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Instrumental variables estimates of peer effects in social networks

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ABSTRACT

Estimating peer effects with observational data is very difficult because of contextual confounding, peer selection, simultaneity bias, and measurement error, etc. In this paper, I show that instrumental variables (IVs) can help to address these problems in order to provide causal estimates of peer effects. Based on data collected from over 4000 students in six middle schools in China, I use the IV methods to estimate peer effects on smoking. My design-based IV approach differs from previous ones in that it helps to construct potentially strong IVs and to directly test possible violation of exogeneity of the IVs. I show that measurement error in smoking can lead to both under- and imprecise estimations of peer effects. Based on a refined measure of smoking, I find consistent evidence for peer effects on smoking. If a student's best friend smoked within the past 30 days, the student was about one fifth (as indicated by the OLS estimate) or 40 percentage points (as indicated by the IV estimate) more likely to smoke in the same time period. The findings are robust to a variety of robustness checks. I also show that sharing cigarettes may be a mechanism for peer effects on smoking. A 10% increase in the number of cigarettes smoked by a student's best friend is associated with about 4% increase in the number of cigarettes smoked by the student in the same time period.

1. Introduction

Sociology has been interested in studying peer effects for a long time. Early examples can be traced back to Coleman's studies on diffusion of innovations (Coleman et al., 1957) and adolescent society (Coleman, 1960) and Duncan, Haller, and Portes' study on peer aspirations (Duncan et al., 1968). Ever since, there has been a large number of studies seeking to examine peer effects in a variety of contexts, including job attainment (Granovetter, 1973), spreading of health behaviors and outcomes (Christakis and Fowler, 2007; Liu et al., 2010), agreement on political preference (Yamaguchi, 2013), etc.

But it turns out to be very difficult to causally estimate peer effects because of contextual confounding, peer selection, simultaneity bias, and measurement error, to name only a few. In this paper, I point out that measurement error in the outcome can have double-detrimental effects: not only biasing the estimated peer effects toward zero but also inflating their standard errors. The net result is that it becomes more difficult to reject the null hypothesis of no peer effects. I also show how instrumental variable (IV) methods can help to address the aforementioned problems and provide causal estimates of peer effects. The basic idea is to utilize the exogenous variations in the IVs to facilitate identification. The success of the IV methods relies critically on the strength and exogeneity of the IVs. In this paper I argument the conventional IV methods with a variety of robustness checks including sensitivity analysis, falsification tests, matching analysis, and sub-sample

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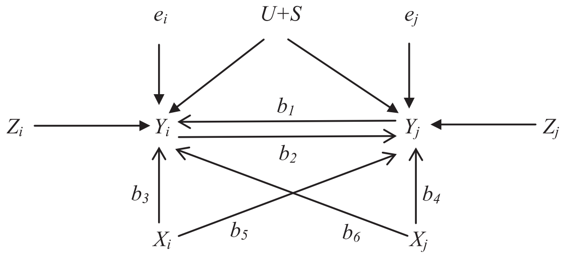

Fig. 1. Difficulties in identifying endogeneous peer effects.

analysis in order to provide cross-validating causal evidence. Perhaps more distinctly, in the survey I used to collect data for this study I have designed questions that would provide information that can be used to construct potentially strong IVs. I have also designed a question to solicit respondent's friendship preference, which enables me to evaluate in a direct fashion the exogeneity of the IVs.

Specifically, I applied the IV methods to estimating peer effects on smoking based on data collected from over 4000 students in six middle schools in China. I find that correcting measurement error is crucial for both accurate and precise estimates of peer effects. The estimated peer effects are both larger and statistically more significant when a refined measure of smoking is used. The OLS estimate accounting for peer selection shows a peer effect of about one fifth. The IV estimate is larger, indicating that if a student's best friend has smoked within the past 30 days, that student was about 40 percentage points more likely to smoke in the same time period. I also show that sharing cigarettes may be a mechanism for peer effects on smoking. The number of cigarettes smoked by a student would increase by about 4% in the past 30 days if the number of cigarettes smoked by his or her best friend increased by 10% in the same time period.

This paper proceeds as follows. I first review past research on IV methods, with close attention paid to their applications in studying peer effects. I also outline the motivations for using IV methods to address the various difficulties in estimating peer effects. Then I introduce the data and statistical methods. The fourth section presents the results, where I also provide a variety of analysis for checking the robustness of the findings. Last, I conclude and discuss possible limitations of this study.

2. Difficulties in Identifying endogenous peer effects

Given there is a strong correlation in peers' outcomes, the correlation can come from four possible sources. First is from peer influence (or so-called endogenous peer effects), namely, an individual's outcome is affected by peer's outcomes. Second is from exogenous peer effects, namely, an individual's outcome is affected by peer's exogenous characteristics (e.g., gender, age, family background) (Ammermueller and Pischke, 2009). Third is from contextual effects, namely, peers' outcomes are simultaneously affected by their shared social and environmental factors (e.g., neighborhood, school policy). And last is from selection of peers based on factors related to the outcome.[1]

Much of the research on peer effects has been focused on how to appropriately estimate endogenous peer effects while teasing out other confounding effects (Coleman et al., 1957; Bulte and Lilien, 2001; Christakis and Fowler, 2007, 2013; Cohen-Cole and Fletcher, 2008a, 2008b; Fowler and Christakis, 2008; An, 2011a; VanderWeele and An, 2013). In particular, as to whether smoking is socially contagious, despite many studies (e.g., Ennett and Baumann, 1993; De Vries et al., 2003; Pollard et al., 2010) have found a strong correlation in peer's smoking behaviors, others (e.g., Ennett and Baumann, 1994; De Vries et al., 2006; Mercken et al., 2009) show that peer selection has predominately produced or significantly contributed to the correlation.

Besides difficulties in teasing out competing causes, two other problems, which have by and large been ignored in the literature, may complicate the estimations of peer effects. One is simultaneity, namely, an individual may affect peers in the same time being affected by them. As a consequence, the individual's outcome may appear also as a predictor in the regression predicting peer's outcomes and vice versa. It has been shown that under simultaneity, estimates by conventional Ordinary Least Squares (OLS) are inconsistent, because peer's outcomes are correlated with the error terms by construction (Wooldridge, 2009: 207).

The other problem is measurement error. In the case of simple regressions with just one outcome variable and one explanatory variable, measurement error in the outcome alone will reduce the precision of the estimates while measurement error in the explanatory variable alone will bias its estimated coefficient toward zero (Wooldridge, 2010: 70–75).[2] Measurement error in models of peer effects has a unique feature. Measurement error in the outcome can transfer into

[1] Different peer effects are relevant for different policy interests. If the policy interest is to achieve changes through affecting the group composition, then exogenous peer effects may be more relevant (Graham et al., 2010). If the interest is to target the behavior of a group, leaving group composition intact, then endogenous peer effects are more relevant.

[2] The bias direction may be unpredictable if there is measurement error in multiple variables. But the general consequences of measurement error may still carry on in many empirical contexts.

measurement error in the explanatory variable and vice versa, because of the existence of simultaneity. The result can be double-detrimental: not only attenuating the estimated peer effect but also inflating their standard errors. Thus the null hypothesis of no peer effects becomes more difficult to be rejected. This indicates that previous critiques that peer effects have been overestimated may be less grounded when measurement error is present.

To see the difficulties in identifying peer effects more clearly, I have made Fig. 1, where Y_i represents subject i's smoking status, Y_j his or her best friend's smoking status, U unmeasured contextual factors that affect both subjects' smoking status, S the selection mechanism that leads subject i to nominate subject j as the best friend, X_i and X_j the covariates for each student, respectively, and e_i and e_j two random error terms. In addition, Z_i represents variables that are correlated with Y_i, but uncorrelated with U, S, and the error terms, and similarly is for Z_j.[3] Fig. 1 is written in the spirit of directed acyclical graphs (DAGs) (Pearl, 2000), except that there is a loop between Y_i and Y_j. A line between two variables means there is a causal effect flowing between them. The coefficient b_1 reflects the endogenous peer effect from Y_j to Y_i while b_2 reflects the endogenous peer effect from Y_i to Y_j. The coefficients b_3 and b_4 reflect the effects of the subjects' own covariates on their own outcomes while b_5 and b_6 indicate exogenous peer effects from peer's covariates. Contextual confounding and selection bias arise when U and S are omitted from the statistical models, respectively. Simultaneity bias arises because bi-directional peer influence leads peer's outcome to be correlated with the error term. Issues related to measurement error result if smoking status is measured inaccurately.

A few methods have been proposed in the literature to address these problems in order to provide causal estimates of peer effects (See VanderWeele and An (2013) for a review). Some of them require dynamic network data, for example, the stochastic actor-oriented model (Steglich et al., 2010), in which both peer selection and peer influence are modeled in a system of equations evolving as Markov chains. Other methods resort to experiments, either through random assignment of social contacts (Sacerdote, 2001; Falk and Ichino, 2006) or through partial assignment of random interventions (Duflo and Saez, 2003; An, 2011b). Both of the above methods face certain limitations. The stochastic actor-oriented model imposes strong parametric assumptions on how behaviors and networks evolve over time while not clearly addressing the contextual confounding problem. The experimental methods may not be ideal in cases where the cost of conducting experiments is very high or it is unethical to conduct randomization. The external validity of experiments may also be relatively weak.

When only static observational data is available, instrumental variable (IV) methods have been argued to be useful in identifying endogenous peer effects (Duncan et al., 1968; Bramoullé et al., 2009; O'Malley et al., 2014).[4] The methodology of using IVs to address omitted variable bias (either due to contextual confounding or peer selection) (Wooldridge, 2010: 105–107), simultaneity bias (Wooldridge, 2010: 209–225), and measurement error (Wooldridge, 2009: 480–482) has been detailed in prior work. The basic idea is to utilize the exogenous variations in the IVs to facilitate identification (See Section 3 for more detail).

There have only been a few studies using IV methods to estimate peer effects. Interested in peer effects on occupational and educational aspirations, Duncan et al. (1968) used friend's intelligence as an IV for friend's occupational and educational aspirations. Angrist and Lang (2004) used the predicted number of transferred-in disadvantaged students to study their effects on the academic performance of students in the receiving schools. Bramoullé et al. (2009) argued that in an intransitive triad, the outcome of one side subject can be used as an IV to identify peer effect of the middle subject on the other side subject. Zhang (2009) used measures of new peers as IVs for estimating peer effects on academic achievement. O'Malley et al. (2014) used genetic alleles as IVs to estimate peer effects on weight status.

In this paper, I also use IV methods to study peer effects, but my approach differs from previous ones in several notable aspects. First, my approach is designed-based. With prior knowledge about IV methods, I have designed specific survey questions to collect information that can be used as potentially strong IVs and to directly test some aspects of the exclusion condition for the IVs. Second, I have explicitly measured and incorporated peer selection into the analyses in order to tease out the effect of peer selection from that of peer influence. Third, I demonstrate the detrimental effects of measurement error in estimating peer effects – it may lead to not only underestimated peer effects, but also imprecisely estimated peer effects. Fourth, I point out the challenges imposed by simultaneity in modeling peer effects and clarify what simultaneous models are identifiable in this context. Fifth, I employ a series of strategies including sensitivity analysis, subsample analysis, falsification tests, and matching analysis to assess the robustness of the IV estimates. Last, besides providing estimates of peer effects, I also show a mechanism (i.e., sharing cigarettes) that may account for such peer effects.

3. Data and methods

3.1. Data

The data comes from a two-wave survey of over 4000 students in six middle schools in China, conducted between 2010 and 2011. The high prevalence of smoking and the early onset of smoking in China make adolescent smoking an important topic to study. Recent studies have shown that about two-thirds of the males and 3% of the females above 15 years old in

[3] Similar diagrams can be found in Duncan et al. (1968) and An (2011a).

[4] See Morgan (2002), Stock and Trebbi (2003), Sovey and Green (2011), and Bollen (2012) for reviews on IVs.

China are smokers (Hu et al., 2008) while in contrast, only about one-fifth of the adults in the United States (with not much gender difference) are smokers (Center for Disease Control and Prevention, 2011). More importantly, research has shown that three of every five smokers in China start smoking as teens (Cheng, 1999).

Three of the selected schools are from rural areas and the other three are from suburban areas. The school size ranges from 526 to 1074 while the class size varies between 28 and 71. The survey response rates across the schools are between 78% and 97% with an average of 92%. In the end, I collected 4094 valid surveys. See Table A1 for more information about the schools.

3.1.1. Dependent variable

Students were asked to report whether they smoked within the past 30 days before the baseline survey. I coded it as 1 if they reported yes and 0 otherwise. Since adolescents tend to under-report their smoking (Kenkel et al., 2003), I also use peer reports to correct possible under-reports. Recent work (An and Doan, 2014) shows that using three or four positive peer reports can greatly alleviate the under-reporting problem. Hence, I change a student's smoking status from nonsmoking to smoking if four or more alters have reported the student as a smoker. To differentiate it from self-reported smoking status, I call it the refined smoking status. For comparison, I also provide analyses that use at least three positive peer reports to correct self-reported nonsmoking (shown in Appendix).

3.1.2. Main explanatory variable

Each student was asked to provide up to ten of their best friends in the school. For simplicity of analysis, each student is matched with one best friend he or she nominated. This best friend's smoking status is the main explanatory variable.

3.1.3. Covariates

Students were surveyed on a list of personal and family factors from which I construct covariates, including a binary variable for boys (1 = yes; 0 = no), continuous variables for age, height (in decimeter), and weight (in kilogram /10), ordinal variables for academic ranking in class (1 = "bottom ten" to 5 = "top ten"), happiness in school (1 = "very unhappy" to 5 = "very happy"), and personality (1 = "very pessimistic" to 5 = "very optimistic"), and categorical variables for father's and mother's education (1 = no formal education, 2 = preliminary school, 3 = middle school, 4 = high school, and 5 = junior college or above), father's and mother's occupation (1 = worker, 2 = farmer, 3 = civil official, 4 = professionals like teacher, scientist, and doctor, 5 = business manager or owner, and 8 = others), and family's economic situation (1 = "very good" to 5 = "very difficult"). To account for differences in smoking across schools, I create a school indicator to include in the analysis.

3.1.4. Instrumental variables

Six variables are used as IVs, including: (1) Parental attitudes toward their children's smoking (1 = against, 2 = not against, and 3 = do not know), (2) Father's smoking status (1 = yes, 2 = used to, but quitted, and 3 = never), (3) Sibling's smoking status (1 = yes, 2 = no, 3 = no siblings, and 4 = do not know), (4) Whether any relatives are sick due to smoking (1 = yes, 2 = no, and 3 = do not know), (5) Whether cigarettes are stored at home year-round (1 = yes, 2 = no, and 3 = do not know), and (6) Distance from home to the nearest cigarette store (1 = less or equal to 60 m, 2 = more than 60 m but less than 400 m, and 3 = more than 400 m).[5] The responses from a student's best friend are used as IVs for the best friend's smoking status. All IVs except the fourth one are from the baseline survey.

3.1.5. Friendship preference

In the survey I designed a variable to measure respondent's preference for not making friends with smokers. The question reads as "Will you not make friends with smokers" and students can respond with one out of four options: Yes, Maybe, No, or Don't Know. To facilitate later analysis, I merge the responses of "Don't Know" with those of "Maybe" so that the variable can be treated as ordinal.

3.2. Statistical methods

I specify a linear simultaneous model for peer effects as shown below.

$$Y_i = b_1 \times Y_j + b_3 \times X_i + b_5 \times X_j + b_7 * Z_i + e_i \quad (1)$$

$$Y_j = b_2 \times Y_i + b_4 \times X_j + b_6 \times X_i + b_8 * Z_j + e_j \quad (2)$$

[5] Wherever applicable, I provided an option for students to report "Don't Know". Hence students (especially smokers) would be less likely to make up an answer or leave the questions unanswered.

Table 1
Information on selected student characteristics by best friend's smoking status.

	Best friend did not smoke					Best friend smoked					P
	Obs	Mean	SD	Min	Max	Obs	Mean	SD	Min	Max	
Age	3456	13.40	1.13	10	17	603	13.77	1.17	11	17	0.00
Boy	3451	0.36	0.48	0	1	601	0.90	0.31	0	1	0.00
Academic ranking in class	3348	2.58	1.16	1	5	576	3.13	1.16	1	5	0.00
Happiness in school	3386	3.23	0.89	1	5	583	3.09	0.96	1	5	0.00
Father's education	3369	3.08	0.66	1	5	584	3.06	0.70	1	5	0.49
Family's economic situation	3381	3.07	0.60	1	5	585	3.04	0.67	1	5	0.27
Smoking status	3477	0.07	0.25	0	1	617	0.41	0.49	0	1	0.00

Note: The variables are coded as follows. Boy is a binary variable (1 = yes; 0 = no), Academic ranking in class an ordinal variable (1 = "bottom ten" to 5 = "top ten"), happiness in school an ordinal variable (1 = "very unhappy" to 5 = "very happy"), father's education an ordinal variable (1 = no formal education, 2 = preliminary school, 3 = middle school, 4 = high school, and 5 = junior college or above), family's economic situation an ordinal variable (1 = "very good" to 5 = "very difficult"), and smoking status a binary variable (1 = yes; 0 = no).

In the model, Y_i and Y_j represent respondent's and the nominated friend's smoking status, X_i and X_j their covariates, Z_i and Z_j their instrumental variables, and e_i and e_j two random error terms. The first equation specifies the effect of the best friend's smoking status on the respondent's smoking status. The second equation specifies the effect of the respondent's smoking status on the nominated friend's smoking status. The first equation is what I am interested in estimating.

Given the outcome is binary, it may be attempting to use binary models to fit the data, for example, the bivariate Probit or simultaneous Logit models. However, these nonlinear models are generally unidentified unless the endogenous peer effects are assumed to be equal or one of them is zero (Maddala, 1983: 119; Wissen and Golob, 1990: 233). Neither of the assumptions is ideal in reality. In addition, even if these models are identified, they are prone to specification errors. Any simple deviation from the model specifications could lead to dramatically biased estimates (Angrist and Pischke, 2008: 190). Also, binary simultaneous models tend to be computationally instable (Angrist and Krueger, 2001; Angrist and Pischke, 2008).[6] Hence, linear simultaneous models are preferred in this context (Angrist and Pischke, 2008: 147–148).

As the benchmarks, I first estimate the model by OLS. In the first regression, I predict the respondent's smoking status based on the covariates of both the respondent and his or her best friend. In the second one, I additionally adjust for the propensity of the respondent selecting a smoker as the best friend.[7]

Then I estimate the model by two stage least squares (2SLS). Specifically, to estimate Eq. (1), in the first stage all the covariates (X_i and X_j) and instruments (Z_j) are used to predict the friend's smoking (Y_j). In the second stage, the predicted friend's smoking (rather than the original Y_j) is used to predict the respondent's smoking Y_i. To show the properties of the 2SLS estimator, we can write Eq. (1) in the matrix form, $Y_I = Y_J b_1 + e$, where Y_I is a vector containing the respondents' smoking status, Y_J a vector containing the friends' smoking status, and covariates are omitted for conciseness. Slightly adapting previous notations (Angrist and Pischke, 2008: 206–208), the 2SLS estimator for peer effects can be shown as follows.

$$\hat{b}_1 = (\widehat{Y}'_J \widehat{Y}_J)^{-1} \widehat{Y}'_J Y_I = b_1 + (Y'_J P_z Y_J)^{-1} Y'_J P_z e, \tag{3}$$

where $\widehat{Y}_J = P_z Y_J$, $P_z = Z(Z'Z)^{-1}Z'$, and Z the vector of friend's IV. The variance of $\hat{b}_1$ is

$$\mathrm{Var}(\hat{b}_1) = (Y'_J P_z Y_J)^{-1} [Y'_J P_z V P_z Y_J] (Y'_J P_z Y_J)^{-1}, \tag{4}$$

where $V = \mathrm{diag}(\sigma_1^2, \ldots \sigma_n^2)$ is the variance for the heteroskedastic error term.[8]

For the 2SLS estimator to work, the IVs have to satisfy two conditions.

1. The relevance condition, namely, Cov $(Z, Y_J) \neq 0$. Otherwise, the IV estimate is a priori zero and even worse, $(Y'_J P_z Y_J)^{-1}$ is undefined.
2. The exclusion condition, namely, Cov $(Z, e) = 0$, meaning the IV cannot correlate with the error term. Otherwise, the second term in Eq. (3) will introduce bias into the estimated coefficient for b_1.

In the results section below, I provide a variety of analysis to address problems from possible violation of the two conditions of the IVs.

[6] See Appendix A1 for more discussion on these and related models.

[7] Specifically, I create a dummy which equals to 1 if a student nominated a smoker as the best friend and to 0 otherwise. I fit a Logit model on the dummy variable to predict a student's propensity to choose a smoker as the best friend based on the covariates of the student and the friend, their friendship preferences, and whether they are from the same class. The predicted propensities are then used as weights in the OLS model (Dehejia and Wahba, 1999; Hirano et al., 2003). Without correcting uncertainties in estimating the propensity scores, the standard errors of the estimated peer effects may be larger than necessary (Abadie and Imbens, 2009; An, 2010). But a more conservative inference may be preferred in light of previous critiques that peer effects have been overestimated.

[8] Assuming homoscedasticity, the variance can be simplified to $\sigma^2 (Y'_J P_z Y_J)^{-1}$, where σ^2 is the common variance of the error term.

Table 2
Prediction of best friend's smoking status using best friend's IVs and all covariates.

	(1) Original IVs		(2) Empirical IVs	
	Coef.	SE	Coef.	SE
Parental attitudes about your smoking				
Against				
Not against	0.09	0.09		
Don't Know	0.05	0.02**		
Father's smoking status				
Yes				
Used to, but quitted	−0.01	0.02	−0.01	0.02
Never	−0.06	0.01***	−0.06	0.01***
Sibling's smoking status				
Yes				
No	0.00	0.01		
No Siblings	0.00	0.04		
Do not Know	−0.02	0.03		
Whether relatives are sick due to smoking				
Yes				
No	−0.06	0.03*	−0.06	0.03*
Don't Know	−0.07	0.02***	−0.07	0.02***
Whether cigarettes are stored at home year-around				
Yes				
No	0.06	0.04		
Don't Know	0.03	0.01*		
Distance from home to nearest tobacco store (meters)				
⩽60				
>60 & ⩽400	0.04	0.02*		
>400	−0.03	0.02		
F-statistic	5.09		229.7	
Relevance test	0.05		0.00	
Overidentification test	0.21		0.56	
Adjusted *R*-squared	0.26		0.26	
Observations	1479		1479	

Note: Logistic regressions are used to predict best friend's smoking status from best friend's IVs and all covariates. Shown coefficients are for best friend's IVs only. Standard errors are clustered at the school level. The *P* values for the overidentification tests are calculated based on the non-clustered standard errors.
* Significance pattern: $P < 0.1$.
** Significance pattern: $P < 0.05$.
*** Significance pattern: $P < 0.01$.

Eqs. (3) and (4) also help us better understand the consequences of measurement error. First, measurement error in the outcome leads to a weaker correlation between peers' outcomes (i.e., $\widehat{Y}_J'Y_I$ is smaller), which will bias $\hat{b}_1$ toward to zero. Second, measurement error in the outcome may be absorbed into the error term and so unnecessarily inflates the variance of $\hat{b}_1$.

4. Results

Table 1 presents the summary statistics for selected student characteristics by best friend's smoking status (based on the refined measure of smoking). Compared to students whose best friend smoked cigarettes, students whose best friend did not smoke on average are younger, tend to be girls, are academically ranked higher, feel happier in school, have more educated father, and have better family economic condition. Except for the last two variables, the means of all other variables differ significantly across the two groups (all $P < 0.01$). In addition, there is a strong correlation in the smoking status of the students and their best friends. Among students whose best friend smoked, 41% of them smoked. In contrast, only 7% of the students whose best friend did not smoke smoked.

4.1. Assessing the quality of the IVs

I first conducted analysis to check both the strength and the exogeneity of the IVs. Table 2 shows the results of the first-stage regressions that predict the best friend's smoking status from the covariates and the best friend's IVs. Model 1 shows the estimated coefficients for the original six IVs. Except for sibling's smoking status, all other IVs are statistically significant predictors (all $P < 0.1$). Overall, the best friend's IVs are jointly significant predictors of the best friend's smoking status ($F = 5.09$, $P = 0.05$). Thus it appear that the IVs as a group satisfy the relevance condition.

In general, there is no way to empirically test the exclusion condition as we do not observe the error term. But as I have collected information to measure student's preference for making or not making friends with smokers, I can provide a partial test of that. Specifically, I use an ordinal logistic regression to predict the respondent's friendship preference based on the

Table 3
Estimated effects of best friend's smoking status on respondent's smoking status.

	A. Self-reported smoking status				B. Refined smoking status			
	(1) OLS-1	(2) OLS-2	(3) 2SLS-1	(4) 2SLS-2	(5) OLS-1	(6) OLS-2	(7) 2SLS-1	(8) 2SLS-2
Best friend's smoking	0.16**	0.12**	0.20**	0.34	0.25**	0.21**	0.40***	0.41***
	0.04	0.04	0.08	0.23	0.06	0.07	0.07	0.17
Endogeneity test			0.49	0.65			0.14	0.45
Adjusted R-squared	0.20	0.20	0.20	0.17	0.27	0.27	0.25	0.25
Observations	2049	1970	1479	1479	2049	1970	1479	1479

Note: OLS-1 shows the estimates when friend selection is not accounted for while OLS-2 shows the estimates when friend selection is accounted for. 2SLS-1 shows the estimates based on the original IVs and 2SLS-2 shows the estimates based on only the empirical IVs. For conciseness, the coefficients for other covariates are not shown. Standard errors are clustered at the school level and reported below the coefficients. Endogeneity test shows the P value testing the null hypothesis that best friend's smoking status is exogenous.
* Significance pattern: $P < 0.1$.
** Significance pattern: $P < 0.05$.
*** Significance pattern: $P < 0.01$.

nominated friend's IVs and covariates. The friend's IVs, if valid, should not correlate with the respondent's friendship preference. The regression results are shown in Table A2 in the Appendix. Three IVs including father's smoking status, sibling's smoking status, and whether having relatives who are sick due to smoking are not statistically significant predictors (all $P > 0.1$) and so appear to be valid IVs. Since sibling's smoking status does not satisfy the relevance condition, I use only the other two IVs as what I call the empirical IVs (namely, the IVs that are more likely to satisfy the exclusion condition). Column (2) of Table 2 shows their partial correlations with the friend's smoking status. The F-statistics testing their joint significance are much larger ($F = 229.71$, and $P < 0.01$), indicating they are much stronger IVs.

Table 2 also provides results for the overidentification test which evaluates the exogeneity of the IVs with the assumption that at least one of them is exogenous (Sargan, 1958).[9] Whether the original IVs or only the empirical IVs are used, there is no evidence to reject the null hypothesis that the IVs are exogenous ($P = 0.49$ and $P = 0.56$, respectively).

4.2. Estimated peer effects on smoking

Table 3 presents the estimated effects of best friend's smoking status on respondent's smoking status. When self-reported smoking status is used, the OLS estimates indicate that holding everything else constant, if a student's best friend smoked in the past 30 days, the student's probability of smoking during that period would increase by 0.16 ($P < 0.05$) when friend selection is not accounted for or by 0.12 ($P < 0.05$) when friend selection is accounted for. The 2SLS estimate based on the original IVs is 0.2 and statistically significant ($P < 0.05$). The 2SLS estimate based on the empirical IVs is 0.34, but statistically insignificant ($P = 0.14$). The insignificance could be a result of measurement error in the self-reported smoking status.

Panel B of Table 3 presents the main results with the refined smoking status (The complete results can be found in Table A3). The OLS estimates increase to 0.25 ($P < 0.05$) when friend selection is not accounted for or 0.21 ($P < 0.05$) when friend selection is accounted for. The 2SLS estimates increase to 0.4 ($P < 0.01$) when the original IVs are used or 0.41 ($P < 0.05$) when only the empirical IVs are used. The new 2SLS estimates are not only larger but also more precise.[10] These results suggest that measurement error in smoking has substantial consequences.

The endogeneity tests (Wooldridge, 1995) show that the best friend's smoking status may be exogenous conditional on the included covariates.[11] Hence, the OLS estimates probably are unbiased, but we would not know this have we not used the IVs. In addition, that the 95% confidence intervals of the OLS and 2SLS estimates overlap substantially just provides cross-cutting evidence on peer effects.

4.3. Addressing possible weak IV problems

Prior research (Bound et al., 1995; Staiger and Stock, 1997; Hahn and Hausman, 2002, 2003; Stock et al., 2002) has shown that if the correlation between the IV and the endogenous variable is weak, it can lead to large bias and uncertainty in the estimates.[12] I adopt four strategies to address possible such weak IV problems. First, I provide limited information likelihood (LIML)

[9] Note that heterogeneous peer effects may mud the validity of this test.
[10] Note that the estimated peer effects are conditional on the existing friendships. Hence they refer to what happens when an existing friend starts smoking rather than when finding a new friend who is a smoker.
[11] The endogeneity test amounts to adding the residuals from the first-stage regressions to Eq. (1) as an additional covariate and testing its statistical significance. If its coefficient is statistically significant, then it signals endogeneity of the suspected variable. This is because after teasing out the part induced by exogenous IVs, the residuals in the first stage regressions are supposed to include things that are correlated with the main outcome. Note that the test's validity is conditional on that the IVs are exogenous.
[12] Weak IVs lead to a smaller value in $(Y_J' P_Z Y_J)$, which tends to magnify finite sample bias from any chance correlation between Z and e, as indicated by Eq. (3). A smaller value in $(Y_J' P_Z Y_J)$ also tends to inflate the variance of $\hat{b}_1$, as indicated by Eq. (4).

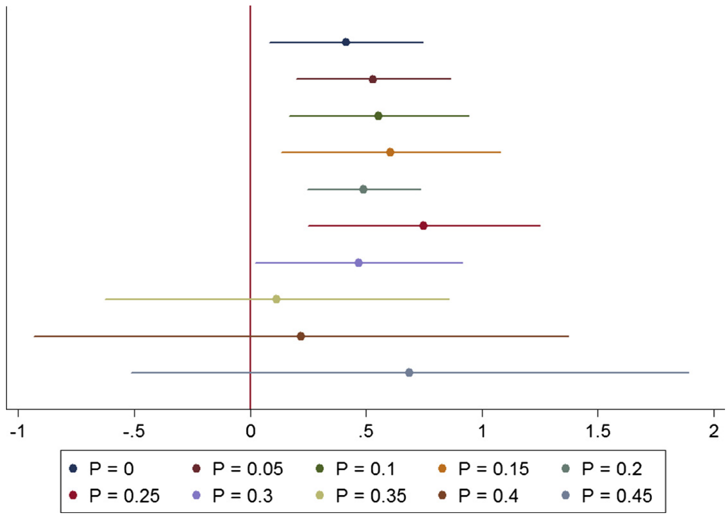

Fig. 2. Estimated peer effects by the strength of the simulated IVs. *Note*: P controls the strength of the IVs and is the proportion of random noise that is added to the empirical IVs. When it increases, the simulated IVs include more noise and so become weaker. Each bar represents the 95% confidence intervals for an estimate based a set of simulated IVs.

estimates. LIML estimates are less precise but also less biased (Angrist and Pischke, 2008: 213; Greene, 2008). The results in Table A4 show that the LIML estimate is 0.48 ($P < 0.01$) when the original six IVs are used and is 0.44 ($P < 0.05$) when the empirical IVs are used. Both are slightly larger than but still comparable to the corresponding 2SLS estimates.

Second, I provide estimates based on each IV, respectively. Sing IV estimate is just-identified and median-unbiased (Angrist and Pischke, 2008: 213). The results are shown in Table A5. The point estimates are comparable to the ones based on multiple IVs and their 95% CIs also overlap with those of the latter (See Fig. A1). By using the F-statistics for each individual IV in the first stage regression as a criterion, parental attitudes toward respondent's smoking stands out as the best IV. The corresponding estimate is 0.28 ($P < 0.1$). These results lend further support for the robustness of the 2SLS estimates.

Third, I fit a reduced-form regression that predicts respondent's smoking status from friend's IVs and covariates. Peer effects are proportional to the effects of the friend's IVs that are estimated in this regression. If the latter is zero, then the former may be zero too (Angrist and Krueger, 2001; Chernozhukov and Hansen, 2008). [13] The results are shown in the first column of Table A6. According to the F-test, the IVs are collectively significant predictors of respondent's smoking status whether the original IVs are used ($P = 0.01$) or only the empirical IVs are used ($P = 0.03$). Hence, we have evidence to support the necessary condition for there to be peer effects.

Fourth, I conduct sensitivity analysis by manipulating the strength of the IVs based on a approach similar to that proposed by Conley et al. (2012). Basically, I simulate new IVs (Z^*) by adding some noise to the original IVs according to

$$Z^* = (1 - P) \times Z + P \times S, \tag{5}$$

where S is a random variable representing noise. The value of P runs from 0 to 0.45 via a step of 0.05, with a larger value indicating more noise and weaker simulated IVs. I use the simulated IVs to re-estimate peer effects and examine how the estimates vary by the strength of the IVs. Fig. 2 presents the results. It is only when P reaches 0.35 (equivalently, 35% of each of the two empirical IVs is composed of noise) does the 95% CIs of the IV estimate start to contain zero. The results show that the IV estimates are reasonably robust to weak IV problems.

4.4. Addressing possible endogeneity of the IVs

I also conducted analysis to address possible endogeneity of the IVs. First, I fit another reduced-form regression that predicts respondent's smoking status based on the best friend's smoking status, the best friend's IVs, and covariates. The best friend's IVs, if valid, are expected to have no direct impact on the respondent's smoking once the best friend's smoking status is controlled for. Thus whether the coefficients for the IVs in this regression significantly differ from zero provides a (conditional) test of the exclusion condition.[14] The regression results are shown in the second column of Table A6. Some

[13] This can be better seen if we express the 2SLS estimator as a Wald estimator Cov(Z, Y_I)/Cov(Z, Y_J).

[14] The coefficients for the IVs show the direct effects of the IVs on respondent's smoking plus a bias term. Suppose U, Y_I, and Y_J are pairwise positively correlated. If Cov(Z, Y_J) > 0, then both the direct effects of the IVs and the bias tend to be positive. If the coefficients for the IVs are close to zero, then the direct effects of the IVs are close to zero too. Similarly is true when Cov(Z, Y_J) < 0.

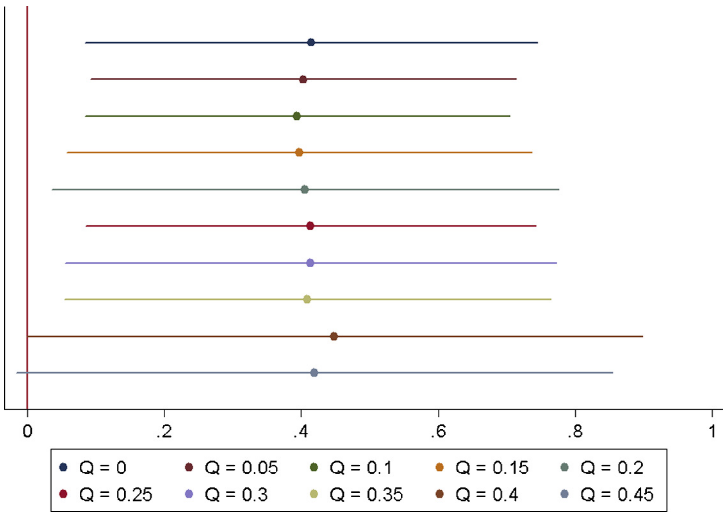

Fig. 3. Estimated peer effects by the hypothetical endogeneity of the simulated IVs. *Note:* Q controls the degree of endogeneity of the IVs. When it increases, the empirical IVs are viewed as more endogenous and a larger proportion of them is controlled for in the second stage regressions. Each bar represents the 95% confidence intervals for a corresponding estimate.

Table 4
Results of the falsification tests and matching analysis.

A. Falsification tests	Pseudo friend's smoking			Best friend's knowledge	
	2SLS-1	2SLS-2		2SLS-1	2SLS-2
	0.02	0.00		−0.29	−0.23
	0.16	0.33		0.28	0.31
B. Matching estimates	APE	APES	APEN	Sensitivity analysis	
	Est.	Est.	Est.	Gamma	*P*+
	0.12***	0.19***	0.10**	2.70	0.05
	0.04	0.06	0.04	3.00	0.10

Note: 2SLS-1 shows the estimates based on the original IVs and 2SLS-2 based on only the empirical IVs. Panel A shows the results based on pseudo friends and pseudo outcome, respectively. Panel B shows the matching estimates. APE refers to the average peer effect, APES the average peer effect for respondents whose best friend smoked cigarettes, and APEN the average peer effect for students whose best friend did not smoke. Sensitivity analysis is conducted for APES only. Gamma represents the odds ratio of two otherwise similar students selecting a smoker as the best friend due to the influence of an unobserved binary confounder (Rosenbaum, 2002). *P*+ is the upper bound of the *P* value for the estimated peer effect.
* Significance pattern: $P < 0.1$.
** Significance pattern: $P < 0.05$.
*** Significance pattern: $P < 0.01$.

IVs are predictive of respondent's smoking ($P < 0.01$), indicating their possible endogeneity. In contrast, the coefficients for the two empirical IVs are not statistically significant ($P = 0.33$), suggesting the results based on them are more credible.

Second, I select a subsample in which the respondents (approximately) do not know their best friend's father.[15] The IV estimates for this subsample should be more robust as the smoking environment at the best friend's home is less likely to directly affect the respondent's smoking. The results are shown in Table A7. The OLS estimate is 0.29 ($P < 0.01$) and the 2SLS estimate is 0.61 ($P < 0.01$). Both suggest significant and substantial peer effects.

Third, I conduct another sub-sample analysis in which the students are divided into three groups according to their friendship preference: one in which they reported they might or might not make friends with smokers, another one in which they reported that they would not make friends with smokers, and the third one in which they reported they would make friends with smokers. The first group indicated a weaker preference for making or not making friends with smokers than the latter two groups. Thus the IV estimates for the first group may be subject to less peer selection bias. The results are shown in Table A8. For the first group, both the OLS and the 2SLS estimates are statistically significant (Coef. = 0.21, $P < 0.05$ and Coef. = 0.6, $P < 0.1$, respectively). In contrast, for the respondents who indicated a strong preference for or against making

[15] In the survey I have designed a variable that measures whether a student's friend knows the respondent's father. Based on the responses to this question and assuming that the respondent knows the best friend's father as well as the best friend knows the respondent's father, I split the samples into two groups: one group in which the respondents (approximately) know their best friend's father and the other group they do not.

Table 5
Peer effects on the number of cigarettes smoked.

	(1) OLS	(2) OLS-S	(3) 2SLS-1	(4) 2SLS-2
Logged number of cigarettes smoked by the best friend	0.23**	0.19**	0.34***	0.42**
	0.06	0.06	0.09	0.20
F-statistic			2.17	28.34
Relevance test			0.21	0.00
Overidentification test			0.11	0.44
Endogeneity test			0.42	0.44
Adjusted *R*-squared	0.24	0.24	0.23	0.21
Observations	2049	1970	1479	1479

Note: OLS-1 shows the estimate when friend selection is not accounted for while OLS-2 shows the estimate when friend selection is accounted for. 2SLS-1 shows the estimate based on the original IVs and 2SLS-2 shows the estimate based on only the empirical IVs. For conciseness, the coefficients for other covariates are not shown. Standard errors are clustered at the school level and reported below the coefficients.
* Significance pattern: $P < 0.1$.
** Significance pattern: $P < 0.05$.
*** Significance pattern: $P < 0.01$.

friends with smokers, the OLS estimates and the 2SLS estimates are inconsistent. Thus the evidence on peer effects appears to be robust for the first group while less so for the other two groups.

Fourth, I provide sensitivity analysis by varying the degree of hypothetical endogeneity of the IVs (See Conley et al. (2012) and DiPrete and Gangl (2004) for similar approaches). Basically, I simulate variables W to represent some unobserved confounding factors and additionally control for W in the regressions. Specifically, the W is generated according to

$$W = Q \times Z + (1 - Q) \times T, \tag{6}$$

where Z is the best friend's IVs and T a random variable representing noise. The value of Q runs from 0 to 0.45 via a step of 0.05, with a larger value indicating a larger influence of the unobserved confounding factors. Fig. 3 presents the results. It is only when Q reaches 0.4 (equivalently, when the confounding factors represent 40% of the influence of each of the empirical IVs) does the 95% CIs of the IV estimate start to include zero. The results suggest that the IV estimates are robust to a moderate degree of endogeneity of the IVs.

4.5. Additional robustness checks

I also conducted additional robustness checks of the findings. First, I present two falsification tests. In the first test, I artificially pair each respondent with a randomly selected student in the same school and re-estimate the peer effects. I repeat this process 100 times (to even sample variation) and report the average estimates and standard errors.[16] The results are shown in the first part of Panel A in Table 4. As expected, neither of the average estimates based on the original and empirical IVs shows evidence for peer effect. In the second test, I use knowledge about tobacco use as a pseudo outcome for estimating peer effects.[17] Knowledge about tobacco use is measured according to a student' score in a six-question test about tobacco use included in the survey.[18] Because the test was answered independently by each student, a priori no peer effects are to be expected on this measure. The results are shown in the second part of Panel A in Table 4. Again, neither of the IV estimates indicates the best friend's knowledge about tobacco use affects the respondent's.

Second, I also estimate peer effects through matching.[19] Matching is nonparametric and so offers safeguards against possible model specification errors. The results are shown in Panel B of Table 4. The average peer effect (APE) is estimated to be 0.12 ($P < .01$). It appears that the average peer effect for the respondents whose best friend smoked (APES) is larger than the average peer effect for the respondents whose best friend did not smoke (APEN) (0.19 versus 0.1, but both $P < 0.05$). The effects seem to be quite robust according to the associated sensitivity analysis (Rosenbaum, 2002). It is not until the size of the potential bias gets to 3 does the estimated peer effect become indistinguishable from zero at the 0.1 significance level.[20] A bias size 3 is equivalent to that the odds of selecting a smoker as the best friend for students with similar profiles can differ as much as three

[16] In the literature, this test is called pseudo treatment test (Heckman and Hotz, 1989; Liu et al., 2010). By definition, a pseudo treatment is known to have null effect. If we find a significant effect from a pseudo treatment, it suggests the used model may not be reliable for drawing causal inference.

[17] By definition, a pseudo outcome is known a priori to be unaffected by the treatment. Precedents of the pseudo outcome test can be found in Cohen-Cole and Fletcher (2008b), for example.

[18] The test includes questions like "One out of how many smokers die of a disease attributable to smoking","What percent of fire incidences are caused by smoking yearly", etc. For each question students are provided with multiple choices to choose from.

[19] Specifically, I match each pair of respondent and friend with another pair in the same school who is most similar except that the friend's smoking status is different. Similarity between pairs is gauged by the Mahalanobis distance in the covariates, namely, $\sqrt{(X_i - X_j)^T A^{-1} (X_i - X_j)}$, where X_i and X_j are the covariates of two pairs, and A is the sample covariance of the covariates (Sekhon, 2011). Given the salient difference in smoking prevalence by gender, I also requested exact matching on gender between pairs.

[20] The sensitivity analysis is conducted only for APES due to the applicability of the method (Rosenbaum, 2002).

times because of the influence of an omitted binary confounder. Since a rich set of covariates (including friendship preference) have been included in the matching, it is doubtable there exists such a powerful confounder.

In addition, the results are also found to be robust to different thresholds for constructing the refined smoking status (See Fig. A2).

4.6. Causal mechanism

As a response to the call for mechanism-based research (Hedstrom and Swedberg, 1998; Pearl, 2000; Chapter 8 of Morgan and Winship, 2007), I investigate whether sharing cigarettes can be a mechanism for peer effects. Specifically, I examine whether there is a correlation in the (logged) number of cigarettes smoked by the respondents and their best friends by using the same regression models I have used to identify peer effects. Table 5 presents the results. It shows that if the number of cigarettes smoked by a student's best friend increased by 10% in the past 30 days, the number of cigarettes smoked by the student in the same time period would increase by about 2% according to the OLS estimates and about 4% by the more credible estimate based on the empirical IVs. To clarify, identifying sharing cigarettes as a plausible mechanism for peer effects does not preclude the operations of other mechanisms, for example, role modeling (Duncan et al., 1968; Christakis and Fowler, 2007; An, 2011b).

5. Conclusion and discussion

In this paper I used instrumental variable methods to estimate peer effects. The IV methods are appropriate because they help to address a variety of problems in estimating peer effects, such as contextual cofounding, peer selection, simultaneity bias, and measurement error. The findings of my study can be summarized as follows. First, I show that correcting measurement error is crucial for both accurate and precise estimates of peer effects. I find that the estimated peer effects are both larger and statistically more significant when a refined measure of smoking is used. Second, both the OLS and IV estimates show a significant and substantial peer effect on smoking. The OLS estimate with friend selection accounted for shows a peer effect of about one fifth ($P < 0.05$). The IV estimate shows even a larger peer effect: if a student's best friend has smoked in the past 30 days, that student is about 40 percentage points more likely to smoke in the same time period. Prior research (Imbens and Angrist, 1994; Angrist et al., 1996; Angrist and Pischke, 2008: 130) has shown that IV estimates tend to be local average treatment effects for compliers. Thus the larger IV estimate may only apply to a subset of the students, for example, those who will stick to their friend assignment regardless of their friend's smoking status. The findings are shown to be robust across a variety of robustness checks. Last, I show that sharing cigarettes can be a mechanism accounting for peer effects on smoking. Specifically, if the number of cigarettes smoked by a student's best friend increased by 10% in the past 30 days, the number of cigarettes smoked by the student in the same time period would increase by about 4%.

Despite these findings, there are some limitations in this study. First, I have focused on the effect of a single best friend and have assumed dyadic independence, namely, peer influence only operates within but not across friend pairs. This is a widely held assumption (e.g., Christakis and Fowler, 2007; Liu et al., 2010). However, future work can try to relax this assumption and examine peer effects from multiple contacts (Lee et al., 2010) and indirect peer effects. Second, the data comes from a particular site in China. It is unclear to what degree the results are applicable to other contexts. I would like to call for more studies to cross-validate or extend the findings.

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Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.ssresearch.2014.08.011.

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