Wages and Firm Performance: Evidence from the 2008 Financial Crisis

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November 2014

Abstract

We use the 2008 crisis to examine the causal effect of higher wages on firm performance. Our sample includes UK firms which signed long-term agreements with their employees prior to September 2008, thereby giving workers pay raises. Using a difference-in-differences approach, we exploit heterogeneity in the timing of signing these contractual agreements. We show that firms that had signed binding long-term wage agreements which extend deep in the crisis realize greater ex-post performance gains, in terms of sales, profits and market shares, as compared to a set of control firms with agreements with no or modest overlap with the crisis. Sales are higher for these firms by 18%-23% during the 2010-2012 post-crisis years, relative to their unconstrained peers. This result is especially pronounced when the long-term contract covers supervisory occupations, in high-skill, and low employee turnover industries, and less pronounced when long-term contracts cover low-skill, blue-collar workers. Our results survive a battery of robustness tests, including a placebo test which suggests that our findings are not driven by other confounding factors associated with the timing of the long-term wage agreement.

JEL classification: J41, J30, J24, G01

Keywords: labor contracts, wages, financial crisis, human capital.

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Acknowledgments: We would like to thank Alex Edmans, Lorenzo Garlappi, Xavier Gabaix, Mariassunta Giannetti (discussant), Josh Gottlieb, Kai Li, Gordon Phillips, Chris Stanton, Geoff Tate, John van Reenen as well as conference participants at UBC Summer Conference, and seminar participants at UBC Economics, Vanderbilt University, University of Illinois, UNC, CU Boulder, Northwestern (Kellogg), FOM Conference for helpful discussions and comments.

I Introduction

In his seminal contribution, Akerlof (1982) argues that paying employees above their opportunity costs induces them to exert higher effort. Other conceptually similar mechanisms have been put forward in the literature showing that higher wages can reduce shirking when effort is not perfectly observed (Shapiro and Stiglitz, 1984), decrease turnover and thus costs associated with hiring and training (Salop, 1979), and attract a better pool of applicants (Weiss, 1980). An important implication of this literature, which introduced the notion of efficiency wages, is that wages, an important budget line for firms, play a key role in incentivizing employees and increasing worker productivity. In this paper, we examine the benefits to the firm of paying higher wages. We draw inferences from the 2008 financial crisis, a time when wage flexibility was particularly valuable to firms and wage stability was particularly valuable to employees.

Testing the effect of higher wages on firm performance has been proven difficult as it is challenging to distinguish the impact of higher wages on firm outcomes from factors that otherwise affect firm performance, such as investment opportunities. In this paper, we overcome this challenge using an empirical setting where firms increase wages for plausibly exogenous reasons. Our sample consists of UK firms subject to long-term wage contracts at the outbreak of the 2008 recession. Using the heterogeneity in the timing of these long-term wage contracts, we first identify a sample of firms which are required to pay significant wage increases during the crisis. These firms (treated) agreed to the wage increases before the crisis, in anticipation of better economic times and tighter labor markets. These long-term contracts are binding, cannot be renegotiated downwards and must extend at least 15 months into the crisis. We then compare these treated firms to a sample of control firms that also signed long-term contracts before the crisis, but whose long-term wage contracts have modest or no overlap with the crisis.

Following the crisis outbreak, the treated firms are locked into paying higher wages. Alternatively, the control firms have greater flexibility to adjust wages in reflection of changes in the labor market. This leads to a wedge in wages between treated firms and their unconstrained peers. To the extent that the timing of these long-term wage contracts is plausibly exogenous to firm performance, an assumption supported by the data, this empirical set-up shows that firms which provided their workers with greater wage increases during the 2008 recession realized stronger ex-post firm performance in terms of sales, profits and market share.

In order to empirically test the implications of higher wages on firm performance, we must first document that the treated firms indeed increase wages more during the crisis, as compared to control firms. As predicted, treated firms increase wages 7% higher, as compared to control firms, in 2009. Long-term agreements typically cover only guaranteed wages. As such, firms could offset any increase in wages by a reduction in employment. However, this is unlikely to occur. The firms in our sample (treated and control) are all unionized and, hence, workers are afforded employment protections. In empirical tests, we confirm no difference in changes in employment around the crisis between the two groups.

While treated and control firms show parallel trends before the crisis, we observe a divergence in firm performance after the crisis. We document that sales at treated firms increase between 18% and 23% more during the 2010-2012 post-crisis years, as compared to control firms. We find a more muted effect when we explore return on assets (ROA), reflecting the fact that this measure incorporate changes in both sales and wages. ROA at treated firms increases between two and three percent more during the post-crisis years. This is an important result as it demonstrates that the overall benefits of incentivizing employees exceed the cost of the wage premium. We also find similar results when looking at market shares, indicating results are not driven by random coincidental changes in industry performance following the crisis.

These results are consistent with efficiency wage arguments where workers can chose levels of effort and can, thus, affect firm performance (Akerlof 1982, 1984). Thus, benefits to the firm from paying higher wages may exacerbate the costs. As discussed above, however, there may be more than one mechanism through which wage premiums affect performance. The purpose of this study is to demonstrate the importance of paying higher wages and not to distinguish between the different mechanisms through which higher wages are beneficial to the firm. We demonstrate that at a time where flexibility is particularly prized by firms, binding labor contracts induce labor rents for some firms, which lead to better firm performance, more than offsetting the higher wage expenses. These results are compelling given our finding that more constrained firms (locked in by the wage agreements) outperform their unconstrained peers. Implicit in our findings is the assumption that the unconstrained firms did not realize the value of efficiency wages amidst the uncertainty of the 2008 crisis.

To further bolster our hypothesis, we parse the ex-post performance results into subgroups where we expect to observe either stronger or weaker treatment effects. We start by exploring cross-sectional variation in the occupations covered by the long-term wage agreement. Agreements which cover supervisory roles should lead to a greater impact on performance at the treated firms, consistent with the notion that higher effort by employees in supervisory positions can impact a larger scale of a firm's operations. On the other hand, agreements covering low-skill, blue-collar workers, should have a more modest effect on performance, consistent with the idea that lower effort is less costly in terms of foregone output for this type of workers. As predicted, we document that the positive effect on sales at treated firms is stronger (weaker) when the long-term deal covers occupations with a relatively greater (more modest) impact on firm performance. These results are also consistent with a Shapiro-Stiglitz (1984) framework, where workers do not respond positively to higher pay but are inhibited from shirking by higher pay. Shirking could be more important for supervisory roles where there is more opportunity to exercise discretion, and perhaps less important for low-skill, blue-collar workers where shirking is easier to detect and monitor.

We also show that paying higher wages leads to better performance in high-wage industries. High-wage industries employ more skilled labor, where human capital output is more difficult to monitor. In the absence of effective monitoring, incentives associated with efficiency wages become more valuable (Shapiro-Stiglitz, 1984). Moreover, we find that the effect on the treated is larger in industries with low employee quit rates. Implicit in our argument that higher wages will lead to greater firm performance is the assumption that workers which receive the higher wages remain at the firm.

We are able to argue our results are consistent with causality due to the following three reasons. First, long-term agreements are the outcome of bargaining between unions and management and are not initiated in response to future business conditions. Instead, firms typically sign wage contracts with their employees as part of a pre-set cycle of negotiations relating to wage and work rules. Second, the 2008 financial crisis was generally unanticipated by both control and treated firms. Among treated firms, the long-term contract agreed to by the firm presumably reflects an acceptable pay appreciation during the forecasted business environment anticipated at the time of the contract agreement. Finally, wage agreements are binding and cannot be renegotiated downwards. Thus, the sudden and unexpected decline in the business and labor markets during the crisis leaves firms with existing long-term wage contracts unable to re-optimize following this shock.

The key identifying assumption is that, conditional on controls, treated and control firms are only randomly different. To further argue there are no systematic differences between our treated and control firms, we conduct several tests. First, we compare several firm characteristics for our treated and control samples for 2007 and we show that they are similar pre-treatment.[1] Second, we control in our specifications for time-invariant firm characteristics, by including firm fixed effects, and for time-varying industry characteristics, by including industry-level controls and interacted industry and year fixed effects. Third, we perform a dynamic analysis taking leads and lags of our treated variable. We find no significance prior to the shock, while significance remains post-treatment. This evidence suggests there are no pre-trends in the data.

In a series of additional tests, we sort firms in our sample into treated and control groups using the same methodology as in our earlier tests but shifting the timing to periods that do not overlap with the crisis, thus creating placebo crises. If our results are driven by

[1] We find weak evidence that firm leverage is higher at treated firms. However, controlling for debt by either matching treated and control firms on ex-ante leverage ratios or controlling directly for firm leverage does not change our findings.

an omitted variable correlated with signing a long-term wage agreement, then we should observe similar results in our placebo treated sample. Instead, our results are insignificant following these "placebo crises". A remaining potential concern for our identification is that firms may enter into long-term agreements if they anticipate they are better prepared to do well in a downturn. Our placebo tests do not address this concern as placebo crises are unfolding outside a recession. We mitigate this concern performing a variety of tests. Our results do not seem to be driven by the fact that treated firms can do better in recessions. Results reported in the paper are also not conditional on a unique control group. We repeat our estimations using different matched samples. We find that the positive effect of the treated on performance is robust to these changes.

The article relates to a number of strands of literature in both economics and finance. It contributes to a vast empirical literature in economics providing supportive evidence on efficiency wages: applicants queue for jobs paying rents (Holzer, Katz and Krueger, 1991), workers shirk less if they are better paid (Cappelli and Chauvin, 1991), wages and monitoring are substitutes (Krueger, 1991). Raff and Summers (1987) discuss the case of Ford where doubling pay for most employees in 1914, led to substantial queues for Ford jobs, productivity and profit increases. Evidence consistent with efficiency wages has also been reported in a number of experimental settings, such as in Fehr and Gachter (2002) and Gneezy and List (2006). This paper does not directly examine whether firms pay efficiency wages, but rather uncovers the benefits of paying higher wages to the firm, and provides evidence in line with the intuition of efficiency wages.

Our paper also contributes to a growing literature in finance that studies the interaction of labor markets with firm outcomes. Brown and Matsa (2014) shows that firms in financial distress have lower ability in attracting job applicants. Benmelech, Bergman and Enriquez (2012) show that firms in financial distress renegotiate wages downwards. Edmans (2011) provides evidence that employee satisfaction is positively correlated with shareholder returns. Simintzi, Vig and Volpin (2014) document that labor market rigidities, as measured by employment protection legislations crowd out finance by reducing firms' leverage. Atanassov and Kim (2009) find that firms experiencing a sharp decline in operating performance are more likely to sell assets in countries where laws favor labor unions, reducing the firm's ability to make large layoffs or wage cuts.

Our paper parallels the approach used in Almeida, Campello, Laranjeira and Weisbenner (2011) which looks at heterogeneity in the maturity of long-term debt contracts prior to the 2008 crisis and adds to the growing literature on the impact of the 2008 financial crisis on firm employees. Chodorow-Reich (2014) finds that firms which borrowed from lenders deeply affected by the Lehman bankruptcy reduced employment relatively more during the crisis. Benmelech, Bergman, and Seru (2012) show that financial constraints and credit availability predict future employment changes.

The remainder of the paper is organized as follows: Section II provides background information on long-term agreements. Section III describes the data, Section IV lays out the empirical strategy, and Sections V, VI, VII present the results. Finally, Section VIII concludes.

II Long-term Pay Agreements in the UK

Long-term wage agreements are typically the outcome of collective bargaining. Collective bargaining in the UK is highly decentralized and takes place mainly at the firm or establishment-level in the private sector. Collective bargaining in the UK is closer to the US model than that of other European countries, notably, being voluntary in nature. The terms of collective agreements are incorporated into individual contracts of employment that are enforced by law. No opt-outs in collective agreements are allowed. According to the 2004 Workplace Employment Relations Survey (WERS) (Emery (2012)), collective bargaining affects approximately 40% of employees in the UK.

The timing and terms agreed to in long-term wage agreements reflect bargaining between unions and management. The month of the negotiations is typically pre-determined, since there is an anniversary date when negotiations traditionally take place. Thus, it is random whether an agreement is signed in January or June of the same year. Since both parties voluntarily agree to the long-term wage agreement, the agreed upon wage changes will reflect both parties' expectations about future economic conditions and wage changes that would be expected if a series of short-term wage agreement were agreed to instead.

Firms often anticipate advantages when signing a multi-year agreement. For example, long-term agreements may lead to greater cooperation with unions during the period in which wages cannot be renegotiated (Hashimoto and Yu (1980)).[2] Employees also gain from the greater certainty about future pay raises.

Committing to long-term wage contracts has modest consequences for firms if they are able to accurately forecast business conditions at the time when the contract is signed. However, as compared to short-term wage contracts, long-term wage contracts can potentially lead to higher wage increases in weak labor markets as they are rigid and cannot be renegotiated down, even in downturns (Beaudry and DiNardo (1991), Hashimoto and Yu (1980), Hall and Lazaer (1984), Lemieux, MacLeod and Parent (2012)). As such, firms are typically less willing to commit themselves to long-term wage contracts amidst an uncertain climate. The 2008 recession is a case in point as the incidence of new long-term deals decreased sharply.

III Data Description

Our data includes information on long-term workers' pay settlement agreements at UK firms over the years 2004-2012. The sample includes 711 long-term wage deals. Long-term wage agreements are effective for more than one year. The average (median) long-term contract is in effect for 2.4 years (2 years). All firms in our samples are unionized and all long-term agreements are recognized by at least one union. The firms in our sample are regularly signing wage agreements with their workers. The variation exists in whether a firm signed a long- or short-term contract, in a given year.

[2]The Treasury in its Bargaining Report (2002) is encouraging long-term agreements by characterizing them *"a more constructive partnership based approach between management and unions on pay"*.

The data is provided by two sources. Our first source is Income Data Services (henceforth IDS), an independent private sector research and publishing company specializing in the employment field.[3] IDS is the leading organization carrying out detailed monitoring of firm-level pay settlements and pay trends in the UK, providing its data to several official sources such as the UK Office for National Statistics (ONS) as well as the European Union. Data is also provided by the Labour Research Department (LRD), an independent research organization which provides research for third-party subscribers, primarily unions. LRD was founded in 1912 and is a leading authority on employment law and collective bargaining. In support of their research mission, LRD collects information on short and long-term pay settlement agreements signed by its subscribing and affiliated unions.

The two samples have significant overlap but also provide unique observations not found in the other sample. For example, the LRD data has more complete coverage of the transportation sector while IDS has greater coverage of the manufacturing industries. We repeat key tests using each sample individually and find consistent, albeit statistically weaker results. By using two sources of data, we have attempted to collect the largest possible sample of all long-term pay settlement agreements in the UK over our time period. However, we acknowledge that we cannot confirm that our sample is exhaustive and firms with long-term deals may be missing from our sample.[4]

A typical long-term agreement in our sample looks like the following agreement signed between Hanson Building Products (Hanson Brick) and its unions. The agreement is a two-year agreement signed as of January 1, 2012. The pay rise in the first year was 2.9%, while the pay rise agreed for the second year starting as of January 1, 2013 is 2.6%. The agreement covers 7,300 workers and is not linked to inflation.[5]

[3]IDS was established in 1966 and has been acquired by Thomson Reuters (Professional) UK Limited in 2005. It has been collecting data on pay settlement agreements since 1995.

[4]In all regressions, both our treated and control firms are taken from the same sample of firms included in the IDS/LRD data with at least one identified long-term wage contract occurring before September 2008 (but where the timing of the long-term deal differs). As such, any bias in firms which are covered by IDS/LRD will be present in both the control and treated samples.

[5]This long-term agreement is not part of our sample and is simply used to provide an example.

We match the IDS/LRD pay settlement data to the Amadeus Bureau van Dijk (BvD) database with a matching success rate of over 90%. Amadeus provides comparable financial information for both public and private companies in the UK, which is particularly important in our case since our sample includes both public and private companies. Our sample period is 2003-2012. After matching, our sample is comprised of 344 unique firms, though sample size varies across specifications and over time because of missing observations for some variables used in the analysis.

We also calculate a number of industry-level controls which are included in certain regression specifications. Industry is defined using 3-digit SIC classifications. We use the entire Amadeus data for the UK to compute median values of log(sales) and ROA for the industry-years in our sample. In addition, we compute median values of market-to-book at the industry-year level, defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity, using UK data from the Worldscope database.[6]

IV Empirical Strategy

All firms in our sample have long-term wage agreements. Firms are assigned to the treated or control groups based on the timing of their long-term wage contracts. Treated firms include firms which signed long-term agreements prior to the onset of the recent crisis (prior to September 2008) and were bound by those agreements for at least 15 months during the crisis. In other words, our treated firms include firms that agreed to a multi-year settlement before September 2008 and that the multi-year settlement expired only after January 2010. Control firms include firms which signed long-term agreements before September 2008, but where the agreement does not apply for at least 15 months into the crisis. Both our treated and control firms have signed, on average, 2 contracts during the sample period. Long-term agreements expire, on average, 22 months after the onset of the

[6]We use Worldscope to compute market to book since Amadeus does not provide information on market values.

crisis for treated firms, and after 2 months for control firms.[7]

Both IDS and LRD samples span a wide range of industries. Table 1 presents the frequency of observations across industries. Industries are defined based on 1-digit SIC codes. Columns 1 and 2 show the distribution of frequencies for the treated and control firms respectively, while Column 3 shows the distribution for the entire sample. It can be seen that both our treated and control groups span a wide range of industries. 41% of the sample covers manufacturing industries, 30% of the sample covers transportation and communication services, 26% of the sample covers retail trade and other services. Columns 4 shows the distribution of frequencies for the sample from LRD. Column 5 shows the distribution of frequencies for the sample from IDS.

We pick September 2008 as the start of the crisis. Lehman Brothers' filing for bankruptcy in September 2008 was an unanticipated event and characterized the onset of the global financial crisis which deeply affected the British economy. Figure 1 shows that a few months following the triggering event, there was a sharp dip in wages in the private sector in the UK.[8] Figure 2 shows that the financial crisis deeply affected the labor market with unemployment and redundancy rates in the UK increasing slowly in mid-2008 and then sharply to record highs post-September 2008.

In order to argue that the difference in outcomes we observe between the treated and control firms is causally linked to the treatment, we need to argue that assignment to the treated or control group is exogenous, or at a minimum, that no omitted variable which predicts assignment into the treated or control group would also predict our outcome variables. We make our case via the following points. First, discussions with industry experts revealed no one particular motivation associated with signing a long-term contract that would also predict ex-post performance. Instead, the timing of the deals tends to reflect the culmination of years of bargaining between firms and unions.

[7]70% of control firm agreements have no overlap with the crisis, and 30% overlap, on average, by 6.6 months.

[8]This sharp decline in wages is not driven only by the finance industry. Plotting total pay in manufacturing industry, we also observe a similar pattern.

Moreover, we document summary statistics for our treated and control samples for 2007, the pre-treatment year in our sample. Table 2 provides these summary statistics and two types of tests. First, we compare mean values for treated and control firms and report t-test statistics. Second, we compare the entire distributions of treated and control firms and report results from a Kolmogorov-Smirnov test of distributional differences. Treated and control firms are similar in terms of sales (our main dependent variable), interest coverage ratios, ROA (net income over assets), cash/assets, average wage per employee, and market share, but there is weak evidence that treated firms have higher leverage (defined as total debt over book value of assets). The t-test for a difference in means is weakly significant for leverage, with a p-value of .09. However, the Kolmogorov-Smirnov test of distributional differences shows that the distributions are not significantly different.[9]

Even if firms are similar in terms of observable characteristics, it is possible that unobservable differences exist between our control and treated samples. Thus, to more rigorously exclude this possibility, we consider a placebo test in which we explore differences in outcomes between firms with and firms without long-term contracts during a period of time which does not overlap with the crisis. The results of our falsification test are presented in our robustness section.

IV.1 Baseline Wage and Employment Results

Our empirical approach is based on the assumption that the 2008 crisis was generally unexpected and resulted in a significant slackening in the labor market (Figure 1). Following this event, firms without long-term agreements in our sample (control firms) had the flexibility to provide very low or even negative wage growth to their employees, given the reduced demand for labor and the limited outside employment options of their employees. On the contrary, firms with long-term agreements (treated firms) must keep to the wages guaranteed in the long-term contract. As such, we should empirically observe higher wages at firms with long-term agreements in effect, during the crisis years.

[9]Despite the weak evidence, we include leverage as a control variable throughout our analysis.

To carefully identify relative changes in wages during the crisis, we employ a difference-in-differences (DID) approach. We first estimate the change in wages at firms with a long-term wage contract in effect, relative to firms without a contract in effect. We then estimate whether the effect of having a long-term contract on wages is different during the 2008 financial crisis as compared to non-crisis years. We estimate regressions of the following form, using wages per employee (log transformed) as our dependent variable:

$$\begin{aligned}\text{Log(W/E)}_{it} =& \alpha_t \cdot \alpha_j + \lambda_i + \delta_0 \cdot \text{Ltapplies}_i + \delta_1 \cdot post_{(1,t=2009)} \cdot \text{Ltapplies}_i \\ &+ \delta_2 \cdot post_{(2,t=2010)} \cdot \text{Ltapplies}_i + \beta \cdot X_{it} + \theta \cdot Z_{jt} + \epsilon_{it}. \end{aligned} \tag{1}$$

where i, j, and t index firms, industries and years; $post_{(t=\tau)}$ takes value of 1 for crisis years τ=2009 and 2010; Ltapplies is an indicator variable which takes a value of 1 for firms with long-term agreements in effect; X_{it} and Z_{jt} are time-varying firm level and industry level control variables, and ϵ_{it} is the usual error term. X_{it} includes controls for profitability (net income/assets), sales (log transformed) and leverage (total debt/assets). Z_{jt} includes controls for profitability, sales and market/book defined at the three-digit SIC industry level. The coefficients δ_0, δ_1, and δ_2 capture the average within-firm change in wages per employee of having a long-term deal in place, after controlling for any coincidental systematic changes in wages per employee of firms in the same industry but not covered by a long-term contract. We exclude year 2008 as this is a transition year. We start our sample in 2005 to provide sufficient years to estimate the baseline wage growth for firms and end our sample in 2012, the last year of available data from Amadeus. All variables are winsorized at the 1% level. Standard errors are robust and clustered at the firm level.

Table 3 finds an insignificant coefficient on the variable capturing whether a long-term wage contract applies. This is consistent with the intuition that a long-term wage contract by itself should not lead affected firms to have higher wage growth, in the absence of an unexpected change to business conditions. However, in 2009, the impact of having a long-term wage contract apply does lead to a positive and statistically significant change in wages, as evidenced by the coefficient on the 2009 interaction term in Column 1, as compared to a long-term contract outside the crisis years.

Ideally, we would also observe a positive and significant coefficient on the 2010 interaction variable as well. However, a significant fraction of our long-term contracts expire in 2010, limiting our power in regards to estimating this coefficient. Moreover, the effect on wages in this regression is estimated with some noise given that contracts do not always cover all employees in a firm. The effect is also economically significant. A firm covered by a long-term agreement pays 6.5% higher average wages in 2009 and 3% higher wages in 2010 compared to an otherwise similar firm that has not signed a binding long-term contract (Column 1).

In all regressions, we control for unobserved time-invariant firm heterogeneity (firm fixed effects), changing macroeconomic conditions (year fixed effects) and industry specific changing conditions (one-digit SIC industry times year fixed effects). To minimize the number of fixed effects, we define industry at the one-digit SIC code level when interacting industry fixed effects with year dummies. However, to ensure that we are capturing industry trends specific to our sample firms, we also control for median 3-digit SIC code industry-year sales, ROA, and market/book in Column 2. There is no evidence that controlling for industry characteristics decreases our coefficients of interest. The coefficients of interest are economically unchanged in magnitude and statistical significance remains.

Column 3 adds firm level controls for profitability, proxied by ROA, and log sales to the controls included in Column 2. We also control for firm leverage (defined as total debt /assets) as our t-test of difference in means in Table 2 suggested that this variable is significantly different between the control and treatment samples pre-treatment.[10] Despite adding these firm-level controls, the estimated treatment effect in 2009 is still significant at 5%, and similar in magnitude. In sum, these results show that firms with long-term wage contracts in place during the crisis increased wages more, relative to their peer firms.

One concern is that even if firms with long-term agreements are required to pay higher

[10]Given the modest sample sizes, we avoid dropping observations when adding controls by replacing missing observations with the sample median.

wages per employee, they could mitigate this cost by reducing employment more aggressively, as compared to control firms. Columns 4-6 of Table 3 estimate the differential effect in log employment at firms with long-term agreements in place during the crisis, as compared to firms without such agreements. We find no evidence that firms with long-term agreements reduce employment more vis-à-vis firms not covered by these agreements during crisis years, regardless of the specification. This is also consistent with our discussions with industry experts, which point out that the firms in our sample are unionized and labor protections afforded to their employees will make it difficult to implement layoffs.

V Ex-Post Performance

V.1 Baseline Results

The evidence presented above suggests that having a long-term contract in effect at the onset of the crisis led to higher wages. In this section we explore the central question of our study: Do higher wages then lead to higher ex-post performance for treated firms relative to control firms? Theoretical arguments in the gift exchange hypothesis (Akerlof 1982, 1984) suggest that workers respond to wages above market-clearing rates by providing greater effort. In this paper, we attempt to answer the question as to whether these gains in employee productivity translate into firm performance.

We measure firm performance in terms of sales (log transformed). We concentrate on sales because sales are less subject to potential manipulation, as compared to accounting profits, and report results in Table 4. We compare firm performance between firms that have signed long-term agreements and these agreements extend into the crisis by at least 15 months (treated) compared to firms that have signed long-term agreements that do not expire deep in the crisis (control).[11] Thus, we estimate regressions of the following form:

[11]A firm enters our treated group if it has signed a long-term contract before September 2008 that expires post-January 2010.

$$y_{it} = \alpha_j \cdot \alpha_t + \lambda_i + \delta_1 \cdot post_{(1,t=2009)} \cdot Treated_i + \delta_2 \cdot post_{(2,t=2010)} \cdot Treated_i + \delta_3 \cdot post_{(3,t=2011)} \cdot Treated_i + \delta_4 \cdot post_{(4,t=2012)} \cdot Treated_i + \beta \cdot X_{it} + \theta \cdot Z_{jt} + \epsilon_{it} \quad (2)$$

where i, j, and t index firms, industries and years; $post_{(t=\tau)}$ takes value of 1 for years τ=2009, 2010, 2011, and 2012; treated is an indicator variable which takes a value of 1 for firms in our treated group; X_{it} and Z_{jt} are time-varying firm level and industry level control variables; and ϵ_{it} is the usual error term. The coefficients of interest, δ_1, δ_2, δ_3, and δ_4 capture the effect of the long-term contract during the crisis and post-crisis years on our dependent variable. We exclude year 2008 as this is a transition year. All variables are winsorized at the 1% level. Standard errors are robust and clustered at the firm level.

Columns 1-3 in Table 4 present our baseline specifications. Column 1 includes firm fixed effects and interacted one-digit SIC industry and year fixed effects but does not include any other controls. As predicted, we find that sales in the treated firms increase by 18% more in 2010, as compared to control firms, significant at the 10% level. Column 1 also reports a positive and statistically significant increase in sales at treated firms by 21% in 2011, significant at the 5% level, and by 20% in 2012, which is just outside of regular levels of significance (p-value=0.11), relative to control firms.

In Column 2, we control for additional industry level controls. As reported, sales increase by 19% more in 2010 (significant at 5% level), 23% more in 2011 (significant at the 5% level), and 21% more in 2012 (significant at 10% level), as compared to control firms. In Column 3, we additionally control for firm leverage. The coefficients on the interaction terms are stable across the three different specifications. The fact that the additional controls for industry and leverage have little impact on the results indicates that our results are not driven by differential industry trends between the two groups or by differences in leverage between the two groups.[12]

[12]One concern when using panel data is that firms can go bankrupt over time, resulting in a survivorship bias when the worst performing firms leave the sample. We confirm that 13 firms (4%) leave our sample prematurely due to bankruptcy. Three of these firms are treated (23%) and nine

Our identification relies on the key assumption that treated and control firms follow parallel trends prior to the crisis. In Columns 4-6 of Table 4, we perform a dynamic analysis to establish that this is indeed the case. We augment the baseline specification by including two new terms $post_{(1,t=2006)} \cdot Treated_i$ and $post_{(1,t=2007)} \cdot Treated_i$. The coefficient on these two terms allows us to assess whether any effects can be found prior to signing these agreements. Finding such an effect prior to the crisis could be symptomatic of differential pre-treatment trends in firm performance between the groups or reverse causation.

Across all specifications, we find that the estimated coefficients for 2006 and 2007 are insignificant and negative or small in magnitude. Moreover, as in our baseline specifications, the coefficients for the interactions of treated with 2010, 2011 and 2012 are positive and significant: sales increase by 20% more (significant at 10% level) in 2010, 23% more (significant at the 5% level) in 2011, and 22% more (significant at 10% level) in 2012, as compared to control firms (Column 4). Similar to the pattern in Columns 1-3, we control for additional industry level controls (Column 5) and firm level leverage (Column 6) and results remain principally unchanged.

These results suggest that there are no pre-trends, or in other words, that our treated and control firms are not following different trends before the event. This finding validates a key assumption of the difference-in-differences methodology that allows attributing the difference in sales between treated and control firms to the event and not to differences in pre-treatment trends.

It is worth noting that firms could reduce fringe benefits to offset the higher cost of wages or erode working conditions – changes unobservable to the econometrician. To the extent that wages and fringe benefits are imperfect substitutes, the effect of higher wages cannot be fully offset with reductions in other forms of compensation.[13] Moreover, in our setting, part of union negotiations with firms include safeguarding working conditions

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are control, paralleling the sample wide statistics where 30% of the full sample are treated.

[13]See Dickens, Katz, Lang, and Summers (1989) for relevant discussion, and Holzer, Katz, and Krueger (1991) in the context of minimum wages.

for their employees. It thus seems highly unlikely that treated firms cut other forms of compensation to offset higher agreed wages.

These results are consistent with an argument that higher wages have a positive effect on firm performance. This follows from the gift exchange hypothesis developed in Akerlof (1982, 1984), where higher wages may increase firm loyalty and may lead to greater employee effort.[14] Our results can also be interpreted in the light of other mechanisms put forward in the efficiency wages literature such that higher wages can reduce shirking when effort is not perfectly observed (Shapiro and Stiglitz, 1984), decrease turnover and thus costs associated with hiring and training (Salop, 1979), and attract a better pool of applicants (Weiss, 1980). Here, we do not try to distinguish between the different channels proposed in the literature, but rather establish the positive link between paying wages above market clearing rates and firm performance. It is worth noting that the effects we identify are quite persistent as magnitudes seem to be quite stable during the post-crisis years. However, in later tests when we measure performance using ROA, thus accounting for the costs of higher wages at these firms, we find more modest changes in firm performance and statistical significance is less persistent.

An interesting notion underlying Akerlof's 1982 paper, besides the idea that employees make reciprocal gifts when paid above market clearing rates, is the use of a reference point when individuals decide about the fairness of a transaction, or in this case, their compensation. This idea has been expressed more clearly in Akerlof and Yellen (1990) fair wage effort hypothesis where workers will exert lower effort if their wage is lower compared to their perceived wage. Kahneman, Knetsch, and Thaler (1986) present evidence that perceptions of fairness have little to do with workers' opportunity costs. Blinder and Choi (1990), Bewley (1995), and Campbell and Kamlani (1997) document a wide-spread perception among wage-setters that wage cuts will lead to negative performance. In further support, Mas (2006) provides empirical evidence that following wage arbitration agreements

[14] Greater loyalty can lead to lower employee turnover, which can help to preserve firm specific human capital and reduce hiring and training costs, as in Cohen (1983). Moreover, greater employee loyalty can increase worker productivity as in Jaramillo et al (2005) and Riketta (2002).

perceived as unfair, police officers exerted less effort leading to lower productivity. Kube et al (2013), using a field setting, show that wage cuts for temporary library staff are followed by productivity declines. Propper and van Reenen (2010), using data from English hospitals and the regulated pay for nurses, show that talent is hard to attract and retain if wages are below the competitive level leading to falls in quality.

It is worth emphasizing here that our paper could also be interpreted in light of the "fairness and norms" idea, under the assumption that workers in our control firms perceived the wage cuts as unfair. We cannot formally distinguish whether higher relative performance of treated firms is due to workers exerting higher effort to reciprocate to higher wages (treated firms), or due to perceiving wage cuts as unfair (control firms). However, we posit it is rather unlikely that workers of control firms perceived wage cuts as unfair. Control firms were unlikely to cut wages in nominal terms and workers are less cognizant of cuts in real wages (Blinder and Choi, 1990). Moreover, the scenario of workers putting lower effort due to unfair wages seems less likely given the severity of the 2008 crisis and the weak labor markets.

It is surprising that we find the treated firms, which are constrained by their pre-existing long-term wage agreements, outperform their unconstrained peers. In not copying the high-wage strategy of the constrained firms, the unconstrained firms made an error of judgment. This error may stem from an ignorance of the benefits of efficiency wages, or more specifically, of the benefits of efficiency wages during the crisis – a period when workers might be more favorable to such incentives. Alternatively, managers at the control firms may have either misjudged the depth of the crisis or the extent to which the labor markets would recover. Finally, managers at control firms may have been more focused on short-term goals, such as conserving cash during the crisis, as opposed to maximizing long-run profitability.

V.2 Cross-sectional Results

Our economic intuition is that workers exert higher effort as a response to higher wages. It is natural then to expect that the effect will be higher in cases where employee effort will have a greater impact on firm performance. In this section, we sort firms into groups where we expect to find a stronger response to having signed the long-term deal. First, we exploit cross-sectional variation in the type of workers covered by the long-term agreement and we find that the effect is more pronounced for deals that cover employees more likely to impact firm performance. Second, we show that the positive effect of treated on firm performance is more pronounced in sectors that employ skilled workers and where employee turnover is low.

V.2.1 Occupations

In Table 5, we explore cross-sectional variation in the type of workers covered by the long term deal. We separately code a dummy variable for deals which cover the job functions that can have greater impact in a given firm and a dummy variable for deals which cover the job functions that can have least impact in a given firm. We expect to observe a greater impact on firm-level performance when more senior employees, such as supervisors, are covered by the deal given the greater ability of more senior employees to impact all levels of a firm's operations. On the contrary, we expect to observe a diminished effect when low-skill blue-collar workers (which represent occupations associated with the least influence on firm-level performance) are covered by the agreement.[15]

Columns 1-3 of Table 5 augment our baseline specification with interaction terms of our treated variable and the "includes supervisory employees", which takes a value of 1 if contracts cover these high influence occupations. As in our baseline, the effect of treatment is positive and significant for 2010 and 2011. However, our coefficients of interest in this specification are the interaction terms. To the extent that the omitted variables

[15]Data on occupations covered by each deal is not available for all long-term deals and treated firms with missing information on occupation are dropped from the sample for these tests.

are uncorrelated with types of occupations covered, the estimate can be interpreted as a triple-difference effect. Column 1 presents the baseline specification. The triple difference coefficient is positive and significant at 10% level for 2009 and at 5% level for 2012, while the coefficients for 2010 and 2011 are just outside of regular significance levels with p-values of 0.11 and 0.12 respectively. Moreover, these results are robust to the inclusion of additional industry controls, as reported in Column 2, and to firm leverage as reported in Column 3.

On the other extreme, we expect to find the opposite effect when we examine the effect of deals covering low skill blue-collar workers on treatment, as these workers should have the lowest ability to influence firm performance. Thus, we interact our treated variable with an indicator, which takes a value of 1 if the contract covers low-skill, blue-collar workers. The interaction terms in Columns 4-6 of Table 5 are all negative and significant for 2010 and 2012 at 10% or 5% level of significance. The results are robust across specifications.

V.2.2 Industry Wages and Turnover

Second, we predict that the difference in performance between treated and control firms should be especially pronounced in sectors that rely more on skilled labor. Human capital is known to be a relatively more important source of value in high skill industries, as shown in Zucker, Darby and Brewer (1998), Darby, Liu and Zucker (1999), and Zingales (2000). Efficiency wages are also predicted to be more valuable in industries reliant on skilled labor where output is more difficult to monitor (Abrams and Yoon, 2007). In the absence of effective monitoring, incentives associated with efficiency wages become more valuable (Shapiro and Stiglitz 1984, Leonard 1987, Krueger, 1991).

We identify industries reliant on skilled labor using industry wages, as measured in 2007. We define a dummy variable which takes the value of 1 if the firm is in an industry where average industry wages was in the top quartile for the sample. This is a triple difference estimation and therefore, the coefficients on the interaction terms are the variables of interest. To the extent that omitted variables have a similar impact on performance across skill-groups, this test also helps address identification concerns. The results are reported in Table 6. To conserve space, the coefficients on the interactions of high industry wages with year fixed effects are not reported. Column 1 shows the baseline specification, Column 2 adds industry level controls and Column 3 additionally controls for leverage. The interaction coefficients are positive and significant at 10% for 2011 and at 5% for 2012. These results are consistent with the prediction that returns to greater employee effort will be higher in industries where monitoring less effective.

Next, we explore cross-sectional variation in industry average employee quit rates. Our intuition is that if employee turnover is high, then the fraction of incentivized employees (due to higher wages) who remain at the firm will decrease more rapidly over time thereby weakening the impact on firm performance, especially in later years. We measure industry average quit rates using Job Openings and Labor Turnover (JOLTS) data produced by the US Bureau of Labor Statistics (BLS), as measured in 2007.[16] Data is available at the two-digit NAICS level and is not available for all industries.[17] We define a dummy variable which takes the value of 1 if the firm is in an industry where average industry turnover is in the bottom quartile for the sample and estimate similar specifications as before. As reported in Columns 4-6, we find a greater effect in industries with low turnover and a consistent pattern across specifications. The interaction term between treated and the turnover indicator is positive and significant for 2011 at 1% level of significance throughout specifications and for 2012 at 5% or 10% level of significance.

VI Robustness

In this section we report key robustness tests for our baseline results. First, we do a falsification test and find no effects of long-term agreements on sales following the enactment of a long-term agreement during a non-crisis period. Second, we investigate differences

[16]The use of US data to measure industries' characteristics (employee turnover in this case) was first introduced by Rajan and Zingales (1998). It relies on the assumption that some industries experience higher employee turnover than others due to structural (e.g. technological) reasons which are similar across countries.

[17]Observations are dropped in these tests if turnover data is not available.

in treatment intensity. Third, we show our performance results are robust to different measures of firm outcomes. Fourth, we perform a matching analysis and find similar results.

VI.1 Falsification Test

While there is no evidence that firms agree to long-term wage agreements when anticipating an increase in sales in subsequent years, the possibility that an unobserved omitted variable drives both the timing of the long-term agreement and the subsequent change in sales remains. To address this concern, we consider a placebo test. In this test, we sort firms in our sample into placebo treated and placebo control groups using the same methodology as used in our earlier tests but shifting the timing to a period that does not overlap with the crisis. If there is a correlation between agreeing to a long-term contract and a future increase in sales, then we should observe a significant and positive coefficient between the placebo treated firms and future sales. If, instead, the relation observed in the earlier results is not driven by the long-term agreement per se but by higher wages stemming from a combination of a long-term wage agreement and a crisis, then we should observe no significant relation between the placebo treated firms and future sales.

We perform three different falsification tests, comparing results if the placebo crisis occurred in September 2004, September 2005 and September 2006.[18] We repeat the same methodology as was used to create the primary sample, with all dates shifted backwards. For example, considering the September 2004 placebo test, we assign firms to the placebo treated group if the firm signed a long-term labor agreement prior to September 2004 and this long-term labor agreement extended for at least 15 months into the placebo crisis.

We report the results of this placebo test in Columns 1-3 of Table 7. We follow the same specification as in Columns 1-3 in Table 4 but with the shifted timeline. As in the primary sample, we exclude the transition year of the placebo crisis, in this case, 2004,

[18]Our data on firm performance starts in 2003 thereby restricting our first placebo year to 2004, where we can also estimate a baseline year. Our last placebo test is in 2006 to allow sufficient separation from the real crisis. Sufficient separation is important as the average long-term deal in our sample lasts 29 months.

and we start our sample in 2003. To parallel the baseline tests, we also drop all firm-year observations following the final year in the interaction.[19] In all of the specifications, we report coefficients on the interactions of treated and year dummies that are of modest economic magnitude and always statistically insignificant.

Next, we consider a placebo crisis starting in September 2005 (in Columns 4-6) and in September 2006 (in Columns 7-9). As in the primary sample, we exclude the transition year of the placebo crisis, 2005 in columns 4-6, and 2006 in columns 7-9 and all years which follow the final year in the interaction terms. In all of the specifications, we report coefficients on the interactions of treated and year dummies that are of modest economic magnitude, often negative, and always statistically insignificant.

Thus, regardless of the timing of the placebo crisis and control variables used, we are unable to replicate the finding of increasing sales in the placebo treated sample. These results are consistent with our argument that the results observed in the primary tests are caused by the treatment effect of paying labor higher wages during the crisis, as opposed to an omitted variable which drives both the long-term labor agreement and future sales performance.

VI.2 Results Accounting for Treatment Intensity

Treatment intensity may vary depending on the deal characteristics. To gauge the effect of the long-term deals on ex-post firm performance, we scale the treatment effect by the intensity of the treatment. We redefine our indicators $post_{(t=\tau)}$ accounting for the number of months of the long-term deal post-September 2008, using two different approaches. The longer the deal extends into the crisis, then, presumably, the greater the wedge in wages between treated and control firms and the greater the effect of treatment.

In Column 1 of Table 8, we scale our treatment indicator by duration, defined as

[19]We drop these years so that the interaction variables reflect the change in sales as compared to only previous years. We also repeat using all years of available data and find similar results. The coefficients on all interaction terms in all nine regressions are insignificant.

the logarithm of the number of months during the crisis over which the long-term deal applies. This value is set to 0 for control firms. Columns 1-3 repeat the same specifications as in Columns 1-3 of Table 4. Coefficients δ_2 and δ_3 are significant at 5% level in all specifications; coefficient δ_4 is significant at 10% level after controlling for industry-level controls in Columns 2 and 3. In Columns 4-6, duration is instead defined as the ratio of the duration of the long-term contract that coincides with the crisis divided by the total duration of the deal signed. Coefficients δ_1, δ_2 and δ_3 are significant across specifications.

The advantage of this estimation is that we don't treat all deals equally and, therefore, we can more precisely estimate the magnitudes of the effect of long-term deals signed prior to the crisis on ex-post firm performance. The average duration of a long-term deal in our treated sample is 22 months, with a minimum of 15 and a maximum of 58 months. If the duration of the deal which extends into the crisis increases from 15 to 22 months (a 47% increase), then sales are expected to be 3.2% higher in 2010, 3.5% higher in 2011, and 3.3% higher in 2012 (using the coefficient estimates in Column 3). This is a cumulative increase in treated firms' sales of 10% relative to their peers. Alternatively, if we increase the fraction of the long-term deal at treated firms which overlaps in the crisis by 18pp (for example from 61% in the 25th percentile to 79% in the third quartile), then sales are expected to be 5.2% higher in 2009, 6.8% higher in 2010, and 8% higher in 2011 (using the coefficient estimates in Column 6). This corresponds to a cumulative increase in treated firms' sales of 20% between 2009 and 2012 relative to their peers.

VI.3 Alternative Measures of Firm Performance

Throughout the analysis, we have used sales as the measure of firm performance. In Table 9, we show that our results are robust to alternative measure of performance: an accounting measure of performance (ROA) and a product-market based measure of performance (market shares).

First, we consider accounting profits, as measured by ROA.[20] Tests of profits can rule

[20]This variable is winsorized at 5% level given the fact that is highly skewed. Results are robust

out the possibility that treated firms do indeed increase sales but do so in an inefficient manner due to high costs. Thus, we repeat the specifications in Columns 1-3 of Table 4 and show the effect of treated on profits without controls in Column 1, with industry level controls in Column 2, with firm leverage in addition to industry level controls in Column 3. Profitability increases in treated firms, relative to control firms, in 2009 and 2010, while the effect for 2011 and 2012 is positive but not significant. Thus, profits are higher by 3% in 2009 and by 2.2% in 2010 respectively, and the effect is significant at the 5% level (Column 3).

Second, we look at market share. Market share is measured as the logarithm of the percent of sales attributed to the firm as compared to total industry sales, where industry sales is defined in sample based on three-digit SIC codes. This variable is winsorized at 1% level. Tests of market share can rule out the possibility that our results are driven by changes in industry performance, not unique firm performance. Columns 4-6 repeat the specifications in Columns 1-3 of Table 4. Across specifications, the interactions of treated and year dummies are all positive, and the coefficients on 2010 and 2011 are statistically significant. Market share is 19% higher in treated firms vis-à-vis control firms in 2010 and 21% higher in 2011 and the coefficient is statistically significant at 5% level for both years (Column 6).

These results strengthen our conclusion that higher wages are beneficial to firms as they translate to economic gains exceeding the wage premium. In line with an efficiency wage argument, it is optimal to the firm to pay wages above market clearing rates as the gains from higher wages will exacerbate the costs of the increased wage bill.

VI.4 Matching

Our main identifying assumption is that treated and control firms are similar firms, except for the fact that treated firms have higher wage growth relative to control firms. Table 2 shows that, with the exception of leverage, there are no statistical differences across several

to winsorizing ROA at 1% level and are reported in the Appendix.

observables. However, even if firm characteristics between the two groups tend not to be statistically significant, it is possible that subtle differences between the groups could lead to different ex-post performance. Thus, in this section, we perform a matching analysis to minimize pre-treatment differences between the treated and control groups.

We match in three different ways: by sales, leverage and size (as measured by assets) based on pre-treatment values at the time the binding contract is signed for each treated firm. Matching is done with replacement and firms that cannot be matched are dropped from the estimation. Performing a t-test of the difference in means pre-treatment in our matched sample, we find that treated and control firms are similar along the dimensions we match: t-test is 0.46 for sales, 0.23 for leverage, and 0.55 for assets.[21]

Table 10 presents the results on sales, profits and market shares. Columns 1, 4, and 7 match by sales. Columns 2, 5, and 8 match by leverage. Columns 3, 6, and 9 match by assets. Across specifications, we control for firm fixed effects, interacted industry times year fixed effects, industry level controls and firm leverage. Results are robust to these alternative samples, and are stronger in terms of significance. The stronger results after matching alleviate concerns that pre-treatment differences in control and treated firms are driving our results.

VII Alternative Explanations

In this section we pose and subsequently refute alternative interpretations of our key results. We discuss the possibility that the results are driven by superior performance as cash constrained firms need to operate more efficiently and whether treated firms may be more resistant to negative shocks.

[21]We do not match by industry in these tests as we control for interacted industry times year fixed effects in our regressions, and we want to be able to pick the closest possible match without imposing a lot of conditions, given our sample size. However, we do repeat our matching analysis taking industry into account and we get similar results in terms of significance.

VII.1 Response to a Negative Cash Flow Shock

Given the long-term agreements are binding for the treated firms, it might be possible that treated firms may be responding to a negative cash flow shock associated with having to pay relatively higher wages compared to their peer firms. The intuition is that rigid labor contracts may make treated firms more vulnerable to the negative shock of the 2008 financial crisis. As a response, treated firms may need to operate more efficiently vis-à-vis their peers with minimal overhead costs, creating economic value.

If a negative cash flow shock is the underlying mechanism of better long-run performance for the treated firms, we should observe other differences between treated and control firms that are consistent with such an explanation. First of all, it is natural to expect that higher wages during the crisis will lead to some substitution of labor with capital. We thus examine the effect of treated on capital expenditures. In unreported regressions, we find that coefficients are negative across all coefficients but statistically insignificant. Of course, the finding of lower capex would not be unique to a CF shock interpretation, but absence of significance weakens support for this alternative story.

Moreover, a cash flow shock interpretation would support other differences between treated and control firms that affect balance sheet items such as cash and short term assets. As argued in Cooper and Haltiwanger (2006), reducing investment is often a costly means to conserve cash and firms may prefer to conserve cash via other means such as reducing cash balances (Almeida et al 2004), inventories (Fazzari and Petersen (1993)) or accounts receivable (Bakke and Whited (2012)). In fact, looking at firms exposed to negative cash flow shocks following the expiration of long-term debt contracts during the crisis, Almeida et al (2011) find that firms cut capital expenditures in an amount equal to about 12% of expiring debt. In contrast, they reduced cash balances by an amount equal to 41%, reduced share repurchases in an amount equal to 10%, and reduced inventories by an amount equal to 7% of expiring debt.

We explore these alternative predictions in unreported regressions. We look at changes in cash, working capital and current assets. All three variables are normalized by total assets.[22] In all the regressions, there is no significant and negative relation between having a long-term contract during the crisis and changes in cash, working capital, and current assets. Instead, we show weak evidence that working capital increases. We also look at changes in leverage and find no increase in leverage at treated firms. We are cautious not to over-interpret our results given the weak results. But, the lack of any evidence of a decrease in cash, working capital, or current assets, or an increase in leverage among treated firms during the crisis are in contrast to a cash flow shock interpretation of our findings. In other studies, these variables tend to be significantly associated with negative cash flow shocks (Almeida et al. (2004), Almeida et al. (2011)).

VII.2 Treated Firms More Resistant to Crisis

Our falsification tests mitigate the concern that firms sign long-term agreements when anticipating sales increases in subsequent years. However, it is still possible that firms sign long-term agreements when they anticipate they can better manage a downturn, as compared to their peers. The falsification tests do not directly address this concern as long-run performance in the placebo tests is always estimated during a growing economy.

We show evidence inconsistent with this alternative interpretation using two approaches. First, it might be possible that firms more resistant to downturns will be more likely to sign a long-term wage contract. The more often a firm signs a long-term contract, the more likely the firm will have signed one shortly before the crisis and be in our treated group. To avoid this potential bias, we apply an equally restrictive window for identifying our control firms. By shortening the window during the period which we must observe a contract for a firm to be included in the control sample, we increase the probability that our control firms also frequently agree to long-term wage contracts.

We narrow the window that we require all sample firms (treated and control) to have signed a long-term contract to the years 2006-2008. Columns 1,3, and 5 of Table 11 present

[22] Inventory and accounts receivable are not among the variables available in Amadeus. Furthermore, given our sample consists of private and public firms, information on share repurchases is not broadly available.

our baseline specifications for our three measures of performance, controlling for firm-level controls and leverage. Column 1 presents the effect of treated on sales. Despite the smaller sample size, we are able to replicate our results using this alternative sample, and we get even stronger significance. Coefficients are positive and significant across all specifications at 5% or 10% level. Column 3 repeats the same specification for profits and Column 5 for market shares. The coefficient on profits is significant for 2009 at 5% level and positive for the remaining years, and the coefficient on market share is positive for all years and significant for 2010 and 2011 at the 10% and 5% level respectively.

In a further refinement of this test, we consider the possibility that there are time varying changes in how robust a firm is to a downturn. Firms believe they are more resistant to a downturn when signing a long-term wage agreement but that this resistance may be temporary. To refute this alternative explanation, we limit the sample to firms which signed long-term wage agreements in 2006 or 2007. We limit the window during which we observe long-term wage contracts to two years so that the treated and control firms have similar time-varying resistance to a downturn. We use 2006 and 2007 specifically as we observe treated and control firms in both years. All firms which sign long-term deals in 2008 are coded as treated, by definition.

The results of this test are reported in Columns 2, 4, and 6 of Table 11. Results look very similar to those in Columns 1, 3, and 5 in terms of both statistical significance and magnitudes, albeit at a cost of lower power. This may explain why we cannot replicate significance in our profitability estimation with this alternative sample.

Second, we add firm-specific trends to the list of controls of our baseline specification, by multiplying year fixed effects with pre-treatment firm-specific characteristics. Previous research has argued that larger and more profitable firms are more resistant to downturns (Haltiwanger, Fort, Jarmin, and Miranda 2013), and firms with higher leverage are more vulnerable to downturns (Opler and Titman 1994). We thus take these three variables measured pre-treatment in 2007, sales, leverage and ROA, which have all been shown to predict how well a firm will perform during a downturn and interact them with year fixed effects. This estimation will control for any differential performance trends by larger, more profitable and lower leverage firms during the crisis.

We report results in Table 12 for sales in Columns 1-3, for profits in Columns 4-6, and for market shares in Columns 7-9. We find no evidence that controlling for observable pre-treatment characteristics correlated with firm performance during downturns is driving our results. Instead, our results are even stronger in many cases as compared to the baseline. It might still be the case that treated firms differ in their ability to manage a downturn based on unobservable characteristics. However, the fact that controlling for observable variables known to predict performance during downturns, on average, strengthens our findings of the effect of treatment indicates it is unlikely differences in resilience to a downturn is driving our results.

VIII Conclusion

The debate as to whether firms should pay workers wages above their opportunity costs has been contentious. In this paper, we revisit this long-standing controversy and attempt to shed light on the debate by answering: can higher wages lead to better firm performance? The answer to this question is important as it forms the basis of the efficiency wages argument. It is efficient for firms to be paying wages above market-clearing rates, as wages can be an important part of workers' effort equation.

We explore the impact of wages above market-clearing rates on future firm performance using a sample of firms operating in the UK during the Great Recession of 2008. Plausibly exogenous variation in wages during the crisis comes from variation in the timing of long-term wage agreements. A subset of the sample (treated firms) happened to have signed long-term wage contracts shortly before the crisis, agreeing to wage increases which could not be renegotiated as macroeconomic conditions changed. As a result, treated firms maintain historic wage growth trends during the recession. Alternatively, control firms were more likely to cut wages, especially in real terms, or at a minimum, keep wage growth below historic norms. Wages at treated firms are likely to be perceived as gifts by their employees, predicting relatively higher employee effort at these firms, as suggested in theoretical works by Akerlof (1982, 1984).

Our results are unique to the 2008 crisis and therefore, it is hard to extrapolate outside that context. However, our conclusions are important in light of the heated debate spurred by the recent crisis on how firms should be shaping employment policies to better survive a downturn. A common belief is that wage cuts can prevent layoffs leading to welfare improving outcomes, such as lower unemployment. Our results cast some doubt on that view. We show that even a small increase in wages can have big and persistent effects on firm performance in the long-run. Our results do not intend to offer a definitive answer to these issues but prompt the need for further research as the answer seems to be less than clear cut. What we can conclude is that more than 30 years of research on efficiency wages seem to fit the facts in the context of the 2008 crisis with striking evidence on firm long-run performance.

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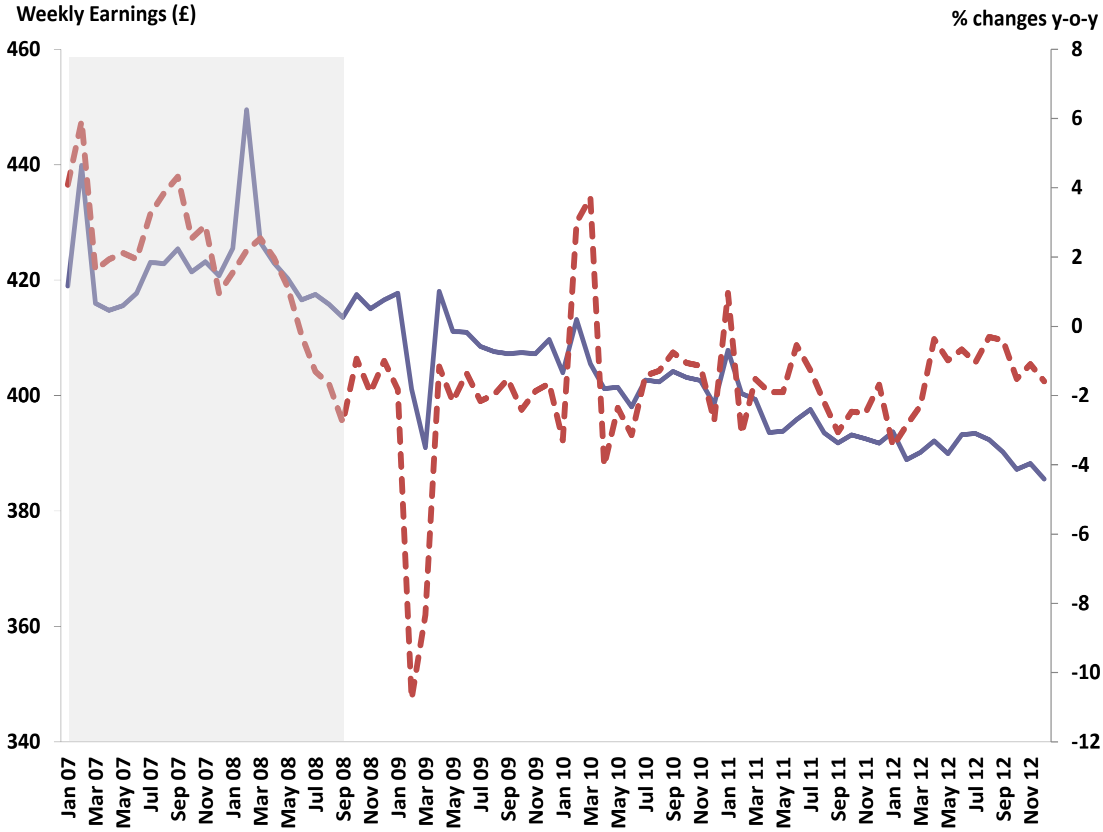

Figure 1. Average Weekly Earnings in the UK Private Sector

This figure shows average real weekly earnings for the Private sector in the UK between January 2007 and December 2012. The data are in monthly frequencies and they are seasonally adjusted. The solid line (left axis) presents averages of real weekly earnings in british pounds. These include bonuses but exclude arrears of pay. The dashed line (right axis) presents year-on-year real growth rates of weakly earlings. The changes are based on single-month averages. We highlight in grey the period before the Lehman Collapse in September 2008. Source: Office for National Statistics (ONS), UK.

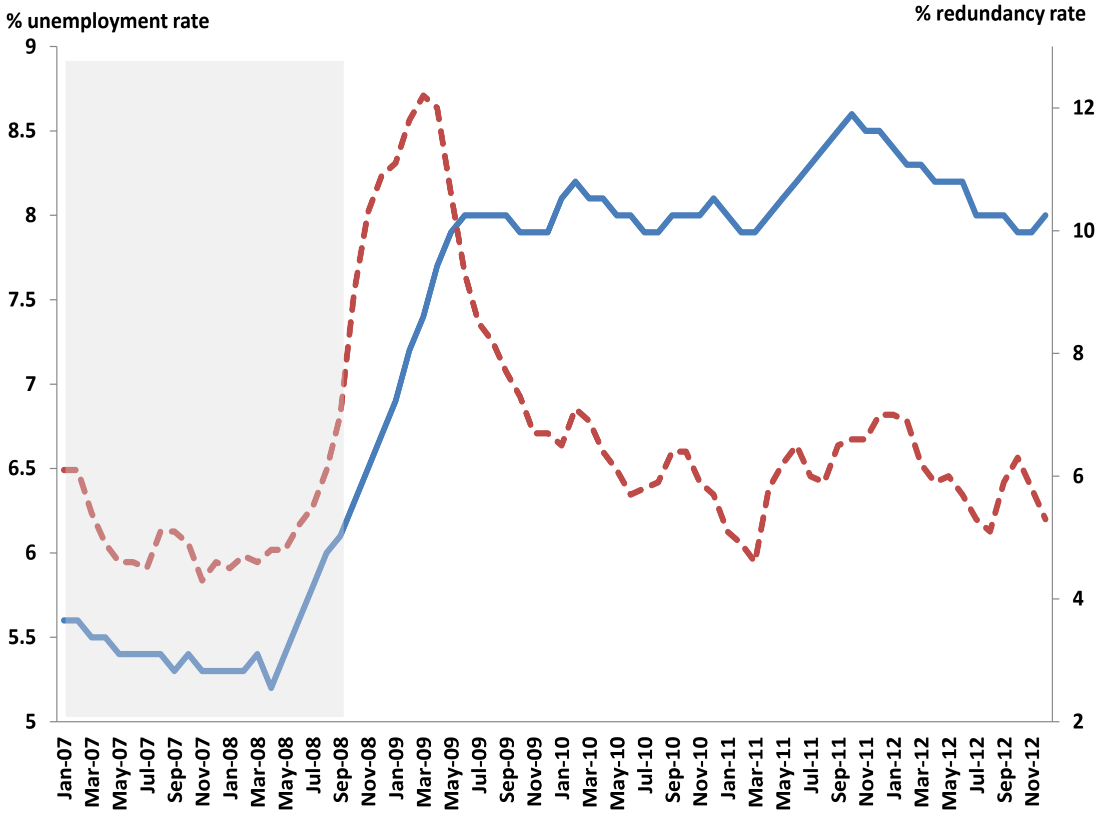

Figure 2. Unemployment Rates and Redundancy Rates in the UK
This figure plots unemployment rates and redundancy rates for the British economy during January 2007 and December 2012. Unemployment rates (solid line, left axis) and Redundancy rates (dashed line, right axis) are seasonally adjusted and defined following the ILO definition. Data are in % and in monthly frequencies. We highlight in grey the period before the Lehman Collapse in September 2008. Source: Office for National Statistics (ONS), UK.

Table 1: Distribution of Observations by Industry

This table reports the industry distribution of firms in the treated and control groups. Treated firms are defined as those which had signed a long-term agreement before September 2008 and are bound by this agreement for at least 15 months of the crisis. The control firms include firms which have signed long-term agreements before the crisis, but with no or modest overlap with the crisis. Column 1 reports the percent of treated firms which are in a given 1-digit SIC code. Column 2 reports the percent of control firms which are in a given 1-digit SIC code. Column 3 reports the percent of sample firms which are in a given 1-digit SIC code. Column 4 reports the percent of firms in the LRD sample which are in a given 1-digit SIC code. Column 5 reports the percent of firms in the IDS sample which are in a given 1-digit SIC code.

Industry	% of treated firms	% of control firms	% of sample firms	% of LRD sample firms	% of IDS sample firms
1000-1999	2.38	1.50	1.84	0.66	2.87
2000-2999	19.84	13.00	15.64	19.74	12.07
3000-3999	23.02	27.50	25.77	22.37	28.74
4000-4999	34.92	27.00	30.06	33.55	27.01
5000-5999	2.38	7.00	5.21	4.61	5.75
6000-6999	12.70	14.50	13.80	13.16	14.37
7000-7999	3.97	8.00	6.44	4.61	8.05
8000-8999	0.00	0.50	0.31	0.66	0.00
9000-9999	0.79	1.00	0.92	0.66	1.15

Table 2: Pre-crisis Characteristics of Treated and Control Firms, as of 2007

This table reports summary statistics for key financial variables of treated and control firms, as measured in 2007 (the year prior to crisis). Treated firms are defined as those which had signed a long-term agreement before September 2008 and are bound by this agreement for at least 15 months of the crisis. The control firms include firms which have signed long-term agreements before the crisis, but with no or modest overlap with the crisis. Column 1 reports means. Column 2 reports standard errors. Column 3 reports the p-values from a t-test for the difference in means between treated and control firms. 25th, 50th and 75th percentiles are reported in Columns 4-6, while Column 7 presents p-values from the Kolmogorov-Smirnov Test for differences in the distribution of firm characteristics between treated and control groups in 2007.

		Mean	Standard Errors	p-value of difference	25th percentile	50th percentile	75th percentile	Kolmogorov-Smirnov Test p-value
Sales (m. pounds)	Treated	1,030	(252)	0.88	35.9	178	670	0.18
	Control	960	(347)		40.4	118	440	
Total Debt/Assets	Treated	0.383	(0.027)	0.09	0.137	0.381	0.597	0.20
	Control	0.324	(0.021)		0.087	0.294	0.503	
Interest Coverage Ratio	Treated	24.05	(12.01)	0.62	-0.17	1.58	5.21	0.69
	Control	17.74	(6.86)		-0.63	1.29	7.91	
ROA	Treated	0.059	(0.008)	0.84	0.014	0.05	0.112	0.79
	Control	0.061	(0.007)		0.007	0.05	0.105	
Cash/Assets	Treated	0.094	(0.015)	0.54	0.007	0.036	0.123	0.78
	Control	0.106	(0.013)		0.003	0.037	0.144	
Wages/Employee	Treated	35.99	(1.616)	0.35	25	34	44	0.99
	Control	38.93	(2.277)		24	35	46	
Market Share	Treated	6.54	(1.236)	0.31	0.157	1.184	5.580	0.34
	Control	4.94	(0.965)		0.146	0.774	3.588	

Table 3: Wages and Employment

This table reports the effect of the 2008 crisis on wages and employment of firms covered by long-term wage agreements (LTapplies=1) compared to a set of firms not covered by these agreements. The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. The dependent variable in Columns 1-3 is wages per employee (log transformed). The dependent variable in Columns 4-6 is total employment (log transformed). ROA is measured as net income/assets. Leverage is measured as total debt to assets. Sales is log-transformed. Industry median values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. *** indicates p< 0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Wages/Employees)			Log(Employees)		
	(1)	(2)	(3)	(4)	(5)	(6)
$LTapplies$	-0.021	-0.021	-0.022	0.039	0.040	0.021
	(0.014)	(0.014)	(0.014)	(0.024)	(0.024)*	(0.023)
$Post_{1,(t=2009)} * LTapplies$	0.066	0.072	0.078	-0.009	-0.015	0.039
	(0.032)**	(0.032)**	(0.033)**	(0.081)	(0.082)	(0.075)
$Post_{2,(t=2010)} * LTapplies$	0.028	0.026	0.027	0.071	0.071	0.034
	(0.035)	(0.034)	(0.034)	(0.055)	(0.055)	(0.048)
Industry ROA		-0.065	-0.033		0.284	0.490
		(0.190)	(0.193)		(0.345)	(0.273)*
Industry Sales		-0.115	-0.115		0.108	0.102
		(0.061)*	(0.061)*		(0.108)	(0.087)
Industry Market/Book		0.001	0.001		-0.009	-0.007
		(0.003)	(0.003)		(0.008)	(0.007)
ROA			-0.206			-0.142
			(0.127)			(0.236)
Log(Sales)			0.018			0.400
			(0.030)			(0.093)***
Leverage			0.032			0.035
			(0.061)			(0.058)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.87	0.87	0.87	0.97	0.97	0.98
Obs.	1,755	1,755	1,755	1,792	1,792	1,792

Table 4: Ex-Post Performance: Baseline Results and Sales Dynamics

This table reports changes in sales (log-transformed) at treated firms during the recession and post-recession years as compared to a set of control firms. The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Leverage is measured as total debt to assets. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. *** indicates p< 0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Sales)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{-2,(t=2006)}$*Treated				-0.007	-0.003	-0.004
				(0.088)	(0.088)	(0.089)
$Post_{-1,(t=2007)}$*Treated				0.050	0.055	0.062
				(0.078)	(0.078)	(0.079)
$Post_{1,(t=2009)}$*Treated	0.128	0.138	0.141	0.143	0.155	0.161
	(0.093)	(0.093)	(0.094)	(0.112)	(0.113)	(0.114)
$Post_{2,(t=2010)}$*Treated	0.185	0.192	0.200	0.200	0.210	0.220
	(0.099)*	(0.099)**	(0.097)**	(0.120)*	(0.120)*	(0.118)*
$Post_{3,(t=2011)}$*Treated	0.214	0.226	0.228	0.228	0.243	0.248
	(0.101)**	(0.102)**	(0.102)**	(0.114)**	(0.115)**	(0.116)**
$Post_{4,(t=2012)}$*Treated	0.203	0.210	0.212	0.217	0.227	0.231
	(0.126)	(0.127)*	(0.127)*	(0.133)*	(0.134)*	0.134)*
Industry ROA		0.388	0.360		0.405	0.379
		(1.119)	(1.124)		(1.124)	(1.129)
Industry Sales		-0.115	-0.117		-0.116	-0.118
		(0.057)**	(0.057)**		(0.057)**	(0.057)**
Industry Market/Book		-0.031	-0.030		-0.031	-0.030
		(0.027)	(0.027)		(0.027)	(0.027)
Leverage			-0.162			-0.166
			(0.185)			(0.185)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.93	0.93	0.94	0.93	0.93	0.94
Obs.	1,826	1,826	1,826	1,826	1,826	1,826

Table 5: Cross-Sectional Regressions: Occupations

This table reports changes in sales (log-transformed) at treated firms during the recession and post-recession years as compared to a set of control firms. The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Superv. takes value of 1 if the long-term contract covers workers in supervisory roles. Blue-Collar takes value of 1 if the long-term contract covers low-skill blue-collar workers. Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. *** indicates p< 0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Sales)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{1,(t=2009)}$*Treated	0.125	0.140	0.144	0.256	0.271	0.284
	(0.106)	(0.106)	(0.107)	(0.116)**	(0.115)**	(0.117)**
$Post_{2,(t=2010)}$*Treated	0.179	0.192	0.198	0.338	0.354	0.357
	(0.104)*	(0.104)*	(0.103)*	(0.139)**	(0.140)**	(0.140)**
$Post_{3,(t=2011)}$*Treated	0.210	0.229	0.228	0.335	0.339	0.336
	(0.104)**	(0.105)**	(0.105)**	(0.143)**	(0.143)**	(0.143)**
$Post_{4,(t=2012)}$*Treated	0.185	0.193	0.193	0.380	0.400	0.393
	(0.131)	(0.131)	(0.131)	(0.159)**	(0.162)**	(0.163)**
$Post_{1,(t=2009)}$*Treated*Superv.	0.475	0.449	0.449			
	(0.277)*	(0.272)*	(0.271)*			
$Post_{2,(t=2010)}$*Treated*Superv.	0.421	0.401	0.403			
	(0.266)	(0.260)	(0.261)			
$Post_{3,(t=2011)}$*Treated*Superv.	0.481	0.469	0.462			
	(0.311)	(0.315)	(0.306)			
$Post_{4,(t=2012)}$*Treated*Superv.	0.569	0.562	0.556			
	(0.287)**	(0.307)*	(0.298)*			
$Post_{1,(t=2009)}$*Treated*Blue-Collar				-0.172	-0.175	-0.188
				(0.163)	(0.161)	(0.161)
$Post_{2,(t=2010)}$*Treated*Blue-Collar				-0.217	-0.224	-0.220
				(0.121)*	(0.122)*	(0.123)*
$Post_{3,(t=2011)}$*Treated*Blue-Collar				-0.165	-0.140	-0.137
				(0.128)	(0.127)	(0.126)
$Post_{4,(t=2012)}$*Treated*Blue-Collar				-0.273	-0.293	-0.283
				(0.149)*	(0.149)**	(0.148)*
Industry Controls		Yes	Yes		Yes	Yes
Firm-level Controls		Yes	Yes		Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.93	0.93	0.93	0.93	0.93	0.93
Obs.	1,656	1,656	1,656	1,656	1,656	1,656

Table 6: Cross-Sectional Regressions: High Skill and Low Employee Turnover Industries

This table reports changes in sales (log-transformed) at treated firms during the recession and post-recession years as compared to a set of control firms. The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Industry in Columns 1-3 takes a value of 1 if the firm is in an industry where average industry wages was in the top quartile for the sample. Industry in Columns 4-6 takes a value of 1 if the firm is an industry where average industry turnover is in the bottom quartile for the sample. Interactions of the Industry variable with year fixed effects are also estimated but not included to conserve space. We measure turnover using industry average quit rates in Job Openings and Labor Turnover (JOLTS) data produced by the US Bureau of Labor Statistics (BLS). Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. . *** indicates p <0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Sales)					
	High Skill			Low Turnover		
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{1,(t=2009)}$*Treated	0.107	0.112	0.113	0.145	0.196	0.190
	(0.100)	(0.100)	(0.100)	(0.179)	(0.174)	(0.171)
$Post_{2,(t=2010)}$*Treated	0.122	0.130	0.135	0.160	0.187	0.192
	(0.090)	(0.090)	(0.089)	(0.202)	(0.203)	(0.208)
$Post_{3,(t=2011)}$*Treated	0.080	0.089	0.088	-0.248	-0.217	-0.209
	(0.087)	(0.088)	(0.088)	(0.173)	(0.175)	(0.176)
$Post_{4,(t=2012)}$*Treated	0.022	0.041	0.041	-0.325	-0.263	-0.249
	(0.119)	(0.120)	(0.120)	(0.212)	(0.228)	(0.234)
$Post_{1,(t=2009)}$*Treated*Industry	0.169	0.174	0.189	0.069	0.006	0.017
	(0.298)	(0.300)	(0.301)	(0.198)	(0.188)	(0.182)
$Post_{2,(t=2010)}$*Treated*Industry	0.315	0.310	0.318	0.008	-0.029	-0.034
	(0.311)	(0.312)	(0.309)	(0.197)	(0.189)	(0.192)
$Post_{3,(t=2011)}$*Treated*Industry	0.657	0.669	0.674	0.453	0.417	0.415
	(0.351)*	(0.360)*	(0.360)*	(0.166)***	(0.167)***	(0.167***
$Post_{4,(t=2012)}$*Treated*Industry	0.872	0.826	0.840	0.536	0.452	0.442
	(0.400)**	(0.400)**	(0.400)**	(0.229)**	(0.245)*	(0.243)*
Industry Controls		Yes	Yes		Yes	Yes
Firm-level Controls			Yes			Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.94	0.94	0.94	0.94	0.94	0.94
Obs.	1,823	1,823	1,823	887	887	887

Table 7: Falsification Test

This table reports changes in sales (log-transformed) at placebo-treated firms during a placebo recession and post-recession years as compared to a set of control firms. Firms are included in the placebo treated group if the firm signed a long-term labor agreement prior to September 2004 (Columns 1-3), September 2005 (Columns 4-6), September 2006 (Columns 7-9) and if this long-term labor agreement extended for at least 15 months past the placebo crisis. Control firms include all observations not assigned to the placebo treated group. The sample timeline begins in 2003 and ends in 2008 in Columns 1-3, in 2009 in Columns 4-6, and in 2010 in Columns 7-9. Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. . *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Log(Sales)								
	2004 Placebo Crisis			2005 Placebo Crisis			2006 Placebo Crisis		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Post_1$*Placebo	0.107	0.105	0.102	0.026	0.027	0.023	0.023	0.022	0.022
	(0.114)	(0.113)	(0.114)	(0.065)	(0.065)	(0.066)	(0.079)	(0.080)	(0.080)
$Post_2$*Placebo	0.112	0.111	0.106	0.004	0.003	-0.002	0.078	0.070	0.071
	(0.099)	(0.098)	(0.102)	(0.091)	(0.091)	(0.092)	(0.106)	(0.107)	(0.107)
$Post_{3,}$*Placebo	-0.058	-0.059	-0.063	0.026	0.023	0.019	-0.012	-0.022	-0.022
	(0.114)	(0.113)	(0.117)	(0.103)	(0.103)	(0.103)	(0.128)	(0.129)	(0.128)
$Post_4$*Placebo	-0.059	-0.064	-0.063	-0.001	-0.002	-0.001	-0.243	-0.256	-0.256
	(0.152)	(0.150)	(0.151)	(0.120)	(0.120)	(0.120)	(0.178)	(0.181)	(0.181)
Industry Controls		Yes	Yes		Yes	Yes		Yes	Yes
Firm-level Controls			Yes			Yes			Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.97	0.97	0.97	0.97	0.97	0.97	0.95	0.95	0.95
Obs.	1,094	1,094	1,094	1,355	1,355	1,355	1,623	1,623	1,623

Table 8: Treatment Intensity

This table reports changes in sales (log-transformed) at treated firms during the recession and post-recession years as compared to a set of control firms, accounting for the intensity of treatment. The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Duration in Columns 1-3 is measured as the logarithm of the number of months during the crisis over which the long-term deal applies. Duration in Columns 4-6 is measured as the ratio of the duration of the long-term contract that coincides with the crisis divided by the total duration of the deal signed. In both cases, these variables are set to 0 for control firms. Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. . *** indicates p <0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Sales)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{1,(t=2009)}$*Treated*Duration	0.0413	0.0440	0.0454	0.207	0.219	0.225
	(0.0299)	(0.0301)	(0.0303)	(0.124)*	(0.125)*	(0.125)*
$Post_{2,(t=2010)}$*Treated*Duration	0.0634	0.0655	0.0678	0.263	0.274	0.282
	(0.0325)**	(0.0324)**	(0.0317)**	(0.135)*	(0.135)**	(0.133)**
$Post_{3,(t=2011)}$*Treated*Duration	0.0707	0.0746	0.0753	0.305	0.326	0.327
	(0.0330)**	(0.0334)**	(0.0334)**	(0.137)**	(0.139)**	(0.139)**
$Post_{4,(t=2012)}$*Treated*Duration	0.0668	0.0690	0.0698	0.249	0.257	0.260
	(0.0413)	(0.0416)*	(0.0415)*	(0.172)	(0.173)	(0.172)
Industry Controls		Yes	Yes		Yes	Yes
Firm-level Controls			Yes			Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.93	0.93	0.94	0.93	0.93	0.94
Obs.	1,826	1,826	1,826	1,826	1,826	1,826

Table 9: Other Outcome Measures

This table reports changes in profits (Columns 1-3) and market shares (Columns 4-6) at treated firms during the recession and post-recession years as compared to a set of control firms. Profits are measured as ROA (net income/assets) and market share is market share by sales (log-transformed). The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. . *** indicates p <0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	ROA			Log(Market Share)		
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{1,(t=2009)}$*Treated	0.0232	0.0246	0.0271	0.130	0.150	0.154
	(0.0119)**	(0.0119)**	(0.0118)**	(0.096)	(0.096)	(0.097)
$Post_{2,(t=2010)}$*Treated	0.0159	0.0175	0.0216	0.173	0.185	0.194
	(0.0110)	(0.0111)	(0.0107)**	(0.100)*	(0.100)*	(0.098)**
$Post_{3,(t=2011)}$*Treated	0.0106	0.0124	0.0141	0.200	0.210	0.212
	(0.0126)	(0.0126)	(0.0124)	(0.102)*	(0.103)**	(0.102)**
$Post_{4,(t=2012)}$*Treated	0.0146	0.0163	0.0179	0.212	0.219	0.221
	(0.0115)	(0.0114)	(0.0110)	(0.146)	(0.144)	(0.143)
Industry Controls		Yes	Yes		Yes	Yes
Firm-level Controls			Yes			Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.52	0.52	0.53	0.95	0.95	0.95
Obs.	1,824	1,824	1,824	1,806	1,806	1,806

Table 10: Matching

This table reports changes in sales (Columns 1-3), profits (Columns 4-6) and market shares (Columns 7-9) at treated firms during the recession and post-recession years as compared to a set of control firms based on a matched sample. Sales are log transformed, profits are measured as ROA (net income/assets) and market share is market share by sales (log-transformed). We match by sales (Columns 1, 4, 7), leverage (Columns 2, 5, 8), and assets (Columns 3, 6, 9) based on pre-treatment values at the time the binding contract is signed for each treated firm. Matching is done with replacement and any firms that cannot be matched are dropped from the estimation. The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. . *** indicates p <0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Sales)			ROA			Log(Market Share)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Post_{1,(t=2009)}$*Treated	0.176	0.128	0.181	0.0343	0.0222	0.0241	0.168	0.149	0.186
	(0.093)*	(0.100)	(0.098)*	(0.0137)**	(0.0117)*	(0.0123)**	(0.097)*	(0.102)	(0.101)*
$Post_{2,(t=2010)}$*Treated	0.219	0.232	0.260	0.0212	0.0155	0.0210	0.194	0.223	0.238
	(0.092)**	(0.109)**	(0.106)**	(0.0112)*	(0.0111)	(0.0106)**	(0.094)**	(0.108)**	(0.106)**
$Post_{3,(t=2011)}$*Treated	0.191	0.224	0.231	0.0172	0.0114	0.0174	0.161	0.214	0.215
	(0.101)*	(0.111)**	(0.106)**	(0.0134)	(0.0122)	(0.0127)	(0.098)*	(0.112)*	(0.104)**
$Post_{4,(t=2012)}$*Treated	0.166	0.257	0.220	0.0189	0.0207	0.0200	0.143	0.306	0.211
	(0.126)	(0.142)*	(0.127)*	(0.0118)	(0.0112)*	(0.0113)*	(0.130)	(0.159)*	(0.132)
Industry Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.95	0.94	0.94	0.54	0.56	0.55	0.96	0.95	0.96
Obs.	1,542	1,637	1,636	1,477	1,625	1,628	1,529	1,618	1,618

Table 11: Firms Resistant to Downturns (I)

This table reports changes in sales (Columns 1-2), profits (Columns 3-4) and market shares (Columns 5-6) at treated firms during the recession and post-recession years as compared to a set of control firms. For this estimation we require that both treated and control firms have signed at least one long-term wage agreement between 2006-2008 (Columns 1, 3, 5) and 2006-2007 (Columns 2, 4, 6). Sales are log transformed, profits are measured as ROA (net income/assets) and market share is market share by sales (log-transformed). The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. . *** indicates p <0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Sales)		ROA		Log(Market Share)	
	(1)	(2)	(3)	(4)	(5)	(6)
	2006/08	2006/07	2006/08	2006/07	2006/08	2006/07
$Post_{1,(t=2009)}$*Treated	0.205	0.184	0.0267	0.0160	0.185	0.198
	(0.125)*	(0.155)	(0.0133)**	(0.0146)	(0.128)	(0.158)
$Post_{2,(t=2010)}$*Treated	0.308	0.230	0.0151	0.0021	0.285	0.216
	(0.169)*	(0.172)	(0.0140)	(0.0161)	(0.162)*	(0.165)
$Post_{3,(t=2011)}$*Treated	0.419	0.389	0.0025	0.0044	0.386	0.349
	(0.185)**	(0.193)**	(0.0157)	(0.0183)	(0.175)**	(0.179)**
$Post_{4,(t=2012)}$*Treated	0.336	0.344	0.0085	0.0073	0.301	0.383
	(0.199)*	(0.203)*	(0.0142)	(0.0161)	(0.196)	(0.206)*
Industry Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.94	0.94	0.58	0.58	0.95	0.95
Obs.	1,223	937	1,217	931	1,207	926

Table 12: Firms Resistant to Downturns (II)

This table reports changes in sales (Columns 1-3), profits (Columns 4-6) and market shares (Columns 7-9) at treated firms during the recession and post-recession years as compared to a set of control firms. Sales are log transformed, profits are measured as ROA (net income/assets) and market share is market share by sales (log-transformed). The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. These estimations control for firm-specific trends by multiplying year fixed effects with pre-treatment firm-specific characteristics, measured in 2007. We take three variables: sales (Columns 1, 4, 7), leverage (Columns 2, 5, 8), and ROA (Columns 3, 6, 9). Industry controls include median industry ROA, Sales and Market/Book. Industry values are estimated on an annual basis, using 3-digit SIC codes. Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity. Firm level controls include leverage. Missing values for control variables are replaced with the sample median. All regressions include firm and (one-digit SIC) industry times year fixed effects and robust standard errors clustered at the firm-level. . *** indicates p <0.01, ** indicates p< 0.05, and * indicates p< 0.1.

	Log(Sales)			ROA			Log(Market Share)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Post_{1,(t=2009)}$*Treated	0.129	0.106	0.120	0.0342	0.0260	0.0290	0.124	0.0988	0.112
	(0.0747)*	(0.0698)	(0.0780)	(0.0130)***	(0.0118)**	(0.0113)***	(0.0809)	(0.0766)	(0.0844)
$Post_{2,(t=2010)}$*Treated	0.213	0.177	0.224	0.0308	0.0229	0.0252	0.190	0.154	0.196
	(0.0931)**	(0.0947)*	(0.101)**	(0.0113)***	(0.0108)**	(0.0102)***	(0.0934)**	(0.0958)	(0.100)**
$Post_{3,(t=2011)}$*Treated	0.177	0.243	0.246	0.0155	0.0067	0.0095	0.155	0.210	0.211
	(0.0995)*	(0.107)**	(0.109)**	(0.0132)	(0.0127)	(0.0113)	(0.0099)	(0.104)**	(0.108)**
$Post_{4,(t=2012)}$*Treated	0.242	0.212	0.246	0.0218	0.0191	0.0202	0.262	0.197	0.257
	(0.125)**	(0.123)*	(0.132)*	(0.0118)*	(0.0112)*	(0.0108)*	(0.143)*	(0.138)	(0.152)*
Industry Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.94	0.94	0.93	0.55	0.55	0.59	0.95	0.96	0.95
Obs.	1,703	1,637	1,619	1,589	1,633	1,723	1,687	1,619	1,602