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Changes in Bargaining Status and Intra-Plant Wage Dispersion in Germany. Much Ado about Nothing?

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Abstract

A number of studies have reported that union decline is associated with rising overall wage inequality, not least in Germany where the phenomenon has been linked to economic resurgence. The present paper takes an unconventional approach to this potential source of rising inequality by examining *intra-plant* wage dispersion in the wake of establishments exiting from or entering into collective bargaining. Our estimation strategy hinges upon the identification of comparable groups of establishments and on both short-run and medium- to long-term changes in the wage structure. Using two main empirical strategies, we report at most a modest widening effect on dispersion for plants abandoning collective agreements. The converse does not apply for those establishments joining collective agreements. These two effects cast doubt on some of the more exaggerated claims as to the importance of deunionization to wage inequality.

JEL Classification: J31, J51, J53

Keywords: Germany, sectoral collective bargaining, deunionization, intra-plant wage inequality, changes in bargaining status

I. Introduction

Unions are associated with lower earnings dispersion or reduced wage inequality for two main reasons. First, they lower skill differentials in union establishments – even if, as a practical matter, rather little work has focused on union effects at plant level (major exceptions are Freeman, 1982, and Lewis, 1986). Second, unions standardize pay among workers with similar measured attributes. On the other hand, the union wage gap or ‘between group’ effect of unions serves to raise dispersion. Unions therefore have a theoretically ambiguous effect on wage dispersion, although on balance it is now, if not heretofore, widely held that their within sector effects dominate the between group effect, and that on net unions narrow dispersion. The corollary is that union decline in unionization has been allied to rising wage inequality, with much attention being accorded the decades of the 1980s and 1990s – intervals characterized by material declines in union density and often sharply rising inequality. The wider backdrop is of course that within-firm wage inequality accounts for approximately 60 percent of wage dispersion across all individuals in the economy (Lazear and Shaw, 2007; Fournier and Koske, 2013).

Notwithstanding ambiguities attaching to measurement of union wage effects, changing union goals, and contrary evidence (see respectively, Rios-Avila and Hirsch, 2014; Pontusson, 2013; Addison, Bailey, and Siebert, 2007), the time seems ripe to investigate German wage inequality for a number of reasons. First, the decline in German unions has continued apace in recent years, after the decline in many other nations has leveled off. Second, in addition to the growth of individual bargaining, there has also occurred increasing flexibility in collective bargaining with the decentralization of sectoral agreements, as indexed by the contractual innovations of *opening clauses* and so-called *pacts for employment and competitiveness* (see Addison et al., 2017). Third, the combination of both developments has been allied not only to falling real wages and rising wage inequality – widely observed in the German literature (see section II below) – but also, and more controversially, as providing the key to that nation’s resurgent economy both during and subsequent to the Great Recession. In particular, Dustmann et al. (2014) have argued that the decline in union power (and the strengthened role of works councils in wage determination) rather than the labor market reforms (under the Hartz Plan) was

the prime mechanism restoring German competitiveness. Since these authors also argue that their results are consistent with the increased importance of firm level differences in wages (section II), it seems of no small interest to examine the course of intra-plant wage differentials attendant upon a change in bargaining status, especially at a time of profound changes in collective bargaining coverage. Even if this line of inquiry is only a part of the picture, it is one of the more obvious channels of union influence.

The present paper examines *intra-plant* wage dispersion in the wake of establishments exiting from or entering into collective bargaining. Our estimation strategy hinges upon the identification of comparable groups of establishments and on both short-run and medium- to long-term changes in the wage structure. To anticipate our results, although the data confirm that there has been a substantive increase in both overall (worker) and within-establishment wage dispersion since the mid-1990s, we find insufficient evidence to support the claim that this outcome is produced by *shifts* in collective bargaining. To repeat, this still leaves open the role of contractual innovations on which more evidence is urgently required. In the interstices, however, the role of one popular explanation – deunionization – might have been exaggerated.

II. Research on Earnings Dispersion¹

The German literature has indicated that the correlation between rising wage inequality and declining unionism, is not a phenomenon confined to the Anglo-Saxon (and other) countries. What distinguishes Germany is that the increase came later, or better put came later at the lower tail of the wage distribution. Reflecting the disparate wage trends in the two halves of Germany (on which, see Kohn and Lembcke, 2007), the studies examined here focus on western Germany. The best-known study is that of Dustmann, Ludsteck, and Schönberg (2009), using the IABS (a 2 percent random sample of social security records) and the LIAB (a linked employer-employee dataset). The authors report that wage inequality increased over these three decades – during the 1980s at the top of the distribution and during the 1990s at the bottom end as well. The authors explore several explanations for the increase in wage inequality.

Focusing here on the 1990s, the authors argue that episodic events – rather than changes in workforce composition and the polarization of work (i.e. demand shifts

favoring the high-skilled relative to the low-skilled) – explain the widening of the wage distribution at the bottom. Episodic events include changes in labor market institutions and labor supply shocks. In particular, the decline in union coverage 1995-2004 (12 percentage points in the case of sectoral agreements) is found to contribute significantly to the widening in dispersion over these years. Using the kernel reweighting decomposition procedure of Dinardo, Fortin, and Lemieux (1996) to recover the counterfactual wage distribution that would have obtained had workforce composition/unionization rates remained unchanged, it is estimated that workers throughout the distribution would have enjoyed higher wage growth over the period. And this higher wage growth would have been most pronounced at the lower end of the wage distribution. Specifically, between 1995 and 2004 the overall 80-50 wage gap rose by 0.068 log points, whereas the increase in this upper tail inequality would have been 0.059 points had unionization coverage remained at its 1995 level. This reduction of 13 percent is dwarfed at the lower end of the distribution, however, where deunionization can account for 28 percent of the increase in the overall 50-15 wage gap. Episodic factors largely fail to explain trends in the differential between high skilled and medium skilled workers; rather, the source of widening at the top of the distribution is attributed to technological change.

A somewhat more differentiated picture is presented by Antonczyk, Fitzenberger, and Sommerfeld (2010), using the 2001 and 2006 repeated cross sections of the German Structure of Earnings Survey (GSES). This study investigates the contribution of firm effects, bargaining effects (i.e. deunionization) and personal characteristics to rising inequality over a period in which unionization is estimated to have declined by between 16.5 percentage points in the case of males and rather more than that for females.² The study uses a set of linear quantile regression estimates to analyze the effects of workplace related effects and personal characteristics, and estimates a sequence of counterfactual wage distributions. It is found that, although the sharp decline in collective bargaining contributes to the material rise in earnings inequality – driven by real wage increases at the top of the wage distribution and real wage losses below the median – it is altogether less important than firm effects and much smaller than estimated by Dustmann, Ludsteck, and Schönberg (2009).³

Greater firm heterogeneity is also identified in another recent study by Card, Heining, and Kline (2013), using the IAB Integrated Employment Biographies (IEB) dataset for the period 1985-2009. The authors divide their sample period into four overlapping intervals and fit separate linear models to each with additive person and establishment fixed effects, namely the component of individual pay that is portable across jobs and that part which is a pay premium offered by different employers. It is reported that the model provides a good approximation of the wage structure and explains nearly all of the steep rise in wage inequality over the sample period. Specifically, increasing dispersion is attributed in large part to rising heterogeneity between workers and rising dispersion in the wage premiums of different establishments. Heightened assortativeness in the assignment of workers to establishments, captured by the correlation between the worker and establishment fixed effects, also plays a material role, so that individuals expected to earn more at any job are *increasingly* located in establishments offering above-average wages to all employees.

Finally, in a more recent study, Dustmann et al. (2014) argue that the decentralization of the wage setting process – pure decentralization or deunionization as manifested in bargaining between firms and their workers individually as well as the decentralization associated with contractual innovation in sectoral agreements – is consistent with the above finding that the rise in firm-level differences contributes strongly to the rise in wage inequality. But they do not examine the consequences of changes in bargaining structure over their sample period in any detail. Rather, they contrast observed wage growth 1995-2008 with the counterfactual growth that would have occurred had union coverage maintained its beginning-period value, again using a reweighting approach. As in Dustmann, Ludsteck, and Schönberg (2009: 862), this exercise suggests that wages would otherwise have been higher across the board but particularly at the lower end of the distribution. The authors also provide descriptive data on wage growth for three percentiles of the wage distribution in covered and uncovered sectors. On the basis of both pieces of evidence, Dustmann et al. (2014: 179) conclude that the rise in overall inequality between 1995 and 2008 is produced by three factors: “... the shift of workers from the covered sector to the uncovered sector (which led ... to an increase in lower tail inequality), the increase in inequality in the covered sector, and the increase in

inequality at the top of the wage distribution in the uncovered sector.” They proceed to attribute changes in wage inequality within the covered sector to the decentralization of sectoral bargaining. Decentralization is equated with opening clauses, although no independent analysis of these contractual innovations is offered.

It follows that the German evidence is decidedly mixed once one proceeds beyond the simple correlation of rising wage inequality and falling union coverage (and density). That the contribution of unionism is unclear is not really a question of the now accepted small impact of coverage on average wages (e.g. Addison et al., 2014) as most studies also indicate that unions are associated with a narrowing of the wage distribution,⁴ even if attempts to account for worker and firm heterogeneity again point to very different results.⁵

We would conclude that a good case can be made for examining the consequences of changes in union coverage more directly, if only partially, by investigating the effects of an establishment of joining/leaving a collective agreement on the wage distribution of the establishment in question. In this admittedly first-pass procedure, our working hypothesis is that abandoning (joining) a collective agreement will be associated with increased (decreased) intra-plant earnings dispersion.

III. Study Design

Our study seeks to analyze the impact of collective bargaining status on firm wage dispersion. However, since an establishment cannot be both ‘covered’ and ‘uncovered,’ a proper strategy for identifying the causal effect requires the construction of a relevant comparator. We follow two main estimation strategies: (a) a conditional difference-in-differences approach with propensity score matching and selection of separate treatment and control groups; and (b) a panel fixed-effects approach that identifies in a unified framework all the treatment and control groups before and after treatment. As shown below, the two approaches generate an equivalent set of estimates, namely of short-, medium-, and long-run difference-in-differences effects of collective bargaining transitions.

The Conditional Difference-in-Differences Approach

This first approach uses the Roy-Rubin model or the ‘model of potential outcomes’ (Roy, 1951; Rubin, 1974). In practice, with cross-section data and letting X capture all the relevant (observable) characteristics, the method amounts to assuming the *conditional independence assumption* (CIA). Specifically,

$$E(Y_0 | D = 1, X) = E(Y_0 | D=0, X), \quad (1)$$

where Y_0 is the outcome for the *untreated* units and D is the indicator of participation (i.e. $D = 1$ for the *treated* and $D=0$ for the *untreated*). Ideally, the construction should be such that treated and untreated establishments are identical, differing only in their collective bargaining status. Under this assumption, the average treatment effect on the treated, γ , will be given by $\gamma = E(Y_1 - Y_0)$, where Y_1 denotes the outcome for the treated units.

Since selection into the treatment is unlikely to be exogenous, even after controlling for observables X , the control group of, say, uncovered establishments is unlikely to be entirely comparable to the group of covered establishments. One way to deal with this problem is to use longitudinal data and generate first differences (between, say, t_0 and t_1), assuming that unobserved traits are time invariant. In combination with matching, this approach results in a (conditional) difference-in-differences method, yielding the average treatment effect (average treatment on the treated)

$$\gamma = E(Y_{1t_1} - Y_{1t_0}) - E(Y_{0t_1} - Y_{0t_0}). \quad (2)$$

In this case, the relevant difference-in-differences identifying assumption is given by

$$E(Y_{0t_1} - Y_{0t_0} | D = 1, P(X)) = E(Y_{0t_1} - Y_{0t_0} | D = 0, P(X)), \quad (3)$$

where $P(X)$ denotes the propensity score or the probability of an establishment being treated, given X .

There are several matching methods available. One approach would be to use *one-to-one matching*, where each treated unit is compared with just one untreated unit. *Kernel matching* is another possible route, assigning a positive weight to units of the control group whose propensity score lies within the estimated influence range around a participant. In this case, for an establishment j outside the influence range, we have $w_j = 0$, with the distribution of w_j determined by a kernel density function. However, as described below, our preferred approach is to use *radius matching*, which amounts to assuming that each treated unit can be compared with a variety of untreated units located

within a defined distance, with each unit of the control group entering with a certain weight, w_j , depending on a given criterion, to yield a slightly different causal effect; that is,

$$\gamma = E(Y_{1t_1} - Y_{1t_0}) - E(w_j(Y_{0t_1} - Y_{0t_0})). \quad (4)$$

Regarding the outcome measure, our chief indicator is the (degree of) intra-establishment wage dispersion, given by the conditional wage differential, σ_{jt} (after Winter-Ebmer and Zweimüller, 1999). Given that worker wages are top-censored (see below), the computations are based on censored regressions. For any given year t and establishment j , our method amounts to conducting a censored regression of the daily worker (log) wage, $\ln W_{ijt}$, on a set of observables, X_{ijt} , and then using the corresponding standard error of the regression (or σ_{jt}) – one for each firm and year – as our selected measure of inequality in establishment j . Our conjecture is that the ‘adjusted’ wage dispersion, or residual inequality (that is, the wage dispersion of observationally equal individuals) will depend on collective bargaining presence. In particular, in our conditional difference-in-differences approach this magnitude will be captured by the change in residual inequality observed in the group of collective agreement leavers (joiners), net of the corresponding change in the control group of always members (never members). For completeness, we complement this analysis by providing results based on an alternative outcome, namely the difference between the 90th and 10th (and 50th and 10th) percentiles in log wage residuals.

Clearly, our (conditional) difference-in differences estimate can only be identified if there are collective bargaining switchers in the sample. Moreover, given that in year t establishment j is either covered or not covered, we observe in principle two types of switchers (i.e. joiners and leavers) along with two types of stayers (i.e. collective bargaining never members and always members). Accordingly, for the subset of joiners (never members) we have $\Delta U_{jt} = 1$ ($\Delta U_{jt} = 0$), and for the subset of leavers (always members) we have $\Delta U_{jt} = -1$ ($\Delta U_{jt} = 0$), where U_{jt} denotes the dummy for collective bargaining presence. By construction, we assume that the causal effects of leaving and joining a collective bargaining are not necessarily symmetric.

The entire identification strategy hinges therefore on the assumption that the set of establishments at risk of switching collective bargaining status allows for the estimation

of the parameter of interest: the effect of collective bargaining on wage dispersion. Presumably, the obtained estimate will not be valid for those establishments where the joining/leaving event is very unlikely, which observations are of course dropped in our matching exercise.

Throughout this study we focus on both immediate and medium- to long-run effects. To this end, we present results for 1-year and 4-year difference-in-differences. In practice, this exercise is carried out by pooling all establishments that are observed in t and $t+1$, t and $t+2$, t and $t+3$, and t and $t+4$, and then running, in separate regressions, the changes in observed and unobserved (residual) inequality on the corresponding indicator of collective bargaining transition.

Although this framework permits us to investigate short- and longer-term effects, it has the disadvantage of using disjoint sets of establishments to capture the different sets of estimates. Given that an establishment is much more likely to be observed in t and $t+1$ than in t and $t+4$, the latter requiring in practice five consecutive presences in the survey, the 4-year effect is naturally based on a much smaller sample than the 1-year effect. To address this problem, we perform an extended set of robustness tests. In particular, as described in the next subsection, we offer an exercise in which the short- to long-term effects are extracted from a unified panel framework. In the context of the conditional difference-in-differences approach, we also offer two alternative procedures: first, by running a regression model without propensity score matching; and, second, using propensity score matching but within a fixed sample. For completeness, we will also report results from a placebo experiment in which we seek to examine the sensitivity of the results to falsifying collective agreement status; although for the reasons given below this experiment will be restricted to the case of leavers.

The Panel Fixed-Effects Approach

The modeling strategy described above has the benefit of defining treatment and control groups in an intuitive fashion. Its disadvantages are twofold. First of all, the sample used in the estimation is strongly declining across experiments. Second of all, the procedure does not sufficiently control for differences in the pre-treatment (or pre-separation) period(s). Inspired by the literature on the earnings of displaced workers (e.g. Jacobson,

LaLonde, and Sullivan, 1993), we shall also implement a panel model in which the post-separation effects are net of pre-treatment developments.

The proposed exercise requires us to observe establishments over a number of consecutive years, a requirement that necessarily limits the number of establishments that can qualify for our experiment. We circumvent this constraint by pooling all possible events in a single framework. This is to say, all 1-, 2-, and 3-year effects described in the previous subsection will now be computed in a single fixed-effects panel regression, with controls for unobserved heterogeneity and pre-treatment differences across groups.

Formally, in its simplest version, the panel model is given by

$$Y_{jt} = \alpha_j + \gamma_t + X_{jt}B + \sum_{k=-2}^{k=2} D_{jt}^k \delta_k + \epsilon_{jt} , \quad (5)$$

where Y_{jt} denotes the selected outcome indicator, α_j is a time-invariant unobserved establishment effect, γ_t represents the year-specific unobserved factors, X_{jt} are the observed establishment characteristics, and ϵ_{jt} is the error term. The key dummy variable D_{jt}^k is equal to 1 if establishment j changes its sectoral status in $t - k$, 0 otherwise, with k indicating the number of years before (if $k < 0$) or after (if $k \geq 0$) separation.

Establishments in this setting are therefore required to be observed for at least five consecutive years. Since our observation window is given by the interval 1996-2010, the first year of separation (either an abandonment or adoption of a sectoral agreement) is 1997; and so for establishment j that separates in 1998 and is observed from 1996 to 2000, we will have $D_{j,96}^{-2} = 1$; $D_{j,97}^{-1} = 1$, $D_{j,98}^0 = 1$, $D_{j,99}^1 = 1$, $D_{j,00}^2 = 1$; 0 otherwise. Moreover, for any establishment with a maximum of five consecutive observations, we assume that either the status before (after) the first (last) year is unchanged. Abandoning this assumption would force us to assign a missing value to a number of D^k variables, thereby reducing the number of useable observations.

Observe that in pooling all separation events we are assuming that, after adding the year dummies and the set of covariates, any establishment separation in 2001 will have the same effect on the 2003 outcome as, for example, a corresponding separation in 2005 will have on the 2007 outcome (and similarly for the pre-separation outcomes). More importantly, by running a fixed-effects regression on model (5) we are actually obtaining difference-in-differences estimates. That is, in the case of $k \geq 0$ the coefficients

δ_k provide a measure of an establishment's outturn in comparison with what would have occurred had it not separated k years earlier (or k years after for $k < 0$), after controlling for macro shocks and time-invariant establishment-specific unobserved effects. All else constant, the expectation is that, after leaving a sectoral agreement, establishments are more likely to generate greater inequality than otherwise, in which case we will have $\delta_k > 0$ for all $k \geq 0$; and symmetrically for joiners, with $\delta_k < 0$ for all $k \geq 0$.

Finally, since the separation effect δ_k may be greater or smaller according to establishment-specific characteristics, we also implement an extension of model (5) in which we interact the variables F^1 and F^2 with a selected set of time-varying establishment characteristics. Here F^1 and F^2 denote, respectively, the establishment-specific trend term for the pre-separation period *and* the corresponding dummy for the post-separation period, taking the value of 1 if the establishment is a leaver (joiner), 0 otherwise. Note that it is not feasible to interact all possible time-varying characteristics with all D^k variables without substantially increasing the number of parameters to be estimated. The use of F^1 and F^2 has therefore the virtue of simplifying the estimation work, by keeping the number of parameters to be estimated within reasonable bounds. Note, too, that any time-invariant (observed or unobserved) characteristic is cancelled out in this panel fixed-effects framework.

To simplify, we illustrate this model extension by introducing just one establishment characteristic, namely, the trend in the share of low-skilled workers, x_1 , to yield

$$Y_{jt} = \alpha_j + \gamma_t + X_{jt}B + \sum_{k=-2}^{k=2} D_{jt}^k \delta_k + F_{jt}^1 x_{1jt} \varphi_1 + F_{jt}^2 x_{1jt} \varphi_2 + \epsilon_{jt}. \quad (6)$$

In this setting, the coefficients φ_1 and φ_2 offer a test of whether, say, an increasing share of low-skilled workers in firm j increases (or decreases) the estimated pre-treatment and post-treatment effects on wage inequality.

IV. Data

Our analysis is based on the LIAB, a unique linked employer-employee dataset for Germany (see Jacobebbinghaus and Seth, 2010) provided by the IAB (*Institut für Arbeitsmarkt- und Berufsforschung*/Institute for Employment Research of the *Bundesagentur für Arbeit*/Federal Employment Agency). The LIAB combines official social

security insurance data on individuals with establishment data from the IAB Establishment Panel/*Betriebspanel*. Data access was provided remotely via the Research Data Center/FDZ of the German Federal Employment Agency at the IAB.

The individual data are taken from the Integrated Employment Biographies/IEB database. The IEB merges official information on employment subject to social security (since 1975), marginal employment (since 1999), unemployment (since 1975), social benefits (since 1975), registered job seekers (since 2000), and participants in employment or training programs (since 2000). All employees and trainees subject to social security are covered by the data, other than certain types of civil servants (*Beamte*), the self-employed, and family workers. In total, more than 80 percent of all employed persons in Germany are included in the IEB (Dorner et al., 2010).

The IAB Establishment Panel is a large-scale annual establishment survey that covers up to 16,000 establishments every year, beginning in 1993 in West Germany and extended in 1996 to the former East Germany. The participating establishments are surveyed on a large number of employment policy-related subjects. These include employment development, business policy and performance, investment, collective bargaining, personnel structure and recruitment, remuneration, and working time. This survey is unique in Germany, since it is representative of all industries and establishment sizes nationwide and was conceived as a longitudinal survey from the outset. (For further information on the IAB Establishment Panel, see Fischer et al., 2009.)

The LIAB connects the IEB and the Establishment Panel through an establishment identifier available in both datasets. We note parenthetically that the establishment identifier does not distinguish between a *birth* – that is, a newly-founded establishment – and what might be termed a *re-birth* – that is, a pure change in ownership. However, for present purposes, any misclassification of an establishment as a birth is unlikely to introduce bias because the wage policy will tend to remain unchanged.

In the estimation sample, we treat multiple changes in sectoral agreement status as response errors. Valid sectoral agreements are treated as a dichotomous variable, taking the value of 1 if the establishment is covered by a sectoral agreement, 0 if it is not covered by a sectoral agreement. For example, in effecting a 2-year comparison between leavers and stayers (see the column labeled ‘2-year effect’ in Table 4) we test the

sequence 100 against 111 and therefore discard the 101 case. Correspondingly, in the '3-year effect' column we compare 1000 with 1111 and discard the multiple switching cases (namely, 1010, 1011, and 1101). The same combinations are involved in Table 5 where we compare joiners versus leavers.

We should also emphasize that, in the interests of clarity, we ignore those establishments covered by firm-level agreements and focus solely on sectoral agreements. An alternative would have been to use a single unified collective bargaining category – either covered or not covered by a collective agreement of any type – but ultimately we considered that any amalgamation of the two groups would offer less transparency (see section II). This decision is underscored by the modest size of sample of firms practicing bargaining at this level, firm-level agreements constituting just 7 percent of the whole sample. In other words, the sequences scb-fcb-fcb and fcb-scb-scb, where 'fcb' ('scb') denotes the presence of a firm (sectoral) agreement, are strictly outside of the remit of the present paper.

We note parenthetically that the collective agreement question was framed in a slightly different way in the 1996 and 1997 versions of the IAB survey. Up until the 1998 survey, respondents were asked whether a firm-level contract existed in their establishment. From 1998 onward, however, the question was asked more precisely by inquiring of the management respondent whether a firm-level contract *concluded by the establishment with a union* existed in the establishment. Thus, respondents might earlier have responded in the affirmative if they had simply applied agreements from other firms (or industries) with the result that the frequency of firm-level agreements might have been artificially inflated in those earlier years. That said, there is no resulting discernible impact on the incidence of sectoral agreements, and since we focus on the latter we see no gain in sacrificing two years of data.

Furthermore, given the heterogeneity in wage trends between western and eastern Germany, we focus on the former region alone. Also consonant with the German literature reviewed earlier, given that our dataset does not contain precise information on the number of hours worked we restrict our analysis to those individuals who are full-time workers.

Our raw data cover the period 1996-2010. They contain some 18 million worker-level observations and approximately 60,000 establishment-level observations in respect of establishments with at least 20 employees, the latter restriction being imposed by the need to construct a meaningful measure of intra-establishment wage dispersion. Establishments lacking (financial) turnover information are also excluded from the sample (chiefly from banking, financial services, and public administration sectors). Annually, we observe between 2 and 5 thousand establishments and from 0.9 to 1.4 million workers.

The key variable is the average daily gross wage (in logs).⁶ As noted earlier, the raw wage variable is top-censored at the contribution limit set by the German social security system, and in computing the intra-establishment residual inequality, σ_{jt} , we used censored regressions as described in section III above. Since we wish to provide some comparable descriptive evidence at *worker level*, and not just intra-establishment statistics (see Table 1 below), we also sought to impute wages implementing the approach developed by Gartner (2005). Thus, wages above the contribution limit were imputed using separate censored estimations at worker level and for each year. A truncated normal distribution was then constructed by using the predicted values from the censored regressions as moments and by setting the lower truncation point equal to the contribution limit. As a final step, censored wage observations were replaced by values randomly drawn from this truncated normal distribution. (We deflated wages using the Producer Price Index published by the German Federal Statistical Office; specifically, all wages are expressed in year 2005 values.)

V. Some Preliminary Evidence

Table 1 illustrates some basic characteristics of the wage data: first, at worker level for benchmarking purposes as regards overall wage inequality (columns (1) through (4)); and, second, at establishment level to provide some initial evidence on intra-establishment wage inequality (columns (5) through (9)). As can be seen in column (1) of the table, there has been an increase in wages of about 3 log points over the 1996-2010 interval. This figure cannot, however, be generalized to the entire population. (We recall that our sample is comprised of LIAB establishments with at least 20 employees.) As documented by Card, Heining, and Kline (2013: Table I), using information from the IEB

data file for 1995-2009, there has been a decrease of about 5 log points in the daily wage of the whole population/sample of full-time workers aged 20-60 years in western Germany. Trends in wage inequality are nevertheless similar in the LIAB and IEB datasets, as will be shown below.

[Table 1 near here]

Column (2) of the table indicates that there has been a pronounced increase in wage inequality; specifically, a rise of 14 log points in the standard deviation. By German standards, characterized by a highly compact wage structure, this increase is quite remarkable. (The coefficient of variation in Germany of about 0.1 is between one-third and one-quarter that in Anglo-Saxon countries.) Again, for purposes of comparison, we note that Card, Heining, and Kline (2013: Table I) report an increase of 13 log points in the standard deviation, while the corresponding coefficient of variation falls in the 0.1-0.15 interval, with a slight upward trend over time.

The same pattern is reflected in other indicators of wage inequality. Thus, columns (3) and (4) of the table show that the gap between the 50th and 10th percentiles and between the 90th and 10th percentiles increased over the period by 14 and 22 log points, respectively. Another useful benchmark here is provided by Antonczyk, Fitzenberger, and Sommerfeld's (2010: Table 1) finding of an increase of 10 and 13 log points in the 50th-10th and 90th-10th wage gaps, respectively, in the case of males. The corresponding figures for females are 6 and 11 log points. These latter results are obtained using GSES data and pertain to full-time workers aged 25-55 years in establishments with 10 or more employees

Intra-establishment wage inequality is next examined using two types of evidence. The first is an unadjusted indicator, based on observed (and imputed) daily wages (columns (6) through (8)); the second is the residual inequality (column (9)), a refined indicator that controls for (observed) human capital variables as was explained in section III. For completeness, column (5) reports the mean establishment wage, which unsurprisingly is smaller than the worker-level mean shown in column (1), and is indicative of the higher wages in larger establishments. In turn, the overall (i.e. worker) wage inequality in column (2) is higher by a margin of 8 to 16 log points than the (mean)

within-establishment wage inequality measure in column (6). *Vulgo*: workers are more heterogeneous than establishments.

Columns (7) and (8) show the (mean) unadjusted 50th-10th and 90th-10th wage gaps within establishments. The evidence is that the wage inequality within establishments, over the 14-year interval is in the 23-36 log point range in respect of the former gap and between 37 and 77 log points for the latter. In both cases, we observe a steady upward trend of 9 and 13 log points in dispersion over the sample period.

The last column of the table presents the residual inequality or intra-establishment adjusted wage inequality. The results are striking. First, most of the observed inequality within establishments can be explained by observed human capital variables as the mean residual inequality is approximately two-thirds of the intra-establishment wage inequality reported in column (6). Second, and more important, the trend in residual inequality replicates that observed for the standard deviation of worker daily wages. These trends are graphed in panel (a) of Figure 1. The trends in residual inequality and intra-establishment wage gaps are also similar, as can be seen from panel (b) of the figure.

[Figure 1 near here]

The next issue is of course the extent to which the changes in wage inequality can be allocated to shifts in collective bargaining. To repeat, since coverage by collective agreement in our data pertains to the establishment, we will focus exclusively on establishment-level information in what follows.

VI. Regression Results

The sample size of the selected comparison groups in our raw longitudinal dataset is given in Table 2. The groups are defined using observed establishment collective bargaining status. They comprise collective agreement leavers and collective agreement always members (in panel (a)) and collective agreement joiners and collective agreement never members (in panel (b)). Recall that collective agreements are those reached under sectoral bargaining. In each panel, establishments are observed between t and $t + \tau$, $\tau = 1, 2, 3, 4$, yielding the four columns shown in the table. Accordingly, over the entire 1996-2010 interval, we have in panel (a) a total of 22,867 cases in which it is possible to observe an establishment in two consecutive years, with 21,751/22,867 or 95 percent

recording no change in sectoral agreement status and 1,116/22,867 or 5 percent switching from sectoral bargaining into no collective bargaining at all. Similarly, in panel (b) we have 8,195 establishments that are either not covered by any sectoral agreement over two consecutive years (a total of 7,235 cases) or not covered in t but covered in $t+1$ (960 cases).

[Table 2 near here]

The raw evidence on changes in intra-establishment residual inequality is given in Table 3. The first column of the table, for example, suggests that leaving a collective agreement implies increased wage inequality within establishments one year after the regime shift vis-à-vis always members. And a similar indication can be found in the other three columns for the 2-, 3-, and 4-year effects. As far as joiners are concerned, the evidence in panel (b) suggests that joiners, too, tend to have a higher residual inequality than the comparison group of never members. But the magnitudes of the standard deviations in both panels (a) and (b) strongly suggest that the comparator groups need to be further refined. As a first step, we tackle this issue by presenting the matching procedures and the corresponding diagnostic tests. We then estimate the causal effect in a conditional difference-in-differences framework as described in section III. We will also provide several robustness tests (with and without using propensity score matching), and then complement the analysis with a fixed-effects panel estimation as also described in section III.

[Table 3 near here]

We therefore begin by reporting our matching procedure that is based on a probit estimation of the probability of leaving/joining a collective agreement, controlling for a extensive set of covariates. These include the ratio of labor costs to sales, log sales, the share of female/part-time/fixed-term contract/and low-skilled employees, the proportion of employees subject to the German social insurance scheme, and a large set of dummy variables denoting the legal form of the establishment (five categories), firm profitability (five), firm size (four), industry (thirteen), location (seven), and foreign ownership. On total, fifty seven coefficients were estimated in each of the two regressions: leavers versus

always members *and* joiners versus never members. (The probit estimates are given as Tables A.1 and A.2, respectively, in the supplemental online appendix.)

From the sparse literature on collective agreement survivability (e.g. Addison et al., 2013), there is no straightforward anticipation of the influence of each selected covariate as sampling is likely to be a factor. In our case, which covers the 1996-2010 interval and establishments with at least 20 employees, the regressions indicate that year dummies are clearly positive and increasing over time (although non-monotonically) for leavers and negative and increasing in absolute size for joiners, suggesting that in comparison with 1996 (the omitted year), leavers (joiners) are increasing (decreasing) over the sample period in any of the four selected scenarios (i.e. 1-, 2, 3- and 4-year transitions). In the case of industry dummies, it follows from the model estimates that, all else constant, establishments in Construction, for example, are less (more) likely to leave (join) than establishments in Manufacturing (metal, electrical equipment, precision and optical equipment, machinery and motor vehicles), the omitted industry group, while large establishments have a lower (higher) chance to leave (join) relatively to their smaller counterparts. In turn, a higher share of part-time and female workers, and a higher proportion of employees subject to the German social insurance scheme, tend to generate a lower probability of leaving and a higher probability of joining. In contrast, foreign ownership seems to reduce both the chance of leaving *and* joining, given the sectoral agreement status in the base year. A similar result is found in the case of the share of low-skilled workers.

The corresponding propensity scores obtained from the probits were next used to conduct a radius matching exercise, with a caliper of 0.001 around the estimated values. The corresponding diagnostic statistics are provided in Appendix Tables 1 and 2; and in all cases except two (in the last two rows of Appendix Table 2) the mean and median standardized bias is smaller than 5.0, which is not a 'large' bias. (Rosenbaum and Rubin, 1985, suggest that a value of 20 can be taken as 'large'.) These results were obtained using the Stata procedure *psmatch2*.

After applying the matching procedures, we implemented a treatment effects model to obtain our set of difference-in-differences estimates of the effect of collective bargaining on wage inequality. These results are given in column (1) of Table 4 for leavers

vis-à-vis always members, and in column (1) of Table 5 for joiners versus never members. The exercise uses two types of outcomes that are given in the upper and lower panels of each table. Specifically, (observed) establishment wage dispersion is provided in panel (a) and (unobserved) residual inequality in panel (b), including in the latter results for the 50th-10th and 90th-10th wage gaps based on log wage residuals as described in section III. In the remaining columns of Table 4 (and Table 5), we test the sensitivity of the conditional DiD estimates to alternative modeling strategies, providing the results of a difference-in-differences regression without matching. Each table gives results for the short-, medium- and long-run scenarios.

[Table 4 near here]

Beginning with the results for leavers in Table 4, the results in column (1) of panel (a) fail to indicate any unambiguous impact of exiting sectoral bargaining on either the standard deviation of intra-establishment wages or with respect to the 50th-10th and 90th-10th wage gaps. That is, although the coefficient estimates are in all cases except one positive in sign – pointing to increased inequality upon exit – the coefficients are rarely statistically significant. The exception is the 3-year effect: over a period of three years, leaving generates an increase in the intra-establishment standard deviation of observed wages, as well as in the 50th-10th and 90th-10th wage gaps, of 2, 8, and 6 log points, respectively.

The results in column (2) of panel (a) largely confirm the findings in column (1). Indeed, both the size and statistical significance of the estimated coefficients are mostly unchanged if conditional DiD with propensity score matching is replaced by a DiD regression with controls for all the observables used in the matching exercise. The exception is provided by the more restricted 4-year case, where the results in column (2) are now statistically significant and larger than in the corresponding column (1) cases (see the 50th-10th and 90th-10th gaps). By fixing the sample in column (3), we are also able to closely replicate the results obtained in column (1). Parenthetically, in the interests of completeness, we implemented a placebo or falsification exercise in which we computed the 1-year effect for leavers in $t+1$ versus leavers in $t+2$. The assumption here is that in year $t+1$ the two groups might be sufficiently close in terms of bargaining status so that no statistical difference in outcomes would be expected. We find in fact that irrespective

of the selected measure of intra-establishment wage inequality, the corresponding coefficient is never statistically significant at conventional levels.⁷ Taken in the round, therefore, and pending any improved test for the 4-year effect, it seems fair to conclude from panel (a) of Table 4 that the 1-, 2- and 3-year effects – if not the 4-year effect – are sufficiently robust across experiments.

In panel (b) we return to the main exercise, this time using the adjusted wage inequality as the outcome measure. In terms of statistical significance, the results for the 1-year effect in column (1) are slightly improved in comparison with their counterparts in panel (a), while the 3-year effect seems to be weakened. In any event, the magnitude of the statistically significant effects is small (i.e. not higher than 3 log points), while the majority of the coefficients in all four columns (1) remain mostly statistically insignificant (in 7 out of 12 cases). The results in columns (2) and (3) tend to generate a higher number of statistically significant parameters, especially in column (2), but again the bottom line is that the coefficient estimates are sufficiently stable across experiments, with a no exemplary presence of post-separation effects, other than in the case of residual inequality (σ_{jt}), where the evidence points to a robust 1 to 3 log points increase in adjusted inequality in the wake of exiting a sectoral agreement.

[Table 5 near here]

Table 5 presents a similar exercise for collective agreement joiners versus never members. The background here is that the number of observations is roughly one-half that in Table 4. There are two main reasons for this disparity: first, given that collective bargaining is in decline, the number of joiners is smaller than the number of leavers; second, given that sectoral bargaining remains the dominant regime, we have necessarily fewer never members than always members in the sample. For both observed and unobserved inequality, we duly observe a smaller number of statistically significant coefficients. Only for the 1-year effect case – in which the number of observations is relatively large – are the estimated effects strongly statistically significant in both panels of the table. And they are surprisingly positive, albeit small in magnitude. That said, the 2-year effect in panel (a) is negative and statistically significant in one case (the 50th-10th gap). At issue is the persistence of this expected negative sign. In fact, the indications are

to the contrary. That is, we fail to obtain in panel (a) any statistically significant effect in the 3-year effect case, while in the 4-year case the results show strong instability across columns (1) and (2). For their part, the 2-, 3-, and 4-year effects in panel (b) are statistically insignificant throughout other than in 2 instances (out of 24).

In Tables 4 and 5 the relevant groups are defined in order to obtain short and long-run effects in separate estimations. Conditional on the observed characteristics, the treatment effect model gives, in year $t + \tau$ ($\tau = 1, 2, 3, 4$) the impact on wage inequality of, say, an establishment separation from a sectoral agreement in year t , in comparison with a sectoral agreement stayer. Its main disadvantage, as discussed in section III, is that it ignores possible differences in pre-separation trajectories across treated and control groups, an aspect that could not be accounted for in the context of Tables 4 and 5 without substantially reducing the number of useable observations, especially in the computation of the 2-, 3- and 4-year effects. For example, to accommodate a 2-year pre-separation interval in the 2-year effect (or the 100 vs 111 case) in Table 6, the series would have to be enlarged to become 11100 and 11111, respectively.

Controlling for the pre-separation effects in the estimation of post-separation effects in practice requires the pooling of all switchers, followed by the use of panel techniques capable of capturing the relevant effects in an unified framework, namely using a fixed-effects approach. The results of this modified approach, for the two types of switchers (i.e. leavers and joiners) are presented in Tables 6 and 7, respectively. In each table we have two model specifications, given by models (5) and (6). In both cases, we evaluate the impact of separation on each of the six selected outcomes.

According to model (5), recall that δ_{-1} and δ_{-2} give the impact on wage inequality one and two years prior to separation, respectively. For example, δ_{-2} results from the comparison, in $t-2$, of a stayer with a leaver in t . In other words, δ_{-1} and δ_{-2} show whether there is evidence of the presence of any 1- and 2-year dip/uptick in firm wage inequality one and two years before separation, respectively. For their part, δ_0 , δ_1 and δ_2 give the post-separation effects (i.e. the 1-, 2-, and 3-year separation effects). As shown in the section III, the fixed effects implementation of model (5) and the treatments effect model in Tables 4 (in Table 5 for joiners) yield comparable difference-in-difference estimates,

although with an obvious advantage in favor of the former as model (5) controls for the difference in the pre-treatment trajectories.

For leavers and always members in Table 6, our panel exercise comprises a maximum of 2,032 units and 13,677 observations. By construction, establishments are all observed for at least five consecutive years. For joiners and never members in Table 7, we have a maximum of 626 units and 3,706 observations. Beginning with the results in Table 6, the estimates from model (5), given in the upper panel of the table, show that there is no strong evidence of any dip/uptick in wage inequality prior to actual separation in the sense that only in 3 out of 12 cases are the coefficients δ_{-2} and δ_{-1} statistically different from zero, and only marginally so at the .10 level. After separation, there is also scant evidence that leaving a sectoral agreement generates any increase in wage dispersion either. As the third, fourth and fifth rows of the table show, the post-separation effects – δ_0 , δ_1 , and δ_2 – are never statistically significant except in one case (given by δ_1 in column (2)). Interestingly, the restricted set of time-varying establishment characteristics, comprising the share of part-time/female and low-skilled employees, reported in the sixth, seventh, and eighth rows, indicate that intra-firm wage inequality seems to increase if leavers have a higher share of part-time and low-skilled workers in the workforce, while there is no evidence that feminization has a strong role in wage inequality developments.

[Table 6 near here]

The bottom half of the table shows the results from fitting model (6) to the data. In this case, it will be recalled that we first interact the selected set of time-varying establishment-specific characteristics with the establishment-specific trend for the pre-separation period (F^1) to obtain the effect of the time-varying covariates on the pre-separation wage inequality, and, second, with the dummy for the post-separation status (F^2) to evaluate their role on the post-separation wage inequality. Since the results of model (5), in the upper half of the table, provide no evidence of any strong difference in pre-separation inequality across the two groups, it is not surprisingly that, with one exception in column (5), φ_{11} , φ_{12} , and φ_{13} are not statistically different from zero. On the other hand, although model (5) yields practically no effect of separation on post-

separation wage inequality, there is some strong indication from (6) that the higher the share of female workers in the firm, the higher is the wage inequality upon leaving a sectoral agreement (see the φ_{22} coefficient). A similar result obtains for the share of low-skilled workers (φ_{23}). In other words, feminization and low skill sets of the workforce do seem to result in increasing inequality for leavers, even if a higher share of part-time workers seems to produce the opposite result (φ_{21}). In turn, the results for the pre-separation effects (δ_{-2} and δ_{-1}) remain largely unchanged, in the sense that only one coefficient (out of 12) achieves statistical significance, while the results for the post-separation effects (δ_0 , δ_1 , and δ_2) show meager support for the notion that leaving a sectoral agreement generates a higher degree of intra-firm wage inequality. Indeed, if anything the evidence suggests otherwise as six coefficients out of eighteen are both negative and statistically significant.

[Table 7 near here]

Finally, Table 7 shows the corresponding estimates for the set of joiners and never members. Once again, the evidence is extracted from a smaller set of establishments than in the case of leavers and never members; the sample size being reduced by approximately two-thirds. Nevertheless, the one constant is the absence of any strong evidence to the effect that a shift in collective agreement status – this time joining a sectoral agreement – yields a material impact on wage inequality. Indeed, either in model (5) or model (6) in just four out of thirty instances are the δ_k coefficients statistically different from zero. In particular, δ_{-2} and δ_{-1} are never statistically significant in model (11), while δ_{-1} is statistically significant in one case (see column (5) of model (5)). The bottom line is that no obvious pre-separation effects are detected in these data. For their part, while negative in most cases as expected, δ_0 , δ_1 , and δ_2 are not statistically significant other than in three instances in model (5) and four in model (6), out of a total of eighteen cases in each corresponding model.

Turning to the role of the interaction terms φ_{11} , φ_{12} , and φ_{13} , observe that for model (6), just two coefficients (out of eighteen) are statistically significant. This is unsurprising given that no clear-cut presence of pre-separation effects was found in model (5) in the upper half of the table. With respect to φ_{21} , φ_{22} , and φ_{23} , there is some evidence that, all

else constant, a higher share of female workers does tend to produce somewhat higher inequality in the post-separation period, while part-time worker incidence operates in the opposite direction. In both cases, the magnitude of the interaction terms suggest that firm wage inequality in the post-separation period is quite possibly not immune to the change in the share of these workers. However, no comparable evidence is found for the share of low-skilled workers.

VII. Conclusions

Despite the growing number of studies pointing to collective bargaining decline as one of the main 'culprits' behind rising wage inequality, the exact contribution of that decline is still a matter of debate. This study has approached the deunionization question by examining intra-plant wage dispersion in the wake of establishments either exiting from or entering into collective agreements. Our expectation was that for the set of establishments at risk of leaving/joining, we should observe a clear tendency towards reduced/heightened wage compression one, two, three, and four years after the change in collective bargaining status. Expressed differently, we would not expect to observe diminishing within-establishment wage dispersion among workers in plants exiting collective agreements, or the converse in plants joining them

To address the source of rising wage inequality, we focused on actual establishment collective bargaining transitions over the course of a 15-year interval in which the decline in collective agreements amounted to almost 25 (20) percentage points in the proportion of establishments (employees) covered. Two distinct estimation strategies were pursued: a panel model and a difference-in-differences regression model. The panel model, with controls for pre-separation effects, showed no obvious after-separation effects in either the short-, medium-, or long runs. The short-, medium-, and longer-term effects estimated for separate matched sub-samples also failed to confirm unambiguous impacts on wage inequality.

In short, despite the increase in both overall (worker) and within-establishment (both unadjusted and adjusted) wage dispersion since the mid-1990s, the role of one popular 'bad actor' seems opaque if not contraindicated. The contribution of other institutional developments – including the alleged internal erosion of collective bargaining – now merits closer attention.

Endnotes

1. A survey of the Anglo-Saxon and other country research is contained in the supplemental online appendix (Review: Unionization and Earnings Dispersion in Anglo-Saxon and Other Nations).
2. The gender wage gap is a central focus of this study but is not further discussed here as reduced collective bargaining plays almost no role in the gender wage gap.
3. Firm coefficient effects (largely driven by sector affiliation) dominate both in overall terms and especially at the bottom of the wage distribution. For their part, personal characteristics, if not their coefficients, have changed in a way that serves to reduce wage inequality.
4. See, inter al., Kohn and Lembcke, 2007, Fitzenberger, Kohn, and Lembcke, 2013.
5. For example, in a study also using linked employer-employee data, Gürtzgen (2016) argues that the flattening of the wage structure associated with collective bargaining is a chimera produced by selectivity bias; with workers with low levels of observed skills tending to be positively selected and workers with higher levels of observed skills tending to be negatively selected into covered firms. But see also Antonczyk (2010) who attempts to tackle the endogeneity of sectoral bargaining by using novel instrumental variables. He finds that coverage, while having no effect on the wage gap, lowers the conditional standard deviation of log wages by 26 percent.
6. According to the FDZ DatenReport 04/2007 (Spengler, 2007, p. 26), “to determine the gross daily wage, the wage for the duration of the given period is divided by the number of calendar days within the period and the value obtained rounded to two decimal points.” In practice, given that we are looking only at full-time workers, the denominator is equal to 365 days if the worker has just one full time job in a year. According to Card, Heining, and Kline (2013), a full-time worker in Germany (IEB data) holds some 1.1 jobs in a year. The corresponding figure in LIAB is necessarily smaller given that large establishments are over-represented.
7. Due to small sample size, this exercise cannot meaningfully be replicated in the case of joiners. Observe that the falsification test for leavers comprises 198 leavers and 96 false leavers. Results are available from the authors upon request.

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Table 1: Summary statistics and residual inequality for the LIAB sample of full-time workers in establishments with at least 20 employees, western Germany, 1996-2010

Year	Worker daily wage				Mean establishment daily wage [number of firms] (5)	Intra-establishment wage inequality			Intra-establishment residual inequality Mean (s.d.) (9)
	Mean [number of workers] (1)	s.d. (2)	50 th -10 th gap (3)	90 th -10 th gap (4)		Mean (s.d.) (6)	50 th -10 th gap (7)	90 th -10 th gap (8)	
1996	4.656 [1,331,036]	0.333	0.329	0.793	4.535 [2,396]	0.278 (0.081)	0.267	0.639	0.182 (0.070)
1997	4.668 [1,152,628]	0.333	0.326	0.780	4.541 [2,032]	0.280 (0.083)	0.267	0.638	0.184 (0.068)
1998	4.715 [1,118,614]	0.472	0.347	0.872	4.574 [2,247]	0.353 (0.139)	0.283	0.725	0.207 (0.116)
1999	4.712 [1,032,329]	0.318	0.332	0.712	4.577 [2,373]	0.291 (0.133)	0.299	0.641	0.207 (0.108)
2000	4.684 [1,303,170]	0.401	0.374	0.857	4.535 [4,473]	0.347 (0.155)	0.324	0.729	0.223 (0.121)
2001	4.835 [1,432,439]	0.388	0.376	0.859	4.550 [5,156]	0.332 (0.147)	0.327	0.721	0.216 (0.122)
2002	4.732 [1,352,581]	0.386	0.359	0.852	4.561 [4,972]	0.330 (0.146)	0.316	0.708	0.221 (0.123)
2003	4.721 [1,209,241]	0.402	0.377	0.860	4.549 [4,554]	0.334 (0.140)	0.330	0.729	0.216 (0.110)
2004	4.729 [1,369,304]	0.414	0.391	0.863	4.538 [4,872]	0.344 (0.147)	0.337	0.744	0.224 (0.118)
2005	4.698 [1,370,436]	0.420	0.392	0.882	4.507 [4,766]	0.347 (0.154)	0.336	0.745	0.226 (0.120)
2006	4.663 [1,269,254]	0.435	0.427	0.929	4.462 [4,560]	0.349 (0.156)	0.339	0.746	0.229 (0.123)
2007	4.660 [1,117,687]	0.454	0.449	1.003	4.445 [4,501]	0.351 (0.156)	0.345	0.750	0.230 (0.118)
2008	4.631 [1,088,525]	0.481	0.466	1.053	4.397 [4,330]	0.365 (0.160)	0.352	0.770	0.235 (0.128)
2009	4.691 [1,072,183]	0.465	0.453	1.031	4.450 [4,450]	0.355 (0.156)	0.342	0.749	0.233 (0.120)
2010	4.685 [888,722]	0.471	0.464	1.012	4.446 [4,059]	0.363 (0.167)	0.357	0.766	0.234 (0.126)

Notes: Gross daily wages (in logs) are deflated using the producer price index and are expressed in year 2005 values. Wages above the contribution limit to the social security system were imputed using the procedure suggested by Gartner (2005). Both observed and imputed wages are used to compute the values reported in columns (1) through (8). Column (5) gives the mean establishment daily wage across all establishments in the sample. The value reported in column (6) is obtained by taking the average over all intra-establishment standard deviations in a given year, and the corresponding standard deviation over all establishments is provided in parentheses in the same column. Column (9) gives the intra-establishment residual inequality, or σ_{it} (see section III).

Table 2: Sample size of selected groups, establishments with at least 20 employees, LIAB data, western Germany, 1996-2010

	Sample size			
	t+1	t+2	t+3	t+4
(a) Leavers and always members				
Number of leavers (i.e. establishments leaving a sectoral agreement between t and $t + \tau$, $\tau = 1, 2, 3, 4$)	1,116	450	251	158
Number of always members (i.e. establishments that are always covered by a sectoral agreement between t and $t + \tau$, $\tau = 1, 2, 3, 4$)	21,751	14,500	9,919	6,824
Total	22,867	14,950	10,170	6,982
(b) Joiners and never members				
Number of joiners (i.e. establishments joining a sectoral agreement between t and $t + \tau$, $\tau = 1, 2, 3, 4$)	960	430	237	141
Number of never members (i.e. establishments that are never covered by a sectoral agreement between t and $t + \tau$, $\tau = 1, 2, 3, 4$)	7,235	4,369	2,788	1,808
Total	8,195	4,799	3,025	1,949

Note: The total of 22,867 in the third cell of the first column of the table, for example, is the sum of 1,116 leavers and 21,751 stayers, from the initial state in which an establishment is covered by a sectoral agreement.

Table 3: Changes in residual intra-establishment wage dispersion for selected groups between t and $t + \tau$ ($\tau = 1, 2, 3, 4$), establishments with at least 20 employees, LIAB data, western Germany, 1996-2010, before matching

	t+1	t+2	t+3	t+4
	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
(a) Leavers and always members				
Leavers (i.e. establishments leaving a sectoral agreement between t and $t + 1$ and did not join collective agreement in $t + \tau$, $\tau = 1, 2, 3, 4$)	0.006 (0.132)	0.008 (0.137)	0.022 (0.109)	0.015 (0.114)
Always members (i.e. establishments that are always covered by a sectoral agreement between t and $t + \tau$, $\tau = 1, 2, 3, 4$)	0.002 (0.098)	0.005 (0.106)	0.006 (0.111)	0.008 (0.015)
(b) Joiners and never members				
Joiners (i.e. establishments joining a sectoral agreement between t and $t + 1$ and did not leave sectoral agreement in $t + \tau$, and $t + \tau$, $\tau = 1, 2, 3, 4$)	0.010 (0.117)	0.009 (0.103)	0.011 (0.120)	0.021 (0.118)
Never members (i.e. establishments that are never covered by a sectoral agreement between t and $t + \tau$, $\tau = 1, 2, 3, 4$)	0.003 (0.104)	0.004 (0.118)	0.005 (0.126)	0.009 (0.131)

Notes: The value reported in the first cell, for example, is the mean 1-year difference in $\Delta\sigma_{jt}$ for leavers; the corresponding standard deviation is in parenthesis. σ_{jt} gives the degree of (residual) intra-establishment wage inequality (see section III).

Table 4: Estimates of the effect of collective bargaining using collective agreement leavers versus always members, western Germany

	1-year effect			2-year effect			3-year effect			4-year effect	
	Conditional DiD	Regression DiD	Conditional DiD on a fixed subsample	Conditional DiD	Regression DiD	Conditional DiD on a fixed subsample	Conditional DiD	Regression DiD	Conditional DiD on a fixed subsample	Conditional DiD	Regression DiD
(a) Intra-establishment (log) wage dispersion											
Standard deviation	0.008	0.007	0.016	-0.004	0.002	0.006	0.020*	0.018*	0.020*	0.027	0.022
50 th -10 th gap	0.018	0.016*	0.024	0.024	0.021	0.041	0.083***	0.093***	0.083***	0.021	0.054**
90 th -10 th gap	0.015	0.010	0.025	0.024	0.022	0.031	0.063*	0.076***	0.063*	0.018	0.048*
(b) Intra-establishment residual inequality											
σ_{jt}	0.012**	0.012***	0.029***	0.014*	0.014**	0.017*	0.029***	0.024***	0.029***	0.024*	0.021*
50 th -10 th gap	0.009	0.009**	0.022**	0.009	0.011*	0.013	0.01	0.011	0.010	0.021	0.013
90 th -10 th gap	0.015*	0.014**	0.037**	0.019	0.02**	0.024	0.025	0.027*	0.025	0.031	0.023
N	444 8,810	444 8,813	213 5,843	226 5,769	226 5,970	118 3,818	120 3,831	121 4,016	72 2,507	72 2,374	73 2,717

Notes: Column (1) gives the conditional difference-in-difference estimates using propensity score radius matching. For the 1-year effect in panel (a), for example, the treatment group is made up of all 10 sequences (leavers), while the control group is given by all 11 cases (always members), observed in the 1996-2010 interval. The 1-year effect gives the impact of leaving on the selected outcome in the separation year (i.e. $t+1$, conditional on observables X and given the sectoral agreement status in the base year (t)). The same procedure is followed for 2-, 3-, and 4-year effects, which are obtained using the sequences 100 versus 111, 1000 versus 1111, and 10000 versus 11111, respectively.

In column (2) the treatment effects model is replaced by a DiD regression model that includes as regressors the full set of observables used in the propensity score matching implemented in column (1).

In column (3) the 1-year effect is based on a restricted subsample containing all the establishments used to compute the 2-year effect (i.e. the restricted subset is made up of the sequences 100 and 111). The same procedure is followed in column (3) in the 2- and 3-year effect; the former being obtained using the sequence 1000 and 1111, the latter using 10000 and 11111.

Table 5: Estimates of the effect of collective bargaining using collective agreement joiners versus never members western Germany

	1-year effect			2-year effect			3-year effect			4-year effect	
	Conditional DiD	Regression DiD	Conditional DiD on a fixed subsample	Conditional DiD	Regression DiD	Conditional DiD on a fixed subsample	Conditional DiD	Regression DiD	Conditional DiD on a fixed subsample	Conditional DiD	Regression DiD
(a) Intra-establishment (log) wage dispersion											
Standard deviation	0.023***	0.015*	0.006	-0.0003	-0.0001	-0.028	-0.004	0.006	-0.004	0.002	-0.005
50 th -10 th gap	0.008	0.002	-0.030	-0.043**	-0.033	-0.044	-0.007	-0.006	-0.007	-0.148**	-0.005
90 th -10 th gap	0.007	0.001	-0.017	-0.041	-0.026	-0.053	0.011	-0.007	0.011	-0.175**	-0.005
(b) Intra-establishment residual inequality											
σ_{jt}	0.016***	0.015***	0.019	0.001	0.006	0.007	0.009	-0.002	0.009	0.023	0.004
50 th -10 th gap	0.019***	0.013**	0.011	0.013	0.016	0.013	0.034*	0.009	0.034*	-0.010	0.002
90 th -10 th gap	0.030***	0.025***	0.023	0.014	0.018	0.004	0.036	0.005	0.036	-0.008	-0.004
N	340 3,749	340 3,750	125 2,127	139 2,340	141 2,341	53 1,412	69 1,481	69 1,481	41 798	41 820	42 932

Note: See notes to Table 4.

Table 6: Panel estimates of the effect of collective bargaining using collective agreement leavers versus always members, western Germany

	Intra-establishment (log) wage dispersion			Intra-establishment residual inequality		
	Standard deviation	50 th -10 th gap	90 th -10 th gap	σ_{jt}	50 th -10 th gap	90 th -10 th gap
Model (5)	(1)	(2)	(3)	(4)	(5)	(6)
δ_{-2}	-0.002	-0.032*	-0.033*	-0.013*	-0.011	-0.018
δ_{-1}	0.008	-0.017	0.002	-0.007	-0.003	-0.006
δ_0	0.006	0.006	0.002	-0.002	0.004	0.002
δ_{+1}	-0.010	-0.035**	-0.028	-0.009	-0.005	-0.013
δ_{+2}	-0.0003	-0.006	-0.024	0.002	-0.005	-0.002
Share of part-time workers	0.012	0.001	0.038	0.012	0.032***	0.054***
Share of female workers	-0.009	0.008	-0.026	0.001	-0.005	0.003
Share of low-skilled workers	0.018**	0.038***	0.042***	0.013**	0.018***	0.025**
Always members (obs.)	12,264	12,384	12,384	13,677	13,677	13,677
Leavers (obs.)	975	989	989	1,127	1,127	1,127
Establishments (total)	1,944	1,944	1,944	2,032	2,032	2,032
Model (6)						
δ_{-2}	0.003	-0.012	-0.009	-0.013	-0.008	-0.014
δ_{-1}	0.013	0.009	0.032*	-0.008	-0.000	-0.003
δ_0	-0.004	-0.024	-0.034*	-0.006	-0.003	-0.011
δ_{+1}	-0.019*	0.064***	-0.061***	-0.013	-0.011	-0.024*
δ_{+2}	-0.008	-0.032	-0.053**	-0.001	-0.011	-0.013
Share of part-time workers	0.014	0.010	0.054*	0.013	0.037***	0.058***
Share of female workers	-0.010	0.012	-0.025	-0.001	-0.006	-0.001
Share of low-skilled workers	0.018**	0.032**	0.037**	0.014**	0.017***	0.026**
φ_{11} (Share of part-time workers* F^1)	0.0004	-0.021	-0.023	-0.003	-0.015*	-0.010
φ_{12} (Share of female workers* F^1)	0.0009	0.004	0.002	0.006	0.007	0.011
φ_{13} (Share of low-skilled workers* F^1)	0.000	-0.005	-0.004	-0.001	0.002	-0.001
φ_{21} (Share of part-time workers* F^2)	-0.053	-0.054	-0.214**	-0.032	-0.068**	-0.079
φ_{22} (Share of female workers* F^2)	0.064*	0.006	0.112*	0.046*	0.052**	0.106**
φ_{23} (Share of low-skilled workers* F^2)	0.006	0.206***	0.175***	-0.013	0.019	-0.002
Always members (obs.)	12,264	12,384	12,384	13,677	13,677	13,677
Leavers (obs.)	975	989	989	1,127	1,127	1,127
Establishments (total)	1,944	1,944	1,944	2,032	2,032	2,032

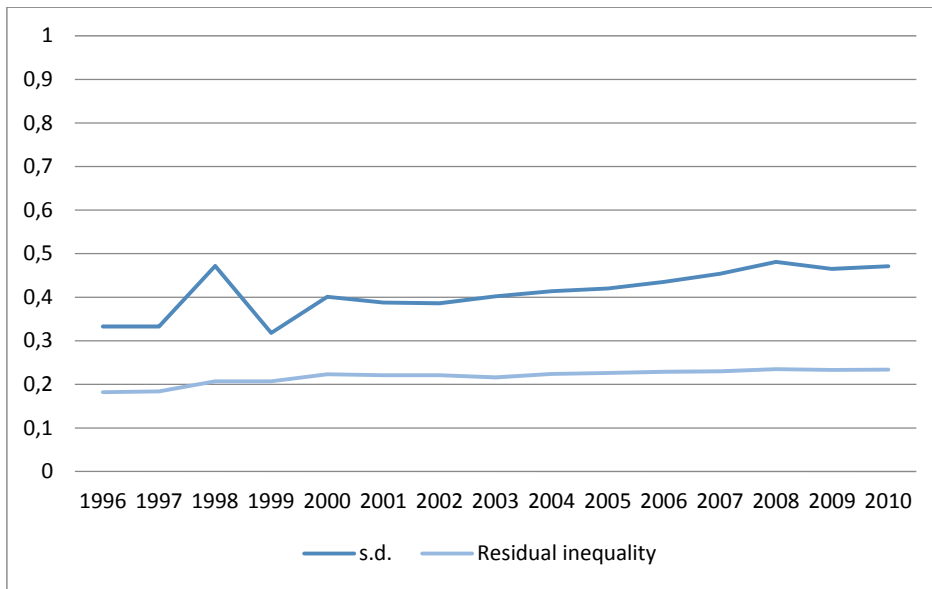
Note: *, **, *** denote statistical significance at the .10, 0.05, and 0.01 levels, respectively.

Table 7: Panel estimates of the effect of collective bargaining using collective agreement joiners versus never members, western Germany

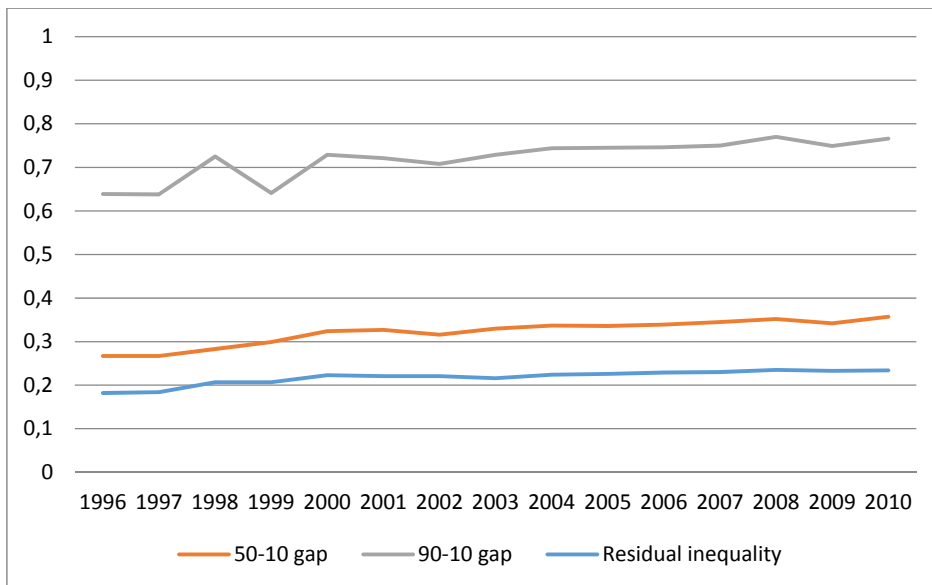
	Intra-establishment (log) wage dispersion			Intra-establishment residual inequality		
	Standard deviation	50 th -10 th gap	90 th -10 th gap	σ_{jt}	50 th -10 th gap	90 th -10 th gap
Model (5)	(1)	(2)	(3)	(4)	(5)	(6)
δ_{-2}	-0.025	0.007	-0.003	-0.017	-0.031	-0.058
δ_{-1}	-0.013	0.017	0.028	-0.017	-0.030*	-0.036
δ_0	-0.005	0.007	0.046	-0.013	-0.029*	-0.035
δ_{+1}	-0.021	0.0005	-0.002	-0.021*	-0.030*	-0.0398
δ_{+2}	-0.0196	-0.002	0.005	-0.011	-0.019	-0.024
Share of part-time workers	0.052*	0.187***	0.173***	0.007	-0.002	0.036
Share of female workers	-0.012	-0.048	0.014	-0.005	0.009	0.0179
Share of low-skilled workers	0.038**	0.120***	0.135***	0.004	-0.003	0.014
Never members (obs.)	3,049	3,100	3,100	3,706	3,706	3,706
Joiners (obs.)	418	422	422	479	479	479
Establishments (total)	582	583	583	626	626	626
Model (6)						
δ_{-2}	0.006	0.009	-0.004	0.007	-0.013	-0.029
δ_{-1}	0.024	0.018	0.025	0.014	-0.007	-0.000
δ_0	-0.020	0.007	0.046	-0.025**	-0.040*	-0.052
δ_{+1}	-0.027	0.006	0.002	-0.027**	-0.037*	-0.050
δ_{+2}	-0.027	0.000	0.006	-0.017	-0.025	-0.033
Share of part-time workers	0.065**	0.199***	0.177***	0.015	0.002	0.046
Share of female workers	-0.022	-0.063	0.003	-0.008	0.006	0.012
Share of low-skilled workers	0.036**	0.128***	0.141***	0.000	-0.007	0.006
φ_{11} (Share of part-time workers* F^1)	-0.031	-0.023	0.018	-0.034	-0.019	-0.035
φ_{12} (Share of female workers* F^1)	-0.018	0.016	0.010	-0.024*	-0.010	-0.022
φ_{13} Share of low-skilled workers* F^1)	0.024	0.002	-0.006	0.029*	0.024	0.041
φ_{21} (Share of part-time workers* F^2)	-0.171**	-0.170	-0.127	-0.107*	-0.050	-0.136
φ_{22} (Share of female workers* F^2)	0.150***	0.145	0.122	0.081**	0.069	0.116
φ_{23} Share of low-skilled workers* F^2)	0.035	-0.068	-0.050	0.043	0.045	0.083
Never members (obs.)	3,049	3,100	3,100	3,706	3,706	3,706
Joiners (obs.)	418	422	422	479	479	479
Establishments (total)	582	583	583	626	626	626

Figure 1: Wage inequality for the LIAB sample of full-time workers in establishments with at least 20 employees, western Germany, 1996-2010

(a) Standard deviation of worker daily wages and intra-establishment residual inequality, σ_{jt}



(b) 50th-10th and 90th-10th intra-establishment wage gaps and intra-establishment residual inequality, σ_{jt}



Appendix Table 1: Diagnostic statistics of the matched sample used in Table 4

	Pseudo R ²	LR	P-value	Mean bias	Median bias
Column (1) in panel (a)					
1-year effect	0.005	57.55	0.099	1.1	0.8
2-year effect	0.008	50.72	0.142	2.2	2.0
3-year effect	0.021	65.59	0.005	2.5	1.6
4-year effect	0.022	36.38	0.451	4.1	3.4
Column (1) in panel (b)					
1-year effect	0.002	33.62	0.894	1.4	1.2
2-year effect	0.006	43.56	0.405	2.1	1.6
3-year effect	0.015	52.91	0.068	3.4	2.3
4-year effect	0.032	60.61	0.009	4.7	3.0

Notes: The pseudo-R² and likelihood ratio (LR) statistics are drawn from the propensity score regression (probit) run after matching the treated and untreated groups. The likelihood ratio tests the joint significance of all included variables in the probit regression. The mean and median bias are summary indicators of the distribution of the absolute value of the bias.

Appendix Table 2: Diagnostic statistics of the matched sample used in Table 5

	Pseudo R ²	LR	P-value	Mean bias	Median bias
Column (1) in panel (a)					
1-year effect	0.010	50.80	0.223	2.2	1.7
2-year effect	0.024	59.81	0.046	3.8	3.3
3-year effect	0.062	54.41	0.064	5.9	3.9
4-year effect	0.214	47.78	0.110	13.2	10.5
Column (1) in panel (b)					
1-year effect	0.003	15.41	1.000	1.3	0.9
2-year effect	0.017	45.87	0.354	3.7	3.0
3-year effect	0.036	39.07	0.557	6.2	5.3
4-year effect	0.181	50.45	0.056	11.1	9.8

Note: See notes to Appendix Table 1.

Supplemental Online Appendix
NOT FOR PUBLICATION

Unionization and Earnings Dispersion in Anglo-Saxon and Other Nations

A number of what are commonly described as “second generation” studies, using reweighting approaches to recover the counterfactual wage distribution, have established that unions in Anglo-Saxon nations reduce wage inequality within establishments, within the union sector, and across the economy. At the price of some over-demarcation, a related literature has examined how much of the economic growth in wage inequality in such nations has been due to the decline in private sector unionism. Thus, in examining the rise in wage inequality in the United States over the 1979-1988 period, Dinardo, Fortin, and Lemieux (1996) suggest that shifts in unionization accounted for 10-15 percent of the rise in male wage dispersion in the 1980s, with most of the effect concentrated in the middle to upper half of the wage distribution, although the contribution of changing unionism in the case of female dispersion was minor (see below). Similarly, Card (2001) reports that shifts in unionization can explain some 15-20 percent of the rise in male wage inequality over 1973-1993, while in a treatment that is notable in accounting for union effects on nonunion wages, Western and Rosenfeld (2011) find that the decline in unionization from 1973 to 2007 explains between one-fifth and one-third of the growth in wage inequality (the upper value reflecting a reduction in the threat effect and norm setting upon union decline).

For the United Kingdom, a study by Bell and Pitt (1998) reports that declining unionization explains between 10 and 25 percent of the overall rise in wage inequality in Britain between 1981 and 1993, according to the data source used. Somewhat larger effects of declining union recognition, 1983-1991, of some 37 percent are reported by Machin (1997). Results more in line with the former study are reported by Gosling and Lemieux (2001), who suggest that shifts in union coverage between 1983 and 1998 explain up to one-third of the rise in male wage inequality even if such changes were found to have only a very small effect on female wage inequality.

The three-nation study by Card, Lemieux, and Riddell (2003, 2004) remains perhaps the most complete institutional analysis of the evolution of unionization and inequality for both men and women in Anglo-Saxon nations (but see also Frandsen, 2012; Lemieux, 1998). The countries covered in this study – Canada, the United Kingdom, and

the United States – have the advantage of comparable data and similar collective bargaining institutions. Focusing here on the authors' results for the evolution of wage inequality over time, it is reported that between 14 and 31 percent of the 0.082 increase in the variance of male wages in the United States, 1973/1974-2001, can be accounted for by the decline in union membership.¹ In the case of the United Kingdom, between 9 and 29 percent of the 0.087 growth in the variance of log wages over the same interval can be explained by union decline. For Canada, on the other hand, as since overall inequality was stable the decline in unionism if anything tended to overstate its compression effect. Note that in all three cases the small decline in female membership had little effect on wage inequality because in the case of female workers union coverage is concentrated toward the top of the skill distribution and because there is no tendency for unions to flatten skill differentials across groups.

Card, Lemieux, and Riddell also examine cross-country differences in wage inequality across their three nations and report that the pattern of cross-country differences is consistent with the pattern of union wage compression effects. Cross-country studies provide the most recent development in this area. Before briefly reviewing one recent cross-country study, however, we pause to consider some findings for continental European nations where research has tended to examine the effects of combinations of different levels of bargaining on wages and wage dispersion. Results are mixed. For Denmark, a study by Dahl, le Maire, and Munch (2013) provides evidence that a wage premium attaches to firm-level bargaining vis-à-vis sectoral bargaining and that the returns to skills are higher under firm-level bargaining. Further, wages are also found to be more dispersed under firm-level bargaining. The authors interpret decentralization as indicative of a reduction in worker bargaining power, and this evidence as therefore consistent with findings for Anglo-Saxon countries reviewed earlier, despite their very different bargaining structures. Support for these empirical results and implications are reported by Plasman, Ruinek, and Rycx (2005) for Belgium (and Denmark), although not for Spain where unions are said to use company agreements to compress the wage distribution. Furthermore, in their analysis of wage data for Belgium, Spain, and Italy, Dell'Aringa and Pagani (2007) report that pay is no more compressed among workers

covered by sectoral agreements alone than for versus single employer and multi-employer bargaining combined.²

The U.S. study by Western and Rosenfeld (2011) reviewed earlier raises the important point that the union effect may spuriously result from economic changes (computerization, deregulation, and globalization). So a real issue attends the parsimony of the estimating equations in the standard labor economics studies (see, for example, Katz and Autor, 1999). Recent cross-country studies are notable in examining the role of unions in the context of the normal economy (i.e. supply and demand forces explaining the relative demand for skills) and alongside other institutional forms.

One of the more interesting treatments is by Jaumotte and Osorio-Buitron (2013), who examine at the experience of some 20 advanced economies from the early 1980s to 2010 (but see also Salverda and Checchi, 2015). Focusing here on the authors' panel regression analysis, Jaumotte and Osorio-Buitron first regress two measures of gross income inequality (namely the top 10 percent income share and the Gini coefficient of gross income) on labor market institutions. The controls in addition to country and time fixed effects comprise technology, globalization, an index of financial reform, the top marginal tax rate, and a banking crisis variable. The labor market institutions are union density, excess collective bargaining coverage (viz. coverage less density), the value of the minimum wage in relation to the median wage, the first-year gross unemployment benefit replacement rate, and a measure of the employment protection afforded regular and temporary employment contracts. A separate regression analysis of the Gini coefficient of net income inequality is also provided in which a key RHS variable is the Gini coefficient of gross income; the other arguments being a subset of those considered earlier. In each case, a benchmark equation is run containing all the controls plus union density (and the minimum wage). Next, the estimating equation is augmented by an excess collective bargaining coverage variable (i.e. coverage less density). Finally, the rest of the labor market arguments are added (and excess collective bargaining coverage dropped). For gross inequality it is found that a weakening of unions is associated with an increase in the top 10 percent income share, while its association with the Gini coefficient of gross income is also negative. Specifically, in the baseline equation, a 10 percent decline in union density is associated with a 5 percent increase in the top 10

percent income share. Interestingly, in the first augmentation, excess bargaining coverage is positively associated with inequality while the union density coefficient estimate is basically unchanged.

Finally, the impact of density is also unaffected by the inclusion of the other labor market institutions, the role of which is not commented upon here. As for the determinants of redistribution, apart from the result that the coefficient estimate of the Gini of gross income is less than unity, which indicates the impact of redistribution, the role of union density is again negative and well determined.³ Even if the channels through which deunionization operates are not altogether transparent in this study, the suggestion is again that unions influence redistribution which role has therefore been eroded by their decline

Notes:

1. To demonstrate the basis of the calculation using results for the authors' simplified model (i.e. without controls for observable skills): the variance of male wages increased from 0.258 to 0.340 over the period while the effect of unions on the variance of wages declined by 0.026 percent from -0.047 to -0.021. Had the union effect remained constant, therefore, the measure of wage inequality would have grown by $0.026/0.082$ (or 31 percent) less than it actually did.

2. Summaries of other European studies on unions and wage dispersion are contained in Salverda and Checchi (2015), while Rios-Avila and Hirsch (2014), using a common methodology, provide separate estimates of the (broadly similar) effects of unions on wage levels and wage dispersion for Bolivia, Chile, and the United States.

3. Given the rather surprising results of this study – after all, the orthodox view is that changes in unionism affect middle- and lower-income workers – Jaumotte and Osorio-Buitron embark on an exhaustive series of robustness checks. These include controlling for sectoral shifts/deindustrialization, skill biased technical change, social preferences in favor of greater inequality, the role of the finance sector, rising levels of formal schooling, and even the endogeneity of the union variable. In each case, the union argument proved robust, actually increasing in absolute magnitude vis-à-vis the benchmark equation.

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Table A.1: Probit estimates of the probability of an establishment leaving a collective agreement in $t + \tau$, given that it is covered by a collective agreement in t .

	t+1	t+2	t+3	t+4
Share of labor costs	-0.539 ** (0.213)	-0.472 (0.300)	-0.840 ** (0.406)	-0.758 (0.526)
(Lagged) log sales	-0.160 *** (0.042)	-0.175 *** (0.062)	-0.304 *** (0.084)	-0.301 *** (0.110)
Share of part-time workers	-0.436 *** (0.161)	-0.700 *** (0.236)	-0.781 ** (0.306)	-0.852 ** (0.407)
Share of fixed-term contract workers	0.443 ** (0.202)	0.372 (0.315)	0.782 ** (0.375)	1.614 *** (0.472)
Share of employment subject to social security	-0.616 *** (0.204)	-1.149 *** (0.294)	-1.315 *** (0.381)	-1.788 *** (0.498)
Share of female workers	0.393 *** (0.121)	0.518 *** (0.168)	0.751 *** (0.217)	0.629 ** (0.285)
Share of low-skilled workers	-0.315 *** (0.089)	-0.370 *** (0.128)	-0.433 *** (0.165)	-0.447 ** (0.212)
Foreign ownership	-0.220 ** (0.091)	-0.315 ** (0.142)	-0.190 (0.178)	-0.850 ** (0.399)
Firm size (reference 20-49 employees)				
50-99 employees	0.039 (0.063)	0.215 ** (0.089)	0.416 *** (0.119)	0.412 *** (0.156)
100-199 employees	-0.118 (0.088)	-0.024 (0.127)	0.231 (0.169)	0.389 * (0.214)
200-499 employees	-0.264 ** (0.117)	-0.060 (0.168)	0.315 (0.225)	0.314 (0.292)
500 or more employees	-0.534 *** (0.174)	-0.833 *** (0.298)	-0.369 (0.392)	-0.364 (0.524)
Industry (reference: manufacture metal, electrical equipment, precision and optical equipment, machinery and motor vehicles)				
Primary sector	-0.946 *** (0.167)	-1.192 *** (0.268)	-1.302 *** (0.366)	0.000 (.)
Manufacture of food products	-0.130 (0.105)	-0.100 (0.137)	-0.107 (0.169)	-0.054 (0.207)
Manufacture of textiles or wood product, furniture and other products	-0.128 (0.091)	-0.008 (0.117)	-0.096 (0.150)	-0.115 (0.188)
Manufacture of chemicals, rubber, plastic, non-metallic mineral products)	-0.099 (0.076)	-0.163 (0.107)	-0.245 * (0.140)	-0.181 (0.171)
Construction	-0.656 *** (0.087)	-0.664 *** (0.116)	-0.689 *** (0.147)	-0.769 *** (0.182)
Trade	-0.126 * (0.074)	-0.238 ** (0.104)	-0.318 ** (0.138)	-0.584 *** (0.188)
Transport und Warehousing	0.011 (0.097)	-0.288 * (0.152)	-0.215 (0.191)	-0.212 (0.235)
Financial and insurance services	-0.111 (0.094)	-0.038 (0.123)	-0.020 (0.157)	-0.068 (0.203)
Hotel business and gastronomy	-0.663 *** (0.130)	-1.291 *** (0.255)	-1.819 *** (0.418)	-1.893 *** (0.489)
Education	0.506 * (0.259)	1.132 *** (0.408)	1.675 (1.131)	0.000 (.)
Human health	0.076 (0.130)	-0.190 (0.195)	-0.551 ** (0.277)	-0.591 (0.362)
Other industries	-0.336 ** (0.165)	-0.381 (0.232)	-0.497 (0.322)	-0.370 (0.370)
Non-industrial organizations, public administration	0.762 ** (0.327)	0.825 * (0.499)	1.364 * (0.747)	0.000 (.)

Year (reference: 1996)				
1997	0.016 (0.155)	0.144 (0.213)	-0.353 (0.276)	-0.173 (0.314)
1998	0.293 ** (0.142)	0.291 (0.200)	-0.006 (0.233)	0.089 (0.278)
1999	0.235 (0.144)	0.017 (0.220)	-0.195 (0.251)	-0.104 (0.304)
2000	0.131 (0.132)	0.147 (0.188)	-0.066 (0.209)	0.058 (0.251)
2001	0.256 ** (0.128)	0.239 (0.183)	0.016 (0.204)	0.052 (0.256)
2002	0.217 * (0.130)	0.354 ** (0.180)	0.077 (0.203)	0.138 (0.250)
2003	0.303 ** (0.128)	0.352 * (0.181)	0.227 (0.196)	0.303 (0.241)
2004	0.373 *** (0.128)	0.476 *** (0.177)	0.231 (0.195)	0.352 (0.240)
2005	0.402 *** (0.127)	0.515 *** (0.177)	0.290 (0.195)	0.289 (0.243)
2006	0.382 *** (0.129)	0.420 ** (0.182)	0.293 (0.198)	0.340 (0.244)
2007	0.320 ** (0.131)	0.406 ** (0.181)	0.190 (0.198)	0.000 (.)
2008	0.350 *** (0.130)	0.486 *** (0.178)	0.000 (.)	0.000 (.)
2009	0.072 (0.137)	0.000 (.)	0.000 (.)	0.000 (.)
2010	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Constant	1.546 ** (0.670)	2.034 ** (0.984)	4.195 *** (1.336)	4.498 *** (1.725)
Legal form (5 dummies)	yes	yes	yes	yes
Firm profitability (4 dummies)	yes	yes	yes	yes
Region (7 dummies)	yes	yes	yes	yes
Likelihood ratio	582.17 ***	387.38 ***	263.74 ***	180.23 ***
Log likelihood	-2315.77	-1149.25	-686.64	-414.67
Pseudo R2	0.112	0.144	0.161	0.179
Number of observations	12,100	7,985	5,473	3,570

Note: *, **, *** denote statistical significance at the .10, 0.05, and 0.01 levels, respectively.

Table A.2: Probit estimates of the probability of an establishment joining a collective agreement in $t + \tau$, given that it is not covered by a collective agreement in t .

	t+1	t+2	t+3	t+4
Share of labor costs	-0.441 *	-0.597	-0.623	0.471
	(0.246)	(0.375)	(0.538)	(0.793)
(Lagged) log sales	-0.141 ***	-0.230 ***	-0.362 ***	-0.359 **
	(0.051)	(0.079)	(0.120)	(0.183)
Share of part-time workers	0.337 *	0.194	0.557	0.727
	(0.186)	(0.282)	(0.400)	(0.663)
Share of fixed-term contract workers	0.268	0.662 **	0.661	2.306 ***
	(0.244)	(0.331)	(0.516)	(0.830)
Share of employment subject to social security	0.467 **	0.637 *	2.017 ***	2.687 ***
	(0.234)	(0.367)	(0.624)	(1.040)
Share of female workers	-0.483 ***	-0.654 ***	-0.946 ***	-1.980 ***
	(0.135)	(0.209)	(0.314)	(0.551)
Share of low-skilled workers	-0.435 ***	-0.527 ***	-0.627 ***	-0.369
	(0.105)	(0.157)	(0.232)	(0.322)
Foreign ownership	-0.277 ***	-0.120	-0.156	-0.089
	(0.107)	(0.152)	(0.226)	(0.306)
Firm size (reference 20-49 employees)				
50-99 employees	0.071	0.059	0.050	-0.022
	(0.073)	(0.110)	(0.163)	(0.240)
100-199 employees	0.122	0.262 *	0.419 *	0.340
	(0.106)	(0.159)	(0.238)	(0.349)
200-499 employees	0.136	0.223	0.465	0.715
	(0.143)	(0.222)	(0.329)	(0.471)
500 or more employees	0.483 **	1.047 ***	1.431 ***	1.342 *
	(0.216)	(0.318)	(0.469)	(0.704)
Industry (reference: manufacture metal, electrical equipment, precision and optical equipment, machinery and motor vehicles)				
Primary sector	0.135	0.659 *	0.651	1.309 *
	(0.277)	(0.358)	(0.581)	(0.760)
Manufacture of food products	0.351 **	0.533 **	1.104 ***	1.837 ***
	(0.141)	(0.213)	(0.279)	(0.381)
Manufacture of textiles or wood product, furniture and other products	0.236 **	0.187	-0.191	0.000
	(0.119)	(0.193)	(0.395)	(.)
Manufacture of chemicals, rubber, plastic, non-metallic mineral products)	0.158 *	0.363 ***	0.439 **	0.731 ***
	(0.090)	(0.128)	(0.183)	(0.245)
Construction	0.711 ***	0.990 ***	0.966 ***	1.383 ***
	(0.135)	(0.183)	(0.267)	(0.335)
Trade	0.439 ***	0.595 ***	0.790 ***	1.048 ***
	(0.086)	(0.131)	(0.197)	(0.288)
Transport und Warehousing	0.327 ***	0.199	0.400	0.832 **
	(0.114)	(0.191)	(0.287)	(0.410)
Financial and insurance services	-0.209 **	-0.120	0.100	0.146
	(0.092)	(0.141)	(0.195)	(0.287)
Hotel business and gastronomy	0.973 ***	1.141 ***	1.854 ***	2.110 ***
	(0.196)	(0.294)	(0.423)	(0.652)
Education	0.139	0.494 *	0.394	0.523
	(0.207)	(0.270)	(0.423)	(0.619)
Human health	0.272 **	0.497 **	0.625 **	1.258 ***
	(0.131)	(0.201)	(0.295)	(0.446)
Other industries	0.000	0.262	0.592 *	0.000
	(0.172)	(0.239)	(0.305)	(.)
Non-industrial organizations, public administration	1.493 ***	2.407 ***	0.000	0.000
	(0.457)	(0.792)	(.)	(.)
Year (reference: 1996)				
1997	-0.292	-0.981 **	-0.533	0.000
	(0.278)	(0.489)	(0.586)	(.)
1998	-0.204	-0.506	-0.073	-0.118
	(0.240)	(0.349)	(0.466)	(0.621)
1999	0.047	-0.095	0.289	0.175
	(0.221)	(0.300)	(0.408)	(0.540)

2000	-0.126 (0.207)	-0.346 (0.285)	-0.166 (0.404)	-0.233 (0.533)
2001	-0.199 (0.206)	-0.260 (0.279)	0.045 (0.385)	-0.172 (0.514)
2002	-0.149 (0.205)	-0.355 (0.275)	-0.298 (0.389)	-0.435 (0.524)
2003	-0.271 (0.206)	-0.424 (0.274)	-0.174 (0.380)	-0.258 (0.505)
2004	-0.326 (0.205)	-0.590** (0.279)	-0.596 (0.398)	-0.619 (0.525)
2005	-0.398* (0.207)	-0.639** (0.282)	-0.728* (0.408)	-0.999* (0.546)
2006	-0.474** (0.207)	-0.474* (0.274)	-0.391 (0.384)	-0.422 (0.504)
2007	-0.316 (0.203)	-0.554** (0.276)	-0.382 (0.382)	0.000 (.)
2008	-0.384* (0.204)	-0.437 (0.270)	0.000 (.)	0.000 (.)
2009	-0.873*** (0.213)	0.000 (.)	0.000 (.)	0.000 (.)
2010	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Constant	1.064 (0.828)	2.180* (1.289)	2.602 (1.936)	1.192 (2.988)
Legal form (5 dummies)	yes	yes	yes	yes
Firm profitability (4 dummies)	yes	yes	yes	yes
Region (7 dummies)	yes	yes	yes	yes
Likelihood ratio	260.94 ***	171.17 ***	127.05 ***	115.11 ***
Log likelihood	-1635.43	-717.85	-352.84	-185.49
Pseudo R2	0.074	0.107	0.153	0.237
Number of observations	5,845	3,513	2,210	1,298

Note: *, **, *** denote statistical significance at the .10, 0.05, and 0.01 levels, respectively.