

**A Comment on
Thompson, Peter and Melanie Fox-Kean, “Patent citations and the
geography of knowledge spillovers: A reassessment”**

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The measurement of knowledge spillovers is subject, at root, to a fundamental identification problem, which implies that any empirical result in the area must be treated with caution. Professor Robert Langer of MIT, for example, is one of the world’s leading experts in tissue engineering, and is the author of over 120 patents in the area. A large fraction of the citations to these patents are geographically localized.¹ Are they local just because the authors of the citing patents lived in the same city and hence were more likely to learn about Langer’s work (i.e. knowledge spillovers)? Or because Boston is one of the world’s centers for tissue engineering, and so people working in the area are disproportionately likely to live in Boston (i.e. geographic collocation due to other common factors)? Or perhaps it is the case that Boston is one of the world’s centers for tissue engineering precisely because firms locate in the area in order to be able to take advantage of spillovers from people like Robert Langer?

In our 1993 QJE paper (Jaffe *et al* 1993, henceforth JTH) we attempted to address this problem by constructing controls for the geographic distribution of citing patents at the three-digit patent class level. In their “Reassessment”, Thompson and Fox-Kean (2005, henceforth TFK) argue that three-digit level controls are misleadingly broad, and instead construct controls at the patent *subclass* level. Using this more fine-grained control sample TFK obtain only marginally significant evidence of localization; then, using an even more restrictive sample in which not only the citing and control patents but also the originating patent are constrained to share a subclass, TFK find no evidence of *intranational* spillovers at all. They interpret these results as casting doubt on our original findings of localized knowledge spillovers, and in fact on any attempt to assess the localization of spillovers with the help of patent and citations data.

Given the importance of this issue for our understanding of the determinants of economic growth and for economic policy we are, of course, delighted to see further work in this area, and TFK are to be congratulated for their careful data collection and construction work, and for their thoughtful raising of a number of important issues. We take issue, however, both with some of the key assumptions of their paper and with their interpretation of their more detailed quantitative results.

¹ Langer’s patents received 1,264 citations up to December 1999. These citing patents were authored by 3,553 inventors, 982 of which (i.e. 28%) resided in Boston and its surroundings. The other 72% were scattered over 500 locations.

The basic premise of the paper is that 3-digit patent classes are too broad and noisy for the purpose of identifying control patents, since there is a great deal of technological heterogeneity within classes. TFK suggest instead the use of the much more detailed subclass classification, which should render in their view “closer” technologically matched controls. As a matter of fact, though, there is no systematic evidence supporting this view.² We have to remember that the patent classification system has been morphing and growing over time in response to the evolving needs of patent examiners faced with fast changing technologies. While the 3-digit patent classification, comprising about 450 classes, has evolved relatively slowly (i.e. only a few dozen classes have been added or changed in the past 30 years), the subclass classification layer has changed quite rapidly, and it consists by now of about 150,000 patent subclasses.³ Furthermore, there are big differences in the technological scope and “width” of subclasses across 3-digit patent classes. Thus, it is exceedingly difficult to establish the extent to which subclasses correspond to anything akin to well-circumscribed technologies, or to particular industries, markets, or products, and the fact that there are 150,000 of them makes it virtually impossible to assess such correspondence by means of case studies. All this suggests that, regardless of their potential merit, we are not quite yet in a position to use patent subclasses in economic research with any degree of confidence – much more background work is needed. By contrast, a great deal of work has been done using the 450 3-digit patent classes, and for all their drawbacks we have a better understanding of what they stand for.

TFK’s empirical strategy involves the choice of successive samples of citing and control patents based on ever stricter matching criteria, and the testing of the degree of geographic localization of the former *vis a vis* the latter (as in JTH). The fundamental objection to their procedure is that the authors obtain their key result of lack of intra-national localization only after implementing drastic reductions in sample size (from 18,551 to 7,627 to 2,724 to 2,122 control patents) that can hardly be seen as random, but rather are quite likely to lead to sample selection biases that might unwittingly rig the results.

They start with all patents granted between January 1976 and April 2001 that cite the 2,724 patents issued in January 1976; there are 18,551 such *citing* patents. In order to identify a control patent for each of them, TFK search for a non-citing patent classified in the same primary or secondary subclass as the primary subclass of the citing patent, issued within +/- 6 months of the citing patent. However, for a full **40%** of the citing patents no control patents were found this way. This is hardly surprising, given that for most of the period the number of patent subclasses was *larger* than the number of patents granted during any given year, and therefore the probability of finding two patents in the same subclass within a year is very low. Such a drastic reduction in sample size immediately raises the vexing question of possible sample selection bias. In order to be

² Rhetorical devices may be fun, but the authors choice of patent class 231 “Whips and whip apparatus” to make their point is rather misleading. This is hardly a typical class: as TFK themselves point out, in the 25 years of the sample just 78 patents were issued in this class out of over 2 million patents granted during the period, making it by far one of the smallest (and oldest) classes out of the 450 patent classes available.

³ Unfortunately, there is no intermediate level of aggregation between patent classes and subclasses.

fully comfortable with the comparison, one must believe that the factors that lead any particular citing patent not to have a “close” technological match within a year are uncorrelated with the factors that affect geographic location – quite a stretch, given that we know that the patent classification evolve in concert with industry evolution, and that localization effects are likely to fade over time. Suppose, for example, that newly emerging industries tend both to be more geographically clustered and to generate patents at a faster rate than older ones. In such a case, constructing controls at the more detailed level might be selecting for younger industries that are inherently more geographically concentrated, thus spuriously increasing the probability of accepting the null hypothesis of no localization effects.

Table 3, column 5 in TFK, reveals that even after relying on patent subclasses rather than on 3-digit classes for identifying the controls, and even after the drastic (and potentially non-random) reduction in sample size, the localization result still holds at the country, state and CMSA levels. The result goes away (marginally) in column 6 for the state and CMSA level, after further cutting back the sample from 7,627 to 2,466 observations, this time by requiring that the *primary* subclass of the citing patent matches the *primary* subclass of the controls. No explanation is given of why this may be a “better” selection criterion for the test at hand, which is particularly troublesome given that the key result of the paper hinges on it. It could be argued that by demanding that the *primary* subclass of the citing *and* of the control patents be the same one controls in a “tighter” way for technology; that may be so, but then one has to weigh the presumed benefits of the better controls versus the hazards of further cutting the sample size by a whopping 2/3, once again in a potentially non-random way.

The strongest result is shown in Table 3, column 7: this time the sample is further restricted to cases where each triad of patents (the originating, citing and control patents) share a subclass, bringing the sample down to 2,122 observations. This further demand is much harder to justify: after all, spillovers occur not just between innovations in the same narrowly defined field, and hence there is no reason whatever to limit the sample to cases where the originating patent belongs to the same field as the citing and control patents (since the latter two belong by construction to the same subclass, the demand that the control and the originating patent share a subclass amounts to demanding that the citing and the originating patent belong to the same subclass). In fact, doing that implies restricting our attention to the narrowest scope of knowledge spillovers, whereas normally we would like to do exactly the opposite. To return to our opening example, Professor Robert Langer’s patents are widely cited in fields beyond tissue engineering. It is quite clear that taking these wider citations into consideration would render a richer test of the localized spillovers hypothesis, than restricting attention to citations within the narrow field of tissue engineering itself.⁴ This is particularly relevant in this case, in view

⁴ Just to exemplify, one of the first patents by Langer, patent 4373023, received 21 citations, 11 of which of assignees other than MIT. Of these, *none* was classified in patent subclass 435/2, the subclass of the originating patent. On the other hand, 5 of the 11 were localized, i.e. the inventors’ addresses were in Boston. If we were to follow TFK sample selection procedure, all of them would be deleted from the sample.

of the fact that a significant fraction of the work in the area is authored by Langer's graduate students.

Another point of concern regarding the construction of the TFK sample refers to patents sharing the same inventor (those belonging to the same assignee are excluded, which is fine). Since the location of patents is determined by the address of the inventor(s), not of the assignee, cases in which the citing and the control patents are granted to the same *inventor(s)* may not quite correspond to the experimental design of the test. Consider for example the case whereby a given inventor obtains two patents in the same field and close in time but switches employers between them (it happens!). If one of these patents cites a prior patent included in the sample of originating patents and the other does not, the former will appear as citing patent, the other as the corresponding control patent. Obviously this is not a proper control since by necessity the location of both control and citing patents is the same.⁵ Thus, citing and control patents sharing the same inventor(s) should be excluded from the sample.

Having said all this, it is certainly true that “controlling for” technology in order to identify knowledge spillovers is very tricky, and that the exercise in JTH can hardly be regarded as conclusive in that respect. The underlying forces run both ways: knowledge spillovers provide incentives to co-locate, and conversely, the existence of co-location to begin with may encourage “cross-pollination.” An example of a more structured approach to this issue that may help disentangle these forces can be found in Jaffe and Trajtenberg (2002), chapter 7, where they estimate the probability of citations across countries, controlling for technological proximity. One could think of doing the same across states, CMSAs, or any other geographical units within the US, using refined measures of technological proximity. Surely there is room to use patent subclasses as well,⁶ but as said more research is needed to grasp what these finely defined technological categories stand for, before we can rely upon them for identifying proper technological controls.

TFK are well aware of the limitations of their testing exercise, both with respect to the use of subclasses and to the hazards of sample selection, and much to their credit they openly acknowledge them. We fully support their call to link out patent citations data with other types of technological and industrial data in order to address these and related issues: there is so much that can be expected from the empirical analysis of self-contained data (such as the patent data), particularly when it comes to highly elusive issues such as knowledge spillovers, and it is only when these data are coupled with independent sources that their full significance may come to light. Once again, it is extremely important to keep challenging empirical results, however accepted they may

⁵ One could raise the same objections when using 3-digit controls, but then the probability of getting the same inventor is nil and hence one can safely ignore this issue.

⁶ Here is a pragmatic suggestion of how to incorporate subclasses without jeopardizing sample size: for each citing patent, chose the control patent by matching on the primary 3-digit patent classes, and searching for the patent that has the highest overlap of subclasses with the citing patent (in particular the overlap could be zero).

be, in order to promote better and more innovative research, and TFK ought to be congratulated for having done so in this case.

References

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