# How Do Teams Work?

## A Social Commitment Experiment for Smoking Cessation*

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**Abstract**

This paper presents evidence on the nature of peer effects in a social commitment intervention that offers team incentives for smoking cessation. Using data from a field experiment in Thailand, we test predictions from a theoretical model of self-control in teams that highlights three channels through which teammates may affect each other: the strength of their social ties, *ex ante* predictions of a teammate's outcomes, and a teammate's actions. Exploiting random team formation, we find that the team-based intervention yielded large team effects via all of these channels. A teammate who has quit increases the probability of an index person quitting by 36% points. The team effects are heterogeneous with respect to *ex ante* self-assessed quit predictions, such that less confident individuals get a positive spillover effect from having a more confident teammate, but more confident individuals are unaffected by teammate type. This implies that a sorting rule of heterogeneous pairings would be expected to yield higher overall quit rates than random or homogeneous pairings.

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# 1 Introduction

Many individuals struggle to resist temptation. John Stuart Mill (1871) put it succinctly: individuals "pursue sensual indulgences to the injury of health, though perfectly aware that health is the greater good." Researchers have long puzzled over how to prevent individuals' self-control failures.[1] Behavioral economists have directed much of their attention to commitment contracts, in which a person voluntarily agrees to incur a penalty for failure to show self-control (Bryan, Karlan and Nelson, 2010). Yet, commitment contracts pose certain limitations, most notably that demand is modest and individuals who are overly confident about their future self-control will often fail to put enough at stake to motivate themselves.[2] An alternate approach pursued by only a handful of studies is to mobilize peer pressure as a social commitment mechanism (Gugerty, 2007; Kast, Meier and Pomeranz, 2012; Kullgren et al., 2012; Dupas and Robinson, 2013). Collectively, these studies offer qualified support for peer monitoring and social pressure as a way to help individuals to follow through on their goals. Yet, despite the common perception that social incentives can drive behavior, the nature of peer effects in social commitment interventions remains largely unexplored.

Peer support groups have been a common approach to behavior change, as witnessed by the popularity of organizations such as Weight Watchers and Alcoholics Anonymous. Advocates of these approaches often highlight their ability to provide members with knowledge, motivation, and emotional support. However, such team-based approaches can also be harmful under certain circumstances. In particular, if a person fails to follow through on a goal, her teammate(s) may become discouraged, performing worse than if acting alone. This discouragement effect could account for the lack of success of some peer support interventions (Park, Tudiver and Campbell, 2012).

While researchers have dealt extensively with peer support, they have rarely tried to mobilize peer pressure explicitly. Hence, in this study, we enlist team incentives that activate peer pressure.[3] Team incentives, which condition awards on team production, may trigger peer pressure by inducing a variety of responses, including: a sense of responsibility; feelings of guilt, shame, and embarrassment; fear of social sanctions; and a desire to be liked or respected. This confluence can lead individuals to exert more effort and to achieve improved

[1] Psychologists have led the way in proposing theories of self-control (Mischel, 1974; Baumeister et al., 1998; Ainslie, 1992; Bandura, 1997).

[2] For example, our intervention is modeled after the CARES trial in the Philippines, in which 11% of smokers agreed to open a commitment contract for smoking cessation, two-thirds of whom failed to quit (Giné, Karlan and Zinman, 2010). These overly optimistic agents are "partially naïve" about their self-control problems, according to the nomenclature of O'Donoghue and Rabin (2001).

[3] The social effects of peer pressure have been documented across many settings (Falk and Ichino, 2006; Mas and Moretti, 2009; Gerber, Green and Larimer, 2008). Research on social pressure dates back at least to the classic social psychology experiments of the 1950s and 1960s (Asch, 1951; Milgram, 1963).

productivity, as the literature on team compensation shows (Hamilton, Nickerson and Owan, 2003; Jones and Kato, 1995; Knez and Simester, 2001; Bandiera, Barankay and Rasul, 2013). Although team incentive schemes have been studied in some research contexts such as microfinance,[4] they have received considerably less attention in the health domain, even though peer pressure has long been recognized as a contributor to risky health behaviors. One exception is Babcock et al. (2011), who conduct a brief intervention of team incentives for gym attendance and find that undergraduates value their teammates' payoff two-thirds as much as their own.

In this study, we test a theoretical model of self-control in teams developed by Battaglini, Bénabou and Tirole (2005), in which present-biased agents learn about their likelihood and ability to show self-control by observing teammates. We use the model to address untested hypotheses about three channels through which incentivized teammates may influence each other's behavior: 1) the strength of teammates' social ties, 2) *ex ante* predictions of a teammate's outcomes, and 3) a teammate's actions. To understand these channels, we draw on data from a field experiment in rural villages of Thailand that tested the effects of a commitment contract overlaid with team incentives.[5] We offered the team incentives equivalent to roughly four days of household income. We made a refund of contributions to the commitment contract contingent on a person's own smoking abstinence and the team incentives contingent on the abstinence of both the person and her teammate, biochemically assessed at three months. We allowed participants to pre-select a teammate or to be randomly assigned a teammate from the same village at enrollment. We restrict most analyses to randomly assigned teams that were eligible for team incentives (118 smokers). We exploit the random team assignment to credibly identify the team effects and to overcome the well-known challenges of identifying social effects.[6]

First, we test how the strength of social ties of teammates influences quitting behavior, using several ego-centric measures of tie strength. Some prior work indicates that strong ties improve outcomes among joint-liability teams to promote loan repayment and gym attendance (Karlan, 2007; Babcock and Hartman, 2011). Compared to strangers or acquaintances, friends and family members may be better able to motivate and monitor each other and to provide each other with emotional and logistical support. We also leverage the design feature allowing for pre-selected and randomly formed teams to compare the

[4] A key finding from microfinance is that team incentives promote free-riding (Olson, 1965). Shirking is not a concern in our setting, because the payoffs depend on both agents exerting effort.

[5] In prior work, we reported results comparing the control and treatment groups (White, Dow and Rungruanghiranya, 2013); here, we test hypotheses related to the team effects.

[6] Challenges include self-selection into peer groups, common contextual factors shared by peers, and the "reflection problem," whereby each peer affects the others simultaneously (Manski, 1993).

outcomes of teams with naturally occurring and arbitrarily assigned social ties. We find that pre-selected teams do not outperform randomly formed teams, although we do detect large positive effects of being randomly paired with a best friend or a top-five best friend in the trial.

Second, we test the degree to which teammates' beliefs yielded positive or negative spillover effects for our study participants. In particular, we test a theoretical prediction from Battaglini, Bénabou and Tirole (2005) that "good news" about a teammate's ability to show self-control enhances an index person's performance and "bad news" hinders performance. In our model, *ex ante* quit predictions serve as the carrier of news about a teammate's ability. Consistent with the prediction, we find heterogenous effects: being paired with a teammate of High type—one who has a high self-assessed probability of quitting—leads to positive spillovers relative to being paired with a teammate of Low type. Thus, the gains to team membership appear to outweigh the costs in our sample. We further investigate the preferred rule that a social planner might use to assign teams in order to maximize the number of quitters, in line with recent attempts to find optimal policies for sorting individuals into teams (Bhattacharya, 2009; Carrell, Sacerdote and West, 2013; Graham, Imbens and Ridder, 2014). We find that the preferred rule consists of sorting individuals into heterogeneous teams of a teammate of High type and a teammate of Low type.

Third, we test the effect that a teammate's outcome has on an index person. We use weekly self-reports of teammates' smoking status and deposit contributions to determine the extent to which teammates' actions coincide. A temporal correspondence after controlling for past behavior and potential confounders is suggestive that teammates act in a coordinated manner. Our results indicate that a person is far less likely to have smoked in a given week if a teammate did not smoke or was believed not to have smoked that week. We then refine this analysis to examine the causal relationship between teammates' outcomes at the end of the intervention. We quantify the impact of a teammate quitting on a person's own quit status using an instrumental variables framework. We elicit subjects' *ex ante* predictions for the quit probability of every other participant from the same village, and use non-team members' mean quit predictions for a randomly assigned teammate as an instrument for the teammate's subsequent outcome. We show that a person's probability of quitting more than doubles (a 36%-point increase) if his or her teammate quits.

Our paper is closely related to a large literature in health and education on peer effects and the influence and relationships among social network ties (e.g., Sacerdote, 2001, 2011; Smith and Christakis, 2008; Leahey et al., 2010). Several studies have exploited variation in class, roommate, or small group assignment among high school or college students and found the presence of peer effects across a number of health dimensions, including physical fitness

(Carrell, Hoekstra and West, 2011), alcohol use (Duncan et al., 2005; Kremer and Levy, 2008; Fletcher, 2012; Eisenberg, Golberstein and Whitlock, 2014), and drug use (Card and Giuliano, 2012). Studies on peer effects for tobacco use have hewn to two lines of inquiry: smoking initiation among adolescents (Norton, Lindrooth and Ennett, 1998; Krauth, 2007; Fletcher, 2010; Harris and González López-Valcárcel, 2008) and spousal influence on cessation (Cutler and Glaeser, 2010; McGeary, 2013). The broader effects that peers have on decisions to quit smoking have not been well identified, although Christakis and Fowler (2008) report large correlations in the quit behavior of social network ties.

We extend the existing literature in several respects. First, we study peer effects for smoking in a novel team environment. In light of the early promise of social commitment mechanisms for overcoming self-control problems, the nature of team effects in such schemes is an unanswered but crucial issue to address for policy purposes. The application of such schemes to smoking is particularly important, as smoking is the second leading risk factor for death worldwide (Lim et al., 2012). Most of these deaths occur in middle-income countries such as Thailand (Mathers and Loncar, 2006). Second, our identification strategy relies on exogenous team formation to cleanly identify the team effects. Our field experiment also takes advantage of existing social networks in participating communities, a setting where teammates are likely to interact regularly and likely to care about each other's payoffs. Third, we highlight multiple channels through which team effects occur. Prior work has focused exclusively on the effects of a peer's behavior; we show that other pathways such as beliefs about outcomes may also transmit social spillovers. Finally, our study offers some of the first rigorous evidence on the presence of peer effects for smoking cessation among adults. The degree to which smokers influence each other's quit behavior has implications for the social multiplier of tobacco control policies and may point the way toward the development of interventions that tap into the social dynamics of smoking and quitting. Team incentive schemes that harness the power of peer pressure may be one viable approach for coaxing smokers to follow through on their plans to quit smoking.

# 2 A Model of Self-Control in Teams

## 2.1 Model Overview

We introduce here a social learning model of self-control in teams adapted from the work of Battaglini, Bénabou and Tirole (2005), hereafter BBT. We augment the model to capture the notion that lapses are costly (Figure 1). The model yields predictions about how smokers afflicted with a bias for the present will influence each other when placed in two-person teams analogous to our intervention.

A key feature of the model is that present-biased agents learn about their own likelihood of exerting self-control by observing the actions of a teammate. Social learning operates in our setting through two channels. First, teammates' actions directly enter each others' payoffs via the team bonus. A person's motivation and choice of effort will depend on her self-assessed probability of earning the team bonus, which in turn depends on how likely she deems her teammate to show self-control.[7] Second, a person may gain (or lose) self-confidence after observing the successes (or failures) of a teammate. This occurs because agents possess two traits: imperfect self-knowledge and imperfect recall of past actions.[8] Imperfect self-knowledge leads a person to try to intuit her ability to show self-control by examining her own past actions. She fears creating behavioral precedents, whereby a lapse today increases the likelihood of impulsivity in the future, leading to a concern for self-reputation (Bénabou and Tirole, 2004). However, imperfect recall of past actions means that a self-evaluation of one's history is not reliable. Consequently, a person turns to others to glean information about her own ability to show self-control. The model characterizes the impact of teammates on individuals with weak self-control ("weak types"), for whom good news or bad news from a teammate can be decisive, as opposed to strong-willed agents ("strong types") who show self-control regardless of teammate type.

BBT show that teams can produce positive or negative spillover effects for weak types. Although the positive aspects of teamwork are often touted, in theory team-based interventions could be harmful. Encouraging reports of a teammate's self-control increase one's own chances of exerting self-control in a "good news equilibrium" and discouraging reports about a teammate's self-control decrease one's own chances of exerting self-control in a "bad news equilibrium". We refer to the positive spillovers from good news as an encouragement effect and the negative spillovers from bad news as a discouragement effect. According to the model, two factors determine the equilibrium state: 1) beliefs about a teammate's self-control and 2) informativeness of a teammate's actions. Beliefs matter, as stated above, because of teammates' correlated payoffs and a person's reputational concerns. Informativeness is based on the similarity of teammates, both in terms of how similar

[7] We assume in this section that the agent is female, and her teammate is male.

[8] The cognitive psychology literature has long studied imperfect self-knowledge and people's poor insight into their own cognitive processes (Bem, 1967; Nisbett and Wilson, 1977; Ross, 1977). Recall of cravings, pain, and discomfort tend to be systematically biased (Loewenstein, 1996; Loewenstein and Schkade, 1999; Kahneman, Wakker and Sarin, 1997). In addition, people selectively "forget" past lapses, often attributing successes to personal factors and failures to situational factors (Miller and Ross, 1975; Bradley, 1978). This can manifest itself as overconfidence in one's skills and abilities (Svenson, 1981). Several studies find that individuals are overoptimistic about their ability to exercise self-control, which is compatible with partial naïveté with respect to present bias (DellaVigna, 2009).

they perceive each other's self-control to be and the strength of their social ties. As the "correlation" between teammates strengthens, self-control and welfare improve in the good news equilibrium and deteriorate in the bad news equilibrium.

## 2.2 Model Setup

We follow the general setup of BBT (Figure 1). We also embed peer pressure and monetary commitment in the model to tailor it to our context.[9] Imagine a game with two periods, $t = 1, 2$, each with two subperiods (e.g., morning and evening). The dynamic setup enables agents to generate concerns for self-reputation and thus gives rise to informational externalities from teammates. A present self and a future self decide consumption of an addictive good at $t_1$ and $t_2$, respectively. In the first subperiod, the agent decides whether or not to exert self-control over the addictive behavior, say smoking. Choosing to smoke, denoted no self-control ($NW$), delivers an immediate payoff $a$, whereas exercising self-control ($W$), delivers no immediate payoff.[10] In the second subperiod, a decision maker who chose $W$ either lapses ($R$) or abstains from smoking ($A$). Abstaining delivers an immediate cost $c > 0$ from effort, nicotine cravings, and withdrawal symptoms and delivers a delayed benefit ($V$) that is a function of the health gains and monetary rewards contingent on quitting. Lapsing in the second subperiod entails a cost $d > 0$ in the presence of social sanctions or forfeited deposits from a commitment contract, both of which are discounted to the present. A lapse yields a delayed benefit $b > a$ such that $a < b < V$, implying that some restraint has value as a signal to oneself and to others about the degree of self-control one possesses. Self-signaling restraint can induce a future self to show additional restraint.

The model incorporates a hyperbolic discounting parameter for present bias $\beta \in [0, 1]$, where a time-consistent agent has $\beta = 1$ and a present-biased agent has $\beta < 1$.[11] The present-biased smoker places undue emphasis (relative to *ex ante* preferences) on satisfying an immediate urge in the first subperiod and similarly discounts the future benefits of quitting too heavily in the second subperiod because the cravings and withdrawal are particularly salient ($\beta < 1$).[12]

[9] Adding a projection bias parameter to the model does not change our theoretical predictions.

[10] Self-control is the ability to control one's own behavior. Willpower is the ability to motivate oneself to carry out a specified course of action. For the sake of clarity, we ignore these differences and use the term self-control throughout.

[11] Building on the work of Strotz (1955), Pollak (1968), and others, the $\beta$-$\delta$ model generates preference reversals by embedding in the standard utility function an additional discount factor $\beta$ on utility earned in future time periods (Laibson, 1997). Hyperbolic discounting is also an empirical regularity (Ainslie, 1992).

[12] In principle, the self-control parameter could differ in each subperiod (Bénabou and Tirole, 2004). Because our main concern is the choice at the decision node between $A$ and $R$ we assume without loss of generality that $\beta$ is stationary.

Two main features of the model are: 1) state-contingent present bias and 2) imperfect self-knowledge about one's degree of present bias. Degree of self-control is represented as $\beta \in \{\beta_L, \beta_H\}$, where $\beta_L$ implies weak self-control and $\beta_H$ strong self-control.[13] Smokers do not know their type at the start of Period 1; rather, they have common priors $\rho$ and $1-\rho$ on $\beta_H$ and $\beta_L$. These beliefs may be interpreted in several ways. They correspond roughly to predicted self-control, $\hat{\beta}$, in the $\beta$-$\delta$ model (O'Donoghue and Rabin, 2001). As $\hat{\beta} \rightarrow \beta$, an agent is more aware of her time-inconsistency and more likely to seek out forms of pre-commitment to maintain self-control. More generally, the priors may be interpreted as *self-efficacy beliefs* about quitting smoking. Self-efficacy refers to self-confidence in one's abilities to undertake a set of actions (Bandura, 1977).

We first consider the equilibrium in the absence of external costs to lapsing ($d = 0$). In Period 1, abstaining is a dominant strategy for a strong-willed person ($\beta_H$), whereas a weak type ($\beta_L$) prefers not to exercise self-control in the absence of reputational concerns (i.e., if current behavior will not influence future decisions):

$$V - \frac{c}{\beta_L} < b - d < V - \frac{c}{\beta_H} \tag{1}$$

The exposition below concentrates on the decisions of weak-willed agents, whose choices depend on self-reputation and social spillovers. The maximum value of self-reputation is the discounted difference between choosing no self-control ($NW$) and choosing self-control but lapsing (Bénabou and Tirole, 2004), as seen in Equation 2. A weak type abstains (chooses $A$) in Period 1 if:

$$V - \frac{c}{\beta_L} + \delta(b-a) > b - d \tag{2}$$

In other words, the person shows restraint when the benefits from abstaining, including from self-signaling, eclipse the costs.

At the start of Period 2, the smoker shows self-control only if sufficiently confident that her future self will do the same. Otherwise, the craving costs are not worth enduring. Let $\rho'$ denote the person's updated prior in Period 2. *Ex post* the weak type, who is tempted to light up, chooses $W$ if:

$$\rho'(V-c) + (1-\rho')(b-d) > \frac{a}{\beta_L} \tag{3}$$

Equation 3 implies a threshold condition for the level of self-confidence needed to choose $W$

[13] Bénabou and Tirole (2004) and Duflo, Kremer and Robinson (2011) follow a similar approach. Alternatively, BBT specify that agents differ in the severity of their cravings and withdrawal, such that $c \in \{c_L, c_H\}$. We adopt the former approach, given that commitment contracts are hypothesized to relate to short-term time preferences. In contrast, pharmacological aids, such as nicotine gum, act by reducing craving costs $c$.

in Period 2: $\rho' > \rho^\star$, where $\rho^\star$ is defined as:

$$\rho^*(V - c) + (1 - \rho^*)(b - d) \equiv \frac{a}{\beta_L} \tag{4}$$

At the point of indifference between $W$ and $NW$, the payoff from lighting up is balanced by the expected utility from attempting to exert self-control.

## 2.3 Equilibrium Self-Control

We characterize the equilibrium strategy for the subgame where the decision node between $A$ and $R$ has been reached in Period 1 using a perfect Bayesian equilibrium as the solution concept.[14] The outcome of this subgame determines the success of any quit attempt.

Following BBT, we adopt a single-agent benchmark for assessing equilibrium behavior. Let $x_s(\rho)$ represent the strategy of a single agent. In equilibrium, a strong-willed smoker always abstains in Period 1 (Equation 1). A weak-willed smoker abstains with probability 1 only if her confidence is sufficiently high, that is, if $\rho \geq \rho^*$. For lower levels of self-confidence such that $\rho < \rho^*$, the weak type will only show self-control (i.e., pool with the strong type) if observing abstinence at $t_1$ is sufficiently good news as to raise Self 2's posterior probability from $\rho$ to $\rho^*$. At that point, the person would be willing to randomize between $W$ and $NW$. BBT call this condition the *informativeness constraint*, $\Pr_{x,\rho}(\beta = \beta_H|A) = \rho^*$. It uniquely defines the equilibrium strategy for the weak single agent as an increasing function $x_s(\rho)$, shown in Figure 2. The probability of abstaining in Period 1 increases with self-confidence, starting at the origin and reaching one at $\rho = \rho^*$.

Turning to the two-agent case, the equilibrium outcome depends on predictions of a teammate's self-control and the similarity in the degree of self-control between teammates. An agent relies on observing the smoking decisions and display of self-control from a teammate in order to learn about her own ability to show self-control. The extent to which a person learns from others depends on how relevant she views the display of self-control of those around her. A setting with homogeneous pairings provides the key testable predictions for our study.[15] Let members $i \in [1, 2]$ of dyad $j$ have the self-confidence level, $\rho^1 = \rho^2 = \rho$. Further assume that the agents undertake the same strategy, $x^1 = x^2 = x$. Let $\theta \in [0, 1]$

[14] PBE is appropriate for cases in which an agent is one of several types (e.g., strong-willed and weak-willed) and information about type is incomplete.

[15] BBT extend the model to heterogenous pairs and find qualitatively similar results, with somewhat richer predictions that we are under-powered to test. A person's *ex ante* welfare is hump-shaped with respect to her teammate's probability of exercising self-control in Period 2. A person maximizes *ex ante* welfare when paired with a teammate who has a slightly worse self-control problem than one's own, making his successes more encouraging and his failures less discouraging.

denote the degree of informativeness of a teammate's self-control, where $\theta = 0$ implies that a teammate's self-control is independent of the index person's beliefs and $\theta = 1$ implies that the teammate's self-control fully determines the index person's beliefs. BBT define $\theta$ as part of the conditional probabilities of being a strong or weak type:

$$\begin{aligned} \pi_{HH} &\equiv \Pr(\beta' = \beta_H | \beta = \beta_H) = \rho + \theta(1-\rho) \\ \pi_{LL} &\equiv \Pr(\beta' = \beta_L | \beta = \beta_L) = \theta\rho + (1-\rho) \end{aligned} \tag{5}$$

We can denote $\mu_{AR}(x; \rho, \theta)$ as the posterior probability that Agent 1 is a strong type, given that she abstained (chose $A$) but her teammate Agent 2 lapsed (chose $R$) in the first period, where weak types play $A$ with probability $x$. Let $\mu_{AA}(x; \rho, \theta)$ be the posterior that both teammates played $A$ in the first period. The event $AA$ is a "good news" state where the agent observes her teammate displaying self-control, and the event $AR$ is a "bad news" state where the agent observes her teammate succumbing to cravings. BBT show that in equilibrium, the following equation holds:

$$x_{AR}(\rho; \theta) \leq x \leq x_{AA}(\rho; \theta), \tag{6}$$

where

$$\begin{aligned} x_{AA}(\rho; \theta) &\equiv \max\{x \in [0,1] | \mu_{AA}(\rho; \theta) \geq \rho^*\}, \\ x_{AR}(\rho; \theta) &\equiv \min\{x \in [0,1] | \mu_{AR}(\rho; \theta) \leq \rho^*\} \end{aligned} \tag{7}$$

Equation 6 says that a person whose teammate lapses has a weakly lower probability of showing self-control than a person whose teammate abstains. This condition defines two curves in Figure 2, a shift up of the single-agent curve in the good news state to $x_{AA}(\rho; \theta)$ and a shift down in the bad news state to $x_{AR}(\rho; \theta)$. Intuitively, bad news (teammate plays $R$) reduces a person's reputational gain from playing $A$, a *discouragement effect* that lowers the person's probability of abstaining. Good news (teammate plays $A$) does the reverse, leading to an *encouragement effect* that increases a person's probability of abstaining. Both equilibria exist for an intermediate range of values $x_I(\rho; \theta)$, characterized in equilibrium as a downward-sloping curve. As $\theta$ increases, $x_{AR}$ pivots down and $x_{AA}$ pivots up. In other words, as a teammate's actions become more informative, the probability of self-control improves with good news and deteriorates with bad news.

BBT formalize the equilibrium self-control as follows:

**Proposition 1.** *The set of equilibria is fully characterized by two threshold functions*

$\rho_1(\theta) : [0, 1] \to [0, \rho^*]$ *and* $\rho_2(\theta) : [0, 1] \to [0, \rho^*/(1-\theta)]$ *such that:*

(i) *For* $\rho < \rho_1(\theta)$ *there is a unique equilibrium of the "bad news" type:* $x = x_{AR}(\rho : \theta)$.

(ii) *For* $\rho > \rho_2(\theta)$ *there is a unique equilibrium of the "good news" type:* $x = x_{RR}(\rho : \theta)$.

(iii) *For* $\rho \in [\rho_1(\theta), \rho_2(\theta)]$ *there are three equilibria:* $x_{AR}(\rho : \theta), x_I(\rho : \theta)$, *and* $x_{AA}(\rho : \theta)$.

*Moreover, for any* $\theta > 0, \rho_1(\theta) < \rho_2(\theta)$, *but as correlation converges to zero, so does the measure of the set of initial conditions for which there is a multiplicity of equilbiria:* $\lim_{\theta\to\infty} |\rho_2(\theta) - \rho_1(\theta)| = 0$

## 2.4 Comparative Statics

Some comparative statics follow directly from the model. While our empirical model is under-identified for estimating the structural parameters, we are able to test several theoretical predictions derived from the model.

The model suggests that the probability of showing self-control increases with: a person's self-confidence ($\rho^1$) and a teammate's self-confidence ($\rho^2$). The key testable prediction is that team effects are heterogeneous with respect to the "correlation" between a person and her teammate's confidence in showing self-control ($\theta$). For an agent who is confident in her ability to show self-control, the probability of self-control increases with "good news" about a teammate's type, such that $\frac{\partial x}{\partial \theta} > 0$. For an agent who is not confident in her ability to show self-control, the probability of self-control decreases with "bad news" about a teammate's type, such that $\frac{\partial x}{\partial \theta} < 0$. As $\theta$ increases, the non-monotonic nature of the team effects are reinforced, strengthening the encouragement and discouragement effects that accompany good and bad news. In the latter case, team incentives may exacerbate self-control problems, particularly among pairs in which both members have low self-confidence.

The strength of social ties between teammates enters the model in two ways. On the one hand, a stronger partnership increases the social cost of failure ($d$), which is predicted to increase the likelihood of abstaining. On the other hand, stronger social ties will increase the informativeness of a teammate's actions ($\theta$). In such a case, a stronger tie will accentuate the team effects, whether positive or negative. *Ex ante* a stonger dyadic relationship will make the pairing of two strong types more effective (via both channels), and will make the pairing of two weak types less effective only if the informativeness of observing a close friend outweighs the social cost of letting down that friend.

The team incentives increase the probability of quitting by enhancing the returns to quitting ($V$). Team incentives increase the degree to which a teammate's self-confidence matters for one's own effort choice ($\theta$) by introducing correlated payoffs.

# 3 Study Design

## 3.1 Study Site and Participants

Thailand was selected as a study setting for several reasons. First, Thailand has been more aggressive than its neighbors in implementating tobacco control policies. Regulations include pictorial warning labels on cigarette packs, relatively high excise tax rates, bans on the display of tobacco at the point of sale, and comprehensive advertising bans. As global tobacco control efforts spur increased regulation and greater demand for quitting worldwide, Thailand's experience with smoking cessation may provide a model for other countries. Second, demand for quitting is relatively high. For example, 10% of Thai smokers quit smoking following a substantial cigarette tax increase in 2006 (White and Ross, 2013). A latent demand for quitting is essential for incentive-based smoking cessation interventions to succeed. Third, use of conventional smoking cessation aids is uncommon in Thailand such that 90% of all quit attempts do not involve a smoking cessation aid or professional support (World Health Organization, 2009). While smoking cessation programs have expanded in Thailand in recent years, they remain limited to select hospitals and community pharmacies, mostly in urban areas. Thailand's early adoption of tobacco control policies, high demand for quitting, and low use of professional services for smoking cessation make it an excellent setting for testing an innovative behavioral approach to quitting.

We recruited smokers from 42 villages in six subdistricts in central Thailand.[16] Each village has about 500 residents (400 adults), and most people from the same village know each other. Median household income in the area is $10 per day (Thailand National Statistics Office, 2008). Even though the study area lies within 100 miles of Bangkok, the local economy is predominantly agrarian. The area includes a mix of majority-Buddhist and majority-Muslim communities, and, for many residents, community life is oriented around religious activities and celebrations held at the local place of worship.

White, Dow and Rungruanghiranya (2013) describe the findings from a census of current smokers in the study area conducted just prior to the roll-out of the intervention. In total, 2,055 smokers were found in the 42 communities. Smoking prevalence was 23% for men and 2% for women. About 59% of the smokers use handrolled tobacco that costs as little as $0.10 per pack-equivalent, as opposed to manufactured cigarettes that cost roughly $2 per pack (Hammond et al., 2008). Another 11% of smokers are dual users of handrolled tobacco and manufactured cigarettes. Individuals tend to be long-time smokers, who initiated more than 20 years earlier on average. Daily consumption is about 14 cigarettes per day. Only

[16] The subdistricts, which span three districts in Nakhon Nayok province, are: Bueng San, Chumpon, Khao Phoem, Klong Yai, Ongkharak, and Pak Phli.

20% reported an intention to quit smoking within the next six months. In contrast, half of smokers nationwide reported a quit attempt in the prior year, according to the Global Adult Tobacco Survey (World Health Organization, 2009).

The characteristics of the treatment group are similar to the general smoking population in the study area (Column 1 of Appendix Table A1). About 89% are male. Mean age is 52 years. Mean monthly household income is about $400. Nearly two-thirds work in agriculture. About 48% use handrolled tobacco only and 19% are dual users of handrolled tobacco and cigarettes. The sample initiated smoking nearly 33 years earlier on average. Participants made a mean of 2.6 past quit attempts (top-coding at 10; median of 2). Whereas one-fifth of the smoking population reported plans to quit smoking in the subsequent six months, 83% of participants in the treatment group had plans. When asked to predict their likelihood of being smoke-free in three months (at the end of the intervention), respondents gave a mean response of 79%. About half of smokers (48%) stated that all of their five best friends were smokers.

## 3.2 Experimental Procedures

The study design is shown in Appendix Figure A1. All current smokers aged 20 and older who resided in a study community were eligible to enroll. Smoking status at enrollment was based on self-report and verified with eyewitness reports by community health workers. During enrollment meetings held from December 2010 to March 2011, 215 smokers from 30 villages enrolled in the trial. In 12 eligible villages, community health workers did not recruit any participants. Enrollment meetings were held in public spaces within each village. All enrollees signed a consent form agreeing to take up the intervention (i.e., to pay the minimum required deposit) if assigned to the treatment group. Participants were told during the consenting process that they would return later for urine testing, although specific testing dates were not announced until the week of the follow-up. Prior to randomization, participants completed a screening questionnaire.

The study followed a two-step stratified randomization procedure: 1) assignment to a two-person team and 2) random allocation by team to the treatment or control group. In the first step, participants were able to select a teammate prior to enrollment ("pre-selected" pairs) or to be randomly assigned to a teammate at enrollment. Randomly assigned teams were stratified by village and sex.[17] For village-sex strata with an odd number of at least three non-pre-selected enrollees, the "extra" person was retained in the sample ($n = 13$), and faced the same treatment allocation probabilities as those randomly assigned a teammate

[17] We stratified by sex in an effort to be sensitive to cultural differences in gender roles in Thailand.

and those in a pre-selected pair. We dropped 14 individuals from the sample, 12 of whom belonged to a village-sex strata with one person and thus had no probability of being assigned a teammate (e.g., the lone female recruit from a given village) and two of whom arrived late to the enrollment meeting. The final sample included 201 participants.

In the second step, teams were randomly allocated to the control group or treatment group in a 1:2 ratio. At each enrollment meeting, a programmer implemented the random team and allocation sequences using computer-generated random numbers, concealing the sequence from other field staff and participants. Only treated participants were informed of the identity of their randomly assigned teammate. Control group members who pre-selected a teammate were not given any instructions regarding how to interact with their teammate; other control group members were assigned a "synthetic teammate" whose identity was never revealed and used only for analysis.

While the randomization procedure took place, a smoking cessation counselor provided a group counseling session to all participants. The field coordinator then announced treatment status assignment, and the control group was dismissed. Treated participants learned their teammate's identity, completed a baseline questionnaire, met briefly with their teammate to discuss plans such as a proposed frequency of contact and the preferred nature of their social interactions, made a contribution to a commitment savings account, and then were dismissed.

The control group had no intervention-related activities following enrollment. The treatment group received three additional components. First, each treated individual opened a commitment savings account with the project at enrollment. The account had a minimum opening balance of $1.67 (50 Thai baht). For 10 weeks after enrollment, a community health worker (CHW) visited the participant weekly to collect additional, voluntary contributions to the account. The project added a $5 starter contribution to each treated participant's account and an extra $5 (THB 150) if the person reached an account balance of $10. The project refunded the deposits and matching contribution only if the person had quit smoking as assessed at three months. Second, if the person and assigned teammate both abstained from smoking at three months, each received a cash bonus of $40 (THB 1200), about 16% of median monthly household income.[18] The expected value of the team bonus is much smaller after accounting for a teammate's expected probability of failure. Third, the project sent weekly text messages to boost the frequency and intensity of deposits and to increase the strength and salience of teammate monitoring and support.

Participants returned to the same meeting site three months after enrollment.[19] At that

[18] By comparison, Volpp et al. (2009) offered some of the largest cash incentives for quitting to date: roughly 27% of household income (our calculations).

[19] We also verified smoking status at six months and collected self-reports at 12 to 15 months. See White,

time, a brief survey was administered. Then, participants were tested for smoking abstinence using a NicCheck™ urine test for nicotine and its metabolite, cotinine.[20] The color-coded test strips give results on a 14-point scale in 15 minutes. A person passed the test if he or she had a score of 0, implying undetectable levels of nicotine and cotinine.[21] Anyone who disputed the test results could request a second test, although field staff encountered only two disputes. The assessor of the urine test was blinded to treatment allocation; urine containers were labeled with a unique identification number. Treated participants received monetary rewards (as described above) if they passed the test and self-reported abstaining from smoking for at least seven days.[22] For all participants who did not attend the three-month meeting (30%), the field coordinator contacted the person by phone or else through a CHW to ascertain the person's self-reported smoking status. All individuals who reported having quit were visited at home to verify their status by urine test.[23] Thus, there is no attrition in our sample. Control group participants received an inconvenience fee of $3 for their attendance at the three-month meeting.

# 4 Empirical Framework

## 4.1 Data and Key Variables

Our analysis draws on several kinds of data. Field workers administered a screening and baseline survey at enrollment and a three-month survey at the conclusion of the deposit intervention. One month after enrollment, project staff contacted all participants by phone to determine their self-reported smoking status. Another data source comes from the deposit collection visits from community health workers. CHWs were charged with visiting participants on a weekly basis during the deposit period. In practice, some CHWs admitted visiting participants every other week. During each visit, CHWs recorded the deposit amount and responses to questions about whether the participant smoked in the last week, whether the person had contact with the assigned teammate in the last week, and whether the person

Dow and Rungruanghiranya (2013) for details.

[20] Participants went one at a time into public bathroom facilities to provide urine samples. Research staff monitored participants to ensure that they did not carry any containers into the bathroom. The same research staff worked at enrollment and follow-up, allowing them to verify a participant's identity with near certainty. Some CHWs were also on-hand at follow-up.

[21] According to the manufacturer, the test has both a sensitivity and specificity of 97% and a detection period of 3-4 days for a smoker of 5-10 cigarettes per day and 5-6 days for a smoker of 20-30 cigarettes per day. Participants and field staff were not informed of the detection period.

[22] We independently verified the self-reports against eyewitness reports from CHWs. These reports concorded for all but two participants.

[23] None of these participants passed the urine test. One subject declined to report his smoking status at three months. We count him as a continuing smoker.

believed the teammate had smoked in the prior week. Three to five months after enrollment, the research team conducted semi-structured qualitative interviews with 15 trial participants to detail the experiences of participants during the trial.[24]

The primary outcome variable is biochemically-verified quit status at three months. Secondary outcomes include self-reported quit status and an indicator for deposit contributions, both of which were reported to CHWs each week of the deposit period. Figure 3 shows the quit patterns by week, based on self-reported and biochemically verified smoking status.

A key independent variable in our analysis is participants' *ex ante* beliefs about their ability to quit smoking, as measured by a self-assessed prediction of the probability of not smoking in three months' time. During the screening questionnaire, we used a visual scale labeled from 0-100% to elicit the predictions, and participants reported the subjective probability in 10% increments. This variable represents our measure of the parameter $\rho'$ from the theoretical model. Prior to the announcement of pairings, treated participants also gave predictions of the probability that each participant from the same village would have quit smoking in three months. For members $i \in 1, 2$ of dyadic teams $j = 1, \ldots, J$, let $p^1_{1j}$ be the index person's self-prediction, $p^2_{1j}$ be the index person's prediction for a teammate, $p^2_{2j}$ be the teammate's self-prediction, and $\bar{p}^1_{ik}$ be the mean predictions of others (from all teams $j \neq k$) for the index person. Figure 4 shows the distribution of predictions about the index person from the perspective of the index person, a teammate, and all others: $p^1_{1j}$, $p^1_{2j}$, and $\bar{p}^1_{ik}$.

## 4.2 Descriptive Team Characteristics

Table 1 lists team characteristics before and after the start of the intervention, overall and by three-month quit status. We include pre-selected and randomly formed teams in this sample. Only 12 participants in the treatment group (10.6% of the sample) pre-selected a teammate. The majority of participants were willing to be randomly assigned a teammate from the same village. Pre-selecting a teammate did not predict quitting relative to being randomly assigned a teammate. At baseline, we asked participants to enumerate their five closest friends in order, among their fellow villagers who participated in the trial. About one-third of participants were matched with their closest friend in the trial, and another one-third were matched with their second to fifth closest friends.[25] Being matched with

[24] The sample of participants was randomly selected, stratified by subdistrict, quit status, and receipt of the bonus, although we used a convenience sample to find replacements for unavailable participants.

[25] The large percentage of individuals assigned to a "best friend" is due to low enrollment in some villages. For example, if a village had only two participants, we would classify them as being best friends if they named each other as friends. To account for cross-village variation in the probability of being assigned a friend as

one's five best friends is positively related to quitting at three months ($p = 0.03$) None of the other aspects of team relationships reach statistical significance at conventional levels. Teammates tended to live about 1 km (0.5 mile) apart. About 57% of treated participants had a close friend or relative as a teammate. Only two participants reported that their teammate was a stranger. Nearly two-thirds of teammates interacted with their teammate at least weekly prior to the start of the trial, and a quarter of participants interacted with their teammate monthly or less frequently (including never).

Next we turn to the social characteristics of teams after enrollment. These endogenous social interactions provide valuable information on how the social component of the intervention was carried out in practice. Of those in the treatment group, 27.3% (36/132) earned the team bonus, significantly greater than would be predicted by chance. Among quitters in the treatment group, 59% received the team bonus. These team outcomes were not evenly distributed by treatment status. In the control group, zero individuals were in pre-selected or synthetic teams in which both members quit at six months, 28.6% in teams in which one quit and one smoked, and 71.4% in teams in which both failed to quit. In contrast, the breakdown for the treatment group is significantly different: 27.3%, 37.9%, and 34.9%, respectively ($\chi^2(2) = 27.7, p < 0.001$). That treated participants were far more likely to be in teams in which both members quit and far less likely to have both smoke is consistent with the idea that the encouragement effect of having a teammate who succeeds outweighs any discouragement effect from having a teammate who fails.

The frequency of teammate contact during the intervention period mirrored the pre-trial pattern.[26] Participants do not appear to have sought out their teammate more than they otherwise would have. Engaging in post-enrollment conversations with a teammate is not correlated with quitting at three months, although frequently discussing smoking or the project is strongly related to quitting ($p < 0.01$). About 60% of participants had a teammate who asked or tried to convince them to quit on more than one occasion. A similar proportion initiated the entreaties, and those participants are marginally more likely to have quit, perhaps because they exerted some amount of direct peer pressure on their teammate. About 56% of participants received advice more than once about quit strategies from their teammate, and a similar proportion gave advice. Those who gave advice were more likely to quit by the intervention's end ($p = 0.02$). About 41% of participants reported that their teammate had calmed them down when feeling stressed or irritated. Finally, nearly one-quarter of participants lit up with their teammates after enrolling in the study. This highlights a challenge for team interventions. Some teammates may enable or tempt each

a teammate, our multivariate analyses control for the number of enrollees at each meeting.

[26] Some of these items are taken from a standardized questionnaire (Cohen and Lichtenstein, 1990).

other into engaging in negative behaviors, even as other teammates act as a deterrent or source of motivation for overcoming temptation.

## 4.3 Empirical Strategy

Our analytic approach is to test how certain features of teammates—their social ties with the index person, beliefs, and outcomes—affect the behavior of an index person ("ego"). Let $Y_{1jt} \in \{0,1\}$ be the quit status of index person 1 in pair $j$ at time $t$. We assume that ego's outcome depends on a latent variable $Y^*_{1jt}$ of his or her propensity to abstain from smoking at time $t$.[27] Our multivariate analyses follow the general form:

$$Y^*_{1jt} = \alpha + \beta T_{2jt} + \mathbf{X}_{1j}\gamma + \epsilon_{1jt} \tag{8}$$

where index person 1's quit status is a function of teammate 2's characteristics or behavior at time $t$. In various specifications below, $T$ is substituted for measures of a teammate's social ties to the index person, a teammate's beliefs, and a teammate's actions. The equation also includes $\mathbf{X}$, a set of baseline socio-demographic and smoking characteristics of person 1.

We use two identification strategies to determine the impact of team features on the index person. First, our analyses of social ties and beliefs exploit the random assignment of individuals into teams. For the subset of randomly formed teams, the social distance between teammates and the ability of a teammate to quit (as embedded in quit predictions) is exogenously determined. Second, our analysis of the impact of teammates' contemporaneous quit decisions relies on an instrumental variables estimator. We instrument for a teammate's quit status using other participants' mean predictions for one's teammate, excluding the predictions of the index person and teammate. We restrict the analysis to the sample of randomly assigned teams in the treatment group for whom this instrument is randomly assigned.

# 5 Results

## 5.1 Social Ties of Teammates

We test the effect on smoking abstinence of the strength of social ties between teammates. According to our theoretical model, the sign of the effect is ambiguous. On the one hand, the cost of failing to quit increases with the closeness of social ties, and in a good news equilibrium (the teammate has a high self-assessed quit prediction) individuals will benefit from having

[27] Throughout, we present the linear form of our models, although some models use a probit estimator.

a close relationship with a teammate. On the other hand, in a bad news equilibrium (the teammate has a low quit prediction) individuals will be more adversely affected by having a close relationship with a teammate. We test this hypothesis using several ego-centric measures of the strength of teammates' social ties: whether a teammate is pre-selected or not, the self-reported geographic distance between teammates' houses, the nature of the pre-trial relationship between teammates (acquaintance, close friend, or relative), the frequency of social contact prior to the trial, and whether prior to team assignment the index person listed her teammate as her closest or top five closest friends, among those participants enrolled in the trial. The decision to pre-select a teammate is endogenous to quitting, although we report the relationship because it is of substantial practical significance for interventional design. For all other analyses of social ties, we restrict the sample to individuals in the treatment group who were placed in randomly formed teams. In so doing, we use random variation in the social distance between teammates to identify the team effects.

Table 2 presents the effects of social ties on quitting at three months. Of our six measures of social tie strength, two are statistically significant. Participants paired with their closest or one of their five closest friends in the trial were 23.5% points and 26.5% points more likely to quit smoking at three months (Models 2 and 3). In those models, we control for meeting size, because a person's likelihood of being matched to a friend from the same meeting varies with meeting size. Endogenously formed, pre-selected teams did not outperform randomly formed teams, and the sign of the coefficient is negative albeit statistically insignificant (Model 1). Several explanations could account for this finding. Close friends may be better able to ignore the social costs of failing to quit, under a belief that their friendship could withstand the disappointment. Alternatively, close friends may enable each other to smoke, for example, sharing a cigarette during social gatherings. As we saw in the last subsection, about one-third of participants smoked with their teammates after enrolling in the trial. When we look at that percentage by type of pairing, we see that 17.8% of randomly formed, treated teams smoked together after enrollment as compared to 66.7% of pre-selected, treated teams. Finally, it is conceivable that participants pre-selected a teammate based on whether the person was a close friend or family member, rather than on whether the teammate would be likely to abstain or likely to support the index person's quit attempt.

## 5.2 Beliefs of Teammates

Next, we test whether a teammate's quit beliefs at baseline predict the quit behavior of the index person at the end of the intervention. Although ego's self-predictions $p_{1j}^1$ may be endogenous to his or her subsequent quit status, the effect of a teammate's self-predictions

$p_{2j}^2$ are cleanly identified among the subset of teams that were randomly assigned. Thus, we again restrict the sample to members of randomly formed teams in the treatment group.

Table 3 displays the relationships between baseline quit beliefs and subsequent smoking behavior. In the basic model, we regress ego's quit status on the teammate's *ex ante* quit predictions $p_{2j}^2$, controlling for ego's self-predictions (Model 1) as well as other potential confounders (Model 2). In contrast with our theoretical model, an index person's self-predictions do not strongly predict quit status at three months after controlling for a full set of covariates (Model 2). However, we are unable to rule out that the true effect size is substantial. Another possibility is that confident individuals may be more likely to succeed but also more likely to overestimate their ability to show self-control. The behavioral economics literature highlights the perils of being overly optimistic (or naïve) about one's self-control (DellaVigna, 2009). Figure 4 provides some evidence that individuals evaluate themselves as much more likely to follow through on their plans than do others.

We find that a teammate's baseline self-predictions lead to a significant increase in ego's likelihood of quitting. Increasing the teammate's prediction by 10% points corresponds to a a 4.5%-point increase in ego's quit probability (Model 2). In the context of our theoretical model, we might interpret this relationship as ego's will being fortified after observing a teammate's self-confidence. Moreover, the teammate's display of self-assuredness may signal to ego that he or she has a greater likelihood of earning the team incentives, leading to increased effort and motivation on the part of ego.

We also test a specification that replaces $p_{2j}^2$ with ego's quit prediction for the teammate $p_{1j}^2$ (Model 3). Upon initial inspection, the latter measure seems more tightly linked to the theoretical model. However, a teammate holds private information about his own self-control that is revealed to ego only after the trial has begun. Thus, the informational spillovers could be more likely to be transmitted through a teammate's self-predictions than for ego's social predictions for the teammate.[28] Ego's prediction for her teammate is not significantly related to ego's own quit probability (Model 3), although the large standard error does not allow us to rule out potentially large effects.

Based on the theoretical model and the empirical literature (e.g., Bandiera, Barankay and Rasul, 2010; Babcock et al., 2011), we expect that the nature of the team effects varies across teams. In particular, good news about a teammate's abilities is hypothesized to have an encouragement effect on the index person's probability of quitting and bad news is hypothesized to have a discouragement effect. To test for the potential heterogeneity induced by teammates' quit predictions, we first dichotomize baseline self-predictions at the median (between predictions of 70% and 80%): $\tilde{p} \in \{\underline{p}, \bar{p}\}$, where $\underline{p}$ is a Low type and $\bar{p}$ is a

[28] Our theoretical model does not allow for incomplete information about a teammate's ability.

High type. Let $r_{ijm} = \mathbb{1}\{\tilde{p}_{1j}^1 \times \tilde{p}_{2j}^2\} = \{r_{ij1}, r_{ij2}, r_{ij3}, r_{ij4}\}$, corresponding to team types $\{$(Low, Low), (Low, High), (High, Low), (High, High)$\}$, where the first item in parentheses denotes ego's type and the second the teammate's type. Then, we run the model:

$$Y_{1j}^* = \alpha + \beta_1 r_{1j2} + \beta_2 r_{1j3} + \beta_3 r_{1j4} + \mathbf{X}_{ij}\gamma + \epsilon_{1j} \tag{9}$$

In this equation, a negative coefficient on $r_{1j2}$ implies that Low types (i.e., less confident individuals) are diferentially affected by a teammate's type and a post-estimation test of $\beta_2 < \beta_3$ would support the presence of differential effects for High types (more confident individuals).[29]

Models 5 and 6 of Table 3 show the results from the regression analysis of Equation 9. As hypothesized, the team effects are non-monotonic in teammate's self-confidence. A team of (Low, High) type is 46.9% points more likely to quit smoking, compared to a (Low, Low) dyad, meaning that a person's quit probability increases dramatically when paired with a self-confident teammate. We fail to reject a post-estimation test that High types have different outcomes when paired with a teammate of Low type versus High type ($p =$ 0.68). These findings are one form of evidence that Low types experience an encouragement effect when paired with a more-capable teammate, where High types do not experience a discouragement effect from being paired with a less-capable teammate.

To better interpret the estimates of the heterogeneous team effects, we simulate the predicted probability of quitting for each of the four team types. The simulated predicted values capture estimation uncertainty as well as how far the outcome could deviate from expectation due to unmodeled random factors (King, Tomz and Wittenberg, 2000). We approximate the probability distribution of our simulated parameters using 1,000 sets of parameter estimates from the coefficient covariance matrix and assuming mean values for all other variables. The top panel of Figure 5 shows the results of the Monte Carlo simulation. This differential effect could be interpreted as an encouragement effect from the perspective of an index person paired with a High type or as a discouragement effect from the perspective of an index person paired with a Low type. Given that Low types in the control group have a similar average quit probability as the (Low, Low) pairings, we consider this as suggestive but not conclusive evidence that the differential is driven by an encouragement effect for (Low, High) types. In contrast, High types are not significantly affected by a teammate's type. The theoretical model poses a plausible explanation: High types may be analogous to

[29] Appendix Figure A2 provides a side-by-side comparison of the unadjusted and regression-adjusted model. The patterns are qualitatively similar. While a teammate's self-prediction is exogenous to the index person, the index person's self-predictions may be endogenous. As such, we prefer the adjusted model, which controls for potential confounders.

"strong" types from the theoretical model, i.e., individuals who would have quit regardless of teammate assignment.

Finally, we forecast the consequences of using different rules for sorting individuals into teams. We use the fitted values from Equation 9 to predict the overall quit probability under three scenarios: 1) the actual pairings as assigned, 2) homogeneous pairings such that all participants are randomly assigned to a teammate of the same type, i.e., (Low, Low) and (High, High) and 3) heterogeneous pairings such that all teammates are of the opposite type, i.e., (Low, High) and (High, Low). Among the intervention's actual team pairings, the fitted probability of quitting is 48.2%. Under the scenario with homogeneous parings, 39.9% are predicted to quit. Under the scenario with heterogeneous pairings, 54.1% are predicted to quit. The difference between these hypothetical scenarios is statistically significant. Matching more confident individuals with less confident individuals leads to an encouragement effect for the less confident individuals without incurring any large discouragement penalty for the more confident individuals.[30]

## 5.3 Outcomes of Teammates

In addition to beliefs and social ties, teammates may influence each other directly through their behavior. We evaluate this relationship for weekly actions and for our main outcome of quitting at the end of the intervention.

### 5.3.1 Weekly Actions

We analyze how teammates strategically respond to each other's behavior using weekly information on teammates' smoking status and contributions to the commitment savings accounts. Let $Y_{1jt}$ be ego's self-reported smoking status in Week $t$ of the 10-week deposit period. We model three measures of a teammate's actions $A_{2jt}$: whether the teammate made a deposit that week, whether the teammate self-reported smoking that week, and whether ego believed that a teammate had smoked that week. For the latter, we drop responses of "don't know." We also test the effect of lagged actions $A_{2j(t-1)}$ from the prior week for each of our three measures. We run three different estimators to analyze the impact of teammate's actions: a pooled model with baseline controls, a pooled model with controls and a lagged dependent variable, and an individual fixed effects model. The fixed effects model controls for all person-specific unobserved characteristics that may affect quitting and isolates the within-person responses of the index person to her teammate's actions, although

[30] We also tested these scenarios using others' mean predictions for ego and the teammate. The results are similar but noisier. Self-predictions are the clearest contributor to heterogeneous team effects.

it is analytically demanding given our available sample size. The three model specifications take the following form:

$$Y^*_{1jt} = \alpha + \beta A_{2jt} + \mathbf{X}_{ij}\gamma + \phi_t + \epsilon_{1jt} \tag{10}$$

$$Y^*_{1jt} = \alpha + \beta_1 A_{2jt} + \beta_2 Y_{1j(t-1)} + \mathbf{X}_{ij}\gamma + \phi_t + \epsilon_{1jt} \tag{11}$$

$$Y^*_{1jt} = \alpha + \beta A_{2jt} + \phi_t + \mu_i + \epsilon_{1jt} \tag{12}$$

where $\phi_t$ denotes week fixed effects. We also test similar models of a person's weekly decision to make a deposit, omitting the specification with lagged quit status.

Figure 6 displays the bivariate graphical relationships between teammates' weekly smoking status and deposit patterns. The sample is restricted to randomly formed teams. A person is more likely to abstain from smoking in a given week if his or her teammate did not smoke or is believed not to have smoked that week. The differential by teammate's smoking status grows over the 10-week period. In contrast, a person's decision to make a deposit is not related to whether a teammate had smoked that week. This behavior is consistent with the incentive structure of the trial. Commitment contributions are not subject to teammate behavior, whereas the team incentives are directly tied to teammate behavior.

Regression analyses confirm that participants behave in ways that appear to be in response to teammates' actions (Table 4). Ego's smoking decisions relate closely to the actual or believed quit status of his or her teammate. If ego believed that a teammate had not smoked that week or if a teammate had self-reported not smoking that week, ego was 20-23% points more likely to abstain from smoking that week in the individual fixed effects model (Model 3) and 7-8% points more likely to abstain in the lagged dependent variable model.[31] The teammates' lagged quit status also predicts ego's quit status in a given week, although the coefficients are not consistently significant across specifications and close to zero in the models with a lagged dependent variable. We do not find a robust relationship between ego's quit status and whether a teammate made a deposit.

Columns 4 and 5 of Figure 6 show how ego's deposit decision in a certain week relates to a teammate's actions. A person is 7–26% points more likely to contribute to the commitment account if the person's teammate made a deposit that week. In agreement with the graphical depiction in Figure 6, depositing seems independent of a teammate's smoking status.

[31] Angrist and Pischke (2009) argue that these two estimators bound a causal effect (p. 245–46). However, we do not instrument for the lagged dependent variable with an earlier lag because it would not be credible in our context. Thus, we do not make any causal claims from the lagged dependent variable models.

### 5.3.2 Quitting at Intervention's End

We posit that a teammate's outcome has a causal impact on ego's quit outcome. A major challenge in the estimation is the joint determination of teammates' behavior, potentially leading to simultaneity bias and omitted variables bias (e.g., correlated shocks). To infer the causal effect of a teammate's quit status, we use an instrumental variables (IV) estimator. The mean quit predictions of all others from the same village (from all teams $k \neq j$) for that teammate $\bar{p}^2_{ik}$ serves as an excluded instrument for the teammate's quit status at follow-up.[32] We restrict the analysis to the sample of randomly assigned teams. By definition, the exclusion restriction is met among this subset. The monotonicity condition is also likely to hold based on our theoretical prediction of positive spillovers and correlated payoffs (due to the team incentives). In other words, "defiers" — individuals who quit only if assigned to a teammate who fails to quit — are unlikely to exist in our setting.

We specify our model as a two-stage least squares (2SLS) procedure, and we also run a bivariate probit estimator that some research suggests is more robust to non-normality of error terms (Bhattacharya, Goldman and McCaffrey, 2006). The reduced form effect on ego's quit status of others' quit predictions for the teammate is:

$$Y^*_{1j} = \alpha_0 + \beta_0 \bar{p}^2_{ik} + \mathbf{X}_{ij}\gamma_{\mathbf{0}} + v^0_{1j} \tag{13}$$

The first and second stages of the two-stage setup are:

$$\begin{aligned} Y^*_{2j} &= \alpha_1 + \beta_1 \bar{p}^2_{ik} + \mathbf{X}_{ij}\gamma_{\mathbf{1}} + v^1_{1j} \\ Y^*_{1j} &= \alpha_2 + \beta_2 \hat{Y}_{2j} + \mathbf{X}_{ij}\gamma_{\mathbf{2}} + v^2_{1j} \end{aligned} \tag{14}$$

where $v^1_{1j}$ and $v^2_{1j}$ are the first- and second-stage error terms and $\hat{Y}^*_{2j}$ is the fitted value of a teammate's quit status. The coefficient $\beta_2$ may be interpreted as the causal effect of a teammate's quit status on the index person's quit status. Our bivariate probit specification allows for correlation between $v^1_{1j}$ and $v^2_{1j}$. We bootstrap the standard errors on the bivariate probit estimates using 1,000 replications, as boostrapping helps account for the overly narrow confidence intervals produced by the estimation procedure (Chiburis, Das and Lokshin, 2012).

We first investigate whether baseline characteristics are balanced above and below the median of the dichotomized IV (Appendix Table A1). Ultimately, the exclusion restriction is untestable, but this check that the instrumental variable is independent of

[32] We also interacted the excluded instrument with our measure for the strength of baseline social ties, but did not detect any heterogeneous effects, possibly due to a lack of statistical power.

observed characteristics can provide some assurance that the IV may also be independent of unobserved characteristics correlated with quitting. We find that baseline characteristics are fairly well balanced, with one $p$-value below 0.05 and two of marginal significance. We control for all three of these variables, as well as several others, in the IV analysis.

The results of the IV estimation are provided in Table 5. In the reduced form equation, the coefficient of interest implies that a 10%-point increase in others' mean predictions for a teammate leads to about a 6%-point increase in ego's abstinence. Models 3 and 4 show the first stage of the two-stage procedure, which allows us to assess the strength of the instrument. A major concern is that the instrument, if weak, would amplify any bias in the reduced form equation.[33] The $F$-statistic of the excluded instrument indicates that it is moderately strong ($F(1, 58) = 11.2$). Accounting for the maximum possible distortion in the critical value due to weak instruments, the expected actual size of our critical value is 10-15% (Stock and Yogo, 2002). The corresponding $F$-statistic for our probit model is: $\chi^2(2) = 8.8$. Moreover, the standard errors from the naïve probit estimator (Model 7) and the bivariate probit estimator (Model 6) are of similar magnitude. Put together, these results give a measure of confidence that our estimates are not severely biased from use of a potentially weak instrument.

The second-stage estimates imply that a teammate who quits smoking significantly increases ego's likelihood of quitting by 49.2% in the OLS model and 35.8% in the bivariate probit model. The estimated coefficients are large relative to the average treatment effect produced by our intervention of 28-32% points at three months (White, Dow and Rungruanghiranya, 2013). We can interpret this local average treatment effect (LATE) as applying to the subpopulation for whom a teammate's quit status was decisively affected by others' assessments of his or her quality. We estimate that this group of *compliers* constitutes about 28.2% of our sample of randomly formed teams.[34] *Always-takers* who quit if regarded by others as likely to quit constitute 33.9% of the sample, and *never-takers* who do not quit if regarded by others as unlikely to quit constitute 37.9%. Assuming that the exclusion restriction holds, we may also calculate the estimated average outcomes for different compliance groups as one test for the presence of heterogeneous treatment effects. Outcomes vary from 45.8% for never-takers to 30.4% for compliers who do not quit. This suggests that never-takers may be substantially different from compliers, and the LATE

[33] Stock and Yogo (2002) provide the critical values for the first-stage Wald $F$-statistics to determine the expected actual size of a nominal 5% significance test. We are not aware of any comparable values for an equation with a binary dependent variable. As such, we focus on the linear probability model in Model 3.

[34] We can approximate the population shares of different compliance types, under the assumptions that our instrument is valid and monotonic (i.e., no *defiers*). Let $T_i \in \{0, 1\}$ be observed treatment status for person $i$ and $Z_i \in \{0, 1\}$ the dichotomized values of our instrument. Denote *never-takers* as $\pi_n = \mathbf{E}[T_i = 0|Z_i = 1]$, *always-takers* as $\pi_a = \mathbf{E}[T_i = 1|Z_i = 0]$, and *compliers* as $\pi_c = 1 - \pi_n - \pi_a$ (Imbens and Rubin, 1997).

that we observe may not be informative for never-takers. In contrast, the average quit probability for always-takers is very similar to that for compliers who quit (59.1% versus 59.0%), suggesting that the differences between always-takers and compliers are considerably smaller.

The pattern of quitting by compliance type may explain why the naïve estimator in Model 7 gives a much smaller, nonsignificant coefficient compared to the bivariate probit model. We speculate that compositional differences between the compliers used in Model 6 and the full sample used in Model 7 are at play. In particular, we already noted that, relative to compliers, never-takers are more likely to quit and thus potentially less sensitive to the predictions of others. The absence of an upward bias between Models 6 and 7 also implies that our model is omitting any common contextual factors that might lead teammates to have the same outcome.

# 6 Discussion

This study assesses the team effects generated by an intervention that offers team incentives for smoking cessation. We designed the intervention as a form of social commitment, such that the feelings of peer pressure triggered by the intervention might help individuals to follow through on their plans to quit smoking. Our theoretical model of self-control in teams pointed toward three separate channels through which teammates may affect each other's quit behavior: the strength of social ties, *ex ante* beliefs about each other's ability, and realized outcomes. Peer effects are notoriously challenging to estimate, and the literature has focused in large part on this last dimension. Exploiting random team assignment, we find that participants had strong effects on each other via all three channels.

Our most limited findings arise from our analyses of the strength of teammates' social ties. Several measures of tie strength had no significant relationship to quitting. Yet, being paired with a close friend, as identified during an enumeration exercise, increased the probability of quitting by more than 20% points. Our small sample size may be most limiting for this analysis, as information about tie strength does not vary greatly between team members. Some researchers assert that quitting spreads through social networks (Cutler and Glaeser, 2010; Christakis and Fowler, 2008). Our study shows that stronger ties may facilitate this process, although a larger evaluation is needed to discern whether this relationship is robust and whether the coordinated quit attempts of friends are able to change the smoking norms within a person's social network, promote quitting, and reduce recidivism.

We find that team effects are heterogeneous with respect to teammates' baseline beliefs about quitting, as the theoretical model predicts. An index person who is assigned a confident teammate is the beneficiary of positive social spillovers, an encouragement effect. We find no evidence of a discouragement effect for individuals assigned a less confident teammate. The team effects imply that the preferred rule involves sorting individuals into heterogeneous teams with one teammate of high self-assessed ability and one individual of low self-assessed ability.[35] This rule concords with the optimal sorting reported by Ryvkin (2011), who finds that a social planner maximizes effort by maximizing variation across groups if the effort cost function is sufficiently steep. Identifying rules for optimal assortative matching is an exciting new area of research (Bhattacharya, 2009; Graham, Imbens and Ridder, 2009), although the task warrants caution; empirically driven assignment rules can lead to unanticipated outcomes. Carrell, Sacerdote and West (2013) test a sorting rule developed from historical observational data (as opposed to the experimental data we use) and find a negative treatment effect. Future research should attempt to replicate our findings.

We carry out an instrumental variables analysis to show that an index person is causally affected by a teammate's contemporaneous outcomes. The bivariate probit estimation points to an impact of a teammate quitting of 36% points, larger than the overall impact of the intervention at the same point in time (28-32% points). The magnitude of these team effects demonstrate the extent to which the team incentives influenced the quit decisions of participants. Both social-support buddy interventions and individual-based incentive programs have failed to consistently promote quitting smoking (May et al., 2006; Park, Tudiver and Campbell, 2012; Cahill and Perera, 2011). Team incentive approaches offer a promising alternative to current behavioral approaches. The incentives did not induce a discouragement effect from having a "low-quality" teammate, and peer pressure did not lead to long-lasting interpersonal costs. When asked in the three month survey to report on a Likert scale if the intervention had "hurt your relationship with your teammate," all respondents said "not at all." Our team incentive scheme harnessed the power of social effects without producing any detectable social costs of failure.

Our multi-part intervention, which combines team incentives and a commitment contract, challenges our ability to attribute the large team effects solely to the team incentives. Yet, several pieces of evidence suggest that the team incentives were key. The lack of correlation between depositing behavior and a teammate's quit status hints that the deposit contributions are less important for the team effects. In addition, during the qualitative interviews, several participants attributed their success to the team aspect of the intervention. For example, one participant said, "I like [team] competition because I would procrastinate

[35] Alcoholics Anonymous pairs new members with a sponsor who has been abstinent long-term. Many self-help groups have similar programs. It is conceivable that this mechanism may serve a similar purpose to the one we uncover.

if I had to quit all by myself." Other participants credited the bonus with strengthening the social interactions with the teammate: "I thought about the bonus all the time because I knew that I could definitely quit....This also made me talk to my teammate more because both of us would get the bonus if we succeeded. We motivated each other using this bonus." Our study design purposely lacked structured social interactions between teammates, because we did not expect that such a component would be part of a scaled-up version of the intervention. We believe that attempts to strengthen the social aspects of the intervention (e.g., through regular team meetings) could lead to corresponding increases in the magnitude of the team effects.

Our study has several limitations. First, our sample size precluded us from taking a more granular look at the types of pairings that inhibit and promote goal attainment. Second, our measure of quit beliefs relies on predictions that were not elicited in an incentive-compatible manner, leaving open the possibility that the self-reported predictions are somehow systematically biased. Some studies find that incentivized and unincentivized predictions are similar (Delavande, Giné and McKenzie, 2011), although we are unable to confirm that subjects reported their true beliefs. Third, as noted, we cannot fully disentangle the extent to which the observed team effects are directly attributable to the team incentives, as opposed to another aspect of the intervention.

While studies have shown the presence of peer effects for smoking initiation among adolescents and cessation among spouses, our study is among the first to identify the broader peer effects of quitting smoking in an adult population. Our findings may be transferrable across a number of low-income settings, but they are especially relevant for smoking populations in Asia, where the majority of the world's smokers live. Team incentives may offer a viable, cost-effective alternative to current smoking cessation approaches in low-resource settings.[36] In light of the strong peer effects produced by our team-based intervention, there is a need for research that examine the social multiplier of more commonly implemented tobacco control policies. The findings raise exciting new possibilities for mobilizing peer pressure to effect positive health behavior change.

[36] White, Dow and Rungruanghiranya (2013) shows that the team-based intervention studied here is more cost-effective in Thailand than conventional smoking cessation aids, such as nicotine gum and prescription medication.

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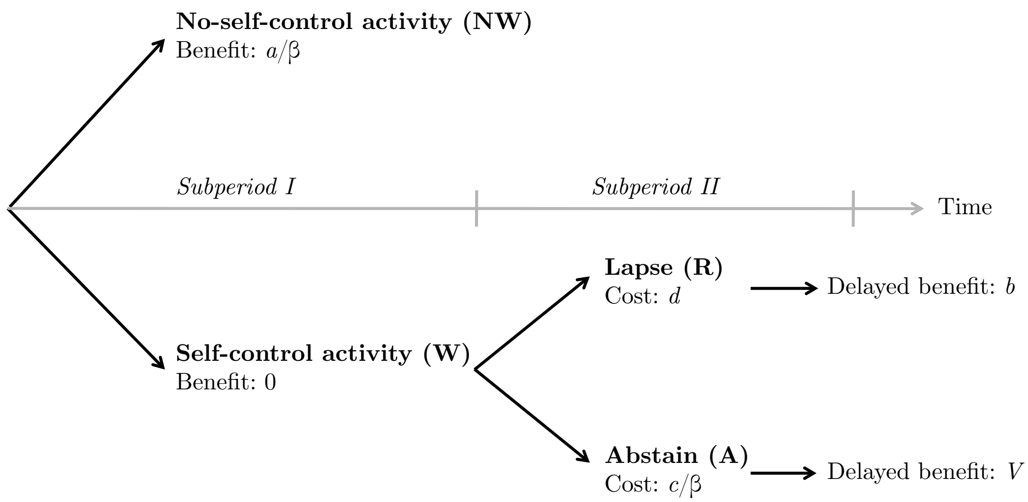
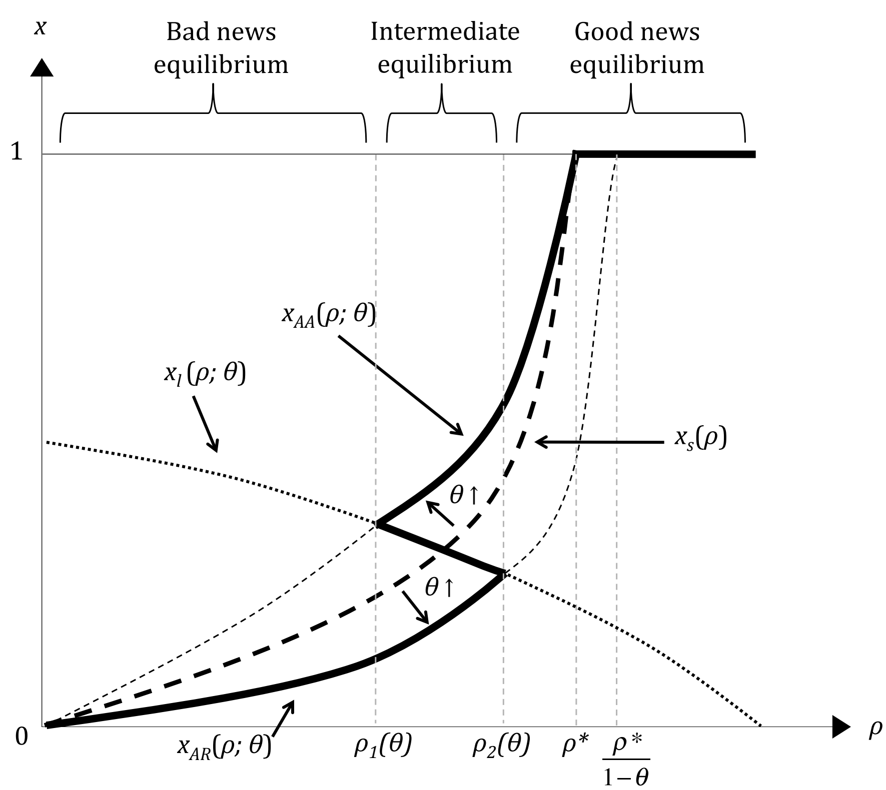
Figure 1: Decision Tree of Payoffs for Any Given Period $t = 1, 2$



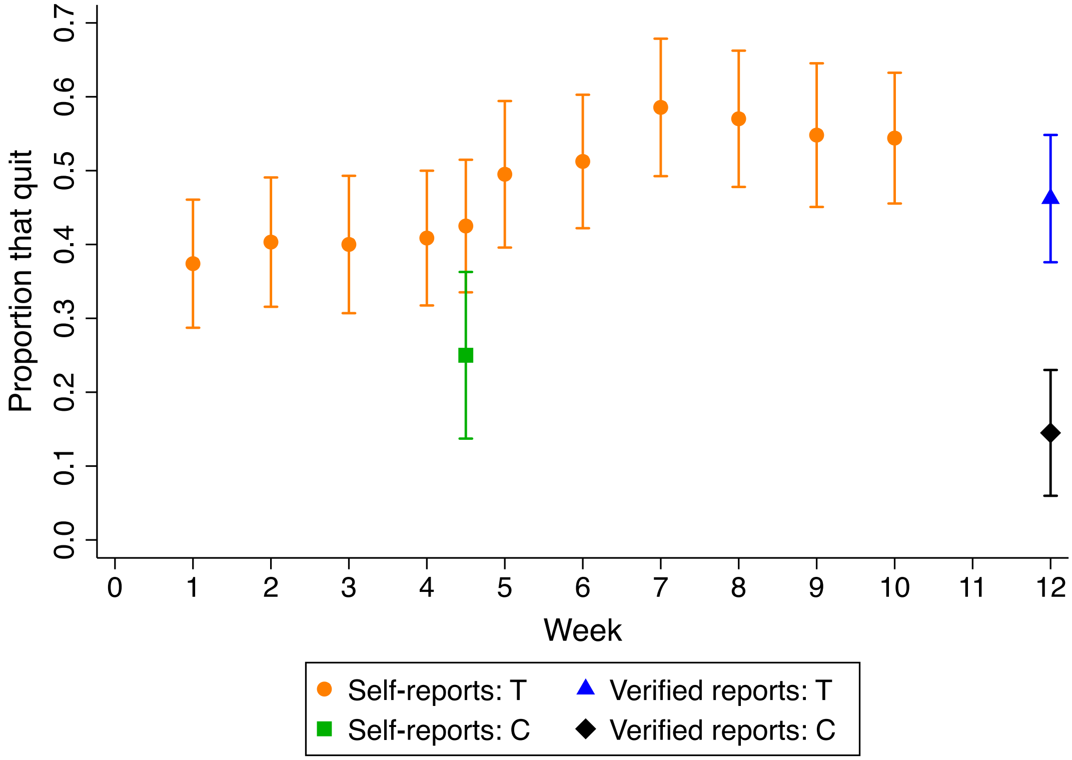

Note: Adapted from Battaglini, Bénabou and Tirole (2005). The key alteration is the addition of a cost from a lapse, $d$.

Figure 2: Equilibrium Self-Control



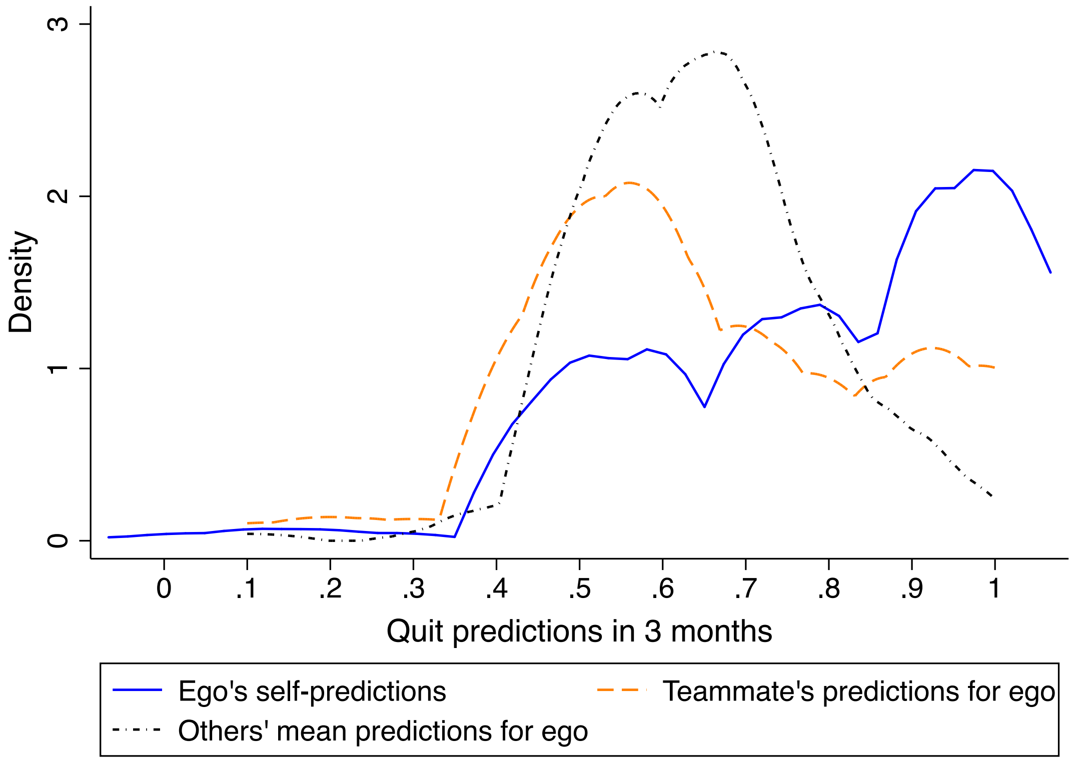

Note: Adapted from Battaglini, Bénabou and Tirole (2005). The upward-sloping dashed line (▬ ▬ ) denotes the single-agent case; the solid line (▬▬) denotes the two-agent case.

Figure 3: Quitting by Week




Note: Error bars represent a 95% confidence interval. Quit status at 4.5 weeks (one month) was self-reported to project staff over the phone. Other self-reports were made in person to deposit collectors. Quit status at 12 weeks (three months) was verified using a urine test. T = treatment group, C = control group

Figure 4: *Ex Ante* Quit Predictions




Note: Baseline predictions of the probability that an index person (ego) will not be smoking in three months. The kernel densities are derived from an Epanechnikov function with optimal bandwidth.

Table 1: Team Characteristics

| | | | By quit status at three months | | Difference in means: |
|---|---|---|---|---|---|
| | $N$ | Mean | Smoke | Quit | (4) - (3) |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A. Pre-enrollment team characteristics* | | | | | |
| Pre-selected teammate | 132 | 0.106 | 0.127 | 0.082 | -0.045 |
| Teammate is best friend in the trial | 132 | 0.318 | 0.268 | 0.377 | 0.109 |
| Teammate is 1 of 5 best friends in the trial | 132 | 0.659 | 0.577 | 0.754 | 0.177** |
| Distance between teammates' houses (km) | 130 | 0.994 | 0.962 | 1.031 | 0.069 |
| Pre-enrollment relationship | | | | | |
| Acquaintances or strangers | 132 | 0.333 | 0.268 | 0.410 | 0.142* |
| Close friends | 132 | 0.288 | 0.296 | 0.279 | -0.017 |
| Relatives | 132 | 0.288 | 0.268 | 0.311 | 0.043 |
| Pre-enrollment contact $\geq$ weekly | 117 | 0.624 | 0.579 | 0.667 | 0.088 |
| *Panel B. Post-enrollment team characteristics* | | | | | |
| Earned team bonus | 132 | 0.273 | 0.000 | 0.590 | 0.590*** |
| Post-enrollment contact $\geq$ weekly | 117 | 0.632 | 0.596 | 0.667 | 0.071 |
| Post-enrollment conversations about smoking or trial $\geq$ weekly | 117 | 0.530 | 0.404 | 0.650 | 0.246*** |
| Teammate asked or tried to convince ego to quit 2+ times | 102 | 0.598 | 0.548 | 0.633 | 0.085 |
| Teammate gave ego advice about how to quit 2+ times | 102 | 0.559 | 0.524 | 0.583 | 0.059 |
| Teammate calmed ego down when stressed or irritated 2+ times | 101 | 0.406 | 0.317 | 0.467 | 0.150 |
| Teammate expressed pleasure/confidence in ego's quit efforts 2+ times | 102 | 0.578 | 0.476 | 0.650 | 0.174* |
| Ego asked or tried to convince teammate to quit 2+ times | 102 | 0.598 | 0.500 | 0.667 | 0.167* |
| Ego gave teammate advice about how to quit 2+ times | 102 | 0.539 | 0.405 | 0.633 | 0.228** |
| Teammate and ego have ever smoked together | 102 | 0.578 | 0.548 | 0.600 | 0.052 |
| Teammate and ego have smoked together since enrolling in the trial | 102 | 0.235 | 0.262 | 0.217 | -0.045 |

Note: This table includes pre-selected and randomly formed teams in the treatment group. Responses were reported by the index person (ego) during a survey at three months. Significance: * 0.10 ** 0.05 *** 0.01.

Table 2: Effect of Social Ties of Teammates on Three-Month Quit Status

| | | Randomly formed teams in the treatment group | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pre-selected teammate | -0.117 | | | | | |
| | (0.143) | | | | | |
| Teammate is best friend in trial | | 0.235** | | | | |
| | | (0.108) | | | | |
| Teammate is 1 of 5 best friends in trial | | | 0.265** | | | |
| | | | (0.119) | | | |
| Distance between teammates' houses (km) | | | | 0.003 | | |
| | | | | (0.028) | | |
| Pre-enrollment relationship with teammate | | | | | | |
| Acquaintances or strangers (ref) | | | | | | |
| Close friends | | | | | -0.083 | |
| | | | | | (0.127) | |
| Relatives | | | | | -0.084 | |
| | | | | | (0.132) | |
| Pre-enrollment contact $\geq$ weekly | | | | | | 0.128 |
| | | | | | | (0.095) |
| Control for meeting size | No | Yes | Yes | No | No | No |
| Number of participants | 132 | 116 | 116 | 116 | 108 | 104 |
| Number of teams | 66 | 59 | 59 | 58 | 54 | 58 |
| Log likelihood | -90.8 | -78.0 | -77.4 | -80.3 | -74.4 | -71.1 |

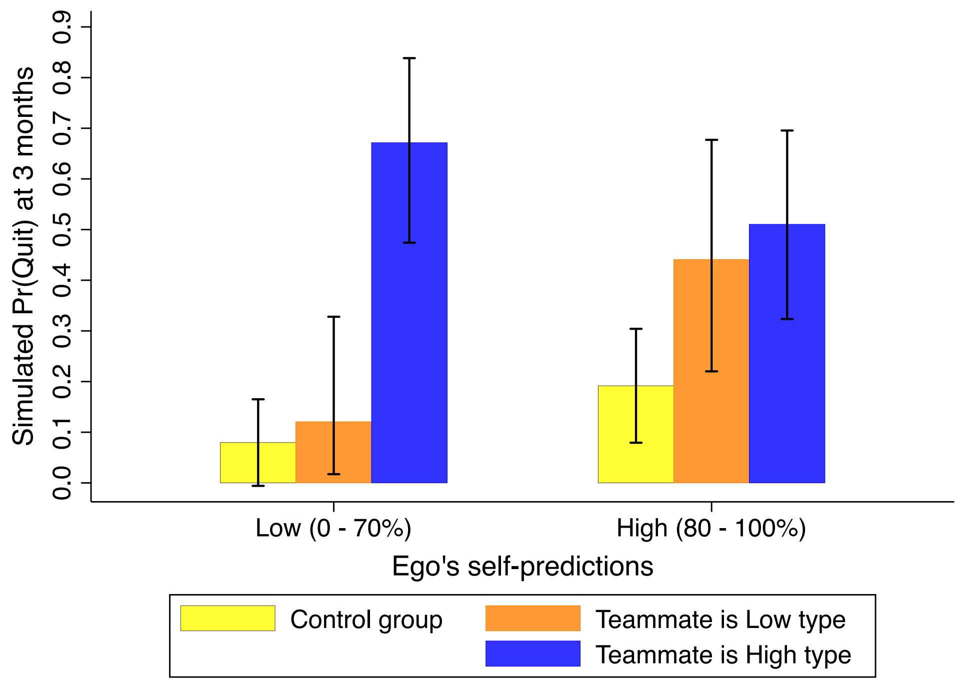
Note: This table reports average marginal effects of quitting at three months, based on probit models. Robust standard errors clustered at the team level are in parentheses. Significance: * 0.10 ** 0.05 *** 0.01.

Table 3: Teammates' Quit Predictions
(Randomly Formed Teams in the Treatment Group)

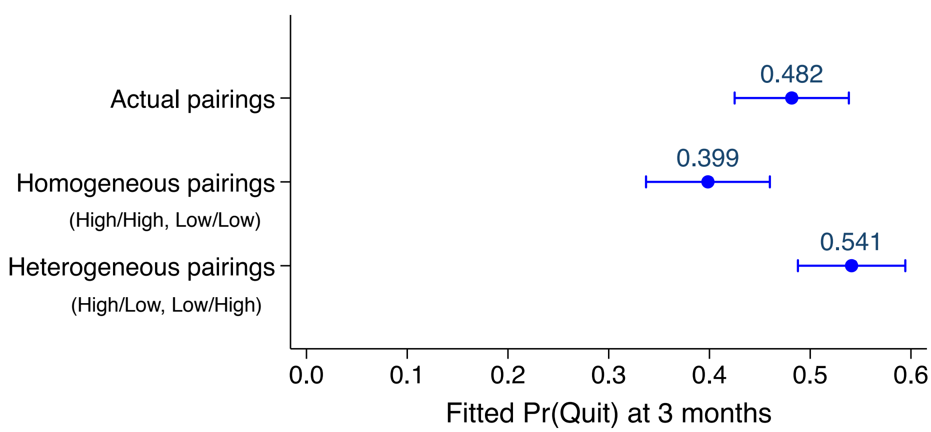
| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Teammate's self-predictions | 0.423** | 0.453*** | | | |
| | (0.195) | (0.175) | | | |
| Ego's self-predictions | 0.635*** | 0.178 | 0.236 | | |
| | (0.173) | (0.207) | (0.234) | | |
| Ego's predictions for teammate | | | 0.322 | | |
| | | | (0.226) | | |
| Team type, based on self-predictions | | | | | |
| Ego low, teammate low (ref) | | | | | |
| Ego low, teammate high | | | | 0.324** | 0.469*** |
| | | | | (0.144) | (0.117) |
| Ego high, teammate low | | | | 0.361** | 0.280* |
| | | | | (0.144) | (0.126) |
| Ego high, teammate high | | | | 0.447*** | 0.327*** |
| | | | | (0.133) | (0.110) |
| Controls | No | Yes | Yes | No | Yes |
| Number of participants | 116 | 116 | 102 | 116 | 116 |
| Number of teams | 59 | 59 | 59 | 59 | 59 |
| Pseudo-$R^2$ | 0.03 | 0.29 | 0.28 | 0.07 | 0.32 |
| Log likelihood | -78.3 | -56.8 | -50.9 | -74.5 | -54.7 |

Note: This table reports average marginal effects of quitting at three months based on probit models. Robust standard errors clustered at the team level are in parentheses. Control variables are listed in Appendix Table A1, including in the table's note. Team type in Models 4 and 5 is based on the self-predictions of ego and teammate dichotomized as low (0-70%) and high (80-100%). Significance: * 0.10 ** 0.05 *** 0.01.

Figure 5: Heterogeneous Team Effects
(Randomly Formed Teams in the Treatment Group)



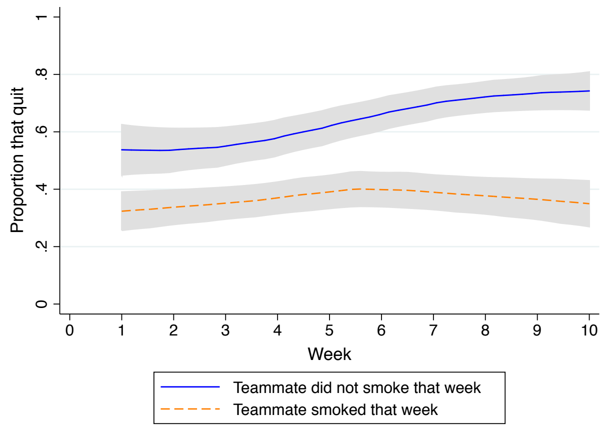

(a) Effect of teammates' self-predictions on simulated Pr(Quit) at 3 months



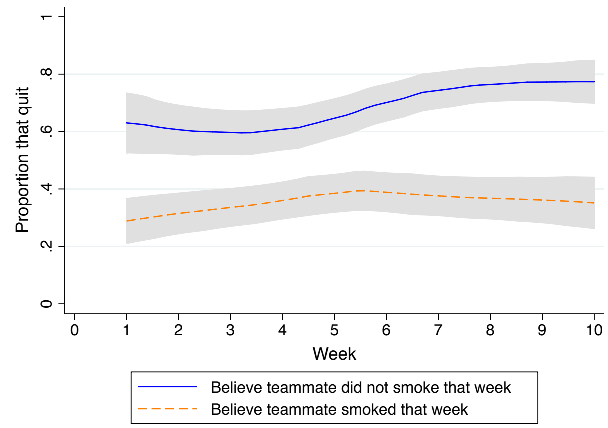

(b) Average fitted Pr(Quit) under 3 scenarios

Note: Self-predictions for quitting are dichotomized at the median into low ($0 - 70\%$) and high ($80 - 100\%$). Panel (a) is derived from a Monte Carlo simulation of Model 5 in Table 3 (1,000 repetitions). Panel (b) shows the predicted outcomes based on actual pairings as assigned and two hypothetical pairing regimes: homogeneous pairings in which both teammates are low types or both are high types, and heterogeneous pairings in which one teammate is low type and one is high type.

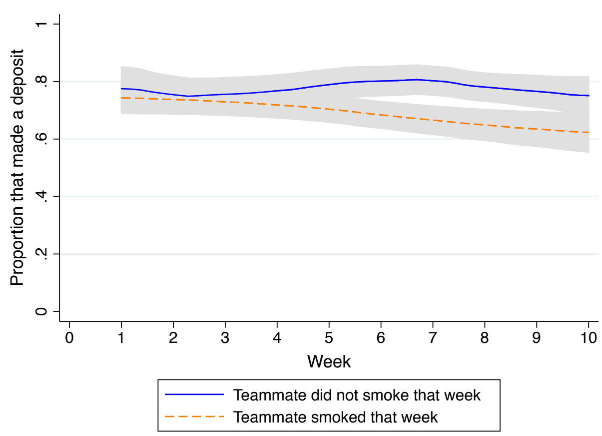
Figure 6: Association Between Ego's Outcomes and Teammate's Quit Status (Randomly Formed Teams in the Treatment Group)



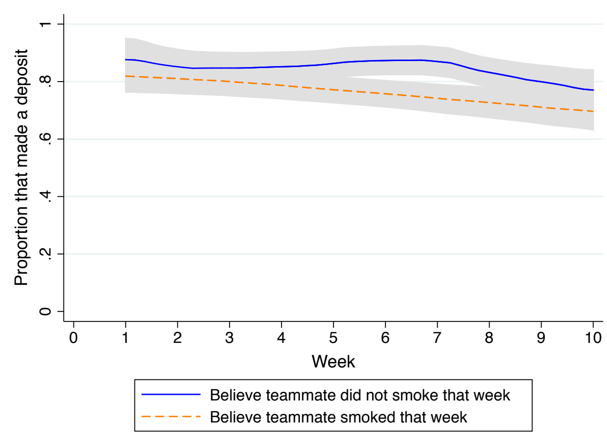

(a) % quit, by teammate's smoking status that week




(b) % quit, by one's beliefs about teammate's smoking status that week




(c) % made a deposit, by teammate's smoking status that week




(d) % made a deposit, by one's beliefs about teammate's smoking status that week

Note: Displayed are kernel-weighted local polynomial regressions using an Epanechnikov kernel. Gray bands represent a 95% confidence interval.

Table 4: Association Between Teammates' Behavior by Week

| | Ego's quit status | | | Ego made a deposit | |
|---|---|---|---|---|---|
| | Probit | Probit | OLS | Probit | OLS |
| | (1) | (2) | (3) | (4) | (5) |
| Teammate made a deposit that week | 0.101* | 0.008 | 0.001 | 0.256** | 0.068** |
| | (0.056) | (0.018) | (0.028) | (0.049) | (0.034) |
| | [1128] | [916] | [1128] | [1128] | [1128] |
| Teammate made a deposit the week before | 0.114** | 0.027 | -0.019 | 0.325*** | 0.067** |
| | (0.052) | (0.018) | (0.034) | (0.046) | (0.028) |
| | [1128] | [916] | [1128] | [1128] | [1128] |
| Teammate reported not smoking that week | 0.202*** | 0.078** | 0.227* | 0.058 | 0.032 |
| | (0.076) | (0.034) | (0.115) | (0.056) | (0.024) |
| | [1073] | [872] | [1073] | [1073] | [1073] |
| Teammate reported not smoking the week before | 0.155** | -0.003 | 0.119 | 0.027 | 0.017 |
| | (0.066) | (0.018) | (0.093) | (0.062) | (0.026) |
| | [805] | [792] | [805] | [805] | [805] |
| Ego believes teammate did not smoke that week | 0.228*** | 0.073** | 0.195** | 0.060 | -0.002 |
| | (0.067) | (0.035) | (0.076) | (0.055) | (0.029) |
| | [999] | [824] | [999] | [999] | [999] |
| Ego believes teammate did not smoke the week before | 0.170*** | -0.002 | 0.124* | 0.057 | 0.009 |
| | (0.064) | (0.019) | (0.066) | (0.064) | (0.026) |
| | [742] | [740] | [742] | [742] | [742] |
| Week fixed effects | Yes | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | No | Yes | No |
| Quit status in prior week | No | Yes | No | No | No |
| Person fixed effects | No | No | Yes | No | Yes |

Note: Each coefficient, reported as an average marginal effect, is drawn from a separate regression conducted at the person-week level. Quitting refers to abstaining from smoking as self-reported that week. Robust standard errors clustered at the team level are in parentheses. The number of observations from each regression is in brackets. The enrollment week is omitted from all models and Week 1 from models with lags. Significance: * 0.10 ** 0.05 *** 0.01.

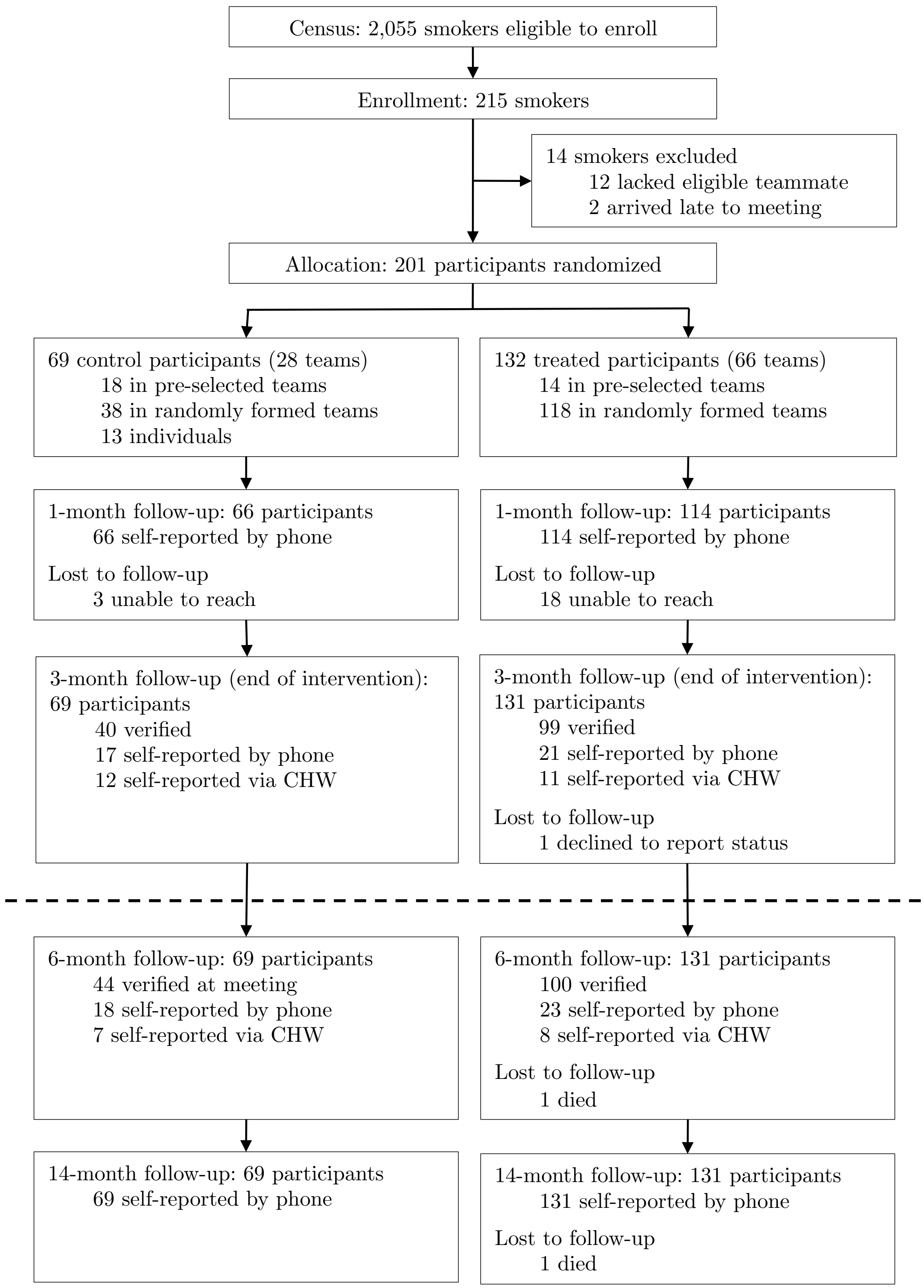
Table 5: Effect of Teammate's Quitting on Ego's Quitting at 3 Months
(Randomly Formed Teams in the Treatment Group)

| | Ego's quit status (Reduced form) | | Teammate's quit status (First stage) | | Ego's quit status (Second stage) | | |
|---|---|---|---|---|---|---|---|
| | OLS | Probit | OLS | Probit | 2SLS | Bivariate probit | Probit |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Teammate's quit status | | | | | 0.492* | 0.358*** | 0.176 |
| | | | | | (0.270) | (0.136) | (0.119) |
| Mean predictions of others for teammate | 0.593* | 0.550* | 1.204*** | 1.239*** | | | |
| | (0.347) | (0.320) | (0.360) | (0.341) | | | |
| Constant | -0.208 | | -0.537 | | 0.056 | | |
| | (0.425) | | (0.395) | | (0.318) | | |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of participants | 117 | 117 | 117 | 117 | 117 | 117 | 117 |
| Number of teams | 59 | 59 | 59 | 59 | 59 | 59 | 59 |
| *F* statistic of instrument | | | 11.2 | 8.8 | | | |

Note: Coefficients are reported as average marginal effects, with robust standard errors clustered at the team level in parentheses. All models control for sex, age, income, occupation, religion, cigarettes per day, type of tobacco, and ego's self-predictions for quitting. The two-stage least squares (2SLS) and bivariate probit models in Columns 5 and 6 instrument for teammate's quit status at three months using all participants' mean quit predictions for the teammate at baseline, excluding the predictions of the index person and the teammate herself. Model 6 includes bootstrapped standard errors. Model 7 is the naïve estimator. Significance: * 0.10 ** 0.05 *** 0.01.

# Appendix A Additional Figures and Tables

Figure A1: Study Profile



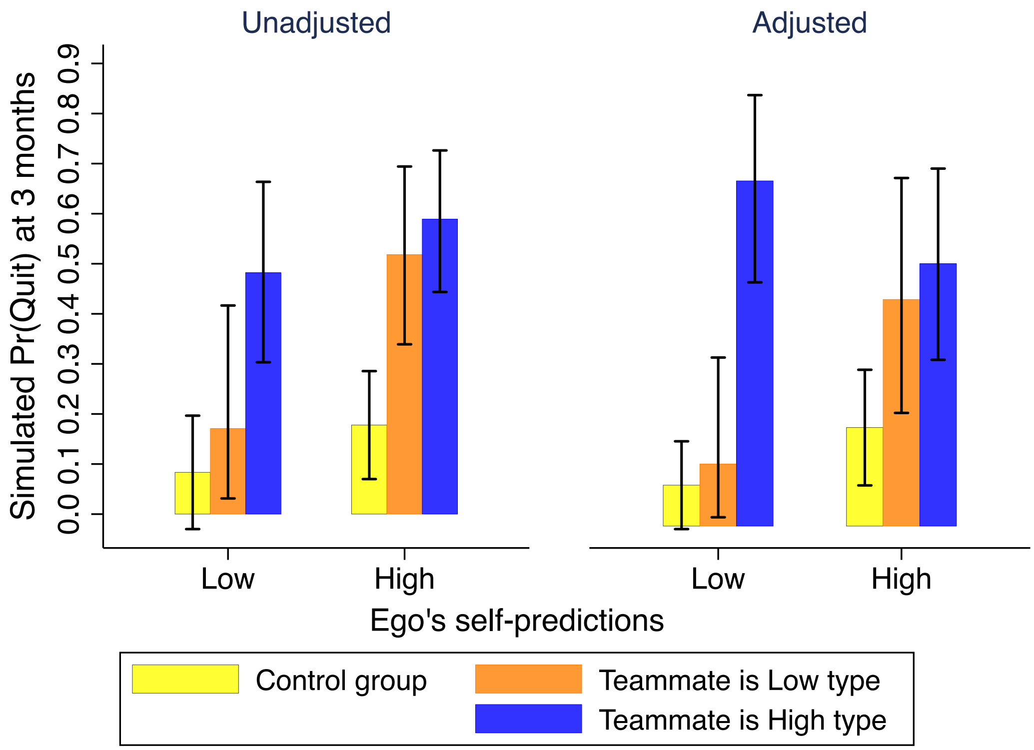

Note: This study only uses data listed above the horizontal dotted line.
CHW = community health worker

Table A1: Baseline Characteristics, by Dichotomized IV
(Randomly Formed Teams in the Treatment Group)
(IV = Others' Mean Predictions for Teammate)

| | All | Dichotomized IV below median | Dichotomized IV above median | Difference in means: (3) - (2) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Male | 0.888 | 0.881 | 0.895 | 0.014 |
| Age | 52.07 | 51.92 | 52.23 | 0.31 |
| | (13.71) | (14.47) | (13.00) | |
| Monthly household income, in $100s | 4.108 | 3.734 | 4.495 | 0.761 |
| | (6.040) | (5.006) | (6.975) | |
| Education | | | | |
| 0-3 years | 0.474 | 0.508 | 0.439 | -0.069 |
| 4-6 years | 0.233 | 0.254 | 0.211 | -0.043 |
| 7+ years | 0.293 | 0.237 | 0.351 | 0.114 |
| Buddhist | 0.741 | 0.864 | 0.614 | -0.250** |
| Currently married | 0.802 | 0.847 | 0.754 | -0.093 |
| Works in agriculture | 0.655 | 0.576 | 0.737 | 0.161* |
| Self-rated health | 0.284 | 0.271 | 0.298 | 0.027 |
| Average cigarettes smoked per day | 11.86 | 10.95 | 12.80 | 1.850 |
| | (9.08) | (8.02) | (10.04) | |
| Type of tobacco used | | | | |
| Manufactured cigarettes only | 0.328 | 0.373 | 0.281 | -0.092 |
| Handrolled cigarettes only | 0.483 | 0.492 | 0.474 | -0.018 |
| Both | 0.190 | 0.136 | 0.246 | 0.110* |
| Number of past quit attempts | 2.565 | 2.339 | 2.798 | 0.459 |
| | (2.689) | (2.258) | (3.076) | |
| Number of years since initiated smoking | 32.59 | 32.64 | 32.53 | -0.110 |
| | (13.28) | (14.87) | (14.47) | |
| Self-prediction of Pr(Quit) in 3 mos. | 0.787 | 0.805 | 0.768 | -0.037 |
| | (0.231) | (0.213) | (0.249) | |
| Planning to quit within 6 mos. | 0.828 | 0.814 | 0.842 | 0.028 |
| Belief that quitting is very important | 0.767 | 0.729 | 0.807 | 0.078 |
| Number of other adult smokers in HH | 0.698 | 0.729 | 0.667 | -0.062 |
| | (0.962) | (0.827) | (1.091) | |
| 5 best friends are all smokers | 0.483 | 0.458 | 0.509 | 0.051 |
| Number of observations | 116 | 59 | 57 | |

Note: Mean and standard deviation (in parentheses) of each variable are reported.
Significance: * 0.10 ** 0.05 *** 0.01.

Figure A2: Heterogeneous Team Effects
(Randomly Formed Teams in the Treatment Group)




Note: Self-predictions for quitting are dichotomized at the median into low $(0-70\%)$ and high $(80-100\%)$. Figures are derived from a Monte Carlo simulation of Models 4 and 5 in Table 3 (1,000 repetitions). Error bars represent the 95% confidence interval.