Teaching Labor Laws: Evidence From a Randomized Control Trial in South Africa

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Abstract

We assess whether imperfect knowledge of labor regulation hinders job creation at small and medium-sized firms. We partner with a labor law expert that provides information about labor regulation via newsletters and access to a specialized website. We randomly assign 1800 firms to get access to this service for a 21-week period. Six months later, the average employment level at treatment firms was 12% higher than at control firms. The intervention decreased the perception that labor regulation is a constraint to hiring and increased optimal employment level.

Keywords: Labor demand, Labor laws

JEL Codes: J23, J63, J64, J68.

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1 Introduction

A large literature has been devoted to understanding what hinders the growth of small firms in poor countries. The role of credit market imperfections, regulation and corruption have been widely researched. In more recent years, researchers have turned their attention to other constraints that might be especially binding for smaller firms: informational barriers. A series of recent papers have asked whether the managers and owners of small firms might lack the knowledge that is required to best operate and grow their business. A particular focus of this recent literature has been to assess whether limited literacy about finance and accounting might be a hurdle to the success of small firms in emerging markets. A related branch of this literature has shown that imperfect knowledge of modern management practices may hinder the productivity and growth of businesses. In particular, Bloom et al. (2013) show that providing medium-sized firms with consulting services on management practices resulted in large improvements in productivity; their work further singled out informational barriers as the primary factor explaining the lack of adoption of these superior management practices.[1]

In this paper, we extend this literature by focusing on another input into business decision-making that may also be subject to informational barriers: the knowledge and understanding of the laws and rules that govern a firm's business decisions. Indeed, the same way firms may be held back because of information barriers in the adoption of best management practices or lack of financial knowledge, firms may also be held back because they do not know enough about the legal and regulatory environment to be able to optimize their decisions. While much prior work has documented the regulation of business activity across many areas (such as in the World Bank's Doing Business Reports following the seminal work of Djankov et al. (2002)) and measured how the stringency of this regulation affects economic outcomes, we hypothesize that, independently of stringency, knowledge and understanding of the regulation can also matter for economic outcomes. Because such knowledge requires financial resources and time, we expect that many firms, but especially the smaller ones, may be hindered in their ability to operate efficiently because they only have a limited understanding of the legal context.

Using South Africa as the setting for our investigation, we ask whether the growth of small and medium-sized firms is impeded by a limited knowledge of the rules and laws that govern these firms' interactions with their workers and the labor market at large. Several factors make the South African context particularly relevant. South Africa has been struggling to create jobs, with an unemployment rate above 20% throughout the last quarter century and as high as 27% in 2019.[2]

Also, both the recency and complexity of the regulation of labor in South Africa suggest the possibility that informational gaps may exist. Labor laws in South Africa were rewritten at the end of the Apartheid regime and a set of Acts quickly adopted after 1994 led to profound changes in the regulation of labor. The Labour Relations Act (Act No. 66) of 1995 formed the new basis of labor law and set the new conditions

[1] See J-Pal (2019a,b) for summaries of recent evidence on management practices and business skills programs

[2] See https://data.worldbank.org/indicator/SL.UEM.TOTL.ZS?locations=ZA. This does not include individuals that have stopped looking for work; when these discouraged workers are accounted for, the unemployment rate reaches close to 40%. See for example https://www.biznews.com/briefs/2019/10/29/unemployment-rate-record-youth-lings. Note that South Africa is also characterized by relatively low levels of informality. According to **?**, the share of informal employment in total employment is 34%, the lowest share in Africa (where it averages at 85.8%).

for hiring and firing. Other Acts quickly followed that regulate conditions of employment and employment equity.[3] Two new institutions were also established in 1997: the Commission for Conciliation, Mediation and Arbitration (CCMA), the body in charge of labor dispute resolution, and the Labour Courts, specialist courts with an exclusive labor law jurisdiction. The majority of the cases referred to the CCMA are claims of unfair dismissals and the Labour Courts can review arbitration awards made by the CCMA. While hiring costs (such as the social security and health costs associated with hiring a worker) and firing costs (such as costs of terminating the employment of an individual in terms of notice period requirements and severance pay) are not particularly high in South Africa by international standards, hiring and firing procedures are viewed as quite rigid, as reflected in the data collected by Botero et al. (2004) and the World Bank Doing Business report. In particular, Benjamin (2014) notes that there is widespread perception (particularly amongst employers and within the media) that it is more difficult to dismiss an employee in South Africa than virtually anywhere else in the world. Indeed, labor market regulation is the most commonly cited obstacle to business by firms, especially among small and medium-size firms (Rankin, 2006), and firms are reluctant to increase hiring due to labor regulation (Godfrey, 2007). Yet, as explained by South African labor scholars, employer perceptions about the difficulty to fire and hire workers might be a reflection of their limited information about, and understanding of, the legal context. For example, while it is true that employees in South Africa can easily refer unfair dismissal disputes to the CCMA for arbitration, Bhorat and Cheadle (2009) note that this process is quite efficient, with a large share of referrals quickly resolved through conciliation. While the review of arbitration awards by the Labour Courts is much slower and less efficient, only a small share of cases referred to the CCMA are reviewed in the Labour Courts.[4] Yet these relatively rare cases are heavily publicized, which likely resulted in biased employer perception about the difficulty to fire workers. Other examples of complexity, and possible employer confusion, in South African labor law include the handling of probation periods, the regulation of retrenchments, or rules regarding the use (or misuse) of fixed-term and part-time contracts. Finally, there is anecdotal evidence that an active industry of labor consultancies may have exploited this confusion, and strategically describe the legal requirements as more complex than they truly are as they stand to profit from this confusion (Van Niekerk, 2007; Bendeman, 2006).

To proceed with this study, we partnered in 2013 with a labor law expert in South Africa, the UCT Law@Work Club, that provides information to local firms about major topics regarding labor regulation via newsletters and access to a specialized website. The newsletters are designed to send concise, relevant labor law information and updates that can be quickly read. They also act as a bridge to the website by motivating participants to read more about a given topic on the website or to ask additional labor law questions on the Club's online forum. The website itself contains, among other things, a case law library, a discussion forum, video tutorials and a database of legal template documents (such as employment contracts, disciplinary notices, policy templates and termination agreements).

Sampling from an administrative database, we randomly assigned 1800 small and medium firms to receive

[3]The Basic Conditions of Employment Act (Act No. 75) of 1997 regulates overtime working hours and related pay. The Employment Equity Act (Act No. 55) of 1998 promotes equal opportunity and fair treatment in employment with the purpose of eliminating unfair discrimination and lays out affirmative action rules devoted to ensure equitable representation in the workforce.

[4]According to Bhorat and Cheadle (2009), only 10 percent of arbitration awards end up being reviewed by the Labour Courts.

free access to this information service for a 21-week period. Quizzes included in survey data collected prior to randomization confirmed our hypothesis that the person in charge of human resources at these firms has poor knowledge of labor law. The take-up of the intervention was satisfactory, with only 23.5% of treatment firms for which we record no opening of newsletters or access to the website.

We find that access to the labor regulation information was associated with substantial employment gains. In particular, about six months post-randomization, we find that the average employment level at treatment firms was 12% higher than at control firms, with the biggest absolute gains in employment occurring for permanent workers and workers under fixed-term contracts. We further show that these employment gains occurred throughout the distribution of firm size, even if more precisely estimated among the smallest firms in our sample.

We also provide some evidence on the mechanisms driving this effect. We find that the intervention decreases the percentage of firms that view labor regulation as a constraint to hiring workers, consistent with a lowering of adjustment costs among treated firms. Additional results rule out that a reduction in current adjustment costs is the only channel for the overall employment effects: in particular, we find employment increases even among firms that desired downward adjustment to their workforce at baseline. These findings suggest that the employment gains we observe are more consistent with the intervention increasing optimal employment at treated firms, as firms learn how to more efficiently operate their workforce given the labor laws and/or adjust downwards their perception of future firing costs.

Interestingly, and consistent with the anecdotal evidence above of a labor consultancy business exploiting employers' confusion, we also show that the effect of the intervention appears stronger among firms that contract out all or part of their human resource operations.

Overall, our results suggest that an imperfect knowledge of the legal environment governing firms' relationships with their workers and the labor market may be a previously under-appreciated barrier to firm growth.

Our paper is related to a large literature that has investigated the impact of labor regulation on firm growth. Botero et al. (2004) have described the heterogeneity that exists across countries in labor regulation and showed that a correlation exists between the strength of labor regulation and employment and growth-related outcomes. Many other papers have taken a within-country approach and studied how changes in labor regulation over time affect economic outcomes (see for example Besley and Burgess (2004) for the case of India, or Autor et al. (2004) for the US). The differential regulation of fixed-term and permanent contracts has also received a lot of attention, such as in (Blanchard and Landier, 2002; Kahn, 2007; Cahuc et al., 2016). Most if not all of this prior work assume, even if implicitly, that employers fully understand the regulatory environment and optimally adjust to it. Our work instead takes seriously the possibility that this understanding might be limited, which in itself might affect employment outcomes and firm growth. In this respect our paper also fits within the burgeoning literature about behavioral development economics and firms (Kremer et al., 2019).

As indicated above, our paper is most closely related to a set of recent field experiments that have aimed to assess whether informational imperfections related to finance, accounting or management practices are hindering firm growth. For example, Karlan and Valdivia (2011) have studied the impact of teaching basic

finance concepts to micro-entrepreneurs while Drexler, Fischer and Schoar (2014) have compared the efficacy of providing micro-entrepreneurs with standard training in accounting vs. a simplified rule-of-thumb training that teaches basic financial heuristics. Studies focused on addressing more general informational barriers related to management practices include Bloom et al. (2013), who study the impact of intensive consulting services on the business practices of medium- to large-size firms in the Indian textile industry and Bruhn et al. (2018), who conduct a randomized evaluation of consulting services in which they pair small businesses with a local management consultant for one year.

The rest of the paper proceeds as follows. We describe the intervention in Section 2. The experimental design is discussed in Section 3. Section 4 reports our main results on employment effects, while Sections 5 and 6 investigate mechanisms. We conclude in Section 7.

2 The Intervention

The information intervention includes two main components: biweekly newsletters and access to a labor law website. Both components were implemented and managed by a private partner, the UCT Law@Work Club.[5] Before partnering with us for this study, the Club had around 100 active members on its website and access to the Club's services was priced at R395/month (about USD40/month). Our intervention, which took place in 2013, offered free access to UCT Law@Work Club's content over a 21-week period.

2.1 Newsletters

On Tuesdays and Thursdays of each week of the membership, newsletters were sent electronically by the Club to each participant, for a total of 41 newsletters.

The newsletters were designed to achieve two goals. The first goal was to send concise and relevant labor law information and updates that could be read quickly by the participants. Second, the newsletters acted as a bridge to the Club's website by motivating participants to read more about a given topic on the website or to ask additional labor law questions on the Club's online forum.

Thursday newsletters focused on South African case law and gave labor law advice based on each new case introduced every week. The newsletter summarized in a few short paragraphs the context and the outcome of each case, and provided some key advice related to the case's topic. The newsletter then invited the participant to watch a video tutorial on the Club's website about the case presented by one of the Club's labor law experts. While hiring and firing rules were prominent topics, and especially rulings for the CCMA, Thursday newsletters covered a wide range of labor regulation topics such as ill-discipline (insubordination, consistency, vehicle tracking, racism in the workplace, etc.), rulings from labor acts (occupational health and safety, trade unions, etc.) and employee-employer relations (settlement agreements, charging employees, etc.). Tuesday newsletters were not as tightly tied to labor regulation and focused mainly on human resource management advice summarized in a list of two to six key points that are easy to remember and apply. Topics included

[5] `www.laborlawclub.co.za`. The Law@Work Club does not have any direct affiliation to the University of Cape Town (UCT). The Club pays royalties to UCT for using their name.

employee-employer relations (effective employee feedback, dealing with immediate resignations, mediating difficult workplace conversations, etc.) and recruitment advice (hiring interns, hiring casual staff, interview questions revealing the ideal candidate, etc.).[6] Table C.1 lists all the newsletters sent by the Club, with their titles and topics.

2.2 Website

The second component of the intervention consisted in free private access to the Club's website. The website is organized around seven main sections: i. Acts and Amendments, ii. Case Law Library, iii. Discussion Forum, iv. Learning Centre, v. Templates, vi. Video Tutorials and vii. Weekly Newsletters.

The "Acts and Amendments" section allowed members to browse all relevant South African labor law legislation.[7] The "Case Law Library" section gave members access to a large number of legal cases covering a variety of topics and different types of court (CCMA, Labour Court, Labour Appeal Court, Supreme Court of Appeal, Industrial Court, Metal and Engineering Industries Bargaining Council, etc).[8]

The "Discussion Forum" section offered the most tailored service to the Club's members. Each member could ask any labor law-related question and expect an answer from one of the Club's labor law experts within 24 hours. Discussions on the forum were organized by topic and sub-forum and included questions on, among others, CCMA processes, overtime compensation, drafting of employment contracts, terminations, and unions (see Table C.3). Members could browse the discussions by sub-forum or use the advanced search functionality to search for a specific key word. To post a new topic, a member would have to first assign a sub-forum to it (or leave it uncategorized), enter a subject, a brief description (optional) and write any questions to the Club's labor law experts.

The main objective of the Centre section was to allow members to access from one central location all the resources available on the website about a given topic. The section listed key labor law topics organized in five main categories: employment law, collective labor law, labor disputes, social security and industry specific information (see Table C.4). When clicking on a specific topic, members could access three types of resources: a short definition and description of the topic, the main legal rules applying to this topic as defined by labor legislation, and links to other resources on the website presenting this topic (video tutorials, case law, forum discussions).

The "Templates" section provided members with a database of legal template documents that could be

[6] This combination of newsletters was part of the Law@Work Club's standard service to its members, with the Tuesday newsletters offering some respite from the more technical topics while still promoting usage of the website, which was solely focused on information about labor law and labor regulation. Nevertheless, we cannot formally rule that our results below might be in part driven by the value treated firms found in this human resource management advice.

[7] This section reproduced in full the following acts: Basic Conditions of Employment Act (1997), the Constitution, the Labour Relations Act (1995), the Compensation for Occupational Injuries and Diseases Act (1993), the Occupational Health and Safety Act (1993), the Employment Equity Act (1998), the Skills Development Act (1998), Codes of Good Practice (Arrangement of Working Time, Pregnancy, Disability in the Workplace, HIV/Aids and Employment, Sexual Harassment, Operational Requirements), Proposed Amendments (Basic Conditions of Employment Amendment Bill (2012), Labour Relations Amendment Bill (2012), Employment Equity Amendment Bill (2012)). Note that most of those acts are also publicly available (for instance on the Department of Labour website: `www.labor.gov.za/DOL/legislation`).

[8] The library was organized by categories, as listed in Table C.2. The judgment or award of each case was fully reproduced on the website and could either be viewed directly in this section or downloaded as a PDF document. We note that some of these judgments or rulings were publicly available, for instance on the CCMA website (`www.ccma.org.za/Display.asp?L1=45&L2=154`) or on the Labour Appeal Court website (`www.justice.gov.za/laborcourt/jdgm-lbac/lbac2013.html`).

viewed directly on the website or could be downloaded as Word documents. For some of the templates, the Club displayed a few notes to further explain in which situations the template could be used and if any sector-specific or company-specific information should be added. The list of templates was organized by main categories and subcategories (see Table C.5). In particular, members could download employment contracts, disciplinary notices, policy templates and termination agreements.

Members could find in the "Video tutorials" section all the videos introduced by Thursday newsletters, organized by labor law topic (see topics of Thursday newsletters in Table C.1).

Finally, all Tuesday and Thursday newsletters sent to the Club's members, including newsletters sent before our treatment participants joined the Club, could be viewed in the "Weekly newsletters" section. The newsletters were organized using the same categories as the "Video Tutorials" section.

3 Experimental Design

3.1 Research Sample

The research sample was drawn from the set of firms registered with the Unemployment Insurance Fund (UIF) between 1990 and 2012. Registration with UIF is mandatory for any firm as soon as the firm has at least one employee working for at least one hour in the month. For each firm, the UIF database contains number of employees, industrial sector and contact information.[9]

We targeted small and medium-sized firms with 10 to 300 employees.[10,11] Within this sample, we further focused on firms in manufacturing and trade and excluded those in agriculture, mining and most services. The list of industrial sectors included and excluded from the study can be found in Table B.1. These size and sectoral restrictions resulted in a population of 22,114 firms.

With a target sample size of 1,800 firms in the experiment, we randomly drew a sample of 9,741 firms from this population and randomly divided this sample into five groups of firms which were randomized into the experiment in five successive waves over a four-month period, starting in April 2013.

For each group of firms, we first conducted a baseline survey. Firms within a group were randomly sorted and contacted for the baseline survey up to the point where the number of successful contacts reached roughly 20% of the target sample size. The initial number of firms in each group/wave as well as the number of firms surveyed are reported in Table B.2.

The baseline survey was conducted by phone. The surveyors were instructed to identify and talk to the person in charge of human resources decisions at the firm. Out of 1,523 questionnaire completed, 1,060 were indeed completed by the main person in charge of HR decisions at the firm. The baseline survey was short,

[9]While the information on number of employees can be used as a rough indicator of company size, it cannot be considered an accurate measure of the size of the workforce due to too infrequent updating of the UIF database. The UIF data can therefore not be used to track experimental outcomes. We learned of the existence of superior quality datasets at the Treasury. However, access to these datasets is restricted and despite several attempts, we were unable to obtain access.

[10]Because domestic employers can register their domestic workers to the UIF, we asked UIF to provide us with a database that excluded any employer with less than 10 employees as our intervention was not relevant to domestic employers.

[11]According to the 2003 World Banks Investment Climate Assessment (ICA), a notion survey of South Africa's manufacturing sector, employment at the median firm was 90 employees. Twenty-seven percent of firms in that survey had fewer than 50 employees, 25 percent had between 50 and 99 employees and 19 percent had between 100 and 199 employees (see Rankin (2006)).

with only about 20 questions. Besides collecting firm characteristics, the survey respondents were also asked a few questions to test their knowledge of labor regulation.

At the end of each of the five waves of baseline data collection, we randomly allocated half of the firms to the treatment group and the other half to the control group. Table B.2 reports the randomization date and number of firms in treatment and control for each wave.

3.2 Invitation to Participate and Communication with Treatment Firms

The invitation procedure was implemented by group/wave. For each group, we gave the Club contact details for the firms that were assigned to the treatment group and handed over to the Club, from there on, all the communication with these firms (except for the endline survey).

The Club sent out an invitation email to each of the firms in the treatment group (912 firms). The email was addressed and sent to the person that was interviewed for the baseline survey. The email explained that J-PAL Africa was offering to sponsor a 21-week membership with the UCT Law@Work Club.

The Club subsequently followed up with each participant with up to three phone calls. The purpose of the phone calls was threefold. First, the Club verified that each firm received the invitation email and re-sent the email if that wasn't the case. The Club updated all the contact details information of each participant and tried to replace any general contact email address (e.g. info@company.com) with the participant's own email address. Secondly, the phone calls served a marketing goal: convincing participants to log into the website and engage with its content. Thirdly, as the intervention was proceeding, phone calls were placed to remind participants to browse and use the website's resources and not limit their participation to only reading the newsletters.

Finally, members received an email from the Club on the last Thursday of their 21-week membership notifying them about the end of their sponsored subscription.

3.3 Endline Survey

For each group/wave, a few days after the end of their sponsored 21-week membership with the Club, the research team sent an email to all the firms in that group (treatment and control) inviting them to complete an online endline survey. The research team followed up by phone with firms who hadn't completed the online survey and offered them the option to complete the survey over the phone.

The endline survey had about 30 questions, covering three main types of outcomes: current employment level (including a breakdown of permanent, fixed-term contract and casual/non-contracted workers), perception of labor regulations (as a constraint to increasing and decreasing staff) and knowledge of labor regulation (7 quiz questions, based on topics presented in the newsletters or on the Club's website).

The endline survey was conducted over a 7-month period, starting in September 2013.

Overall, 1,510 firms (82.8% of our sample) completed the endline survey. We also gathered some information as to the reason for non-response as part of the surveying process, and identify two main categories:

probable refusal and probable closure. Overall, we estimate that 11.78% of the firms did not answer the endline survey because of probable refusal and 5.43% because of probable closure.

The response rate to the endline survey was well balanced between treatment and control firms, as seen in the last line of Table 1. Of the 1,510 firms answering the endline survey, only 1,466 answered the three questions our endline employment variable is built upon; however, this pattern of partial response is also balanced between groups. When investigating reasons for non-response, some imbalance does appear, with somewhat fewer firms in the treatment than control group not participating in the endline because of probable closure.[12]

3.4 Empirical Specifications

To measure the causal impact of our intervention, we focus on ITT estimates:

$$y_i = a + bT_i + u_i \tag{1}$$

We estimate this basic regression with robust standard errors on the sample of respondents to our endline survey.

We also estimate a specification that includes covariates. Because the potential set of covariates is large, and to avoid the risk of specification search, we use the *double post lasso procedure* developed in Belloni et al. (2014) to determine covariate selection. We implement this procedure using the iterated lasso procedure developed by the authors in which the penalization is computed iteratively from the data and for which a *stata* command is available (Ahrens et al., 2018):

$$y_i = a + bT_i + selected(x)_i c + u_i \tag{2}$$

We also run regressions in which we interact the treatment variable with a set of variables defining a partition of the sample :

$$y_i = a + b_1 T_i \times Cat_1 + \cdots + b_K T_i \times Cat_K + c_1 Cat_1 + \cdots + c_K Cat_K Cat + selected(x)_i c + u_i \tag{3}$$

in which $Cat_1 + \cdots + Cat_K = 1$. This allows us to estimate treatment effects in the corresponding sub-populations, as well as to test for the homogeneity of impacts with respect to the considered partition ($b_1 = \cdots = b_K$).

3.5 Summary Statistics and Balance

Table 1 reports summary statistics and balance checks for the full sample of 1,824 firms retained in our sample (left panel) as well as for the 1,510 firms that responded to the endline survey (right panel).

[12]The share of firms that did not answer the endline survey for probable closure is significantly larger in the control group (6.69%) than in the treatment group (4.17%).

The treatment coefficients in these balance checks are close to zero and never statistically significant. We cannot reject the assumption of the joint nullity of all these coefficients. The table further reveals that differences in mean baseline characteristics between the full sample and the sub-sample of endline survey respondents are very small.

Examining the baseline characteristics of firms in our study, we see that, according to UIF data, 46.8% of firms have less than 50 employees, 26.3% between 50 and 99 employees, and 26.9% 100 employees or more. Agriculture and manufacturing (25.9%), wholesale and retail trade (24.1%) and construction and mining (20.3%) are the three most prominent sectors in our research sample.

The table also summarizes the share of correct answers to the six basic questions about labor regulation included in the baseline survey. Except for the question related to unfair dismissal (which 86.2% of respondents correctly answered), knowledge of the other labor regulation topics is poor. For example, only 17.8% of respondents provided correct answers to the question related to the conditions for the validity of an employment contract and only 18.2% knew the maximum number of months' salary to pay to an employee in case of unfair dismissal procedure. Overall, these patterns confirm our initial intuition that firms have a limited and inaccurate knowledge of labor regulation.

3.6 Compliance: Usage of Website and Newsletter

During the intervention, the Club collected usage data related to both the newsletters and the website. This data allows us to report on the take-up of the intervention.

First, the Club was able to monitor the opening rates of each newsletter sent to the treatment firms.[13] Secondly, the Club collected data on the activity of the clients on its website. In particular, the Club was able to track "events" such as logging in the website, watching a video tutorial, clicking on a section of the website, clicking on a topic of the forum, posting a message on the forum, or viewing a template.

Figure 1 presents the distribution of the number of "actions" taken across treatment firms, combining newsletter- and website-related activities. As can be seen from the figure, firms' take-up of the intervention was satisfactory, with only 23.5% of treatment firms for which no action was recorded. Further examination shows that 70.6% of employers opened at least one newsletter and 36.7% had at least one event on the website.[14] While participants' engagement with the newsletters was higher on average than with the website, there is a wide dispersion with some firms making intensive use of the website.

[13]However, due to some initial technical limitations, the Club wasn't able to fully monitor the opening rates of the first 28 newsletters, as shown in Table C.1 under the column "All emails tracked?" For these newsletters, the Club could only see if they had been opened using a web mail (Gmail, Yahoo, etc.) but not through an email client (Outlook, Thunderbird, etc.). Therefore the opening rates of these newsletters are very likely underestimated. This technical issue was solved starting with newsletter 29 sent on July 16th, 2013. Our measure of opening rates of the newsletters is therefore different for each group of firms, as 32% of the newsletters sent to group 1 were fully tracked, compared to 46% for group 2, 61% for group 3, 85% for group 4 and 93% for group 5.

[14]The opening rate of newsletters was broadly similar across topics.

4 Main Results

4.1 Employment

Table 2 presents our main results on employment at endline. We define employment as total current staff, which is measured as the sum of permanent staff, fixed-term staff and casual/non-contracted staff, each of these being top coded at the 99 percentile.[15] The upper panel of Table 2 reports results of the estimation of equation 1 while the lower panel reports results from the specification that includes covariates.

We measure employment in level in column (1). The impact of the intervention is large and significant with an ITT coefficient of 11.83. This represents a 15.2% increase compared to the control mean (81.11). When we define employment as change compared to baseline (column (3)), the estimated coefficient is 11.34, very close to the estimated coefficient in column (1). We also consider parallel log specifications in columns (2) and (4) and obtain quantitatively similar results, with employment estimated to be 12 to 13% higher in treatment firms compared to control firms after the intervention.[16]

The lower panel presents results where we add to the baseline specification the set of covariates that were selected following the iterated lasso procedure. Out of the 101 covariates considered, the procedure selected 14 when the outcome variable is measured in level and 11 when it is measured in log. The inclusion of these covariates does not lead to any substantial changes in the estimated coefficients on the treatment variable, but it substantially improves the precision of these estimates in columns (1) and (2). These gains in accuracy are comparable to those obtained when expressing the dependent variable as a change in employment in the basic regression (columns (3) and (4) in the upper panel).[17,18]

To further validate the robustness of the estimated impacts, we examine in Figure 2 the difference in the cumulative distribution between treatment and control firms for the two main outcome variables (total employment at endline and change in employment between endline and baseline). The cumulative distribution for the treatment group is always to the right of the cumulative distribution for the control group, and often outside the confidence interval. We formally test the hypothesis of identical distributions for the treatment and control groups. We use the Wilcoxon-Mann-Whitney rank sum test for which we compute p-values using 10,000 permutations. For both employment level (and change in employment, the p-values are very small (3.27% and 0.84%, respectively) so that we can reject the null hypothesis of identical distributions.

In the upper panel of Appendix Table B.3, we explore heterogeneity of impact with respect to firm size. We primarily run these regressions as an additional robustness check, to corroborate that the relationship we

[15] The specific question firms were asked was "How many of the following types of employees does this business currently have on its staff?" with the different categories of staff being permanent staff, fixed-term staff, casual staff.

[16] To validate inference, we also implemented permutation tests for the level of employment and the change in employment level (column (1) and (3) of Table 2). Out of 10,000, 121 (resp. 22) permutations of assignment provided a Fisher statistic above the one obtained with the true assignment, corresponding to an estimated p-value of 1.21% and 0.22%. The results strongly validated results obtained using asymptotic distribution.

[17] Indeed, when the dependent variable is expressed in change from baseline, the double post lasso procedure does not lead to substantial improvements in precision. In fact, the iterated lasso procedure does not select any variable to add in column (3).

[18] As an additional check we also categorized treatment firms into 3 groups based on the number of actions they took during the intervention:([1,5), [6,15), and 15 or more) and directly examined the correlation between the number of actions taken during the intervention and endline employment. We consider this regression either for the whole sample or the treatment sample (in which case the reference group is the group of firm with 0 actions). We detect a positive relationship between actions taken during the intervention and endline total staff. The test of the joint nullity of the three action variables are only significant, however, when adding the experimental control group to the reference category.

detect is not due to a small subset of firms. But we also hypothesize that smaller firms might benefit more from the intervention than larger ones, as they might be the ones with the poorest prior knowledge of labor regulation. In particular, we estimate equation (3) with a partitioning of the sample according to firm size. We consider three categories of firm size: less than 50 employees, 50 to 99 employees, and 100 employees and more. We observe positive and statistically significant impacts on the two smaller firm size categories. While also positive, the estimated impacts on larger firms is not statistically significant. The estimated coefficients for the smallest and medium-sized firms are very similar, suggesting proportionally similar effects of the intervention for these two categories of firms. Despite the smaller point estimates for the larger firms, we cannot reject the hypothesis of similar proportional impacts across size categories.

An additional robustness check, presented in the lower panel of Appendix Table B.3, consists in reintegrating into the regression sample the firms that did not answer the endline survey for reasons that are likely related to closure. We impute zero (endline) total staff to the firms that did not complete the endline survey because of probable closure and run the same analysis as before, focusing on the specifications where employment is expressed in level. The estimated impacts are even larger in this extended sample of firms.[19]

4.2 Hirings and Dismissals

The endline survey also asks firms about the number of workers who were hired and dismissed during the last 6 months. This corresponds theoretically to the period between randomization and the endline survey. We thus expect results consistent with our previous findings on employment level. The results are reported in Table 3. Columns (1) and (2) present the results for the total number of workers hired over the last 6 months, with column (2) adding control variables following the lasso selection procedure described above. Columns (3) and (4) do the same for the total number of workers dismissed over the last 6 months. Note that dismissals are not meant to include workers who quit or workers whose contract ended and was not renewed; it is therefore an underestimation of the flow of workers who left the firm over the last 6 months. The results we obtain are not consistent with our previous finding. In particular, we do not detect any economic or statistical impact.

These results are puzzling. We explored several explanations as to why we were unable to detect effect on hiring and firing. One explanation relates to the timing of the endline survey. The survey was meant to be implemented 6 months after random assignment and asks about hirings and firings over the last 6 months. However, in practice, the endline surveys were not always completed after 6 months, as seen in Appendix Figure B.3, with a significant share of surveys completed several months after the theoretical 6 months. Thus, part of the 21-week period during which firms were offered access to the website and newsletters is not covered in the empirical last 6 months for a significant share of firms. However, when we restrict the sample to those firms for which the endline survey was completed 6 months after random assignment, we obtain qualitatively similar results to those in Table 3.

[19] Appendix Figure B.2 reproduces Figure 2 on this extended sample. Again, not surprisingly, the differences between the cumulative distributions are more pronounced than before. While the differences between the two distributions in Figure 2 were more pronounced at the top of the distribution for the change in staff specification, starker differences now also appear at the bottom of the distribution. One possible interpretation is that the information provided via the intervention was useful in limiting staff reduction when such reduction had been scheduled; another interpretation is that the information provided may have helped firms in weathering negative shocks without as substantive cuts in employment.

Another explanation is that the hiring and firing variables are measured with errors. First, as already indicated above, the number of workers dismissed is not the same as the number of workers that left the firm for any reason, which would be the variable needed for an accurate mapping between the flow variables and the change in stocks. Second, and most importantly, it seems reasonable to assume that it might be much more difficult for the survey respondents to remember and accurately report the total number of workers hired and fired over a given period of time (the last 6 months) than it is to simply report the number of workers currently employed at the firm. The right panel of Appendix Figure B.3 shows the scatter plot of the change in employment measured by the difference between hiring and firing and the change in employment measured by the difference between total staff at endline and total staff at baseline. The graph clearly shows that there is an attenuation in the change reported using hiring and firing compared to the change in total staff. Relatedly, we also observe an "excess" mass of firms reporting both zero hiring and zero firing over the last 6 months. So, while we unfortunately cannot provide a definitive answer, the evidence points towards measurement error in the two flow variables as the most likely explanation for the findings in Table 3.

5 Adjustment Cost, Optimal Level of Employment and Workforce Composition

In Appendix A, we present a simple theoretical framework that builds on Hamermesh and Pfann (1996) and Cahuc et al. (2014) to examine the likely effects of an improved understanding of labor regulation by employers. In this simple model, firms have a desired employment level that may differ from their actual employment level due to adjustment costs. Actual employment in a given period depends on both this desired employment level and employment level in an initial period. The higher the adjustment costs firms face in the current period, the closer actual and initial employment levels remain and the bigger the (absolute) gap between actual and desired employment levels.

Desired employment level depends on labor productivity and a variety of labor costs. Included in the labor costs is a per worker cost of fulfilling labor regulation requirements. Desired employment level also depends on future adjustment costs. As is well understood (see for example Acemoglu and Angrist (2001)), the risk that a new hire has to be fired in the future is a cost component that firms take into account when deciding about employment level.

Given this set-up, there are two main channels via which the information intervention we perform may affect employment levels at treatment firms. A first channel is via a reduction in *current adjustment costs* (see Panel (b) of Figure B.1). If the intervention gives firms the ability to adjust their workforce at a reduced cost, then treated firms will be closer to their desired level of employment. This channel alone implies that the intervention will have a positive effect on employment at firms that desire an upward adjustment to their workforce at baseline, but a negative effect at firms that desire a downward adjustment to their workforce.

A second channel is what can be loosely referred to as a *profitability* channel and it corresponds to a change in the desired employment level. In particular, we envision that the information provided to the treatment

firms may have increased their desired employment level. This could be because this information allowed firms to more effectively comply with labor regulation[20] or because of a reduction in future adjustment costs. Importantly, under this channel, we would expect even those firms that desire a downward adjustment to their workforce at baseline to experience employment gains compared to their counterfactual in the control group (see Panel (c) of Figure B.1).

The data we have collected at baseline and endline allows us to comment on the relative relevance of these two distinct channels. We present this analysis in Table 4. We first report on how the intervention affected employers' perception of labor regulation as a constraint to increasing or decreasing employment. Employers were asked at endline as to whether they perceive labor regulations as constraints in increasing or decreasing employment: "*Are labor regulations constraining you from decreasing/increasing the staff in this business?*" This question can be viewed as a reasonable proxy for how the intervention may have affected perceived adjustment costs. The shares of control firms agreeing at endline that labor regulations are a constraint in decreasing and increasing staff are 19.43% and 26.14% respectively. Consistent with the first channel discussed above, we find that treated employers are less likely to perceive labor regulations as a constraint to hiring and firing. The reductions are 3.26 and 5.59 percentage points, or 16.8% and 21.4% of the control means, respectively. The effect is however only statistically significant for the hiring margin.

In the bottom panel of Table 4, we assess whether these changes in perception about hiring and firing costs differ across employers with different desired adjustment of their staffing at baseline. In particular, firms were asked at baseline about both their current and optimal staffing levels.[21] While 43.9% of firms do not want to adjust their workforce at baseline, 25.6% desire a downward adjustment while 30.5% desire an upward adjustment (see Table 1). We consider a partitioning of the sample according to this baseline desired level of adjustment of employment and estimate equation (3). Interestingly, and while we cannot reject equality of all treatment coefficients, the impact on the perception of labor regulation as a constraint in decreasing staff is negative and significant only for firms which planned to adjust their staff downward. For the two other categories the impact is negative and non-significant. Symmetrically, when it comes to the perception of labor regulation as a constraint to increase staff, we find a negative and significant coefficient only among firms which desired upward adjustment to their staffing level at baseline.

We first note that the equality of all the treatment coefficients is largely accepted in each of these 5 columns. We therefore are careful in drawing too strong inferences from the point estimate differences we observe across groups. That being said, we find the largest and most precisely estimated effects on endline employment (column (1)) among the firms that desired to adjust their staff upward at baseline. In contrast, the estimated coefficients are small and never significant for firms that do not desire adjustment to their staffing level at

[20] As discussed in section 2, topics covered by the intervention were broader than hiring and firing rules and costs. For example, Table C.1 shows that the newsletters discussed topics such as rules regarding sick leave, overtime work, compensation, health and safety at the workplace, or the disciplining of employees.

[21] The measurement of optimal staffing level was based on answers to the following question: "*What do you think the optimum workforce size is for this business at its current level of operations? That is, how many employees in each of the following categories would you say this business should ideally have on its staff?*". The specified categories of employees were, as for actual employment: workers under permanent contracts, workers under fixed-term contract and casual/non-contracted labor. We define optimal employment level by aggregating answers for these three categories. While a perfect mapping between employers answers and the theoretical concept of a no-adjustment cost situation is unrealistic, we do expect that employers might be able to recognize, and abstract way from, some temporary mis-adjustments in the size of their workforce.

baseline. We however also find a positive effect on employment among the firms that desired to adjust their staffing downwards at baseline. This suggests that firms that wanted to reduce their staff did so less strongly than what they would have done absent the intervention. The positive effect we estimate for these firms speaks against a pure reduction in current adjustment costs explanation for our findings. And indeed, in column (2), we find that the impact on the optimal employment level is positive and significant at the 10% level for these firms and of about the same magnitude as the impact on actual employment. In other words, for this category of firms, the change in actual employment is a reflection of the change in optimal employment.

While the evidence in these first two columns is consistent with the first channel discussed above (reduction in adjustment costs), the findings in columns (3) and (4) strongly speak against this being the sole channel via which the intervention affected employment levels. In particular, the intervention was also associated with an increase in reported optimal staffing level at endline (column (3)), which is consistent with the second channel discussed above (increase in the profitability of labor).[22] Also, and directly inconsistent with the view that the employment effects are solely a reflection of a reduction in current adjustment costs, the intervention was associated with increases in actual staffing not solely at firms that desired upward adjustment of their workforce at baseline but also at firms that desired downward adjustment (column (4), bottom panel).

Another way to assess the relevance of the two potential mechanisms underlying our main findings is to look at quantile treatment effects. The results are presented in Figure B.4. The upper panel presents results for change in employment when the sample is limited to endline survey respondents while the lower panel also includes firms that did not complete the endline survey due to probable closure. Large positive impacts are detected at the top of the distribution. Moreover, the figures do not show any negative quantile treatment effect. For example, quantile treatment effects tend to be positive rather than negative at the bottom of the distribution, a finding that strengthens when firms which are likely to have closed down are reintroduced.

In the last column of Table 4, we assess how the intervention impacted knowledge of labor regulation, where knowledge is proxied for based on 7 questions asked in the endline survey.[23] We computed a firm-specific knowledge score defined as the proportion of good answers (rescaled to belong to [0,100]). Of course, one has to be careful in drawing too strong a conclusion from this knowledge proxy given that labor regulation is a very large domain and the set of questions asked at endline was limited. We do not observe that the intervention increased the knowledge of labor regulation among the firms. The coefficient is very small compared to the control mean and not significant at the 5% level. Nevertheless, we do observe that the intervention appeared to have increased the knowledge of labor regulation among the firms for which we find the largest (based on point estimates) employment gains at endline: the firms that desired to increase their workforce at baseline.

While not incorporated in our conceptual framework, better information about labor regulation may also

[22] We set to missing the optimal staff level as long as at least one of the three underlying items is missing. We end up with a sample of 1450 firms for this analysis, compared to 1466 firms for the prior analysis.

[23] The seven knowledge questions included in the endline quiz are: Q1: An employee who earns above the earnings threshold has no legal right to claim overtime; Q2: What is the annual earnings threshold, as defined by the Basic Conditions of Employment Act?; Q3: Health and safety representatives in the workplace are elected solely by the employer; Q4: When is maternity leave effective from?; Q5: What is the maximum number of months' salary that can be awarded to an employee as compensation for an unfair dismissal?; Q6: According to the BCEA how much times of the normal wage must the remuneration for over time be? Q7: An employee filling in for an absentee staff member for more than 6 months falls under the Temporary Employment Service.

lead to substitution between different types of labor contracts. Recall that our main findings are with regard to the sum of workers under permanent, fixed term and casual/non-contracted.[24] In Table 5, we re-estimate the impact of the intervention, but break down total staff into workers employed under these different contract types. Employment levels increase for all contract types, even though the estimated effects are not always statistically significant. The largest impact is obtained for workers under permanent contract, for which the estimated coefficient is 6.3 (upper panel), or about a 10% increase compared to the control mean (65.3). There is also a positive and statistically significant impact on fixed-term staff (3.5), which corresponds to a larger proportional increase (about 25%) compared to the control mean. The point estimate suggests an addition of 4.3 workers to the casual workforce, which corresponds to about a 26% increase to the control mean. Overall, we do not observe much evidence that the intervention led to substitution effects across types of contracts (right panel). There is a small insignificant reduction in the share of workers with permanent and fixed-term contracts and a small increase in the share of casual staff (significant at the 5% level). We weakly reject a test of the joint nullity of the impacts on shares (carried out using just the two first shares) in the regression without covariates and accept it in the regression with covariates. In summary, while there might be a small adjustment in the composition of the workforce post intervention, the large increase we observe in total staffing is not related to any specific contractual arrangement.

6 Additional Heterogeneity Analysis: Baseline Knowledge and External HR Support

In this final section, we assess heterogeneity of the main effects we have uncovered based on two other firm attributes: their baseline knowledge of labor regulation and whether or not the firms rely on external services for the management of their human resources needs. The results are reported in Panels A and B of Table 6.

Using the information collected at baseline, we construct for each firm in our sample variables that proxy for 1) lack of knowledge of labor regulation, 2) pro-employer bias in knowledge (i.e. the firm believes that the labor regulation is more pro-employer than it actually is) and 3) pro-employee bias in knowledge (i.e. the firm believes that the labor regulation is more pro-employee than it actually is).[25]

We hypothesize that our information intervention might be particularly useful to firms that lack knowledge of labor regulation if such lack of knowledge impedes firms' ability to maximize the profitability of their workforce. Also, firms that believe that labor regulation is more pro-employee than it actually is may be particularly induced to increase their hiring when faced with more accurate information; symmetrically, firms that believe that the labor regulation is more favorable to employers than it actually is may be induced to hire less. As before, an important caveat is that labor regulation is a vast domain and the measurement of

[24] This last category was intentionally kept broad so that firms can include non-contracted workers in their count without having to provide an exact number.

[25] For example, the baseline questionnaire asks firms about compensation to be paid to employees for unfair dismissals. The precise question is: *The maximum number of months salary that can be awarded to an employee as compensation for an unfair dismissal is: a) 6 months b) 12 months c) 18 months d) 24 months e) Don't know.* The correct answer is b). We define firms as having a pro-employer bias in their perception of labor laws if they answered a) and a pro-employee bias if they answered c) or d). We were able to measure such bias for 4 out of the 6 knowledge questions in our baseline questionnaire.

incomplete and/or biased knowledge above is based on a very limited number of questions.

Overall, we do not find evidence of much heterogeneity in the impact of the intervention based on baseline knowledge. However, the few statistically significant interaction terms that emerge are consistent with our priors. In particular, we find that a pro-employer bias in baseline knowledge is associated with a smaller impact of the intervention on both actual staffing and optimal employment level at endline.

Finally, in Panel B of Table 6, we assess heterogeneity of impact based on whether or not a firm reports using external services for the management of its human resources. In particular, firms were asked the following question in the endline survey: "*Has this business ever been a member of an employer organization OR contracted the services of an external HR consulting company OR made use of a labor lawyer?*" We built a dummy variable that we call "External HR" that equals 1 for firms that answered "yes" to this question. While the question was unfortunately only asked at endline, there is no difference in reliance on such "external HR" between treatment and control firms: 63.4% in the treatment group and 64.4% in the control group report relying on external HR and the difference between these two averages is not statistically significant. We therefore cautiously proceed in studying heterogeneity of effects across firms based on this variable.[26]

There are several reasons as to why we might expect differential effects based on firms' reliance on external HR, with opposite implications for the sign of the interaction term. On the one hand, firms that rely on external HR may have already paid for access to all the services and information provided by the Law@Work Club. Hence, we may expect smaller impacts for these firms as the information provided to them is more likely to be redundant. On the other hand, firms that rely on external HR may do so because they are particularly uninformed about labor regulation. In this case, reliance on external HR might be a proxy for poor and incomplete knowledge about labor regulation, and we might expect a larger effect for these firms. Furthermore, it is possible that external HR consultants may find it strategically beneficial to exaggerate the complexity and stringency of labor regulations as a way to justify the value of their services to customers. In this case again, we would expect a larger effect of the intervention for the firms that rely on external support, as these firms gain a more truthful and objective understanding of the regulatory landscape via the intervention.

For all but one outcome variable, we cannot reject the hypothesis of similar impacts across the two categories of firms. However, we find that employment impact is larger and only significant for firms which outsource at least part of the HR function. It also appears that the intervention only significantly reduced perceptions of how difficult the law makes it to hire and fire workers among firms that use external HR services. On the other hand, any measurable impact on the knowledge of labor regulation appears concentrated among firms that do not have external support. With the exception of this last result, the patterns in Panel B of Table 6 strongly suggest larger intervention effects among firms that relied on external HR services. While reliance on external support might be a proxy for poor internal knowledge, another interpretation for this finding, as suggested above, is that external providers of HR services find it in their financial interest to paint

[26]To check further the independence of our HR variable with treatment, we ran the iterated lasso procedure, seeking to explain the "External HR" variable using our broad set of covariates described in section 3.4 but also adding the full set of these variables interacted with the treatment variable. This leads to a very large set of potential covariates (the total number is close to 200) of which just one was selected by the iterated lasso procedure (the first of the 11 size variables). Importantly, none of the variables interacted with the treatment variable was selected. This somewhat improves our confidence in studying heterogeneity along this dimension despite it being measured at endline.

an unduly dark picture of the labor regulation environment in order to both attract more business as well as charge higher fees for their services.[27]

7 Conclusion

In this paper, we have shown that providing firms with information about the laws and rules that shape their interactions with their workers and the labor market at large resulted in large employment gains at these firms. While our study is limited in its ability to dive deeply into mechanisms, our results suggest that the intervention's success was in large part driven by an increase in desired employment level at treatment firms: a better knowledge of the regulation of labor appears to have resulted in the rise of the marginal product of labor. With the intervention costing only about $200 per firm, its cost per job created is extremely low (less than $20).

While it may appear ex-post obvious that a poor knowledge of the legal and regulatory environment may prevent firms from making the most out of their inputs, our paper is among the first to causally test this hypothesis and suggest that these types of informational barriers might be as relevant to (low) firm growth as the more commonly studied informational barriers.

Future work should consider replicating and expanding on these findings. One weakness of our experiment is its sole reliance on survey data to track the key outcomes of interest; ideally, our findings should be replicated in a context where administrative data is of sufficiently high quality to be used for measurement. Replication in other emerging markets would also be extremely valuable: South Africa might not be representative in that its labor regulation was subject to a massive overhaul post-Apartheid, which may make the informational imperfections we focus on particularly acute there. Future work should also be dedicated to more thoroughly unpacking mechanisms. In particular, our study falls short of assessing which elements of the multi-faceted suite of informational services provided by our labor law partner was particularly helpful to firms. Also, while we uncover interesting heterogeneity related to the reliance on external consultants, we unfortunately cannot fully explain this heterogeneity. Additional research is needed to understand how these intermediaries operate and, in particular, whether they strategically try to keep the rules of the game as murky as possible to drum up business. Finally, while our paper has focused on the labor domain, future research may also consider other legal and regulatory domains where poor knowledge may be a barrier to the profitability and growth of small and medium-sized firms.

[27]The use of external HR among firms depends on firm size: respectively 59%, 66.3% and 69.5% in firms that report at baseline respectively less than 50, between 50 and 99 and more than 100 employees. To check the robustness of the results in the bottom panel of Table 6, we have introduced the size categories and the size categories interacted with the treatment variable as additional variables. The previous results on external HR are robust to these additional controls in the treatment heterogeneity function (not reported).

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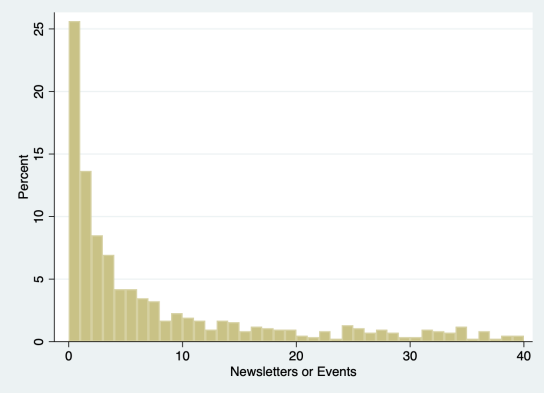
8 Figure and Tables

Figure 1: Take-up of the experiment

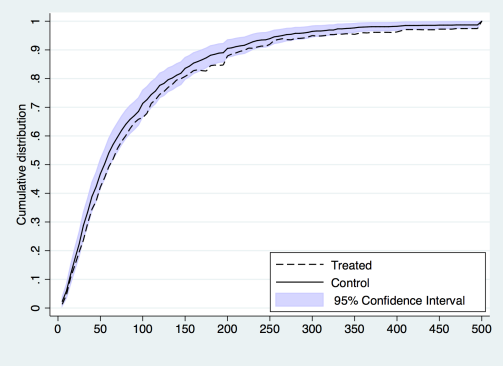


Share of firms with at least one action: 76.5%

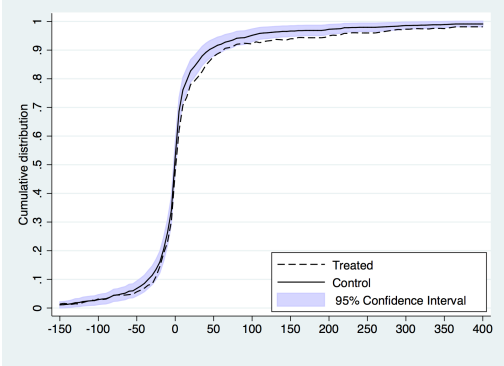
Histogram of the number of actions taken across treatment firms. The actions counted are either the opening of a newsletter or a connection to the website.

Figure 2: Impact on cumulative distribution and rank sum tests on employment – endline levels and endline-baseline changes

Endline



Change Endline–baseline



Mann Whitney test: p values obtained from 10,000 permutations within strata

$p = 327/10000$ $p = 84/10000$

For an outcome variable y we define the dummy variable $d_x = 1(y \leqslant x)$, with x the value on the x-axis. The solid and dotted lines in the graph report averages in the control and treatment groups (c_x and t_x). The shaded area corresponds to the confidence interval with bounds $c_x \pm 1.96 s_x$, where s_x is the standard error of their difference.

Mann Whitney test p values: proportion of pseudo random draw of the treatment variable out of 10,000 for which the statistic of the ranksum test is above the one computed with the true assignment

Table 1: Balancing on baseline data

	Whole sample			Endline respondent		
	Control mean	Difference	Significant difference	Control mean	Difference	Significant difference
Agriculture and manufacturing	25.9	-0.0	.	26.8	-1.9	.
Construction and mining	20.3	-0.3	.	19.8	0.4	.
Wholesale and Retail Trade	24.1	-0.7	.	23.1	0.7	.
Transport Storage and Communication	8.9	0.4	.	9.0	0.6	.
Restaurant Hospitality Services	14.1	-1.4	.	14.2	-1.5	.
Financial Insurance and Business	6.7	2.0	.	7.0	1.7	.
groupe 1	20.8	0.2	.	20.6	0.2	.
groupe 2	21.3	0.8	.	20.7	1.5	.
groupe 3	23.9	-0.4	.	25.1	-2.4	.
groupe 4	23.7	-0.4	.	22.3	1.6	.
groupe 5	10.3	-0.1	.	11.3	-0.9	.
size < 50	46.8	-0.7	.	45.8	-0.9	.
$50 \leqslant size \leqslant 110$	26.3	0.5	.	26.3	1.2	.
size > 100	26.9	0.1	.	27.9	-0.3	.
Average knowledge score	43.9	0.7	.	44.7	-0.0	.
(1) Contract validity	17.8	2.7	.	19.3	0.7	.
(2) Disciplinary procedure	42.9	-0.8	.	44.1	-1.4	.
(3) Unfair dismissal	86.2	1.1	.	85.8	1.9	.
(4) Sanction for fraud	50.1	0.8	.	51.3	-1.2	.
(5) Definition of incapacity	48.0	0.3	.	48.9	0.1	.
(6) Compensation unfair dismissal	18.2	-0.2	.	18.9	-0.2	.
Negative adjustment < -10	14.8	-2.9	*	14.7	-2.5	.
Negative adjustment $\geqslant -10$	13.4	-1.2	.	13.1	-1.0	.
No adjustment	41.2	2.9	.	41.4	2.6	.
Positive adjustment < 10	14.1	1.2	.	14.6	0.8	.
Positive adjustment $\geqslant 10$	16.4	-0.0	.	16.1	0.0	.
Share member of union	21.6	1.9	.	21.2	2.2	.
Share of permanent staff	76.4	0.1	.	76.9	-1.5	.
Share of casual staff	16.3	0.4	.	16.6	0.9	.
Share of casual staff	7.3	-0.4	.	6.5	0.6	.
Share of staff left (in 12 months)	19.7	2.8	.	17.5	5.8	.
Share of staff hired (in 12 months)	35.5	-1.1	.	32.7	1.4	.
Share of permanent staff hired	14.2	-0.5	.	12.9	0.7	.
Total staff	78.0	-0.2	.	79.2	0.2	.
Completed endline questionnaire	82.57	0.44	.			
# observations and p-value of joint test	1824	0.978	.	1510	0.881	.

The table reports the control mean average as well as the regression coefficient (labeled as "Difference") of the characteristic on the treatment variable using equation 1.

The knowledge variables correspond to the following questions: (1) Asks about conditions for the validity of an employment contract; (2) Asks about the standard notice period that must be given to an employee to prepare for a disciplinary inquiry; (3) Asks whether a dismissal will be unfair in case an employer is unable to prove that the dismissal of an employee is related to his/her conduct, capacity, or operational requirements; (4) Asks about the appropriate sanction in case an employee commits fraud; (5) Asks the definition of the " Incapacity" of an employee; and (6) Asks the maximum number of months salary that can be awarded to an employee as compensation for an unfair dismissal.

Adjustment is defined based on the difference between optimal and actual staff

Table 2: Impact on employment – endline levels and endline-baseline changes

	Endline		Endline–baseline	
	Level	Log	Level	Log
	(1)	(2)	(3)	(4)
Without control variables				
Treated	11.83**	0.13**	11.36***	0.13***
	(4.71)	(0.06)	(3.71)	(0.04)
Randomization Inference based on 10,000 replications				
p-value	0.0121		0.0022	
With control variables				
Treated	11.97***	0.12***	11.36***	0.13***
	(3.74)	(0.04)	(3.71)	(0.04)
Control mean	81.11	3.92	2.13	-0.07
Observations	1466	1466	1466	1466

Upper panel: estimation of equation (1). Lower panel: estimation of equation (2), adding covariates following the procedure described in Section 3.4
Column (1) and (2) consider endline total staff, columns (3) and (4) endline – baseline changes
Randomisation inference p-value: proportion of pseudo random draw of the treatment variable out of 10,000 for which the Fisher test statistic of a zero effect is above the one computed with the true assignment
Robust standard errors in parenthesis, * corresponds to significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 3: Impact on hiring and dismissal over the last 6 months preceding endline survey

	Hired		Dismissed	
	(1)	(2)	(3)	(4)
Treated	1.17	1.06	0.84	0.81
	(1.26)	(1.20)	(0.67)	(0.66)
With control variables	No	Yes	No	Yes
Control mean	13.39		5.86	
Observations	1443		1454	

Column (1) and (3): estimation of equation (1). Column (2) and (4): estimation of equation (2), adding covariates following the procedure described in Section 3.4.
Hired: sum of answers of question "How many of the following types of employees joined this business (i.e. were hired) in the last 6 months?". Types are: Permanent staff, Fixed-term contract staff, Casual/non-contracted staff
Dismissed: same as above for the question "How many of the following types of employees were dismissed in the last 6 months?".
Robust standard errors in parenthesis. * corresponds to significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 4: Impact on optimal level of employment and perception of labor regulation

	Are LR constraining you from Decreasing staff (1)	Are LR constraining you from Increasing staff (2)	Optimal staff level (3)	Actual staff level (4)	Knowledge score (5)
Without control variables					
Treated	-3.26	-5.23**	9.38*	11.83**	1.29
	(1.98)	(2.19)	(4.79)	(4.71)	(1.02)
With control variables					
Treated	-3.26	-5.59**	9.72**	11.97***	1.37
	(1.98)	(2.18)	(3.92)	(3.74)	(0.94)
As function of desired adjustment at baseline					
< 0	-7.80**	0.42	13.26*	15.07*	-1.31
	(3.98)	(4.31)	(7.59)	(7.82)	(1.82)
$= 0$	-2.45	-6.25*	2.91	7.03	1.78
	(3.00)	(3.27)	(5.75)	(5.57)	(1.44)
> 0	-0.31	-9.61**	15.22**	15.62**	3.07*
	(3.51)	(4.01)	(7.52)	(6.67)	(1.70)
p-value global	0.21	0.02	0.06	0.01	0.15
p-value same	0.35	0.22	0.35	0.54	0.20
Control mean	19.43	26.14	82.43	81.11	46.08
Observations	1489	1497	1450	1466	1510

Upper panel: estimation of equation (1). Lower panel: estimation of equation (3) with interacted variables built from desired adjustment at baseline defined as the difference between the "optimal" and actual level of employment

Dependent variable in column (1) (and symmetrically column (2)) is based on the answer "yes/no" to the question "Are labour regulations (LR) constraining you from decreasing the staff in this business"

Optimal level of employment in column (3) is the sum of the answers to the question "What do you think the optimum workforce size is for this business at its current level of operations? That is, how many employees in each of the following categories would you say this business should ideally have on its staff" for the three categories of staff: permanent staff, fixed term staff, casual/non-contracted staff.

The knowledge score in column (5) is defined as the proportion of good answers out of seven question of a mini-quiz on labor regulation. See footnote 23 for the list of questions.

Robust standard errors in parenthesis. * corresponds to significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 5: Impact on employment by type of contract

	Levels			As share of total current staff		
	Permanent	Fixed term	Casual/ Non-contracted	Permanent	Fixed term	Casual/ Non-contracted
	(1)	(2)	(3)	(4)	(5)	(6)
Without control variables						
Treated	6.28	3.52**	1.27*	-1.03	-0.56	1.59**
	(4.25)	(1.72)	(0.76)	(1.48)	(1.36)	(0.70)
p-value joint nullity		0.050			0.072	
With control variables						
Treated	8.27**	2.84*	1.14	-0.36	-1.09	1.36**
	(3.43)	(1.53)	(0.74)	(1.29)	(1.23)	(0.66)
p-value joint nullity		0.025			0.092	
Control mean	65.29	12.89	4.35	79.66	15.39	4.95
Observations	1503	1483	1480	1466	1466	1466

Upper panel: estimation of equation (1). Lower panel: estimation of equation (2), adding covariates following the procedure described in Section 3.4

Disaggregation of total staff at endline into the three main categories: permanent staff, fixed term staff and casual/"non contracted" staff. Column (1) to (3) levels, columns (4) to (6) shares in total staff.

Sample sizes differ across regressions due to partial non response to the staffing questions by type of contract.

p-value joint nullity: joint test of null effects on each of the three estimated impacts (in levels or shares).

Table 6: Heterogeneity of impact with respect to baseline knowledge and use of external human resources services

	Are LR constraining you from				
	Decreasing staff	Increasing staff	Optimal staff level	Actual staff level	Knowledge score
	(1)	(2)	(3)	(4)	(5)
Knowledge at baseline (share among answers)					
Treated	-2.84	-5.60**	9.65**	11.99***	1.34
	(1.99)	(2.19)	(3.93)	(3.77)	(0.94)
Do not know	-0.12	2.12	1.27	1.65	1.05
	(2.10)	(2.36)	(4.34)	(4.08)	(1.01)
Pro-employer bias	0.22	-2.03	-6.96*	-6.75*	0.41
	(2.26)	(2.41)	(4.19)	(3.95)	(1.03)
Pro-employee bias	-3.21	-1.70	-1.94	-1.41	-1.11
	(2.01)	(2.30)	(4.21)	(3.98)	(0.98)
p-value global	0.27	0.04	0.03	0.01	0.25
p-value same	0.41	0.41	0.21	0.18	0.38
External human resources services					
With	-4.68*	-7.68***	12.50**	14.02***	0.21
	(2.63)	(2.81)	(5.01)	(4.63)	(1.17)
Without	-0.47	-2.02	5.16	9.52	3.61**
	(2.87)	(3.40)	(6.37)	(6.44)	(1.60)
p-value global	0.20	0.02	0.03	0.00	0.08
p-value same	0.28	0.20	0.37	0.57	0.09
Control mean	19.46	26.17	82.40	81.08	46.07
Observations	1486	1494	1447	1463	1507

Dependent variables: see table 5. Upper and lower panel: estimation of equation (3).

Upper panel uses as interacted variables the direction of errors for a set of questions from the baseline mini-quizz on knowledge of labor regulation. We identify correct answers (omitted category), errors made in a pro-employer (alternatively pro-employee) direction and whether the respondent says he/she does not know (see footnote 25) for a precise example. We compute individual shares and standardize them over the sample.

The lower panel uses as interacted variables the answer yes/no to the question "Has this business ever been a member of an employer organization OR contracted the services of an external HR consulting company OR made use of a labour lawyer". The interacted variable comes from the endline survey (see footnote 26).

p-value global corresponds to the p-value of the test of the joint nullity of the coefficients of the interacted partitioning variables.

p-value same corresponds to the p-value of the test of the equality of the coefficients of the interacted partitioning variables.

A Appendix: Conceptual framework

Consider a firm planning its employment level for period 1, as well as for future periods $t > 1$. Assume that the firm starts from an employment level of n_0. The firm's production function at date t is $f_t(n_t)$, and is subject to random demand and technology shocks [MB: IID should we be more specific?]. The firm's wage bill at date t is $(w_t + cr_t)n_t$, with cr_t a parameter representing the per worker cost of fulfilling labor regulation requirements.

Adjustment in the level of employment from $t-1$ to t has a cost $c(n_t, n_{t-1}, t)$. We assume that this adjustment cost function is piece-wise linear and involves two parameters: cd_t and cu_t, for downward and upward adjustments respectively (see Hamermesh and Pfann (1996)):

$$c(n_t, n_{t-1}, t) = cd_t|n_t - n_{t-1}|1(n_t - n_{t-1} < 0) + cu_t|n_t - n_{t-1}|1(n_t - n_{t-1} > 0) \tag{4}$$

The value of the firm at date 1 can be written as:

$$V(\underline{n}_1, n_0) = E_t\left(\sum_{t=1}^{\infty} \beta^{t-1}\left(f_t(n_t) - (w_t + cr_t)n_t - c_t(n_t, n_{t-1})\right)\right) \tag{5}$$

[MB: t argument missing in adjustment cost?]

where β is the actualization rate.

The first order condition for value-maximizing employment at date 1 is given by:

$$\frac{\partial f_1}{\partial n}(n_1) - (w_1 + cr_1) + cd_1 1(n_1 < n_0) - cu_1 1(n_1 > n_0) - \beta cd_2 Pd + \beta cu_2 Pu = 0 \tag{6}$$

where Pd and Pu are the probabilities of future downward and upward adjustments, respectively.[28]

Finally, it is useful to define n^* as the optimal employment level at date 1 under the assumption of zero adjustment costs at date 1. The first order condition for n^* is given by:

$$\frac{\partial f_1}{\partial n}(n^*) - (w_1 + cr_1) - \beta cd_2 Pd + \beta cu_2 Pu = 0 \tag{7}$$

We can loosely refer to shifts in n^* caused by the intervention as a *profitability channel* as n^* does not depend on the current employment situation n_0 or current adjustment cost parameters cd_1 and cu_1; n^* however depends on the cost of labor regulation parameter at date 1, cr_1, as well as all future parameters (cr_t, cd_t, cu_t) for $t > 1$. For example, a better understanding of labor regulation would translate into a reduction in cr_1 and increase n^*. Also, changes in future adjustment costs will also impact n^*. For example an intuitive and important potential mechanism is related to future downward adjustment cost cd_2. In the decision about current employment, absent any current adjustment costs, a firm integrates the risk, and related costs, that

[28] Fully solving this model is complex as $Pd = P(n2 < n_1)$ and $Pu = P(n2 > n1)$ are both endogenous. See Bertola (1992); Bentolila and Saint-Paul (1994) and Cahuc et al. (2014) for a discussion. [MB: do we need this?] The literature has shown that when the future state of the economy is uncertain, i.e. there is a non-zero probability that current hires will have to be laid off in the future, a reduction in firing costs can have a positive impact on employment. This result, however, depends heavily on the technology of production and the type of uncertainty.

new hires will have to be fired in the future. A reduction in future firing costs will thus result in an increase in n^*. It is worth noting that a similar but opposite side effect exists for a change in cu_2: everything else the same, lower hiring costs in the future give firms an incentive to hire less today.

Costs of labor regulation cr or adjustment parameters cd and cu at later dates also have an impact but it is a less direct one. For example cr_2 has an impact on n_1 but it is through the impact it has on the distribution of n_2, which is a random variable at date 1, and more precisely through the probabilities of upward and downward adjustment. The distribution of n_2 also depends on the distribution of random terms in the future. The impact of cr_2 (and other future parameters) is likely to be small if the related variability is large.

We also consider another channel solely related to a change in *current adjustment costs*, i.e. a change in cd_1 and/or cu_1, with all other parameters (cr_1 and (cr_t, cd_t, cu_t) for $t > 1$) left unchanged.

If we approximate the function $\partial f_1/\partial n(n) - (w_1 + cr_1) - \beta cd_2 Pd + \beta cu_2 Pu$ around n^* as $-\phi(n - n^*)$, we can rewrite the first order condition in equation 6 as:

$$-\phi(n - n^*) + cd_1 1(n - n_0 < 0) - cu_1 1(n - n_0 > 0) = 0 \tag{8}$$

The adjustment rule is very simple and is described in Panel (a) of Figure B.1:

- Upward adjustment if the current employment is too low: $n_0 < n^* - cu_1/\phi$. The firm adjusts to a level which depends on n^* and cu_1: $n_1 = n^* - cu_1/\phi$
- No adjustment if the current employment is in an *inaction band*: $n^* - cu_1/\phi < n_0 < n^* + cd_1/\phi$. The firm does not adjust: $n_1 = n_0$
- Downward adjustment if the current employment is too high: $n_0 > n^* + cd_1/\phi$. The firm adjusts to a level which depends on n^* and cd_1: $n_1 = n^* + cd_1/\phi$

This simple framework helps to disentangle the different possible changes caused by the intervention. In particular, we can make a distinction between the two polar cases of *current adjustment cost* and *profitability* channels and a third mixed case:

a. *Current adjustment costs channel*. The intervention only changes the current adjustment cost parameters $c_{d,1}$ and $c_{u,1}$ and let the employment level n^* unchanged. Panel (b) in Figure B.1 describes the impacts on employment of a reduction in both adjustment costs.

   There are to main effects: (1) when the current employment level n_0 is such that the firm adjusts, then the adjustment is stronger both upward and downward. This is represented by the two (upward and downward) vertical arrows on the figure, and (2) firms adjust more frequently. The width of the inaction band $[\underline{n}, \overline{n}]$ tightens.

   Also, the adjustment is heterogeneous: positive for firms which would like to increase their employment; zero for firms with intermediate values of desired employment change and negative for firms which would like to reduce their employment. As a result, the average impact on employment is ambiguous.

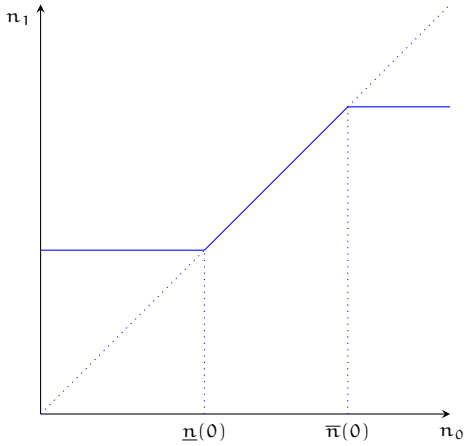
b. *Profitability channel*. The intervention leads to an increase in the employment level n^*: $\Delta n^* = n^*(1) - n^*(0) > 0$. The main mechanisms are reductions in the current cost of labor regulation cr_1 and cost of downward adjustment in the future cd_2. As discussed above, these changes can be mitigated if future upward adjustment costs decrease.

   The resulting adjustment of employment is shown in Panel (c) of Figure B.1. The employment level schedule $n_1(n_0)$ shifts upward. This is represented by the two (upward) vertical arrows on the figure. The inaction band shifts to the right. There are firms which now adjust but would not have absent the intervention. There are firms which do not adjust but would have and last there are firms which do not adjust and would not have absent the intervention. Hence, although the impact is still heterogeneous in this case, it is unambiguously either positive or zero.

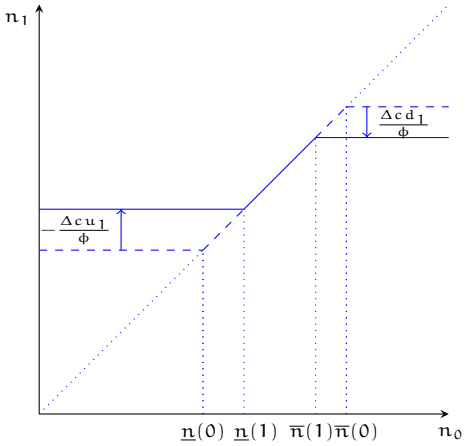
c. A final case involves a mix of the two former situations: a reduction in the current adjustment cost parameters and a change in employment level n^* triggered by the other cost parameters. For example, the intervention could lead to a reduction in downward and upward adjustment costs in the current and the future periods. This case is illustrated in Panel (d) Figure B.1. The pattern of employment adjustment is a combination of the two previous cases. The impact on firms which would adjust upward absent the program is unambiguously positive. There is still a range of firms for which there is no employment level adjustment. Finally, the impact on firms which would not adjust their employment downward absent the intervention is ambiguous.

B Appendix Figures and Tables

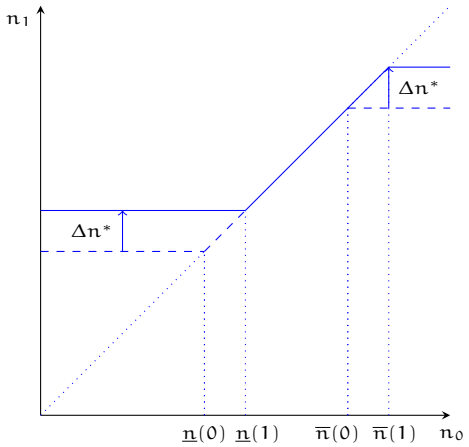
Figure B.1: Labor adjustment under different changes induced by the program



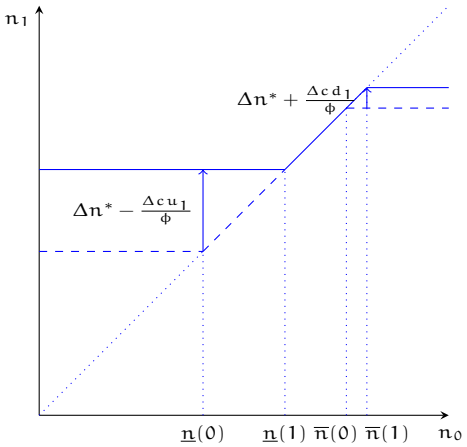
(a) Adjustment pattern



(b) Change in adjustment cost parameters



(c) Change in cost of labor regulation and future cost parameters



(d) Both changes

$\underline{n}(0)$ and $\overline{n}(0)$ are thresholds which trigger adjustment absent the intervention:

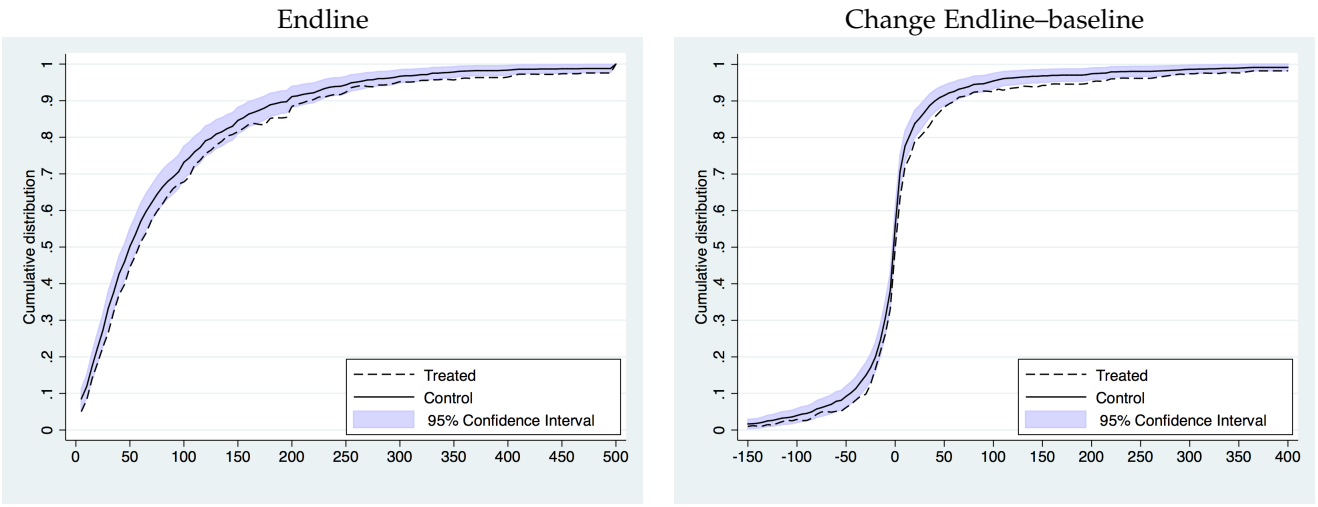
- $\underline{n}(0)=n^*(0)-\frac{cu_1(0)}{\phi}$ and $\overline{n}(0)=n^*(0)+\frac{cd_1(0)}{\phi}$

$\underline{n}(1)$ and $\overline{n}(1)$ are these same thresholds with the intervention. They depend on the type of change caused by the intervention:

- $\underline{n}(1)=n^*(1)-\frac{cu_1(1)}{\phi}$ and $\overline{n}(1)=n^*(1)+\frac{cd_1(1)}{\phi}$

case (b): $n^*(1) = n^*(0)$; case (c): $cd_1(1)=cd_1(0)$ and $cu_1(1)=cu_1(0)$

Figure B.2: Impact on cumulative distribution and rank sum tests adding firms with probable closure

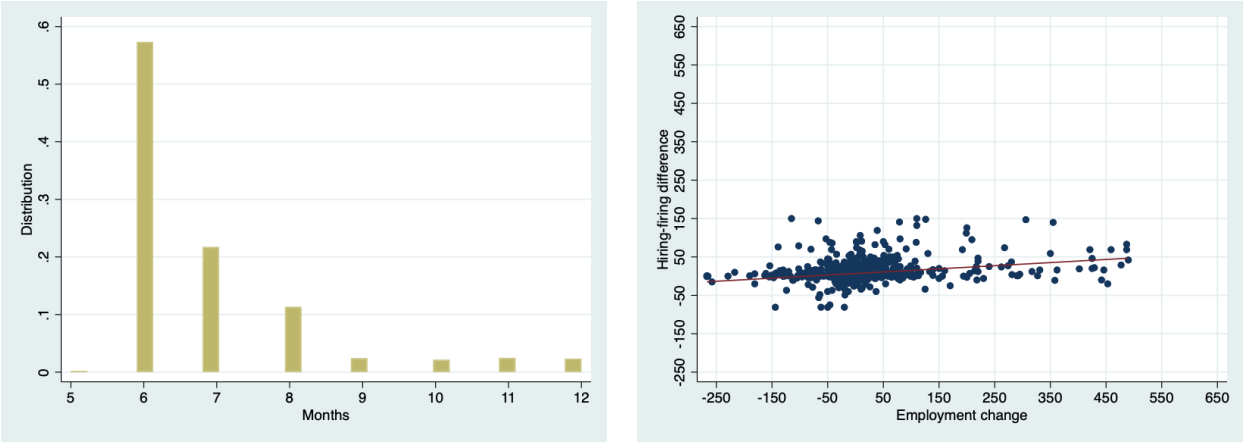


Mann Whitney test: p values obtained from 10,000 permutations within strata[a]

$p = 50/10000$	$p = 13/10000$

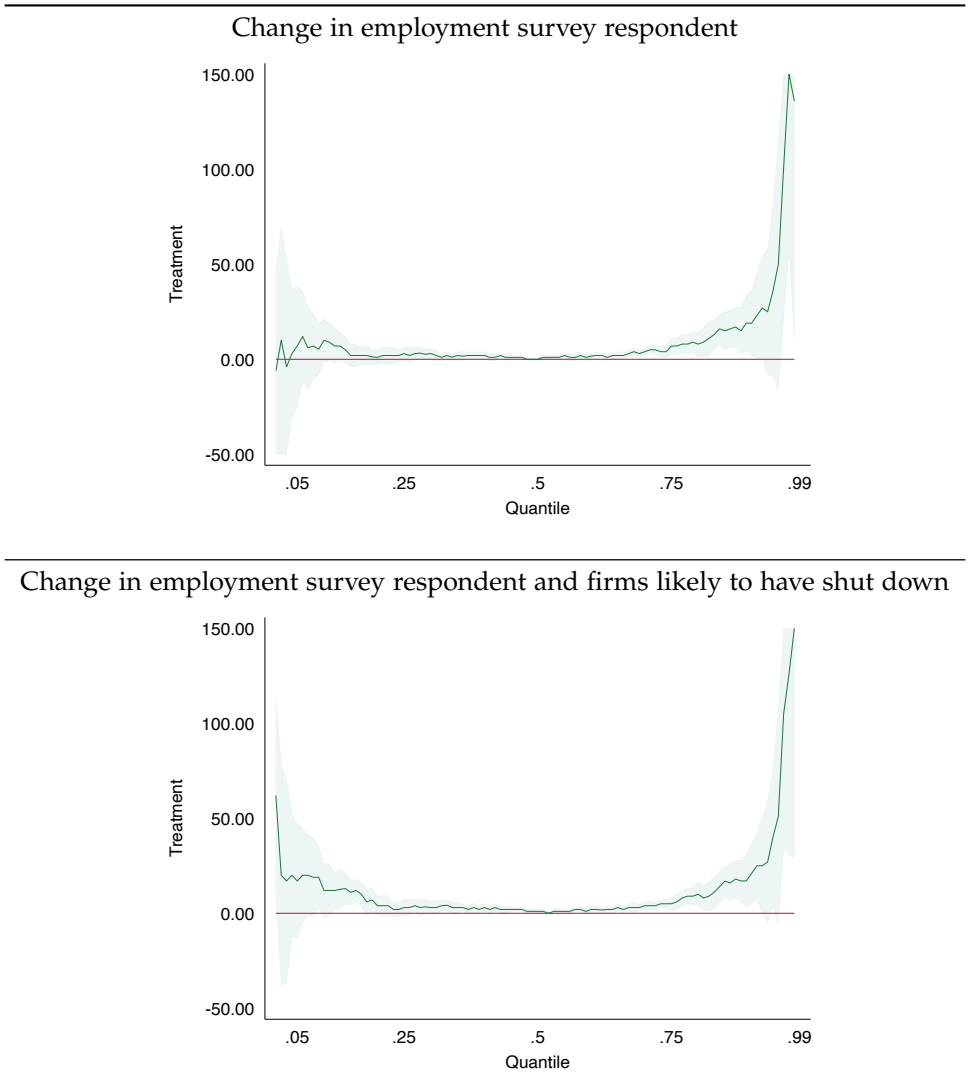
This figure replicates Figure 2 adding to the sample firms that did not complete the endline survey due to probable closure (see footnote 12)
For an outcome variable y we define the dummy variable $d_x = 1(y \leqslant x)$, with x the value on the x-axis. We compute the average in the control group c_x (reported as the blue line in the graph) and t_x in the treatment group (reported as the red line in the graph) as well as s_x the standard error of their difference. The graphs also report a shaded area with bounds defined as $c_x \pm 1.96 s_x$ and have the same length as the 95% confidence interval of the difference.
Mann Whitney test p values: proportion of pseudo random draw of the treatment variable out of 10,000 for which the statistic of the ranksum test is above the one computed with the true assignment

Figure B.3: Duration between the date of survey completion and random assignment and relation between hiring-firing and endline-baseline changes in employment



Left panel: distribution of the number of months between the date of endline survey completion and random assignment (a few observations with shorter durations have been discarded) .
Right panel scatter plot of the change in employment as measured by the difference between the number of hired and dismissed workers and the change in employment as measured by the difference between endline and baseline total staff. The estimated slope coefficient is 0.081 with a standard error of 0.014

Figure B.4: Quantile treatment effects



Quantile treatment effects as a function of the quantile (solid line) as well as the 95% confidence interval (grey area).
Dependent variable is employment change as measured by the difference between endline and baseline total staff
Upper panel: respondent to endline survey. Lower panel: adding to the sample firms that did not complete the endline survey due to probable closure (see footnote 12)

adding to the sample firms that did not complete the endline survey due to probable closure (see footnote 12)

Table B.1: Sampling frame sectors

Kept in UIF sampling frame	Sectors
Yes	Building and Construction
	Food, Drinks, Tobacco
	Textiles
	Wood Industry, Upholstery
	Printing and Paper
	Rubber, Oil, Paint, Chemicals
	Leather
	Glass, Brick, Tiles, Concrete
	Iron, Steel, Garages
	Trade, Commerce
	Air, Road Transport, Hauliers
No	Agriculture and Forestry
	Taxi Industry
	Fishing
	Mining and Quarrying
	Jewellers, Diamonds, Asbestos
	Banking, Finance, Insurance
	Local Authorities
	Personal Services, Hotels, Flats
	Entertainment and Sport
	Medical Services
	Professional Services
	Educational Services
	Charitable, Religious and Political Organisations

Table B.2: Timeline of baseline survey and randomization

Group	Initial #	Baseline Start Date	Random. Date	Surveyed	(5)/(2)	Treatment	Control	End Date
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	901	Feb. 19	April 4	382	42.4	190	192	Aug. 29
2	2510	March 19	April 25	395	15.7	194	201	Sept. 19
3	2548	April 16	May 16	432	17.0	218	214	Oct. 10
4	2487	May 9	June 20	428	17.2	216	212	Nov. 14
5	1295	May 29	July 4	187	14.4	94	93	Nov. 28
Total	9741			1824	18.7	912	912	

All surveys were implemented in 2013

The table reports, for each group/wave: number of firms in the initial list to contact (column 2); baseline start date (column 2); number of firms enrolled (column 3); randomization date (column 4); number and share of firms surveyed (columns 5 and 6); number of firms assigned to the treatment group (column 7) and to the control group (column 8); date at which the free access to the web site ended (column 9).

Numbers in columns (2) and (6) are different for the first group/wave. An early pilot was attempted to check the quality of the contact details on that group and showed us that we should expect around a 20% success rate when trying to reach firms. Numbers for the first group are for the firms which were successfully reached during the pilot. For the other groups, we report in column (2) the number of firms in the UIF database which were initially assigned to the group.

Table B.3: Heterogeneity of impact on total staff with respect to firm size

	Level		Endline–baseline	
	(1)	(2)	(3)	(4)
Heterogeneity of impact on total staff with respect to firm size				
< 50	10.14**	9.60**	10.90**	10.90**
	(4.90)	(4.83)	(4.84)	(4.84)
$[50, 100]$	16.28**	17.04**	16.96**	17.41**
	(7.65)	(7.65)	(7.50)	(7.50)
> 100	7.96	6.92	6.84	6.34
	(8.83)	(7.55)	(7.69)	(7.68)
Control mean	81.11	81.11	2.13	2.13
p-value global	0.02	0.02	0.01	0.01
p-value same	0.73	0.61	0.63	0.58
Employment results accounting for probable closure				
Treated	13.33***	14.27***	14.01***	13.85***
	(4.55)	(3.66)	(3.67)	(3.64)
Control mean	74.96	74.96	-3.59	-3.59
Add covariates	No	Yes	No	Yes

Upper panel (1466 observations): estimation of equation 3 using firm size as interacted variable. Size categories are defined as firms below 50 employees (667 firms, 45.5%), firms between 50 and 100 employees (395 firms, 26.8%) and firms above 100 employees (406 firms, 27.7%).

p-value global corresponds to the p-value of the test of the joint nullity of the coefficients of the interacted partitioning variables.

p-value same corresponds to the p-value of the test of the equality of the coefficients of the interacted partitioning variables.

Lower panel (1565 observations): The table replicates Table 2 for variables in level adding to the sample firms that did not complete the endline survey due to probable closure (see footnote 12.

C Tables and Figures Describing the UCT Law@Work Club Services

Table C.1: List of newsletters

Date	Day	Title	Topic	All emails tracked?	Group 1	Group 2	Group 3	Group 4	Group 5
2013/04/09	Tue	2 Rules to remember when hiring an intern	Recruitment	no	yes	no	no	no	no
2013/04/11	Thu	Medical Certificates	Leave	no	yes	no	no	no	no
2013/04/16	Tue	5 Steps to create a positive work environment	Management Tips	no	yes	no	no	no	no
2013/04/18	Thu	Sick Leave	Leave	no	yes	no	no	no	no
2013/04/23	Tue	What colour is your brain: 6 unconventional interview questions to reveal the ideal candidate	Recruitment	no	yes	no	no	no	no
2013/04/25	Thu	Charging employees Case law	Employee-Employer Relations	no	yes	no	no	no	no
2013/04/30	Tue	Avengers Assemble: 3 ways to build an effective team	Management Tips	no	yes	yes	no	no	no
2013/05/02	Thu	Misconduct: Intent/Negligence	Poor Performance and Incapacity	no	yes	yes	no	no	no
2013/05/07	Tue	The R-Factor: Creating a winning retention plan	Management Tips	no	yes	yes	no	no	no
2013/05/09	Thu	Settlement Agreements	Employee-Employer Relations	no	yes	yes	no	no	no
2013/05/14	Tue	Gather around: 3 meetings you should be having each year.	Management Tips	no	yes	yes	no	no	no
2013/05/16	Thu	Dishonesty Case Law	Employee-Employer Relations	no	yes	yes	no	no	no
2013/05/21	Tue	The Safety Dance: 3 tips to deal with employee theft	Ill-discipline	no	yes	yes	yes	no	no
2013/05/23	Thu	Consistency	Ill-discipline	no	yes	yes	yes	no	no
2013/05/28	Tue	4 tips to deal with excessive absenteeism	Ill-discipline	no	yes	yes	yes	no	no
2013/05/30	Thu	Office Romance	Employee-Employer Relations	no	yes	yes	yes	no	no
2013/06/04	Tue	Don't be casual about it: things you need to know about "casual staff"	Recruitment	no	yes	yes	yes	no	no

continued . . .

Table C.1: List of newsletters

Date	Day	Title	Topic	All emails tracked?	Group 1	Group 2	Group 3	Group 4	Group 5
2013/06/06	Thu	Insubordination	Ill-discipline	no	yes	yes	yes	no	no
2013/06/11	Tue	It's training day: 4 tips to ensure development success	Skills Development	no	yes	yes	yes	no	no
2013/06/13	Thu	Further Particulars	Ill-discipline	no	yes	yes	yes	no	no
2013/06/18	Tue	You're up coach: 3 tips on effective employee feedback	Employee-Employer Relations	no	yes	yes	yes	no	no
2013/06/20	Thu	Occupational Health and Safety	CCMA / Labour Court / Acts / Legislation	no	yes	yes	yes	no	no
2013/06/25	Tue	The Minute Man: How to effectively deal with immediate resignations	Employee-Employer Relations	no	yes	yes	yes	yes	no
2013/06/27	Thu	Occupational Health and Safety part 2	CCMA / Labour Court / Acts / Legislation	no	yes	yes	yes	yes	no
2013/07/04	Thu	Polygraph Case Law 2	Ill-discipline	no	yes	yes	yes	yes	no
2013/07/05	Tue	Sector Specific Newsletters	Sector Specific	no	yes	yes	yes	yes	yes
2013/07/09	Tue	Tweet tweet: Is social media your best recruitment tool?	Recruitment	no	yes	yes	yes	yes	yes
2013/07/11	Thu	Operational Requirements Terminations	Retrenchments	no	yes	yes	yes	yes	yes
2013/07/16	Tue	CCMA: 2 simple labor tips to keep productivity high and disputes low	CCMA / Labour Court / Acts / Legislation	yes	yes	yes	yes	yes	yes
2013/07/18	Thu	Earnings Thresholds	Compensation	yes	yes	yes	yes	yes	yes
2013/07/23	Tue	Unfair Dismissals: Do your employees know the rules?	Dismissals	yes	yes	yes	yes	yes	yes
2013/07/25	Thu	Earnings Threshold and overtime	Compensation	yes	yes	yes	yes	yes	yes
2013/07/30	Tue	Sexual Harassment: What Vavi taught us	Harassment	yes	yes	yes	yes	yes	yes
2013/08/01	Thu	Previous Warnings	Ill-discipline	yes	yes	yes	yes	yes	yes
2013/08/06	Tue	Workplace Skills Plan: SA's answer to the skill shortage epidemic	Skills Development	yes	yes	yes	yes	yes	yes
2013/08/08	Thu	Discipline and the sick employee	Ill-discipline	yes	yes	yes	yes	yes	yes
2013/08/13	Tue	How do we solve a problem like Charlie: 3 steps to deal with poor performance	Poor Performance and Incapacity	yes	yes	yes	yes	yes	yes

continued . . .

Table C.1: List of newsletters

Date	Day	Title	Topic	All emails tracked?	Group 1	Group 2	Group 3	Group 4	Group 5
2013/08/15	Thu	Disciplining the Shop Steward	Ill-discipline	yes	yes	yes	yes	yes	yes
2013/08/20	Tue	The Perfect Storm: The 3 elements hindering productivity	Poor Performance and Incapacity	yes	yes	yes	yes	yes	yes
2013/08/22	Thu	Resignation before disciplinary	Ill-discipline	yes	yes	yes	yes	yes	yes
2013/08/27	Tue	Lucy's First Day - 4 tips that could make her a success	Management Tips	yes	yes	yes	yes	yes	yes
2013/08/29	Thu	Resignation before disciplinary part 2	Ill-discipline	yes	no	yes	yes	yes	yes
2013/09/03	Tue	All Aboard! 3 techniques to reduce resistance to change	Management Tips	yes	no	yes	yes	yes	yes
2013/09/05	Thu	Insubordination Case Law	Ill-discipline	yes	no	yes	yes	yes	yes
2013/09/10	Tue	The Charlie Sheen Conundrum: Handling difficult employees	Employee-Employer Relations	yes	no	yes	yes	yes	yes
2013/09/12	Thu	Protection of Personal Information (POPI) Act Part I	CCMA / Labour Court / Acts / Legislation	yes	no	yes	yes	yes	yes
2013/09/17	Tue	Workplace stress: A valid illness?	Leave	yes	no	yes	yes	yes	yes
2013/09/19	Thu	Protection of Personal Information (POPI) Act Part II	CCMA / Labour Court / Acts / Legislation	yes	no	no	yes	yes	yes
2013/09/23	Tue	These 3 questions are effective for employee performance evaluations	Management Tips	yes	no	no	yes	yes	yes
2013/09/26	Thu	Case Law: Following procedurally fair processes	Dismissals	yes	no	no	yes	yes	yes
2013/10/01	Tue	Youth Tax Incentive: What you need to know	CCMA / Labour Court / Acts / Legislation	yes	no	no	yes	yes	yes
2013/10/03	Thu	The Breathalyzer Test	Ill-discipline	yes	no	no	yes	yes	yes
2013/10/08	Tue	Department of Youth: 3 tips to understand your young employees	Management Tips	yes	no	no	yes	yes	yes
2013/10/10	Thu	Operational Requirements Terminations	Retrenchments	yes	no	no	no	yes	yes
2013/10/15	Tue	Feud Control: 2 steps to manage workplace conflict	Management Tips	yes	no	no	no	yes	yes
2013/10/17	Thu	Equal work, equal pay	Employment Equity	yes	no	no	no	yes	yes

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Table C.1: List of newsletters

Date	Day	Title	Topic	All emails tracked?	Group 1	Group 2	Group 3	Group 4	Group 5
2013/10/22	Tue	Exit Music: 3 methods for a successful exit interview	Management Tips	yes	no	no	no	yes	yes
2013/10/24	Thu	Regional Demographics	Employment Equity	yes	no	no	no	yes	yes
2013/10/29	Tue	How Should You Be Disciplining a Manager?	Management Tips	yes	no	no	no	yes	yes
2013/10/31	Thu	Vehicle Tracking	Ill-discipline	yes	no	no	no	yes	yes
2013/11/05	Tue	Employment Services Bill: 3 Key Changes You'll Need to Know	In the news	yes	no	no	no	yes	yes
2013/11/07	Thu	Constructive Dismissal Part III	Dismissals	yes	no	no	no	yes	yes
2013/11/12	Tue	We Need to Talk: Mediating Difficult Workplace Conversations	Employee-Employer Relations	yes	no	no	no	yes	yes
2013/11/14	Thu	Trade Union Liability	CCMA / Labour Court / Acts / Legislation	yes	no	no	no	no	yes
2013/11/19	Tue	The Abe Lincoln Method: Managing someone you dislike	Management Tips	yes	no	no	no	no	yes
2013/11/21	Thu	Racism in the Workplace	Ill-discipline	yes	no	no	no	no	yes
2013/11/26	Tue	The 2020 Workplace: What you'll need to know	In the news	yes	no	no	no	no	yes

Table C.2: Case Law Library - list of categories

Main Category	Sub-Category (if applicable)
Appeal and review	
Contracts of Employment	Breach, Fixed term, Legal existence & validity, Repudiation
Dismissal - Operational requirements	
Dismissal - Procedural fairness	Disciplinary procedure, Dismissal or resignation, Non-appearance by party, Probationary periods, Right to disciplinary enquiry, Right to training/counselling, Right to representation
Dismissal - Substantive fairness	Absenteeism & latecoming, Abusive language, Alcohol, drug abuse, Assault, Breach of trust, Damage to property, Employment contract, Employment relationship, Firearms, Fraud, Group action, Imprisoned employees, Incapacity & poor performance, Insubordination, Internet & email abuse, Intimidation, Misrepresentation, Negligence, Restructuring, Retirement, Retrenchment, Sexual harassment, Theft
Grievance/Unfair Labour Practices	Affirmative action, Bias in discipline, Breakdown of working relationship, Change in terms and conditions, Constructive dismissal, Discrimination and harassment, Grievance procedures, Payments and benefits, Suspension of employees
Independent contractors	
Industrial action and bargaining	
Interdicts	
Leave	Maternity leave, Sick leave
Notice periods	
Resignations	
Retirement	
Rules of the court	Jurisdiction, Rescission and review, Setting down for arbitration
Union representatives	Breach of trust, Constructive dismissal, Freedom of association, Insubordination, Intimidation, Reinstatement

Table C.3: Discussion forum - list of categories

Forums	Description	Sub-forums
Uncategorised	This is a “holding space" for forum topics that have yet to be categorised	-
Special requests	-	Case law, Policies and documents
Articles and Current Affairs	A place for members to post and comment on the latest news	-
CCMA and Labour Court	Discussions relating to the concilliation, mediation and artibration processes	Court Jurisdiction
Compensation Issues	Discussions relating to the quantity or timing of hours worked and salaries	Overtime, On termination, Weekends and Public Holidays, UIF
Contracts	Issues relating to contracts and agreements	Employment Contracts, Matters of ownership
Disciplinary Issues	Discussions relating to disciplinary processes and procedures	Polygraph testing
Discrimination Laws	Discussions relating to workplace conflict or discrimination	-
Employment Equity	-	Reporting
Harassment	Discussions relating to all forms of harassment in the workplace	-
Health and Safety in the workplace	Discussions involving issues related to health and safety matters	Ill health/incapacity in the workplace, Workplace accidents
Hiring, Retaining, Promoting	Discussions relating to the acquiring of new staff, or the promotion of current employees	Conflicts of interest
Labour Law Clarification	Discussions relating to the scope and implications of current labor laws	-
Leave Laws	Discussions relating to absence of work for various reasons	Disability Leave, Pregnancy and Maternity Leave, Sick Leave
Operations Management	Issues relating to workplace operations	-
Terminations	Discussions involving the firing of employees	Reason: Misconduct, Reason: Retrenchments, Reason: Incapacity/ill health, Reason: Death or winding up of business
Unions	Discussions involving the violation of union rules and regulations	Bargaining Councils, Shop Stewards
Website Issues	All queries and suggestions relating to the Labour Law Club website	-
Workplace Policy	Queries relating to policies implemented and enforced in the workplace	-
Testimonials	-	-

Table C.4: Learning center - list of categories

Main Category	Category	Sub-topic 1	Sub-topic 2
Employment Law	1. Contract Of Employment	1.1 Identifying the parties	1.1.1 Recognising an employee
			1.1.2 The employer
			1.1.3 Temporary employment services
		1.2 Types of contract	1.2.1 Fixed term contracts
			1.2.2 Indefinite contracts
			1.2.3 Illegal contracts
			1.2.4 Contracting agreements
		1.3 Particulars of the contract	1.3.1 Express terms
			1.3.2 Disciplinary clauses
			1.3.3 Restraint of trade clauses
			1.3.4 Avoid these clauses!
			1.3.5 Amending the contract
		1.4 Reasons for terminating the contract	
	2. Basic Conditions Of Employment	2.1 Regulation of working time	2.1.1 Ordinary hours of work
			2.1.2 Overtime
			2.1.3 Sundays and night work
			2.1.4 Meal periods
		2.2 Leave	2.2.1 Annual leave
			2.2.2 Sick leave
			2.2.3 Maternity leave and pregnancy
			2.2.4 Family responsibility leave
	3. Equality In The Workplace	3.1 Affirmative action	3.1.1 Introduction to the Employment Equity Act
			3.1.2 Designated employers
			3.1.3 Designated groups (people)
		3.2 Discrimination	
		3.3 Sexual harassment	
	4. Unfair Labour Practices		
	5. Dismissals And Discipline	5.1 Misconduct	5.1.1 Absenteeism
			5.1.2 Alcohol in the workplace
		5.2 Incapacity and Ill-health	
		5.3 Operational requirements	
	6. Automatically Unfair Dismissals		

continued . . .

Table C.4: Learning center - list of categories

Main Category	Category	Sub-topic 1	Sub-topic 2
	7. Transfer Of Businesses		
Collective Labour Law	1. Trade unions	1.1 Terms and definitions	
		1.2 Rights of trade unions	
	2. Collective bargaining		
	3. Workplace forums		
	4. Strikes, lock-outs, protest action		
Labour Disputes	1. Bargaining councils		
	2. Statutory councils		
	3. CCMA		
	4. Labour Court and Labour Appeals Court		
	5. Workplace forums		
Social Security	1. UIF		
	2. Occupational Injuries and diseases		
	3. Pensions		
	4. Healthcare and medical aid		
	5. Skills development	5.1 Workplace skills training	
		5.2 Learnerships	
		5.3 Youth wage subsidy	
	6. Expanded Public Works Programme		
Industry Specific Information	1. Communication		
	2. Construction		
	3. Manufacturing		
	4. Retail		
	5. Transport		

Table C.5: Templates - list of categories

Categories	Templates
Contracting	Contracting Agreement
Disciplinary Notices	Notification of Disciplinary Inquiry, Written Warning, Written Warning (Final)
Dismissals	Notice of Dismissal (with notice), Notice of Dismissal (without notice)
Employment Contracts	Contract (Part-time), Contract (Permanent), Contract (Fixed Term), Contract (w/ restraint of trade), Executive Contract (1), Executive Contract (2), Non-disclosure agreement
Employment Equity Reporting	Employment Equity plan
Grievances	Grievance procedure, Grievance notification form
Performance Appraisals	-
Policy Documentation	Policy Documentation, Disciplinary Policy, Employment Equity Policy, Sexual Harassment Policy, Smoking Policy, Small Businesses
Rescission Contracts	CCMA
Retrenchments	Notification letter to employees, Termination agreement, Termination agreement 2

D Additional tables for referee #3

Table D.1: Optimal level of employment and perception of LR

	Are LR constraining you from				
	Decreasing staff	Increasing staff	Optimal staff level	Actual staff level	Knowledge score
	(1)	(2)	(3)	(4)	(5)
Treated	-8.61***	-9.18***	9.69*	9.38**	0.91
	(2.86)	(3.31)	(5.08)	(4.75)	(1.38)
[50, 100]	4.00	4.63	3.85	7.59	-0.01
	(4.76)	(5.32)	(8.98)	(8.91)	(2.31)
> 100	15.37***	8.50	-4.23	1.57	1.54
	(4.85)	(5.20)	(10.15)	(9.43)	(2.27)
p-value global	0.00	0.03	0.06	0.01	0.47
p-value same	0.01	0.26	0.78	0.69	0.77
As function of desired adjustment at baseline					
< 0	-14.25***	-3.29	13.07	12.09	-2.17
	(4.59)	(5.19)	(8.67)	(8.62)	(2.17)
= 0	-7.80**	-9.39**	2.93	4.59	1.19
	(3.71)	(4.02)	(6.06)	(5.84)	(1.69)
> 0	-5.56	-13.13***	16.01*	13.43*	2.54
	(3.87)	(4.70)	(8.49)	(7.70)	(2.00)
[50, 100]	4.44	4.29	3.18	7.30	0.26
	(4.76)	(5.31)	(9.10)	(9.00)	(2.32)
> 100	15.85***	8.22	-4.39	1.31	1.91
	(4.85)	(5.24)	(10.13)	(9.42)	(2.29)
p-value global	0.01	0.04	0.12	0.05	0.28
p-value same	0.24	0.25	0.33	0.55	0.15
Control mean	19.43	26.14	82.43	81.11	46.08
Observations	1489	1497	1450	1466	1510

Same table as Table 4 adding size variables to the list of variables interacted with the treatment variable
See notes on Table 4

Table D.2: Heterogeneity of impact with respect to baseline knowledge and external HR services

	Are LR constraining you from				
	Decreasing staff	Increasing staff	Optimal staff level	Actual staff level	Knowledge score
	(1)	(2)	(3)	(4)	(5)
Knowledge at baseline (share among answers)					
Treated	-7.99***	-9.14***	9.70*	8.98*	0.74
	(2.89)	(3.34)	(5.27)	(5.02)	(1.39)
Do not know	0.68	2.83	0.93	1.92	1.09
	(2.12)	(2.42)	(4.43)	(4.18)	(1.01)
Pro-employer bias	0.98	-1.52	-7.35*	-6.65*	0.45
	(2.26)	(2.42)	(4.25)	(4.04)	(1.03)
Pro-employee bias	-2.65	-1.39	-2.42	-1.55	-1.00
	(2.01)	(2.32)	(4.20)	(3.98)	(0.99)
[50, 100]	3.80	4.74	4.13	8.76	0.10
	(4.78)	(5.36)	(9.05)	(9.05)	(2.30)
> 100	14.81***	8.15	-4.85	1.52	1.99
	(4.91)	(5.32)	(10.30)	(9.69)	(2.32)
p-value global	0.02	0.06	0.08	0.02	0.42
p-value same	0.45	0.37	0.19	0.17	0.41
External HR services					
With	-10.87***	-12.07***	12.94**	11.36*	-0.45
	(3.46)	(3.86)	(6.47)	(6.06)	(1.60)
Without	-5.36	-5.58	5.26	6.90	2.98*
	(3.41)	(4.07)	(6.66)	(6.61)	(1.80)
[50, 100]	5.03	5.83	3.74	7.90	0.49
	(4.75)	(5.35)	(9.11)	(9.03)	(2.32)
> 100	15.67***	9.17*	-5.39	0.94	1.74
	(4.86)	(5.19)	(10.32)	(9.63)	(2.28)
p-value global	0.01	0.03	0.09	0.02	0.25
p-value same	0.16	0.14	0.36	0.59	0.09
Control mean	19.46	26.17	82.40	81.08	46.07
Observations	1486	1494	1447	1463	1507

Same table as Table 6 adding size variables to the list of variables interacted with the treatment variable
See notes on Table 6