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# Incentive programmes for smoking cessation: cluster randomized trial in workplaces in Thailand

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## ABSTRACT OBJECTIVE

To compare several monetary incentive programmes for promoting smoking abstinence among employees who smoke at workplaces in a middle income country.

## DESIGN

Parallel group, open label, assessor blinded, cluster randomized controlled trial.

## SETTING

Large industrial workplaces in metropolitan Bangkok, Thailand.

## PARTICIPANTS

Employees who smoked cigarettes and planned to quit within six months recruited from 101 worksite clusters (84 different companies).

## INTERVENTIONS

Worksites were digitally cluster randomized by an independent investigator to usual care or usual care plus one of eight types of incentive programmes. Usual care consisted of one time group counseling and cessation support through a 28 day text messaging programme. The incentive programmes depended on abstinence at three months and varied on three intervention components: refundable deposits, assignment to a teammate, and bonus size (\$20 (£15; €17) or \$40).

## MAIN OUTCOME MEASURES

The primary outcome was biochemically verified seven day point prevalence smoking abstinence at 12 months. Secondary outcomes were programme acceptance at enrollment and smoking abstinence at three months (end of intervention) and at six months. All randomized participants who had complete baseline information were included in intention-to-treat analyses; participants with missing outcomes were coded as continuing smokers.

## RESULTS

Between April 2015 and August 2016, the trial enrolled 4190 participants. Eighteen were omitted because of missing baseline covariates and death before the primary endpoint, therefore 4172 participants were included in the intention-to-treat analyses. Programme acceptance was relatively high across all groups: 58.7% (2451/4172) overall and 61.3% (271/442) in the usual care group. Abstinence rates at 12 months did not differ among deposit programmes (336/2253, 14.9%) and non-deposit programmes (280/1919, 14.6%; adjusted difference 0.8 points, 95% confidence interval -2.7 to 4.3, P=0.65), but were somewhat lower for team based programmes (176/1348, 13.1%) than individual based programmes (440/2824, 15.6%; -3.2 points, -6.6 to -0.2, P=0.07), and higher for \$40 bonus programmes (322/1954, 16.5%) than programmes with no bonus (148/1198, 12.4%; 5.9 points, 2.1 to 9.7, P=0.002). The \$40 individual bonus was the most efficacious randomization group at all endpoints. Intervention components did not strongly interact with each other.

## CONCLUSIONS

Acceptance of monetary incentive programmes for promoting smoking abstinence was high across all groups. The \$40 individual bonus programmes increased long term smoking abstinence compared with usual care, although several other incentive designs did not, such as team based programmes and deposit programmes. Incentive design in workplace wellness programmes might influence their effectiveness at reducing smoking rates in low resource settings.

## TRIAL REGISTRATION

ClinicalTrials.gov (NCT02421224).

## Introduction

Tobacco use is associated with 6.4 million deaths each year, with nearly three quarters occurring in low and middle income countries.<sup>1</sup> Although many low and middle income countries have passed regulations to discourage tobacco use, major gaps in tobacco control remain. Less than 10% of middle income countries and only 1% of low income countries cover the cost of nicotine replacement therapy and smoking cessation services, in part because these treatment options are relatively expensive.<sup>2</sup> In the absence of funding to support standard treatment options, a need exists for alternative cost effective approaches to promote smoking cessation. The workplace has been a popular environment for promoting tobacco cessation. Many

## WHAT IS ALREADY KNOWN ON THIS TOPIC

Previous studies have shown a modest effect of incentives on smoking cessation rates in high income countries, including in workplace settings

A lack of evidence exists on the impact of incentives in a workplace setting in low and middle income countries

Few studies have compared the effectiveness of different incentive designs for smoking cessation in any setting

## WHAT THIS STUDY ADDS

Monetary incentives for smoking cessation can increase abstinence compared with usual care in a workplace setting in a middle income country

Different incentive designs could have different effects on abstinence

Programmes that offered deposit contracts and team incentives were less effective treatment strategies than programmes that offered an individual bonus

employers have integrated tobacco cessation into their workplace wellness programmes, motivated by increasing worker productivity and decreasing medical costs. Workplace wellness programmes are a promising setting for scaling interventions because employers have the financial resources to sustain the programme over time, and offer a way to access some hard-to-reach groups (eg, the blue collar workers in this study). However, employers in low and middle income countries have been slower to implement such programmes. For example, 89% of large employers in the United States offer tobacco cessation programmes to workers compared with 22% in countries in Asia Pacific and 24% in Latin America.<sup>3</sup>

Workplace wellness programmes increasingly use monetary incentives to encourage employees to quit smoking, and incentives for tobacco cessation is a common component of these programmes.<sup>4</sup> The incentives might spur programme participation, improve worker satisfaction, and in principle could reduce healthcare costs. A 2019 Cochrane review found evidence of the effectiveness of incentives for improving smoking cessation rates at long term follow-up across multiple settings.<sup>5</sup> Recent trials conducted in workplaces in high income countries found that monetary incentives increase smoking abstinence rates when combined with usual care, smoking cessation group training, e-cigarettes, or nicotine replacement therapy.<sup>6-8</sup> These studies address some of the shortcomings identified in previous work, including small sample sizes, high attrition rates, and small incentive amounts.<sup>9</sup> However, evidence in low and middle income countries remains scant.<sup>5</sup> We are not aware of any studies of workplace based incentives for smoking cessation in low and middle income countries, although two studies have found incentives to promote smoking abstinence in rural communities of a middle income country.<sup>10 11</sup>

In recent years, researchers have tested different incentive designs to promote healthy behavior, including conditional payments, team incentives, and refundable deposits. Studies have found that conditional payments (bonuses) are effective in motivating a range of healthy behaviors, including in low and middle income countries.<sup>12 13</sup> Team incentives might be designed to leverage competition, cooperation, peer influence, or social support, although it is not well established whether they improve upon individual schemes.<sup>14 15</sup> Refundable deposits, or deposit contracts, ask users to put their own money at risk. This self-management technique has been commonly used in applied behavioral economics research to help people struggling with addiction or self-control to commit to meeting a health goal that involves avoiding tempting options.<sup>16</sup> Refundable deposits, which harness psychological tendencies towards loss aversion and over optimism, have been effective in promoting change in behavior, including alcohol abstinence and weight loss.<sup>17 18</sup> Refundable deposits for smoking cessation have been found to improve long term smoking cessation rates in

the US, the Philippines, and Thailand.<sup>6 10 11</sup> However, few studies have compared incentive designs for smoking cessation head to head, and research in low and middle income countries is lacking. Moreover, a stark gap was found in evidence about the possible synergy of incentive components when offered together.

In this study, we evaluated multiple incentive programmes that were based on an individual bonus, a team bonus, refundable deposits, and a combination of the bonus types and deposits. The SMILE trial (Social and Monetary Incentives for Smoking Cessation at Large Employers) was implemented in a scalable workplace setting in the Bangkok metropolitan area of Thailand. We supplemented deposits with an individual or team bonus to overcome low programme acceptance rates (10-15%) and high failure rates (>50%) found in previous studies of deposit programmes for smoking cessation.<sup>6 10 11</sup> The key hypotheses included: programme acceptance would decrease in groups that offered refundable deposits and increase in groups that offered a bonus, with greater increases for larger bonus amounts; and smoking abstinence would increase in groups offered a refundable deposit or bonus, with greater increases for larger bonus amounts and for team versus individual bonuses. This study seeks to advance the literature on the use of refundable deposits and monetary incentives to improve health behavior by focusing on workplaces in a middle income country setting where previous research is limited.

## Methods

### Study design

We undertook a nine group cluster randomized trial at factory worksites in a setting with high smoking prevalence. In 2014, adult smoking prevalence was about 20% overall in Thailand, but it was more than 34% among factory workers in the metropolitan area of Bangkok.<sup>19 20</sup> Worksites were randomly assigned to usual care or to usual care plus one of eight incentive designs. The randomization groups varied according to three prespecified intervention components: presence of a deposit contract, assignment to a teammate or not, and size of the cash bonus (\$0, \$20 (€15; €17), or \$40). Table 1 shows intervention components for all randomization groups. Our analyses were conducted by randomization group and by intervention component to facilitate hypothesis testing and interpretation of the results. Institutional review boards at the University of California, Berkeley (USA) and Mahidol University (Thailand) approved the trial protocol. We did not make any changes to the prespecified methods after trial commencement. The supplementary appendix provides details of the study protocol.

### Study setting and participants

Thailand has experienced a large decline in smoking prevalence over the past three decades, mostly attributable to its adoption of comprehensive tobacco control policies.<sup>20 21</sup> Most smokers in Thailand attempt

Table 1 | Randomization groups and intervention components

No	Randomization group	Intervention component				
		Usual care	Deposits	\$20 bonus	\$40 bonus	Team
1	Control	✓	—	—	—	—
2	\$20 individual bonus	✓	—	✓	—	—
3	\$40 individual bonus	✓	—	—	✓	—
4	Team bonus	✓	—	—	✓	✓
5	Deposits	✓	✓	—	—	—
6	Deposits plus teammate	✓	✓	—	—	✓
7	Deposits plus \$20 individual bonus	✓	✓	✓	—	—
8	Deposits plus \$40 individual bonus	✓	✓	—	✓	—
9	Deposits plus team bonus	✓	✓	—	✓	✓

Usual care consisted of brief group counseling and text messaging programme for smoking cessation. Reference groups for components are: no deposits (v deposits), no bonus or \$0 (v \$20 or \$40 bonus), and individual based (v team based). \$1.00=£0.78, €0.85.

to quit unassisted and many smoking treatment services are limited to large hospitals.<sup>22 23</sup> The combination of a high demand for quitting and low use of professional services for smoking cessation make Thailand an excellent setting for testing innovative approaches to promote quitting.

The study setting was 101 large factory worksites of 84 companies located in the metropolitan area of Bangkok. Nearly all participating companies were in the manufacturing sector, with the largest proportion involved in the manufacture of food products (22%), machinery and equipment (12%), and motor vehicles and other transport equipment (10%). The worksites were spread across six provinces in and around Bangkok proper: Ayuthaya, Bangkok, Nakhon Pathom, Pathum Thani, Samut Prakan, and Samut Sakhon.

We recruited worksites through multiple channels. Study staff invited companies to participate at workshops sponsored by a workplace health consortium, contacted companies located in Bangkok area industrial zones, and asked participating companies for referrals. Inclusion criteria for worksites were at least 200 workers (our definition of a large employer), at least 30 smokers based on company estimates, willingness to follow the study protocol, and located in the metropolitan area of Bangkok. These criteria were verified during pretrial meetings with a company representative.

There were several inclusion criteria for participants at each participating worksite: being a full time employee at the worksite, aged 18 years or older, having ever smoked at least 100 cigarettes, smoking at least 10 cigarettes per week on average, and wanting to quit smoking within the next six months. We anticipated that requiring full time employment would decrease loss to follow-up. Exclusion criteria were plans to leave the company within the next year, being unable or unwilling to provide informed consent, and being pregnant. A company employee at each worksite distributed a brief self-administered screening questionnaire to each employee, and then eligible people were invited to a baseline interview with study staff, during which screening responses were verified for final determination of eligibility. Informed consent was administered before the baseline interview.

### Randomization and masking

After baseline surveys were completed, worksites were randomly assigned in equal proportion to one of the nine randomization groups. The cluster randomized design mitigated concerns of within worksite contamination and employers viewed the design as more acceptable than person level randomization. We followed a covariate adaptive procedure in which we minimized the P value from a joint F test as a balance criterion implemented over 1000 iterations.<sup>24 25</sup> In Monte Carlo simulations, minimization has been found to facilitate covariate balance.<sup>26</sup> The randomization procedure included several worksite level covariates: province, mean age, mean proportion born in Thailand, mean cigarettes per week, mean proportion who want to quit smoking within six months, number of employees, and estimated smoking prevalence based on baseline data.

A study investigator implemented the random allocation sequences by using computer generated random numbers, concealing the sequence from field staff, company employees, and participants until after the baseline survey was completed. Participants were informed of their assignment in an enrollment invitation letter sent after the baseline survey. The assessor of the biochemical urine test was masked to randomization groups. We were not able to mask other aspects of the trial.

### Interventions and procedures

The trial included nine randomization groups (table 1). Participants in the control group received usual care only. Participants in the other groups received one or more of the intervention components. We use an exchange rate of \$1=30 Thai baht. All participant compensation was overseen and paid for by the study rather than the employer.

At an initial enrollment meeting, attendees received study details, provided written informed consent, completed a brief interview, and received group counseling. Follow-up assessments were performed at three, six, and 12 months when participants completed a brief survey, provided a urine sample, and received brief one-to-one smoking cessation counseling. All participants who completed the baseline survey were asked to complete the follow-up assessments, regardless of whether they took part in the main

intervention. Participants earned \$5 for completing each of the assessments at baseline, enrollment, three months, six months, and 12 months, for a total of up to \$60. During each visit participants chose either \$5 in cash or a gift of equivalent value (eg, a t shirt).

**Usual care**—All participants received usual care consisting of two elements: in-person group cessation counseling and cessation support through text messaging. Group counseling consisted of 90 minutes of counseling delivered at each worksite by a counselor trained in smoking cessation. Group counseling provides an opportunity for social learning and also has lower costs per smoker than individual counseling. The text messaging programme, developed by the Thai Health Professional Alliance against Tobacco, provided one to three messages per day for 28 days, which included advice, support, and encouragement for quitting smoking. Similar programmes have been found to be efficacious.<sup>27</sup> All materials were in Thai. Participants had the option to specify the phone number of a household member where the messages could be sent.

**Deposits**—Participants in deposit programmes (groups 5-9) were asked to provide refundable deposits contingent on smoking abstinence. These participants made a minimum initial contribution of \$3 at the enrollment meeting, which was kept safe by an appointed company representative. The low initial contribution was designed to boost programme acceptance. Participants then received a personal deposit box, made of metal and designed to be tamper proof (shown in supplementary fig B1). Participants were free to make additional voluntary contributions to the box until the three month follow-up assessment. Study staff encouraged participants to contribute at least as much as they had typically spent on tobacco. Participants gave the project an additional \$5 as collateral for the safe return of the box to deter tampering or theft. At the three month follow-up assessment, study staff opened each box with a can opener and recorded the total balance. All deposits were returned to participants if they were confirmed to be abstinent during the three month assessment. Deposits were forfeited to the project if participants were found to have smoked.

**Teammate**—Participants in team based programmes (groups 4, 6, and 9) were randomly assigned to another participant from the same worksite as a teammate. Team assignment was stratified by work shift and native language to facilitate opportunities for communication. Pairings were announced at the enrollment meeting at each worksite.

**Cash bonus**—Participants in groups 2 and 7 were eligible for a cash bonus of \$20 for abstaining from smoking at three months. Participants in groups 3 and 8 were eligible for a bonus of \$40 for abstinence at three months. These amounts were roughly equivalent to one and two days' wages, respectively. Participants in groups 4 and 9 were eligible for a team bonus of \$40 each that depended on both team members abstaining from smoking at three months. The team bonus was

also designed to activate a sense of social commitment and peer pressure to quit. In teammate assignment stratum with an odd number of participants, extra participants were not paired with teammates; instead they were eligible for a \$40 individual bonus.

**Randomization groups**—The nine randomization groups consisted of the following: (1) control group (usual care only); (2) \$20 individual bonus; (3) \$40 individual bonus; (4) team bonus; (5) deposits; (6) deposits plus teammate (no bonus); (7) deposits plus \$20 individual bonus; (8) deposits plus \$40 individual bonus; (9) deposits plus team bonus (table 1). Groups 2 and 3 were similar to incentives for smoking cessation used in many studies.<sup>28</sup> Incorporating both bonus amounts allowed us to determine whether larger bonuses were more effective, as some researchers have hypothesized.<sup>9</sup> Group 5 is a pure deposit contract (with no bonus except for the return of the person's deposits) that replicates the intervention group in the CARES trial in the Philippines.<sup>11</sup> Group 6, which consisted of teammates and deposits but no other team based incentives, allows for identification of the independent effect of buddy based peer support, for which the evidence is mixed.<sup>29-30</sup> Groups 7 and 8 combine deposits with an individual bonus for abstaining, similar to the individual deposit based intervention in a prominent trial led by Halpern.<sup>6</sup> Finally, group 9 combines a deposit contract with the same \$40 team bonus in group 4, which replicates the intervention used in our pilot study.<sup>10</sup>

## Outcomes

The primary outcome was biochemically verified seven day point prevalence smoking abstinence obtained 12 months after enrollment (more than nine months after all incentives were awarded), as recommended by experts for abstinence measures.<sup>31-32</sup> To be classified as having abstained, participants had to self-report having abstained for the seven days before the test and to test negative for nicotine and its metabolite cotinine using a rapid urine test. We used the COT One Step Cotinine Test (Alfa Scientific Designs), an immunoassay that detects urine cotinine at a cut-off concentration of 200 ng/mL within about 10 minutes. Participants who did not complete a survey or urine test were coded as continuing smokers. At each endpoint, we recorded whether the participant indicated active use of nicotine replacement therapy or e-cigarettes to help with smoking cessation. In awarding incentives and in analyses, users of nicotine replacement therapy or e-cigarettes who reported smoking abstinence were treated as abstinent. If participants did not attend follow-up visits, field team members would contact them by phone to determine their self-reported smoking status. For participants who self-reported to have abstained, field team members scheduled a time within the next 48 hours to collect a urine sample for biochemical verification. This allowance was necessary because sometimes the visits took place during work hours when some managers did not allow participants to complete the assessment.



Secondary endpoints included biochemically verified seven day point prevalence abstinence at three months (end of intervention) and at six months (three months after the incentives ended). Another secondary outcome was programme acceptance of interventions, defined as attending the onsite enrollment meeting, consenting to enter the trial, and, if applicable, making at least the minimum deposit contribution.

### Statistical analysis

Our main analyses estimate the intention-to-treat effect of each randomization group and each intervention component on programme acceptance and smoking abstinence at each endpoint. The intention-to-treat sample includes all participants who completed the baseline questionnaire, regardless of whether they enrolled in the main intervention. During the follow-up assessments, we attempted to contact everyone in the intention-to-treat sample.

The primary outcome variable was verified abstinence, in which we considered participants who reported nicotine replacement therapy or e-cigarette use for cessation as abstinent as long as no cigarettes were used. We used generalized linear mixed effects models with a logit link for the binary outcome and a random intercept at the worksite level to adjust for the clustering of participants within a worksite. We reported risk differences from unadjusted and adjusted models. The fixed part of the unadjusted models included randomization group or intervention component only. The adjusted models also included prespecified variables known to be related to the outcomes, including baseline personal characteristics and smoking history. Personal characteristics were age, sex, household income per capita, educational attainment, marital status (married, not married), any children, and place of childhood (urban Thailand, rural Thailand, other countries). Smoking characteristics were average cigarettes per day, moderate to high nicotine dependence (Fagerström test for nicotine dependence<sup>33</sup> score  $\geq 5$ ), number of past attempts to quit, number of years since starting smoking, and quit intentions (wanting to quit within three months or not). Regression analyses included squared terms for age, education, income, and cigarettes per day to account for a potential non-linear association with smoking abstinence.<sup>34 35</sup>

Effect estimates by randomization group were principally evaluated relative to the control group. Effect estimates by intervention component were principally evaluated relative to the omitted category: no deposits (*v* deposits), individual (*v* team) intervention, and no bonus (*v* \$20 or \$40 bonus). We also evaluated additional prespecified comparisons that are listed in supplementary table A1. Several contrasts were designed to isolate the added value of a particular intervention component: deposits group versus deposits plus teammate group; deposits group versus deposits plus individual bonus group; deposits plus teammate group versus deposits plus team bonus group; team bonus versus deposits plus team

bonus group. Several other contrasts were designed to compare the relative effectiveness of the individual and team designs: \$40 individual bonus group versus (the equal sized) team bonus group; pooled \$20 and \$40 individual bonus groups versus team bonus group; deposits plus \$40 individual bonus group versus deposits plus team bonus group; and pooled groups with deposits plus an individual bonus versus deposits plus team bonus. We also compared the \$20 versus \$40 individual bonus groups to test whether abstinence increases with bonus size.

In subanalyses, we pooled groups 2-9 to compare the average effect of receiving usual care plus any incentive programme compared with usual care alone (group 1) by using adjusted and unadjusted mixed effects models similar to those described. We also investigated interaction effects between intervention components by using the same adjusted mixed effects models. We further investigated whether those in a bonus group or deposit group were more likely to relapse after the end of the incentive period.

We performed four sensitivity analyses to assess the robustness of the estimated intervention effects. Firstly, we conducted the analysis on a per protocol basis; that is, among those who had accepted their assigned intervention. Secondly, we used complete outcome data without assuming that participants with missing data had smoked. This analytic strategy is common in the smoking cessation literature,<sup>36 37</sup> although the approach could be a concern for incentive based interventions in which missingness cannot be ignored. Thirdly, we excluded from the analysis the outcome data from participants who reported currently using nicotine replacement therapy or e-cigarettes. Finally, we performed multiple imputation using chained equations (50 iterations) to impute missing outcome data at each endpoint.<sup>38 39</sup>

The study was designed to provide 80% power to detect absolute differences of at least 7.5 percentage points in smoking abstinence rates for pairwise comparisons between any of the incentive based randomization groups versus the usual care control group. The detectable effect size is the same as that used by Halpern and colleagues.<sup>6</sup> The power calculations, generated using the Optimal Design software package (version 3.01)<sup>40</sup> were derived from a model for a two level cluster randomized trial with a binary variable, where the two levels are person (level 1) and worksite (level 2, the level of randomization). The binary outcome was modelled using a Bernoulli trial with a logit link. The model included a random intercept for each worksite. Our sample size calculations used a type I error of  $\alpha=0.05$  and assumed that we would recruit 100 worksites, each with 500 employees, 75 screened people (anticipated to include five former or non-smokers), and 70 smokers. We assumed that 60 of 70 smokers per worksite would complete the baseline questionnaire and therefore would be eligible for the trial and part of the randomized sample. We further assumed that 36 of 60 randomized people per worksite would

participate in the main intervention (60% programme acceptance). At 60 randomized people and 36 intervention participants per worksite, we anticipated a total sample size of 6000 randomized people and 3600 intervention participants. Assumptions about anticipated recruitment and enrollment at each worksite were based on preliminary data obtained from worksites about smoking rates and from our pilot trial.<sup>10</sup> Additional details are provided in the supplementary appendix. Analyses were performed in Stata (version 14.2, Stata, College Station, TX).

**Patient and public involvement**

Participants were not involved directly in designing the study, although we solicited feedback from company representatives at workshops held before the start of the intervention. We conducted interviews (n=18, two per randomization group) lasting 45-60 minutes with study participants after the 12 month assessment to learn about their experience during the trial. We solicited feedback on pooled (not group specific) results at workshops held with company representatives after the end of the study period.

**Results**

**Sample characteristics**

Figure 1 presents the study flow diagram. In total, 101 worksites were recruited, and 7910 smokers at these sites completed the screening survey. Of these, 4190 smokers were deemed eligible and completed the baseline survey between April 2015 and August 2016. The 101 sites were then randomly allocated across the nine groups, with 11-12 sites in each group, and an average of 53 eligible participants per site. Eighteen smokers (<0.05%) were omitted because of missing baseline covariates (n=8) and death before the primary endpoint (n=10), providing a denominator of 4172 for intention-to-treat analyses.

Table 2 presents baseline characteristics of the 4172 participants by randomization group. Personal and smoking characteristics were generally well balanced across randomization groups, with a few exceptions that reinforced the importance of presenting adjusted results. Median age varied across randomization groups from 30 to 35 years old; median educational attainment varied from 9 to 12 years; and the

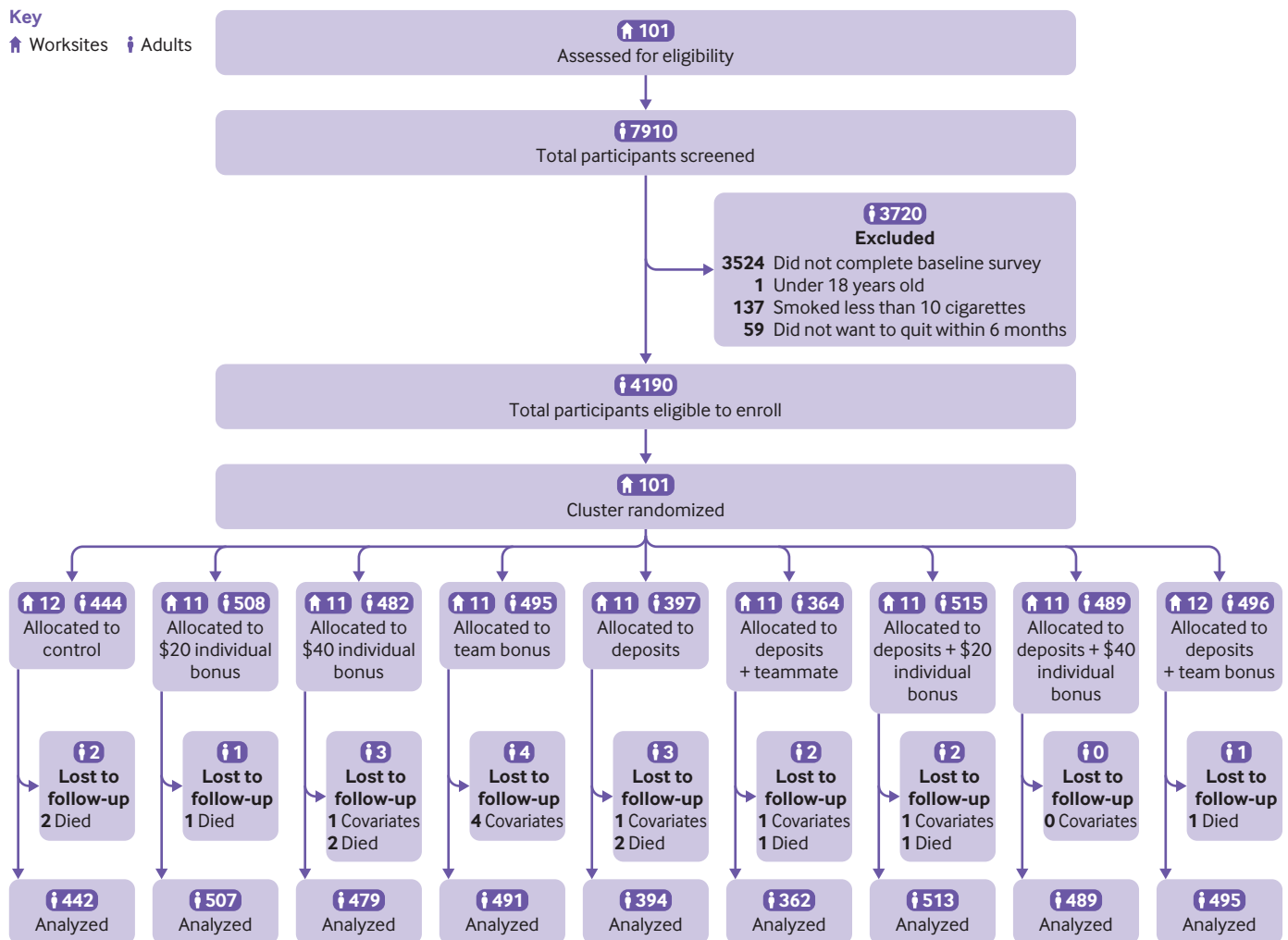


Fig 1 | Study flowchart. 4172 participants were included in the primary analysis. \$1.00=£0.78, €0.85

Table 2 | Baseline characteristics by randomization group. Data are numbers (percentages) unless stated otherwise

Characteristics	Randomization group									
	Full sample (n=4172)	Control (n=442)	\$20 individual bonus (n=507)	\$40 individual bonus (n=479)	Team bonus (n=491)	Deposits (n=394)	Deposits+ teammate (n=362)	Deposits+\$20 individual bonus (n=513)	Deposits+\$40 individual bonus (n=489)	Deposits+team bonus (n=495)
<b>Panel A: personal characteristics</b>										
Median (IQR) age (years)	32 (26-40)	32 (26-39)	31 (26-38)	33 (27-42)	30 (25-38)	32 (27-40)	35 (28-42)	34 (27-40)	31 (25-38)	32 (25-40)
Men	4061 (97)	439 (99)	487 (96)	475 (99)	484 (99)	376 (95)	347 (96)	503 (98)	464 (95)	486 (98)
Median (IQR) household income per capita*	2.7 (1.8-3.7)	2.8 (1.9-3.7)	2.5 (1.6-3.7)	2.8 (1.8-3.8)	2.6 (1.8-3.6)	2.9 (1.8-4.0)	2.6 (1.7-3.7)	2.7 (1.8-3.6)	2.6 (1.7-3.6)	2.6 (1.8-3.7)
Median (IQR) education	9 (6-12)	9 (6-12)	10 (8-12)	9 (6-12)	9 (7-12)	12 (6-14)	9 (7-12)	9 (6-12)	9 (6-12)	9 (6-12)
Married	2889 (69)	325 (74)	346 (68)	338 (71)	327 (67)	276 (70)	248 (69)	371 (72)	338 (69)	320 (65)
Any children	1879 (45)	218 (49)	233 (46)	203 (42)	222 (45)	170 (43)	172 (48)	243 (47)	214 (44)	204 (41)
Place of childhood:										
Urban Thailand	943 (23)	87 (20)	138 (27)	107 (22)	103 (21)	100 (25)	79 (22)	98 (19)	105 (21)	126 (25)
Rural Thailand	2386 (62)	290 (66)	297 (59)	309 (65)	289 (59)	220 (56)	237 (65)	359 (70)	271 (55)	314 (63)
Other countries	643 (15)	65 (15)	72 (14)	63 (13)	99 (20)	74 (19)	46 (13)	56 (11)	113 (23)	55 (11)
<b>Panel B: smoking characteristics</b>										
Median (IQR) cigarettes per day	6 (4-10)	6 (4-10)	6 (4-10)	8 (4-10)	6 (3-10)	5 (3-10)	6 (4-10)	5 (4-10)	5 (3-10)	7 (4-10)
Nicotine dependent	491 (12)	63 (14)	62 (12)	55 (11)	59 (12)	47 (12)	42 (12)	54 (11)	53 (11)	56 (11)
Median (IQR) past quit attempts	1.0 (1.0-2.0)	1.0 (1.0-3.0)	1.0 (1.0-3.0)	1.0 (1.0-2.0)	1.0 (1.0-3.0)	1.0 (1.0-3.0)	1.0 (1.0-2.0)	1.0 (1.0-2.0)	1.0 (1.0-2.0)	1.0 (1.0-2.0)
Median (IQR) years since started	13 (8-21)	13 (8-21)	12 (7-20)	15 (9-23)	12 (7-20)	13 (8-22)	16 (10-24)	15 (9-21)	13 (7-20)	14 (8-21)
Want to quit within three months	2422 (58)	250 (57)	297 (59)	269 (56)	276 (56)	258 (65)	208 (57)	263 (51)	316 (65)	285 (58)
<b>Panel C: worksite characteristics</b>										
Mean (SD) employees at worksite	1075 (999)	583 (363)	1329 (1020)	1030 (1019)	1305 (798)	1116 (1183)	857 (787)	920 (564)	1366 (1559)	1070 (912)
Mean (SD) smokers at worksite	305 (268)	287 (308)	468 (421)	247 (130)	300 (173)	182 (106)	253 (152)	223 (123)	380 (257)	365 (357)
Field team	217.6 (52)	308 (70)	241 (48)	173 (36)	333 (68)	261 (66)	151 (42)	254 (50)	220 (45)	235 (47)
Implementation issues	579 (14)	32 (7)	29 (6)	78 (16)	36 (7)	111 (28)	64 (18)	52 (10)	117 (24)	60 (12)

IQR=interquartile range.  
\$1.00=£0.78, €0.85.  
\*Income values are monthly, given in hundreds of dollars (\$1=30 Thai baht).

proportion married varied from 65% to 74%. The sample comprised 2.7% of women, consistent with smoking trends in Thailand.<sup>20</sup>

Smoking characteristics were closely balanced across groups as expected owing to emphasis on smoking characteristics in the covariate adaptive randomization routine used to improve balance. Median cigarettes per day varied across randomization groups, from five to eight. Worksite trial related characteristics were also balanced across groups. The typical smoker in the sample can be characterized as male, married, with some high school education, and from rural Thailand before moving to Bangkok; he smokes six cigarettes per day, has smoked for 13 years, has tried to quit once previously, and wants to quit smoking within three months.

Follow-up completion was 68.9% (2873/4172) at three months, 64.6% (2694/4172) at six months, and 64.4% (2686/4172) at 12 months (supplementary table B2). Among those still alive and lost to follow-up at 12 months, 55.4% (823/1486) had left the company, 28.7% (427/1486) were not permitted by their supervisor to leave their work post to complete the follow-up assessment, 8.5% (126/1486) had withdrawn from the study, 2.0% (29/1486) were on sick or personal leave on the day of the follow-up assessment, and 1.4% (21/1486) had moved to a different worksite. No difference in follow-up completion was found between randomization groups at 12 months (P=0.57). Participants who did not complete the 12 month follow-up assessment tended to be younger, less likely to be married, foreign, and from larger worksites (supplementary table B3).

### Programme acceptance

Programme acceptance was relatively high across all groups: 58.7% (2451/4172) overall and 61.3% (271/442) in the control group (supplementary table B1). We present the unadjusted and covariate adjusted differences in programme acceptance by randomization group (table 3) and by intervention component (table 4). Programme acceptance decreased by a non-significant 8.6 percentage points in the deposit only group (95% confidence interval -22.6 to 5.3, P=0.23) and by a non-significant 12.9 points in the deposit plus teammate group (-28.0 to 2.2, P=0.09) compared with the control group.

When we pooled groups by intervention component, we found that being assigned to a deposit programme was associated with an adjusted decrease in acceptance of 7.6 points (95% confidence interval -14.2 to -1.0, P=0.03). Acceptance did not differ greatly between team programmes and individual programmes (1.7 points, -6.4 to 9.8, P=0.68). We also found that being assigned to a \$20 bonus increased acceptance by 6.8 points compared with no bonus (-2.5 to 16.1, P=0.15), while being assigned to a \$40 bonus increased acceptance by 4.6 points (-3.5 to 12.8, P=0.27) compared with no bonus. Therefore, acceptance did not significantly increase with bonus size.

Table 3 | Effect of randomization groups on programme acceptance and smoking abstinence

Randomization group	Difference in programme acceptance*		Difference in abstinence at three months*		Difference in abstinence at six months*		Difference in abstinence at 12 months†	
	Unadjusted (1)	Adjusted (2)	Unadjusted (3)	Adjusted (4)	Unadjusted (5)	Adjusted (6)	Unadjusted (7)	Adjusted (8)
Control group (reference)								
\$20 individual bonus	6.4 (-5.2 to 18.1)	6.7 (-5.1 to 18.6)	4.1 (-3.0 to 11.2)	5.0 (-2.6 to 12.5)	2.1 (-4.9 to 9.2)	3.0 (-4.4 to 10.3)	5.5 (-2.2 to 13.1)	6.2 (-1.7 to 14.2)
\$40 individual bonus	1.2 (-14.2 to 16.6)	1.1 (-14.1 to 16.4)	10.6 (2.5 to 18.6)	10.6 (2.7 to 18.5)	12.3 (3.5 to 21.0)	12.9 (4.2 to 21.6)	12.6 (5.3 to 20.0)	12.9 (5.6 to 20.2)
Team bonus	-0.4 (-12.6 to 11.7)	-0.2 (-12.4 to 12.1)	2.5 (-3.7 to 8.8)	2.7 (-3.3 to 8.8)	-0.3 (-6.8 to 6.2)	-0.1 (-6.3 to 6.1)	2.8 (-3.4 to 9.0)	3.0 (-2.8 to 8.8)
Deposits	-7.4 (-20.7 to 6.0)	-8.6 (-22.6 to 5.3)	6.9 (-1.0 to 14.8)	4.7 (-2.4 to 11.9)	3.1 (-4.5 to 10.6)	1.0 (-5.6 to 7.5)	5.3 (-1.5 to 12.0)	3.2 (-2.9 to 9.4)
Deposits plus teammate	-12.1 (-27.0 to 2.7)	-12.9 (-28.0 to 2.2)	2.5 (-4.0 to 9.1)	1.9 (-4.1 to 8.0)	1.9 (-5.4 to 9.2)	1.5 (-5.4 to 8.3)	4.3 (-2.3 to 10.9)	3.5 (-2.4 to 9.5)
Deposits plus \$20 individual bonus	-5.4 (-18.7 to 7.8)	-5.8 (-19.3 to 7.7)	4.8 (-1.1 to 10.7)	4.6 (-1.2 to 10.5)	3.0 (-3.1 to 9.0)	2.7 (-3.3 to 8.8)	4.8 (-2.1 to 11.6)	4.5 (-2.0 to 11.1)
Deposits plus \$40 individual bonus	-8.3 (-20.0 to 3.3)	-8.5 (-20.5 to 3.5)	9.2 (-0.4 to 18.7)	8.5 (0.1 to 16.9)	10.4 (1.2 to 19.6)	9.2 (1.1 to 17.3)	9.5 (1.9 to 17.2)	8.6 (2.1 to 15.1)
Deposits plus team bonus	3.3 (-8.4 to 15.0)	3.8 (-7.8 to 15.4)	5.3 (-2.5 to 13.0)	6.4 (-1.2 to 14.0)	3.6 (-3.1 to 10.3)	4.9 (-2.0 to 11.9)	3.8 (-2.2 to 9.8)	5.0 (-1.0 to 11.0)
Full set of covariates	No	Yes	No	Yes	No	Yes	No	Yes
Mean dependent variable for control group (%)	61.3	61.3	9.0	9.0	10.9	10.9	9.5	9.5

Percentage point differences in programme acceptance at enrollment and seven day smoking abstinence at each endpoint (three, six, and 12 months) for each randomization group relative to control group (omitted category). Endpoints are defined as time since enrollment when the intervention began. Estimates are from generalized linear mixed effects models with a logit link, random intercept by worksite, and robust 95% confidence intervals (in parentheses). Models 2, 4, 6, and 8 also adjust for personal and smoking characteristics listed in table 2, and squared terms for age, education, income, and cigarettes per day. Number of observations=4172; number of clusters=101. \$1.00=€0.78, €0.85.

\*Secondary outcomes.  
†Primary outcome.

Smoking abstinence

Supplementary table B2 reports smoking status at each endpoint. We obtained urine cotinine test results from 58% (2423/4172) of all participants and 91% (635/700) of self-reported abstainers at three months. We biochemically confirmed the self-reported smoking status for 78% (547/700) of self-reported abstainers. Of those not confirmed, 9% (65/700) did not complete a test and 13% (88/700) were discordant (9% if we ignore participants who used nicotine replacement therapy or e-cigarettes for cessation). Only 1% (30/4172) of participants across all randomization groups reported using nicotine replacement therapy or e-cigarettes for cessation at three months. Similar follow-up rates, concordance of self-reported and biochemically verified results, and nicotine replacement therapy and e-cigarette use were found at six and 12 months, although urine testing rates declined over time.

Smoking abstinence at three and six months (secondary endpoints)—Table 3 and table 4 report the intention-to-treat effects of randomization groups and intervention components on smoking abstinence at the end of the three month intervention (when all abstinence incentives were paid) and at six months (three months after the intervention ended). About 9% (40/442) of the control group abstained at three months. No randomization group had abstinence below that of the control group, though most had overlapping confidence intervals. The highest three month abstinence rate of 18.4% (88/479) occurred in the \$40 individual bonus group (adjusted difference 10.6 points, 95% confidence interval 2.7 to 18.5, P=0.009). Deposit programmes increased abstinence at three months by an insignificant 2.0 points (-1.8 to 5.8, P=0.30) compared with no deposit programmes. Team programmes marginally decreased abstinence by 3.3 points (-7.4 to 0.8, P=0.11) compared with individual programmes. Bonus programmes of \$20 had no effect on abstinence (1.9 points, -2.7 to 6.5, P=0.42) compared with no bonus programmes, whereas \$40 bonus programmes increased abstinence by 5.8 points (1.4 to 10.3, P=0.01), which is equivalent to a 64% increase in abstinence. The effects of randomization groups and intervention components on smoking abstinence at six months (three months after the incentives ended) are similar to those at three months. Abstinence at six months increased 6.9 points in the \$40 bonus programmes compared with no bonus programmes (95% confidence interval 2.3 to 11.5, P=0.003) and decreased 4.6 points in the team programmes compared with the individual programmes (-8.8 to -0.4, P=0.03).

Smoking abstinence at 12 months (primary endpoint)—The pattern of abstinence remained broadly similar at the primary endpoint of 12 months (table 3 and table 4). Abstinence rates between three and 12 months increased among seven of the nine groups, underscoring the low rate of relapse in this sample. Deposit programmes had a negligible effect on abstinence compared with no deposit



Table 4 | Effect of intervention components on programme acceptance and smoking abstinence

Intervention component	Difference in programme acceptance*		Difference in abstinence at three months*		Difference in abstinence at six months*		Difference in abstinence at 12 months†	
	Unadjusted (1)	Adjusted (2)	Unadjusted (3)	Adjusted (4)	Unadjusted (5)	Adjusted (6)	Unadjusted (7)	Adjusted (8)
Deposits	-7.1 (-13.7 to -0.6)	-7.6 (-14.2 to -1.0)	2.7 (-1.3 to 6.7)	2.0 (-1.8 to 5.8)	2.4 (-1.5 to 6.3)	1.5 (-2.2 to 5.3)	1.6 (-2.0 to 5.2)	0.8 (-2.7 to 4.3)
Team	1.4 (-6.8 to 9.5)	1.7 (-6.4 to 9.8)	-4.1 (-8.4 to 0.2)	-3.3 (-7.4 to 0.8)	-5.6 (-10.0 to -1.2)	-4.6 (-8.8 to -0.4)	-4.0 (-7.7 to -0.3)	-3.2 (-6.6 to 0.2)
Bonus size								
\$20	6.1 (-3.1 to 15.4)	6.8 (-2.5 to 16.1)	0.6 (-3.9 to 5.2)	1.9 (-2.7 to 6.5)	-0.3 (-4.8 to 4.1)	0.9 (-3.6 to 5.4)	1.1 (-3.8 to 5.9)	2.3 (-2.6 to 7.1)
\$40	4.0 (-4.2 to 12.1)	4.6 (-3.5 to 12.8)	5.1 (0.3 to 9.8)	5.8 (1.4 to 10.3)	6.1 (1.2 to 11.0)	6.9 (2.3 to 11.5)	5.0 (0.9 to 9.2)	5.9 (2.1 to 9.7)
Full set of covariates	No	Yes	No	Yes	No	Yes	No	Yes
Mean dependent variable for control group (%)	61.3	61.3	9.0	9.0	10.9	10.9	9.5	9.5

Percentage point differences in programme acceptance at enrollment and seven day smoking abstinence at each endpoint (three, six, and 12 months) for each intervention component. Omitted categories are no deposits (v deposits), individual (v team), and \$0 bonus (v \$20 and \$40). Endpoints are defined as time since enrollment when intervention began. Estimates come from generalized linear mixed effects models with a logit link, random intercept by worksite, and robust 95% confidence intervals (in parentheses). Models 2, 4, 6, and 8 also adjust for personal and smoking characteristics listed in table 2, and squared terms for age, education, income, and cigarettes per day. Number of observations=4172; number of clusters=101. \$1.00=£0.78, €0.85.

\*Secondary outcomes.

†Primary outcome.

programmes (0.8 points, 95% confidence interval -2.7 to 4.3, P=0.65). Team programmes decreased abstinence by 3.2 points compared with individual programmes (-6.6 to 0.2, P=0.07). Bonus programmes of \$40 increased abstinence by 5.9 points (2.1 to 9.7, P=0.002) compared with no bonus programmes, and \$20 bonus programs had a smaller effect (2.3 points, -2.6 to 7.1, P=0.40), although this effect size did not differ statistically from no bonus programmes or \$40 bonus programmes (P=0.16). We further assessed smoking abstinence at 12 months using several prespecified contrasts among randomization groups (supplementary table B10). Pooling both individual bonus groups was 9.6 points more effective than the control group (3.1 to 16.0, P=0.004) and 6.6 points more effective than the individual team bonus (0.5 to 12.6, P=0.03). The \$40 individual bonus was 9.9 points more effective than the equal valued team bonus (2.9 to 16.8, P=0.005). Several other comparisons did not match the hypothesized relations, including that the deposits plus team bonus (group 9) would dominate the deposits alone, the team bonus alone, and the deposits plus teammate.

*Any incentive programme*—We next pooled groups 2-9 to compare the average effect of receiving usual care plus any incentive programme compared with usual care alone (supplementary table B5). We found that being assigned to any incentive programme increased abstinence at three months by 5.6 points (95% confidence interval 0.6 to 10.5, P=0.03), and this effect persisted to 12 months (5.9 points, 1.1 to 10.7, P=0.02).

*Interaction of intervention components*—Table 5 shows whether the intervention components interacted with each other to act as substitutes that dampened abstinence or as complements that enhanced abstinence when offered in combination. Across interaction models for abstinence at all endpoints, the main effect for the team based programmes is negative, ranging from -7.5 points at three months (95% confidence interval -14.6 to -0.4, P=0.04) to -8.9 points at 12 months (-15.2 to -2.7, P=0.005). The main effect for the \$40 bonus programmes is positive, ranging from 10.7 points at three months (2.3 to 19.0, P=0.01) to 12.4 points at 12 months (4.7 to 20.1, P=0.002). Across all interaction models, the interaction between deposits and a bonus was negative though not statistically significant, suggesting that the two components might substitute in part for one another. The interaction between deposits and having a teammate was positive, with an effect size of 8.9 points at 12 months (0.0 to 17.7, P=0.05). This result implies that the deposits were more effective when a participant had a teammate, a surprising finding given that the deposit plus teammate group had among the lowest abstinence rates at three months.

*Relapse*—Relapse after the end of the incentive period was uncommon (supplementary table B11). Among those who abstained at three months, 29.4% (169/575) had returned to smoking at six months, including 37.5% (15/40) of those in the control

group. Relapse rates were not statistically different when comparing those in a deposit programme with those in a no deposit programme, and comparing those in a bonus programme with those in a no bonus programme.

**Sensitivity analyses**—The sensitivity analyses of intervention effects on abstinence were consistent with the main analyses (supplementary tables B6–B9). In per protocol analyses, the magnitude of the intervention effects increased for most randomization groups and most intervention components. Team based programmes were associated with a marginally significant decrease in abstinence at 12 months of 4.3 points relative to individual programmes (95% confidence interval –8.8 to 0.2,  $P=0.06$ ), and \$40 bonus programmes increased abstinence by 8.5 points relative to no bonus programmes (2.8 to 14.1,  $P=0.003$ ). In complete case analyses, omitting people with missing outcome data, the effects were magnified further so that team based programmes decreased abstinence at 12 months by 5.5 points (–11.0 to 0.0,  $P=0.048$ ) and \$40 bonus programmes increased abstinence by 9.5 points (3.6 to 15.4,  $P=0.002$ ). When we omitted participants who used nicotine replacement therapy and e-cigarettes for cessation we found negligible effects on the estimates. Multiply imputing missing data also produced similar results.

## Discussion

The SMILE trial is a cluster randomized controlled trial that evaluated the effectiveness of several incentive programmes for smoking cessation among employees at 101 large workplaces in a middle income setting. We found evidence that assignment to usual care plus one of the incentive programmes reduced smoking abstinence at 12 months compared with usual care only. The \$40 bonus programmes raised 12 month abstinence rates by approximately 6 percentage points, more than 60% higher than in programmes with no bonus. The \$20 bonus programmes had point estimates about one third as large and were too imprecisely estimated at that size to distinguish effects

versus no bonus groups, although suggestive evidence of a dosage effect exists. We found no evidence that the refundable deposit programmes increased smoking abstinence. Compared with individual based incentive programmes, the team based programmes reduced smoking abstinence across several model specifications.

We observed high programme acceptance across all randomization groups. Nearly three in five smokers employed at these sites accepted the offered programme, suggesting high unmet demand for smoking cessation support. A 2017 national surveillance survey found that among adult smokers in Thailand who had ever tried to quit, only 2% used nicotine replacement therapy, 2% used care at a medical center, and less than 1% used the national quitline.<sup>41</sup> Whether the low use of those formal cessation methods is owing to low availability, low awareness, high cost, or other reasons remains unclear. However, this low baseline use of outside quitting support could help to explain why abstinence rates remained high between the three month and 12 month assessments. Other incentive based studies have tended to find declining abstinence over time in settings where enrollees are disproportionately composed of people who have already failed to quit with standard methods.<sup>42</sup> The finding of high programme acceptance points to the need in Thailand and other low resource settings for convenient, on site smoking cessation support for low wage workers who could have limited time and ability to access clinic based services.

Additionally, we observed large variation across worksites in the percentage of smokers who participated in our study, varying from 20% to 100% (supplementary fig B5). These stark cross site differences hint at the importance of institutional support in rolling out wellness programmes. We do not believe that the high rates of programme acceptance are owing to coercion from employers. Study staff reminded employers at multiple points not to pressure workers to participate, and we did not hear any complaints from workers about undue pressure. Employers

**Table 5 | Effect of uninteracted and interacted intervention components on smoking abstinence**

Intervention component	Difference in abstinence at 3 months*		Difference in abstinence at 6 months*		Difference in abstinence at 12 months†	
	(1)	(2)	(3)	(4)	(5)	(6)
Deposits	2.0 (–1.8 to 5.8)	5.7 (–3.0 to 14.3)	1.5 (–2.2 to 5.3)	1.2 (–6.9 to 9.2)	0.8 (–2.7 to 4.3)	4.0 (–3.9 to 11.9)
Team	–3.3 (–7.4 to 0.8)	–7.5 (–14.6 to –0.4)	–4.6 (–8.8 to –0.4)	–12.0 (–19.9 to –4.2)	–3.2 (–6.6 to 0.2)	–8.9 (–15.2 to –2.7)
\$20 individual bonus	1.9 (–2.7 to 6.5)	5.3 (–3.6 to 14.2)	0.9 (–3.6 to 5.4)	2.8 (–5.6 to 11.2)	2.3 (–2.6 to 7.1)	6.5 (–2.5 to 15.5)
\$40 individual bonus	5.8 (1.4 to 10.3)	10.7 (2.3 to 19.0)	6.9 (2.3 to 11.5)	11.5 (3.6 to 19.5)	5.9 (2.1 to 9.7)	12.4 (4.7 to 20.1)
Deposits×team	—	3.9 (–6.4 to 14.3)	—	12.1 (0.9 to 23.3)	—	8.9 (–0.0 to 17.7)
Deposits×\$20 bonus	—	–5.8 (–17.2 to 5.5)	—	–1.2 (–12.2 to 9.8)	—	–5.5 (–16.8 to 5.9)
Deposits×\$40 bonus	—	–7.2 (–18.9 to 4.4)	—	–3.7 (–14.8 to 7.4)	—	–7.2 (–17.0 to 2.7)
Deposits×team×\$40 bonus	—	1.4 (–9.9 to 12.7)	—	–4.1 (–15.0 to 6.8)	—	–3.5 (–12.0 to 4.9)
Full set of covariates	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent variable for control group (%)	9.0	9.0	10.9	10.9	9.5	9.5

Percentage point differences in seven day smoking abstinence at each endpoint (three, six, and 12 months) for each intervention component. Omitted categories are no deposits (v deposits), individual (v team), and \$0 bonus (v \$20 and \$40). Estimates come from generalized linear mixed effects models with a logit link, random intercept by worksite, and adjustment for personal and smoking characteristics listed in table 2, and squared terms for age, education, income, and cigarettes per day. Confidence intervals are provided in parentheses. Models 2, 4, and 6 include interaction terms among the intervention components. Number of observations=4172; number of clusters=101. \$1.00=£0.78, €0.85.

\*Secondary outcomes.

†Primary outcome.

had no specific incentive to pressure workers into participating, other than wanting to support smoking cessation. By contrast, some individual managers might have discouraged participation because of the required workday interruption, and so we cannot rule out coercion as one possible contributor. However, employer pressure should not have differed across randomization groups and can be considered part of the broader contextual effects of workplace smoking cessation interventions.

### Comparison with other studies

Previous studies of incentive based interventions for smoking cessation show that these programmes often have a modest effect on quit rates. Before 2015, the evidence had been graded as low owing to inadequate randomization and allocation procedures, deficient outcome reporting, and confounding.<sup>42</sup> Recently, several large trials conducted in workplaces in high income countries have found that monetary incentives increase smoking abstinence rates when combined with usual care, smoking cessation group training, e-cigarettes, or nicotine replacement therapy.<sup>6-8</sup>

Halpern and colleagues<sup>6</sup> reported a comparable effect from an individual bonus and refundable deposits in a study from a single employer in the US. While the Halpern study found larger effects at six months than we have observed, the 12 month results for individual bonus programmes from the two studies are similar (4.1 point increase in Halpern *v* 2.1 and 6.0 point increase for the \$20 and \$40 bonuses in our study, respectively), despite the somewhat smaller bonus amount in our study. The Halpern study found that deposit based programmes were highly effective at promoting abstinence among participants who would accept the deposit (30.8 point increase *v* usual care), whereas we did not observe a significant increase. This difference could be explained by several study design elements varying across the two studies, including that deposits in the Halpern study were paid by the programme rather than the participant, and the deposits were far larger relative to income than our initial minimum contribution (\$150 *v* \$3). Perhaps as a result of the larger deposits, programme acceptance of the deposit based programmes was much lower in the Halpern study, implying that self-selection was much stronger.

Our study is the first workplace trial to use incentives for smoking cessation in a middle income setting. As in other contexts, workplace cessation incentives for low wage smokers in middle income countries can provide a powerful inducement to quit smoking, which is also consistent with the sparse literature using incentives for other types of health behaviors in middle income countries.<sup>13</sup> Two studies have tested incentive based cessation programmes outside of a worksite setting in middle income countries.<sup>10 11</sup> White and colleagues<sup>10</sup> found that refundable deposits plus a team bonus promoted smoking abstinence among rural villagers in Thailand. In that study, most teammates had a pre-existing relationship with each other. In our study,

many teammates did not know each other before the study and did not interact during the study period. Interventions that better leverage existing social networks might produce larger and more positive peer effects than we observed in this study.<sup>43</sup>

Two additional factors could explain why abstinence rates were lower in the team bonus groups than the individual bonus groups. Firstly, the expected value of the team bonus was much lower than an equal sized individual bonus after accounting for a teammate's expected probability of failure. Secondly, it might have been demotivating to observe a teammate fail, or be likely to fail, to quit smoking.<sup>44</sup> In the other study in a middle income country, Giné and colleagues<sup>11</sup> found that refundable deposits were effective at promoting abstinence in rural villages of the Philippines. They used a home based deposit collection from community health workers, as did the rural Thailand study. Our reliance on participants to make voluntary deposits without any visits or reminders could have led to low use (supplementary table B12). A mobile based deposit account in which contributions are made automatically or with little effort could be a more effective approach, especially because smartphone penetration continues to increase in Thailand and other low and middle income countries.

Finally, few studies have compared the effectiveness of different incentive designs for smoking cessation. An exception is the study by Halpern and colleagues,<sup>6</sup> which compared two types of bonus programmes and two types of deposit programmes. The study concluded that bonus based programmes attract more smokers than deposit based programmes, but conditional on acceptance, deposit based programmes are superior to bonus based programmes. Even with the lower efficacy, bonus based programmes could achieve a greater population impact if they induce considerably higher acceptance rates than deposit based programmes (90.0% *v* 13.7% in Halpern; 64.3% in individual bonus programmes *v* 51.0% in pure deposit programme in our study). In meta-regression analyses, incentive amounts have not been consistently associated with effectiveness for health related behaviors in general or smoking cessation in particular,<sup>5 12 45</sup> although analyses have been limited by small samples. In contrast, we found that smoking abstinence increased with incentive amount. Our \$40 bonus constituted 13.5% of mean monthly household income in our sample compared with roughly 21.1% in the Halpern study<sup>6 46</sup> (our calculation) and 17.4% in the van den Brand study.<sup>8</sup> Therefore, our incentives were smaller in absolute terms (eg, \$40 *v* \$800 in Halpern) but more similar as a percentage of income, highlighting the importance of accounting for differences in living standards in making cross study comparisons. More research comparing multiple incentive designs is needed to understand which types of incentive designs most effectively promote smoking cessation.

Previous studies have found that refundable deposits have increased longer term abstinence, a finding we did not replicate. This difference might be directly

related to the relatively small initial deposit levels in our study; with only a small amount at risk, the commitment device might have simply been too weak to be effective. By comparison, Halpern and colleagues required US smokers to make a \$150 deposit, which is an order of magnitude higher relative to income.<sup>6</sup> Future studies can examine whether higher deposit levels are beneficial on balance because they might improve abstinence rates and also lead to greater financial losses for those who do not abstain.

#### Strengths and limitations of this study

The strengths of this study include the robust randomized controlled design, with randomization at the cluster level, the fully powered sample size, the comparison of multiple types of incentive programmes, the short and longer term follow-up assessments, and biochemical measurement of the primary outcome. Therefore, this study addresses many of the limitations of previous evaluations of workplace interventions focused on incentives for smoking cessation.<sup>9</sup> The large sample size of more than 4000 participants is nearly twice as large as other studies in the literature on incentives for smoking cessation. The large number of worksites suggests that our results might be closer to effects at scale than those reported from similar papers conducted in a single company such as the US. Overall, the study was powered to detect pairwise differences of 7.5 points between any randomization group versus the control group, the same as in the study by Halpern and colleagues.<sup>6</sup> However, the number of randomized people in each site was less than anticipated: 42 actual versus 60 anticipated. Moreover, the study was not powered to detect all potentially meaningful contrasts (supplementary table A1). A further limitation is that we did not adjust for multiple testing in design and analysis, potentially leading to over rejection of null hypotheses. This decision was made because the analysis focused on a subset of prespecified comparisons (supplementary table B10).

Another strength is that the intervention had high acceptance among participating employers. During a survey of employers around the 12 month assessment, all but two expressed interest in participating in future projects similar to this one. Moreover, 67% of respondents said that their company would be willing to pay for activities sponsored under a similar future project (supplementary table B13).

Furthermore, we performed several sensitivity analyses to check the robustness of our results. Although the study had a 36% loss to follow-up with primary outcome assessment at 12 months, loss to follow-up does not appear to have occurred differentially by randomization group. Our results are robust to multiple assumptions about missingness. Moreover, the dropout rate is similar to that observed in other population based smoking cessation trials at 12 months.

The conduct of the study in a novel environment—workplaces in a middle income country—is both a strength and a limitation. Globally, nearly 80% of

smokers live in low and middle income countries, and three quarters of smoking attributable deaths occur in these locations.<sup>1</sup> While workplace wellness programmes have flourished in high income countries, they remain relatively rare in many low and middle income countries. We are not aware of any previous studies that evaluate incentives for smoking cessation in a workplace setting in a low or middle income country. However, the contextual differences make it difficult to generalize our findings to other settings and to compare our results with the existing literature, which is based in high income settings. Important contextual factors might include the smoking prevalence, the availability of tobacco cessation services, the acceptability of incentives, and institutional support for quitting.

#### Conclusions

This study shows that monetary incentives for smoking cessation can increase smoking abstinence compared with usual care in a middle income country. The study also shows that different incentive designs could have different effects on abstinence. Programmes that offered refundable deposits and team incentives were less effective intervention strategies than a programme that offered an individual bonus. Our study highlights the important role that incentives can play in promoting smoking cessation, which is consistent with recent evidence that incentives for smoking cessation are at least as effective as traditional pharmacological approaches.<sup>7</sup> Areas for future research include the replication of these findings in other workplace settings, investigation of the types of intervention strategies that are most effective in combination with incentive programmes, and whether incentive programmes can be effectively and efficiently offered using digital technology.

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**Data sharing:** The full dataset and statistical codes will be made available in the Harvard Dataverse data repository by 31 August 2021.

**Transparency:** The lead author (JSW) affirms that the manuscript is an honest, accurate, and transparent account of the study being reported; that no important aspects of the study have been omitted; and that any discrepancies from the study as planned and registered have been explained.

**Dissemination to participants and related patient and public communities:** Study staff presented pooled (not group specific) results at workshops held with company representatives following the end of the study period. No further plans exist to disseminate the results of the research to study participants or the relevant participant community

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#### Web appendix: Supplementary appendix