

### Works Councils and Employment Growth: A Rejoinder to Uwe Jirjahn's Critique

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**Works Councils and Employment Growth:  
A Rejoinder to Uwe Jirjahn's Critique\*\***

**Abstract** – In a characteristically combative treatment, Jirjahn (2008a) argues that Addison and Teixeira's (2006) finding of a negative relationship between works council presence and employment growth is a chimera produced by the way in which establishment size is measured. We reject his assertion of misspecification for two reasons; the second of which undoubtedly contributed to leading Jirjahn astray. And while Jirjahn's treatment is of interest in its own right, he does a poor job of portraying our *overall* analysis. Thus, he neglects our treatment of survival bias while ignoring our presentation of a dynamic labor demand model. Elsewhere he seems to grudgingly support the former (Jirjahn 2008b), and implicitly to accept our findings pertaining to employment adjustment (where we report that works councils do not slow the tortuous pace of employment adjustment in Germany). At root, the thrust of his treatment is adversarial and his position on the economic effects of works councils over-optimistic. But the main lesson of Jirjahn's critique is that more work is required of all of us in this area. Issues raised by the present exchange, apart from the need for a wider set of covariates and longer time frame, include the selection of firms *into* collective bargaining and works councils and *out of* the system, and the consequences for the raw point estimates. Pending this work, it would be idle to overstate the robustness of the extant results. We hinted at this in our own treatment in comparing cross-sectional results with dynamic panel estimates.

**Betriebsräte und Beschäftigungswachstum:  
Eine Antwort auf Uwe Jirjahns Kritik**

**Zusammenfassung** – Jirjahn (2008a) argumentiert, dass der Befund von Addison und Teixeira (2006), nach dem ein negativer Zusammenhang zwischen der Existenz von Betriebsräten und dem Beschäftigungswachstum vorliege, ein methodisches Artefakt sei, welches aus der Art der Messung der Betriebsgröße resultiere. Wir widersprechen dieser These mit zwei zentralen Argumenten. Obwohl Jirjahns Argumentation für sich genommen interessant ist, spiegelt sie unsere Analyse insgesamt nicht vollständig wider, weil unsere Behandlung des „survival bias“ vernachlässigt und unsere Präsentation eines dynamischen Arbeitsnachfragemodells nicht zur Kenntnis genommen wird. Die Hauptbotschaft von Jirjahns Kritik ist, dass mehr Forschung notwendig ist, u.a. mit mehr Kontrollvariablen, längeren Zeithorizonten und zur Selektion der Unternehmen, in denen ein Betriebsrat gegründet oder wiederabgeschafft wird. Bis hierzu weitere Ergebnisse vorliegen, muss die Robustheit der schon vorliegenden Ergebnisse vorsichtig bewertet werden.

Key words: **Works Councils, Employment Growth, Employment Adjustment,  
Survival Bias**

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## 1. Introduction

The effect of worker representation on employment growth has long been examined in the Anglo-Saxon literature. Contrary to their effect on some other indicators of firm performance, there has been comparatively little disputation over the effect of *unions* on employment. Thus, the British (Blanchflower/Millward/Oswald 1991; Booth/McCulloch 1999; Bryson 2004a; Addison/Belfield 2004), Australian (Wooden/Hawke 2000), and U.S. (Leonard 1992) evidence all points to reduced employment growth under unionism.<sup>1</sup> Indeed, one can go further: the literature suggests that unionized establishments tend to grow by around 3 percent less per year than similar unorganized plants. In other words, a near common result obtains holding constant the concentration of unions in declining industries and older workplaces, *inter al.*

Now works councils are not unions. Formally at least works councils do not bargain over wages, while the (offsetting) efficiency or voice arguments often made for unionism are theoretically stronger in the case of works councils. In sum, we should not necessarily expect to observe similar effects at workplace level of worker representation: workplace unions in the Anglo-Saxon case and works councils in the German case. And if Bryson's (2004a) findings with respect to the important contributory role of bargaining power to the employment change outcome are correct, then we might expect very different results for German works councils than for their Anglo-Saxon 'counterparts' in favor of the former.

This, then, is the wider backdrop to the inquiry by Addison and Teixeira (2006) into the effects of works councils on employment change, the direct motivation being the virtual absence of any comprehensive German study on the works council-employment nexus other than (disputation as to their implications for) labor turnover (e.g. Kraft 1986; Frick 1996; see also Frick/Möller 2003; Kraft 2006). In the light of what follows, we would also emphasize that our study proceeded several steps beyond a simple analysis of employment change to incorporate allowance for survival bias while also presenting a dynamic labor demand model charting works council effects on the speed of employment adjustment and, now less centrally, on employment growth.

The paper by Jirjahn (2008a) focuses solely on the specification of the simple employment change model and ignores (here at least) those other aspects of our treatment. His principal conclusion is that our test procedures are flawed because of the misspecification of the employment size regressor. And from the outset, he nails

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<sup>1</sup> Admittedly, some minor controversy was occasioned in the early British literature by Machin and Wadhvani's (1991) claim that union plants were at this time more likely to be shedding labor on efficiency grounds. Their argument was that unionized firms in the early-to-mid 1980s were more likely to see an erosion of restrictive practices (allied to overmanning practices and demarcation). But Booth and McCulloch (1999) were later to report that the union effect on employment was indeed robust to the inclusion of organizational change measures. That said, while rebutting the Machin-Wadhvani challenge, this debate alerts us to the important consideration that employment effects are not an unambiguous measure of performance and have to be viewed alongside the totality of union effects on economic outcomes.

his flag to the mast. First of all, he appears to be astonished that one can obtain such negative results on the basis of the wider works council literature that points to “neutral-to-positive effects of codetermination on economic outcomes” (he is presumably referring to workplace codetermination rather than board membership; otherwise, we feel entitled to claim that he would find the extant financial empirical literature even harder to bear [e.g. Gorton/Schmid 2004]). Subject to the caveat that employment growth is an elusive measure of efficiency, we think this reading of the wider evidence is a tad partisan (for a survey, see Addison/Schnabel/Wagner 2004). Second of all, he concludes with some results of an unpublished study of his (Jirjahn 2008b) to the effect that works councils actually *increase* employment once one allows for the endogeneity of the institution. Frankly, most observers will view this as a more astonishing claim. Less adversarially put, it is incumbent upon him to establish the point using the present dataset or at least to emphasize that the results are from a study covering a different dataset, a different sector (manufacturing) and time interval (1994-1997), and to acknowledge the well-known difficulty of accounting for the endogeneity of the institution which is accentuated in this temporal frame.

## 2. The misspecification charge

Jirjahn’s basic criticism is that our finding of a negative relationship between works council presence and employment growth is a chimera produced by the way in which we measure establishment (employment) size. To illustrate he runs three employment growth regressions each of which contains a variant of establishment size (fashioned from level of base-year employment). In the first, establishment size is measured linearly; in the second, it enters as a quadratic; and in the third it assumes a proportional logarithmic form. For the first two equations the coefficient estimate for the works council dummy variable is negative and statistically significant at conventional levels. But in the third equation although the coefficient estimate retains its negative sign it fails to achieve statistical significance. Moreover, this selfsame pattern is repeated for different representations of the data (plants in which works council status – either presence or absence – was unchanged over the entire sample period, and plants with between 21 and 100 employees) and the dependent variable (with growth measured as the difference in log employment rather than absolute employment change divided by one half the sum of employment at beginning and end of period).

However, to argue that our result is the result of specification error on this account would seem to rest on some pretty strong assumptions. Apart from the fact that like is not strictly being compared with like, this accusation would seem to rest on the notion that Gibrat’s law is contraindicated. Now while mean reversion might seem to be suggested by Jirjahn’s finding of a statistically significant negative coefficient estimate for  $\ln$  employment size the fact of the matter is that his employment change equation is not a test of Gibrat’s law (any more than our own). A proper test would presumably use panel unit root tests and not just an employment change equation (see for example, Harris/Trainor 2005). To repeat, the employment growth equations used in the two studies do not constitute a test of the principle that each firm faces the same distribution of growth possibilities so that growth follows a random walk – on the rejection of which Jirjahn’s charge of misspecification ultimately rests

What does the German *evidence* on employment change actually suggest in this regard? It does not appear to indicate that the relation between firm size and firm growth is decreasing in employment size. Thus, for example, in an early study using manufacturing establishment data for Lower Saxony, 1978-89, Wagner (1992) reports that there is no indication that smaller manufacturing firms grew systematically faster or slower than their larger counterparts over the sample period, even if there were some signs of 'persistence of chance' in the data. Lest it be thought that these results are peculiar to this *Land* and sector, Wagner et al. (2008) reach a broadly similar conclusion using nationally representative data for the industrial and service sectors, 1999-2005. Specifically, they conclude that there is little to support the popular notion that small and middle-sized enterprises serve as the 'job-motor' of the economy.

At this point, we have to admit to an error of omission on our side that escaped identification in the proof-reading stage: our establishment size proxy was in fact measured in logarithmic form. Jirjahn was not to know this and we commend him for experimenting with several variants of establishment size based on employment. That said, if we led him astray we would nonetheless contend from the general tenor of his response that he was eager to be led.

### 3. Housekeeping

We next deal with some points of housekeeping before turning to examine those parts of our analysis conspicuously ignored by Jirjahn. Let us start with questions having to do with sample size before turning to the covariates, even if the two issues are related. Jirjahn's first set of estimates are for plants on which he has the necessary *employment* data at beginning and end of period. As far as works council status is concerned, he is at this stage only interested in beginning-of-period values. (For our part, we were concerned to have these data on firms for all years given our subsequent analysis of annual employment changes; see section 5 below).<sup>2</sup> As a result there are some major sample differences between our two studies: 906 in the case of Jirjahn's initial sample (see his Table 2) and 600 in our case. To be sure, Jirjahn next presents results (his Table 3) for plants that had the same works council status at beginning and end of period as a result of which his sample falls by 22.2 percent to 705 establishments. As he notes, this difference is considerably larger than the number of changes in works council status. Although his main finding is undisturbed by this precipitous drop in sample size, the basic point is that the size of our two samples differ. Sample sizes can matter a lot because of differences in their composition. Note that employment growth rates differ markedly for our two samples: -5.5 percent for Addison and Teixeira and -18 percent for Jirjahn.

Sample size also differs because of (missing data associated with) differences in variables. We do not wish to nickel and dime Jirjahn on his choice of variables, some of which we find rather compelling. He eschews our choice of regressors largely on the grounds that they proved insignificant – as indeed might his for a different sample – but he pauses long enough to take a swipe at our use of an average wage argument. He reasons that it is endogenous and likely to be a function of works council influ-

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<sup>2</sup> Though we excised the small number of firms who changed their works council status.

ence. Although we intended the variable to reflect general insider power, and were aiming at consistency with our dynamic labor demand analysis, we take his point. As a check, did he substitute the wage variable for the works council dummy in any specifications?

Finally, Jirjahn criticizes our exclusion of agriculture and forestry, hunting and fishing on the one hand and banking and insurance on the other. To mix metaphors, this seems more a case of carping. It is fairly conventional to exclude agriculture etc. since most of these sectors are populated by family businesses, with obvious implications for works council formation. We exclude banking and finance because of differences in output measures: output in the form of sales is required for the second part of the paper dealing with dynamic labor demand and we wanted to maintain consistency between the two parts of the paper. This leaves the question of our use of five industry dummies; Jirjahn uses nine (as well as nine dummies for the old Federal states/city states). We were more parsimonious to sidestep problems raised by changes in industry classification.

The bottom line for us is that since our two samples differ considerably we are not necessarily surprised that Jirjahn comes up with different results for the principal variable of interest. The more surprisingly result is perhaps that the influence of the work council is so well determined in specifications using establishment size in linear or quadratic form. Intrinsically we see little to criticize in Jirjahn's choice of variables. But it is perhaps no accident that antagonists in the early British debate over the role of unions in retarding employment growth pursued that debate using very different specifications.

#### 4. Survival bias

An important part of the Addison and Teixeira (2006) paper was an analysis of survival bias. There is no mention of this in Jirjahn's critique. His treatment is based exclusively on a sample of surviving establishments. As is well known, this raises a potential selection issue. If works councils increase the probability of workplace closure, any negative impact that they are found to have on employment growth may be understated. How sensitive are his results to this problem? We are not told.

It was long thought in the Anglo-Saxon literature that although union rent seeking might well limit employment growth, unions would not push firms over the edge (e.g. Freeman/Kleiner 1999: 512). The early British literature using the Workplace Industrial Relations Survey seemed to confirm this prediction: there was no indication that either union bargaining presence or the predicted mean union wage differential influenced plant closures (Machin 1995; Stewart 1995). The more recent evidence points in a quite different direction: the sign of the coefficient estimate using the 1998 Workplace Employment Relations Survey is both positive and well determined. But there is no simple explanation for this sea change in union effect given that union power was *in decline* over the interval covered by the two sets of studies.<sup>3</sup>

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<sup>3</sup> Bryson (2004b) now contends that the reduction in union bargaining power was responsible for the worsened outcome. His argument is that unions have to be strong to be an effective vehicle of pro-productive voice *and* to act as an authoritative agent of the em-

Given this body of evidence, Addison and Teixeira (2006) offer two tests of survival bias. On balance, they conclude that failing to account for plant closings most probably does *not* lead to an understatement of the negative effect of works councils on employment change. We are surprised that Jirjahn did not attempt a similar set of tests for potential survivor bias. Elsewhere at least he seems content to rely on the results of our test procedures (Jirjahn 2008b: 22).

### **5. The tortuous path of employment adjustment in Germany**

In the final part of our paper we discussed another aspect of works council potential influence. We estimated a dynamic labor demand model to determine whether the process of labor demand *adjustment* at micro level is sensitive to the presence of the institution. In short, do works councils impact employment inertia? To this end we exploited the panel nature of the data set and no longer just two cross sections of data. (Note, too, that as a secondary output our estimating equation allows us to recoup the effect of works councils on employment growth by interacting the works council dummy with a time trend.) Since Jirjahn does not inform the reader, what do we find? First, we report that works councils seemingly do not exacerbate the already tortuous path of employment adjustment in Germany. They do not, then, actually add to the sluggishness of the employment adjustment process. This is a nontrivial result given the powers of the works council in the area of dismissals and manpower adjustment more generally. Second, although negatively signed, the coefficient estimate for the interaction between the works council dummy and the time trend is not statistically significant. On this measure, then, works council do not materially effect any trend growth in employment, even though we would (and did) argue that the issue of the growth differential between works council plants and non works council plants is best tackled using a wider change interval than is permitted by this short panel.

### **6. Concluding remarks**

In his article on works councils and employment growth, Jirjahn (2008a) uses our paper as a something of a whipping boy to make the case that works councils do not act as a 'brake' on employment, a result which he finds in accord with the wider literature on the economic effects of works councils. We argue to the contrary that the thrust of his particular criticism is wide of the mark and that he unreservedly views works councils through rose-tinted lenses. We were no less concerned that he inadequately represents our overall analysis.

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ployer principal. *Vulgo*: the Thatcher legislation so weakened unions that they could neither deliver voice nor police the worker side. For their part, Addison, Heywood, and Wei (2003) report that the positive effect on plant closings only holds for establishments that are part of multi-establishment undertakings. In these cases, the decline in union power likely emboldened employers to close unprofitable units. Unions, so the argument goes, had hitherto kept open unprofitable plants by threatening to strike profitable ones. The new need to ballot members under Tory legislation destroyed the credibility of this mechanism because workers whose jobs were *not* in danger jeopardy would not vote for a strike.

The accusation of misspecification requires a refutation of Gibrat's law. Jirjahn nowhere provides the requisite evidence on mean reversion, while the admittedly sparse *German* literature if anything might suggest that firm growth rates are independent of firm size. This does not mean that Jirjahn's own works council result is flawed, merely that it is not right and ours wrong for the reason he gives: misspecification.

Our charge of partiality rests in part on Jirjahn's interpretation of the wider evidence and more substantially on his failure to consider the important components of our paper examining whether the presence of survival bias understates any negative effect of works councils on growth and whether works council presence slows the process of employment adjustment in Germany. Let us remind the reader that we detected few signs of survival bias in the data and even less indication that works councils slowed the process of employment adjustment. We even noted – subject to caveats – that dynamic employment equations do not support the finding in the first half of our paper that works councils retard employment growth. Jirjahn totally neglects these aspects of our paper in what is clearly an adversarial treatment.

He does however react to material in our paper that at first blush at least gores one of his oxen: unfavourable works council effects on employment that are if anything seemingly slightly amplified in the presence of collective bargaining proper. This runs counter of course to the argument in Hübler and Jirjahn (2003) that any works council rent seeking should be more contained under (industry-wide) collective bargaining with the implication that any negative employment effects should be mitigated in such circumstances. We made light of *our* result in our paper, consigning the results to the appendices. Specifically, we discounted the marginally significant estimates in ultimately concluding more ecumenically that the employment effects of works councils were likely invariant with respect to collective bargaining regime. Much more attention is required on the works council-collective agreement nexus than we were able to give it, not least because of (the ability to exploit) changes in collective bargaining status. Why did Jirjahn himself not exploit the collective bargaining question in the dataset? Was it because the 1993 questionnaire does not contain this information, as he correctly if acidly observes. Perhaps. In any event, let us assure him that we did not rely on interpolation and that the sample period of the appendix regressions in question is 1995-2001 rather than 1993-2001.<sup>4</sup>

We end with one such criticism of our own. In his very concluding remark, Jirjahn reminds us that the key variable of interest – works council presence or otherwise – is itself endogenous. He argues that once one allows for the fact that works councils are seemingly established by the workforce to protect its interests when the establishment is *in extremis*, the effect on employment is in fact significantly positive! But can this result be generalized to the IAB Establishment Panel, when it is based on regressions using three years of employment change data from one *Land* and the manufacturing sector alone (Jirjahn 2008b)? In fact, it is notoriously difficult to endogenize

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<sup>4</sup> We might also note that the sample sizes in Appendix Tables A1 and A2 were incorrectly stated. In Table A1 they should have read: 221, 221, 1,075 and 1,075 (rather than 232, 232, 1,105, and 1,105). Similarly, the correct number of observations in columns (1) and (4) of Table A2 is 138 and 581 (not 143 and 600).



works councils – principally because so few establishments change their status through time.<sup>5</sup> This does not mean that one should not try to correct for this form of selection but, rather, be very careful in drawing inferences about net works council effects based on particular (IV) estimates that ignore other forms of selection into and out of the system.

We did our best to offer a well rounded study of the effects of works councils upon employment change and adjustment. We see the need to improve our analysis along a number of dimensions including new covariates (including some of those suggested by Jirjahn and other subsequent studies including our own) and exploiting the longer time periods now available with the evolution of the IAB Establishment Panel. We remain open to the possibility that our point estimates may not be robust to time period, sample, and control for selection. So, too, should Jirjahn. He might usefully be reminded of the old Anglo-(if not Nieder-) Saxon adage: people who live in glass houses shouldn't throw [too many] stones. Or, if we might adapt a German saying: one revisits every equation at least once.

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<sup>5</sup> For a summary of a matching model deployed by one of the present authors based on works council innovations/adoptions using the IAB Establishment Panel, 1996-2000, and examining four performance outcomes, see Addison, Schnabel, and Wagner (2004, p. 274 et seq.). This particular study of works council formation fails to detect differences in employment growth between the 31 innovating establishments and their matched noninnovating counterparts.

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