

Design Document and Analysis Plan

Project Name: Reducing Choice Errors among Enhanced Silver-Eligible Enrollees

Date Finalized: 12/20/2020

This document serves as a basis for distinguishing between planned (confirmatory) analysis and any unplanned (exploratory) analysis that might be conducted on project data. This is crucial to ensuring that results of statistical tests will be properly interpreted and reported. For the Analysis Plan to fulfill this purpose, it is essential that it be finalized and date-stamped before we begin looking at outcome data. Once this plan is finalized, a date is entered above, and the document is shared with the primary customer for the project.

Project Objective

Using letters and email reminders to reduce choice errors among Enhanced Silver-eligible renewing members in Gold and Platinum plans

Evaluation Design

Test Arms / Treatment Conditions:

This is a block randomized design where households enrolled in choice error plans as of October 2020 were assigned to one of four arms: (1) a control group, (2) a letter + 1 email group, (3) a letter + 3 email group and (4) a letter + 5 email group. Households assigned to treatment arms will receive letters and emails that encourage them to switch metal tiers (from Gold or Platinum to Enhanced Silver) so as to save on premium and out-of-pocket costs. The outreach materials include average premium savings, out-of-pocket savings and peer comparisons.

Total Number of Observations:

N = 20,900 households enrolled in CSR choice error plans as of October 2020

Randomization / Assignment:

Randomization was done at the household level and we blocked on FPL and metal tier such that there are three blocks: (1) Silver 87-eligibles in Gold, (2) Silver 94-eligibles in Gold, and (3) Silver 94-eligibles in Platinum.

Power:

To arrive at an estimate for the minimum detectable effect (MDE), we assume a baseline metal tier switch rate of 16 percent, which is in line with what we've observed in prior years. In a pairwise comparison (e.g. Control vs. 1 Letter + 1 Email), we'd be powered at the 80% level to detect a 2 percentage point difference in Enhanced Silver enrollment rates.

Meaningful Effect Size:

In previous RCTs designed to induce plan switching, we observed intent-to-treat (ITT) effects between 1.5 to 4 percentage points. Given the low-cost nature of this nudge (i.e. approximately \$0.60 per household), even a 1 percentage point increase in Enhanced Silver take-up would be meaningful.

Likely Effect Size:

Based on prior nudges carried out by Covered California, we would expect to observe an ITT effect between 1-4 percentage points.

Data and Data Structure

This section describes variables that will be analyzed, as well as changes that will be made to the raw data with respect to data structure and variables.

Outcomes:

The primary outcome of interest will be an indicator for whether a consumer is enrolled in an Enhanced Silver plan for the 2021 enrollment year by the end of the Open Enrollment period. Our secondary outcome is an indicator for whether a consumer called the Service Center.

Data:

We used the HBEX and DataMart tables to prepare the experimental population, and we will use these tables for our Enhanced Silver take-up analysis. For the call rate outcome, we will use the SC_DataMart tables.

Quality Control Checks:

After carrying out the randomization, we checked for balance across several observable covariates (e.g. language spoken, email availability, FPL %), which indicated there were no significant dissimilarities across treatment arms.

	Control	1 Letter + 1 Email	1 Letter + 3 Emails	1 Letter + 5 Emails
Subsidy FPL %	166%	167%	167%	166%
English Language Preference	89%	89%	89%	88%
Spanish Language Preference	9%	9%	9%	8%
Email	82%	82%	82%	82%
Head of Household Age	41	41	41	41
N	5,217	5,267	5,193	5,249

Anticipated Limitations:

Based on prior randomized evaluations that span coverage years, we expect there to be attrition, wherein not all 2020 members continue to 2021. Given the randomization, we expect attrition to be evenly distributed across treatment arms. To address attrition, we will estimate bounds on the ITT effect. But attrition does have the effect of reducing our sample by 7-10%, which will limit our ability to detect treatment effects smaller than ~2 percentage points.

We also expect there to be one-sided noncompliance since not all households have a valid email address. To address this, we will use email engagement data (i.e. who was sent emails) to identify compliers as part of a complier average causal effect (CACE) analysis.

Statistical Models & Hypothesis Tests

This section describes the statistical models and hypothesis tests that will make up the analysis—including any follow-ups on effects in the main statistical model and any exploratory analyses that can be anticipated prior to analysis.

Statistical Models:

Intent-to-treat: to estimate treatment effects, our primary analysis will be an intent-to-treat (ITT) specification, examining the effect of treatment assignment. We will estimate the effect of each treatment arm using ordinary least squares (OLS) regression. That is, we will regress the outcome of

interest (e.g. Silver enrollment) for household i on the set of indicator variables for each of the treatment groups:

$$outcome_i = \alpha + \beta_1 Letter1Email_i + \beta_2 Letter3Emails_i + \beta_3 Letter5Emails_i + \epsilon_i$$

The coefficient β_1 will be the estimate of the causal effect of the intent to treat of the letter and one email. The coefficient β_2 will be the estimate of the causal effect of the intent to treat of the letter and three emails. And the coefficient β_3 will be the estimate of the causal effect of the intent to treatment of the letter and five emails.

Because of differential benefits from switching, we will estimate separate regressions for each of the three blocks: Silver 87-eligibles in Gold, Silver 94-eligibles in Gold and Silver 94-eligibles in Platinum. And to the extent there are meaningful differences across treatment arms, we will test for equality of coefficients.

Complier average causal effect: as noted above, we expect noncompliance among those households assigned to receive emails since not all households have email addresses, and some with email addresses may have opted out of email communications or provided an invalid email address.¹ Thus, to augment our ITT analysis, we will also estimate treatment effects based on treatment receipt, using two-stage least squares regression (2SLS).

Follow-Up Analyses

We will examine treatment heterogeneity by language (e.g. Spanish vs. non-Spanish), by age (e.g. above and below the mean and median) and by enrollment pathway (e.g. whether the household passively renewed into 2020 or actively chose their plan). For all of the heterogeneity analyses, we will interact these categories with the treatment indicators.

Inference Criteria, Including Any Adjustments for Multiple Comparisons:

Because we are examining a small set of outcomes, we will not perform any corrections for multiple hypothesis testing, and we will use two-tailed tests with p-values ≤ 0.05 to denote statistically significant effects.

¹ Based on previous experience with direct mail RCTs, we assume near-full compliance of the direct mail treatment.