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Comments to the Dissertation of Michal Franta (Prague)

The dissertation is composed of three papers which cover a broad space in the discipline of economics. One common point of the papers is their nature as efforts of empirical research. Michal Franta uses the methods required to achieve results with great skill truly up to the standards of a Ph. D. dissertation. However, some questions could be posed which will be addressed in the following. These concern mainly the way the analyses and their results are presented. In some cases the inclusion of additional literature would be useful.

I. The topic of the first paper is the “Time aggregation bias in discrete time models of aggregate duration data”. The paper estimates a model of aggregate duration data for the Czech Republic from 1992 - 2007 to disentangle trends in unemployment in trends in incidence, unobserved heterogeneity and duration dependence. Thereby, it deals with a complicated subject and with difficult identification problems. The paper is based on publications by Abbring et al. (2001, 2002).

One major contribution of Franta’s article is that this decomposition is done for a transformation economy, which in itself should be seen as interesting. Convincingly, the author argues that one should use monthly inflow data into unemployment instead of quarterly stock data to account for the time aggregation bias. Franta demonstrates that a part of the literature suffers from this bias. The description of the problem is interesting and so are the results of the aggregate duration model. However, the results are to be interpreted with caution since the model does not pass all specification tests.

It seems that there is both strong negative duration dependence and unobserved heterogeneity in the data. The macroeconomic influences seem not to be very strong and only the first and the last quarter are significantly related to the inflow composition. Finally, it seems that all these factors change over time. Since the macroeconomic influences are not strong the results can be taken to be supportive for work with micro-data, since this data facilitates the control of heterogeneity.

Major Comments:

1) Identification:

Franta states that his model is identified since Abbring et al. (2001, 2002) show identification. However, the author should at least demonstrate that his models 2, 2’ are special cases of the one used by Abbring et al. (2001, 2002) to guarantee that the identification arguments of Abbring et al. apply. It would be very helpful, if the author could do more in showing that he has really understood the identification of the model and discuss the main arguments. In addition, the assumptions needed to identify the model are not given by Franta. He does not discuss

bounded support assumption of the unobserved heterogeneity nor does he discuss the independence assumption, which is crucial for identification.

2) Notation:

Equation (1) is written as if the author is interested in the common distribution of duration, time and unobserved heterogeneity $h(t,d,v)$, but econometric models generally (and the one used here) rely on conditional distributions $h(d/t,v)$. The author is interested in the distribution of durations conditional on the time of entrance and unobserved heterogeneity, not on the joint distribution of them.

3) Relevant time spans:

There is an inconsistency between the time spans described in the text and the appendix. While in the text the author states that the duration categories are “6-9” and “9 +” (e.g. figure 4 and 5), in the appendix the time spans are “6-12” and “12+” (figure without name in Appendix B). Which are the correct ones?

Minor Comments:

1) The language needs improvement. Not all sentences are comprehensible, sometimes words are missing.

2) Are the data about the year 1992 reliable? There should be an explanation whether the quality of the data for a year shortly after the fall of the iron curtain is good enough.

3) The author describes the specification tests in Van den Berg and Van Ours (1996). He states that the test relies on the finiteness of the support of the distribution, whereas it is the positiveness and not the finiteness which is important to construct the testing procedure.

4) The author states that the w_q 's are related to the number of entrants in season q : This is not understandable, since the w_q 's bear information on the relative quality of entrants in season q , not on the number. Is this just a typing error? (p.17)

5) It is not understandable, why the time period is restricted through specification tests (p. 26). The author should find convincing arguments why for some time spans the model is applicable and for others not.

6) Model 2' is not the most general model, since it restricts the business cycle dependency to only two values (p.26).

7) Doesn't the fact that (12c) is not supported by the data mean that the model is wrongly specified (p.28)?

8) The author asserts that the estimated coefficients do not differ between males and females. Did he test this (p.31)?

9) The author could do more to convince the reader by discussing why a transformation country is an interesting case to look at the question of relative importance of duration dependence and unobserved heterogeneity. What can be expected to be special? Which theories might be better adaptable to transformation than to other countries?

10) The text of the introduction is often hard to read.

II. The topic of the second paper (written together with Martin Guzi) is the “Unequal access to higher education in the Czech Republic”. Here, the main research problem is the application of young people to universities and the process of acceptance of these applicants by the uni-

versities. The uneven distribution which results from these two processes is interpreted as being due to effects of cost savings and to information advantages.

Remarks:

1) The unequal distribution of the access to higher education has at least two aspects: It is related to the life prospects of individual people and it is related to the regions where these people live. The second aspect is largely ignored in the paper, though it is important for the growth of regions. It would be interesting to hear more about the consequences of the selectivity of the educational system. Since the existence of a university is a property of a region, it is an obvious question to ask for the consequences of the locations of institutions of higher education.

2) A second point is the small role which is attributed to human capital investment calculations in the process of application to higher education. This is rather astonishing, since human capital theory is the dominating approach concerning this investment. Only implicitly, via the cost argument these calculations on investment are taken into account. One could expect at least a paragraph in the paper discussing this relationship.

III. The theme of the third paper, which is written together with Branislav Saxa and Katerina Smidkova, is "The role of inflation persistence in the inflation process in the New EU Member States". This is a question highly relevant for political decision in Europe. Two different types of models are applied, time-varying mean models and ARFIMA models, which are autoregressive fractionally integrated moving average models. There are several steps required in order to set up the models properly, which includes Kalman filtering and Bayesian estimation techniques. Statistical tests show that the first type of model is the preferred one. In effect there are two groups of countries, one with a low persistence of inflation, the other one with a relatively high persistence due to intrinsic and expectations-base inflation persistence.

Remarks:

1) The presentation of the actual inflation process and the perceived inflation target has its surprises. For many years and countries the target is lower than current inflation, as can be seen from the presented graphs. Normally, a central bank does not have the problem to increase inflation but to restrict it. For Bulgaria obviously there is a phase of hyperinflation in 1997 (p. 140). It breaks down very fast, but the inflation target remains for some phase at rates of over 100 %. Is it possible to apply a model of the kind presented in a period of hyperinflation? This is not clear.

Apart from this, inflation shows a number of spikes for several countries. It could be important to understand what is behind them. At least for the Czech case (p. 120) it should be possible to explain their nature. The persistence of inflation could not be very high if fluctuations of the kind presented occur.

2) There are many theories discussing inflation which are not covered in the paper but are relevant for the research question at hand. The approach developed by Layard, Nickell, Jackman relates inflation to the labour market. The Balassa-Samuelson effect is important to understand different inflation rates in countries with trade relationships between each other. It would be helpful to relate the findings to these theories.

3) The exposition has its weaknesses. This starts with the title of the paper which uses the term “inflation” two times, while it is required only once. The introduction is again hard to read. It is not easy to see what the problem of the paper is and what the proposed accomplishments are.

Finally: What is meant by micro-data (p. 102 & 107) in the present context? Normally this term refers to information about individual economic agents, but this could no be the case.

Final assessment:

On the basis of the draft of M. Franta’s dissertation I finally come to the conclusion that the dissertation warrants a defense and eventual award of a doctorate title.