



# Pre-analysis plan Workplace giving 2

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Note this pre-analysis plan was edited for layout prior to final publication, the content has not changed.

## Policy problem

Workplace giving (WG), also referred to as payroll giving, provides employees with an automated way to donate to their charity of choice. Participation in WG is low in the Australian Public Service at 2.84 per cent. This is about half the national average of 4.71 per cent.

## Trial aim

In a trial conducted in the Department of Prime Minister and Cabinet (PM&C), we found that sending a behaviorally informed email to staff increased WG signups more than a simple informational email.

This trial aims to build on these results, by testing the effect of varying the messenger (who the email is sent from) and by testing a simplification of the WG onboarding system.

## Interventions

All staff members enrolled in this trial will receive an email encouraging them to sign up to WG. The trial will be a 2x2 factorial design, thus we will have two independent variables (IV) each with two levels.

### IV.A - Messenger

The email will be sent from either a member of the DSS senior executive service (SES) or a non-SES staff member (peer). Emails will differ only in the name of the sender and their photo.

### IV.B - Signup system

Emails will include a link to either the current signup information page (current) or a simplified signup form (simplified).

The table below shows the notation used to refer to individual groups formed from our two independent variables.

	Messenger SES	Messenger Peer
Current signup	A0B0	A1B0
Simplified signup	A0B1	A1B1

## Outcome measures

The primary outcome for this trial will be workplace-giving prevalence. This will be derived from a binary outcome variable where providing any amount of money through WG will count as 1 and otherwise 0.

We will assess the average amount (in dollars) given by staff through WG as a secondary outcome.

## Population and sample selection

All permanent ongoing and non-ongoing staff with tenure of six months or longer regardless of their location are eligible for inclusion. This is approximately 2,342 individuals.

Staff involved in the design and implementation of the trial, along with Senior Executive Service (SES) staff at the Deputy Secretary and Secretary level will be excluded from the trial. We anticipate this will give us a total sample size of approximately 2,250 individuals.

## Hypotheses

H1. Peer email  $\neq$  SES email ( $A1 \neq A0$ )

H2. Simplified signup  $>$  current signup system ( $B1 > B0$ )

H3. Any email  $>$  no email (assessed with a non-RCT before/after comparison)

## Randomisation

Randomisation will be at the level of individual staff members. Randomisation will occur in blocks, defined by the following pre-treatment covariates: Current workplace giving status (yes/no), and Income level (below median/above median).

Within each block individuals will be randomly assigned to four treatment groups using complete random assignment. Assignment will be balanced (that is, an equal number in each treatment group) to the extent that blocks allow.

Randomisation will be implemented via an R script using the 'block\_ra' command from the 'randomizr' package. We will set a seed in order to ensure the reproducibility of the randomisation process. Randomisation code will be verified by another BETA staff member not directly involved in the project.

## Balance checks

We will conduct pre-trial balance checks to judge whether observed covariate imbalances are larger than would be expected from chance alone. To this end, we will perform a multinomial

logistic regression in which treatment status will be regressed on pre-treatment covariates. We will assess the hypothesis that all coefficients on covariates are zero.

A p-value of 0.01 or less will prompt a review of the random assignment procedure and possible data-handling mistakes. If the review finds no errors, we will report the imbalance test and proceed on the assumption that the imbalance is due to chance, and report estimates with and without the pre-specified covariate adjustment.

## Sample size and power

The sample for the RCT will be approximately 2,250 individual staff. We performed power calculations using simulation for our two main effects making use of the regression specification outlined in the 'method of analysis' section. This indicated that at an alpha of 5%, we will have 80% power to detect a standardised effect of 0.11. This is equivalent to a change in workplace giving participation from 1.4% to 3.1%. These calculations assume no interaction effect between the impact of the messenger and the impact of the signup system.

## Method of analysis

The principal analysis of the effect of the intervention will be intent-to-treat and will consist of a covariate-adjusted comparison of our primary outcome for our two main effects (corresponding to H1 and H2, see Hypotheses). This estimate, confidence intervals and p values will be derived from a linear regression model with the following specification:

$$y = \alpha + \tau_1 A + \tau_2 B + \gamma_1 x_A + \gamma_2 x_B + \epsilon$$

The coefficient on A is the main effect of changing the email messenger, the coefficient on B is the main effect of changing the signup system and x is a vector of mean centred covariates, including block indicators (see 'covariates'). These variables will be interacted with treatment indicators as per Lin (2013).

To assess for synergism or antagonism between our treatments, we will run a similar model to the above including an interaction term for AB. We do not expect to see a strong interaction and our trial is underpowered to detect one. Therefore, this test will be treated as a secondary analysis. If there is evidence of a strong interaction, we will report simple main effects.

Our hypothesis test for H1 will be a two sided test while the test for H2 will be one sided (see 'Section 4. Hypotheses'). See section 13 for a discussion on the interpretation of resulting p-values.

We will also perform a non-experimental before/after comparison in order to estimate the effect of sending any email (H3). We will use regression to perform this calculation using the same covariates listed in section 12.

We will calculate robust (HC2) standard errors for all linear models. Because our primary outcome is binary, we will run a robustness check using an equivalent logistic regression specification. We will calculate and report average marginal effects from this model.

Exploratory subgroup analyses will also be performed by factors such as gender, substantive position. We will clearly label these analyses as exploratory.

## Covariates

As well as block indicators accounting for our stratified randomisation (based on income and pre-trial WG status), we will also adjust for the following pre-randomisation variables in our regression:

- Age (in years, treated as continuous)
- Gender (binary variable, 1 = male)
- Location (binary variable, 1 = national office)

If blocks and controlling variables do not have a major impact on results (as judged by their inclusion/exclusion not altering the interpretation of results), we will report non-adjusted estimates and include the adjusted estimates in an appendix.

## Interpretation and reporting

We will make use of p-values to aid in the interpretation of our results. However, we will avoid taking a 'bright line' approach, in which a threshold (usually 0.05) is used to determine a meaningful finding. Instead, we will consider the p-value together with prior evidence, effect size, outcome variability and design limitations in order to assess the strength of a finding.

Varying the messenger (main effect A) is a low cost intervention, meaning that even a small effect could be practically meaningful. The intervention is also low risk, so there would be little consequence to acting upon a false positive result. Therefore, we will take effects seriously in the presence of a p-value  $> 0.05$ .

We expect that simplification of the signup system (main effect B) will have a strong positive effect. This is based on prior evidence and our assessment that the signup system in its current form is a major barrier. Because we are confident in a positive effect, we will make use of a one-tailed significance test.

We will report the  $n$ , group means or proportions for all treatment groups on our primary and secondary outcomes. We will also report average treatment effects, 95% CIs and p-values for all comparisons and hypothesis tests that we run.

## Pre-analysis plan commitments

- No trial data have been collected/no analysis has been undertaken prior to the completion of this pre-analysis plan.
- We will be transparent about, and provide justification for, any deviations (additions or omissions) from this plan.