Evaluation of the OSHA On-site Consultation Program (OSC): The Effect of High-Rate Letters on OSC Requests

Final Report

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OSC-HRL Memo—Evaluation of the Effect of High-Rate Letters on OSC Requests

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Executive Summary

The ***Site-Specific Targeting (SST) Inspection Program*** is a key component of the Occupational Safety and Health Administration's (OSHA) enforcement effort. The ***High Rate Letter (HRL)*** was a tool used by the SST Program to encourage work establishments that had injury and illness rates above the national average to consider hiring an outside safety and health consultant, talk with their insurance carrier, contact the workers' compensation agency in their state for advice, or ask for assistance from OSHA's On-site Consultation (OSC) program. OSHA sent the HRL to establishments with high rates of injuries and illnesses informing them that they may be selected for an inspection. These high-rate letters urged establishments with fewer than 250 employees to contact the On-Site Consultation Program (OSC).

The OSC Program, which is available in all 50 states, DC, and several U.S. territories, is administered in each state by a state agency or university, and is operated separately from OSHA's inspection program. The service is free, and there are no fines even if problems are found. The HRLs mailed to high hazard rate establishments told employers how to contact the OSC programs in their respective states. The letter also encouraged employers to consider developing and implementing injury and illness prevention programs as a proactive approach to find and fix workplace hazards before workers were hurt.

This document reports the results of an evaluation of the extent to which receipt of the high-rate letter increases requests for OSC consultations. The U.S. Department of Labor Chief Evaluation Office (DOL/CEO) asked Abt to explore this relationship as part of an ongoing contract to evaluate the effect of marketing on demand for OSC services. The exploratory results presented in this document provide preliminary evidence that the high-rate letter increases requests for OSC consultations.

ES.1 Evaluation Strategy

This analysis builds on the design of a broader ongoing evaluation of the SST Program. For that study, IMPAQ International randomly assigned some workplaces to receive a high-rate letter, some to receive a letter and a follow-up programmatic inspection, and others to receive neither a high-rate letter nor an inspection. The random assignment design also provides an opportunity to explore the impact of receipt of a high-rate letter on requests for OSC services. To estimate the impact of letter receipt on OSC requests, we compare the request rate for establishments that did and did not receive a high-rate letter as part of the IMPAQ study.

Data on OSC requests for this analysis were provided by DOL/OSHA from their Integrated Management Information System (IMIS). The study team linked these OSHA administrative records to data on the randomly assigned sample, provided by IMPAQ. Because there was no common identifier, the study team linked records between the two datasets using a probabilistic matching algorithm that compared establishment name and address information. This methodology was further strengthened by using geographic information system (GIS) software to compute the location (longitude and latitude coordinates) of each address in each data system. Potential matches were then determined by geocoding, based on precise geographic coordinates. The Abt team estimates that matching was completed with an error rate of less than 1 percent.

ES.2 Findings

For small and medium-sized workplaces, which are the focus of the OSC program and which were called out as eligible for the OSC program in the high-rate letter, the estimated impact is 1.9 percentage points;

without the high-rate letter 8.8 percent of these workplaces would have requested a consultation; with the high-rate letter 10.6 percent requested a consultation. This represents a 21.2 percent increase in the consultation request rate. Conventional statistical analysis provides some preliminary evidence that the result is not due to chance (p=0.074).

ES.3 Discussion

The analysis preliminarily suggests that that receipt of a high-rate letter by small and medium size firms (fewer than 250 employees) increased requests for a consultation by 21 percent. However, small sample sizes imply that even for this large impact, there is still a moderate possibility that the results were due to chance (7.4 percent, where results between 5 and 10 percent are conventionally interpreted as providing "some, but not definitive evidence").

In summary, these results provide preliminary yet encouraging evidence that the high-rate letters increase requests for OSC consultations. To draw more definitive conclusions, a replication of the study would be needed.

1. Introduction

The Occupational Safety and Health Administration (OSHA) of the U.S. Department of Labor (DOL) runs a voluntary program that provides free and confidential advice on approaches to avoiding injuries and illnesses to small and medium-sized establishments.[1] This effort, known as the On-site Consultation Program (OSC), operates in addition to—but totally separate from—OSHA's enforcement efforts. Nationwide, OSC performs approximately 27,000 consultations per year impacting a total of over 1.25 million workers. Separately, OSHA targets hazardous workplaces for enhanced enforcement activities through its Site-Specific Targeting (SST) Program. Each year, the SST program sends high-rate letters to establishments with particularly high rates of injuries and illnesses informing them they may be selected for an inspection and advising them of the availability of OSC services. A portion of the establishments that receive letters are subsequently inspected.

This OSC-High-Rate Letter (HRL) Study considers the interaction between the OSC program and the SST program. The OSC-HRL Study tests the hypothesis that receiving a high-rate letter induces establishments to request voluntary consultations at a higher rate than they would otherwise. There are a number of reasons—aside from the simple mention of the OSC program in the high-rate letter—why the high-rate letters might have this effect. The reasons include increased cognizance on the part of management that the company has an unsafe work environment, the fear of inspection and subsequent fines, and the knowledge that establishments participating in the OSC program are generally exempt from further programmed inspections while engaged in this voluntary review. These suppositions have been anecdotally supported by state OSC staff who reported during project interviews that consultations in their state annually increased at about the time that the high-rate letters were sent out. In addition, the study explores how various subsets of employers (e.g., with different numbers of employees) behaved in response to the high-rate letter.

The OSC-HRL Study builds on two existing studies of OSHA's programs:

1. In 2011 IMPAQ International randomly assigned 7,888 high-hazard establishments to various treatment conditions to test the effectiveness of OSHA's Site-Specific Targeting (SST) Inspection Program. As part of the IMPAQ International study (the *Evaluation Study of the Impact of SST Inspections*), 7,048 establishments were randomly assigned to receive a high-rate letter informing them that they were on the SST list, and a portion of these establishments were assigned to receive a follow-up inspection. An additional 840 establishments were randomly assigned to receive no letter and no inspection. Summit Consulting, LLC is currently analyzing the impact of the high-rate letter and subsequent inspection on injuries and illnesses.
2. Abt Associates is conducting an experimental study to test whether directly marketing the OSC program to establishments using messages based on behavioral theory can increase the OSC

[1] For the purposes of the OSHA On-site Consultation Program, a small business is defined as an employer having fewer than 250 employees at a fixed worksite and no more than 500 employees corporate wide.

request rate. Marketing brochures were mailed in spring 2014 and OSC requests will be measured using data recorded in OSHA's Integrated Management Information System (IMIS).[2]

The OSC-HRL Study builds on the experimental framework of the IMPAQ/Summit SST Impact Study to assess whether receiving a high-rate letter induces establishments to request OSC services. Because letter receipt was decided by lottery, we can estimate this impact simply by comparing establishments that received a letter with those that did not. Using administrative data from IMIS, we measure the same outcomes as for the OSC marketing study so that the impact estimate will be comparable to the estimate from that study.

This analysis builds on OSC administrative data from IMIS that were provided by DOL/OSHA for this analysis, which is in the spirit of recent OMB guidance encouraging the use of existing administrative data for statistical analyses (OMB, 2014). The study team linked these OSHA administrative records to data on the randomly assigned sample provided by IMPAQ. Because there was no common identifier, the study team linked records between the two datasets using a probabilistic matching algorithm that compared establishment name and address information across data systems. Relative to earlier uses of probabilistic matching with OSHA data (Gray, 1996), this implementation was augmented and improved by using geographic information system (GIS) software to compute the location (longitude and latitude coordinates) of each address in each data system. Potential matches that geocoding suggested were closer together based on geographic coordinates were considered more likely to be matches. This document presents Abt's findings regarding the impact of high-rate letters on requests for consultation. In brief, we find suggestive evidence that the high-rate letter may have increased the OSC request rate for establishments with fewer than 250 employees, which are the focus of the OSC program. This finding is only marginally statistically significant at conventional levels and should thus be viewed with caution. In part, the lack of greater significance appears to be due to sample size. This study was sized to detect impacts on injuries and illnesses rather than consultation requests. The resulting sample sizes are such that we can only be marginally confident that an impact as large as 21 percent is not due to chance. These top-line results will be discussed in greater depth in the remainder of this report.

The next chapter (Chapter 2) presents an overview of the OSC and SST programs, the IMPAQ/Summit SST Impact Study, and the OSC Marketing Study, which together provide a foundation for this analysis. The third chapter provides details on our methods and design. The fourth chapter presents the results of the analysis and discusses the possible implications of these results on demand for the OSC program in light of OSHA's proposed electronic reporting requirement. The fifth chapter discusses the potential study limitations, and the final chapter presents a discussion and interpretation of the findings. Appendices provide details on the high-rate letter, sample selection and random assignment, the matching algorithm used to merge sample with outcome data, and regression coefficients.

[2] OSHA recently transitioned to a new data system called the OSHA Information System (OIS), which records the same set of variables as IMIS. Abt's marketing study will use data from both IMIS and OIS. Because outcomes used for the OSC-HRL Study were measured before OIS was in place, we exclusively refer to OSHA's data system in this document as IMIS.

2. Background

This study considers the interaction between two OSHA programs:

- The On-site Consultation (OSC) Program that provides free and confidential voluntary safety and health consultations, and
- The Site-Specific Targeting (SST) Program that targets unsafe workplaces for enforcement actions. Specifically, the study examines the SST program's high-rate letter.

In particular, this study tests the hypothesis that employers are more likely to request OSC services after being informed, through the high-rate letter, that they may be targeted for SST inspection.

This chapter begins with background information on these two programs. This is followed by a description of the theory of change underlying the hypothesis as well as an overview of two ongoing evaluations of OSHA programs upon which this study is built.

2.1 On-site Consultation (OSC) Program

OSHA's On-site Consultation Program (OSC) is a free and confidential voluntary service offered to smaller businesses in high-hazard industries. The purpose of the program is to help employers identify and correct workplace hazards and improve their safety and health management systems. After an employer schedules a visit, an OSC consultant travels to the worksite to evaluate potential hazards, work practices, and the employer's safety and health management program. The consultant discusses findings with the employer and recommends improvements. All serious hazards identified must be corrected within a specified period of time.

The program is implemented by state governments or their designees, which receive funding through sections 21(d) and 23(g) of the Occupational Safety and Health Act of 1970. States are broadly divided into two groups for the purposes of OSC: Federal States receive only 21(d) funding for private sector consultations; State Plan States receive both 21(d) and 23(g) funding for private and public sector consultations. Two states, Kentucky and Washington, and Puerto Rico are funded through a slightly different mechanism and are not included in this study.[3]

The OSC program is completely separate from OSHA's enforcement activities, and the results of consultations are kept strictly confidential. No citations or penalties result from these consultations. In addition, businesses that participate in OSC receive a deferral from routine OSHA inspections during the time an OSC visit is in progress. A visit is considered to be "in progress" from the beginning of the OSC opening conference to the end of the correction due dates (including extensions). However, businesses that receive a consultation are required to correct any hazards identified during the visit in progress. If an employer fails to correct an imminent danger or a serious hazard noted as part of a consultation, the consultant is required to report that danger or hazard to OSHA enforcement. This combination of failure to correct and a resulting referral to OSHA enforcement appears to be rare.

The OSC program is voluntary; the first step towards receiving a consultation is an employer request. Most requests are made via phone or fax, though employers occasionally submit requests by email or by

[3] A full list of the State Plan States can be found at https://www.osha.gov/dcsp/osp/faq.html

filling out forms on their states' OSC websites. Some requests are initiated in response to one of OSHA's existing marketing efforts, such as mailed brochures, radio advertisements, or booths at trade fairs. States' OSC staff record requests in the federal OSHA Information System (OIS) or its predecessor, the Integrated Management Information System (IMIS), typically within one week of the actual request date. Requests can be withdrawn for any reason before the initial consultation visit. Analysis of IMIS records indicates that approximately 7 percent of requests are withdrawn.

OSHA's OSC program is not the only provider of health and safety consultation services. Similar free services are sometimes offered by state and local government entities as well as insurers. There are also private sector consultants that offer similar services for a fee. At least two factors might explain why establishments would pay a fee for a service that is provided by OSHA for free. First, establishments might—incorrectly—perceive that an OSC consultation will increase the likelihood of an OSHA enforcement visit. Second, a non-OSHA, for-fee consultation does not necessitate the employer taking any remediation steps even if hazards are identified.

2.2 Site-Specific Targeting Program

The purpose of the SST program is to target limited OSHA enforcement resources to the country's most dangerous workplaces. Establishments are targeted for inclusion in the program based on their response to the annual OSHA Data Initiative (ODI) survey. The ODI survey collects data on injuries and illnesses attributable to work-related activities in private sector industries. This information is then used to calculate establishment-specific injury/illness rates such as the Days Away, Restricted, and Transfer (DART) rate and Days Away from Work (DAFWII) rate. The survey has historically been sent annually to approximately 80,000 establishments in select non-construction, high-hazard industries. To be included in the survey, establishments must employ at least 20 individuals.[4] Out of the 80,000 employers sampled for the ODI survey each year, approximately 15,000 are selected for inclusion in SST because of their high injury and illness rates. The overall objective of the SST program is to increase compliance with federal workplace safety and health standards and thereby reduce injury and illness rates.

The SST program includes two types of enforcement activities:

1. ***High-rate letters*—**In most years, these letters are sent to all establishments that exceed an established injury/illness threshold as reported on the ODI survey. The letter informs such establishments that they have higher than typical injury and illness rates and are thus at increased risk of being inspected. The letter also urges the establishments to take corrective action, and informs them of the availability of the free On-Site Consultation resources.[5]

2. ***Inspections***—OSHA Area Office Compliance Safety and Health Officers (CSHOs) conduct "programmed" inspections of sites with the highest injury/illness rates to determine compliance with federal workplace safety and health standards. When violations are found, fines and citations

[4] The ODI survey was fielded for calendar years 1996 to 2011. It has not been fielded in the last three years.

[5] A reproduction of the April 2011 high-rate letter is provided in Appendix A.

may be issued. Employers are also required to take corrective action regarding each violation in a timely fashion.[6]

The primary benchmark OSHA uses for targeting inspections, and previously used for identifying establishments to receive the high-rate letters, is the Days Away/Restricted or Job Transfer (DART) rate, which is calculated from injuries resulting in days away from work, restrictions from normal job duties, or both, as reported on the ODI.[7] By design, approximately 15,000 employers exceed the minimum threshold for receiving a high-rate letter. However, OSHA does not have sufficient enforcement resources to inspect all 15,000 establishments. To aid efficient allocation of OSHA resources, establishments that had exceeded the basic SST threshold were categorized and prioritized into three tiers depending on their injury/illness rates. OSHA described these tiers as the primary list, secondary list, and high-rate letter only list. Enforcement resources are devoted first to establishments on the primary list, and then to establishments on the secondary list. Establishments that fell below the secondary threshold did not typically receive programmed inspections.

OSHA compiles these lists in March and mailed the high-rate letters in April. In June, OSHA Area Offices begin conducting inspections for establishments on the primary inspection list.

The time required to complete an inspection varies from several weeks to several months, depending on factors including establishment size (number of employees) and complexity (e.g., many types of machinery). During an inspection, CSHOs record safety and health violations. OSHA must issue a citation and proposed penalty within six months of the violation's occurrence; in some cases OSHA policies allow for penalties to be reduced for employers acting in good faith. Penalties, if any, relate to the number and severity of violations as well as other factors such as the size of the employer.

These procedures were slightly modified for the 2011 round of the SST (SST11) to accommodate an evaluation of the SST program, which is ongoing. Additional details of this experimental design are included in Chapter 3. In brief, a control group of SST-eligible establishments, selected at random, were not sent the high-rate letter or any other notification that their injury/illness rates exceeded the SST threshold. Likewise, establishments that were sent a high-rate letter (i.e., the SST-letter treatment group) were randomly assigned to receive an inspection either in 2011 or 2012.

For SST11, establishments were sorted into the three lists (primary, secondary, and high-rate letter only) as usual but the procedures for prioritizing inspections were modified to accommodate the evaluation. Exhibit 2.1 shows this arrangement.

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[6] A general overview of what to expect during the inspection process can be found on OSHA's website at https://www.osha.gov/OshDoc/data_General_Facts/factsheet-inspections.pdf

[7] The DAFWII rate (Days Away From Work Injury and Illness) rate is also used for targeting. The DART/DAFWII threshold varies by industry, with separate thresholds calculated for (1) manufacturing; (2) nursing and personal care; and (3) other sectors.

Exhibit 2.1: Primary, Secondary, and High-Rate-Letter-Only Lists

List	Injury/Illness Rate	Inspection Priority (Typical)	Inspection Priority (SST11)
Primary	Highest	Highest priority for inspection	Some establishments randomly assigned to receive inspection
Secondary	Intermediate (below primary list threshold but well above SST threshold)	Inspected after all establishments on the primary list have received inspections and only if resources allow	Some establishments randomly assigned to receive inspection
High-rate letter only	Exceeds SST threshold but lower than secondary list threshold	No programmed inspections	No programmed inspections[a]

[a]Regardless of random assignment status, any establishment in any tier could have received a *non-programmed* inspection if serious hazards were reported that posed an imminent danger to workers.

SST11 adhered to the same timeline as in a typical year, with inspections beginning in June 2011 for establishments that had been randomly assigned to receive programmed inspections (in both the primary and secondary inspection lists).

2.3 Study Background

This study addresses the interaction between the OSC program and the SST program's high-rate letters. As mentioned above, the high-rate letters are sent to establishments with high injury/illness rates and are designed to inform establishments that they are at increased risk of receiving an inspection. The letter also urges employers to take corrective action and informs them of the availability of the OSC program. This study, therefore, tests the hypothesis that receiving a high-rate letter induces establishments to request voluntary consultations at a higher rate than they would otherwise. There are a number of reasons to support the hypothesis that high-rate letters might have this effect, including the following:

- ***Increased cognizance of safety issues***—High-rate letters clearly convey a message that an establishment's work environment is less safe than comparable worksites. An employer could have been unaware of this previously, and upon realizing the discrepancy with comparable firms, such an employer might decide to take proactive steps to improve safety conditions. This could include scheduling an OSC visit, as suggested by the letter.
- ***Fear of inspections and penalties***—High-rate letters send an explicit signal to establishments that they are at increased risk of receiving a full (programmed) OSHA inspection. Such inspections may result in financial penalties and other costs. Responding to that signal, an establishment may schedule an OSC visit to identify and correct violations before they result in penalties.
- ***Attempt to delay an inspection***—OSC participants generally receive a one-year exemption from routine OSHA inspections. An establishment that was aware of this exemption might schedule an OSC visit specifically to delay an inspection.

DOL tasked Abt Associates with testing this hypothesis. To complete this work, Abt built upon two ongoing evaluations of OSHA's programs:

1. **Impact Evaluation of the SST Program—**In 2011 IMPAQ International randomly assigned 7,888 high-hazard establishments to various treatment conditions to test the effectiveness of OSHA's Site-Specific Targeting (SST) Inspection Program. Data analysis is presently being undertaken by Summit Consulting. As part of the IMPAQ/Summit study, 7,048 establishments were randomly assigned to receive a high-rate letter informing them that they may be on the SST list. An additional 840 establishments were randomly assigned to receive no letter. As noted in Section 2.2, this study design required some temporary modifications to the procedures for prioritizing programmed inspections in SST11.

2. **OSC Marketing Study—**Abt Associates is conducting an experimental study to test whether directly marketing the OSC program to establishments by mailing and/or emailing brochures using messages based on behavioral theory can increase the OSC request rate. Brochures were mailed in spring 2014 and OSC requests will be measured using data recorded in IMIS.

This study estimates whether randomly assigned high-rate letter recipients in the IMPAQ/Summit evaluation requested consultations at a higher rate in the year following letter receipt compared to their counterparts who did not receive letters. Requests for consultation services are measured using administrative data from IMIS. These outcomes are matched to the SST Impact Study evaluation sample using a probabilistic matching algorithm originally developed by Wayne Gray for the Bureau of Labor Statistics (BLS) (Gray, 1996) and subsequently modified for the OSC marketing study.

3. Methods

This chapter describes the study research questions and design, including sample selection and random assignment. It then describes the two data sources used for the study along with how they are matched to create the final analytic dataset. Baseline descriptive statistics for this sample are then provided. The chapter concludes by explaining how data will be analyzed.

3.1 Research Questions

This study addresses four closely related research questions about the high-rate letter's impact on requests for consultation:

1. Does mailing high-rate letters increase the rate of OSC requests in the short term (defined as a two-month period after high-rate letters are mailed)?
2. Does mailing high-rate letters increase the rate of OSC requests in the long term (defined as a 10-month period beginning two months after the high-rate letters are mailed)?
3. Does mailing high-rate letters increase the total annual rate of OSC requests (defined as the 12 months after the high-rate letters are mailed)?
4. If OSHA expanded the Site-Specific Targeting Program to cover the universe of all establishments instead of only establishments in the ODI survey, how many additional consultation requests should OSHA expect during the first year after high-rate letters are mailed?

These research questions reflect various hypotheses about how employers might react to a high-rate letter. One hypothesis is that most of the impact on consultation requests (if any) will occur in the two months immediately following mailing of the high-rate letter, while the letter is still fresh in the minds of recipients. This is based on the premise that receipt of a high-rate letter would immediately increase the likelihood of a request for consultation. The first research question above partially addresses this hypothesis by comparing short-term request rates in the treatment and control groups using the entire sample of establishments that were randomly assigned by IMPAQ.

However, a competing hypothesis could explain a short-term increase in requests. Rather than increasing total requests, the high-rate letter could instead shift the timing of requests, for example from later in the year to earlier in the year. (Perhaps high-risk establishments may already have intended to get a consultation but upon receiving the letter decided to "get ahead" of a potential inspection.) This hypothesis would imply that in months 3–12 after receipt of the high-rate letter, requests for consultation would fall (relative to no high-rate letter).

Conversely, some establishments that schedule a consultation in response to the high-rate letter may wait to do so, either to correspond with a period of low activity or due to inertia. This hypothesis would imply that in months 3–12 requests for consultation would rise relative to the control group (but perhaps by less than in the first two months). The net impact is likely to be a combination of these two effects. Which effect is stronger is unclear, a priori. Nevertheless, it seems plausible that the total number of requests for consultation over the full 12-month period will rise. Research questions two and three are designed to distinguish between these competing hypotheses.

Finally, at the time of this report, OSHA has proposed requiring firms with 250 or more employees, as well as firms with 20 or more employees that are in designated (high-hazard) industries, to electronically submit their form 300 data each year.[8] Until 2012, the ODI requested these data from a sample of only 80,000 firms. Thus, the proposed regulation might plausibly result in significant expansion in the number of establishments receiving high-rate letters and perhaps requesting consultations as a result. Research question four addresses this pending policy change. Using the average treatment effects estimated for research questions one to three, we calculate the impact on consultations that would be anticipated due to OSHA's proposed policy.

Subgroup Analysis

We address research questions one to four for the entire sample on average. In addition, we estimate impacts for key subgroups of establishments defined using baseline characteristics, and test whether the impact of the high-rate letter differs among subgroups. The subgroups of interest are defined by the following:

1. ***Inspection history***—Some establishments on the SST list have previously received inspections. The high-rate letter may have a smaller impact on the behavior of these employers, as they may already be cognizant that they are operating an unsafe workplace. Alternately, some may believe that they have already taken appropriate steps to mitigate hazards in response to their inspection.

2. ***Primary versus secondary list***—Establishments on the primary inspection list have higher rates of injuries/illnesses than establishments on the secondary inspection list, and may therefore have a stronger incentive to request a consultation (either to avert injuries or to mitigate the potential penalties from an inspection—which they might perceive, correctly, to be more likely).

3. ***Industrial sector***—The possibility of an inspection may be of more concern to establishments in certain industries. We estimate impacts separately for (1) manufacturing establishments with high-hazard machinery in OSHA's Amputation National Emphasis Program (NEP); (2) nursing and residential care facilities; and (3) other high-hazard manufacturing establishments, identified as those that are on any of the 2010-2013 high-hazard lists *or* are included in one or more current National Emphasis programs.[9]

4. ***Establishment size***—The text of the high-rate letter states: "An excellent way for employers with 250 or fewer workers to address safety and health in their workplaces is to ask for assistance from OSHA's consultation program." Consistent with this language, the analysis in the body of the report focuses on establishments with 250 or fewer employees. However, large establishments do sometimes make requests, and it is possible that the language in the high-rate letter could affect the request rate for such establishments. For example, it might dissuade them from requesting a consultation because they might perceive that such a request would not be filled or at least that it would be assigned very low priority. Exhibit 4.1 and Appendix E present results for large establishments and for the entire SST Study sample.

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[8] This regulation will apply to all establishments currently required to keep injury and illness records under OSHA's regulations for recording and reporting occupational injuries and illnesses.

[9] We define these groups using a list of NAICS codes. The full list is provided in Abt's "Pilot Design Memo: Evaluation of OSHA's On-Site Consultation Program," revised draft submitted July 17, 2013.

While these hypotheses about the impact in subgroups and the differential impact between subgroups are compelling, as we discuss in Chapter 5 our power to detect such impacts is low.

3.2 Study Design

The sample for this study consists of all establishments that were randomly assigned as part of the SST impact study introduced above. Sample selection and random assignment were completed by OSHA before Abt became involved in the study. Here we provide a brief overview of the sampling and random assignment processes. Additional details can be found in Appendix B.

Sampling

Sampling proceeded as follows. First, OSHA selected establishments into the 2010 ODI survey sample using Dun & Bradstreet's database. Only high-hazard, non-construction establishments with at least 20 employees are eligible for ODI. From the resulting list of approximately 170,000 employers, OSHA selected the 80,000 that would be required to respond to the ODI survey in 2010. A total of 65,000 establishments were selected from the set of industries targeted by that year's ODI (representing a near universe of establishments in those industries), and the remaining 15,000 were selected because they had among the highest injury/illness rates of all establishments in the previous (2009) ODI survey, regardless of industry.

Second, OSHA used the results from the 2010 ODI survey to identify eligible establishments based on injury and illness rates from *Federal States*.[10] With the exception of Connecticut, New Jersey, and New York, *State Plan States* were not included in the evaluation. Establishment-level Days Away, Restricted, and Transfer (DART) and Days Away From Work Injury and Illness (DAFWII) rates were used to group eligible establishments into three tiers, corresponding to inspection priority: (1) primary list sites, (2) secondary list sites, and (3) sites that did not exceed the secondary inspection threshold. IMPAQ's sampling frame consisted of 15,697 such establishments. Of these, a total of 7,888 establishments were categorized as exceeding the primary or secondary inspection thresholds and were included in random assignment.

Random Assignment

OSHA randomly assigned all 7,888 establishments that exceeded the secondary list threshold to eight distinct groups for the purpose of the SST Impact Study: six study arms and two groups of establishments that were ultimately excluded from the study because of limited resources available for follow-up data collection. For the OSC-HRL Study, we consolidated the eight original SST Impact Study groups into two study arms: a single treatment group of establishments that had been mailed a high-rate letter in 2011 and a single control group of establishments that had not been mailed a letter. Exhibit 3.1 shows the sample size corresponding to each of the eight original randomly assigned groups and indicates how those groups were consolidated into two study arms for the OSC-HRL Study.

[10] States are divided into two groups for the purposes of OSC: "Federal States" receive only 21(d) funding for private sector consultations; "State Plan States" receive both 21(d) and 23(g) funding for private and public sector consultations. All Federal States participate in the ODI by default; this is not true of State Plan States.

Exhibit 3.1: Consolidation of SST Treatment Arms for OSC-HRL Study

SST Impact Study Treatment Status	Abt OSC-HRL Treatment Status and Sample Size (# Establishments)	
	Treatment Group (sent letter)	Control Group (no letter)
Exceed Primary Inspection Threshold		
Treatment Group A (Received letter but no inspection)	420	
Treatment Group B (Received letter plus inspection)	420	
Control Group A (No letter and no inspection)		420
Excluded from SST Impact Study (Received letter)	2,377	
Exceed Secondary Inspection Threshold		
Treatment Group C (Received letter but no inspection)	420	
Treatment Group D (Received letter plus inspection)	420	
Control Group B (No letter and no inspection)		420
Excluded from SST Impact Study (Received letter)	2,991	
TOTAL Analysis Sample	**7,048**	**840**

Random assignment into the original eight groups was stratified by list type (primary/secondary), two-digit SIC (Standard Industry Classification) code, OSHA region, and number of employees.[11] As Exhibit 3.1 shows, establishments in six of the randomly assigned groups (those in the first column) were mailed a letter and constitute our treatment arm; establishments in the remaining two groups (the second column) were not mailed a letter and thus constitute our control arm. Note that the OSC-HRL Study includes establishments in two groups that were randomly assigned to receive letters but were otherwise excluded from the SST Impact Study. They were excluded from that study because OSHA did not have sufficient resources to collect outcome data, through inspections, for the entire sample. Because we measure outcomes using administrative data from IMIS, we do not face this constraint. Our analysis sample includes all 7,888 establishments indicated as belonging to either the treatment (letter) or control (no letter) groups in Exhibit 3.1.

3.3 Data Sources

The OSC-HRL Study relies on two primary data sources: (1) the SST Impact Study's analysis sample file; and (2) IMIS consultation data (forms 20, 30, and 40) from April 2006 through April 2012. The following subsection describes these two data sources and how they were matched to create an analysis file.

SST Study Sample of Establishments

The list of establishments that were randomly assigned for this study was provided by IMPAQ International and was developed before Abt became involved in the study. The file includes establishment-level data on 15,697 SST-eligible establishments; of these 7,888 exceeded the secondary list threshold, were randomly assigned by OSHA, and are included in Abt's OSC-HRL Study. The file is

[11] Details of the random assignment process are presented in Appendix B.2.

a compilation of establishment-level data from several sources as described in IMPAQ's data documentation. These include Dun & Bradstreet sample file information (used to develop the sampling frame), the 2010 ODI survey responses, the SST11 high-rate letter list (indicating which establishments were selected to receive high-rate letters), and IMIS inspection data from 2000 through 2011 (used to develop inspection histories). The file also includes each establishment's randomly assigned status for the SST Impact Study, which we used to consolidate establishments into a single treatment arm and a single control arm for the Abt OSC-HRL Study.

OSHA Integrated Management Information System (IMIS)

We obtained information about OSC requests from OSHA's Integrated Management Information System (IMIS). IMIS is a nationwide database that collects information from several interrelated forms: OSHA forms 20, 30, and 40. Collectively, these forms gather detail on the consultation request, the consultation visit and hazards identified.

For this study, OSHA provided Abt with IMIS data on requests made between April 2006 and April 2012. These data allowed us to track requests made for one year after the mailing of the SST high-rate letter, as well as obtain previous consultation request histories for each establishment. Consultation data from April 2006 through March 2011 were used to construct baseline (i.e., pre-randomization) measures of consultation requests (e.g., "number of OSC requests last five years"), while data from April 2011 through April 2012 were used to construct short-term (April through June) and long-term (July through April) binary measures of the outcome (i.e., "made a consultation request").

For this analysis we primarily used data fields drawn from OSHA form 20, which records the consultation request, including basic identifying information about each establishment making a request along with the date of the request. Identifying information includes the establishment name, address (street address, city, state, and ZIP), phone number, and primary industrial classification. The OSHA form 20 also includes details about the request itself, such as the date the request was made and the type of consultation requested.

The IMIS dataset includes one record for each OSC request. Therefore, an establishment that made seven requests between April 2006 and April 2012 would have seven records in the IMIS database, one for each of the requests. IMIS contains no common establishment identifiers to link together multiple requests by the same establishment.

Matching Process

To create a final analysis file, we matched establishments in the SST Impact Study sample with OSC requests that had been recorded in the IMIS data file. Because the IMIS dataset contains multiple OSC requests from some establishments (e.g., some establishments requested a consultation one month after the letter was sent and then again after six months), each establishment could potentially be matched with several IMIS requests. However, each IMIS record could only be matched with up to one SST Impact Study sample record—and would be matched with no establishments in the sample file if the request was made by an out-of-sample establishment. This resulted in a matched request-level (rather than establishment-level) file. We subsequently reduced the size of this file, including exactly one record per sample member, with variables indicating the number of requests made by each establishment during each follow-up period.

Because no unique establishment identifier was common to both lists, we had to establish a method for determining matches. We drew upon an existing SAS program developed by Wayne Gray (1996). This

program uses a probabilistic matching algorithm to determine likely matches. Unlike a deterministic match, probabilistic matching does not require exact correspondence between the two datasets, which is an unfeasible standard given the many potential variations in business names and addresses. Instead it uses previously established matching algorithms and thorough human review to ensure the greatest accuracy.

Gray's existing program was designed to match IMIS to BLS data and required only minor modifications for the current study. In particular, we updated the program to (1) read in the current IMIS format; (2) read in the Dun & Bradstreet data format (rather than the BLS data format); (3) match on the set of characteristics common to the IMIS and Dun & Bradstreet datasets; and (4) incorporate GIS-derived latitude and longitude information. As a result of these modifications, it was also necessary to re-calibrate the program's internal weights. Details of the calibration process are given in Appendix D.

The resulting algorithm considers every possible match between IMIS and the SST Impact Study sample records, and for each pair assigns a likelihood of that pair being a match. The likelihood is determined by considering agreement or non-agreement on a set of key traits, with agreement typically increasing the match likelihood and non-agreement decreasing it. For example, a request record from the IMIS dataset that has the exact same name, same address, and same phone number as an establishment on the SST list would likely be assigned a "high" likelihood of being a match. Conversely, a pair of records that does not match on any of these characteristics (and may not even be in the same city), would be assigned a "low" likelihood. To minimize the influence of possible data entry errors, each matching characteristic was disaggregated to the maximum degree feasible (e.g., address was disaggregated as street number, street name, city, and ZIP) with each characteristic considered independently. A list of these fields and the weight assigned for agreement or non-agreement on each is given in Appendix D.

The program was calibrated using a known set of true matches between the two samples. This allowed us to calculate the relative increase in the probability of a match due to each characteristic. To implement the calibration process we first obtained a list of true matches by running a naïve version of the algorithm on a geographically defined subset of data followed by extensive hand-checking. Once true matches were known for this subsample, we calibrated the program by setting weights for agreement and non-agreement based on a series of tests as specified in Gray (1996). As a final step in the calibration process, we identified two threshold likelihood values: one above which a potential match would be accepted as a true match and another below which a potential match would be rejected as a non-match. The small number of potential matches that fell between the two thresholds was checked by hand.

After calibration was completed, we implemented the matching algorithm using the full SST Impact Study sample and IMIS data files to generate a consolidated file with one record for each establishment on the SST list. For each establishment, we generated variables indicating the number of requests and the date of each request. From these, we created binary analysis variables for each of the three main outcomes. All told, 3,703 of the 7,888 establishments in the SST Impact Study sample were matched with at least one request in the IMIS file (including requests made in the five years before the study).

3.4 Descriptive Characteristics of the Sample

Baseline descriptive statistics of the analytic sample, by treatment status, are presented in Exhibit 3.2. The sample consists of 7,888 total establishments, of which 7,048 are in the OSC-HRL treatment group (i.e., received a high-rate letter) and 840 are in the control group (i.e., did not receive a letter).

Exhibit 3.2: Baseline Descriptive Characteristics of Establishments in the SST Impact Study Sample

Characteristic	Treatment Group Mean	Control Group Mean	Difference	P-value of Difference
Primary list	45.6%	50.0%	-4.4%**	0.0167
Number of employees	127.21	122.32	4.89	0.7117
More than 250 employees	7.5%	8.7%	-1.2%	0.2275
Total hours worked (thousands)	239.84	225.97	13.87	0.5433
Consultation Request History				
Made an OSC request in past 5 years	19.4%	19.3%	0.1%	0.9315
Number of prior OSC requests	0.50	0.49	0.02	0.5020
Inspection History				
Received an inspection in past 5 years	34.0%	33.7%	0.3%	0.8407
Number of prior inspections	0.565	0.55	0.01	0.6906
OSHA Region				
I Boston	12.2%	13.7%	-1.5%	0.2020
II New York City	12.5%	10.5%	2.0%*	0.0891
III Philadelphia	13.3%	14.0%	-0.8%	0.5368
IV Atlanta	13.9%	14.2%	-0.2%	0.8446
V Chicago	21.4%	21.5%	-0.1%	0.9345
VI Dallas	14.8%	15.0%	-0.2%	0.8592
VII Kansas City	6.0%	6.7%	-0.6%	0.4662
VIII Denver	4.7%	3.7%	1.0%	0.1716
IX San Francisco	1.2%	0.7%	0.4%	0.2414
Industry				
Amputation NEP	21.7%	22.9%	-1.2%	0.4292
Nursing and personal care facilities	7.4%	7.9%	-0.4%	0.6491
High-hazard manufacturing	20.4%	19.3%	1.1%	0.4410
Injury/Illness Rates				
DART	9.71	10.09	-0.38*	0.0541
DAFWII	5.346	5.615	-0.27	0.1387

Note: The first two columns report sample means (XX,XXX) or sample proportions (T.TT/C.CC). The Difference column reports the difference between the means or proportions (calculated as Treatment–Control). The p-value for this difference is given in the final column; significance reported as *** =1 percent, ** =5 percent, * =10 percent.

Exhibit 3.2 demonstrates that establishments in the SST Impact Study sample are particularly high risk, with an average DART rate of nearly 10 cases per 100 full-time employees (12.2 for establishments on the primary list and 7.6 for establishments on the secondary list), compared with the 2010 national

average for private sector establishments of 1.8 cases per 100 full-time employees.[12] Most establishments in the sample (92 percent) would be eligible for OSC based on the number of employees working at the establishment (i.e., fewer than 250), but only 19.4 percent had actually made a consultation request within the past five years. In contrast, 34 percent had received an inspection in the past five years. Approximately half are on the primary list and half on the secondary list. We found some evidence of baseline imbalance in injury/illness rates (presumably due to chance): control group establishments are somewhat more likely to be on the primary inspection list (50.0 percent in the control group versus 45.6 percent in the treatment group; $p<0.05$) and they have a correspondingly higher DART rate (10.1 in the control group versus 9.7 in the treatment group; $p<0.10$). Our regression analyses control for these characteristics.

Exhibit 3.3 presents descriptive characteristics for small and medium-sized workplaces, which are the focus of the OSC program and of this report. It is these firms that were explicitly identified as eligible for the OSC program in the high-rate letter.[13] Because such establishments comprise the bulk of the overall sample (7,285 of the 7,888 total establishments, or 92 percent), it is not surprising that the characteristics of this subsample closely resemble those for the full sample presented in Exhibit 3.2.

Exhibit 3.3: Selected Baseline Descriptive Characteristics of Establishments in the Study Sample, for Small and Medium Size Establishments (<250 Employees)

Characteristic	Treatment Group Mean	Control Group Mean	Difference	P-value of Difference
Primary list	46.8%	51.0%	-4.2%**	0.0287
Number of employees	79.26	76.81	2.45	0.2108
Consultation Request History				
Made an OSC request in past 5 years	19.8%	19.4%	0.4%	0.818
Number of prior OSC requests	0.51	0.48	0.03	0.5876
Inspection History				
Received an inspection in past 5 years	32.8%	33.5%	-0.7%	0.7067
Number of prior inspections	0.527	0.533	-0.006	0.8612
Industry				
Amputation NEP	21.6%	22.8%	-1.2%	0.4464
Nursing and personal care facilities	7.6%	8.2%	-0.6%	0.5213
High-hazard manufacturing	20.7%	18.6%	2.1%	0.1925
Injury/Illness Rates				
DART	9.780	10.202	-0.422**	0.0439
DAFWII	5.437	5.763	-0.326*	0.0907

Note: The first two columns report sample means (XX,XXX) or sample proportions (T.TT/C.CC). The Difference column reports the difference between the means or proportions (calculated as Treatment–Control). The p-value for this difference is given in the final column; significance reported as *** =1 percent, ** =5 percent, * =10 percent.

[12] See Chart 1 in the BLS news release at http://www.bls.gov/news.release/pdf/osh.pdf

[13] The distribution by OSHA region does not differ by establishment size, and is thus omitted from Exhibit 3.3 for brevity.

Appendix Exhibit E.1 presents descriptive characteristics for large establishments, i.e., those with 250 or more employees.

3.5 Analytic Methods

The experimental design implemented by OSHA/IMPAQ implies that a simple treatment-control mean outcome comparison would provide an unbiased estimate of true impact across all establishments. However, in our main analysis we calculate impact estimates using multivariate regression models. Using such regression models controls for any chance variation across the sample in baseline measures (e.g., DART rate). Control variables also increase statistical precision of the impact estimates for a given sample size (Orr, 1999) and reduce attrition bias from missing data (Puma et al., 2009). That is, for a given sample size, regression adjusted estimates will have smaller standard errors and are more likely to detect impacts (when present).

We estimate a regression model with the basic form of equation (1) below:

(1) $Y_i = \alpha + \delta T_i + \sum_{k=1}^{K} \beta_k X_{ki} + \sum_{m=1}^{M} \gamma_m D_m + \varepsilon_i$

In this model:

Y_i is the outcome of interest (i.e., consultation request) for the i^{th} establishment;

T_i is a dummy variable equal to 1 if establishment i was assigned to the treatment group;

X_{ki} are baseline explanatory characteristics (e.g., number of employees); and

D_m are dummy variables representing binary randomization strata (provided by OSHA).

The coefficient δ is interpreted as the intent-to-treat impact of being assigned to receive a high-rate letter on the outcome. The regression coefficients, β_k, reflect the influence of establishment background characteristics on the control group mean and are generally not of interest to interpret and discuss. They include characteristics such as inspection history. We analyze the main outcome of interest, which is a binary outcome, using a linear regression model because such models have been shown to be remarkably robust (Lumley et al., 2002; Judkins & Porter, 2013). The model is estimated separately for each outcome (i.e., requests for consultation in the short term, in the long term, and total). Coefficients, standard errors, and R-squares from the regression model are presented in Appendix F for each of the three primary outcomes.[14]

We also estimate impacts for several key subgroups of establishments: by threshold status (primary versus secondary list); number of employees (fewer than 250 and 250+); inspection history in previous five years (yes or no); and industrial classification (Amputation NEP, nursing homes and care facilities, and high-hazard industries). The regression models for subgroup analyses include an interaction term to separately estimate the impact for establishments assigned to each subgroup. The regression model has the basic form of equation (2):

[14] Neither our impact estimates nor standard errors account for uncertainty in the probabilistic matching process. It is possible that matching errors could bias the impact estimates if they are asymmetric in the treatment and control groups. Because we anticipate small matching errors, we believe the effect of such asymmetry would be trivial.

(2) $Y_i = \alpha + \delta_1 T_i + \delta_2 (T_i * Subgroup_i) + \sum_{k=1}^{K} \beta_k X_{ki} + \sum_{m=1}^{M} \gamma_m D_m + \varepsilon_i$

where $Subgroup_i$ is a dummy variable indicating membership in the subgroup of interest. In this model, the regression coefficient δ_1 represents the impact on establishments not in the subgroup of interest, and the regression coefficient δ_2 represents the differential impact on establishments that are members of that subgroup. We note that the study's power to detect a differential impact between subgroups is quite low.

Results are presented as regression-adjusted differences in means: We present the regression-adjusted treatment group mean, the regression adjusted control group mean, and the difference, which is the estimated impact (and is equivalent to the estimated coefficient in a linear model). Reported standard errors are heteroskedasticity-robust.

4. Results

This chapter presents the analytic results. We first present estimates of the change in consultation requests over the full year following mailing of the high-rate letter. We then present results for the short term (two months after letters were mailed) followed by results for the long term (months 3–12 after letters were mailed).

The results can be summarized as follows:

- We find suggestive evidence that the letter was effective at increasing the request rate for establishments with fewer than 250 employees (p=0.074).
- Also for small and medium-sized establishments, we find suggestive evidence that the high-rate letter increased the request rate in some subgroups of establishments, both over the full year after letter receipt and in months 3–12 after receipt.
- We do not find evidence of an impact for large establishments; however, we also do not find statistically significant evidence of an impact across the pooled group of all establishments that received high-rate letters.
- For other subgroups of interest (SST list type, inspection history, industry type), we do not find evidence of statistically significant differences in impact between subgroups of interest. We note, however, that given our small sample sizes, our ability to detect such differential impacts across subgroups is extremely limited.

In particular, our estimates suggest that the letter may have been effective at increasing the request rate for small and medium-sized establishments on the primary inspection list, and for small and medium-sized establishments that had not recently received an OSHA inspection. This pattern of findings is consistent with program rules that prioritize consultations for small, high-hazard establishments. However, because none of these subgroup analyses were pre-specified as confirmatory, we encourage readers to use caution when interpreting these results.

4.1 Overall Impacts

Estimating the overall impact on the consultation request rate for the full year after high-rate letters were mailed directly addresses the third research question specified in Section 3.1: *Does mailing high-rate letters increase the total annual rate of OSC requests (defined as the 12 months after the high-rate letters are mailed)?*

We estimate the impact separately for three groups of establishments: (1) small and medium-sized workplaces, those with fewer than 250 employees and therefore eligible for a consultation by OSC and encouraged by the high-rate letter to request one; (2) large workplaces, those with 250 or more employees and therefore not priority recipients of OSC consultation services and not encouraged by the high-rate letter to request one; and (3) the pooled group of all randomly assigned workplaces, both large and small-to-medium sized establishments. The impact on the pooled sample will be used to estimate the potential impact of OSHA rule changes in Section 4.4. Exhibit 4.1 presents the key results; i.e., the estimated impact of receiving the high-rate letter on requests for consultation over the following year for each group of establishments.

Exhibit 4.1: Impact of High-Rate Letter on One-Year Consultation Request Rate

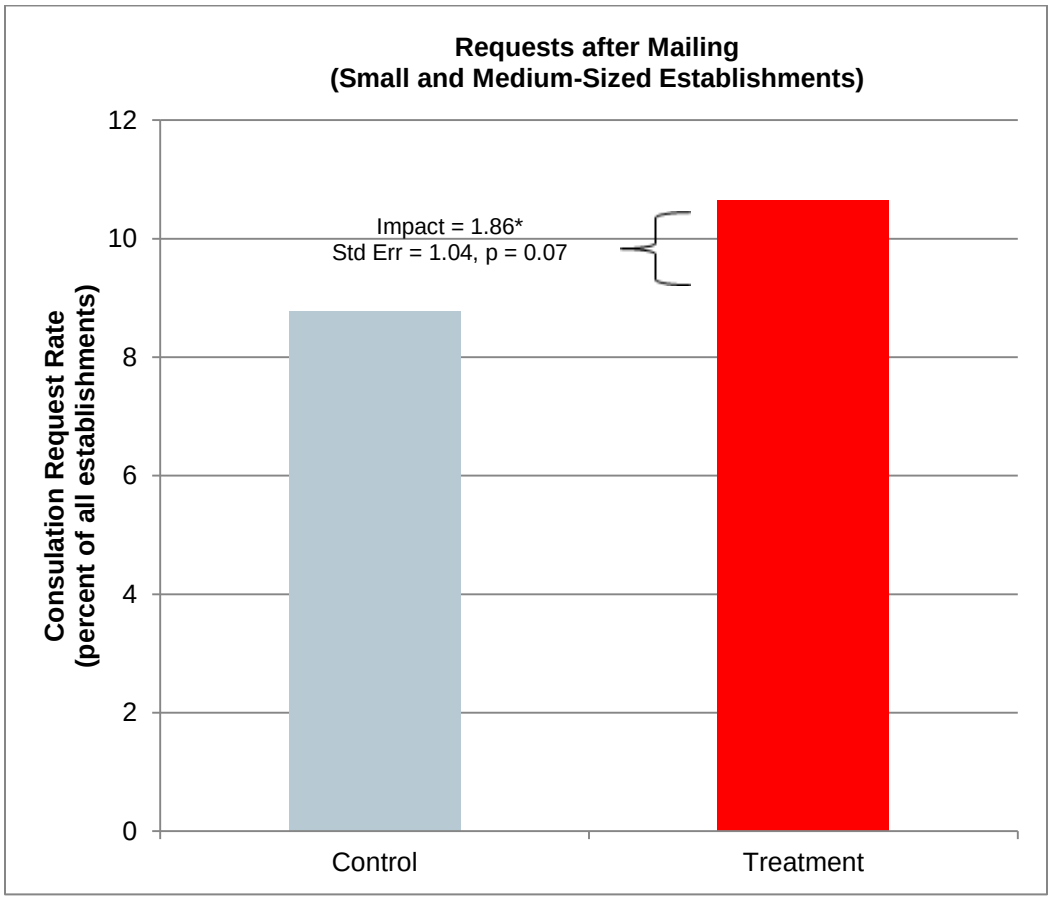
Number of Employees	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Fewer than 250 employees	7,285	8.78	10.64	1.86*	1.04	0.0742	21.2%
250+ employees	603	6.21	5.56	-0.65	3.29	0.8424	-10.5%
Pooled (all establishments)	7,888	8.67	10.25	1.58	0.99	0.1101	18.2

Two-sided test: *p<0.1, **p<0.05, ***p<0.01.

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups. A test for homogeneity across the two subgroups defined by employee size shows no evidence of a significant difference (p=0.261).

The first row of Exhibit 4.1 presents the results for small and medium-sized workplaces. We estimate that the letter increased the consultation request rate by more than 20 percent (1.9 percentage points) for such establishments; this impact is marginally significant (p=0.0742). Specifically, the request rate increased from 8.8 percent in the control group, which did not receive the letter, to 10.6 percent in the treatment group, which did (see Exhibit 4.2). This subgroup is of particular interest because of the text of the high-rate letter, which explicitly informs recipients that the OSC program is available to establishments with fewer than 250 employees and (by inference) not to larger establishments.

Exhibit 4.2: Impact of High-Rate Letter on One-Year Consultation Request Rate for Small and Medium-Sized Establishments



The second row of Exhibit 4.1 presents the estimated impact for large establishments, i.e., those with 250 or more employees. The high-rate letter did not recommend that these establishments contact the OSC program, and there is no evidence that the high-rate letter had an impact on requests for a consultation in this group. The statistical test indicates that the result could be due to chance (p=0.84). In fact, the estimate suggests that large establishments were more than 10 percent less likely to request a consultation after receiving the high-rate letter (see Appendix E for additional information).

The third line in Exhibit 4.1 presents an estimate of the average impact across all establishments in the sample. We do not find statistically significant evidence that the high-rate letter increased the request rate across all establishments. In particular, we estimate that the high-rate letter increased the consultation request rate over the full year by 1.6 percentage points, from 8.7 percent of all establishments in the control group, which did not receive the letter, to 10.3 percent in the treatment group, which did receive the letter. Stated differently, our estimates suggest that the high-rate letter resulted in an 18 percent increase in the consultation request rate. However, the p-value for this estimate (p=0.11) does not meet the conventional threshold for statistical significance (i.e., it is possible that the estimate is due to random variation in the outcome among establishments included in the study). We therefore cannot reject the null hypothesis that there was no impact on average for these establishments. The 95 percent confidence interval suggests that the true impact may be as small as -0.2 percentage points or as large as 3.9 percentage points.

Because small and medium-sized workplaces are the focus of the OSC program, were called out as eligible for the OSC program in the high-rate letter, and comprise the bulk of the study sample, we focus on such establishments in the remainder of this chapter. Results for large establishments and results for the pooled sample of small and large establishments (i.e., all establishments in the sample) can be found in Appendix E.

In addition to the average impact across the sample of small and medium-sized workplaces, we estimate impacts for several subgroups of particular interest, defined by SST categorization, inspection history, and industry type. These impact estimates are presented in Exhibit 4.3.

Exhibit 4.3: Impact of High-Rate Letter on One-Year Consultation Request Rate for Small and Medium-Sized Establishments

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	7,285	8.78	10.64	1.86*	1.04	0.0742	21.2%
SST Categorization	Test for homogeneity across subgroups: p=0.316						
Primary list	3,442	10.92	13.81	2.89*	1.60	0.0715	26.5%
Secondary list	3,843	7.01	7.81	0.80	1.32	0.5451	11.4%
Inspection History	Test for homogeneity across subgroups: p=0.075*						
Received inspection	2,397	12.86	11.91	-0.94	2.05	0.6452	-7.3%
No inspection	4,888	6.75	10.02	3.27***	1.18	0.0056	48.4%
Establishment Group	Test for homogeneity across subgroups: p=0.812						
Amputation NEP	1,584	13.14	16.11	2.97	2.52	0.2372	22.6%
Nursing homes	556	5.90	11.21	5.31*	3.07	0.0836	89.9%
High-hazard industries	1,489	10.10	13.12	3.02	2.77	0.2754	29.9%

Two-sided test: *p<0.1, **p<0.05, ***p<0.01.

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

We do not generally find statistically significant evidence of differences between the subgroups in Exhibit 4.3 (i.e., we do not find compelling evidence that the impact was larger in some subgroups than in others). The results of these tests are presented at the start of each subsection listed in Exhibit 4.3—for example, the ***test for homogeneity across subgroups*** defined by inspection history is marginally significant (p=0.075). Because these statistical tests do not show compelling evidence of differences between subgroups, we emphasize that the estimated impact for any individual subgroup should be viewed as suggestive rather than definitive. That said, our analysis suggests that the high-rate letter may be effective at increasing the consultation request rate within some subgroups of establishments such as those that have not been previously inspected and those on the primary list.

First, we find a marginally statistically significant impact of 27 percent (2.9 percentage points) for establishments on the primary inspection list (p=0.072<0.10). In this subgroup of particularly high-hazard establishments, our analysis suggests that the request rate increased from 10.9 percent in the control group to 13.8 percent in the treatment group. In contrast, establishments on the secondary inspection list—which had a substantially lower control-group request rate of 7.0 percent versus 10.9 percent in the primary

inspection list—appear to have been unmoved by the letter. We find a negligible and statistically insignificant impact for this subgroup. We were unable to reject the null hypothesis of no difference between subgroups.[15]

Second, our analysis suggests a large and statistically significant impact on the subgroup of establishments that had not received an OSHA inspection in the previous five years. We estimate that letter receipt increased the request rate in this subgroup by 48 percent (3.3 percentage points), from 6.8 percent in the control group to 10.0 percent in the treatment group ($p=0.006<0.01$). The corresponding impact estimate for establishments that had received an inspection is small (negative 7 percent) and insignificant. The difference in impacts between these two subgroups is marginally statistically significant ($p=0.075<0.10$).

Finally, we examined the impact for three subgroups of establishments defined by industrial sector. The estimated impact in two of these three subgroups is large in magnitude—30 percent for a collection of high-hazard industries, and almost 23 percent for establishments covered by OSHA's Amputation National Emphasis Program—but not statistically significant, so we cannot reject the null hypothesis that the true impact is zero. The impact for nursing homes, an estimated 90 percent increase in the request rate, is marginally significant ($p=0.08<0.10$).

4.2 Short-Term Impact

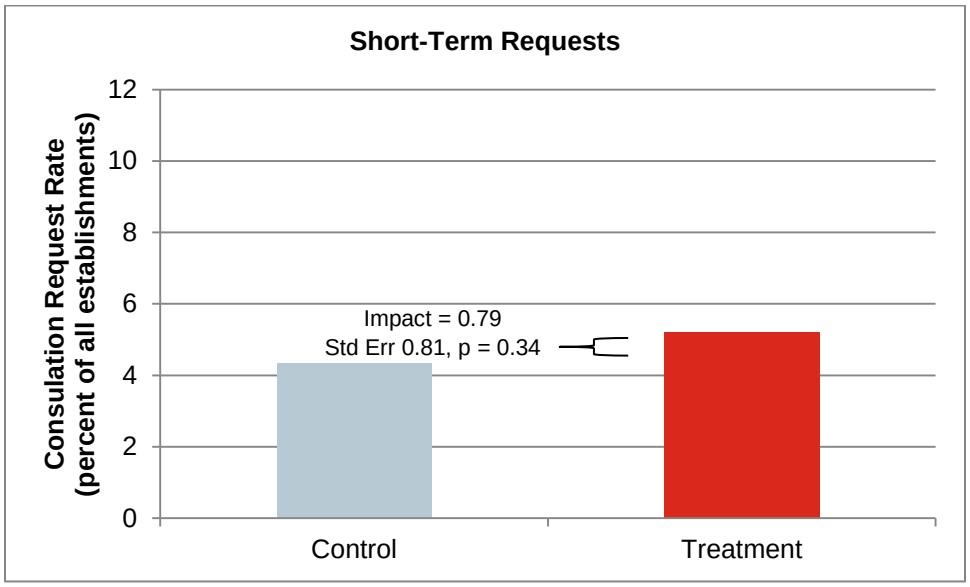
Estimating the impact on the short-term consultation request rate directly addresses the first research question specified in Section 3.1: *Does mailing high-rate letters increase the rate of OSC requests in the short term (defined as a two-month period after high-rate letters are mailed)?*

This question reflects the hypothesis that most of the impact on consultation requests (if any) would occur in the two months immediately following mailing of the high-rate letter, while the letter is still fresh in the minds of recipients. As in the preceding section, we first estimate this impact on average across all small and medium-sized workplaces in the sample before examining the impact for various subgroups of interest. Estimates for large workplaces and for all workplaces in the sample are presented in Appendix E.

Across this short-term time frame we do not find statistically significant evidence of an impact on average across all small and medium-sized establishments. However, the magnitude of the impact estimate suggests that the high-rate letter increased the short-term consultation request rate by approximately 0.8 percentage points, from 4.6 percent of all establishments in the control group, to 5.4 percent in the treatment group, (see Exhibit 4.4). Stated differently, the magnitude of the estimate suggests that the high-rate letter resulted in a 17 percent increase in the consultation request rate. Again, this estimate is not statistically significant ($p=0.34$) and, as such, we cannot reject the null hypothesis that there was no impact on average for these establishments.

[15] This finding is not surprising due to the limited sample sizes available for analysis. Detecting a differential impact between equally large subgroups requires a sample many times as large as the sample required to detect an impact in either subgroup individually.

Exhibit 4.4: Impact of High-Rate Letter on Short-Term Consultation Request Rate for Small and Medium-Sized Establishments



In addition, we estimate short-term impacts for the same subgroups as for the full-year impacts. These subgroups are again defined by SST list type, inspection history, and industry type. Subgroup impact estimates are presented in Exhibit 4.5.

Exhibit 4.5: Impact of High-Rate Letter on Short-Term Consultation Request Rate for Small and Medium-Sized Establishments

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	7,285	4.62	5.41	0.79	0.81	0.3346	17.0%
SST List	Test for homogeneity across subgroups: p=0.391						
Primary list	3,442	5.49	6.97	1.47	1.25	0.2369	26.9%
Secondary list	3,843	3.94	4.01	0.08	1.04	0.9405	2.0%
Inspection History	Test for homogeneity across subgroups: p=0.543						
Received inspection	2,397	5.72	5.76	0.05	1.56	0.9763	0.8%
No inspection	4,888	4.08	5.24	1.16	0.95	0.2223	28.4%
Establishment Group	Test for homogeneity across subgroups: p=0.327						
Amputation NEP	1,584	7.39	7.53	0.13	2.08	0.9492	1.8%
Nursing homes	556	5.23	7.45	2.22	2.91	0.4468	42.4%
High-hazard industries	1,489	2.62	6.78	4.16**	1.72	0.0159	158.8%

Two-sided test: *p<0.1, **p<0.05, ***p<0.01

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

Because of the small size of the subgroups, the impact estimates in Exhibit 4.5 are inherently imprecise. We find no statistically significant differences in the magnitude of the impact between subgroups, and thus cannot say with confidence that the letter was more effective in some subgroups than in others. We also find only one statistically significant short-term impact among the 10 subgroups of establishments, which could be due to chance. Therefore, we again encourage readers to exercise caution when interpreting these estimates.

For the subgroup of primary-list establishments, we find a statistically insignificant impact of 26.9 percent. The estimated impact of 2.0 percent for establishments on the secondary list is also insignificant. We find similar results for subgroups defined by inspection history. Consistent with the full-year outcomes, our analysis shows a large, although statistically insignificant, impact on the subgroup of establishments that had not received an OSHA inspection in the previous five years. We estimate that letter receipt increased the request rate in this subgroup by 28.4 percent. The corresponding impact for establishments that had received an inspection was near zero (0.8 percent) and also insignificant. We are again unable to say with confidence that any of the differences between these subgroups is greater than zero.

The only statistically significant impact among the three subgroups of establishments defined by industrial sector is for establishments in high-hazard industries ($p=0.016<0.05$). We estimate that letter receipt resulted in a 159 percent increase in consultation requests within this group. The 42 percent increase associated with letter receipt among nursing homes is statistically insignificant. Our results suggest that letter receipt is associated with only a small (2 percent) increase in requests for establishments in the Amputation NEP sector, and this estimate is likewise insignificant.

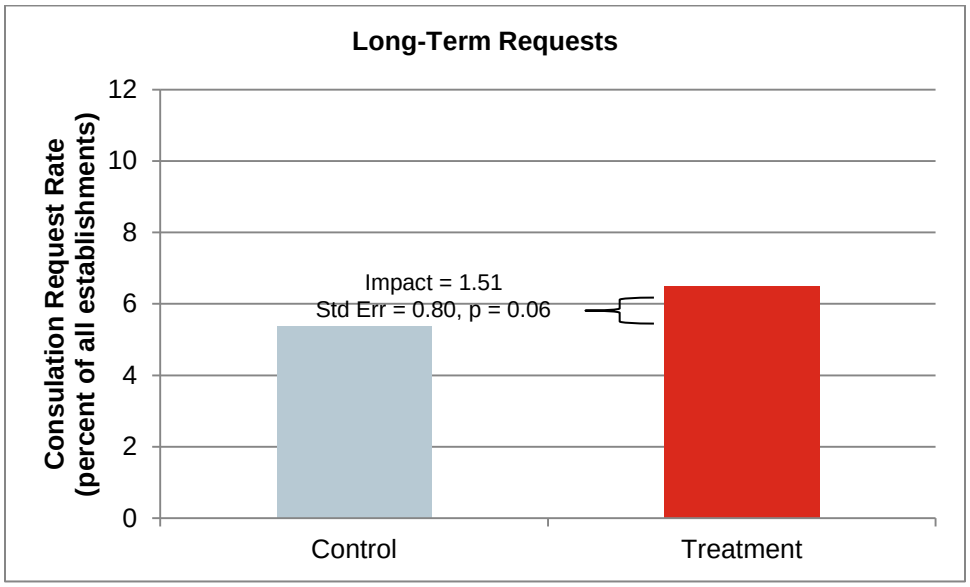
4.3 Long-Term Impact

Estimating the impact on the long-term consultation request rate directly addresses the second research question specified in Section 3.1: *Does mailing high-rate letters increase the rate of OSC requests in the long term (defined as a 10-month period beginning two months after the high-rate letters are mailed)?*

This question reflects the hypothesis that some establishments inclined to schedule a consultation in response to the high-rate letter will wait to do so. As in the preceding two sections, we first estimate this long-term impact on average across all small and medium-sized establishments in the sample before examining the impact for various subgroups of interest. Estimates for large workplaces and for all workplaces in the sample are presented in Appendix E.

The findings for long-term impacts generally mirror those for full-year impacts—both overall and for subgroups of interest—in magnitude and statistical significance. Averaged across all establishments, we find a marginally statistically significant impact of the high-rate letter on consultation requests ($p=0.06<0.10$). However, the magnitude of the impact estimate suggests that that the high-rate letter may have increased the long-term consultation request rate by about 29 percent. This is equivalent to an increase in the request rate of approximately 1.5 percentage points, from 5.2 percent of all establishments in the control group, which did not receive the letter, to 6.7 percent in the treatment group, which did receive the letter (see Exhibit 4.6). As with the full-year estimate presented in Section 4.1, this estimate is only marginally statistically significant and thus we encourage readers to exercise caution when interpreting the estimate.

Exhibit 4.6: Impact of High-Rate Letter on Long-Term Consultation Request Rate for Small and Medium-Sized Establishments



Long-term impacts for subgroups of interest, defined by SST list type, inspection history, and industry type are presented in Exhibit 4.7.

Exhibit 4.7: Impact of High-Rate Letter on Long-Term Consultation Request Rate for Small and Medium-Sized Establishments

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	7,285	5.18	6.69	1.51*	0.80	0.0580	29.1%
SST List	Test for homogeneity across subgroups: p=0.497						
Primary list	3,442	6.82	8.86	2.04	1.26	0.1043	30.0%
Secondary list	3,843	3.79	4.75	0.96	0.97	0.3233	25.4%
Inspection History	Test for homogeneity across subgroups: p=0.020**						
Received inspection	2,397	9.01	7.61	-1.40	1.67	0.4033	-15.5%
No inspection	4,888	3.28	6.24	2.97***	0.84	0.0004	90.6%
Establishment Group	Test for homogeneity across subgroups: p=0.209						
Amputation NEP	1,584	8.31	10.68	2.37	1.97	0.2298	28.5%
Nursing homes	556	0.90	5.77	4.86***	1.29	0.0002	538.1%
High-hazard industries	1,489	7.81	8.23	0.42	2.38	0.8595	5.4%

Two-sided test: *p<0.1, **p<0.05, ***p<0.01

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

We find no evidence of differences in impact between subgroups defined by SST list categorization or industry type. We do find statistically significant evidence of a larger impact for establishments that had

not been recently inspected than for recently inspected establishments. As with the full-year analysis reported in Exhibit 4.3, Exhibit 4.7 suggests that the high-rate letter may have large impacts on certain subgroups. In particular, our long-run analysis indicates a large and statistically significant impact on the subgroup of establishments that had not received an OSHA inspection in the previous five years. The estimated impact is larger than the full-year estimate of 48 percent: We estimate that letter receipt increased the request rate in this subgroup by 91 percent ($p<0.01$) in the long run. The corresponding impact for establishments that had received an inspection was negative (-16 percent) and insignificant. The difference between these subgroups is itself statistically significant ($p=0.02<0.05$), so we are more confident in concluding that the high-rate letter is more effective at increasing consultation requests for establishments that have not received an inspection in the previous five years than for recently inspected establishments. However, because this result has not been adjusted for the large number of hypothesis tests conducted as part of this analysis, we again urge caution when interpreting the result.

We also examine the long-term impact for three subgroups of establishments defined by industrial sector. The only statistically significant impact among these three subgroups is for nursing homes. We estimate that letter receipt caused a 538 percent increase in consultation requests in this group ($p<0.01$). We find a smaller and statistically insignificant long-run impact of 29 percent for establishments in the Amputation NEP sector and a long-run impact of only 5 percent for establishments in high-hazard industries.

4.4 Application of Results to Potential ODI Expansion

As of the time of this report, OSHA has proposed requiring firms over a certain size to electronically submit their form 300 data each year. Until 2012, the ODI obtained this information for SST purposes by surveying a sample of 80,000 firms using the process described in Section 3.2. A new regulation of this type could result in significant expansion of the primary and secondary groups in the high-rate letter list, and therefore an increased burden on OSHA consultation resources due to additional requests. Using the overall (i.e., across all establishments) average treatment effect estimated in Section 4.1, we calculate a rough estimate of the impact on consultations that would be anticipated due to OSHA's proposed policy.

The context of this estimate is important. In particular, establishments may behave differently the first time they receive the letter than they will subsequently. This would be especially true if the letter increases requests primarily because it increases cognizance of safety issues (i.e., conveys the important (and previously unknown) information that the employer's establishment is less safe than comparable worksites, leading the employer to take proactive steps to improve safety).[16] If the letter operates through this mechanism, then the rule change would have a much larger impact in the first year—when many employers receive this information—than in the following year, when many establishments will be receiving the letter a second time. We consider the estimate presented here to be an estimate of the likely impact in the first year after the rule change.

The universe of establishments that constitutes the ODI sampling frame consists of approximately 170,000 establishments. The findings presented in Section 4.1 for the pooled sample indicate that the high-rate letter increased the consultation request rate over the full year by approximately 1.6 percentage points, from 8.7 percent of all establishments in the control group, which did not receive the letter, to 10.3 percent in the treatment group, which did receive the letter. Multiplying the impact estimate of 1.6

[16] The finding that the long-term impact is significantly larger among establishments that have not recently been inspected supports this hypothesis.

percentage points by the number of ODI-eligible establishments yields a rough estimate of 2,720 additional consultations in the first year following adoption of the electronic reporting rule. Relative to the approximately 27,000 consultations provided annually by OSHA, this represents a 10 percent increase in the number of consultations that OSC would be asked to provide. However, this estimate is imprecise. Multiplying the number of consultations by the lower and upper bounds of the (pooled) impact estimate's 95 percent confidence interval (-0.06ppts, 3.2ppts) suggests that the true impact on consultation requests could range anywhere from a slight decline (102 fewer requests) to a relatively large increase (5,440 additional requests).

5. Study Limitations

The experimental design implemented by OSHA/IMPAQ provides a rigorous framework in which to conduct this analysis. In particular, using random assignment to create a treatment-control contrast implies that a simple treatment-control mean outcome comparison would provide an unbiased estimate of true impact across all establishments. Likewise, using administrative data to measure outcomes implies that the study does not suffer from survey nonresponse, which in some circumstances could bias impact estimates. Creating a final analysis file required matching the survey sample with IMIS records on consultation requests, and this process could result in bias under some circumstances. However, the circumstances of matching—which was blind to random assignment status and which we estimate to have a small error rate—suggest that any such bias would be minimal.

The primary limitation of this study is that sample sizes were not chosen with these particular analyses in mind and, as a result, are insufficient to provide the statistical power necessary to detect even fairly large impacts. The substantially unbalanced random assignment ratio of 9:1 also contributed to limited power to detect impacts. Ex-post power calculations reveal that the study was powered to detect impacts, averaged across all establishments, of approximately 3 percentage points for the full year (about a 35 percent increase), 2.3 percentage points in the short term (about a 52 percent increase), and 2.3 percentage points in the long term (about a 42 percent increase). Thus, the study could only plausibly have been expected to detect very large impacts. In fact, we estimate reasonably large impacts: averaged across all establishments, impact estimates range from 18 percent to 21 percent. Even if the true impact were in fact equal to our estimated impact, the samples would not have been large enough to detect it. Because the intervention has already been completed, it is not possible at this time to increase the sample size.

6. Discussion

This study has built on the random assignment of workplaces to receive high-rate letters, conducted as part of a study of the impact of the SST program on injuries and illnesses. Unfortunately, the sample sizes for the main study are not large enough to make definitive statements about the impact of the high-rate letters on requests for consultation.

Taken alone, the estimates for establishments with fewer than 250 employees—the workplaces encouraged by the high-rate letter to request an OSC consultation—suggest that receipt of a high-rate letter increased requests for a consultation by 20 percent. However, small sample sizes imply that even for this large impact, there is still a moderate possibility that the results were due to chance (7.9 percent, where results between 5 and 10 percent are conventionally interpreted as providing "some, but not definitive evidence").

In sum, these results provide preliminary yet encouraging evidence that the high-rate letters increase requests for OSC consultations among the small and medium firms targeted by the high-rate letter. However, a replication would be needed to draw more definitive conclusions.

Early in this report, we suggested several hypotheses about how the high-rate letter might operate. In particular, it is possible that the letters could spur an increase in OSC requests for the following three reasons: (1) increased cognizance of safety issues on the part of employers; (2) fear of inspections and penalties; and/or (3) employers wishing to delay an inspection. This study was not designed—and does not have sufficient statistical power—to distinguish between these mechanisms. However, the findings in this report preliminarily suggest that it may be increased cognizance of safety hazards, rather than the threat of an inspection, that leads small employers to schedule consultations after receiving the letter. This conclusion is based on the evidence that impacts are larger for previously uninspected establishments than for recently inspected establishments. Recently inspected establishments should be more cognizant of safety hazards than establishments that had not been inspected in the previous five years. The lack of evidence for a larger short- than long-term impact helps to preliminarily rule out the remaining two mechanisms, both of which would suggest a larger short-term impact. A study could be designed to test this intriguing hypothesis, by comparing (for example) the impact of letters that purely convey information with letters that convey an inspection threat.

Finally, our findings suggest that a proposed regulatory change requiring firms over a certain size to electronically submit their form 300 data each year would impose only a modest additional burden on OSC resources in the first year following adoption. If the electronic reporting rule applied to all establishments currently in the ODI sampling frame, we estimate that approximately 2,720 additional consultations would be requested in the first year following adoption of the regulation. This would be an approximately 9 percent increase in the number of annual consultations currently provided by OSHA. For reasons explained earlier, we caution that this impact is imprecise and the true impact could range anywhere from a slight decline (102 fewer requests) to a relatively large increase (5,440 additional requests).

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Appendix A: High-Rate Letter

Date
Name of the Employer
Address

Dear_____:

Last year, the Occupational Safety and Health Administration (OSHA) surveyed employers to collect workplace injury and illness data. The Agency used these data to identify the approximately 14,600 workplaces with the highest Days Away from work, Restricted, or Transferred (DART) rates; your workplace was one of those identified. This means workers in your establishment are being injured at a higher rate than in most other businesses in the country.

I am writing you to indicate my concern about the high DART rate at your establishment and to identify ways that you can obtain assistance in addressing hazards in your workplace. OSHA recognizes that your elevated DART rate does not necessarily indicate a lack of interest in safety and health. Whatever the cause, a high rate is costly to your company in both personal and financial terms. In addition, you should be aware that OSHA may target up to 4,500 general industry workplaces identified in the survey for inspection in the next year.

Over the years OSHA has found that many employers lack expertise in the field of workplace safety and health and welcome assistance by experts in this field. You may wish to consider hiring an outside safety and health consultant, talking with your insurance carrier, or contacting your state's workers' compensation agency for advice. Your workers can help identify hazards and find solutions. In addition, if you have a union at your site, please discuss with them how to reduce hazards in your workplace.

An excellent way for employers with 250 or fewer workers to address safety and health in their workplaces is to ask for assistance from OSHA's consultation program. This program is administered by a state agency and operated separately from OSHA's enforcement program. The service is free and confidential, and there are no fines even if problems are found. Designed for small employers, the consultation program can help you identify hazards in your workplace and find effective and economical solutions for eliminating or controlling them. In addition, the OSHA state consultant can assist you in developing and implementing a safety and health management system for your workplace.

In your state, the OSHA consultation program may be contacted at:

Name, Project Manager
Name of Organization
Address - first line
Address - second line
Telephone number

I encourage you to consider these suggestions as well as visit OSHA's home page at www.osha.gov for information to help you ensure safe and healthful working conditions in your establishment. As it was last year, a list of all the employers receiving this letter will be available from the OSHA home page.

Sincerely,

David Michaels, PhD, MPH
Assistant Secretary

Appendix B: SST Impact Study Sampling Details

Sample selection was conducted by OSHA staff before Abt's involvement in the SST Impact Study. The following text and diagrams describing sample selection are copied unchanged from IMPAQ International's 2013 OSHA SST Evaluation white paper (pages 3–7).

> Exhibit 2 presents the selection process used in SST11 to determine the sites to be targeted. As the first step in the process, OSHA obtained data from Dun & Bradstreet (D&B)[1] in 2009 that provided a list of sites that satisfied the following criteria: 1) they were not in the construction sector; 2) they were in high-hazard industries, as defined by the injury/illness data collected by the Bureau of Labor Statistics (BLS);[2] 3) they had at least 20 employees; and 4) they were not in the six states that opted to not participate in the ODI survey. This process resulted in an initial list of about 170,000 employers in 526 high-hazard 4-digit SIC industries. For the SST11 program, OSHA divided the list of targeted industries into three groups: manufacturing, nursing and personal care facilities, and all other industries.[3]
>
> The next step in the process was to identify the sample of employers from this list that would be required to respond to the 2010 ODI survey. OSHA used the following criteria to generate a 2010 ODI survey sample of about 80,000 sites:
>
> - Sites that responded to the 2009 ODI survey and were among the sites with the highest reported injury/illness rates in that survey were automatically added to the 2010 ODI sample (about 15,000 sites).
> - From the remaining sites on the initial list of 170,000, OSHA selected roughly 65,000 sites from certain industries for inclusion in the 2010 ODI sample.[4]
>
> In June of 2010, all sites in the 2010 ODI sample were mailed paper copies of the survey instrument (OSHA form 196B) to collect information for calendar year 2009. This instrument asked sites to provide the following information: 1) number of employees, 2) total number of hours worked, 3) number of workplace injury and illness incidents, 4) whether such incidents resulted in workers experiencing time away from work, restricted work activity, or job transfers,

---

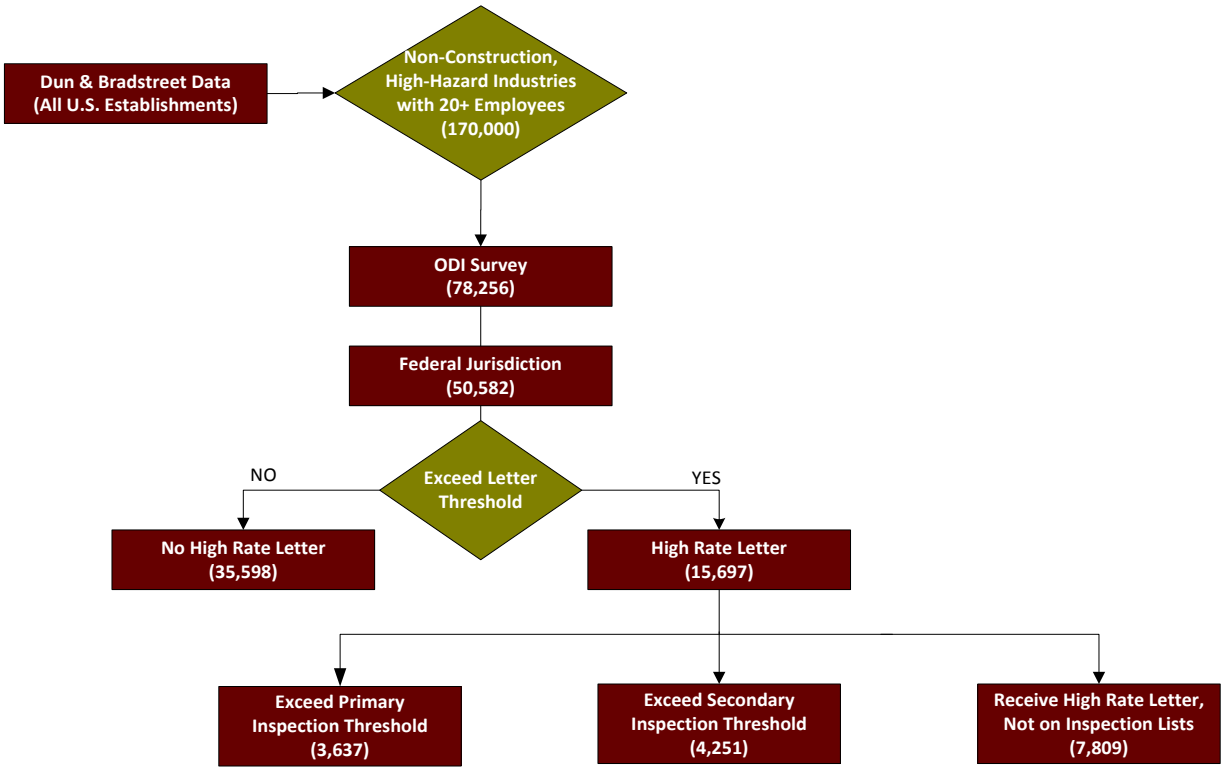
1 These data collect information on more than 99 percent of the entire universe of U.S. businesses.

2 BLS' *Injuries, Illnesses, and Fatalities Program* provides annual data on workplace injuries, illnesses, and fatalities by incident, industry, geography, and other characteristics (for more information, see http://www.bls.gov/iif/home.htm). OSHA uses this information to identify industries with the highest overall workplace injuries and illnesses.

3 Manufacturing covers Standard Industrial Classification (SIC) codes 2000-3900; nursing and personal care facilities cover SIC codes 8051, 8052, and 8059; and the other industries category covers selected 4-digit SIC codes from multiple sectors, including agriculture, transportation, sanitary services, wholesale trade, and retail trade. For the full list of industries included in the ODI, see Appendix A of OSHA Directive 11-03, *Site-Specific Targeting 2011*.

4 OSHA divides the pool of 170,000 sites into three groups of industries, with each group roughly the same size. In a single year, the 65,000 sites added to the ODI are generally the universe of sites from one of the three groups.

5) number of days away from work, restricted work activity, or job transfers, and (6) other employer characteristics (industry, location, etc.). Sites were offered the option to complete the paper survey instrument or complete the ODI survey online. Completed surveys were due to OSHA in July 2010.



As shown in Exhibit 2, the 2010 ODI survey collected data from 78,256 sites, of which 50,582 were under Federal jurisdiction (i.e., non-State-plan States) and were thus included in SST11. The remaining 27,674 sites were drawn from the 16 State plan States and were not part of SST11. At the beginning of 2011, OSHA used the completed 2010 ODI surveys to calculate two site-specific measures of injury/illness rates for the sites covered by the SST program. The first measure is the DART rate, which captures the number of workplace injury/illness cases that resulted in employees experiencing days away from work, restricted work activity, or job transfers. The DART rate is produced using the following formula:

$$\text{DART} = (C/H) \cdot 200{,}000$$

In the DART formula, *C* is the number of injury/illness cases involving employees experiencing days away from work, restricted work activity, and job transfers; and *H* is the number of hours worked by all employees, including temporary workers and contractors. The ratio of cases per hour is multiplied by 200,000 to produce an assumed rate per 100 full-time equivalent employees. The second measure is the DAFWII rate (days away from work due to injury and illness), which has the same formula as the DART rate, except that the *C* in the DAFWII formula only includes cases involving days away from work (i.e., DAFWII excludes cases involving temporary transfers to another job and cases involving restricted work).

The next step in the selection process was to use the DART and DAFWII rates for the 50,582 SST-eligible sites to determine which sites would receive the program's interventions (high-rate letter and inspection). First, using the calculated DART rate, OSHA set a threshold that determined which sites would be mailed the high-rate letters. OSHA selected a DART threshold of 2.5, meaning that any site with a DART rate of 2.5 or greater was placed on the high-rate letter list. Using this threshold, 14,984 sites were scheduled to receive letters under SST11. Then, OSHA used the DART and DAFWII rates to categorize these sites into three lists:

- *Primary inspection list* – Includes the 3,637 sites with the highest DART and DAFWII rates among those that received the high-rate letter.[5] These sites were assigned the highest priority for receiving an inspection.
- *Secondary inspection list* – Includes the 4,251 sites with DART and DAFWII rates that were lower than the rates of sites on the primary inspection list, but still above the high-rate letter threshold.6 These sites were to be inspected only after all primary inspection list sites in an Area Office's jurisdiction were inspected.
- *High-rate letter only list* – Includes the 7,809 sites with the lowest injury/illness rates among those receiving the letter. These sites would not receive an SST inspection.

[5] Due to industry differences in injury/illness rates, OSHA uses separate thresholds for inclusion in the primary inspection list: 1) manufacturing: DART≥7 or DAFWII≥5; 2) nursing and personal care: DART≥16 or DAFWII≥13; and 3) other sectors: DART≥15 or DAFWII≥14.

[6] The thresholds for inclusion in the secondary inspection list are 1) manufacturing: 7>DART≥5 or 5>DAFWII≥4; 2) nursing and personal care facilities: 16>DART≥13 or 13>DAFWII≥11; and 3) other industries: 15>DART≥5 or 14>DAFWII≥4.

Appendix C: SST Impact Study Random Assignment Details

OSHA staff report that they performed random assignment for the SST study following the procedures outlined in the document "Sample Design for a Statistically Valid Evaluation of Accuracy and Completeness of an Establishment's OSHA Mandated Employer Records" (*NORC 4699*). They described the specific steps they took to conduct random assignment as follows:

First, divide the sampling frame into primary list and secondary list components. Then, perform the following steps separately for each of the two lists:

1. Sort the sample by two-digit SIC code
2. Within each two-digit SIC code, sort by region number
    a. Sort by ascending region order within odd-numbered SIC codes
    b. Sort by descending region order within even-numbered SIC codes
3. Within each (two-digit SIC code * Region) block, sort by number of employees
    a. Sort by ascending number of employees within blocks where (SIC code + region) is odd numbered
    b. Sort by descending number of employees within blocks where (SIC code + region) is even numbered
4. Choose a random starting point *S* somewhere on the list
5. Select the number of establishments that should be in each study arm (e.g., *n*=420 establishments in "Treatment A," *n*=420 in "Treatment B," and *n*=420 in "Control"), and calculate the appropriate skip interval
6. Starting with *C*=*S*, select every k^{th} establishment *C* (i.e. *C* = *C*+*k*) into the sample until the sample is of the desired size *n* (cycling back to an earlier position *C*-*n* on the list when C exceeds the list size). In the example just above, *n*=420 for Treatment A
7. Using the same skip interval, repeat step 6 for the remaining study arms
8. Establishments remaining in the sample after all study arms have been selected are excluded from the STT Impact Study (but are included in the SST-HRL Study)

Our regression models include list type (primary/secondary), two-digit SIC code, OSHA region, and number of employees to account for this process.

Appendix D: Matching Algorithm, Weights, Probabilities, and Final T-Scores

The analysis faced a difficult matching challenge. We have the list of establishments who were randomly assigned to receive or not to receive the high-rate letter. We also have, from IMIS, the list of establishments that requested a consultation. However, the two datasets have no common identifier. In particular, establishment names and addresses on the request for a consultation will often not align exactly with how that information is recorded in the random assignment database, which was itself drawn from Dun & Bradstreet information.

To address this lack of a common identifier, we adopted a probabilistic matching approach. Unlike a deterministic match, a probabilistic match does not require exact correspondence across multiple fields. Instead, a probabilistic match considers the information in each pair of records (one from each of the datasets) and determines the probability of these two records being the same establishment based on similarities across multiple fields (e.g., name, address, phone number, and other factors). Such a program requires extensive calibration using a training file, or set of known matches, to assign weights to each of the matching criteria.

The matching algorithm adopted for this study is a modified version of an algorithm developed by Wayne Gray (1996) for a study that required matching IMIS data with BLS data. This algorithm assesses agreement or disagreement for each of several pairwise comparisons of variables, ultimately calculating a value for each potential match called a T-score. The T-score, which is the sum of weights assigned to agreement/disagreement for each comparison, is proportional to the probability that a potential match is a true match. The Gray algorithm was implemented as a SAS program. We modified this existing program to use OSHA Data Initiative (ODI)/Dun & Bradstreet rather than BLS data, as well as to take advantage of newer technologies, such as geographic information software (GIS) and improved computing power since the program's initial development in 1997.

Preparation of Datasets

The matching process merges the following two datasets:

1. The sample of establishments selected by OSHA for Site-Specific Targeting in 2011 and subsequently randomly assigned for the IMPAQ/Summit SST Impact Study. The SST Impact Study sample file includes data from several sources including responses to the ODI survey, historical data on inspections from IMIS, and establishment-level identifying information from Dun & Bradstreet (e.g., standardized address and contact and industry information for each establishment). This data file was provided to us by IMPAQ International.

2. The list of requests made for OSC services from April 2006 through April 2012 as recorded in OSHA's Integrated Management Information System (IMIS) database. We obtained OSC requests directly from IMIS.

The SST Impact Study sample file is an establishment-level data file (i.e., includes one record per establishment), while the IMIS data file is a request-level dataset (i.e., includes one record per OSC request). Establishments in the SST Impact Study sample may appear multiple times in the IMIS data file.

The SAS matching program developed by Gray (1996) includes procedures for cleaning and standardizing data files to ensure consistency across the two datasets. This converts all establishment names and addresses to upper case. It also removes hyphens and other characters that could be ambiguous. In addition, the procedure extracts street number, PO Box number, and route or highway number from long-form addresses and places them into separate data fields. We modified this program to clean SST Impact Study sample data, which has a different format than the BLS data read in by the original program.

Developing a Training File

Before being implemented with the entire sample, the matching program must be calibrated using a subsample of the data in which the incidence of "true matches" (i.e., records that could reliably be assumed to be referring to the same establishment) and "true non-matches" (i.e., records that could be reliably said to be referring to different establishments) is known. We refer to this subsample as the "training file."

We created the training file using all data (IMIS records and SST Impact Study sample establishments) from three states (Illinois, South Dakota, and Massachusetts). Specifically, within these states we extensively hand-matched and hand-verified potential matches identified by a naively calibrated version of the matching algorithm. We selected the three states for this exercise by considering several factors including size, location, industry mix, and urban composition. Our goal was to have a training file that roughly resembles the overall sample to be matched in terms of industry mix, with a geographic mix representing more than one region of the country. Given these considerations, we chose states according to size, such that the number of potential matches could be hand-verified within our budget and time constraints.

Developing the training file then proceeded as follows. First, we implemented our modified matching program using the final set of weights calibrated for Gray's (1996) analysis (i.e., the weights used to match IMIS and BLS data). Using these weights (with new variables such as GIS set to receive zero weight), the program generated a naïve T-score for each pair of potential matches.[1] At this stage in the process the program was not expected to determine exact matches but rather to cast a wide net for possible matches. Those possible matches would then be reviewed by hand. The program compared every OSC request within each state to every HRL establishment in that state. Across the three states, there were over six million pairs of potential matches. We identified potential matches as those with a T-score value above five, which was lower (i.e., included more potential matches) than the T-score threshold used to identify matches in Gray (1996).[2]

For these three states, this approach required hand-review of 8,801 potential matches. Research assistants were trained to identify true matches and rule out non-matches using establishment identifying information supplemented by further research when required. Research staff were encouraged to use Google Maps to identify business locations as well as to search Dun & Bradstreet's Hoover's database to determine more details about potential matches. For consistency, each potential match was reviewed by at least two research assistants, who were instructed to use a set of common rules when possible to

[1] Gray (1996) and Fellegi and Sunter (1969) explain the T-score in detail.

[2] T-scores in this initial step ranged from -15 to 61. As it turns out, hand-verification identified no true matches with a T-score lower than 9.

determine match veracity. For example, all reviewers were told to consider a pair a match if the two establishments had identical names as well as street addresses that were only off by one digit. Because each record was reviewed by multiple reviewers, the hand-coded files were merged and any case with disagreement between reviewers was discussed until a consensus was reached. Reconciling discrepancies between reviewers resulted in a final list of true matches and non-matches within the subsample of three states.

Program Calibration

The calibration process is described in detail in Gray (1996) and Fellegi and Sunter (1969). This section provides a brief overview of the calibration process as it applied to this study. Basically, the training file enables calibration of the program by providing a reference from which to determine the relative importance of each of the matching criteria. For example, consider two potential matched pairs, one of which agrees on the first four digits of the street name and the other of which does not agree on this field. The training file can be used to determine whether one of these pairs is more likely to be a true match than the other, and thus whether a positive or negative weight should be placed on four-digit street-name agreement when calculating the match probability score (or T-score). The importance or weight of each criterion was calculated using the formula given in Gray (1996), which Felligi and Sunter (1969) show leads to an optimal decision rule (i.e., one that leads to the desired probabilities of Type I and Type II errors). We briefly describe the formula and decision rule here.

Within the training file, records can be divided into the datasets M (true matches) and U (unmatched, or all true non-matches). For each matching criterion *G*, two probabilities can be calculated; m(*g*), the probability that a pair matches on criterion *G* given that it is included in dataset M; and u(*g*), the probability that a pair matches on criterion *G* given its inclusion in dataset M. From these two values the weight is calculated as w(*g*)=log(m(*g*)/u(*g*)). This weight is rounded to the nearest integer and summed with the weights of other criteria to determine a final value, or T-score, which is proportional to the match probability for each pair. The final step of the program involved using the training file to calculate a pair of T-score cutoff values above and below which there would be no hand-reviews. The low T-score cutoff (i.e., the threshold below which no matches would be accepted and no hand matching would occur) was set to equal the highest T-score value below which there were no true matches identified in the training file. This strategy proved infeasible for setting the high T-score cutoff (i.e., the threshold above which all matches would be accepted and no hand matching would occur) because the distribution of T-scores for true non-matches exhibits a rather long right tail. We instead set this threshold at a value such that, if only true matches were accepted in the hand-verification range, more than 99 percent of matches in the final list of identified matches (including all those with T-scores above the threshold) would be true matches. All records with a T-score between the low and high T-score cutoffs were later reviewed by hand because the training file showed that both true matches and non-matches existed within this range.

Table D.1 shows the rounded weights for each of the criteria from Gray (1996) and from our analysis. Several criteria from the original program were omitted in the current version, and we incorporated several new factors. For example, Gray (1996) compared four-digit SIC codes which were then the standard for classifying industries. Because SIC codes have largely been replaced by six-digit NAICS codes in the last decade, the new program uses NAICS codes in place of SIC codes. Also, the availability of GIS allowed us to incorporate geographic latitude and longitude coordinates for each address in the match criteria. This could be helpful in identifying matches for an establishment that is located at an intersection, and uses a different street address in each of our datasets. County names were not included in

the 2014 datasets and therefore were not used in the revised program. However, the benefits of both county and ZIP code data are largely captured by the new GIS field. Finally, we omitted the field corresponding to "more than 500 employees" because the revised version of the matching algorithm will be implemented for Abt's pilot study of OSC marketing, which includes only small employers.

Exhibit D.1: T Scores for Variables

Field	Criteria	Weight from Wayne Gray	Weight from SST Study
Name	Agree on all letters	+8	+10
	Agree on first 6 letters	+6	+7
	Agree on 6 letters elsewhere	+4	-2
	Disagree	-2	-3
Street	Agree on all letters	+7	+30
	Agree on first 4 letters	+4	+5
	Agree on 4 letters elsewhere	+2	N/A
	Disagree	-3	-3
Street number	Agree	+5	+6
	Disagree	-3	-3
Box number	Agree	+4	+7
	Disagree	-2	-0
Route	Agree	+3	+8
	Disagree	-2	N/A
County	Agree	+2	N/A
	Disagree	-3	N/A
ZIP code	Agree exactly	+4	+6
	Agree within 100	+1	N/A
	Disagree	-3	-4
SIC	Agree at 4-digit level	+5	N/A
	Agree at 3-digit level	+4+	N/A
	Agree at 2-digit level	3	N/A
	Disagree	-2	N/A
NAICS	Agree at 6-digit level	N/A	+4
	Agree at 4-digit level	N/A	+3
	Agree at 2-digit level	N/A	+1
	Disagree	N/A	-2
Employment	Over 500 workers and agrees within 10%	+2	N/A
Phone number	Exact match	N/A	+7
GIS match	6-digit coordinate match	N/A	+11
	4-digit coordinate match	N/A	+6

Final Modifications and Matching Process

We revised Gray's (1996) SAS code to incorporate the newly calibrated weights and to incorporate two changes due to improved technology. First, due to limitations of computing power in 1996, the original program included a blocking step that restricted the program to only comparing those records that matched on certain criteria (such as a two-digit SIC code or part of a ZIP code). We removed this step without consequence in terms of computational efficiency.

Second, we updated the program to account for additional address information in the HRL file. The SST dataset includes two addresses—a mailing address and a physical address for the establishment. The final version of the program attempts to match on both these addresses and only penalizes a match if neither of the addresses corresponds across the two datasets.

We implemented this revised program with the full sample of SST and IMIS files (i.e., all states, not merely the three used in hand matching). The thresholds were selected based on pooled data from the three hand-matched states such that 99 percent of true matches in the training file were identified as possible or definite matches, and likewise less than 1 percent of pairs classified as definite matches would be considered erroneous.[3] Specifically, the revised program assigned a T-score to each potential match, and this score was used to classify each pair as: (1) a definite match, (2) a definite non-match, or (3) a possible match. Pairs classified as possible matches (i.e., group 3) were hand-reviewed by the same team that had previously hand-checked the sample for the calibration procedure. That check classified each possible match as either a definite match or definite non-match.

Finally, we generated a final establishment-level dataset from the request-level dataset consisting of all definite matches—after hand-matching of possible matches.

[3] We set a final T-score threshold of 9 for a possible match and 25 for a definite match.

Appendix E: Impact Estimates for Large Establishments and All Establishments

The body of this report focuses almost exclusively on the sample of small and medium-sized establishments, which are eligible for OSC services and which were called out in the high-rate letter. This appendix presents results for larger establishments.

Exhibit E.1 presents descriptive characteristics for large establishments, i.e., those with 250 or more employees. The high-rate letter did not recommend that large establishments contact the OSC program, and such establishments comprise a relatively small portion of the overall sample (603 of 7,888 establishments, or 8 percent of the sample).

Exhibit E.1: Selected Baseline Descriptive Characteristics of Establishments in the Study Sample, for Large Establishments (≥250 Employees)

Characteristic	Treatment Group Mean	Control Group Mean	Difference	P-value of Difference
Primary list	31.3%	39.7%	-8.4%	0.1506
Number of employees	716.95	600.48	116.47	0.4195
Consultation Request History				
Made an OSC request in past 5 years	14.9%	17.8%	-2.9%	0.5187
Number of prior OSC requests	0.42	0.53	-0.11	0.4973
Inspection History:				
Received an inspection in past 5 years	48.9%	35.6%	13.3%**	0.0335
Number of prior inspections	1.03	0.72	0.31	0.1675
Industry				
Amputation NEP	22.3%	23.3%	-1.0%	0.8444
Nursing and personal care facilities	5.7%	4.1%	1.6%	0.5857
High-hazard manufacturing	17.5%	2.6%	-8.5%*	0.0809
Injury/Illness Rates				
DART	8.816	8.877	-0.061	0.9035
DAFWII	4.23	4.066	0.164	0.7365

Note: The first two columns report sample means (XX,XXX) or sample proportions (T.TT/C.CC). The Difference column reports the difference between the means or proportions (calculated as Treatment–Control). The p-value for this difference is given in the final column; significance reported as *** =1 percent, ** =5 percent, * =10 percent.

In comparison with smaller establishments, large employers were somewhat less likely to have requested a consultation in the previous five years, and somewhat more likely to have received an inspection. Their injury/illness rates as measured by DART and DAFWII are slightly lower. However, given the small size of the subsample, it is possible that any or all of these differences could be due to chance.

Turning to impact, Exhibits E.2-E.4 present impact results for large establishments. Exhibits E.5-E.7 provide impact results for the pooled sample of all establishments (i.e., both large and small-to-medium sized establishments).

Exhibit E.2: Impact of High-Rate Letter on One-Year Consultation Request Rate (Large Establishments)

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	603	6.21	5.56	-0.65	3.29	0.8424	-10.5%
SST List	Test for homogeneity across subgroups: p=0.326						
Primary list	195	4.38	7.67	3.29	4.91	0.5026	75.3%
Secondary list	408	7.86	4.54	-3.32	4.46	0.4572	-42.2%
Inspection History	Test for homogeneity across subgroups: p=0.262						
Received inspection	285	2.41	6.32	3.92	4.63	0.3978	162.8%
No inspection	318	8.22	4.85	-3.37	4.49	0.4524	-41.0%
Establishment Group	Test for homogeneity across subgroups: p=0.441						
Amputation NEP	135	4.00	4.51	0.50	7.89	0.9492	12.6%
Nursing homes	33	37.45	9.59	-27.9	21.90	0.2045	-74.4%
High-hazard industries	112	9.99	10.86	0.87	6.86	0.8991	8.7%

Two-sided test: *p<0.1, **p<0.05, ***p<0.01.

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

Exhibit E.3: Impact of High-Rate Letter on Short-Term Consultation Request Rate (Large Establishments)

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	603	0.77	2.91	2.14	1.57	0.1741	276.7%
SST List	Test for homogeneity across subgroups: p=0.727						
Primary list	195	2.94	4.30	1.36	3.36	0.6856	46.2%
Secondary list	408	-0.42	2.25	2.66*	1.47	0.0696	-640.0%
Inspection History	Test for homogeneity across subgroups: p=0.841						
Received inspection	285	0.52	3.04	2.51	1.85	0.1752	480.8%
No inspection	318	0.88	2.80	1.92	2.28	0.4011	217.2%
Establishment Group	Test for homogeneity across subgroups: p=0.706						
Amputation NEP	135	-0.87	1.82	2.69	2.89	0.3525	-309.6%
Nursing homes	33	4.19	6.25	2.06	5.02	0.6823	49.0%
High-hazard industries	112	6.96	5.03	-1.93	4.80	0.6875	-27.8%

Two-sided test: *p<0.1, **p<0.05, ***p<0.01.

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

Exhibit E.4: Impact of High-Rate Letter on Long-Term Consultation Request Rate (Large Establishments)

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	603	6.58	4.00	-2.58	3.25	0.4277	-39.2%
SST List	Test for homogeneity across subgroups: p=0.366						
Primary list	195	4.35	5.26	0.92	4.65	0.8434	21.1%
Secondary list	408	8.32	3.39	-4.93	4.45	0.2677	-59.3%
Inspection History	Test for homogeneity across subgroups: p=0.157						
Received inspection	285	1.79	4.84	3.05	4.50	0.4988	170.1%
No inspection	318	9.14	3.21	-5.92	4.42	0.1805	-64.8%
Establishment Group	Test for homogeneity across subgroups: p=0.553						
Amputation NEP	135	4.62	3.57	-1.04	7.25	0.8855	-22.6%
Nursing homes	33	35.29	6.47	-28.8	24.44	0.2393	-81.7%
High-hazard industries	112	9.55	6.65	-2.90	6.54	0.6573	-30.4%

Two-sided test: *p<0.1, **p<0.05, ***p<0.01.

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

Exhibit E.5: Impact of High-Rate Letter on One-Year Consultation Request Rate (Pooled Sample)

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	7,888	8.67	10.25	1.58	0.99	0.1101	18.2%
SST List	Test for homogeneity across subgroups: p=0.136						
Primary list	3,637	10.43	13.50	3.07**	1.53	0.0445	29.4%
Secondary list	4,251	7.36	7.47	0.11	1.26	0.9316	1.5%
Inspection History	Test for homogeneity across subgroups: p=0.169						
Received inspection	2,682	11.77	11.33	-0.44	1.90	0.8158	-3.8%
No inspection	5,206	7.08	9.69	2.61**	1.14	0.0218	36.8%
Establishment Group	Test for homogeneity across subgroups: p=0.968						
Amputation NEP	1,719	12.39	15.21	2.82	2.39	0.2389	22.8%
Nursing homes	589	7.31	11.12	3.82	3.16	0.2276	52.2%
High-hazard industries	1,601	9.94	12.99	3.04	2.55	0.2320	30.6%

Two-sided test: *p<0.1, **p<0.05, ***p<0.01.

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

Exhibit E.6: Impact of High-Rate Letter on Short-Term Consultation Request Rate (Pooled Sample)

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	7,888	4.33	5.22	0.89	0.76	0.2380	20.6
SST List	Test for homogeneity across subgroups: p=0.384						
Primary list	3,637	5.28	6.83	1.55	1.18	0.1893	29.5
Secondary list	4,251	3.60	3.83	0.24	0.94	0.8031	6.5
Inspection History	Test for homogeneity across subgroups: p=0.635						
Received inspection	2,682	5.12	5.48	0.36	1.42	0.7983	7.1
No inspection	5,206	3.92	5.08	1.16	0.89	0.1914	29.6
Establishment Group	Test for homogeneity across subgroups: p=0.389						
Amputation NEP	1,719	6.72	7.08	0.36	1.91	0.8513	5.3
Nursing homes	589	5.01	7.40	2.39	2.79	0.3918	47.7
High-hazard industries	1,601	2.89	6.69	3.81**	1.63	0.0199	131.8

Two-sided test: *p<0.1, **p<0.05, ***p<0.01

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

Exhibit E.7: Impact of High-Rate Letter on Long-Term Consultation Request Rate (Pooled Sample)

Establishment Group	Sample Size	Control Group Request Rate	Treatment Group Request Rate	Impact on OSC Request Rate (T/C Difference)	SE	p-Value	% Change
Overall	7,888	5.36	6.48	1.12	0.78	0.1474	21.0
SST List	Test for homogeneity across subgroups: p=0.236						
Primary list	3,637	6.63	8.68	2.05*	1.21	0.0896	31.0
Secondary list	4,251	4.40	4.61	0.20	0.98	0.8354	4.6
Inspection History	Test for homogeneity across subgroups: p=0.077*						
Received inspection	2,682	8.29	7.32	-0.97	1.57	0.5375	-11.7
No inspection	5,206	3.87	6.05	2.18**	0.85	0.0104	56.4
Establishment Group	Test for homogeneity across subgroups: p=0.578						
Amputation NEP	1,719	7.98	10.13	2.15	1.90	0.2584	27.0
Nursing homes	589	2.50	5.80	3.30*	1.85	0.0749	131.7
High-hazard industries	1,601	7.89	8.15	0.26	2.22	0.9068	3.3

Two-sided test: *p<0.1, **p<0.05, ***p<0.01

Due to rounding, reported impacts (T-C differences) may differ from differences between reported regression-adjusted means for the treatment and comparison groups.

Appendix F: Regression Coefficients

Exhibit F.1 reports regression parameters (coefficient estimates and standard errors) and R-squares for the OLS regression for each of the three outcomes for small and medium-sized establishments. Each column corresponds to a single outcome. The main impacts from these linear regressions were used to generate the impact estimates in Exhibits 4.2, 4.4, and 4.6 in Chapter 4.

Exhibit F.1: Regression Coefficients (Small and Medium-Sized Establishments)

	Any Request	Short Term	Long Term
Letter	0.019* (0.011)	0.007 (0.008)	0.015 (0.009)
Primary group	0.013 (0.010)	0.007 (0.007)	0.012 (0.008)
Number of employees (1,000)	-0.244*** (0.073)	-0.065 (0.057)	-0.166*** (0.060)
OSHA Region 1	-0.042 (0.033)	-0.035 (0.025)	0.015 (0.027)
OSHA Region 2	-0.008 (0.033)	-0.030 (0.025)	0.051* (0.027)
OSHA Region 3	-0.064** (0.032)	-0.049* (0.025)	-0.009 (0.027)
OSHA Region 4	-0.034 (0.032)	-0.043* (0.025)	0.029 (0.026)
OSHA Region 5	-0.081** (0.032)	-0.054** (0.025)	-0.008 (0.026)
OSHA Region 6	-0.033 (0.032)	-0.041 (0.025)	0.0212 (0.026)
OSHA Region 7	-0.065* (0.034)	-0.054** (0.026)	-0.001 (0.028)
OSHA Region 8	-0.145*** (0.035)	-0.094*** (0.027)	-0.047* (0.028)
Requested OSC before letter	0.055*** (0.004)	0.021*** (0.003)	0.048*** (0.003)
Number of OSC requests before letter	0.085*** (0.013)	0.047*** (0.010)	0.0433*** (0.011)
DART rate (divided by 1000)	-0.156 (0.795)	-0.436 (0.614)	0.199 (0.649)
Previous inspection	-0.023* (0.012)	-0.013 (0.009)	-0.013 (0.010)
Number of previous inspections	0.009 (0.006)	0.003 (0.005)	0.003 (0.005)
Intercept	0.022 (0.286)	0.038 (0.221)	-0.033 (0.233)
R-squared	0.147	0.057	0.129

*.05 p < .10, **.01 p < .05, ***p <.01.

Note: The regression models also contained covariates to account for difference in 2-digit SIC code to control for industrial classification. The regression estimates for these 55 dummy variables are omitted from the table.

Exhibit F.2: Regression Coefficients (Large Establishments)

	Any Request	Short Term	Long Term
Letter	0.019* (0.011)	0.007 (0.008)	0.015 (0.009)
Primary group	0.013 (0.010)	0.007 (0.007)	0.012 (0.008)
Number of employees (1,000)	-0.244*** (0.073)	-0.065 (0.057)	-0.166*** (0.060)
OSHA Region 1	-0.042 (0.033)	-0.035 (0.025)	0.015 (0.027)
OSHA Region 2	-0.008 (0.033)	-0.030 (0.025)	0.051* (0.027)
OSHA Region 3	-0.064** (0.032)	-0.049* (0.025)	-0.009 (0.027)
OSHA Region 4	-0.034 (0.032)	-0.043* (0.025)	0.029 (0.026)
OSHA Region 5	-0.081** (0.032)	-0.054** (0.025)	-0.008 (0.026)
OSHA Region 6	-0.033 (0.032)	-0.041 (0.025)	0.0212 (0.026)
OSHA Region 7	-0.065* (0.034)	-0.054** (0.026)	-0.001 (0.028)
OSHA Region 8	-0.145*** (0.035)	-0.094*** (0.027)	-0.047* (0.028)
Requested OSC before letter	0.055*** (0.004)	0.021*** (0.003)	0.048*** (0.003)
Number of OSC requests before letter	0.085*** (0.013)	0.047*** (0.010)	0.0433*** (0.011)
DART rate (divided by 1000)	-0.156 (0.795)	-0.436 (0.614)	0.199 (0.649)
Previous inspection	-0.023* (0.012)	-0.013 (0.009)	-0.013 (0.010)
Number of previous inspections	0.009 (0.006)	0.003 (0.005)	0.003 (0.005)
Intercept	0.022 (0.286)	0.038 (0.221)	-0.033 (0.233)
R-squared	0.147	0.057	0.129

*.05 p < .10, **.01 p < .05, ***p <.01.

Note: The regression models also contained covariates to account for difference in 2-digit SIC code to control for industrial classification. The regression estimates for these 55 dummy variables are omitted from the table.

Exhibit F.3: Regression Coefficients (Pooled Sample)

	Any Request	Short Term	Long Term
Letter	-0.007 (0.026)	0.021 (0.020)	-0.026 (0.024)
Primary group	-0.026 (0.026)	0.006 (0.019)	-0.023 (0.023)
Number of employees (1,000)	-0.005 (0.008)	-0.002 (0.006)	-0.003 (0.007)
OSHA Region 1	-0.118 (0.105)	-0.192** (0.080)	0.104 (0.095)
OSHA Region 2	-0.140 (0.105)	-0.200 (0.079)	0.077 (0.094)
OSHA Region 3	-0.146 (0.105)	-0.207*** (0.079)	0.092 (0.094)
OSHA Region 4	-0.168 (0.105)	-0.222*** (0.079)	0.080 (0.095)
OSHA Region 5	-0.165 (0.103)	-0.224*** (0.078)	0.075 (0.093)
OSHA Region 6	-0.079 (0.104)	-0.193 ** (0.078)	0.137 (0.093)
OSHA Region 7	-0.183 (0.106)	-0.226*** (0.080)	0.056 (0.096)
OSHA Region 8	-0.116 (0.112)	-0.214** (0.085)	0.117 (0.101)
Requested OSC before letter	0.049*** (0.010)	0.0209*** (0.008)	0.050*** (0.009)
Number of OSC requests before letter	0.140*** (0.038)	0.0472 (0.029)	0.104*** (0.035)
DART rate (divided by 1000)	-1.569 (2.854)	-1.216 (2.151)	-1.144 (2.568)
Previous inspection	0.009 (0.021)	0.008 (0.016)	0.004 (0.019)
Number of previous inspections	-0.003 (0.006)	-0.004 (0.004)	0.001 (0.005)
Intercept	0.183 (0.180)	0.214 (0.136)	-0.047 (0.162)
R-squared	0.328	0.213	0.298

*.05 p < .10, **.01 p < .05, ***p <.01.

Note: The regression models also contained covariates to account for difference in 2-digit SIC code to control for industrial classification. The regression estimates for these 55 dummy variables are omitted from the table.