The Barriers to Workers' Safety in SMEs: Lessons Learned from a Set of RCTs *

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October 31, 2016

Abstract

We evaluate the impact of providing different types of information to different agents within SMEs, using in a field experiment with four different interventions. In three cases, emails were sent to the manager providing either a summarized safety plan, a comparison of accident rates to firms in one's sector or a monetization of the costs of accidents. In the fourth case, printed pamphlets were sent to workers (through the firm). We find that only this last intervention significantly lowered the accident rates of firms for as long as 12 months. The email comparing the firm's accident rate to the sector average also reduced accidents but only for firms that were informed their accident rate was above the industry average. We find evidence that these two interventions raised the rate of safety training but that this is unlikely to be able to explain all of the decrease in accident rates. Finally, the workers' package also moved firms' wage distributions to the left, suggesting that managers were able to pass onto workers some of the safety improvement costs. All these results suggest that there are some asymmetric information problems within firms that limit safety improvements in small firms and that can be solved through providing specific information to specific agents.

*We thank FUCYT for financing the study through the grant "Evaluación de impacto de las actividades de prevención ejecutadas por la ACHS - PO128". We thank seminar participants at Universidad Diego Portales and Universidad Adolfo Ibáñez for comments. All remaining errors are our own.

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1 Introduction

Worker safety might be perceived as a low priority for the owners or managers of small and medium enterprises (SMEs). The fact that SMEs have significantly higher accident rates than comparable larger firms has been well documented (Sorensen et al, 2007). This "trailing" behavior of SMEs is also manifested in other domains, such as formality of employment, provision of training (OECD, 2013), or adoption of management practices (Bloom et al., 2013). It is not surprising then that policy making and research has increasingly addressed the problems of SMEs. However, the small size of these firms creates a big problem for policy design: coverage typically has to be traded-off with depth of interventions given the large number of small firms. While regulations specific to SME may be useful, they only solve the problem partially, particularly so in the context of developing countries where legislation and enforcement are usually weaker. In this paper we propose and show that increasing the awareness and knowledge of firms may be a more efficient route to follow. We study the impact of four distinct information campaigns for SMEs conducted by one of the non-profit organizations in charge of the workers' compensation program in Chile, using a randomized control trial methodology. We try to differentiate between reaching managers and workers directly to test who is the most efficient agent of change to increase firm safety in small and medium-sized firms in this context. We also differentiate the specific information we provide to managers in order to separate different barriers to decision making.

The context we use is interesting for many reasons. Chile has a relatively sophisticated system of workers' safety where firms are mandated to contribute to an insurance policy for each of their workers and the policy then provides pension benefits (i.e., pays out to the worker in case of an accident), provides prevention services (e.g., training), and healthcare benefits (i.e., medical treatment in case of workplace accidents and illnesses). SMEs are subject to the same legislation as larger firms in this setting. We argue that the provision of workplace safety is riddled with asymmetric information problems between employees and firm owners (Pouliakas and Theodossiou, 2013). While workers typically have more information regarding optimal safety prevention, owners, partly because of the existence of an insurance policy and non-internalized hidden costs of safety, have limited incentives to pursue preventive measures. We argue that in this scenario the identity of the person receiving the safety information may matter.

For our randomized control trial, we worked with the "Asociación Chilena de Seguridad" (ACHS), which is the largest organization in Chile in terms of occupational safety and health (OSH) services, whose affiliates correspond to 40% of all Chilean workers who participate in this system. Each month, ACHS experts visit roughly 1/12 of its 31,500 SME clients, in the context of the "SME community" prevention program. In these visits, the ACHS collects information about the firm and then sends a report which includes an assessment and a suggested non-mandatory prevention plan that is designed to account for the specificities of the firm. We conducted a double-randomization of the firms that were visited in each month for a period of 6 consecutive months in 2013. First, firms were ei-

ther allocated to receive no email, or one of three different types of information provided to the safety monitor of the firm. Orthogonally, a fraction of firms were also selected to receive personalized safety information in 20 pamphlets to be distributed to the firm's workers. All the information provided to the firm stemmed from information collected by the ACHS in the year before the visit or information collected by the safety expert who visited the firm in the month before. We then follow the firms through administrative data for at least 12 months following the receipt of the information to evaluate its impact. The administrative data provide us with information on the number of accidents, the execution of safety training and subsequent evaluations of safety made by experts' visits in the following months.

Our experiment tries to address four different challenges which may impede firms from undertaking specific measures to reduce their levels of accidents, as has been emphasized in other literature. First, the report and the prevention plan that is handed to the company are relatively complex and hard to follow, namely a document with 20 or more pages written in a legalistic style. Although this document provides plenty of detail in terms of preventive steps, its structure and style makes it hard to understand, particularly for busy SMEs owners. Our first message thus simplifies this document into a smaller and briefer file, with information organized as a checklist-like report to facilitate implementation. This message is sent to the safety monitor of the firm, who is very often the manager of the firm.

A second barrier lies in the fact that the costs related to safety may not be salient enough to firms for them to pay attention. We thus provide a simple email to firms where we translate their own accidents record into monetary costs. We did so using the fact that their insurance rate is dependent on their past accident rates.

A third barrier may be due to the fact that firms may believe accidents are simply "normal" and that all firms in their industry must be facing similar safety problems. We try to address this by presenting each firm with a graph which contrasts their own safety record to that of the average firm in their sector (at the 2-digit ISIC level). This treatment attempted to tap into social comparison and reference points, particularly for those companies that lagged in their sector in terms of safety.

The fourth and final barrier we addressed is the fact that there may be some agency problem preventing information from getting to the individuals who can take most measures to diminish accidents: the workers themselves. We thus send to each firm printed material to be distributed to the workers of the firm.

Our randomization produced a balanced set of firms that received a treatment and those that did not. Information regarding take-up suggests, however, partial compliance since many emails and printed packages go unopened. We present an Intent-to-Treat estimate since it is not clear how we would exactly measure take-up for all treatments.

Our results suggest that only the last two interventions have the potential for altering the behavior

of the firm. The comparative information on accident rate treatment seems to decrease the accident rates of firms who were told their safety records were worse than the industry average. At the same time, providing information directly to the workers lowers accidents in a long-lasting future. We observe consistent decreases in accident rates 6 and 12 months after the information was provided. The magnitudes are not small. Accidents per 100 workers per year fall by about 0.005 to 0.01 in each of these measures from a base for the control of about 0.05, implying a change of almost 15 percent.[1] We find these results to be relatively robust to the introduction of controls and to differences in the specification.

We find evidence that the two successful interventions also increased the firms' use of training programs for their workers, suggesting that part of the decrease in the number of accidents may be linked to the increased knowledge of the firms. However, back-of-the-envelope calculations suggest that the magnitude (and timing) of the change in accident rate is unlikely to come simply from more training being provided to the workers of the firm. Furthermore, in the case of firms being shown their relative position compared to the industry average, we see a decrease in the accident rates only for those who were given "bad news" but an increase in class taking for all types of firms suggesting that either classes are not uniformly useful at decreasing accidents or firms implemented other measures to reduce their accidents.

To better understand the mechanisms behind this impact, we explore how the results differ by firm characteristics. If the information provided to workers is more effective not because it is provided to workers but because it was provided in a printed format, the relationship between the number of pamphlets sent and the number of workers should be irrelevant. As long as a letter was sent to the firm with one copy of the pamphlet, it would be sufficient to trigger the improvement in safety. However, we find that the decrease in accidents in response to the workers' materials is more important in firms that had fewer workers than the number of pamphlets we sent. We see this as indicative that it is the recipient of the information and not the media that was relevant. Secondly, we may think that workers are able to solve simple safety problems but those that require significant investments or change in organization must be undertaken by the firm themselves. We find evidence of this since the effect was particularly concentrated in firms that were classified as safe during their visit, although the differences are not statistically significant. We find no other evidence of heterogeneity by other characteristics of the firm.

We finally document that, during the first 6 months of 2014, between a bit more than a year and 4 months after the interventions, depending on the month in which the firm received the visit, we observe wage differentials between the firms depending on the type of information that was provided to them. Firms where the information was provided to the workers had lower average wages than firms who had not received such a package. This would give credential to our hypothesis of asymmetric information since by providing information to the workers, the increased safety would

[1] We normalize our monthly measures so that all our statistics are in accidents per 100 workers per year. Thus, the number of accidents in the 6 month period is multiplied by 2 to obtain an "annual" figure.

have been able to be "charged" to workers through their wages. Our other successful intervention in terms of safety was not accompanied by lower wages in 2014. Following our hypothesis of asymmetric information, this is consistent with the fact that may not be aware of how risky their employment is. Since our experiment only informed managers, the information may not have been shared with workers (after all, the comparison was not good news for the firms who responded) and not leading to any changes in the wages. We find this result particularly interesting since we are not aware of any previous studies that calculated compensating wage differentials in an experimental setting.[2] The magnitudes of our result suggest that workers would have been willing to pay about 20$US, aproximately 2 percent of their wage, to reduce the accident rate by 10 percent. This is much larger than what would have been obtained from a cross-sectional estimation of the compensating differentials in our setting.

This paper relates to many strands of the literature on workers' safety (for an overview of the literature on this topic see Pouliakas and Theodossiou, 2013). Our understanding of worker's compensation schemes has focused strongly on the impact of its benefits on workers' behavior. Bronchetti (2012) finds that workers' compensation provides substantial benefits in terms of consumption smoothing. A large number of studies (Bronchetti and McInerney, 2012; Guo and Burton, 2010; Krueger, 1990; Meyer et al., 1995; Neuhauser and Raphael, 2004) have documented that an increase in the generosity of workers' compensation benefits increase the number of claims and their duration. This suggests that workers' compensation would suffer from moral hazard since workers would be more likely to report or incur accidents when benefits are more generous. We abstract from this debate by focusing entirely on changing the information set and not the benefits workers receive.

As many other mandated insurance programs, workers' compensation has been shown to be shifted, at least partially, to workers through a reduction of their wage. Gruber and Krueger (1990) suggest that an increased in the mandated costs of a safety program decreases local employment with an elasticity of about -0.5.

Another strand of the literature, closer to our paper, has studied the impact of the structure of the insurance program on safety. Chelius (1982) argues that higher benefits encourage firms to improve their safety measures, decreasing accident rates, but that this is in part counteracted by the moral hazard on the size of the workers, increasing claims. Moore and Viscusi (1989) study this empirically and find that the first effect seems to dominate as an increase in the benefits level reduces fatalities on the job. Ruser (1985) adds that workers' compensation is experience-rated and that larger firms are more highly experience-rated. He shows that higher benefits lower injury rates less strongly in large firms, something that is partially panned out in the data. This suggests, as we will show in our paper, that the insurance mutes some incentives for firms to increase their safety. Viscusi (1979, 1986) focuses more strongly on penalties imposed on firms for bad safety measures and argues that the US penalty rates were, at least historically, too low for firms to have the incentives to implement safer

[2] Carpenter et al. (2015) estimates experimentally compensating wage differentials to income risk by altering experimentally the probability that the wage will be paid.

work environments. This seems to also be the case in Chile.

Another set of studies have questioned the impact of training on job safety. Although training has been proven effective in improving safety awareness and behavior, as reported in the surveys by Cohen and Colligan (1998) and Robson et al. (2010), there is no consensus in terms of objective outcomes, such as workplace accidents. Burke et al. (2007, 2011) argue that studies about occupational safety and health (OSH) training do not always show objective outcomes because they do not make a proper distinction among different types of training: only "engaging" training methods seem to generate an impact. This is confirmed, but less strongly, by Brahm and Singer (2013) for training provided by the ACHS. However, none of these studies have used experimental methods to answer the question so it is difficult to evaluate whether they have really estimated the causal impact of job safety training.

There is a nascent but still small literature analysing workplace safety in SMEs (see for example Arocena and Núñez, 2010; Cagno et al., 2011; Kines et al., 2013; MacEachen et al., 2010). Cagno et al. (2011) provide a description of the situation in Italian SMEs showing that they are lagging in terms of OSH policies and awareness. In a more qualitative study, Eakin (1992) argues that SME owners are not willing or able to take safety measures and thus usually elect to leave it to the workers. This point is also stressed by Cagno et al. (2013). In terms of the impact of prevention efforts (Arocena and Núñez, 2010; Kines et al., 2013; MacEachen et al., 2010), the literature shows a positive impact on behavior and awareness, but the results are mixed in terms of diminishing accidents. In terms of regulations, MacEachen et al. (2010) indicate that the typical OSH regulations do not fit well within the reality of SMEs, impairing prevention and worsening outcomes.

This paper also fits, through one of our treatments, into the literature of altering social norms and using comparisons as encouragement for changing behavior. Allcott (2011) shows that by providing information on how one's consumption of electricity compared to that of one's neighbor, one can reduce electricity usage by those who are scored as "below average" compared to "good." Similar results were obtained by Ayres et al. (2013) for energy consumption (electricity and gas) and by Ferraro and Price (2013) for water usage. Costa and Kahn (2013) show that this type of comparison is most effective with political liberals; conservatives are more likely to sign out of the service and dislike the information provided. Overall, our study shows that such comparison may also be useful for firm safety and that comparative information can also alter firms decisions, particularly SMEs.

The rest of the paper is organized as follows. First, section 2 highlights the regulation of safety by firms in Chile and how regulatory mandates may affect the incentives of workers and firm owners to improve safety in their firms. Next, section 3 presents a simple framework to understand why firms may not have the proper incentives to improve their safety conditions, and why workers might not act on their own to improve it either. Section 4 then presents the detail of our randomization strategy and data while the following section documents our results. The final section concludes.

2 Firm safety in Chile

In Chile, workers' safety and compensation in case of an accident is under the responsabilities of "mutuales de seguridad", private, non-profit institutions. There are three such institutions: IST (Instituto de Seguridad del Trabajo), ACHS (Asociación Chilena de Seguridad) y la Mutual de Seguridad CChC (Camera Chilena de la Construcción). The Chilean system is an integrated one: these institutions provide all the services required in an OSH system, namely prevention (e.g., training), medical treatment in case of accidents and provision of monetary compensation in case of accidents (e.g., wage for workers during the period of absence, pensions in case of long-term or permanent disability). The system covers accidents that occur both at work and also when traveling to and from the workplace.

Each of these institutions has been in place for more than 50 years and the law that governs them is Ley # 16.744, dating back to 1968. Each firm functioning in Chile is mandated to affiliate to one of these three institutions. The contributions to the insurance plan are thus made each month by the employer and provides coverage to the individual in case of accidents or work-related illnesses. From 2013 onward, this rate depends in part on the historical rate of accidents of the firm and in part on the rate of the sector in which the company is operating. Almost 85 percent of workers are affiliated with one of the mutuals, with ACHS accounting for half of the market. The remaining 15 percent are served by a state organization called Instituto de Seguridad Laboral (ISL) with the same attributions. The firms are free to choose any institution they desire, with the exception of those with a bad safety performance, in which case they remain locked for three years to a particular institution. This provides incentives for the companies to implement preventive activities and avoids adverse selection problems in the market.

In terms of workers benefits, sick leave is available for a maximum period of 52 weeks, renewable for another full year if needed. It covers 100 percent of wage earnings of the last 3 months. In case of partial disability, the compensation is between 1.5 to 15 times the base salary, as given by the average of the 6 months previous to the accident. Finally, in case of a fully debilitating accident, the pension covers 100 percent of the worker's previous salary if the worker cannot work, and a lower percentage if his work capacity has been lowered by 40 to 100 percent. This is supplemented if the person also becomes disabled. The medical treatment is fully covered by the insurance and it is carried out in facilities that are 100 percent devoted to treating workplace accidents. These facilities are run by these three organizations and have a high reputation in the health sector; as an example, some excess capacity in these facilities is contracted to the private healthcare sector.

In terms of prevention services, a mandatory minimum percentage of 12 percent of total expenditures of the mutual benefit societies must be committed to prevention activities. Among these activities, the most important one is OSH training. During 2012, the ACHS alone provided training to approximately 500,000 workers, which is roughly one tenth of the formal workforce in Chile and

half of the amount of state-subsidized vocational training taking place in Chile. This training is free for insured companies. ACHS has a wide offer of courses, from safety basics to training covering specific issues of a particular industry (e.g., proper use of pesticides). Ninety percent of this training is provided on site, with the remaining portion provided in public classrooms and using online training methods. This training is mainly delivered by specialized external providers, many of which had historically been attached to the ACHS.

As mentioned before, there is an experience rating in this market. This implies that as a firm experiences more and more accidents, its rate of coverage will increase. Specifically, firms pay a base contribution of 0.95 percent of the monthly taxable wagebill. On top of that is added an extra contribution from 0 to 3.4 percent depending on the industrial sector, and an extra from 0 to 2.4 that depends on the past accidents of the company. With these additions, the insurance rate can go from 0.95 to a maximum of 6.8 percent. These rates are adjusted every two years for the affiliated companies. Several researchers have shown that on top of these direct insurance costs, indirect costs of accidents, such as additional compensation to workers, decreased morale and productivity, loss of revenue due to a halt in the production, civil claims by injured workers, etc., are twice as big as direct costs (e.g., Pouliakas and Theodossiou, 2013; Viscusi and Aldy, 2003).

At the national level, the accident rates show an overall downward trend over the last 10 years. In 2002, the annual rate was at 8.72 percent; in 2009, is was 6.5 percent; and in 2013 it went down to 5.3 percent. The incidence of work-related fatalities is around 7.2 deaths per 100,000 workers around 2013, having experienced a similar but less pronounced downward trend as the incidence of accidents.

ACHS and its SME Affiliates. Prevention for SMEs is executed by ACHS using the "SME community" program. In 2013, there were approximately 31,500 SMEs affiliated with ACHS. Firm are classified twice a year (January and July) as an SME when the average number of employees in the previous 6 months was lower than 50. SMEs account for 76 percent of all the firms affiliated to ACHS; in terms of number of employees, SMEs account for 52 percent of the total employees affiliated to ACHS. The average size of an SME is 12 employees, 40 percent of SMEs have a size between 1 and 5 employees, 21 percent between 6 and 19, 26 percent between 11 and 25, and 13 percent between 26 and 50. The distribution across the country reflects economic activity: 53 percent are located in the metropolitan region, 12 percent in the VII region, and 5 percent in the V region (the rest is quite evenly distributed in 12 remaining regions). In terms of industrial sectors, SMEs are distributed as follow: 31% in services, 22% in retail, 16% in agriculture, 12% in manufacturing, 9% in transportation, and 9% in construction.

At the start of 2013, the SMEs of ACHS had an accident rate of 5.32 accidents in a year per 100 workers which was 22 percent higher than non-SMEs firms. On top of this, they have more serious accidents, reflected in a 44 percent higher amount of working days that were lost because of acci-

dents. The more risky sectors are construction and manufacturing with accident rates of 7.2 and 7.9 respectively. The less risky sectors are services and commerce, with accident rates of 2.9 and 3.6 respectively. The worse performance of SMEs in comparison to non-SMEs firms is fairly stable across industrial sectors, with a slightly larger difference in the construction and manufacturing sectors.

The "SME community" prevention plan was launched in 2011 as a pilot. Prior to this program, SMEs were undifferentiated (i.e. they were served in the same way as larger firms affiliated with ACHS), which led to a relative lower attention because of the fixed costs of attending any single firm (travel, administrative work, etc.). For example, a large proportion of SMEs were left unattended. With this program, ACHS attempted to turn this situation around, fulfilling its (mandated) social role in terms of prevention. The design principles of this program were the following: i) include the specific traits of SME into the program, ii) provide coverage to 100 percent of SMES, so that all SMEs are visited at least once a year, iii) promoting the adoption of OSH prevention by the SME owner, iv) standardize and systematize the service to secure cost-effectiveness and scalability both in volume and geographical reach. Special resources were devoted to design and execute this plan. A specific manager and his team were granted authority and responsibility. Also, in Santiago's Metropolitan Region, the team of ACHS' "monitors", who are the ACHS' personnel who visit the affiliated firms for on-the-field support, to provide advice and council, to help solve problems, etc. was split, creating a separate team of "monitors" who were exclusively focused on SME affiliates. In the rest of the country, where density of firms is lower, "monitors" were catering to all firms, SMEs and non-SMEs.

Operation of the ACHS' SME Program. First, the SME received the visit of a "monitor." The number of visits to a firm by the monitor were predefined according to the firm's initial accident rate. Thus, SMEs that were classified as having a critical condition received one visit a month; those that were classified as having a moderate condition to critical condition received a visit every other month; the rest received a visit once a year (for this group the executive had some leeway to decide which firms to visit). As with the classification into the SMEs group, this classification of firms was done every 6 months, in July and January. In this visit, the monitor engages with the firm, preferably with the owner, and then carries out a diagnosis of the firm, aided by an IT tool.

The second step of the program is to send, a couple of days after the visit, a "prevention plan" that is tailored to the company. This plan is based on a template that, by changing its parameters, provides this customization ability. This document is lengthy, approximately 30 to 40 pages, and contains four parts: 1) an assessment of the compliance of the firm with the basics of safety, especially in terms of regulation, 2) an assessment of the potential emergencies that could happen in a the firm (e.g., flooding, fire, etc.), 3) an assessment of the workplace risks that could lead to accidents (e.g., working at heights, working under water, etc.), and 4) a proposed prevention plan (e.g., specific training courses). Coupled with this e-mail, the ACHS also sends safety signs and posts that are to be located in specific places of the SME's site (in case the firm did not have these in place).

The third element of the program is to provide permanent support with the use of a dedicated SME website (which contains plenty of information about prevention, risks, etc.), of an e-learning website and of a call-center. Also, continual contact with the SME is fostered by way of e-mailing campaigns (e.g., newsletters, new courses, personalized content), executive/call-center check calls. In terms of numbers, within 2012, the ACHS's SME Community program delivered 750,000 signs, made 130,000 inbound-outbound calls, sent 70,000 e-mails, they experienced 77,000 downloads of preventive sheets for specific risks in their website, had almost 55,000 YouTube views, and training 4,000 "monitors."

The program started in September 2011, where 214 visits were made. There was a progressive ramp up during 2012, so that by the start of 2013, just a few months prior to our experimental intervention, ACHS' SME program was visiting 2,500 to 3,000 SMEs per month, reaching its full design capacity. Also, by March 2013, the ACHS had executed 32,123 visits, having reached 90% of its SME population.

3 Theoretical Framework

Given the institutional set-up we have just described, in which firms are mandated to pay for a worker's compensation insurance, there are basically three reasons why firms may not be taking measures to improve on-the-job safety.

First, employers may be indifferent about having a safer or riskier environment if the cost of improving safety will be completely offset by the ability to pay for lower wages. If workers also receive the fully compensated differential for working in a riskier environment, they may not be interested in pushing for a safer workplace. However, in that case, providing firms with information should have no impact on accident rate as firms are already fully conscious of their risks and costs. If we observe effects on safety, they should be accompanied by countervailing shifts in wages.

Providing information to the workers may be more useful if workers are able to increase the safety of their workplace without their employer noticing and thus without seeing their wages lowered. However, since this would require them to take some extra safety measures that may be costly to them (take the time to put their safety goggles on), they would only do it if the cost were lower than the benefit. In that case, we would observe higher on-the-job safety and limited impact on wages.

Secondly, firms may not be able to reduce their accident risks because they do not have all the information available to them. This may be because they have trouble processing the information they obtain or because workers have more information than firm managers but do not have the incentives to implement measures of change. In both cases, providing information to firm managers should decrease accident rates since firm managers are now able to implement the actions that are needed. However, they may also think that these actions are impossible, which is why giving them

an industry benchmark may be useful as a comparison point. If the problem stems from the fact that workers do not transmit the relevant information to the management, then providing information to the workers should prove useless in this context. Providing information to manager may not lead to impacts on wages if the managers do not share this with workers. However, if workers have more information than managers, they may notice very quickly the improvements and employers may be able to pass down some of the costs to them.

Finally, safety incentives may not be optimal because of the existence of an insurance policy. Workers, on the one hand, may not fully internalize the need for increased safety since they are receiving a compensation in case of an accident. Firm management, on the other hand, also suffers from moral hazard since it does not face the full cost of its increased accident rate through experience rating. In this case, providing information may not be sufficient unless firm managers have more information than workers, and because of the insurance policy, choose not to implement changes. In that case, providing information to the workers could lead them to demand that firm management implement of some measures; management would then find it optimal to respond in order to keep either the workforce satisfied or to prevent higher wage demands.

Thus, if providing information to management improves safety outcomes, this would suggest that management lacks knowledge required to reduce work-related injuries. If providing information to workers improves safety outcomes, this would suggest that workers are at a knowledge disadvantage vis-a-vis their employer and that providing them with information reduces that asymmetry. In that later case, employers may also be able to push down some of the costs of the change in safety measures onto workers as wage decreases since workers are now better informed about the risks and provided a better sense of how safer their new environment is. Providing information to management would not necessarily affect wages since workers have a knowledge disadvantage and management may not be able to pass down the costs to them in that context.

One can also see some parallels between our treatments and behavioral biases that may be present in addition to informational asymmetries we just discussed. If firm managers struggle with complex problems, the email with a simplified plan should reduce these barriers. If firm managers disjoin benefits and costs over time, the email making costs more salient should reduce this. If the problem lies in receiving low feedback from the choices they are making, the email with a comparison to the sector could provide assistance. Finally, if there is an issue with how frequently the employers have to make a choice, we may think that reaching the individuals who have to take decisions more frequently, namely the workers, would be helpful.

4 Empirical Strategy and Data

Given the theoretical factors that could be at play in our context, we devised a randomized control trial to be able to separate the different reasons why safety may be sub-optimal. Our interven-

tion consists in four types of information provided to the firm, through two separate randomization strategies. Our universe consists of all firms that were visited by the ACHS with the "SME community" program between March and November of 2013. At the end of each month, a list of all firms visited was sent to the research team. We randomly allocated around 500 firms to receive one of three email treatments. We initially did the randomization such that each firm would receive only one type of email but starting in August, multiple treatments were allowed by the ACHS. We use the initial allocation as our random variable and not whether the email was actually sent since this may not be entirely random. The emails were selected to highlight the challenges that firms could face. First, if the difficulty lies in processing complex information, the email would send a much more simplified and shorter version of the document officially sent to the firm. You can see an example of this email in Figure 1. The second email was trying to make the firm compare itself with firms in the same sector. For this, we computed the average annual accident rate for 2012 in the sector in which the firm was included and compared it to the firm's accident rate in the same year. The email that was sent to an example firm is shown in Figure 2. This figure makes it clear that the impact of this treatment may be radically different for a firm that has an accident rate above the mean and one that has an accident rate below the mean. We will thus distinguish these in our analysis. Finally, if the difficulty lies in the lack of salience of the costs of low security, we monetized the costs of accidents faced by the firm last year in our last email treatment. We did so by multiplying the total days that were lost by the firm with the average wage rate of the sector. Also, we stressed the fact that additional hidden costs were large. An example of this email is displayed in Figure 3.

Simultaneously, we conducted another randomization where, in selected months, 1000 firms were allocated to receive an envelope including the letter presented in Figure 4 and a set of 20 pamphlets, an example of which is shown in Figure 5. The letter clearly specified that the information was provided for distribution to the workers. It also had a slogan encouraging the participation of workers in the safety process by including at the top of the pamphlet a note stating "Security is everybody's concern." This pamphlet included specific information regarding the safety hazards of the firm based on the report of the expert. They were mailed within the month following the visit, for the first 5 months of the intervention.

Table 1 summarizes the sample sizes we assigned to each combination of treatments. "Take-up" is a difficult concept to measure here. For emails, we have information regarding whether the email was opened and, in some cases, whether individuals clicked on the attachment. However, none of these two actions guarantee that the information was actually acquired by the firm. We nevertheless present in Table 1, the opening rate of emails for each treatment. We find that emails reasonable opening rates in this context, around 25 percent for all treatment. While common for this type of intervention, it does imply that, despite our large sample sizes, low compliance will diminish the statistical power of our experiment. We also find that the simplified legal plan, the only one with an attachment, was the one most opened. While not reported here, we have tried correlating "take-up" with firms baseline characteristics and have been unable to find a relationship with anything else than

size. For physical mail, we have no indication of whether the packages were opened and distributed. We nevertheless conducted a small-scale survey to a random subsample of our treated firms. We attempted to reach 476 randomly selected firms who had been assigned to receive the package and managed to reach 251 of them. Of these 251 firms, 111 firms, or 44 percent of them, recorded having received the package we had sent. Of those who remembered receiving the package, 84 percent declared having distributed them to their workers. Overall, our "take-up" rate for the pamphlets is thus only a bit higher than that of the most popular email treatment with around 35 percent of workers in firms where packages were sent having received them.

Since all our take-up information is partial, the analysis will focus on an Intent-To-Treat estimation and not a treatment on the treated. Formally, we will estimate the following equation:

$$Y_{jst+x} = \beta T_{jst} + \gamma Y_{jst_0} + \delta_{st} + \varepsilon_{jst+x},$$

where we regress the outcome of a firm j in cluster s and observed at time $t + x$ on whether that firm was assigned to a treatment in period t, dummies for each treatment clusters and, in some cases, values of our regressors before the intervention as additional control variables. The coefficient β should thus capture how being assigned to a particular treatment had a on the outcomes of interest. We allow the standard errors of the regression to be heteroscedastic.

These outcomes are all obtained from the administrative data of the organization who partnered with us. These data include the number of accidents reported to the organization, the courses that were requested by the firm for training workers on safety issues and the degree of compliance observed by the monitor during his following visit. While there may be concerns about the validity of these data since they are administrative, we see little reason to think that our intervention would alter the report of accidents by workers, instead of the actual occurrence of accidents. We further test that none of our treatments significantly altered the probability of reporting any accident through the period in study, which is where the misreporting margin may be most visible. We also have to be careful since we will only be able to obtain outcomes for firms that remain within the same organization over our sample period. However, this includes less than 2 percent of our sample, thus leading us to think that we do not have a very serious sample attrition problem.

Table 2 shows, in the second column, the summary statistics of the firms that are involved in the study. Depending on the outcome, we have between 14,500 and 16,500 firms in total. On average, in the 12 months preceding the visit of the mutual, they had about half an accident per month. These firms are SMEs with an average of a bit less than 14 workers although the distribution is very concentrated among smaller firms. Combining both numbers gives us a monthly accident rate of about 0.05 in the year previous to the receipt of information. Looking at the full calendar year of 2012 for all firms, we see that on average, firms lose about 13 days of work per year for work-related accidents and so about 1 day lost per worker per year. We also have information regarding the training that the firm completes with the organization. Only 4 percent of the firms had completed such a course

in the year before the intervention.

The next columns of Table 2 show the difference between each treatment and their respective control, controlling for the randomization strata. The standard error on that difference is presented below. We find a small number of differences that are statistically significant between our treatment and control but nothing that suggests that the randomization failed since a small number of outcomes would differ at the 10 percent level by construction. Furthermore, we will control for these pre-treatment variables in the regressions to show that the random imbalances in some of the variables we have is not causing the results we document.

5 Results

5.1 Main Impact

We first start by presenting the impact of each of the information treatments on the number of reported accidents in that particular firm over a 2, 6 and 12 month period. All outcomes are the rate per 100 workers per year, making each coefficient comparable across specifications. In our sample, the average accident rate is about 0.047 per 100 workers per year. Table 3 shows, in the first three columns, the impact without the inclusion of any controls while the last three columns include the accident rate for the 12 months previous to the intervention, firm size (in terms of workers) over that period and the legal rating given to the firm in the last visit. Each panel correspond to a different treatment. We find no evidence that the simplified plan or the monetization of the costs of accidents significantly decreased accidents over the 12 month period following the intervention. If anything, our results suggest that making the costs more salient to the firm manager leads to a higher number of accidents being reported. On the other hand, the simplified plan and the comparison show some coefficients being negative but never very large nor statistically significant. This differs very greatly from the results concerning the workers' package which show an initially smaller decrease of 0.006 after two months but this effect remains similar in magnitude as time goes by and the significance is strengthened as well. This corresponds to a consistent fall in the accident rate of 15 percent over a period of 12 months. The addition of controls does little to alter the results, either in magnitude or in significance, which lends credibility to the randomization.

While not presented here, we find that this decrease in the accident rates after the receipt of the worker's package is not linked to a decreased in the number of months where an accident was reported. Thus the effect we report seems to capture more the intensive than the extensive margin of accidents. We also tried using the data we have about days lost to measure the severity of accidents. We find no evidence of a change which may be due to two different factors. First, the number of days lost in a given month is influenced not just by the accidents that occurred in that month but also by accidents that occurred previously and that are still costing the firm days in the following months.

This would make finding an impact of our treatment more difficult since that number has substantial auto-correlation making the change before and after the treatment less stark. We also think that this may indicate that the accidents that are avoided in this case are likely not to be the ones that generated the most days lost and may be related to less severe workers' injuries.

We then turn to additional outcomes we can measure in the administrative data to try to shed some light on the channel through which accident reductions may have been achieved. In Table 4, we look at whether the firm completed a training in 2013 and 2014. We also include in this table an evaluation made by the association regarding the legal compliance of firms at the next visit. However, this outcome is problematic since we only have it for firms that received a visit within the 12 months following the information intervention. Having a visit so close to the previous one usually implies that there was a complaint or that an investigation was required after an accident. Thus, the sample could be strongly biased towards firms with lower levels of security. As in the previous table, the first three columns correspond to the regressions without controls while the last three include measures of the size of the firm and the accident rate 12 months previous to the experiment, the level of legal compliance in the last visit and whether the firm completed any classes in 2012.

The results suggest that the comparison email and the workers' package had a statistically significant impact on the probability that the firm completed a class in 2013 and in 2014. The effects are between a 0.5 and 1 percentage point increase, which is substantial given that the probability that a firm completed a class in 2012 was only 3.3 percent. Thus, at least part of the effect seems to have come from firms being more sensitive to the needs of acquiring safety training. However, the timing of the classes and of the benefits in terms of safety suggests that more effort must have been exerted in addition to asking for more training from ACHS. As a proxy, we can use the impact of training measured in Brahm and Singer (2013), whose results suggest that one more class (which translates into about 225 more hours of training for the workforce) would reduce the number of accidents in a representative Chilean firm by 6.8 percent the following year. Given that we raised the probability of taking a class by 1 percentage point, this would translate into about a 0.068 percent decrease in accidents which would suggest that our increase in training could explain a maximum 20 percent of our decline, suggesting that other changes must have also occurred in the firm to justify the decrease we document.

While not reported here, we find no evidence that the effects we report here are driven by co-linearity between treatments. Including all treatments simultaneously in one single regression do not change the results substantially, as it should given our design. We also tried to measure whether there were any changes in the number of days lost because of work accidents in the months following the treatment.

We next examine a bit more carefully the impact of the comparison treatment. If we really believe that this treatment was meant to show bad performance-firms their relative position compared to the mean, we should find that only firms that received "bad" news responded to the information

campaign. We explore this in Table 5 where we report the impact of receiving the comparison treatment for firms who were below the average for accidents in their industries and then the interaction of being above one's industry average in the past with being assigned to receiving the comparison treatment. In the first panel, we report the impact on accidents, as in Table 3. There, we find that the entire effect we estimated previously comes from firms with above median accident rates. For those with rates below the median, there is no evidence of reduction in the number of accidents. We do find that firms that were shown to be above the median did lower their accident rate more than those who were shown to be below. The difference is statistically significant for the 6 months rate but the pattern is constant over the three different time horizons. However, the next panel shows the impact the treatment had on class taking and legal compliance. We find no evidence that class taking was concentrated amongst firms with higher than average accident rates in their industry. If anything, the coefficients point to the opposite relationship, even though it is rarely significant. Thus, this suggests that firms who were told by the information treatment that they had higher than average accident rates did not reduce that rate by making their employees attend more classes but by other methods. Overall, we conclude that the comparison treatment helped firms that received "bad" news improve their safety record, but not necessarily through higher training within the organization.

One may think that being ranked below the average may also capture other characteristics of the firms which would impact the way they would react to any safety treatment. However, we looked at the impact of the treatment by various other characteristics in the next section and found no evidence that riskier firms in general responded more to this intervention. Also, we found no difference in the way the pamphlets to workers impacted the safety record of the firm based on that distinction. This suggests that the impact of the comparison treatment lied specifically in what one was being compared to and not other firm characteristics.

While we designed the randomization of emails and letters to be orthogonal, we never ensured that we had sufficient power to be able to detect interactions between interventions. While we do not report these formally [3], we found that the two treatments that had a positive effect on safety had no additional benefit when combined. However, the sending of pamphlets to workers appears to have lowered accident rates particularly when the firm manager received the simplified plan email and have led to smaller increases when the manager received the monetization of costs email. This would suggest that providing information to employees appears to be particularly relevant when employers are also provided with incentives to improve safety standards but that those on their own do not appear to be sufficient.

Finally we were able to secure data on the distribution of wages in the sample of firms that were subject to the experiment but only for the first 6 months of 2014, namely less than a year after our intervention. We were unable to obtain the data for months before or during our intervention, thus forcing us to simply compare treatment and control firms at this point in time, without being able

[3] Results available upon requests.

to say anything about the path of wages through time. Furthermore, for confidentiality reasons, we were provided only with the firm's average, median, 25th and 75th percentile of the wage distribution over those 6 months. Additionally, we must clarify that these are not necessarily representing the full compensation of workers but only the fraction of the wage which is subject to the contribution to workers' compensation payments. This implies that very high wages are capped at the maximum bound imposed by law (although our data on percentiles and medians should be unaffected by this cap) and that firms may evade some of their tax obligations by paying only a fraction of the wage through the official channel and offering cash transfers for any additional amounts. Nevertheless we see this data as very interesting since, to our knowledge, there are no studies of experimentally measured compensating differentials related to safety.

Using the wage data (average and median), we thus compare treatment and control firms using our same regression but without using baseline wages as controls. The results of the regressions are presented in Table 6. We first document that for the three treatments that did not alter the safety measures, no differences in wages are visible between the treatment and the control firms. Not only are the point estimates not statistically significantly different from zero but they are very small, around 2 to 7 US dollars per month. While not presented here, in the case of the comparison email, we found no difference in the wage distribution between those who received good news and bad news. For the only treatment that informed the workers directly about the measures they could take to improve safety, we see a large and significant decrease in the average wage for firms randomly assigned to that treatment. This would suggest that firms where workers were informed directly saw the most improvement in safety but this came at the cost of their wages being lowered. The magnitudes we document are around CLP 15,000, or between 25 to 30 USD per month. Given that the average monthly wage in the sample is a bit below CLP 500,000, this corresponds to about 3 to 4 percent of the control mean. While not reported here, we observe very similar shifts at the 25th and 75th percentile of the wage distribution, suggesting that the impacts were widely shared and not concentrated in some region of the distribution. Given that this is the only treatment where workers were involved, this suggests that when workers are informed about the safety risks they face and that these risks are corrected, part of the cost of the improvement is passed down to them in the form of lower wages. The pass-through suggest that workers were willing to pay about 2 percent of their wage to decrease the accident rate in their firm by about 10 percent. This is more than ten times larger than what would be estimated in a typical cross-sectional estimation where cross-firm variation in accidents would be correlated with cross-firm variation in wages in our sample. This highlights the importance of obtaining such estimate through experimental variation.

Finally, we also show how firm growth may have responded to these treatments by comparing the number of workers in treatment and control firms. Here, given that we were slightly unbalanced in terms of firm size in baseline, the results that include controls are particularly relevant. We find that the workers' package led firms that were randomly assigned to receive it to have 0.7 more workers than firms that were not assigned to receive it. This represents about 5 percent of the control work-

force. This would suggest that improving safety through providing workers with more information may help firms in their growth or their recruitment process, thus leading them to have a larger workforce. We do not find that any other treatment led to an increased firm size; if anything, the simplified plan appears to have lowered firm growth.

5.2 Heterogeneity

We now turn to looking at which types of firms were more likely to respond to the information treatment. Given that we found only two of the information experiments to be successful, we focus on those. However, we find limited evidence that looking at subgroups would have led us to find significant impacts of the other two alternatives.

We first divide our sample by whether the firm had more or less than 20 workers. We chose this cut-off since the workers' package included only 20 pamphlets. It would thus be logical that if the pamphlets reaching the workers is key to the result we showed previously, firms with less than 20 workers should have been particularly benefitted by the intervention. We explore this in Table 7, where we only present the results with controls, since they are very similar to the ones without them. One must be careful here in interpreting the results for the comparison treatment since while 22 percent of small firms were ranked above the mean in terms of their accident rate, that number was 43 percent for firms with more than 20 workers. Thus, size could also partially capture the previous result we discussed. Nevertheless, the results suggest there is no statistical difference in the impact of the comparison email by firm size. However, we do find that the impact of the workers' package on the accident rate was entirely concentrated in small firms. The magnitude of the coefficients would basically imply a null impact for firms with more than 20 workers. While this may be supportive of our argument that the workers' package worked through the pamphlets given to the workers and not through other alternatives, we cannot exclude that there may be other characteristics of small firms that made them more responsive to the package. In Panel B, we again find evidence that training is not the only channel through which accident reduction was achieved since we do not observe the same size divergence in training as in accident decrease. The organization we partnered with has a minimum number of workers required for them to be willing to give a class on location to a firm. This could explain the fact that small firms are not more likely to complete a course since it is more difficult for them to obtain such a class.

We next turn to whether the comparison or workers' package had differential impacts depending on how risky the firm was before the intervention. The organization in charge of firm safety gives three different colors to the firms they supervise: green, yellow and red. The first type is reserved for firms with low levels of accidents in the past, followed by those with higher rates as yellow and those with very high rates as red. Table 8 shows that there is no statistically significant impact of the comparison email or the workers' package in firms that were previously classified as more problematic. If anything, the point estimates suggest that "red" firms did not benefit in any way

in terms of accident rates from the workers' package and only for the first two months in the case of the comparison email. This suggests that our intervention can more easily solve problems that can be fixed by workers' involvement and not those that are likely to require structural changes, for example. While not presented here, we find that "red" firms are more strongly benefitted by the simplified plan email, maybe because this is the type of information that is required for making more substantial adjustments in terms of safety. Again, we find limited variation in terms of training completed and in particular, little correlation between our measure of accident rates and training undertaken.

While not reported, we have also explored whether there is any heterogeneity in terms of geographical location and whether the firm had taken a training in 2012. We find no evidence that our information treatment impacted firms in these different groups in a distinct manner.

6 Conclusion

Overall, this study has shown that providing information to SMEs can reduce the incidence of work-related injuries. However, the informational problem does not seem to stem from the complexity of the information provided to all firms or from the lack of salience that work-related injuries may have in the bottom line of SMEs. There seems to be a value for information that allows SMEs to know that other firms have been able to generate a safer environment within their given industry, allowing for a realistic benchmark. Furthermore, there is evidence that closing the informational asymmetry between workers and firms may have substantial benefits in terms of safety. All this suggests that informational barriers are in part to blame for the lack of safety in SMEs in a growing emerging economy. The fact that informing workers led to a safer work environment but lower wages suggest an even bigger role for differential information sets by workers and firm managers. This also provides one of the first estimate of compensating wage differentials estimated in an experimental setting.

There are multiple lessons we can draw from this experiment. The informational asymmetry between workers and firms may lead to differences in perceived riskiness of the employment, which would impede the mechanism of compensated wage differential to be fully at play. Thus, some additional work is required to explore the role of information on wage adjustments. We find some evidence of this but suggest that there is much more that could be learned from experimental study about the willingness of workers to trade safety for lower wages.

Secondly, this also suggests that managers of SMEs do face significant constraints in time or knowledge in this type of setting to satisfactorily fulfill all their managerial duties. This is very close to the literature that emphasizes that small firms have limited management skills and highlight the role that simple, targeted information may play at diminishing these constraints. We see our results as an interesting avenue for other managerial limits of SMEs.

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7 Figures and Tables

Figure 1. Example of information provided in the simplified plan

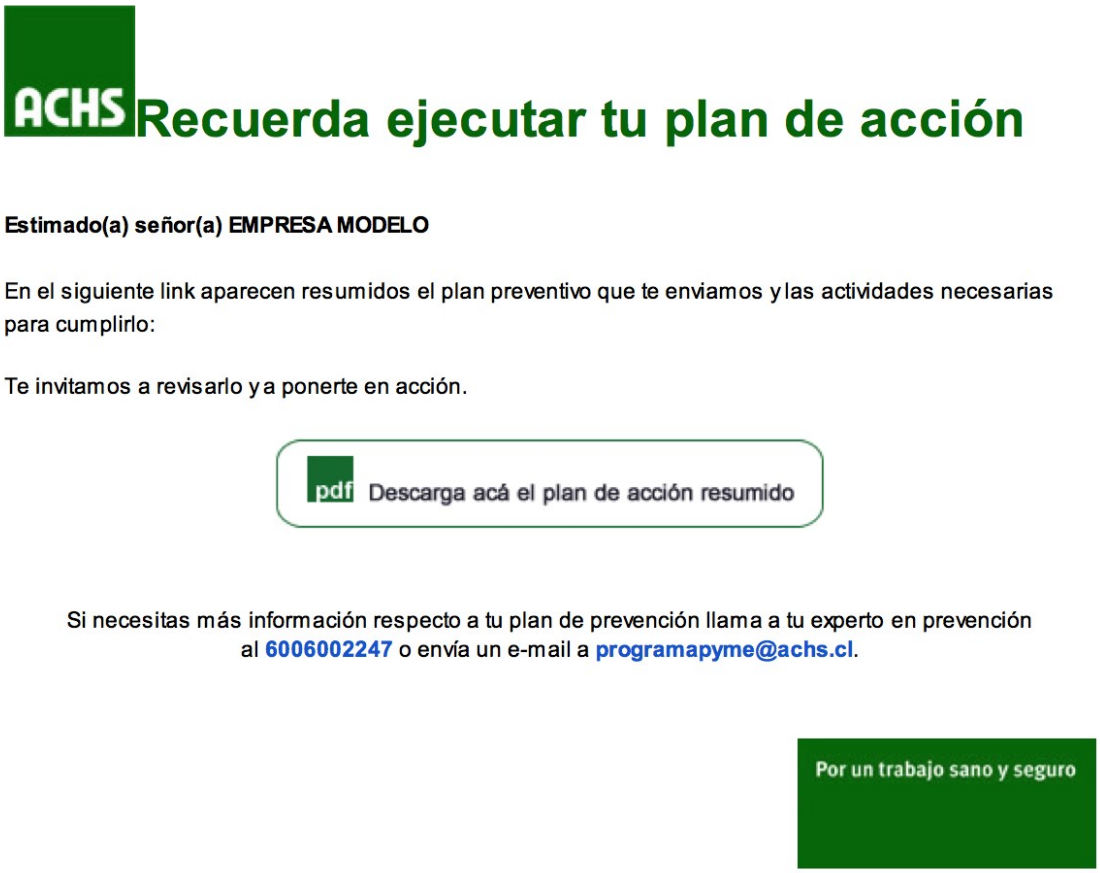


Figure 2. Example of information provided in the comparison plan

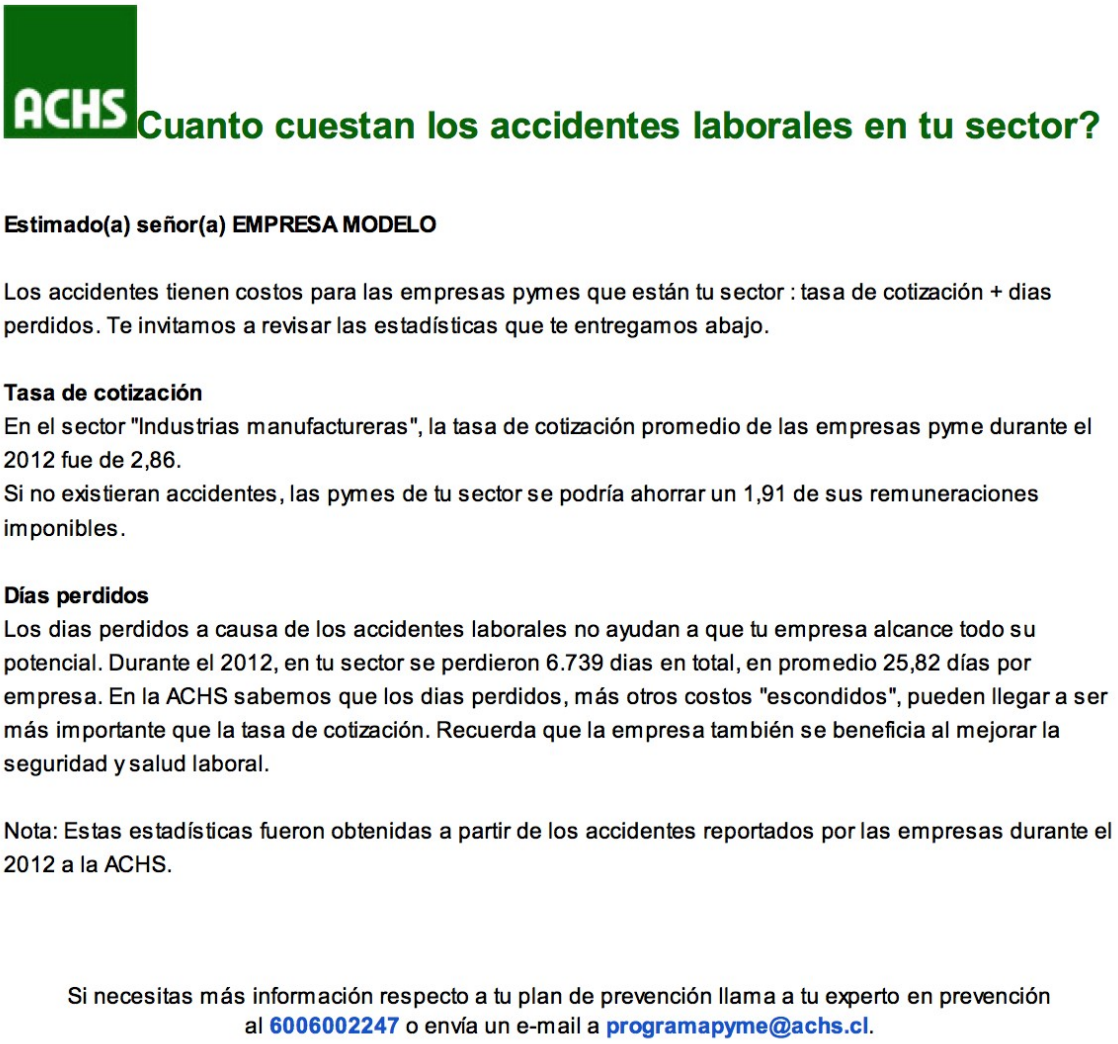
Estimado(a) señor(a) EMPRESA MODELO

En el siguiente gráfico puedes ver como se compara tu tasa de accidentes del 2012 con la tasa de otras empresas pymes del sector "Industrias manufactureras".

Otras PYMES en tu sector tuvieron menos accidentes. Te invitamos a redoblar los esfuerzos para mejorar tu gestión preventiva.

Si necesitas más información respecto a tu plan de prevención llama a tu experto en prevención al **6006002247** o envía un e-mail a **programapyme@achs.cl**.

Figure 3. Example of information provided in the monetization plan



ACHS

Cuanto cuestan los accidentes laborales en tu sector?

Estimado(a) señor(a) EMPRESA MODELO

Los accidentes tienen costos para las empresas pymes que están tu sector : tasa de cotización + dias perdidos. Te invitamos a revisar las estadísticas que te entregamos abajo.

Tasa de cotización
En el sector "Industrias manufactureras", la tasa de cotización promedio de las empresas pyme durante el 2012 fue de 2,86.
Si no existieran accidentes, las pymes de tu sector se podría ahorrar un 1,91 de sus remuneraciones imponibles.

Días perdidos
Los dias perdidos a causa de los accidentes laborales no ayudan a que tu empresa alcance todo su potencial. Durante el 2012, en tu sector se perdieron 6.739 dias en total, en promedio 25,82 días por empresa. En la ACHS sabemos que los dias perdidos, más otros costos "escondidos", pueden llegar a ser más importante que la tasa de cotización. Recuerda que la empresa también se beneficia al mejorar la seguridad y salud laboral.

Nota: Estas estadísticas fueron obtenidas a partir de los accidentes reportados por las empresas durante el 2012 a la ACHS.

Si necesitas más información respecto a tu plan de prevención llama a tu experto en prevención
al **6006002247** o envía un e-mail a **programapyme@achs.cl**.

Figure 4. Example of letter accompanying the workers' package

Santiago, XX de XXXX de 2013

SEÑOR(A)
<< NOMBRE CONTACTO EMPRESA >>
<< NOMBRE EMPRESA >>
PRESENTE

Estimado Señor(a):

En la ACHS sabemos que el trabajo de prevención y seguridad laboral representa un esfuerzo importante para usted y sus trabajadores. También sabemos que la prevención de accidentes se logra mediante un esfuerzo conjunto entre todos los miembros de la empresa.

En este sobre usted encontrará material informativo acerca de los principales riesgos a los que está afecta su empresa y cómo prevenirlos.

Lo invitamos a que distribuya esta información entre sus trabajadores, contribuyendo al trabajo en equipo en prevención. Le recomendamos además que los folletos sean publicados en lugares visibles para recordarles permanentemente a sus trabajadores acerca de los riesgos que corren y de las posibilidades de prevención a las que tienen acceso.

Esperamos poder seguir ayudándolos en el trabajo de prevención, que es trabajo de todos. Si tiene alguna pregunta o requiere alguna información adicional no dude en contactar a su Experto en Prevención en el fono 6006002247 o e-mail programapyme@achs.cl.

Se despiden sinceramente,

Equipo PYME ACHS

Figure 5. Example of information provided in the workers' package

Table 1. Assignment by email information and take-up

Month of visit	Simplified plan			Comparison			Monetization			No emails		Workers' package Total
	Assigned	Opened	Workers' package	Assigned	Opened	Workers' package	Assigned	Opened	Workers' package	Assigned	Workers' package	
March	556	190	204	555	126	204	563	127	207	1053	387	1002
April	550	176	193	603	136	211	613	111	215	1100	382	1001
May	568	130	208	549	142	201	557	111	204	1065	388	1001
June	520	161	216	470	98	196	479	78	199	942	391	1002
July	672	220	336	506	88	252	271	42	136	839	418	1007
August	423	106	0	318	51	0	161	20	0	512	0	0
September	406	47	0	265	12	0	269	13	0	467	0	0
Total	3,695	1030	1157	3266	653	1064	2913	502	961	5978	1966	5013

Table 2. Summary statistics of pre-intervention firm characteristics and balance tests

	N	Mean	Difference T-C			
		(Std. Dev.)	Simplified plan	Comparison	Moneti-zation	Workers' package
N. of accidents, 12 months bef.	16429	0.6705	-0.0296	-0.0072	-0.0055	0.0514
		(1.6014)	(0.0313)	(0.0288)	(0.0293)	(0.0340)
N. of workers, 12 months bef.	16429	13.3596	-0.9279*	-0.1493	-0.4626	1.4083*
		(26.5246)	(0.5271)	(0.3439)	(0.3223)	(0.7830)
Monthly Acc. Rate, 12 months bef.	16145	0.0491	0.0018	0.0011	0.0015	0.0007
		(0.1653)	(0.0031)	(0.0031)	(0.0033)	(0.0027)
Days lost in 2012	15047	12.9616	0.5873	-0.8587	0.7137	1.1371
		(41.7001)	(0.8891)	(0.8626)	(0.8571)	(0.8805)
Days lost per worker, 2012	14731	0.9485	0.0278	-0.1070	-0.0068	-0.0599
		(4.4458)	(0.0972)	(0.0943)	(0.0812)	(0.0724)
Training finished 2012	16617	0.0339	0.0038	0.0020	-0.0049	0.0039
		(0.1811)	(0.0036)	(0.0035)	(0.0037)	(0.0033)

*** p<0.01, **p<0.05, *p<0.1

Table 3. Impact of information on accident rates, by number of months since the intervention

	Without controls			With controls		
	2 months	6 months	12 months	2 months	6 months	12 months
	(1)	(2)	(3)	(4)	(5)	(6)
Simplified plan	-0.0018	0.0017	0.0020	-0.0026	0.0014	0.0018
	(0.0066)	(0.0045)	(0.0041)	(0.0067)	(0.0046)	(0.0042)
R^2	0.005	0.008	0.010	0.017	0.016	0.019
N	14,583	14,691	14,723	14,446	14,518	14,540
Comparison	-0.0036	0.0007	-0.0017	-0.0036	0.0005	-0.0019
	(0.0063)	(0.0039)	(0.0033)	(0.0063)	(0.0039)	(0.0033)
R^2	0.005	0.008	0.011	0.022	0.019	0.022
N	13,866	13,946	13,974	13,833	13,900	13,921
Monetization	0.0077	0.0064	0.0052	0.0071	0.0061	0.0049
	(0.0069)	(0.0040)	(0.0033)	(0.0069)	(0.0040)	(0.0033)
R^2	0.005	0.008	0.011	0.022	0.019	0.022
N	13,868	13,948	13,976	13,835	13,902	13,923
Workers' package	-0.0057	-0.0069**	-0.0060**	-0.0062	-0.0074**	-0.0066***
	(0.0055)	(0.0029)	(0.0024)	(0.0055)	(0.0029)	(0.0025)
R^2	0.003	0.003	0.003	0.016	0.012	0.012
N	14,586	14,694	16,032	14,449	14,521	15,848

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls (for columns (4) to (6) also include the average accident rate and the average number of workers over the 12 months before the intervention as well as the legal rating given by the mutual during its last visit. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4. Impact of information on other measures

	Without controls			With controls		
	Class in 2013 (1)	Class in 2014 (2)	Legal rating (3)	Class in 2013 (4)	Class in 2014 (5)	Legal rating (6)
Simplified plan	-0.0070	-0.0026	0.0011	-0.0056	-0.0025	0.0020
	(0.0043)	(0.0036)	(0.0066)	(0.0043)	(0.0036)	(0.0062)
R^2	0.019	0.014	0.063	0.093	0.055	0.215
N	15,252	15,252	7,893	14,803	14,803	7,793
Comparison	0.0020	0.0065*	-0.0066	0.0076*	0.0057	-0.0066
	(0.0036)	(0.0038)	(0.0067)	(0.0046)	(0.0038)	(0.0061)
R^2	0.013	0.010	0.066	0.079	0.046	0.219
N	14,434	14,434	7,544	14,167	14,167	7,520
Monetization	-0.0002	-0.0059	0.0152**	0.0010	-0.0055	0.0106*
	(0.0048)	(0.0036)	(0.0068)	(0.0047)	(0.0036)	(0.0062)
R^2	0.011	0.010	0.067	0.079	0.046	0.219
N	14,436	14,436	7,546	14,169	14,169	7,522
Workers' package	0.0105**	0.0056*	0.0009	0.0087**	0.0051	-0.0059
	(0.0043)	(0.0034)	(0.0058)	(0.0042)	(0.0034)	(0.0054)
R^2	0.004	0.003	0.007	0.083	0.044	0.173
N	16,617	16,617	8,266	16,145	16,145	8,165

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls (for columns (4) to (6) also include the average accident rate and the average number of workers over the 12 months before the intervention as well as the legal rating given by the mutual during its last visit. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Impact of comparison email on administrative outcomes, by whether the firm was shown to be above or below industry average

	Without controls			With controls		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Accident rates					
	2 months	6 months	12 months	2 months	6 months	12 months
Comparison	0.0021	0.0051	0.0018	0.0022	0.0052	0.0019
	(0.0056)	(0.0039)	(0.0030)	(0.0056)	(0.0039)	(0.0030)
Comparison	-0.0076	-0.0188*	-0.0123	-0.0072	-0.0186*	-0.0120
*> ind. avg	(0.0188)	(0.0108)	(0.0102)	(0.0187)	(0.0108)	(0.0101)
R^2	0.012	0.020	0.024	0.019	0.026	0.031
N	13,106	13,162	13,177	13,105	13,160	13,174
	Panel B: Other outcomes					
	Class in 2013	Class in 2014	Legal rating	Class in 2013	Class in 2014	Legal rating
Comparison	0.0135**	0.0097**	0.0028	0.0119**	0.0086**	0.0023
	(0.0054)	(0.0044)	(0.0081)	(0.0052)	(0.0042)	(0.0075)
Comparison	-0.0181	-0.0098	-0.0207	-0.0156	-0.0081	-0.0184
*> ind. avg	(0.0120)	(0.0095)	(0.0146)	(0.0118)	(0.0092)	(0.0134)
R^2	0.016	0.014	0.074	0.065	0.051	0.225
N	13,406	13,406	7,165	13,395	13,395	7,164

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls (for columns (4) to (6) also include the average accident rate and the average number of workers over the 12 months before the intervention as well as the legal rating given by the mutual during its last visit. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Impact of information on the workforce in the first 6 months of 2014

	Without controls			With controls		
	Average wage (1)	Median wage (2)	N. workers (3)	Average wage (4)	Median wage (5)	N. workers (6)
Simplified plan	-3.8076	-3.8018	-1.9135***	-3.2378	-3.6910	-0.8415***
	(5.7393)	(5.9629)	(0.6486)	(5.7497)	(5.9819)	(0.2604)
R^2	0.100	0.088	0.080	0.110	0.096	0.852
N	14,320	14,320	14,320	14,154	14,154	14,154
Comparison	3.5645	2.4891	-0.3401	3.7922	2.6383	-0.0704
	(5.9350)	(6.2032)	(0.4246)	(5.9001)	(6.1835)	(0.1962)
R^2	0.105	0.093	0.014	0.116	0.100	0.874
N	13,614	13,614	13,614	13,568	13,568	13,568
Monetization	0.4409	1.2218	-0.5056	1.1465	1.9624	-0.1290
	(6.1666)	(6.4620)	(0.4093)	(6.1191)	(6.4350)	(0.2044)
R^2	0.105	0.093	0.015	0.116	0.100	0.874
N	13,616	13,616	13,616	13,570	13,570	13,570
Workers' package	-14.7992***	-15.1792***	2.5201***	-15.4562***	-15.4360***	0.7429**
	(5.3608)	(5.5469)	(0.9057)	(5.3700)	(5.5625)	(0.2973)
R^2	0.003	0.003	0.012	0.015	0.012	0.835
N	15,615	15,615	15,615	15,449	15,449	15,449

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls (for columns (4) to (6) also include the average accident rate and the average number of workers over the 12 months before the intervention as well as the legal rating given by the mutual during its last visit. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Impact of information on rates of accidents, by firm size

	Comparison email			Workers' package		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Accident rates					
	2 months	6 months	12 months	2 months	6 months	12 months
Treatment	-0.0050	-0.0000	-0.0025	-0.0086	-0.0098***	-0.0080***
	(0.0080)	(0.0049)	(0.0042)	(0.0070)	(0.0037)	(0.0031)
Treatment*	0.0072	0.0018	0.0022	0.0106	0.0110**	0.0064
$>$ 20 workers	(0.0095)	(0.0061)	(0.0051)	(0.0085)	(0.0049)	(0.0040)
R^2	0.022	0.019	0.022	0.016	0.012	0.012
N	13,726	13,793	13,814	14,240	14,312	15,626
	Panel B: Other outcomes					
	Class in 2013	Class in 2014	Legal rating	Class in 2013	Class in 2014	Legal rating
Treatment	0.0009	-0.0027	-0.0071	0.0068*	0.0046*	-0.0101
	(0.0039)	(0.0028)	(0.0073)	(0.0037)	(0.0028)	(0.0064)
Treatment*	0.0259	0.0424***	0.0030	0.0041	-0.0075	0.0132
$>$ 20 workers	(0.0162)	(0.0141)	(0.0133)	(0.0139)	(0.0116)	(0.0114)
R^2	0.102	0.076	0.223	0.076	0.057	0.175
N	14,058	14,058	7,470	15,921	15,921	8,061

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls (for columns (4) to (6) also include the average accident rate and the average number of workers over the 12 months before the intervention as well as the legal rating given by the mutual during its last visit. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Impact of information on rates of accidents, by riskiness

	Comparison email			Workers' package		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Accident rates					
	2 months	6 months	12 months	2 months	6 months	12 months
Treatment	-0.0003	0.0008	-0.0027	-0.0083*	-0.0091***	-0.0080***
	(0.0066)	(0.0042)	(0.0036)	(0.0049)	(0.0031)	(0.0027)
Treatment*	-0.0082	-0.0060	0.0096	-0.0075	0.0041	-0.0073
Yellow	(0.0190)	(0.0116)	(0.0105)	(0.0181)	(0.0103)	(0.0088)
Treatment*	-0.0115	-0.0008	0.0204	0.0405	0.0439	0.0285
Red	(0.0445)	(0.0288)	(0.0255)	(0.0490)	(0.0328)	(0.0290)
R^2	0.008	0.012	0.015	0.005	0.007	0.009
N	13,215	13,276	13,295	13,293	13,357	14,547
	Panel B: Other outcomes					
	Class in 2013	Class in 2014	Legal rating	Class in 2013	Class in 2014	Legal rating
Treatment	0.0077*	0.0077**	0.0078	0.0052	-0.0004	-0.0098
	(0.0046)	(0.0037)	(0.0048)	(0.0041)	(0.0033)	(0.0059)
Treatment*	-0.0107	-0.0048	-0.0042	0.0225	0.0181	0.0047
Yellow	(0.0239)	(0.0174)	(0.0252)	(0.0213)	(0.0158)	(0.0174)
Treatment*	0.0620	-0.0588	0.0427	-0.0044	0.0742	0.0520
Red	(0.1247)	(0.0902)	(0.1188)	(0.1294)	(0.1088)	(0.0766)
R^2	0.088	0.067	0.021	0.084	0.062	0.181
N	13,547	13,547	13,660	14,851	14,851	7,586

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls (for columns (4) to (6) also include the average accident rate and the average number of workers over the 12 months before the intervention as well as the legal rating given by the mutual during its last visit. *** p<0.01, ** p<0.05, * p<0.1