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Cross Country Estimates of Peer Effects in Adolescent Smoking Using IV and School Fixed Effects

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Abstract

This paper presents micro-econometric evidence on peer effects in adolescent smoking between classmates aged 15/16 years across 13 European countries. Both instrumental variables and school fixed effects are used for identification. Omitting school fixed effects, as in some existing IV studies of peer effects, is shown to lead to substantial overestimates consistent with endogenous sorting into schools. When fixed effects are included, estimated peer effects range from 0.04 to 0.34 depending on the instrument set. The preferred estimate uses the smoking behaviour of peers' older siblings to instrument for peer smoking behaviour and suggests a statistically insignificant peer effect of 0.16. This estimate is robust to restricting the sample by dropping schools that non-randomly sort pupils into classes. Ultimately, we cannot rule out zero peer effects in smoking between adolescent classmates in Europe.

JEL classification: I00, Z13

Keywords: Peer effects, social interactions, smoking, tobacco, adolescents, fixed effects, endogenous sorting

1. Introduction

Research that improves our understanding of why adolescents smoke tobacco can help effective policy design to reduce the prevalence of these behaviours among young people. This matters because tobacco smoking is the largest single cause of avoidable death in the EU (European Commission, 2010). Furthermore, most smokers begin to do so in adolescence, with adolescent smoking a strong predictor of smoking in adult life (e.g. see Merline et al., 2004).

It is widely believed that one of the key factors influencing whether an adolescent smokes is the smoking behaviour of his or her peers. But such peer effects – the causal links between peer behaviour and individual behaviour that Manski (2000) calls *endogenous social interactions* – are notoriously hard to quantify. And despite a growing literature that seeks to estimate this causal link in adolescent smoking between school classmates or 'grade-mates' using instrumental variables (IV) methods, the evidence remains inconclusive. One reason is that the most common IV approach – using peer characteristics and/or peer family background to instrument for peer behaviour, as followed by Gaviria and Raphael (2001), Powell et al. (2005) and others – relies in part on some questionable assumptions, including the assumption that adolescents are sorted randomly into schools.

More recently, a handful of studies have attempted to control for endogenous sorting by including school or school grade fixed effects alongside IV (see Lundborg, 2006; Sen, 2009; Fletcher, 2010). These studies all find large, positive and statistically significant peer effects, although these estimates are smaller in magnitude than the equivalent estimates omitting school fixed effects. Although critics can always point to remaining doubts about instrument validity in these studies, e.g. because of endogenous sorting *within* schools or school grades, collectively they provide the most credible IV estimates of peer effects in adolescent smoking to date. Clark and Lohéac (2007) take a different approach, using lagged peer behaviour in place of IV, again alongside school fixed effects, and suggest only small and marginally significant peer effects. But critics of this approach can point to the arbitrary lag structure imposed together with concerns about correlation between lagged and current peer behaviour (Fletcher, 2010).

This paper estimates peer effects in adolescent smoking using a rich micro data set drawn from 13 European countries. The standard IV approach of Gaviria and Raphael (2001) is followed, but augmented to include school fixed effects along the lines of Lundborg (2006), Sen (2009) and Fletcher (2010). Sensitivity to using different sets of instruments in this IV-FE approach is then explored. Finally, sensitivity to restricting the sample by dropping schools that might endogenously sort pupils into classes is examined. These sensitivity analyses, the broader coverage of the data, and ultimately the estimation results themselves, enable the paper to make a contribution additional to that of Lundborg (2006), Sen (2009) and Fletcher (2010).

The key results are as follows. First, omitting school fixed effects can lead to substantial overestimates of peer effects, consistent with endogenous sorting into schools. Second, peer effects estimates are sensitive to instrument choice, even with school fixed effects. The preferred estimates presented here – using the most convincing of the potentially available instruments – suggest peer effects that are smaller in magnitude than those found by Lundborg (2006), Sen (2009) and Fletcher (2010), and that are statistically insignificant at standard levels. Third, these estimates don't change in magnitude when the sample is restricted to schools that plausibly sort pupils randomly into classes. Ultimately, zero peer effects in adolescent smoking between classmates cannot be ruled out. The paper's main conclusion is therefore closer to that of Clark and Lohéac (2007) than it is to existing IV-fixed effects studies in the literature.

The remainder of this paper is set out as follows. The following section provides a brief literature review. Section 3 sets out this paper's empirical approach in the light of the well known challenges involved in identifying endogenous social interactions. Section 4 describes the data, drawn from the 2007 European Schools Survey Project on Alcohol and Other Drugs (ESPAD). Section 5 presents the estimates of peer effects and discusses their sensitivity to instrument choice, to including school fixed effects, and to restricting the sample to drop schools most likely to sort endogenously within schools. Section 6 concludes.

2. Existing Estimates

There are several studies that examine the impact of peer behaviour on adolescent use of tobacco where the school grade or school class is treated as the reference group. In most cases the outcome variable of interest is whether the individual has used tobacco in the last 30 days. Given a lack of experimental evidence, the discussion here is restricted to those estimates that use IV or longitudinal data methods to try to identify the causal impact of peer smoking on own smoking.

The most common IV approach to identifying peer effects in adolescent substance use is to use peer characteristics or peer family background factors such as household structure and parental education as instrumental variables for peer substance use, under the assumption of no contextual effects, i.e. assuming that peer characteristics do not directly influence the individual's behaviour. Various studies use this method with US data. Gaviria and Raphael (2001) estimate that a one percentage point increase in peer smoking leads to a one sixth of a percentage point increase in the probability of own smoking, with school grade treated as the reference group. Powell et al. (2005) estimate that a one percentage point increase in peer smoking leads to a one half percentage point increase in the probability of own smoking with the school class treated as the reference group. Fletcher (2010) estimates that a one percentage point increase in peer smoking leads to between a two fifths and one half percentage point increase in the probability of own smoking with the school grade treated as the reference group. Outside of the US, McVicar (2011) estimates a peer effect of around two fifths, across 26 European countries, with the school class treated as the reference group. There are various other studies that adopt a similar approach. All these estimates, however, are susceptible to the endogenous sorting critique, to varying degrees.

Three earlier studies have attempted to address the endogenous sorting critique by augmenting the IV approach with school or school grade fixed effects. Lundborg (2006) does so using Swedish data, estimating a peer effect of around one half, with the school class treated as the reference group. Sen (2009) does so using Canadian data, also estimating a peer effect of around one half, with same gender classmates taken as the reference group. Fletcher (2010) does so using Add Health data for the US, estimating a peer effect of between one third and one half – only slightly smaller than his equivalent estimates without school fixed effects – with the school grade treated as the reference group. Taking an alternative approach, Clark and Lohéac (2007) use lagged peer behaviour from the US Add Health study in place of instrumented current peer behaviour, along with school fixed effects, and estimate a peer effect of less than one tenth from male peers (significant at the 10% level) and essentially zero from female peers, with the school grade treated as the reference group.

Taken together the balance of evidence from these studies – drawn from many different countries, covering a range of adolescent ages and with a variety of reference groups –

suggests positive and mostly statistically significant peer effects. Estimated magnitudes vary, but generally fall in a range between zero and one half, or one sixth and one half if we leave out the apparently outlying (and non-IV) study of Clark and Lohéac (2007). Further, Gaviria and Raphael (2001) style IV estimates have so far appeared *reasonably* robust to inclusion of school fixed effects, although we only have three studies to go on to date. This could indicate that endogenous sorting is not leading to large biases in estimates of smoking peer effects, or it might indicate that the most relevant sorting occurs at the class or grade level rather than the school level, in which case we still need to be cautious about interpreting these IV estimates. Here further evidence is presented on this issue.

3. Empirical Approach and Identification

This paper combines elements from the approaches of Gaviria and Raphael (2001), Fletcher (2010), McVicar (2011), and, from the school performance peer effects literature, Atkinson et al. (2008). First, the familiar linear in means model of Gaviria and Raphael (2001), without school fixed effects, is estimated:

$$y_{ijsc} = \alpha \bar{y}_{ijsc} + X_{ijsc}\beta + u_{ijsc} \tag{1}$$

where y_{ijsc} denotes the tobacco use of individual i in class j in school s in country c; $\bar{y}_{ijsc}$ is the proportion of i's classmates that use tobacco (excluding i); and X_{ijsc} is a vector of individual and family background characteristics for i. The u_{ijsc} term is allowed to be arbitrarily correlated across individuals in the same class or school. The coefficient of interest is α, which indicates the extent to which the substance use participation of the individual is influenced by the substance use participation of classmates, i.e. the *endogenous social interaction*.

Following Gaviria and Raphael (2001), Powell et al. (2005), Lundborg (2006) and others, (1) makes the assumption that peer characteristics (e.g. their family background) do not directly impact on own behaviour, i.e. there are no *contextual effects*. This assumption allows us to provide initial estimates of α using OLS.[1] Gaviria and Raphael (2001) and Powell et al. (2005) justify this assumption by the argument that social interaction between classmates

[1] If peer characteristics can influence individual behaviour, then in a linear model we cannot separately identify the impact of peer behaviour on individual behaviour from that of peer characteristics on individual behaviour, at least not without IV.

takes place mostly at school, away from potential peer family background influences. If there is endogenous sorting into or within schools, however, then observed and/or unobserved characteristics of peers may be correlated with individual behaviour through correlation with the individual's own unobserved characteristics. Also, if some classmates are also friends outside of school, then social interaction between classmates may also occur outside of school, closer to potential peer family background influences. We return to both points below.

Even without endogenous sorting or direct contextual effects, we know from Manski (2000) that OLS estimates of α will be subject to various biases. Some of these biases are likely to be positive, but others may be negative, so we cannot even be sure that OLS gives us an upper bound on any causal relationship from peer behaviour to individual behaviour. First, there may be school level unobservables, e.g. the school policy on smoking or the existence of a shop near the school gates willing to sell cigarettes to adolescents, which will influence both the individual's and the classmates' behaviour. These are examples of what Manski (2000) calls *correlated effects* and they are likely to impart an upward bias on the OLS estimate of α.[2] Second, because I am a peer of my peers, my smoking may affect my peers' smoking at the same time as my peers' smoking affects my smoking – Manski's *reflection problem* – which may impart a further positive bias on the OLS estimate of α. Third, if peer substance use is measured with error, e.g. because not all classmates take part in the survey, then this may impart a downward bias on the OLS estimate of α (see Micklewright et al., 2010; Nikaj, 2011).

By making the assumption of no contextual effects, Gaviria and Raphael (2001) and others are able to use excluded peer characteristics and peer family background variables as instruments for peer substance use. To the extent that these instruments can be treated as exogenous – e.g. in the absence of endogenous sorting – then the resulting IV estimates can give a consistent estimate of α. This paper also provides estimates following this approach, first using class averages (minus the individual) of *all* observable peer characteristics, i.e. $\bar{X}_{ijsc}$, as instruments, and second by using the smoking behaviour of peers' older siblings as a single instrument. The latter version of the model helps to avoid potential problems associated with weak instruments and also allows the assumption of no contextual effects to

[2] Given that this study uses cross-country micro data, by including country dummies in (1) *country-level* correlated effects, e.g. related to tobacco price, availability or legal constraints, can be controlled for (see McVicar, 2011).

be relaxed so that other peer characteristics *can* directly impact on own smoking behaviour. Fletcher (2010) also presents IV estimates first using the full set of peer characteristics as instruments and second using only whether peers have an older sibling and whether peers' households contain a smoker.[3]

Unfortunately, peer background instruments may not be validly excluded from (1) if there is endogenous sorting into schools (Lundborg, 2006; Fletcher, 2010). For example, sorting could take place if there is something unobserved about the school that attracts parents with similar observed and unobserved characteristics. This kind of correlated effect will impart upward bias on OLS estimates of α that cannot be removed (and may be exacerbated) by the peer-background-IV approach. Only if the characteristics on which individuals are sorted into schools are observed and controlled for, or are irrelevant for substance use, can we be confident that peer family background variables are not invalidated as instruments on these grounds. This is more likely for the single instrument case than for the full set of instruments – in part because all other observed peer characteristics can enter (1) directly as controls – but endogenous sorting into schools still cannot be entirely ruled out.

Different studies have attempted to deal with this problem in different ways. Gaviria and Raphael (2001) explore differences in the estimated magnitude of peer effects for recent movers and longer term residents with the idea that biases due to endogenous sorting will be larger for the recent movers.[4] Powell et al. (2005) explore sensitivity to including observed school level variables, e.g. on school anti-smoking policies, which may influence sorting into schools.[5] But if other, *unobserved,* school level factors can drive endogenous sorting into schools, and our purpose is to control for such differences rather than to explain such differences, then the school/grade fixed effects approach of Lundborg (2006), Sen (2009) and Fletcher (2010) is to be preferred. By including such fixed effects, unobserved school-level factors are controlled for and peer characteristics – differences from the school level means – are more likely to be validly excludable from (1). This paper therefore follows Fletcher

[3] Fletcher's intuition for the first of these instruments is that, once family size is controlled for, the presence of an older sibling is likely to have an impact on own substance use behaviour but is unlikely to be related to sorting into schools. This instrument is less attractive in the current paper because the ESPAD data do not allow the same degree of control for family size as in Fletcher's case. Fletcher uses a similar argument for his second instrument. The ESPAD data for the selected countries only contain smoking information for older siblings rather than all household members, but, having controlled for the presence of an older sibling, the same intuition applies.

[4] They find no statistically significant differences in smoking peer effects between the two groups.

[5] Gaviria and Raphael (2001) also explore sensitivity to adding school level observed factors, although only for their OLS estimates.

(2010) by presenting additional sets of IV estimates for α, using the same peer background instruments as before – the full set and the peer's older sibling behaviour – but now including school fixed effects in (1). To support the conjecture of instrument validity, over-identification tests are presented for the models with and without school fixed effects (where possible) and the extent to which peer characteristics are correlated with individual observed characteristics with and without fixed effects is explored.[6]

School fixed effects, however, do not control for the possibility of endogenous sorting into classes or grades *within* schools, and class or grade level correlated effects might still impart upward bias even on IV-FE estimates of a. Neither Lundborg (2006) nor Fletcher (2010) explicitly address such within-school sorting, but both argue it is unlikely given the reference groups they have defined (Swedish classes in the case of Lundborg and US grades in the case of Fletcher).[7] But because this paper estimates across 13 countries, treating the school class not the school grade as the reference group, it is less easy to credibly rule out such sorting here ex ante.[8] Instead, sensitivity is explored to restricting the sample by dropping schools where endogenous sorting into classes is more likely. Broadly following the approach of Atkinson et al. (2008), a dummy for achieving high grades last term is regressed on class dummies separately for each school. If pupils are streamed into classes within a school according to their academic ability, or according to observed or unobserved characteristics correlated with academic ability, then the regression will have explanatory power for the school concerned, in which case it is dropped from the sample.

4. Data

The data used here are drawn from the ESPAD 2007 survey (see Hibell et al., 2009). The ESPAD 2007 is an international collaboration across 35 European countries collecting cross-sectional information on substance use for the cohort of adolescents born in 1991. The ESPAD uses broadly similar sampling frames, methods and questionnaires across countries, resulting in survey data that is highly comparable across countries. Essentially, each country

[6] If there is no sorting into schools then individual and peer characteristics such as household structure and parental education should be orthogonal.
[7] Although Lundborg (2006) includes grade fixed effects, fixed effects cannot be used to control for sorting into classes within grades.
[8] For some countries there is only data on one class per grade in each school, so including grade fixed effects or using the grade as the reference group are not really viable. Further, even at the grade level we cannot rule out endogenous sorting, e.g. in those countries where the sample includes grades that are beyond the minimum school leaving age.

randomly selected a representative sample of schools, in most cases with pupils in a single class asked to complete the ESPAD questionnaire, anonymously, during school hours, and under exam conditions. The completed forms were then sealed in envelopes, collected by staff, and posted to the respective survey teams. The resulting database contains information on almost 100,000 adolescents across more than five thousand schools.

Around one in six (955) schools returned questionnaires for more than one class. In many cases this reflects the fact that those born in 1991 are spread over two school grades (or sometimes more than two in countries where some students skip or repeat grades), with one class from each relevant grade randomly selected for the survey. In other schools several classes from a particular grade were returned. The data from these schools are particularly useful for the purpose of identifying peer effects in adolescent smoking at the class level because they allow school fixed effects to be included in (1) to control for unobserved school level factors that may be correlated with smoking. Attention is therefore restricted to this subset of schools.[9] After further restricting to schools where each returned class includes at least two individuals and where a common set of questions is asked about smoking and family background, detailed data remain for 23761 adolescents, in 1877 classes, in 780 schools, across 13 European countries (see Table 1). Not only is this sample attractive because of its cross country nature, it is also larger than that used by Fletcher (2010), Sen (2009), Clark and Lohéac (2007), and Lundborg (2006), potentially allowing more precise estimation of peer effects and reducing concerns over finite sample biases with IV estimates (e.g. see Murray, 2006).

<Table 1 here>

In each of these countries, the ESPAD questionnaires ask a number of questions relating to substance use. This paper focuses on the following: *How frequently have you smoked cigarettes during the last 30 days?* Responses are on a seven-point scale, ranging from ‘none/not at all’ through to ‘more than 20 cigarettes per day’. For consistency with previous studies these responses are dichotomised into binary dummies for tobacco use, with 0 indicating not having smoked in the last 30 days and 1 indicating having done so on at least one occasion in the last 30 days. Figure 1 shows the resulting sample proportions reporting at

[9] Because this restriction is largely country-based – some countries return multiple classes per school and others do not – there are significant differences on most observed behaviours and characteristics between the individuals in multi-class schools and those in single class schools. The restricted sample should not therefore be treated as representative of the wider ESPAD 2007 sample.

least some smoking in the last 30 days in each of the 13 countries. Reported smoking participation rates range from 22% (Greece) to 45% (Austria), with an overall rate of 31%.

<Figure 1 here>

Individuals are also asked to provide information on a range of questions about their own characteristics and family backgrounds which, where data are consistently collected across the countries, can be used to construct control variables (the X_{ijsc} in (1)). These include controls for prior academic performance, presence of siblings, presence and substance use of older siblings, household structure and parental education levels. A list of variables and sample means are given in Table 2. Country dummies are also included to capture unobserved differences at the national level, e.g. in price and availability of tobacco, although these are dropped for the models with school dummies.

<Table 2 here>

Because only pupils born in 1991 are surveyed, where a class includes pupils born before or after 1991 the ESPAD does not give us data on the whole class. To be precise, this paper therefore estimates peer effects between classmates born in the same calendar year as distinct from peer effects between all classmates in the same school cohort. We also cannot rule out measurement error in the peer behaviour variables where same-age classmates were absent on the day of the survey or did not return a questionnaire for other reasons (see Micklewright et al., 2010).

5. Results and Discussion

Before moving on to the estimated peer effects it is worth briefly commenting on the estimated impacts of the individual controls in (1). These provide potentially useful information on the impact of individual characteristics and family background on adolescent smoking, drawing on microeconomic data across 13 countries.[10] Academic ability (prior school grades) is negatively correlated with smoking participation (e.g. McVicar, 2011). Having an older sibling that smokes is associated with a higher probability of smoking (e.g. Griesbach et al., 2003). Parental education level is not strongly associated with adolescent smoking once the individual's own school grades are controlled for, but the weak

[10] To keep Table 3 more manageable, estimated coefficients for these controls are not reported. Full estimation results are available on request.

associations are in the direction we might expect with higher levels of parental education correlated with lower probability of smoking (e.g. Gaviria and Raphael, 2001; Powell et al., 2005). Finally, household structures other than those including two biological parents are associated with an increased probability of smoking, with the correlation stronger for those living in a step family household or where no biological or step parent is present compared to those living in a single parent household (Griesbach et al., 2003). These conclusions are robust across the different versions of the model.

Now consider the OLS estimate of peer effects, i.e. column 2 of Table 3. This estimate suggests that a one percentage point increase in the class proportion using tobacco is associated with a .32 percentage point increase in the probability that an individual uses tobacco. This is comfortably within the range of (single equation) estimates from earlier studies.

<Table 3 here>

Next consider the IV estimates, first using all observed peer characteristics and background variables as instruments, as presented in column 3 of Table 3.[11] This estimate is larger in magnitude than the OLS estimate (.39 compared to .32). Gaviria and Raphael (2001) also present IV estimates that are larger than the corresponding OLS estimates. If we are prepared to assume that this IV strategy is valid – that there are no contextual effects and no endogenous sorting – then the implication is that negative biases outweigh positive biases in the OLS estimates. Measurement error may contribute to this (see Micklewright et al., 2010; Nikaj, 2011). So too could the bounded nature of the peer smoking variable (Nikaj, 2011). Gaviria and Raphael (2001) also suggest that any simultaneity bias could be negative, although this seems counterintuitive.

The evidence here, however, suggests that this may not be a valid IV method, at least not for these data with this set of instruments. In addition to ex ante concerns regarding instrument validity, over-identification testing suggests that at least some of the variables in the set of peer characteristics cannot be treated as exogenous and therefore cannot be excluded from (1). Fletcher (2010) presents similar evidence and draws a similar conclusion for estimating peer effects in adolescent smoking using the Add Health data. His interpretation is that this reflects endogenous sorting into schools.

[11] The F-statistic for the joint significance of the instruments in the first stage regression is above 10, although not all of the instruments are individually significant.

Next consider the alternative IV estimate using the single peer older sibling behaviour instrument, presented in column 4 of Table 3. The F-statistic for instrument relevance is considerably higher in this case. The peer effects estimate itself is slightly larger than the full-IV equivalent (.43 compared to .39), with a larger standard error. And although over-identification tests are not possible with a single instrument, it is easier to make an ex ante case for the validity of this instrument as compared to full set of peer background variables. For one thing, the assumption of no contextual effects is relaxed for this version of the model, with only the instrument itself excluded from (1) and all other peer variables included as controls. This also allows the validity of the assumption of no contextual effects to be directly explored. In this case, only the proportion of the class with 'low' grades is statistically significant at the 5% level in (1), although the proportion living in a step-family household is only just outside the 5% level of significance. So assuming no contextual effects appears questionable, but not *that* questionable.

Next consider the full-IV estimates including school fixed effects, presented in column 5 of Table 3. As in Fletcher (2010), over-identification tests where school fixed effects are included in the model suggest that the set of peer characteristic instruments *can* now be validly excluded from (1), although we pay a price in terms of a reduced F-statistic for the set of instruments in the first stage regression (now below 10, despite the large number of instruments). There is also far less correlation between the instruments and individual characteristics than in the case where school fixed effects are omitted (see Tables 4 and 5), consistent with better control for sorting into reference groups. As we would expect, the estimated peer effect is smaller than the IV equivalent without fixed effects (.26 compared to .39). It is also smaller than the OLS estimate. But this IV-fixed effects estimate is still well within the range of existing estimates in the literature, and still suggests large, positive and statistically significant peer effects for adolescent smoking.

<Tables 4 and 5 here>

Even when school fixed effects are included, however, there are reasons to be cautious regarding estimated peer effects. We still rely on the assumption of no contextual effects and there is still more correlation between some of the instruments and individual characteristics than we would expect were adolescents allocated randomly to classes conditional on the school fixed effects (see Table 5). For example, the proportion of classmates with an older sibling appears correlated with several individual characteristics, as does the proportion of classmates living with a step parent. The first stage F-statistic is also rather low. For these reasons the estimate presented in column 6 of Table 3, i.e. the IV-FE (alt) estimate using only the peer older sibling behaviour variable to instrument peer behaviour, is arguably to be preferred. The first stage F-statistic is higher (and above 10). From Table 5 we see that the instrument is essentially uncorrelated with individual characteristics once school dummies are included.[12] Further, none of the contextual effects included in (1) are statistically significant when school dummies are included. As a final check on instrument validity the model is re-estimated with the full set of instruments excluding the older sibling behaviour instrument, which is instead included in (1) as a contextual effect control. The results indicate that the proportion of classmates with an older sibling that smokes has no significant contextual effect on individual behaviour.

Turning to the peer effects estimate itself, the coefficient on peer smoking is around three fifths that obtained with the full set of instruments (.16 compared to .26) and the standard error around twice as high. So once we condition on school fixed effects and omit potentially weak and/or invalid instruments, the point estimate of peer effects is towards the bottom of the range of existing estimates in the literature. The reduction in the magnitude of the point estimate coupled with the increase in the standard error leaves an estimate that is statistically insignificant from zero at standard levels, given the number of observations available here. Ultimately, we cannot rule out zero peer effects in smoking between classmates using these data.

Table 6 further explores the sensitivity of the smoking peer effect estimate to instrument choice by starting with the single older sibling instrument and adding other instruments one by one, with the aim of uncovering the instrument or instruments that drive the sensitivity between the full-IV-FE and alternative-IV-FE estimates. The estimated coefficient varies as different instruments are added, but in most cases in a fairly narrow band around the Table 3 point estimate of .16. Just over half of these variations imply a statistically insignificant peer effect. The variable which when added as an instrument has the biggest impact on the estimated coefficient – becoming close in magnitude to the full-IV-FE estimates and statistically significant at the 5% level – is the proportion of classmates with high grades last

[12] With random sorting into classes we would expect an instrument to be significantly correlated with between one and two individual characteristics at the 5% level.

term. If we *replace* the older sibling behaviour instrument with the high grades instrument, the peer effects estimate is larger still.[13]

<Table 6 here>

The final set of estimates in Table 3 uses the same peer older sibling smoking instrument, again with school fixed effects, but for a reduced sample of schools for which regressions of the dummy for high grades last term on class dummies have no explanatory power (as indicated by F-statistics for joint significance with p-values greater than .05). In other words, we repeat the alternative-IV-FE estimation on schools which appear to randomly sort pupils into classes, assuming academic ability is the most likely mechanism for such sorting.[14] For bias in the IV-FE estimate using the single older sibling behaviour instrument we would require correlation between the instrument and that part of academic ability not captured by the prior grades dummies or other controls.[15] In the event, however, the point estimate of the peer effect is highly robust to this restriction, although there is a further increase in the standard error given the reduction in sample size. So endogenous sorting within schools does not seem to bias the preferred estimate of peer effects here, but we still cannot rule out zero peer effects in smoking between classmates.

6. Conclusions

This paper presents micro-econometric evidence on the existence and magnitude of peer effects in adolescent smoking between classmates across 13 European countries. Both instrumental variables and school fixed effects are used for identification. The paper supports the earlier conclusions of Lundborg (2006) and Fletcher (2010) by showing that omitting school fixed effects can lead to substantial overestimates of peer effects, consistent with

[13] One reason to be concerned about the possible validity of the prior grades instrument is if pupils are sorted into classes within schools by academic ability. Because the controls for academic ability used here are rather blunt, some part of ability is unobserved. Sorting on academic ability therefore implies correlation between the instrument and unobserved determinants of peer smoking, along with correlation between unobserved determinants of peer smoking and unobserved determinants of own smoking.

[14] The ESPAD surveys more than one school type in most countries (e.g. vocational, academic and industrial in Croatia). Partly as a consequence of this, most countries have some schools that appear to sort randomly into classes and others that appear to stream by academic ability. Denmark is the country with the highest proportion of schools that appear to allocate students randomly to classes. Bulgaria is the country with the lowest proportion, with all schools having classes sorted on ability. One contributing factor may be that 25% of the 1991 ESPAD cohort are no longer in school at the time of the survey, which when coupled with the fact that ESPAD in Bulgaria samples over two grades, suggests classes in the older grade (Year 10) will have a higher proportion of high performers than those in the younger grade. Note that we don't have information on prior grades for Cyprus, so all Cypriot schools are omitted from the restricted sample.

[15] We know that academic ability, as measured by the prior grades dummies, is highly correlated with smoking behaviour in the wider sample.

endogenous sorting into schools which confounds peer effects with correlation in substance use behaviour because of shared unobserved characteristics. Where Lundborg (2006) and Fletcher (2010) demonstrate this using Swedish and US data respectively, here the result is shown to generalise across several European countries.

Even with school fixed effects, however, instruments can still be weak and/or invalid, e.g. where there is endogenous sorting into classes *within* schools or where direct contextual effects from peer family background to individual behaviour are possible. As well as presenting IV-fixed effects results where all peer characteristics are used as instruments, IV-fixed effects results are therefore also presented where a single instrument – more plausibly exogenous and with good first stage explanatory power – is used, with all other observed peer characteristics included as controls. Finally, using a method borrowed from the school performance peer effects literature and not previously used in the substance use peer effects context, this estimate is shown to be robust to omitting schools which appear to stream pupils into classes by academic ability.

The preferred estimate – using the single instrument, with school fixed effects – suggests peer effects that are smaller in magnitude than those found commonly found in the literature, including those found by Lundborg (2006), Sen (2009) and Fletcher (2010), and that are statistically insignificant at standard levels. Ultimately, zero peer effects in adolescent smoking between classmates cannot be ruled out using this approach with these (cross-country) data. In this respect, the paper's main conclusion is perhaps closer to that of Clark and Lohéac (2007), which until now has been something of an outlier, than it is to most existing IV studies of smoking peer effects in the literature.

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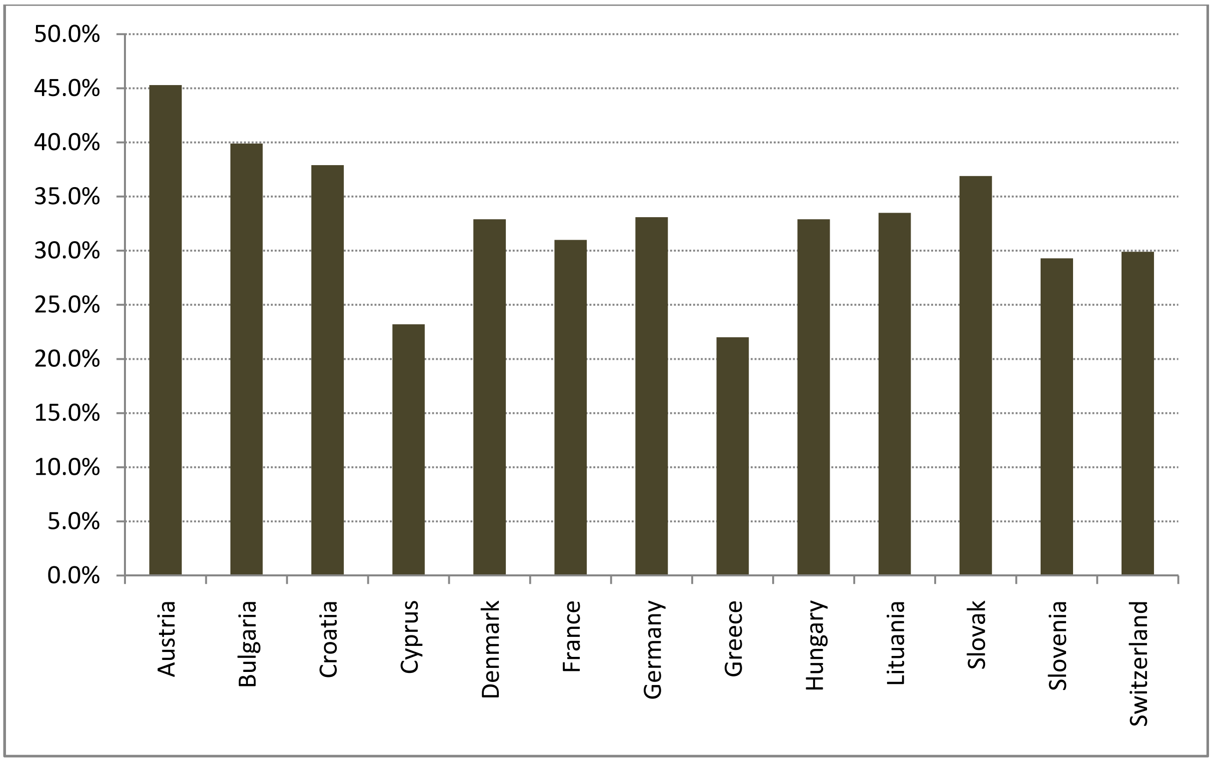
Figure 1: Sample Proportions Using Tobacco in Last 30 Days, by Country



Note: These sample proportions are based on the whole ESPAD 2007 sample for each country, including schools with only single classes surveyed.

Table 1: Sample Characteristics: Countries, Schools, Classes, Observations

	Number of schools	Number of classes	Number of observations
Austria	78	215	2062
Bulgaria	6	12	94
Croatia	112	260	2904
Cyprus	50	416	6307
France	127	253	2340
Germany	17	35	320
Greece	90	185	2048
Hungary	26	54	589
Lithuania	91	206	1871
Slovak Republic	70	188	2060
Slovenia	46	95	1775
Switzerland	42	88	663
Denmark	25	58	728
Total	780	1877	23761

Note: These sample proportions are based on the sample restricted to schools with more than one class surveyed and where a minimum of two individuals are reported for each class.

Table 2: Sample Proportions, Cross Country

	Sample Proportion, %
Used tobacco in last 30 days	30.7
Male	49.6
Low academic performance	8.4
High academic performance	13.9
Missing academic performance	27.5
At least one brother in household	53.4
At least one sister in household	49.6
Has older sibling	71.4
Older sibling smokes	29.0
Father education missing	38.0
Father some secondary	10.4
Father secondary	22.0
Father some college	7.3
Father college	14.5
Missing mother education	35.3
Mother some secondary	8.5
Mother secondary	24.1
Mother some college	8.6
Mother college	15.8
Well off family	40.5
Two parent household, one or more step	7.5
One parent household	11.5
Non-standard household	2.2
Number of observations	23761

Table 3: Peer Effects in Smoking, Main Estimates

	OLS	2SLS (Full)	2SLS (alt)	2SLS-FE (full)	2SLS-FE (alt)	2SLS-FE (alt, no sorting)
% of peers smoking	.323*** (.020)	.386*** (.041)	.432*** (.062)	.263*** (.067)	.164 (.127)	.158 (.257)
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Contextual effects	No	No	Yes	No	Yes	Yes
Country dummies	Yes	Yes	Yes	No	No	No
School fixed effects	No	No	No	Yes	Yes	Yes
R^2 (centered for IV, within for FE)	.106	.105	.102	.042	.050	.063
Number of observations	22285	22285	22285	22285	22285	8376
F stat for excluded instruments		18.4	129.0	7.61	51.9	13.3
Hansen J-stat p-value		.002	-	.885	-	-

Notes: ***/**/* denote statistical significance at 1%/5%/10% levels respectively. Standard errors clustered at class level (OLS and 2SLS without fixed effects) or at the school level (with fixed effects) are shown in parentheses. Cyprus is the omitted country dummy. 2SLS (full) uses all observed peer characteristics as instruments. 2SLS (alt) uses the proportion of classmates with an older sibling that smokes as the single instrument.

Table 4: Correlation between Peer Characteristics and Individual Characteristics, Selected Instruments, Without School Dummies

	% of peers with older sibling	% of peers with older sibling who smokes	% of peers with high grades	% of peers with step family household
Male	.016***	.010**	-.026***	.002
	(.004)	(.004)	(.005)	(.002)
Low grades	-.004	.014**	-.017*	.024***
	(.006)	(.007)	(.009)	(.004)
High grades	-.052***	-.047***	.225***	-.015***
	(.006)	(.006)	(.014)	(.003)
Has at least one brother	.012***	.008***	-.020***	-.005**
	(.003)	(.002)	(003)	(.002)
Has at least one sister	.012***	.005**	-.015***	-.003
	(.003)	(.002)	(.003)	(.002)
Father secondary	.010*	.003	.063***	-.011***
	(.005)	(.005)	(.009)	(.002)
Father college	-.074***	-.053***	.131***	-.007**
	(.007)	(.005)	(.011)	(.003)
Mother secondary	-.002	.0009	.064***	-.005**
	(.005)	(.005)	(.009)	(.003)
Mother college	-.061***	-.050***	.132***	-.007**
	(.006)	(.005)	(.011)	(.003)
Step family household	.010*	.019***	-.027***	.039***
	(.005)	(.005)	(.006)	(.008)
One parent household	-.007*	.004	.003	.015***
	(.004)	(.004)	(.005)	(.002)
Number of significant correlations at 95% (/21)	13	14	16	12

Notes: ***/**/* denote statistical significance at 1%/5%/10% levels respectively. Standard errors clustered at the class level are shown in parentheses.

Table 5: Correlation between Peer Characteristics and Individual Characteristics, Selected Instruments, With School Dummies

	% of peers with older sibling	% of peers with older sibling who smokes	% of peers with high grades	% of peers with step family household
Male	-.0002	.0006	-.004*	.002
	(.002)	(.002)	(.002)	(.001)
Low grades	-.0006	-.002	-.007**	.005*
	(.003)	(.004)	(.003)	(.003)
High grades	.003	-.0005	-.005	-.005***
	(.003)	(.003)	(.006)	(.002)
Has at least one brother	-.004**	-.002	-.002	-.001
	(.002)	(.002)	(.001)	(.001)
Has at least one sister	-.004**	-.00003	.001	-.001
	(.002)	(.002)	(.001)	(.001)
Father secondary	.002	-.001	.0004	-.001
	(.002)	(.002)	(.002)	(.001)
Father college	-.007**	-.002	.004	-.0006
	(.003)	(.002)	(.003)	(.002)
Mother secondary	-.0008	.0005	-.002	.0006
	(.002)	(.002)	(.002)	(.001)
Mother college	-.003	-.00001	.002	-.002
	(.003)	(.003)	(.003)	(.002)
Step family household	.005	.005	-.007***	-.031***
	(.004)	(.004)	(.003)	(.005)
One parent household	.001	.0001	.003	.009***
	(.003)	(.003)	(.002)	(.002)
Number of significant correlations at 95% (/21)	6	1	2	3

Notes: ***/**/* denote statistical significance at 1%/5%/10% levels respectively. Standard errors clustered at the school level are shown in parentheses.

Table 6: Sensitivity to Instrument Choice

	Peer effect estimate	F-stat for excluded instruments	Hansen J-Stat p-value
2SLS-FE (alt)	.164	51.9	
	(.127)		
+ % male	.207*	26.6	.332
	(.120)		
+ % low grades	.126	38.2	.606
	(.110)		
+ % high grades	.232**	44.9	.358
	(.092)		
+ % with brother(s)	.212*	29.6	.218
	(.119)		
+ % with sister(s)	.201*	27.8	.160
	(.121)		
+ % with older sibling	.143	26.1	.344
	(.129)		
+ % father secondary	.163	26.0	.711
	(.127)		
+ % father college	.166	26.0	.594
	(.127)		
+ % mother secondary	.169	27.1	.857
	(.124)		
+ % mother college	.154	26.3	.613
	(.127)		
+ % well off household	.167	26.6	.850
	(.125)		
+ % step household	.218*	29.3	.214
	(.116)		
+ % one parent household	.164	27.6	.986
	(.122)		
+ % non-standard household	.177	26.4	.094
	(.125)		
+ all sibling variables	.214*	15.2	.239
	(.117)		
+ all grades variables	.190**	40.2	.496
	(.081)		
+ all parental education variables	.227*	5.3	.911
	(.117)		
+ all three household structure variables	.228**	15.4	.201
	(.112)		
Older sibling smoking replaced by % low grades	.044	24.9	
	(.207)		
Older sibling smoking replaced by % high grades	.343*	36.7	
	(.135)		

Notes: ***/**/* denote statistical significance at 1%/5%/10% levels respectively. Standard errors clustered at school level are shown in parentheses. 2SLS (alt) uses the proportion of classmates with an older sibling that uses tobacco as the instrument.