

Vancouver, July 27<sup>th</sup>, 2012

Dear Professor Jeong,

I have carefully read Peter Ondko's dissertation "Essays on Access to External Finance, Acquisitions, and Productivity". In short, I think Peter's work is of high quality: it addresses important topics, it is carefully executed empirically, and it is nicely written. The whole package conveys a sense of purpose, as the three articles are closely related. One of the articles has already been published in a widely-circulated finance journal and the other two are promising working papers. Hence, the current draft certainly merits a dissertation defense at CERGE-EI and I strongly recommend he is awarded a doctoral degree. My recommendation is based on the high standards we impose on our own PhD students at the University of British Columbia. I also think that the current draft is enough for him to fulfill the dissertation requirement and it requires no major changes for that purpose. Last, I think the last two articles have potential, and I would encourage Peter to polish them after graduation and to submit them for publication to top economics or finance journals. I briefly discuss each of the three articles below.

Chapter 1, which is co-authored with Jan Bena and titled "Financial Development and the Allocation of External Finance", is already published in the *Journal of Empirical Finance* 19 (1), pages 1-25. This means this paper has already survived the rigorous scrutiny of demanding referees in leading academic journals. Hence, I will only say that (i) the question of whether firms in need of external finance are more able to raise capital in countries with more developed financial markets is very relevant from a policy perspective, (ii) the analyses are based on a rich dataset and are carefully executed, and (iii) the conclusion that more developed financial markets allocate capital more efficiently is compelling.

Chapter 3, which is also co-authored with Jan Bena and titled "Productivity Gains from Services Liberalization in Europe", is well developed and has potential for publication in a good economics journal. I found the focus on services of special interest and a fruitful research angle, since most papers I know focus solely on industrial firms. The paper studies the impact of deregulation, and the associated increase in product market competition, on the productivity of services firms. The paper exhibits careful empirical tests based on solid econometric skills and it documents very large productivity gains from deregulation (measured by total factor productivity).

There are some broad-picture questions which come to my mind. I would not ask Peter to address these questions as part of his dissertation, but perhaps it would be interesting to hear his thoughts on these issues during his dissertation defense.

- 1) Is there a way to quantify how productivity gains in services firms spill over to productivity gains on customer firms in other industries?
- 2) What can be said about the economic mechanism through which deregulation increased productivity, e.g., what is the distribution of productivity gains across the various factors of production?
- 3) Is there evidence on the impact of deregulation on firms' investment, innovation, and technology choices?

- 4) There is no evidence that the more productive firms grew disproportionately more in size due to liberalization, but this is something one would expect to occur. Why this is not the case?
- 5) Is there any evidence on the extent to which these productivity gains benefit consumers, e.g., through lower prices of the services?
- 6) Services industries, like utilities, are often associated with natural barriers to entry, like a need of large initial investment and an associated minimum efficient scale. Given this, is there any conceptual difference between what deregulation should accomplish in services industries and in naturally more competitive industries in which entry is easier?

There are also some minor issues:

- 1) In table 3.2, should one scale the changes in the IOL index by something? That is, don't we need to account for the initial level of the IOL index, e.g., using a percentage change? I would let Peter decide on this.
- 2) I agree that clustering the standard errors at the country-industry level is the correct approach, but did not see an explanation of why this is needed in the paper's text. This perhaps can be briefly explained somewhere.
- 3) What is the breakdown on private and publicly trade firms in the sample? Is there any reason to believe deregulation could impact private and public firms differently?
- 4) Are some of the services firms in the sample wholly or partially owned by the government? If so, how does this affect the analyses of the impact of deregulation?

Chapter 2, "Selection and Productivity Gains in Horizontal and Vertical Acquisitions", is a bit less polished than the other chapters and contains several typos which need to be fixed. This chapter deals with an important issue: Why do firms merge, are there gains, and where do the gains come from? It also uses novel data to address this question in a European setting, which is also important given that most existing evidence is based on US data. The paper is well executed empirically.

There are competing theories which aim to explain why firms merge which are based on different assumptions on the redeployability of intangible capital. The paper's insight is that the neoclassical theory applies largely to horizontal mergers (intangible capital is redeployable) while the search and matching model built on the property rights theory of the firm applies to vertical mergers (intangible capital is not redeployable). At an intuitive level, the point is that horizontal and vertical mergers are likely to be driven by very different factors, and thus any empirical analyses must take this key difference into account. The paper shows that both theories are supported in the data once they are tested in the appropriate subset of merger deals, and thus sheds light on why prior work which ignores this distinction obtains mixed results. I think this is an important contribution to the literature and that the paper has the potential to be published in a good journal.

I have a few questions about the execution which do not need to be extensively addressed in the dissertation but are worth thinking about. These are listed below:

- 1) The Rhodes-Kropf and Robinson (2008) model assumes that there is contractual incompleteness which prevents market contracting between vertically related firms. In this setting, a merger accomplishes vertical integration and seems to be the only solution. There are, however, some differences in the degree of contractual incompleteness across countries. For example, contracts are likely to be better in the UK than in Greece. Hence, perhaps one would expect that mergers between vertically related firms are less prevalent in the UK than in other countries. Can these cross-country differences in contract completeness be exploited to refine the tests? Also, I did not see information on the sample composition by country. I would be curious to know, for example, which fraction of all merger pairs in the sample are recorded in the UK.
- 2) The sample includes only mergers among firms in the same country and excludes cross-border mergers. On page 70 it is stated that mergers across borders might be driven by factors other than those investigated in this paper. I imagine this restriction has a large impact on the set of mergers studied and thus I believe this key decision needs a more detailed justification. Given the integration of the EU, cross-border mergers should be fairly common. Is it possible to quantify which fraction of total mergers in Zephyr is between firms in the same country and which fraction is among firms in different countries? Is intangible capital not transferrable across countries, even if firms operate in the same industry but neighboring countries? Are differences in regulation across countries so large as to prevent synergies between vertically related firms?
- 3) One difference between horizontal and vertical acquisitions is that the former are more likely to generate anti-trust concerns. The latter might generate concerns about foreclosure, but I think these are less prevalent issues. Hence, one will only observe horizontal mergers which do not lead to market concentration. Does this have any impact on the analyses?
- 4) In the analyses the standard errors are clustered at the match level, which seems reasonable, but I did not see a conceptual explanation of why it should be done in this manner.

I hope you will find my feedback on Peter's work useful. Please extend my congratulations to Peter on a wonderful dissertation.

Sincerely,

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