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 in South Africa that provides information to local firms about major topics regarding labor regulation via newsletters and access to a specialized website. We randomly assign 1800 firms to receive free access to this information service for a 21-week period. Three-quarters of the firms offered the service took it up. Six months later, the average employment level at treatment firms was 12 to 15% higher than at control firms, with absolute employment gains concentrated among workers under permanent and fixed-term contracts. The intervention increased optimal employment level and decreased the perception that labor regulation is a constraint to hiring among treatment firms.

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 particularly 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 affect 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 small and medium firms are hindered in their ability to grow because of their limited knowledge of the rules and laws that govern firms' interactions with their workers and the labor market at large.

South Africa provides an interesting setting for this work in that its labor regulation is complex and has been subject to drastic changes in the last quarter century. Labor laws in South Africa were rewritten at the end of the Apartheid regime. A set of Acts adopted quickly after 1994 led to profound changes in the regulation of labor. The Labour Relations Act (Act No. 66 of 1995) forms the new basis of labor law and set the new conditions for hiring and firing; this Act also led to the creation of the Commission for Conciliation, Mediation and Arbitration (CCMA), the new body for labor dispute resolution. 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; this Act also lays out affirmative action rules devoted to ensure equitable representation in the workforce. Both the recency and complexity of the regulation of labor in South Africa suggests the possibility that informational gaps may exist. Furthermore, there is some anecdotal evidence that the perception of the new legal environment when it comes to hiring and firing rules may not be in line with the reality of that environment. In particular,

[1]This finding has not been shared by all studies. In fact, as McKenzie and Woodruff (2013) discuss in a review paper, teaching management practices more typically results in changes in management practices but with limited effects on firm performance. One explanation might be related to the content of management programs, with the training provided in Bloom et al. (2013) being relatively quite intensive. Another explanation recently put forward in the literature is that it is less teaching hard skills that matters but instead changing mindsets (see for example results in Campos et al. (2017)).

while data collected by Botero et al. (2004) and the World Bank Doing Business report suggest that South Africa's labor law post-Apartheid was strongly perceived as more "pro-worker" than before, legal scholars in South Africa such as Bhorat and Cheadle (2009) suggest that this may not be unambiguously true. For example, Bhorat and Cheadle (2009) stress that while hiring procedures (such as restrictions on part-time and temporary contracts) and firing procedures are quite rigid, 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 high by international standards. Such disagreements between scholars further suggest the possibility that employers in South Africa might be confused about the stringency of the labor regulation.

To proceed with this study, we partnered 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 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 the 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, we find that treatment firms had about 12 more workers six months post-randomization, which corresponds to about a 15% increase compared to the control group mean, 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, which we analyze via a simple model of labor demand with adjustment cost. In particular, we show that the information intervention increases self-reported optimal employment level in treatment firms, suggesting a labor profitability channel for the effect. We also find that the intervention decreases the perception among treatment firms that labor regulation is a constraint to hiring workers. Interestingly, 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' relationship 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 contract 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.

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. Most of these field experiments have been conducted in emerging markets. 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. Section 3 lays out a simple conceptual framework to guide our analysis. The experimental design is discussed in Section 4. Section 5 reports our main results on employment effects, while Sections 6 and 7 investigate mechanisms. We conclude in Section 8.

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.[2] 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 offered free access to UCT Law@Work Club's content over a 21-week period.

2.1 Newsletters

Twice a week, newsletters were written and sent electronically by the Club to each participant during their 21-week membership, for a total of 41 newsletters.[3]

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.

The first weekly newsletter was sent on Tuesdays, while the second one was sent on Thursdays. The main purpose of the Tuesday newsletters was to give relevant and up-to-date management advice summarized in a list of two to six key points that are easy to remember and apply. The newsletter layout consisted in one main block of text with short paragraphs and one right-side column which included additional information: a reminder with the username and password to log into the website, a fact of the week, links to the latest templates on the website, links to the latest discussions on the forum, and links to a weekly quiz. Table B.1 lists all the newsletters sent by the Club, with their titles and topics. The most frequent topics

[2] `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. The Club's main motivation for using the UCT name is to benefit from UCT's overall excellent reputation in the South African education sector and gain credibility when contacting potential new customers.

[3] On the Thursday of week 21, an end-of-membership email replaced the usual Thursday newsletter, hence the total of 41.

were management advice (increase retention, reduce resistance to change, disciplining a manager, understanding young employees, etc.), 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.).[4]

Thursday newsletters had the same layout as Tuesday newsletters but differed in their content. Their main purpose was to focus on South African case law and give 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. The most common topics were ill-discipline (insubordination, consistency, vehicle tracking, racism in the workplace, etc), rulings from the Commission for Conciliation, Mediation and Arbitration (CCMA), labor acts (occupational health and safety, trade unions, etc) and employee-employer relations (settlement agreements, charging employees, etc.).

Starting with the Tuesday newsletter sent on August 6th, a new layout was introduced for both Tuesday and Thursday newsletters. The aim of the new layout was to strengthen the "bridge-to-the-website" functionality of the newsletters and to increase the participants' use of the website. By including all the management key advice (for Tuesday newsletters) or all the main lessons from a case (for Thursday newsletters) in the body of the newsletter, there was little incentive for participants to go to the website after reading the newsletter. We therefore decided, jointly with the Club, to include only the first half of the newsletter (which introduced the weekly topic) and invite the participant by a "click here to read more" link to read the second half of the newsletter on the website, which included the main labor law and management advice. The rest of the newsletters' content remained unchanged.

2.2 Website

The second component of the intervention consisted in free private access to the Club's website.[5] 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.[6]

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,

[4] A sector-specific newsletter was sent to all groups on July 5th. Based on the information collected at baseline and when phoning participants, the Club identified ten main sectors and created a newsletter for each of them: agriculture and mining, business services, communication, construction, hospitality, legal, manufacturing, NPO, retail and transport. The Club then emailed one of the ten newsletters to each participant depending on the participant's firm's sector. The layout of this newsletter was very similar to the usual Tuesday newsletter but the content focused more on the latest developments in each of these sectors in South Africa. The main characteristics and challenges of each sector were presented, as well as the latest policies the Government was planning to introduce.

[5] The login details were sent to each participant in the invitation email and were included in each newsletter. The website did not have a mobile version and was therefore best viewed on a computer.

[6] The home page, where members would land after logging in, is organized as follows: at the top, links to the seven sections of the website were displayed (they would also appear on the top of each page of the website). In the middle part, members could click on a link to post a question on the forum, or on the one-minute infographic to learn about the services offered by the Club. The bottom part included a news feed with the latest discussions on the forum as well as the latest video tutorial, which could be played directly from the home page. A right-side column showed information and contact details about the Club's team (labor law experts and community manager).

[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`).

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-forums and included questions on, among others, CCMA processes, overtime compensation, drafting of employment contracts, terminations, and unions (see Table B.3). Members could browse the discussions by sub-forums 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. Members usually received detailed answers and pertinent legal advice from the Club’s experts who also referred to other resources on the website (video tutorials, case law). However, the experts’ service was limited to their answers on the forum and, contrary to labor law attorneys hired by companies, did not include any other type of legal or HR service (such as writing up contracts or preparing court hearings).

The main objective of the “Learning 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 B.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). Under the “Learning Centre” section, members could also find a series of quizzes (of five questions each) based on topics covered in video tutorials or in Tuesday newsletters. The quiz score along with the correct answers were displayed after completing the quiz and were emailed to the member.

The “Templates" section provided members with a database of legal template documents that could be 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 B.5). In particular, members could download employment contracts, disciplinary notices, policy templates and termination agreements.[9]

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 B.1). After clicking on a specific video title, members could either watch the video directly on the website or download it to their computer.[10] Each tutorial discussed a labor law topic by presenting a legal case and giving legal advice based on the case outcomes.

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 for the “Video Tutorials” section.

[8]The library was organized by categories, as listed in Table B.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`).

[9]Links to new templates were included in the weekly newsletters sent to the Club’s members.

[10]The average length of the videos was 6 minutes, with a minimum of 4 minutes and a maximum of 9 minutes. The video tutorial was a recording of a slide show presentation, with a small bottom-right corner screen showing the Club’s labor law expert presenting the tutorial slides.

3 Conceptual Framework

Consider a firm planning its employment level for period 1, as well as for future periods $t > 1$. Assume the firm starts from an employment level of n_0. The firm's production function at date t is $f_t(n_t)$, possibly including some random shock on demand or technology, with n_t the level of employment. The firm's wage bill at date t is $\lambda_t w_t n_t$, with λ_t a parameter representing how the firm optimizes its labor cost. Adjustment in the level of employment at date t from a level n_{t-1} to a level n_t has a cost $c(n_t, n_{t-1}, t)$. We assume that the adjustment cost function is piece-wise linear and involves two parameters: $c_{d,t}$ and $c_{u,t}$ for downward and upward adjustments respectively (see Hamermesh and Pfann (1996)):

$$c(n_t, n_{t-1}, t) = c_{d,t}|n_t - n_{t-1}|1(n_t - n_{t-1} < 0) + c_{u,t}|n_t - n_{t-1}|1(n_t - n_{t-1} > 0) \tag{1}$$

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

$$V_1(n, \underline{\theta}_1) = f_1(n) - \lambda_1 w_1 n - c(n, n_0, 1) + \beta V_2^*(n, \underline{\theta}_2) \tag{2}$$

where β is the actualization rate, $V_2^*(n, \underline{\theta}_2)$ is the optimized expected net value of the firm in period 2 if its level of employment at period 1 is n, and $\underline{\theta}_t = (\lambda_t, c_{d,t}, c_{u,t}, \underline{\theta}_{t+1})$ the set of parameters for period t and following.

We define $n^*(\lambda_1, \underline{\theta}_2)$, which we simply refer to as n^*, as the optimum level of employment at date 1, maximizing $\phi_1(n, \lambda_1, \underline{\theta}_2) = f_1(n) - \lambda_1 w_1 n + \beta V_2^*(n, \underline{\theta}_2)$. Thus, n^* depends on the profitability parameter, λ_1, as well as all future parameters, $\underline{\theta}_2$. However, n^* does not depend on the current adjustment cost $c_{d,1}$ and $c_{u,1}$.

In our analysis of the program we consider the possibility of shifts in n^* caused by program participation. We will refer to this as the *profitability channel*. These shifts might be related to a change in the firm's ability to manage its labor costs in period 1 (λ_1) but also to the possibility that the firm's experience with the services offered by the Law@Work Club had an impact that lasted beyond period 1. In particular, the program may have improved the firm's knowledge about how to best operate given the labor laws, and this might have changed the sequence of parameter λ_t as well as the sequence of adjustment costs parameters $c_{d,t}$ and $c_{u,t}$ for $t \geqslant 2$.

We also consider another channel solely related to a *reduction in current adjustment costs*, i.e. a reduction in $c_{d,1}$ and/or $c_{u,1}$, with all the other parameter λ_1 and $\underline{\theta}_2$ left unchanged. The literature on adjustment costs has usually considered changes in adjustment costs that apply to the current date as well as all future dates.[11] In contrast, we separate the impact of changes in current and future adjustment costs.[12] The main reason for this is that the most direct effect of the program might have been to make it easier for firms to make adjustments to their workforce *while* they were clients of the Club. Instead, whether or not the program had an impact on future adjustment costs depends on whether or not firm learned from the services offered to them during the program.

We approximate the function ϕ_1 around n^* at the second order as $\phi_1(n) = \phi_0 - 0.5\phi(n - n^*)^2$.[13] The

[11]This 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. See Bertola (1992); Bentolila and Saint-Paul (1994) and Cahuc et al. (2014) for a discussion

[12]Notice that we are never able to disentangle profitability impact corresponding to a change in λ_1 from those corresponding to a change in $\underline{\theta}_2$.

[13]The first order derivative is zero as n^* is the optimum employment level. Moreover, given the shape of the adjustment cost

objective of the firm then simply writes as:

$$V_1(n, \underline{\theta}_1) = \phi_0 - 0.5\phi(n - n^*)^2 - c_{d,1}|n - n_0|1(n - n_0 < 0) - c_{u,1}|n - n_0|1(n - n_0 > 0) \quad (3)$$

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

- if $n^* < n_0 - c_{d,1}/\phi$, the firm adjusts its employment downwards to $n = n^* + c_{d,1}/\phi$
- if $n_0 - c_{d,1}/\phi < n^* < n_0 + c_{u,1}/\phi$, the firm does not adjust its employment and $n = n_0$
- if $n_0 + c_{u,1}/\phi < n^*$, the firm adjusts its employment upwards to $n = n^* - c_{u,1}/\phi$

This framework helps to disentangle the different possible changes caused by the intervention. In particular, we can make a distinction between two polar cases and a third case mixing the two previous ones:

a. *Reduction in current adjustment costs channel*. As previously explained, in this case the intervention only changes the current adjustment cost parameters $c_{d,1}$ and $c_{u,1}$, but it has no impact on the firm's ability to reduce its wage bill either in the current or future periods and no impact on future adjustment costs. In this case, the firm just "consumes" the services offered by the Club, without learning from these services. In other words, there is no change in the desired employment level n^*. Panel (b) in Figure 1 describes the most likely situation in this case, where firms experience a reduction in adjustment costs while they are Clients of the Club.

   In this case, there are two factors impacting the level of employment and both correspond to changes in the current adjustment process: first, for a given value of the initial level of employment, the range of values of the desired employment n^* for which there is no adjustment is reduced and second, when the firm adjusts, the adjustment is larger. The predicted impact of the participation in the program on level of employment is heterogeneous as it depends on the desired level of employment n^*. The adjustment is positive for firms which would like to increase their employment; it is zero for firms with intermediate values of desired employment level and it is negative for firms which would like to reduce their employment. As a result, the average impact on employment is ambiguous. In our empirical analysis, we will examine the heterogeneity according to whether the firms desired upward or downward employment adjustment at baseline.

   While access to the services of the Club may most naturally ease labor force adjustment, it is also possible that some firms might have had a biased perception of labor regulation, viewing it as more pro-employer than it is in practice. The intervention could increase the adjustment costs parameters for such firms. In our empirical analysis, we also will examine heterogeneity of results after sorting firms into groups corresponding to whether, based on their answers to a baseline quizz about labor regulation, they are likely to have a perception of labor laws biased in a pro-employer vs. pro-employee direction.

b. *Profitability channel*. In this case, the intervention improves firms' current labor profitability (i.e. λ_1 decreases) and/or increases future labor force profitability and/or lowers future adjustment costs. These forces result in an increase in n^*: $\Delta n^* = n^*(1) - n^*(0) > 0$. The resulting adjustment of employment due to the program is shown in Panel (c) of Figure 1 as a function of $n^*(0)$. The participation in the program causes an upward shift of the schedule of the employment level n as a

function, the second order derivative of ϕ_1 is simply the second order derivative of the production function. The program is not supposed to change the shape of the production function, thus the parameter ϕ is not affected by the intervention.

function of the desired level of employment absent the program, $n^*(0)$. This shift has several implications. First, firms which absent the program would have adjusted positively their employment still adjust positively, but the adjustment is now quantitatively larger. Second, there are firms which absent the program would not have realized the positive adjustment they desired due to adjustment costs and which are now adjusting positively because of the increase in n^*. Third, there are firms for which there is no impact: the change in the desired employment level is not large enough to trigger an upward adjustment. Fourth, there is a category of firms which would have adjusted their employment level downwards absent the program and for which the upward shift in the desired employment level is large enough to suspend the downward adjustment or even trigger a positive adjustment. Last, there are firms which would have adjusted downward absent the program and which still adjust downwards, even though their adjustment is now smaller. Hence, although the impact is still heterogeneous in this case, it is unambiguously either positive or zero compared to the no program counterfactual.

c. A final case involves a mix of the two former situations: a change in the current adjustment cost parameters and a change in the current profitability parameter λ_1 and/or future parameters. For example, the intervention could lead to a reduction in firing/hiring costs in both the current and future periods. This case is illustrated in Panel (d) Figure 1. The patterns of employment adjustment in this case would naturally be a combination of the two previous cases. The impact on firms which would like to adjust their employment upward absent the program is unambiguously positive compared to the no program counterfactual. There is still a range of firms for which there is no employment level adjustment. Finally, the impact on firms which would like to adjust their employment downward is ambiguous.

4 Experimental Design

4.1 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.[14] For each firm, the UIF database provides information on number of employees, industrial sector, as well as contact information such as company name, postal address, phone number and email address. While the information on number of employees can be used as a rough indicator of company size, it cannot be considered as an accurate measure of the size of the workforce due to too infrequent updating of the UIF database.

We targeted small and medium-sized firms with 10 to 300 employees.[15] Within this sample, we also focused on a subset of industrial sectors for which we anticipated higher labor turnover and hence for which we expected the labor regulation information to be most relevant. The lists of industrial sectors included and excluded from the study can be found in Table A.1. These size and sectoral restrictions resulted in a population of 22,114 firms.

With a target sample size of 1800 firms in the experiment, we randomly drew a sample of 9741 firms from this population and further randomly divided this sample into five groups of firms which were

[14] see http://www.labour.gov.za/DOL/legislation/acts/basic-guides/basic-guide-to-uif-registration.

[15] According to the Department of Labour Annual Report 2013/2014 (https://www.gov.za/sites/default/files/Department_of_Labour_AnnualReport2014.pdf), there were 1,465,218 employers registered in 2012. 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.

randomized into the experiment in five successive waves over a five-month period. The main reason for dividing our sample into five groups/waves was to minimize the amount of time between the completion of the baseline survey and the invitation to join the Law@Work Club.

For each group of firms, we first conducted a baseline survey.[16] 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 1.

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. As can be seen in Table A.2, a large majority of the baseline (and endline) surveys were indeed completed by the main person in charge of HR decisions at the firm. The baseline survey was short, with only about 20 questions. Besides collecting firm characteristics, the survey respondents were also asked a few questions to test their knowledge of the labor regulation in South Africa.

To ensure high quality of the survey data, all the phone calls were recorded, which allowed us to conduct random back-checks with an average of 39% of the completed phone surveys (across the five groups). Also, during each wave of data collection, a random sub-sample of phone surveys were listened to by one of our research team members and, whenever necessary, respondents were recontacted to correct any mistake made by the surveyors.

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 1 reports the randomization date and number of firms in treatment and control for each wave.

4.2 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 motivation behind the study and the sponsored membership, as well as a short description of the services offered by the Club, were also included in the email[17] Lastly, the email provided instructions on how to log onto the website (URL and login details) and referred to an infographic (titled the "The 1-minute guide to the UCT Law@Work Club") that further described the Club's suite of services.[18]

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 (some email addresses were incorrectly spelled; the invitation email was also sometimes filtered as spam). 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. Receiving a free online membership by email from an unknown company may seem suspicious to some participants and such an email may not

[16] For both baseline and endline surveys, we sent a R60 (about USD6) gift voucher to each respondent who completed the survey (and who accepted to receive the voucher).

[17] The motivation behind the research was described as follows: *The research study aims at improving our understanding of businesses' employment flows and labour regulation knowledge.*

[18] Publicly available at: `www.labourlawclub.co.za`.

have been prioritized in their busy schedule. Therefore, the Club often had to first build trust with the participants before explaining the main features of the website and how the free services could benefit the company. 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.

In addition to the phone calls, the Club sent up to two SMSs to each participant. The first SMS (or "marketing email SMS") asked whether the participants had received the newsletter and reminded them to log into the website. The second SMS (or "final month reminder SMS") was sent one month before the end of the free membership. The SMS asked whether the participants had received an email from the Club about their final month as a member, and reminded them to log into the website as well.[19]

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. The email thanked members for engaging with the Club and included a link to the different membership pricing options (either monthly, quarterly or yearly) so that interested participants could keep their membership active.[20]

4.3 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 follow-up survey. We then phoned respondents who hadn't completed the online survey and offered them to complete it over the phone. The online and phone surveys were identical and had about 30 questions, covering three main types of outcomes: current employment level (including a breakdown of permanent, fixed-term contract and casual 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 early September 2013 and ending in early April 2014.[21] Lastly, we followed the same data quality protocols as for the baseline survey by recording all the phone calls and conducting random backchecks with an average of 35% of the completed phone surveys (across the five groups).[22]

Overall, 1510 firms (82.8% of our sample) completed the endline survey (see Table A.3). 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 remarkably well balanced between treatment and control firms. Table A.4 presents the results of regressions of attrition-related variables on the treatment status. As can be seen, the difference in response rate between the two groups is 0.44 percentage points. Despite our best efforts, there was also some partial non-response. In particular, of the 1510 firms answering the endline survey, only 1466 answered the three questions our endline total staff variable is built upon. As

[19]The Club didn't have cell phone numbers for all participants so not all received the SMSs. For some firms, the Club did have up to two points of contacts, and for those cases, the Club sent SMSs to both contacts in the firm.

[20]Among the 912 firms assigned to the treatment, only 5 renewed their membership. This might be considered at first glance as a sign that the intervention was not well received and that its impact should be small. However, there are multiple examples in other settings where the demand for a service or product is limited even when that service or product has been shown to be highly effective. See for example Kremer and Miguel (2007) and other examples from the preventative health domain at https://www.povertyactionlab.org/fr/node/24618.

[21]Because most of the firms in our sample tended to close down or reduce activities over the December-January holiday season (like the rest of the country), the endline surveying was paused over that period and resumed in mid-January 2014.

[22]In addition to survey data we could also have used administrative data to measure employment. The UIF data that we use to select firms to include in the experiment are known to provide only rough indication about employment levels. There are, however, other administrative data in South Africa that provide rich and accurate employment variable. Unfortunately we were unable to access this dataset.

can be seen from the Table (column (2)), this pattern of partial non-response is however also balanced between treatment and control groups. When investigating non-response for the two broad reasons listed before, some imbalance does appear, with somewhat fewer firms in the treatment than control group not participating because of probable closure.

4.4 Empirical Specifications

To measure the causal impact of our intervention on an outcome variable y, we focus on ITT estimates:

$$y_i = a + bT_i + u_i \tag{4}$$

We estimate this basic regression with robust standard errors on the sample of respondents to our endline survey. However, given the limited sample size and the likely heavy tail distribution of an outcome variable such as employment, we also consider several additional inference methods. First, we implement a hypothesis testing of zero average treatment effect using permutation tests. Second, we estimate impacts on the distribution of employment, also using permutation tests for inference.

We also estimate a specification that includes covariates. The set of potential covariates is comprised of all variables available for the balance-check procedure, which includes industrial sector (6 sectors) from the UIF database and the following variables collected in the baseline survey: baseline employment, labor law knowledge questions, difference between firms' actual employment level at baseline and their desired employment level, share of workers under different contracts (permanent, fixed term or casual), and share of employees that belong to a union.[23] We created categories for baseline employment, labor law knowledge and desired employment adjustment and considered their cross products together with their cross products with the share of unionized workers and with the share of workers under different contracts. In the end, we considered up to 101 covariates to be included as potential controls in the regressions.

Instead of introducing directly the whole set of covariates in the regression, we implement the *double post lasso procedure* developed in Belloni et al. (2014). This procedure amounts to first selecting the set of covariates to introduce in the regression, and then to run the controlled regression only with these selected covariates. As is well understood, the main advantage of this procedure is that it allows us to improve the accuracy of the ITT estimates while avoiding the risk of specification search. In particular, the covariates that are ultimately included in the regression are mechanically selected by the algorithm and are not at the discretion of the researcher:

$$y_i = a + bT_i + selected(x)_i c + u_i \tag{5}$$

As described in Belloni et al. (2014), the covariates selected for inclusion in the regression is the joint set of variables selected in two separate lasso procedures. The first lasso seeks to explain the dependent variable y_i while the second lasso seeks to explain the treatment variable T_i. We implement this procedure using the iterated lasso procedure developed by the author in which the penalization is computed iteratively from the data and for which a *stata* command is available (Ahrens et al., 2018).[24] To give a general

[23] Although answering the baseline survey was a condition for firms to participate in the experiment, there are a few cases in which answers are missing in the baseline for some questions. This is for example the case for the breakdown of actual and optimal staff into three categories, permanent, fixed-term and casual staff. This partial non-response at baseline was limited but in order to keep the sample as large as possible, we did some imputation replacing these missing observations by zeros. Optimal level of staff at baseline was more frequently missing. To deal with this situation we did some imputation based on covariates. We then introduced dummy variables to identify these imputations. Not doing so would have reduced the sample size in the regression by almost 9%.

[24] Note that, given random assignment, we expect that the procedure will not select any variables in the second lasso.

idea of the gains to expect from this procedure, when we estimate an OLS model explaining our main employment outcome using the set of covariates selected by the iterated lasso procedure (the so-called post lasso) we get an R^2 of 0.36. This means that we can roughly expect a reduction in standard errors of $1 - \sqrt{1 - R^2} = 20\%$.

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

$$y_i = a + b_1 T_i \times Cat_1 + \cdots + b_K T_i \times Cat_K + c_1 Cat_1 + \cdots + c_K Cat_K Cat + selected(x)_i c + u_i \tag{6}$$

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

Finally, we also estimate in a robustness check the impact of the intervention on employment after accounting for the potential closure of firms. For that test, we reintegrate in the sample the firms that did not answer the endline survey due to probable closure, setting total staff at zero for these firms.

4.5 Summary Statistics and Balance

Table A.5 report summary statistics and balance checks for the full sample of 1824 registered firms retained in our sample (left panel) as well as for the 1510 firms that responded to the endline survey (right panel).

The treatment coefficients in these balance checks are close to zero and never statistically significant. As reported in the last row, we cannot reject the assumption of the joint nullity of all these coefficients. Not surprising given these balance checks, the iterated lasso procedure (section 4.4) applied to the treatment variable (second lasso) selected none of the large set of covariates that were considered for inclusion. The table also 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. These questions related to: 1) the conditions for the validity of an employment contract; 2) the standard notice period that must be given to an employee to prepare for a disciplinary inquiry; 3) whether a dismissal will be unfair in a case where the employer is unable to prove that the dismissal of an employee is related to his/her conduct, capacity, or operational requirements; 4) the appropriate sanction in case an employee commits fraud; 5) what defines an employee's "incapacity" and 6) the maximum number of months' salary that can be awarded to an employee as compensation for an unfair dismissal.

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.

4.6 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. 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 B.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.

Secondly, the Club collected data on the activity of the clients on its website. The Club used a web analytics platform (Kissmetrics) which provided visualization tools on how the Club members were interacting with the website. Each activity done by a member was recorded as an "event." These events included: 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, viewing a template, etc.

By merging this data with our baseline data, we are able to track intervention-related activity among the firms assigned to the treatment. Figure 2 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. Figure A.3 further presents the distribution of the number of newsletters opened and the number of events on the website. 70.6% of employers opened at least one newsletter and 36.7% had at list one event on the website.[25] 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.

5 Main Results

5.1 Employment

Table 2 presents our main results on employment. We define employment as total current staff, which is measured as the sum of permanent staff, fixed-term staff and casual staff, each of these being top coded at the 99 percentile.[26] The upper panel of Table 2 reports results of the estimation of equation 4 while the lower panel reports results from the specification that includes covariates (equation 5).

Column (1) presents results using employment in level as the dependent variable. As can be seen from the table, 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

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

[26]The precise question firms are asked is "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.

firms after the intervention.

The lower panel presents results where we add to the baseline specification the set of covariates that were selected according to the iterated lasso procedure described in section 4.4. 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. As can be seen from the table, the inclusion of these covariates does not lead to any substantial changes in the estimated coefficients on the treatment variable. For example in column (1), the estimated coefficient is 11.97 in the model with covariates compared to 11.83 in the basic regression (4). The addition of controls however substantially improves the precision of the estimates. Controlling for the covariates leads to a reduction in standard errors of around 20% when the dependent variable is expressed in level and 27% when it is expressed in logs. 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). Indeed, when the dependent variable is expressed in change from baseline, the double post lasso procedure does not lead to substantial improvements in precision.[27]

To validate inference, we also implement permutation tests using 10,000 permutations. Results are presented in Table 3. We run these tests for the level of employment and the change in employment, the two main outcome variables in Table 2. The table presents first the p-value associated with the asymptotic distribution of the t-statistic and then in the second and third columns the estimated exact p-value and its confidence interval. Results strongly validate the asymptotic distribution of the t-statistics. For example, for employment level, the F-statistic for the test of the nullity of impact on current staff in the simple linear regression model in column (1) of Table 2 is 6.31, with a p-value of 1.21%. We find that 10,000 permutations lead to 111 draws for which the computed statistic was above 6.31. This corresponds to an estimated p-value of 1.11% (column (2)). We conclude that the asymptotic inference seems valid and rely on it going forward.

To further validate the robustness of the estimated impacts, we examine the difference in the cumulative distribution between treatment and control firms for the two main outcome variables. Figure 3 presents the results. Impacts on the cumulative distribution of total current staff at endline are in the left panel while impacts on the cumulative distribution of change in total staff between endline and baseline are in the right panel. Consistent with a positive impact of the intervention on total staff, the cumulative distribution for the treatment group is always to the right of the cumulative distribution for the control group. Also reported in each panel is a confidence interval around the cumulative distribution in the control group. This confidence interval is simply defined as adding or subtracting 1.96 times the standard error of the difference of the two corresponding proportions.[28] The figure shows that the cumulative distribution for the treatment group is often outside the confidence interval. More formally, we also report the results of a test of the hypothesis of identical distributions in the treatment and control group. The test we use is the Wilcoxon-Mann-Whitney rank sum test for which we compute p-values using 10,000 permutations. For both current staff (left panel) and change in staff (right panel), the p-values are very small (3.27% and 0.84%, respectively) so that we can reject the null hypothesis of identical distributions. Table 3 presents the same comparison of p-values using the asymptotic distribution and estimated exact p-values as for the t-statistic. Results are here again very close, although the p-value using the asymptotic distribution is outside the 95% CI for the change in employment.

In Table A.7, we explore heterogeneity of impact with respect to firm size. In particular, we estimate

[27] In fact, the iterated lasso procedure does not select any variable to add in column (3).

[28] More precisely, for a given number of employee n, we built the variable corresponding to having less employees than this number. We compute the average in the control group c_n (reported as the blue line in the graph) and we run the regression corresponding to our basic specification and get the coefficient θ_n and its standard error s_n. We define the average t_n in the treatment group as $c_n + \theta_n$ and the "confidence interval" $c_n \pm 1.96 s_n$. By construction, if t_n falls into the confidence interval, the impact on the proportion of firms with less than n employees is significant.

equation (6) 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 primarily run these regressions as an additional robustness check, to corroborate that the relationship we detect is not due to a small subset of firms. 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.

Columns (1) and (2) present results for total staff in level, without and with covariates respectively. 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. When we measure employment in log, we again observe positive and significant average impacts for firms in the two smaller categories and positive but insignificant impact for the larger firms. The estimated coefficients for the smallest and medium-size 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. Finally, across all specifications in Table A.7, we always reject the hypothesis that three coefficients are jointly equal to zero.

An additional robustness check consists in reintegrating into the regression sample the firms that did not answer the endline survey for reasons that are likely related to closure. As already discussed, Table A.4 shows that there are 6.69% of such firms in the control group and significantly less in the treatment group (difference is -2.52%). 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. These results are presented in Table A.6 . Not surprisingly given the patterns of imbalance in Table A.4, the estimated impacts are even larger in this extended sample of firms. Appendix Figure A.1 reproduces Figure 3 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 3 were more pronounced 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.

Table 4 explores the correlation between the number of "actions" taken during the intervention and endline employment. As before, an action corresponds to either the opening of a newsletter or any "event" recorded on the website. As the estimated causal ITT is positive, we may expect that we will also detect a positive and increasing relation between the number of actions and employment. Before we proceed, it is worth stressing that the parameter we estimate here is, of course, a mixture of the causal effect of the intervention and selection effects of firms who had a specific interest in consulting the newsletters or logging into the website. There might thus be many spurious sources of correlations in that firms with positive or negative employment shocks might show differential interests in the services offered by the intervention. In other words, our experimental design does not allow us to causally test the impact of differential information dosage as dosage was not randomized.

With this important caveat in mind, we proceed in categorizing treatment firms into 3 groups based on the number of actions they took during the intervention: [1,5), [6,15), and 15 or more. In columns (1) to (2), we restrict the sample to firms assigned to treatment; in other words, the "reference" category in these columns is the set of treatment firms with no action related to the intervention. We also consider regressions that include the entire sample, adding to the reference category the experimental control group (columns (3) and (4)). Odd columns include covariates while even columns do not. All specifications deliver similar findings. 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 (columns (3) and (4)). Moreover, while the coefficients are all positive, there is no sign of a relation that would be increasing in the number of actions taken.

In the lower panel of Table 4, the explanatory variable is defined as a dummy that equals 1 if the firm took at least one action during the intervention period and 0 otherwise. We again observe a positive relationship. The estimated coefficients in column (3) or (4) are respectively 13.10 and 15.24 and are highly significant. Finally, in the last two columns of Table 4, we estimate the same regression as in columns (3) and (4) but use treatment assignment as an instrument for taking at least one action during the intervention. This could loosely be considered as a LATE estimate. The estimated coefficients are 15.43 (column (5), with covariates) and 15.26 (column (6), no covariates), very close to the OLS results in columns (3) and (4).

5.2 Hirings and Dismissals

Our 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. Results appear in Table 5. 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. As can be seen from the Table, the results we obtain are not consistent with our previous finding. In particular, we do not detect any economic or statistical impact. This is not due to our estimates lacking accuracy: while the estimated coefficients are small, the standard errors are as well. Adding covariates do not change much the value of the estimated coefficients and, as expected, improve standard errors.

The results in Table 5 are puzzling.[29] 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 hiring and firings over the last 6 months. However, in practice, the endline surveys were not always completed after 6 months. In the left panel of Appendix Figure A.2, we report the distribution of durations between baseline and endline surveys. The figure shows that a significant share of surveys were 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 5.

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

[29] Table A.8 present additional results when breaking down the number of workers hired or fired by categories of contract. Results do not change much, except that we detect, for fixed-term contract, a small significant positive impact on the number of workers fired.

report the number of workers currently employed at the firm. The right panel of Appendix Figure A.2 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 5.

6 Adjustment Cost, Optimal Level of Employment and Workforce Composition

As we discuss in our conceptual framework in Section 3, the impact we detect on employment level can be linked to several underlying mechanisms. A first mechanism is via a *reduction in current adjustment costs* (see Panel (b) of Figure 1). Firms might desire an adjustment of their workforce to an optimal level but do not implement it because that is too costly; the intervention may give firms the ability to make such adjustment at a reduced cost. We expect an heterogeneous impact of the intervention on employment levels: the effect will be positive for firms that would like to increase their labor force and negative for firms that would like to decrease it. Under this channel, treated firms should be closer to their optimum employment level at endline. A second mechanism is via a *profitability channel* (see Panel (c) of Figure 1). A better knowledge of labor regulation may allow firms to make more out of their workforce and reduce their wage bill for a given level of employment. Under this channel, the optimal level of employment will increase at treated firms and this should be partly transmitted into an increase in actual employment. Moreover, the impact on employment will be more homogeneous across firms compared to the previous case as it is either positive or zero.

6.1 Findings

Employers were directly asked in the endline survey about the optimal level of employment at their firm:"*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 before: workers under permanent contracts, workers under fixed term contract and casual labor. We define optimal employment level by aggregating answers for these three categories. We also consider desired staffing adjustment at endline, which we define as the difference between the optimal level of employment and actual employment (both measured at endline).[30] The endline survey also asks employers 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.

Results are presented in Table 6. The upper panel provides results of the regression without control variables while the lower panel adds covariates following the selection procedure described in section 4.4. Column (1) considers the optimal employment level. The estimated coefficient in the upper panel

[30] 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.

is 9.38.[31] This is a large increase corresponding to 11.5% of the control mean. This result is not affected by the introduction of covariates in the lower panel. Column (2) considers the absolute value of the desired adjustment, defined as the absolute value of the difference between the optimal and actual staff level: $|n^* - n|$.[32] The table shows the coefficients are small and not significant, whether or not we include controls. These results speak against a reduction in current adjusment costs as the sole mechanism for our main findings. Indeed, if only this mechanism were at play, we would have expected the optimal level of employment to be unchanged and the absolute value of the difference between optimal and actual employment to reduced.

Column (3) assesses how the intervention impacted knowledge of labor regulation, where knowledge is proxied for based on 7 questions asked in the endline survey.[33] 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. 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.

Column (4) and (5) report on how the intervention affected the perception of labor regulation as a constraint to increasing or decreasing employment. The shares of control firms agreeing that labor regulations are a constraint in decreasing and increasing staff are 19.43% and 26.14% respectively. The estimated impacts of the intervention on these perceptions are negative, indicating that the information provided led to a reduction in the proportion of firms perceiving 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 other words, firms exposed to the treatment appear to view labor regulation as less of a constraint in their hiring of workers.[34]

Another way to assess the relative importance of the two potential mechanisms underlying our main findings is to look at quantile treatment effects. Indeed, while both mechanisms predict positive effects among firms that desired sufficiently larger employment levels at baseline, the mechanisms make different predictions for firms that desired to reduce their employment levels at baseline: the effect on employment for these firms could not be positive under a pure reduction in current adjustment costs story, while it could be positive under the profitability channel.

Quantile treatment effects have to be interpreted carefully. They are a useful tool to analyze heterogeneous treatment effects and link this heterogeneity to the predictions of a theoretical model, as shown for example in the analysis of the Job First program in (Bitler et al., 2006). However, they are simply the differences between quantiles in the treatment and control groups and cannot be interpreted as treatment

[31] The test of a zero average treatment impact leads to a Fisher statistic of 3.83 with a p-value of 5.05%. Running a permutation test on this simple regression with 10,000 permutations leads to 490 estimations with a larger statistic and thus an estimated p-value of 4.90%.

[32] Looking at the distribution of this variable reveals that the desired adjustment is zero for 57% of respondents. A natural concern is whether there is measurement error and whether this might affect our results. The questions about current and optimal level of staff are asked one after the other in the survey. This might lead some firms, say in proportion $(1-p)$, to simply report the same figures for current and optimal staff levels. In such a case, there would be an attenuation bias and the estimated coefficients in the regression are scaled down by a factor p.

[33] 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.

[34] The lower panel adds covariates, following the procedure described in Section 4.4. Note that no covariates are selected by the procedure in column (3); only one covariate is selected in column (4), leading to a very modest change in the estimation result.

effect at quantile. Such an interpretation relies on the demanding rank preservation assumption, which means that the intervention does not change the rank of firms with respect to the considered outcome variable in the treatment and control groups. While we do not make the rank preserving assumption, we also notice that there is nothing in the conceptual framework that would contradict such an assumption.

The results are presented in Figure 4. The upper panel present results for change in employment when the sample is limited to endline survey respondents while the intermediate panel also include firms that did not complete the endline survey due to probable closure. The figure clearly shows that we never observe a negative quantile treatment effect. Moreover, when we also include firms which are likely to have closed down (intermediate panel), we observe some positive quantile treatment effect even for the lowest quantiles. The bottom figure presents the quantile treatment effects for the difference between optimal and actual employment at endline, $n^* - n$. Again, the findings are inconsistent wit a pure reduction in current adjustment costs mechanism. Indeed, this mechanism, we would have expected a positive impact first (for example going from -10 to -5), then a zero effect, and finally a negative impact (for example going form 10 to 5).

On the opposite, in case of an increase in profitability there should be almost no impact on $n^* - n$. Indeed, Panel (c) of Figure 1 shows that there is a non zero impact on such adjustment variable only for firms with desired employment levels between $\underline{n}(1)$ and $\underline{n}(0)$ and between $\overline{n}(1)$ and $\overline{n}(0)$.

To complement the analysis in Figure 4, we also make use of information collected at baseline and look at *conditional treatment effects*. In particular, just like at endline, firms were asked at baseline about both their current and optimal staffing levels. This enables us to build variables related to the desired adjustment at baseline, in the same way as what we did for the endline survey. We consider a partitioning of the sample according to this baseline desired level of adjustment of employment and estimate equation (6). 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 A.5). Panel A of Table 7 examines the differential effect of the intervention across these three types of firms on endline employment (columns (1)), optimal level of employment (columns (2)), knowledge of labor regulations (based on the seven quizz questions asked at endline; column (3)) and the two labor regulation perception variables (columns (4) and (5)).

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 (columns (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 baseline. We however also find 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 against speak against a pure reduction in current adjustment costs explanation for our findings. And indeed, in columns (2), we find that the impact on the optimal employment level is positive significant at the 10% level for firms these firms and about of the same magnitude as the impact on actual employment. In other words, for this category of firms, the change in actual employment are a reflection of the change in optimal employment.

Columns (4) and (5) present results on the perception of labor regulation as a constraint in adjusting employment downward (column (4)) and upward (column (5)). Remarkably, 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. These results do suggest that, while our evidence as a whole in Table 6, Figure 4 and Panel A of Table 7 strongly speaks against our results being solely driven by a reduction in current adjustment costs, the intervention did however also change firms' perceptions about these costs. Recall however from our conceptual framework that, as long as these changes in perceptions are long lasting, they will also impact n^*.

While not incorporated in our conceptual framework, better information about labor regulation may also lead to substitution between different types of labor contracts. In Table 8, we re-estimate the impact of the intervention, but break down total staff into workers employed under different contract types. We consider both impacts on the number of workers under each contract type (permanent, fixed, casual; left panel) as well as on the share of the total workforce under each contract type (right panel). The upper panel does not include controls (equation 4), while the lower panel does (equation 5). Starting with the left panel, we see that all the estimated impacts are positive but not always significant. We however reject in both the upper and lower panels the joint nullity of the three estimated coefficients.[35] The largest impact is obtained for the staff working 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. While the point estimates is smaller (3.5), this corresponds to a larger proportional increase (about 25%) compared to the control mean. We detect limited impact on the size of the casual workforce. However, here again given the low control mean (4.3), this corresponds to a large relative impact of around 26%. Overall, we do not observe much evidence that the intervention led to substitution effects across type 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.

7 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 B and C of Table 7.

In particular, 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).[36]

[35]To run the test, we estimate each equation separately but then also estimate their joint variance matrix; when doing so, we introduce the joint set of covariates selected separately in each lasso regression as a common set of regressors across all equations.

[36]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

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

Overall, as seen in Panel B of Table 7, we do not find evidence of much heterogeneity in the impact of the intervention on employment (actual or optimal) or knowledge and perception of labor regulation constraints based on baseline knowledge. However, the few statistically significant interaction terms that emerge in Panel B are consistent with our priors. In particular, we find that a greater lack of knowledge of labor regulation at baseline is associated with a larger impact of the intervention on endline staffing; this effect is however not significant. We also find that more 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 C of Table 7, 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.[37]

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.

As shown in Panel C of Table 7, for all but one outcome variable, we cannot reject the hypothesis of similar impacts across the two categories of firms. However, the table shows that impacts on actual and optimal employment at endline are larger and only significant for firms which outsource at least part

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.

[37]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 4.4 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 variable interacted with the treatment variable was selected. This somewhat improves our confidence in studying heterogeneity along this dimension despite it being measured at endline.

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 C of Table 7 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 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.

8 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 thus 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. An interesting hypothesis is related to a potential rent-seeking role by these intermediaries: 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 for their current and potential future clients. 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.

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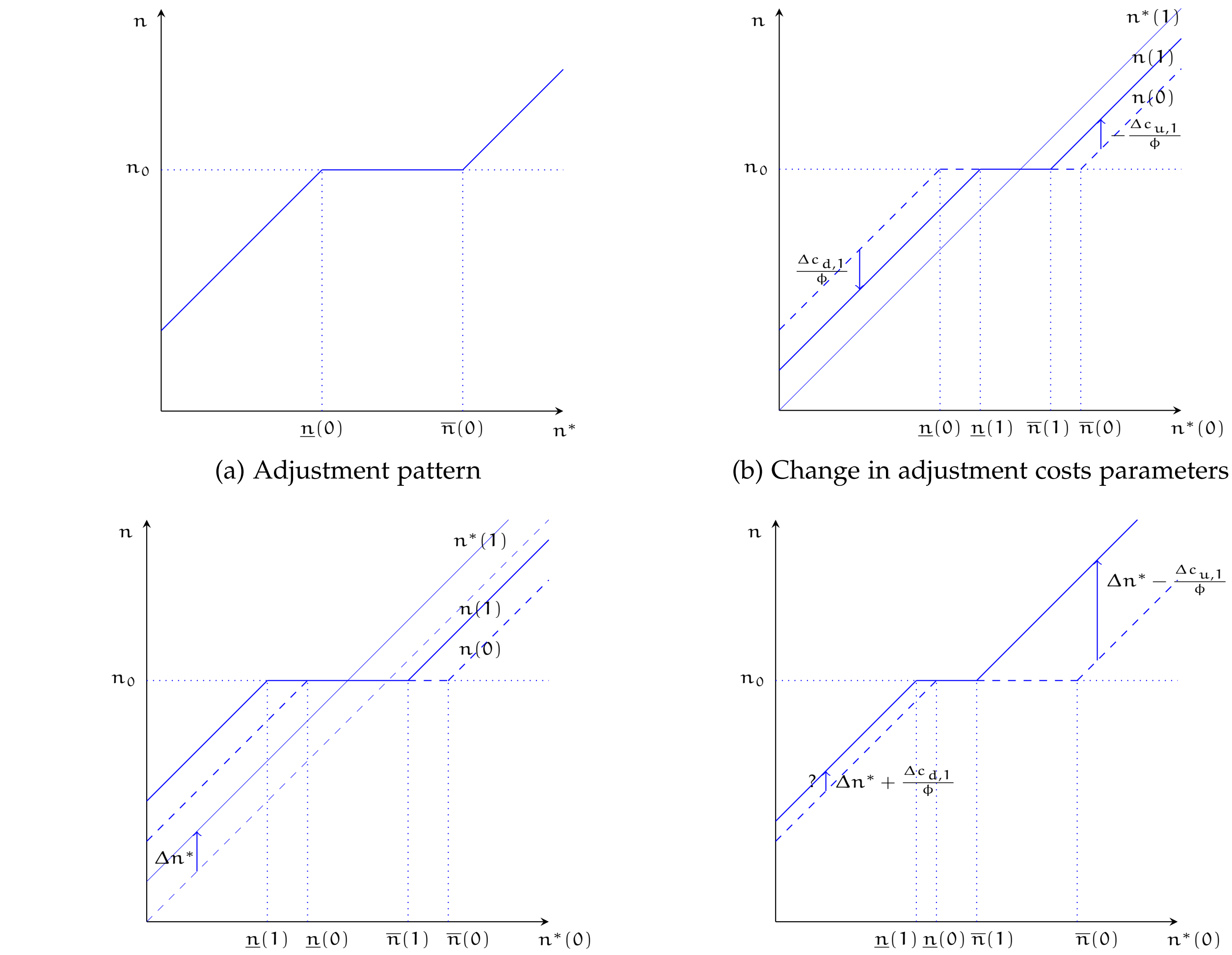
9 Figure and Tables

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



(a) Adjustment pattern

(b) Change in adjustment costs parameters

(c) Change in current management and future parameters

(d) Both changes

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

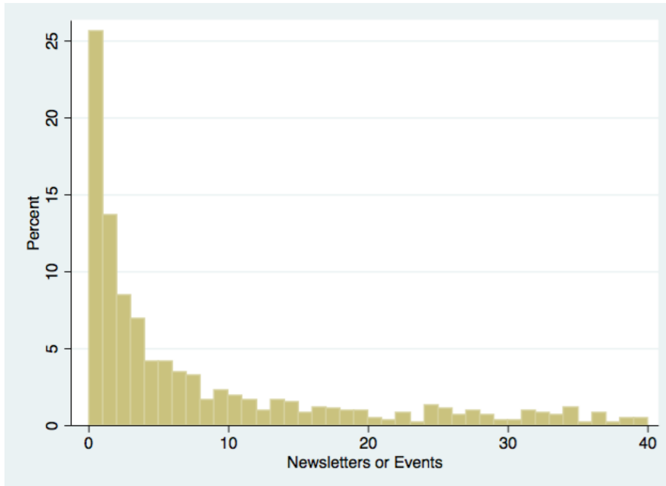
- $\underline{n}(0) = n_0 - \frac{c_{d,1}(0)}{\phi}$ and $\overline{n}(0) = n_0 + \frac{c_{u,1}(0)}{\phi}$

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

- $\underline{n}(1) = n_0 - \frac{c_{d,1}(1)}{\phi} - \Delta_n^*$ and $\overline{n}(1) = n_0 + \frac{c_{u,1}(1)}{\phi} - \Delta_n^*$

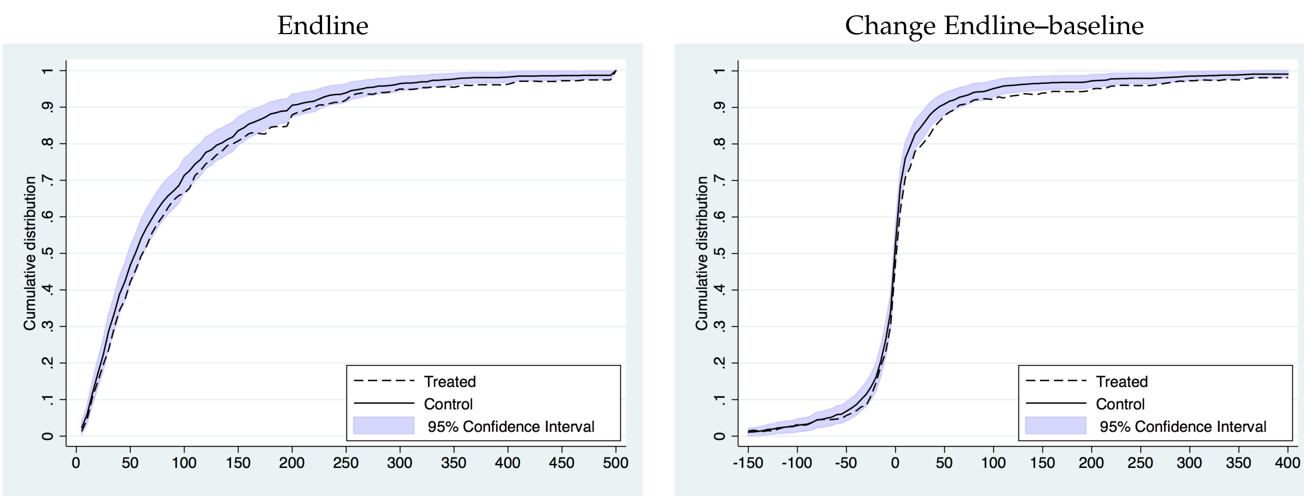
case (b): $\Delta_n^* = 0$; case (c): $c_{d,1}(1) = c_{d,1}(0)$ and $c_{u,1}(1) = c_{u,1}(0)$

Figure 2: Take-up of the experiment



Share of firms with at least one action: 76.5%

Figure 3: Impact on cumulative distribution and rank sum tests



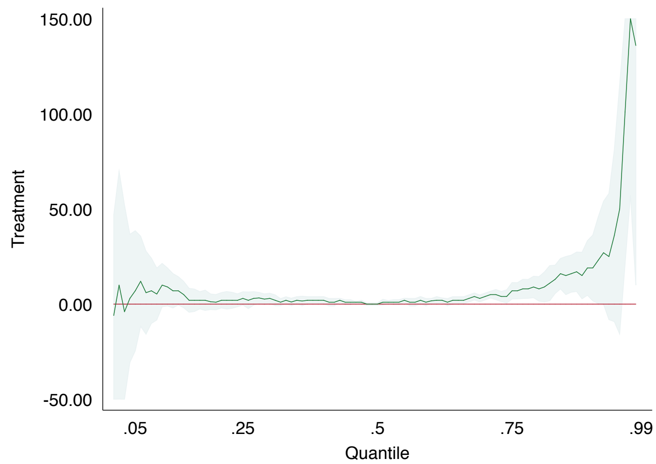
Mann Whitney test: p values obtained from 10,000 permutations within strata[a]

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

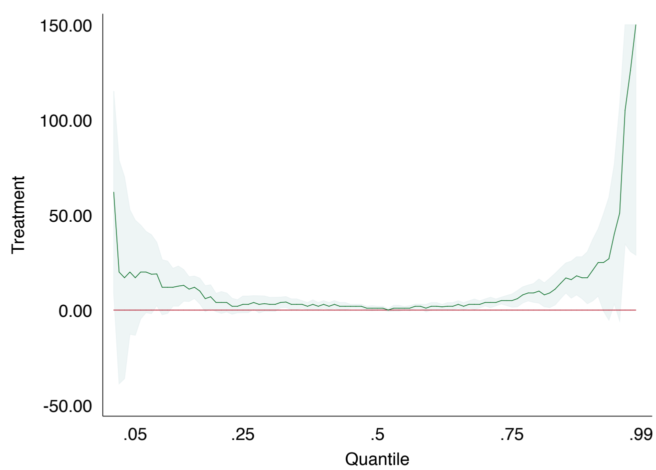
The solid line provides the average m_0 of the outcome variable y in the control group of the dummy variable $d = 1(y \leqslant x)$, with x the value on the x-axis.
The dashed line provides the average m_1 of d in the treatment group.
The shaded area is delimited by adding to m_0 $\pm 1.96std$ where std is the standard error of the difference between m_1 and m_0
The p-value is computed as the ratio of the number of times the statistics from a permuted assignment variable was found larger than the statistic obtained with the true assignment variable to the total number of permutations. Asymptotic p-values are very close to those obtained from permutations: 3.33% and 0.97% respectively - see table 3.

Figure 4: Quantile treatment effects

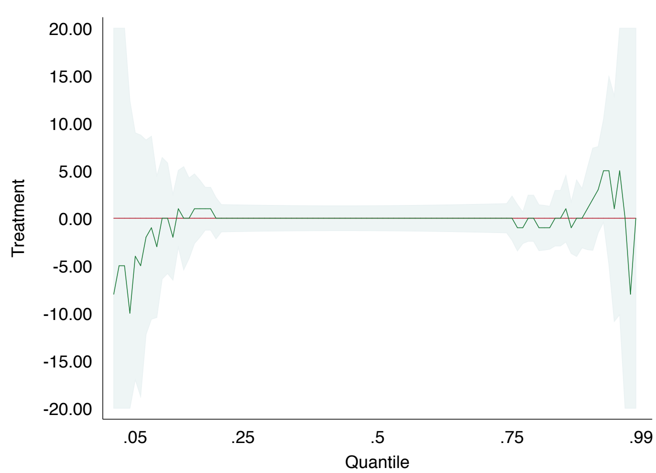
Change in employment survey respondent



Change in employment survey respondent and firms likely to have shut down



Desired adjustment: $n^* - n$



The figures report quantile treatment effects as a function of the quantile (solid line) as well as the 95% confidence interval (grey area).

Table 1: 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	

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 2: Employment results

	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)
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

Columns 1 and 2 in the upper panel provide results for the estimation of equation (4) for endline total staff. Columns 3 and 4 in the upper panel estimate the same regression for change in total staffing between endline and baseline.

The lower panel provides results for the same outcome variables but estimate equation 5, i.e. adding control variables selected according to the procedure described in Section 4.4.

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

Table 3: Randomized inference

	t-test			rank sum		
	Asymptotic	Permutation		Asymptotic	Permutation	
		$\widehat{p}$	95% CI		$\widehat{p}$	95% CI
Employment level	1.21%	1.11%	[0.94%,1.34%]	3.33%	3.27%	[2.83%, 3.52%]
Change in employment	0.22%	0.17%	[.10 %, .27%]	0.96%	0.84%	[.93%, 1.36%]

The table presents for each test we perform the p-values (in %) computed using the asymptotic approximation or the confidence interval from a permutation test using 10,000 permutations.

Table 4: Direct evidence on employment and use of resources provided by the intervention

# events in 1-5	7.70	13.45	12.11**	13.47*		
	(8.38)	(9.50)	(5.96)	(7.00)		
# events in 6-15	13.56	19.60*	14.40*	19.62**		
	(9.57)	(11.28)	(7.46)	(9.27)		
# events > 15	9.66	14.58	14.15**	14.61*		
	(8.62)	(10.03)	(6.01)	(7.71)		
p-value	0.51	0.28	0.01	0.02		
# events > 0	8.44	15.22*	13.11***	15.24***	15.43***	15.26**
	(7.04)	(8.15)	(4.06)	(5.04)	(4.79)	(6.06)
Method			OLS			IV
Control variables	Yes	No	Yes	No	Yes	No
Only treatment group	Yes	Yes	No	No	No	No
Observations	722			1466		

The upper panel reports on the relationship between use of the Law@Work Club services (number of recorded events) and employment. The two first columns only include treated firms ("control" group is therefore firms that did not use the site) while the following columns also include the experimental control group.
The lower panel present the same results but use a dummy variable that equals 1 for firms with at least one event, 0 otherwise.
Columns 1 to 4 are estimated using OLS. Columns 5 and 6 present IV results that use the random assignment variable as the instrument.

Table 5: Hiring and firing over the last 6 months

	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	

The table presents estimation results without covariates (equation 4; columns 1 and 3) and with covariates (equation 5; columns 2 and 4) selected following the procedure described in Section 4.4.
Robust standard errors in parenthesis. * corresponds to significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 6: Optimal level of employment and perception of LR

	Endline			Are LR constraining you from	
	Optimal level	|Desired adjustment| ^{a}	Knowledge score	Decreasing staff	Increasing staff
	(1)	(2)	(3)	(4)	(5)
Without control variables					
Treated	9.38*	1.39	1.29	-3.26	-5.23**
	(4.79)	(1.54)	(1.02)	(1.98)	(2.19)
With control variables					
Treated	9.72**	1.40	1.37	-3.26	-5.59**
	(3.92)	(1.52)	(0.94)	(1.98)	(2.18)
Control mean	81.37	0.91	46.08	19.43	26.14
Observations	1450	1443	1510	1489	1497

a Desired adjustment is defined as |optimal - current staff| at endline.
The upper panel presents results of the estimation of equation (4) while the lower panel presents the results of the estimation of equation (5) (following the procedure described in section 4.4).
Robust standard errors in parenthesis. * corresponds to significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 7: Heterogeneity of impact with respect to desired baseline adjustment, baseline knowledge and external HR services

	Actual level	Optimal level	Knowledge score	LR constraints in Decreasing staff	LR constraints in Increasing staff
	(1)	(2)	(3)	(4)	(5)
Desired adjustment at baseline					
< 0	15.07*	13.26*	-1.31	-7.80**	0.42
	(7.82)	(7.59)	(1.82)	(3.98)	(4.31)
$= 0$	7.03	2.91	1.78	-2.45	-6.25*
	(5.57)	(5.75)	(1.44)	(3.00)	(3.27)
> 0	15.62**	15.22**	3.07*	-0.31	-9.61**
	(6.67)	(7.52)	(1.70)	(3.51)	(4.01)
p-val global	0.01	0.06	0.15	0.21	0.02
p-val same	0.54	0.35	0.20	0.35	0.22
Knowledge at baseline (share among answers)					
Treated	11.99***	9.65**	1.34	-2.84	-5.60**
	(3.77)	(3.93)	(0.94)	(1.99)	(2.19)
Do_not_know	1.65	1.27	1.05	-0.12	2.12
	(4.08)	(4.34)	(1.01)	(2.10)	(2.36)
Pro-employer	-6.75*	-6.96*	0.41	0.22	-2.03
	(3.95)	(4.19)	(1.03)	(2.26)	(2.41)
Pro-employee	-1.41	-1.94	-1.11	-3.21	-1.70
	(3.98)	(4.21)	(0.98)	(2.01)	(2.30)
p-val same	0.18	0.21	0.38	0.41	0.41
External HR services					
With	14.02***	12.50**	0.21	-4.68*	-7.68***
	(4.63)	(5.01)	(1.17)	(2.63)	(2.81)
Without	9.52	5.16	3.61**	-0.47	-2.02
	(6.44)	(6.37)	(1.60)	(2.87)	(3.40)
p-val global	0.00	0.03	0.08	0.20	0.02
p-val same	0.57	0.37	0.09	0.28	0.20
Control mean	81.11	82.43	46.08	19.43	26.14
Observations	1466	1450	1510	1489	1497

The table presents the results of the estimation of equation (6) in which the treatment variable has been interacted with variables corresponding to a partitioning of the sample in three categories of baseline desired adjustment (upper panel), type of knowledge at baseline (intermediate panel) and use of external HR services (lower panel).

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.

Table 8: Employment composition

	Levels			As share of total current staff		
	Permanent	Fixed term	Casual	Permanent	Fixed term	Casual
	(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

The table decomposes total staff at endline into its three main categories: permanent contract, fixed term contract and casual labor. The dependent variables are expressed in levels in the first three columns and in shares of total current staff in the last three columns.

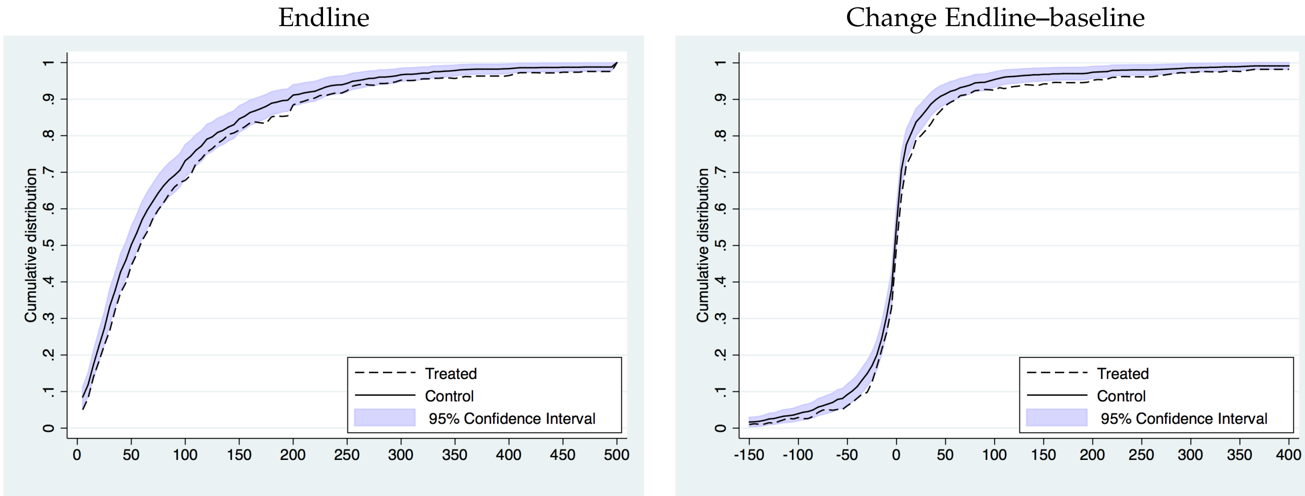
The upper panel presents estimation results without covariates (equation 4); the lower panel present estimation results with covariates (equation 5) (following the procedure described in section 4.4).

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

p-value joint nullity corresponds to the test of a simultaneous null effect on each of the three estimated impacts (in levels or shares).

A Appendix Figures and Tables

Figure A.1: Impact on cumulative distribution and rank sum tests

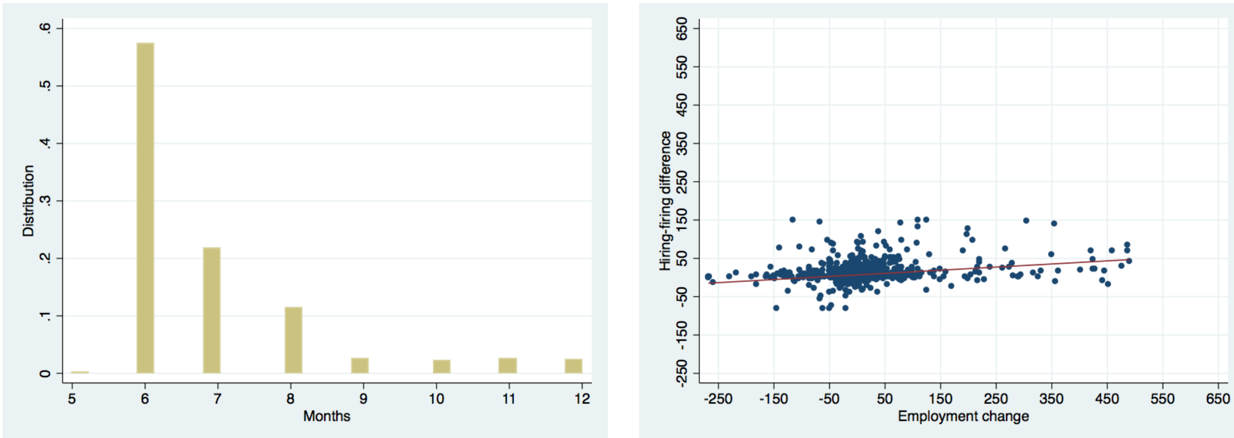


Mann Whitney test: p values obtained from 10,000 permutations within strata[a]

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

The solid line provides the average m_0 of the outcome variable y in the control group of the dummy variable $d = 1(y \leqslant x)$, with x the value given on the x-axis.
The dashed line provides the average m_1 of d in the treatment group.
The shaded area is delimited by adding to m_0 $\pm 1.96 std$ where std is the standard error of the difference between m_1 and m_0.
The p-value is computed as the ratio of the number of times the statistics from a permuted assignment variable was found larger than the statistic obtained with the true assignment variable to the total number of permutations. Asymptotic p-values are very close to those obtained from permutations: respectively .49% and 0.15%
Compared to Figure 3, the sample used for this figure includes firms that we were unable to reach at endline due to probably closure.

Figure A.2: Duration between the date of survey completion and random assignment and relation between hiring-firing and endline-baseline changes in employment



The left panel presents the 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) .
The right panel presents the scatter plot of the change in employment as measured by the difference between hiring and firing and the change in employment as measured by the difference between endline and baseline total staff.

Table A.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 A.2: Person who answered the survey

Endline	Baseline: Main person	Advisor to main person	Neither one or the other	Total
Main person	785	273	2	1,060
Advisor to main person	225	234	4	463
Total	1,010	507	6	1,523

The precise question asked to the person who answered the survey was *To what extent are you involved in the hiring/firing decisions of this business?*.

Table A.3: Reasons for not answering the endline survey

Status	# firms	percentage
Online survey completed	155	8.50
Phone survey completed	1,355	74.29
Total	1510	82.79
Call back - reschedule	55	3.02
Asked to have email sent again	46	2.52
Not interested/Refused	98	5.37
Phone survey partially completed	5	0.27
Online survey partially completed	11	0.60
Total	215	11.78
No answer (6 attempts)	35	1.92
Repeatedly engaged/Line out of service	18	0.99
Business closed down	15	0.82
Wrong number/number does not exist	31	1.70
Total	99	5.43

Reported in italics are reasons we consider to be related to probable firm closure.

Table A.4: Endline respondent and reasons for not answering

	Survey completed	staff non missing	Survey not completed: ≈ Refused	≈ business closed
	(1)	(2)	(3)	(4)
Treatment	0.44	-2.41	2.08	-2.52**
	(1.77)	(1.86)	(1.51)	(1.06)
Control mean	82.57	81.58	10.75	6.69

Column (1) provides the result of the regression of an endline survey completion dummy on the treatment variable.
Column (2) adjust the survey completion dummy to account for partial non response on staff variables.
Column (3) and (4) explore reasons for non completion of the endline survey: likely refused or probable business closure (see Table A.3).

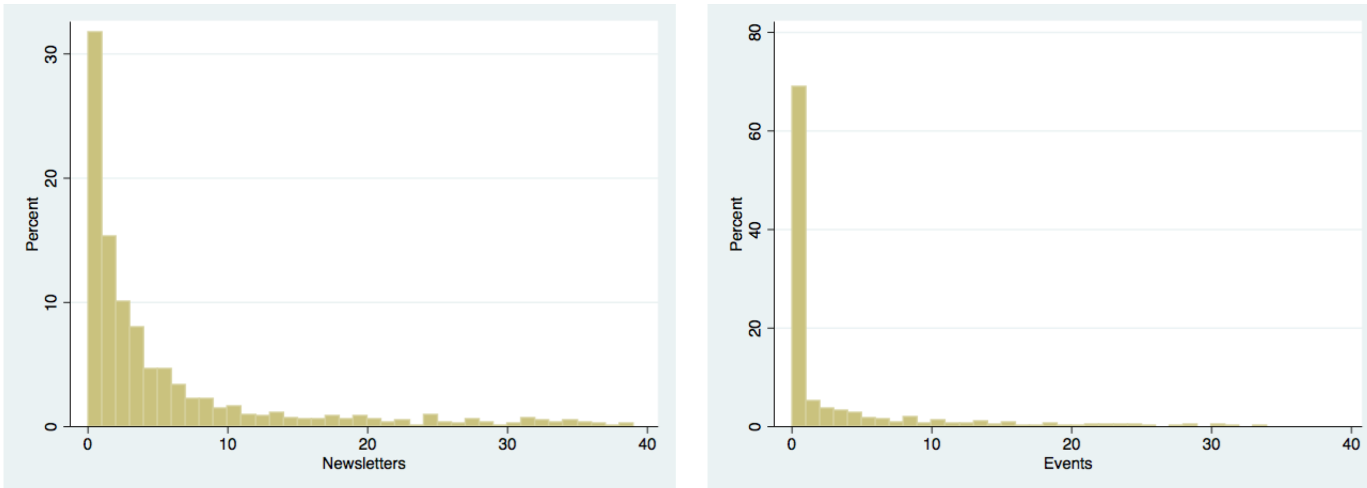
Table A.5: Balancing on baseline data

	Whole sample			Endline respondent		
	Cont	Coef	Sign.	Cont	Coef	Sign.
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	.
Obs Test	1824	97.8	.	1510	88.1	.

The table reports the control mean average as well as the regression coefficient of the characteristic on the treatment variable.

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.

Figure A.3: Take-up of the experiment



Share of firms with at least one newsletter	Share of firms with at least one event
70.6	36.7

Table A.6: Employment results accounting for probable closure

	Endline		Endline–baseline	
	(1)	(2)	(3)	(4)
Treated	13.33***	14.27***	14.01***	13.85***
	(4.55)	(3.66)	(3.67)	(3.64)
Add covariates	No	Yes	No	Yes
Control mean	74.96		-3.59	
Observations	1565			

This table replicates the level results in Table 2 but add to the sample firms that did not complete the endline survey due to probable closure.
Robust standard errors in parenthesis. * corresponds to significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.7: Heterogeneity of impact on total staff with respect to firm size

	Level		Log	
	(1)	(2)	(3)	(4)
< 50	10.14**	9.60**	0.13*	0.12*
	(4.90)	(4.83)	(0.07)	(0.06)
$[50, 100]$	16.28**	17.04**	0.14**	0.16**
	(7.65)	(7.65)	(0.07)	(0.07)
> 100	7.96	6.92	0.05	0.07
	(8.83)	(7.55)	(0.07)	(0.06)
With covariates	No	Yes	No	Yes
Control mean	81.11		3.92	
p-value global	0.02	0.02	0.05	0.01
p-value same	0.73	0.61	0.57	0.59

The table presents the results of estimation of equation (6) in which the treatment variable has been interacted with variables corresponding to a partitioning of the sample in three size categories defined at baseline: 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.

Table A.8: Hiring and firing over the last 6 months

	Total		Permanent		Fixed term		Casual	
	Hired	Fired	Hired	Fired	Hired	Fired	Hired	Fired
Without control variables								
Treated	1.17	0.84	0.74	1.06**	0.32	-0.41	0.58	0.34
	(1.26)	(0.67)	(0.63)	(0.46)	(1.04)	(0.47)	(0.47)	(0.23)
With control variables								
Treated	1.06	0.81	0.84	1.07**	0.14	-0.47	0.67	0.31
	(1.20)	(0.66)	(0.61)	(0.45)	(1.00)	(0.46)	(0.46)	(0.23)
Control mean	13.39	5.86	5.45	3.07	5.71	2.26	2.46	0.73
Observations	1443	1454	1486	1484	1475	1481	1459	1464

Hiring corresponds to the number of workers entering the firm over the last 6 months. Firing corresponds to the number of workers dismissed during the last 6 months; firing does not account for workers who quit or workers whose contract ended and was not renewed during the last 6 months.
The upper panel presents estimation results without covariates (equation 4) while the lower panel presents results with covariates (equation 5) (following the procedure described in section 4.4).
Robust standard errors in parenthesis. * corresponds to significance at the 10% level, ** at the 5% level and *** at the 1% level.

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

Table B.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

continued . . .

Table B.1: List of newsletters

Date	Day	Title	Topic	All emails tracked?	Group 1	Group 2	Group 3	Group 4	Group 5
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
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

continued . . .

Table B.1: List of newsletters

Date	Day	Title	Topic	All emails tracked?	Group 1	Group 2	Group 3	Group 4	Group 5
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
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

continued ...

Table B.1: List of newsletters

Date	Day	Title	Topic	All emails tracked?	Group 1	Group 2	Group 3	Group 4	Group 5
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
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 B.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 B.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 B.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 B.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 conciliation, 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 B.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