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Is workplace safety everybody's business? Experimental evidence on prevention information, accidents and compensating wage differentials in SMEs *

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Abstract

We randomly distributed safety-related information treatments to SMEs –three to the firm manager and one to the firm workers. We find that only information provided to workers leads to a large and lasting decrease in accidents and increased prevention. It also leads to an interesting wage response: wages fall but only after accidents drop. Further analysis indicates convincing presence of a compensating wage differential, much larger than hedonic estimates. Whom to inform appears to matter since workers seem to be more responsive and be willing to pay for increased safety through wages.

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1 Introduction

Even if worker safety has improved in recent years, it is still a relevant source of costs for workers, particularly when they suffer significant injuries, for the firms, and for the economy[1]. Worker safety might be perceived as a low priority for the owners or managers of small and medium enterprises (SMEs).[2] The fact that SMEs have significantly higher accident rates and lower protection levels than comparable larger firms has been well documented (Sørensen et al., 2007). Is job safety in SME so much lower because managers do not have the time or the capacity to take on this task, as has been discussed for other aspects (see for example OECD 2013, or Bloom et al. 2013), or because some key agents, namely workers, do not have the right information to push for changes? We study this question by randomly distributing four types of information to SMEs in Chile: three of them directed to the firm manager –each one addressing a specific behavioral barrier– and one directed to the firm workers -addressing awareness of risks and prevention options. We try to differentiate between reaching managers and workers directly to test who is the most efficient agent of change to increase safety in SMEs. We then employ our experiment as a source of exogenous variation on workplace riskiness and measure the willingness to pay of workers through changes in wages, thus estimating an experimental measure of compensating wage differentials.

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. On top of this asymmetric information problem, workers and/or managers might not fully understand how safe or unsafe the firm is, or might not be aware of the main risks when these are less salient (for example chronic injuries rather than very visible relatively minor accidents).

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. Each month, ACHS experts visited roughly 1/12 of its 31,500 SME clients, in the context of the "SME community" prevention program. In each of these visits, the ACHS collected information about the firm and sent a report which included an assessment of safety conditions and a suggested non-mandatory prevention plan that is designed to account for the specific situation of the firm. We conducted a double-randomization of

[1] Li and Singleton (2018) mention estimates of the cost of on-the-job injuries in the order of $192 billion for the United States. For Chile, the institutions in charge of managing the OHS programs have estimated the costs to be around 4% of GDP. In many cases estimates do not include other costs, such as lost earnings, and thus could be a lower bound. The estimates also do not consider potential investments required to reduce accidents or other health problems.

[2] In our study we use the definition of SME used by our partner, which corresponds to firms with 50 or fewer employees. This definition is closer to the concept of small firms in several other countries.

the firms that were visited in each month for a period of 6 consecutive months in 2013 and delivered our treatments within two months after the visit. First, firms were either allocated to receive no email, or one of three different types of information provided by email 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 and is thus highly personalized to the firm's reality. We then follow the firms using administrative data for at least 12 months after the receipt of the information to evaluate the impact of our interventions. The administrative data provide us with information on the number of accidents, the number of workers and the execution of safety training in months following the information intervention. We also obtained the 25th, 50th, 75th and mean of the wage distribution of each firm, the number of workers and the workers' turnover rate in each firm.

Our experiment tries to address four different challenges which may impede firms from undertaking measures to reduce their levels of accidents. 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 simpler 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. It details the main risks being faced by workers in that particular firm and offers solutions to prevent them, such as available contacts and resources at ACHS. Importantly, the pamphlet distributed to workers highlight the same risks detected by the ACHS expert during his visit as the ones reported to the manager in the simplified plan. We can also highlight that this

intervention informs of the visit of the expert and the generation of an action plan to the workers which may make the manager more likely to implement it.

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 with the information to workers clearly being the one with the highest and longest-lasting impact. 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. In that case, we find only significant evidence of a decrease in accidents 6 months after the intervention. On the other hand, providing information directly to the workers lowers accidents in a long-lasting way. 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 0.006 to 0.007 in each of these measures from a base for the control of about 0.05, implying a change of almost 15 percent.[3] We find these results to be robust to the introduction of controls and to differences in the specification.

We find evidence that the two successful interventions in terms of safety 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 workers thanks to training. 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 particularly 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

[3] 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.

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.

Finally, we also find that the impact of the worker's package is particularly important when the firm manager also received the simplified plan. We interpret this as implying that while workers may be able to take some safety decisions themselves, they may need to approval or help of management for others, and that this approval or help is facilitated if the manager has a clear-cut and simple action plan he can quickly understand and act upon. This suggests that workers actions on safety are more effective if managers can readily support and engage with them, pointing again at the importance of informing workers in order to generate change.

Having shown that providing information to workers reduces accidents, we then study what is the impact of that intervention in terms of wages. While we do not have information regarding individual workers' compensation, we have data regarding 4 points of the distribution of wages for each firm in each month. We find evidence that while the safety impacts are more immediate, wage impacts appear 9-12 months after the program was implemented. We observe the full wage distribution moving downward at that moment, decreasing wages by 0.8 percent in firms that received the workers' package compared to those who did not. This would give credential to our hypothesis of asymmetric information since by providing information to the workers, the increased safety would have been able to be "charged" to workers through their wages.

We argue that the change in wage can be used to calculate a compensating differential in an experimental fashion. This is because these firms appear not to have changed the number of employees contemporaneously with the wage fall and not have experienced increased rotation compared to the control. This would allow us to interpret the fall in wages relative to the control as a pricing of the added safety passed on to workers. We find that workers are willing to pay about 1.3 percent (or about 6 $US) to reduce their accident rate in 0.01, or by about 20 percent. This is very different than what would have been obtained from a cross-sectional estimation of the compensating differentials in our setting, where the estimate would have been negative. We find this result particularly interesting since we are not aware of any previous studies that calculated compensating wage differentials in an experimental setting.[4] As a fraction of wages, our estimate is significantly larger than those obtained through hedonic wage regressions (about 5 times those of for example Cousineau et al. 1992, Kuhn and Ruf 2013 and in general above those surveyed by Viscusi and Aldy 2003).[5] This may suggest that, as it has been shown by Konings and Vanormelingen (2015), Dearden et al. (2006) and Conti (2005) in the case of firm training, that firms can use the asymmetric information between workers and firms to reap more benefits from the safety improvement than the workers themselves.

Interpreting these results as compensating differential requires that the only way in which the

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

[5] However, since wages are relatively low in Chile, the monetary value is below most studies in the literature.

treatment affected the wages is through the fall in accidents and that the fall in the wage represents a willingness to pay on the part of workers and not necessarily from other channels. Let us first note that hedonic wage regressions will normally suffer from the same issue. In our case, we may be particularly worried about the fact that the fall in accidents that was generated by the workers' package led to changes in productivity. Nevertheless, we think that these are likely to bias downward our estimates if they are at play since most of the literature has emphasize that there are positive impacts of safety on productivity. Brody et al. (1990) argues that there are numerous costs that must be born when an accident occur while Sobhani et al. (2015) shows that production line costs are higher when the work-related safety is lower. More directly, Gowrisankaran et al. (2015) show that mines that suffer a work-related accident have lower productivity than those who do not. Neumann and Dul (2010) review the existing evidence and argue that interventions that improve the way the production is organize to reduce accidents also improve productivity. Given this evidence, we thus think that it is likely that our estimate is a lower-bound one. If productivity was to actually fall in response to our intervention, we may then interpret our IV as the willingness to pay by the worker and the firm for safety, which we think is still highly relevant.

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. We complement this by providing an estimate of compensating wage differential, not in response to worker's benefits but with respect to safety.

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 work environments. This seems to also be the case in Chile. Finally, Cohn and Wardlaw (2016) shows that financing constraint may play a role in making firms underinvest in safety.

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.

A few, recent studies have explored the impact of inspections on workers' safety trying to attack the problem of causality more carefully. Levine et al. (2012) uses a randomization of these inspections and find that these inspections lowered injury rates by 9.4%. Interestingly, in this case, they found no impact on sales, employment or firm survival. They were unable to measure wages, however. Li and Singleton (2018) use a RD design and find a fall in accident rates and like us, find the biggest impact in firms that were less risky.

There is a nascent but still small literature analyzing 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 Workplace 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 system operates as an insurance system, which is funded by firms paying a monthly premium, determined as a percentage of the taxable wage bill. The contributions to the insurance plan are thus made each month by the employer and provide 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.[6] 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, who 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

[6]Before that year, it was fully dependent on the firm's historical accident rate.

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.

In terms of prevention services, a mandatory minimum percentage of 12 percent of total revenues 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 accidents. The riskier 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 them to receive less attention because of the fixed costs of attending any single firm (travel, administrative work, etc.). As a result, 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 principles behind 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) promote 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" continued 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 engaged with the firm, preferably with the owner, and then carried out a diagnosis of the firm, aided by an IT tool.

The second step of the program was to send, a couple of days after the visit, a "prevention plan" tailored to the company. 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 in heights, working under water, etc.), and 4) a proposed prevention plan (e.g., specific training courses). Coupled with this e-mail, the ACHS also sent safety signs and posters that are to be located in specific places in the firm (in case those were not 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.

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

Our experimental treatments, described in detail in the next section, provide information to either owners/managers or workers of SMEs with the purpose of changing their behavior. The goal was to change the firm and worker behavior, the frequency of prevention and the incidence of accidents.

In this section, we discuss the theoretical mechanisms that might generate different behavior between owners/managers and workers of SMEs in response to the information provided in our treatments. We inform the discussion with knowledge about the institutional details of our setting.

We address two elements. First, we discuss how the information we provide might expand or harness the knowledge that workers and owners already have in the domain of workplace safety. The second, we discuss how the allocation of costs and benefits of safety may lead to the different parties being more incentivized to take preventive actions.

On the first issue, if information is public and all the parties -workers and owners- can process it and act on it without prohibitive costs, then providing information should not change behav-

ior/outcomes. Alternatively, if there is imperfect/incomplete information, which is likely in safety (Pouliakas and Theodossiou, 2013), providing information should matter. In our setting, we argue that this will depend on the knowledge that each party already has. Knowledge about safety tends to be divided in firms. Managers and owners tend to have more information about (and access to) the "context of safety", that is, explicit knowledge about the costs, policies, legal requirements, funding, relevant contacts, prevention agencies, accident performance and the like. Workers on the other hand, possess tacit knowledge about working conditions, production details, operational processes, etc., being better equipped to implement prevention efforts. For workers, safety and prevention information directly relates to their everyday productive activities. For managers/owners, safety information is added to the daily (over) flow of managerial and administrative information.

One consequence of this knowledge distinction is that the challenges in information provision are different. When providing explicit information to managers the challenge is to provide it in the best way possible so that it is actually incorporated, leading to eventual actions. This means, for example, harnessing potential biases in information processing such as addressing mismatch between cost and benefits, ease of the message, or increasing salience by providing comparative information (Thaler and Sunstein, 2008). This is what we execute in our first three treatments, aimed all at SMEs managers/owners.

For workers, the challenge is to provide them with information regarding the safety of their firm, the prevention tools and alternatives provided by ACHS. Given that workers are the best agents for immediate action in terms of safe behavior, the problem is less so about information packaging or framing but about information availability. Again, this is what we aimed with the fourth treatment: we provide practical information about prevention alternatives to all (or most) of the workers of the SMEs that we treat.

A second consequence of this knowledge difference is that the information asymmetry can be exploited by the parties. Managers and owner hold more information about the context of safety (particularly at the hiring stage) and they might choose to selectively report this information to workers. For example, bad performance in safety might be minimized, or be opaquely transmitted, in order to avoid backlash from the workforce. The evidence that employee voice and representation -for example through mandatory joint safety committees- is beneficial for firm safety (e.g., Eaton and Nocerino, 2000) can be interpreted as a signal of underlying information asymmetry problems.

In principle, if incomplete information is assumed, our treatments should equally promote change in behavior among managers and workers. However, asymmetric information would suggest that workers might benefit more and thus, be more responsive to information provision.

Next, we explore how incentives may play a role in this problem. Workers and managers might be facing different incentives, affecting their propensity to take preventive actions. In our setting, incentives are dominated by who is the party that bears the different costs of unsafe workplaces. The costs of accidents are many: production losses, lower worker morale, medical attention, perma-

nent health consequences, psychological distress, inability to continue working, prevention efforts to avoid future accidents, etc.

Many of these costs are financed in Chile by an insurance scheme (lost production, morale and psychological distress are not included in the coverage). The premium is a percentage of the wage bill that is levied on the company. Every other year this percentage gets updated. Half of the rate depends on the safety performance of the firm, and the remaining half on the safety performance of the firm's sector. Given some competition in labor and good markets, this percentage would be largely absorbed by the company. This indicates that firms do bear costs of unsafe workplace and thus they do have incentives to take action. These incentives are boosted when adding the costs of lost production and lower worker morale, which have been estimated to be of a similar size than the premium (Brody et al., 1990). However, the degree to which firms actually internalize these costs is not clear yet. Some studies argue that firms systematically fail to do so (Hopkins, 1999; Pouliakas and Theodossiou, 2013) while other document the opposite (Yakovlev and Sobel, 2010).

Benefits or compensations provided to workers might lead to moral hazard, a dampening of their incentives to act. Although the Chilean system provides medical and financial support to workers that suffer an accident, the benefits are not enough to cover the entirety of the costs experienced by workers, many of which can be long term (Pouliakas and Theodossiou, 2013). This is especially true when compared to standards of developed countries (Brahm et al., 2011). This would suggest that worker moral hazard might not be highly relevant. Workers bear substantial costs beyond what is provided by the insurance system.

Overall, the assessment of distribution of costs and incentives indicates that both owners/managers face incentives to act upon information. The evidence of partial internalization by firms and the lower compensations to workers as compared to developed countries (both on the insurance and on the wage differentials), might suggest that in our setting the workers might be more incentivized than owner/managers. If one adds to the mix the information asymmetry discussed earlier, this possibility becomes more likely as well.

One interesting implication of this discussion is the wage consequence of workers acting on the information. If worker react to their treatment and safety is improved, then the assumption of managers holding more information would translate into a lower wages for workers. As they have information about the stance of the firm in terms if safety, they might be able to (partially) charge the workers for the benefit they are now receiving. There is evidence that compensating wage differential also apply in this direction, not only when safety worsens (Kniesner et al., 2014). This is particularly true in our setting, because workers are not fully compensated by the insurance coverage. Only if workers somehow manage to reduce risk without managers noticing, would they be able to avoid a wage reduction.

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 intervention 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. It is a visual representation of the simplified plan email sent to the firm manager. The bottom of the pamphlet invites the worker to use ACHS resources to lower her risks. They were mailed within the month following the visit, for the first 5 months of the intervention.

In the first case, our stratification strategy included month of visit and region. In the last four months, we were able to include industry as an additional strata. In the second set of randomization, only the month of the visit and the random assignment to the email were employed as strata.

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.[7] 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. This would suggest that a difference in take-up, and thus statistical power, is unlikely to explain the differences in results we obtain.

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. We also obtained information regarding the degree of compliance that the firm had on the subsequent visit of the expert but given that those new visits were sometimes driven by an accident and was only available for a subset of firms, we are not presenting this measure as an outcome. While there may be concerns about the

[7]Specifically, for the simplified plan, we find no significant correlations between firm characteristics and opening rate. For the comparison email, firms with more employees were more likely to open the email. Finally, for the monetarization treatment, larger firms and those with more accidents in the past are more likely to open the email.

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 safety "mutual" over our sample period. However, since fewer than 2 percent of our sample are not continuously reporting to ACHS, we think that we do not have a very serious sample attrition problem.

Finally, we also obtained information regarding wages in these firms. Each month, firms declare the wage of each worker that is subject to the payment of workers' compensation. This does not correspond to the full wage since bonus for food/transportation are not subject to payment of the workers' compensation. Furthermore, the payment is capped for the very top of the distribution but we seem to have very few workers in that range of wages. While we were not able to access the individual wage data, we did obtain for each month and firm, the 25th, 50th and 75th percentile of the wage distribution, in addition to its mean. We also measured the number of workers paid and the fraction of workers that were in the previous month with the firm, to measure turnover.

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 month. 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 average of the firm's average wage was about 455,000 CLP which is about twice the minimum wage and corresponds almost exactly to the average wage of Chilean workers in the economy in that year ($CLP 427,130).

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. We had no wage nor information regarding the number of workers at the moment of the randomization, making it impossible to ensure balance on those variables. Furthermore, we will control for these pre-treatment variables in the regressions to show that the random imbalances in some of the variables is not driving 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 as controls. Each panel corresponds 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 a decrease of 0.006 after two months. 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.[8]

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. 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

[8] Adding controls for previous mean wages only increases the magnitude.

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, 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 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. 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 depended specifically on what one was being compared to and not on 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 [9], 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. 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.

5.2 Channels

We now explore heterogeneity in firms' responses in order to better understand the channels through which the provision of information led to fewer accidents. 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 divided 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 6, 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, we find 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

[9] Results available upon request.

them more responsive to the package.[10] 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 email 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 7 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.

5.3 Wage impacts and compensating wage differentials

Having shown that information provided to workers appear to have lowered accident rates, we now question whether this was accompanied by a change in the wages of workers. As we discussed in Section 3, whether or not wages adjust may help us in understanding where the "market failure" existed previously.

Using the wage data (average and median), we compare treatment and control firms using our same regression but adding previous average wages as part of the controls. We include information on the changes in the wages 6-9 months after the intervention and 10-12 months after it. We do this because there may be wage rigidities in the market that would make it difficult for employers to pass down the cost of the added security immediately after safety has been improved. We also

[10] We tried including a linear control for firm size interacted with the treatment dummy and while the results for the interaction of the treatment dummy with the indicator variable for firms above 20 workers remain of similar magnitude, it loses its significance, preventing us from claiming that there is a discontinuity in exactly 20 workers.

use the log of the average wage instead of the levels but the results are robust to employing instead the level. The results of the regressions are presented in Table 8. 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, except for the monetization information. Not only are the point estimates not statistically significantly different from zero in the case of the two first interventions 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, but only 10 months after the intervention. 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 0.8 percent of the wage or CLP 4,000. It is interesting to note that this arises only 10-12 months after the intervention and not before. This suggests that the wage response required some time to appear.[11]

It is interesting that we observe a similar magnitude in the impact of the median and average wage of the firm. This suggests that it is the full distribution of wages that moved and not just that some workers at the bottom of the distribution saw their wages lowered. 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. The magnitudes are slightly larger for those in the bottom of the distribution but still very similar overall.

Can we use our intervention as an instrument to obtain a causal relationship between workers' safety and wages? To do that, we would require that our intervention only affected the average wages through a change in workers' safety. The main potential violations of our exclusion restriction would be if the intervention changed the wage not through improved safety but by firing/hiring of workers that are different than the initial pool we had. First, we must note that the rotation in this sample is relatively low. In the 12 months before the intervention, an average of 89 percent of workers in a given month were employed the month before in the same firm (with a median even higher). This does seem to be a setting of limited worker turnover. However, the results obtained in Table 8 suggest that firms that randomly received the workers' package increased their workforce by more than half a worker more than those who did not one year after the intervention or about 5 percent of the control workforce. Nevertheless, we think that this is unlikely to explain the wage effect since the increase in the workforce is almost immediate while the fall in the wage takes a fair amount of delay to operate. This is particularly true in Chile since the legal code prevents a firm from lowering nominal wages but does not restrict the wage that one could offer to a new employee. Thus, if our impact on wages was because of the new hiring, one would anticipate that it should occur simultaneously with the hiring and not 6 months later. Secondly, the fact that the full distribution is shifted also seems to go

[11] Controlling for initial wages is key here. Without that control, we would find results earlier and much larger in magnitudes.

against this hypothesis. However, the information provided to workers could have altered the wage distribution by changing the mix of workers within the firms. This would prevent us from using the experimental variation as an instrument. Nevertheless, the last column of Table 8 suggests that this is unlikely to be an explanation. We find that the fraction of current workers that worked the month before in the same firm was not different in those randomly assigned to receive the workers' package than those who were not. We thus argue that our exclusion restriction is likely to hold.

Our next table thus provides an estimate of the relationship between wages and a firm's accident rate. We first run the typical correlation between accidents and wages in our sample. While this is normally done at the level of the worker, we use the average wage in the 12 months prior to our intervention and the accident rate over the same period at the firm level. We find a strong and *negative* correlation between the accident rates and the wage level, suggesting that firms that are less safe are actually paying their workers less.[12] In column (2), we include controls for industrial sectors and find that this negative correlation shrinks substantially but remains statistically significant. We then contrast this traditional approach to calculating compensating wage differentials to our experimental one. In the last two columns of Table 9, we show the IV estimate of the accident rate in the 12 months after the intervention on the log average wage in the months 10-12 after the intervention, instrumented with the randomized assignment to receiving the workers' package. We find now a positive relationship between the two variables and much larger in magnitude than before. The results suggest that workers are willing to give up 1.3 percent of their wage to reduce the accident rate in 0.01, which corresponds to about 20 percent of the baseline accident rate. This is larger than the literature that has usually found a coefficient below 0.5 in hedonic wage regressions similar to this.[13] This would suggest that dangerous jobs have other unobservable characteristics negatively correlated with wages or that workers sort such that those who are the least risk averse select the most dangerous jobs, thus requiring less wage payments as a compensation.

6 Conclusion

Overall, this study has shown that providing information to SMEs can reduce the incidence of work-related injuries but that who to provide this information to appears to be key. There is evidence that closing the informational asymmetry between workers and firms may have substantial benefits in terms of safety. The fact that informing workers led to a safer work environment initially but lower wages afterwards 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 in an experimental setting, and is found to be much larger than the typical hedonic wage estimates.

[12] While surprising, this result is not rare in hedonic wage estimates, see for example Parada-Contzen et al. (2013) and Kuhn and Ruf (2013).

[13] Note, however, that in US dollars, the amounts we are finding are below most of the estimates in the literature because average wages in Chile are much lower than in more developed economies.

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

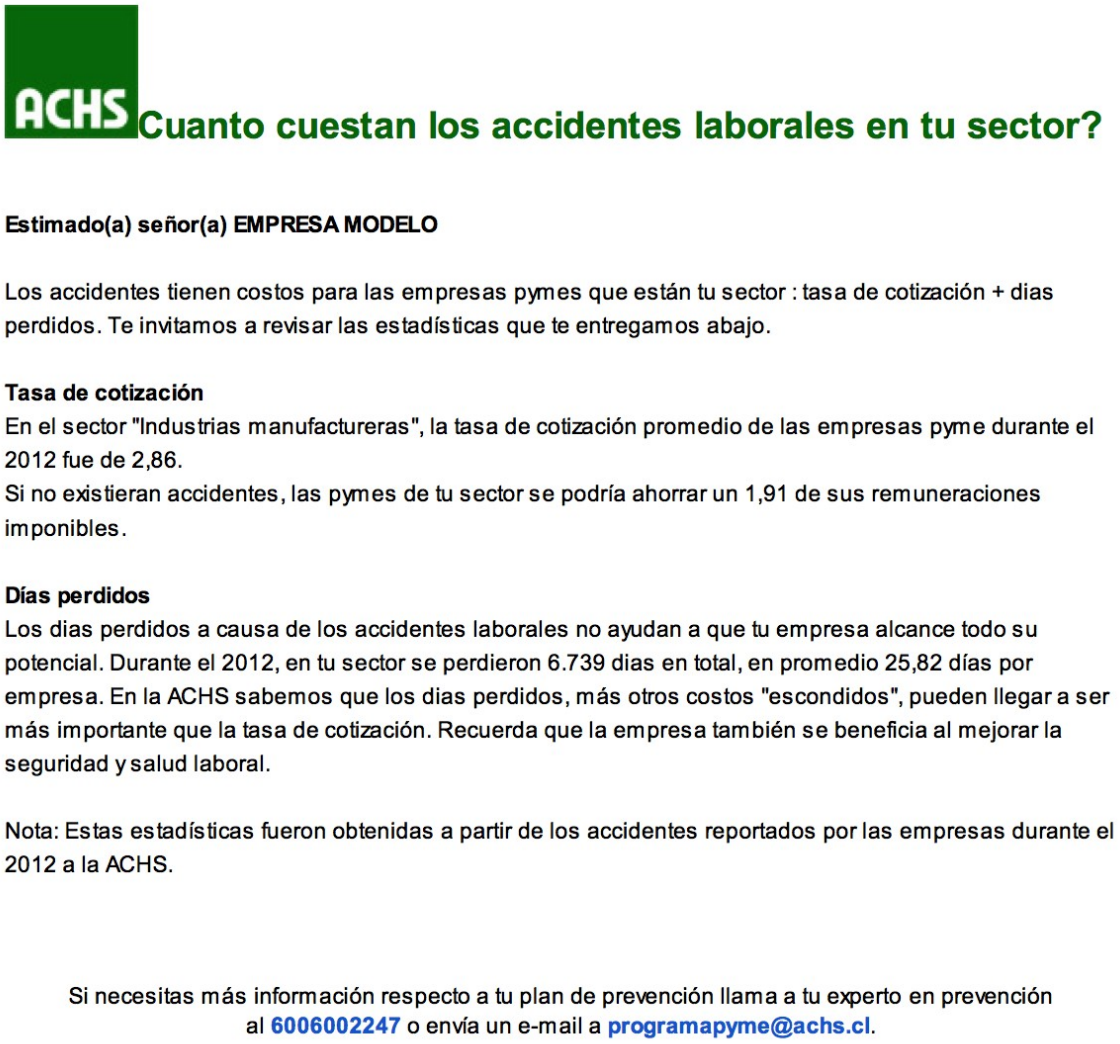
Estimado(a) señor(a) EMPRESA MODELO

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)	(0.0307)	(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)	(0.5007)	(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)	(0.8882)	(0.7856)	(0.8571)	(0.8805)
Days lost per worker, 2012	14731	0.9485	0.0278	-0.1070	-0.0068	-0.0599
		(4.4458)	(0.0881)	(0.0832)	(0.0812)	(0.0724)
Training finished in 2012	16617	0.0339	0.0038	0.0020	-0.0049	0.0039
		(0.1811)	(0.0036)	(0.0036)	(0.0037)	(0.0033)
Average wage 12 months bef.	15475	455731	-3060	2264	1154	12700**
		(283990)	(5385)	(5459)	(5700)	(5001)

*** 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)	Class in 2013 (3)	Class in 2014 (4)
Simplified plan	-0.0070	-0.0026	-0.0056	-0.0025
	(0.0043)	(0.0036)	(0.0043)	(0.0036)
R^2	0.019	0.014	0.093	0.055
N	15,252	15,252	14,803	14,803
Comparison	0.0020	0.0065*	0.0076*	0.0057
	(0.0036)	(0.0038)	(0.0046)	(0.0038)
R^2	0.013	0.010	0.079	0.046
N	14,434	14,434	14,167	14,167
Monetization	-0.0002	-0.0059	0.0010	-0.0055
	(0.0048)	(0.0036)	(0.0047)	(0.0036)
R^2	0.011	0.010	0.079	0.046
N	14,436	14,436	14,169	14,169
Workers' package	0.0105**	0.0056*	0.0087**	0.0051
	(0.0043)	(0.0034)	(0.0042)	(0.0034)
R^2	0.004	0.003	0.083	0.044
N	16,617	16,617	16,145	16,145

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls (for columns (3) to (4) also include the average accident rate and the average number of workers over the 12 months before the intervention, whether the firm implemented a class in 2012 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		Class in 2013	Class in 2014	
Comparison	0.0135**	0.0097**		0.0119**	0.0086**	
	(0.0054)	(0.0044)		(0.0052)	(0.0042)	
Comparison	-0.0181	-0.0098		-0.0156	-0.0081	
*> ind. avg	(0.0120)	(0.0095)		(0.0118)	(0.0092)	
R^2	0.016	0.014		0.065	0.051	
N	13,406	13,406		13,395	13,395	

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls 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. In the bottom panel, a control for whether the firm also undertook classes in 2012 is added. *** p<0.01, ** p<0.05, * p<0.1

Table 6. 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		Class in 2013	Class in 2014	
Treatment	0.0009	-0.0027		0.0068*	0.0046*	
	(0.0039)	(0.0028)		(0.0037)	(0.0028)	
Treatment*	0.0259	0.0424***		0.0041	-0.0075	
$>$ 20 workers	(0.0162)	(0.0141)		(0.0139)	(0.0116)	
R^2	0.102	0.076		0.076	0.057	
N	14,058	14,058		15,921	15,921	

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls 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. In the bottom panel, a control for whether the firm also undertook classes in 2012 is added. *** p<0.01, ** p<0.05, * p<0.1

Table 7. 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 2014		
Treatment	0.0077*	0.0077**		0.0052	-0.0004	
	(0.0046)	(0.0037)		(0.0041)	(0.0033)	
Treatment*	-0.0107	-0.0048		0.0225	0.0181	
Yellow	(0.0239)	(0.0174)		(0.0213)	(0.0158)	
Treatment*	0.0620	-0.0588		-0.0044	0.0742	
Red	(0.1247)	(0.0902)		(0.1294)	(0.1088)	
R^2	0.088	0.067		0.084	0.062	
N	13,547	13,547		14,851	14,851	

Robust standard errors in parentheses. All regressions include dummies for each strata of the randomization. Controls 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. In the bottom panel, a control for whether the firm also undertook classes in 2012 is added. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Impact of information treatments on wage distribution and number of workers

	6-9 months after				10-12 months after			
	Average wage	Median wage	N. workers	% old workers	Average wage	Median wage	N. workers	% old workers
Simplified plan	0.0008	0.0022	-0.8310***	0.0004	0.0010	0.0013	-0.5604**	-0.0011
	(0.0049)	(0.0050)	(0.2568)	(0.0025)	(0.0050)	(0.0051)	(0.2509)	(0.0027)
N	14,028	14,028	14,028	14,028	13,788	13,788	13,788	14,028
Comparison	0.0014	-0.0023	-0.0630	0.0022	0.0030	-0.0005	-0.0997	-0.0004
	(0.0051)	(0.0052)	(0.1770)	(0.0023)	(0.0053)	(0.0055)	(0.1741)	(0.0027)
N	13,446	13,446	13,446	13,446	13,228	13,228	13,228	13,228
Monetization	-0.0066	-0.0053	-0.0896	-0.0027	-0.0077	-0.0096*	-0.2182	0.0022
	((0.0052)	(0.0053)	(0.1851)	(0.0025)	(0.0057)	(0.0058)	(0.1852)	(0.0027)
N	13,448	13,448	13,448	13,448	13,229	13,229	13,229	13,229
Workers' package	0.0004	0.0006	0.6205**	0.0026	-0.0080*	-0.0077*	0.7245***	0.0027
	(0.0044)	(0.0045)	(0.3138)	(0.0020)	(0.0046)	(0.0046)	(0.2512)	(0.0023)
N	15,268	15,268	15,268	15,268	14,993	14,993	14,993	14,993

Robust standard errors in parenthesis. All regressions include dummies for each strata of the randomization. Controls 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, whether the firm implemented classes in 2012 and the average wage in the 12 months before the intervention.*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9. Effect of safety on wages, OLS versus IV

	OLS		IV	
	(1)	(2)	(3)	(4)
Accident rate	-0.238***	-0.131***	1.291*	1.278*
	(0.046)	(0.037)	(0.729)	(0.726)
N	15440	15436	14993	14989

Robust standard errors in parenthesis. All regressions include dummies for strata of the workers' package randomization. Controls include the average number of workers 12 months before the intervention, the legal rating given by the mutual during its last visit, whether the firm implemented classes in 2012. Column (2) and (4) also include fixed effects for industrial sector. The first two columns are a regression of the average wage on the accident rate 12 months before the intervention. The last 2 columns are a 2SLS regression of the wage in the 10-12 months after the intervention on the accident rate in the 12 months following the intervention, instrumented for the assignment to the workers' package. The last two columns also control for the average wage in the 12 months previous to the intervention.