Wages and Firm Performance: Evidence from the 2008 Financial Crisis

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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 paid higher wages but also realized 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. 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.

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I Introduction

In his seminal contribution, Akerlof (1982) argues that paying employees above market-clearing rates 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 firms from 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 UK firms increase wages for plausibly exogenous reasons due to heterogeneity in the timing of long-term wage contracts. We first identify a sample of firms subject to long-term wage contracts at the beginning of the 2008 recession. These firms (treated) agreed to binding and significant wage increases before the 2008 crisis, in anticipation of better economic times and tighter labor markets. 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, leaving these firms with greater flexibility to adjust wages in response to changes in the labor market.

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. Sales at treated firms increase between 18% and 23% more in the post-crisis years of 2010-2012, as compared to control firms. We find a more muted effect when we explore return on assets (ROA), reflecting the fact that this measure incorporates changes in both sales and wages. ROA at treated firms increases between 2% and 3% more in the post-crisis years, as compared to control firms. This result demonstrates that the overall benefit of incentivizing employees exceeds the cost of the wage premium. We also find similar patterns when looking at market shares, indicating the results are not driven by random coincidental changes in industry performance following the crisis.

These results are consistent with efficiency wage models where a relationship between relative wages and worker productivity affects firm performance (Akerlof 1982, 1984). The intuition relies on psychological factors which play an important motivational role for workers. Workers perceive higher wages as a "gift" and reciprocate by exerting higher effort.

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 managers of the unconstrained firms either made decisions that ex-post were not value-maximizing or were more focused on short-term goals, such as conserving cash during the crisis, as opposed to maximizing long-run profitability. Consistent with our conjecture, we find a weaker treatment effect when looking at firms known to have better quality managers or to have a more long-term focus, as proxied by concentrated ownership.

To further bolster our hypothesis, we parse the 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 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. We also show that paying higher wages leads to relatively better performance in high-wage industries. High-wage industries employ more skilled labor, workers which can have a bigger effect on firm operations.

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.[1] 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 show that they are

[1] Under UK law, binding employment contracts cannot be changed without both parties approval except if the firm is insolvent or acquired. Furthermore, even in these two cases, changes to pre-existing contracts must meet stringent criteria. Moreover, time series information on labor contracts do not indicate any renegotiations happening before the contracts' expiry dates.

similar pre-treatment.[2] 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 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". Remaining potential concerns for our identification are that firms may enter into long-term agreements at times when they anticipate they are better able to manage a negative shock or that our results are conditional on a unique control group. Additional robustness tests controlling for time varying performance based on firm ex-ante characteristics and alternate control samples mitigate these concerns.

The article contributes to a vast literature in economics providing supportive evidence on efficiency wages models: applicants queue for jobs paying rents (Holzer, Katz and Krueger, 1991); workers shirk less if they are better paid (Cappelli and Chauvin, 1991); high quality workers are easier to attract and retain when firm pay compared to outside alternatives is higher (Propper and Van Reenen, 2010); wages and monitoring are substitutes (Krueger, 1991); and, higher wages lead to higher productivity (Raff and Summers 1987, and Mas 2006). Evidence consistent with efficiency wages has also been reported in a number of experimental and field settings, such as in Fehr, Kirchsteiger, and Riedl (1993), 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

[2] 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.

wages to the firm, and provides evidence in line with the intuition of efficiency wages.

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. Brown and Matsa (2014) shows that firms in financial distress have lower ability in attracting job applicants.

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-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 close to random whether an agreement is signed in January or June of the same year.

Multi-year agreements provide several advantages. Firms benefit as long-term agreements can lead to greater cooperation with unions during the period in which wages cannot be renegotiated (Hashimoto and Yu (1980)).[3] Employees also gain from the greater certainty about future pay raises.

Since both parties voluntarily agree to the long-term wage agreement, the agreed upon wage changes should reflect both parties' expectations about future economic conditions. As such, committing to long-term wage contracts will typically have 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 binding and cannot be unilaterally renegotiated down (Beaudry and DiNardo (1991), Hashimoto and Yu (1980), Hall and Lazaer (1984), Lemieux, MacLeod and Parent (2012)). Under UK law, binding employment contracts cannot be changed without both parties approval except for rare exceptions.[4] We observe the time series of contracts for firms in our sample which remain solvent and there are no contract renegotiations.[5]

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

[3]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"*.

[4]Contracts can only be amended unilaterally if the firm is insolvent or acquired. Moreover, even in these two cases, changes to pre-existing contracts must meet certain criteria as defined in the Business Transfers Directive, passed in 1978, updated in 2001, the Transfer of Undertakings Regulation of 2006 and the Insolvency Act of 1986 (Schedule B1).

[5]Our sample differs from Benmelech, Bergman and Enriquez (2012) which, when looking at the troubled airline industry, observe firms renegotiating wages when near bankruptcy.

long-term agreements are recognized by at least one union. The firms in our sample are regularly signing wage agreements with their workers. Within-firm, time series variation exists as to whether a long-term or short-term contract is applicable for a given year.

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.[6] 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. 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. 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.[7]

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

[6]IDS was acquired by Thomson Reuters (Professional) UK Limited in 2005.

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

important in our case since our sample includes both public and private companies. After matching, our sample is comprised of 344 unique firms, though sample size varies across specifications 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 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.[8]

IV Empirical Strategy

All firms in our sample have signed long-term wage agreements. Firms are assigned to the treated or control group 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 this 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. Long-term agreements expire, on average, 22 months after the onset of the crisis for treated firms, and after 2 months for control firms.

Both the IDS and LRD samples span a wide range of industries. Table 1 presents the frequency of observations across industries. Industries are defined based on one-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

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

the sample covers manufacturing industries, 30% of the sample covers transportation and communication services, 26% of the sample covers retail trade and other services. Column 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. 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.

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

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 placebo tests in which we explore differences in outcomes between firms with and firms without long-term contracts during periods of time which do not overlap with the crisis. We also address concerns that firms enter into these agreements when they are more resistant to downturns. The results of these tests 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.

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 and 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:

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

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 coefficient δ_0, captures 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. The coefficients δ_1 and δ_2 capture the differential effect of having a contract in place in 2009 and 2010 on wages per employee. We exclude year 2008 as this is a transition year. We start our sample in 2005 to provide sufficient years to estimate baseline wages for each firm and end 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 does not predict 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 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, as 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 (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.[9] 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 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 on 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

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

crisis years, regardless of the specification. [10]This is 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 makes it difficult to implement layoffs.

V Ex-Post Performance

V.1 Baseline Results

The evidence presented above shows that firms with a long-term contract in effect at the onset of the crisis paid relatively 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. Higher wages may also 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 higher wages and 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 before the 2008 crisis and these agreements extend into the crisis by at least 15 months (treated) to firms that have signed long-term agreements that do not expire deep in the crisis (control).[11] We estimate regressions of the following form:

[10]Appendix Table A1 shows similar results but uses the methodology followed in our baseline results.

[11]A firm is classified as treated if it signed a long-term contract before September 2008 that expired post-January 2010.

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

where i, j, and t index firms, industries and years; $post_{(t=\tau)}$ takes a 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.[12] As predicted, we find that sales in the treated firms increase by 18% more above the baseline years 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. All these coefficients capture an increase in sales relative to the baseline, as compared to changes from the baseline at control firms.

In Column 2, we control for additional industry level controls, defined at the three-digit SIC level. 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 changes from the baseline at 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

[12] In Appendix Table A2, we show that key results are robust to including two-digit SIC industry times year fixed effects.

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.[13]

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 $pre_{(-2,t=2006)} \cdot Treated_i$ and $pre_{(-1,t=2007)} \cdot Treated_i$. 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. 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 our treated and control firms are not following different trends before the event. These findings validate 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.[14] Moreover, in

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[13]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. 13 firms (4%) leave our sample prematurely due to bankruptcy. Three of these firms are treated (23%) and nine are control, paralleling the sample wide statistics where 30% of the full sample are treated. In Appendix Table A3 we show that key results are robust to dropping bankrupt firms.

[14]See Dickens, Katz, Lang, and Summers (1989) for relevant discussion, and Holzer, Katz, and

our setting, part of union negotiations with firms include safeguarding working conditions for their employees. It thus seems highly unlikely that treated firms cut other forms of compensation to offset higher agreed wages.

We acknowledge that we are limited in our ability to differentiate whether the effect is driven by a gift-exchange hypothesis where workers at the treated firms provide greater effort or a hypothesis where workers at the control firms lower effort in response to wages perceived to be unfair. 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 is further developed in the fair wage effort hypothesis in Akerlof and Yellen (1990) where workers will exert lower effort if their wage is lower compared to their perceived fair wage.[15] 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. However, we posit it is less likely that workers of control firms perceived lower wages 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.

V.1.1 Magnitudes and Persistence

Sales are 18% higher for treated firms relative to the baseline in 2010 and treated firms retain this boost in sales through the end of our available data, in 2012 (Column 1, Table 4). These are economically significant and persistent effects. In this section, we discuss the magnitudes and duration of our effects as compared to findings in the existing literature.

Krueger (1991) in the context of minimum wages.

[15]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.

Mas (2006), who studies the effect of wage arbitration agreements on the productivity of police departments, finds a 12% higher clearance rate, as compared to when the arbitration decision awards the lower wage with an average persistence of 22 months, and a 22% increase in the probability of incarceration conditional on the charges imposed if the union wins.[16] In a field study, Gneezy and List (2007) also document an increase in worker effort when wages are shocked upwards but this superior performance is short-lived. After a few hours, there is no difference in performance between the high- and low-wage groups. Other studies have made similar arguments, pointing towards a more short-lived "gift-exchange" effect. Higher wages may create entitlement effects or a new "status quo", where workers adapt and then feel they have a right to these higher wages (Falk, Fehr, Zehnder 2006).

We attribute the greater persistence in our study to the following points. First, in our study, the higher wage treatment lasts for at least 15 months, as compared to shorter treatment periods in the earlier literature, such as Gneezy and List (2007). Second, a gift exchange may be more effective when selected participants are long-term employees, as opposed to short-term contract workers. Indeed, in later tests, we find that treatment effects are stronger when the contract covers specific employee groups, such as employees with supervisory responsibilities and who work in high-wage occupations. Fourth, as argued in Mas (2006), group polarization, where social interactions reinforce and augment individual responses, may lead to longer persistence.[17] Finally, a separate line of research suggests that certain life events can change set points in a manner that the individual does not return to the baseline level of well-being (Lucas et al 2004).

It is also worth emphasizing that it is hard to extrapolate from one setting to another. Our setting is the 2008 financial crisis, and treatment effects could be stronger during the crisis. Perhaps employees are more receptive to higher wages during the crisis, a period when risk-averse workers would place relatively greater value on extra income. During a crisis, feelings of entitlement may be less likely to occur. Lazear, Shaw and Stanton

[16] See Figure 3 and Table V respectively in Mas (2006).

[17] See Isenberg (1986) for a review of the theories and empirical evidence in support of group polarization.

(2014) finds that employee productivity is higher during the 2008 recession in locations that experienced more negative employment shocks. Finally, survey evidence in Bewley (1995) argues that increasing pay makes it easier for the firm to attract and retain the best workers. It is possible the short-term effect we observe is driven by the immediate impact of a gift exchange on workers and the longer-term effect is due to a change in the quality of employees at treated firms, as compared to control firms.

Finally, in these regressions with sales as the measure of firm performance, we are only measuring the benefit of efficiency wages and not also considering the costs of the higher wages. In later tests when we measure performance using ROA, we find more modest and less persistent changes in firm performance.

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 (less) pronounced for deals that cover employees more (less) likely to impact firm performance. Second, we show that the positive effect of treated on firm performance is more pronounced in sectors that employ more skilled workers.

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 job functions that typically have greater impacts on firm performance and a dummy variable for deals which cover job functions that typically have more moderate impacts on firm performance. We expect to observe a greater effect 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 lower effect when low-skill workers, such as janitors, are covered by the agreement.[18]

Columns 1-3 of Table 5 augment our baseline specification with interaction terms of our treated variable and an indicator ("Supervisors"), which takes a value of 1 if the contract covers 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 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 2011 and at 5% level for 2012, while the coefficients for 2009 and 2010 are just outside of regular significance levels with p-values of 0.15 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 workers on treatment, as these workers should have the lowest ability to influence firm performance. Thus, we interact our treated variable with an indicator ("Low Skill"), which takes a value of 1 if the contract covers low-skill 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.

These tests also help address a concern that wage differentials in our sample could be driven by changes in work hours rather than hourly wage rates. Although, we do not directly observe information on hours worked in our data, the fact that our results are more pronounced for supervisors (whose compensation is less likely to be based on hourly rates) and less pronounced for low skill workers (whose compensation is more likely to be based on hourly rates) alleviates this concern.

[18]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.

V.2.2 Industry Wages

Next, 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, Katz 1986, Leonard 1987, Krueger, 1991).

We identify industries relatively more reliant on skilled labor using average industry wages, as measured in 2007 at the three-digit SIC code level. 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 5% for 2011 and 2012. These results are consistent with the prediction that returns to greater employee effort will be higher in industries where human capital is a more important source of value.

V.3 Why Do Unconstrained Firms Perform Worse?

Common wisdom would suggest that the unconstrained firms, namely those not subject to long-term wage contracts during the crisis, would outperform their constrained peers. Yet, we find, surprisingly, the opposite: Firms covered by long-agreements perform better in the long-run. Ex post, it is apparent that in not copying the high-wage strategy of the constrained firms, the unconstrained firms made an error of judgment. In this section, we discuss two possible explanations why this may be the case.

First, it is possible that managers of the control firms made a mistake.[19] This 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.[20] It is intuitive that firms with better management practices, presumably those firms with better managers, are less likely to make mistakes, predicting a weaker treatment effect.

To proxy for good managers, we use data on management practices in the UK, aggregated at the two-digit industry level, from Bloom and Van Reenen (2007). According to Bloom and Van Reenen (2007), better management practices are correlated with firm and industry characteristics. For example, better management practices are more prevalent when product market competition is stronger as firms with worse managers are relatively less likely to survive in competitive industries. Thus, we define an indicator (Mgt. quality) which takes a value of 1 if the average industry management score is in the top quartile for our sample, and 0 otherwise. In Columns 1-3 of Table 7, we interact the treated times year dummy terms with our indicator variable for whether firms follow good management practices. To conserve space, the coefficients on the interactions of high management quality industries indicator with year fixed effects are not reported. Column 1 presents the results including industry controls, Column 2 additionally controls for leverage, while Column 3 adds year fixed effects times firm size (measured as of 2007) to control for the possibility that firms with different management practices may also differ in terms of size. The triple

[19]A similar argument is presented in Acharya and Richardson (2009). They show that the largest financial institutions, those firms which are traditionally assumed to be some of the most informed and savvy market participants, made an error in retaining too much exposure to mortgage backed securities.

[20]Barberis (2013) extends on this argument by suggesting that a well-established behavior bias may also have played a role. He argues that cognitive dissonance leads individuals to bias their beliefs when taking actions which would otherwise conflict with their desire to maintain a positive self-image. In the context of our setting, an unbiased manager may anticipate that reducing wages or wage growth will have a negative impact on long-run firm performance. However, a manager displaying cognitive dissonance will instead convince himself that these actions will have little or no effect on firm performance – thereby removing himself from the cause of any damage to the firm and making it more likely to approve such wage cuts.

difference interaction coefficients are all negative and significant at the 10% and 5% level in 2011 and 2012 respectively. This evidence is suggestive that, indeed, when managers are less prone to mistakes, the difference between treated and control firms is mitigated.

Second, it is also possible that, 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.[21] Cutting wages or wage growth to increase current performance during a downturn, at the expense of future performance, is consistent with survey evidence. In a recent survey of CFOs, Graham, Harvey, and Rajgopal (2005) find that 78% of their respondents would forgo long-term gains to smooth earnings today. Similarly, Asker, Farre-Mensa and Ljungqvist (2015) find evidence that firms distort investment to increase short-term performance at the expense of long-term gains. To determine whether short-termism could explain our results, we use the proxy in Asker, Farre-Mensa and Ljungqvist (2015). We argue private firms are subject to fewer short-term pressures and, hence, we expect they will more closely follow the approach of the treated firms and, in turn, suffer less of a reduction in performance in the long-term.

We include observations we can identify as public or private and we interact the treated times year dummy terms with an indicator variable for whether the firm is public. We also interact the year dummies with an indicator variable for whether the firm is publicly traded, however, we do not report these coefficients to conserve space. The results are reported in Columns 4-6 of Table 7. We find suggestive evidence consistent with the short-termism argument. The treatment effect is weaker at public firms in the short term (although this difference is not statistically significant.) Then, in the long-run, the treatment effect is greater at public firms. Column 4 includes industry controls and Column 5 additionally controls for leverage. Given public and private firms differ in terms of size, the results could be picking up differential treatment effects by firm size. Thus in Column 6, we include firm size measured in 2007 interacted with year dummies and show very little change to the coefficients of interest.

[21]Findings in Edmans (2011) gives further strength to this argument. He reports that the stock market does not fully incorporate the value of high employee satisfaction.

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 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 specific evidence that firms agree to long-term wage agreements when anticipating an increase in sales in subsequent years, this does not rule out the possibility that an unobserved omitted variable is driving both the timing of the long-term agreement and the subsequent change in sales. 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 tests, comparing results if the placebo crisis occurred in September 2004, September 2005 and September 2006.[22] 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

[22] 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.

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 beyond September 2004.

We report the results of this placebo test in Columns 1-3 of Table 8. 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, 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 interactions.[23] 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 placebo 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 the sample and control variables used, we are unable to replicate the finding of increasing sales outside of the crisis. 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 timing of 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

[23] 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 results are reported in Appendix Table A4.

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 9, we scale our treatment indicator by duration, defined as 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). 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).

VI.3 Alternative Measures of Firm Performance

Throughout the analysis, we have used sales to measure firm performance. In Table 10, we show that our results are robust to alternative measures of performance: an accounting measure of performance (ROA) and a product-market based measure of performance (market share).

First, we consider accounting profits, as measured by ROA.[24] Tests of profits can rule 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). It is worth noting, however, that all four coefficients are economically significant and do not seem to be statistically different from each other. Moreover, in Appendix Table A5, Columns 4-6, where we limit the sample to firm-year observations where sales is non-missing, we find significant differences in ROA even in the later years.

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 the 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. These results also strengthen our conclusion that we are picking up changes in individual firm performance, not differences

[24] This variable is winsorized at 5% level given the fact that is highly skewed. Results are robust to winsorizing ROA at 1% level and are reported in Appendix Table A5.

in industry performance.

VI.4 Matching

Our main identifying assumption is that treated and control firms are similar, except for the fact that treated firms have higher wage growth relative to control firms. Table 11 shows that, with the exception of leverage, there are no statistical differences across several 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 by size (as measured by assets) and leverage based on pre-treatment values at the time the binding contract is signed for each treated firm. Matching is done with replacement from the control sample and we keep the three best matches for each treated firm. 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: p-values of the t-test are 0.67 for assets, and 0.72 for leverage. Similarly, a Kolmogorov-Smirnov test of distributional differences returns a p-value of 0.57 for assets and 0.97 for leverage.

Table 11 presents the results on sales, profits and market shares. Columns 1, 3, and 5 match by assets. Columns 2, 4, and 6 match by leverage. 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. These results using a matched sample 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 at firms facing higher operating leverage and whether treated firms may be more resistant to negative shocks.

VII.1 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 at times 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 these selection concerns in the following tests. First, we limit the sample (treated and control firms) to those firms which signed a long-term contract between 2006-2008. This restriction creates a sample where both treated and control firms have signed long-term agreements within a relatively narrow window. Columns 1, 3, and 5 of Table 12 show results for our three measures of performance using this alternative sample and controlling for industry-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 Columns 2, 4 and 6, we further limit the sample to firms which signed long-term wage agreements in 2006 or 2007. 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. By limiting the window during which we observe long-term wage contracts to two years, we alleviate even more concerns that treated and control firms may differ in how they can survive a downturn. Results look very similar to those in Columns 1, 3, and 5 in terms of magnitudes, albeit at a cost of lower power. This may explain why we cannot replicate significance in our profitability estimation with this alternative sample.[25]

Second, we control for differential performance during the crisis based on pre-crisis firm characteristics. We multiply year fixed effects with pre-treatment firm-specific characteristics which previous research has shown to predict firm performance during a downturn. Fort, Haltiwanger, Jarmin, and Miranda (2013) show that larger and more profitable firms are more resistant to downturns. Opler and Titman (1994) show firms with higher leverage are more vulnerable to downturns. We, thus, take these three variables measured pre-treatment in 2007, sales, leverage and ROA. This estimation controls for any differential performance during the crisis by larger, more profitable and lower leverage firms.

We report results in Table 13 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.

VII.2 Operational Leverage

The existence of binding wage contracts paying above-market wages increases the operational leverage at treated firms. A standard prediction of this channel would be that treated

[25]At the expense of lower power, we further narrow our sample to treated and control firms who signed two-year contracts during 2006-2008 or 2006-2007 and we repeat the specifications in Table 12. Results are still significant in this very stringent specifications and are presented in Table A6 of the Appendix. Although there is limited cross-sectional variation in contract duration within our sample (most contracts are for 2 or 3 years) these tests help also address concerns that differences in durations of long-term agreements between treated and control groups could be biasing our results. Such concerns are especially relevant and discussed in detail in studies looking at corporate debt maturities (Almeida et al 2012, Carvalho 2014).

firms should perform worse during downturns, the opposite of our findings. However, there might be a bright side of operational leverage, parallel to the literature on the benefits of high financial leverage. Jensen (1989) suggests that high leverage can create a crisis environment where managers become more innovative and agency conflicts are minimized following a negative industry shock. These firms must meet performance targets or risk bankruptcy. Like debt, high wages are a fixed cost and may force managers to work even harder to avoid bankruptcy.

To investigate the possibility that our results reflect a benefit to high operational leverage, we consider a number of tests. The benefits to higher leverage accrue when managers anticipate bankruptcy as a relatively more likely scenario and, hence, take drastic actions to avoid this outcome, such as large layoffs or asset sales. Given union contracts limit the ability of treated firms to pursue large layoffs, we instead focus on asset sales, which are shown to increase firm value in the context of negative operating performance (Kang and Shivdasani, 1997). Moreover, Atanassov and Kim (2009) find that firms resort to asset sales when operating performance is depressed and the firm is limited in its ability to adjust labor inputs. In unreported results, we look at differences in asset growth between treated and control firms during and after the crisis and find no difference in terms of statistical significance as well as economic significance.

Moreover, if higher wages make treated firms more vulnerable to the negative shock of the 2008 financial crisis, then we should observe firms cutting capital expenditures during the crisis years when we observe the higher wages (Almeida, Campello, Laranjeira, and Weisbenner 2011). In unreported regressions, we observe no effect on capital expenditures for firms having to pay higher wages during the crisis. The same channel would also predict stronger results at firms which are more cash constrained prior to the recession. We identify firms as being relatively more cash-constrained by using different proxies as of 2007 (total assets, sales, cash to assets, ROA) and we exploit cross-sectional heterogeneity in our data. We find no evidence that would support this explanation. We are cautious not to over-interpret our results, however, we find no evidence indicating that our results are driven by benefits to high operational leverage.

VIII Conclusion

The debate as to whether firms should pay workers wages above market-clearing rates 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?

We explore the impact of higher then 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.

The higher wages at treated firms are likely to be perceived as gifts by their employees, predicting relatively higher employee effort at these firms, as suggested by Akerlof (1982, 1984). We find evidence supportive of the predictions in Akerlof (1982, 1984) as the treated firms subsequently outperform control firms, as measured by sales, profits or market share.

Our results are unique to the 2008 crisis and we are limited in our ability to extrapolate outside this unique setting. However, our conclusions are important in light of the heated debate spurred by the recent crisis on how firms should be shaping wage and employment policies to better survive a downturn. One argument would be that wage cuts can prevent layoffs, leading to welfare improving outcomes, such as lower unemployment. Our results add nuance to this argument. While wage cuts that minimize job losses may improve total welfare, they are costly to the firm. 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.

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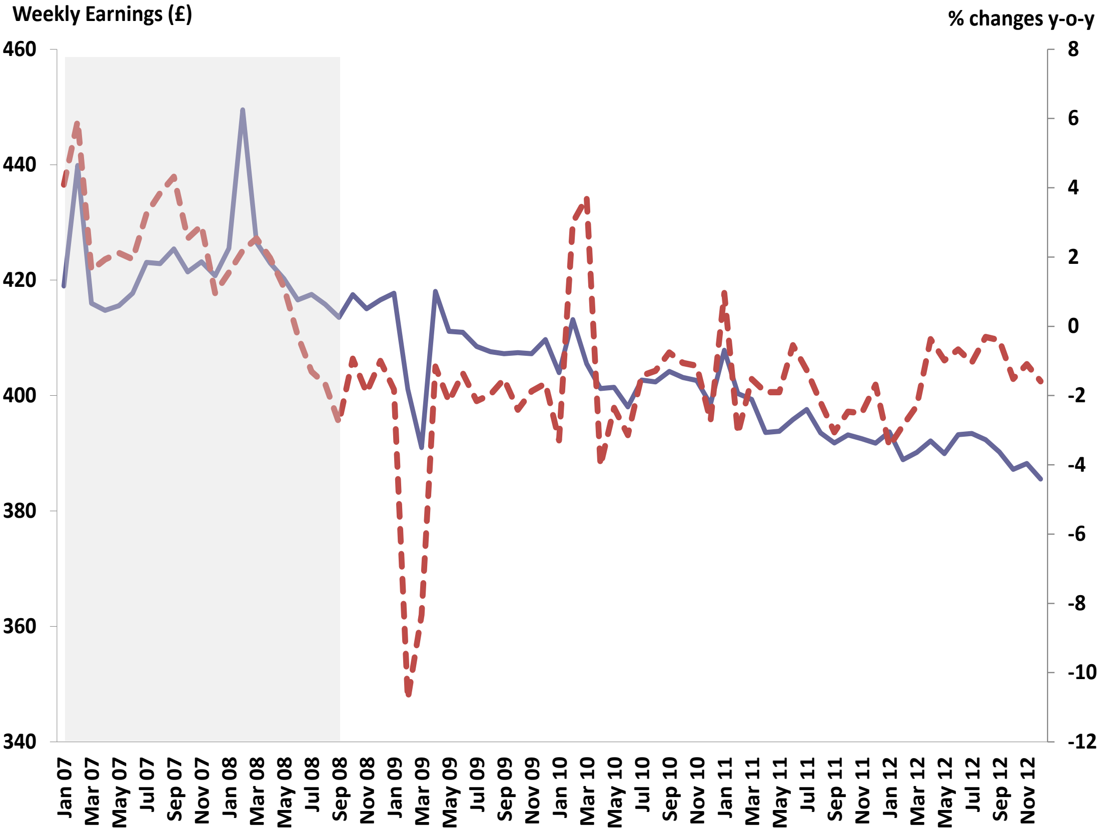

Figure 1. Average Weekly Earnings in the UK Private Sector
This figure shows average real weekly earnings for the private sector in the UK between January 2007 and December 2012. The data are in monthly frequencies and seasonally adjusted. The solid line (left axis) presents averages of real weekly earnings 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.

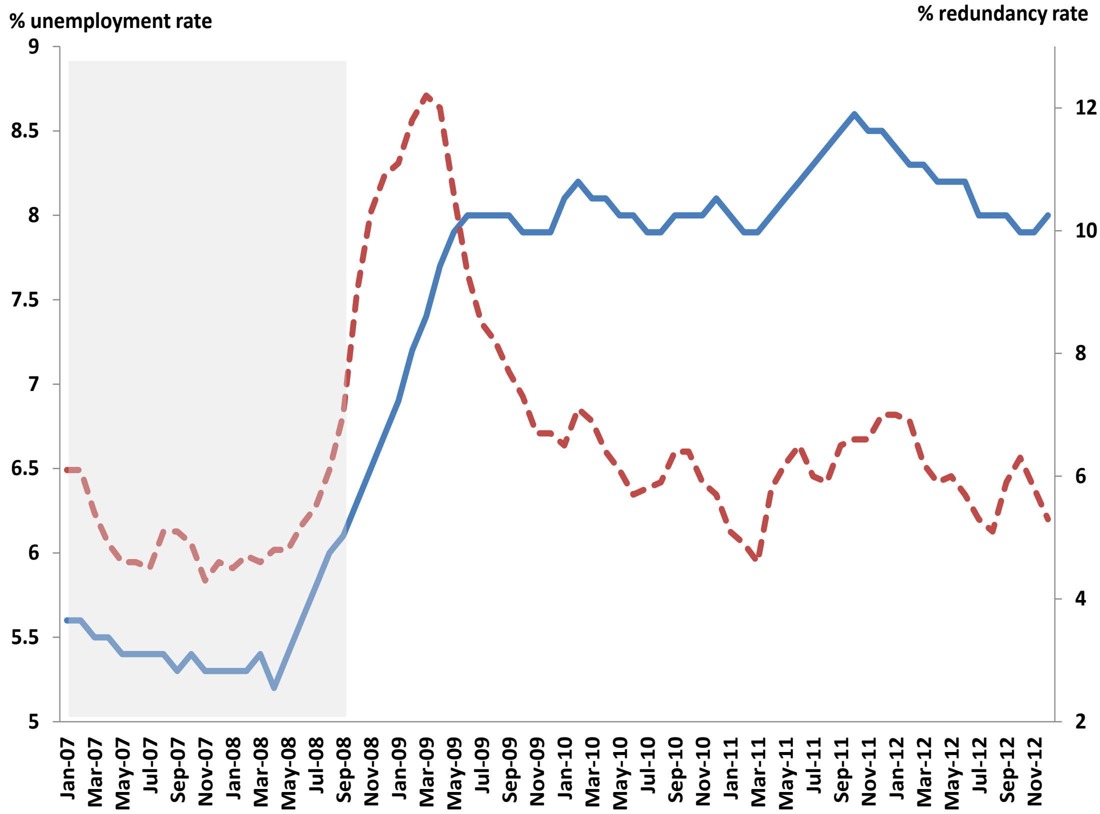

Figure 2. Unemployment Rates and Redundancy Rates in the UK
This figure plots unemployment rates and redundancy rates for the British economy between January 2007 and December 2012. Unemployment rates (solid line, left axis) and redundancy rates (dashed line, right axis) are seasonally adjusted and reported in percentages and 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 have 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 have 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 currently covered by long-term wage agreements (LTapplies=1) compared to a set of firms not currently 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)
$Pre_{-2,(t=2006)}$*Treated				-0.007	-0.003	-0.004
				(0.088)	(0.088)	(0.089)
$Pre_{-1,(t=2007)}$*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. Low Skill takes value of 1 if the long-term contract covers low-skill 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	0.106	0.118	0.123	0.215	0.224	0.235
	(0.107)	(0.108)	(0.109)	(0.118)*	(0.117)*	(0.119)**
$Post_{2,(t=2010)}$*Treated	0.171	0.182	0.188	0.326	0.334	0.337
	(0.103)*	(0.103)*	(0.101)*	(0.140)**	(0.141)**	(0.140)**
$Post_{3,(t=2011)}$*Treated	0.213	0.230	0.230	0.364	0.363	0.362
	(0.102)**	(0.104)**	(0.103)**	(0.142)**	(0.142)**	(0.142)**
$Post_{4,(t=2012)}$*Treated	0.205	0.212	0.213	0.451	0.465	0.460
	(0.131)	(0.131)	(0.131)	(0.165)***	(0.167)***	(0.168)***
$Post_{1,(t=2009)}$*Treated*Superv.	0.295	0.282	0.276			
	(0.204)	(0.204)	(0.205)			
$Post_{2,(t=2010)}$*Treated*Superv.	0.277	0.264	0.261			
	(0.176)	(0.175)	(0.175)			
$Post_{3,(t=2011)}$*Treated*Superv.	0.350	0.329	0.329			
	(0.194)*	(0.196)*	(0.194)*			
$Post_{4,(t=2012)}$*Treated*Superv.	0.412	0.399	0.399			
	(0.194)**	(0.194)**	(0.191)**			
$Post_{1,(t=2009)}$*Treated*Low Skill				-0.128	-0.125	-0.137
				(0.165)	(0.164)	(0.164)
$Post_{2,(t=2010)}$*Treated*Low Skill				-0.206	-0.204	-0.200
				(0.116)*	(0.116)*	(0.117)*
$Post_{3,(t=2011)}$*Treated*Low Skill				-0.192	-0.163	-0.161
				(0.120)	(0.120)	(0.119)
$Post_{4,(t=2012)}$*Treated*Low Skill				-0.341	-0.356	-0.347
				(0.148)**	(0.148)**	(0.148)**
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	0.93	0.93	0.93	0.93
Obs.	1,702	1,702	1,702	1,702	1,702	1,702

Table 6: Cross-Sectional Regressions: High Skill 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. We identify industries reliant on skilled labor using average industry wages, as measured in 2007 at the three-digit SIC code level. Interactions of the High Skill variable with year fixed effects are also estimated but not included to conserve space. 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)
$Post_{1,(t=2009)}$*Treated	-0.301	-0.294	-0.318
	(0.389)	(0.387)	(0.388)
$Post_{2,(t=2010)}$*Treated	-0.329	-0.320	-0.319
	(0.470)	(0.469)	(0.467)
$Post_{3,(t=2011)}$*Treated	-0.829	-0.802	-0.806
	(0.491)*	(0.487)	(0.486)*
$Post_{4,(t=2012)}$*Treated	-1.010	-0.988	-1.000
	(0.545)*	(0.544)*	(0.545)*
$Post_{1,(t=2009)}$*Treated*High Skill	0.0116	0.0117	0.0124
	(0.0113)	(0.0113)	(0.0114)
$Post_{2,(t=2010)}$*Treated*High Skill	0.0138	0.0138	0.0140
	(0.0136)	(0.0136)	(0.0135)
$Post_{3,(t=2011)}$*Treated*High Skill	0.0278	0.0274	0.0276
	(0.0143)**	(0.0142)**	(0.0142)**
$Post_{4,(t=2012)}$*Treated*High Skill	0.0322	0.0318	0.0322
	(0.0156)**	(0.0156)**	(0.0156)**
Industry Controls		Yes	Yes
Firm-level Controls			Yes
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes
R^2	0.94	0.94	0.94
Obs.	1,826	1,826	1,826

Table 7: Why do unconstrained firms perform worse?

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. Mgt. quality takes value of 1 if the firm is in a (two-digit SIC) industry where management practices are in the top quartile of the sample. Interactions of the Industry variable with year fixed effects are also estimated but not included to conserve space. We measure management practices using UK data in Bloom and Van Reenen (2007). Public is a dummy variable which takes a value of 1 for publicly listed firms, and 0 for private 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 and additionally interacted year fixed effects with firm size measured pre-treatment (Columns 3, 6). 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. Columns 3 and 6 control for size trends, multiplying year fixed effects with size measured in 2007. *** 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	0.148	0.142	0.160	0.268	0.273	0.270
	(0.165)	(0.164)	(0.159)	(0.155)*	(0.156)*	(0.154)*
$Post_{2,(t=2010)}$*Treated	0.220	0.214	0.215	0.096	0.104	0.101
	(0.152)	(0.150)	(0.152)	(0.134)	(0.131)	(0.129)
$Post_{3,(t=2011)}$*Treated	0.150	0.144	0.146	0.126	0.128	0.127
	(0.147)	(0.146)	(0.147)	(0.124)	(0.126)	(0.124)
$Post_{4,(t=2012)}$*Treated	0.213	0.210	0.211	-0.017	-0.015	-0.015
	(0.163)	(0.163)	(0.165)	(0.158)	(0.159)	(0.156)
$Post_{1,(t=2009)}$*Treated*Mgt. quality	-0.209	-0.201	-0.253			
	(0.206)	(0.205)	(0.198)			
$Post_{2,(t=2010)}$*Treated*Mgt. quality	-0.272	-0.268	-0.270			
	(0.200)	(0.199)	(0.203)			
$Post_{3,(t=2011)}$*Treated*Mgt. quality	-0.370	-0.366	-0.369			
	(0.200)*	(0.198)*	(0.198)*			
$Post_{4,(t=2012)}$*Treated*Mgt. quality	-0.434	-0.430	-0.434			
	(0.216)**	(0.213)**	(0.214)**			
$Post_{1,(t=2009)}$*Treated*Public				-0.249	-0.249	-0.246
				(0.204)	(0.204)	(0.207)
$Post_{2,(t=2010)}$*Treated*Public				0.160	0.160	0.152
				(0.196)	(0.196)	(0.201)
$Post_{3,(t=2011)}$*Treated*Public				0.146	0.148	0.151
				(0.204)	(0.203)	(0.209)
$Post_{4,(t=2012)}$*Treated*Public				0.443	0.445	0.448
				(0.264)*	(0.264)*	(0.271)*
Industry Controls	Yes	Yes	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	0.97	0.97	0.97	0.94	0.94	0.94
Obs.	800	800	800	1,821	1,821	1,821

Table 8: 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, ends in 2009 in Columns 4-6, and ends in 2010 in Columns 7-9. We drop the placebo transition year or 2005 in columns 1-3, 2006 in columns 4-6 and 2007 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 9: 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 10: 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 using 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 11: Matching

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 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 size proxied by assets (Columns 1, 3, 5), and leverage (Columns 2, 4, 6) 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)
$Post_{1,(t=2009)}$*Treated	0.143	0.0721	0.0200	0.0240	0.172	0.103
	(0.0913)	(0.0938)	(0.0117)*	(0.0112)**	(0.0925)*	(0.0926)
$Post_{2,(t=2010)}$*Treated	0.232	0.206	0.0205	0.0162	0.233	0.205
	(0.106)**	(0.121)*	(0.0100)**	(0.0113)	(0.106)**	(0.119)*
$Post_{3,(t=2011)}$*Treated	0.196	0.189	0.0158	0.0122	0.185	0.177
	(0.109)*	(0.119)	(0.0126)	(0.0126)	(0.104)*	(0.115)
$Post_{4,(t=2012)}$*Treated	0.184	0.210	0.0247	0.0219	0.202	0.287
	(0.130)	(0.133)	(0.0108)**	(0.0119)*	(0.130)	(0.138)**
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	0.93	0.57	0.55	0.96	0.95
Obs.	2,889	2,779	2,859	2,776	2,860	2,746

Table 12: 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 13: 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

Appendix

Table A1: Wages and Employment

This table reports the effect of the 2008 crisis on wages and employment of 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. The dependent variable in Columns 1-2 is wages per employee (log transformed). The dependent variable in Columns 3-4 is total employment (log transformed). 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 ROA and sales (Columns 1, 3), and additionally leverage (Columns 2, 4). ROA is measured as net income/assets. Leverage is measured as total debt to assets. Sales is log-transformed. 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)
$Post_{1,(t=2009)}$*Treated	0.0586	0.0576	0.0843	0.0834
	(0.0344)*	(0.0345)*	(0.0758)	(0.0760)
$Post_{2,(t=2010)}$*Treated	0.0262	0.0247	0.0615	0.0599
	(0.0337)	(0.0336)	(0.0529)	(0.0529)
$Post_{3,(t=2011)}$*Treated	0.0169	0.0160	0.0382	0.0374
	(0.0314)	(0.0315)	(0.0571)	(0.0570)
$Post_{4,(t=2012)}$*Treated	-0.0351	-0.0358	0.0700	0.0694
	(0.0477)	(0.0478)	(0.0685)	(0.0685)
Industry Controls	Yes	Yes	Yes	Yes
Firm-level Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
R^2	0.87	0.87	0.98	0.98
Obs.	1,755	1,755	1,792	1,792

Table A2: Two-digit SIC Industry Controls

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. For each computation, the sample excludes unique observations at a given two-digit SIC industry and year. 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 (two-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	0.166	0.173	0.177	0.0153	0.0165	0.0193	0.175	0.192	0.198
	(0.108)	(0.110)	(0.110)	(0.0131)	(0.0130)	(0.0128)	(0.110)	(0.111)*	(0.111)*
$Post_{2,(t=2010)}$*Treated	0.203	0.210	0.218	0.0157	0.0167	0.0212	0.184	0.202	0.213
	(0.109)*	(0.110)*	(0.107)**	(0.0122)	(0.0123)	(0.0116)*	(0.107)*	(0.108)*	(0.106)**
$Post_{3,(t=2011)}$*Treated	0.255	0.262	0.266	0.0107	0.0124	0.0151	0.239	0.251	0.257
	(0.111)**	(0.112)**	(0.112)**	(0.0147)	(0.0148)	(0.0143)	(0.109)**	(0.109)**	(0.109)**
$Post_{4,(t=2012)}$*Treated	0.234	0.236	0.240	0.0140	0.0153	0.0176	0.187	0.191	0.196
	(0.134)*	(0.134)*	(0.133)*	(0.0130)	(0.0130)	(0.0124)	(0.143)	(0.141)	(0.140)
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	0.94	0.94	0.94	0.54	0.54	0.55	0.96	0.96	0.96
Obs.	1,727	1,727	1,727	1,724	1,724	1,724	1,712	1,712	1,712

Table A3: Drop Bankrupt Firms

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 does not include firms that go bankrupt during our sample period (13 firms in total). 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	0.118	0.128	0.132	0.0216	0.0231	0.0257	0.122	0.141	0.146
	(0.093)	(0.093)	(0.094)	(0.0119)*	(0.0119)**	(0.0118)**	(0.0957)	(0.0963)	(0.0967)
$Post_{2,(t=2010)}$*Treated	0.181	0.189	0.196	0.0154	0.0170	0.0212	0.171	0.182	0.192
	(0.100)*	(0.100)*	(0.098)**	(0.0110)	(0.0111)	(0.0107)**	(0.100)*	(0.100)*	(0.098)**
$Post_{3,(t=2011)}$*Treated	0.220	0.232	0.235	0.0105	0.0122	0.0140	0.206	0.216	0.219
	(0.101)**	(0.102)**	(0.102)**	(0.0126)	(0.0127)	(0.0124)	(0.102)**	(0.103)**	(0.102)**
$Post_{4,(t=2012)}$*Treated	0.201	0.209	0.211	0.0142	0.0159	0.0174	0.212	0.218	0.221
	(0.126)	(0.127)*	(0.127)*	(0.0115)	(0.0114)	(0.0110)	(0.146)	(0.144)	(0.143)
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	0.94	0.94	0.94	0.52	0.52	0.53	0.96	0.96	0.96
Obs.	1,803	1,803	1,803	1,806	1,806	1,806	1,783	1,783	1,783

Table A4: Falsification Test: Robustness

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 2012 in all specifications. As in earlier specifications, we drop the placebo transition year or 2005 in columns 1-3, 2006 in columns 4-6 and 2007 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	0.165	0.168	0.169	-0.093	-0.092	-0.090	0.096	0.101	0.100
	(0.184)	(0.182)	(0.183)	(0.107)	(0.107)	(0.107)	(0.101)	(0.101)	(0.101)
$Post_2$*Placebo	0.169	0.171	0.174	-0.091	-0.088	-0.086	0.162	0.162	0.160
	(0.171)	(0.169)	(0.169)	(0.087)	(0.087)	(0.087)	(0.104)	(0.104)	(0.104)
$Post_{3,}$*Placebo	0.007	0.012	0.014	-0.0079	-0.0099	-0.0081	0.090	0.088	0.086
	(0.127)	(0.127)	(0.126)	(0.112)	(0.111)	(0.112)	(0.112)	(0.111)	(0.111)
$Post_4$*Placebo	0.0100	0.009	0.009	0.0015	0.0010	0.0003	-0.153	-0.157	-0.159
	(0.106)	(0.106)	(0.106)	(0.108)	(0.108)	(0.108)	(0.096)	(0.097)	(0.098)
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	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94
Obs.	2,191	2,191	2,191	2,186	2,186	2,186	2,181	2,181	2,181

Table A5: Profits Robustness

This table reports changes in profits 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 are winsorized at 1% level in Columns 1-3, and at 5% level in Columns 4-6. The sample timeline begins in 2005 and ends in 2012 with year 2008 excluded. Our sample in Columns 1-3 is the same as in Table 10, and is limited to firm-year observations where sales is non-missing in Columns 4-6. 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					
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{1,(t=2009)}$*Treated	0.0352	0.0366	0.0403	0.0293	0.0288	0.0314
	(0.0183)*	(0.0182)**	(0.0183)**	(0.0130)**	(0.0129)**	(0.0128)**
$Post_{2,(t=2010)}$*Treated	0.0210	0.0227	0.0289	0.0192	0.0192	0.0239
	(0.0177)	(0.0179)	(0.0173)*	(0.0118)*	(0.0117)*	(0.0111)**
$Post_{3,(t=2011)}$*Treated	0.0208	0.0227	0.0253	0.0103	0.0110	0.0130
	(0.0205)	(0.0207)	(0.0204)	(0.0132)	(0.0132)	(0.0129)
$Post_{4,(t=2012)}$*Treated	0.0125	0.0143	0.0166	0.0182	0.0183	0.0202
	(0.0181)	(0.0181)	(0.0175)	(0.0118)	(0.0117)	(0.0113)*
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	0.44	0.44	0.46	0.54	0.54	0.56
Obs.	1,824	1,824	1,824	1,676	1,676	1,676

Table A6: Firms Resistant to Downturns

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 two-year 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	0.257	0.210	0.0157	0.0406	0.245	0.197
	(0.127)**	(0.179)	(0.0152)	(0.0179)**	(0.142)*	(0.184)
$Post_{2,(t=2010)}$*Treated	0.283	0.319	0.0073	0.0197	0.274	0.316
	(0.203)	(0.205)	(0.0152)	(0.0210)	(0.202)	(0.202)
$Post_{3,(t=2011)}$*Treated	0.463	0.516	-0.0019	0.0236	0.409	0.453
	(0.209)	(0.303)*	(0.0179)	(0.0234)	(0.199)**	(0.274)*
$Post_{4,(t=2012)}$*Treated	0.270	0.522	-0.0055	0.0204	0.196	0.555
	(0.231)	(0.269)*	(0.0160)	(0.0207)	(0.227)	(0.266)**
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	0.93	0.93	0.61	0.63	0.94	0.95
Obs.	831	576	845	585	820	568