

Comment

Ajay Agrawal
University of Toronto

THIS ESSAY OFFERS SOME THOUGHTS on the paper “Location Effects, Locational Spillovers, and the Performance of Canadian Information Technology Firms” by Steven Globerman, Daniel Shapiro and Aidan Vining. The paper was prepared for the Micro-Economic Policy Analysis (MEPA) division at Industry Canada and presented at the conference “Services Industries and the Knowledge-Based Economy” in Winnipeg, in October, 2003. The comments presented in this essay are based on my discussant remarks presented at that conference.

The paper by Globerman et al. has much to offer; it is interesting, important, and provocative. It is interesting because it addresses the topical and intellectually-challenging issue of regional advantage and is one of the few empirical papers to do so using Canadian data. It is important because this topic has clear implications for Canadian productivity and economic growth, and the nature of this research lends itself directly to informing public policy. And it is provocative because it draws conclusions that suggest some reasonably radical implications for public policy: it estimates a significant performance gain that results from agglomeration and thus questions the notion of spreading government support for the founding of new information technology (IT) firms across “have” and “have not” regions. Instead, the paper proposes focusing attention on just a few select “have” regions, such as Toronto. Research results that stimulate thinking and also have direct policy implications are surprisingly rare and deserve recognition.

However, it is precisely because this paper offers a compelling argument in favour of some radical rethinking of public policy that it is important to examine its limitations and consider the implications of these limitations with respect to its overall findings.

It is worthwhile to begin by clarifying the research question that this paper addresses. The authors state that their intention “is to identify *why* specific locations in Canada are more or less supportive of successful IT firms.”²² While the paper does suggest some reasons why regions may vary in their ability to support economically-successful IT firms, the empirical analysis is actually focused only on whether, in fact, regions do vary.

For example, results presented in Table 4 suggest that there is an Ontario effect but do not explain why. The paper begins to examine “scientific infrastructure” but only reports two elements (number of top 50 universities and

total dollars of research performed by sample firms) that are likely concentrated in Ontario. The results, however, do not control for the identified Ontario effect. Similarly, at the CMA level, the focus is on the presence of a Toronto effect, rather than on why Toronto offers IT firms a growth advantage. Again, partial support is found for CMA university research measured in dollars, but this is likely concentrated in Toronto and the results presented do not control for the identified Toronto effect. Finally, similar results are presented in Table 6 that reflect the presence of regional advantage at the two-digit postal code level. (Incidentally, it is interesting to note that these results are not robust enough to accommodate the addition of a control for export intensity. So it would be useful to include prior results at the city and provincial level that control for export intensity, given its significance at the two-digit postal code level.) Thus, it is important to clarify that while the paper does offer a thoughtful literature review concerning theories that explain why some regions offer greater advantages for growth, the empirical results reported here only explore whether or not there actually *is* regional variation in the sales growth of IT firms in Canada. Even so, that is still a very interesting and important research question.

There are many competing theories that explain why there might be variance in regional advantage. This paper builds on the theories of agglomeration and clusters. Although these terms are used interchangeably in the paper, they have been employed with distinct meanings in certain other settings. Namely, agglomeration may specifically refer to the degree to which an industry is geographically-concentrated above and beyond what would be expected given a random distribution of firms relative to the general distribution of economic activity as well as controlling for the discreteness of firms (Ellison and Glaeser 1994). Clusters may refer to the degree to which related organizations (inter-connected companies, specialized suppliers, service providers, firms in related industries, and associated institutions such as universities, standards agencies and trade associations) are co-located (Porter 1998; Marshall 1890).

In addition to the literature referenced in the paper, other studies offer alternative theories that seek to explain why some cities provide better growth environments than others. Although it is beyond the scope of this article to review that literature here, I will mention a few alternative theories to offer the reader a flavour of the other types of explanations that have been proposed. Saxenian (1994) argues that regions with particular industrial cultures (open rather than secretive, cooperative rather than self-sufficient, adventurous rather than risk-averse, decentralized rather than centralized) are more conducive to firm success. Almeida and Kogut (1999) argue that inter-firm mobility (the propensity for engineers to move from one company to another) varies across cities and that those areas with greater circulation within their regional labour networks offer locational advantages.

Additionally, Florida (2002) argues that cities with a “creative ethos” (diverse, tolerant and open to new ideas) offer a regional advantage for firm pro-

ductivity. Agrawal and Cockburn (2003) argue that cities with one or more “anchor tenants” (large, local, R&D-intensive firms with related technological interests) provide a superior environment for growth. In other words, there is a growing list of explanations for regional advantage, but few studies have offered empirical evidence that supports one theory over others. This seems like an obvious direction for new studies to pursue, and the interesting work here by Globberman et al. offers evidence that regions do matter to economies, including Canada’s, and so highlights the need for further research to understand why.

Next, consider the concept of performance that lies at the heart of the empirical analysis. While the dependent variable (sales growth) selected for the analysis is certainly reasonable, it does have a few drawbacks and one in particular that should be noted. The prescriptive tone of the paper suggests that policy should be concerned with maximizing “success,” which is measured here by growth in sales over time. However, to some extent, this measure does not reflect overall performance considering costs. As a result, particularly under conditions where firms may be temporarily focused on market share rather than profitability, such as was arguably the case in the IT industry during much of the period under investigation, sales may be a biased measure of performance.

For example, if labour costs are significantly higher in bigger cities and labour comprises a significant portion of total software development costs, then software firms in larger cities must sell more units, at the same price, than their smaller-town rivals in order to generate the *same* profits. Under this scenario, the bigger city firms would appear to be more successful, even though all firms have achieved equal performance in terms of profitability. Having noted this concern, I am sympathetic to the authors’ decision to use sales growth for their dependent variable. I recognize the difficulty in collecting profit data, particularly from private companies, and so raise this point simply as a caveat for the reader.

It is also worthwhile to note the limitations to measuring “distance” in kilometres. At one point in the paper, the authors estimate the degree to which spillovers from Toronto diminish systematically with distance. Given the geographic distribution of firms in the sample, which are located in a reasonably small number of cities and are unevenly distributed among them (more than 75 percent of firms are located in Toronto, Ottawa, Vancouver, Montreal or Calgary), this metric seems questionable. Is Vancouver further from Toronto than Calgary (or even Winnipeg) in any meaningful way? Similarly, some economists have argued that San Francisco is “closer” to Boston than Kansas since ideas seem to travel faster between the two coastal cities, as evidenced by citation analysis. Why do economists think that distance matters? Usually they cite reasons associated with costs, which include travel time and convenience, among other factors. Perhaps some alternative measures, such as frequency of flights between two destinations, would complement measurements in kilometres to offer a more meaningful measure of distance.

The paper is rather brief in its description of its data sample. While it does note the source of the list of firms (the Branham 300 list of high technology companies in Canada — www.branhamgroup.com), the authors are silent on how this list was originally constructed. Specifically, the reader is left without any ability to gauge the potential for sample selection bias.

However, it is possible to deduce a potential for bias. The following excerpt was taken from the Branham Group Web site, which solicits entries for its list:

As done over the past ten years, Branham is looking for your support and assistance in creating the most comprehensive intelligence on Canadian Information Technology companies. Participating in this listing is an excellent opportunity to receive **FREE** National exposure for your **firm**.²³

The Branham Group is located in Ottawa, Ontario. Consider the following scenario. There is a cost (effort) associated with filling out the application form, especially since it requests specific financial performance information, and so not every firm does it. The Branham Group needs firms on its list and so encourages the high performance firms that it is familiar with to fill out the survey. Since the Branham Group is located in Ontario, it is likely most familiar with firms located there. This would not necessarily bias results if the Ontario firms encouraged to apply are randomly drawn. However, if the Branham Group disproportionately encourages higher performing Ontario firms to apply, it would bias results towards an Ontario performance effect. Far fetched and contrived? Perhaps, but it would be nice to have this type of sample selection concern allayed.

The study is based on data that is drawn from a unique period (1998–2001) in economic history in general and for the information technology sector in particular. The technology-laden Nasdaq Composite Index rose approximately 220 percent from 1,570 on January 1, 1998 to just over 5,000 in March 2000. As of October 2003, it stood at around 1880, only 20 percent above the January 1998 value. Arguably, information technology was among the most volatile components of the Nasdaq during the period under investigation. For example, while the Nasdaq Index doubled in that period, Cognos, the top ranked software firm on the list in 1999, increased in price by over 500 percent. Like many firms in the industry, however, Cognos, lost more than 80 percent of that increase by late 2001.

The paper briefly acknowledges that this was an unusual period and offers two sets of results: 1998–2000 and 1998–2001. However, it would be useful to provide a discussion that: 1) explains in detail why examining these two periods addresses the unusual volatility experienced in this sector over the period in question, 2) interprets the difference in results across these two periods, and 3) offers the authors' thoughts on their remaining level of concern regarding the ability to generalize their results to a more "normal" economic timeframe. While the paper does focus on sales growth (not market capitalization), the unusual conditions of inflated valuations, low cost of capital, and overall "irra-

tional exuberance” could not have left sales figures unaffected. This issue is important and demands consideration.

The paper would benefit from more descriptive data. In particular, it would be useful to know the distribution of sales growth and revenue size across firms, perhaps cross-tabulated with CMAs. One conceivable scenario, for example, is that most firms had modest sales growth, with the exception of only a very few companies that grew substantially over the period under investigation. To what extent are such firms in the tail of the distribution dictating the slope of the regression line? If this is not the case, a table providing these data would allay such concerns. Either way, providing descriptive statistics would confirm that the authors have suitably addressed any particular distributional properties of the data adequately with appropriate econometric techniques.

The paper could offer even more insight by providing some sense of how to interpret key coefficients in terms of their relative magnitudes. While the authors discuss the sign and statistical significance of key coefficients, they do not offer an interpretation of their magnitudes. It is worthwhile to unscramble the functional form and discuss the economic significance of the coefficients, at least at the mean. In other words, certain coefficients are statistically significant, but are they economically important? Such interpretations would help these results offer further insight for policy makers. For example, is the performance premium for locating in Toronto large enough to compensate for the higher labour costs in that city? Conversely, if the federal government continues to encourage the founding of IT companies in “have not” regions, how much is that distributional policy costing in terms of forgone sales growth?

While I fully appreciate the difficulty in doing so, this paper and many like it would benefit greatly if it offered some empirical evidence of causality rather than simple correlations. This is perhaps the single greatest general weakness of studies on regional advantage. The sheer number of competing hypotheses explaining regional advantage that are able to comfortably coexist are testament to the general paucity of evidence that directly supports causality arguments. Clearly, generating such evidence is not trivial. However, any efforts made in this regard would be useful, and acknowledging that the results presented may be suggestive of a causal relationship, but are actually only correlations, would clarify the nature of the paper’s contribution. To reiterate though, this comment applies to the literature in general, not just this paper.

Finally, since this paper offers additional evidence that agglomeration enhances performance, it further inspires the need to increase our understanding of *why* it is that geographic proximity to related firms has this positive effect. As Krugman (1991) points out, agglomeration leads to thicker factor markets, greater availability of non-traded inputs and increased spillovers. Given progress in communications technologies, one might wonder why spillovers could still be mediated by geographic distance. A preliminary study suggests that technology spillovers may occur disproportionately between individuals with social relationships (Agrawal et al. 2003). To the extent that social relation-

ships are mediated by geographic proximity, spillover benefits from agglomeration are understandable.

To conclude, I enjoyed reading this paper. It tackles an important question that is central to much of the thinking behind recent Canadian policy on science, technology and industry as evidenced by the increasing usage of terms such as “technology clusters” and “industry clusters” that have appeared in both federal and provincial government publications. The authors offer compelling empirical evidence that “location matters” and that while distributional policies might appear socially just, they come at the cost of reduced sales performance. The critical points offered above should not minimize the important contribution of this paper, but rather highlight the challenges associated with conducting this type of research. By illustrating the importance of regional variations in economic behaviour, this paper will inspire future research to further explore mechanisms at the regional level that influence firm productivity. Thus, this paper and the stream of research that follows will better inform regional and federal public policy.

ENDNOTES

- 22 Globerman (2003), p. 2, emphasis mine.
 23 Quotation from Branham Web site, October 5, 2003; emphasis theirs.

BIBLIOGRAPHY

- Agrawal, A., and I. Cockburn. 2003. “The Anchor Tenant Hypothesis: Examining the Role of Large, Local, R&D-Intensive Firms in University Knowledge Transfer,” *International Journal of Industrial Organization*, forthcoming.
- Agrawal, A., I. Cockburn, and J. McHale. 2003. “Gone But Not Forgotten: Labor Flows, Knowledge Spillovers, and Enduring Social Capital,” Working Paper 9950. Cambridge, MA: National Bureau of Economic Research.
- Almeida, P., and B. Kogut. 1999. “Localization of Knowledge and the Mobility of Engineers in Regional Networks,” *Management Science*, 45 (7): 905–917.
- Ellison, G., and E. Glaeser. 1994. “Geographic Concentration in US Manufacturing Industries: A Dartboard Approach,” Working Paper 4840. Cambridge, MA: National Bureau of Economic Research.
- Florida, R. 2002. *The Rise of the Creative Class*. New York, NY: Basic Books.
- Krugman, P. 1991. *Geography and Trade*. Cambridge, MA: The MIT Press.
- Marshall, A. 1920. *Principles of Economics*. (8th edition). New York, NY: Macmillan and Co., Ltd. (First edition published 1890.)
- Porter, M. 1998. “Clusters and Competition: New Agendas for Companies, Governments, and Institutions,” *On Competition*. Boston, MA: Harvard Business School Press.

Saxenian, A. 1994. *Regional Advantage: Culture and Competition in Silicon Valley and Route 128*. Boston, MA: Harvard University Press.