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ABSTRACT

On the Role of Legislation as a Driver of Incentive Management Practices in Europe*

This paper investigates one aspect of the structured management practices literature which has argued that exogenous legislative changes leading to reductions in union power (identified with the passage of RTW laws) serve to increase the use of management incentives practices often resisted by unions as giving too much discretion to the employer. Capturing such alterations in the business environment by compound legal changes in employee representation protection we investigate whether corresponding changes in the use of incentive management practices are found in European nations. Our baseline difference-in-differences model shows that reductions in the protection offered employees are associated with increased adoption of "people management," while increases in employee representation protection point to more strongly significant negative treatment effect estimates. Each finding is corroborated in a complementary analysis using synthetic control methods. Future discussion of management practices might be expected to take explicit account of the value of employee voice.

JEL Classification: D22, J8, L2, M11, M50

Keywords: structured management practices, incentives management practices, right-to-work laws, employee representation protection, difference-in-differences, synthetic controls, CBR Labour Regulation Index, European Company Survey

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1. Structured Management Practices

A distinctive body of research on the management of human resources has adopted a management as a technology approach, identifying certain key aspects of human resource management as a technology or best practice the adoption of which would improve the performance of the typical firm. As described by its architects, Bloom and Van Reenen (2007), the enabling World Management Survey (WMS), an open-ended evaluation tool, identifies and scores some 18 management practices from 1 (worst practice) to 5 (best practice). The individual scores are then converted from this one-to-five scale to *z*-scores with the unweighted average across *z*-scores serving as the main measure of overall management practice. The main categories of these structured management practices are monitoring, targets, and incentives.[1] The monitoring component seeks to assess how well companies track production and can build upon this as a basis for continuous improvement. For its part, targeting focuses on the type, functionality, transparency, range, and connectivity of performance indicators, while incentives encompass promotion practices, pay, and bonuses, as well as the treatment of star performers and dismissal/transformation of bad performers. Data for the WMS cover medium-sized firms (with 50 to 5,000 employees. The survey was first administered in the summer of 2004 for four countries (France, Germany, the U.K., and the U.S.), the sample of countries having since grown to more than 35 nations. As a practical matter, the multi-wave WMS has been supplemented by a new closed-end survey of structured management practices in the form of the Management and Organizational Practices Survey (MOPS). The MOPS, which was first fielded for the U.S. in 2011, and again in 2016, contains 16 management questions in the three main areas of monitoring, targets, and incentives, as well as separate questions on other organizational practices to include decentralization. Subsequently, corresponding surveys have been conducted for 13 additional countries.

Using both surveys of structured management, especially the WMS, attention has focused on explaining the correlates of management practices/scores and the consequences of poor management practices. Antecedents have included product market competition and family ownership, with the former boosting and the latter detracting from management scores. As regards consequents, individual WMS studies have for example investigated the role of management practices as determinants of productivity, profitability, innovation, survival, and growth in 732 medium-sized firms in the United States, France, Germany, and the United Kingdom (Bloom and Van Reenen, 2007), and in influencing firm productivity in up to 35 countries (Bloom et al., 2017). For its part, research using data from two waves of the new MOPS has for example considered the determinants of productivity, survival rates, and employment growth for a sample of 35,000 U.S. manufacturing plants in 2010 and 2015 (Bloom et al., 2019). An enduring finding

of the burgeoning literature using *both* surveys is the positive association between better management and improved firm performance in developed economies. By way of illustration, the last-mentioned study reports that the spread in structured management practices accounts for 21.6 percent of the variation in plant productivity as compared with the separate contributions of research and development (21.6%), information and communication technologies (12.0%), and human capital (15.9%). Moreover, the role of management practices remains large in the presence of the three other factors, their joint contribution to the spread in productivity being 44.1 percent.

But at this stage the extent to which *best practices* are mediated by other factors – the subject of the present inquiry – is still under investigation. Thus, it might be the case that their influence reflects the manner in which firms conduct their selection of managers. If so, management practices are only effective to the extent that firms are first able to select quality managers who possess a greater ability to implement them. Now this possibility is an extension of the human capital argument, albeit one that emphasizes the need to control for unobserved worker ability, particularly that of managers.

One such approach has been followed by Bender et al. (2018) using three waves of the WMS for Germany, and building on the framework developed by Abowd, Kramarz, and Margolis (1999) whose (AKM) model decomposes wages into worker- and establishment-specific components. In addition to a linear index of time-varying individual characteristics (and a residual pay component), German longitudinal wage data are decomposed into plant-specific pay premia for each workplace, or establishment effects, and worker effects, that is, person-specific measures of earnings capacity or human capital (both potentially observable like education or unobserved like cognitive ability or ambition).

For their part, the establishment effects are a measure of the financial incentives for staying with the firm, while the worker effects serve to raise or lower a worker's productivity irrespective of the place of work. Bender et al. first report that plants with higher management scores have higher management skills. Lacking data on occupations the authors proxy manager quality by the average quality over the top quarter of workers. In correlations between management (practice) score and average employee and managerial ability, it is managerial ability that matters more, and this result is robust across specifications that include controls for the share of college-educated workers as well as other measures of observable human capital such as experience and tenure. Next, in estimating production functions, firms with higher worker and managerial ability are found to have higher productivity although a much larger share is attributable to the human capital of the highest paid workers or managers.[2] Meantime, management scores remain significant across specifications. Although their association with productivity is reduced by 30 to 50 percent with the addition of controls for various measures of employee ability, the contribution

of management practices to productivity remains strong and is described as "striking" (Bender et al., 2018: S406).

2. A More Detailed Look at Causality

The work by Bender et al. can be seen an attempt to get to grips with heterogeneity with the goal of attaining stronger, more robust associations in the data. A development with stronger causal links has focused on the drivers of management practices such as labor market regulation and learning spillovers, namely arguments with credible causal identification strategies facilitated by multiple changes observed over the period covered by the waves of the new datasets as well as material spatial variation. Another path has been charted by attempts to develop structural estimations of formal models of management as a technology, using panel data to recover key parameters, make testable predictions of the effects of management on firm performance inter al., and establish the degree to which management practices contribute to performance (see Bloom et al., 2017). Other studies of this genre have been direct field experiments seeking to inform productivity models and provide empirical content to those models by examining the performance effects of individual management practices among categories of highly skilled professional workers such as aircraft captains (as in Gosnell et al., 2020) or the behavior of a randomly selected sample of firms offered free management consultants versus a control group provided no such assistance (as in the study of Indian textile firms by Bloom et al., 2023).

Given our interest concerns the role of labor market regulation in shaping the quality of management practices, we focus here on a study by Bloom et al. (2019) of the drivers of such management practices that has paid close attention to the role of the business environment, by which is meant the legal rules governing the employment relation in the specific form of right-to-work laws. The key insight of the authors is their recognition that if such legislative changes are sufficiently exogenous a powerful and causal effect of specific management practices can be estimated.[3]

Bloom et al. use state-level introductions of RTW laws in Michigan and Indiana in 2012 to construct a Difference-in-Differences design (see below) where the contiguous states of Ohio, Illinois, and Kentucky form comparison groups. In this quasi-experimental DiD study of the determinants of management practices (and, subsequently, their impact on several indicators of firm performance) a basic distinction is drawn between two sets of management practices among the sixteen identified in the new MOPS dataset. These are, on the one hand, eight "incentive practices" tying pay, promotion, and dismissals to employees' ability and performance and, on the other, the remaining eight practices covering monitoring and targets. Here the argument is that unions frequently oppose the former set of

management practices on the grounds that they give too much discretion to employers. Accordingly, Bloom et al. argue that if the introduction of RTW laws serves to weaken unions, then incentive practices will receive more emphasis (higher scores) after the passage of such legislation by reducing the effective price of introducing them. It is reported that the effect of the *treatment* on incentive practice management scores is strongly positive, whereas "a precisely estimated 0 coefficient" is obtained for non-incentive management (i.e., the balance of the remaining management practices). RTW also has a positive and statistically significant effect on establishment employment, while reducing union density. Moreover, Bloom et al. conclude that this business environment variable, as they term it, increases the adoption of structured management practices that in overall terms account for about one-fifth of the cross-firm spread in productivity.

In addition to these DiD findings, the authors also deploy a regression discontinuity (RD) design comparing plants in counties located within 100 miles of state borders that divide states with different RTW rules and where distance to the RTW border is the running variable and crossing the border the discontinuity threshold. The results of this RD design mirrored the DiD approach in that in the case of incentives management practices there was a sharp discontinuity in their adoption at the border in the data – indicative of a causal effect of RTW legislation – but no such discontinuity in the case of non-incentive practices.

This treatment of antecedents by Bloom et al. (2019) very much informs our own analysis of the effect on specific management practice scores of (changes in) analogous labor market regulation in 28 European nations. Before turning to that analysis, however, we pause to consider a European study from the consequents side – namely, of the macroeconomic effects of changes in worker participation in corporate governance attendant upon the passage of codetermination laws – that informs the technical part of our own empirical inquiry. Using a cross-country event-study design exploiting major reforms of the codetermination process in 10 out of 14 European nations over the 1960-2019 interval, Jäger et al. (2022) chart the effects of these changes on GDP growth per capita and four other macroeconomic outcomes.[4] For each country reform event, the authors construct a synthetic control unit from the other countries in the sample that have no reforms within a 10-year interval around that reform date, with synthetic control weights based on pre-reform macroeconomic characteristics. Next, stacked event study specifications are estimated by pooling country-reform units and synthetic control units, aligned by event time, and then running DiD regressions comparing the outcomes of the treated and synthetic control groups in years before and after the codetermination reforms. Supplementary event studies are also conducted for two industrial relations outcomes – strike intensity and growth in the fraction of a country's

workers who belong to a union – as well as a simple cross-section exercise for 2015 charting the correlation between the 'extent' or strength of codetermination laws and management perceptions of the cooperativeness of industrial relations.

As far as the aggregate economic outcomes are concerned, there is little suggestion that codetermination reforms have had any impact: average post-reform coefficients for all outcome indicators cluster around zero and are statistically insignificant. For the industrial relations outcomes, there is an indication of some modest increase in strike intensity and union density, while for its part the cross-section correlation between codetermination and cooperativeness is positive but lacking.

We think it important to borrow from the analysis of Jäger et al., while strengthening several analytical components of that contribution: firstly, by expanding the set of European countries and amplifying the synthetic control methodology; and, secondly, by conducting the analysis of the impact of labor regulation both at aggregate (country) and micro (establishment) level. We also propose to look at employee representation protection rather than just codetermination issues, the latter constituting but one aspect of worker representation at the workplace. This means that we will also examine issues such as the right to collective bargaining, a duty to bargain, and the extension of collective agreements, inter al.

We conclude these introductory remarks by (a) briefly introducing the counterparts of RTW used in the present exercise as well as the three European establishment-level datasets used in our inquiry, (b) listing the paper's main conjectures and structure, and (c) offering a review of its findings. Beginning with data, our (direct) counterparts of RTW laws are essentially threefold, namely legal changes leading to reductions in collective bargaining rights, in the employer's duty to bargain, and in the extension of collective agreements. Such changes are predicted to increase the use of management practices and their scores. But such laws may also be strengthened, so that we also offer an indirect (i.e., symmetric) RTW test pointing to lessened use of management practices/scores in such circumstances. The legal context of employee representation protection is taken from the comprehensive CBR Labour Regulation Index, as in Jaeger et al. (2022), and is essential to all regulation studies involving different countries. In turn, information on management practices at establishment level is obtained from three repeated cross sections of the European Company Survey which contain relevant information on management practices in 28 European countries, particularly in the area of incentive management, for the sample years of 2009, 2013, and 2019. These practices are distinct from those included in MOPS, but sufficiently informative and differentiated both over time and across countries, thereby offering us the opportunity to test

whether the changes in employee representation protection have a causal impact on the adoption of incentive management practices.

Our broad conjecture is therefore that a pro-business environment in the form of a reduction in employee representation protection is expected to boost incentive management practices; and that, symmetrically, an increase in employee representation protection, for example by increasing collective agreement extension mechanisms or expanding codetermination rights having to do with the information and consultation of workers and board membership leads to a reverse set of predictions. We therefore propose to look at the effects of both phenomena – extension and regression – individually, by running separate tests across samples of firms in countries experiencing reductions in employee representation protection and those witnessing the opposite.

Our baseline model is a standard difference-in-differences specification exploiting legislative changes in the treated countries vis-à-vis the controls to determine the impact of the treatment as indexed by management practice scores. We control for pre-policy trends, add sector and country trends, and experiment with a variety of falsification tests. We also complement our analysis using synthetic control methods to allow for more plausible estimates of the counterfactual outcomes.

Abstracting from the results for overall management practices which, pari passu with Bloom et al. (2019), are altogether weaker than for the distinct set of practices grounded in incentives management, we find that a reduction in employee representation protection is associated with increased adoption of the latter practices. Moreover, there is no evidence favoring the existence of any enabling pre-treatment trend in this regard or for that matter that sector and country trends play any decisive role. Similarly, a simple attribution of false treatment status fails to generate a significant DiD coefficient. Parallel results for the case of an increase in employee representation protection point to more strongly significant negative DiD coefficient estimates, again no less in line with the Bloom et al. hypothesis.

These results are confirmed in our synthetic control approach with multiple treated units and varying treated years, using aggregate (i.e., country-level) rather than establishment-level data. Thus, for the case of a legislative change reducing worker representation protection a positive gap emerged, while in the case of legislative changes in opposite direction there was confirmation of a negative treatment effect. In both exercises we found that countries in the donor pool should be assigned different weights, while in neither case was there a suggestion that the placebo effect exceeded the treatment effect in absolute value.

3. Modeling

Our management practices model assumes management practices to be an additional input in production, which in a generalized Cobb-Douglas two-input production function framework can be specified as follows:

$$Y = AL^{\alpha}e^{\beta M}, \quad (1)$$

where M is a given set of *structured management practices* (Bloom et al., 2019: 1656) and L denotes labor.

Assuming L and M are observable, from (1) we can easily derive an empirically testable relationship in which labor productivity is a function of M:

$$log\frac{Y}{L} = \log A + (\alpha - 1)logL + \beta M. \quad (2)$$

In this framework, changes in labor market institutions are treated as a possible source of exogenous variation in management practices. In particular, it is conceivable that M depends on some labor institution, R. Denoting the unit price of M by w_M, we have therefore w_M as a function of R; for example, expressed in log form, $\log w_M = \theta R$.

Using model (1) and setting $M = \log \widetilde{M}$, we obtain $Y = AL^{\alpha}\widetilde{M}^{\beta}$, where $\widetilde{M}$ denotes the stock of managerial capital. Cost minimization of $w_L L + w_{\widetilde{M}}\widetilde{M}$ subject to $Y = AL^{\alpha}\widetilde{M}^{\beta}$ then yields the following conditional demand function for $\widetilde{M}$:

$$\log \widetilde{M} = a + \frac{\alpha}{\alpha+\beta}\log w_L - \frac{\alpha}{\alpha+\beta}\log w_{\widetilde{M}} + \frac{1}{\alpha+\beta}\log Y\,, \quad (3a)$$

$$\Leftrightarrow M = a + \frac{\alpha}{\alpha+\beta}\log w_L - \frac{\alpha}{\alpha+\beta}\theta R + \frac{1}{\alpha+\beta}\log Y\,, \text{ or} \quad (3b)$$

$$M = a + \frac{\alpha}{\alpha+\beta}\log w_L + \gamma R + \frac{1}{\alpha+\beta}\log Y\,, \quad (3c)$$

where θR now denotes the log of $w_{\widetilde{M}}$.

Finally, focusing on incentive management practices, all that is required is longitudinal evidence on *R* so that we can run a standard DiD regression using pooled data at country and firm level, specified as follows:

$$M_{ict} = \delta_1(R_c * Post_t) + X_{ict}\beta + \omega_c + \lambda_t + \varepsilon_{ict}, \quad (4)$$

where R_c is an indicator for countries that introduced a legislative change (i.e., 1 if country *c* has changed its employee representation protection over the sample interval, 0 otherwise), while $Post_t$ is equal to 1 for the years after the legislative change has been introduced, 0 otherwise. X_{ict} are other observable controls, ω_c are country dummies, λ_t are time dummies, and ε_{ict} is an error term. Specifically, given our three repeated cross-sections, t = 2009, 2013, 2019, while $c \in \{1, \ldots, 28\}$ and $i \in \{1, \ldots, 500\}$, $i \in \{1, \ldots, 1000\}$ or $i \in \{1, \ldots, 1500\}$, depending on whether establishment i is from a 'small', 'medium' or

'large country'. The context is therefore one in which countries are observed over time but not establishments.[5]

In the case of a reduction in protection, δ_1 is positive under the hypothesis that formal worker representation does not favor intensification of incentive management practices and with it higher management scores.[6] Conversely, an increase in employee representation protection is expected to generate the opposite effect, or δ_1<0.

This is our baseline model. Implicitly one would also wish to estimate a separate specification that replaces the subset of incentive management practices with overall management practices, the clear expectation being that our RTW-analog will impact mainly incentives practices management rather than other management practices.

We further experiment by controlling for pre-treatment trends that may have been present in the treated countries to test for pre-treatment effects as a result of which even in the absence of policy changes incentives management practices might have increased. We also add sector- and country-specific trends to the baseline model specification, and experiment with falsification tests. In the latter, by way of illustration, we use the subsample of control countries (i.e., countries with no record of legislative changes over the sample period) and conduct an exercise in which half of them are assumed to have changed employee representation protection after 2009. We then evaluate the effect of an increase in employee representation protection, as well as the effect of a decrease, in separate regressions. Clearly, in both cases the placebo (or false treatment) effect should be zero.

The DiD model in equation (4) assumes equal weights across countries – or that countries are equally comparable, given observables. But whether, say, countries *A* and *B* are good comparators for treated country *D* is a matter that should be tested. In principle, only a subset of the donor pool countries should be eligible (or given positive weights) in the construction of some optimal comparator for country *D*. To this end we implement a Synthetic Control Method-SCM, after Abadie et al. (2010), in which different country weights are allocated so that pre-treatment characteristics across treated and untreated units are as similar as possible. In any given post-intervention period, the treatment effect is then computed for each treated unit and the overall gap (the treatment effect) calculated as an average over all the treated countries. Hopefully, the SCM approach should also yield a positive (negative) treatment effect in the case of a reduction (increase) in employee representation protection.

Clearly, in our implementation of SCM we have multiple treated units (countries) and varying treated years. For example, for France 2019 is the treatment year and 2009 and 2013 are the pre-treatment years, while for Spain 2013 and 2019 are the treatment years and 2009 is the pre-treatment

year. This is due to the fact that, as described in the data section below, in France the legislative change is recorded in 2016, while in the case of Spain the relevant changes occur in 2011 and 2012. Based on this illustration it also follows that although the data comprise just three data points (i.e., 2009, 2013 and 2019), in practice we have two pre-treatment periods and two post-treatment periods (denoted, to simplify, as *t* = -2, -1; and *t* = 0, +1, respectively). A "stacked" synthetic control estimator is then required, and to this end we use the *allsynth* command in Stata 16, a routine designed for multiple treated units and longitudinal/panel data (Wiltshire, 2024).

As mentioned earlier, in our repeated cross-country data only countries are observed over time, not establishments. We have therefore to take averages over each country in order to obtain the required longitudinal data structure. Given that our sample comprises 28 countries observed in 3 data points, we will have as a result a maximum of 84 observations. In comparison with the pooling, firm-level case in model (4), the SCM approach will imply as a consequence a substantial reduction in the estimation sample. The advantage resides in having better comparison groups.

Formally, let *I* and *J* be the set of treated and untreated (control) units, respectively, with $i \in I$ and $j \in J$, while, for any $i \in I$, Y_{it}^{NT} and Y_{it}^{T} denote the potential outcome for *no intervention* (treatment) and *intervention* (treatment), respectively. In this setting we therefore have the estimated marginal effect (or gap) given by $\tau_{it} = Y_{it}^{T} - Y_{it}^{NT}$.

For each treated unit i, Y_{it}^{T} is observed in $t = 0$ and $t = 1$ (the two post-treatment periods in our case). One therefore needs an estimate of Y_{it}^{NT}, or the counterfactual $\widehat{Y}_{it}^{NT}$, which is given by the outcome of the synthetic group in t, calculated as a weighted average of the outcome values of the donor pool units, that is, $\widehat{Y}_{it}^{NT} = \sum_{j \in J} \widehat{w}_j^i Y_{jt}$, for $t = 0$ and $t = 1$, $0 \leq \widehat{w}_j^i \leq 1$, and $\sum_{j \in J} \widehat{w}_j^i = 1$.

Typically, the weights $\widehat{w}_j^i$ are computed to minimize, in the pre-treatment period, the distance between i and its donor pool using the Euclidean norm. Thus, admitting k predictors of the outcome (including pretreatment values of the outcome), the estimated weights are obtained by solving the constrained minimizing problem as follows: $\{\widehat{w}_1^i, \widehat{w}_2^i, \dots, \widehat{w}_J^i\} = \arg min_{w_1^i, w_2^i, \dots, w_J^i} \left\| X_i - \sum_{j=1}^{J} w_j X_j \right\|$, with $X_i = (x_{1i}, x_{2i}, \dots, x_{ki})'$ and $X_j = (x_{1j}, x_{2j}, \dots, x_{kj})'$. Thus, assuming the same number of predictors of the selected outcome for the treated and donor pool units, the minimizing problem amounts to minimize $(\sum_{h=1}^{k} (x_{hi} - w_1^i x_{h1} - w_2^i x_{h2} - \dots - w_J^i x_{hJ}))^{\frac{1}{2}}$.

Given *k* predictors, one wants in addition to give a higher weight to variables with a higher predictive power. A set of prior weights, $\{v_1, v_2, \dots, v_k\}$, has therefore to be computed in a first stage, and several methods of estimating these weights are possible, including minimizing the mean squared

prediction error-MSPE (over the entire pre-treatment period).[7] According to this procedure,[8] the minimizing problem ultimately consists in finding $\{\widehat{w}_1^1, \widehat{w}_2^1, \ldots, \widehat{w}_j^1\}$ to minimize $(\sum_{h=1}^{k} v_h^i(x_{hi} - w_1^i x_{h1} - w_2^i x_{h2} - \cdots - w_j^i x_{hJ}))^{1/2}$.

The typical set of assumptions and requirements for SCM comprise (i) the existence of a comparable donor pool (each of the control units is similar to at least one treated unit); (ii) no interference or spillover effects, that is, Y_{it} and $Y_{jt'}$ cannot affect one another, for any $i \neq j$ and for any t and t'; (iii) no anticipation effect; and (iv) sufficient pre-treatment and post-treatment information. Under these conditions, the synthetic control estimates offer a plausible approximation of the counterfactual outcome, allowing the calculation of the estimated treatment effect on the treated units as the difference between a treated unit's outcome value and that of its estimated synthetic control.

As a final exercise we run falsification tests with synthetic controls. In this case, we iteratively reassign treatment to a control unit in the donor pool and estimate placebo effects in each iteration. This procedure is intended to evaluate whether the treatment effect obtained by comparing a treated unit with a synthetic control is sufficiently different or large enough *relative to the effect estimated for a (false) treated unit chosen at random.* Note that in this exercise the number of comparison countries (and consequently the number of placebos) becomes very large which is certainly good news for proper inference. Indeed, given the number of units in the sample (both treated and untreated), the number of average placebo gaps to be computed becomes very high.[9] We therefore simplify the computations by using 1,000 randomly sampled placebos in order to compute the average placebo gap, with the set of donor (i.e. control) units comprising all the 18 countries that make up the donor pool in the reduction of employee representation protection case; 19 in the case of an increase, in a separate analysis.

4. Data

The research design described in the previous section considers the interplay between legally determined employee representation extension mechanisms (taken to be analogous to RTW) and structured management practices at establishment level for all 27 EU member states and the United Kingdom. For information of the first type, we rely on the *CBR Labour Regulation Index* (see Adams et al., 2023), while for actual selected management practices (and other establishment-specific data) we will use three repeated cross-sections of the European Company Survey for 2009, 2013, and 2019 (see Eurofound, 2009, 2013, and 2020).[10]

Beginning with the latter information set, while inspired by the three main dimensions of monitoring, targets, and incentives in the WMS, measurement of management practices using ECS

information falls well short of replication. There is, for example, no equivalent measure for the "treatment of star performers and the firing/fixing of bad performers" (see Bloom and Van Reenen, 2007: 1361). There are also some marked differences in the composition of the set of management practices across the three ECS surveys. Specifically, the number of raw domains is equal to 3 in 2019, 5 in 2013, and 3 in 2009, and, ultimately, we have to focus on just one model-centered domain, namely incentive management practices, to obtain a sufficiently homogenous index over time.[11] Despite these limitations, the ECS does offer a wider sectoral coverage (i.e., it is not confined to manufacturing as in the WMS case) and contains information on a large set of relevant countries with material cross-sectional and temporal variability in management practices.

The subset of *incentive management practices* and the corresponding coding is summarized in Appendix Table 1. Fortunately, for this subset of practices all three surveys collect information on the percentage of employees receiving variable pay (or payment by results) of various types. Clearly, we were less fortunate with the all the other management practice domains. In effect, although the set of documented practices in 2013, for example, is rather large, discrepancies over time are palpable and we were forced to restrict the subset of other management practices to monitoring (in 2019), performance appraisal (in 2013), and other schemes (in 2009).

To ensure a common scale, we follow Bloom and Van Reenen and standardize the included items. To illustrate for the three-element set of incentive management practices in 2019, each item is standardized using the mean and standard deviation across all establishments to obtain the corresponding *z*-score per establishment. Next, for each establishment we take an unweighted average of the three *z*-scores and then standardize again so that a single-domain (i.e., incentive practices) indicator of mean zero and standard deviation equal to 1 is generated. For the same year, a similar indicator is constructed for the monitoring/other management practices domain, this time using two selected items. Finally, we take an unweighted average over the two single-domain indicators and then standardize again to produce our overall management z-score. *Mutatis mutandis* for 2009 and 2013.

Our pooled dataset is obtained by firstly merging the raw Management (MM) and Employee (ER) surveys to construct for each year a unique ER-MM record in which the information obtained from management and the employee representative is correctly matched, including the proper incentive and overall management practices z-scores. We then append the three generated merged datasets into a single file. This pooled dataset contains a single data point per establishment and three data points per country, comprising a raw total of 68,262 establishment-year observations. Recall that the longitudinal component at country level is crucial in our implementation of the synthetic control method. Finally, key

establishment-level variables such as sector affiliation and establishment size (number of employees), were also recoded to ensure that their definitions are time invariant.

Our measure of employee representation protection has a basis in element D of the five-element coding protocol identified in the CBR *Labour Regulation Index*. It comprises seven components: right to unionization, right to collective bargaining, duty to bargain, extension of collective agreements, closed shops, codetermination (board membership), and codetermination and information/consultation of workers. Each component is assigned a score between 0 and 1, where 0 stands for zero legal protection and 1 for the maximum legal protection offered. Most but not all component indicators of employee representation use graduated rather than binary scores. A nation's history of legal regulation since 1970 is described in terms of this coding, which can be summarized for the countries in our sample over the three years covered by the ECS; that is, we will identify changes in the strength of legislation as of 2009, 2013, and 2019.

By way of illustration, for a country in our 28-country sample with no relevant legislative change in 2009, 2013, or 2019 we assign the sequence 000, while for a country in which, say, a single relevant change was observed in the period after 2009 we assign the sequence 011. In the notation of the modeling section, this means that the former (i.e., the country recording no legislative change) will be assigned a non-treatment status, while the latter is considered a treated unit, with 2009 on the one hand and 2013 and 2019 on the other being flagged as pre-treatment and post-treatment years, respectively.

Full coding of country-level *reductions* in employee representation is given in Appendix Table 2a, using the information taken from the CBR Labour Regulation Index file, namely Appendix 1-CBR-LRI Coding Protocols and Appendix 2-Country Tables, items/variables 25-31 of Area D (Employee Representation). As can be seen from the table, 2009 is the pre-treatment year in the cases of Romania, Slovakia, and Spain, while for Czechia, France, and the U.K. the years 2009 and 2013 are considered pre-treatment and 2019 as post-treatment. We have therefore a total of six treated countries in our first DiD exercise. By construction all the other countries belong to the set of donor units, with the exception of Ireland, Hungary, and Cyprus. For the latter, an *increase* in employee representation protection can be charted, as shown in Appendix Table 2b. For these three countries a new set of donor units is constructed, with the obvious exclusion of all the six treated units earlier identified in Appendix Table 2a. This newly constructed set will constitute the basis for our second – and symmetric – DiD exercise in which we evaluate the effect on management practice scores of an *increase* in employee representation protection.

5. Findings

Table 1 gives the difference-in-differences estimates of the effect on management practices of a country-level reduction in employee representation protection. The case of incentives management is presented in the first column, and in panel A we have the results for the baseline model given in equation (4). As can be seen, the DiD coefficient, δ_1, is positive and significant at the 0.05 level, from which we conclude that cross country differences in "people management" (in particular, aspects related to performance-based pay linked to individual or team performance) are causally related to the degree of labor regulation. In short, it seems that Bloom and Van Reenen's (2010) earlier conjecture of there being an inverse correlation between this type of management practice and the extent of labor market regulation across a large set of countries might well be causal after all.[12]

In the exercise in panel B of the table we control for the existence of a pre-treatment trend. This amounts to a falsification test in which the treated countries are assumed to have introduced the legislative change one period earlier than the actual intervention. As a placebo test, it is intended to evaluate whether even in the absence of policy changes incentive management practices might have increased. Although certainly a full test would require a longer time length, using our three data points we found no evidence of the existence of a pre-treatment trend. The sign of the DiD coefficient is positive and of a higher magnitude but displays a lower level of statistical significance than in panel A.

The existence of possible time trends is further addressed in panel C, where sector and country dummies are interacted with a linear time trend. For reasons of data confidentiality, sector disaggregation in the ECS survey is unfortunately very low. Our test is therefore mainly indicative. However, as in panel A, we obtain a DiD coefficient that is significant at the 0.05 level and of approximately the same magnitude.

Finally, in panel D, we assign treatment status to a selected set of donor countries. The reported case is of course just one in many possible permutations. For purposes of illustration, we report only the situation in which half of the untreated countries (i.e., countries with no record of legislative innovation over the sample period) are assumed to have reduced employee representation protection after 2009. As expected, the false treatment status does not generate any significant effect.

Column (2) of the table reports the effect of a reduction in employee representation protection, this time on the *overall* management practice score. Given the strong limitations in the construction of an overall score, we do not expect any particular strong effect. In practice, the DiD estimate is negative in panels A and C and, after controlling for the pre-treatment trend, insignificant in panel B. The results from

the placebo test in panel D are also counterintuitive. In short, and not surprisingly, the results in column (2) are largely inconclusive.

The European Union context, similar to that of the United States case, provides a rich environment for institutional experimentation. In our data it is indeed possible to observe opposing changes in labor law across countries and at in different points in time, as was documented in the data section. Table 2 presents the results of a symmetric exercise in which we test for the effects of an intervention that now *increases* employee representation protection. In this case, we have a smaller number of treated countries (a total of three) and approximately the same number of control units in the donor pool. The point is whether there will be any evidence of the presence of a symmetric treatment effect, that is to say, whether there is any indication that greater protection *reduces* incentive management practices.

As before, we have four main specifications, in panels A through D, and two alternative measures of management practices, in columns (1) and (2). Quite strikingly, the DiD coefficient in the first three panels of column (1) is negative and highly significant, while the placebo effect in panel D is insignificant. Column (2), for the overall management score case, also shows a negative effect although with an expected lower level of statistical significance.

As discussed earlier, in our dataset neither the pre-treatment nor the post-treatment periods are very long. One way to strength our results is to take advantage of the fact that the sample contains multiple treated units and a non-trivial number of units in the donor pool. We have therefore the opportunity to improve the quality of the counterfactuals by using the synthetic control approach. To this end, we need data aggregated at country level, as countries are indeed observed in three consecutive surveys. The aggregation procedure of course assumes away within-country differences, but we are encouraged by the results in Tables 1 and 2 – using micro (establishment) data – where we found that the results are not overly sensitive to either sector or country trends. That said, the present goal is to offer further evidence on the relationship between management practices and the business environment, now using now a framework in which the quality of the control group counterfactuals is tested and improved.

Figure 1 gives the treatment effect on the incentive management score of a *reduction* in employee representation protection using synthetic control units. As in Table 1, we have six treated units and a pool of eighteen potential control units. The treated units in particular have a varying number of treated years which implies, in practice, two pre-treatment and two post-treatment periods.

As can be seen in the figure, based on the selected set of prediction variables, there seems to be a good fit for the observed outcomes across treated and untreated units before intervention. This is shown by the virtually zero average gap in the incentive management score case, both in t=-2 and t=-1. After

intervention, we have an average gap of 0.129 in *t*=0 and 0.162 in *t*=+1. On average, the synthetic (optimal) control unit contains 5 countries with a weight greater than 0.05. It is therefore confirmed that countries in the donor pool should be assigned different weights, including possibly a zero weight.

Figure 2 gives the SCM analogue of column (2) in Table 1. It shows that the evidence on the effect of the introduction of a legislative change unfavorable to employee representation is much weaker in the case of the overall management score. The pre-treatment match (at *t*=-2 and *t*=-1) in this case is less than fully attained and the effect after treatment is virtually zero, at +0.012 in *t*=0 and -0.024 in *t*=+1.

The case of an *increase* in protection using synthetic control units is given in Figures 3 and 4. For incentive management, in Figure 3, there is confirmation of a negative effect, at -0.313 and -0.207 in *t*=0 and *t*=+1, respectively, with the gap in the pre-treatment period not too different from zero.[13] The results for the management score, in Figure 4, also reproduce those obtained in Table 2; in this case showing that the evidence on the presence of a treatment effect is too weak to be conclusive, with an average gap of +0.031 in *t*=0 and -0.044 in *t*=+1.

The final part of our analysis comprises a thorough falsification exercise. The key and novel aspect here is the construction of placebos in a multiple treated units and varying treatment year setting. We note that in the single treated unit case, randomization of the placebo is straightforward as it amounts to (a) permuting the treated unit by one of the untreated units at a time, (b) constructing the corresponding synthetic control unit (with the exclusion of the permuted unit), and (c) computing the gap for each placebo construction. The case with multiple treated units is more cumbersome. In this case, the treated unit *i* is permuted across all untreated units in *i*'s donor poll (with the remaining untreated units constituting the donor pool for the constructed placebo) and the corresponding placebo effect calculated. This is repeated for all the other units in the treatment group and then an average treatment effect (or "gap") calculated over the *i*'s. The procedure, however, generates very many placebo average gaps. This number, it will be recalled, is given by $N_G = \prod_{i=1}^{I} J_i$, which is increasing in *I* and *J* (especially in the number of treated units, *i=1, 2, …, I*). Accordingly, one is forced to select, randomly, a number of cases, S, with $S < N_G$, and then plot the actual average gap for the treated units, $\hat{\bar{\tau}}_i$, and the placebo average $\hat{\bar{\tau}}_i^s$ for each *s*, *s=1, 2, … S*. Having obtained the estimates, one has then as a last step to compare visually the actual treatment effect with the sample permutation distribution of placebo effects.

We start with the incentive management practices/reduction in employee representation protection case, using 1,000 randomly sampled placebo averages, as plotted in Figure 5. The solid line replicates the treatment effect given in Figure 1. Clearly, in the vast majority of the cases, the average

placebo gap lies below the solid line, indicating that the treatment effect is likely to comfortably exceed the placebo effect.

Figure 6 offers in turn a quite contrasting pattern. In this case, there is no evidence of any visible difference between the treatment effect and the placebo effect for the management score outcome. Inconclusiveness is therefore confirmed, as expected.

Finally, in Figures 7 and 8 we plot the placebos for the case in which there is an increased protection of employee representation. Figure 7 shows the permutation distribution of placebos for the selected outcome of the incentive management score. Again, the solid line denotes the actual average gap for treated units (as given in Figure 3). As can be seen the estimated placebo effects are visibly above the solid line, an indication that the placebo effect is not likely to produce a larger negative effect than the treatment effect. For the overall management score, in Figure 8, the test is again inconclusive as both the treatment effect (taken from Figure 4) and the placebo effects, on average, do not seem to be different from zero.

6. Conclusions

Perhaps of most surprise to readers is the neglect of unions in the study of structured management practices – itself one of the most impressive contributions to the business economics/personnel economics literatures in recent years. Recently, that reticence has been breached in an analysis of the determinants of management practices in U.S. manufacturing by Bloom et al. (2019) in which it has been reported that RTW laws that weaken unions are a key driver of improved management performance by facilitating the growth of incentive management practices resisted by unions. In the present paper we have revisited this argument for a large sample of European countries, using a business/legal regime analogous to RTW (via reductions in employee representation protection) and a similar set of incentive management practices; the former being drawn from the CBR Labour Regulation Index and the latter from three waves of the European Company Survey.

We report findings that are very much in line with those of the architects of the structured management practices literature. Specifically changes in the legal environment yielding reductions in employee representation protection are associated with increased incentives management practices (and higher scores). No less important, the reverse is the case for changes in the business environment serving to increase employee representation protection. Our DiD findings are robust to controls for pre-treatment trends and to the inclusion of industrial sector and country trends. Also, a simple false-treatment placebo test fails to yield statistically significant DiD coefficient estimates. Extension of the model to incorporate

an altogether more sophisticated set of placebo tests using synthetic controls suggests that in the case of incentive management practices and a decrease in employee representation protection there is a good match between treated and untreated units before intervention and that following intervention there is a positive treatment gap. Correspondingly, an increase in employment protection using synthetic control units offers confirmation of a negative treatment effect for incentive management practices. In neither of the two cases involving overall management practices are the tests other than weak. Each of these findings receives support from a set of thorough falsification tests.

The bottom line of the RTW causality test provided in the structured management practices literature for the U.S. is, then, echoed in what are a broadly parallel set of findings from 28 European countries. Having said this, we do not mean to imply that role of unionism is now settled as it seems incontrovertible that the success of management practices hinges in part on the input and attitudes of the workforce as articulated via collective and direct voice, even if the role of non-autonomous unions may have been neglected in the U.S. because of the relevant sections of the Wagner Act having to do with company unions (viz. section 8(a)(2)). One ecumenical point and suggested line of further inquiry is offered by the words of Freeman and Medoff (1984: 179) in assessing union impact on productivity in the U.S.: "*… unionism per se is neither a plus nor a minus to productivity. What matters is how unions and management interact at the workplace.*" That interaction merits greater consideration by the management practices literature and likely invigorated by a distinction between union, nonunion representative, and direct voice.

Endnotes

1. A fourth component is shop floor operations, covering the introduction, rationale for, and functioning of lean production.

2. The authors estimate that the manager share is similar in size to that mediated through employer-specific pay premia.

3. Note that in a separate event-study exercise Bloom et al. also consider how structured management practices might come to permeate labor markets. Specifically, they examine the role of changes in information stemming from learning spillovers associated with the labor market entry of million-dollar plants, typically multinationals. Identification of a causal effect is via plant location in either winning or runner-up counties in the competition for new plants, the higher management practices scores of plants in the former jurisdictions being underwritten by the movement of *managerial labor* from million-dollar plants to local firms

4. Namely, wage growth, labor's share, total factor productivity, and net capital formation.

5. Equation (4) is a standard DiD model and as such the corresponding DiD estimand does not require tracking individuals over time (Athey and Imbens, 2006). But, clearly, the regressor of interest varies only at country level requiring proper (adjusted) standard errors, as recommended by Angrist and Pischke (2009, p. 237).

6. In Germany, for example, changes in practices related to monitoring of the behavior or performance of the employees are required to have the consent of the works council (§87 (1) of the Works Constitution Act), and, in case of a disagreement, the implementation is decided by conciliation committee chaired by a neutral arbiter. As also noted by Grund et al. (2024), collective bargaining agreements in Germany may also include clauses that restrict employers in the design of their performance assessment practices (see their footnotes 5 and 17, respectively).

7. Other approaches are discussed in Abadie et al. (2015).

8. See Yan and Chen (2023) and Wiltshire (2024) for a description of this procedure.

9. The number of placebos is given by $N_G = \prod_{i=1}^{I} J_i$ where, to recall, *I* and *J* denote the set of treated and untreated (control) units, respectively (Wiltshire, 2024). Clearly, N_G is increasing in both *I* and *J*, especially in the former units.

10. The corresponding Management Survey-MM (as well as the companion Employee Representative Survey-ER) are available at the U.K. Data Service site (*https://www.ukdataservice.ac.uk*/). Establishments in the survey have at least 10 employees and, per country, the number of interviewed establishments is around 500, 1,000, and 1,500 units in small, medium, and large countries, respectively, yielding a total of 22,738, 25,309, and 20,015 units across the 28 selected countries in our baseline estimation samples of 2009, 2013 and 2019, respectively. With the exception of agriculture, virtually all sectors of activity are included in the surveys. A full description of the three datasets can be found in Addison and Teixeira (2020 and 2025).

11. In essence, the 2019 ECS contains a total of 11 items/practices in 3 domains, comprising: 1-Operations (1 item); 2-Monitoring (2 items), and 3-Incentives (8 items), while the 2013 survey collects an extended set of 17 items and 5 domains: 1-Work organization practices and monitoring (3 items); 2-Team working (1 item); 3-Performance appraisal (1 item); 4-Incentive/performance-based pay (5 items); and 5-Employee involvement (7 items). For 2009, the usable information is restricted to 3 domains and 5 items: 1-Team work (1 item); 2-Incentive/performance-based pay (2 items); and 3-other schemes (2 items).

12. The included countries in Bloom and Van Reenen's illustration are Australia, Brazil, Canada, China, France, Germany, Great Britain, Greece, India, Italy, Japan, Northern Ireland, Poland, Portugal, Republic of Ireland, Sweden, and USA; a total of 17 countries from 5 continents. The management score refers to incentive management and is based on questions 7 and 13-18 of the WMS covering hiring, firing, pay, and promotions. The strength of labor market regulations, or constraints faced by firms in adjusting their workforces, is based on the World Bank employment rigidity index from the Doing Business database (*http://www.doingbusiness.org/ExploreToppics/Employing Workers).* The authors' correlations indicate that tougher labor market regulation is significantly associated with incentive management scores though not management scores in other domains (see also Bloom et al., 2011).

13. Alternatively, we can also compare the results in Figure 3 with those from a DiD model using group (country) averages rather than micro (establishment) data as in Table 2. In this case, as pointed out by Angrist and Pischke (2009: 313), the adjusted standard errors reflect the group structure (and therefore control for the possible intra-class, or intra-country, correlation). By way of illustration, using panel A in Table 2 the grouped-equation model yields a slightly higher standard error, with the DiD coefficient remaining significant.

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Table 1. Difference-in-Differences Estimates for the Effect on Management Practices of a *Reduction* in Employee Representation Protection

	Incentives management	Management practices (overall score)
	(1)	(2)
A. Baseline model		
DiD coefficient	0.036** (0.017)	-0.033* (0.017)
Sample	N = 66,922 observations Treated countries: 6 Control countries: 18	N = 67,670 observations Treated countries: 6 Control countries: 18
B. Controlling for the pre-treatment trend		
DiD coefficient	0.168* (0.101)	0.112 (0.087)
Pre-treatment coefficient	0.162 (0.120)	0.124 (0.104)
Sample	N = 66,922 observations Treated countries: 6 Control countries: 18	N = 67,670 observations Treated countries: 6 Control countries: 18
C. Controlling for sector and country trends		
DiD coefficient	0.036** (0.017)	-0.035** (0.017)
Sample	N = 66,922 observations Treated countries: 6 Control countries: 18	N = 67,670 observations Treated countries: 6 Control countries: 18
D. Placebo test		
DiD coefficient	-0.024 (0.018)	0.031* (0.018)
Sample	N = 47,851 observations Treated countries: 9 Control countries: 9	N = 48,366 observations Treated countries: 9 Control countries: 9

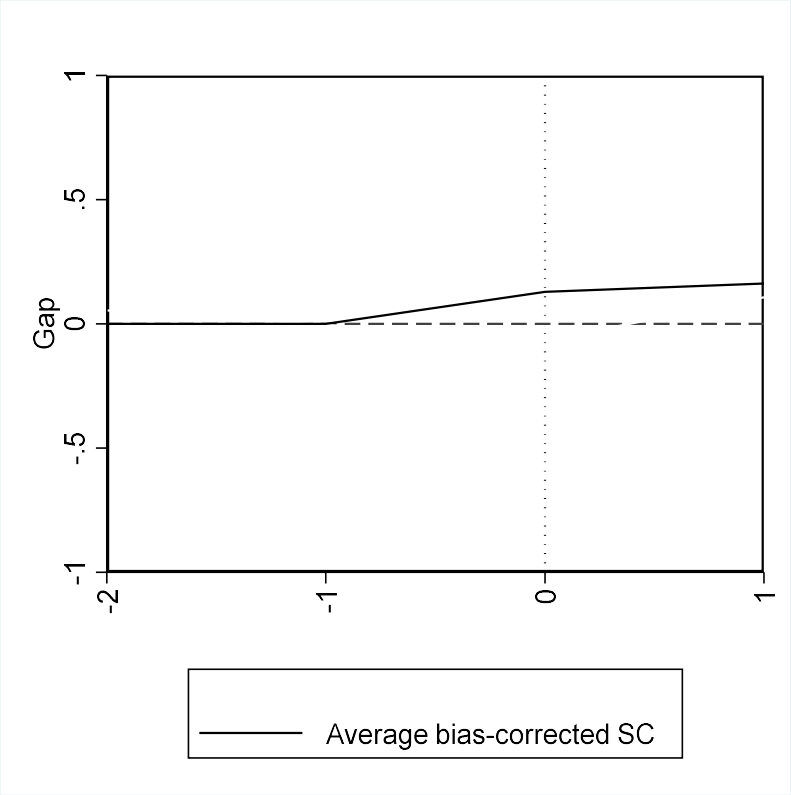
Notes: The DiD model is given in equation (1) in the text and is estimated using the multilevel mixed-effects linear regression (*mixed*) command in Stata 16, controlling for country-year random intercepts. Sector and year dummies are included in all specifications. The dependent variable is the overall management practices score and incentives management score in columns (1) and (2), respectively. The results for the baseline model are given in panel A, while panels B through D offer further experimentation. In panel B we control for pre-treatment trends that may have been present in the treated countries, in panel C we add sector- and country-specific trends to the model specification, while in panel D we construct a placebo test. In the latter, we use the subsample of control countries (i.e., countries with no record of legislative changes over the sample period) and construct an exercise in which half of them are assumed to have reduced employee representation protection after 2009. Standard errors, given in parentheses, while ** and * denote statistical significance at the 0.05 and 0.10 levels.

Table 2. Difference-in-Differences Estimates for the Effect on Management Practices of an *Increase* in Employee Representation Protection

	Incentives management	Management practices (overall score)
	(1)	(2)
A. Baseline model		
DiD coefficient	-0.238*** (0.068)	-0.128* (0.067)
Sample	N = 54,296 observations Treated countries: 3 Control countries: 19	N = 54,901 observations Treated countries: 3 Control countries: 19
B. Controlling for the pre-treatment trend		
DiD coefficient	-0.241*** (0.070)	-0.130* (0.067)
Pre-treatment coefficient	-0.122*** (0.046)	-0.100 (0.065)
Sample		
C. Controlling for sector and country trends		
DiD coefficient	-0.210*** (0.045)	-0.065 (0.127)
Sample	N = 54,296 observations Treated countries: 3 Control countries: 19	N = 54,901 observations Treated countries: 3 Control countries: 19
D. Placebo test		
DiD coefficient	-0.063 (0.122)	-0.116 (0.092)
Sample	N = 50,660 observations Treated countries: 9 Control countries: 10	N = 51,208 observations Treated countries: 9 Control countries: 10

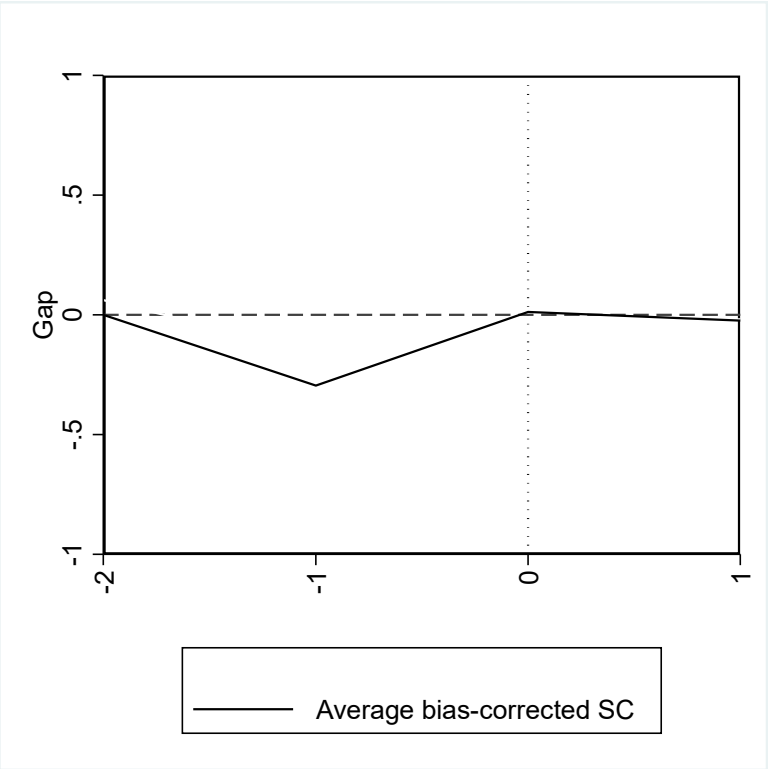
Notes: The DiD model is given in equation (1) in the text and is estimated using the multilevel mixed-effects linear regression (*mixed*) command in Stata 16, controlling for country-year random intercepts. Sector and year dummies are included in all specifications. The dependent variable is the overall management practices score and incentives management score in columns (1) and (2), respectively. The results for the baseline model are given in panel A, while panels B through D offer further experimentation. In panel B we control for pre-treatment trends that may have been present in the treated countries, in panel C we add sector- and country-specific trends to the model specification, while in panel D we construct a placebo test. In the latter, we use the subsample of control countries (i.e., countries with no record of legislative changes over the sample period) and construct an exercise in which half of them are assumed to have increased employee representation protection after 2009. Standard errors, given in parentheses, and are clustered at country level. *** and * denote statistical significance at the 0.01 and 0.10 levels.

Figure 1. The Treatment Effect on the Incentive Management Score of a *Reduction* in Employee Representation Protection, Synthetic Control (SC) Case



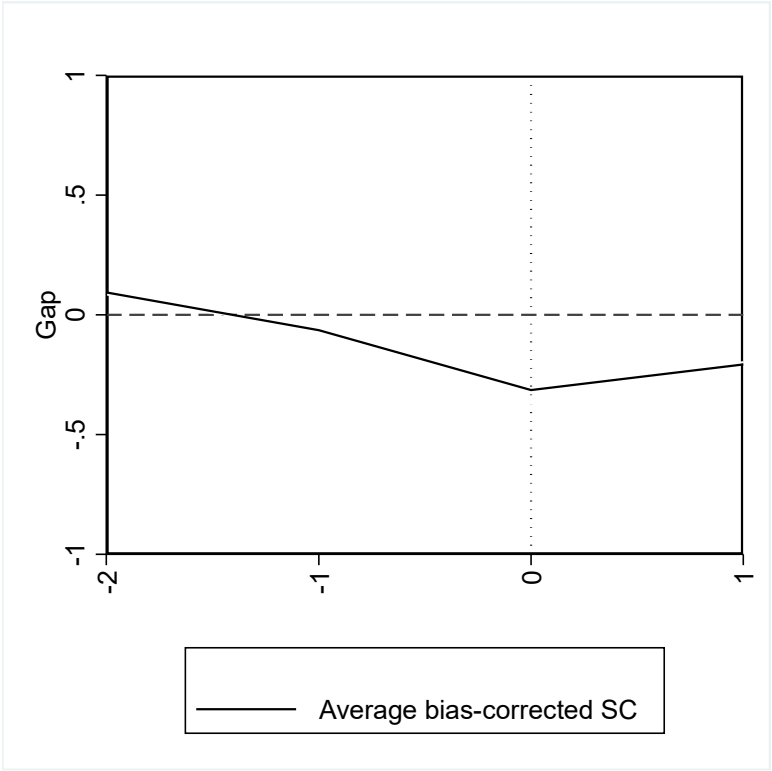
Notes: The figure reports the average gap (i.e. the treatment effect) of a *reduction* in employee representation protection in six European countries using the *allsynth* command in Stata 16 for the synthetic control case (Wiltshire, 2024). The set of donor (i.e., control) units contains eighteen countries. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively. The variables sector, size, and linear combinations (in time) of the incentive management score are the selected predictors for the pre-treatment outcome.

Figure 2. The Treatment Effect on the Overall Management Practices Score of a *Reduction* in Employee Representation Protection, Synthetic Control (SC) Case



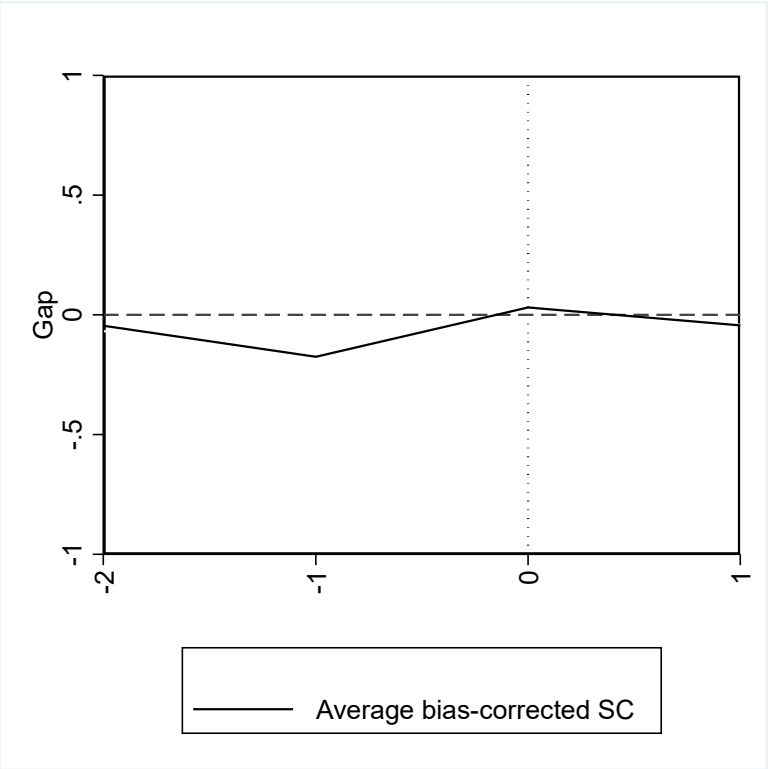
Notes: See notes to Figure 1. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively. The variables sector, size, and linear combinations (in time) of the overall management score are the selected predictors for the pre-treatment outcome.

Figure 3. The Treatment Effect on the Incentive Management Score of an *Increase* in Employee Representation Protection, Synthetic Control (SC) Case



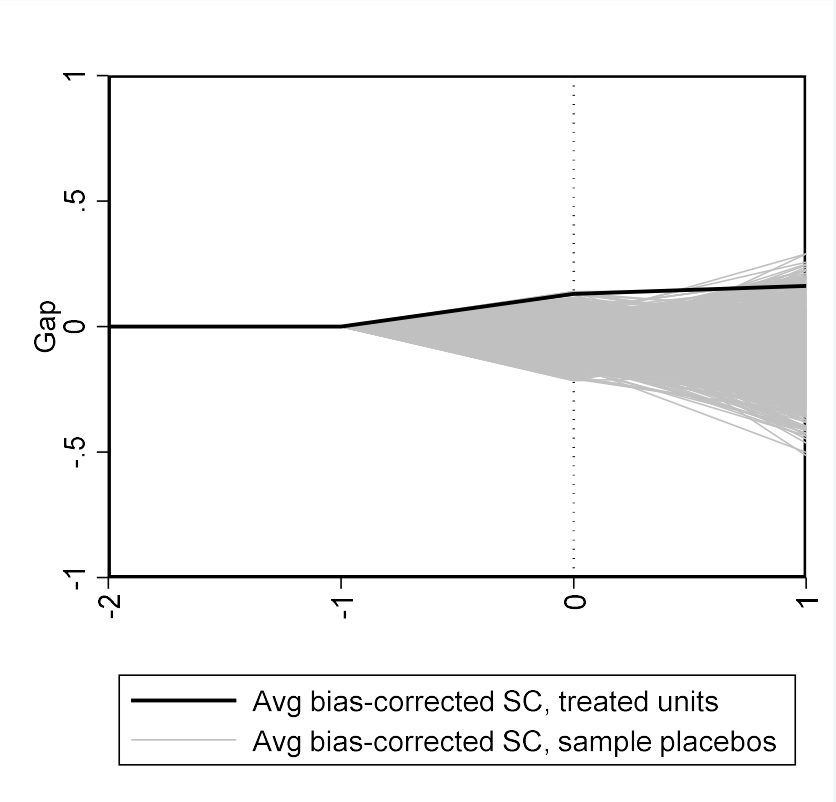
Notes: The figure reports the average gap (i.e., the treatment effect) of an *increase* in employee representation protection in three European countries using the *allsynth* command in Stata 16 for the synthetic control case (Wiltshire, 2024). The set of donor (i.e., control) units contains nineteen countries. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively. The variables sector and size are the selected predictors for the pre-treatment outcome.

Figure 4. The Treatment Effect on the Overall Management Score of an *Increase* in Employee Representation Protection, Synthetic Control (SC) Case



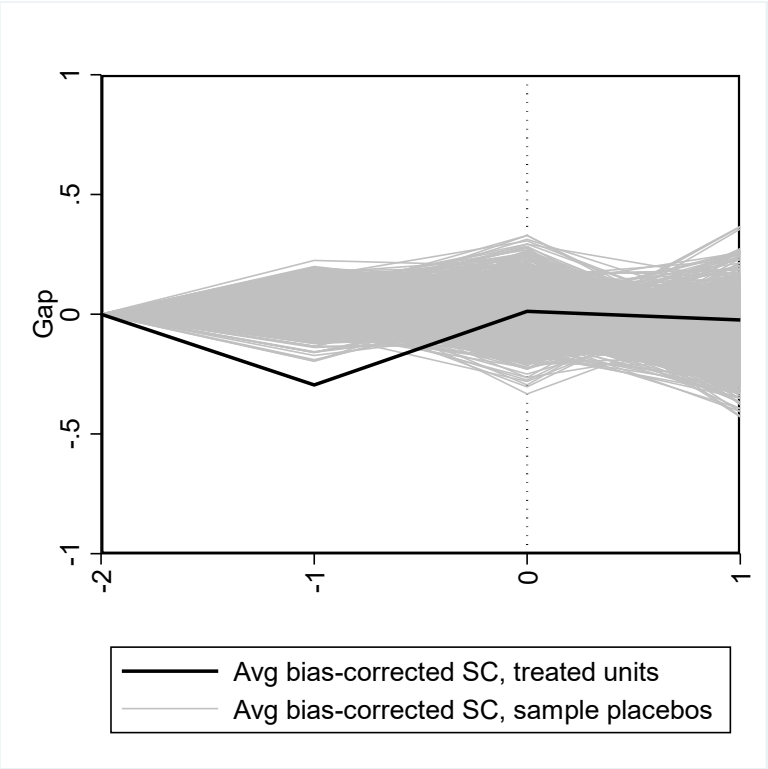
Notes: See notes to Figure 3. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively. The variables sector and size are the selected predictors for the pre-treatment outcome.

Figure 5: The Average Gap (Treatment Effect) for Treated Units of a *Reduction* in Employee Representation Protection on the Incentive Management Score and the Corresponding Average Placebo (or Falsification) Gap, Synthetic Control (SC) Case



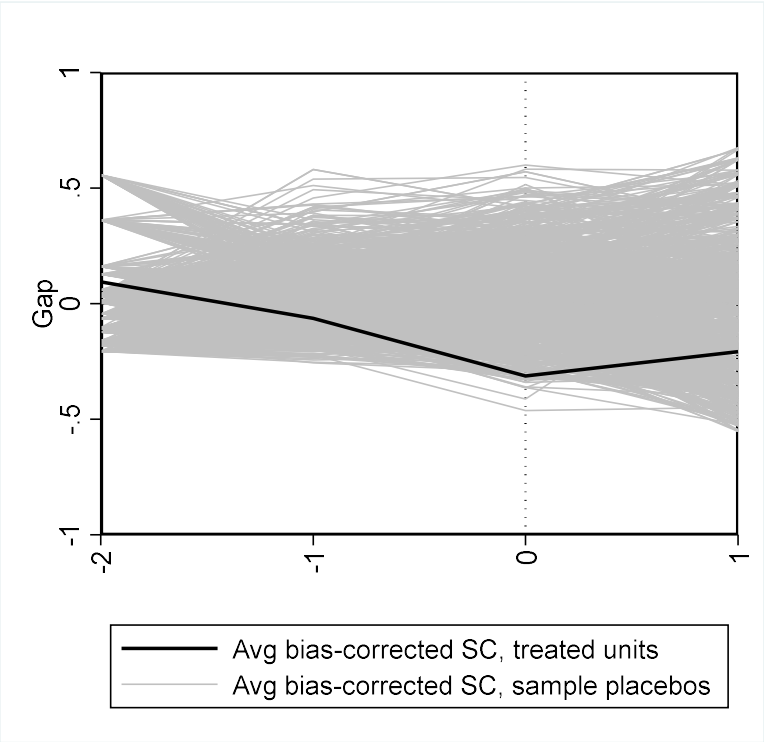
Notes: The figure plots the average gap for treated units (i.e. the treatment effect) and the average placebo gap of a *reduction* in employee representation protection in six European countries using *allsynth* command in Stata 16 for the synthetic control case (Wiltshire, 2024). The average gap for treated units is the same as in Figure 1; the average placebo gap is based on 1,000 randomly sampled placebo averages. The set of donor (i.e., control) units contains eighteen countries. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively. The specification is the same as in Figure 1, with sector, size, and linear combinations (in time) of the incentive management score included in the specified predictors.

Figure 6: The Average Gap (Treatment Effect) for Treated Units of a *Reduction* in Employee Representation Protection on the Overall Management Score and the Corresponding Average Placebo (or Falsification) Gap, Synthetic Control (SC) case



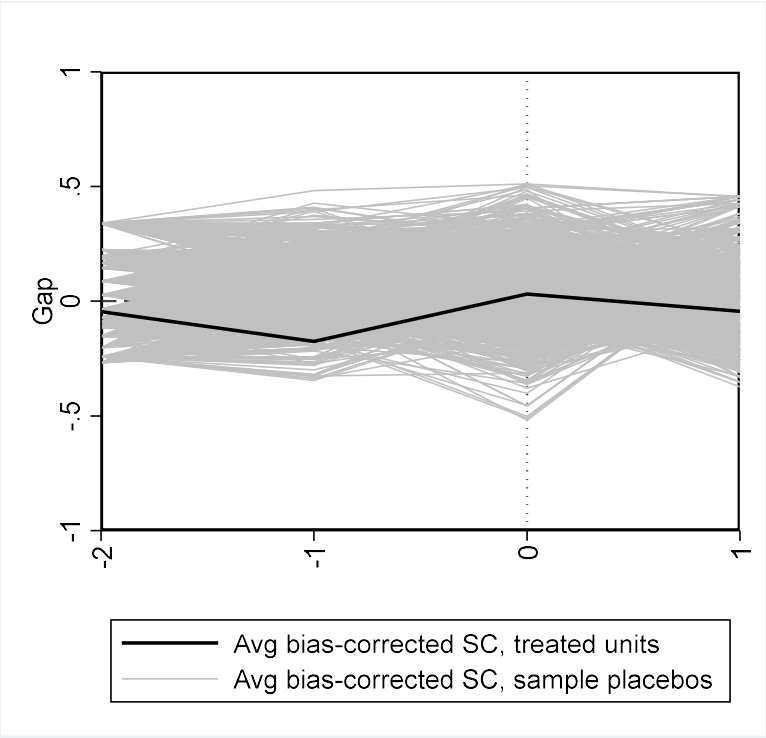
Notes: See notes to Figure 5. The average gap for treated units is the same as in Figure 2; the average placebo gap is based on 1,000 randomly sampled placebo averages. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively.

Figure 7: The Average Gap (Treatment Effect) for Treated Units of an *Increase* in Employee Representation Protection on the Incentive Management Practices Score and the Corresponding Average Placebo Gap, Synthetic Control (SC) Case



Notes: The figure plots the average gap for treated units (i.e. the treatment effect) and the average placebo gap of an *increase* in Employee Representation Protection in three European countries using *allsynth* command in Stata 16 for the synthetic control case (Wiltshire, 2024). The average gap for treated units is the same as in Figure 3; the average placebo gap is based on 1,000 randomly sampled placebo averages. The set of donor (i.e., control) units contains nineteen countries. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively. The specification is the same as in Figure 3, with sector and size included in the specified predictors.

Figure 8: The Average Gap (Treatment Effect) for Treated Units of an *Increase* in Employee Representation Protection on the Overall Management Practices Score and the Corresponding Average Placebo Gap, Synthetic Control (SC) Case



Notes: See notes to Figure 7. The average gap for treated units is the same as in Figure 4; the average placebo gap is based on 1,000 randomly sampled placebo averages. The values -2, and -1 in the x-axis denote the second and first periods before treatment, respectively, while 0 and 1 denote the first and second periods after treatment, respectively.

Appendix Table 1. Description of the Selected Management Practices, 2009, 2013, and 2019

Year	Domain	Practice	Survey question	Description
2019	Monitoring (2 items)	Uses of data analytics to monitor employee performance	Q23 Q26	0-1 ordinal variable in ascending order: 1 if the establishment uses data analytics to monitor employee performance. 0-1 ordinal variable in ascending order: 1 if managers create an environment in which employees can autonomously carry out their tasks.
	Incentives/performance-based pay linked to individual or team performance (3 items)	Payment by results Extra pay linked to individual performance Extra pay linked to team performance	Q46	0-3 ordinal variable in ascending order in which, for each item, the respondent gives, in an ordered 0 (none at all) to 3 (all) scale, the percentage of employees at the establishment receiving the corresponding type of variable pay: 0 if the percentage of employees is 0%; 1 if less than 20%; 2 if 20 to 39%; 3 if equal to or greater than 40%.
2013	Performance appraisal (1 item)	Performance appraisal	H9	0-3 ordinal variable in ascending order: 0 if the percentage of employees who have a performance appraisal or evaluation interview at least once a year is 0%; 1 if less than 20%; 2 if 20 to 39%; 3 if equal to or greater than 40%.
	Incentives/performance-based pay linked to individual or team performance (3 items)	Payment by results Extra pay linked to individual performance Extra pay linked to team performance	H23	0-1 ordinal variable in ascending order, 1 if the corresponding practice has been available in the establishment.
2009	Incentives/performance-based pay linked to individual or team performance (2 items)	Pay linked to individual performance Pay linked to team performance	MM455 MM456	0-3 ordinal variable in ascending order: 0 if the percentage of employees who have pay linked to individual performance (team performance) is 0%; 1 if less than 20%; 2 if 20 to 39%; 3 if equal to or greater than 40%.
	Other schemes (2 items)	Profit sharing Ownership scheme	MM460 MM463	0-1 ordinal variable in ascending order: 1 if there is any profit sharing (ownership scheme) offered in the establishment.

Notes: A detailed description of the variables for 2013 and 2019 can be found in Addison and Teixeira (2025); for 2009 the selected variables were derived from the ECS dataset described in Addison and Teixeira (2020).
Sources: 2009, 2013, and 2019 European Company Survey, Management Questionnaire.

Appendix Table 2a. Country-level Reduction in Protection of Employee Representation Between 2009 and 2019

Country	Year	Description of the legislative change in Area D (Employee representation, items 25-31 of the CBR Labour Regulation Index)
France		
Item 28 Extension of collective agreements	1970: 1 2016: 0.75	Measures providing for extension of sector-level agreements date back to 1936, and were strengthened in 1950 and again in 1982 as part of the Auroux Laws: LC Art. L.133-1, now LC Art. L. 2261-19. With effect from 2016 the El Khomri law (loi 2016-1088) allows increased scope for company level agreements to derogate from sectoral agreements, although not on (inter alia) wage rates, job classifications and equality issues).
		We made the assumption that the 2013 change in item 30 (Codetermination: board membership), which extended board membership rules to all companies with at least 5,000 employees is not a sizable change in the sense it only has an impact on a very restricted set of very large firms.
		No changes are reported in the other items of Area D. Summary of our coding for France: 2019 is the treatment year; 2009 and 2013 are the pre-treatment years.
Spain		
Item 27 Duty to bargain	1970: 1 1973: 1 2011: 0.67	Law 99/1959, Art. 8: implicit duty to bargain as terms could be declared binding by the General Directorate of Labour if either side failed to attend negotiations. Law 38/1973, Art. 12, Workers' Statute, Art. 89: obligation to bargain in good faith. Inter-confederal agreement of 6 February 2011 and Decree 3/2012: opt-out for firms suffering persistent drop in revenues.
Item 28 Extension of collective agreements	1970: 0.5 1973: 0 1980: 1 2011: 0.67 2012: 0.5	Law 99/1958: extension possible on the application of employers and unions concerned. Law 38/573: collective agreements applicable either to an individual undertaking, a group of employers, all undertakings subject to specific labour law regulations, or all employers and unions in a particular region. Workers' Statute 1980, Ch. 1, Div. 1: collective agreements can bind at national and inter-occupational level, as well as at local or firm level; Art. 92, extension possible by ministerial order. Degree 7/2011: encouragement of company level agreements over sector bargaining. Decree 3/2012: company level agreements given priority over sectoral agreements. Decree 32/2021: restores priority of sectoral agreements over basic wages and salaries.
		No changes are reported in the other items of Area D. Summary of our coding for Spain: 2013 and 2019 are the treatment years; 2009 is the pre-treatment year.
United Kingdom		
Item 31 Codetermination and information/consultation of workers	1970: 0 1976: 0.33 2000: 0.5 2013: 0.33	There is no legal requirement for works councils or similar standing bodies in the UK. From 1976 information and consultation requirements were introduced for collective redundancies (Employment Protection Act 1975) and from 1981 for business transfers. Information and consultation obligations were extended from 1999 for transnational companies required to have European Works Councils (SI 1999/3233, implementing Directive 94/45/EC) and from 2004 for other companies above a certain size threshold (SI 2004/3426, implementing Directive 2002/14/EC; the threshold was reduced in 2020). However, in both cases, particularly the latter, considerable flexibility was accorded to employers in meeting these obligations, and the bodies concerned do not have co-decision making powers. From 2013 the content of the obligation to inform and consult over collective redundancies was reduced (Enterprise and Regulatory Reform Act 2013).
		No changes are reported in the other items of Area D.

		Summary of our coding for the United Kingdom: 2019 is the treatment year; 2009 and 2013 are the pre-treatment years.
Slovakia		
Item 28 Extension of collective agreements	1993: 1 2004: 0.33 2007: 1 2011: 0.33	CBA-Collective Bargaining Act No.1/1991 provided for extension of collective agreements without the consent of the employer. From 2004, under Act No.585/2004 a collective agreement could bind a non-member employer only with his/her consent. In 2007, Act No. 328/2007 removed this requirement, and Act No. 557/2010 re-introduced it.
		No changes are reported in the other items of Area D. (Item 31/codetermination is assumed invariant. The small changes in opposite directions in 2011 and 2013 are therefore taken as negligible.) Summary of our coding for Slovakia: 2013 and 2019 are the treatment years; 2009 is the pre-treatment year.
Romania		
Item 28 Extension of collective agreements	1990: 1 2011: 0	Law 130/1996, Art. 26(1)(c): collective agreements conducted at sector level by representative parties were binding on all enterprises in the sector concerned. The 2011 Law on Social Dialogue now states that collective agreements are binding only on those actually represented in the process.
		No changes are reported in the other items of Area D. Summary of our coding for Romania: 2013 and 2019 are the treatment years; 2009 is the pre-treatment year.
Czechia		
Item 30 Codetermination: board membership	1990: 1 2014: 0.25	Employees have the right to elect one third of the members of the supervisory board in public limited companies with more than 50 employees and in state-owned companies. From 1.1.14 employee board membership was no longer compulsory in private sector companies.
		No changes are reported in the other items of Area D. Summary of our coding for Czechia: 2019 is the treatment year; 2009 and 2013 are the pre-treatment years.

Notes: The table documents the introduction of country-level legislative changes that have decreased the level of protection of employee representation. The information is taken from the CBR Labour Regulation Index file and corresponding Appendix 1-CBR-LRI Coding Protocols and Appendix 2-Country Tables, items/variables 25-31 of Area D (Employee representation). Items 25-31 are described as follows: 25–Right to unionization; 26–Right to collective bargaining; 27–Duty to bargain; 28–Extension of collective agreements; 29–Closed shops; 30–Codetermination (board membership); and 31–Codetermination and information/consultation of workers.
Source: Adams et al. (2023), CBR Labour Regulation Index, Centre for Business Research, University of Cambridge, U.K. Version December 2023, https://www.jbs.cam.ac.uk/centres/business-research-cbr/

Appendix Table 2b. Country-level Increase in Protection of Employee Representation Between 2009 and 2019

Country	Year	Description of the legislative change in Area D (Employee representation, items 25-31 of the CBR Labour Regulation Index)
Hungary		
Item 26 Right to collective bargaining	1990: 0 2012: 1	There was no provision in the 1949 Constitution for collective bargaining. Now under Art. 17(2) 2011 the Constitution employees, employers and their representative bodies have a statutory right to bargain and conclude collective agreements, and to take any joint action or hold strikes in defense of their interests. Item 31-Codetermination and information/consultation of workers is assumed as invariant in practice.
		No changes are reported in the other items of Area D. Summary of our coding for Hungary: 2013 and 2019 are the treatment years; 2009 is the pre-treatment year.
Cyprus		
Item 27 Duty to bargain	1970: 0.5 2012: 1	Before 2012, collective bargaining was widely observed in practice under the terms of the non-binding Industrial Relations Code. Under Law 55 2012 an employer is under a legal duty to bargain once an order to this effect has been issued by the court.
Item 28 Extension of collective agreements	1970: 0 2013: 1	In February 2013 a law was approved to enable the ministerial extension of sector-level agreements by order. No changes are reported in all the other items.
		No changes are reported in the other items of Area D. Summary of our coding for Cyprus: 2013 and 2019 are the treatment years; 2009 is the pre-treatment year.
Ireland		
Item 27 Duty to bargain	1970: 0 2015: 0.5	There is no duty on an employer to recognize or bargain with a union: *Abbott and Whelan v ITGWU and the Southern Health Board* (1982) 1 JISLL 56. The Industrial Relations (Amendment) Act 2001 gives the Labour Court the power to issue a legally binding ruling on pay and conditions of employment when an employer refuses to recognize a trade union but still does not require the employer to recognize a trade union or unions or to negotiate with them. Under the Industrial Relations (Amendment) Act 2015 while there is still no duty to bargain as such, the Labour Court is given enhanced powers to impose terms and conditions where the employer refuses to engage in collective bargaining. Change in item 28 on the extension of collective agreements was short-lived, and for that reason is ignored here.
		No changes are reported in the other items of Area D. Summary of our coding for Ireland: 2019 is the treatment year; 2009 and 2013 are the pre-treatment years.

Note: See notes to Appendix Table 1.
Source: Adams et al. (2023), CBR Labour Regulation Index, Centre for Business Research, University of Cambridge, U.K. Version December 2023, https://www.jbs.cam.ac.uk/centres/business-research-cbr/