

## Works Councils and Employment Growth: A Reply to Addison and Teixeira

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Veröffentlichungsversion / Published Version  
Replik / replication

Zur Verfügung gestellt in Kooperation mit / provided in cooperation with:  
Rainer Hampp Verlag

### Empfohlene Zitierung / Suggested Citation:

Jirjahn, U. (2008). Works Councils and Employment Growth: A Reply to Addison and Teixeira. *Industrielle Beziehungen : Zeitschrift für Arbeit, Organisation und Management*, 15(4), 436-448. <https://nbn-resolving.org/urn:nbn:de:0168-ssoar-353219>

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Uwe Jirjahn\*

## **Works Councils and Employment Growth: A Reply to Addison and Teixeira\*\***

**Abstract** - In this reply, I argue that Addison and Teixeira's rejoinder to my study is characteristically misleading and erroneous. While the authors agree that my investigation is interesting in its own right, they doubt that my findings indicate misspecification. But they ignore almost all of the German studies on employment growth. While Addison and Teixeira admit several serious mistakes in their tables, they do a poor job in comparing their paper with my study. Basically, they compare apples with oranges. Addison and Teixeira complain that I do not appreciate the overall contribution of their analysis. Yet, this simply distracts from the fundamental issue. Finally, the authors come up with personal allegations and attacks that are far away from good scientific style. In their attempt to discredit my study, Addison and Teixeira do even not provide correct citations of what I wrote.

## **Betriebsräte und Beschäftigungswachstum: Eine Replik auf Addison und Teixeira**

**Zusammenfassung** - In der hier vorliegenden Replik argumentiere ich, dass Addison und Teixeiras Erwiderung auf meine Studie irreführend ist. Auch wenn die Autoren meine Ergebnisse als solche anerkennen, so bezweifeln sie doch, dass die Resultate einen Hinweis auf Fehlspezifikationsprobleme liefern. Dabei übersehen sie fast alle deutschen Studien, die den Zusammenhang zwischen Betriebsgröße und Beschäftigungsentwicklung untersuchen. Während Addison und Teixeira eine Reihe von Fehlern in ihren Tabellen zugeben, ist ihre Erwiderung ebenfalls durch Fehler gekennzeichnet. Um unsere abweichenden Ergebnisse zu erklären, vergleichen sie die deskriptiven Statistiken von Wachstumsraten, die auf unterschiedlichen Definitionen basieren. Der Vorwurf, ich würde ihr Gesamtwerk nicht würdigen, lenkt nur vom Hauptproblem ab. Addison und Teixeiras Erwiderung ist durch heftige persönliche Angriffe und Wertungen gekennzeichnet, die nichts mit einem guten wissenschaftlichen Stil zu tun haben. Überdies zitieren die Autoren mich nicht korrekt.

Key words: **Works Council, Employment Growth, Misspecification Error, Error of Omission, Argumentum ad Hominem**

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\*\* Artikel eingegangen: 3.11.2008.

## 1. Introduction

In my study on works councils and employment growth (Jirjahn 2008a), I find that the estimated growth effect of workplace codetermination depends on the specification of establishment size. The results show no influence of establishment size but a negative effect of works councils when establishment size enters linearly in the growth regression. Yet, the estimates confirm a negative influence of establishment size and no significant effect of workplace codetermination when the logarithm of establishment size is included. These findings support the hypothesis that a misspecification of establishment size can yield biased estimates of the growth effects of works councils. This follows from two empirical regularities. First, establishment size and the presence of a works council are positively correlated. Second, several international and German studies show that larger establishments exhibit lower growth rates. Hence, if establishment size is not correctly specified in the growth regression, the estimated coefficient on workplace codetermination is very likely to reflect the negative relationship between size and growth.

In what follows, I will argue that Addison and Teixeira's (2008) rejoinder to my study is characteristically misleading and erroneous. First, while Addison and Teixeira (2008) agree that my study is of interest in its own right, they doubt that my results provide evidence of a misspecification error. They claim that the German experience does not indicate a negative relationship between establishment size and employment growth. Yet, Addison and Teixeira (2008) ignore almost all of the German studies on this issue. Those studies are in line with international evidence and confirm a negative association between size and growth also for Germany. Moreover, in this reply, I now also show that – based on the Akaike information criterion – the regression model with the logarithmic specification of establishment size indeed has to be preferred over a model with a linear specification. This underscores that I find a negative growth effect of works councils only in the inferior but not in the superior regression model.

Second, Addison and Teixeira (2008) admit several serious mistakes in the tables presented in their earlier paper on works councils and employment change (Addison/Teixeira 2006). These mistakes refer to some erroneous information on the number of observations and the time period examined. Most importantly, they now claim that they provided false information on their specification of establishment size. They claim that they have simply forgotten to mention that they used a logarithmic specification and not a linear specification. The surprised reader is left with the question of how to reconcile our conflicting results. In light of Addison and Teixeira's claim, their former paper does not only imply a negative growth effect of works councils but also a *positive* growth effect of logarithmic establishment size. While the authors correctly point to different sample sizes, they do a poor job in comparing their paper with my study. Indeed, a sound comparison appears to be difficult as the authors provide almost no descriptive statistics in their former paper and do not eliminate this shortcoming in their rejoinder. However, there is one exception. Addison and Teixeira (2006) present descriptive statistics on a single growth rate. In the rejoinder, the authors contrast these descriptive statistics with the descriptive statistics presented in my

study. Yet, they compare apples with oranges as they compare growth rates based on different definitions. Obviously, Addison and Teixeira continue to make mistakes even in their rejoinder.

Third, Addison and Teixeira (2008) fail to recognize that their *positive* growth effect of establishment size is sharply at odds with the extant literature. Even if the authors believe in Gibrat's law, this does not imply that they can produce any arbitrary result. They cannot justify their arbitrary result by simply claiming that they do not provide an appropriate test of Gibrat's law. Hence, the basic problem still exists. To the extent establishment size is correlated with works council incidence, doubts on Addison and Teixeira's *positive* growth effect of establishment size carry over to their estimated growth effect of workplace codetermination.

Fourth, the authors complain that I do not appreciate the overall contribution of their former paper. They argue that the paper was motivated by the "virtual absence of any comprehensive German study on the works council-employment nexus other than (disputation as to their implications for) labor turnover." At this point it may be mentioned that the paper is not that innovative as the authors let us believe. There are several studies on employment growth in Germany that also take into account the effect of works councils. These studies are basically ignored by the authors. Addison and Teixeira may be the first who attempt to examine the role of a potential survivor bias and to investigate the effect of works councils on the pace of employment adjustment. Yet, while these issues are interesting in their own right, they simply do not address the basic point of my study. In that study, I focus on a specific and most problematic issue, namely the estimated growth effect of works councils. This is absolutely legitimate. Large parts of their rejoinder completely miss the point. Instead of discussing or defending their negative growth effect of workplace codetermination in more detail, they summarize parts of their analysis that are (at least for the moment) not under discussion. This distracts from the basic problem and obscures the authors' mistakes. However, reinvestigating the other parts of their earlier paper stands as future research. Just to provide an example, Addison and Teixeira (2006) do not only find little evidence of a survivor bias but also that works councils have no significant effect on establishment closures. The latter finding is sharply at odds with a previous study by Addison, Bellmann and Kölling (2004) showing that councils are associated with an increased probability of closure. Even though they cite this study, Addison and Teixeira make not the slightest attempt to reconcile the conflicting results.

Fifth, instead of apologizing for their mistakes and providing reasonable arguments to reconcile their paper with my study, the authors come up with personal attacks mixed with a strategy to distract from the shortcomings of their paper. Such *argumenta ad hominem* are far away from good scientific style. In their keen attempt to discredit my study, Addison and Teixeira (2008) are even not able to provide correct citations of what I wrote.

In contrast to Addison and Teixeira, I will try to distinguish between scientific arguments and personal notes for most parts of my reply. Even though it is tempting to adopt their rhetorical and nebulous style of writing, I will try to focus on sound scientific reasoning. However, in the concluding section I will also make some personal notes to the authors. Addison and Teixeira end their rejoinder with a useful reminder.

I am so indebted for those profound thoughts that I feel obliged to reciprocate with some useful recommendations.

## 2. A summary of key results

As my paper on works councils and employment growth (Jirjahn 2008a) is written in German, I provide an English summary of my key results to inform the international readership. Like Addison and Teixeira (2006), I also use the IAB Establishment Panel to examine the relationship between works councils and employment growth in West German establishments. I also follow the authors in regressing employment growth between 1993 and 2001 on establishment characteristics in 1993. However, our sets of control variables only partially overlap. While I exclude some of Addison and Teixeira’s insignificant variables, I include a couple of other control variables that are not considered by the authors but are relevant determinants of employment growth. In my study, I use two different growth rates. The first growth rate is:

$$\text{Growth Rate 1} = \frac{\text{Employment}(2001) - \text{Employment}(1993)}{0.5 \cdot (\text{Employment}(2001) + \text{Employment}(1993))}. \tag{1}$$

Changes in employment are divided by the average of employment (Davis and Haltiwanger 1992). Using this growth rate may reduce the impact of outliers. A second definition often found in the literature uses logs (e.g., Evans 1987). That growth rate is:

$$\text{Growth Rate 2} = \ln(\text{Employment}(2001)) - \ln(\text{Employment}(1993)). \tag{2}$$

To address Addison and Teixeira’s rejoinder, I will also consider the standard growth rate in this reply:

$$\text{Growth Rate 3} = \frac{\text{Employment}(2001) - \text{Employment}(1993)}{\text{Employment}(1993)}. \tag{3}$$

Table 1 presents descriptive statistics on the three growth rates. The initial estimates are shown in Table 2.

**Table 1: Descriptive statistics**

All Establishments (N = 906)	
Variable	Mean, Std. Dev.
Growth Rate 1	-0.1498, 0.4610
Growth Rate 2	-0.1820, 0.6132
Growth Rate 3	-0.0495, 0.4651
Only Establishments without Changes in Works Council Status; Without Agriculture and Forestry, Hunting and Fishing, Banking and Insurance (N = 652)	
Variable	Mean, Std. Dev.
Growth Rate 1	-0.1537, 0.4483
Growth Rate 2	-0.1801, 0.5653
Growth Rate 3	-0.0583, 0.4440

**Table 2: Determinants of employment growth; all establishments**

Dependent Variable	Growth Rate 1		Growth Rate 2		Growth Rate 3	
	Model A	Model B	Model A	Model B	Model A	Model B
Presence of a Works Council	-0.1075 (2.69)***	-0.0465 (0.94)	-0.1299 (2.60)**	-0.0483 (0.75)	-0.1305 (3.43)***	-0.0607 (1.30)
Number of Employees	8.21e-07 (0.28)	---	-9.87e-08 (0.02)	---	-2.13e-06 (0.82)	---
Log(Number of Employees)	---	-0.0348 (2.29)**	---	-0.0465 (2.19)**	---	-0.0396 (2.73)***
R <sup>2</sup>	0.1158	0.1218	0.0944	0.1006	0.1055	0.1131
AIC	1115.62	1109.36	1654.15	1647.96	1141.89	1134.11
N	906	906	906	906	906	906

Method: OLS. Robust *t*-statistics are in parentheses. \*\*\*:  $\alpha = 0.01$ ; \*\*:  $\alpha = 0.05$ . All regressions include variables for short-time work, overtime work, shift work, work on Saturdays, legal form, vintage of production technology, share of apprentices, share of skilled blue-collar workers, share of unskilled blue-collar workers, industry affiliation and federal states.

For each growth rate, the results of two regression models are reported. *Model A* includes linear establishment size (number of employees in 1993) as explanatory variable while *Model B* is based on a logarithmic specification of establishment size. A dummy variable for the presence of a works council is included in all regressions. Control variables for short-time work, overtime work, shift work, work on Saturdays, legal form, vintage of production technology, workforce structure, industry affiliation and federal states are also included in each estimation. The results on the control variables are suppressed to save space.

The estimates show a clear pattern of results. When establishment size enters linearly in the growth regression, it is no significant determinant of employment growth while the negative coefficient on works council incidence is statistically significant. The results change dramatically when the logarithm of establishment size is included. The results now confirm a significantly negative growth effect of establishment size whereas workplace codetermination is no longer a statistically significant covariate of employment growth. This pattern of results supports the hypothesis that a misspecification of establishment size can yield biased estimates of the growth effects of workplace codetermination. This hypothesis is based on two regularities. First, establishment size and the presence of a works council are positively correlated. Second, there is substantial evidence of a negative relationship between establishment size and employment growth. Thus, if establishment size is not correctly specified in the growth regression, the estimated coefficient on works council incidence is very likely to pick up the negative growth effect of establishment size.

In this reply, I additionally present the Akaike information criterion (*AIC*) that helps to select between nonnested regression models.<sup>1</sup> As shown in Table 2, *Model B*

<sup>1</sup> In our case the number of parameters is the same in the two models. Hence, the Akaike information criterion implies that the choice between *Model A* and *Model B* simply comes down to choosing the model with the higher log-likelihood function.

has always a smaller *AIC* than *Model A*. The regression model with the logarithmic specification of establishment size has to be preferred over the model with the linear specification. Hence, I find the negative relationship between size and growth in the superior but not in the inferior regression model while I find a negative growth effect of workplace codetermination not in the superior but only in the inferior model. This can be seen as additional support for my hypothesis.

In this reply, I provide a further check of robustness. I follow Addison and Teixeira (2006) and exclude agriculture and forestry, hunting and fishing, and banking and insurance from the analysis.<sup>2</sup> I also exclude establishments with a change in their works council status. As can be seen in Table 3, this exercise does not change the basic pattern of results.

**Table 3: Determinants of employment growth; only establishments without changes in works council status; without agriculture and forestry, hunting and fishing, banking and insurance**

Dependent Variable Explanatory Variable	Growth Rate 1		Growth Rate 2		Growth Rate 3	
	Model A	Model B	Model A	Model B	Model A	Model B
Presence of a Works Council	-0.0902 (1.87)*	-0.0077 (0.12)	-0.1121 (1.76)*	-0.0165 (0.19)	-0.1124 (2.64)***	-0.0328 (0.67)
Number of Employees	-6.21e-07 (0.17)	---	3.18e-07 (0.02)	---	-2.84e-06 (0.91)	---
Log(Number of Employees)	---	-0.0387 (2.17)**	---	-0.0449 (1.96)*	---	-0.0373 (2.36)**
<i>R</i> <sup>2</sup>	0.1357	0.1427	0.1171	0.1230	0.1471	0.1535
<i>AIC</i>	764.11	758.80	1080.27	1075.87	742.66	737.75
<i>N</i>	652	652	652	652	652	652

Method: OLS. Robust *t*-statistics are in parentheses. \*\*\*:  $\alpha = 0.01$ ; \*\*:  $\alpha = 0.05$ ; \*:  $\alpha = 0.1$ . All regressions include variables for short-time work, overtime work, shift work, work on Saturdays, legal form, vintage of production technology, share of apprentices, share of skilled blue-collar workers, share of unskilled blue-collar workers, industry affiliation and federal states.

### 3. Establishment size and employment growth

The interpretation of my findings is based on the assumption that there exists indeed a negative relationship between establishment size and employment growth. Several international studies confirm such a negative relationship (e.g., Evans 1987 for the US; Dunne/Hughes 1994 for Britain; Blanchflower/Burgess 1998 for Australia; Weiss 1998 for Austria; Audretsch/Santarelli/Vivarelli 1999 for Italy). Acs and Audretsch (1990) go so far to contend that the negative relationship between size and growth is a “stylized fact.”

Addison and Teixeira (2008) claim that the negative association between size and employment growth does not hold true for Germany. Hence, they reject the interpretation that my findings indicate problems of misspecification. They conclude that “the

<sup>2</sup> In Jirjahn (2008a), I already mention in footnote 2 that excluding those sectors does not change the basic pattern of results.

admittedly sparse *German* literature if anything might suggest that firm growth rates are independent of firm size.” The authors cite only two papers to support their view. The first paper is a very early econometric attempt by Wagner (1992) to investigate the link between size and employment growth in Germany.<sup>3</sup> The second paper provides only descriptive statistics (Wagner/Koller/Schnabel 2008). This is a remarkably selective and misleading review of the extant literature. The German literature is not nearly as sparse as the authors would have us believe. Addison and Teixeira completely ignore recent econometric studies showing a negative relationship between size and employment growth also for Germany. Harhoff and Stahl (1995), Harhoff, Stahl and Woywode (1998), Gerlach and Jirjahn (1999), and Almus and Nerlinger (1999, 2000) provide evidence of a negative relationship for West Germany. Harhoff and Stahl (1994), Hinz, Wilsdorf and Ziegler (1997), and Steil and Wolf (1999) provide evidence for East Germany.

#### 4. How to keep readers guessing

In their rejoinder, Addison and Teixeira admit several serious mistakes in the tables presented in their own paper on works councils and employment change. In Table A1 and Table A2, Addison and Teixeira (2006) show the results of regressions that additionally control for collective bargaining agreements and the interaction of works councils with collective bargaining agreements. The titles of the tables and the number of observations suggest that the authors here also regress the employment change between 1993 and 2001 on establishment characteristics in 1993. Yet, this is amazing. In Jirjahn (2008a), I point out that the 1993 wave of the IAB Establishment Panel does not contain any information whether the establishment is covered by a collective bargaining agreement. There is only information whether there is a collective contract in the industry. Using this information does not make any sense as establishments are only covered if they are members of an employer’s association or the firm directly negotiates with the union. The surprised reader now experiences that the information given in the tables is wrong. Addison and Teixeira (2008) claim that the sample period in question is 1995-2001 and that the numbers of observations shown in both tables are also not correct.

Most importantly, Addison and Teixeira (2008) admit that they provided false information on their specification of establishment size. In their paper on works councils and employment change, Addison and Teixeira (2006) present a linear specification. They now claim that they have simply forgotten to mention that their results are based on a logarithmic specification of establishment size and not on the linear specification. They call this “an error of omission on our side that escaped identification in the proof-reading stage.” This comes really as a surprise. On March 13th, I sent an email to the authors and asked them to send me their do files. On August 1st, more than four months later, I received a long email from Teixeira.<sup>4</sup> While he summarized several steps to assemble the dataset, he also wrote that he did not keep record of any

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<sup>3</sup> Addison and Teixeira might have wished to cite another early study by Schmidt (1995). This study also finds no systematic relationship.

<sup>4</sup> At that time, I had finished my own study.

of the files. But of course, even though their do files are not available, it is well possible that the authors at least still have their log files. And these log files might have revealed the authors' true specification.

So given the error of omission, the results of Addison and Teixeira (2006) do not only imply a negative employment growth effect of workplace codetermination but also a *positive* employment growth effect of logarithmic establishment size. This obviously raises the question of how to reconcile our conflicting findings. While Addison and Teixeira (2008) correctly point to different sample sizes, they do a poor job in comparing their paper with my study. The authors provide almost no descriptive statistics in their earlier paper and do not eliminate this shortcoming in their rejoinder. Yet, providing a table with descriptive statistics and clear variable definitions is a minimum standard every empirical study should meet. This would be particularly helpful in our case. While our sets of control variables do not perfectly coincide, there are several variables used in both studies. These variables could form the basis for a sound comparison. If the authors still have their log files, they might also still have some descriptive statistics for their regression sample.

However, in Table 1 of their paper, Addison and Teixeira (2006) present descriptive statistics on a single growth rate. In their rejoinder, they contrast these descriptive statistics with the descriptive statistics on a growth rate presented in Table 1 of my study (Jirjahn 2008a). This aims to demonstrate that the compositions of our samples markedly differ. Addison and Teixeira (2008) conclude that they "are not necessarily surprised that Jirjahn comes up with different results for the principal variable of interest." Yet, their comparison fails for two reasons. First, the sample for their descriptive statistics is obviously not identical with their regression sample ( $n = 786$  vs.  $n = 600$ ). Hence, their descriptive statistics are not necessarily helpful in comparing our different regression samples. Second and most importantly, Addison and Teixeira compare apples with oranges. They compare their descriptive statistics on the standard growth rate (Growth Rate 3) with my descriptive statistics on a growth rate measured as the difference in log employment (Growth Rate 2). Note that Addison and Teixeira call their comparison housekeeping. I must admit that I am frightened by such housekeeping. To provide a sound comparison, Table 1 of this reply presents also descriptive statistics on the standard growth rate. As can be seen, the average growth rates are very similar: -5.5% for Addison and Teixeira and -5% (-5.8% in the restricted sample) for Jirjahn.

Moreover, Addison and Teixeira (2008) fail to recognize that their *positive* growth effect of establishment size is sharply at odds with the extant literature. Even if the authors believe in Gibrat's law, this does not imply that they can produce any arbitrary result. They cannot justify an arbitrary result by simply claiming that they do not provide an appropriate test of Gibrat's law. Hence, the basic problem persists. To the extent establishment size is correlated with works council incidence, doubts on Addison and Teixeira's *positive* growth effect of establishment size carry over to their estimated growth effect of workplace codetermination.

## 5. The distraction

Addison and Teixeira complain that I do not appreciate their overall contribution. Yet, this complaint simply misses the point. I focus on a specific and most problematic issue, namely the estimated growth effect of works councils. This is absolutely legitimate. Their complaint just distracts from the basic point and obscures the shortcomings of their paper. The authors motivate their paper by a virtual absence of any comprehensive study on works councils and employment. At this point, it may be mentioned that the paper is not as innovative as the authors would have us believe. For example, Gold (1999), Gerlach and Jirjahn (1999), and Meyer and Pfeifer (2005) also consider the role of works councils in employment growth. It might have been very fruitful, if the authors – at least in the rejoinder – would have compared their paper with those studies. Yet, the studies are basically ignored. Addison and Teixeira (2006) may be the first who attempt to examine the potential role of a survivor bias and the effect of works councils on the pace of employment adjustment.<sup>5</sup> They find little evidence of a survivor bias and also no evidence that works councils influence the pace of employment adjustment. While these issues are interesting in their own right, they do not address the point of my study. In their discussion, Addison and Teixeira (2008) provide a lengthy summary of their overall contribution. Yet, this is completely unrelated to my study.

While large parts of the rejoinder are simply distracting, a sound discussion of the overall contribution might have been very fruitful. The focus of such a discussion might have been on the question of whether their paper provides a coherent pattern of results. Is it plausible that works councils have a negative impact on employment growth but no influence on the pace of employment adjustment? If it is not plausible, what are the implications? If the authors believe in the negative growth effect of works councils, does it make sense that there is no influence on the pace of employment adjustment? Or to put it another way, if the authors believe in the lack of effect on the pace of employment adjustment, does it make sense that they obtain a negative effect on employment growth? Some theoretical considerations to answer these questions would have been very useful.

Moreover, it would have been helpful if the authors would have put their results in the context of the wider works council literature. Several studies indicate that works councils neither inhibit investment nor innovation (Addison/Schnabel/Wagner 2001, Dilger 2002, Hübler 2003, Blume/Gerstlberger 2007). Quite the contrary, there is evidence that works councils are positively associated with specific types of investments and innovations (Jirjahn 2005, 2006 for a survey). Is a negative employment growth effect of workplace codetermination compatible with a nonnegative effect on investment and innovation? Some considerations to answer this question would have been nice.

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<sup>5</sup> But note that the survivor bias has been already examined by several German studies that do not control for works council incidence (e.g., Harhoff/Stahl 1995; Harhoff/Stahl/Woywode 1998; Steil/Wolf 1999). Needless to say, these studies are ignored.

Of course, it would have been also a good idea to relate the results to studies showing less favourable effects of works councils. Addison and Teixeira (2006) find no significant relationship between works councils and establishment closures.<sup>6</sup> In contrast, a previous study by Addison, Bellman and Kölling (2004) found a significantly negative association. Addison, Bellman and Kölling also found that works council incidence interacts with collective bargaining coverage while Addison and Teixeira fail to find a significant interaction effect in their survivor regressions. Addison and Teixeira (2006, 2008) make not the slightest attempt to reconcile the conflicting findings. One might imagine that they would point again to different sample sizes. But this does not help. Either the works council has an influence on closures or it has no influence on closures.

Addison and Teixeira's line of reasoning is evocative of Paul Feyerabend's (1975) "Anything Goes." Yet, most readers will object to this epistemological anarchism.

## 6. A reminder on scientific style

Addison and Teixeira (2008) complain that my study is "a characteristically combative treatment." However, science is competition of the best ideas and analyses. And even if this competition is sometimes very hard, there is nothing wrong as long as competing researchers follow the rules of science and fairness. One basic rule is to avoid the ad hominem fallacy. Yet, Addison and Teixeira come up with personal attacks. They accuse me that I unreservedly view works councils through rose-tinted lenses. Obviously the authors want to suggest that I am not able to provide an objective study. Hence, they conclude that I am eager to be led astray. They go even further and implicitly suggest that I view both workplace codetermination and board-level codetermination through rose-tinted lenses:

"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 empirical literature even harder to bear [e.g. Gorton/Schmid 2004])."

Yet, in their keen attempt to discredit my study, Addison and Teixeira are even not able to provide a correct citation of what I wrote. The relevant sentence in the English abstract of my paper is:

"Recent econometric studies show a neutral-to-positive effect of workplace codetermination on economic outcomes."

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<sup>6</sup> Addison and Teixeira (2008) claim that Jirjahn (2008b) relies on the results of their test procedure. However, in that unpublished paper, I examine a potential panel attrition bias and not a survivor bias. Interestingly, the authors never asked me to send them my unpublished work. Perhaps they downloaded the paper from a conference homepage.

Obviously, here we have a further delicate “error of omission” on Addison and Teixeira’s side. They omitted the crucial word “workplace.”<sup>7</sup> Addison and Teixeira provide also misleading indirect citations. To give an example:

“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 accentuated in this temporal frame.”

This is a caricature of what I wrote. In the concluding section of my study, I point to an important limitation of my analysis (a limitation shared with Addison and Teixeira’s treatment). The study does not take into account the potential endogeneity of works councils. I propose that using the IAB Establishment Panel to investigate the issue of endogeneity stands as important future research. To motivate this proposal, I refer to my unpublished work (Jirjahn 2008b) and stress that this work is based on the well-known Hannover Firm Panel.

## 7. The issue of endogeneity

As Addison and Teixeira appear to criticize my unpublished work, it may be useful to clarify key results. I find no relationship between works councils and employment growth in OLS regressions and a positive relationship when the endogeneity of works councils is taken into account. OLS appears to yield downward biased estimates of the growth effect of works councils. In my study, there are unobserved factors positively influencing the incidence of a works council and negatively influencing employment growth. This supports the hypothesis that workers are more likely to be interested in work councils when the establishment is facing (financial) pressure. This would seem to run counter to Addison and Teixeira’s beliefs. Yet, science is not a matter of beliefs. International studies taking the endogeneity of worker representation into account point into a similar direction (Machin/Wadhvani 1991; Bryson/Dale-Olson 2008). Moreover, cross-country studies indicate that cooperative and trustful industrial relations play a crucial role in employment growth (Blanchard/Phillipon 2006; Feldmann 2006). Addison and Teixeira criticize my study for being restricted to manufacturing establishments in one federal state of Germany. Yet, this clearly helps to avoid a bias due to heterogeneity across industries and regions. Probably it would be a good idea

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<sup>7</sup> Clearly, even some studies on works councils find negative effects of workplace codetermination. Addison, Bellmann and Kölling (2004) provide an example. Yet, all in all, the extant recent literature obtains neutral-to-positive effects of workplace codetermination on economic outcomes. I recommend Addison and Teixeira to read the more recent surveys by Jirjahn (2005, 2006) and Frick (2008). I recommend them to carefully check if my surveys reflect an unreserved view through rose-tinted lenses. The literature on board-level codetermination appears to be rather inconclusive. But I recommend Addison and Teixeira to consider also the recent studies by FitzRoy and Kraft (2005) and Renaud (2007).

for future research to perform separate estimates also with the IAB Establishment Panel.

## 8. Concluding remarks

Addison and Teixeira end their rejoinder with a useful reminder. I feel obliged to reciprocate with some useful recommendations. It seems that the authors invent a new academic quiz show called “*What’s Right And What’s Wrong With Our Paper?*” Such a quiz show is obviously no good idea. I also do not think that we should adopt a rhetorical style of writing that obscures the basic issues. I strongly encourage the authors to return to a sound scientific discussion. Addison and Teixeira are certainly right with their conclusion that more work is required of all of us to examine the effects of works councils. In this sense, I look forward to our future *scientific* exchange.

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