) who “suggests that it may not just be own drinking that influences own sexual behavior, but also opposite-gender peers' drinking”. This aspect seems more relevant for identification purposes. In fact, “In case of this relationship, there is less doubt about the way the causality goes”. I think that the paper would benefit from a more detailed motivating section.

I am skeptical about some aspects of the identification strategy used:

- The author uses younger schoolmates' sexual behavior in order to capture school-specific level of sexual behavior. I would believe more to some class-specific control. In fact, I think that a relevant role in student education is played by the teacher.
- As widely described in the preceding referee report I think that the author misses to provide convincing evidence about the goodness of his identification strategy, thus I think that the paper would benefit from a better discussion of the empirical strategy.
- The author claims that sexual behavior is very different between males and females and that peer effects might work differently for males and females. I could agree, but this should be tested and not assumed a priori.
- the paper is very weak in the concluding part which is basically a summary and doesn't contain real conclusions. In terms of policy conclusions the paper would require a lot of work.

Paper 3: The Impact of Early Retirement Incentives on Labor Market Participation: Evidence from a Parametric Change in the Czech Republic

This paper investigates the impact of a change in the Czech early retirement scheme on the labor force participation of older male workers. The authors exploit the change due to the law and by using a diff-in-diff method they show that labor force participation reacts to the reduction in early retirement benefits.

The paper contributes to the vast literature on pension scheme and the effects of the changes in retirement benefits on labor market. Thus, the main contribution of the paper is to analyze in depth the characteristics of the Czech labor market. The authors exploit the policy change that became effective in July 2001 and cut early retirement benefits by approximately 3% for new claimants.

The is well structured and it provides some compelling evidence about the policy change. However there are some aspects that would benefit from a more detailed and extensive discussion:

- Page 74. “ 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”. This paragraph is very difficult to understand for a non Czech reader. I do believe that the authors choose the period window in a correct way, but they should better discuss these aspect that are crucial for their identification.
- Annex 4 (Descriptive statistics). I strongly suggest the authors to test for the difference in sample means. Their identification strategy rely on the fact that the treated individuals and the individual belonging to the control group do not differ apart from the policy change. Thus, the authors should provide evidence that the treated and the controlled have the same observable characteristics.
- Why do the authors focus their analysis only on males? Are there compelling reasons for this choice?
- Was the policy change expected? In this case we could expect that many individuals could have anticipated their retirement choice. I think that the authors should provide a discussion on this aspect in order to rule out any other possible interpretation of their results.