

## Assignment 3 Answers

### Paper 1: Shibayama, S., Walsh J.P. & Baba, Y. (2012)

Q: As is known, academic entrepreneurship could accelerate technology being applied into practice, bring convenience to the public and promote economic development. But according to the paper, high level of academic entrepreneurship is likely to discourage the academic exchange or sharing behaviors and in turn have negative effect on academic research. I would like to know, if taken effects in both directions into consideration, is academic entrepreneurship good or bad for economic development and the living standard of general public?

A: Your question is something we want to answer, though I don't have a good answer for it. The challenge is that AE and conventional science have various impacts on economy or on society, and each of the impacts is difficult to measure. Even if we can measure them, the next challenge is to make comparison between them; for example, we cannot easily compare the negative impact on academic cooperation vs. the positive impact on technology transfer from academia to industry. The paper tells that there is at least one negative aspect in AE so simply pursuing AE is not appropriate. A more comprehensive view needs further research.

Q: Based on the findings from the report, can University entrepreneurship inhibit the rate of innovation and science development by limiting the amount unconditional sharing? For instance if one scientist wants research from another scientist, but can only contribute to a third scientist, then, if only direct sharing is possible no trade is to be made and the first scientist cannot complete his research. If so, how can the University society adapt to keep its more entrepreneurial form without losing its inventing power (ability to advance science)?

A: Yes, that's what the paper claims. The paper does not really tell the answer to your question of how AE can be compatible with conventional science. Assuming that academics are being commercialized and care more about direct returns, I think we have to give direct incentives for academic cooperation (e.g., sharing) as well. That is, if an academic shares material, s/he gets some credit for it from the community. Coauthorship is one way to do this but it is not perfect because the recipient may fail to publish after all. There should be some other ways. For example, there is a website on which we can claim our efforts for peer reviews (e.g., how many papers we have reviewed last year).

Q: The paper claims that the shift from generalized exchange-based sharing to direct exchange-based sharing, which it has found, may lead to some scientists being unable to request research when they have little research to give themselves. On the other hand, the 'payment' of requesting material was, according the interviewed scientists, only actions of co-authorship, acknowledgement, data feedback and in some very few cases monetary payment. They were only expected to give back research material in the future. This is in my opinion conflicting with your statement about some researcher being left out, because these are things even researcher with no research themselves can give. Would you like to elaborate?

A: I understand your point. The argument in the paper might be imprecise. For the direct-return mechanism to be functional, it is important that donors (who give materials) can expect something in return. As you say, anyone can promise to give coauthorship, etc., but donors might not think that coauthorship indeed realizes, for example, because the recipient is low performers. This is how those who have little to give (in a broad sense) can be excluded from the community.

Q: Without knowing the outcomes if the hypotheses are true, why are they interesting to study? Why is this an interesting research topic? What value does it give to the society?

A: Before this study, literature claimed that academics breach open-science norms only if they are themselves entrepreneurial. This implies that open science and AE can be sustainable at a higher (community) level. But, this study suggests a deeper problem that any scientist can lose open-science norms regardless of their direct involvement in AE. This view implies that we need to implement more direct incentive systems to facilitate academic cooperation, which were not needed in the previous view.

Q: When academic entrepreneurship is highly developed, is it possible that the exchange of materials will become more universal instead?

A: Theoretically it is possible. Commercialism could facilitate transactions mediated by some direct incentives, just as "money" can facilitate economic transactions. A problem is that academics do not use money as a medium of transaction, and that no better medium has been invented yet. At least empirically, the result of the paper showed that transactions decreased.

## **Paper 2: Bercovitz, J., Feldman, M.P. (2008)**

Q: In the Figure 1, what does "=1" actually mean in each part of the matrix?

A: Each quadrant in the matrix is assigned a dummy variable that will be used in regressions. Dummy variables take the value of either 0 or 1. The figure explains what each dummy variable indicates when its value is 1.

Q: Sample question. The writer find 1780 faculty members and in 15 matched departments at the medical schools because he think they are a key venue for academic activity with commercial potential. However, why must choose faculty members in schools, whether it is too limited? Many firms or companies also have the ability in technology transfer, and maybe what they transfer is more typical. Whether this can be affected the writer's conclusion?

A: First, this paper is about "academic" entrepreneurship. That is why the authors focus on tech transfer from universities. Licensing between firms is discussed in different lines of literature, and their conclusions should be different. What the authors meant by "key venue" is that the field of medicine is more entrepreneurial than other fields.

Q: The writer didn't do follow-up survey. As far as we know, some technologies' change need some more time, Now not transformation doesn't means not transfer later, maybe it will cause new entrepreneurs. I doubt the correctness of some datas.

A: Follow-up is nice to do, but of course it is costly. It is a matter of tradeoff between the cost and potential benefit that can be gained through additional survey. By the way, the authors did measure invention disclosure twice in 1991 and 95.

Q: The paper use medical school to analyze the idea. However, I am curious about whether the idea can apply to social science or engineering etc. Since if we consider about the innovation in finance, there are also many theoretical concepts that are applied to the business level. We can also see the engineering sectors have those criteria.

A: Empirical evidence showed that biomed is one of the most entrepreneurial fields. I suppose that is the reason for their sample choice. Social sciences are tricky as their discoveries (e.g., new theories) may not be patentable. Anyway, I think that the theories of this paper is general enough, not peculiar to specific fields.

Q: What are the most important reasons that make so difficult to embrace academic entrepreneurship, besides monetary subjects?

A: I think that the biggest difficulty in facilitating AE is that AE is not appreciated as much as conventional scientific research is. As in the second part of today's lecture, academics are evaluated on the basis of research performance (pub performance). Given that, engaging in AE is understandably considered as a waste of time.

### **Paper 3: Butler, L. (2003)**

Q: Loss of quality and outputs is a classic consequences of the relation supplier-company when it is run in silos and by KPI. In some industries the notion of shared risk and benefits is used to solve this status quo. How could it apply to university - private investors relationship?

A: I'm not sure how the business case can be transferred to academia. One thing that academics are trying to do is to develop better KPIs that incorporate qualitative aspects. Citation count is one of them. We also have H-index.

Q: This question is maybe derived from lack of my understanding. How did the author measure "Expected citation"(Fig.5)?

A: Expected citations is the same as impact factor (IF), because IF is the average of citation counts of all papers on a journal.

Q: Even if Australian falling trend of Relative citation Impact are similar to that of Relative journal impact, the cause of the former may not be the latter. Is not there another possibility that the cause of the falling trend of RCI may be the lower attraction of the Australian publication?

A: I agree. I think that journal impact is only a partial explanation. An alternative explanation is simply that Australian pubs became boring because they had to publish a lot and do not pay sufficient attention to each paper.

Q: What is the reason such an extensive amount of results are presented? Wouldn't one regression study have been enough? How do they complement each other?

A: The claim of the paper is that government's intervention has an impact on pub performance at the national level, but this claim is very difficult to prove because a direct link between the policy and pub performance cannot be shown. I guess this is why the author presented so many indirect evidences that are consistent with his argument. That is, he essentially tries to establish his argument not by showing one robust evidence but by many weak evidences from different perspectives.

Q: When you design a method like this, how well do you have to know in beforehand that you will be able to easily gather the data? (In other terms:) How (rather than where from) did you gather all (non-questionnaire) data?

A: We generally have very good access to sophisticated pub data these days, though the situation may be less sophisticated in the 90s. Nonetheless, I suppose data collection wasn't a very big deal in this study.

Q: How do you feel affected by this academic culture? I.e. what is your view on your career? And what would you recommend someone in the pursuit of an academic career?

A: Having clear evaluation criteria is nicer than having no criteria (as in Japan), since it provides a sense of fair competition. But, having wrong criteria may be worse than having none. In particular, using pub indices (however you measure it) is limited because indices are highly aggregated, essentially discarding a lot of information in each pub. The only way to avoid this is to read the whole paper, which is called "peer review". Obviously, such an effort cannot be always made. Thus, we tend to come to a conclusion that we should use peer review only in important decision making. If any of you intend to pursue academic careers, I would suggest you to understand the culture and mechanism of evaluation in each country, as systems are very different from country to country.

Q: An enquiry conducted by government inferred that government outlays impact on the academic performance. Does it mean that there is a positively correlation between them and is there any lag between investment and the impact they effect on scientific outputs?

A: I agree that the link between government intervention and pub performance is ambiguous. This study followed the time trend of pub performance and blamed the government's policy for the declining pub quality, but you could say that the evidence is weak. I think, however, that is the best the author could do. There should be some lag between the intervention and any effect of the intervention. The paper covers 10 years after the intervention, and I think this is long enough to take the potential lag into consideration.

Q: Why international co-authored articles will gain significant higher average cpp rate than articles written by Australian authors. And I am wondering it is a specific phenomenon in Australia or it is common for all countries.

A: International coauthored pubs are more cited because each author has some acquaintances in his/her country who can cite the paper. This is common in many fields and across countries.

Q: When stressing the importance of international co-authored publications, the author inputs whole counting methods. To have a better analysis, I am wondering whether it is necessary to differentiate first author and second author, and introduce their contribution.

A: You are right. But, my guess is that it won't change the result substantially. From our experience, we know that changing a bit about pub performance measurements does not change overall results.

Q: Is there a relationship between the growth rate of national shares of publication and citation performance? For instance, there may exist a relationship that if the shares of publication increase in a regular and rational rate, it will have a positive effect on citation performance, while if the growth rate is too rapid, it will have a negative influence on citation performance.

A: That is an interesting proposition, though I haven't seen a study testing it. My guess is that the growth of quantity is faster than growth of quality.

Q: About the citation count, I think we should use a concept of effective citation. For example, in China, many research groups will cite articles from their own group to increase the citation number even there's no reason for that, which makes Chinese articles have a rather low IF but relatively high citation number. So I think we should use a concept of effective citation to eliminate the negative impact of self-citation. (But this can be unrealistic to tell what's effective citation, so this is just a hypothesis.)

A: I agree, but I am skeptical that what you suggest was feasible in the time of this paper for a country-wide study. It is not a big deal nowadays, though. To the extent that self-citation occurs to a similar extent across countries, we can give confidence to the results.

Q: The article says that Finland, unlike Australia, not only focuses on research indicators, but also focuses on teaching activities. But is teaching really matter when talking about teachers' researching ability? Will it be an efficient indicator?

A: I agree. Teaching performance is not very relevant in this paper's context. I think that the paper refers to teaching performance only to suggest that the Finish system is different from the Australian's.

Q: Instead of teaching activities, I think maybe peer evaluation would be a better idea. Because teachers in one department do similar researches, so they can understand each other's research more easily and evaluate the research fairly. What do you think of this indicator?

A: Yes, peer review is the ultimate option. But, the problem is that peer review is very costly. We tend to use it only when the stake is very high; for employment, for example.

#### **Paper 4: Shibayama, S. & Baba, Y. (2015)**

Q: From my previous knowledge, many other countries like UK and US also emphasize the IF, but why their proportion of low-citation and low-IF papers is much lower than that of Japan?

A: I think Japan gives a limited emphasis to IF so far, good or bad. I think this partially explains the pub portfolio of Japan that is biased toward low-IF. And, this is also why we see the different IF orientation between Japanese domestic researchers and researchers who came back from the US.

Q: As long as Japanese government wants to boost the biotech innovation, the trend of pursuing publication impact is unavoidable, because the simplest way to arouse the enthusiasm of scientists is awards on funding. Then, according to Mathew's Effect, scientists with better publication performance will absolutely cause concentration. That is to say, the situation now is due to the innovation policy that won't change. So How can we emphasize the publication impact and enhance equality at the same time?

A: I think that high performers should be given more budget than low performers, but it does not mean that the highest performer should take all while others have none. There should be some optimal degree of skewness in funding distribution. This study implies that the current skewness is too much and should be somehow decreased. One thing we can do is to offer a base funding (equal amount for everyone) and competitive funding based on performance. This is something Japan used to do but basically stopped doing.

Q: Since lack of mobility was found to be a key factor, how can we target the source of this issue to increase researcher's perspectives?

A: Lack of mobility in the Japanese case is attributable to several sources. They include inbreeding-oriented employment practices, limited choice of universities if one sticks to career building in Japan (for example, if you get a degree in UTokyo, you find not many better institutions.), unwillingness to go abroad, non-transparent and closed employment/promotion practice, and so forth. These are mutually related. I would tackle the employment/promotion practice first to create a sense of fairness in employment. Then, productive academics can move with a greater confidence in their future employability.

Q: Is there a way to measure the best locations/labs/institutions that particularly improve a researcher's perspective and mimic the process happening there?

A: Literature is not very informative about this, because we need to see the inside of labs (working style, etc.) that are not observable unless we actually go there (or survey them). Labs at the scientific frontier is an obvious option; reasonably experienced academics can usually identify who are at the frontier. Of course, frontier labs may not be the best model to learn from, though. Regardless of exact locations, international mobility is often found to be influential.

Q: In addition to the "publish or perish" mentality, there is tremendous variation in how well written papers are. A paper may not have the most ground breaking result, but it may present ideas in an interesting way for people in or interested in the field. Is this something we should consider? Can we measure how much a paper improves thinking in a field even (This was addressed in part in section 5.1.2)

A: This is an interesting point. Writing skills set aside, making one's discussion appear relevant and contemporary is important to get cited and to get accepted by a good journal. For example, if an academic actively engages in frontier discussions in his/her field, s/he will be able to present the relevance of his/her paper and to appeal to the editors of top journals. Indeed, some interviewees said that being inside (or close to) the leading community that sets a research trend is the most important way to publish in high-impact journals.

Q: How do we find and collect the groundbreaking papers from obscure journals? How do we discard the poorly written papers from high impact journals?

A: One example that came into my mind is “F1000” (<http://f1000.com/prime/thefaculty>). This website gives recommendation to interesting papers (regardless of journal prestige) and good scientists (regardless of conventional pub performance indices). The recommendations are based on peer reviews by academics who dislike the Impact Factors and find difficulty in conventional “quality” measures.