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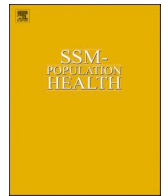
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## Peer effects in smoking cessation: An instrumental variables analysis of a worksite intervention in Thailand

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### ABSTRACT

While smoking is widely acknowledged to be a social activity, limited evidence exists on the extent to which friends influence each other during worksite-based tobacco cessation interventions. Drawing on data from adult smokers (N = 1823) in a large, cluster randomized controlled trial in worksites in Thailand, this study examines the presence of social spillovers in the decision to abstain from smoking. We leverage a unique aspect of social network structure in these data—the existence of non-overlapping friendship networks—to address the challenge of isolating the effects of peers on smoking behavior from the confounding effects of endogenous friend selection and bidirectional peer influence. We find that individuals with workplace friends who have abstained from smoking during the trial are significantly more likely to abstain themselves. Instrumental variables estimates suggest that abstinence after 3 and 12 months increases 26 and 32 percentage points, respectively, for each additional workplace friend who abstains. These findings highlight the potential for workplace interventions to use existing social networks to magnify the effect of individual-level behavior change, particularly in low- and middle-income countries where tobacco cessation support tends to be limited.

### 1. Introduction

Despite a decrease in the global prevalence of smoking over the past 30 years, tobacco use remains a significant public health concern across much of the developing world. Recent estimates suggest that tobacco smoking and secondhand smoke contributed to 6% of global disability-adjusted life years and approximately 6.4 million deaths in 2015, making it the second leading contributor to the disease burden worldwide (Reitsma et al., 2017). Approximately 80% of the world's smokers live in low- and middle-income countries (LMICs), where nearly three-quarters of all smoking-related deaths occur yet resources for prevention and cessation are limited (Jha et al., 2015). As such, developing innovative and cost-effective strategies to encourage sustained smoking cessation are priorities for both global public health and economic development.

There is widespread acknowledgement that smoking is a social activity and that one's smoking behavior may be in part influenced by those in her or his social network (Christakis & Fowler, 2008; Cutler & Glaeser, 2010). While a growing body of evidence points to the role of peers and friendship network characteristics as determinants of smoking among adolescents (Alexander et al., 2001; Ali & Dwyer, 2009; Fletcher,

2010; Fujimoto & Valente, 2012; Powell et al., 2005), less is known about the effects of peers on adult smoking, and most studies among adults have been correlational in nature (Christakis & Fowler, 2008; Hitchman et al., 2014). Moreover, despite the prevalence of smoking in these contexts, few studies have examined the role of peer influences among adults in LMICs.

Social network interventions are an increasingly common approach to modifying individual behaviors and enhancing the effectiveness of behavior change interventions (Valente, 2012). Several mechanisms may underlie the effects of peers on smoking behavior in our context, including social learning, changes in perceived smoking norms, direct support or encouragement from peers, and other pathways rooted in the structure and characteristics of social networks (see Hunter et al., 2019 for a review of social networks in the context of health). Team- or partner-based approaches, which draw on existing social ties to encourage peer-to-peer support or information spillovers, are often a key component of adult smoking cessation interventions (Faseru et al., 2018). In theory, such interventions may encourage individuals to modify their behavior by eliciting emotional or material support from friends, family members or co-workers, or by appealing to accountability, fear of social punishment, or shame.

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These intervention approaches may take a variety of forms, ranging from group therapy, to one-on-one cessation support, to team-based incentive designs in which rewards or penalties are contingent upon the behaviors of a group of smokers (Faseru et al., 2018; Stead et al., 2017; White et al., 2013). These team-based incentive designs may be particularly successful by leveraging peer pressure or peer support (Haisley et al., 2012; Kullgren et al., 2013; Patel et al., 2016) or by encouraging social interactions between teammates that result in individuals exerting more effort to achieve their goals (Babcock 2019). To date, however, the evidence is limited on the extent to which friends influence each other in the context of such interventions (Faseru et al., 2018; May & West, 2000; Westmaas et al., 2010), and even less is known about peer influence in workplaces, where adults spend a large amount of their time.

The potential for worksite smoking cessation interventions to generate social multipliers makes them especially promising approaches in low-resource settings. Worksite approaches may improve take-up rates, are easily scalable, and present an opportunity to reach many smokers simultaneously (Cahill & Lancaster, 2014). In addition, many adults form strong social ties at the workplace, and the size of work-based social networks has been shown to be associated with health status among older workers (Suzuki et al., 2009). However, while worksite interventions for smoking cessation have proven successful in some high-income countries, they remain underutilized and understudied in LMICs. In a recent systematic review, only 2 worksite-based studies took place in LMICs, and these studies examined the effectiveness of group-based behavioral therapy (India) and pharmacological approaches (Malaysia) – two intervention strategies with a stronger evidence base than peer or social support programs (Cahill & Lancaster, 2014).

The goal of this study is to examine the role of peer effects in the decision to abstain from smoking in a workplace setting in Thailand. As described in more detail below, this study examines peer effects among smokers using an instrumental variables analysis to identify peer effects independently from the confounding effects of endogenously formed peer groups and simultaneity. To our knowledge, this is the first study to explicitly examine peer effects among adult smokers in a low-resource setting, and it provides the first rigorous evidence of positive effects across friendship networks in worksites.

## 2. Methods

The SMILE Trial (Social and Monetary Incentives for Smoking Cessation at Large Employers) was a cluster randomized controlled trial designed to examine the effectiveness of monetary and social incentives to promote smoking cessation. The study took place in Thailand, where adult male smoking prevalence was 30.9% in 2015 (Reitsma et al., 2017). Our study targeted employees in 101 large factories ( $\geq 200$  workers each) in the Bangkok metropolitan area. The unit of randomization for this study was the worksite ( $N = 101$  worksites from 89 companies). Additional detail on company eligibility criteria and the recruitment of companies for participation in the study is presented elsewhere (White et al., 2020).

The 3-month intervention consisted of randomly assigning worksites to one of eight treatment arms or a control arm following a two-by-three factorial design. To mitigate within-worksite contamination and improve the likelihood of participation, the unit of randomization was the worksite, rather than the individual. The eight treatments included: 1) deposits boxes to which participants were encouraged to make voluntary monetary contributions throughout the 3-month period (all contents were forfeited if the participant failed to quit after 3 months); 2) individual bonus amounts of \$20 or \$40 (equivalent in Thai Baht) received by the participant contingent upon smoking abstinence after 3 months; or 3) a team bonus of \$40, received by each member of a randomly assigned pair contingent upon both participants successfully abstaining after 3 months (see Appendix Table A1). All study arms

received smoking cessation counseling with trained nurses at the time of enrollment.

Key eligibility criteria for participants included being a full-time employee aged 18 or older at a participating worksite, smoking  $\geq 10$  cigarettes per week on average, and wanting to quit smoking within 6 months. The main intervention sample included 4182 eligible individuals.

### 2.1. Data and key variables

This analysis combines biochemically-verified smoking status and survey data from trial participants collected at baseline, the start of the intervention, and after 3 and 12 months to quantify peer effects in smoking behavior. The primary outcome was smoking abstinence at the 3- and 12-month end points. The 3-month assessment occurred at the end of the intervention period and assessed the immediate program effects, while the 12-month assessment captured more persistent effects. The key independent variables leverage data on personal friendship networks of smokers as reported by each participant during the baseline survey. Participants were asked by trained enumerators to select their five closest friends from a list that contained the names of pre-identified smokers in each worksite, beginning with their best friend. Participants were assured that no information would be shared with anyone else in the worksite. This approach to name generation is commonly employed to identify friendship networks (Campbell & Lee, 1991) and has been used in recent studies of peer effects in smoking (Robalino & Macy, 2018).

For simplicity, we refer to the index person as “ego”, and their friend as “alter”; that is, each “ego” was asked to list up to five “alters” who they considered to be their closest friends at the worksite. From these data we constructed ego-centric measures of friendship networks including the overall number of alters in ego’s network, the size of ego’s reciprocal friendship network, and the mean number of abstainers in a group comprised of alter’s friends that did *not* include any of ego’s own friends or ego themselves. This non-overlapping peer group, referred to hereafter as “alter’s excluded network,” is central to our identification strategy described in Section 2.3.

Individual- and worksite-level characteristics collected at baseline were used as covariates in the regression analyses. Individual socioeconomic and demographic characteristics included age (18–25, 26–35, 36–45,  $\geq 46$ ), gender, household income per capita, educational attainment (0–3, 4–6, 7–12,  $\geq 13$  years) marital status (married, not married), any children, and place of childbirth (urban Thailand, rural Thailand, foreign country). Pre-intervention smoking characteristics included average number of cigarettes per day, number of past quit attempts, number of years since initiated smoking, an indicator for wanting to quit or not, and an indicator for moderate-to-high nicotine dependence as measured by a score of  $\geq 5$  on the Fagerström Test for Nicotine Dependence (Heatherton et al., 1991). At the worksite level, we control for the number of employees, the number of smokers at each worksite, the randomly assigned treatment arm, the field enumeration team tasked with implementing the surveys, and the number of “implementation issues” at baseline (e.g., non-cooperation or delays).

### 2.2. Analytic sample

The participation rate across all intervention arms ( $N = 4182$ ) was approximately 60 percent, with no significant differences in program acceptance across intervention arms. The overall abstinence rate at 12 months (primary outcome) was 15%, and we found similar rates of abstinence after 3 and 6 months (14% and 15% respectively). Abstinence rates at 12 months were significantly higher among individual bonus arms (15% and 22% for the \$20 and \$40 incentives, respectively) than those without bonuses (12.3%), and we found cessation rates to be lower among all team-based intervention arms (13.0%) (White et al., 2020). As described in more detail below, we restrict the sample in this

analysis to enrolled individuals who reported at baseline at least one friend who had a friend not listed by ego (i.e., at least one non-overlapping friend with ego). These restrictions reduced the final analytic sample size to 1823 at 100 worksites. Individuals who were listed as a friend by ego or as a friend by one of ego's friends (i.e., were included in alters' excluded network) but chose not to enroll in the study themselves were assumed to be continuing smokers. The decision to classify participants with missing outcome data as continuing smokers is a common approach in tobacco cessation trials as it is thought to yield conservative estimates of cessation (Hall et al., 2001; Nelson et al., 2009).

Table 1 presents baseline sociodemographic, smoking, social network, and worksite characteristics of all enrolled participants (column 1) and the main analytic sample of enrollees who met the criteria above (column 2). The typical smoker who enrolled in the study can be characterized as male, married, has 7–12 years of education, smokes approximately 8 cigarettes per day, has smoked for over 15 years, has tried to quit twice previously, and wants to quit smoking within 3 months. On average, each participant reported about 4 friends at baseline, one of whom was reciprocal (ego listed alter and vice versa), and were listed as a friend by 3 others (range 0–22). Individuals in this sample averaged having between 8 and 9 non-overlapping friends (i.e., friends of alter that do not include ego or ego's friends). Among the analytic sample, 348 (19%) and 332 (18%) successfully abstained at 3 and 12 months, respectively. The overall correlation between abstinence status at both endpoints was 0.57. Of the 348 enrollees who abstained

**Table 1**  
Baseline Characteristics among all enrolled participants.

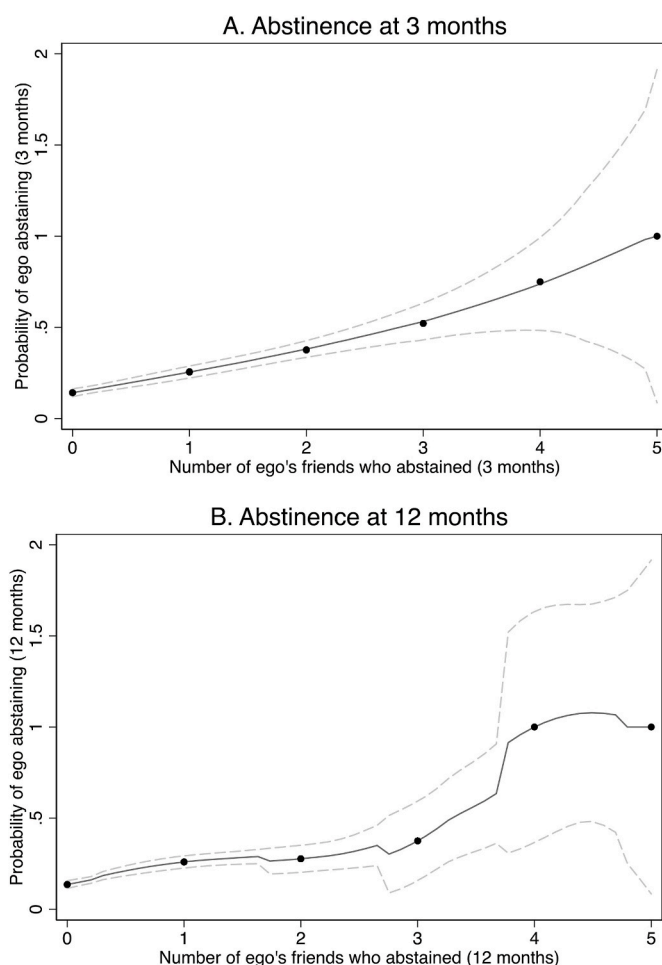
	(1) All enrollees (n = 2458)	(2) Analytic sample (n = 1823)
<b>Panel A. Sociodemographic characteristics</b>		
Age (%)		
18-25	510 (20.7%)	363 (19.9%)
26-35	961 (39.1%)	734 (40.3%)
36-45	672 (27.3%)	500 (27.4%)
46+	315 (12.8%)	226 (12.4%)
Male (%)	1776 (97%)	1776 (97.4%)
Mean household income per capita in \$100s (SD)	3.0 (1.8)	3.0 (1.8)
Education level		
0-3 years	93 (3.8%)	49 (2.7%)
4-6 years	592 (24.1%)	403 (22.1%)
7-12 years	1367 (55.6%)	1052 (57.7%)
13+ years	406 (16.5%)	319 (17.5%)
Married (%)	1726 (70.2%)	1305 (71.6%)
Any children (%)	1133 (46%)	878 (48.0%)
Place of childhood (%)		
Urban Thailand	568 (24%)	436 (23.9%)
Rural Thailand	1522 (61.9%)	1183 (64.9%)
Foreign country	368 (15.0%)	204 (11.2%)
<b>Panel B. Smoking characteristics</b>		
Pre-trial cigarettes per day, mean (SD)	7.6 (5.8)	7.8 (5.8)
Pre-trial nicotine dependent (%)	274 (11.1%)	206 (11.3%)
No. past quit attempts, mean (SD)	1.9 (2.3)	1.9 (2.3)
No. years since initiated smoking, mean (SD)	15.5 (9.1)	15.5 (8.9)
Want to quit within 3 months (%)	1470 (59.8%)	1083 (59.4%)
Quit at 3-month follow-up (%)	482 (19.6%)	348 (19.1%)
Quit at 12-month follow-up (%)	467 (19.0%)	332 (18.2%)
<b>Panel C. Friend network characteristics</b>		
No. friends reported at baseline, mean (SD)	3.1 (1.9)	3.8 (1.5)
No. reciprocal friends at baseline, mean (SD)	0.8 (1.0)	1.0 (1.1)
No. times listed as friend, mean (SD)	2.5 (2.4)	3.0 (2.5)
No. excluded friends in network (SD)	6.2 (6.2)	8.4 (5.8)
<b>Panel D. Worksite characteristics</b>		
No. employees at worksite, mean (SD)	1050.2 (1012.1)	946.2 (917.9)
No. smokers per worksite, mean (SD)	298.3 (273.2)	294.3 (284.6)

**Notes:** Enrollees are defined as all eligible employees who elected to participate in the study. The analytic sample is restricted to enrollees who reported having at least one friend at baseline and whose friend(s) had at least one non-overlapping friend with ego.

after 3 months, 222 (64%) continued to abstain after 12 months (Appendix Table A2). Appendix Table A3 presents normalized differences between the analytic sample (N = 1823) and the enrolled participants who were excluded from the current analysis (N = 635). With the exception of the enrollees' place of childhood, worksite size, and the characteristics of ego's friend network (the latter of which demonstrates substantial differences between the two samples as a result of the inclusion criteria), the two samples are well-balanced across covariates according to the commonly-used threshold of normalized differences below 0.25 (Imbens & Rubin, 2015).

Fig. 1 shows the relationship between the number of ego's friends who abstained and ego's probability of abstaining at 3 and 12 months. The relationship for 3-month abstinence is strikingly linear. The relationship for 12-month abstinence is somewhat less linear but still displays a positive association: ego's probability of abstaining increases as the abstaining friend count increases from 0 to 1 and even more so from 3 to 4. For both endpoints, the 95% confidence bands increase substantially above 3 friends, reflecting the relatively few enrollees in the sample who had more than 3 friends abstain. The average number of abstaining friends at 3 and 12 months was 0.42 and 0.43, respectively.

These figures indicate that individuals are more likely to abstain when their friends successfully abstain; however, while this descriptive analysis is suggestive, it may be confounded by a combination of identification issues described below. In the following sections, we build on



**Fig. 1.** Probability of ego abstaining by number of friends who abstained  
**Notes:** The figures above show the relationship between the number of ego's friends who abstained and the mean abstinence probability among all individuals in each bin. The nonparametric regression line and 95% confidence band (dashed line) is estimated using a second-degree polynomial smooth with an Epanechnikov kernel.

these descriptive results using an instrumental variables framework to more plausibly identify the effect of friends' smoking behavior on ego's own outcomes.

### 2.3. Empirical strategy

A large body of theoretical and empirical literature highlights at least two challenges for identifying peer effects (Advani & Malde, 2018; Angrist, 2014; Sacerdote, 2014). First, individuals may sort into peer networks based on unobservable characteristics that make network members more or less likely to smoke, a phenomenon known as endogenous peer selection. Second, researchers often cannot distinguish the effect of group behavior on an individual from the effect of an individual on the group due to the potential simultaneous occurrence of these actions (Manski, 1993). Manski (1993) termed this simultaneity the "reflection problem." Not accounting for these factors can lead to biased estimates of peer effects, and explicitly accounting for these identification concerns has been the focus of a rapidly expanding literature examining peer effects across domains (for example, see Nicoletti & Rabe, 2019, and Babcock et al., 2019 for recent applications in school achievement and health behaviors, respectively).

We address the concerns of endogenous selection and reflection by following an identification strategy introduced by Bramoullé et al. (2009) and De Giorgi et al. (2010). This strategy leverages the fact that peer groups are formed at the individual level and are rarely perfectly overlapping with one another; this results in a group of "excluded", or non-overlapping, peers of peers for each individual. The mean outcome among the excluded network serves as an instrument for outcomes among ego's immediate friends under the assumptions that ego is not directly influenced by their excluded peers and that the mean outcome among alter's friends is uncorrelated with group-level shocks that simultaneously affect ego's friends.

In the present analysis, we adopt this identification strategy and use information on self-reported friendships of co-workers measured at baseline to generate non-overlapping groups of alters' friends for each study enrollee. Letting  $y_i$  denote the biochemically verified smoking status for ego  $i$ , (binary for abstained or not) at the 3- and 12-month endpoints, our objective is to estimate the following equation for each individual  $i$ :

$$y_i = \alpha + \beta G_i + \gamma_1 SIZE_i + \gamma_2 TREAT_i + \delta X_i + u_i \quad (1)$$

In this model,  $G_i$  is the number of abstainers in ego's self-reported friend network,  $SIZE$  is a variable between 0 and 5 denoting the size of ego's friend network,  $TREAT_i$  denotes ego's randomly assigned intervention arm, and  $X_i$  is a vector of ego's sociodemographic and smoking characteristics reported at baseline. We instrument for  $G_i$  using the mean count of abstainers in alter's excluded network. Formally, we define the variable  $q_j$  as the number of abstainers in each alter  $j$ 's excluded network, where  $j$  ranges between 1 and 5. The instrument  $Z_i$  is constructed as the sum of ego's  $j$  friends who abstained divided by the size of ego's network:

$$Z_i = \frac{\sum_{j=1}^5 q_j}{SIZE_i} \quad (2)$$

We calculate  $Z_i$  separately for smoking status at 3 and 12 months and use this instrument to estimate the first and second stage of the following two-stage instrumental variables (IV) procedure:

$$G_i = \alpha_1 + \beta_1 Z_i + \gamma_1 TREAT_i + \delta_1 X_i + \nu_i \quad (3)$$

$$y_i = \alpha_2 + \beta_2 \hat{G}_i + \gamma_2 TREAT_i + \delta_2 X_i + u_i \quad (4)$$

In Eq. (3), we regress the number of abstainers in ego's self-identified friend group on the instrument  $Z_i$ , controlling for treatment arm assignment, ego's characteristics (including the number of ego's friends), and worksite-level covariates. In the second stage (Eq. (4)) we

regress ego's smoking status on the fitted values from the first stage and control for the same covariate set. If  $Z_i$  is a valid instrument for  $G_i$ , we can recover the effect of the number of abstainers in ego's network on ego's smoking status as the coefficient  $\beta_2$ . The direct effect of the mean number of abstainers in alter's excluded network on ego's own smoking status is represented by  $\beta_0$  in the following reduced-form equation:

$$y_i = \alpha_0 + \beta_0 Z_i + \gamma_0 TREAT_i + \delta_0 X_i + u_i \quad (5)$$

Importantly, the analysis rests on two key assumptions about the instrument. First, there must be a strong association between the number of abstainers in alter's excluded friend network ( $Z_i$ ) and the number of abstainers in ego's immediate friend network ( $G_i$ ). The second assumption, which is not empirically testable, states that the instrument only influences ego's smoking status through its impact on ego's immediate friend network.

We estimate IV-probit models and express estimates as average marginal effects (i.e., average risk differences, as calculated using the margins command in Stata 14) at 3- and 12-month endpoints. For comparison, we also estimate a linear probability model using a two-stage least squares procedure, as well as a probit model that assumes strict exogeneity. Using the linear probability model, we estimate the Montiel Olea-Pflueger effective F-statistic for excluded instruments, which tests the strength and relevance of our proposed instrument in a setting with non-homoskedastic errors (Andrews et al., 2019). All analyses are performed in Stata, v14.2 (StataCorp, 2015) using robust standard errors clustered at the worksite level.

## 3. Results

### 3.1. Association between abstinence of ego and peers

Table 2 presents characteristics of ego's friend network overall (column 1) and stratified by ego's smoking status at 3 and 12 months (columns 2–3 and 5–6, respectively). Columns 4 and 7 present p-values calculated from Pearson chi-squared tests (for categorical variables) or t-tests (for continuous variables) for differences in means between continuing smokers and abstainers. Continuing smokers had slightly larger friend networks than those who abstained (3.9 versus 3.7 friends reported), a difference that was statistically significant at 3 months and approached statistical significance at 12 months (panel A). At both end points, abstainers had fewer total non-overlapping friends with their friends than continuing smokers, but the number of abstainers among their excluded network was significantly higher as compared to continuing smokers.

Panel B in Table 2 extends the descriptive results presented in Fig. 1 to illustrate the relationship between ego's smoking status and that of ego's immediate friends. Those who had a larger proportion of friends who abstained at both endpoints were more likely to abstain themselves: 34% of abstainers had at least one friend in their immediate friend network who abstained compared to only 23% among continuing smokers ( $p < 0.001$ ). Furthermore, 16% of successful abstainers had more than one friend who also successfully abstained at 3 months, compared to only 5.4% of continuing smokers ( $p < 0.001$ ). The patterns are similar for abstinence at 12 months. We also see that successful abstainers were more likely to have best friends who also abstained than continuing smokers: 26% versus 11% at 3-months ( $p < 0.001$ ) and 23% versus 12% at 12-months ( $p < 0.001$ ).

### 3.2. Effect of friends' quitting on ego's quit status

As motivation for the IV analysis, Fig. 2 shows the reduced-form relationship between the mean number of abstainers in alter's excluded network (the instrument) and ego's abstinence probability. The instrument ranges from 0 to 2.5 at 3 months and 0 to 3 at 12 months, with mean 0.21 (SD 0.35) at both endpoints. Fig. 2 displays mean abstinence probabilities at each value of the instrument combined with a



**Table 2**  
Network characteristics, overall and stratified by ego’s smoking status at 3- and 12-month assessments (main analytic sample).

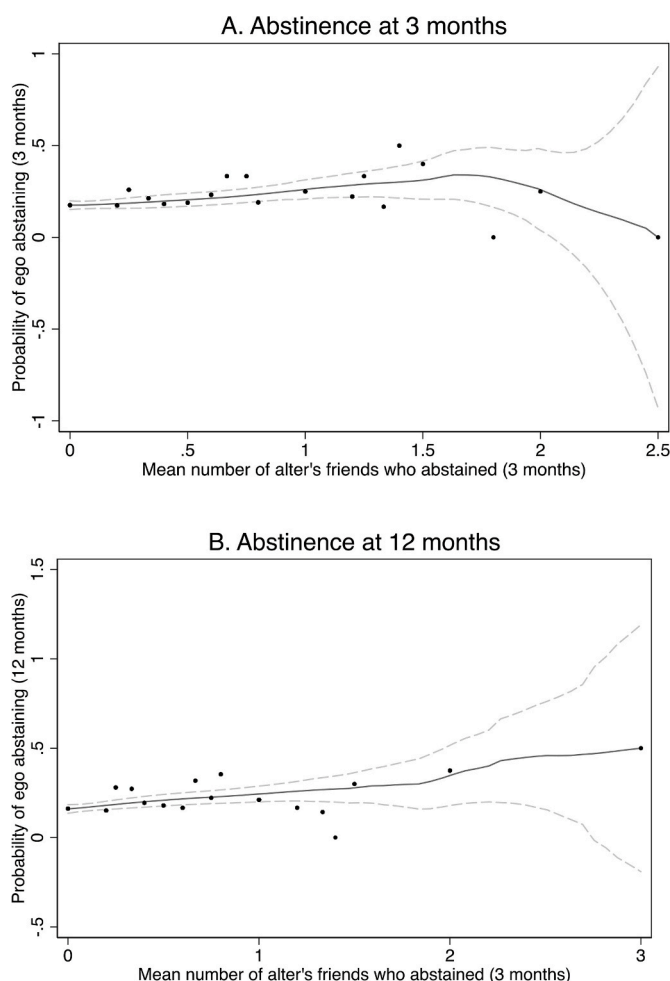
	Full sample	Abstinence at 3 months			Abstinence at 12 months		
	(1) (n = 1823)	(2) Smoke	(3) Abstain	(4) p-value	(5) Smoke	(6) Abstain	(7) p-value
<b>Panel A. Characteristics of ego’s friend network</b>							
Total number of friends reported	3.84 (1.47)	3.87	3.69	0.040	3.87	3.70	0.067
Total number in excluded network	8.39 (5.78)	8.56	7.70	0.013	8.53	7.79	0.036
Mean count of abstainers in alter’s excluded network (3 mo)	0.238 (0.364)	0.230	0.290	0.003	–	–	–
Mean count of abstainers in alter’s excluded network (12 mo)	0.248 (0.365)	–	–	–	0.234	0.313	<0.001
<b>Panel B. Abstinence among ego’s friends</b>							
<b>Abstainers among friends at 3 months (%)</b>							
No friends abstained	1228 (67.4%)	71.5%	50.0%	<0.001			
1 friend abstained	458 (25.1%)	23.1%	33.6%	<0.001			
2–5 friends abstained	137 (7.5%)	5.4%	16.4%	<0.001			
Best friend abstained at 3 months (%)	219 (14.0%)	11.1%	25.8%	<0.001			
<b>Abstainers among friends at 12 months (%)</b>							
No friends abstained	1187 (65.1%)				68.8%	48.5%	<0.001
1 friend abstained	505 (27.7%)				25.1%	39.5%	<0.001
2–5 friends abstained	131 (7.2%)				6.10%	12.0%	<0.001
Best friend abstained at 12 months (%)	216 (13.8%)				11.7%	22.9%	<0.001

Notes: Standard deviations reported in parentheses for continuous variables. P-values for difference in means between abstainers and continuing smokers are calculated from Pearson chi-squared tests (categorical variables) or t-tests (continuous variables).

second-degree polynomial smooth plot of the bivariate relationship. At both endpoints, the effect of abstainers in alter’s excluded network on ego’s abstinence is positive and appears to increase linearly across most values of the domain, although the small sample size at the higher values of the instrument leads to large 95% confidence interval bands.

Table 3 displays the main regression estimates from the probit and IV-probit models with three sets of control variables, where panels A and B present marginal effect estimates of ego’s likelihood of quitting at 3 months and 12 months, respectively. Each column shows two specifications: a raw probit model using the count of ego’s friends who abstained as the primary independent variable (left sub-column), and an IV-probit specification using the instrument described above (right sub-column). The estimates in the first row of each panel in Table 3 can be interpreted as percentage-point changes in ego’s likelihood of abstaining given one additional friend who abstained. In the probit and IV-probit specifications we see positive and statistically significant evidence of peer effects at both endpoints. In the unadjusted model (column 1), the IV estimates suggest that an additional abstaining friend increases the likelihood of ego abstaining by 26 percentage points after 3 months, and by 32 percentage points after 12-months ( $p < 0.001$ ). Adding socio-demographic, smoking, and worksite-level covariates does not substantially alter the estimates in either model. In general, the IV estimates are much larger in magnitude than the probit estimates—nearly three times as large at 3 months (e.g., 0.267/0.097 from column 3, panel A) and 3–5 times larger at 12 months. The effects are only slightly smaller in magnitude when estimating the same models on the larger sample of all enrollees (Appendix Table A6).

The first-stage regressions for the fully adjusted model (column 3 in Table 3) are presented in Appendix Table A7. While the coefficients on the instruments at 3 and 12 months are statistically significant (0.184 and 0.114,  $p < 0.01$ ), the Kleibergen-Paap F-statistics are 8.41 and 7.47, respectively. This is below the oft-used threshold of 10, suggesting that both instruments are somewhat weak according to traditional metrics. The preferred Montiel-Pflueger effective F-statistic yields only slightly larger values. Following emerging best practices in cases of IV models with weak instruments, we conduct Anderson-Rubin (AR) chi-squared tests of the coefficients on the endogenous regressor in each model; under classical assumptions, this test is robust to weak instruments (Andrews et al., 2019). The p-values are all below 0.01 (with one exception that is less than 0.05), suggesting that we can confidently reject the null hypothesis that the coefficients on even the weakly identified endogenous regressors are equal to zero.



**Fig. 2.** Probability of ego abstaining by mean number of alters’ friends who abstained

Notes: The figures above show the reduced-form relationship between the mean number of alter’s friends who abstained (excluding ego and overlapping friends with ego) and the mean abstinence probability of ego at each value of the instrument. The nonparametric regression line and 95% confidence band (dashed line) is estimated using a second-degree polynomial smooth with an Epanechnikov kernel.

**Table 3**  
Marginal effects from probit and IV-probit models (main analytic sample).

	(1) Unadjusted		(2) Partially adjusted		(3) Fully adjusted	
	Probit	IV-Probit	Probit	IV-Probit	Probit	IV-Probit
<b>Panel A: Abstinence at 3 months</b>						
Count of ego's friends who abstained (percentage point change)	0.111*** (0.013)	0.258*** (0.063)	0.106*** (0.013)	0.267*** (0.060)	0.097*** (0.014)	0.267*** (0.068)
Pseudo-R <sup>2</sup>	0.053	–	0.097	–	0.105	–
Montiel-Pflueger Effective F-statistic	–	10.02	–	9.643	–	8.493
P-value from AR weak-IV-robust test	–	0.0082	–	0.0070	–	0.0146
<b>Panel B: Abstinence at 12 months</b>						
Count of ego's friends who abstained (percentage point change)	0.090*** (0.012)	0.318*** (0.054)	0.079*** (0.012)	0.319*** (0.058)	0.069*** (0.013)	0.339*** (0.067)
Pseudo-R <sup>2</sup>	0.042	–	0.082	–	0.093	–
Montiel-Pflueger Effective F-statistic	–	10.88	–	10.99	–	7.539
P-value from AR weak-IV-robust test	–	0.0019	–	0.0028	–	0.0095
<b>Controls</b>						
Sociodemographics	N	N	Y	Y	Y	Y
Smoking characteristics	N	N	Y	Y	Y	Y
Worksite variables	N	N	N	N	Y	Y
Number of observations	1823	1823	1821	1821	1821	1821
Number of clusters	100	100	100	100	100	100

Notes: Table displays marginal effects from probit and instrumental variables (IV) regressions of ego's abstinence on the count of ego's friends who abstained at 3 months (panel A) and 12 months (panel B). All regressions include controls for treatment arm and the size of ego's friend network. Standard errors in parentheses are clustered at the worksite level. \*p < 0.05, \*\*p < 0.01, \*\*\*p < 0.001.

3.3. Validity of the instrument

The validity of the instrument used in this analysis rests upon the key assumption that the mean count of abstainers in alter's excluded network is uncorrelated with unobserved determinants of ego's smoking status and affects ego directly through friends' smoking status. While this is an untestable assumption, we can begin to assess the plausibility of this exclusion restriction by observing how the instrument varies across ego's observed characteristics. Table 4 presents a comparison of ego's baseline characteristics stratified by the dichotomized instrumental variable at the 3-month assessment (Appendix Table A9 presents 12-month results). We define Z<sub>i</sub> equal to 1 if any member of alter j's excluded network abstained at 3 months, and Z<sub>i</sub> equal to 0 otherwise. Column 3 displays the difference in means for each baseline characteristic.

Most sociodemographic and smoking characteristics appear to be balanced across the dichotomized instrument, with some small, but statistically significant, exceptions. Participants with ≥1 excluded friend who abstained tended to be slightly older, more likely to be born in rural Thailand, and less likely to be foreign-born. These participants also reported smoking significantly more cigarettes at baseline and initiated smoking at a younger age. Despite the small magnitude of these differences, we may still be concerned that these factors are correlated with unobserved characteristics that influence smoking decisions among ego or ego's friends. However, including these covariates in the regression specifications in columns 2 and 3 of Table 3 does not substantially alter the point estimates relative to column 1, suggesting that potential unobserved correlates of these variables are also unlikely to be confounding the IV estimates.

To examine another source of potential bias in the IV estimates, Table 5 presents results from a simulation in which we introduce classical measurement error in the number of abstaining friends (the endogenous variable) and re-estimate the fully adjusted models from Table 3. Under an assumption of classical measurement error in the endogenous variable and/or IV—that is, random noise that is uncorrelated with all variables in the model and the error term – we would expect estimates from the raw probit model to be attenuated but not those from the IV model (Greene, 2008).

To simulate this measurement error, we add random noise to the number of ego's friends who abstained by drawing a random variable from a standard normal distribution (mean of 0 and standard deviation

**Table 4**  
Baseline characteristics for ego, by dichotomized instrument at 3-months.

	Value of dichotomized IV		Difference in means: (2)–(1)
	(1) Z = 0	(2) Z = 1	
	<b>Socioeconomic and demographic characteristics</b>		
Age (SD)	33.3 (8.97)	34.7 (8.82)	1.401***
Male	96.7	98.1	1.167
Mean household income per capita in \$100s (SD)	2.99 (1.87)	3.04 (1.82)	0.0488
<b>Education level</b>			
0–3 years	3.01	2.30	–0.703
4–6 years	22.7	21.5	–1.19
7–12 years	57.1	58.6	1.53
13+ years	17.3	17.7	0.362
Married	71.0	72.2	1.20
Any children	47.6	48.9	1.25
<b>Place of childhood</b>			
Urban Thailand	23.8	24.1	0.374
Rural Thailand	62.0	68.4	6.34**
Foreign country	14.2	7.52	–6.71***
<b>Smoking characteristics</b>			
Pre-trial cigarettes per day	7.42 (5.62)	8.34 (5.98)	0.916***
Pre-trial nicotine dependent	12.5	9.82	–2.71
Number of past quit attempts	1.89 (2.31)	1.98 (2.26)	0.0878
Number of years since initiated smoking	14.9 (9.06)	16.1 (8.74)	1.15**
Want to quit within 3 months	59.8	58.9	–0.911

Notes: Columns 1 and 2 present means and standard deviations (in parentheses) of each variable, stratified by a binarized version of the instrument. The binary instrument takes on a value of 1 if any member of alter's network (excluding ego and ego's mutual friends) abstained at 3 months, and a value of 0 otherwise. The final column reports the difference in means between values of the dichotomized instrument. \*p < 0.05, \*\*p < 0.01, \*\*\*p < 0.001.

of 1) multiplied by the standard deviation of the number of ego's friends who abstained. Measurement error in the instrument is constructed similarly by adding noise drawn from a separate standard normal distribution to the number of abstainers in each alter's excluded friend network and then taking the mean across all alters in ego's network.

**Table 5**  
Simulation with measurement error in number of abstaining friends (main analytic sample).

	(1) Base case		(2) Error added to endogenous var.		(3) Error added to endogenous var. and IV	
	Probit	IV-Probit	Probit	IV-Probit	Probit	IV-Probit
<b>Panel A: Abstinence at 3 months</b>						
Count of ego's friends who abstained (percentage point change)	0.097*** (0.014)	0.267*** (0.068)	0.058*** (0.009)	0.235*** (0.039)	0.058*** (0.009)	0.285*** (0.065)
Pseudo-R <sup>2</sup>	0.105	–	0.0946	–	0.0898	–
Montiel-Pflueger Effective F-statistic	–	8.493	–	4.940	–	0.606
P-value from AR weak-IV-robust test	–	0.0146	–	0.0130	–	0.2348
<b>Panel B: Abstinence at 12 months</b>						
Count of ego's friends who abstained (percentage point change)	0.069*** (0.013)	0.339*** (0.067)	0.037*** (0.009)	0.291*** (0.030)	0.037*** (0.009)	0.309*** (0.033)
Pseudo-R <sup>2</sup>	0.0927	–	0.0849	–	0.0831	–
Montiel-Pflueger Effective F-statistic	–	7.539	–	2.521	–	0.255
P-value from AR weak-IV-robust test	–	0.0095	–	0.0104	–	0.1736
Number of observations	1823	1823	1821	1821	1821	1821
Number of clusters	100	100	100	100	100	100

*Notes:* Table displays marginal effects from regressing ego's abstinence at 3- and 12-months on the number of ego's friends who abstained, controlling for all covariates in model 3 of Table 4. Estimates in column 1 are identical to those in column 3 in Table 3. Column 2 simulates measurement error (ME) in the number of ego's friends who abstained by adding one standard deviation of variation to each observation drawn from a standard normal distribution. Column 3 estimates the model with ME in the number of ego's friends who abstained *and* in the number of abstainers in each alter's excluded network. \* $p < 0.05$ , \*\* $p < 0.01$ , \*\*\* $p < 0.001$ .

Column 2 in Table 5 presents estimates from both specifications after introducing this measurement error in the endogenous variable, and column 3 presents estimates after introducing measurement error in both the endogenous variable and the instrument. Relative to column 1 (the same estimates as column 3 in Table 3) the introduction of measurement error in the endogenous variable only (column 2) decreases the magnitude of the probit estimates by half, while the IV estimates are unchanged. Further, adding measurement error to both (column 3) reduces the estimated effects in the probit model relative to column 1 while those from the IV model do not meaningfully change.

#### 4. Discussion

In light of the high prevalence of smoking and smoking-related illness in low- and middle-income countries, there is a critical need for cost-effective approaches for tobacco cessation. In general, little is known about the efficacy of abstinence programs in these settings, and even less is known about worksite-based approaches. The current analysis finds evidence of worksite peer effects in smoking cessation among adults in Thailand. We find that individuals with abstaining friends were substantially more likely to abstain themselves after a 3-month smoking cessation intervention and after 12 months. To our knowledge this is the first study to use experimental data to identify causal peer effects among adult smokers in a LMIC, despite the fact that nearly three-fourths of smoking-related deaths occur in these settings.

Our findings are consistent with a limited but growing literature on peers as a determinant of adult smoking behavior, yet the majority of existing studies have methodological limitations that prevent investigators from drawing causal inferences. One notable exception is an instrumental variables analysis conducted by Cutler and Glaeser (2010), which leveraged exposure to workplace smoking bans as an instrument to estimate the effect of smoking abstinence among spouses. This study estimated a 40% decrease in the probability of smoking if one's spouse abstained from smoking, an effect similar in magnitude to our study finding among workplace friends.

The potential to harness social multipliers in smoking behavior is especially relevant for smoking cessation efforts in low-resource settings, where access to state-of-the-art pharmacotherapy or comprehensive cessation services is limited. However, little evidence exists on the extent to which friends influence each other during tobacco cessation interventions, and even less is known about the effect of these approaches in LMICs. A meta-analysis of 14 high-quality randomized

controlled trials leveraging peer or social support as the key intervention component included only one trial in a low-income setting (Indonesia) and rated the overall quality of the evidence across all studies as low (Faseru et al., 2018). In their review, Faseru and colleagues attribute an overall lack of effectiveness in peer support interventions to the difficulty in successfully increasing the support that smokers receive from peers.

The present study, in which we find evidence that individuals are more likely to abstain if their friends also abstain, points to the role of leveraging naturally-forming friendship networks as a strategy to overcome this challenge. In the main intervention trial, we did not find significant increases in abstinence among participants in any of the treatment arms that involved random assignment to teammates within the same worksite (White et al., 2020). In a similar team-based intervention for smoking cessation in villages in Thailand, White and Dow (2014) find that the strength of social ties (as measured by the type of pre-existing relationship between partners) was associated with the likelihood of abstaining. These pieces of evidence are consistent with existing research suggesting that team or "buddy" interventions with endogenously-formed support groups (or family members) are likely more efficacious than those with random teammate assignment (Babcock et al., 2019; Carrell et al., 2013; Cutler & Glaeser, 2010).

This study has several strengths, including the use of biochemically verified quit status as the primary outcome measure as opposed to self-report data. By assessing smoking behavior after 3 months and one year, the present study is also among few to examine the short-term and sustained effect of a smoking cessation interventions in a LMIC setting. From a methodological perspective, the present study leverages the presence of non-overlapping friendship networks to address the identification challenges related to endogenous peer selection and simultaneity biases. While many existing studies rely on randomized peer groups to identify social spillovers, naturally-forming peer groups are likely more plausible settings for most public health interventions in workplaces and schools.

This study also has several limitations. As is true in all instrumental variables analyses, the plausibility of the exclusion restriction cannot be directly tested. In our study, non-overlapping friendship networks may not be fully exogenous to index persons, although we use networks measured at baseline to mitigate the possibility that smoking behavior may affect characteristics of ego or alters' networks. Nonetheless, it is possible that unobserved characteristics of ego's excluded friends are correlated with ego's abstinence, a violation of the exclusion restriction



that could bias our estimated effects. Further, existing papers demonstrate that two-stage least squares estimators with weak instruments may perform poorly in finite samples, even when sample sizes are larger than the present study (Bound et al., 1995; Wooldridge, 2010). To the extent that our IV approach may suffer from finite sample bias, we may in fact be underestimating the true IV effect. An additional challenge in our estimation approach is the use of a two-stage instrumental variable procedure to estimate the binary outcome of smoking abstinence. While the use of IVs is becoming increasingly common in non-linear settings (Rassen et al., 2009; Greenland, 2000), there is concern in the literature about when such models are identified, and consistent estimation relies on more restrictive functional form assumptions (An, 2015; Lewbel et al., 2012). Nevertheless, our estimates were robust to IV-probit versus IV linear probability model specifications (Appendix Table A5).

Importantly, we cannot ensure that ego did not interact with the individuals in his or her alters' excluded friend networks, and this concern may be particularly relevant in smaller worksites. It is possible that our estimates may overstate the magnitude of true peer effects if there are other unobserved worksite characteristics that affect the smoking behavior of all employees, such as changes in the culture around smoking or workplace policies. We also did not ask participants to report the frequency of interactions among coworker friends during the trial, and thus our estimates can only be interpreted as the effect of *having* a friend who abstains on one's own abstinence.

Given this limited information on the nature of peer interactions, we cannot rule out alternate explanations for the observed relationship between ego's abstinence and that of their peers. For example, it is noteworthy that we also find suggestive evidence that individuals with stronger social network ties – as measured by the size of their excluded network – were *less* likely to abstain from smoking. This finding is consistent with existing literature finding that network centrality and popularity is positively related to smoking behavior (Lakon & Valente, 2012; Valente et al., 2013) and could point to network mechanisms other than those we attribute to be positive spillover effects between friends.

More generally, existing research suggests that the nature of relationships or “dosage” of peer interactions may have substantial implications for the likelihood or magnitude of spillovers. For example, White and Dow (2014) find evidence of peer effects from confident to less confident teammates, but not among individuals who report having high confidence *ex ante* in their likelihood to successfully abstain. A recent review by Advani and Malde (2018) and work by Carrell et al. (2013) points out how not accounting for such heterogeneous characteristics within groups may lead to biased estimates of peer effects in traditional models. Despite these challenges, there is recent work showing that traditional regression-based approaches to estimating peer influence can perform well relative to newer, less-restrictive analytic techniques that jointly model network dynamics and selection processes (Ragan et al., 2019).

Recent theoretical work also highlights potential biases in using IV approaches to estimate peer effects (Hinke et al., 2019). Consistent with other studies using outcomes among excluded peers as instruments, the IV estimates in our study are larger than those estimated using ordinary least squares (De Giorgi et al., 2016, 2010; Nicoletti & Rabe, 2019). Our inability to observe the within-group dynamics described above, along with possible violation of the exclusion restriction, could explain this result, yet the simulation of measurement error in Section 3.3 suggests a more likely explanation. Mis-measurement could be due to incomplete data on participants' entire friendship networks (e.g., over half of participants listed the maximum of five friends, suggesting that this could

be the lower bound the number of close friends they have) and the assumption that non-participants are assumed to be continuing smokers. Further, Feld and Zölitz (2017) show that in scenarios with non-random peer group assignment, measurement error can inflate IV peer effect estimates. In the present setting where friend groups are self-selected, this effect—combined with attenuation of the OLS estimates—is one likely reason why our IV estimates are substantially larger, rather than being an issue of weak instruments.

Lastly, this study takes place in Thailand, where unique aspects of the local culture may contribute to or facilitate the diffusion of smoking abstinence across friendship networks. First, there has been widespread effort in Thailand to encourage smoking cessation through the implementation of strict, nationwide tobacco control policies such as bans smoking in some public places and tobacco advertising restrictions (Levy et al., 2008). These campaigns have been successful in significantly reducing the prevalence of smoking in Thailand over the past several decades and have likely contributed to a widespread awareness of the risks associated with tobacco use. This strong institutional support for smoking cessation points to a widespread emphasis on a collectivist society, which permeates other aspects of Thai life including business practices and family relationships (Hofstede, 2001; Thanakwang, 2015). Future research should examine whether the evidence we find of peer effects in one's decision to abstain from smoking extends to settings where there is less institutional support for tobacco control or more individualistic views on behavior change. Uncovering the mechanisms through which these peer effects operate, as well as the potential complementarities with social or financial incentives, is also an important topic for future work studying peer effects in behavior change interventions.

#### Ethics approval statement for Social and Monetary Incentives for Smoking Cessation at Large Employers (SMILE)

The study received institutional review board approval (Protocol number 2012-11-4792) from the University of California, Berkeley as the reviewing IRB and University of California, San Francisco as the relying IRB. The study received local institutional review board approval from Mahidol University's Institute for Population and Social Research (Protocol number 2014/1-1-06).

#### Credit roles

**Christopher Lowenstein:** Formal analysis, Data curation, Writing – original draft, Writing – review and editing; **Software;** **William H. Dow:** Conceptualization, Methodology, Investigation, Writing – original draft, Writing – review and editing, Supervision, Project Administration, Funding acquisition; **Justin S. White:** Conceptualization, Data curation, Methodology, Investigation, Writing – review and editing, Supervision, Project Administration, Funding acquisition.

#### Declaration of competing interest

None.

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## Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.ssmph.2020.100659>.

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