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Report on Teodora Paligorova's dissertation manuscript *Essays on Managerial Pay Structures*

This thesis manuscript comprises three empirical essays on managerial pay, the two first exploiting Czech data and the third essay using data from the United States.

Essay 1 deals with firms' managerial pay structures and the extent to which these can understood as the outcome of incentive schemes set up in order to provide incentives in promotion competitions as in tournament models. More specifically, the author tests two key predictions of tournament theory: a convex pay-rank relationship with an "extra" large increase in pay at the top of the hierarchy, and pay spreads increasing in the number of contestants in the tournament. These hypotheses have been tested before, but the empirical literature on tournaments using business data is rather small, and this is the first study making use of data from a post-communist country, the Czech Republic. The paper is well written and although not much is novel, it contributes new evidence for the Czech Republic and furthermore demonstrates that the Czech managerial pay structures in the beginning of this century (2001) resemble those in mature market economies with few, if any, traces of the former command economy.

My main concern with the analysis has to do with the way the job hierarchy has been operationalised in the study. In order to construct a hierarchy, the author uses wage grids data (ironically enough a remaining product of the communist system) to construct seven job levels for the managerial employees, which she then uses to compute measures of within-firm pay spreads.. The problem is that because of the very compressed structure of the wage grids the majority of the managerial employees below the CEO level end up in the highest job level (4,593 out of 8,666). With such a strong congestion in the highest level below the CEO level, one can question the extent to which the set of job level dummies used in the empirical analyses does a good job in describing the actual managerial job hierarchy. Of course, this is not something the author can do much about, as there are no alternative ways of constructing job levels, but it is important to recognise the weakness of the otherwise high quality data in this respect.

Some minor comments:

Tournament theory is about competition *within* organisations. What information is there for the Czech economy concerning the importance of internal labour markets?

In testing the size of the tournament hypothesis, the group of contestants is defined as the managers in job level 7, which as was noted above, is rather inflated.

Page 5; I think footnote 10 is so important that it should be part of the main text. I have tried myself to construct managerial hierarchies and to distinguish between CEOs and other top managers from TREXIMA data sets and it is not a straightforward task. In fact, the author has done an excellent job, my only marginal worry is that in the case when she "observes" more than one top manager in a firm, this may be because one is entering and the other is leaving the firm in year 2001.

Page 9: in the middle of the last paragraph in a sentence beginning "Column 3 shows…" some words are missing

Page 12: 1.5.3 second row: Rosen (1986) should be (1989), a chapter in a book edited by Lars Werin.

Essay 2, which is co-authored with Stephan Juradja, is concerned with gender differentials in managerial compensation. While there is large number of studies of the gender gap for employees in general, and this is also true for the Czech Republic, studies of gender differences in pay for managers are surprisingly, very scarce. In fact there are only a handful of previous studies, two of which are referred to in the study are from the U.S. Evidence from CR is welcome also because of the increase in wage inequality and between the genders that has been observed during the post-communist period. The paper makes another important contribution by applying a non-parametric matching method for the decomposition of the gender gap. The traditional Oaxaca-Blinder composition is likely to suffer from a no support problem which gives rise to an over-estimation of the "unexplained" component in the decomposition.

This is a well-executed piece of applied economic research, which I enjoyed reading, and has in my mind a clear potential for international publication. I have only a few marginal comments.

One thing that is not discussed in the paper is that because there are relatively few women who make it to the top of firms, you would expect them to be a highly selected group wrt ability. Hence, when you compare average pay for male and female managers you are averaging over very different ability distributions. Lazear and Rosen have had a discussion of this, and in a paper about glass ceilings, Booth and Fransesconi accounted for this in their analysis. This selection might be of some importance also when you apply the matching method to the decomposition of the gender gap. As you write on page 45 "a condition for the correct identification of the ATT" is the absence of differences in unobservables across groups. Well, unobserved ability may be one because there are so few women.

In a journal version I would cut section 2.8 as it does not really add much interesting.

The finding that the managerial gender gap is smaller in firms where there are more women at other hierarchical levels is an interesting one and reflects within-firm promotions. When you discuss policy implications, you claim that focussing on promotion policies in more prestigious companies would be the most effective way to enhance equality. I cannot follow you there. In view of your results an alternative implication could be to mitigate the gender segregation by industry or firm size?

The third essay also deals with managerial compensation but here the focus is on U.S. chief executives and the determinants of their pay pre and post the 2002 corporate governance reform called the Sarbannes-Oxley Act (SOX for short). The analysis carried out in this chapter builds on two previous studies by Bertrand and Mullainathan (2001) and Wang (2005), respectively. Like Wang, she uses the independence of the board as a measure of the reform's treatment. Wang (2005) is concerned with CFOs arguing that this is the group of executives that was affected most. CEOs are likely to be affected as well, but its extent is an empirical matter which the author sets out to examine.

Bertrand and Mullainathan studied whether there was a difference in rent extraction behaviour between chief executives of well and poorly governed companies by looking at the extent to which they are rewarded for "luck". The author applies the same technique to investigate whether rent extraction behaviour changes after SOX and whether there is a difference in the change between firms with and without independent boards. Clearly, this is a well motivated study on an important topic. This is a fine paper that should attract considerable international interest.

I find the analysis of the change in the pay-performance relation before and after SOX compelling and have nothing to add to that. The analysis of rent extraction is also interesting but raises some questions about the measurement of "luck" in company performance. Here the author follows Bertrand and Mullainathan using a regression-based method for computing industry- and firm-specific performance, of which the former is considered as "luck". Standard principal-agent models tell us that one way of improving the performance measure in the performance-pay relation is by using relative performance measure (RPE), typically relative to industry. However, a big puzzle is why most firms do not seem to use RPE. Why are executives rewarded/punished for good/bad macro-conditions, although it would be easy to filter out uncontrollable risks from the performance measures? The author mentions a study by Garvey and Milbourn (2006) that has tried to provide an answer. The same authors (J of Finance, 2003) have also given an earlier supply side answer and other authors like Aggarwal and Samwick (1999) and Oyer (2004) have come up with additional explanations. This literature suggests that there might be good reasons for why RPE is scarce. At the same time, it indicates that one should be careful in interpreting the industry-specific performance component as "luck".

Some minor comments:

Page 70 and Table 3.1: note, that there were some changes already before SOX; all independency measures increased somewhat 1998-2001. What about before 1998? Is there a trend which is strengthened by SOX, or is some of the changes made mandatory by SOX already implemented in an increasing share of firms during the nineties?

Page 77, last line: 14-year period must be wrong? Should be 7?

Page 78 and Table 3: calling the value of stock options "option value" is not a good idea as it may be quite confusing. Use the term "value of stock options" instead.

Page 80, second row from bottom and Table 3.4: need to clarify in the table that this is showing the share of *members.*

Page 88 and Table 3.7: CEOs pay for luck in dependent board companies falls after SOX, but it is still higher (main + interaction effect) than in independent firms. Also the sensitivity to firm specific ROA is higher in the dependent board firms after SOX. How can this be explained?

Page 90, section 3.9 first row: I would write “determinants of executive pay”, not “dynamics…”

All in all, this thesis manuscript is a good piece of research, with contributions to the empirical literature on managerial compensation. Two of the studies, and with some luck all three, has potential for being published in good international outlets. In my opinion the thesis is more or less ready for defense. My few comments and suggestions are predominantly minor and should be fairly easy to address.

Aarhus, July 13, 2007

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