A low-cost, low-intensity contingency management smoking cessation programme with students: Experimental evidence

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ABSTRACT

Tobacco consumption is a pressing global issue, leading to more than five million deaths each year. In South Africa, the smoking prevalence rate is stubbornly high, implying that a successful smoking cessation programme could have large social benefits, particularly if it targets young smokers. Contingency management interventions, which provide cash transfers conditional on biochemically-verified abstinence, have been effective in bringing about increased smoking cessation rates. However, contingency management programmes are typically very costly and involve frequent monitoring. This dissertation presents results of randomised controlled trial evaluating a low-cost, low-intensity contingency management smoking cessation programme conducted on a sample of treatment-seeking student smokers at the University of Cape Town in 2017. There is a statistically significant treatment effect, that is robust across multiple specifications, which increases the likelihood of abstinence by 13-20%. In addition, the programme as a whole decreased the smoking intensity of non-abstainers. This study suggests, therefore, that a low-cost, low-intensity contingency management smoking cessation programme is efficacious in promoting abstinence amongst treatment-seeking students, and that it should be added to the tobacco control toolkit in South Africa.

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1. INTRODUCTION

Tobacco consumption is a pressing and widespread issue, internationally and in South Africa. Tobacco consumption has been identified as one of the leading causes of preventable death, with direct tobacco use leading to approximately five million deaths each year (World Health Organization (WHO) 2013). In addition, more than 600 000 people die each year as a result of second-hand smoke (WHO 2013). Tobacco consumption also has environmental costs: cigarette butts are one of the most commonly discarded pieces of waste worldwide and contribute approximately 1.69 billion pounds in weight to toxic waste (Eriksen et al. 2015). In terms of the economic costs, tobacco consumption raises the burden on the government's budget by increasing health care costs and it is estimated that tobacco-related deaths lead to half a trillion dollars of economic damage each year worldwide (WHO 2013). Clearly tobacco consumption is a deep social, economic, and public health issue that demands the attention of policy makers.

A number of options exist to help control tobacco consumption. On the supply-side, governments can regulate the manufacturing, marketing, and sale of tobacco products by banning deceptive cigarette labels; prohibiting advertising, promotion, and sponsorship; and requiring large, graphic health warning labels (Tobacconomics 2018). On the other hand, to control the demand for cigarettes, governments can increase taxes on, and therefore prices of, tobacco products. In addition, they can enforce smoke-free policies, i.e., prohibit smoking in certain places, such as in or near buildings, restaurants, bars and/or other public spaces (WHO 2013). Other demand-side information interventions, such as social marketing campaigns, aim to decrease consumption by raising awareness about the health and economic consequences of tobacco use. Finally, smoking cessation interventions, of which contingency management (CM) programmes are one type, have been designed to help smokers quit using tobacco.

In South Africa, the Tobacco Products Control Act (1993) and subsequent amendments (2000, 2009) define government policy towards tobacco (Campaign for Tobacco-free kids 2017; Steyn et al. 2002). These laws govern many aspects of tobacco control, including: restrictions on public smoking (e.g., smoking is banned in partially covered outdoor areas and cars carrying children under 12); bans on tobacco advertising and sponsorships; and restrictions on the labelling and packaging of tobacco products (Campaign for Tobacco-free kids 2017). Largely attributed to the tightening controls, tobacco consumption has declined significantly. However,

the last few years have seen the smoking rate plateau (Peer et al. 2009). Prevalence rates for men and women who smoke daily are 22.2% and 9%, respectively (Eriksen et al. 2015). While the male prevalence rate is lower compared to other middle-income countries, the female prevalence rate is higher.[1] In addition, the South African National Health and Nutrition Examination Survey estimates that smoking intensity is 5.9 cigarettes per day for people aged 15-24 and 8.5 cigarettes per day for the population as a whole (Shisana et al. 2013). Consequently, there is still a high mortality rate attributed to smoking in South Africa: the percentage of deaths caused by tobacco is 7% for men and 3.8% for women, which corresponds to 366 men and 245 women being killed by tobacco every week (Eriksen et al. 2015).

Given the stubbornly high smoking prevalence and smoking intensity rates, alternative interventions may be required to help people quit. CM programmes are incentive-based interventions which provide participants with rewards contingent on a predetermined behavioural change. Several decades of research have demonstrated the effectiveness of CM programmes across a range of populations and psychoactive substance addictions, including tobacco (Cahill et al. 2015; Petry 2001; Prendergast et al. 2006). Although CM programmes are empirically-supported behavioural treatments for tobacco addiction, the cost, complexity, and staff burden of CM interventions act as a barrier to their widespread adoption (Secades-Villa et al., 2014).

Given the potential benefits of this approach, while cognisant of the issues mentioned above, this dissertation presents the results of a randomised controlled trial (RCT) investigating the efficacy of a low-cost, low intensity CM smoking cessation programme with a sample of student smokers at the University of Cape Town (UCT). I focus on two questions: Does a low-cost, low-intensity CM intervention increase smoking cessation rates and, if not, does it decrease the smoking intensity of non-abstainers?

This research is novel because, compared to other CM interventions in the literature, the programme provides relatively modest abstinence-contingent cash rewards and has a relatively low staff burden. In addition, student smokers are the ideal sample to target for a number of reasons: they are easy to recruit and track; their lifetime exposure to cigarettes and their

[1] Prevalence rates for selected countries are: Brazil – men: 16.6%, women: 11.1%; India – men: 23.2%, women: 3.2%; Botswana – men: 21.5%, women: 6.1% (Eriksen et al. 2015).

smoking intensity tend to be low compared to older smokers so they may be more susceptible to an intervention (Mayhew et al. 2000); their relatively low incomes mean that the rewards may be more salient; and, if the programme is successful at getting them to quit, the personal and societal benefits are large and continue to accrue over time.

This dissertation proceeds as follows: Section 2 discusses different models of addiction and provides a review of the theoretical and empirical CM literature. Section 3 outlines the design of the CM intervention. Section 4 explains the statistical methodology and Section 5 presents the results. Section 6 discusses the results and concludes.

2. LITERATURE REVIEW

In this section I outline the theoretical and empirical CM literature, with particular emphasis on the treatment of tobacco addiction. A natural first step is to discuss addiction from an economic standpoint. This provides a rationale for the use of CM to treat tobacco addiction and a segue into the discussion of the relevant literature.

2.1 Understanding and modelling addiction

The disease model of addiction is a longstanding interpretation of addiction (Heyman 2009). The basic argument is that addiction is a disease: substance use produces a disease state typified by compulsive drug taking. On this account, addiction is understood to have a biological basis such that use of psychoactive substances can convert a voluntary drug user into an involuntary one. Adherents of the disease model argue that it provides a humane interpretation of addiction and that it leads to better treatment for addicts. The disease model dates back to the 17th century and remains a common view among the medical fraternity, the public, and the media (Heyman 2009, p. 17; Warner 1994).

However, many researchers have challenged the notion that addiction is a disease. Heyman (2009) argues that addiction should rather be understood as a disorder of choice, involving voluntary behaviour.[2] His argument is that addictive drug use involves voluntary behaviour

[2] Heyman (2009, p. 104) defines voluntary behaviour as "… the degree to which an activity is voluntary is the degree to which it systematically varies as a function of its consequences, and the degree to which it is feasible to apply such consequences."

since it varies systematically as a function of its costs and benefits: when the costs of addictive consumption increase or the benefits decline, addicts reduce or stop consuming their target of addiction. Heyman cites particularly convincing evidence from the four largest epidemiological studies of addiction conducted in the United States: the Epidemiologic Catchment Area study (Anthony and Helzer 1991); the National Comorbidity Survey (Warner et al. 1995); the National Comorbidity Survey Replication (Kessler, Berglund, et al. 2005; Kessler, Chiu, et al. 2005); and the National Epidemiologic Survey on Alcohol and Related Conditions (Stinson et al. 2005). Heyman finds that the majority of addicts quit using their targets of addiction by their early thirties and do so without any kind of formal treatment. This suggests that as addicts move into adulthood where personal and professional demands are typically greater than at younger ages, many addicts successfully manage to quit their targets of addiction (Hofmeyr 2015). This implies that addictive consumption is a voluntary behaviour that responds to changes in consequences.

The view that addiction is a disorder of choice suggests that CM interventions may be an effective form of treatment. CM interventions change the consequences associated with consuming the target of addiction by providing abstinence-contingent incentives that increase the benefits of abstinence and increase the opportunity cost of consuming the target of addiction. If addiction is a voluntary behaviour that varies as a function of its consequences then CM interventions could prove to be an effective form of treatment.

This dissertation adopts the view that addiction is a disorder of choice involving voluntary behaviour. While this interpretation of addiction appears to stand in direct opposition to the view that addiction is an involuntary disease, Ross et al. (2008) argue that the two contradictory models of addiction can be reconciled. They argue that addiction can be understood at two levels: the molecular and molar (i.e., behavioural) levels. According to Ross et al. (2008), the disease model of addiction provides a molecular account of addiction while Heyman's choice-based interpretation provides a molar account of addiction.[3] Thus, that both the disease and choice-based models of addiction contribute to our understanding of addiction and should be judged at their respective levels of analysis.

[3] The reconciliation of the disease and choice-based models of addiction is not the focus of this dissertation. See Ross et al. (2008) for further details.

Given the assumption that addiction is a disorder of choice, I will now examine how economics has modelled addiction. I will discuss economic models of addiction that highlight the molar scale factors that can result in substance dependence and which provide a theoretical justification for the use of CM to treat addiction. While these choice-based accounts provide a partial understanding of addiction they are not, as noted above, at odds with a molecular level interpretation of addiction.

Becker and Murphy (BM, 1988) developed a theory of rational addiction which assumes that agents take into account all past, present, and future implications of their consumption choices and implement a consistent plan to maximise their welfare through time (Bernheim and Rangel 2007). Specifically, agents in the model solve an intertemporal optimisation problem: in each time period the agent chooses an optimal mix of addictive and non-addictive goods. In the case where the agent chooses to consume the addictive good, his/her stock of addictive capital increases. As the stock of addictive capital increases, the short-term welfare of the agent increases and this makes addictive consumption more attractive in the future while lowering long-term welfare.

The BM model employs an additively-separable intertemporal utility function with an exponential discount factor and thus agents in the model make time-consistent choices.[4] This means that agents either choose to consume the addictive good or not: they either consume the addictive good now and enjoy the short-term benefit of addictive consumption while suffering the long-term costs, or they abstain now and enjoy the long-term benefits that arise due to a decrease in the stock of addictive capital. In the case where agents decide to consume the addictive good, they will do so until they have run out of money; they will not deviate from this plan unless there is some exogenous shock such as a change in price (Hofmeyr 2015).

Addicts are commonly observed to regret their choices and tend to expend resources to prevent or limit their consumption of addictive goods. In addition, the typical course of addiction involves multiple unsuccessful quit attempts, before final abstention (Ross 2010). This suggests that addicts make time-inconsistent choices with regard to addictive consumption, which conflicts with the BM model's assumption of time consistency. The BM model's

[4] Loosely, time consistency means that an agent's ranking of different consumption bundles does not change through time. In other words, if the agent prefers bundle A to bundle B at time point *t*, she will also prefer A to B at all other time points (Hofmeyr 2015).

inability to account for the observed time-inconsistent choice behaviour of addicts implies that the model is too restrictive.

Numerous economic models of addiction have subsequently been developed which improve our understanding of addiction by accounting for time-inconsistent choice behaviour. Broadly, these are dual system and dual self models. These models are either diachronic (Bénabou and Tirole 2004; Gruber and Köszegi 2001), synchronic (Benhabib and Bisin 2004; Bernheim and Rangel 2004), or both (Fudenberg and Levine 2006, 2011, 2012). Dual system models are typically synchronic: multiple systems or processes compete for control of behaviour at the same time. In contrast, dual self models are usually diachronic: each self has full control of the agent's cognitive and other capacities at a single point in time but there is a succession of selves over time. These models adopt different mechanisms to account for time-inconsistent choice behaviour[5] and, in doing so, incorporate the behavioural patterns of addictive consumption mentioned previously.

The economic models of addiction I have discussed have clear implications for policy. The BM rational choice model implies that there is no room for government intervention because any restriction on consumption of the addictive good, except those reducing negative externalities associated with consumption of the addictive good, would be welfare reducing (Becker and Murphy 1988, p. 691). In contrast, the dual self and dual system models, which better account for the observed behaviour of substance-dependent people, suggest that addicts may benefit from interventions to curb their substance use (Hofmeyr 2015). They suggest that an addict who exhibits time-inconsistent choice behaviour (i.e., in the present decides to consume the target drug because the net benefit is positive, but also wants to quit using the addictive substance) may benefit from commitment devices (e.g., commitment contracts for abstinence, checking into a rehabilitation centre) and other interventions that adjust their cost-benefit calculus with regard to addictive consumption. CM is one example of a policy intervention that alters the intertemporal cost-benefit trade-off that the addict faces and which has proven particularly effective in treating addictive behaviour. The next section discusses the theoretical and empirical CM literature.

[5] See Hofmeyr (2015) and Ross (2011, 2014a, 2014b) for detailed reviews.

2.2 Treating addiction: Contingency management

CM is a method which uses reinforcement, and less commonly punishment, to promote behavioural change (Higgins 2010). CM involves identifying an objectively defined target behaviour, frequently monitoring that behaviour, and delivering tangible incentives for reaching the target behaviour (Alessi and Petry 2014). CM interventions are based on the behaviour analysis theoretical framework[6] and are most commonly applied to the treatment of substance-use disorders.[7] According to this framework, drug use and addiction are maintained through the reinforcing biochemical effects of the addictive substance and their relationship with the environment, e.g., a feeling of euphoria, social inclusion, etc. (Ledgerwood and Petry 2010; Prendergast et al. 2006). CM programmes can therefore be used to treat substance use disorders because they offer incentives for behaviours incompatible with substance use and thus compete with the reinforcing attributes of drug use. The behaviour analysis framework that underpins CM interventions fits squarely within Heyman's (2009) choice-based interpretation of addiction: both theories view addiction as a behaviour that can be modified by changing the consequences of addictive consumption.

While CM has predominantly been used to treat substance use disorders, the approach dates back to token economy procedures developed in the 1960s and 1970s. Specifically, tokens, exchangeable for other items, were used to reinforce targeted behaviours in the treatment of people with chronic mental illness (Higgins et al. 2017; Petry 2001). In the early 1990s, the use

[6] Behaviour analysis is the study of the behaviour-environment relationships that influence learning (Meredith et al. 2014). Specifically, it is concerned with principles of operant conditioning (i.e., learning processes through which behaviour is controlled or shaped by its consequences) and the application of these principles to modify behaviour (Meredith et al. 2014; Prendergast et al. 2006).

[7] There has been a proliferation of incentive-based programmes targeting health-related behaviours such as weight loss, exercise, medication adherence, and abstinence from addictive substances (Cahill et al. 2015; Meredith et al. 2014). CM and conditional cash transfers (CCTs) are two types of incentive-based interventions that have been used to target health-related behaviours. While CM has largely been used to treat substance use disorders, CCTs are prominent in development economics and have been used to target behaviour in a wide range of areas, such as: education; employment; health and nutrition; empowerment; and savings, investment, and production (Bastagli et al. 2016). Higgins (2010) argues that CM and CCT represent a common approach (i.e., using contingent reinforcement to promote behaviour change), but are treated as distinct areas of inquiry because they evolved out of different intellectual traditions: CM has its foundations in behavioural psychology and CCT in microeconomics. He argues that there are bi-directional benefits to integrating the two literatures. According to Higgins (2010), CCT could benefit from CM's extensive behavioural psychology knowledge base and extensive research into the effect of moderators of incentive-based interventions (e.g., the magnitude of reinforcement, temporal delays between behaviour change and delivery of the reinforcement, etc.). He argues that CM, which has been criticised for its lack of widespread dissemination, could benefit from the large and emerging body of practical experience that has come from the dissemination of CCT programmes in everyday community practices and public policy.

of contingent reinforcement as a treatment for substance use disorders gained attention with the work of Higgins and colleagues (see Higgins et al. 2017 for a review). They used CM as part of multi-element interventions aimed at treating cocaine dependence in outpatients. In combination with intensive counselling, these interventions provided cocaine addicts with vouchers, exchangeable for retail items, contingent on objective evidence of recent abstinence from cocaine use. These interventions proved effective in a context where most other treatments had failed to bring about abstinence. Following Higgins and colleagues' work, research into the efficacy of using CM procedures to treat addictive disorders bourgeoned. Several decades of research have provided evidence in support of various CM techniques for treating a range of psychoactive substance addictions (Cahill et al. 2015; Petry 2001; Prendergast et al. 2006). Debate has thus shifted from whether CM interventions are efficacious, to the relative success or limitations of the different design features that may account for the variation in effect sizes across interventions. The next section outlines the design features typical of CM interventions.

2.2.1 The design features of CM interventions

CM interventions vary considerably across a number of dimensions: who is targeted, what behaviour is targeted, and the incentives that are provided. I discuss variation along these dimensions in what follows. In terms of the targeted population, CM interventions have been conducted with community members, worksite employees, and people in a number of other institutions (e.g., drug rehabilitation centres, clinics, hospitals, etc.).[8] Some CM interventions target the behaviour of more than one individual (Meredith et al. 2014). For example, Halpern et al. (2015) group individuals into teams of six and provide contingent-rewards to the individual for each group member who achieved abstinence from smoking. Such interventions draw on social influences to bring about the desired behavioural change (i.e., abstinence).[9]

In terms of the targeted behaviour, CM interventions treating substance use disorders have targeted abstinence from a range of different drugs, including alcohol, illicit drugs, and

[8] Within these institutions, CM programmes often target special populations such as pregnant women and patients diagnosed with pulmonary disease (Cahill et al. 2015).

[9] Behaviour analytic research and theory suggests that targeting a single individual may be more effective: when incentives are contingent on an individual's behaviour, the correspondence between the targeted behaviour and the contingent-incentive is more direct and therefore increases the influence that the incentives have over behaviour (Meredith et al. 2014).

tobacco, where abstinence is generally biochemically verified with a relevant test (Meredith et al. 2014).[10] Some CM interventions target polydrug abuse rather than a single drug: the programmes require abstinence from all drugs and pay out the contingent-incentive if the subject provides biological samples that are negative for all targeted drugs.[11] Other programmes target reduced drug use rather than complete cessation. For example, some programmes have used qualitative urinalysis testing to promote reductions in benzoylecgonine metabolites to encourage initial attempts at cocaine abstinence (Petry 2001). Such programmes aim to achieve behavioural change amongst "hard-to-treat" patients by offering incentives based on goals that are within the reach of each individual (Meredith et al. 2014).[12] In addition to targeting addictive substances themselves, some programmes also target non-drug behaviours as part of the patient's overall treatment (Petry 2001; Petry and Ledgerwood 2010). For example, some programmes reinforce attendance at therapy sessions, submission of biological samples, and other goal-related behaviours[13] (e.g., taking steps to find steady employment or dealing with legal issues).

In terms of the incentive provided, CM programmes vary significantly, and it is useful to distinguish between the characteristics of the incentive itself and the way in which the incentive is delivered. The characteristics of the incentive vary in three ways. First, the *direction* of the incentive: treatment providers can either give the subject a reward for achieving abstinence or impose a penalty if they fail to do so. At the midpoint between reward-based and penalty-based

[10] Biochemically verifying self-reported claims of abstinence is considered good programme design (Benowitz et al. 2002). However, CM programmes do not necessarily biochemically verify all patients' self-reports: some programmes only verify self-reports of those individuals claiming abstinence (i.e., those individuals who may be eligible for the abstinence-contingent reward), while others verify self-reports among a random sample of those claiming abstinence (Cahill et al. 2015).

[11] Behaviour analytic theory suggests that CM programmes targeting multiple drugs may require incentives of significant magnitudes to be effective (Meredith et al. 2014). Specifically, remaining abstinent from multiple drugs simultaneously requires a considerable response effort (i.e., the difficulty associated with engaging in the target behaviour) from the subject and abstinence-contingent incentives therefore need to compete with the reinforcing effects of multiple drugs. Research suggests that targeting one drug at a time may be a more effective way to target polydrug abuse: subjects who achieve abstinence from a single drug may then be motivated to abstain from the primary as well as secondary drug(s) (Petry 2001). Most CM studies that target abstinence from a single drug find reductions in other drug use as well (Petry 2001).

[12]. These programmes, as well as those that target one drug at a time rather than multiple drugs simultaneously (see Footnote 9), reinforce successive approximations: they reinforce behavioural responses that bring the subject closer to the desired result (i.e., each small step along the way is reinforced to establish a pattern of drug abstinence) (Petry 2001).

[13] Targeting and reinforcing non-drug behaviours, in addition to targeting the addictive substance itself, typically occurs in CM programmes treating patients addicted to "hard-core" drugs (e.g., cocaine, heroin) that are often associated with drug-related issues.

interventions are deposit-refund interventions: subjects deposit their own money[14] into an account and the money is either recovered or forfeited depending on whether the subject remained abstinent or not. Second, the *type* of incentive provided varies. A number of different financial incentives have been provided in CM programmes, including: cash payments, vouchers exchangeable for goods[15], salary bonuses, and other items (e.g., take-home doses of methadone) (Cahill et al. 2015; Prendergast et al. 2006). While this research focuses on financial or material incentives, it is worth noting that numerous other non-monetary incentives (e.g., clinic privileges, more flexible work schedules) have been used in CM interventions (Meredith et al. 2014; Petry 2001; Petry and Ledgerwood 2010). While the use of non-monetary rewards reduces programme costs, it may not bring about the desired behavioural change: the subject may not have a preference for the non-monetary reward and, even if they do, the reward may lose its effectiveness as the subject becomes satiated by its repeated delivery (Meredith et al. 2014). Finally, the *magnitude* of the incentives (or total value of abstinence-contingent rewards available to the subject over the duration of the intervention) varies considerably across CM interventions, ranging from $100 (Volpp et al. 2006) to $800 (Halpern et al. 2015)

With regard to incentive delivery, there are three ways in which it can vary. First, incentive delivery can differ in terms of the *immediacy of the reward* in relation to the occurrence of the target behaviour (i.e., abstinence). A meta-analysis of CM interventions finds that incentive immediacy is associated with larger effect sizes (Prendergast et al. 2006).[16] Second, the *frequency* and *duration* of incentive delivery varies: some programmes run for four weeks and incentives are provided daily, while others last several months and incentives are provided weekly. Behaviour analytic theory suggests that CM programmes targeting substance use may require frequent and prolonged reinforcement to establish and maintain behaviour change because drug abstinence is considered a "complex" behaviour which involves repeated choices over an extended period of time (Kane et al. 2004; Meredith et al. 2014). This contrasts to "simple" health-related behaviours (e.g., arriving for an HIV test or arriving for a once-off

[14] Some CM programmes conducted in clinic settings offer different refunds or rebates (Petry 2001). Some clinic-based programmes require subjects to pay a fee upon entry into the treatment programme, which is refunded if the subject completes the programme and maintains abstinence. Others reduce fees for service or provide a rebate of the treatment costs when abstinence is achieved and maintained.

[15] As mentioned, voucher-based reinforcement therapy (VBRT) - as the procedure is often called - was popularised by Higgins and colleagues and remains a commonly used CM incentive.

[16] To reduce the delay between behaviour change and incentive delivery, CM interventions typically use on-site test kits which provide results immediately (Petry 2001). The decision regarding which test method to use does, however, also depend on the half-life of the drug and the cost of the test-kit (Benowitz et al. 2002; Petry 2001).

immunisation appointment) that are commonly targeted in more general incentive-based programmes and may require less frequent and less prolonged reinforcement.

Third, the *schedule* according to which incentives are delivered differs markedly. The schedule of reinforcement can be either fixed/uniform (i.e., the incentive is the same for each biological sample testing negative for the target drug) or variable. There are a number of variable reinforcement schedules that have been used, including an escalating schedule of reinforcement with a reset contingency, front loading of incentives, and intermittent reinforcement. In programmes adopting an escalating reinforcement schedule with a reset contingency, the target behaviour (i.e., abstinence) is initially reinforced with small monetary incentives (Meredith et al. 2014). If the subject continues to remain abstinent, the incentive magnitude increases with each biological sample that tests negative for the target drug. If the subject lapses, the next time they test negative for the target drug they are reinforced with an incentive that has been reset to the initial, smaller amount. This reinforcement schedule interacts dynamically with the behaviour of the subject and aims to promote continuous abstinence (Meredith et al. 2014; Prendergast et al. 2006).

Front-loaded incentive schedules are the opposite of an escalating reinforcement schedule; they provide larger incentives initially which then reduce gradually over time. This incentive schedule aims to establish the target behaviour initially and maintain the treatment gains by providing smaller incentives over time (Meredith et al. 2014). Finally, the intermittent reinforcement schedule, or "fishbowl" method as it is colloquially termed, was developed by Petry (2001) to curtail the costs of CM interventions. Compared to programmes that offer rewards with certainty, programmes adopting an intermittent reinforcement schedule provide subjects with incentives for a proportion of behaviours. Specifically, each time the subject achieves his/her target behaviour (e.g., submitting a negative biochemical sample) he/she earns the chance to draw a slip of paper from a bowl (hence the name). A certain proportion of the draws say "Good Job!" while the remaining draws result in winning a prize which typically ranges in value from $1 to $100, and the chances of winning are inversely related to the prize value (Benowitz et al. 2002; Petry 2001; Prendergast et al. 2006).

2.2.2 CM interventions targeting smoking abstinence

I now turn to a discussion of the CM literature that draws on Cahill et al. (2015) and which is relevant to this dissertation. Specifically, I will review 21 published studies evaluating CM interventions offering smokers material or financial incentives for abstinence from tobacco use, and discuss them in relation to the features of CM interventions outlined above. All included studies are RCTs[17] that report cessation rates and follow study participants for at least six months[18] from the start of the intervention. The 21 studies cover 8400 individuals, with study sample sizes ranging from 45 (Alessi and Petry 2014) to 2538 (Halpern et al. 2015).

With regard to the setting and targeted population, 15 of the studies were conducted in the United States, and the remaining studies were conducted in the United Kingdom (Paxton 1980, 1981, 1983), Spain (Secades-Villa et al. 2014), the Philippines (Giné et al. 2010), and Thailand (White et al. 2013). Ten of the studies were conducted with individuals in clinics or health centres, seven in worksites (Glasgow et al. 1993; Halpern et al. 2015; Hennrikus et al. 2002; Jason et al. 1995; Rand et al. 1989; Volpp et al. 2009; Windsor et al. 1988), two in academic institutions (Ledgerwood et al. 2014; Tevyaw et al. 2009), one in an urban community (Giné et al. 2010), and one in villages served by community health workers (White et al. 2013).

With regard to the targeted behaviour, all studies targeted abstinence from smoking. Abstinence has been defined in multiple ways. Hughes et al. (2003) recommends that trials should report prolonged abstinence (sustained abstinence after an initial grace period) and point prevalence abstinence (PPA) (prevalence of abstinence during a time window immediately preceding a meeting, commonly 7 days). All 21 included studies report either or both of these measures. In addition to targeting abstinence, two studies targeted smoking reductions initially

[17] While several of the studies include multiple treatment arms, this review focuses on the comparison of the financial/material incentive treatment and the appropriate control condition. In most cases the appropriate control/comparison group typically receives some form of minimal intervention because CM is predominantly conducted on a 'platform' of another treatment/support programme rather than as a stand-alone treatment (Prendergast et al. 2006). For example, some programmes provide information (e.g., provision of self-help/quit guides, information about local smoking cessation services), some provide counselling and support (e.g., cessation skill training, help with creating a quit plan), and some provide medication (e.g., varenicline, bupropion, and nicotine replacement therapies such as patches and gum) (Meredith et al. 2014; Petry 2001; Petry and Ledgerwood 2010).

[18] The purpose of the follow-up period is to establish the rate of remission across the treatment and control groups and to establish whether a treatment effect remains after the contingency period is over. Follow-up sessions should be tied to subjects' quit dates (Hughes et al. 2003).

and subsequently targeted abstinence[19], and several studies also included incentives for participation/attendance and compliance, e.g., submitting a biological sample (Alessi and Petry 2014; Crowley et al. 1995; Gallagher et al. 2007; Giné et al. 2010; Halpern et al. 2015; Hennrikus et al. 2002; Ledgerwood et al. 2014; Rand et al. 1989; Tevyaw et al. 2009; Volpp et al. 2006; Volpp et al. 2009).

There is significant variation across studies both in terms of the characteristics of incentives provided and the delivery of the incentives. Regarding the incentive direction, six studies adopted deposit contracts[20] (Giné et al. 2010; Halpern et al. 2015; Paxton 1980, 1981, 1983; White et al. 2013)[21] and all remaining studies offered positive incentives. Regarding the type of incentive provided, nine studies provided cash incentives[22] (Drummond et al. 2014; Gallagher et al. 2007; Halpern et al. 2015; Jason et al. 1995; Rand et al. 1989; Tevyaw et al. 2009; Volpp et al. 2006; Volpp et al. 2009; Windsor et al. 1988), two studies provided prize draws (Alessi and Petry 2014; Ledgerwood et al. 2014), two studies provided both cash and a prize or cash draw (Glasgow et al. 1993; Hennrikus et al. 2002), two studies provided vouchers exchangeable for goods (Secades-Villa et al. 2014; Shoptaw et al. 2002), and one study provided lottery tickets (Crowley et al. 1995). Regarding incentive magnitude, total available earnings varied from $50 (Drummond et al. 2014) to $800 (Halpern et al. 2015).[23]

Regarding the immediacy of incentive delivery, most studies offered incentives immediately, although Hennrikus et al. (2002) provides a cash draw every 6 months (i.e., not immediately) in addition to immediate cash incentives. The duration of the intervention period (i.e., the period over which incentives were delivered) ranged from several weeks (Alessi and Petry

[19] Tevyaw et al. (2009) offered incentives for reductions in smoking intensity in the first week of the programme ($1 for a 25-49% reduction in the CO reading, $2 for a 50-74% reduction, and $3 for a reduction of 75% or more) and incentivised complete abstinence from the second week onwards. Ledgerwood et al. (2014) offered incentives for CO readings that had reduced by at least 3 ppm in the first week and thereafter incentivised complete abstinence.

[20] Halpern et al. (2015) includes two deposit-refund treatments. The first treatment provided subjects with their own deposited money as well as additional cash payments for abstinence. In the second treatment, subjects were placed into groups of six and received their own deposited money for abstinence, as well as additional cash payments per successful quitter in their group.

[21] While participants in Giné et al. (2010) and Paxton (1980, 1981, 1983) received their deposited cash conditional on abstinence, participants in Halpern et al. (2015) and White et al. (2013) received their deposited cash as well as bonus cash payments (i.e., they received deposit repayments as well as positive incentives).

[22] In addition to the two deposit-refund treatments, Halpern et al. (2015) includes two cash-based treatments. As with the deposit-refund treatments, the first treatment is individual-based and the second is group-based: the first provides cash rewards contingent on abstinence, while the second groups participants into teams of six and provides subjects with cash rewards contingent on their own abstinence plus additional cash rewards for each team member who is abstinent.

[23] These figures are for studies providing positive incentives and do not include deposit-refund studies.

2014) to a full year (Glasgow et al. 1993), and the frequency of incentive delivery varied significantly with some studies delivering incentives daily (Alessi and Petry 2014; Crowley et al. 1995; Jason et al. 1995; Ledgerwood et al. 2014; Tevyaw et al. 2009) and others providing incentives only at the end of the intervention period (Giné et al. 2010). Finally, studies employed numerous incentive schedules, including: a uniform incentive schedule (Crowley et al. 1995; Glasgow et al. 1993; Hennrikus et al. 2002; Jason et al. 1995; Rand et al. 1989), an escalating incentive schedule (Gallagher et al. 2007), an escalating incentive schedule with a reset contingency (Drummond et al. 2014; Secades-Villa et al. 2014; Shoptaw et al. 2002; Tevyaw et al. 2009), a frontloaded/tapering incentive schedule (Halpern et al. 2015), and an intermittent reinforcement schedule (Alessi and Petry 2014; Ledgerwood et al. 2014). With regard to the incentive schedules for deposit contract studies, three studies (Paxton 1980, 1981, 1983) varied the repayment schedules (the deposits were either returned uniformly or the repayments thinned out) as opposed to returning deposits at a single time point, which all other studies did.

Before discussing the results of the 21 studies, I will examine the potential sources of bias that may limit one's ability to draw valid and robust causal inferences from these studies. The Cochrane Library has developed a taxonomy for sources of bias that may arise from design and implementation issues and I will use it as a framework for discussing the 21 RCTs under review (The Cochrane Collaboration 2008). First, selection bias refers to systematic differences between baseline characteristics of the groups in a study.[24] Randomisation procedures can be used to prevent selection bias in allocating treatment to study participants. Achieving successful randomisation requires that there is adequate sequence generation (i.e., there is a specified rule for allocating treatment to participants which is based on some random process) and that the upcoming treatment allocation is concealed from potential participants during enrolment into the RCT. Cahill et al. (2015) concludes that seven studies conducted adequate randomisation procedures: sequence generation and allocation concealment (Drummond et al. 2014; Halpern et al. 2015; Ledgerwood et al. 2014; Volpp et al. 2006; Volpp et al. 2009; White et al. 2013; Windsor et al. 1988); four studies followed adequate sequence generation procedures but did not achieve adequate allocation concealment (Alessi and Petry 2014; Gallagher et al. 2007; Secades-Villa et al. 2014; Shoptaw et al. 2002); five studies gave

[24] Selection bias in economics typically refers to the unrepresentativeness of a sample relative to the population it is supposed to represent. This bias can have a number of causes, including self-selection. The Cochrane Library's definition is, therefore, restrictive (see Wooldridge 2010, p. 790-792).

insufficient detail for the integrity of the randomisation procedure to be assessed (Glasgow et al. 1993; Hennrikus et al. 2002; Jason et al. 1995; Rand et al. 1989; Tevyaw et al. 2009); and the remaining studies used inadequate randomisation procedures.

Second, performance bias refers to systematic differences between groups' outcomes that arise from knowledge of treatment allocation, rather than the treatment itself. After enrolment into the study, blinding of participants and personnel to the treatment allocation may reduce the risk of performance bias. Blinding personnel may help ensure that compared groups receive equal attention, ancillary treatment, and diagnostic investigations. Blinding of participants may reduce the risk of a Hawthorne, John Henry, or resentment and demoralisation effect.[25] However, because of the explicit reward mechanism in CM interventions, blinding of personnel is very difficult and complete blinding of subjects is not possible. In particular, while medical trials testing new drugs can blind personnel and subjects to treatment allocation relatively easily (i.e., it is impossible to distinguish between the drugs and the placebo that the treatment and control subjects receive, respectively), the personnel and treatment subjects in a CM programme know whether the abstinence-contingent incentive is being delivered. Only four studies reported any attempt to blind subjects or assessors (Crowley et al. 1995; Jason et al. 1995; Tevyaw et al. 2009; Volpp et al. 2006).

Third, attrition bias refers to systematic differences in withdrawals from a study across groups. Cahill et al. (2015) deem eleven studies to be at low risk of attrition bias and nine studies to be at unclear risk. Rand et al. (1989) is rated at high risk, with significantly higher attrition in the treatment group, although reasons for drop-out were largely unrelated to the cessation intervention (e.g. heart attack, pregnancy, relocation for work). In terms of data analysis, fifteen studies treated drop-outs and losses to follow-up as continuing smokers, and conducted the analyses on an intention-to-treat basis (Alessi and Petry 2014; Crowley et al. 1995; Drummond et al. 2014; Gallagher et al. 2007; Giné et al. 2010; Glasgow et al. 1993; Halpern et al. 2015; Jason et al. 1995; Ledgerwood et al. 2014; Rand et al. 1989; Secades-Villa et al. 2014; Volpp et al. 2006; Volpp et al. 2009; White et al. 2013; Windsor et al. 1988).

[25] A Hawthorne effect is when the treatment group works harder than normal. A John Henry effect is when the comparison group starts competing with the treatment group. A resentment and demoralisation effect is when the members of the comparison group resent missing out on treatment which may lead them to behave in ways that worsen their outcomes (Glennerster and Takavarasha 2013).

There is one further issue, specific to CM studies, which could lead to bias and limit the reliability of results. In CM studies, there is a possibility of systematic differences in reports of abstinence amongst treatment and control subjects: treatment subjects may be tempted to misreport abstinence because of the contingent nature of the rewards. Biochemically verifying abstinence is important for mitigating such deception.[26] All included studies used some form of biochemical validation of abstinence, either by testing breath samples for carbon monoxide (CO) levels (Crowley et al. 1995; Rand et al. 1989), or by testing cotinine (a metabolite of nicotine) levels in blood, saliva, or urine (Giné et al. 2010; Halpern et al. 2015; Hennrikus et al. 2002; Paxton 1980, 1981, 1983; Volpp et al. 2006; Volpp et al. 2009; White et al. 2013; Windsor et al. 1988), or both (Alessi and Petry 2014; Drummond et al. 2014; Gallagher et al. 2007; Glasgow et al. 1993; Jason et al. 1995; Ledgerwood et al. 2014; Secades-Villa et al. 2014; Shoptaw et al. 2002; Tevyaw et al. 2009). Studies either tested individuals to validate reports of abstinence, tested a random sample of quitters, or tested all participants irrespective of their reported abstinence/failed abstinence. Of the 9 studies that reported disparities between self-reported abstinence and biochemically verified abstinence, five studies reported a good correspondence (Jason et al. 1995; Paxton 1980, 1981, 1983; Windsor et al. 1988) and four studies reported significant disparities (Crowley et al. 1995; Gallagher et al. 2007; Glasgow et al. 1993; Hennrikus et al. 2002).[27]

These issues notwithstanding, the 21 studies included in this review point to some noteworthy findings. Cahill et al. (2015), who reanalyse the 21 studies' data collectively, report that the cessation rates at the *six-month* follow-up demonstrate an unequivocal benefit for the incentive groups, with an adjusted odds ratio (OR) of 1.72 (95% confidence interval (CI) 1.43 to 2.08;

[26] A related issue is that potential participants may be tempted to misreport their smoking status to gain entry into the CM programme. Biochemical verification at baseline ensures that non-smokers do not gain entry into the programme. Nine studies took measures to verify smoking status at baseline (Crowley et al. 1995; Gallagher et al. 2007; Halpern et al. 2015; Ledgerwood et al. 2014; Rand et al. 1989; Secades-Villa et al. 2014; Shoptaw et al. 2002; Tevyaw et al. 2009; Windsor et al. 1988). Cahill et al. (2015) argue that in those studies requiring subjects to submit baseline samples, 6% returned negative samples and 14% did not return a sample, suggesting that up to 20% of participants trying to enrol may have been non-smokers. This points to the importance of biochemically verifying subjects' smoking status before entry into a CM intervention.

[27] The Cochrane Library (2008) identifies two further sources of bias: detection bias and reporting bias. Detection bias refers to systematic differences between groups in how outcomes are determined. Blinding personnel who are assessing outcomes may reduce the risk that knowledge of treatment allocation, rather than treatment itself, affects outcome measurement. Blinding outcome assessors is particularly important in studies measuring subjective outcomes (e.g., the degree of postoperative pain). Outcomes in CM interventions are typically objective (e.g.., biochemically verified abstinence) and so blinding of personnel – which, as mentioned, is difficult in CM studies – is not as critical. Reporting bias refers to systematic differences between reported and unreported findings. Within published work this is the tendency to report statistically significant rather than non-significant results. This issue is germane to all published work.

6945 participants). The cessation rate at *longest* follow-up for each study is slightly lower, with an adjusted OR of 1.42 (95% CI 1.19 to 1.69; 7715 participants). However, only three trials find a significant effect of incentives on smoking abstinence at their longest follow-up, and two of these studies provided the most substantial cash payments (Halpern et al. 2015; Volpp et al. 2009; White et al. 2013).[28] The overall picture that emerges from the remaining studies is that incentives boost cessation rates while they are in place, but cessation rates often decline in the long term unless the incentives are substantial during the intervention.[29]

Despite the evidence suggesting that CM smoking cessation interventions are efficacious, the cost, complexity, and staff burden of CM interventions act as a barrier to their widespread adoption (Secades-Villa et al., 2014). As a response to this issue, this dissertation sets out to test whether a low-cost, low-intensity CM intervention can prove to be an effective form of treatment for tobacco addiction in a student population. The programme provides relatively modest abstinence-contingent cash rewards and has a relatively low staff burden. In addition, student smokers are the ideal sample to target for a number of reasons: they are easy to recruit and track; their lifetime exposure to cigarettes and their smoking intensity tend to be low compared to older smokers so they may be more susceptible to an intervention (Mayhew et al. 2000); their relatively low incomes mean that the rewards may be more salient; and, if the programme is successful at getting them to quit, the personal and societal benefits are large and continue to accrue over time.

In this dissertation, I focus on two questions: Does a low-cost, low-intensity CM intervention increase smoking cessation rates and, if not, does it decrease the smoking intensity of non-abstainers? I outline the details of the study design in Section 3 below.

[28] Halpern et al. (2015) provides $800 for sustained abstinence at six months and Volpp et al. (2009) provide $750. The third study, White et al. (2013), is a deposit contract intervention.

[29] Two additional findings are worth noting. First, Cahill et al. (2015) compare the impact of guaranteed rewards (i.e., vouchers or cash) versus uncertain rewards (i.e., lotteries or prize draws) and find that the level of certainty of receiving a reward has little impact on the success of the intervention. Second, Cahill et al. (2015) compare deposit-refund and reward-based programmes. They find that deposit-refund trials may be prone to relatively low uptake rates compared to reward-based programmes, but that those who do sign up for deposit-refund trials achieve higher rates compared to reward-based studies.

3. DESIGN AND METHODS

The CM smoking cessation programme consisted of a recruitment period, a 6-week intervention period, and a 6-month follow-up period, and was designed to fit within the university academic calendar. Figure 1 provides an overview of the study. The recruitment period included a screening session; the intervention period included a baseline session (after which the subjects had one week to quit smoking; their quit window) and four programme sessions; and the follow-up period included two sessions, 3 months and 6 months after the subject's quit date.[30] In terms of the study's treatment arms, subjects in the control group were given an aid-to-quit document in the baseline session and had their quit attempt monitored in the programme and follow-up sessions. Subjects in the treatment group were, additionally, given abstinence-contingent incentives in the programme sessions.[31] I outline the details of the study design and methodology below.

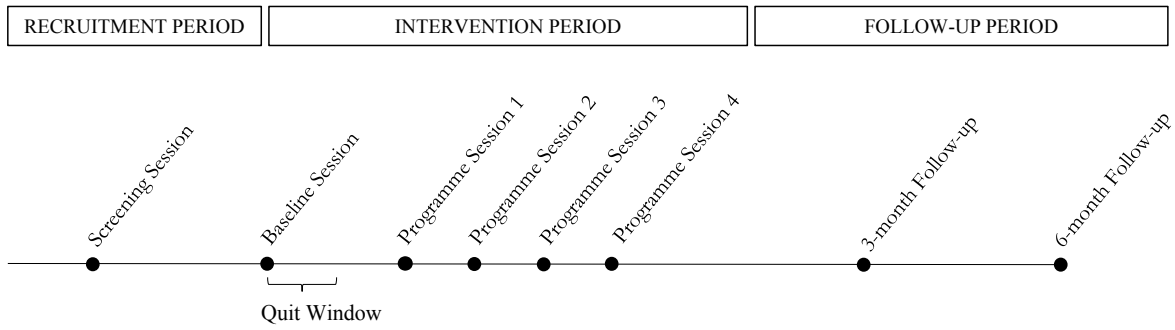

Figure 1: *Study Overview*

3.1 Participants and setting

The study was conducted at UCT. Participants ($N = 87$) were treatment-seeking student smokers. Inclusion criteria were: the student was at least 18 years old; had smoked at least 100 cigarettes in his/her life; had smoked in the last 10 hours; smoked at least 5 cigarettes a day; reported an interest in quitting smoking and taking part in a smoking cessation programme;

[30] The quit date was the last day that the subject could smoke.

[31] As mentioned, CM is predominantly conducted on a 'platform' of another treatment/cessation support programme rather than as a stand-alone treatment (Prendergast et al. 2006). In our study, control subjects received an aid-to-quit document and were monitored during the intervention and follow-up phases of the study. In addition to the "care" that control subjects received, treatment subjects received abstinence-contingent rewards. Any treatment effect would therefore suggest that the abstinence-contingent rewards given to treatment subjects is effective over and above the "care" (information and monitoring) that control subjects receive.

and had a carbon monoxide (CO) in expired air reading of at least 8 parts per million (ppm).[32] Figure 2 shows the flow of participants through the recruitment, intervention, and follow-up phases of the study.

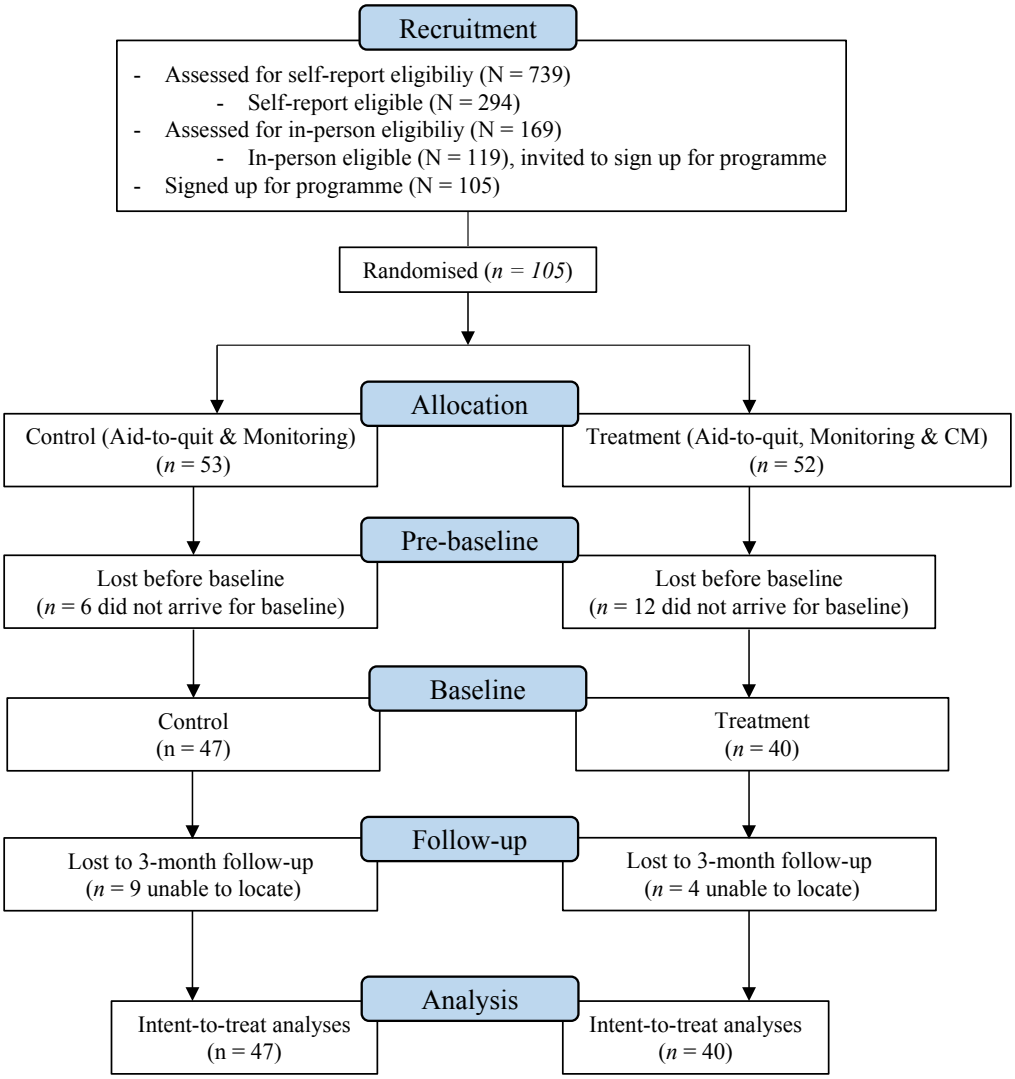

Figure 2: *The flow of participants from the point of initial contact through to data analysis*

[32] We measured carbon monoxide (CO) levels in expired air using a Micro+ Smokerlyzer® monitor (Bedfont Scientific Ltd.). The sensitivity and specificity of CO measurements are both around 90% (Benowitz et al. 2002). The sensitivity is limited by the rapid elimination of CO: with sedentary activity, the half-life of CO is approximately 2 – 3 hours and, for the average smoker, CO levels are no different than those of a non-smoker after 24 hours of not smoking. Specificity is limited by endogenous and environmental sources of CO. Expired air CO measurements have been used by researchers and clinicians for decades to biochemically verify self-reported abstinence from smoking, and the Smokerlyzer brand of monitor has been used throughout the world in clinical research, tobacco treatment, and tobacco prevention education since the early 1980s (Benowitz et al. 2002).

3.2 Procedures

3.2.1 Recruitment and screening

After receiving ethics approval[33] to conduct the study and permission to access students, potential participants were recruited via an email sent to all students through UCT's central mailing list. The email provided an outline of the research study, and specified the expected time burden and earnings. Interested students were asked to complete a short online questionnaire that included 13 questions on demographics, smoking patterns, and whether the potential participant had an interest in quitting smoking and taking part in smoking cessation programme (i.e., whether they were treatment seeking). This information was used to screen out students who were ineligible based on self-reported information.[34] Over 700 students showed an interest in taking part in the study and 294 students were eligible based on their self-reported information (see Figure 2).

Subjects deemed eligible based on self-reported information were invited to sign up for a screening session, via an announcement sent through the university's virtual learning environment (VLE). The announcement explained what the screening session would entail, provided further details on the study, and asked subjects to sign up for the screening session. Of the 294 students who were contacted, 169 students signed up for and attended a screening session (see Figure 2).

The screening sessions ran on week days from 19 - 26 June 2017 in an office at UCT.[35] Screening sessions were 15-minute, one-on-one sessions with a research assistant (RA)[36] and students were paid a R50 show-up fee. An email and two SMS-reminders were sent to students to remind them of their session.[37] Following written informed consent, the RA verbally ran

[33] Ethics approval reference: REC/2017/05/002
[34] This meant that a smaller, more refined sample of students attended the screening sessions, thereby reducing both the time taken to screen potential participants and the cost of the study.
[35] To provide students with flexibility and to ensure that they were not prevented from attending due to clashes with their lectures, the screening sessions ran from 09:00-12:00 and 13:00-17:00 every day. The same approach was adopted for the programme and follow-up sessions.
[36] We limited the number of people running the screening sessions (myself and one other RA) to ensure consistency across sessions. The same approach was adopted for the programme sessions.
[37] The email was sent the day before the student's session, and the SMS-reminders were sent at 17:00 the day before the session and at 08:00 on the day of the session. The same applies to the reminders sent before the baseline, programme, and follow-up sessions.

through the same questionnaire that students had completed online and took a CO reading to determine potential participants' smoking status.[38] Before leaving, subjects were paid the R50 show-up fee and were told that they would be contacted if they were eligible to take part in the study. Of the 169 students who attended the screening sessions, 119 students were eligible (see Figure 2), based on the criteria outlined in Section 3.1.

Eligible students were invited to sign up for the baseline session, via an announcement sent through the university's VLE. The announcement informed students that they were eligible to take part in the study, repeated the information about what the study would entail, and asked students to sign up for the baseline session. The baseline session marked the start of the smoking cessation programme and sign-up for it was, therefore, sign-up for the programme itself. Of the 119 eligible students that were contacted, 105 students signed up for the baseline session (see Figure 2).

3.2.2 Randomisation

After subjects had signed up for the baseline session[39], randomisation to either treatment or control occurred using stratified random assignment (see Glennerster and Takavarasha 2013, p.153-158), where the stratification variables were gender, race, and CO reading. The 105 subjects were split between the treatment and control groups ($n = 52$ and $n = 53$ for treatment and control, respectively).

[38] As discussed, findings from previous studies point to the importance of biochemically verifying subjects' smoking status before entry into a CM intervention because non-smokers may try to enrol. CO readings were also taken during the programme to verify abstinence (see discussion below).

[39] Randomisation happened after sign-up for the baseline session but before the baseline session itself for three reasons. First, because sign-up for the baseline session was sign-up for the programme itself, those subjects who signed up for the baseline session needed to be randomised into either the treatment or control group. Second, randomisation in the baseline session itself was not possible because, due to our relatively small sample size, we needed to stratify; an impossibility if we randomised in the session itself. Third, while it would have been preferable to randomise after the baseline session (this would have allowed for stratification on the information collected in the baseline session, particularly updated CO readings), treatment subjects needed to be informed, during the baseline session, that they would receive abstinence-contingent rewards in the programme sessions; treatment subjects needed this information for it to affect their quit attempt. The decision about when to randomise – in particular, the decision to randomise after baseline sign-up – meant that the baseline sessions were run with treatment and control subjects together. Furthermore, it meant that subjects randomised into the treatment or control group could "drop out" before the baseline session/start of the programme. Figure 2 indicates that 18 subjects dropped out before the baseline session – see Section 5.4.2 for a discussion of this issue.

3.2.3 Baseline

The baseline sessions ran from 14 - 18 August 2017 in a computer lab at UCT and involved both a group and individual component. It was the longest of all the study sessions, lasting approximately two hours, on average. An email and two SMS-reminders were sent to subjects to remind them of their session. Seven sessions[40] took place in total and the mean group size was 13 participants. Each subject received a R50 show-up fee and had the opportunity to make additional earnings in the two decision-making tasks (see below); subjects earned R604, on average, during the session. I assumed the role of presenter for every session, to ensure consistency across the sessions, and three RAs were employed to help administer subject payments, answer questions, and take CO readings.

Upon arrival at the lab, subjects were randomly allocated to computer terminals and the group component of the baseline session commenced. Following written informed consent, for both the baseline session itself and the remaining study sessions, I delivered a presentation explaining, in detail, what the smoking cessation programme would entail. Subjects then signed up for the programme sessions through the university's VLE. Following sign-up, subjects completed two decision-making tasks designed to elicit their risk and time preferences. Subjects received both written and audio-visual instructions for the tasks and the order of the tasks was counter-balanced across sessions so subjects either performed the risk or time preference task first.[41]

The risk preference task presented subjects with choices between two lotteries; these lotteries were displayed as pie charts with accompanying text that listed the prizes and their probabilities.[42] The task used prize magnitudes between R0 and R1320 and probabilities which varied in increments of 0.05 between 0 and 1. Subjects made 90 choices in this task. It took subjects 20 minutes, on average, to complete the task and subjects earned R226, on average. The time preference task presented subjects with choices between smaller, sooner (SS) and

[40] To provide students with flexibility and to ensure that they were not prevented from attending due to clashes with their lectures, the seven baseline sessions were spread out over the week.

[41] The task order was counter-balanced to avoid potential order effects, where behaviour in a task may be influenced by the prior treatment/task (Friedman and Sunder 1994). Hofmeyr (2015) found evidence of order effects in similar tasks run on the same population.

[42] As mentioned in Section 2.2, a number of CM studies offer uncertain rewards (e.g., prize or cash draws, where not all draws result in the subject receiving a prize). While this approach has been used to reduce study costs, we could not provide uncertain rewards as it would have interacted with the risk preference information we collected.

larger, later (LL) monetary rewards. The task employed two smaller, sooner (SS) rewards (R250 and R400) and nominal annual interest rates that varied between 0% and 250% to determine the larger, later (LL) rewards. Subjects made 60 choices in this task. It took 10 minutes, on average, for subjects to complete the task and subjects earned R331, on average.[43]

Upon completion of the tasks, subjects were instructed to complete the baseline questionnaire. The questionnaire included: 10 questions on socioeconomic and demographic information; the Barratt Impulsiveness Scale (Patton et al. 1995); four validated psychological screens focussing on depression (the Beck Depression Inventory-II, Beck et al. 1996), anxiety (the Beck Anxiety Inventory, Beck et al. 1988), drinking behaviour (WHO Alcohol Use Disorders Identification Test, WHO 2001), and gambling behaviour (The Problem Gambling Severity Index, Ferris and Wynne 2001); and the tobacco and nicotine use module from the National Epidemiological Survey on Alcohol and Related Conditions-III (NESARC-III, Grant et al. 2014).[44] In addition to the NESARC-III tobacco and nicotine use module, the questionnaire included additional modules and questions related to cigarette smoking: it administered the Fagerstrom Test for Cigarette Dependence (FTCD, Fagerström 2012; Heatherton et al. 1991), the Smoking Abstinence Self-Efficacy Questionnaire (SASEQ, Spek et al. 2013), the Minnesota Tobacco Withdrawal Scale-Revised (MTWS-R, Etter and Hughes 2006), and the Reasons for Quitting Questionnaire (RFQ, Curry et al. 1990). Additionally, it included questions about past quit attempts (Ontario Tobacco Research Unit 2015), motivation to quit (National Centre for Smoking Cessation and Training 2017), attitudes towards UCT's new smoking policy, and environmental factors that may affect a quit attempt, e.g., friends, family, or co-inhabitants who smoke (Ontario Tobacco Research Unit 2015).

Following completion of the questionnaire, subjects were taken one at a time to the individual component of the baseline session that took place in private rooms in the computer lab. RAs provided subjects with a summarised explanation of the cessation programme and took their CO reading. Subjects were given their cash earnings[45], an aid-to-quit document[46], and a printed

[43] The risk and time preference tasks will not be discussed in any further detail because they are not the focus of this dissertation.
[44] These modules have been widely used to measure impulsivity, depression, anxiety, and alcohol and gambling disorders, respectively. These screens will not be discussed in any further detail because they are not the focus of this dissertation.
[45] Some subjects made additional earnings in the time preference task that were paid to them at a future date. RAs collected subjects' banking details and this money was paid to the subjects via electronic transfer on the relevant date.
[46] The aid-to-quit document was Helpguide.com's (2017) online guide for helping people quit smoking.

copy of the cessation programme explanation (with their programme session dates filled in). Subjects were then free to leave. Following the baseline session, subjects had one week to quit smoking.

In addition, after taking treatment subjects' CO readings, RAs informed treatment subjects that they had been randomly selected to earn abstinence-contingent incentives during the programme sessions. Given that baseline sessions were run with treatment and control subjects together (see Footnote 39), treatment subjects were informed about the abstinence-contingent rewards (i.e., provided with treatment information) in the individual component of the baseline sessions. Up until this point the treatment allocation was concealed from subjects (i.e., subjects were not informed about treatment before sign-up for the programme), because, as mentioned, allocation concealment - along with an adequate sequence generation procedure – helps avoid a selection bias issue. However, the explicit reward mechanism in CM interventions makes complete blinding of subjects impossible and creates a potential for performance bias. To minimise the possibility of performance bias, we chose to reveal treatment information to treatment subjects but not to control subjects. This design reduces the possibility of a John Henry effect, and a resentment and demoralisation effect. While there is certainly the possibility for a Hawthorn effect, Harrison and List (2004) question the extent to which the Hawthorn effect affects behaviour.

An additional benefit of informing treatment subjects about treatment in the individual component of the baseline session was that we could ensure comprehension of the abstinence-continent rewards, a necessity for the effectiveness of a CM programme (Meredith et al. 2014). In particular, RAs explained when and under what conditions the abstinence-contingent rewards would be paid, and subjects were able to ask any questions they may have had. In addition, when providing treatment subjects with treatment information, it was emphasised that they had been randomly selected to form part of the group that received the additional incentives. This was done so that if control subjects found out from treatment subjects that there was a group receiving additional incentives, they would know that the process by which treatment subjects were selected was random, and therefore fair (Glennerster and Takavarasha 2013).

3.2.4 Programme sessions

The programme sessions ran from 28 August - 22 September 2017 in an office at UCT. The purpose of the programme sessions was to monitor subjects' abstinence/quit attempt and to provide treatment subjects with abstinence-contingent incentives to positively reinforce abstinence from smoking. Each subject attended four weekly programme sessions, starting one week after their quit date. The sessions were 15-minute, one-on-one meetings with a RA. An email and two SMS-reminders were sent to subjects to remind them of their session. In each of the sessions, subjects earned a R50 show-up fee and treatment subjects earned an additional R150 payment if they were abstinent in the previous week (see below). In each of the four programme sessions, a RA ran through a short questionnaire with the subject, took their CO reading[47], and paid the subject his/her earnings.[48]

The programme session questionnaires employed the timeline followback (TLFB) method to determine smoking behaviour in the 7 days prior to the session (Robinson et al. 2014), the SASEQ, the MTWS-R, and the FTCD.[49] TLFB is a calendar-based method used to capture retrospective daily substance use. The method was originally developed to obtain self-reports of alcohol use, but has been extended to other addictive behaviours (i.e., cigarette, marijuana, and other drug use). The method involves asking subjects to retrospectively estimate, and fill in on a calendar, their substance use in the period (between 7 days to 2 years) prior to the interview date. The method can be administered by an interviewer, by a computer, or by the subject him/herself. Psychometric evaluations of the method indicate that TLFB can be used to collect psychometrically sound information about substance use in treatment-seeking and nontreatment-seeking populations for periods ranging up to 12 months prior to the interview (Robinson et al. 2014). In the current study, TLFB was administered by a RA to capture subjects' self-reported cigarette[50] use in the 7 days prior to each programme session. Each

[47] In contrast to a number of studies in the literature, we took the CO readings of all subjects and not just those who reported abstinence.

[48] Data from the questionnaire and CO reading were captured in the Qualtrics survey framework, and the script read by the RA when paying the subject was presented in the same framework – see Section 3.2.4.1.

[49] The questionnaire in the fourth and final programme session included several additional questions. The questionnaire asked the subject whether they knew other students taking part in the study, how many other students they knew who were taking part, and the names of those students. Additionally, the questionnaire asked the subject to list the reasons for why they thought they had remained, or failed to remain, abstinent, and what approach they had adopted to help them with their quit attempt. Treatment subjects were asked whether, and why, they thought the weekly R150 abstinence-contingent payments had or had not helped with their quit attempt.

[50] The TLFB method was also used to capture whether subjects used cigarette-substitutes to aid them with their quit attempt. Subjects were asked whether they had used nicotine replacement therapy (NRT) or an e-cigarette in

subject was allocated a printed calendar (see Figure 3) that was filled in in each programme session.[51] The calendar was filled in starting with the day before the session date and the RA tracked backwards to cover the full 7-day retrospective period. For each day, the RA asked the subject if they had remained abstinent from smoking and, if they had not remained abstinent, how much they had smoked on that day.[52,] The TLFB data can be used to derive the 7-day PPA abstinence measure, and to capture daily smoking intensity over the four-week period in which subjects attended programme sessions (i.e., from their quit date until their final programme session).

	MON	TUES	WED	THURS	FRI	SAT	SUN
		1 VAC	2 VAC	3 VAC	4 VAC	5 VAC	6 VAC
AUG	7 VAC	8 VAC	9 VAC	10 VAC	11 VAC	12 VAC	13 VAC
	14 **SESSION 1**	15	16	17	18	19	20
	21 **QUIT DATE**	22	23	24	25	26	27
	28 **SESSION 2**	29	30	31	1	2	3
SEP	4 **SESSION 3**	5	6	7	8	9	10
	11 **SESSION 4**	12	13	14	15	16	17
	18 **SESSION 5**	19	20	21	22	23 VAC	24 VAC
	25 VAC	26 VAC	27 VAC	28 VAC	29 VAC	30 VAC	1 VAC
OCT	2	3	4	5	6	7	8
	9	10	11	12	13	14	15
	16	17	18	19	20	21	22
	23	24	25	26	27	28	29
	30	31	1	2	3	4	5
NOV	6 **SESSION 6**	7	8	9	10	11 CONSOLIDATION	12 CONSOLIDATION
	13 CONSOLIDATION	14 CONSOLIDATION	15 EXAMS	16 EXAMS	17 EXAMS	18 EXAMS	19 EXAMS
	20 EXAMS	21 EXAMS	22 EXAMS	23 EXAMS	24 EXAMS	25 EXAMS	26 EXAMS
	27 EXAMS	28 EXAMS	29 EXAMS	30 EXAMS	1 VAC	2 VAC	3 VAC
DEC	4 VAC	5 VAC	6 VAC	7 VAC	8 VAC	9 VAC	10 VAC
	11 VAC	12 VAC	13 VAC	14 VAC	15 VAC	16 VAC	17 VAC
	18 VAC	19 VAC	20 VAC	21 VAC	22 VAC	23 VAC	24 VAC
	25 VAC	26 VAC	27 VAC	28 VAC	29 VAC	30 VAC	31 VAC

Figure 3: *Timeline Followback Calendar*

Note: This is an example calendar for a subject who attended a baseline session (session 1 from the perspective of the subject) on 14 August, attended programme sessions (session 2-5 from the perspective of the subject) on Mondays during the 4-week period over which the sessions were run, and attended the first follow-up session (session 6 from the perspective of the subject) on 6 November.

the 7 days prior to the programme session. Quantitative estimates of these substitutes are not meaningful which is why we only asked subjects whether or not they had used NRT or e-cigarettes, rather than how much they had used (Sobell et al. 1996).

[51] A printed calendar was used because it was easier for the RA to fill in with the subject. The RA captured the data in Qualtrics at the end of each subject's session.

[52] Subjects were told that smoking a puff of a cigarette would be captured as having smoked one cigarette. They were also told that, if they were struggling to recall exactly how many cigarettes they had smoked, they should provide their best estimate of their cigarette use on each day.

After completing the questionnaire, the RA took the subject's CO reading.[53] The subject was considered abstinent if they reported, in the TLFB section of the questionnaire, being abstinent in the previous 7 days *and* if they had a CO reading of 6 ppm or less[54]; both conditions had to be satisfied for the subject to be classified as abstinent. In what follows, a subject is only considered abstinent if they satisfy these two conditions. Every subject received a R50 show-up fee. Treatment subjects who were abstinent were paid an additional R150[55]; this constitutes the positive reinforcement that is CM.[56] To minimise the risk of attrition, each subject was paid a R200 attendance reward in the final programme session if he/she had attended all four programme sessions, irrespective of whether he/she was abstinent.[57]

3.2.4.1 Paying the subject

For a CM programme to bring about its targeted behavioural change, it should not only be designed using the basic principles of CM, outlined in Section 2.2, but it also needs to be administered correctly (Ledgerwood and Petry 2010; Petry and Ledgerwood 2010). To this end, I developed a script for the RAs to read when giving subjects their earnings in each of the programme sessions. The script was drafted to promote abstinence from smoking and attendance at the programme sessions (i.e., there were two targeted behaviours).[58]

I developed the script using the principles outlined in the Contingency Management Competence Scale for Reinforcing Abstinence (CMCS, Ledgerwood and Petry 2010) and the Contingency Management Competence Scale for Reinforcing Attendance (CMCS-RA, Petry

[53] Due to the short half-life of carbon monoxide, the CO readings taken during the four programme sessions do not biochemically verify abstinence for the full 7-day retrospective period on which the subjects reported in the TLFB section of the questionnaire. Instead, the CO readings were used to deter deception and to establish whether the subject had smoked in the previous day/24 hours.
[54] 6 ppm or less is considered a conservative cut-off point between smokers and non-smokers (Benowitz et al. 2002).
[55] The abstinence-contingent R150 incentives paid in the weekly programme session were awarded independently of abstinence in previous programme sessions.
[56] As mentioned in Section 2.2, the incentive schedule of CM programmes varies. This study adopts a uniform incentive distribution; the abstinence-contingent reward was R150 in each of the four programme sessions. The research team on this study have proposed a larger and more complex study for which the current study will serve as a pilot. Amongst other things, the proposed intervention will adopt different incentive schedules and, as such, the current study, with its uniform incentive distribution, serves as a starting point for the proposed study.
[57] Subjects were reminded about the attendance reward in each of the programme sessions to ensure that the reward was salient.
[58] As mentioned in Section 2.2, in addition to addictive substance use itself, CM has been used to target a number of other related behaviours, including attendance at sessions. While this dissertation focuses on a CM programme that provides financial incentives to positively reinforce *abstinence* from smoking, we decided that it was also important to reinforce *attendance* at the programme and follow-up sessions to minimise the risk of attrition.

and Ledgerwood 2010). CMCS and CMCS-RA are 12- and 9-item rating scales used to evaluate whether therapists/interviewers are implementing a CM intervention correctly. Using this framework, I developed scripts for treatment and control[59] subjects that varied depending on whether the subject was abstinent in the current programme session, whether they had attended previous programme sessions, and whether they had been abstinent in previous programme sessions – see Table A in the Appendix. Therefore, behaviour (i.e., attendance and abstinence) in both the current and past sessions determined the script that was read, and the script became increasingly complicated in each subsequent programme session.[60]

CMCS and CMCS-RA emphasise the importance of discussing certain points when paying the subject the incentive designed to positively reinforce the targeted behaviour (i.e., either attendance or abstinence). To this end, the script I developed mentioned specific points when discussing both abstinence and attendance. With regard to abstinence, the RA discussed abstinence in the current, past, and future programme sessions. When discussing abstinence in the current programme session, the RA mentioned three things. First, the RA made note of whether the subject was abstinent and, for treatment subjects, whether the subject received the abstinence-contingent R150 reward. Second, the RA provided a statement of encouragement or support. Third, for treatment subjects, the RA made an explicit connection between the targeted behaviour (i.e., abstinence) and the consequence (i.e., the abstinence-contingent R150 reward); they emphasised that the subject was receiving (was not receiving) the R150 payment because they were abstinent (were not abstinent) in the current programme session.

When discussing abstinence in previous[61] programme sessions, the RA made note of whether the subject had been abstinent in previous sessions[62] and provided a statement of

[59] The CMCS and CMCS-RA are rating scales for CM interventions where all subjects receive treatment (i.e., positive reinforcement for a specified target behaviour). Because all subjects in the current study were receiving incentives for attendance, I drew on the CMCS-RA framework to develop scripts for both treatment and control subjects to discuss attendance. In contrast, only treatment subjects received incentives for abstinence. As such, I developed a script for treatment subjects that drew on the CMCS framework, and subsequently adapted it to create an equivalent script for control subjects. The scripts for treatment and control subjects were identical, therefore, except for the discussion points regarding abstinence: both scripts encouraged abstinence, but the script read to treatment subjects explicitly spoke to incentives that aimed to positively reinforce abstinence.

[60] The Qualtrics survey framework was used to avoid errors when reading the script. Specifically, the questionnaire and CO reading data were captured in Qualtrics and, depending on the data inputted for the subject (i.e., whether they were abstinent in the current programme session, and whether they had attended and were abstinent in previous programme sessions), the relevant script would appear for the RA to read.

[61] This was only relevant from the second programme session onwards. The same applies to the discussion on past attendance.

[62] For example, in the third programme session, the RA made note of whether the subject had been abstinent in either, or both, of the first and second programme sessions.

encouragement or support. When discussing abstinence in future[63] programme sessions, the RA reminded the subject that the goal for the coming week was to remain abstinent and, for treatment subjects, the RA made an explicit connection between abstinence in the future programme session and the possibility of earning the abstinence-contingent R150 reward.[64] Finally, the RA encouraged the subject to refer to the aid-to-quit document if they were struggling with their quit attempt.

With regard to attendance[65], the RA discussed attendance in the current and future programme sessions. When discussing attendance in the current session, the RA mentioned three things. First, they noted that the subject was receiving the R50 show-up fee for attending the current programme session. Second, the RA made a statement of encouragement or support. Third, the RA made an explicit connection between the targeted behaviour (i.e., attendance) and the consequence (i.e., the R50 show-up fee); they emphasised that the subject was receiving the R50 show-up fee because they were attending the current programme session, and that the payment was independent of whether they were abstinent in the current programme session. When discussing attendance in future programme sessions, the RA reminded the subject that they should attend future programme sessions and that, if they did, they would receive the R50 show-up fee; making clear the connection between attendance and the show-up fee, and emphasising that the show-up fee was independent of whether the subject was abstinent in the future programme session. Finally, the RA reminded the subject that if they attended[66] all programme sessions, they would receive the R200 attendance bonus in the final session.[67]

[63] This was not relevant in the final programme session. The RA did, however, remind the subject that there were still two follow-up sessions and that the goal going forward was to remain abstinent. The same applies to the discussion on future attendance: the RA reminded the subject that there were still two follow-up sessions and that, if they attended, they would receive the R200 show-up fee.

[64] Treatment subjects were reminded that the weekly abstinence-contingent incentives were awarded independently of abstinence in previous programme sessions.

[65] Subjects were told that it was important to attend all their sessions, irrespective of whether they were abstinent or not. RAs told subjects that if they had failed to remain abstinent we were also interested in their smoking intensity.

[66] This only applied to subjects who had attended all previous programme sessions. Subjects who had missed a previous programme session were told that they unfortunately no longer qualified for the attendance bonus. They were told that they should keep attending all their programme sessions and that they would continue to receive the R50 show-up fee in each session.

[67] This "attendance bonus reminder" only applied to the first three programme sessions because the bonus was paid in the fourth and final programme session. When paying the R200 attendance bonus to the subject, the RA told the subject that they were receiving (were not receiving) the R200 because they had attended (had not attended) all of the programme sessions.

3.2.5 Follow-up sessions

One week before each of the follow-up sessions, all subjects were invited to sign up for a session, via an announcement sent through the university's VLE. The 3-month follow-up sessions were run from 13 – 17 November 2017 and the 6-month follow-up sessions will be run from 26 February – 2 March 2018.[68] The purpose of the follow-up sessions is to determine the remission rates of treatment and control subjects after a 3-month and 6-month period. The sessions are 15-minute, one-on-one meetings with a RA and subjects are paid a R200 show-up fee.[69] An email and two SMS-reminders are sent to students to remind them of their session. In each of the two follow-up sessions, a RA runs through a short questionnaire with the subject and takes his/her CO reading.

The follow-up session questionnaire employs TLFB to determine smoking behaviour in the 7 days prior to the session, the SASEQ, the MTWS-R, and the FTCD. In addition, in the 3-month follow-up, subjects were asked whether they had smoked since their last programme session and, if they had smoked, what the average number of cigarettes they had been smoking per day since their last programme session.[70]

After completing the questionnaire, the RA took the subject's CO reading and paid them the R200 show-up fee.[71]

3.2.6 Earnings

Subjects received a R50 show-up fee in the screening, baseline, and four programme sessions, and a R200 show-up fee in each of the two follow-up sessions. In the baseline sessions, subjects made additional earnings in the two decision-making tasks; subjects earned R557, on average,

[68] On the date of submission of this dissertation, the 6-month follow-up session is yet to be run. As such, this dissertation only looks at data up until the 3-month follow-up session.
[69] The show-up fee is larger than in previous sessions to minimise the risk of attrition after a prolonged period (3 and 6 months) of not having seen the subjects. Subjects were reminded of the show-up fee in the final programme session, in the sign-up announcement, and in the session reminders (an email and two SMSs).
[70] The 7-day PPA and continuous abstinence measures can be derived from these data.
[71] As with the approach adopted when paying subjects in the programme sessions, the RA told the subject that the R200 show-up fee was for attending the session and that it was irrespective of abstinence. At the end of the 3-month follow-up session, the RA reminded the subject about the 6-month follow-up session and about the R200 show-up fee for that session.

in the tasks. In each of the four programme sessions, treatment subjects who were abstinent were paid the R150 abstinence-contingent incentive, over and above the R50 show-up fee.

4. STATISTICAL METHODOLOGY

This section outlines the statistical methodology adopted to answer the two main research questions: Does a low-cost, low-intensity CM intervention, conducted with a sample of treatment-seeking university students, increase smoking cessation rates and, if not, does it decrease the smoking intensity of non-abstainers?

To answer the first research question, a random-effects probit model is used to examine the probability of abstinence. This model is appropriate because the dependent variable, $Abstinent_{it}$, is a binary response variable. Further, the model accounts for the panel structure of the dataset and therefore the correlation in the standard errors over time. The regression specification is:

$$\Pr\left(Abstinent_{it} = 1 \mid \mathbf{x}_{it}\right) = \Phi\left(B_1 Treatment_i + B_2\mathbf{x}_{2i} + \mathrm{B}_3\mathbf{x}_{3i} + \mathrm{B}_4\mathbf{x}_{4i}\right) \quad (1)$$

where $Abstinent_{it}$ is a binary variable equal to one if the subject is abstinent and equal to zero otherwise.[72] $Treatment_i$ is a binary variable equal to one if the subject is in the treatment group and equal to zero if they are in the control group. Thus, B_1 identifies the average difference in the probability of being abstinent for treatment and control subjects (i.e., the treatment effect).

Vector $\mathbf{x}_2$ includes demographic variables and a session variable: Age_i is a continuous variable; $Male_i$ is a binary variable equal to one if the subject is male and equal to zero otherwise; $Race_i$ is a categorical variable that includes African (the base category), Coloured, Indian, and White; $Income(ln)_i$ is a continuous variable[73]; and $Session_i$ is a categorical variable that includes the baseline session (the base category), the intervention period (i.e., four programme sessions), and the 3-month follow-up session.

[72] As mentioned, two conditions have to be satisfied for the subject to be classified as abstinent: the subject has to report, in the TLFB section of the questionnaire, being abstinent in the past 7 days and has to have a CO reading of 6 ppm or less. $Abstinent_{it}$ is therefore a 7-day PPA measure of smoking abstinence (Hughes et al. 2003).

[73] The $income(ln)_i$ variable is constructed by taking the natural logarithm of subjects' total income (from all sources) in the previous month.

Vector $\mathbf{x}_3$ includes variables related to smoking: *Smoking duration*$_i$ is a discrete variable indicating the number of years that the subject has been a daily smoker; *Smoking intensity*$_i$ is a discrete variable indicating the average number of cigarettes that the subject smokes per day; *FTCD score*$_i$ is a discrete variable ranging from 0 – 10, where larger values indicate a greater level of dependence on cigarettes; *MTWS-R score*$_{it}$ is a discrete variable ranging from 0 – 60, where larger values indicate a greater level of tobacco withdrawal discomfort; and *Baseline CO reading*$_i$ is a continuous variable.

Vector $\mathbf{x}_4$ includes variables related to quitting smoking: *Quit attempt in the past 5 years*$_i$ is a binary variable equal to one if the subject made a quit attempt in the past 5 years and equal to zero otherwise; *SASEQ score*$_i$ is a discrete variable ranging from 0 – 24, where larger values indicate a greater level of self-efficacy regarding smoking cessation; *Importance of current quit attempt*$_i$ is a categorical variable derived from a question asking the subject how determined they are to quit smoking on the current attempt; *Determination for quit attempt*$_i$ is a categorical variable indicating how important the current quit attempt is to the subject[74]; *RFQ intrinsic score*$_i$ and *RFQ extrinsic score*$_i$ are discrete variables constructed from a module asking the subject about their reasons for quitting smoking, where larger values indicate greater levels of intrinsic and extrinsic motivation respectively. [75]

The baseline specification of *Equation (1)* includes *Treatment*$_i$, after which vectors $\mathbf{x}_2$, $\mathbf{x}_3$, and $\mathbf{x}_4$ are included consecutively in the analysis. Vector $\mathbf{x}_2 - \mathbf{x}_4$ covariates are included in the model to control for any differences between subjects, in order to ensure that the results of interest are not statistically significant merely because of correlations with omitted variables. The analysis of abstinence is conducted on an intention-to-treat (ITT) basis. If the subject, either treatment or control, did not attend their session it is assumed that they are not abstinent.[76] Using this assumption to impute abstinence outcomes for non-attenders gives conservative absolute cessation rates (Cahill et al. 2015); see Section 5.4.2 for a discussion of attrition and robustness checks.

[74] The categories for *Importance of current quit attempt*$_i$ and *Determination for quit attempt*$_i$ are "Not at all," Quite," "Very," and "Extremely."
[75] With the exception of *MTWS-R score*$_{it}$, all independent variables were measured at baseline. The same is true of the independent variables included in the random-effects negative binomial model used to analyse smoking intensity. *MTWS-R score*$_{it}$ is a time-variant variable, which was measured in each session.
[76] All *Abstinent*$_{it}$ descriptive statistics are also conducted on an ITT basis.

To answer the second research question (i.e., does the low-cost, low-intensity CM programme reduce smoking intensity amongst those subjects who failed to remain abstinent?), a random-effects negative binomial model is used to examine the smoking intensity of those subjects who failed to remain abstinent. This model is appropriate because the dependent variable, *Smoking intensity*$_{it}$, is a count variable. Further, the model accounts for the panel structure of the dataset and therefore the correlation in the standard errors over time.

For the random-effects negative binomial regression of smoking intensity, let y_{it} be the count dependent variable *Smoking intensity*$_{it}$, which indicates the average number of cigarettes smoked per day for the ith subject in the tth session.[77] We begin with the model $y_{it} \mid \gamma_{it} \sim$ Poisson (γ_{it}), where $\gamma_{it} \mid \delta_i \sim$ gamma (λ_{it}, δ_i) and $\lambda_{it} = \exp(\mathbf{x}_{it}B)$, where δ_i is the dispersion parameter. This produces the model:

$$\Pr\left(Y_{it} = y_{it} \mid \mathbf{X}_{it}, \delta_i\right) = \frac{\Gamma\left(\lambda_{it} + y_{it}\right)}{\Gamma(\lambda_{it})\Gamma\left(y_{it} + 1\right)}\left(\frac{1}{1+\delta_i}\right)^{\lambda_{it}}\left(\frac{\delta_i}{1+\delta_i}\right)^{y_{it}} \quad (2)$$

For a random-effects negative binomial model, δ_i is allowed to vary randomly across subjects, and we assume, therefore, that $1/(1 + \delta_i) \sim$ Beta(r, s). As with the random-effects probit model used to examine the probability of abstinence, independent variables, $\mathbf{x}_{it}$, are included consecutively in the random-effects negative binomial model. All independent variables are identical to those in the random-effects probit model, expect that *Smoking intensity*$_i$ is not included in vector $\mathbf{x}_2$, for obvious reasons.

Together *Equation (1)* and *Equation (2)* constitute a hurdle model. A hurdle-model is a two-part model which specifies one data generating process (DGP) for the "participation" decision ($y = 0$ versus $y > 0$) and another DGP for the "amount" decision (the value of y over positive values). The logic behind a hurdle model is that the limit outcome ($y = 0$) represents a separate and distinct process to the non-limit outcomes ($y > 0$) and this limit outcome hurdle must be jumped or surpassed before one observes non-limit responses. In the context of the decision to smoke, the limit outcome (i.e., zero cigarettes smoked per day) represents the decision to not smoke. The non-limit outcomes (i.e., a positive number of cigarettes smoked per day), by contrast, represent smoking intensity, where smoking intensity is only observed for those

[77] In contrast to *Smoking intensity*$_i$ which is a baseline measure of smoking intensity, *Smoking intensity*$_{it}$ is a time-variant variable constructed from TLFB data on subjects' smoking intensity in the 7 days prior to the programme and follow-up sessions.

individuals who decide to smoke, those individuals who have jumped the hurdle. It seems natural then to model the limit and non-limit outcomes as the result of separate and distinct underlying processes.[78] This is clearly important if one suspects that certain covariates may have opposite effects on these two decisions. In this dissertation, *Equation (1)* models the participation decision (i.e., the decision to smoke or not), and *Equation (2)* models the amount decision (i.e., how much to smoke, conditional on smoking).[79]

5. RESULTS

This section discusses the results of the study. Section 5.1 presents summary statistics and investigates whether baseline balance holds across treatment and control groups. Section 5.2 and Section 5.3 report the results of analyses of smoking abstinence and smoking intensity, respectively. Section 5.4 discusses potential sources of bias and relevant robustness checks. Section 5.5 presents qualitative results based on questions asking subjects about their quit attempt.

5.1 Baseline summary statistics

Table I provides baseline summary statistics of the 87 subjects who took part in the study. Statistics for the combined, control, and treatment groups are provided, along with the results of tests indicating whether there is a significant difference in the observable baseline characteristics of treatment and control subjects.

[78] A defining characteristic of a hurdle model is the conditional independence assumption: the process which determines whether the independent variable *y* is zero or strictly positive and the process which determines the value of *y*, if it is non-zero, are independent conditional on the observable explanatory variables. It is this assumption that allows one to model the two DGPs separately. See Wooldridge (2010) for details.

[79] In the context of a smoking cessation programme, it is intuitive to think of the participation decision as one where the subject is deciding whether to be abstinent from smoking or not. As such, the dependent variable in *Equation (1)* is *Abstinent* which is equal to one if the subject is abstinent and equal to zero if the subject is not abstinent. Importantly, this approach is equivalent to a model which thinks of the participation decision as one where the subject is deciding to smoke or not; the dependent variable would be *Smoke* which is equal to one if the subject smokes and equal to zero if the subject does not smoke. The coefficient results would be economically and statistically identical, but would have opposite signs.

TABLE I
SUMMARY STATISTICS

Variable	Combined	Control	Treatment	Significant difference?[1]
Demographics				
Age	21.632	22.085	21.100	0.269
	(2.985)	(3.741)	(1.614)	
Male[2]	0.779	0.761	0.800	0.663
Population group[2]				
Black/African	28.740	27.660	30.000	0.945
Coloured	28.740	27.660	30.000	
Indian	19.540	19.150	20.000	
White	22.990	25.530	20.000	
Income	2970.000	3376.667	2500.769	0.252
	(2216.077)	(2603.071)	(1568.11)	
Cigarette smoking				
Smoking duration	3.376	3.891	2.769	0.077
	(2.610)	(2.917)	(2.287)	
Smoking intensity	9.857	9.889	9.821	0.756
	(5.895)	(5.180)	(6.696)	
FTCD score	3.459	3.413	3.513	0.816
	(1.842)	(1.784)	(1.931)	
MTWS-R score	17.233	17.213	17.256	0.699
	(9.969)	(10.936)	(8.804)	
CO reading (ppm)[2]	11.149	10.596	11.800	0.952
	(9.449)	(5.508)	(12.662)	
Quitting smoking				
Quit attempt in past 5 years	0.659	0.739	0.564	0.090
SASEQ score	11.310	11.447	11.150	0.791
	(5.593)	(5.559)	(5.700)	
Importance of current quit	2.929	3.000	2.846	0.724
	(0.897)	(0.919)	(0.875)	
Determination for current quit	3.047	3.152	2.923	0.285
	(0.770)	(0.788)	(0.739)	
RFQ intrinsic score	4.555	4.852	4.205	0.097
	(1.749)	(1.500)	(1.964)	
RFQ extrinsic score	2.444	2.557	2.310	0.279
	(1.261)	(1.254)	(1.271)	

Notes: FTCD = Fagerstrom Test for Cigarette Dependence; MTWS = Minnesota Nicotine Withdrawal Scale; SASEQ = Smoking Abstinent Self-efficacy Questionnaire Score

[1] Mann-Whitney test for continuous variables and χ^2 test for categorical variables

[2] Stratification variable

In the combined sample, approximately 78% of subjects are male; and 29% are African, 29% are Coloured, 20% are Indian, and 23% are White. Subjects are approximately 22 years old and have a monthly income of R2970, on average. Focussing on variables related to cigarette smoking ($\mathbf{x}_3$ variables), subjects had been smoking for approximately 3 years, had a smoking intensity of approximately 10 cigarettes per day, had a FTCD score of 3, had a MTWS-R score of 17, and had a CO reading of 11 ppm, on average. With regard to variables related to quitting smoking ($\mathbf{x}_4$ variables), subjects had a SASEQ score of 11, on average, and approximately 66% of subjects had made a previous quit attempt in the past 5 years. On average, subjects regarded the importance of their current quit attempt as "Very important" and reported being "Very determined" for their current quit attempt.[80] Average RFQ intrinsic and extrinsic scores were 5 and 2, respectively.

Mann-Whitney and Chi-squared (χ^2) tests for differences between observable baseline characteristics of treatment and control subjects indicate that the experimental groups are balanced across all observable attributes, except for *Smoking duration*$_i$, *Quit attempt in past 5 years*$_i$, and *RFQ intrinsic score*$_i$. Mann-Whitney tests indicate that control subjects have a longer smoking duration ($p = 0.077$) and higher RFQ intrinsic score ($p = 0.097$) compared to treatment subjects, on average. A χ^2 test indicates that a significantly larger proportion of control subjects had made a previous quit attempt in the past five years, compared to the treatment group ($p = 0.090$). Clearly it will be important to control for these differences in the statistical models to follow.

5.2 Smoking abstinence

This section reports the results of analyses investigating the first research question: Does a low-cost, low-intensity CM smoking cessation programme increase abstinence?

5.2.1 Initial investigation

Table II reports abstinence proportions for the intervention period (i.e., the four programme sessions) and the 3-month follow-up session, for treatment and control subjects.[81] During the

[80] Table I reports the average scores for the *Importance of current quit*$_i$ and *Determination of current quit*$_i$ ordinal variables which correspond to "Very important" and "Very determined" for each variable, respectively.
[81] Abstinence proportions are not reported for the baseline session because no subjects were abstinent; their quit date was one week after their baseline session.

intervention period, the proportion of abstinent subjects in the control group ranges from approximately 6% to 10%. Compared to the control group, abstinence proportions during the intervention period are higher in the treatment group, ranging from approximately 28% to 45%. χ^2 tests indicate that in each programme session the proportion of abstinent subjects in the treatment group is statistically significantly larger compared to the control group ($p < 0.05$ in all cases). In addition, while the intervention-period abstinence proportions oscillate across programme sessions for the control group, they increase in each programme session for the treatment group (from 28% to 45% across the four sessions).

TABLE II
ABSTINENCE PROPORTIONS

	Control	Treatment	Chi-squared test
Programme session 1	0.106	0.275	0.043
	(0.312)	(0.452)	
Programme session 2	0.064	0.300	0.004
	(0.247)	(0.464)	
Programme session 3	0.085	0.400	0.001
	(0.282)	(0.496)	
Programme session 4	0.064	0.450	0.000
	(0.247)	(0.504)	
3-month follow-up	0.021	0.075	0.233
	(0.146)	(0.267)	

Standard errors in parentheses

At the 3-month follow-up session, approximately 8% and 2% of treatment and control subjects are abstinent, respectively, and this difference is not statistically significant. For treatment subjects, this corresponds to a 38 percentage point decrease in the proportion of abstinent subjects from the end of the intervention period, compared to a 4 percentage point decrease for control subjects. This seems to suggest that receiving abstinence-contingent incentives during the intervention period may help treatment subjects remain abstinent but that once the incentives are removed subjects struggle to remain abstinent. This point requires further investigation.

Table III reports transition tables of abstinence for treatment and control subjects. Transition tables report the probabilities of transitioning from one category of a categorical variable to another category of the same variable, over time. Table IIIa reports results for the intervention

period while Table IIIb reports results for the intervention period and the 3-month follow-up together.

The results in Table IIIa indicate that in each programme session approximately 2% of control subjects who were not abstinent became abstinent in the next session, and approximately 58% of control subjects who were abstinent remained abstinent in the next session. In contrast, in each programme session approximately 15% of treatment subjects who were not abstinent became abstinent in the next session, and approximately 87% of treatment subjects who were abstinent remained abstinent in the next session. Both transition probabilities are higher for treatment subjects, suggesting that during the intervention period treatment subjects were more likely to become abstinent if they were not abstinent in a previous session and were more likely to remain abstinent if they were abstinent in a previous session.

TABLE III
ABSTINENCE TRANSITION TABLES

TABLE IIIa: INTERVENTION

Control group

	Abstinent		
Abstinent	No	Yes	Total
No	0.977	0.023	1
Yes	0.417	0.583	1
Total	0.929	0.071	1

Treatment group

	Abstinent		
Abstinent	No	Yes	Total
No	0.852	0.148	1
Yes	0.128	0.872	1
Total	0.617	0.383	1

TABLE IIIb: INTERVENTION & FOLLOW-UP

Control group

	Abstinent		
Abstinent	No	Yes	Total
No	0.983	0.017	1
Yes	0.467	0.533	1
Total	0.942	0.059	1

Treatment group

	Abstinent		
Abstinent	No	Yes	Total
No	0.884	0.117	1
Yes	0.351	0.649	1
Total	0.694	0.306	1

The results in Table IIIb indicate that when the 3-month follow-up data is included in the analysis, the probability of a non-abstinent subject becoming abstinent decreases for treatment subjects (15% to 12%) and decreases marginally for control subjects (2.3% to 1.7%). Further, the probability of an abstinent subject remaining abstinent decreases for both treatment subjects (87% to 65%) and control subjects (58% to 53%).

In sum, the proportion of abstinent subjects is statistically significantly higher during the intervention period for treatment compared to control subjects. In the follow-up session, the proportion of abstinent subjects decreases for both treatment and control groups and the difference in abstinence rates between the two groups is no longer statistically significant. Further, during the intervention period, the probability of a non-abstinent subject becoming abstinent and the probability of an abstinent subject remaining abstinent is higher for treatment compared to control subjects. Both probabilities decrease for treatment and control groups when the 3-month follow-up data is included in the analysis. The next section discusses a more detailed parametric analysis of abstinence.

5.2.2 Regression analysis

Table IV presents results of the random-effects probit model investigating whether the low-cost, low-intensity CM intervention increases abstinence (i.e., whether the probability of being abstinent is statistically significantly higher for treatment compared to control subjects).

Regression IVa indicates that the CM programme has an economically and statistically significant treatment effect: the likelihood of a treatment subject being abstinent is 13 percentage points higher compared to control subjects (p = 0.030). Regressions IVb – IVd include additional covariates to control for any potential differences between subjects. Regression IVb includes demographic characteristics as well as session variables ($\mathbf{x}_2$ variables). After including the additional covariates, the treatment effect remains statistically and economically significant, and increases slightly: the likelihood of a treatment subject being abstinent is now 15 percentage points higher compared to control subjects (p = 0.023). In addition, compared to African subjects, White subjects have a 17 percentage point higher probability of being abstinent (p = 0.063). A Wald test indicates that compared to White subjects, Indian subjects have a significantly lower probability of being abstinent (p = 0.075). Finally, compared to the baseline session, subjects have a 10 percentage point higher probability of being abstinent during the intervention period (p = 0.011).

TABLE IV

ABSTINENCE: RANDOM-EFFECTS PROBIT MODEL

	Regression IVa	**Regression IVb**	**Regression IVc**	**Regression IVd**
Treatment	0.127**	0.154**	0.156***	0.201***
	(0.058)	(0.068)	(0.059)	(0.059)
Age		0.006	0.017	0.025
		(0.007)	(0.013)	(0.016)
Male		0.031	0.044	0.02
		(0.042)	(0.047)	(0.062)
Coloured		0.065	0.043	0.083
		(0.053)	(0.051)	(0.066)
Indian		0.006	0.000	0.033
		(0.038)	(0.048)	(0.073)
White		0.173*	0.201**	0.237***
		(0.093)	(0.097)	(0.077)
Income (ln)		-0.011	-0.004	-0.021
		(0.038)	(0.041)	(0.044)
Intervention period		0.095**	0.106***	0.131***
		(0.043)	(0.041)	(0.046)
3-month follow-up		0.003	0.009	0.015
		(0.005)	(0.010)	(0.015)
Smoking duration			-0.017	-0.027
			(0.015)	(0.020)
Smoking intensity			-0.002	0.001
			(0.004)	(0.005)
FTCD score			0.017	0.017
			(0.013)	(0.015)
MTWS-R score			-0.002	-0.003
			(0.002)	(0.002)
Baseline CO reading			-0.001	-0.003
			(0.002)	(0.002)
Quit attempt in past 5 years				0.124**
				(0.051)
SASEQ score				0.006
				(0.006)
Importance of current quit attempt				0.011
				(0.038)
Determination for current quit				0.014
				(0.042)
RFQ intrinsic score				-0.018
				(0.020)
RFQ extrinsic score				0.012
				(0.031)
N	522	492	430	430
log-likelihood	-163.219	-113.783	-101.652	-97.625

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Robust standard errors in parentheses

Marginal effects reported

As may be expected given the descriptive statistics in Section 5.2.1, subjects' probability of being abstinent at the 3-month follow-up session is not significantly different to baseline. A

Wald test indicates that compared to the baseline session, the probability of being abstinent is significantly lower in the 3-month follow-up session compared to the intervention period (p = 0.020). This decline in the cessation rate at the follow-up session is characteristic of CM smoking cessation interventions: the programmes increase cessation rates during the intervention period while abstinence-contingent incentives are in place, but cessation rates typically decline once the incentives are removed.

Regression IVc includes variables related to cigarette smoking ($\mathbf{x}_3$ variables). The treatment effect remains statistically and economically significant (p = 0.008) and no variables related to cigarette smoking have a statistically significant effect on the probability of abstinence. Regression IVd includes variables related to quitting smoking ($\mathbf{x}_4$ variables). The treatment effect remains statistically and economically significant, and increases slightly: the likelihood of a treatment subject being abstinent is 20 percentage points higher compared to control subjects (p = 0.001). In addition, subjects who had made a quit attempt in the past 5 years have a 12 percentage point higher probability of being abstinent compared to those subjects who had not made a previous quit attempt (p = 0.024).[82]

As a robustness check, Model B:I in Table B (see Appendix) reports the results of a pooled Probit model.[83] Except for Age_i which becomes statistically significant (i.e., subjects who are one year older have a 3 percentage point higher probability of being abstinent; p = 0.040), the results of the pooled Probit model are qualitatively identical to those of the random-effects probit model. The random-effects probit model is the preferred model given that the estimate of ρ = 0.778, which represents the proportion of the total variance contributed by the panel-level variance component, is statistically significantly different to zero, indicating that one must incorporate the serial dependence of the error terms and model them statistically ($p < 0.001$).

In sum, the results in Section 5.2 suggest that a low-cost, low-intensity CM programme increases smoking cessation rates in a sample of treatment-seeking students at UCT. There is a statistically significant treatment effect, that is robust across different models and multiple

[82] *Quit attempt in the past 5 years*$_i$ is a variable that is not balanced across treatment and control groups at baseline. Both *Smoking duration*$_i$ and *RFQ intrinsic score*$_i$, which are significantly different for treatment and control subjects at baseline, do not have a statistically significant effect on the likelihood of abstinence.

[83] The *Session*$_i$ variables could not be included in the pooled Probit model because it does not account for the panel structure of the data. The base category, baseline session, is a perfect predictor of the response probability because no subjects are abstinent in the baseline session, which means the variable drops out of the analysis.

specifications, which increases the likelihood of abstinence by approximately 13-20%. However, noticeable declines in cessation rates are evident at the 3-month follow-up across treatment and control groups.

5.3 Smoking intensity

This section reports the results of analyses investigating the second research question: Does a low-cost, low-intensity CM programme reduce smoking intensity amongst those subjects who failed to remain abstinent?

5.3.1 Initial investigation

Table V reports average smoking intensity across the baseline session, the intervention period, and the 3-month follow-up session – for non-abstinent treatment and control subjects.

TABLE V
SMOKING INTENSITY

	Control	Treatment	Mann-Whitney test
Baseline	9.889	9.821	0.756
	(5.180)	(6.696)	
Programme session 1	3.056	4.296	0.139
	(2.496)	(3.289)	
Programme session 2	2.878	3.518	0.584
	(2.280)	(3.155)	
Programme session 3	3.155	4.020	0.405
	(2.597)	(3.625)	
Programme session 4	4.012	5.565	0.102
	(3.775)	(4.187)	
3-month follow-up	6.305	5.795	0.909
	(4.826)	(3.379)	

Standard errors in parentheses

At baseline, both treatment and control subjects were smoking approximately 10 cigarettes per day on average.[84] Smoking intensity decreases sharply from baseline to the intervention period. During the intervention period, the smoking intensity of non-abstinent control subjects

[84] These are the same statistics reported in Table I because no subjects were abstinent at baseline.

oscillates, ranging from 3 to 4 cigarettes per day on average, whereas the smoking intensity of non-abstinent treatment subjects ranges from 4 to 6 cigarettes per day. The smoking intensity of non-abstinent treatment subjects is higher compared to non-abstinent control subjects in each programme session during the intervention period, however, Mann-Whitney tests indicate that none of the differences are statistically significant. Finally, smoking intensity at the 3-month follow-up is 6 cigarettes per day for both non-abstinent treatment subjects and non-abstinent control subjects and this difference is not statistically significant.

In sum, initial descriptive statistics of smoking intensity of those subjects who failed to remain abstinent indicate that smoking intensity is lower during the intervention period and at follow-up, as compared to baseline levels. Further, there is no statistically significant difference in the smoking intensity of non-abstinent treatment subjects and non-abstinent control subjects in any of the sessions. The next section conducts a more detailed parametric analysis of smoking intensity.

5.3.2 Regression analysis

Table VI presents results of the random-effects negative binomial model investigating whether a low-cost, low-intensity CM intervention decreases the smoking intensity of those subjects who failed to remain abstinent (i.e., whether smoking intensity is statistically significantly lower for treatment compared to control subjects).

In contrast to expectations, Regression VIa indicates that the smoking intensity of non-abstinent treatment subjects is statistically significantly higher compared to non-abstinent control subjects ($p = 0.089$). Regressions VIb - VId include additional covariates to control for any potential differences between subjects. Regression VIb includes demographic characteristics as well as session variables ($\mathbf{x}_2$ variables). In contrast to Regression VIa, the results indicate that non-abstinent treatment subjects do not have a statistically significantly higher smoking intensity than non-abstinent control subjects ($p = 0.505$). In addition, compared to African subjects, Coloured subjects have a higher smoking intensity ($p = 0.040$). Finally, compared to the baseline session, non-abstinent subjects have a statistically significantly lower smoking intensity during the intervention period and at the 3-month follow-up ($p < 0.001$ in both cases). A Wald test indicates that the reduction in the smoking intensity from baseline is larger in the intervention period compared to the 3-month follow-up session ($p < 0.001$). Thus,

the Regression VIb results suggest that while there is no treatment effect, the programme as a whole reduces the smoking intensity of non-abstinent subjects and that, while still statistically significant, this reduction in smoking intensity is smaller at the 3-month follow-up than during the intervention period. This finding is in line with the preliminary investigation of smoking intensity in Section 5.3.1.

Regression VIc includes variables related to cigarette smoking ($\mathbf{x}_3$ variables). The results indicate that the "programme effect" is still present: compared to baseline, smoking intensity is statistically significantly lower during the intervention period and at the 3-month follow-up ($p < 0.001$ in both cases), where the reduction in smoking intensity from baseline is statistically significantly smaller at the 3-month follow-up session compared to the intervention period ($p < 0.001$). In addition, older subjects have a statistically significantly higher smoking intensity, on average ($p = 0.041$). Subjects who have been smoking for longer and subjects with a higher baseline FTCD score have a statistically significantly higher smoking intensity on average ($p = 0.003$ and $p < 0.001$, respectively). Regression VId includes variables related to quitting smoking ($\mathbf{x}_4$ variables). The results are in line with those of Regression VIc. In addition, subjects who reported being more determined to quit smoking at baseline have a statistically significantly lower smoking intensity on average ($p = 0.075$).

As robustness checks Table B (see Appendix) reports the results of a random-effects poisson model and a pooled negative binomial model. In Model B:II, except for $Male_i$ which becomes statistically significant (i.e., male subjects have a higher smoking intensity on average; $p = 0.077$), the results of the random-effects poisson model are qualitatively identical to those of the random-effects negative binomial model. In Model B:III, $Male_i$ is also positive and statistically significant ($p = 0.014$). In addition, Coloured subjects have a statistically significantly higher smoking intensity compared to African subjects ($p = 0.092$). The remaining coefficient estimates are qualitatively identical to those of the random-effects negative binomial.

TABLE VI

SMOKING INTENSITY: RANDOM-EFFECTS NEGATIVE BINOMIAL MODEL

	Regression VIa	**Regression VIb**	**Regression VIc**	**Regression VId**
Treatment	0.998*	0.412	0.39	0.189
	(0.587)	(0.618)	(0.516)	(0.492)
Age		0.105	-0.304**	-0.267*
		(0.111)	(0.149)	(0.147)
Male		0.165	0.947	0.839
		(0.713)	(0.579)	(0.585)
Coloured		1.561**	1.102	1.069
		(0.760)	(0.682)	(0.669)
Indian		0.899	0.006	-0.176
		(0.841)	(0.721)	(0.693)
White		1.243	0.678	0.524
		(0.873)	(0.748)	(0.713)
Income (ln)		0.235	0.434	0.339
		(0.494)	(0.418)	(0.402)
Intervention period		-6.368***	-6.337***	-6.281***
		(0.546)	(0.501)	(0.489)
3-month follow-up		-3.906***	-3.969***	-3.904***
		(0.586)	(0.573)	(0.568)
Smoking duration			0.463***	0.494***
			(0.157)	(0.162)
FTCD score			0.576***	0.696***
			(0.146)	(0.149)
MTWS-R score			0.022	0.023
			(0.021)	(0.021)
Baseline CO reading			0.04	0.039
			(0.030)	(0.030)
Quit attempt in past 5 years				-0.381
				(0.573)
SASEQ score				0.014
				(0.054)
Importance of current quit attempt				-0.269
				(0.389)
Determination for current quit				-0.784*
				(0.440)
RFQ intrinsic score				0.094
				(0.175)
RFQ extrinsic score				-0.023
				(0.266)
N	387	372	361	361
log-likelihood	-1022.2783	-887.16425	-851.04129	-847.03206

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
Standard errors in parentheses
Marginal effects reported

In sum, the results in Section 5.3 suggest that the low-cost, low-intensity CM smoking cessation programme does not reduce the smoking intensity of those subjects who failed to

remain abstinent (i.e., there is no smoking intensity treatment effect). However, the programme as a whole tends to reduce the smoking intensity of all non-abstinent subjects (i.e., both non-abstinent treatment and non-abstinent control subjects), but this effect, while still significant, appears to decline at the 3-month follow-up.

5.4 Potential biases and robustness checks

The review of the CM literature outlined a number of potential sources of bias that may limit one's ability to draw valid and robust causal inferences (i.e., selection bias, performance bias, attrition bias, and "deception" bias). This section discusses two potential biases that threaten the internal validity of the RCT – namely, performance bias and attrition bias - and analyses the robustness of the smoking abstinence and smoking intensity results in light of these issues. This section builds on the discussions in Section 5.2.2 and Section 5.3.2 which demonstrate the robustness of the smoking abstinence and smoking intensity results to different regression models.

5.4.1 Performance bias

Performance bias refers to systematic differences between groups' outcomes that arise from knowledge of treatment allocation, rather than the treatment itself. While blinding of participants may reduce the risk of performance bias, the explicit mechanism of rewards in CM interventions makes complete blinding almost impossible. As mentioned, we chose to reveal treatment information to treatment subjects (treatment subjects were told that they had been randomly selected to receive abstinence-contingent rewards in the programme sessions) but not to control subjects. This reduces the possibility of a John Henry, or resentment and demoralisation effect, but, the possibility for a Hawthorn effect remains.

While the above design choice was made to try reduce the risk of performance bias, we randomised at the level of the individual so we could not mitigate the problem entirely. In particular, we recruited individuals within a single university and randomised them into the treatment or control group, which means that subjects may coincidently know other participants in the study or subjects may have signed up together. Subjects may have therefore learned of the other treatment arms through other study participants and this knowledge, rather than treatment itself, may cause systematic differences between the outcomes of the treatment

and control groups.

Aware of this potential issue, I included questions in the fourth programme session's questionnaire, which asked subjects whether they knew other students taking part in the study and how many other students they knew who were taking part. Approximately 66% of subjects knew at least one other subject taking part in the study. A χ^2 test indicates that this percentage does not differ across treatment and control groups ($p = 0.573$). In addition, subjects knew 2 other study participants on average. A Mann-Whitney test indicates that this does not differ across treatment and control groups ($p = 0.\ 811$).

Table VII reports the results of a random-effects probit model of abstinence (Model VIIa) and a random-effects negative binomial model of smoking intensity (Model VIIb) with an additional variable to capture any effect that knowing other study participants may have: *Know others in the study*$_i$ is a binary variable equal to one if the subject knows at least one other study participant and equal to zero otherwise. The abstinence results in Model VIIa are qualitatively identical to those in Table IV and the *Know others in the study*$_i$ variable is not statistically significant, suggesting that the abstinence results reported in Table IV are robust to this potential bias. The intensity results in Model VIIb are qualitatively identical to those in Table VI, however, *Determination for current quit*$_i$ is no longer statistically significant. In addition, *Know others in the study*$_i$ is statistically significant and positive, suggesting that subjects who know other study participants have a higher smoking intensity on average. Thus, knowing other participants in the study appears to attenuate the "programme effect" on intensity.

TABLE VII
ROBUSTNESS: PERFORMANCE BIAS

	Model VIIa	**Model VIIb**
	RE Probit	RE Negative Binomial
Treatment	0.194***	0.134
	(0.058)	(0.500)
Age	0.024	-0.261*
	(0.017)	(0.149)
Male	0.035	0.713
	(0.062)	(0.609)
Coloured	0.082	0.691
	(0.064)	(0.703)
Indian	0.046	-0.713
	(0.078)	(0.721)
White	0.248***	0.121
	(0.080)	(0.766)
Income (ln)	-0.015	0.335
	(0.045)	(0.414)
Intervention period	0.144***	-6.233***
	(0.045)	(0.512)
3-month follow-up	0.016	-3.922***
	(0.017)	(0.588)
Smoking duration	-0.029	0.535***
	(0.021)	(0.165)
Smoking intensity	0.002	
	(0.005)	
FTCD score	0.016	0.710***
	(0.017)	(0.153)
MTWS-R score	-0.003	0.032
	(0.002)	(0.021)
Baseline CO reading	-0.003	0.027
	(0.002)	(0.030)
Quit attempt in past 5 years	0.128**	-0.509
	(0.052)	(0.591)
SASEQ score	0.006	0.039
	(0.007)	(0.056)
Importance of current quit attempt	0.015	-0.248
	(0.044)	(0.401)
Determination for current quit	0.013	-0.734
	(0.045)	(0.451)
RFQ intrinsic score	-0.015	0.028
	(0.023)	(0.176)
RFQ extrinsic score	0.010	-0.011
	(0.033)	(0.272)
Know others in the study	-0.046	1.051**
	(0.056)	(0.501)
N	414	345
log-likelihood	-96.232643	-808.36894

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
Notes: RE = Random effects
Standard errors in parentheses. SEs are robust in Model VIIa
Marginal effects reported

5.4.2 Attrition bias

Attrition occurs when outcomes cannot be measured for some study participants, which creates problems of missing data and which, in turn, reduces statistical power (Glennerster and Takavarasha 2013). The potential for attrition bias occurs when there is differential attrition - in particular, across treatment and control groups - which undermines the validity of the experiment. As mentioned, students are a particularly convenient sample because they are easy to recruit and easy to track. Nevertheless, additional design choices were made to reduce the risk of attrition bias. First, attendance was incentivized: show-up fees were provided in each session (R50 at baseline and in each of the programme sessions, and R200 in the follow-up sessions) and a R200 attendance bonus was provided in the final programme session if subjects had attended all four programme sessions. To ensure the salience of the attendance incentives, subjects were consistently reminded of the attendance incentives at the end of each session. Second, subjects were sent an email and two SMS reminders to remind them of each of their sessions and the attendant show-up fee.

While the above-mentioned design choices were made to reduce the risk of attrition bias, we randomised at the level of the individual which meant that subjects may coincidently know other participants in the study or subjects may have signed up together. This not only introduces the potential for performance bias, but may also lead to attrition bias: subjects in the control group who find out about the treatment group via other study participants may be more likely to drop out of the study (Glennerster and Takavarasha 2013).

This dissertation defines attrition as those subjects who dropped out of the study after the baseline session and did not arrive for another session. Subjects who missed one (or more than one) programme session, but arrived for a later session are not considered to have dropped out because smoking abstinence and smoking intensity outcomes could still be measured for those subjects; a RA completed the TLFB calendar with the subject for the period that they had missed.[85] In addition, subjects who dropped out after sign-up for the baseline session but before

[85] Abstinence over the "missed" period was not incentivised for treatment subjects because they could not provide a CO reading for the relevant period and therefore could not meet both abstinence conditions: self-reported abstinence for the past 7 days and a CO reading of 6 ppm or less. Thus, treatment subjects had no financial incentive to misreport their smoking abstinence or intensity for the "missed" period – see Section 6 for a discussion of deception.

the baseline session itself (i.e., the official start of the CM programme) are not considered to have dropped out.[86]

The overall attrition rate is approximately 15%. Specifically, 13 subjects dropped out of the study at different points: one subject dropped out after the baseline session, seven subjects dropped out after the first programme session,[87] one subject dropped out after the second programme session, and four subjects dropped out after the fourth programme session (i.e., they did not arrive for the 3-month follow-up session). Of the subjects who dropped out, 9 were control subjects and 4 were treatment subjects, which corresponds to 19% and 10% attrition rates, respectively. A χ^2 test reveals that the attrition rate is not statistically different across the treatment arms, indicating that there is no differential attrition.

Table VIII reports non-attendance/absenteeism proportions (i.e., the proportion of subjects that were absent) in each session, for treatment and control groups.[88] The results indicate that non-attendance proportions range from 2% to 20%. The difference in non-attendance proportions is not statistically significantly different for treatment and control groups, except in the fourth programme session.

TABLE VIII
NON-ATTENDANCE PROPORTIONS

	Control	Treatment	χ^2 test
Programme session 1	0.064	0.025	0.389
Programme session 2	0.213	0.100	0.154
Programme session 3	0.192	0.100	0.233
Programme session 4	0.192	0.025	0.015
3-month follow-up	0.192	0.100	0.233

When there are missing data in a RCT, a common sensitivity analysis is to explore "best" and "worst" case scenarios by replacing missing values with "good" outcomes in the one group and with "bad" outcomes in the other group (Glennerster and Takavarasha 2013; Sterne et al.

[86] As mentioned, 105 subjects signed up for the baseline session and were randomised into the treatment or control group, and 87 subjects arrived for the baseline session (i.e., 18 subjects, 12 treatment subjects and 6 control subjects, dropped out before the start of the programme; see Figure 2). Importantly, those subjects who dropped out before the baseline session had no information about treatment allocation. Drop out is, therefore, unrelated to treatment allocation and should not be considered a threat to the validity of the experiment.
[87] One subject formally withdrew from the study after the first programme session for health reasons unrelated to the CM programme.
[88] The 3-month follow-up non-attendance proportions are equivalent to the attrition rates.

2009).[89] In terms of the abstinence outcomes, a "best" case scenario would be if all absent treatment subjects were abstinent and all absent control subjects were not abstinent, and a "worst" case scenario would be if all absent treatment subjects were not abstinent and all absent control subjects were abstinent. As mentioned in Section 4, the abstinence analysis thus far has adopted the standard assumption that non-attenders, both treatment and control, are not abstinent. A "best" case analysis contributes little to the analysis: adopting the unlikely assumption that all absent treatment subjects are abstinent and all absent control subjects are not abstinent, the results simply confirm those found when the standard assumption is adopted. A "worst" case analysis is useful because it bounds the estimated results from below.

Table IX reports the results of a random-effects probit of abstinence in the "worst" case scenario. The results indicate that there is no statistically significant treatment effect. This suggests that the treatment effect may not be robust to differential reporting of abstinence. But what weight should we give to this lower-bound estimate? The TLFB data on those subjects who were absent for a session(s) but arrived for a later one, shows that only one person reported being abstinent on a week that they did not attend. This implies that adopting the standard assumption that subjects are not abstinent if they are absent is a more appropriate imputation.

[89] Such a sensitivity analysis is possible with the binary $Abstinence_{it}$ outcome variable because there is an obvious good and bad outcome (Sterne et al. 2009). In contrast, such a sensitivity analysis is not possible with the continuous $Smoking\ intensity_{it}$ outcome variable because there is no obvious good or bad outcome.

TABLE IX

ROBUSTNESS: Attrition bias

	Model 1
	Worst-case RE Probit
Treatment	0.098
	(0.063)
Age	0.024
	(0.021)
Male	0.035
	(0.081)
Coloured	0.029
	(0.075)
Indian	-0.013
	(0.064)
White	0.247**
	(0.102)
Income (ln)	0.004
	(0.054)
Intervention period	0.162***
	(0.047)
3-month follow-up	0.046
	(0.029)
Smoking duration	-0.025
	(0.022)
Smoking intensity	0.001
	(0.007)
FTCD score	0.003
	(0.022)
Baseline CO reading	-0.004
	(0.003)
Quit attempt in last 5 years	0.093
	(0.068)
SASEQ score	0.005
	(0.007)
Importance of current quit attempt	0.052
	(0.045)
Determination for current quit	-0.034
	(0.054)
RFQ intrinsic score	-0.029
	(0.024)
RFQ extrinsic score	0.044
	(0.038)
N	474
log-likelihood	-149.90762

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Robust standard errors in parentheses

Marginal effects reported

5.5 What did subjects think about their quit attempt?

Sections 5.2 and 5.3 report the results of a quantitative analysis of smoking abstinence and smoking intensity. This section aims to build on the quantitative analysis by looking at the qualitative data that were collected. The questionnaire in the final programme session asked subjects to list the reasons why they thought they had remained, or failed to remain, abstinent, and what approach/aid they had adopted to help them with their quit attempt. In addition, treatment subjects were asked whether they thought that the R150 abstinence-contingent incentive helped with their quit attempt and were asked to provide reasons for their answer. Albeit subjective, subjects' answers may provide insights into why some subjects remained abstinent, while others did not.

Regarding the reasons for a failed quit attempt, approximately 54% of subjects reported stress (academic and/or personal) and approximately 28% of subjects reported environmental factors/triggers (e.g., parties, alcohol, and friends or housemates who smoke) as reasons for smoking. Some less commonly identified reasons include a poor mindset, a concern for weight gain, boredom, a lack of psychological support, an inability to find a sufficient substitute, and a high baseline smoking intensity which was possible to reduce but not completely curb. Regarding the reasons for a successful quit attempt, subjects' responses varied considerably. The responses include: a good mindset, the use of substitutes (e.g., e-cigarettes, chewing gum, sweets), a desire to prove to that they could quit, health and financial reasons, social pressure and support, and reputation.[90]

Regarding the approach/aid used to help with the quit attempt, three main approaches were used: approximately 45% of subjects went "cold-turkey," approximately 27% of subjects used substitutes (e.g., e-cigarettes, NRT, marijuana, chocolates, hookah pipes, carrots, water), and approximately 22% of subjects reduced their smoking intensity as a step towards complete abstinence. Subjects cite several different techniques for reducing their smoking intensity, which include setting a daily limit and reducing it over time, sharing cigarettes with friends, rolling their own cigarettes rather than buying boxes, and buying single cigarettes rather than boxes. Several subjects also cite exercise and meditation as aids adopted to help with their quit attempt.

[90] One subject was studying medicine and noted that smoking damaged his reputation.

Regarding treatment subjects' responses to whether they thought that the R150 abstinence-contingent reward helped with their quit attempt, 65% of treatment subjects thought that the incentive helped.[91] Subjects cite several reasons for why the incentive helped, which include that it was a good size, it served as an additional source of motivation, it helped to keep them focused on their goal, and it helped with the initial phase of the quit attempt. Several subjects note that while the incentive helped keep them on track when they had "smaller" cravings, it could not help with more intense cravings. Of the subjects who thought the incentive had not helped, approximately 83% felt that the incentive was not large enough to outweigh the "power" of their cravings and their addiction. Other subjects cited that they were intrinsically motivated to quit smoking and felt that they did not need to be extrinsically motivated by means of a financial incentive.

The preceding qualitative analysis highlights that approximately 82% of subjects struggled to remain abstinent because of academic or personal stress, and environmental factors. Further, just under half of the subjects went "cold-turkey," while other subjects either used substitutes to help with their quit attempt or aimed to achieve abstinence by reducing their smoking intensity over time. Finally, while a relatively large number of subjects reported that they thought the abstinence-contingent incentives helped with their quit attempt, those subjects who thought it did not help cited that the incentive was not large enough to outweigh their cravings.

6. DISCUSSION AND CONCLUSION

This research investigated whether a low-cost, low-intensity CM smoking cessation programme could be an effective form of treatment for tobacco addiction in treatment-seeking university students. Specifically, this dissertation focused on two questions: Does a low-cost, low-intensity CM smoking cessation programme bring about abstinence from smoking and, if not, does it reduce the smoking intensity of non-abstainers?

With regard to the first question, the results in Section 5.2 indicate that there is a statistically significant treatment effect, that is robust across different models and multiple specifications,

[91] While 65% of treatment subjects thought that the incentive had helped with their quit attempt, clearly not all of those subjects managed to remain abstinent for the entire programme.

which increases the likelihood of abstinence by approximately 13-20%. However, there are noticeable declines in cessation rates at the 3-month follow-up across treatment and control groups. These findings suggest that a low-cost, low-intensity CM programme increases smoking cessation rates in treatment-seeking student smokers, but, cessation rates are not maintained beyond the intervention period. Importantly, these results replicate the findings in the literature: CM interventions are effective at increasing cessation rates but cessation rates tend to decline after the intervention period once the abstinence-contingent incentives are removed. It is interesting that we managed to replicate the standard result with a *low-cost, low-intensity* smoking cessation programme in a *university setting*, which points to the potential value and feasibility of conducting this type of intervention with this sample. But future research will want to investigate how the programme could be adjusted to help subjects achieve *continuous* abstinence, beyond the end of the intervention period.

The qualitative analysis in Section 5.5 suggests changes to the cessation programme that may prove to be effective in this regard. First, 82% of subjects reported struggling to remain abstinent because of academic or personal stress, and environmental factors. The CM programme was conducted during UCT's third term and did not continue into the fourth and final term which is considered the most stressful time of year for students due to an accumulating work load and end-of-year exams. Subjects' responses suggest that increasing the duration of the programme, so that it starts before their work load begins to accumulate and ends after the end-of-year exams, might help them to achieve continuous abstinence. In addition, an escalating incentive schedule (as opposed to the uniform schedule used in this study), in combination with the increased programme duration, may help maintain cessation rates: students would receive smaller incentives during the less stressful time of year and would receive larger incentives during the most stressful time of year. In principle, increasing the duration of the programme and implementing an escalating incentive schedule need not increase the cost or complexity of the CM programme: the "same" total available abstinence-contingent rewards could be allocated over a longer period with a different schedule of payments.

Second, of the subjects who thought the abstinence-contingent incentives had not helped with their quit attempt, approximately 83% felt that the incentive was not large enough to outweigh the "power" of their cravings and their addiction. This suggests that the incentive magnitude was not sufficiently large to compete with the reinforcing attributes of tobacco use and that

increasing the magnitude of the incentives might help those subjects overcome their cravings and establish continuous abstinence. The increase in the incentive magnitude need not be large because university students' incomes are still low relative to the general population. Future research could investigate what incentive magnitude would be sufficiently large to compete with the tobacco cravings of these "hard-to-treat" subjects. In addition, one could test whether cessation rates increase relative to those found in this programme and whether they are maintained.

With regard to the second research question (i.e., whether the CM programme decreased the smoking intensity of non-abstainers), the results in Section 5.3 indicate that the programme does not have a statistically significant effect on the smoking intensity of non-abstainers. However, the programme as a whole tends to reduce the smoking intensity of all non-abstinent subjects (i.e., both non-abstinent treatment and non-abstinent control subjects), although this effect, while still significant, appears to decline at the 3-month follow-up. This result should, however, be read with caution. Specifically, the effect may in part be driven by UCT's new smoking policy which was implemented shortly before the start of the smoking cessation programme and restricts where individuals can smoke on campus. When asked about the university's new smoking policy, almost all subjects (approximately 95%) knew about the policy change and 44% of subjects thought that it might help them with their quit attempt. χ^2 tests indicate that neither of these responses were significantly different across treatment and control groups ($p = 0.391$ and $p = 0.992$, respectively). While the policy change may have played a role in the decreased smoking intensity, the fact that the decrease was smaller in the follow-up session, compared to the intervention period, suggests that the weekly monitoring during the intervention period did play a role in decreasing intensity.

Section 5.4 addressed two potential sources of bias that may limit one's ability to draw valid and robust causal inferences – namely, performance bias and attrition bias - and analysed the robustness of the smoking abstinence and smoking intensity results in light of those issues. With regard to performance bias, randomisation at the level of the individual meant that a large number of subjects (approximately 66%) knew at least one other subject taking part in the study. Thus, subjects may have learnt of the other treatment arms through other study participants and this knowledge, rather than treatment itself, may cause systematic differences between the outcomes of the treatment and control groups.

As a robustness check, the regression models for smoking abstinence and intensity were re-run, including an additional binary variable indicating whether the subject knew at least one other study participant. The results indicate that knowing another participant has no observable effect on the abstinence results, but knowing another participant does appear to increase the smoking intensity of non-abstainers.

It would be interesting to conduct a social network analysis of the participants in the study because this may allow one to tease out the effect of knowing other study participants: Specifically, does knowing an abstinent participant increase one's likelihood of being abstinent? In addition, is there a relationship between knowing other people in the study and the smoking intensity of non-abstinent subjects? We asked subject for the names of the other participants that they knew, so such an analysis will be possible. Alternatively, future research may consider trying to harness social influences by explicitly "designing this into" the programme. For example, Halpern et al. (2015), who found a significant treatment effect that lasted into the follow-up, grouped subjects so that they not only received individual abstinence-contingent incentives, but also received additional rewards for each successful quitter in their group.

With regard to attrition bias, the study had an overall attrition rate of 15% and this rate did not differ significantly across treatment and control groups. As a robustness check, a worst-case random-effects probit was run, where all absent treatment subjects were assumed to be not abstinent and all absent control subjects were assumed to be abstinent. The results indicate that the abstinent treatment effect is no longer statistically significant, but this worst-case assumption is obviously a very strong one, particularly because the TLFB data on those subjects who did not attend their session shows that only one subject reported being abstinent.

An additional source of bias, not discussed in Section 5.4, is deception bias, which refers to the possibility of systematic differences in reports of abstinence amongst treatment and control subjects; treatment subjects may be tempted to misreport abstinence because of the contingent nature of the rewards. Biochemically verifying abstinence is important for mitigating such deception. For a subject to be classified as abstinent they had to meet two conditions: they had to report, in the TLFB section of the questionnaire, being abstinent in the previous 7 days *and* they had to have a CO reading of 6 ppm or less. Assessing the disparities between self-reports of abstinence and CO readings provides an estimate of the "rate of deception." Looking at the

data reveals that there were 8 instances where subjects self-reported being abstinent but had a CO reading greater than 6 ppm. This corresponds to a relatively low deception rate of 2.3%. These misreports occurred across 7 subjects (one had a mismatch twice), and all but one were treatment subjects. So, it is clear that there is a differential deception rate across the treatment and control groups, and this highlights the importance of biochemical verification.

Two further points warrant discussion. First, the qualitative analysis in Section 5.5 revealed that 27% of subjects used some form of substitute to help with their quit attempt. While the aim of the programme was to target cigarette smoking, it is useful to get a sense of whether, and how much, nicotine substitutes were consumed. TLFB data was collected on e-cigarette and NRT use. During the intervention period, three subjects (i.e., 3.5% of our sample) used nicotine replacement therapy; two were treatment subjects and one was a control subject. In addition, 31 subjects (approximately 36% of the sample) used e-cigarettes: 14 subjects in the control group and 17 subjects in the treatment group. Conditional on use, control subjects used e-cigarettes 3 days per week, on average, and treatment subjects used e-cigarettes 5 days per week, on average; this difference is not significantly different (p = 0.1196). So, while approximately one-quarter of the sample used substitutes, this did not differ across treatment and control groups and cannot, therefore, account for the abstinent treatment effect.

Second, study funding limited the number of subjects that could be recruited, and subject drop out between randomization and the baseline session led to a relatively small sample (n = 87). The sample size is comparable with and, actually larger than, a number of studies in the literature (Alessi and Petry 2014; Crowley et al. 1995; Ledgerwood and Petry 2010; Paxton 1980, 1981; Rand et al. 1989). Notwithstanding this issue, the study serves as a useful pilot for future research, but a larger sample would lend more credence to the results.

In sum, the study found that a low-cost, low-intensity CM smoking cessation programme, conducted on a sample of treatment-seeking university students, increased the likelihood of abstinence. While the abstinent treatment effect declines after the end of the intervention period, there are ways in which the programme could be adjusted to try to maintain cessation rates. To the extent that this proves successful, universities may want to consider offering CM programmes as part of the health care services that they provide to students because increasing student smoking cessation rates will reduce tobacco-related morbidity and thereby decrease the burden on universities' health care services.

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APPENDIX

TABLE A

TREATMENT				
		Programme session 1	**Programme session 2 & 3**	**Programme session 4**
TODAY: Abstinent	**PREVIOUSLY: Abstinent**	*Note: both previous abstinence & attendance = NA* **ABSTINENCE** • For today's session you receive the R150 payment because you were abstinent in the past week • Well done for remaining abstinent! • Because the R150 payment is for abstinence, you only receive it because you were abstinent in the past week • It's great that you were abstinent in the past week, and the goal is to remain abstinent going forward. If, when you come in next week, we find that you are abstinent for the week prior, you will receive R150 payment for abstinence again • Remember that if you are struggling with your quit attempt, you can refer to the quit guide we provided you with (it is also on the Vula site) **ATTENDANCE** • You also receive the R50 show-up fee just for attending today's session • It's great, and we really appreciate, that you attended today's session • Because the show-up fee is for simply attending the session, you would obviously have received this, even if you weren't abstinent in the past week • It's important that you keep attending all your sessions, irrespective of whether you are abstinent in the previous week, because we don't only care about whether you were abstinent, we also want to know, if you weren't abstinent, when and how much you have been smoking in the previous week • That's why, like with today's session, you will receive the R50 show-up fee at each session, irrespective of whether you remained abstinent in the previous week • Also remember that if you keep attending all sessions this term, you will receive a R200 bonus in the last session this term, irrespective of whether you remained abstinent throughout the term • Here is your payment for today. That R200 in total; the R150 (for abstinence in the past week) and the R50 show-up fee. Please can you check the amount and sign the payment receipt. • Thanks for coming to today's session. We will see you at the same time next week. SUBJECT LEAVES.	**ABSTINENCE** • For today's session you receive the R150 payment because you were abstinent in the past week • Well done for remaining abstinent! • Because the R150 payment is for abstinence, you only receive it because you were abstinent in the past week • It's great that you were abstinent in the past week AND the week before that. The goal is to remain abstinent going forward. 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TABLE B

ROBUSTNESS: ALTERNATIVE MODELS

	Model B:I Pooled Probit	**Model B:II** RE Poisson	**Model B:III** Pooled Negative Binomial
Treatment	0.217***	0.388	0.428
	(0.047)	(0.490)	(0.533)
Age	0.032**	-0.300**	-0.337**
	(0.015)	(0.151)	(0.149)
Male	0.017	1.018*	1.319**
	(0.069)	(0.576)	(0.536)
Coloured	0.082	0.864	1.136*
	(0.062)	(0.670)	(0.674)
Indian	0.052	-0.327	-0.231
	(0.070)	(0.703)	(0.687)
White	0.239***	0.495	0.107
	(0.069)	(0.713)	(0.690)
Income (ln)	-0.05	0.437	0.381
	(0.043)	(0.407)	(0.432)
Intervention period		-6.318***	-6.515***
		(0.444)	(0.657)
3-month follow-up		-3.982***	-4.109***
		(0.499)	(0.706)
Smoking duration	-0.031	0.546***	0.582***
	(0.019)	(0.167)	(0.194)
Smoking intensity	0.002		
	(0.005)		
FTCD score	0.017	0.783***	0.819***
	(0.016)	(0.150)	(0.168)
MTWS-R score	-0.001	0.028	0.034
	(0.002)	(0.019)	(0.024)
Baseline CO reading	-0.003	0.032	0.046
	(0.002)	(0.031)	(0.038)
Quit attempt in past 5 years	0.124**	-0.659	-0.406
	(0.051)	(0.578)	(0.713)
SASEQ score	0.007	0.032	0.011
	(0.007)	(0.054)	(0.059)
Importance of current quit attempt	0.006	-0.309	-0.48
	(0.037)	(0.388)	(0.407)
Determination for current quit	0.013	-0.762*	-0.856*
	(0.038)	(0.445)	(0.444)
RFQ intrinsic score	-0.019	0.172	0.09
	(0.019)	(0.172)	(0.190)
RFQ extrinsic score	0.021	-0.129	0.187
	(0.029)	(0.263)	(0.275)
N	430	361	361
log-likelihood	-147.46634	-854.11868	-851.50556

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Standard errors in parentheses. SEs are clustered in Model B:I and B:III

Marginal effects reported