Opponent's Report on Dissertation Thesis

Institute of Economic Studies, Faculty of Social Sciences, Charles University in Prague
Opletalova 26, 110 00 Praha 1, Czech Republic
Phone: +420 222 112 330, Fax: +420 222 112 304

Author:	Lenka Šťastná
Advisor:	Prof. RNDr. Ing. František Turnovec, CSc.
Title of the Thesis:	Three Essays on Local Public Finance
Type of Defense:	**DEFENSE**
Date of Pre-Defense:	June 15, 2010
Opponent	**Mgr. Libor Dušek, Ph.D.**

Content of the Report:

The thesis consists of three papers, one theoretical and two empirical. The theory paper represents an original contribution to the literature. It takes the set-up from Besley and Coate (2003) and shows how their results change if the spillovers from local public goods make them complements, not substitutes.

The contribution of the empirical papers lies in applying existing techniques to the Czech data, and by doing so, answering two questions about the Czech local public finances that had not been asked (let alone answered) before. The second essay investigates spatial spillovers in public goods provisions by estimating the relationship between spending on specific categories of public goods in neighboring municipalities. The estimates are identified out of the assumed structure of spatial correlation of errors, which allows estimation by maximum likelihood.

The last essay, after its major revision, is of very high quality. The author applies several techniques to assess the efficiency of local public spending by Czech municipalities, accompanied by a battery of robustness checks. She also investigates the determinants of relative efficiency of municipalities. The findings are robust, interesting, and believable. They have potentially important policy implications and are ripe to be communicated to a non-academic audience.

The thesis is based on relevant references and follows upon state-of-the-art literature. It demonstrates the technical competence of the author, both in terms of theoretical modeling (1st essay) and empirical techniques which go beyond the core PhD curriculum (2nd and 3rd essay).

The author should be commended for the way she addressed the comments raised in the pre-defense report. In the first essay, a major extension allowing more flexible form of complementarity of spending in two jurisdictions was added. The extension still has the original, perfect complements functional form as a limiting case. In effect the extension is a new paper. To publish the paper in the best-possible journal, I would recommend turning the paper upside down – start the exposition to the model with the general form, and discuss the perfect complements as a special example at the end. That is both standard in the literature

and also the reader can see from the beginning that the paper is not just a technical exercise with one specific functional form. In the empirical essays, she added data, dropped debatable measures of output (population), added appropriate control variables (tax revenues per capita based on centralized formula, share of college educated population), and run several robustness checks (e.g. testing for spatial correlation in grants).

My last concern is about the negative spillovers in environmental expenditures – whether they are real or they are induced mechanically by the fact that some municipalities receive grants for such expenditures while others don't. The author re-estimates the model with the environmental expenditures that are not financed by the grants, and finds negative spillovers as well. This is consistent with true negative spillovers, of course, but, upon second thought, it could be an artifact of the mechanical correlation. The municipalities that do not receive grants inevitably make-up the lack of grants by their own funds, while municipalities that do receive grants can reduce spending from their own funds. Detecting true spillovers in the presence of such grants would require a very different structural model, which would effectively result in a new paper, while the current paper represents sufficient contribution. Moreover, the test for spatial correlation of grants does not detect a negative correlation in the grants themselves, which may imply that the mechanical negative correlation need not be quantitatively important.

The thesis shows that the author has established a well-defined, focused research agenda (local public finance, empirical studies on local public finance using Czech data) which may continue for years to come.

For the purpose of the thesis defense, no further changes are necessary. In my assessment, the overall quality of the thesis safely meets the quality standards at IES FSV UK. The thesis would be defendable at my own institution (CERGE-EI).

It is a pleasure to recommend Lenka Šťastná's thesis to be defended.

Date:	September 2, 2011
Opponent's Signature:	
Opponent's Affiliation:	Mgr. Libor Dušek, Ph.D. CERGE-EI Politických vězňů 7, 111 21 Praha 1

Opponent's Report on Dissertation Thesis

Author:	Lenka Šťastná
Advisor:	Prof. RNDr. Ing. František Turnovec, CSc.
Title of the Thesis:	Three Essays on Local Public Finance
Type of Defense:	**PRE-DEFENSE**
Opponent	**Mgr. Libor Dušek, Ph.D.**

Content of the Report:

The thesis consists of three papers, one theoretical and two empirical. The theory paper represents an original contribution to the literature. It takes the set-up from Besley and Coate (2003) and shows how their results change if the spillovers from local public goods make them complements, not substitutes.

The contribution of the empirical papers lies in applying existing techniques to the Czech data, and by doing so, answering two questions about the Czech local public finances that had not been asked (let alone answered) before. All essays follow upon state-of-the-art literature. In my assessment, the overall quality of the essay meets the quality standards at IES, although certain improvements (suggested below) are warranted.

Specific comments and suggestions:

1st paper:

The paper takes the set-up from Besley and Coate (2003) and shows how their results change if the spillovers from local public goods make them complements, not substitutes. The author shows competence in setting up, solving, and presenting such models. One concern: Almost all the results hinge CRUCIALLY on the assumption on the output of public goods in region i: Gi= min(xi, kx-i). This is a very EXTREME assumption about the responses in the level of public goods to changes in inputs in one district. For high enough xi, reducing spending does not change output in i, while after reaching a threshold, reductions in spending reduce output in i one-for-one and also in districts -i. (Effectively, the districts engage in an equivalent of Bertrand competition). I find this assumption problematic; at the end of the paper, the author motivates certain examples in which the degree of complementarity would be so strong. Still they seem rather rare. Policy conclusions should be drawn with extreme caution.

While changing the assumption would lead to a new paper, the author should stress the importance of the assumption and discuss intuitively which of the results would (and which would not!) survive if instead output in district i would be a continuous decreasing function and output in district -i a continuous increasing function of spending in district i.

2nd paper

This paper is certainly most interesting and valuable, and also most challenging in the sense of convincing the reader that the estimates actually measure the spatial interdependence of local public expenditures and not the effect of omitted variables that are correlated across nearby municipalities. More should (and I believe can) be done to make the results convincing:

1. I am surprised that the author uses only the cross section for one year and not a panel across a number of years, even though from various places in the thesis one can infer that the data on the municipalities' spending is available for several years till 2006. Using the panel would not only increase the data point but would allow identifying the coefficients on the first differences, could exploit a richer error structure to identify the results (e.g. assuming the spatial correlation of errors is constant over time), exploit lags (extra spending in a municipality in year t affecting the spending in neighboring municipalities in t+1) etc.

2. The estimates are identified out of the assumed structure of spatial correlation of errors (equation 3.2), which allows estimation by maximum likelihood. That's fine. But the identifying assumptions should be stated up front, together with a discussion why we think they are likely to be satisfied, and acknowledging plausible situations when they may not.
3. The estimated magnitudes of rho (0.046 to 0.37 in absolute value) seems quite small, suggesting that perhaps this estimator does attribute some of the correlation in the error terms to beta. An alternative estimation strategy would greatly enhance the reader's confidence in the results – cannot you think of a plausible instrument affecting spending in one municipality that is not correlated with the unobservables that determine spending in other municipalities?
4. I have an issue with the selection of the right-hand side variables. Tax revenues per capita are potentially exogenous, and hence excluded (page 49). This is a huge flaw. For one, the tax revenues are undoubtedly an important determinant of spending. For second, with the Czech system of redistributing tax revenues for municipalities, much of the tax revenue per capita *is* in fact exogenous. Central revenues from most taxes were divided between municipalities according to a formula which was a function of local population solely. So while a municipality could adjust the total tax revenues somewhat by changing the population, the tax revenues per capita are given by a formula. The revenue from these taxes can safely be included in the regressions.
5. The discussion of negative interdependence (p. 56-57) argues that the observed negative spillovers are driven by "real" spillovers. I would suggest more prosaic suggestion: They are driven by availability of grants. Infrastructure and environmental capital investment are funded in a large part by large grants (hope I am right), definitely in a larger part than, say, culture and sports. The grants may be spread out equally across the region, but within the region, they are not available to everyone, so if one municipality happens to have a large grant to reconstruct a water treatment plant, it is more likely that the neighboring municipalities don't have such a grant and hence spend less on environmental investment.
6. The effects of grants vs real spillovers can be, to some extent, separated empirically. Why not run the same regression with the part of spending that is not covered by the grants (and hence is solely under the municipality's discretion)? Second, at least for water pollution, the any spillovers should work only downstream (if an upstream town builds a water treatment plant, the downstream town may decide to reduce its own water spending, but not vice versa). If you can identify water spending specifically, you can test for symmetry between such spillovers for upstream vs downstream municipalities in the same watershed.
7. The discussion of positive interdependence is believable. However, here's another case where using panel data could greatly improve the paper. The alternative stories (competition for residence vs. votes) could be separated in a panel. Presumably, the competition for residents is present all the time, while the competition for votes (or mimicking) should be strongest in the election years, so we should see much larger interdependence in the election years than in other years if the results are indeed driven by competition for votes (or mimicking).
8. Stylistic comment: it has become standard to briefly describe the empirical strategy and the key results in the introduction. (It never hurts to follow John Cochrane's 2002 writing tips).

<u>3rd paper</u>

I have little to say about the appropriateness of the efficiency frontier techniques.

1. I do have an issue with measuring outputs. These will be always hard to define, but including population as the measure of output for culture/sports and public safety is unacceptable. The implicit assumption being made is that there is fixed amount of services per capita that *has to* be provided, and then we just measure which municipalities achieve it at the lowest cost.

However, in culture, sports, and public safety, the services can vary hugely and precisely in these areas municipalities have large discretion over spending (e.g., whether to have a city police and how much to fund it). Population is no measure of output and should be struck out from the paper. (It is an appropriate measure of output for the administrative services, where the agenda of the municipalities with extended powers is simply given by the number of people that come for the ID card, vehicle registration etc. There's some variation across municipalities – e.g. those with higher growth rates of incomes must handle more vehicle registrations – but those should be taken care of in the stochastic version of the model).

2. I suggest a simple check of the credibility of the results: Exploit the tax distribution rules which, in 2006, were still creating large discontinuities for the towns' revenues at certain population brackets. Of course, towns just above the brackets have higher revenues, higher spending, and also higher outputs. But if we measure outputs correctly, we should not expect any discontinuity in the efficiency score around the bracket. This can be easily checked by regression discontinuity.
3. When presenting the results for each town (table A.6), sort by the efficiency score and show the names instead of town numbers. We want to know the good guys and the slackers!
4. The efficiency scores are estimated and presented, but what do we make of that? The paper would be complete with an additional analysis of the determinants of the efficiency scores. What determines such large differences in measured efficiency across municipalities? Population density, growth rates, wages, grants, educational structure of the population, political variables, corruption, some historical dependencies? Just making a visual inspection of Table A6 – it is hardly a coincidence that almost all towns scattered around Prague have fairly miserable scores while among the towns in South Moravia or Vysocina there are many with scores exactly one or near 1. Regional variation in wages and prices appears to be the prime culprit: With the tax distribution formula, two towns with the same population receive exactly the same revenue, but the town with higher wages and prices faces higher cost of providing public services, hence it receives lower *real* revenue and should show up as less efficient. Having myself moved from a budget-rich but wage-poor Jihlava (population 50,000, efficiency score 0.971) to a budget-poor but wage-rich Ricany (population 13,000, efficiency score 0.682), I was startled by the drop in the quantity and quality of public services. By following local affairs at both places, I soon began to suspect that the difference is not driven only by the stark difference in the tax revenue per capita, given by the tax distribution formula, but also by higher cost of providing the services (doing any construction work is way lot more expensive around Prague) and differences in human capital of the people in the city halls (private sector does not offer dramatically better employment opportunities for educated people in Jihlava, hence the public sector is able to attract more educated and more able people). The paper could proceed along these lines to provide accounting for the "inequality" in the output of local public goods (decomposing into inequality in revenues and efficiency, further decomposed by the determinants of inefficiency). In this sense the paper so far does not exploit its full potential.

Date:	
Opponent's Signature:	
Opponent's Affiliation:	Mgr. Libor Dušek, Ph.D. CERGE-EI Politických vězňů 7, 111 21 Praha 1