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Put a Bet on It: Can Self-Funded Commitment Contracts Curb Fitness Procrastination? *

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April 11, 2023

Abstract

This paper investigates the use of self-funded commitment contracts to support individuals in achieving their goals of increased physical activity. We compare the effect of soft (non-incentivised) commitment contracts with hard (incentivised) contracts using a randomised experiment with 1629 members of a large gym in Stockholm, Sweden. We find a significant positive impact of being offered a hard contract on monthly visits to the gym and the probability of meeting one's contract goal. Hard and soft commitment contracts increase gym visits by 21% and 8%, respectively, relative to the control group. Trait self-control is negatively associated with the likelihood of accepting a contract and accepting to add stakes. The effect of soft and hard commitment contracts does not, however, differ by trait self-control. Importantly, we find that the effects of both hard and soft contracts were greatest among participants who reported exercising the least at baseline. Our experiment explores the use of a completely self-funded and highly scalable intervention. Our experimental design allows us to shed light on the importance of hard penalties in designing commitment contracts.

Keywords: Gym attendance, Incentives, Commitment contract, Self-control

JEL Codes: I12, D03, C93

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1 Introduction

Walk one square kilometre in any mid-size city and you will likely spot more than one fitness centre. Survey your colleagues about gym membership and the majority probably have one. Ask a random commuter whether they engaged in moderately intense physical activity for at least 2.5 hours in the past week; chances are they have not. The World Health Organisation (WHO) recommends that adults aged 18 to 64 years engage in moderately intense physical activity for a minimum of 150 to 300 minutes per week, or approximately half that time if the activity is vigorous (WHO, n.d.). Unfortunately, only about 25% of adults around the world meet these recommendations. Even amongst gym-goers, many report not visiting the gym as frequently as they would like to (Gerdtham *et al.*, 2020). This lack of physical activity carries important health and economic consequences related to, for instance, overweight and obesity (OECD, 2019), human capital formation (Cappelen *et al.*, 2017; Fricke *et al.*, 2018) and memory (Erickson *et al.*, 2011; Roig *et al.*, 2013). An important challenge facing both individuals and policy-makers is thus *how* to get individuals to be more active. Identifying interventions that successfully increase physical activity and create lasting behaviour change is important. Understanding the demand for those interventions, how they should be structured and who benefits most is also critical.

Most individuals know that it is important to be physically active and many would like to be more active, but struggle to put ambition into practice (Carrera *et al.*, 2018; Gerdtham *et al.*, 2020). Commitment contracts can help address this problem of low self-control. They are effective in encouraging individuals to be physically active (Royer *et al.*, 2015), stay sober (Schilbach, 2019), and achieve their academic goals (Himmeler *et al.*, 2019), among other applications. Commitment contracts involve individuals making a pledge with themselves to achieve a particular goal. They reduce their choice-set or impose penalties on themselves for not reaching that goal (Carrera *et al.*, 2022). The self-imposed penalties might be soft (an individual must live with the disappointment of not having met the contract terms) or hard (the individual has attached stakes to the pledge and forfeits them if the contract terms are not met). Whilst commitment contracts are effective in many domains, it is not yet clear to what extent the financial stakes or the contract itself are driving the behaviour change. Moreover, it has recently been brought to light that commitment contracts may not always be welfare improving (Bai *et al.*, 2017; Carrera *et al.*, 2022; John, 2020). In light of this, developing our understanding of who demands commitment, and the extent to which hard versus soft commitment contracts are effective, is important.

In this study, we investigate the use of self-funded commitment contracts to support individuals in achieving their physical activity goals. We designed a field experiment to explore the effect of commitment contracts (contracts and commitment contracts used interchangeably hereafter) on physical activity, and the extent to which the effect of the contract differs by stakes being attached or not.

We recruited study participants from a population of gym-goers at a large gym chain in Stockholm, Sweden and randomly assigned 1629 participants to either a control group (no contract) or to an offer of a soft or hard contract. Inspired by Bhattacharya *et al.* (2015)'s theoretical finding that nudging towards increased physical activity may not always be welfare improving, we offered all participants the opportunity to tailor their contracts, in terms of length and intensity, to their needs and exercise ambitions. All participants offered a contract first designed it in terms of targeted monthly gym visits and length. After designing the contract, individuals randomised to the hard contract group were offered to add stakes. Those who accepted were free to select an amount greater than zero to put at stake. We followed up participants' training at the gym through records of gym visits, and can thus objectively evaluate the effect of these contract offers on visits to the gym. We also investigate the effect of adding stakes on the likelihood of reaching one's contract goal.

Importantly, the study sample was recruited through a gym. Assuming these individuals had already made a commitment with themselves to be physically active, our study can be seen as relating to the intensive margin of behaviour. Rather than inciting individuals to *become* physically active, we investigate the use of commitment contracts to support these individuals in achieving their goals. We also collect extensive information on individual socio-demographic characteristics, previous exercise behaviours, time preferences and self-control, and can thus shed light on who accepts to sign a contract, and who such contracts benefit the most.

Overall, over 82% of participants in both treatment groups accepted a contract (soft or hard), indicating large demand for commitment contracts in our study population. Within the group offered a hard contract, 41% accepted to add stakes when offered the opportunity to do so. We find that the offer of a hard contract increases visits to the gym by approximately 21% compared to the control group ($p < 0.001$), while the offer of a *soft* commitment contract increases visits by approximately 8% ($p = 0.085$). We find that the probability that those offered a hard contract meet their contract goal is approximately 20%, which is about 5% higher ($p = 0.065$) than the probability among those offered a soft contract. Interestingly, whilst we do find evidence that take-up of contracts is higher amongst those with lower self-control, we find no evidence that the effects of soft and hard contracts differ by trait self-control. Overall, the results provide evidence that, even among a population of current gym-goers, commitment contracts can work in the field to support individuals in increasing their physical activity and meeting their goals. They do not, however, appear to differentially help individuals with low self-control.

This paper is the first, to our knowledge, to compare the effectiveness of both soft and hard commitment contracts experimentally, in the context of physical activity.¹ The randomised nature of the assignment to either a soft or hard contract allows us to investigate the impact of financial stakes on gym visits and the likelihood of meeting one's contract goal. The paper thus contributes to the literature on the role of commitment contracts in increasing (or maintaining) physical activity (Bhattacharya *et al.*, 2015; Carrera *et al.*, 2022; Royer *et al.*, 2015), and to the literature on the use of commitment contracts more broadly. Recent findings by Himmeler *et al.* (2019) indicate that soft commitment contracts can have a positive impact on academic outcomes, while findings by John (2020), Carrera *et al.* (2022) and Bai *et al.* (2017) show that hard commitment contracts may be welfare reducing due to the fines imposed on those who do not reach their goals. Therefore, it is crucial to understand the role of financial stakes in the success of commitment contracts. This is because positive outcomes that can be achieved without stakes presumably carry lower welfare costs. We show that the hard penalty is important, but detectable behaviour change can be achieved through soft contracts.

Another aspect in which our paper stands out is the use of *self-funded* commitment contracts. Previous studies on commitment contracts and physical activity, with the exception of Bhattacharya *et al.* (2015), have featured house money or experimental earnings at stake (Carrera *et al.*, 2022; Royer *et al.*, 2015). This is potentially important for two reasons. First, individuals may behave differently when making decisions about their own money versus other people's money and/or easily earned money (see e.g. Cárdenas *et al.* (2014)). Second, if we assume that individuals are loss averse and attach greater disutility to the potential loss of their stakes than they do to the utility of winning less from the experiment, this could impact their behaviour in response to the commitment contract (Kahneman & Tversky, 2013). Our experiment differs in that the money participants put at stake was their own money. We confirm that

¹Bhattacharya *et al.* (2015) have information on individuals who choose a financial penalty of zero dollars, but such individuals are self-selected, unlike in our setting where individuals were randomised to an offer of a soft or hard contract. Importantly, if participants in our hard contract group accepted the offer to add stakes, they were restricted to choosing a strictly positive amount.

the findings of previous studies can be replicated when own money is at stake, which is useful because the costs associated with the intervention are minimal and it is thus highly scalable.

Our study adds new evidence to the understanding of who is responsive to physical activity commitment contracts. We find that commitment contracts can be effective in increasing physical activity even among current gym goers, who may be generally healthier and already prioritise physical activity more than the general population.

We also contribute to the discussion of the relationship between commitment contract take-up and measures of impatience, time-inconsistency and self-control. Improving our understanding of who demands commitment and who benefits the most is particularly important in light of recent literature highlighting that commitment contracts are not always welfare improving (Bai *et al.*, 2017; Carrera *et al.*, 2022; John, 2020). The previous literature on the relationship between time-inconsistency and demand for commitment contracts has found mixed evidence. Some report a positive relation (Augenblick *et al.*, 2015; Kaur *et al.*, 2015), while others have found weak (Ashraf *et al.*, 2006) or even negative associations between commitment contract take up and time inconsistency (Sadoff *et al.*, 2020). In addition to time inconsistency, we also employ a psychological measure of self-control which we find to be negatively related to contract take-up. In contrast, we do not observe any meaningful relationships between commitment and patience or time inconsistency.

The remainder of the paper is set out as follows: in section 2 we detail the experimental design and set out how we seek to identify the impact of soft and hard commitment contracts on physical activity. In section 3 we describe the empirical strategy and how we investigate the effects of the contracts both during and after the contract is complete. In section 4 we present results on the effects of commitment contracts on gym attendance and habit formation, as well as predictors of the demand for- and design of- contracts in our experiment. We additionally investigate potential mechanisms through exploring heterogeneity in the effects. In section 5, we explore whether our results are sensitive to alternative specifications and using an alternative sample. In section 6 we conclude and discuss the implications of our findings.

2 Experimental design

2.1 Setting

The experiment was conducted between fall 2018 and spring 2019 at a large gym chain in Stockholm, Sweden. Individuals training at any of the gym’s 17 locations were eligible to participate. The study was pre-registered with the American Economics Association RCT Registry under trial number AEARCTR-0001966.²

2.2 Recruitment and treatment assignment

An invitation to participate was sent out in the gym’s monthly newsletter in both August and September 2018. The experiment was also advertised on television screens in the gyms³. The invitation included

²The initial registration specified that this study would be carried out in collaboration with the Uppsala, Sweden branch of the Friskis & Svettis gym. Unfortunately, we were only able to recruit 184 individuals in Uppsala, a far cry from the required sample size of 600 to 2000 individuals (as specified in the trial pre-registration). We thus initiated a collaboration with the larger Stockholm branch of the Friskis & Svettis gym instead, where we were able to recruit 1629 participants, in line with our pre-registered planned sample size of 600-2000 individuals. Descriptives and results for the Uppsala sample are presented in Appendix E.

³Due to the nature of our recruitment strategy, it is likely that individuals self-selected into the study. Whilst this may be perceived as a threat to the external validity of the study, internal validity should remain intact as individuals were randomised to

a link to a baseline survey, which contained questions about background characteristics such as age, sex, country of birth, educational attainment and income. It also contained questions regarding current training status and fitness goals. Additionally, it included a series of questions to elucidate trait self-control and time preferences such as impatience and time consistency. Importantly, in the consent form, participants were notified that some – but not all – would be offered the opportunity to sign a training contract with themselves; and some of those participants would moreover be offered the opportunity to "bet on themselves" by adding financial stakes to their contract. A copy of the invitation (in Swedish) is presented in Appendix F. A copy of the baseline survey instrument (also in Swedish) is presented in Appendix G.

Upon completion of the baseline questionnaire⁴, each participant was randomised to either the control group (no contract) or one of two treatment groups: a soft (contract only) or a hard (option to add stakes) contract group. The control group was not offered a contract, so individuals randomised to this group were simply thanked for their participation and told they would receive a link to a follow-up questionnaire in approximately five months. Both treatment groups were offered the opportunity to sign up for and design a contract with themselves to train a certain amount over a specified period of time. Participants were allowed to choose the length of the contract (minimum one month, maximum four) and the amount they committed to train per month (4, 8, 12, 16, 20 or 24 or more times). After setting up their contract, participants randomised to the soft contract group were thanked for their participation and told that they would receive a follow-up questionnaire in five months' time. They were reminded of the contract terms and that the contract started on the first day of the following month. Individuals randomised to the hard contract group were instead offered the opportunity to add financial stakes to their contract, which would be forfeited to a charity of their choice if they failed to meet their contract terms. Upon accepting this offer, participants chose a positive (greater than zero) monetary amount they wanted to put at stake and selected the charity to which their money would be donated if they were not successful. They were then asked to make a mobile transfer for the amount put at stake to a third-party organisation handling in- and out-going payments for the study. As with the soft contract group, participants were then thanked for their participation, reminded of the contract terms, and told they would receive a follow-up survey in about five months' time. Individuals randomised to a hard contract offer could of course accept the contract but turn down the offer to add stakes. A flow diagram is presented in Figure 1 and a timeline of the experiment is presented in Figure 2.

The set of all individuals who signed up for a contract received an email on the first day of their contract period reminding them their contract was starting. They received no subsequent reminders. They also received an email on the day after their contract ended to remind them that their contract was over. We followed up individually with those participants who signed up for hard contracts, after confirming with the gym to check whether they had successfully completed their contract or not. We did not follow up individually with the participants who were offered a soft contract only, or who turned down the offer to add stakes.

All participants received an invitation to complete the follow-up questionnaire in the fifth month after completing the baseline survey. September starters received the invitation to the follow-up survey in early January 2019 and October starters received the invitation in early February 2019. As a thank-you for completing the follow-up survey, all participants could choose to be sent a scratch card in the post or to donate 30 Swedish Kronor (SEK; the cost of the scratch card) to a charity of their choice.

each treatment group after completing all questions of the baseline questionnaire.

⁴Note that responding to each question was not mandatory. A participant can thus have completed these questions and been randomised without having provided an answer to all questions.

2.3 Visits to the gym

We obtain data on all visits to the gym made by participants between September 2018 and May 2019. We define an individual's first four complete calendar months of participation in the study as the "experimental" period. We term the subsequent four months, after receipt of the invitation to the follow-up questionnaire, as the "post-experimental" period. We keep eight months of observations for all participants. Dividing the study period in this way allows us to evaluate persistence in the effects of soft and hard contracts. Anybody who is not observed in the data in a given month (i.e. does not visit the gym) is coded as having made zero visits.

Participants entered the study by completing the baseline questionnaire in either August or September 2018. The experimental period for those who completed the baseline questionnaire in August thus begins September 1, 2018, while the experimental period for those who completed the baseline questionnaire in September begins October 1, 2018. Because, when obtaining consent, we only requested access to participants' gym visit data from September 2018 onwards, we only have pre-experimental data for the group entering the study after September 1. We use this data to investigate any systematic differences in pre-study gym visits across the treatment and control groups.

Our main outcome of interest is monthly visits to the gym. We additionally investigate the probability of achieving the weekly goal number of gym visits stated in the baseline questionnaire,⁵ and the probability of meeting one's contract target (conditional on accepting a contract).

2.4 The role of time preferences and self-control

Information on trait self-control, present bias and impatience was collected in the baseline survey. Self-control was measured according to Tangney *et al.* (2004)'s measure of trait self-control. The method involves posing participants a series of 13 questions, which respondents rate as applying to them on a scale from one to five. The resulting scores are then usually simply summed together to give a total score for self-control ranging from 13 to 65, where a higher value indicates higher self-control. We follow Cobb-Clark *et al.* (2019) and construct a standardised measure of trait self-control for use in the analyses, by first standardising responses to each question to have mean zero and standard deviation one. We then sum these scores and standardise the sum to get our standardised measure of trait self-control. This method helps to account for potential differences in response behaviour across questions (Cobb-Clark *et al.*, 2019).

Time preferences were measured using two sets of Multiple Price List (MPL) tasks (see Appendix G). In the first set, respondents were asked a series of four questions about receiving a hypothetical amount of money today versus another amount in one month. The amounts compared in the second set of tasks were the same as those in the first set. The difference between the second set and the first set was the time frame, where the decision was between receiving an amount in one year from today versus one year and one month from today. We measure impatience using responses to the first set of questions, where a later switch from selecting the amount today to selecting the larger amount in one month indicates greater impatience. Present bias and future bias are measured by comparing responses to the first and second sets of questions. Present-biased individuals are more patient when making decisions about the future (i.e. switch earlier to preferring the larger future amount in the second set of MPL tasks), whereas future-biased individuals are the opposite. Time consistent individuals exhibit the same

⁵All participants were asked about their goal number of weekly visits in the baseline questionnaire, prior to their treatment group being made salient. We multiply this amount by four to obtain an estimate of participants' monthly goal. Investigating the effect of commitment contracts on the likelihood of achieving this goal allows us to abstract from potential biases introduced due to unobserved factors that went into the decision about which contract target to choose.

amount of patience when making decisions about the present or the future.

In the main analyses, we focus on self-control, but do investigate the role of time preferences in influencing demand for commitment contracts.

2.5 Descriptive statistics

Descriptive statistics for participants who responded to the baseline survey are presented in Table 1, Panel A. Responses to health, exercise and preference-related questions in the follow-up survey are presented in Panel B. Balance tests were conducted by regressing each study characteristic on an indicator for each treatment group. P -values for a test of joint equality of the coefficients on each treatment group are shown in the final column. Generally, it appears that the randomisation generated balanced treatment groups. There is weak evidence ($p = 0.082$, Wald test) of an imbalance in the proportion of males across treatment groups and in the proportion of individuals reporting good or very good health ($p = 0.080$, Wald test). For both of these characteristics, the proportion is highest in the hard contract group. Overall, 77% of participants were female. Across all treatment groups, approximately 50% of respondents train at least three times per week, and between 14% and 17% of respondents train three or more times per week someplace other than the gym. Across all groups, approximately 90% of respondents report that they would like to train more than twice per week, and 62% to 64% say they aim to train more than twice per week *at the gym*. Mean (unstandardised) trait self-control was approximately 45 across all treatment groups, which is generally in line with previous studies that have used this measure (see e.g. Tangney *et al.* (2004), Schmeichel & Zell (2007) and Gerdtham *et al.* (2020)).

Overall, response rates to the follow-up survey were high and broadly comparable, though we do find evidence that they differ significantly across treatment groups ($p = 0.005$). The response rate was higher in the control group (90.5%) than the soft (85.5%) and hard contract (83.8%) groups. As in Panel A, the presented p -values are for a test of joint equality of the coefficients on each treatment group. We reject the null hypothesis that the proportion reporting good or very good health is the same across the three groups at follow-up ($p = 0.022$, Wald test), and the null hypothesis that the proportion experiencing daily stress is the same ($p = 0.036$, Wald test). We additionally reject the null hypothesis that the proportion training someplace *other than* the gym more than two times per week is the same across groups ($p = 0.047$, Wald test), even though approximately 55% of respondents in all groups report training more than twice per week (both at the gym and other places).

3 Empirical strategy

3.1 The effect of commitment contracts on exercise behaviour

In our main analyses, we investigate the effect of being offered a soft or hard contract on exercise behaviour, measured using gym visits, during the experimental period (months 1-4 after completing the baseline survey). Our main outcome of interest is monthly visits to the gym. We also investigate the participants' probability of achieving their goal stated in the baseline questionnaire and the probability of meeting their contract target, conditional on accepting a contract. We are most interested in the intent-to-treat (ITT) effect, that is the effect of a contract offer, as this is the effect of most policy relevance in our case. Our regression model is as follows:

$$Y_i = \alpha + \delta_1 T_{1i} + \delta_2 T_{2i} + \lambda_m + \epsilon_i \quad (1)$$

where Y_i denotes the dependent variable of interest (monthly gym visits, meeting one's stated baseline monthly goal, successfully completing one's contract), treatment groups are denoted by T_1 and T_2 , and λ_m is a vector of year-month fixed effects included to account for seasonality in visits to the gym. The effects of being offered either a soft (T_1) or hard (T_2) contract are denoted by δ_1 and δ_2 , respectively.

We investigate persistence in the effect of the commitment contract in two ways: i) estimating equation (1) for our three outcomes during the post-experimental period (months five to eight, i.e. the four months following receipt of the follow-up survey), ii) estimating the change in the outcome between the experimental and post-experimental periods using the following specification:

$$Y_i = \alpha + \delta_1 T_{1i} + \delta_2 T_{2i} + \beta * post_i + \theta_1 T_{1i} * post_i + \theta_2 T_{2i} * post_i + \lambda_m + \epsilon_i \quad (2)$$

where $post$ is an indicator equal to one in months five to eight, and the coefficients θ_1 and θ_2 provide estimates of the change in each outcome between the experimental and post-experimental periods. In this analysis, we include eight months of observations for all individuals.

In our main analyses, we estimate equation (1) without controls. In additional analyses we estimate equation (1) with controls measured in the baseline survey. In these additional analyses, we follow Jones *et al.* (2019) and use the Lasso double selection method of Chernozhukov *et al.* (2015) to select a set of controls. This method, described in Urminsky *et al.* (2016) uses Lasso regressions to select controls that are predictive of either the i) dependent variable or ii) treatment variable. As detailed in Jones *et al.* (2019), if the treatment is randomly assigned, the control variables should not be predictive of it in expectation. As such, the control variables we select are those that are identified as predictive of the dependent variable only. Including control variables in this way helps to improve the precision of our estimates.

In robustness checks, we investigate whether our results are sensitive to the use of a Poisson model when the outcome is monthly gym visits and a probit model when the outcomes are the probabilities of meeting one's contract target and achieving one's baseline monthly goal.

4 Results

This result section contains four parts. We start by studying the determinants of contract take-up. Thereafter, we investigate treatment effects on gym visits and contract completion, followed by an analysis of heterogeneous treatment effects. In the final subsection, we look at how participants construct their contracts in terms of the stakes involved, the time frame, and the intensity of training.

4.1 Contract take-up

In total, 1,105 participants were offered a commitment contract. Contract take-up was over 82% in both the soft and hard contract groups, with 487 individuals in the soft contract group and 439 in the hard contract group accepting a contract (see Table 1).⁶ Of the 439 randomised to the hard contract group who accepted the offer of a contract, 179 accepted to add stakes.

In Table 2 we explore associations between self-control and other individual characteristics collected in the baseline survey, contract take-up and deciding to add stakes (conditional on accepting a contract). Self-control is an important negative predictor of both accepting a contract and accepting to add stakes. Investigated on its own (columns 1 and 5), a one standard deviation increase in trait self-control is

⁶Note that randomisation to the different treatment groups happened prior to individuals being offered a contract. Treatment group was thus determined prior to individuals accepting the contract offer.

associated with a decrease in the probability of accepting a contract of 5.1 percentage points ($p < 0.001$), and a decrease in accepting to add stakes of 4.7 percentage points ($p = 0.056$).⁷ When we account for time inconsistency (present and future bias), impatience and demographic characteristics (columns 2, 3, 6 and 7), the point estimate on the effect of self-control on accepting a contract decreases somewhat in magnitude, though the point estimate on accepting to add stakes does not. In addition to self-control, one additional year of age is associated with a 0.36 percentage point reduction in the probability of accepting a contract ($p < 0.001$) but is not significantly associated with the probability of adding stakes. While being male is not a significant predictor of accepting a contract, when offered the opportunity males are 13.4 percentage points more likely to add stakes ($p = 0.024$).

In columns 4 and 8, we include variables that might be endogenous to self-control and the point estimate on self-control decreases by approximately half. Of these potentially endogenous variables, the most important predictors of both contract take-up and accepting to add stakes are reporting training less than two times per week in the baseline survey⁸ and being overweight or obese. Those who train less than two times per week at baseline are 11.8 percentage points more likely to accept a contract ($p < 0.001$) and 18.1 percentage points more likely to accept to add stakes ($p = 0.003$), while those who are overweight or obese are 6.3 percentage points more likely to accept a contract ($p = 0.011$) and 12.5 percentage points more likely to add stakes ($p = 0.031$).

4.2 Gym visits and successful contract completion

We plot trends in mean monthly visits, and the probability of achieving one's baseline monthly goal, by treatment group, in Figure 3. We observe an overall downward trend in all three outcomes between September 2018 and May 2019, and clear monthly patterns. Individuals in all groups train more in October, less in December and increase their training again in January. The hard contract group trains more, on average, than the soft contract and control groups, and there is no overlap of the 95% confidence intervals on all three outcomes among the control (no contract) and hard contract groups between September and November 2018 (December 2018 for the probability of achieving one's baseline goal).⁹

Table 3 Panel A shows ITT effects of being offered a hard contract on monthly visits to the gym (columns 1-3), and the probabilities of achieving one's baseline monthly goal (columns 4-6),¹⁰ and meeting one's contract target (column 7, conditional on accepting a contract). Estimates are presented separately for the first four months of the study (experimental period) and the last four months of the study (post-experimental period). Most participants randomised to a contract offer selected a four month contract ($n=736$), so the post-experimental period corresponds to their post-contract period. We investigate persistence in the effect of being offered a soft or a hard contract by estimating equation 2. The estimates presented in columns 3 and 6 correspond to the coefficients θ_1 and θ_2 in equation 2.

⁷In Appendix Figure A.1 we additionally plot the distribution of trait self-control by whether or not participants accepted or rejected the offer of a contract, and accepted or rejected the offer to add stakes. Self-control is lower among those who accept a contract (Mann-Whitney U test: $p < 0.001$) and accept to add stakes (Mann-Whitney U test: $p = 0.0642$).

⁸Previous studies have found that incentives to exercise benefit those who exercised the least at baseline (Charness & Gneezy, 2009). It is thus interesting to investigate the extent to which patterns of exercise prior to entering the study are correlated with contract take-up. In Appendix Figure B.3.1 we provide additional graphical evidence that it is the group of individuals who reported training 0-1 times per week prior to entry into the study who are most likely to accept a commitment contract and accept to add stakes. This contrasts with the finding of Royer *et al.* (2015) that gym members with above-median visit frequency prior to the study were actually more likely to make commitment contracts, though in their case the offer of a commitment contract followed an initial incentive treatment.

⁹The convergence of trends from January 2019 onwards is in line with the fact that the vast majority of participants who accepted a contract completed their contract on or before December 31st 2018 (718 of 926 participants).

¹⁰Since all participants (regardless of treatment group) were presented a question on how often they would like to train at the gym, we can investigate the effect of either a soft or hard contract offer on the likelihood of achieving that goal, regardless of whether a contract was actually agreed to.

The estimates presented in Table 3 Panel A indicate that, during the experimental period, participants offered a soft or hard contract visited the gym significantly more than individuals in the control group. Individuals offered a hard contract visited the gym 1.14 times per month more, on average, than the control group ($p < 0.001$, column 1). This is an increase in monthly gym visits of approximately 20% relative to the control group. Individuals offered a soft contract visited the gym 0.46 times more per month on average ($p = 0.085$, column 1), an increase of approximately 8%, though this effect is no longer significant when controls are included (see Appendix Table C.1.1). Individuals offered a soft or hard contract also had a higher probability of achieving their baseline goal in a given month. Individuals offered a hard contract were nearly twice as likely to achieve their baseline goal compared to individuals in the control group (probability of 20.4% versus 10.7%), while individuals offered a soft commitment contract were about 35% more likely to achieve that goal (probability of 14.4% versus 10.7%).¹¹ Results for specifications including controls selected according to the Lasso double-selection method of Chernozhukov *et al.* (2015) are presented in Appendix Table C.1.1.

The results presented in Table 3 columns 2 and 5 suggest some persistence in the effects of being offered a hard contract. In the post-experimental period, individuals offered a hard contract continue to train a marginally significant amount more than those in the control group ($p = 0.082$), and are marginally significantly more likely to achieve their baseline goals ($p = 0.071$). Approximately 41% of the increase in gym visits remains in the post-experimental period for those offered a hard contract, as well as approximately 27% of the increase in the probability of achieving one's baseline goal. While the point estimates on both gym visits and the probability of achieving one's baseline goal are positive in the post-experimental period for the soft contract group, they are not statistically distinguishable from the estimates for the control group. In Appendix D.1, we present estimates for the effect of hard and soft contracts for each of the four months following contract completion. The size of the point estimates suggests that the effect of the contracts decreases with increasing time since contract end.

In Table 3 column 7, we restrict our analyses to the subset of individuals who accepted to sign a contract. We find that 15% of individuals offered a soft contract meet their contract target, while 19.6% of individuals offered a hard contract meet theirs. The difference is significant at the 10% level ($p = 0.065$).

4.2.1 Implied treatment on the treated effects

Individuals could turn down the offer of a commitment contract or, conditional on accepting a contract, the offer to add stakes. In Table 3 Panel B, we present estimates of the local average treatment effect (LATE) obtained by instrumenting actual contract take-up (potentially endogenous) with the offer of a contract (exogenous). Given that 85% of participants in the soft contract group accepted the offer of a contract (see Table 1), we do not expect the estimates for the soft contract group to differ considerably from the ITT estimates. Take-up of a contract was 82.5% among the hard contract group (Table 1). Of those who accepted a contract and were offered to add stakes, 40.8% accepted (see Table 2). The effect of accepting a hard contract on each of our outcomes is thus likely larger than the estimates ITT effects suggest. Indeed the LATEs of a hard contract on monthly visits to the gym and meeting one's monthly goal during the experimental period, and meeting one's contract target, are considerably larger than the ITT effects. Of note, the control group means in panel B are generally larger than those in Panel A. This could suggest that the respective treatment offers (our instruments) affected participants' behaviour

¹¹Investigating the effect on the baseline stated goal is interesting because some participants might have selected a contract target that was higher or lower than their actual goal because of, for instance, expectations about their future behaviour or wanting to challenge themselves. All participants were, moreover, asked about their baseline goal, so we are not limited to the set of individuals who accepted a contract, as we are when investigating the likelihood of meeting one's contract target (column 7).

directly, violating the exclusion restriction. Alternatively, it could be that those who turned down the contract offer (included in the control group in columns 1-6) or the offer to add stakes (included in the control group in column 7) had additional information about themselves, i.e. that they did not need a contract or to add stakes to achieve their aims (in other words, they have the least self-control problems). Whilst this means that it is challenging to recover the treatment on the treated effects, from a policy perspective, we remain most interested in the effect of the contract *offer* captured by the ITT estimate.

4.2.2 Ruling out pre-treatment differences in gym visit patterns

Several aspects of our data suggest that the results are not driven by those in the hard contract group simply training more than those in the other two groups prior to the experiment. We do not have any data on visits to the gym prior to entry into the study for participants who entered the study by filling in the baseline questionnaire in August 2018 ($n=1,114$), but we do have information on gym visits during September for participants who joined the study by completing the baseline questionnaire in September ($n=515$).¹² Because any participants randomised to a contract offer in this group would only start their contract in October, we can compare differences across the three treatment groups in September to identify any systematic differences in gym visits. As seen in Appendix Figure C.3.1, while the confidence intervals for all three groups overlap in all months, it does seem participants across all three groups trained similarly in September, before those offered a contract increased their training in October. Participants in all three groups moreover converge to a similar average number of gym visits by the end of the post-experimental period. In the baseline survey, we additionally collected self-reported data on the number of times per week individuals trained over the past month.¹³ As shown above (see Table 1), our balance tests do not suggest significant differences in the proportion of individuals who report to train two or more times per week at baseline ($p = 0.714$), nor do we find significant differences in their baseline goals of training two or more times per week at the gym ($p = 0.251$). It is of course possible that respondents were not truthful in their responses (due to e.g. social desirability bias), but as long as their responses were not differentially associated with treatment assignment (made salient only after self-reporting their current activity levels and training goals), our findings that baseline training did not differ significantly across treatment groups should hold.

4.3 Heterogeneous effects

4.3.1 Trait self-control and time preferences

As seen in Table 2, low self-control is an important predictor of take-up of commitment contracts. We hypothesised that the effect of a commitment contract would also be greater among those with low self-control.

In Figure 6 we present a binned scatter plot of monthly gym visits by self-control and treatment group for months one to four (Panel A) and five to eight (Panel B). The figure shows a clear relationship between visits to the gym and self-control in both periods, across all treatment groups. Importantly, whilst the positive effect of commitment contracts is clear, they do not seem to affect individuals differently at the upper or lower ends of the self-control distribution. Indeed, the linear predictions for all three treatment groups are nearly parallel (though in the soft contract group, there is a suggestion that individuals with high self-control benefited relatively more than those with low self-control).

¹²In the consent form we had participants complete, we explicitly requested access to their gym visit data from September 2018.

¹³Specifically, participants were asked the number of times per week, over the past month, they had been physically active for at least 15 minutes over the past month.

Associations between self-control and monthly gym visits, and the probabilities of achieving one's baseline goal and successfully completing one's contract are presented in Table 4. Self-control is significantly positively associated with monthly gym visits during both the experimental and post-experimental periods, and with the probability of achieving one's baseline monthly goal during the experimental period only. It is, however, only weakly associated with the probability of successfully completing one's contract. Interestingly, the magnitude and significance of the effect of both hard and soft commitment contracts is stronger than in our main results (Table 3). This suggests that self-control, as one would expect, is a moderator of the effect of contracts on visits to the gym. As suggested by Figure 6 the effect of a soft or hard contract does not seem to differ by trait self-control.

In addition to investigating the role of self-control by itself, we construct an index of time preferences and an index of thoughts and behaviours using principle component analysis.¹⁴ We do not find evidence of significant differences in the effect of either a soft or hard contract on monthly visits to the gym or the probabilities of achieving one's baseline goal or meeting one's contract target by our indices of time preferences or thoughts and behaviours (see Appendix Tables B.1.1, B.1.2 and B.1.3).

4.3.2 Self-reported baseline training

We additionally investigate whether the estimated effects on visits to the gym and the probability of achieving one's baseline monthly goal differ by the self-reported weekly amount trained before entry into the study. We group individuals who reported training zero to one times, two to three times, or four or more times per week separately.¹⁵ Results are presented in Table 5. We find that the effects are considerably larger and stronger among the population of participants who reported training one time or less per week before the study, compared to those who trained two times or more. Individuals in this group randomised to an offer of a hard contract increased their weekly training during the first four months of the study by 78% compared to individuals in the control group who also trained up to one time per week prior to the experiment ($p < 0.001$), while those randomised to an offer of a soft contract increased their training by 54% ($p < 0.001$). These effects diminish in the post-experimental period but are still meaningful and statistically significant in months five to eight (though only marginally so for the soft contract group, $p = 0.060$). The effect of soft and hard commitment contracts during months one to four are smaller, though still significant (only marginally so for the soft contract group) for the group who trained two to three times per week prior to the study (hard contract $p = 0.001$, soft commitment contract $p = 0.076$). Individuals in this group who were offered a hard contract increased their weekly training during the experimental period by 19% compared to individuals in the control group ($p = 0.001$) and those offered a soft contract increased their training by 10% ($p = 0.076$). These effects are, however, no longer significant in the post-experimental period. The effects on the probability of achieving one's baseline goal are broadly in line with those observed for the count of visits to the gym, in that the largest effect is observed among individuals who reported training at most once per week prior to the study. Being offered a hard contract was also strongly positively associated with meaningful increases in this outcome among individuals who reported training two to three times per week and four or more times per week prior to the study, during the experimental period.

¹⁴The index of time preferences is the first principal component of a set of indicators for being very impatient, rather impatient, quite patient and very patient, and having present-biased preferences, all assigned using responses to Multiple Price List tasks. The index of thoughts and behaviours is the first principal component of a set of four questions asking participants to rate on a scale from one to ten how impulsive they are, how often they tend to postpone things, how much they think about the future, and how much they worry about unemployment. We additionally construct an index of time preferences and self-control, which is the first principal component of the above five indicators of time preferences and trait self-control.

¹⁵Note that this refers to all training, not just training at the gym. Participants reported their baseline training in six bins: 0-1, 2, 3, 4, 5, or 6+ times per week.

4.3.3 Socio-demographic characteristics

It is possible that in addition to differing by baseline self-reported training, the estimated effects of commitment contracts differ by socio-demographic group. As indicated in our pre-analysis plan, we conduct subgroup analyses to investigate whether the main results differ by sex, age, foreign background, educational attainment and income. Results for monthly visits are presented in Appendix Table B.4.1, for achieving one's baseline goal in Appendix Table B.4.2, and for meeting one's contract target in Appendix Table B.4.3. We do not find evidence of significant heterogeneity in the effects by socio-demographic subgroup.

4.3.4 Money put at stake

Our study differs from most previous studies investigating the role of commitment contracts in that participants who accepted a hard contract could choose the amount of money put at stake (an exception is Bhattacharya *et al.* (2015)). Above, we present evidence that individuals offered a hard contract visited the gym more often during the first four months of the study and were more likely to meet their contract target. However, we did not yet address the extent to which the amount put at stake was correlated with each of our three outcomes in the hard contract group.¹⁶ We do not find evidence of a significant relationship between accepting to add stakes, nor the amount of money put at stake (either when the monetary amount is included continuously or an indicator for putting 1000 SEK at stake is used), and monthly visits to the gym, the probability of achieving one's baseline goal, or the probability of meeting one's contract target (see Appendix Table B.2.1).

4.4 Commitment contract design

A contribution of our study is that we allowed participants to choose the length and intensity (target monthly gym visits) of their contracts. Participants could choose a contract of between one and four months, and choose a visit target that corresponded to between one and six visits to the gym per week.

Of the 926 who accepted to create a contract, the majority (n=736) selected a four month contract. The distribution of selected contract lengths and monthly gym visit targets was similar across both soft and hard contract groups (see Figure 4, and the majority of participants chose a monthly target of 8 or 12 visits per month.

After choosing the length and intensity of their contract, participants randomised to the hard contract group were offered to add financial stakes to their contract. These stakes were self-funded and participants agreed for the amount to be forfeited to a charity of their choice if they did not fulfill the contract terms. The amount put at stake was restricted to be positive and the mean amount participants put at stake was 542 SEK. The minimum was 10 SEK and the mode and maximum were 1000 SEK (see Figure 5).

We now explore the influence of exercise behaviour before participating in the study and the lag in days prior to contract start on contract design.

4.4.1 Pre-experiment visit patterns and baseline goals

Actual contract targets were correlated with the goal number of monthly gym visits participants stated in the baseline questionnaire (computed based on their goal number of weekly visits; see Appendix

¹⁶One might assume that a sophisticated agent would choose to put an amount at stake that they think will provide the encouragement necessary to successfully complete their contract. Alternatively, it could be that putting any money at stake increases the probability of successfully completing the contract.

Figure D.3.1). On average, those with goals of up to three visits per week selected a similar contract target (in terms of monthly visits), whereas those whose baseline goal was higher selected lower contract targets than their stated goal. We observe clear positive correlations between stated pre-study visits to the gym¹⁷ and participants' baseline goals and contract targets (see Appendix D.3.2). Additionally, for the cohort of October starters we can compare actual average weekly visits during the month of September to the estimated values and find they are on average comparable.

4.4.2 Lag prior to contract start

Another factor that might have affected the length and intensity of the designed contract is the number of days prior to the contract start date that an individual designed and signed their contract. It is possible that more present-biased individuals would design a contract with a higher target and longer duration when designing that contract further in advance. When offered a contract, respondents were told that the contract was monthly and would start on the first of the following month. When in the month an individual completed the baseline survey thus affected the lag until the start of the contract.¹⁸ We investigate associations between the lag to contract start and the length and intensity of the designed contract in Appendix Table A.1. The length of the lag to contract start is not significantly associated with the monthly visit target, but is significantly associated with the length of contract signed up for. One additional day prior to contract start is associated with an increase in contract length of 0.04 of a month (approximately one day), and this finding is not sensitive to the inclusion of start month fixed effects.¹⁹

5 Additional analyses and robustness checks

We now investigate the sensitivity of our results to the inclusion of controls, alternative model specifications, and the use of an individual fixed effects approach. We also investigate the robustness of our results to potential challenges to internal validity introduced by differences in baseline training at the gym across treatment groups, not all participants who accepted to add stakes actually making the mobile transfer, and some participants failing to actually *submit* the baseline survey.

5.1 Inclusion of controls

The main results are generally not sensitive to the inclusion of controls selected using the Lasso double-selection method of Chernozhukov *et al.* (2015) (see Appendix Table C.1.1). The one exception is that the effect of a soft commitment contract on monthly gym visits is no longer (marginally) significant when controls are included.

5.2 Sensitivity of results to alternative modelling specifications

We investigate the effect of commitment contracts on three different outcomes in our main analyses. The first of these (monthly gym visits) is a count variable while the latter two are binary. One might argue

¹⁷In the baseline survey, we did not ask directly about the number of times per week participants trained at the gym. Participants were asked about the weekly number of times they trained for at least 15 minutes in the past month and the number of times this training was at places *other than* the gym. We compute the the weekly number of gym visits as the difference between the responses to these two questions.

¹⁸It is important to note that the timing of completion of the baseline survey is likely endogenous. We do not, however, find evidence of associations between the lag to contract start and baseline observable characteristics (see Appendix Table A.3).

¹⁹Note that September starters could only fill in the survey from August 23rd, so by design the average lag for September starters is considerably shorter than that for October starters. When we conduct these analyses on the sample of September and October starters separately (see Appendix Table A.2) we find that a longer lag is significantly associated with a higher monthly target, but not contract length, among September starters. The results in Appendix Table A.1 thus appear to be driven predominantly by the group of October starters, despite not being sensitive to the inclusion of start month fixed effects.

that a Poisson model is more appropriate for investigating count data on monthly gym entries, and indeed several participants in the data do visit the gym zero times in any given calendar month. One might also argue that a linear probability model is inappropriate to investigate the binary outcomes. For ease of interpretation, our main results rely on estimates from linear probability models (see Table 3), but we investigate the sensitivity of our results to alternative specifications here. As documented in Table Appendix C.2.1, results obtained using these alternative modelling specifications are comparable to those presented in Table 3.

5.3 Individual fixed effects approach

Whilst our randomisation appears to have worked with regards to observable characteristics (see Table 1), it is possible that some unobservable characteristics of the participants could be confounding our results. To investigate this, we adopt an individual fixed effects approach whereby observable and unobservable characteristics that are constant over time are controlled for, and any changes in individual gym visit behaviour can be attributed to the contract offer.

We requested access to participants' gym training data from September 1, 2018 and thus have information on training at the gym in the "pre-experimental period" (see Figure 2) for the 515 participants who filled in the baseline survey in September 2018 and thus entered the study in October. We can therefore investigate the effect of commitment contracts during the experimental and post-experimental periods in this group using an individual fixed effects approach.²⁰ The results are presented in Appendix Table C.3.1 column 3. Estimated in this way, the effects of commitment contracts during both the experimental and post-experimental periods are larger in magnitude, and the reductions between the experimental and post-experimental periods are smaller in magnitude, than the results obtained using Equation 1 on the entire sample (column 1, replicated from Table 3) and on the sample of 515 participants who completed the baseline survey in September (column 2). Using an individual fixed effects approach and information on all participants to investigate the effect of commitment contracts in the post-experimental period, we find very similar results to the main analyses, though the reduction in gym visits among those offered a soft contract is now marginally statistically significant (column 4, $p = 0.051$), while it was not statistically significant in the main analyses.

5.4 Sensitivity of results to *de facto* treatment status

Our hard contract treatment involved a) accepting to add stakes and b) using a mobile payment application to transfer the agreed upon amount at stake to a third party tasked with in- and outgoing payments by the research group. Unlike Royer *et al.* (2015), who had participants write a cheque,²¹ we relied on participants opening the mobile payment application on their phone to make the transfer, after having exited the baseline survey. As might be expected, not all participants who accepted a hard contract made the transfer. These participants were emailed two reminders to make the transfer. The characteristics of participants who made versus did not make the mobile transfer, directly or after one or two reminders, are presented in Appendix Table C.4.1. We find that individuals who did not make the mobile transfer were significantly more likely to report experiencing daily stress at baseline ($p = 0.030$, t-test) and were also older ($p = 0.086$, t-test). We do not find statistically significant differences between the two groups in terms of the other investigated characteristics.

²⁰The selected contract duration is potentially endogenous. As such, instead of determining exposure based on contract length, we determine exposure to commitment contracts based on being in the experimental (months one to four) or post-experimental periods (months five to eight).

²¹Note that cheques are only very rarely used in Sweden.

In Appendix Figure C.4.1 we show the count and distribution of participants who did versus did not complete the mobile transfer, by the amount they chose to put at stake. The majority of those who did not complete the transfer put 1000 SEK at stake, and the distribution of amounts put at stake between those who completed versus did not complete the transfer differs substantially. Over 65 % of those who did not make the transfer put 1000 SEK at stake, compared to 28% of those who did complete the transfer. It thus appears that decisions not to make the transfer were concentrated among those who put 1000 SEK at stake.

The effect of the hard contract was likely larger among individuals who made the monetary transfer. Making the transfer is, however, likely endogenous to the outcomes investigated. We adopt an instrumental variables approach to investigate this question further. We assign treatment status based on whether or not participants made the monetary transfer. Those who did not are assigned the *de facto* status of having a soft contract. The instrument is the treatment group to which individuals were randomised. As expected, the estimated effect of having a hard contract is even stronger during the experimental period when considering only those who made the transfer as being treated with the hard contract (see Appendix Table C.4.2).²²

5.5 Do results differ if consider only those who submitted the survey?

Not all individuals who were randomised (and agreed to- and designed a contract) *submitted* the baseline questionnaire. We include all individuals who clicked through the entire survey and were thus randomised, regardless of whether they hit the “submit” button at the end or not. This is in order to avoid biases introduced by participants not liking the group to which they were randomised (e.g. not being offered a contract) and therefore not submitting the survey. A drawback of this approach is that those who did not click “submit” may have assumed they were no longer taking part in the experiment. If we restrict the sample to only those individuals who submitted the survey, we lose 60 individuals overall and 48 individuals in the hard and soft contract groups. Appendix Table C.5.1 shows results using our main specifications, restricting the sample to those individuals who submitted the survey. As might be expected, the results are in line with those in Table 3, but slightly larger in magnitude.

5.6 Potential for spillover effects

It is possible that the experiment led to behavioural spillovers between participants in different treatment groups if participants discussed their treatment within their networks and/or modified their own behaviour in response to not only their treatment assignment but also their peers’ treatment assignment. Our *a priori* expectation is that such spillover effects would bias our estimated treatment effect towards the null. This is because we would expect that control group individuals exposed to training partners offered a commitment contract would likely increase their gym visits in response to their training partners’ increased visits. We have no reason to expect that they would decrease their gym visits. Regarding individuals offered a contract, we would expect them, if anything, to visit the gym less than their contract target if fewer of their peers were exposed to a contract offer. It is, however, possible that individuals offered either a soft or hard contract who had several peers also offered a soft or hard contract would visit the gym even more than they would have otherwise.

To identify potential spillover effects, we need to be able to identify an individual’s potential peer group. One way to do this is to identify individuals who enter the gym at the same time, for the same activity,

²²Whilst this may seem counter-intuitive, given that we did not observe a significant relationship between adding stakes, or size of stakes attached, and any of the outcomes of interest, it may be due to the fact that the majority of the 61 participants who did not make the mobile transfer had chosen 1000 SEK as the amount of stakes to attach (see Appendix Figure C.4.1)

multiple times over the study period. We define potential training partners as pairs of individuals who enter the gym within 15 minutes of each other, for the same activity, on at least four occasions during the entire study period (including both the experimental and post-experimental periods). Defining potential training partners in this way, 703 of the participants in our study have at least one potential training partner (see Appendix Figure C.6.1). The maximum number of potential training partners treated with either a soft or a hard contract was four, and the distribution of participants by the number of potential training partners treated with either a soft or a hard contract offer was similar (see Appendix Figure C.6.2).

To investigate spillover effects we estimate equation 1 but include the total count of potential training partners, and the count of potential training partners treated with either a soft or hard contract, as well as interactions between each of these variables and the treatment variables. Results are presented in Appendix Table C.6.1. Estimated in this way, the effect of both soft and hard commitment contracts are larger in magnitude than in our main specification (see Table 3). Throughout both the experimental and post-experimental periods, having more potential training partners is significantly positively associated with monthly visits to the gym and the probability of achieving one’s baseline monthly goal.²³ The numbers of *treated* potential training partners are, however, only significantly (positively) associated with visits to the gym, and not the probability of achieving one’s baseline goal. We do not find strong evidence of differences in the effects of soft and hard commitment contracts by the number of (treated) potential training partners an individual has. Overall, these results suggest that, if anything, spillover effects bias our main estimates towards the null.

6 Discussion and conclusion

We present evidence on the effect of both hard and soft commitment contracts on visits to the gym, the probability of achieving one’s baseline goal and the likelihood of meeting one’s contract target. We find considerable demand for commitment contracts, with 84% of participants accepting to sign a contract (either soft or hard), and 34% of participants in the hard contract group accepting to attach stakes. We find that offers of both hard and soft commitment contracts lead to increased visits to the gym and an increased probability of achieving one’s baseline monthly goal. As might be expected, the incentivised nature of hard commitment contracts seems to confer greater “staying power”, likely due to the larger and more salient penalty associated with not following through. Individuals randomised to an offer of a hard contract visit the gym 21% more times per month than the control group during the first four months of the study ($p < 0.001$), and continue to visit the gym 10% more times per month in the post-experimental period (months five to eight, $p = 0.082$). Participants offered a soft contract visit the gym 8% more often than those randomised to the control group ($p = 0.085$), but the difference is not statistically significant in the post-experimental period. This overall pattern is similar when investigating the effect of commitment contracts on the probability of participants achieving their goal number of gym visits, as stated in the baseline questionnaire.

We find that participants offered a hard contract are five percentage points more likely ($p = 0.065$) than those offered a soft contract to meet their contract target. It is important to note, however, that whilst this effect is economically meaningful, only 15% of participants offered a soft contract and 19.6% of participants offered a hard contract actually met their contract targets. It thus seems that even though we observe rather large and meaningful increases in visits to the gym, these increases do not seem to

²³Note that it is possible that what we are estimating here as spillover effects can actually be attributed to differences in the effects of commitment contracts by an individual’s propensity to train by themselves versus attend group training classes, since many of the potential partners identified are likely to be other individuals attending a given fitness class.

translate into individuals, on average, meeting their contract targets. One possible explanation is that it might not have been salient to all participants just how close they were to meeting their target because we did not send out reminders or provide updates regarding participants' progress. Participants were, however, able to log onto the gym's website to check their training themselves. Overall, we find it striking that we do find such large effects in the absence of reminders or any form of progress reports. One possibility for these large effects could be the timing of the intervention. Most Swedes take several weeks' holiday during the summer. Participants thus entered the study soon after returning from a long period of vacation. They might, moreover, have been extra motivated by having a goal to work towards during the months leading up to the end of the calendar year.

We measure the self-control of participants using Tangney *et al.* (2004)'s measure of trait self-control and can relate this to demand for commitment, monthly gym visits, and the probabilities of achieving one's baseline goal and successfully completing one's contract. We find high take-up of commitment contracts, and that take-up is significantly negatively correlated with self-control. Whilst the distribution of self-control is lower among those who accept versus reject a contract, which is in line with the theoretical prediction that it is sophisticated individuals with low self-control who "should" demand commitment, we do find that a large portion of participants with above-median self-control also choose to sign contracts. One potential reason for this is that in practice individuals may not necessarily view these contracts as a commitment device. They might view them more as a challenge, and thus choose to sign-up for one in the absence of self-control problems. Our self-control variable is moreover unlikely to be free of measurement error.²⁴ Regarding our results, we find that whilst self-control is associated with gym visits, the effect of commitment contracts does not differ along the distribution of trait self-control.

Whilst we are unable to investigate persistence as far in the future as Royer *et al.* (2015), we do uncover some evidence of persistence in the effect of a hard contract in the four months following contract completion. Approximately 41% of the hard contract-induced increase in gym visits remains in the post-experimental period, as well as approximately 27% of the increase in the probability of achieving one's baseline goal. This is comparable to Charness & Gneezy (2009) who find that approximately 50% of the effect of their incentive treatment remains in the post-treatment period. At first glance, our results for persistence appear lower than the effects observed in Royer *et al.* (2015), who find that approximately 88% of the effect of their (hard) commitment contracts persists in the 12 weeks following the end of the contract. Their set-up was, however, different to ours in that participants were initially randomly assigned to be incentivised to attend their company gym, and subsequently randomised to the offer of a two-month long commitment contract. In their study, 31% of the increase in gym visits caused by the initial incentive treatment was retained in the twelve-week period following the end of the commitment contracts.²⁵

Of note, individuals in the control groups in both Charness & Gneezy (2009) and Royer *et al.* (2015) visited the gym on average about three times per month, compared to over five visits per month in the control group in our study. It could thus be that sustained behavioural change is harder to achieve among our population of individuals who already visit the gym more than once per week on average. Indeed, we observe the greatest effects (and persistence in the effects) of commitment contracts among those individuals who reported training at most once per week at baseline. In the hard contract group, these individuals continued to train approximately one time per month more than the control group

²⁴Carrera *et al.* (2022) find that stochastic valuation errors could be the cause of the discrepancy between the theoretical prediction that only those with low self-control should take up commitment contracts and their experimental finding that take-up and self-control do not seem significantly correlated.

²⁵These estimates were obtained by comparing the coefficient on the incentive+commit group in the late post-treatment period (weeks 14-26) to that in the early post-treatment period (weeks 5-13, during which commitment contracts were in place for those who accepted the contract offer) and the in-treatment period (weeks 1-4) in Table 2 of Royer *et al.* (2015).

during the post-experimental period, which translates to a retention of approximately 50% of the effect of the hard contract. In the soft contract group the effect was smaller, but here too, approximately 50% of the effect of the soft contract is retained, with individuals in this group training 0.663 more times per month than the control group ($p = 0.060$) in the post-experimental period.

Both Charness & Gneezy (2009) and Royer *et al.* (2015) find that the effects of their treatments remain fairly stable throughout the duration of their post-treatment follow-up periods, which is something we do not observe. When we investigate persistence by month since contract-end, our results suggest that the effect of the contracts decreases with increasing time since contract end. This reinforces the observation that commitment contracts are important and work, but that individuals may revert to old habits once the contract is over. It could thus be important to sign a subsequent contract. An interesting area of future research would be to investigate how “performance” on the initial commitment contract affects demand and design of future contracts, and whether the effects of future contracts are stronger or weaker compared to the original one.

One potential concern is lack of generalisability of our study. The mean age of our participants is 50 years, and approximately 23% are male. Participants had to opt in to participate and those individuals who opt in to a study like ours may differ from the general population of gym-goers in Stockholm. One might also wonder about the extent to which conclusions drawn on a population of current gym goers who opt in to a study of physical activity can be extended to the general population. The internal consistency of our study is, however, high as we randomise individuals to each treatment group and in our main analyses keep all individuals who progressed far enough through the baseline survey to be randomised (hence we can account for potential differential dropout if individuals did not like the treatment group to which they were assigned).

Another potential concern is that the forfeited stakes were donated to a charity of the participant’s choice. It is thus possible that once individuals fell off track in terms of meeting their goal, they rationalised it by viewing their stakes as a contribution to charity. Our estimated effects might thus be biased downwards compared to a setting where the stakes would be forfeited to a “bad” cause. To alleviate these concerns, we included some charity options, among the 11 we allowed participants to donate to, that we expected some individuals to find less appealing. For example, the Swedish Republican Association is an association lobbying for abolishing the monarchy, and Humanisterna lobbies for secularism. This gave participants the option to choose a charity supporting a cause they do not like (e.g. a supporter of the monarchy could choose the Swedish Republican Association as their charity). The (by far) most popular charity to donate to was, however, Doctors Without Borders.

An additional potential concern is that whilst the proportion of individuals training three or more times per week is similar across the three treatment groups at baseline and follow-up, the proportion training more than two times per week at places other than the gym is significantly lower in the hard contract group at follow-up. This raises the question of whether what we are observing in the treatment groups is crowding out of training at places other than the gym with training at the gym, rather than an actual increase in physical activity. Participants were, however, aware that the contract they designed should target their desired amount of training *at the gym*, as this was all that we could measure. Perhaps reassuringly, approximately 60% of respondents to the follow-up survey, across all groups, expressed wanting to train at the gym three or more times per week.

Our study contributes to the literature on commitment contracts by experimentally investigating the difference in the effect of soft and hard commitment contracts. Two recent studies (Bai *et al.*, 2017; Carrera *et al.*, 2022) find that commitment contracts may be welfare reducing in some settings. Understanding the role of the financial stakes is therefore important. Whilst both the short and long-term effects of

the hard contract are stronger than the effect of the soft contract on all outcomes investigated, we do find some evidence that the soft commitment can have a positive effect, at least during the contract period. This suggests that the simple act of making one's goals salient and committing to them can have a positive effect. If part of the welfare-reducing effect of commitment contracts is that a large number of individuals do not succeed in meeting the contract terms and thus lose money (Carrera *et al.*, 2022), it would seem that soft commitment contracts, if they work, could be useful. The hard contracts in our study were fully self-funded, and the intervention is thus cheap and easily scalable. We find measurable and economically significant effects of commitment contracts among a population of current gym-goers, who are likely healthier and more active already than the general population. We find some evidence of persistence after the end of the intervention, which is strongest among those who reported exercising the least at baseline. Our study provides evidence that commitment contracts can be useful not only at the extensive margin, getting individuals to be physically active, but at the intensive margin as well, getting individuals to be *more* physically active and achieve their goals.

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Tables

Table 1: Descriptive statistics for the study sample

Characteristic	Control Mean [SD]	Obs	Soft contract Mean [SD]	Obs	Hard contract Mean [SD]	Obs	p- value
Panel A: Baseline survey							
<i>Demographics</i>							
Age (in years)	50.00 [14.45]	516	49.23 [14.65]	568	49.26 [14.86]	526	0.625
Male	0.235	514	0.195	564	0.250	525	0.082
Foreign-born	0.121	520	0.104	568	0.108	526	0.647
<i>Socioeconomics</i>							
Have post-secondary education	0.807	517	0.771	567	0.808	522	0.218
Earn >40,000SEK/month	0.731	461	0.720	528	0.707	484	0.706
<i>Health</i>							
Reports good or very good health	0.765	523	0.764	572	0.814	531	0.080
BMI	23.89 [3.43]	521	24.26 [3.51]	569	24.07 [3.30]	526	0.213
Current daily smoker	0.008	523	0.014	573	0.013	531	0.578
Often/always sleep badly	0.207	523	0.192	573	0.158	530	0.120
Experience daily stress	0.083	521	0.082	573	0.075	530	0.894
<i>Exercise</i>							
>30 min “everyday” exercise daily	0.471	524	0.439	572	0.423	530	0.269
Trains >=2 times/week	0.775	524	0.754	573	0.761	532	0.714
Trains >=2 times/week at places other than gym	0.345	524	0.339	573	0.338	532	0.962
Aims to train >=2 times/week	0.998	524	0.995	573	1.000	532	0.204
Aims to train >=2 times/week at gym	0.968	524	0.979	573	0.962	532	0.251
Active gym membership	0.996	524	0.990	573	0.992	532	0.437
<i>Preferences</i>							
Self control (13 = none, 65 = max)	45.65 [7.40]	503	44.87 [7.17]	546	45.31 [7.07]	512	0.217
Present-biased	0.108	492	0.117	529	0.150	500	0.106
Impatient	0.085	492	0.089	529	0.094	500	0.891
Contract take-up	–		0.850	487	0.825	439	0.265
Total observations		524		573		532	Sum total: 1629
Panel B: Follow-up survey							
<i>Health</i>							
Reports good or very good health	0.763	472	0.798	490	0.836	445	0.022
BMI	23.89 [3.38]	470	24.21 [3.45]	485	24.16 [3.45]	439	0.303
Current daily smoker	0.013	474	0.014	490	0.009	446	0.750
Often/always sleep badly	0.186	473	0.166	487	0.152	446	0.392
Experience daily stress	0.087	473	0.076	489	0.045	446	0.036
<i>Exercise</i>							
>30 min “everyday” exercise daily	0.448	473	0.470	489	0.428	444	0.429
Trains >=2 times/week	0.800	474	0.786	490	0.834	446	0.161
Trains >=2 times/week at places other than gym	0.350	474	0.322	490	0.298	446	0.241
Aims to train >=2 times/week	0.996	474	1.000	490	0.998	446	0.364
Aims to train >=2 times/week at gym	0.932	474	0.945	490	0.951	446	0.478
Active gym membership	0.947	474	0.945	490	0.955	446	0.760
<i>Preferences</i>							
Requested scratchcard	0.300	474	0.316	490	0.350	446	0.255
Response rate	0.905	474	0.838	490	0.855	446	0.005

Notes: The reported p-value is from a Wald test for joint equality of the coefficients on each treatment group. These coefficients are obtained from a regression of each study characteristic on an indicator for each treatment group. Standard deviations for non-dichotomous variables are shown in square brackets.

Table 2: Ordinary least squares estimates of predictors of accepting a contract and accepting to add financial stakes (conditional on accepting a contract and being randomised to the hard contract group)

	Accept contract				Accept stakes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Self control (standardised)	-0.0510*** (0.0106)	-0.0484*** (0.0109)	-0.0405*** (0.0111)	-0.0260** (0.0119)	-0.0469* (0.0244)	-0.0468* (0.0258)	-0.0518* (0.0266)	-0.0239 (0.0287)
<i>Time consistency & impatience</i>								
Present-biased		0.0388 (0.0328)	0.0159 (0.0327)	-0.00856 (0.0348)		0.0485 (0.0744)	0.0307 (0.0746)	0.0333 (0.0805)
Future-biased		-0.00670 (0.0371)	0.00176 (0.0353)	-0.00317 (0.0360)		-0.0879 (0.0769)	-0.0892 (0.0790)	-0.125 (0.0765)
Impatient		-0.0654 (0.0469)	-0.0199 (0.0445)	-0.0242 (0.0479)		0.111 (0.0926)	0.118 (0.0934)	0.107 (0.0942)
<i>Demographics</i>								
Age (in years)			-0.00359*** (0.000910)	-0.00435*** (0.000970)			-0.00146 (0.00178)	-0.00246 (0.00192)
Male			-0.0441 (0.0290)	-0.0495 (0.0303)			0.134** (0.0592)	0.129** (0.0644)
Foreign-born			0.0324 (0.0344)	0.0462 (0.0350)			-0.0523 (0.0818)	-0.0264 (0.0817)
<i>Socioeconomics</i>								
Post-secondary education				-0.00298 (0.0315)				-0.0692 (0.0751)
Earn >40,000SEK/month				0.00570 (0.0263)				0.0908 (0.0579)
<i>Health & exercise</i>								
Train<2 times/week				0.118*** (0.0219)				0.181*** (0.0609)
Overweight or obese				0.0630** (0.0248)				0.125** (0.0575)
Good or very good health				0.00940 (0.0280)				-0.0274 (0.0706)
Current daily smoker				-0.107 (0.0925)				-0.0241 (0.222)
Often/always sleep badly				0.0520* (0.0269)				-0.106 (0.0706)
Experience daily stress				0.0274 (0.0348)				0.0781 (0.102)
Constant	0.835*** (0.0114)	0.844*** (0.0134)	1.029*** (0.0456)	1.003*** (0.0618)	0.407*** (0.0239)	0.401*** (0.0294)	0.451*** (0.0925)	0.456*** (0.139)
Observations	1058	986	965	886	422	395	391	358
Take-up	0.838	0.838	0.838	0.838	0.408	0.408	0.408	0.408
R-squared	0.0184	0.0209	0.0462	0.0867	0.00867	0.0210	0.0386	0.0928

Notes: Only participants randomised to an offer of either a soft or hard commitment contract are included in these analyses. In columns 5-8, only participants who accepted the contract offer and were randomised to the hard contract group are included. Besides the indicator for being male, the binary predictors were computed from categorical (foreign-born, current daily smoker) or ordered categorical variables with more than two categories (having a post-secondary education, earning >40,000SEK/month, training<2 times/week, being in good or very good health, often/always sleeping badly, experiencing daily stress). The indicator for being overweight or obese is equal to one if an individual's BMI (computed based on baseline self-reported height and weight) is greater than or equal to 25. Indicators for present-bias, future bias and being impatient were computed based on the measures constructed using multiple price list tasks.

Table 3: Estimates of the effect of being offered a soft or a hard contract on monthly visits to the gym and the probability of achieving one's baseline monthly goal, and meeting one's contract target

	Monthly visits			Monthly goal met			Contract target met
	(1) Months 1-4	(2) Months 5-8	(3) Δ	(4) Months 1-4	(5) Months 5-8	(6) Δ	(7)
Panel A: Ordinary least squares							
Soft contract	0.460* (0.267)	0.213 (0.275)	-0.247 (0.185)	0.0372** (0.0147)	0.0138 (0.0146)	-0.0234* (0.0139)	
Hard contract	1.140*** (0.276)	0.471* (0.271)	-0.668*** (0.191)	0.0966*** (0.0163)	0.0265* (0.0147)	-0.0701*** (0.0154)	0.0460* (0.0249)
Control group mean	5.500	5.141		0.107	0.111		0.150
R-squared	0.021	0.019		0.021	0.009		0.004
Panel B: Instrumental variable estimates							
Soft contract	0.542* (0.315)	0.251 (0.324)	-0.291 (0.218)	0.0438** (0.0173)	0.0163 (0.0172)	-0.0275* (0.0163)	
Hard contract	2.600*** (0.707)	1.036 (0.704)	-1.563*** (0.478)	0.223*** (0.0462)	0.0552 (0.0395)	-0.168*** (0.0414)	0.113* (0.0612)
Control group mean	5.768	5.469		0.114	0.111		0.159
R-squared	0.001	0.010		0.007	0.004		0.002
Observations	6516	6516		6516	6516		926
Individuals	1629	1629		1629	1629		926
Month-year FEs	Yes	Yes		Yes	Yes		No

Notes: Individuals analysed according to the treatment group to which they were randomised. Not all individuals randomised actually submitted the baseline survey. Some participants were not listed in the training data obtained from the gym (for some or all months). These individuals are coded as having trained zero times per month, thus the number of individuals contributing to months 1-4 and 5-8 is the same. The estimates for the difference between months 1-4 and 5-8 (columns 3 and 6) are obtained from a regression that resembles equation (1) but where a dummy variable that equals 1 for months 5-8 is included and interacted with dummy variables for each treatment group. The "control group mean" in column 10 refers to the mean in the soft contract group. Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Associations between trait self-control and monthly gym visits, the probability of achieving one's baseline monthly goal, and the probability of meeting one's contract target

	Monthly visits		Monthly goal met		Contract target met
	(1) Months 1-4	(2) Months 5-8	(3) Months 1-4	(4) Months 5-8	(5)
Soft contract	0.653** (0.268)	0.368 (0.278)	0.0435*** (0.0150)	0.0176 (0.0148)	
x Self control	0.146 (0.271)	0.0813 (0.294)	0.0116 (0.0155)	0.00342 (0.0164)	
Hard contract	1.284*** (0.276)	0.595** (0.272)	0.0977*** (0.0165)	0.0293** (0.0147)	0.0505* (0.0259)
x Self control	0.0133 (0.289)	0.0878 (0.280)	-0.00997 (0.0165)	0.0127 (0.0140)	0.0343 (0.0262)
Trait self control	0.627*** (0.188)	0.518*** (0.188)	0.0214** (0.00867)	0.0123 (0.00921)	0.0304* (0.0169)
Control group mean	5.500	5.141	0.107	0.111	0.150
R-squared	0.042	0.033	0.025	0.012	0.021
Observations	6244	6244	6244	6244	884
Individuals	1561	1561	1561	1561	884
Month-year FEs	Yes	Yes	Yes	Yes	No

Notes: Not all participants randomised to the offer of a contract provided answers to all questions used to compute the measure of trait self-control. The number of individuals contributing to these estimates is thus smaller than the number of individuals offered soft or hard commitment contracts. (Cluster) robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Differences in the effect of a soft or hard contract on monthly visits to the gym and the probability of achieving one's baseline goal, by weekly amount trained prior to the study

	Months 1-4			Months 5-8		
	(1)	(2)	(3)	(4)	(5)	(6)
	0-1 times	2-3 times	4+ times	0-1 times	2-3 times	4+ times
Panel A: Monthly visits						
Soft contract	1.373*** (0.359)	0.555* (0.312)	-0.287 (0.648)	0.663* (0.352)	0.130 (0.334)	0.265 (0.674)
Hard contract	1.998*** (0.392)	1.022*** (0.305)	1.052 (0.688)	1.062*** (0.362)	0.408 (0.327)	0.431 (0.672)
Control group mean	2.564	5.330	8.341	2.381	5.243	7.303
R-squared	0.061	0.027	0.019	0.030	0.024	0.020
Panel B: Monthly goal met						
Soft contract	0.0427* (0.0251)	0.0311 (0.0212)	0.0488 (0.0310)	0.0144 (0.0209)	0.00845 (0.0199)	0.0316 (0.0351)
Hard contract	0.110*** (0.0301)	0.0860*** (0.0232)	0.112*** (0.0335)	0.0180 (0.0205)	0.0397* (0.0208)	0.0144 (0.0336)
Control group mean	0.055	0.118	0.132	0.049	0.117	0.154
R-squared	0.027	0.020	0.023	0.005	0.012	0.010
Observations	1544	3336	1636	1544	3336	1636
Individuals	386	834	409	386	834	409
Month-year FEs	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figures

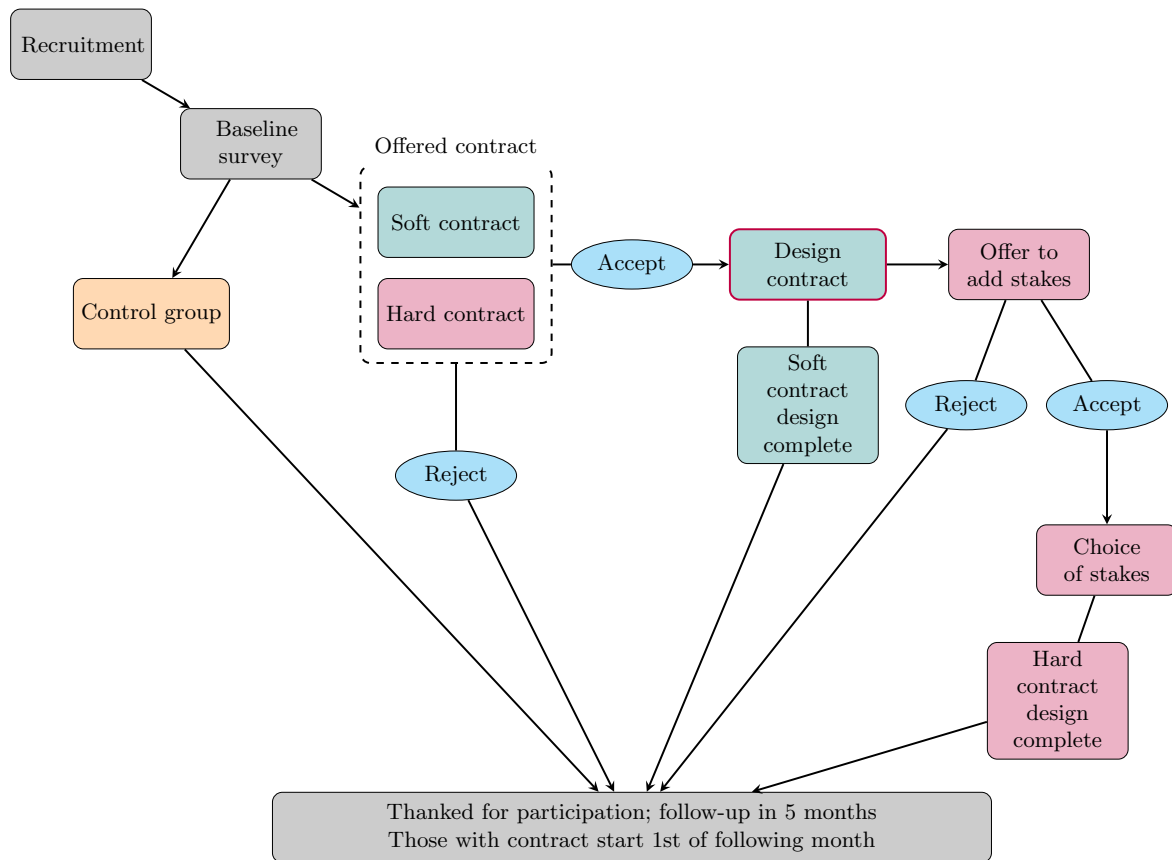


Figure 1: Flow diagram

Notes: Participants were recruited via the gym's monthly newsletter and television screens in the gyms. The invitation to participate included a link to the baseline survey. A copy of the invitation (in Swedish) is presented in Appendix F. A copy of the baseline survey instrument (also in Swedish) is presented in Appendix G. After completing the questions in the baseline survey instrument, participants were either randomised to the control group or one of the soft or hard commitment contract treatment groups. All participants who completed the baseline survey questions, and were thus randomised, are included in the analyses, even if they rejected the contract offer. Whether or not an individual was randomised to the soft or hard contract group was only made salient *after* the contract design was complete. All participants received a follow-up survey approximately five months after they completed the baseline questionnaire.

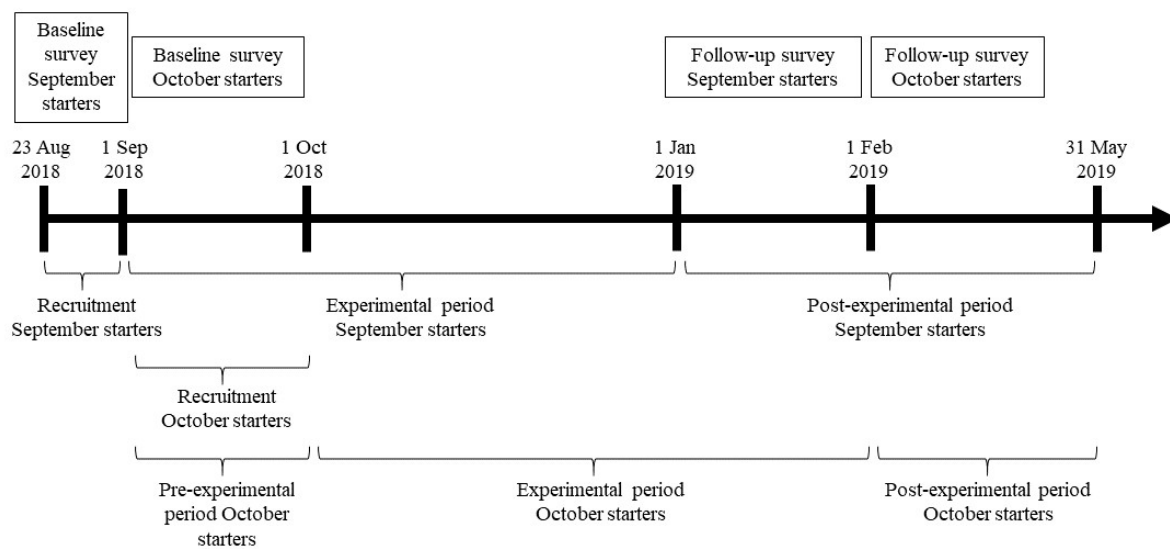
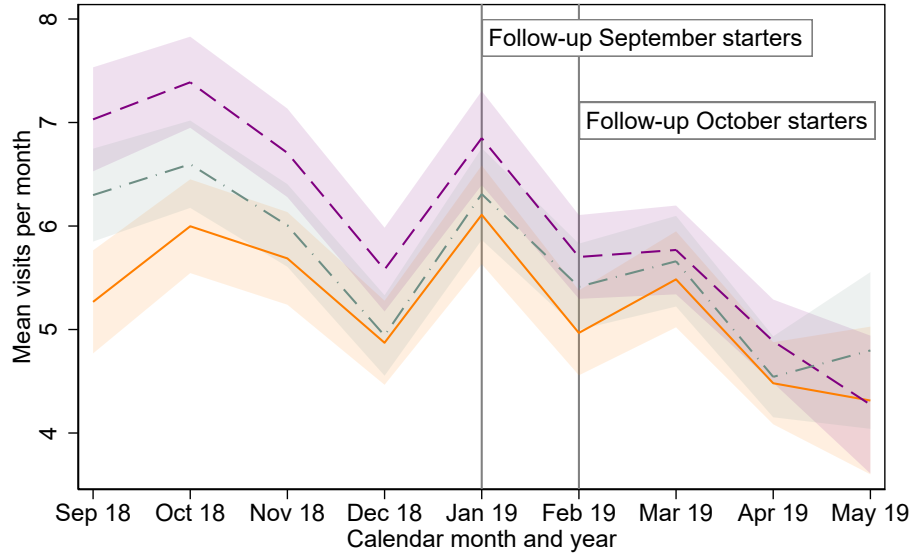


Figure 2: Experimental timeline

Notes: 1,114 participants were included from September 2018 and another 515 from October 2018. Participants randomised to be offered a contract were free to choose their contract length. They may therefore not have been “under contract” for the duration of the experimental period.

Panel A: Monthly visits



Panel B: Probability of achieving baseline goal

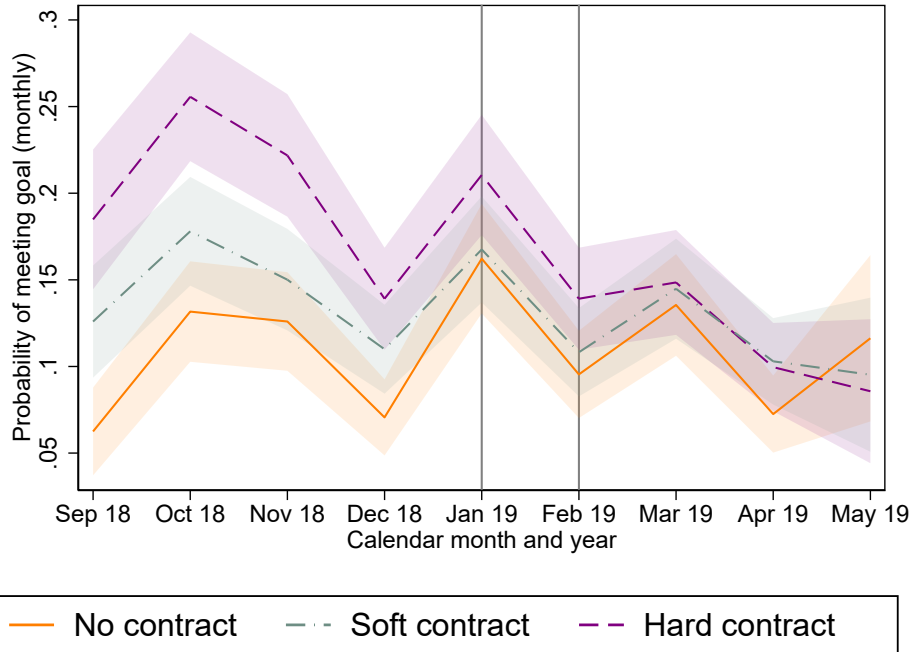


Figure 3: Monthly exercise outcomes, by treatment group

Notes: 1,114 participants were included from September 2018 and another 515 from October 2018. Note that participants randomised to be offered a contract (soft or hard) were free to choose their contract length (ranging from one to four months) and how much they would train per month. Individuals randomised to be offered a contract were moreover free to turn it down. To obtain the estimates presented in the figure we regressed each outcome on dummies for the interaction between indicators of calendar month and treatment group. The regressions include individuals from the month immediately following that in which they completed the baseline survey (i.e. September for those who completed the survey in August, and October for those who completed the survey in September). Regressions do not control for any additional variables. Confidence intervals are based on standard errors clustered at the individual level.

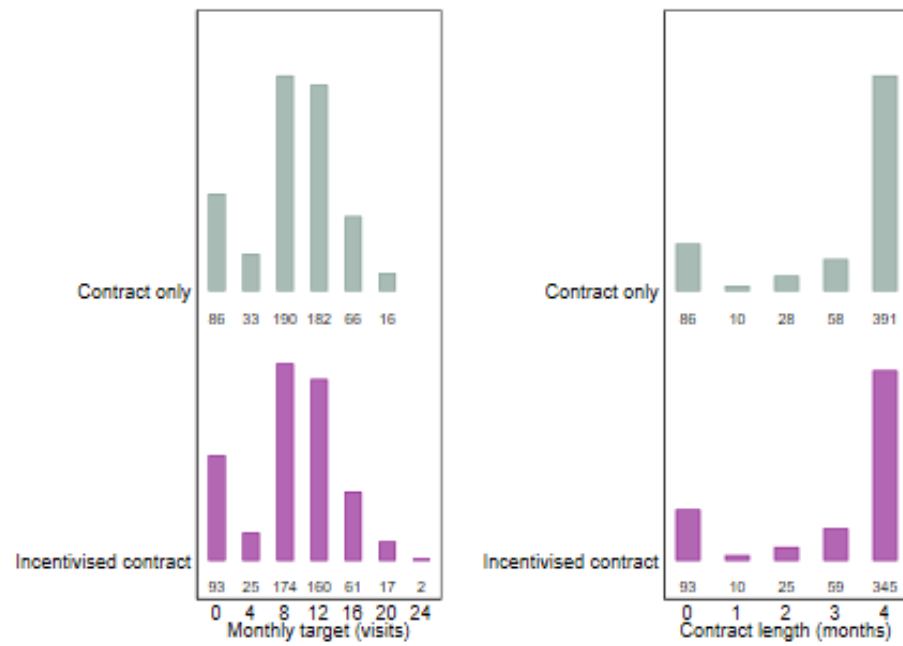


Figure 4: Distribution of selected contract length and monthly target amount of gym visits, by treatment group

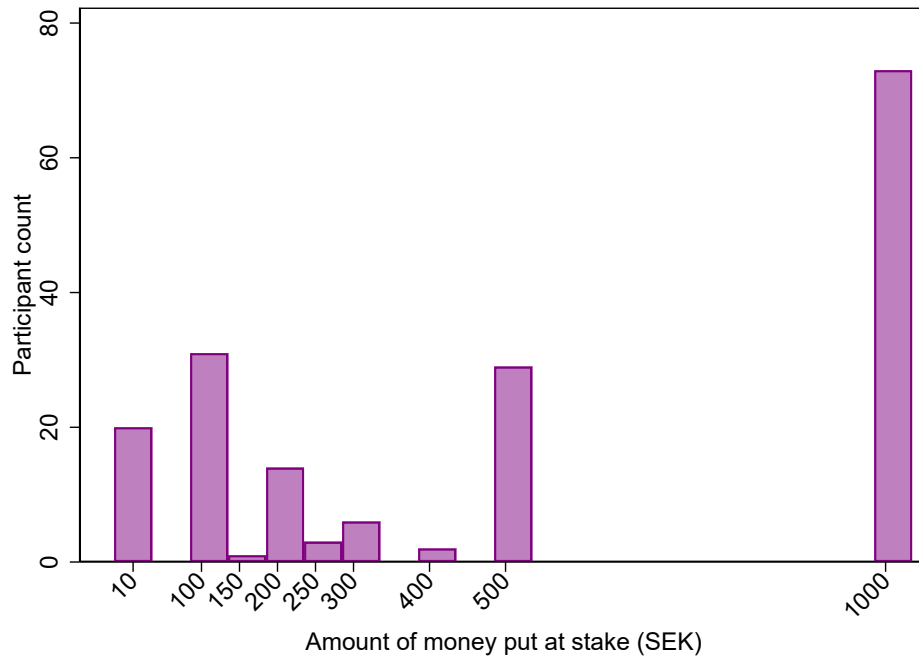


Figure 5: Distribution of amount put at stake (SEK)

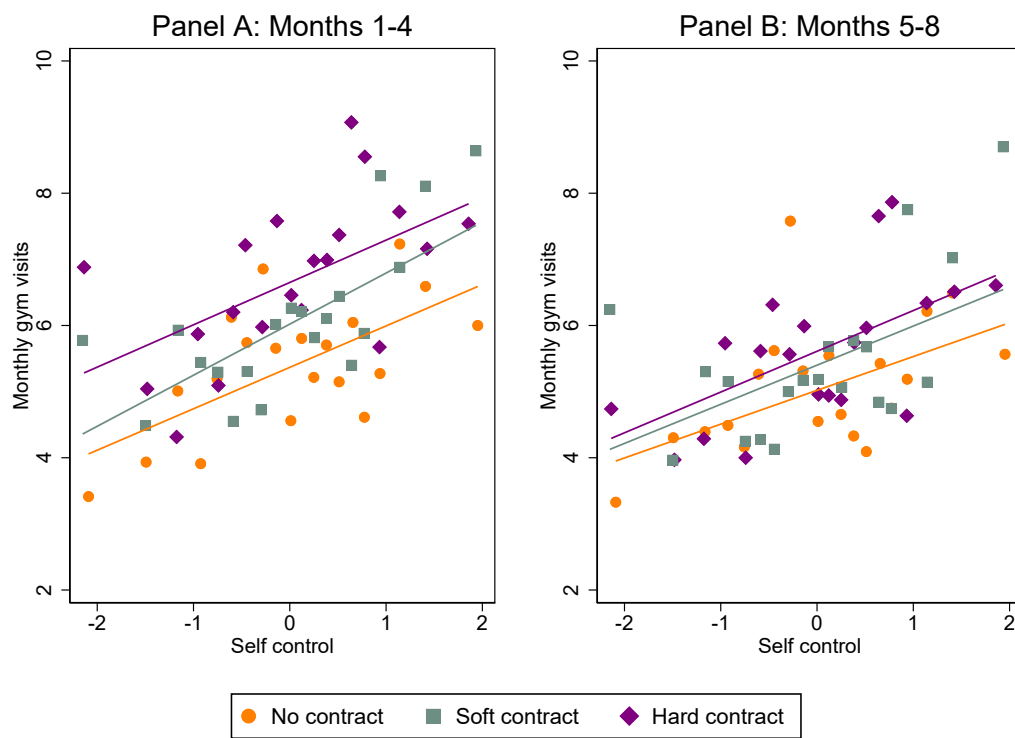


Figure 6: Binned scatter plot of monthly gym visits by trait self-control and treatment group

Notes: Twenty equal-sized bins.

Appendix

A Factors affecting commitment contract design and demand

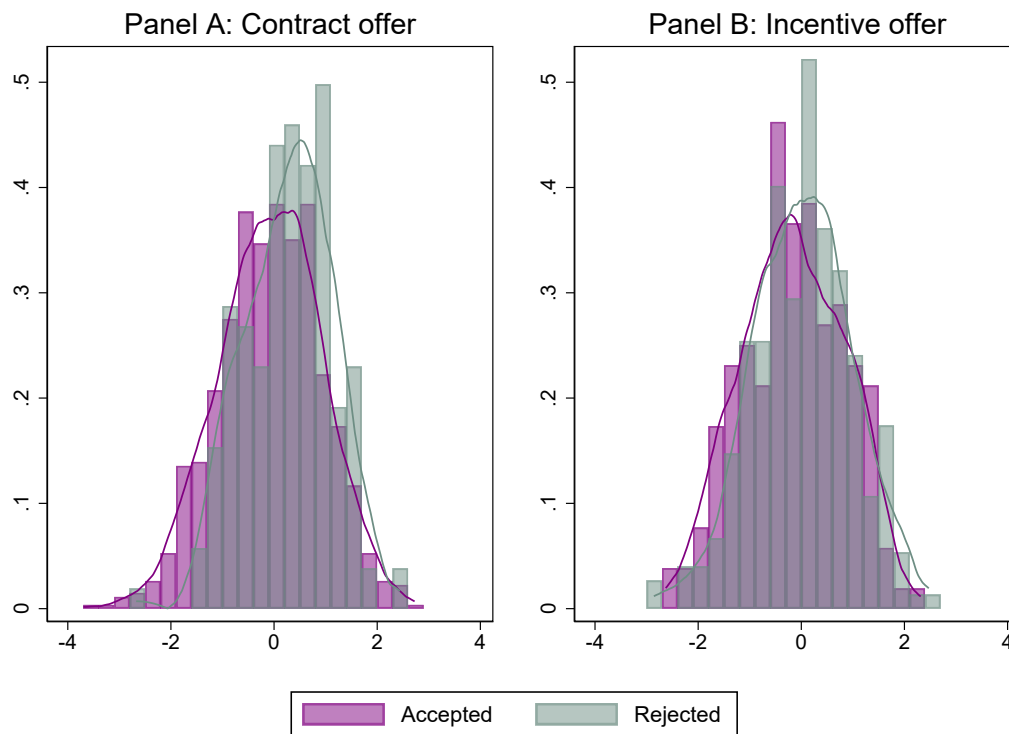


Figure A.1: Histogram of trait self-control by whether accepted or rejected commitment contract (Panel A), and accepted or rejected to add incentives (Panel B)

Notes: 1,105 individuals were offered a commitment contract (Panel A) and 439 were offered to attach a financial incentive (Panel B).

Table A.1: Associations between lag in days prior to contract start and contract length, target and accepting to add stakes

	Contract length		Monthly target		Accepted stakes	
	(1)	(2)	(3)	(4)	(5)	(6)
Lag in days prior to contract start	0.00491 (0.00536)	0.00260 (0.00534)	0.0333 (0.0278)	0.0255 (0.0282)	0.00275 (0.00516)	0.00234 (0.00523)
Constant	3.661*** (0.0439)	3.714*** (0.0466)	10.56*** (0.223)	10.74*** (0.254)	0.389*** (0.0413)	0.398*** (0.0475)
Observations	926	926	926	926	439	439
R-squared	0.001	0.008	0.002	0.004	0.001	0.001
Start month controls	No	Yes	No	Yes	No	Yes

Notes: The lag in days prior to contract start depends on when a participant filled in the baseline questionnaire. All participants who completed the baseline survey between August 23, 2018 and August 31, 2018 and were randomised to a contract offer started their contract on September 1, 2018. Any participants who filled in the baseline questionnaire between September 1 2018 and September 30 2018 and were randomised to a contract offer started their contracts on October 1, 2018. Participants could choose a monthly target amount of gym visits of 4, 8, 12, 16, 20 or 24 times per month. Those presented with a *higher exercise default* were presented with a pre-selected default amount that was equivalent to one time more per week than the goal number of weekly visits to the gym they had stated in the baseline questionnaire.

Table A.2: Associations between lag in days prior to contract start and contract length, target and accepting to add stakes, by start month

	Contract length		Monthly target		Accepted stakes	
	(1)	(2)	(3)	(4)	(5)	(6)
	September	October	September	October	September	October
Lag in days prior to contract start	0.0168 (0.0389)	0.0285*** (0.00899)	0.275*** (0.106)	0.00655 (0.0291)	0.0175 (0.0154)	0.00184 (0.00489)
Constant	3.036*** (0.293)	2.808*** (0.104)	8.904*** (0.793)	10.43*** (0.270)	0.217* (0.114)	0.311*** (0.0427)
Observations	762	343	645	281	357	175
R-squared	0.000	0.018	0.011	0.000	0.003	0.001

Notes: The lag in days prior to contract start depends on when a participant filled in the baseline questionnaire. All participants who completed the baseline survey between August 23, 2018 and August 31, 2018 and were randomised to a contract offer started their contract on September 1, 2018. Any participants who filled in the baseline questionnaire between September 1 2018 and September 30 2018 and were randomised to a contract offer started their contracts on October 1, 2018.

Table A.3: Associations between the lag in days prior to contract start and predetermined characteristics

Characteristic	Association [SD]	<i>p-value</i>
<i>Demographics</i>		
Age (in years)	-0.134 [0.115]	0.246
Male	0.002 [0.003]	0.569
Foreign-born	0.000 [0.002]	0.920
<i>Socioeconomics</i>		
Have post-secondary education	-0.002 [0.003]	0.567
Earn >40,000SEK/month	-0.004 [0.004]	0.254
<i>Health</i>		
Reports good or very good health	0.001 [0.003]	0.786
BMI	0.019 [0.023]	0.410
Current daily smoker	-0.001 [0.000]	0.134
Often/always sleep badly	0.001 [0.003]	0.850
Experience daily stress	-0.003 [0.001]	0.012
<i>Exercise</i>		
>30 min “everyday” exercise daily	0.004 [0.003]	0.256
Trains >2 times/week	-0.003 [0.003]	0.363
Trains >2 times/week at places other than gym	0.000 [0.003]	0.965
Aims to train >2 times/week	0.000 [0.000]	0.294
Aims to train >2 times/week at gym	0.001 [0.001]	0.378
Active gym membership	-0.000 [0.000]	0.850
<i>Preferences</i>		
Self control (13 = none, 65 = max)	-0.005 [0.048]	0.919
Present-biased	0.002 [0.003]	0.373
Future-biased	-0.002 [0.002]	0.180
Impatient	-0.000 [0.002]	0.990

Notes: The lag in days prior to contract start depends on when a participant filled in the baseline questionnaire. All participants who completed the baseline survey between August 23, 2018 and August 31, 2018 and were randomised to a contract offer started their contract on September 1, 2018. Any participants who filled in the baseline questionnaire between September 1 2018 and September 30 2018 and were randomised to a contract offer started their contracts on October 1, 2018.

B Factors associated with successful contract completion

B.1 Trait self-control and time preferences

We conduct principal component analysis (PCA) to combine the raw measures of self-control, impatience and present bias into two indices of time preferences. The index of time preferences is the first principal component of a set of indicators for being very impatient, rather impatient, quite patient and very patient, and having present-biased preferences, all assigned using responses to Multiple Price List tasks.

We additionally collect information on self-reported impulsiveness, tendency to postpone things, frequency of thoughts about the future, and worry about unemployment, and combine responses to these questions into an index of thoughts and behaviours. The index of thoughts and behaviours is the first principal component of a set of four questions asking participants to rate on a scale from one to ten how impulsive they are, how often they tend to postpone things, how much they think about the future, and how much they worry about unemployment. We additionally construct an index of time preferences and self-control, which is the first principal component of the above five indicators of time preferences and trait self-control.

Table B.1.1: Differences in the effect of a soft or hard contract on monthly visits, by trait self-control and time preferences (PCA)

	Dependent variable: Monthly visits						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Soft contract	0.653** (0.268)	0.385 (0.277)	0.515* (0.280)	0.522** (0.266)	0.562** (0.279)	0.576** (0.275)	0.547** (0.276)
x Self control	0.146 (0.271)				0.195 (0.275)	0.325 (0.296)	
x Factor time preferences		-0.227 (0.289)			-0.0552 (0.293)	-0.0747 (0.289)	
x Factor time preferences & self control			-0.103 (0.301)				-0.153 (0.293)
x Factor thoughts & behaviours				-0.00505 (0.322)		0.281 (0.340)	0.171 (0.314)
Hard contract	1.284*** (0.276)	1.092*** (0.284)	1.264*** (0.285)	1.173*** (0.275)	1.245*** (0.284)	1.253*** (0.281)	1.265*** (0.282)
x Self control	0.0133 (0.289)				0.0384 (0.305)	0.267 (0.319)	
x Factor time preferences		-0.306 (0.288)			-0.188 (0.296)	-0.208 (0.292)	
x Factor time preferences & self control			-0.158 (0.289)				-0.276 (0.286)
x Factor thoughts & behaviours				0.0473 (0.320)		0.391 (0.332)	0.352 (0.315)
Self control	0.627*** (0.188)				0.582*** (0.192)	0.314 (0.207)	
Factor time preferences		0.0662 (0.215)			-0.0149 (0.218)	0.0245 (0.214)	
Factor time preferences & self control			-0.251 (0.222)				-0.0817 (0.214)
Factor thoughts & behaviours				-0.545** (0.240)		-0.583** (0.238)	-0.717*** (0.216)
Constant	5.578*** (0.218)	5.726*** (0.223)	5.643*** (0.226)	5.624*** (0.215)	5.634*** (0.226)	5.609*** (0.223)	5.614*** (0.223)
Observations	6244	6084	5828	6440	5828	5796	5796
Individuals	1561	1521	1457	1610	1457	1449	1449
R-squared	0.042	0.021	0.027	0.033	0.041	0.048	0.040
Controls	No	No	No	No	No	No	No

Notes: Cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.1.2: Differences in the effect of a soft or hard contract on the probability of achieving one's baseline goal in a given month, by trait self-control and time preferences (PCA)

	Dependent variable: Baseline goal achieved, monthly						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Soft contract	0.0435*** (0.0150)	0.0363** (0.0153)	0.0401** (0.0157)	0.0383*** (0.0146)	0.0420*** (0.0157)	0.0421*** (0.0155)	0.0414*** (0.0155)
x Self control	0.0116 (0.0155)				0.0157 (0.0154)	0.0208 (0.0159)	
x Factor time preferences		-0.0172 (0.0155)			-0.0111 (0.0160)	-0.0116 (0.0158)	
x Factor time preferences & self control			-0.0154 (0.0162)				-0.0167 (0.0162)
x Factor thoughts & behaviours				0.000460 (0.0159)		0.0107 (0.0170)	0.00400 (0.0165)
Hard contract	0.0977*** (0.0165)	0.0986*** (0.0170)	0.102*** (0.0173)	0.0984*** (0.0162)	0.102*** (0.0173)	0.103*** (0.0172)	0.103*** (0.0172)
x Self control	-0.00997 (0.0165)				-0.0115 (0.0173)	-0.00314 (0.0181)	
x Factor time preferences		-0.0148 (0.0159)			-0.0163 (0.0163)	-0.0189 (0.0162)	
x Factor time preferences & self control			-0.0105 (0.0164)				-0.0172 (0.0165)
x Factor thoughts & behaviours				0.00973 (0.0163)		0.0145 (0.0179)	0.0178 (0.0173)
Self control	0.0214** (0.00867)				0.0198** (0.00895)	0.00439 (0.00928)	
Factor time preferences		0.00419 (0.0104)			0.00379 (0.0106)	0.00603 (0.0104)	
Factor time preferences & self control			-0.00431 (0.0107)				0.00432 (0.0108)
Factor thoughts & behaviours				-0.0320*** (0.0105)		-0.0334*** (0.0115)	-0.0359*** (0.0110)
Constant	0.0781*** (0.0118)	0.0818*** (0.0121)	0.0800*** (0.0123)	0.0787*** (0.0117)	0.0800*** (0.0123)	0.0782*** (0.0123)	0.0780*** (0.0123)
Observations	6244	6084	5828	6440	5828	5796	5796
Individuals	1561	1521	1457	1610	1457	1449	1449
R-squared	0.025	0.022	0.024	0.028	0.028	0.033	0.032
Controls	No	No	No	No	No	No	No

Notes: Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.1.3: Differences in the effect of a soft or hard contract on the probability of meeting one's target, by trait self-control and time preferences (PCA)

	Dependent variable: Target met						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hard contract	0.0505*	0.0606**	0.0661**	0.0495*	0.0634**	0.0663**	0.0681**
	(0.0259)	(0.0261)	(0.0269)	(0.0253)	(0.0270)	(0.0272)	(0.0272)
x Self control	0.0343				0.0250	0.0246	
	(0.0262)				(0.0271)	(0.0299)	
x Factor time preferences		-0.0195			-0.0178	-0.0204	
		(0.0222)			(0.0233)	(0.0233)	
x Factor time preferences & self control			-0.0252				-0.0279
			(0.0236)				(0.0238)
x Factor thoughts & behaviours				-0.0120		-0.00317	-0.00890
				(0.0242)		(0.0284)	(0.0262)
Self control	0.0304*				0.0337**	0.0260	
	(0.0169)				(0.0168)	(0.0185)	
Factor time preferences		-0.0213			-0.0140	-0.0121	
		(0.0161)			(0.0169)	(0.0169)	
Factor time preferences & self control			-0.0253				-0.0186
			(0.0172)				(0.0173)
Factor thoughts & behaviours				-0.0261		-0.0158	-0.0251
				(0.0170)		(0.0193)	(0.0178)
Constant	0.154***	0.148***	0.149***	0.150***	0.152***	0.151***	0.150***
	(0.0170)	(0.0165)	(0.0171)	(0.0165)	(0.0173)	(0.0174)	(0.0173)
Observations	884	869	831	915	831	826	826
R-squared	0.021	0.014	0.019	0.012	0.028	0.030	0.025
Controls	No	No	No	No	No	No	No

Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.2 Money put at stake

We compare our three outcomes among those who accepted a contract with- and without stakes. We also investigate whether the amount put at stake was associated with a greater likelihood of successful contract completion. We investigate these questions among the set of individuals who accepted a contract, using the following regression strategy:

$$Y_i = \alpha + \delta I_i + \gamma S_i + \epsilon_i \quad (3)$$

where Y_i denotes the dependent variable of interest (monthly gym visits, achieving one's baseline monthly goal, successfully completing one's contract), I_i denotes whether or not the participant accepted a contract with incentive and S_i denotes either the amount of money put at stake or a binary indicator of having put 1000 SEK at stake²⁶. We investigate the relationship between accepting a hard contract on each outcome by itself (I_i as the only dependent variable) and in conjunction with the amount put at stake (S_i).

Table B.2.1: Associations between accepting a hard contract and amount of money put at stake, and monthly gym visits, the probability of achieving one's baseline monthly goal and successful contract completion

	Monthly visits			Monthly goal met			Contract target met		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Accepted stakes	-0.362 (0.398)	-0.386 (0.597)	-0.450 (0.489)	0.0231 (0.0285)	0.0165 (0.0434)	0.00649 (0.0345)	0.0465 (0.0392)	0.0702 (0.0593)	0.0495 (0.0472)
Amount at stake		0.000196 (0.000809)			-0.00000547 (0.0000610)			-0.0000436 (0.0000790)	
1000 SEK at stake			0.418 (0.663)			0.0172 (0.0494)			-0.00724 (0.0634)
Constant	6.761*** (0.236)	6.679*** (0.277)	6.679*** (0.277)	0.196*** (0.0158)	0.206*** (0.0186)	0.206*** (0.0186)	0.177*** (0.0237)	0.177*** (0.0237)	0.177*** (0.0237)
Observations	2128	1756	1756	2128	1756	1756	439	439	439
Individuals	532	439	439	532	439	439	439	439	439
R-squared	0.001	0.001	0.001	0.001	0.000	0.000	0.003	0.004	0.003
Controls	No	No	No	No	No	No	No	No	No

Notes: Four individuals who accepted a contract did not provide a sex. This explains the discrepancy between the total observations in columns 1-3 and the total obtained by summing the total male and female observations. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B.3 Self-reported baseline training

²⁶40% of participants put 1000 SEK at stake, 60% of participants put between 10 and 500 SEK at stake.

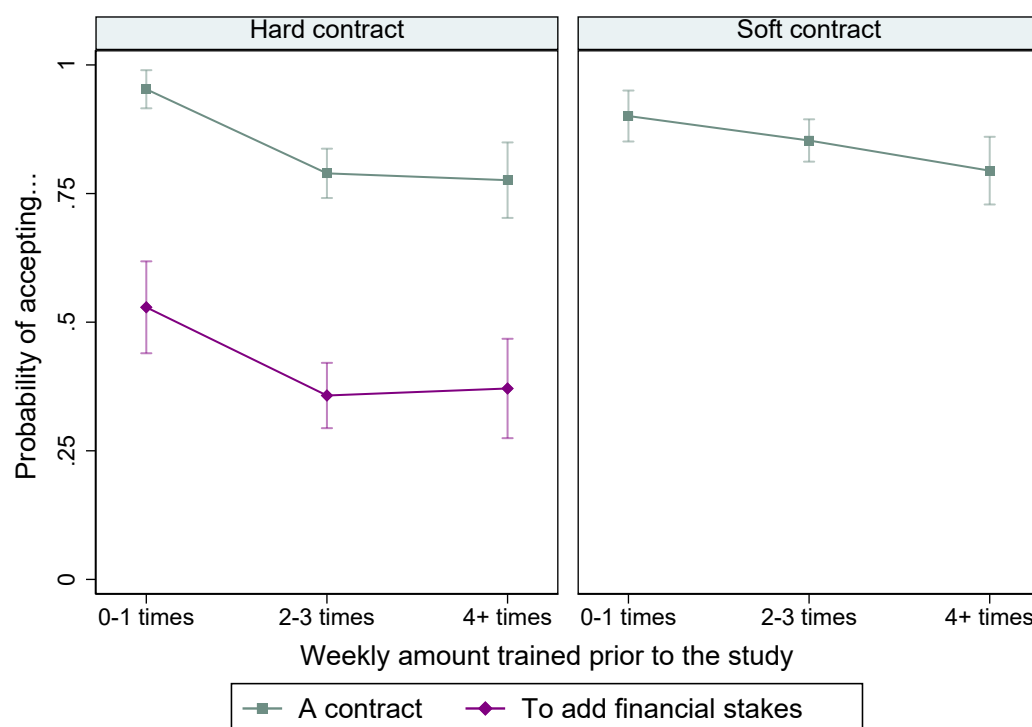


Figure B.3.1: Correlations between the weekly amount trained prior to the study and probabilities of accepting a contract, and if applicable, accepting to add financial stakes

Notes: The probabilities of adding stakes are conditional on accepting a contract (and being randomised to the hard commitment contract group).

B.4 Socio-demographic characteristics

Table B.4.1: Heterogeneity in the effect of a soft or hard contract on monthly visits to the gym, by sex, age, foreign background, education and income

	Dependent variable: Monthly visits					
	(1)	(2)	(3)	(4)	(5)	(6)
Soft contract	0.547* (0.287)	0.737** (0.318)	0.270 (0.417)	0.659** (0.295)	0.911 (0.672)	0.540 (0.589)
x Male		-0.634 (0.719)				
x Aged over 50 years			0.613 (0.575)			
x Born abroad				-0.915 (1.065)		
x Post-secondary educ					-0.487 (0.742)	
x Earns $\geq 40,000$ /month						0.0103 (0.674)
Hard contract	1.214*** (0.293)	1.239*** (0.329)	1.372*** (0.438)	1.231*** (0.299)	1.150 (0.702)	1.407** (0.606)
x Male		-0.161 (0.699)				
x Aged over 50 yrs			-0.285 (0.589)			
x Born abroad				-0.0368 (1.111)		
x Post-secondary educ					0.0848 (0.772)	
x Earns $\geq 40,000$ /month						-0.271 (0.691)
Male		1.338*** (0.518)				
Aged over 50 years			0.507 (0.430)			
Born abroad				0.856 (0.795)		
Post-secondary educ					-0.665 (0.566)	
Earns $\geq 40,000$ /month						0.0551 (0.513)
Constant	5.662*** (0.231)	5.306*** (0.256)	5.408*** (0.346)	5.553*** (0.238)	6.208*** (0.517)	5.621*** (0.456)
Observations	5780	5780	5780	5780	5780	5780
Individuals	1445	1445	1445	1445	1445	1445
R-squared	0.022	0.030	0.027	0.023	0.026	0.022
Controls	No	No	No	No	No	No

Notes: Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.4.2: Heterogeneity in the effect of a soft or hard contract on the probability of achieving one's baseline monthly goal, by sex, age, foreign background, education and income

	Dependent variable: Baseline goal achieved, monthly					
	(1)	(2)	(3)	(4)	(5)	(6)
Soft contract	0.0379** (0.0159)	0.0522*** (0.0179)	0.0391* (0.0214)	0.0425** (0.0170)	0.0683** (0.0336)	0.0134 (0.0316)
x Male		-0.0633 (0.0385)				
x Aged over 50 years			0.000530 (0.0317)			
x Born abroad				-0.0426 (0.0489)		
x Post-secondary educ					-0.0381 (0.0381)	
x Earns $\geq 40,000$ /month						0.0336 (0.0366)
Hard contract	0.0875*** (0.0173)	0.0972*** (0.0196)	0.0838*** (0.0232)	0.0918*** (0.0185)	0.113*** (0.0410)	0.0790** (0.0341)
x Male		-0.0388 (0.0414)				
x Aged over 50 yrs			0.00922 (0.0345)			
x Born abroad				-0.0380 (0.0522)		
x Post-secondary educ					-0.0316 (0.0452)	
x Earns $\geq 40,000$ /month						0.0113 (0.0396)
Male		0.0357 (0.0261)				
Aged over 50 years			0.0338 (0.0209)			
Born abroad				0.00900 (0.0335)		
Post-secondary educ					0.0236 (0.0249)	
Earns $\geq 40,000$ /month						-0.0207 (0.0254)
Constant	0.0852*** (0.0128)	0.0760*** (0.0138)	0.0680*** (0.0166)	0.0841*** (0.0131)	0.0664*** (0.0228)	0.100*** (0.0229)
Observations	5780	5780	5780	5780	5780	5780
Individuals	1445	1445	1445	1445	1445	1445
R-squared	0.019	0.020	0.021	0.019	0.019	0.019
Controls	No	No	No	No	No	No

Notes: Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.4.3: Heterogeneity in the effect of a hard contract on meeting one's contract target, by sex, age, foreign background, education and income

	Dependent variable: Target met				
	(1)	(2)	(3)	(4)	(5)
Hard contract	0.0483*	0.0819**	0.0399	0.0645	0.0466
	(0.0285)	(0.0333)	(0.0262)	(0.0558)	(0.0456)
x Male	-0.00279				
	(0.0612)				
x Aged over 50 yrs		-0.0732			
		(0.0503)			
x Born abroad			0.0490		
			(0.0869)		
x Post-secondary educ				-0.0222	
				(0.0625)	
x Earns $\geq 40,000$ /month					-0.0100
					(0.0551)
Male	0.000667				
	(0.0414)				
Aged over 50 years		0.0721**			
		(0.0328)			
Born abroad			0.0287		
			(0.0562)		
Post-secondary educ				0.00809	
				(0.0380)	
Earns $\geq 40,000$ /month					0.0295
					(0.0359)
Constant	0.150***	0.115***	0.148***	0.143***	0.127***
	(0.0182)	(0.0199)	(0.0171)	(0.0331)	(0.0297)
Observations	915	921	922	917	858
Individuals	915	921	922	917	858
R-squared	0.004	0.009	0.006	0.004	0.004
Controls	No	No	No	No	No

Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C Additional analyses and robustness

C.1 Inclusion of controls

Table C.1.1: Intention to treat (ITT) estimates for the effect of being offered a soft or a hard contract on monthly visits to the gym, and the probabilities of achieving one's baseline monthly goal and meeting one's contract target, with post-Lasso controls

	Monthly visits			Monthly goal met			Contract target met
	(1) Months 1-4	(2) Months 5-8	(3) Δ	(4) Months 1-4	(5) Months 5-8	(6) Δ	(7)
Soft contract	0.476* (0.247)	0.249 (0.257)	-0.242 (0.184)	0.0370** (0.0145)	0.0147 (0.0144)	-0.0229* (0.0139)	
Hard contract	1.159*** (0.258)	0.531** (0.254)	-0.658*** (0.192)	0.0941*** (0.0162)	0.0268* (0.0145)	-0.0692*** (0.0154)	0.0460* (0.0249)
Control group mean	5.500	5.141		0.107	0.111		0.150
R-squared	0.130	0.124		0.032	0.023		0.004
Observations	6456	6456		6504	6504		926
Individuals	1614	1614		1626	1626		926
Month-year FEs	Yes	Yes		Yes	Yes		No
Controls	Yes	Yes		Yes	Yes		Yes

Notes: Individuals analysed according to the treatment group to which they were randomised. Not all individuals randomised actually submitted the baseline survey. Some participants were not listed in the training data obtained from the gym (for some or all months). These individuals are coded as having trained zero times in a given month, thus the number of individuals contributing to months 1-4 and 5-8 is the same. The estimates for the difference between months 1-4 and 5-8 (columns 3 and 6) are obtained from a regression that resembles equation (1) but where a dummy variable that equals 1 for months 5-8 is included and interacted with dummy variables for each treatment group. The “control group mean” in column 7 refers to the mean in the soft contract group. Controls for the analyses were selected using the Lasso double selection method based on data from the entire time period (months 1-8). The vector of controls differs depending on the dependent variable. When the dependent variable is monthly visits, the selected vector of controls includes age (continuous) and indicators for: i) reporting fairly or very good health (in Swedish: ganska gott or mycket gott), ii) reporting vigorous exercise at least three times per week, iii) reporting wanting to train at the gym at least three times per week, and iv) being obese (assigned based on self-reported height and weight). The only selected control variable when the outcome is achieving one's baseline monthly goal is an indicator for reporting fairly or very good health. No control variables are selected when the outcome is whether or not a participant met their contract target. All control variables were measured in the baseline survey. Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.2 Alternative modelling specifications

Table C.2.1: Intention to treat (ITT) estimates from probit and Poisson models for the effect of being offered a soft or hard contract on monthly visits to the gym, and the probabilities of achieving one's baseline monthly goal and meeting one's contract target

	Months 1-4			Months 5-8	
	(1) Monthly visits	(2) Monthly goal met	(3) Contract target met	(4) Monthly visits	(5) Monthly goal met
Panel A: Raw estimates					
Soft contract	0.0804* (0.0470)	0.343** (0.136)		0.0407 (0.0523)	0.134 (0.141)
Hard contract	0.188*** (0.0462)	0.763*** (0.130)	0.323* (0.175)	0.0877* (0.0506)	0.246* (0.136)
Constant	1.733*** (0.0382)	-2.352*** (0.121)	-1.735*** (0.127)	1.831*** (0.0399)	-1.687*** (0.115)
Panel B: Marginal effects					
Soft contract	0.485* (0.283)	0.0432** (0.0173)		0.219 (0.281)	0.0145 (0.0153)
Hard contract	1.136*** (0.277)	0.0962*** (0.0165)	0.0458* (0.0247)	0.472* (0.272)	0.0267* (0.0148)
Model	Poisson	Probit	Probit	Poisson	Probit
R-squared	0.012	0.025	0.004	0.011	0.012
Observations	6516	6516	926	6516	6516
Individuals	1629	1629	926	1629	1629
Controls	No	No	No	No	No
Month-year FEs	Yes	Yes	No	Yes	Yes

Notes: Intention to treat (individuals analysed according to the treatment group to which they were randomised). Not all individuals randomised actually submitted the baseline survey. Individuals not listed in the gym data are coded as having trained zero times in a given month. Cluster robust standard errors in parentheses where appropriate (columns 1-2, 4-5). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.3 Individual fixed effects approach

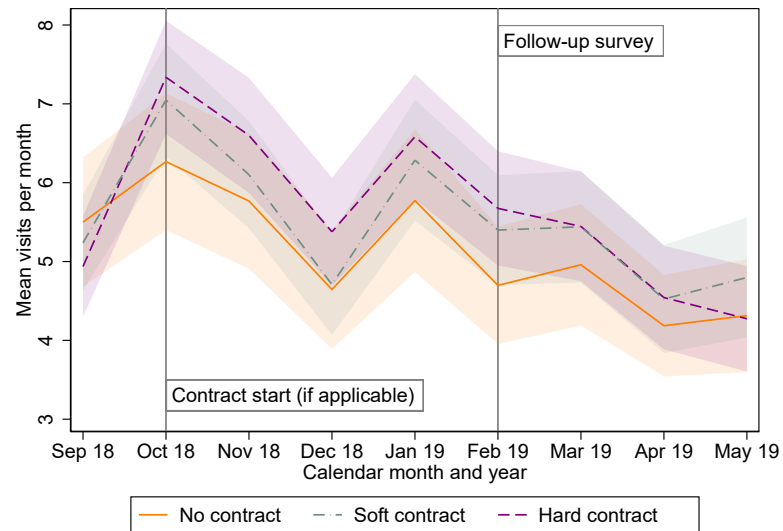


Figure C.3.1: Mean monthly gym visits by treatment group, October starters only

Notes: 515 participants completed the baseline survey in September 2018, starting their contracts on October 1, 2018, if applicable. These individuals consented for us to access their gym data from September 1, 2018 and up to twelve months onwards. Participants randomised to a contract offer (soft or hard) were free to choose their contract length (from one to four months) and monthly target. The estimates presented are from a regression of monthly gym visits on dummies for the interaction between indicators of calendar month and treatment group. No additional variables were controlled for. Standard errors clustered at the individual level.

Table C.3.1: Comparison of ITT estimates of the effect of being offered a soft or a hard contract on monthly visits to the gym: main specification, main specification for October starters only, individual fixed effects approach for October starters only, individual fixed effects approach for entire sample

	(1) Main spec.	(2) Main spec., October only	(3) Indiv. FE, October only	(4) Indiv. FE, all
Soft contract	0.460* (0.267)	0.422 (0.501)	0.684* (0.361)	
x Months 5-8	-0.247 (0.185)	0.0786 (0.349)	0.0786 (0.228)	-0.247* (0.127)
Hard contract	1.140*** (0.276)	0.861* (0.508)	1.424*** (0.357)	
x Months 5-8	-0.668*** (0.191)	-0.416 (0.369)	-0.416* (0.226)	-0.668*** (0.129)
Months 5-8	0.609* (0.319)	-2.312*** (0.348)	-1.356*** (0.278)	0.449** (0.186)
Control group mean, months 1-4	5.500	5.591	5.591	5.500
R-squared	0.024	0.032	0.077	0.059
Observations	13032	4120	4635	13032
Individuals	1629	515	515	1629
Month-year FEs	Yes	Yes	Yes	Yes

Notes: Participants consented for us to access their gym data from September 1, 2018 and up to twelve months onwards. We are thus able to estimate the effect of commitment contracts starting for the 515 individuals who completed the baseline survey in September and started their contract, if applicable, on October 1, 2018, using an individual fixed effects approach. Using this approach, we can also investigate the effect of the experimental period ending for all participants. Because the choice of length of contract is potentially endogenous, we estimate the effect of commitment contracts in the experimental period (months one to four) versus the post-experimental period (months five to eight), regardless of chosen contract length. Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.4 *De facto* treatment status

Table C.4.1: Descriptive statistics for those who made the mobile transfer versus those who did not

	Mean(transfer=0)	Mean(transfer=1)	Diff.	Std. Error	Obs.
Age (in years)	49.377	45.847	3.530*	2.045	179
Male	0.295	0.271	0.024	0.071	179
Foreign-born	0.115	0.094	0.021	0.048	178
Have post-secondary education	0.767	0.829	-0.062	0.063	177
Earn >40,000SEK/month	0.804	0.703	0.100	0.072	174
Reports good or very good health	0.754	0.805	-0.051	0.065	179
BMI	24.517	24.731	-0.214	0.521	178
Current daily smoker	0.016	0.017	-0.001	0.020	179
Often/always sleep badly	0.148	0.162	-0.015	0.058	178
Experience daily stress	0.131	0.042	0.089**	0.041	179
>30 min "everyday" exercise daily	0.344	0.314	0.031	0.074	179
Trains >2 times/week	0.656	0.636	0.020	0.076	179
Trains >2 times/week at places other than gym	0.328	0.288	0.040	0.073	179
Aims to train >2 times/week	1.000	1.000	0.000	0.000	179
Aims to train >2 times/week at gym	0.984	0.915	0.068*	0.038	179
Active gym membership	1.000	0.983	0.017	0.017	179
Self control (13 = none, 65 = max)	43.633	44.150	-0.517	1.148	173
Have time consistent preferences	0.672	0.766	-0.094	0.072	165
Impatient	0.103	0.118	-0.015	0.052	168

Notes: P-values from t-test comparing mean of characteristic among those who completed the mobile transfer versus those who did not. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

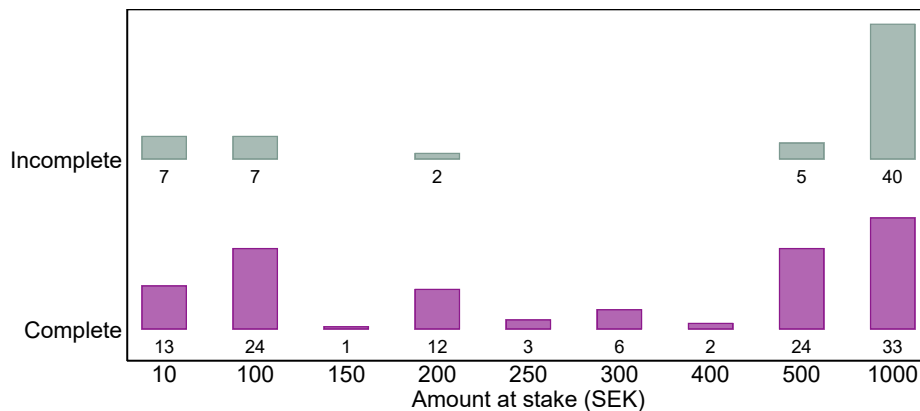


Figure C.4.1: Count of participants who completed and did not complete the mobile transfer, by amount of money put at stake

Notes: In total, 179 participants accepted to add stakes to their contract and selected an monetary amount to put at stake. 118 of these individuals actually completed the mobile transfer while 61 did not. 7 of the 61 participants who did not notified us that they had changed their mind regarding the transfer of the money.

Table C.4.2: Instrumental Variable analyses assigning treatment with a hard contract according to whether actually transferred money

	Months 1-4			Months 5-8	
	(1) Monthly visits	(2) Monthly goal met	(3) Contract target met	(4) Monthly visits	(5) Monthly goal met
Soft contract	0.542* (0.315)	0.0431** (0.0173)		0.272 (0.324)	0.0170 (0.0172)
Hard contract	3.664*** (1.086)	0.318*** (0.0709)	0.171* (0.0921)	1.383 (1.098)	0.0732 (0.0617)
Control group mean	5.768	0.114	0.159	5.469	0.119
Observations	6516	6516	926	6516	6516
Individuals	1629	1629	926	1629	1629
Month-year FEs	No	No	No	No	No

Notes: Cluster robust standard errors in parentheses where appropriate (columns 1, 2, 4 and 5). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.5 Considering only those who submitted the baseline survey

Table C.5.1: ITT estimates of the effect of being offered a soft or hard contract on monthly visits to the gym, and the probabilities of achieving one's baseline monthly goal and meeting one's contract target, for those who submitted the baseline survey only

	Monthly visits			Monthly goal met			Contract target met
	(1) Months 1-4	(2) Months 5-8	(3) Δ	(4) Months 1-4	(5) Months 5-8	(6) Δ	(7)
Soft contract	0.486* (0.269)	0.229 (0.277)	-0.257 (0.186)	0.0396*** (0.0149)	0.0145 (0.0147)	-0.0251* (0.0140)	
Hard contract	1.306*** (0.282)	0.526* (0.278)	-0.781*** (0.196)	0.110*** (0.0171)	0.0270* (0.0151)	-0.0825*** (0.0158)	0.0556** (0.0262)
Control group mean	5.500	5.141		0.107	0.111		0.152
R-squared	0.023	0.019		0.024	0.009		0.005
Observations	6276	6276		6276	6276		878
Individuals	1569	1569		1569	1569		878
Month-year FEs	Yes	Yes		Yes	Yes		No

Notes: Intention to treat (individuals analysed according to randomisation groups). Cluster robust standard errors in parentheses where appropriate (columns 1-6). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.6 Investigating potential spillovers

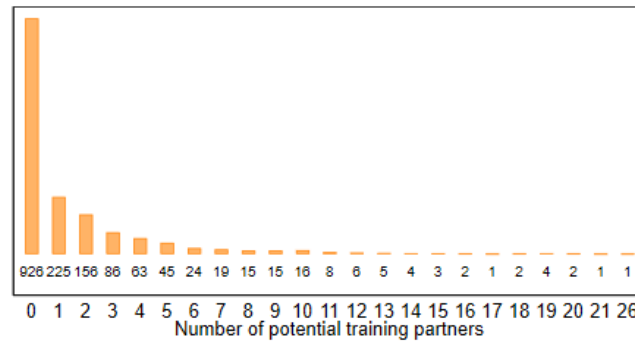


Figure C.6.1: Count of participants by number of potential training partners

Notes: Number of potential partners indicated on the x-axis. Count of participants with the corresponding number of potential partners shown above the x-axis. Potential training partners are defined as participants who entered the gym within 15 minutes of each other on at least four occasions during the eight months of the study (i.e. during both the experimental and post-experimental periods). Note that we only obtained gym visit data for study participants. 703 participants were identified as having at least one potential training partner. The maximum number of potential training partners identified was 26.

Table C.6.1: Spillover effects among potential training partners

	Months 1-4		Months 5-8	
	(1) Monthly visits	(2) Monthly goal met	(3) Monthly visits	(4) Monthly goal met
Nr. potential training partners	0.342*** (0.127)	0.0200*** (0.00589)	0.349*** (0.110)	0.0186*** (0.00677)
Nr. treated pot. training partners (soft)	1.704*** (0.395)	0.0111 (0.0202)	1.851*** (0.382)	0.0307 (0.0232)
Nr. treated pot. training partners (hard)	1.305*** (0.444)	0.0479* (0.0282)	1.311*** (0.451)	0.0322 (0.0278)
Soft contract	0.641** (0.253)	0.0460*** (0.0144)	0.341 (0.251)	0.0177 (0.0139)
× Nr. pot. training partners	-0.0470 (0.155)	-0.0116 (0.00860)	0.0244 (0.148)	-0.00927 (0.00978)
× Nr. pot. training partners (soft)	-0.426 (0.510)	0.00723 (0.0307)	-0.594 (0.547)	0.00155 (0.0344)
× Nr. pot. training partners (hard)	0.420 (0.558)	0.0293 (0.0384)	0.423 (0.580)	0.0431 (0.0372)
Hard contract	1.326*** (0.268)	0.0994*** (0.0165)	0.626** (0.254)	0.0280** (0.0142)
× Nr. pot. training partners	0.174 (0.171)	-0.00294 (0.00915)	0.194 (0.154)	0.00221 (0.00946)
× Nr. pot. training partners (soft)	-0.896* (0.515)	0.0362 (0.0341)	-0.886* (0.506)	0.00103 (0.0330)
× Nr. pot. training partners (hard)	-0.671 (0.544)	-0.0340 (0.0370)	-0.675 (0.544)	-0.0190 (0.0372)
Constant	4.121*** (0.203)	0.0293** (0.0117)	4.668*** (0.204)	0.110*** (0.0132)
Observations	6516	6516	6516	6516
Individuals	1629	1629	1629	1629
Month-year FEs	Yes	Yes	Yes	Yes

Notes: Potential training partners are defined as participants who entered the gym within 15 minutes of each other on at least four occasions during the eight months of the study (i.e. during both the experimental and post-experimental periods). Note that we only obtained gym visit data for study participants. The maximum number of potential training partners identified for any participant was 26. The maximum number of potential training partners treated with either a soft or hard contract was four. Individuals for whom we identify no potential training partners (defined as above) are included in the regressions and coded as having zero potential training partners. Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

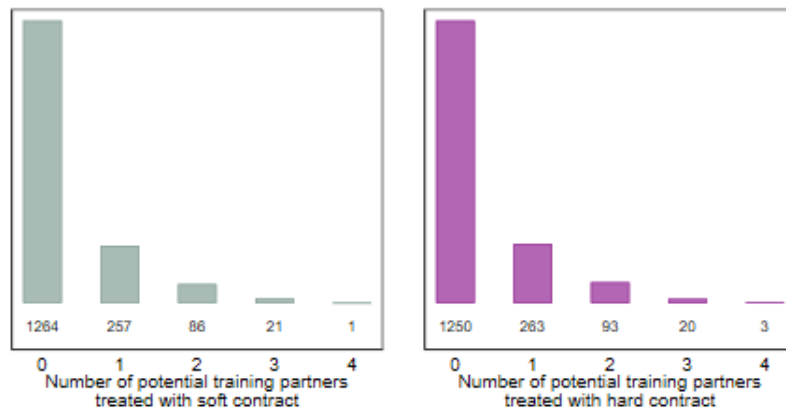


Figure C.6.2: Count of participants by number of potential training partners treated with either a soft or a hard contract

Notes: Number of potential partners indicated on the x-axis. Count of participants with the corresponding number of potential partners shown above the x-axis. Potential training partners are defined as participants who entered the gym within 15 minutes of each other on at least four occasions during the eight months of the study (i.e. during both the experimental and post-experimental periods). Note that we only obtained gym visit data for study participants. The maximum number of potential training partners treated with either a soft or hard contract was four. The total number of treated potential training partners (offered either a soft or hard contract) could be larger than four.

D Supplementary analyses

D.1 Persistence in the effect of commitment contracts

Table D.1.1: ITT estimates of persistence in the effect of being offered a soft or hard contract on monthly visits to the gym and the probability of achieving one's baseline monthly goal

	Monthly visits		Monthly goal met	
	(1)	(2)	(3)	(4)
Soft contract	0.460* (0.268)	0.387 (0.236)	0.0372** (0.0147)	0.0369** (0.0146)
× Month=5	-0.211 (0.214)	-0.194 (0.213)	-0.0172 (0.0203)	-0.0157 (0.0203)
× Month=6	-0.0924 (0.230)	-0.101 (0.230)	-0.0317* (0.0186)	-0.0303 (0.0185)
× Month=7	-0.349 (0.242)	-0.365 (0.241)	-0.0200 (0.0194)	-0.0205 (0.0194)
× Month=8	-0.370 (0.233)	-0.377 (0.231)	-0.0256 (0.0173)	-0.0261 (0.0173)
Hard contract	1.140*** (0.276)	1.123*** (0.247)	0.0966*** (0.0163)	0.0928*** (0.0160)
× Month=5	-0.344 (0.212)	-0.309 (0.213)	-0.0402* (0.0214)	-0.0384* (0.0213)
× Month=6	-0.567** (0.238)	-0.545** (0.240)	-0.0606*** (0.0210)	-0.0588*** (0.0210)
× Month=7	-0.898*** (0.251)	-0.883*** (0.254)	-0.0814*** (0.0214)	-0.0816*** (0.0214)
× Month=8	-0.863*** (0.244)	-0.849*** (0.246)	-0.0980*** (0.0192)	-0.0982*** (0.0193)
Month=5	0.492 (0.323)	0.280 (0.301)	0.00153 (0.0245)	0.000785 (0.0244)
Month=6	0.682 (0.536)	0.308 (0.494)	0.0103 (0.0341)	0.00971 (0.0337)
Month=7	1.401* (0.766)	0.868 (0.700)	0.0406 (0.0453)	0.0426 (0.0448)
Month=8	1.717* (0.958)	1.021 (0.870)	0.0517 (0.0530)	0.0547 (0.0524)
Control group mean				
R-squared	0.024	0.182	0.017	0.031
Observations	13032	12848	13032	13008
Individuals	1629	1606	1629	1626
Controls	No	Post-Lasso	No	Post-Lasso
Month-year FEs	Yes	Yes	Yes	Yes

Notes: The months indicate months since entering the study, for months 5-8. Controls for the analyses were selected using the Lasso double selection method based on data from the entire time period (months 1-8). The vector of controls differs depending on the dependent variable. When the dependent variable is monthly visits, the selected vector of controls includes indicators for: i) reporting vigorous exercise at least three times per week, ii) reporting vigorous exercise at least three times per week at places other than the gym, and iii) reporting wanting to train at the gym at least three times per week. The only selected control variable when the outcome is achieving one's baseline monthly goal is an indicator for reporting fairly or very good health. All control variables were measured in the baseline survey. Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D.2 Differences in the estimated effects across individuals who completed the baseline questionnaire in August versus September

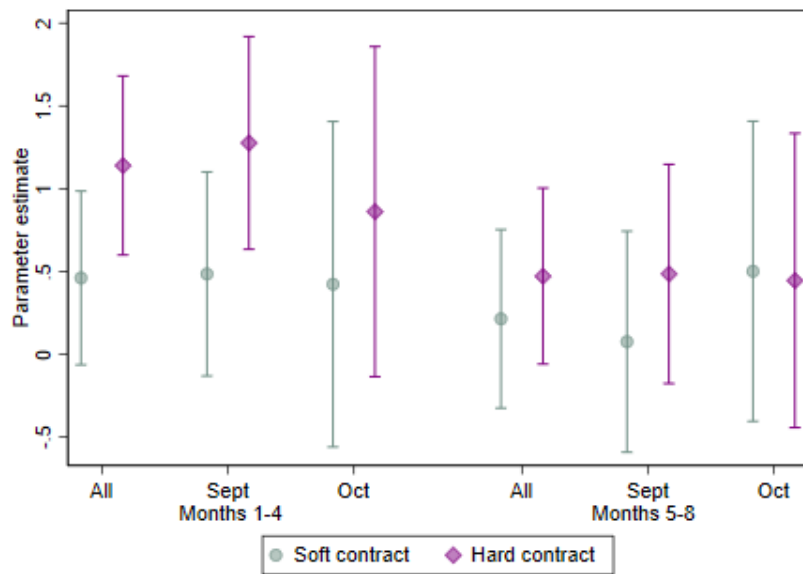


Figure D.2.1: Estimates of the effect of a soft or hard contract on monthly visits to the gym, overall and separately for September and October starters

Notes: 1,114 individuals completed the baseline survey in August 2018 and thus entered the “experimental period” on September 1, 2018. 515 individuals completed the baseline survey in September 2018 and thus entered the “experimental period” on October 1, 2018.

D.3 Differences between the baseline monthly goal and contract target

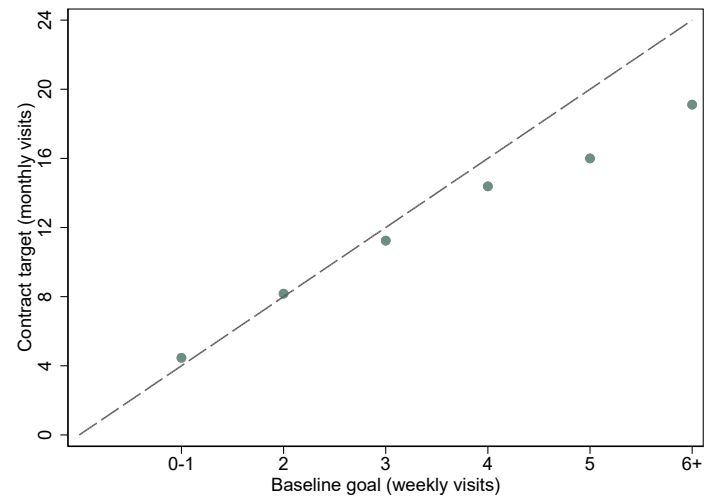


Figure D.3.1: Scatter plot of contract target versus baseline goal

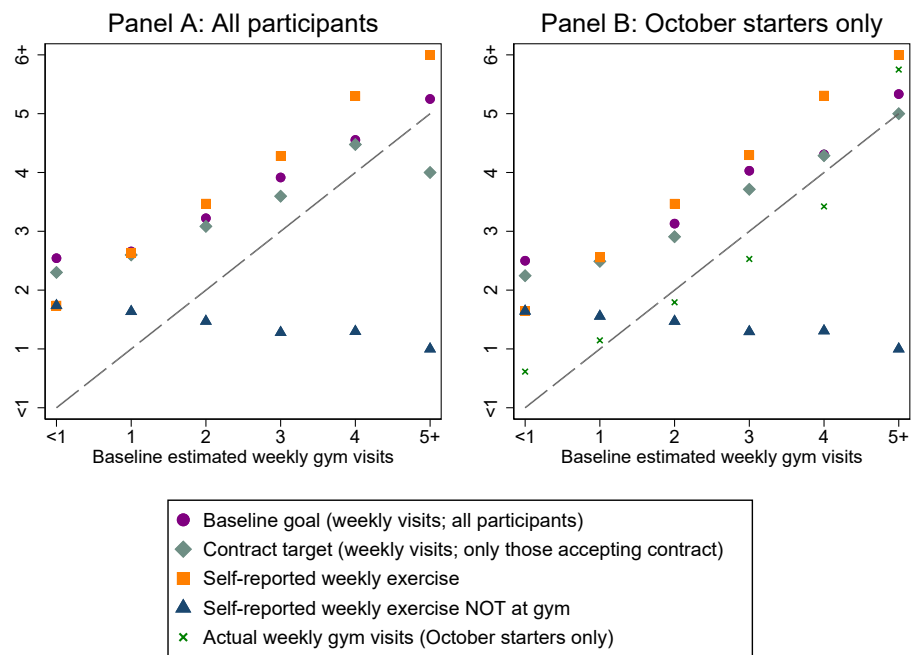


Figure D.3.2: Correlations between baseline goal, contract target, and self-reported and actual exercise prior to entry into the study

Notes: In the baseline survey we asked about the days per week an individual trained for at least 15 minutes, followed by a question about how much of that training (in days) was at places other than the gym. Response options for both questions were: 0-1 times per week, 2 times per week, 3 times per week, 4 times per week, 5 times per week and 6 or more times per week. We compute baseline weekly gym visits by taking the difference between each participant's answers to these two questions.

D.4 The role of nudges

In our pre-analysis plan, we indicated that we would like to investigate the effect of several nudging treatments on contract design. These were i) different default contract target amounts of visits to the gym, ii) an informational nudge about the effectiveness of stakes, and iii) a low (10 SEK) versus high (1000 SEK) financial default when choosing how much money to put at stake (see Appendix Table D.4.1).

We realised *ex post* that we are likely under-powered to make any inferences with regards to these treatments and have thus excluded these investigations from our main analyses.

After selecting the duration of their contract, all individuals who accepted the offer of a contract were randomised to be shown either a default amount of training (gym visits) to be completed during their contract that was *equivalent* to the goal stated in the baseline questionnaire, or equivalent to *one time per week more* than their stated goal. Given our sample size, we had 80% power to detect an effect of the default on monthly visits of at least .69 visits per month. The point estimate obtained in Appendix Table D.4.2 column 1 is, however, considerably smaller. The point estimate is not statistically significant and our sample size does not give us the power to rule this effect out. We have 80% power to detect an effect of the exercise default of accepting stakes of 0.13, or 13 percentage points. The point estimate in column 2 is of -0.035 and is thus considerably smaller than this minimum detectable effect.

Within the hard contract group, some participants were randomised to be presented with an informational nudge about the potential effectiveness of adding financial stakes to their contract. The nudge mentioned that, in other settings, studies have shown that attaching a financial incentive to one's goal can be beneficial and that more people tend to achieve their goals when those goals involve some sort of reward. 532 individuals were randomised to the hard contract group, but since 93 of those individuals turned down the contract offer, the actual size of the sample eligible for exposure to the nudging treatment was 439. Of these, 243 were exposed to the informational nudge. Given 80% statistical power, and the fact that 40.8% of participants in the control group (not exposed to the informational nudge) opted to add stakes, the *ex-post* minimum effect size we would have been able to detect with our sample would have been an increase in take up of stakes of 13 percentage points and an increase in the amount of stakes of 172 SEK. Previous literature on the use of informational nudges (see e.g. Hotard *et al.* (2019)) has generally found smaller effect sizes (percent-wise), and the point estimates obtained in Appendix Table D.4.2 columns 3 and 4 are, indeed, considerably smaller. We have therefore determined that we were under-powered to find an effect of the informational nudge in our case.

Among those individuals who did accept to add stakes to their contract, a group was randomly selected to be presented a default value of 1000 SEK while the other group was presented a default value of 10 SEK. Given that 179 individuals accepted to add stakes, we have 80% power to detect an effect of the default of 172 SEK. The point estimate of 95 SEK presented in Appendix Table D.4.2 column 4 is about 55% the size of the minimum detectable effect. As with the other nudging treatments, we are thus not powered enough to rule out the estimated effect.

While we do not have enough statistical power to properly examine the effects of these different nudging treatments, the estimated effects generally go in the direction we would expect. The higher exercise default is positively associated with the monthly contract target, but negatively associated with the probability of accepting to add stakes. The informational nudge is negatively associated with the probability of adding stakes and positively associated with the amount of money put at stake. The default stakes treatment is positively associated with the amount of money put at stake.

Our results contrast somewhat to Bhattacharya *et al.* (2015), who find significant effects of randomly

assigned default contract lengths on selected contract length. Bhattacharya *et al.* (2015) however collected information on 8,809 users of a website to design and sign commitment contracts and therefore had over 2,800 individuals in each of their three default treatment groups.

Table D.4.1: Detailed overview of treatments

Treatment name		Description
<i>Control group</i>		
1	Control	Baseline questionnaire + follow-up survey after 5 months.
<i>Soft commitment contract</i>		
2a	Contract, low	As 1, but offered contract. If accept, exercise default = baseline goal.
2b	Contract, high	As 1, but offered contract. If accept, exercise default > baseline goal.
<i>Hard commitment contract</i>		
3a1	Incentive, low, 10	As 2, but offered to attach stakes. If accept, default stakes of 10 SEK.
3a1	Incentive, low, 1000	As 2, but offered to attach stakes. If accept, default stakes of 1000 SEK.
3a3	Incentive, low, nudge, 10	As 4, but nudged about effectiveness of stakes.
3a4	Incentive, low, nudge, 1000	As 5, but nudged about effectiveness of stakes.
3b1	Incentive, high, 10	As 3, but offered to attach stakes. If accept, default stakes of 10 SEK.
3b2	Incentive, high, 1000	As 3, but offered to attach stakes. If accept, default stakes of 1000 SEK.
3b3	Incentive, high, nudge, 10	As 8, but nudged about effectiveness of stakes.
3b4	Incentive, high, nudge, 1000	As 9, but nudged about effectiveness of stakes.

Notes: When designing their contract, participants were either presented with a default exercise amount equal to the goal stated earlier in the baseline questionnaire or equivalent to one time per week more than the earlier stated goal. The nudge about the effectiveness of stakes was informational. If accepting to add stakes, participants were either presented with amounts of 10 SEK pre-selected or 1000 SEK pre-selected (the possible options were 10 SEK, 1000 SEK, other).

Table D.4.2: Effect of different nudging treatments

	Monthly target	Accepted stakes		Money at stake
	(1)	(2)	(3)	(4)
Higher exercise default	0.166 (0.243)	-0.0348 (0.0470)		
Nudge about stakes			-0.000756 (0.0473)	3.269 (62.32)
1000kr default				95.26 (62.11)
Constant	10.70*** (0.175)	0.424*** (0.0326)	0.408*** (0.0352)	494.4*** (57.48)
Observations	926	439	439	179
Individuals	926	439	439	179
R-squared	0.001	0.001	0.000	0.014

Notes: Participants could choose a monthly target amount of gym visits of 4, 8, 12, 16, 20 or 24 times per month. Those presented with a *higher exercise default* were presented with a pre-selected default amount that was equivalent to one time more per week than the goal number of weekly visits to the gym they had stated in the baseline questionnaire. The nudge about the effectiveness of stakes was informational. If accepting to add stakes, participants were either presented with amounts of 10 SEK pre-selected or 1000 SEK pre-selected (the possible options were 10 SEK, 1000 SEK, other). Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

E Alternative sample: Uppsala, Sweden

As discussed above, we originally planned to carry out the experiment at the Uppsala, Sweden branch of the gym chain. Unfortunately, we were only able to recruit 191 individuals. Since this was considerably lower than the required sample size of 600 to 2000 individuals specified in the trial pre-registration, we initiated a collaboration with the larger Stockholm branch of the gym. The results presented in the paper pertain to the Stockholm sample. We present descriptive statistics (Table E.1) and estimates of our main results (Table E.2) for the Uppsala sample below. Estimates controlling for individual-level factors selected using the Lasso Double Selection method, are presented in Table E.3. Estimates of the effect of receiving an informational nudge about the effectiveness of stakes and being presented with different financial defaults when adding stakes are presented in Table E.4.

Table E.1: Descriptive statistics for the Uppsala sample

Characteristic	Control		Soft contract		Hard contract		<i>p</i> -value
	Mean [SD]	Obs	Mean [SD]	Obs	Mean [SD]	Obs	
Panel A: Baseline survey							
<i>Demographics</i>							
Age (in years)	42.31 [14.91]	71	44.73 [15.03]	59	43.93 [16.81]	60	0.663
Male	0.239	71	0.241	58	0.133	60	0.242
Foreign-born	0.056	71	0.136	59	0.100	60	0.306
<i>Socioeconomics</i>							
Have post-secondary education	0.732	71	0.797	59	0.783	60	0.656
Earn >40,000SEK/month	0.580	69	0.685	54	0.537	54	0.271
<i>Health</i>							
Reports good or very good health	0.789	71	0.746	59	0.803	61	0.734
BMI	24.28 [3.32]	69	25.05 [3.57]	59	24.38 [3.78]	60	0.426
Current daily smoker	0.014	71	0.000	59	0.000	61	0.432
Often/always sleep badly	0.211	71	0.169	59	0.197	61	0.835
Experience daily stress	0.155	71	0.034	59	0.164	61	0.048
<i>Exercise</i>							
>30 min “everyday” exercise daily	0.521	71	0.559	59	0.377	61	0.105
Trains >=2 times/week	0.761	71	0.695	59	0.492	61	0.004
Trains >=2 times/week at places	0.099	71	0.085	59	0.131	61	0.696
other than gym							
Aims to train >=2 times/week	0.930	71	0.966	59	0.951	61	0.646
Aims to train >=2 times/week at	0.775	71	0.695	59	0.754	61	0.573
gym							
Active gym membership	0.986	71	1.000	59	1.000	61	0.432
<i>Preferences</i>							
Self control (13 = none, 65 = max)	45.69 [7.31]	67	44.00 [6.91]	56	44.10 [9.62]	58	0.417
Present-biased	0.106	66	0.096	52	0.175	57	0.386
Impatient	0.136	66	0.019	52	0.175	57	0.029
							<i>Sum total:</i>
Total observations		71		59		61	191
Panel B: Follow-up survey							
<i>Health</i>							
Reports good or very good health	0.721	68	0.771	48	0.854	48	0.240
BMI	24.24 [3.24]	68	24.79 [3.82]	48	24.45 [3.78]	48	0.714
Current daily smoker	0.015	68	0.000	48	0.000	48	0.496
Often/always sleep badly	0.279	68	0.083	48	0.146	48	0.019
Experience daily stress	0.118	68	0.063	48	0.104	48	0.610
<i>Exercise</i>							
>30 min “everyday” exercise daily	0.515	68	0.583	48	0.500	48	0.679
Trains >=2 times/week	0.691	68	0.521	48	0.563	48	0.145
Trains >=2 times/week at places	0.235	68	0.125	48	0.229	48	0.296
other than gym							
Aims to train >=2 times/week	0.941	68	0.896	48	0.958	48	0.449
Aims to train >=2 times/week at	0.706	68	0.625	48	0.646	48	0.632
gym							
Active gym membership	0.912	68	1.000	48	0.938	48	0.118
<i>Preferences</i>							
Requested trisslott	0.382	68	0.354	48	0.396	48	0.913
							<i>Sum total:</i>
Total observations		68		48		48	164
Response rate		0.958		0.814		0.787	

Notes: The reported *p*-value is from a test for joint equality of the coefficients on each treatment group. These coefficients are obtained from a regression of each study characteristic on an indicator for each treatment group. Standard deviations for non-dichotomous variables are shown in square brackets.

Table E.2: Intention to treat (ITT) estimates for the effect of being offered a soft or a hard contract on monthly visits to the gym, and the probabilities of achieving one's baseline monthly goal and meeting one's contract target: Uppsala sample

	Monthly visits			Monthly goal met			Contract target met
	(1) Months 1-4	(2) Months 5-8	(3) Δ	(4) Months 1-4	(5) Months 5-8	(6) Δ	(7)
Panel A: Ordinary least squares							
Soft contract	-0.0477 (0.881)	-0.0965 (0.967)	-0.0418 (0.664)	0.0518 (0.0428)	-0.00257 (0.0470)	-0.0550 (0.0386)	
Hard contract	-0.146 (0.857)	-0.537 (0.909)	-0.403 (0.661)	0.0773* (0.0428)	0.0265 (0.0476)	-0.0487 (0.0427)	0.131 (0.0850)
Control group mean	5.514	5.856		0.074	0.127		0.130
R-squared	0.019	0.058		0.017	0.030		0.028
Panel B: Instrumental variable estimates							
Soft contract	-0.0626 (1.117)	-0.130 (1.228)	-0.0598 (0.845)	0.0671 (0.0540)	-0.00296 (0.0597)	-0.0706 (0.0495)	
Hard contract	-0.467 (3.081)	-1.880 (3.273)	-1.458 (2.124)	0.191 (0.167)	0.108 (0.166)	-0.0751 (0.153)	0.345 (0.226)
Control group mean	5.000	5.265		0.078	0.181		0.167
R-squared	0.016	0.054		0.015	0.020		.
Observations	764	764		764	764		88
Individuals	191	191		191	191		88
Month-year FEs	Yes	Yes		Yes	Yes		No

Notes: Individuals analysed according to the treatment group to which they were randomised. Not all individuals randomised actually submitted the baseline survey. Some participants were not listed in the training data obtained from the gym (for some or all months). These individuals are coded as having trained zero times in a given month, thus the number of individuals contributing to months 1-4 and 5-8 is the same. The estimates for the difference between months 1-4 and 5-8 (columns 3 and 6) are obtained from a regression that resembles equation (1) but where a dummy variable that equals 1 for months 5-8 is included and interacted with dummy variables for each treatment group. Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E.3: Intention to treat (ITT) estimates for the effect of being offered a soft or a hard contract on monthly visits to the gym, and the probabilities of achieving one's baseline monthly goal and meeting one's contract target: Uppsala sample, with post-Lasso controls

	Monthly visits			Monthly goal met			Contract target met
	(1) Months 1-4	(2) Months 5-8	(3) Δ	(4) Months 1-4	(5) Months 5-8	(6) Δ	(7)
Soft contract	0.252 (0.810)	0.222 (0.896)	-0.0445 (0.664)	0.0518 (0.0428)	-0.00257 (0.0470)	-0.0550 (0.0386)	
Hard contract	0.817 (0.861)	0.459 (0.896)	-0.380 (0.661)	0.0773* (0.0428)	0.0265 (0.0476)	-0.0487 (0.0427)	0.131 (0.0850)
Control group mean	5.514	5.856		0.074	0.127		0.130
R-squared	0.156	0.178		0.017	0.030		0.028
Observations	764	764		764	764		88
Individuals	191	191		191	191		88
Month-year FEs	Yes	Yes		Yes	Yes		No
Controls	Yes	Yes		Yes	Yes		Yes

Notes: Individuals analysed according to the treatment group to which they were randomised. Not all individuals randomised actually submitted the baseline survey. Some participants were not listed in the training data obtained from the gym (for some or all months). These individuals are coded as having trained zero times in a given month, thus the number of individuals contributing to months 1-4 and 5-8 is the same. The estimates for the difference between months 1-4 and 5-8 (columns 3 and 6) are obtained from a regression that resembles equation (1) but where a dummy variable that equals 1 for months 5-8 is included and interacted with dummy variables for each treatment group. Controls for the analyses were selected using the Lasso double selection method based on data from the entire time period (months 1-8). The vector of controls differs depending on the dependent variable. When the dependent variable is monthly visits, the selected vector of controls includes indicators for: i) reporting vigorous exercise at least three times per week, ii) reporting vigorous exercise at least three times per week at places other than the gym, and iii) reporting wanting to train at the gym at least three times per week. No control variables are selected when the outcomes are achieving one's baseline monthly goal or meeting one's contract target. All control variables were measured in the baseline survey. Cluster robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E.4: Effect of receiving a informational nudge about the effectiveness of stakes or being exposed to a higher financial default, among those offered a hard contract: Uppsala sample

	Contract design		Contract outcomes		
	(1) Added stakes	(2) Money at stake	(3) Monthly visits	(4) Monthly goal met	(5) Target met
Nudge about stakes	0.227 (0.150)	-303.6 (232.9)	0.277 (1.869)	-0.0268 (0.101)	-0.0357 (0.151)
1000kr default		91.61 (220.6)	0.117 (1.996)	-0.0610 (0.105)	-0.0397 (0.153)
Constant	0.273*** (0.0973)	673.9*** (217.4)	6.052*** (2.170)	0.226** (0.108)	0.302* (0.153)
Observations	42	16	168	168	42
Individuals	42	16	42	42	42
R-squared	0.055	0.149	0.000	0.006	0.002
Month-year FEs	No	No	Yes	Yes	No

Notes: The nudge about the effectiveness of stakes was informational. If accepting to add stakes, participants were either presented with amounts of 10 SEK pre-selected or 1000 SEK pre-selected (the possible options were 10 SEK, 1000 SEK, other). Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

F Invitation

MOTIONERAR DU SOM DU HADE TÄNKT?

Vi genomför en forskningsstudie om motion.

I studien kommer vi undersöka vad som påverkar hur mycket/ofta människor motionerar.

Vi är särskilt intresserade av att förstå vad som får folk att motionera i enlighet med sina planer/mål.

Vi som genomför studien är forskare vid Lunds universitet.

Professor Ulf-G Gerdtham, docent Erik Wengström och fil dr Linnea Wickström Östervall.

Vill du bidra till forskning och kanske bättre nå upp till dina egna träningsmål?

Läs mer här: motion.lu.se.

G Baseline survey instrument





Avdelning A: Välkommen

```
document.getElementById("button").bgcolor="#00688B"; function showDiv() {  
document.getElementById('welcomeDiv').style.display = "block"; } .mybutton { border-radius: 5px; background-color:  
#00688B; border: none; color: white; padding: 15px 32px; text-align: center; text-decoration: none; display: inline-block; font-  
size: 16px; margin: 4px 2px; cursor: pointer; } Till dig som tränar på Friskis & Sveltis Stockholm
```

Vi genomför en forskningsstudie om motion. Som motionär på Friskis & Sveltis Stockholm är du välkommen att delta. I studien kommer vi att undersöka vad som påverkar hur mycket/ofta människor motionerar. Vi är särskilt intresserade av under vilka omständigheter människor motionerar i enlighet med sina planer/mål.

Studien genomförs av forskare vid Lunds universitet.

Huvudansvarig forskare: Professor Ulf-G Gerdtham, tel. 046-222 48 10, e-post ulf.gerdtham@med.lu.se

Övriga medverkande forskare: Docent Erik Wengström, tel. 046-222 01 23, e-post erik.wengstrom@nek.lu.se Fil Dr Linnea Wickström Östervall, tel. 0708-25 71 93, e-post linneawo@gmail.com

I samtycket berättar vi mer om vilken information vi kommer att be om och hur vi kommer att använda den.

Deltagandet är helt frivilligt och du kan när som helst lämna studien. OBS att du måste vara myndig för att delta!

Om du vill delta i studien ber vi dig godkänna samtycket nedan.

A1. Samtycke till medverkan i studie om motion och träningskontrakt

Det viktigaste i korthet:

Vi behöver den e-postadress du har registrerat hos Friskis & Sveltis Stockholm. Din e-postadress används för att skicka en länk till avslutande enkät och för att ta fram träningsdata från Friskis & Sveltis Stockholm. I och med detta samtycke godkänner du att Friskis & Sveltis Stockholm lämnar ut information om hur du tränat hos dem under studien. Informationen används endast för denna studie. Mobilnummer och kontonummer används endast om du väljer att tacka ja till att "slå vad med dig själv" om att fullfölja ett träningskontrakt. Denna information sparas bara så länge den behövs, för att kunna genomföra återbetalning om kontraktet fullföljs. Laglig grund är samtycke och intresseavvägning. Du kan lämna studien när som helst, tills vi raderat de uppgifter som kan kopplas direkt till en individ. Därefter kan vi inte radera enskildas uppgifter, eftersom vi inte längre kan avgöra vem som är vem. Du måste vara myndig för att delta.

Alla detaljer:

Om du väljer att delta i studien kommer vi be dig svara på frågor om dig, dina träningsvanor och mål, din hälsa och några bakgrundsfrågor. Detta görs online i en inledande och en avslutande enkät.

En del – men inte alla – deltagare kommer att erbjudas att teckna ett träningskontrakt med sig själva. En del av dessa deltagare kommer



1

7

11

9

[illegible]



Vet inte

Annat

[illegible]

Hinner inte pga familjen/sociala engagemang



D4. Hur sover du?

Jag sover oftast gott och tillräckligt. Jag känner mig utvilad när jag vaknar.

Ibland sover jag oroligare eller vaknar under natten och har svårt att somna om. Det händer att jag inte känner mig utvilad när jag vaknar.

Jag sover ofta oroligare eller vaknar under natten och har svårt att somna om. Det händer ofta att jag inte känner mig utvilad när jag vaknar.

Jag sover oroligt eller vaknar under natten och har svårt att somna om. Jag känner mig aldrig utvilad när jag vaknar.

D5. Upplever du att du är stressad? Frågan gäller både på arbete/motsvarande och privat.

D6. Röker du?

Avdelning E: Mer generella frågor

E1. Hur ser du på dig själv?

Markera vad på skalan som bäst passar in på dig.

Inte alls
1

Väldigt
mycket
(ofta)
10

Hur impulsiv är du?

Hur mycket brukar du skjuta upp saker? ☐ ☐

Hur mycket tänker du på framtiden? ☐ ☐



	Inte alls 1	2	3	4	5	6	7	8	9	Väldigt mycket (ofta) 10
Hur mycket brukar du oroa dig för arbetslösheten?	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐

E2. Markera vad på skalan som bäst passar in på dig.

	Inte alls 1	2	3	4	Väldigt mycket 5
Jag är bra på att motstå frestelser.	☐	☐	☐	☐	☐
Jag har svårt att bryta dåliga vanor.	☐	☐	☐	☐	☐
Jag är lat.	☐	☐	☐	☐	☐
Jag säger opassande saker.	☐	☐	☐	☐	☐
Jag gör saker som är dåliga för mig, bara för att det är kul.	☐	☐	☐	☐	☐
Jag avstår sånt som är dåligt för mig.	☐	☐	☐	☐	☐
Jag önskar att jag hade mer självdisciplin.	☐	☐	☐	☐	☐
Jag får ofta höra att jag har benhård självdisciplin.	☐	☐	☐	☐	☐
Jag har svårt att koncentrera mig.	☐	☐	☐	☐	☐
Ibland hindrar nöjen mig från att få jobb gjort.	☐	☐	☐	☐	☐
Jag kan jobba effektivt mot långsiktiga mål.	☐	☐	☐	☐	☐
Ibland kan jag inte förmå mig själv att avstå från att göra något, fast jag vet att det är fel.	☐	☐	☐	☐	☐
Jag agerar ofta utan att tänka igenom alla alternativ.	☐	☐	☐	☐	☐

Avdelning F:

Föreställ dig att du ska få en summa och markera vilka alternativ du föredrar.

OBS: Välj spontant ett alternativ på varje rad/för varje delfråga

F1. Delfråga A:

1000 kr 850 kr om
idag eller en månad

☐	☐
--------------------------	--------------------------

**F2. Delfråga B:**

1000 kr idag eller	1100 kr om en månad
☐	☐

F3. Delfråga C:

1000 kr idag eller	1250 kr om en månad
☐	☐

F4. Delfråga D:

1000 kr idag eller	1500 kr om en månad
☐	☐

Avdelning G: Del 2

Föreställ dig att du ska få en summa och markera vilka alternativ du föredrar.

OBS: Välj spontant ett alternativ på varje rad/för varje delfråga

G1. Delfråga E:

1000 kr om ett år eller	850 kr om ett år och en månad
☐	☐

G2. Delfråga F:

1000 kr om ett år eller	1100 kr om ett år och en månad
☐	☐

G3. Delfråga G:

1000 kr om ett år eller	1250 kr om ett år och en månad
☐	☐

G4. Delfråga H:

1000 kr om ett år eller	1500 kr om ett år och en månad
☐	☐



Avdelning H: Bakgrundsfrågor

H1. Hur lång är du och ungefär hur mycket väger du?

```
$(document).ready(function(){ var thisQuestion =  
$('#question{QID}'); $('input.text:eq(0)',  
thisQuestion).after('centimeter') $('input.text:eq(1)',  
thisQuestion).after('kilogram') });
```

Längd:

--	--	--	--	--	--	--	--	--	--

Vikt:

--	--	--	--	--	--	--	--	--	--

H2. Kön:

Kvinna ☐

Man ☐

Annat/vill inte ange ☐

H3. Vad är din högsta avslutade utbildning?

Grundskola/folkskola ☐

Gymnasium/motsvarande ☐

Högskola/universitet ☐

H4. Din ålder i år:

--	--	--	--	--	--	--	--	--	--

H5. Markera det som passar in på dig.

Jag är född i Sverige och har minst en förälder som är född här ☐

Jag är född i Sverige av föräldrar födda utomlands ☐

Jag är född utomlands ☐



H6. Ange ditt hushålls sammanlagda bruttoinkomst (inkomst före skatt):

- | | |
|-------------------------------|--------------------------|
| 0-19 999 kr per månad | ☐ |
| 20 000 – 29 999 kr per månad | ☐ |
| 30 000 – 39 999 kr per månad | ☐ |
| 40 000 – 49 999 kr per månad | ☐ |
| 50 000 – 59 999 kr per månad | ☐ |
| 60 000 – 69 999 kr per månad | ☐ |
| 70 000 – 79 999 kr per månad | ☐ |
| 80 000 kr eller mer per månad | ☐ |
| Föredrar att inte svara | ☐ |

H7.

H8.

Avdelning I: Kontroll

Om något ovan inte stämmer, klicka på "Föregående" för att gå tillbaka och ändra.

I1. Du har angett att du är {INSERTANS:353664X323X2238lgh} centimeter lång. Stämmer detta?

Ja ☐

I2. Du har angett att du väger {INSERTANS:353664X323X2238wgh} kilogram. Stämmer detta?

Ja ☐

I3. Du har angett att du är {INSERTANS:353664X323X2241} år. Stämmer detta?

Ja ☐

Avdelning J: Träningskontrakt

```
$(document).ready(function(){ $('#moveprevbtn').hide(); });
```

\$(document).ready(function(){ \$('#moveprevbtn').hide(); }); Som vi nämnde i informationen kommer en del deltagare att erbjudas en möjlighet att sätta upp konkreta mål för sin träning på Friskis & Sveltis Stockholm för en viss tidsperiod, i form av ett träningskontrakt med sig själv. Du har slumpats till denna grupp.

Observera att det är med hjälp av träningsstatistik från Friskis & Sveltis Stockholm som vi avgör om du fullföljt kontraktet, som alltså enbart gäller den träning du genomför hos F&S Stockholm. Notera också att kontraktet gäller per kalendermånad. Om du tecknar ett träningskontrakt med dig själv idag, börjar det alltså den första nästa månad.

J1. Vill du teckna ett träningskontrakt med dig själv?

Ja ☐

Nej ☐



J2. Välj hur länge du ska träna, 1-4 månader:

- 1 månad ☐
- 2 månader ☐
- 3 månader ☐
- 4 månader ☐

Avdelning K: Teckna träningskontrakt

Du har valt att du ska träna i {INSERTANS:353664X324X2248}.

Stämmer detta, välj nästa. Om inte, klicka på "Föregående" för att gå tillbaka och välja om. Du kan inte gå tillbaka och ändra dig efter detta steg.

K1.

K2.

K3.

K4.

K5.

K6.

K7.

K8.

Avdelning L: Teckna träningskontrakt, del 2

L1. \$(document).ready(function(){ \$('#moveprevbtn').hide(); });

Välj hur mycket du ska träna per månad på Friskis & Svettis Stockholm:

- 4 gånger per månad (motsvarar i snitt ca 1 gång/vecka) ☐
- 8 gånger per månad (motsvarar i snitt ca 2 gånger per vecka) ☐
- 12 gånger per månad (motsvarar i snitt ca 3 gånger per vecka) ☐
- 16 gånger per månad (motsvarar i snitt ca 4 gånger per vecka) ☐
- 20 gånger per månad (motsvarar i snitt ca 5 gånger per vecka) ☐
- 24 gånger per månad (motsvarar i snitt ca 6 gånger per vecka) ☐



I andra sammanhang har forskning visat att det ofta är effektivt att koppla ekonomiska incitament till sitt mål. Fler lyckas helt enkelt nå sina mål om det finns en belöning eller något att tjäna (ekonomiskt) på att göra det.

M1. Vill du koppla ekonomiska incitament, slå vad med dig själv, om att du kommer lyckas fullfölja ditt träningskontrakt?

Ja ☐

1

Nej ☐

M2. Hur mycket vill du slå vad om?

10 kr ☐1000 kr Annat ☐

5

M3. Hur mycket vill du slå vad om?

10 kr ☐

1000 kr	
---------	--

Annat



M4. Ange belopp mellan 10 och 1000 kr:

[illegible]



Amnesty	
Bris	
Greenpeace	
Humanisterna	
Läkare utan gränser	
Liberalistiska föreningen	
Rädda barnen	
Röda korset	
Svensk miljöförbundet	
Världsnaturfonden	

Avdelning N: Betalning

Du har valt att slå vad med dig själv om {moneyamount.shown} kronor. Swisha pengarna nu till 1233200052. Ange din e-postadress i meddelandet. Enkätfabriken sköter på vårt uppdrag hanteringen av in- och återbetalningar.

```
document.getElementById("button").bgcolor="#00688B"; function showDiv() {
document.getElementById('welcomeDiv').style.display = "block"; } .mybutton { border-radius: 5px; background-color:
#00688B; border: none; color: white; padding: 15px 32px; text-align: center; text-decoration: none; display: inline-block; font-
size: 16px; margin: 4px 2px; cursor: pointer; }
```

Huvudansvarig forskare: Professor Ulf-G Gerdtham, tel. 046-222 48 10, e-post ulf.gerdtham@med.lu.se

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N1. Ange det mobilnummer du swishar pengarna från här, för att vi ska kunna matcha ihop inbetalningen med ditt kontrakt:

[illegible]

N2. Repetera mobilnummer:

[illegible]



Avdelning O: Tack!

Tack för att du deltar! Vi återkommer med en uppföljande enkät efter ditt träningskontrakt eller senast om ca 5 månader.

När du fyllt i den uppföljande enkäten får du möjlighet att välja mellan en trisslott eller att 30 kronor doneras till välgörenhet, som tack för ditt deltagande. Om du väljer en trisslott behöver vi din postadress, men den kommer inte att kopplas till dina enkätsvar eller träningsdata och används inte till något annat än att sända trisslotten.

Om du fullföljer ditt träningskontrakt återbetalas pengarna till dig så snart vi hunnit bekräfta din träning med data från Friskis & Svettis Stockholm, efter kontraktstidens slut. I den uppföljande enkäten kommer vi fråga dig om vilket konto som du vill ha pengarna till.

Du har tecknat ett träningskontrakt med dig själv om att träna {INSERTANS:353664X326X2250} i {INSERTANS:353664X324X2248}. Kontraktet börjar från och med den första nästa månad. Du har slagit vad med dig själv om {moneyamount.shown} kronor att du kommer fullfölja kontraktet.

Du har tecknat ett träningskontrakt med dig själv om att träna {INSERTANS:353664X326X2250} i {INSERTANS:353664X324X2248}. Kontraktet börjar från och med den första nästa månad.

Du har tecknat ett träningskontrakt med dig själv om att träna {INSERTANS:353664X326X2250} i {INSERTANS:353664X324X2248}. Kontraktet börjar från och med den första nästa månad.

`$(document).ready(function(){ $('#moveprevbtn').hide(); });` Tryck på "Skicka" för att avsluta och skicka in dina svar.

Tack för att du deltar! Vi återkommer med en uppföljande enkät om ca 5 månader.

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