

Public Economics 2013
PS2

The purpose of this PS2 is to promote your thinking on research topic. More specifically, this PS is intended to promote you to think about your research topic more boldly. As I said in the class, there are four components in the paper that has a higher possibility of publication.

1. Your research idea is very bold. In other words, your hypothesis that there is a relationship from X to Y is the one that other people never thought about it, but once we start to think about it, it is very plausible and interesting.

2. You ask a very ordinary old question that the economists have kept asking in the past. For example, the effect of price ceiling on output. However, you use a very innovative research design. What I mean by an innovative research design is that you use an interesting natural experiment, quasi-experiment, regression discontinuity, a unique-institutional setting in your country to identify the relationship between from X to Y. In economics, we call this as "a good identification strategy".

3. You use high-powered statistics, econometrics or mathematical tool to analyze the old ordinary question.

4. You collect the data by yourself through conducting the survey by yourself. This also includes the case that you are the member of the government workers and you have access to the government collected data that the other researchers cannot access. For example, if you can access the micro census data with the detail geographical code and if you can match this geographical code to the local information. This can be an very interesting research project.

Those four elements are the biggest factors(and perhaps only factors) that will increase the publication of your research. If none of those four elements are satisfied, frankly speaking, the probability of publication of your paper is very very small.

The elements that can be more closed related with your situation are 1st and 2nd elements. (If you have a special access to the confidential data of your country's government, then 4th element can be an important factor). This implies that it is very important to think seriously about your research topic before you start your project. Once your research project is started, it is very difficulty to make your paper more interesting. In other words, your research hypothesis and identification strategies(what kind of the natural experiment, quasi-experiment you use) are VERY VERY important. My experience also supports this idea. I have several research papers that did not lead to publication. When I look back, I need to confess that my identification strategy was very weak. There are no more important things that the research question and an identification strategy.

Now, here is the things that you need to do in PS#2.

You need to send me and yourself an E-mail every day. In this E-mail, you just write two bold research topics in 4-5 lines every day with the title of E-mail "bold research idea". It is completely fine that your research topic sounds very stupid. Actually, try to be very stupid in order to be very intellectually bold as much as possible. At this point, you do not have to think about whether this research topic is feasible or not. Keep continue this for two months (if you want, you can continue more than two months. Honestly, I do this even now). After two months, review those E-mails and find the one that is interesting feasible. Do not be afraid to think that your idea seems to be too stupid. Many good papers that are cited a lot starts from a very stupid question. Let me give examples. The following questions are ones that are asked in very influential papers.

1. Does abortion ban increase the crime rate of the cohort who are born during the ban period?
 2. What is the effect of worn on economic growth ?
 3. Does being beautiful help advancement in the work place?
 4. Does the presence of natural resource cause the war?
 5. Are sumo wrestler corrupted in Japan?
 6. Do people delay the timing of their own death to avoid the inheritance tax to their child?
 7. Do people change the timing of their child birth to avoid the income tax?
 7. Did the availability of electric appliance cause the baby boom in 20th century?
- etc. etc.

Those are some examples of seemingly stupid research questions. But it turns out those becomes very important papers (published in a very good journal and cited a lot). So please do not be afraid that your research question seems to be very stupid.

Here are hint of thinking about research topics.

1. Take a shower frequently. A very innovative idea comes when you are very relaxed. Cycle of intensive thinking/study and the the relaxed time often generates a good idea.
2. Read the local news paper and magazine in your country(not economics journal). Find the social and the political problem in those media. Then, think about what kind of economic structures are causing those social and political problems. Think about whether you can test your hypothesis by the data.
3. Do not get the idea from the previous literature. If you try to get an idea from the previous literature, your thinking is dragged to the argument in those literature. I do not deny reading literature. In order to increase your background knowledge of economics, you need to read the literature. But this is for increasing your knowledge in economics, not to get the research idea. To get a bold idea, you need to think freely.
4. Chat with your friend and professor in informal situations. Often this helps a lot.

5. Try to find a unique law, institution, law changes, historical background in your country. Those can provide an interesting natural experiments or the regression discontinuity opportunity.

6. Think about your own unique experience in your country. Try to think why happened.