Social Insurance Reform and Workers' Compensation*

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Abstract

This paper uses matched employer-employee data to examine the wage responses to a mandatory social insurance reform program in Ethiopia. By relying on firm-level differences in alternative pre-reform contributory schemes, we examine the extent to which employers shifted the cost of social insurance to workers in terms of lower wages. We find partial switching that varies by workers' employment history. Wages of recent hires by treatment firms show a decline proportional to the mandatory employer contribution rate. Wages of incumbent workers, however, continued to rise after the reform but at a slower rate relative to the control group. The post-reform reduction in wages is larger and significant for production workers and employees of low-wage industries. Treatment workers also experienced reductions in bonuses after the reform while their allowances remained intact.

Key Words: Wage, social insurance reform, wage shifting, firms, Ethiopia.

1. Introduction

There is growing research interest on the implications of social security reforms for welfare and labor market outcomes. The need for more evidence on such implications remains critical in the context of developing countries where social protection programs, which come in the form of contributory social insurance and noncontributory social assistance schemes, have become increasingly popular (Chetty and Looney, 2006; Jung and Tran, 2012).[1] The labor market implications of government mandated social insurance programs that often cover workers in the formal sector hinge primarily on who ultimately bears the cost of social security as it may elicit different responses from employers and employees. Measurement of these responses is crucial in determining whether social insurance reforms entail distortions and inefficiencies in the labor market, and the degree of participation of the labor force in the promised benefits. It is thus unsurprising that concerns remain about the unintended consequences of social protection programs that could potentially stymie the primary objective of consumption smoothing for broader sections of a country's labor force (Levy, 2008). Theoretical models for examining the labor market consequences of social insurance focus on the subjective valuation of the expected benefits in the eyes of employees. If workers have confidence in the promised benefits and consider employers' contributions as deferred cash income, they may accept equivalent wage cuts that would stave off an increase in unit labor cost (Summers,1989; Gruber, 1997). This would prevent distortions in the labor market and leave employment levels largely unaffected by social insurance. Other conditions that allow full switching of the cost of social insurance to workers' wages include inelastic labor supply or infinitely elastic labor demand (Gruber, 1997).

Estimating the effects of social insurance programs on wages and employment is, however, fraught with challenges as the payroll taxes that fund such schemes are often proportional contributions with nation-wide mandates. A widely used identification strategy relies on cross-firm variation in the degree of compliance with a social security reform based on the observed employer contribution rates, i.e., total employer contribution relative to the firm's wage bill, which often differs from the statutory contribution rate (Gruber, 1997; Kugler and Kugler, 2009;

[1] See also Palacios and Robalino (2020) on recent policy issues on social insurance and assistance.

Bennmarker, Mellander and Öckert, 2009). An important challenge in using variation in the empirical contribution rate is measurement error in firm-level wage rate given that wage enters both sides of the econometric model. Equally important in this approach is the difficulty to isolate adjustments in the firm's compensation structure in response to social insurance reform from adjustments in the skill composition of the firm's workforce as a given firm-level mean wage rate could be consistent with different personnel policies on compensation, hiring and retention. Relying on firm-level data also implies ignoring potential heterogeneity in the effects of social insurance reform across workers based on personal and labor market characteristics. Other researchers attempt to identify the labor market effects of such reforms by using aggregate data at the level of cities or larger administrative units to take advantage of statutory differences in payroll taxes across locations within a given country or variation in enforcement intensity (Alemida and Carneiro, 2012; Curces, Galiani and Kidyba, 2010). Obviously, such aggregate studies cannot shed any light on differences in firm-level responses to social insurance within a given locality, and they certainly cannot capture impact heterogeneity across individual workers.

This paper contributes to this literature by using matched employer-employee data to investigate the labor market implications of a social security reform program introduced by the Ethiopian government in mid-2011. The reform mandates pension and disability benefits for private sector employees — benefits that were previously offered exclusively to government employees. While this scheme constitutes a major expansion of social security in Ethiopia, it only covers permanent employees in the formal private sector. We focus particularly on adjustments in wages in response to the reform among formal private manufacturing firms. The paper is among a few studies including Anderson and Meyer (2000), Gruber and Kugler (1991), and Kugler (2005) that use worker-level data to examine the extent to which employers switch the cost of social insurance to workers' wages. Existing worker-level studies on the wage effects of payroll taxes typically do not control for workers' human capital with the exception of Kugler (2005). The latter study, however, relies only on worker-level data and does not control for firm heterogeneity in pay structure which could potentially bias the estimated reform effects. Taking advantage of the matched employer-employee data that we collected in 2016 for this project, we estimate adjustments in wage rates following the social insurance reform while controlling for firm specific factors as well as variation in human capital and personal characteristics of their employees. Different from previous studies, our identification strategy relies on a key provision under the 2011 reform that allows two different systems of social insurance to co-exist. One of them pertains to a group of firms which had voluntarily established so-called “Provident Funds” before the 2011 reform as a form of social security for their employees. The other one is the new scheme introduced by the 2011 reform and managed by the Private Organizations’ Employees Social Security Agency (POESSA) of Ethiopia. While details of the institutional settings will be provided shortly, firms with pre-existing provident funds serve as the control group given that a contributory scheme was already in place before the reform. Firms without provident funds are the treatment group as they are forced to make such contributions for the first time under the new pension law. Thus, we use a difference-in-differences estimator to estimate the wage effects of the reform using data on both groups of firms before and after the reform.

Our analysis shows that employers most impacted by the social insurance reform due to the absence of contributory benefits scheme before the reform were able to shift a substantial part of the cost of social insurance to workers in the form of lower wages. There are, however, important differences across groups of workers. Relative to the control group, we find that workers hired after the reform by treatment firms were offered lower wages that are proportional to the mandatory employer contribution rate. In contrast, for incumbent employees who had been on firm payrolls at the time of the reform, treatment firms reduced wage growth gradually allowing them to partially offset their contributions. The reduction in real wages among workers of treatment firms was concentrated among production workers and employees of low-wage industries, which is consistent with our expectation that the reform has significantly increased the unit labor cost of treatment firms. The employment effect of the reform was thus minimal and appears to dissipate subsequently.

The paper is organized as follows. Section two highlights the institutional background and sets out the context while section three describes the survey design and data. Section four discusses the econometric models to be estimated. Our main findings and robustness checks are discussed in sections five and six, respectively. Section seven concludes the paper.

2. Institutional Background

Before the June 2011 reform, pension and disability benefits were provided exclusively to government employees (civil servants, armed forces and employees of parastatals) through a social security scheme established in the 1960s. The vast majority of urban employees thus had no formal protection against income shocks from unemployment, workplace injury or old age. Similar to many African countries, the Ethiopian government started to introduce a number of social protection programs in the mid-2000s.[2] The June 2011 social security reform is part of this social protection strategy and introduces the first mandatory pension and disability benefits scheme for private sector employees. This defined benefits scheme applies to permanent employees of formal private companies regardless of their size, and it is managed by the Private Organizations' Employees Social Security Agency (POESSA). Private sector workers not covered by POESSA include the self-employed, informal workers, and employees of private companies who already have Provident Funds (PFs) as of June 2011. Provident funds are voluntary schemes that draw contributions from employers and employees and provide lumpsum payments upon separation. The new pension law allows PFs to co-exist with the new scheme, at least for workers hired before the reform, if both employers and employees agreed to keep them while prohibiting the formation of new PFs. Firms with PFs account for 36% of manufacturing firms and 40% of the workers in our sample.[3]

Employer contribution rates under the new scheme started at 7% of gross monthly salary in 2011 and have since been raised three times: to 8% in 2012, to 9% in 2013 and to 11% in 2015. Employee contributions started at 5% in 2011 and increased to 6% in 2013 and to 7% in 2015. Our empirical strategy to estimate the impact of this reform relies on comparing firms with and without pre-existing PFs as the reform brings about a sudden surge in nonwage labor

[2] The 2005 Public Safety Net Program (PSNP) that targeted food-insecure farmers in drought prone areas, and the 2010 Community Based Health Insurance Schemes (CBHI) for rural communities are prominent examples of the government's initiatives that have already been carefully studied by researchers.
[3] In the annual census of manufacturing conducted by the Central Statistical Agency (CSA) of Ethiopia, about 20% firms have provident funds in 2011 (see Shiferaw et al., 2017), suggesting that such firms are overrepresented in this sample.

costs for the latter. Once employers and employees with PFs declared their decisions to retain them instead of joining the new scheme, they largely remain outside the purview of POESSA. The PFs thus remain self-regulated while the employer contribution rates cannot be less than the statutory rates. It should also be noted that workers with PFs only receive lump sum payment at the time of separation regardless of retirement age, while those under POESSA receive annuities after retirement. Employees with PFs can also access their savings before separation for company approved emergencies such as hospitalization and large expenses such as buying a house. In our empirical analyses, we distinguish these two groups of firms with the dummy variable NPF that takes the value of 1 for treatment firms without PFs and zero for those with PFs.

As discussed earlier, distortions in the labor market will be minimal if employees accept wage cuts that offset the employer's pension contributions. Such compensating mechanisms could, however, be hampered by labor market regulations such as minimum wage laws and union-negotiated wages that permeate the rest of the labor market. Both of these restrictions are irrelevant in the Ethiopian context since there are no minimum wages that apply to the private sector, despite there being one for civil servants, and labor unions remain historically weak. On the other hand, the social insurance reform was introduced in the midst of strong macroeconomic expansion where Ethiopia's Gross Domestic Product (GDP) had been growing by about 10% per annum on average. It is likely that this rapid growth allows firms to better absorb the spike in labor costs due to the social security reform. The economy was also relatively stable during our sample period that spans the years 2009 to 2015 and did not experience major socioeconomic shocks. It also excludes the 2007/08 major price hikes and the post-2016 political upheaval that involved state-of-emergency declarations. The sample period overlaps with the 1st Growth and Transformation Plan (GTP-I) of the Ethiopian government for the 2010-2015 period which, among other things, aims to raise the share of manufacturing in GDP by about 10 percentage points from its historically low 5% share.

While the absence of both a minimum wage and strong labor unions may suggest possibilities for shifting the cost of pension benefits to workers' wages, the prevailing macroeconomic conditions do not seem to favor such reallocations. Rapid and sustained economic growth would certainly increase workers' expectations for pay raises making wage cuts highly unpopular and potentially counterproductive assuming that efficiency-wages are relevant. Nonetheless, as described earlier, the reform-induced increase in nonwage labor costs is also very high and inaction by employers is unlikely to be an option. While the spike in nonwage labor costs would certainly call for substantial adjustment of wage rates, the sheer magnitude of the spike would also make it harder for employers to fully and immediately shift the burden to workers in the form of lower wages — keeping in mind that workers also make mandatory contributions. What seems tenable under such circumstances is a reduction in the rate of growth of wages in the ensuring years to mitigate the spike in nonwage labor costs. We expect this to be the preferred adjustment margin for treatment firms without pre-existing PFs as compared to their counterparts with PFs.

3. Data and Descriptive Statistics

Since social security reforms are likely to affect the behaviors of firms and workers, investigating their labor market implications requires data that provides both worker- and employer-level information. We thus collected linked employer-employee data during April and May of 2016 from a random sample of 300 manufacturing firms and 3,000 of their workers. This allows us to address potential biases that arise when researchers attempt to examine wage determination using datasets that do not capture either worker- or firm-level information as discussed in Abowd et al. (1999). Our sampling frame is the 2015 census of Ethiopian manufacturing firms conducted by the Central Statistical Agency (CSA) of Ethiopia that captures all manufacturing firms that use power-driven machinery and employ at least 10 workers.[4] We followed stratified random sampling using regional states as strata. Because of their small number of manufacturing firms, we excluded five regional administrations at this stage of sampling.[5] The survey thus includes the Amhara, Oromia, SNNPR and Tigray regional states and the city administrations of Addis Ababa and Dire Dawa. Since manufacturing firms tend to congregate in major urban centers, the scope of our survey was limited to firms located in the capital cities of the respective regions except for the Amhara

[4] The survey is officially referred to as Large and Medium Scale Manufacturing and Electricity Survey.
[5] These include the Afar, Benishangul-Gumuz, Gambella, Harari and Somali regions, which are often referred to as small states.

region where two large cities were included, i.e., the regional capital Bahir Dar and the city of Gondar. Since nearly 70 percent of manufacturing firms in the CSA census are located in and around Addis Ababa, the same proportion of firms in our sample were selected from the nation's capital. The remaining 30 percent of firms were randomly selected from the other regions each with a six percent share.

Once the firms were selected, the survey was conducted on 10 randomly selected workers from each firm. Retrospective questions were used to collect data on wages and other worker characteristics from administrative records of sample firms. This allows us to capture the evolution of wages and other benefits before and after the pension reform at the worker level without relying on interviewee memories. Such data were collected for the month of March for seven years from 2009 to 2015. Since the reform was introduced in June 2011, we consider 2009-2011 as the pre-reform period. In addition to wages, the survey captures workers' educational attainment, occupation, age, gender, marital status, and parental education. The survey has a module on firm-level information including total number of workers, location, and industry.

Table 1 shows the distribution of educational attainment and real monthly wages in our sample. We use industry-level producer price index collected by the Central Statistical Agency (CSA) of Ethiopia to deflate nominal wages. About 41% of workers have completed secondary education while 20 per cent have completed higher education. Only about 13 percent of workers have primary education or less. Panel B of Table 1 shows that female workers earn about 30% less than males at every level of education.

Figure 1 shows trends in mean monthly wage during the pre- and post-reform periods for treatment and control group firms. While both nominal and real wages of workers with and without PFs appear to show largely parallel trends in the lead to the June 2011 reform, the wage gap grows increasingly wider during 2012-2015. The common trend appears to be stronger when we consider real wages which were stagnant in the pre-period before rising more rapidly for workers with PFs in the post-period relative to the treatment group. A formal test in section 6 fails to reject the parallel-trends assumption and supports our choice of a difference-in-differences estimator to determine if the widening wage gap is a result of the policy shift forcing non-PF firms to pay pension benefits for the first time.

Table 2 shows important differences between treatment and control group firms across a range of variables that will enter our regression models. Firms with PFs pay not only higher wages but also higher bonuses and allowances relative to non-PF firms. While the fraction of workers receiving fringe benefits has increased substantially even among non-PF firms after the reform, it remains lower than that of PF firms. Higher worker compensations at PF firms are consistent with their larger firm size as well as the higher educational attainment and longer experience and tenure of their workforce relative to that of non-PF firms. These observations underscore the importance of accounting for underlying variation in wage determinants between the two groups of firms while estimating the effects of the reform.

Figure 2 shows that the wage gap between treatment and control groups varies by workers' educational attainment both in the pre- and post-period. Consistent with Figure 1, the dashed lines in Figure 2 show a wider post-period wage gap between PF and non-PF firms relative to the wage gap in the pre-period shown in solid lines. Figure 2 shows further a post-period wage gap between the two groups that rises with workers' education. This may not be surprising given the fact that wage inequality normally increases with human capital (Autor, Katz, and Kearny, 2008). But it is interesting to note that post-reform real wages of college graduates at firms without PFs are still lower than pre-reform wages of college graduates at PF firms. Post-reform wage growth among workers without PFs is thus inversely related with pre-reform wage rate and lower than wage growth among workers with PFs.

4. Empirical Approach

As indicated earlier, our primary objective is to examine how manufacturing wages responded to the 2011 social security reform. The mandatory nature of the policy shift, the existence of a voluntary pre-reform benefits scheme, and the structure of our survey allow us to use a difference-in-differences estimator to capture the wage and non-wage effects of the reform on the treatment group. Firms that did not have pre-existing provident funds are forced to make pension contributions for the first time and will be compared with their counterparts who already had such schemes before the reform. In estimating the effects of the reform, it is critical to account for other drivers of wage variation. Unlike most previous studies on this topic, our employer-employee data allow us to estimate a more comprehensive wage equation. Our wage equation (1) thus features both firm and worker characteristics similar to that in Abowd et al. (1999):

$$W_{it} = \mu + X_{it}\beta + Z_{j(i,t)t}\gamma + \theta_i + \psi_{j(it)} + \tau_t + \varepsilon_{it}, \qquad (1)$$

where, W_{it} is logarithm of real wage of worker i at time t, X_{it} represent time varying worker characteristics, and Z_{jt} represent time varying characteristics of firm j in which worker i is employed at time t. The parameters θ_i and ψ_j represent, respectively, worker and firm effects that are time invariant. Some of these fixed effects are measured in the data, while others remain unobserved. τ represents time fixed effects that account for aggregate shocks that affect wages of all private sector workers equally while ε_{it} represent white noise equation errors.

Estimating Eq.1 without the inclusion of either worker or firm characteristics is known to lead to biased coefficients (Abowd et al.,1999). We thus follow a two-step approach that would allow us to estimate the wage effects of the reform while accounting for firm- and worker-level characteristics with different time series features. In the first step, we run the within estimator on a version of Eq.1 to obtain consistent estimates of the coefficient vectors β and γ on all time-varying worker and firm characteristics. Specifically, we run the within estimator on the equation:

$$W_{it} = \beta_1 EXP_{it} + \beta_2 EXP_{it}^2 + \beta_3 Tenure_{it} + \gamma_1 FirmSize_{jt} + \gamma_2 FirmAge_{jt} + \tau_t + \nu_{it}, \quad (2)$$

where, EXP and EXP^2 represent years of potential labor market experience and experience squared, respectively, $FirmSize$ measures the logarithm of firm-level employment, and $FirmAge$ is measured in years. Eq. 2 also includes interaction terms of each time-varying variable with dummy variables representing 11 4-digit ISIC industries and six regional administrations. In doing so we allow the returns to time-varying wage determinants to vary across industries and localities.

Table A1 in the Appendix shows key results from Eq.2. The residual from this regression represents real wages of workers after removing the effects of time-varying firm and worker characteristics, and will be the dependent variable in the second step difference-in-differences regression. This composite error term contains the person (θ_i) and firm (ψ_j) effects that are time invariant. The person effects θ_i can be expressed as:

$$\theta_i = \alpha_i + u_i \eta, \quad (3)$$

where, u_i represents time invariant worker characteristics measured in the data, while α_i represents the unobserved component.

Similarly, the firm effects ψ_j can be expressed as:

$$\psi_j = \varphi_j + q_j \rho, \quad (4)$$

where, q_j represents time invariant firm characteristics that are measured in the data including the firm's PF status, while φ_j represents the unobserved component.

Our diff-in-diff model takes the following form:

$$\nu_{it} = \delta NPF_j + \lambda Reform + \sigma NPF_j * Reform + u_i\eta + q_j\rho + \omega_i + \varepsilon_{it} \tag{5}$$

where, NPF is a dummy variable that takes the value of 1 for firms without pre-existing provident funds and zero otherwise, while $Reform$ identifies the post-reform period. The reform effect is captured by the coefficient on the interaction term $NPF_j * Reform$. The dependent variable ν_{it} is the residual wage from Eq. 2 and captures firm fixed effects, worker fixed effects and a time varying random error term.

The time-invariant worker characteristics in our data, u_i, include education, gender, marital status, parental education and migration status. We also include a dummy variable for workers who received allowances at any point during the sample period to account for any tradeoffs between wages and allowances. Included in q_j are the firm's industry, region and initial size which are believed to play a role in wage setting. We also include firm dummy variables in Eq.5 to account for unobserved firm fixed effects (φ_j). The error term $\omega_i = \alpha_i + \varphi_j$ therefore captures the unobserved time-invariant worker and firm effects.

In addition to comparing real wages of workers in the treatment and control group, we use Eq.5. to examine changes in bonuses and allowances as important aspects of employee compensation. The latter include subsidies for food, clothing, housing and transportation.

A major challenge in using this approach is the endogeneity of firms' decisions to offer provident funds. Voluntary provision of such benefits is arguably indicative of superior productivity. In fact, our data already show that PF firms are larger and pay higher wages than non-PF firms. One way to proxy for such dynamic productivity differences is by including firm size as we do in Eq.2. Moreover, Eq.5 includes initial firm size and firm dummy variables to control for time invariant characteristics such as managerial capacity and personnel policies that affect both wage and nonwage compensations. It should also be noted that while the decision to offer PFs was endogenous before the reform, this option is no longer available for non-PF firms as of June 2011. In other words, the unobserved characteristics that led to the provision of PFs no longer determine workers' access to pension benefits after the reform. The change in the PF and non-PF wage gap after 2011 is thus unlikely to be driven by unobserved heterogeneity between the two groups of firms but rather driven by the policy shift. This claim obviously requires mean wages in treatment and control group firms to exhibit parallel trends before the reform, which happens to be the case as shown in Figure 1 and Table 7.

The other concern in estimating Eq.5 relates to the fact that our survey does not capture the change in the composition of the workforce in sample firms. If workers who experienced sharp reductions in wages had already been separated from the firm before our observation, then the coefficient on the NPF*Reform interaction term will underestimate the true effects of the reform. While this is an important shortcoming of our data, it has an interesting feature that sheds some light on the importance of this selection effect. Workers in our sample started their employment spells at different points in time allowing us to estimate impact heterogeneity for workers with long and short tenure. For the latter, we compare the wage gap between workers hired by treatment and control firms after the reform (2012-2015) with the wage gap among workers hired by the two groups of firms just before the reform (2010 and 2011). For those with longer tenure, we compare the pre- and post-reform wage gap of incumbent workers who have been on the payroll before the reform. Since the probability of separation is known to decline with tenure as underscored by search and matching models of turnover (Jovanovic, 1979; and Moscarini, 2005), a larger negative effect of the pension reform on the wages of recent hires relative to incumbent workers would be consistent with the hypothesis that workers who already left the company after the reform most likely came from employees most impacted by the reform. Such a finding would also indicate that recent hires in our sample — who have a higher hazard of separation — are good proxies for recently separated workers, hence mitigating the downward bias that would have occurred had the sample been comprised entirely of incumbents with longer tenure. As it turns out, our findings in the next section show that post-reform wage cuts were particularly deeper among recent hires by the treatment group.

5. Discussion of Results

Table 3 presents results from Eq.5. Since our dataset comprises a relatively longer panel for incumbent workers and repeat cross-sections for recent hires[6], it gives us more flexibility in estimating the wage effects of the reform across different groups of workers. As such, columns 1-3 report diff-in-diff results using OLS while columns 4 and 5 report results from a panel fixed effects estimator. Column 1 captures the average treatment effect on the treatment group using the entire sample that includes both incumbent workers and new hires observed at any time over the pre- and post-reform periods. In column 2, we conduct the same analysis but using only one pre-period (2011) and one post-period (2015). Comparing 2015 with 2011 allows us to capture the cumulative effect of the reform at the end of the sample period given that employer contribution rates have been rising during 2011-2015. Column 3 restricts the sample to workers hired after 2009 such that we may capture the wage effects only on recent hires. In other words, we are comparing changes in the starting wages of recent hires (2012-2015) by PF and non-PF firms relative to starting wages offered to workers hired by the two groups of firms just before the reform (2010-2011). The idea is that employers may find it easier to fully switch the cost of pension benefits to the wages of recent hires with new job contracts rather than adjusting the wages of incumbents.

The coefficient on the interaction term NPF*Reform in column 1 suggests that employees of firms without provident funds earn about 7.2% less, on average, after the reform as compared to their counterparts with PFs. Given that employer contribution rates started at 7% in 2011 and rose to 11% in 2015, this finding implies partial switching of the cost of social insurance. In column 2, however, the coefficient on the interaction term drops to -10.9% suggesting almost complete switching of pension benefits to workers' wages by 2015 relative to 2011. Although employers without PFs already know the extent and timing of subsequent increases in pension contribution rates as of June 2011, results in column 2 suggest that wage growth was not

[6] While we have repeat observations (unbalanced short panels) for workers hired after the reform (except for those hired in 2015), we cannot use the panel fixed effects estimator to implement the diff-in-diff as we do not observe the pre-reform wages of these workers. We thus treat these short panels as repeat cross sections and use OLS to estimate the diff-in-diff.

adjusted downward right after the reform by the expected increase in contribution rates. This gradual adjustment of wages, however, appears to apply only for incumbent workers. Column 3 shows about 14.3% lower wages for recent hires by firms without PFs relative to their counterparts in the control group. This sharp reduction in starting wages by treatment firms relative to firms with PFs shows that new hires bore the brunt of the adjustment cost. It also indicates employers' ability to embed the cost of pension benefits in new employment contracts as compared to adjusting the wages of existing workers.

To further explore this behavior, we restricted the diff-in-diff analysis to incumbent workers as reported in columns 4 and 5. Since these workers are observed both in the pre- and post-periods, we can use a panel fixed effects estimator to control for unobserved individual fixed effects on top of the group and firm fixed effects accounted for in the OLS estimates. The statistically insignificant coefficient on NPF*Reform in column 4 suggests similar post-reform (2012-2015) real wage gap between incumbent employees of treatment and control group firms as in the pre-reform period (2009-2011). Comparing 2015 to 2011 in column 5, however, shows a 4.5% reduction in real wages among incumbents of treatment firms relative to the other group. Real wage reductions among incumbents of non-PF firms were thus gradual and less severe as compared to the reduction among new hires. They suffered less than a third of the wage reduction experienced by new hires. These findings are consistent with labor markets where wages are sticky downwards, and firms attempt to retain experienced workers who may have firm-specific skills.

Since the wage effect among incumbents appears to be relatively slow and measured, it is important to show the dynamics of wage growth for such workers. To better understand this process, we estimated a model of wage growth in each post-reform year relative to the mean wage of incumbents in the pre-reform period. The model we estimate is thus:

$$\Delta W_{it} = \sigma Base\_Wage_i + \lambda NPF_j + \phi \Delta L_{jt} + \xi_{it} \ , \tag{6}$$

where, ΔW_{it} is the difference in the logarithm of real wage for worker (i) in post reform year (t) relative to the $Base\_Wage$ which is the mean pre-reform (2009-2011) real wage. We also control for firm-level employment growth, ΔL_{jt} , capturing growth in firm size in t relative to average firm size during 2009-2011. While experience and experience squared also increased over time, these are excluded from the model as they changed by the same number of years for all workers during 2011 to 2015. The advantage of Eq.6 is that it accounts for unobserved heterogeneity by taking long-differences in individual real wage while also controlling for initial conditions that may put workers on different earning trajectories. The coefficient λ thus captures the differential growth rate of real wages among incumbent employees of firms without PFs relative to their counterparts with PFs. We use OLS to estimate Eq.6 on 1,799 workers for which such growth measurements are available. This is less than the total sample of 3,000 workers as some workers were hired after 2011. See Figure 2 for the distribution of real wage growth in 2015.

Table 4 reports results from Eq.6. The coefficient on NPF in columns 3 and 4 is negative and statistically significant suggesting about 5.2 and 6.8 percentage points lower real wage growth in 2014 and 2015, respectively, among employees of non-PF firms relative to that of PF firms. The coefficient on NPF is statistically insignificant in 2012 and 2013 suggesting insignificant differences in real wage growth among incumbent employees of the two groups of firms in the immediate aftermath of the reform. Controlling for education in column 5 raises the difference in wage growth to about 7.5 percentage points with a higher level of significance.

The evidence in Table 4 reveals an interesting dynamic where employers do not cut the wages of incumbent workers right after the social security reform. The gradual increase in the size and significance of the negative coefficient on NPF is also consistent with the increase in the mandatory pension contribution which reached its highest rate in 2015. Employers without provident funds seem to be adjusting the rate of growth of real wages of incumbent workers to partially offset the cost of pension benefits. The estimated 6 to 7 percentage points difference in real wage growth in Table 4 translates to about 4-5% lower real wages among incumbent workers of firms without PFs relative to the control group, which is consistent with the treatment effect reported in column 5 of Table 3.

Column 5 of Table 4 shows that real wage growth increases with the level of education. Workers with secondary education experienced real wage growth that is 14 percentage points faster than that of unskilled workers with only primary or less than primary education. College graduates experienced real wage growth rates that are 39 percentage points higher that of the reference group.

We close this section by examining changes in other forms of employee compensation in response to the social security reform. We look specifically at the difference between PF and non-PF firms in bonuses as well as allowances where the latter include subsidies for food, clothing, housing and transportation. This adjustment is examined using Eq.5 and the results are reported in Table 5. Our main finding is a significant cut in bonuses offered by firms without PFs in the post-reform period relative to the control group. The relative reduction in bonuses is in the 23-24% range whether we take the entire sample period or compare 2015 versus 2011 except that the estimates are more precise in the latter. It is worth noticing that this is purely a reduction in the generosity of bonuses as the fraction of non-PF firms offering bonuses has actually increased from 43% in the pre-period to 50% in the post-period while the share of PF firms offering bonuses has risen from 55% to 58% during the same period. The pension reform, however, did not seem to have any significant effect on the provision of allowances by treatment firms. This is perhaps not surprising given that allowances are not only more frequent than bonuses but also more crucial for workers' engagement and productivity. Like bonuses, our data show an increase in the fraction of firms providing allowances after the reform: from 73% to 75% among PF firms, and from 61% to 69% among non-PF firms.

6. Robustness Checks

To interpret the coefficients on the NPF*Reform interaction term as the reform's effect on wages, it is important to show that it is picking up firms' responses to a differential increase in labor cost after the reform based on pre-reform PF status. To this effect, we experiment with four robustness checks. First, we split the sample into high- and low-wage industries using the overall sample mean of monthly wages as the cutoff point for industry-level mean wages. Our data show that the relative ranking of manufacturing industries in terms of mean wages is very stable over the sample period suggesting that it is a structural feature of the manufacturing sector. We hypothesize that the social insurance reform would have a greater impact on low-wage industries where competitiveness relies heavily on cheap labor as compared to high-wage industries. Regression results from Eq.5 for the two groups of industries as reported in columns 1 and 2 of Table 6 seem to support this hypothesis. We use the entire sample for these estimations such that the coefficients are comparable to column 1 of Table 3. The coefficient on NPF*Reform is negative and statistically significant for non-PF firms in low-wage industries showing a 10.3% reduction in real wages relative to firms with PFs. While the coefficient on the interaction term is also negative for high-wage industries, the reduction in real wages is substantially lower and statistically insignificant. This finding supports the notion that the wage reduction in Table 3 is driven primarily by the reform's effect on labor cost among low-wage industries.

Second, we split our sample into production and nonproduction workers using survey data on occupations. Not only are wages of production workers typically lower than that of non-production workers, firms' variable cost of production is also directly affected by compensation of production workers as compared to that of non-production workers. The results from this sample split are reported in columns 3 and 4 of Table 6. The negative coefficient on NPF*Reform shows an 8.4% reduction of real wages among production workers in treatment firms while non-production workers experienced a statistically insignificant reduction in real wages.

Third, we estimate the dynamics of the wage and benefits gap between PF and non-PF firms in the pre-reform years. We test this by interacting the NPF dummy variable with each pre-reform year, which constitutes a formal test of the parallel-trends assumption that appears to hold in Figure 1. Since the reform was introduced in June 2011, this analysis corresponds to incumbent workers. As reported in column 1 of Table 7, the coefficient on the NPF*year interaction term is statistically insignificant for the entire sample suggesting that the pension reform did not have an impact on the real wage gap between treatment and control groups before its implementation. Most importantly, columns 2 and 3 of Table 7 show statistically insignificant coefficients on the interaction term for low-wage industries and production workers – the two groups of workers that have been most impacted by the reform according to Table 6. Column 4 fails to reject parallel trends for workers hired by PF and non-PF firms shortly before the reform. We also find no reform effects in the pre-period on allowances and bonuses as reported in columns 5 and 6. The findings in Table 7 are thus consistent with the assumption that the compensation gap between employees of treatment and control firms would have remained the same absent the June 2011 reform that forced non-PF firms to start providing pension benefits.

Since wages are the most important outcome variable, we further examined the findings in Table 7 using a placebo test on the dynamics of real wage growth before the reform. This approach allows us to control for initial wage of individual workers as well as the firm's employment growth while differencing out worker fixed effects. The results are reported in Table A2 in the appendix where the dependent variable in the first column is wage growth between March 2009 and March 2011. The coefficient on NPF turned out to be statistically insignificant. Columns 2 and 3 report results for low-wage industries and production workers, respectively, given their sensitivity to the pension reform. In both cases, the coefficient on NPF is statistically insignificant implying that wage growth before the reform did not depend on PF status across all industries and groups of workers. This reinforces our findings in Table 6 that the slowdown in wage growth among non-PF firms is triggered by reform-induced spike in labor costs. Table A2 also shows negative association of wage growth with initial wage but positive association with firm growth and workers' education.

Our fourth and last robustness check tests the reform effects on firm-level employment. As stated earlier, employment effects of a social insurance reform would be minimal if employers are able to shift the cost of such benefits to workers in the form of lower wages. Since the preceding results indicated near complete switching to the wages of new hires coupled with a gradual and partial switching to the wages of incumbent workers, we should expect limited impact of the reform on firm-level employment. We test this effect by estimating the following equation:

$$L_{jt} = \beta NPF_j + \theta Reform + \sigma NPF_j * Reform + \lambda_j + \tau_{jkt} + \varphi_{jrt} + u_{jt} \quad (7)$$

where, L_{jt} is the logarithm of employment in firm j at time t. The reform effect is captured by the coefficient on the NPF*Reform interaction term. The model includes τ_{jkt} and φ_{jrt} that represent industry and region fixed effects that are interacted with time fixed effects. We estimate Eq.7 using the within estimator to control for unobserved firm fixed effects. The results are reported in Panel A of Table 8. The first column shows results for all firms while the other two columns show results for low- and high-wage industries. The coefficient on NPF*Reform is statistically insignificant across all columns suggesting no employment effect on treatment firms.

To further examine the dynamics of firm-level employment after the reform, we run a regression of employment growth for each post-reform year as follows:

$$\Delta L_{jt} = \pi Base\_Size_j + \delta NPF_j + \tau_{jk} + \varphi_{jr} + \xi_j \quad (8)$$

where ΔL_{jt} is employment growth in firm j calculated as the difference in the logarithm of firm-level employment in the post-reform years relative to mean employment during 2009-2011. The latter also enters the model as $Base\_Size$. In addition to the firms' PF status, we allow employment growth to vary across industries and regional states by including industry and region fixed effects which are represented by τ_{jk} and φ_{jr}, respectively. Eq. 8 is estimated using OLS for each post reform year to show dynamics in employment growth as pension contributions rise.

The results are reported in Panel B of Table 8. The coefficient on NPF is negative and statistically significant only in 2012 where employment growth among firms without provident funds was slower by 6.8% relative to the control group. Since pension contributions rates in 2011 and 2012 were 7% of base salary, this finding shows the full weight of the reform falling on labor demand. This is consistent with the results in Table 4 where there was no wage shifting in 2012. While employment growth among non-PF firms was lower by one percent in 2013, this difference was statistically insignificant with the growth disadvantage disappearing in 2014 and 2015. This is broadly consistent with our findings in Table 4 where sample firms were able to shift a significant part of the cost of pension benefits to incumbent workers in 2014 and 2015. The negative association of employment growth with initial firm size is consistent with the widely recognized empirical regularity where growth declines with firm size.

7. Conclusions

How wages respond to government mandated social insurance programs remains an important research question. This is particularly important for African countries which started to roll out social protection programs in recent years. This paper examined the 2011 social security reform in Ethiopia that mandated employer provided pension and disability benefits for workers in the formal private sector. Using matched employer-employee data from Ethiopian manufacturing, we examined wage determination in the post-reform period. We used the presence of pre-reform provident funds to distinguish treatment and control group firms and identify the differential effect of the reform. We found partial switching of the cost of pension benefits to workers in the form of lower wages among treatment firms. But the reduction in wages shows substantial heterogeneity across employees. When it comes to workers hired by non-PF firms after the reform, the reduction in wages relative to that of recent hires by PF firms was nearly proportional to the employer contribution rate non-PF firms encountered after the reform. For employees of non-PF firms already on payroll before the reform, there were no immediate wage cuts, but their wages grew 6 to 7 percentage points slower than that of their counterparts with PFs. This amounts to 4 to 5 percent lower real wages by the end of the sample period, which is less than half of the 11 percent increase in nonwage labor costs that treatment firms encountered. A series of robustness checks reveal that this reduction in wage growth is associated with the increase in labor cost that firms without PFs experienced relative to those without PFs. In fact, we find that employment growth suffered only at the beginning of the reform and the effect gradually dissipated as firms started to adjust wage growth subsequently.

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Figure 1: Mean wages at manufacturing firms with and without provident funds.

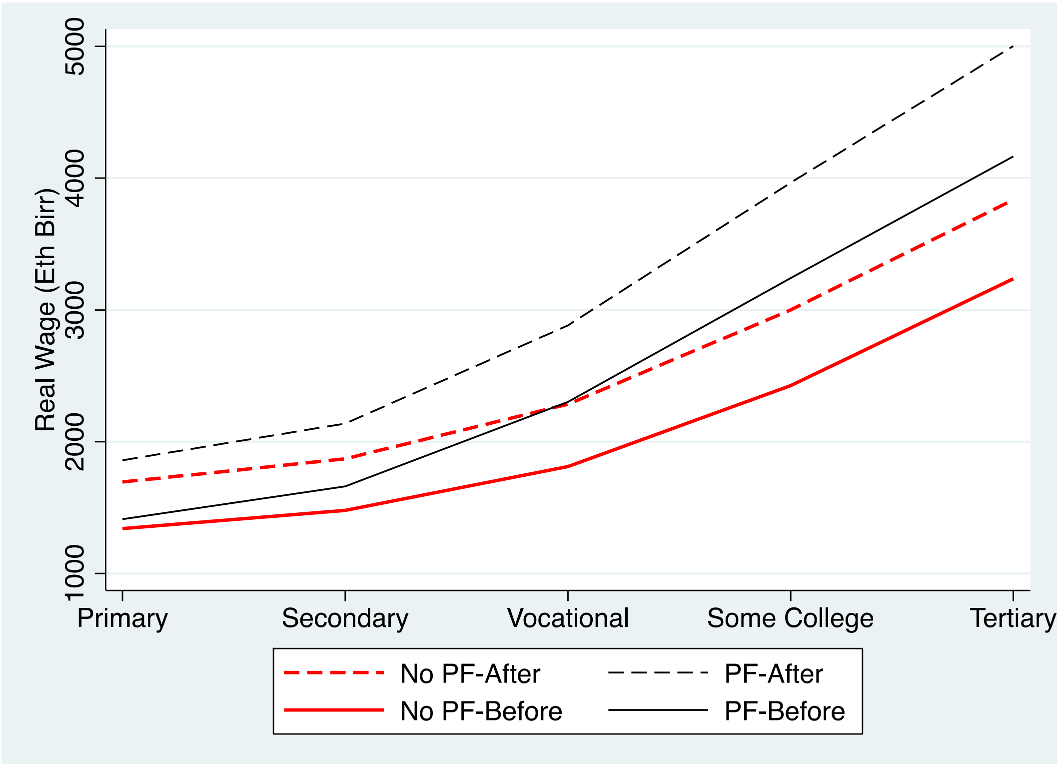

Figure 2: Change in real wages of manufacturing workers by educational attainment.
Note: The lines represent the Nadaraya-Watson Kernel Density regressions of real wages on educational attainment where the kernel is Epanechnikov. “Primary” indicates workers with primary education or less.

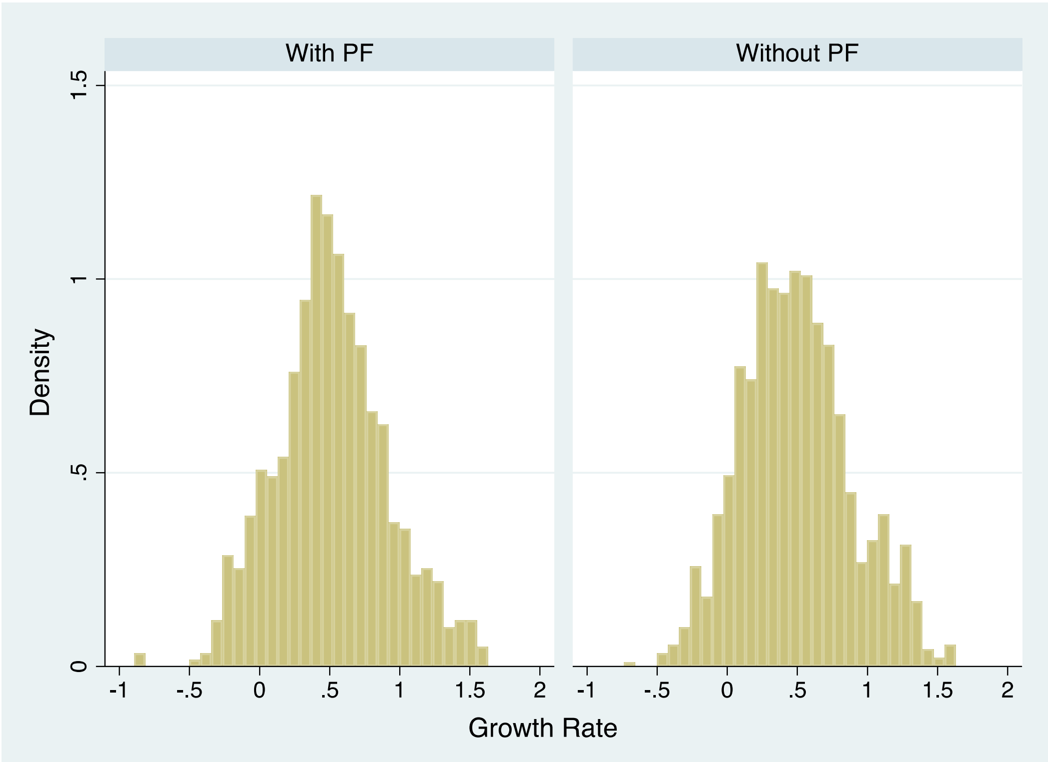

Figure 3: Distribution of real wage growth in 2015 relative to base wage for employees of firms with and without provident funds (PFs). Base wage is mean real wage during 2009-2011

Table 1: Distribution of education and wages by gender

Panel A: Education (%)				
	Male	Female	Total	Number of Workers
Primary or Less	0.1445	0.1111	.1322	377
Secondary	0.4152	0.4103	0.4134	1179
Vocational	0.0950	0.1149	0.1052	300
Some College	0.1256	0.1890	0.1490	425
Tertiary	0.2151	0.1747	0.2002	571
Panel B: Monthly Real Wages (Ethiopian Birr)				
Education	Male	Female	Total	Gender Wage Ratios
Primary or Less	1250.71	884.01	1137.82	0.7068
Secondary	1961.29	1306.23	1723.79	0.6660
Vocational	2586.39	2086.81	2402.87	0.8068
Some College	3521.46	2552.22	3085.70	0.7248
Tertiary	5900.77	4059.98	5298.11	0.6880
Total	2869.27	2022.64	2561.53	0.7049

Note: Authors' computations based on survey data.

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Table 2: Descriptive statistics by firms' PF status

	PF		Non-PF	
	2009-11	2012-15	2009-11	2012-15
Real wage (Eth Birr)	2615.19 (2758.94)	3269.59 (3172.71)	1914.01 (2188.23)	2412.83 (2373.71)
Real bonuses (Eth Birr)	1576.57 (2764.78)	1916.63 (3258.60)	611.22 (1559.70)	898.59 (2017.83)
Real allowances (Eth Birr)	669.45 (1035.15)	786.19 (1283.16)	486.22 (934.03)	638.23 (1068.20)
Workers with bonuses (%)	0.5490	0.5783	0.4324	0.5059
Workers with allowances (%)	0.7272	0.7548	0.6074	0.6956
Potential Experience (years)	19.28 (11.57)	20.23 (11.94)	17.93 (11.92)	19.00 (12.04)
Tenure (years)	8.66 (8.44)	9.00 (8.07)	5.46 (5.52)	6.46 (5.67)
Firm size (employees)	175.94 (228.77)	198.66 (263.88)	78.13 (122.83)	88.17 (124.27)
Firm age (years)	21.07 (18.06)	22.95 (17.36)	14.22 (13.65)	16.76 (13.62)
Educational composition of workers (%)				
Primary or less		0.0978		0.1527
Secondary		0.3666		0.4673
Vocational		0.1279		0.0918
Some College		0.1656		0.1485
Tertiary		0.2421		0.1397
Female Share (%)		0.3581		0.3678
Marital Status of Workers (%)				
Single Never Married		0.2394		0.2931
Married		0.7248		0.6731
Divorced		0.0199		0.0169
Widowed		0.0131		0.0109
Separated		0.0029		0.0060
Father Educated (%)		0.4701		0.4338
Mother Educated (%)		0.3037		0.2911
Migrant (%)		0.5020		0.5219

Note: "Potential Experience" is calculated as age minus years of schooling minus six. "Tenure" measures years since a worker joined the firm at the time of survey. "Initial firm size" measures the mean pre-reform (2009 to 2011) total number of workers of a firm. "Father Educated" is a dummy variable that takes the value one for workers whose father have at least primary education and zero otherwise. "Mother Educated" is also measured in the same manner. "Migrant" is a dummy variable that takes the value one if a worker is not born in the same city where he/she is working at the time of the survey. Numbers in parenthesis are standard deviations for continuous variables.

Table 3: Responsiveness of real wages to pension reform

	OLS			Panel Fixed Effects	
	All Workers		Recent Hires	Incumbent Workers	
	2009-2015	2015 Vs 2011		2009-2015	2015 Vs 2011
	1	2	3	4	5
Secondary	1.0059*** (0.0277)	1.0760*** (0.0537)	1.0520*** (0.0586)		
Vocational	1.6715*** (0.0349)	1.7617*** (0.0686)	1.8453*** (0.0771)		
Some College	1. 7196*** (0.0330)	1.7985*** (0.0610)	1.7719*** (0.0702)		
Tertiary	2.3608*** (0.0324)	2.4706*** (0.0640)	2.3782*** (0.664		
NPF	0.5279** (0.2558)	0.1494 (0.4696)	-0.5003* (0.2632)		
Reform	0.0572*** (0.0211)	0.0649* (0.0370)	0.0789 (0.0480)	0.0067 (0.0119)	0.0244 (0.0161)
NPF*Reform	-0.0721*** (0.0288)	-0.1087** (0.0497)	-0.1428** (0.0644)	-0.0145 (0.0150)	-0.0450** (0.0203)
R^2	0.67	0.85	0.69	0.00	0.00
Observations	14,179	4,536	3,854	12,316	3,722

Note: The dependent variables are residual real wage from Eq.2. Nominal values of compensations are deflated by industry-level producer price indices obtained from the CSA. NPF is a dummy variable that takes the value of 1 for firms without provident firms and zero otherwise. Reform is a dummy variable that takes the value of 1 for 2012 to 2015, and zero otherwise. NPF*Reform is an interaction of NPF and Reform. Bootstrapped standard errors from 500 repetitions are provided in parenthesis. ***, ** and * represent statistical significance of coefficients at the 1 per cent, 5 per cent and 10 per cent levels, respectively. All specifications include an intercept term. We trim observations at the top and bottom 1% of the distribution of the outcome variable to avoid bias due to extremely large/small values.

Table 4: Wage Growth Relative to Pre-reform Average Wage

	2012	2013	2014	2015a	2015b
	1	2	3	4	5
Base-Wage	-0.0660*** (0.0101)	-0.1028*** (0.0119)	-0.1358*** (0.0141)	-0.1623*** (0.0156)	-0.2434*** (0.0166)
NPF	0.0074 (0.0197)	-0.0200 (0.0241)	-0.0521* (0.0303)	-0.0680** (0.0334)	-0.0750** (0.0324)
Firm-Growth$_t$	0.0326 (0.0370)	0.0361 (0.0340)	0.1095*** (0.0382)	0.1244*** (0.0396)	0.1294*** (0.0400)
Secondary					0.1371*** (0.0328)
Vocational					0.1895*** (0.0471)
Some College					0.2544*** (0.0397)
Tertiary					0.3927*** (0.0469)
R^2	0.05	0.07	0.10	0.11	0.21
Observations	1,851	1,851	1,851	1,847	1,799

Note: The dependent variables are growth rates of real wages in each of the post-reform years indicated in the column head relative to the pre-reform average real wage during 2009-2011. "Base-Wage" is the mean of log real wage during 2009-2011. NPF is a dummy variable as defined earlier. "Firm-Growth$_t$" is growth rate of firm size in particular post-reform year indicated in the column head relative to firm size in 2011. ***, ** and * represent statistical significance of coefficients at the 1 per cent, 5 per cent and 10 per cent levels, respectively. The last column also includes industry dummy variables. Standard errors are clustered at the firm level and all specifications include an intercept term.

Table 5: Adjustment in allowances and bonuses after the reform

	Real Allowances		Real Bonuses	
	Entire Sample	2015 Vs 2011	Entire Sample	2015 Vs 2011
	1	2	3	4
Secondary	0.0387	0.0493	0.8127***	0.8392***
	(0.0502)	(0.0519)	(0.0555)	(0.0644)
Vocational	0.1718**	0.1906***	1.2951***	1.3174***
	(0.0865)	(0.0729)	(0.0751)	(0.0957)
Some College	0.1035	0.1275*	1.3584***	1.3711***
	(0.0663)	(0.0685)	(0.0644)	(0.0749)
Tertiary	0.3867***	0.4461***	1.9832***	2.0166***
	(0.0713)	(0.0630)	(0.0757)	(0.0958)
NPF	1.8232***	1.8834***	-0.6447**	-0.6464**
	(0.3723)	(0.2634)	(0.2716)	(0.3094)
Reform	-0.0541	-0.0480	0.1932***	0.1742***
	(0.0499)	(0.0461)	(0.0649)	(0.0657)
NPF*Reform	0.0525	0.0388	-0.2424**	-0.2272***
	(0.0539)	(0.0638)	(0.1033)	(0.0863)
R^2	0.74	0.81	0.79	0.97
Observations	3,132	3,175	2,391	2,420

Note: The dependent variables are residual allowances and bonuses from Eq.2. See notes under Table 3.

Table 6: Heterogeneous effects of the reform across groups of workers

	Low-wage Industries	High-wage Industries	Production Workers	Non-Production Workers
	1	2	3	4
Secondary	1.0310***	0.9779***	1.0364***	0.9933***
	(0.0359)	(0.0398)	(0.0353)	(0.0728)
Vocational	1.6145***	1.7411***	1.7311***	1.6225***
	(0.0462)	(0.0511)	(0.0531)	(0.0818)
Some College	1.6139***	1.7466***	1.7282***	1.6390***
	(0.0466)	(0.0524)	(0.0526)	(0.0766)
Tertiary	2.3048***	2.4055***	2.3858***	2.2673***
	(0.0446)	(0.0437)	(0.0627)	(0.0787)
NPF	-0.0861	-1.0577***	0.4721	0.7717**
	(0.1446)	(0.1472)	(0.3323)	(0.3086)
Reform	0.0786***	0.0288	0.0523*	0.0395
	(0.0293)	(0.0264)	(0.0285)	(0.0262)
NPF*Reform	-0.1041***	-0.0296	-0.0867**	-0.0353
	(0.0383)	(0.0390)	(0.0369)	(0.0342)
R^2	0.67	0.68	0.68	0.70
Observations	7,588	6,591	7,439	6,711

Note: The dependent variables are residual real wages from Eq.2 for different sub-samples of workers. See notes under Table 3.

Table 7: Worker Compensations Before the Reform by PF Status

	Real Wages				Real Allowances	Real Bonuses
	All Workers	Production Workers	Low-wage Industries	Recent Hires		
	1	2	3	4	5	6
Secondary	0.9959*** (0.0488)	1.0482*** (0.0761)	0.9815*** (0.0646)	1.0583*** (0.2503)	0.1299** (0.0618)	0.6928*** (0.0526)
Vocational	1.5946*** (0.0611)	1.5816*** (0.1162)	1.5249*** (0.0828)	1.4890*** (0.2954)	0.1721** (0.0798)	1.2143*** (0.0899)
Some College	1.7303*** (0.0524)	1.7776*** (0.1105)	1.5958*** (0.0767)	1.7941*** (0.2728)	0.1920*** (0.0656)	1.3417*** (0.0637)
Tertiary	2.3224*** (0.0589)	2.3297*** (0.1115)	2.2303*** (0.0823)	2.2127*** (0.2605)	0.3601*** (0.0695)	2.0117*** (0.0687)
NPF	1.1794*** (0.4269)	1.1104** (0.5536)	0.5888** (0.2442)	-0.7592*** (0.2920)	-0.3461** (0.1425)	-0.7630*** (0.2196)
2010	0.0049 (0.0407)	0.0170 (0.0747)	0.0068 (0.0595)	-0.1118 (0.1373)	0.0008 (0.0454)	0.0193 (0.0731)
2011	0.0286 (0.0442)	0.0264 (0.0307)	0.0514 (0.0622)	-0.0094 (0.1368)	0.0047 (0.0504)	0.0856 (0.0545)
NPF*2010	0.0223 (0.0540)	0.0184 (0.1018)	0.0018 (0.0768)	0.1797 (0.1825)	0.0057 (0.0847)	0.0000 (0.0884)
NPF*2011	-0.0034 (0.0594)	0.0240 (0.0335)	-0.0545 (0.0846)	-0.0200 (0.1818)	0.0042 (0.0811)	-0.1179 (0.0729)
R^2	0.69	0.70	0.68	0.90	0.78	0.81
Observations	4,821	2,601	2,575	469	3,137	2,328

Note: Results are based on OLS regression of residual real wages, allowances and bonuses for the 2009-2011 period. Bootstrapped standard errors reported in parenthesis except for recent hires in column 4 (due to limited number of workers).

Table 8: Employment Effects of Pension Reform

Panel A: Dependent variable is firm-level employment				
	All Workers	High-Wage Industries	Low-Wage Industries	
Reform	-0.0590 (0.0835)	-0.0296 (0.1581)	-0.1645 (0.1015)	
NPF*Reform	0.0349 (0.0385)	0.0117 (0.0569)	0.0605 (0.0533)	
R^2	0.14	0.16	0.15	
Observations	2,010	902	1,108	
Panel B: Dependent variable is firm-level employment growth				
	2012	2013	2014	2015
Initial Size	-0.0398** (0.0198)	-0.0302** (0.0118)	-0.0247*** (0.0080)	- 0.0184** * (0.0066)
NPF	-0.0682* (0.0377)	-0.0107 (0.0196)	-0.0049 (0.0151)	0.0055 (0.0126)
R^2	0.08	0.07	0.08	0.08
Observations	287	288	288	288

Note: Panel A reports estimates from the panel fixed effects estimator while Panel B shows OLS estimates. Column heads in Panel B are post-reform years for which employment growth rates are estimated relative to mean employment during 2009-2011. Standard errors are clustered at the firm level and all specifications include an intercept term.

Appendix:

Table A1: Panel fixed effect estimates of wage and other benefits

	Wage	Allowances	Bonus
EXP	0.0853*** (0.0179)	0.0654** (0.0332)	-0.0130 (0.0429)
EXP^2	-0.0006* (0.0004)	-0.0012** (0.0006)	0.0002 (0.0010)
ln(Firm Size)	0.0396 (0.1094)	-0.2918*** (0.1079)	-0.1054 (0.2086)
R^2	0.32	0.15	0.21
Observations	14,643	10,117	7,505

Note: The dependent variables are logarithms of real wage, allowances and bonuses. "EXP" and "EXP^2" represent potential experience and its square term, respectively, while "Firm Size" measures firm-level employment. The model also includes time fixed effects, and interactions of EXP, EXP^2, and ln(Firm Size) with industry and region dummy variables. "Tenure" and "Firm Age" are dropped from the regression due to collinearity. Standard errors are clustered at the worker level.

Table A2: Wage Growth Before the Reform

	All Workers	Low-wage Industries	Production Workers
	1	2	3
Base-Wage $_{(2009)}$	-0.1268***	-0.1230***	-0.1511***
	(0.0141)	(0.0212)	(0.0218)
NPF	-0.0169	-0.0294	0.0031
	(0.0227)	(0.0290)	(0.0256)
Firm-Growth$_{11}$	0.0674*	0.0777**	0.0670
	(0.0356)	(0.0312)	(0.0425)
Secondary	0.0550**	0.0746***	0.0777***
	(0.0226)	(0.0249)	(0.0237)
Vocational	0.0760**	0.0703*	0.0857**
	(0.0339)	(0.0425)	(0.0431)
Some College	0.1054***	0.1406***	0.2016***
	(0.0328)	(0.0472)	(0.0419)
Tertiary	0.1999***	0.1929***	0.2930***
	(0.0364)	(0.0415)	(0.0583)
R^2	0.34	0.33	0.41
Observations	1,455	785	798

Note: The dependent variable is real wage growth in 20011 relative to 2009 for groups of workers indicated in the column heads. "Firm-Growth$_{11}$" is growth rate of firm size in 2011 relative to firm size in 2009. See notes to Table 4 for other variables and symbols. Standard errors are clustered at the firm level and all specifications include an intercept term.