Analysis Plan

Project Name: Ticket to Work Notice Optimization
Project Code: 1902
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Project Description

The Ticket to Work (TTW) program provides Social Security Disability Insurance (SSDI) beneficiaries and Supplemental Security Income (SSI) recipients with disabilities choices for receiving employment services while providing incentives to providers to serve these individuals. The goal of the TTW legislation was to expand the availability of employment services for beneficiaries beyond the existing state vocational rehabilitation (VR) agencies, reducing dependency on disability benefits.

The TTW Notice Optimization project seeks to increase participation in the TTW program by developing an evidence-based approach to targeting outreach to eligible beneficiaries. The project will test changes to Ticket notices including the types of notices sent, the language used, and the timing of the notices. The intervention is tested in a factorial design (i.e., two-by-two) where the re-designed letter constitutes factor #1 and an added cardstock Ticket factor #2.

The project aims to increase the number of beneficiaries who "assign a Ticket" with an Employment Network (EN) or VR agency. The assignment of a Ticket constitutes an agreement between the beneficiary and a service provider of their choice for employment support services. An intermediate goal of the project is an increase in initial calls to the TTW Help Line.

The intervention will run for 18 months, starting September 16, 2020, and outcomes will be measured 9 months after entering the study. Given the current COVID-19 pandemic and its associated economic and public health effects, the time period will allow us to examine treatment effectiveness over a period of recession and, potentially, recovery. While our primary interest is in the effectiveness of the optimized notices regardless of current economic conditions, the ability to examine intervention effectiveness over the entire study period potentially allows us to explore heterogeneous treatment effects in both times of economic and public health recession and, potentially, recovery.

Data and Data Structure

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

Outcome Variables to Be Analyzed:

- Primary outcome: Beneficiary "assigns a Ticket", which constitutes an agreement between the beneficiary and a service provider of their choice for employment support services[1] *(9 months post mailing)*
- Secondary outcome: Beneficiary (or representative payee) ever calls the Ticket Help Line *(9 months post mailing)*

We will be drawing from four major SSA administrative datasets (i.e., the Disability Control File (DCF), the internet Ticket Operations Provider Support System (iTOPPS), the Supplemental Security Record (SSR) and the Master Beneficiary Record (MBR)[2]) and contractor data to collect the following information:

- *Data to determine whether an individual assigns a Ticket*: ENs and VRs assign Tickets using the Ticket Portal, which is a secure portal that feeds directly into iTOPPS.
- *Data to determine whether an individual calls the Ticket Help Line*: SSA's Ticket Program Manager (TPM) tracks this information via iTOPPS. TPM will provide a monthly report to SSA's Office of Research, Demonstration, and Employment Support that tracks whether a beneficiary in our study has called the Help Line and which notice they received.

For each individual, we will observe Ticket assignments and Help Line calls at 9 months after being sent the respective notice (i.e., 9 months post intervention). Historically, beneficiaries who assign their Tickets tend to do so within the first year of benefit receipt.[3] While ENs and VRs are supposed to notify SSA about Ticket assignment right away, there may be a lag in reporting assignments to SSA.

Table1: Overview of outcomes

Outcome type	Outcome name	Timing of measurement
Primary	Ticket assignment	9 month post intervention
Secondary	Help Line call	9 months post intervention

Additional Variables needed for Analysis:

The following additional variables will be used in different analytical specifications.

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[1] In the future, another primary outcome of interest is beneficiary employment and earnings. In the enactment of the TTW program, the intention was for the program to reduce dependency on disability benefits and increase the availability of employment services for beneficiaries. In doing so, the TTW program could also impact beneficiary employment and earnings. Our randomized control trial design provides a unique opportunity to estimate the causal impacts of TTW on employment and earnings, which would add to the current body of evidence on the impacts of the TTW program.

[2] The MBR holds records of people who receive Title II (DI) benefits. The SSR holds the records of people receiving Title XVI (SSI only) benefits.

[3] Internal SSA Analysis (March 2019). "Analysis of Ticket Mailings: Background to Ticket to Work (TTW) Mailer Optimization Project." Social Security Administration, Office of Research, Demonstration, and Employment Support.

- *Data to determine who is sent a TTW notice and which experiment they are part of (i.e., experiment 1, 2, or 3):* This data comes from the DCF and is managed by SSA's Office of Systems.
- *Data to determine the partition each beneficiary was assigned to*: SSA's Office of Systems will automate the random assignment process using data system partitions based on the last two digits of Social Security Numbers (SSNs) to assign eligible beneficiaries to one of the four study groups. This data will identify which partitions were mailed which type of notices (i.e., which experimental conditions they were assigned to).
- *Data to determine which partition corresponds with which experimental condition:* A respective list has been provided to OES by SSA.
- *Date stamp of notice distributio*n: when notice mails were sent out to respective beneficiaries.
- *Beneficiary characteristics (based on the DCF)*:
    - Type of recipient (SSDI, SSI, or both)-current;
    - Claim history (SSI, SSDI, or both);
    - Primary impairment code;
    - Secondary impairment code;
    - Zip code;
    - Gender (if available in the future);
    - Whether the beneficiary has a representative payee;
    - Age;
    - Full 9-digit Social Security number;
    - Whether beneficiary has had recent work, as measured by earnings over the past year;
    - Disability onset date;
    - Date of eligibility;
    - Established Onset Date of disability (EOD)-when a beneficiary enrolled in SSDI/SSI;
    - We will calculate the time from application to award based on:
        - Effective filing date;
        - Date of adjudication;
    - Date of death (if applicable);
    - Type of benefit-worker (SSDI beneficiaries);
    - If available, whether the beneficiary *is* a disabled adult child (SSDI beneficiaries);
    - If available, whether the beneficiary *has* a disabled adult child (SSDI beneficiaries).

Imported Variables:
In addition to the variables above that come from SSA's administrative data systems, we will add the following data based on zip code: unemployment rates, COVID-19 death counts, and county-level total population numbers.

- *Unemployment rates*: We will merge information on the monthly local area unemployment statistics (at the County-level) from the US Bureau of Labor Statistics for the month the notice was sent to each beneficiary.[4] Unemployment rate estimates for counties are produced through a building-block approach known as the "Handbook method." This procedure uses data from several sources, including the Current Population Survey, the Current Employment Statistics program, state unemployment insurance systems, and the Census Bureau's American Community Survey, to create estimates that are adjusted to the statewide measures of unemployment. Depending on the distribution of this measure, we may transform the data to enhance the ease of interpretation.

- *County total population*: The US Census Bureau provides yearly population estimates of the total population of the United States.[5] The yearly population estimate starts with a population base (the last decennial census or the previous point in the time series), adds births, subtracts deaths, and adds net migration (both international and domestic).[6] Births and deaths come from the National Vital Statistics System, and net migration is based on four different data sources.[7]

- *COVID-19 deaths:* Weekly COVID-19 death counts by County as provided by the National Center for Health Statistics (NCHS).[8] Provisional death counts for coronavirus disease 2019 (COVID-19), are based on death certificates and coded by the NCHS. They come from a current flow of mortality data in the National Vital Statistics System. Data are lagged on average 1-2 weeks and are continually revised as new and updated death certificate data are received from the states by NCHS. We will generate monthly averages per county on the basis of unique (i.e., not cumulative) death counts.

- *COVID-19 deaths per 1,000 inhabitants*: On the basis of monthly COVID-19 death counts and county total population data, we will generate a new variable that depicts the number of monthly COVID-19 death counts per 1,000 inhabitants per county. Depending on the distribution of this measure, we may transform the data to enhance the ease of interpretation.

Transformations of Data Structure:
For the confirmatory analysis, we will use a cross-sectional dataset with just one observation per beneficiary for each cohort-based experiment. For exploratory analysis, we will transform the

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[4] US Bureau of Labor Statistics: https://www.bls.gov/lau/#cntyaa
[5] US Census Bureau: https://www.census.gov/data/tables/time-series/demo/popest/2010s-counties-detail.html
[6] US Census: Methodology for the United States Population Estimates: Vintage 2019. https://www2.census.gov/programs-surveys/popest/technical-documentation/methodology/2010-2019/natstcopr-meth v2.pdf
[7] This includes Internal Revenue Service tax return data for ages 0-64; Medicare enrollment data from Centers for Medicare and Medicaid Services for ages 65+; SSA's Numerical Identification File all ages; Changes in the group quarters population estimates by the US Census Bureau.
[8] Centers for Disease Control and Prevention: https://data.cdc.gov/NCHS/Provisional-COVID-19-Death-Counts-in-the-United-St/kn79-hsxy

data structure to a monthly panel-dataset which follows beneficiaries to conduct a survival analysis.

Data Exclusion:
We anticipate no exclusion of beneficiaries. Medical improvement not expected (MINE) diary beneficiaries (other than those with deafness and blindness diagnoses) as well as beneficiaries that have met listing codes for neoplasia and certain neurological conditions are excluded from receiving the TTW notices all together.

Treatment of Missing Data:
If beneficiaries die within 9 months of receiving a respective intervention, they will be excluded from our analyses. Other than that, there should be no missing data because all outcomes, including death, are observed by SSA. Additionally, we do not anticipate significant missing data in the imported variables.[9]

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 exploratory analyses that can be anticipated prior to analysis.

Statistical Models:

Randomization Tests
Randomization to experimental conditions within cohort-based experiments will be determined on the basis of the last two terminal digits of beneficiaries' SSNs. Consecutive terminal digits are assigned in batches (i.e., 00-04, 05-09, …, 95-99) to twenty partitions in SSA's systems. Of these twenty partitions, five consecutive partitions constitute an experimental condition so that there are four experimental groups in total. These packages of partitions cycle through experimental groups every month by one partition (see table). This means that partition numbers 1-5 will be assigned to experimental condition #1 (status-quo notice and no cardstock Ticket) in the first month, but in month two partitions 2-6 will be assigned to that group.

Table 2: Example of TTW randomization scheme for the first three months for experiment 1

	Month 1		**Month 2**		**Month 3**	
SSN terminal digits	Partition Number	Condition	Partition Number	Condition	Partition Number	Condition
00-04	1	1	1	4	1	4
05-09	2	1	2	1	2	4
10-14	3	1	3	1	3	1
15-19	4	1	4	1	4	1

[9] If beneficiaries receive two interventions (for example, one as part of experiment 1 and one as part of experiment 2), we will determine whether to exclude them based on each experiment separately.

20-24	5	1	5	1	5	1
...	...	...	...	...	...	...
75-79	16	4	16	3	16	3
80-84	17	4	17	4	17	3
85-89	18	4	18	4	18	4
90-94	19	4	19	4	19	4
95-99	20	4	20	4	20	4

Before conducting analyses, we will check the initial randomization for the full sample by conducting balance tests by experimental group, using observable demographic characteristics of beneficiaries (e.g., receipt of SSDI vs SSI vs both, type of disability, number of disability classifications (if available), whether the beneficiary has a representative payee, age, gender (if available), zip-code, SSN, whether beneficiary has had recent work, age of disability onset). Balance tests will involve regressing these characteristics on treatment assignment, using HC2 robust standard errors.[10] We will conduct these randomization tests for each of the 3 experiments separately. We will run an omnibus F-test for significant differences between treatment and control across all observable characteristics as a set—for experimental factor #1 and factor #2 separately. If we find imbalances on any covariates, we will—in addition to models without covariates—report average treatment effects adjusting for these covariates following Lin's (2013) saturated regression estimator.[11]

In addition, we will estimate the intraclass correlation coefficient (ICC) for beneficiaries nested in partitions (for each experiment separately). An ICC larger than 0.1 indicates substantial clustering within partitions, indicating a violation of the independence of observations assumption.[12] If this would be the case, we would use robust HC2 standard errors clustered by partitions instead of unclustered standard errors for all analyses outlined below.

Covariate adjustment

All models described in this section will be estimated with and without covariate adjustment following Lin's (2013) estimator. In the models with covariate adjustment, we will include month fixed effects to control for seasonal effects in Ticket assignment rates as well as state fixed effects to control for geographic variation in Ticket assignment rates. We will also adjust for the characteristics that predict TTW uptake including beneficiary age, beneficiary type (SSDI, SSI, or both), age of disability onset[13], and time since last employment.[14] In addition, we will adjust for

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[10] Davidson, R., and J. G. MacKinnon. 1993. Estimation and Inference in Econometrics. New York: Oxford University Press.

[11] Lin, W. 2006. Agnostic Notes on Regression Adjustments to Experimental Data: Reexamining Freedman's Critique. The Annals of Applied Statistics, Vol. 7, No. 1, pp. 295-318.

[12] See also Huang, F. 2018. Using Cluster Bootstrapping to Analyze Nested Data With a Few Clusters. Education and Psychological Measurement Vol. 78, No. 2, pp. 297-318.

[13] Potentially proxied by 12 months before the date of application for SSDI based on SSDI eligibility rules, and date of application for SSI.

[14] Social Security Administration: The National Beneficiary Survey. https://www.ssa.gov/disabilityresearch/nbs.html

whether a beneficiary has a representative payee because having one may be positively or negatively correlated with TTW uptake.[15]

As a result of our study using multiple cohorts of beneficiaries as well as being implemented over a period of 18 months, some beneficiaries will be exposed to multiple doses of the intervention, which we will control for in the analysis. For example, while all beneficiaries in experiments 2 and 3 will have been exposed to a prior notice in the startup and/or the 1 year notification, some of these beneficiaries will have received the business as usual/"old" condition and others will have received the optimized notice and card stock Ticket conditions. Specifically, the beneficiaries that enter experiment 2 or 3 at month 13 onward will have been exposed to a version of the optimized notice (or control) in months 1-6 of our study. In contrast, beneficiaries that enter experiment 2 or 3 prior to month 13 of our intervention will have only been exposed to the business as usual/"old" notice without the revised language or cardstock Ticket. Therefore, we will add a dosage fixed effect (i.e., a dummy variable indicating "zero" for beneficiaries that entered experiment 2 in months 1-12 and "one" for beneficiaries that entered experiment 2 in months 13-18).[16] Note that beneficiaries that entered experiment 2 at month 13 onwards, will have been exposed to the same respective intervention condition in the prior year because the randomization scheme is based on their terminal SSN digits.

Because experiment 3 only has two experimental conditions (in contrast to four in experiment 2), beneficiaries in experiment 3 that enter our study in months 13 onwards may have been exposed to a different experimental condition than when they entered experiment 2. Therefore, we will include a separate fixed effect for beneficiaries that enter experiment 3 in months 13 onwards, in which we will control for the different experimental conditions they were assigned to when they were part of experiment 2 (i.e., a categorical variable indicating "zero" for beneficiaries that entered experiment 3 in months 1-12 and for 13-18 "one" through "four" for each respective experimental condition they were assigned to).

Table 3: Cohort overlap of interventions

	Experiment 1 (start-up cohort)	**Experiment 2 (1-year cohort)**	**Experiment 3 (2-year cohort)**
Have beneficiaries been exposed to a prior TTW notice?	None	Yes; all 1-year cohort beneficiaries received the business-as-	Yes; all 2-year cohort beneficiaries received the business-as-

[15] On one hand, having a representative payee increases the quality of life of many beneficiaries, decreasing the likelihood of homelessness, hospitalization, and substance abuse (National Academy of Sciences. 2016. "Effects of Appointment of Representative Payees on Beneficiaries" https://www.ncbi.nlm.nih.gov/books/NBK367675). As a result, having a representative payee may increase the likelihood of TTW uptake. It is also possible that having a representative payee could decrease the likelihood of TTW uptake, because some beneficiaries experience negative psychological effects after acquiring a representative payee (e.g., loss of autonomy, anxiety, fear) (Luchins DJ, Hanrahan P, Conrad KJ, Savage C, Matters MD, Shinderman M. 2014. An agency-based representative payee program and improved community tenure of persons with mental illness. Psychiatric Services Vol. 49, No. 9, pp. 1218–1222.).
[16] It is important to note that this renders our treatment effect a local treatment effect because we will effectively put zero weight on beneficiaries in months 13-18.

		usual/“old” start-up notice in the previous year. In addition, 1-year cohort beneficiaries who enter experiment 2 in months 13-18 will have been exposed to the same experimental condition that they were assigned to in experiment 1.	usual/“old” 1-year notice in the previous year. In addition, 2-year cohort beneficiaries who enter experiment 3 in months 13-18 will have been exposed to one of the 4 experimental conditions in experiment 2.

Treatment Effects
We will estimate the causal effect of the treatment (more specifically, intention to treat) using differences of means calculated using ordinary least squares (OLS) regressions of two binary outcomes (Ticket assignment and Help Line calls) on treatment assignment. We will estimate the differences in means for two comparisons for each experiment—one for each experimental factor, the so-called *main effects.* For each of the three experiments, we conduct a separate analysis, each looking at different exposure to the intervention based on cohort. This means we will test for instance, the 1-year cohort’s response to a 1-year optimized notice that is separate from the test of the start-up cohort’s response to a 1-year optimized notice.

We will be estimating main effects of the optimized notices (factor #1) and the inclusion of a cardstock Ticket (factor #2) in the first two cohort-based experiments. In the 3rd experiment, we will examine the receipt of an optimized notice plus cardstock Ticket (as a single factor) versus no notice or cardstock Ticket. In the following, we differentiate our confirmatory analyses by each of the three experiments.

<u>Confirmatory Analyses:</u>

Experiment 1 (start-up cohort):
In this analysis we seek to answer the following research questions:

1. Does contacting newly eligible beneficiaries with an optimized notice prompt an increase in the likelihood of Ticket assignment?
2. Does contacting newly eligible beneficiaries with a cardstock Ticket prompt an increase in the likelihood of Ticket assignment?

In the primary specification, the outcome (i.e., Ticket assignment) will be regressed on treatment assignment. In a second specification, we will regress whether a beneficiary called the Help Line (secondary outcome) on treatment assignment. Treatment assignment of both experimental factors is designed as follows:

- Factor #1 (optimized startup notice):
  - ***Treatment:*** if a beneficiary was assigned to receive the newly designed startup notice;
  - ***Control:*** if a beneficiary was assigned to receive the status-quo startup notice.
- Factor #2 (cardstock Ticket):
  - ***Treatment:*** if a beneficiary was assigned to receive a cardstock Ticket;
  - ***Control:*** if a beneficiary was assigned to receive no cardstock Ticket.

For beneficiary *i*:

$$Y_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \epsilon_i \qquad (1)$$

$$Y_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3' V_i' + \epsilon_i \qquad (2)$$

Equation 1 represents a model without covariate adjustment, and model 2 includes a vector (V') which indicates month fixed effects, state fixed effects, and a series of covariates. In our analysis, we will estimate HC2 standard errors. We anticipate a sample size of approximately 288,000-450,000 beneficiaries for the experiment over 18 months, allowing us to detect a 7-14 percent relative change (or 0.08-0.10 percentage point absolute change) for a Ticket assignment baseline that falls in between 0.57-1.14 percent. We will use two-tailed tests and a significance level of 0.05.

Experiment 2 (1-year cohort):
In this analysis we seek to answer the following research questions:

1. Does contacting beneficiaries who have received SSDI/SSI for one year with an optimized notice prompt an increase in the likelihood of Ticket assignment?
2. Does contacting beneficiaries who have received SSDI/SSI for 1 year with a cardstock Ticket prompt an increase in the likelihood of Ticket assignment?

Similar to experiment 1, the outcome (i.e., Ticket assignment) will be regressed on treatment assignment. In a second specification, we will regress whether a beneficiary called the Help Line (secondary outcome) on treatment assignment. Treatment assignment of both experimental factors is designed as follows:

- Factor #1 (optimized 1-year notice):
  - ***Treatment:*** if a beneficiary was assigned to receive the newly designed startup notice;
  - ***Control:*** if a beneficiary was assigned to receive the status-quo startup notice.
- Factor #2 (cardstock Ticket):
  - ***Treatment:*** if a beneficiary was assigned to receive a cardstock Ticket;
  - ***Control:*** if a beneficiary was assigned to receive no cardstock Ticket.

Similar models with the same parameters and set-up will be estimated as in equations (1) through (2).

Experiment 3 (2-year cohort):
In this analysis we seek to answer the following research question:

1. Does contacting beneficiaries who have received SSDI/SSI for 2 years with a combination of a newly designed 3-year notice and a cardstock Ticket (versus no notice and Ticket) prompt an increase in the likelihood of Ticket assignment?

In the primary specification, the outcome (i.e., Ticket assignment) will be regressed on treatment assignment. In a second specification, we will regress whether a beneficiary called the Help Line (secondary outcome) on treatment assignment. Treatment assignment of both experimental factors is designed as follows:

- ***Treatment:*** if a beneficiary was assigned to receive the newly designed 2-year notice plus cardstock Ticket;
- ***Control:*** if a beneficiary was assigned to not receive any notice or cardstock Ticket at all (business as usual).

For beneficiary *i*:

$$Y_i = \beta_0 + \beta_1 T1_i + \epsilon_i \tag{3}$$

$$Y_i = \beta_0 + \beta_1 T1_i + \beta_2' V_i' + \epsilon_i \tag{4}$$

Equation 3 represents a model without covariate adjustment, and model 4 includes a vector (V') which indicates month fixed effects, state fixed effects, and a series of covariates. In our analysis, we will estimate HC2 standard errors. We anticipate a sample size of approximately 288,000-450,000 beneficiaries for this experiment over 18 months, allowing us to detect a 7-14 percent relative change (or 0.08-0.1 percentage point absolute change) for a Ticket assignment baseline that falls in between 0.57-1.14 percent. We will use two-tailed tests and a significance level of 0.05.

Follow-Up Analyses:

<u>TTW during recession and recovery</u>
Given the current COVID-19 pandemic and its associated economic and public health effects, the study period of 18 months will allow us to potentially examine treatment effectiveness over a period of economic and public health recession, and potentially, recovery.[17] The primary interest of this study is the effectiveness of the optimized notices regardless of current economic/public health conditions. However, the ability to examine intervention effectiveness over 18 months allows us to explore heterogeneous treatment effects in times of recession and recovery. In

[17] Note that we follow-up any beneficiary that is included in the study for 9 months.

follow-up exploratory analyses, we will examine both the economic and public health aspects of changes in external conditions.

Contextual changes in the labor market will potentially affect the number of newly eligible SSDI and SSI beneficiaries; it may also affect the number of overall Ticket assignments during the study period, as can be seen from the table below.

Table 4: Monthly TTW mailings and and EN assignments (2020)—based on SSA's iTOPPPS

Month	**Total number of TTW mailings in the start-up cohort**	**Total monthly EN assignments across all three cohorts**
January 2020	20,098	2,468
February 2020	25,871	2,135
March 2020	27,198	1,673
April 2020	27,072	1,709
May 2020	25,929	1,147
June 2020	20,556	1,709

We therefore aim to examine the *moderating effect* of the performance of the labor market on the effectiveness of the optimized notice and cardstock Ticket interventions. Therefore, our three main analyses will be re-estimated including

1) models with a base term of county-level unemployment rates and,
2) models with an interaction term between unemployment rates and both experimental factors.

We will use cluster robust HC2 standard errors, clustered by county for these analyses to account for the level of analysis of the moderating variable.

Another aspect of recovery is the public health situation with regard to COVID-19. When there is less fear among the population of getting infected, eligible beneficiaries may be more likely to seek participation in TTW and subsequent employment. In addition, public health recovery is a potential proxy for ENs ability to provide in-person services.

To get at this aspect of recovery, we examine the *moderating effect* of the number of COVID-19 death counts per 1,000 inhabitants in each county, on the effectiveness of the optimized notice and cardstock Ticket interventions. Therefore, our three main analyses will be re-estimated including

1) models with a base term of county-level COVID-19 death counts and,
2) models with an interaction term between COVID-19 death counts and both experimental factors.

We will use cluster robust HC2 standard errors, clustered by county for these analyses to account for the level of analysis of the moderating variable.

Factorial interaction effects

For experiments 1 and 2, we will also explore whether there are interaction effects between both experimental factors, the optimized notice (factor #1) and the cardstock Ticket (factor #2). In other words, are there complementary or substitutive effects that can be observed when both interventions are combined? Indeed, because of the factorial nature of the experimental design, we can tease-out factorial interactions by including an interaction term between factors #1 and # 2 in the models in equations (1) and (2). Note that the base terms cannot be interpreted as main effects in these models, but as *simple effects* (i.e., they represent the differences from the experimental condition with the status quo notice and no cardstock Ticket).

Subgroup analyses

We will examine the heterogeneity of treatment effects for all three main analyses outlined above. First, we will estimate models separately for each single month. This will yield insights into seasonality effects. In addition we will perform subgroup analysis with regard to the following characteristics: state, type of recipient (SSI, SSDI, or both), type of disability (primary impairment), whether the beneficiary has a representative payee, beneficiary age, beneficiary sex, whether beneficiary has had recent work (or the amount of time passed since they last worked), beneficiary education (at time of application), age of disability onset (see also footnote 12), adjudicative level of award, and the time from application to award. We will also look at specific characteristics of SSDI beneficiaries, including type of benefit (Worker vs. Disabled Adult Child).

In addition to these subgroup analyses, we will also examine treatment effectiveness among 1-year cohort beneficiaries who were assigned to the same condition of the optimized notice intervention in experiments 1 and 2, compared to beneficiaries that have received the business-as-usual/"old" notice prior to entering experiment 2 (i.e., beneficiaries entering experiment 2 between months 1 through 12 compared to beneficiaries entering experiment 2 between months 13-18). Similarly, we will compare treatment effectiveness among 2-year cohort beneficiaries entering experiment 3 in months 13-18 based on their prior year treatment condition, and among beneficiaries that entered experiment 3 in months 1-12, and thus only experienced the business-as-usual/"old" start-up and 1 year notices. In experiment 2, we will also examine intervention effectiveness for beneficiaries in months 1-12 and 13-18 separately which will yield insight into whether receiving the same intervention once or twice impacts our outcomes of interest.

Inference Criteria, Including Any Adjustments for Multiple Comparisons:

All inferential tests will be performed using two-tailed tests and an alpha level of 0.05. For the confirmatory analysis we will correct for multiple comparisons and control for the corresponding false discovery rate within experiments using the Benjamini-Hochberg procedure. Follow-up analyses will be treated as exploratory, and hence no corrections for multiple testing will be employed.

Survival Analysis

We will also estimate the time to calling the Help Line and assigning a Ticket using survival analysis. In the survival analysis, we will estimate the likelihood of calling the Help Line and assigning a Ticket each month after the beneficiary has been contacted, for up to 9 months. We will estimate the model with and without covariate adjustment to test the sensitivity of the point estimates. As an additional analysis, we will run a log-rank test without control variables because this model does not make any assumptions about the distribution of the survival curves.

Limitations:

Calling the Help Line is not a required or necessary step to get a Ticket assignment. Therefore, we are not performing formal mediation analysis. In addition, doing subgroup analysis on Ticket assignment by those who called the Help Line and those who did not is subject to selection problems because our intervention is encouraging people to call the Help Line as one of their first actions.

Our analysis may be limited by our inability to capture EN capacity. In addition, ENs have their own marketing and outreach activities. Even if an individual's latent motivation is impacted, it is possible that we find an insignificant change in our primary and secondary outcomes because there was no EN available or willing to assign a beneficiary's ticket. However, the randomization of beneficiaries to experimental conditions should eliminate this potential bias.