

Oleg Zamulin, Sovkomflot Assistant Professor
Program Director, Master of Arts in Economics
ozamulin@nes.ru

July 22, 2010

Prof. Jan Hanousek, CERGE-EI
Charles University, Academy of Sciences

Dear Professor Hanousek:

In response to your request I have read the PhD thesis of Jan Bruha, “Four Essays on Macroeconomic Aspects of the Transition Process.” Overall, I think that the thesis is satisfactory in terms of doctoral standards. At the same time, there are a few reservations that I have and which I spell out below.

The thesis consists of four papers, of which three are very similar to each other, to the extent that they almost can be thought of as one piece of research, and many passages are repeated verbatim.

The first paper is an empirical exercise that tries to explain what stood behind the fall in the amount of environmental pollution in the Czech Republic during the transition period. The author decomposes this fall into several effects, and then runs an empirical exercise on Czech industries to see what stood behind this fall in pollution. He finds that pollution fell mainly due to “intensity” effect, that is, less pollution per item produced, and that this was mainly associated with an increase in labor productivity.

Although the results are interesting, I have two problems with this paper. First, the author should spend much more time explaining what he does. He does not describe the data and their sources, nor does he provide the empirical model specifications. Furthermore, the decomposition into three effects is unclear and unintuitive. The author should describe them in more detail, perhaps giving examples and anecdotes; otherwise, the reader is left unsure about what exactly the author does.

Second, the empirical exercise is simple and completely theory-free. The author simply took the intensity measured (how obtained?) and regressed it on whatever industry indicators he could find. The results are not well interpreted. Why is labor productivity associated with less pollution intensity? The first thing that comes to my mind is that new technologies helped with both the environment and with the productivity. Is this the story? Or something else? Without looking into specific theories the empirical exercise looks like just a few unexplained correlations.

The other three papers, as I said above, are all very similar and are co-authored with the same person. They all use the model borrowed from papers of Mark Melitz and his co-authors on the intensive investment margin. The first paper investigates whether the appreciation of the currencies in several transition countries could be caused by improvements in product quality rather than

standard Balassa-Samuelson effect. Calibrating the Ghironi-Melitz (2005) model and applying it to the transition case, the authors find the answer to be positive.

Again, I find the result interesting, but my problem with the paper is that it represents a direct application of a very difficult borrowed model to a seemingly simple question. Why was it necessary to explicitly model the investment decisions of the firms in order to answer the question of whether quality improvements could explain real appreciation? If the only question is about the decomposition of the real exchange rate, why does it matter how these decisions were taken? The decisions could be modeled as exogenous, or simply taken as given, which would make the paper easier to read, while obtaining the same result. Perhaps there is a good answer to that question, but as of now, it looks like the complexities of the model are completely unnecessary, and the authors simply borrowed the model they could find in the literature.

Second, the model does not even look very applicable. It is a two-country model, which is good when someone studies the US against the rest of the world. But transition economies are very small, why not build a small open economy model?

Finally, the authors compare two steady states, the ones in 1995 and 2005, altering the level of domestic productivity between them. This seems very strange – productivity growth is a continuous process, how can one view 1995 and 2005 as two steady states? If the authors build a dynamic model, they should then show a dynamic convergence path or a steady state growth path.

Alternatively, they should build a simple static model, if they go back to statics at the end anyway.

In short, the usage of the chosen model seems poorly motivated, and the model may be too complicated and inappropriate to answer the posed question.

The third paper is subject to much of the same criticism, but in this paper the usage of the model seems more warranted; the paper asks deeper and richer questions about the general macroeconomic path of the transition countries, and does study a dynamic path of a smaller economy instead of comparing steady states. The fourth paper is application of the same strategy to the Czech case.

Despite this criticism, the author has demonstrated skills in both modeling and empirical analysis, and deserves the awarding of the PhD degree.

Sincerely,

Oleg Zamulin