

## Review of Branislav Saxa's dissertation by Jan Svejnar

Overall, this is a nice and promising line of research. However, the presentation is terse and a number of things need to be clarified, if not re-estimated. I am generally also surprised that there are very few references beyond the early-to-mid 2000s, even for working papers, many of which are now probably published. This is a rapidly developing field and the author needs to update his literature review and references.

### Ch 1 Learning-by-Exporting or Managerial Quality

This is probably the strongest part of the dissertation and the author should put more work into it along the following lines:

Can he measure directly changes in management or, even better, change in the quality of management? If not, it is not clear that that term should be used in the title because anything else that is unobserved could be causing the outcome. Exporting v. non-exporting can be measured, so that is okay.

Dependent variable: Why use labor productivity ( $\ln Q/L$  or  $\ln VA/L$ ) if it is regressed on labor? Why not use  $\ln Q$  or  $\ln VA$  if  $\ln L$  is on the RHS anyway? And why not have materials on the RHS in the case when output is on the LHS? If not, then the dependent variable is not very interesting, because an increase in materials could bring about a seeming increase in labor productivity at the time of start of exporting.

The discussion of matching needs to be more detailed. On p. 12 the matching is done on the basis of endogenous variables (labor, etc.). Then we learn that the alternative matching procedure relies on a mixture of endogenous and exogenous variables (labor, etc. as well as exchange rates and relative prices) and that this permits the identification of exogenous shocks v. managerial input. How? That is not clear at all. One page later we learn that *firstyear* is instrumented by exogenous variables (only?).

The coding of *firstyear* needs to be clarified. Is it one just in the year of start of exporting? If so, how is captured the higher level of productivity that is present in the subsequent years? Yet above equation 2 it looks like *firstyear* is coded one for all exporting years.

The interpretation of lagged and lead *firstyear* needs to be amplified. It is not clear what is going on. Are the off years supposed to serve as a placebo test?

Having  $l$  and  $k$  lagged one year in equation 1 is very ad hoc. This issue of having  $l$  and  $k$  in both stages of estimation needs to be resolved conceptually rather than by ad hoc lagging of regressors.

Top of p. 14 – do you know about changes in management? Note that changes in relative prices and exchange rates could also bring about changes in management (e.g., there is a huge price shock, the manager does not handle it well and is replaced).

Data:

Why are post 2002 years not included?

How many firms are excluded by balancing the sample? Are they systematically different?

Tables 2 and 3 are sloppy – what are the first three and second three columns standing for? Bottom lines are missing standard errors.

Appendix Tables A3 and A4 have huge coefficients. This needs to be explained.

Chapter 2: Exporting Behavior ...

This is a nice complement to Chapter 1. Here are a few key issues:

Is the model on pp. 40 Saxa's or is it someone else's? This needs to be clarified.

Equation (5) is not a reduced form equation – it has endogenous variables on the RHS. At best it is some first order condition, but the author needs to show how it comes from the model on the preceding page.

Last paragraph on p. 42, is (7) meant to be (5)?

pp. 46-47 – the coefficient on the lagged dependent variable is viewed as a measure of sunk cost. Traditionally, it is viewed as the coefficient of partial adjustment in a simple dynamic model and it indicates how far the firm moves in one time period toward the optimal state. How is this interpretation to be reconciled with the one offered in the paper?

Chapter 3 : Inflation ...

Looks fine to me. What is striking, however, is how little it is related to any literature in top journals and how toothless (not sharp) are the conclusions. Is this not an important and interesting topic? If it is important, then the author needs to show clearly how it relates to important literature or state clearly that there is a major failure in that nobody has studied this and explain why. The conclusions then need to be sharp in that this model should be used and not that one and why. How much does policy err (in Euros lost) when using the wrong model, etc. The reader should be saying "wow this is a major finding". If it is not an interesting topic, then it's not worth pursuing – I hope that is not the case.

Overall, I think that this is going to be a nice thesis.

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