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How to Make a Bad Research Question?

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You cannot write a good thesis without good research questions. Once you have a good research question, you have already finished the half of your thesis. This note is to help students make a good research question for the study in public management and policy analysis.

1. Ten Tips to Make a Bad Research Question

Here are several tips to make a bad research question for you.

1.1 Pick a Trivial and Boring Topic

If your research topic is not interesting to you, you will be less motivated and become sick and tired of drafting the thesis. Imagine a researcher who is most interested in capital budgeting but decides to study managerial ethics. How about a question “Why does a government employee make a typo in a weekly report?” Is it interesting to public servants or citizens? If your question sounds trivial and boring, you have a bad research question now.

1.2 Pick a Topic that You Do Not Know

If you do not know the topic well, it will be painful for you to go through. Imagine a researcher who has neither computer literacy nor basic knowledge of accounting and budgeting, but wants to study how e-government influences government accounting and budgeting systems (only because e-government is a popular topic). It will be costly and time consuming for him to get familiar with the topic. This type of bad question is neither feasible nor manageable; he/she may not be able to complete his/her thesis on time.

1.3 Answer a Question That Everyone Knows

Think about such questions as, “Is a molecular structure of water really H₂O?” “Does Japanese government have a hierarchy?” (Why not?) “Is Japanese bureaucracy identical to American bureaucracy?” “Do general citizens like dictatorship?” (Who like it?). These questions are valueless and accordingly do not deserve any investigation.

1.4 Answer a Question That Nobody Can Answer

Imagine such questions as, “Are there intelligent aliens in the outside of the solar system?” “Is capitalism better than communism?” “Is a bureaucracy bad or good?” “What is the best strategy to improve public services?” “Which organization structure is best?” These questions are lacking specific contexts and thus sound like “What is life?” “What is happiness?” or “Does God really exist?” Nobody can answer these questions correctly or anyone can answer arbitrarily because no one knows the correct answer. Econometrically speaking, these bad questions are *not identified*; they are lacking some key information or having redundant (or

useless) information; some are not solvable, others have no solution or multiple (unlimited) solutions.

1.5 Study a Private Issue

Public management and public policy handle public issues (see the table below). Think about such questions as, “Why do girls like roses?” “Which fruits do I have to grow in my garden?” “Why did Mr. Clinton have an irrelevant relationship with Monica?” and “Why has Psy’s Kangnam Style become so popular in the globe?” These questions will end up with so called Type III error (Imagine a situation where a patient replies, “I cannot see you!” when a doctor asks him, “Can you hear me now?”).

	Public Management	Politics	Economics	Sociology
Unit of analysis	Organization, community, individual	International, nation, organization, individual	National, industry	Community, individual
Public/private	Public	Both	Both	Both
Virtue	Managerial Rationality	Justification	Efficiency	Social Values
Locus	Government bureaucracy	Party and election	Market	Group, individual

1.6 Study an Issue in Other Disciplines

Some bad questions are “Why did Hatoyama resign?” (Political Science) “Why has Japanese economy slumped for the past three decades?” (Economics) “How did the big earthquake happen in March 2011?” (Geology) “How do nuclear radiation influence human beings?” (Medicine). Again these questions make the Type III error. Although there is no clear-cut between public management and other disciplines (e.g., political science, sociology, and economics), your question should have managerial and policy perspectives. The above table summarizes differences among disciplines at the risk of overgeneralization.

1.7 Have a Broad Scope

One example of too broad scope is a global or nationwide question in terms of the unit of analysis, such as global warming and nationwide government reinvention. As illustrated in the table above, the relevant unit of analysis might be a community, an organization, or individual public servant (not a citizen) in public management and public policy. You need to narrow down your focus *from the heaven to the earth!*

Another example is a broad geological or sectoral scope. Tokyo or Minami Uonuma will be better than entire Japan. Japanese telecommunication industry is more plausible than entire Japanese industries. Suppose you want to study the pollution of the Shinano River in Minami Unonuma, Niigata. One common mistake is to study from the origin (Nagano) of the river through the Niigata Harbor. Your thesis will be million pages long! A more realistic geological scope, for instance, is the Shinano River in Nagaoka City because you think that the pollution there is most problematic (I don’t really know if it is true or not).

1.8 Have a Wide Time-span

What if you want to study Shinano River’s water pollution from the birth of the earth through today? Chapter 1 explains what happened in 7 million years ago; chapter 2 for 6 million years ago; ... How many chapters do you need before writing down the Shinano river today? Again

your thesis will be million pages long! This time-span is not feasible. A more realistic time-span will be, for example, from 2010 to today to study the change in the water pollution before and after radioactive materials emitted from the Fukushima Nuclear Power Plant.

1.9 Have a Vague Question

A bad research question is too abstract and general. Public finance, human resource management, inequality and poverty, e-government, social welfare, or corruption is not specific enough. If you say, “I am interested in human resource management,” then your friends will tell you, “So what? Which aspect of human resource management do you have in mind? Job analysis, recruitment, promotion, education and training, or what?” How about social welfare? A more specific question is the success in the nursing home policy of Minami Uonuma after 2000 rather than Japanese welfare policy.

1.10 Challenge What You Cannot Control

In order to make a bad research question, pick up an issue that you cannot control and manage. As a public manager, you may not decide whether your country has to abolish kingdom and introduce democracy (this is a constitutional decision). You don’t have proper authority to determine budgets of your organization (this is a congressional decision). Your thesis should not conclude that increase in your budget will solve the problem (your job is to manage the pending situation given budget and personnel). You may not change or force court decision on corrupt civil servants to whatever you think is desirable (e.g., death penalty on them). These questions are beyond your control and authority.

2. How to Find an Interesting and Significant Problem

The critical issue is to find a problem, which is interesting and significant from public management and policy analysis perspectives. Consider the following suggestions.

- 2.1 Recollect and describe what you have done in your home country. Pick one of your tasks.
- 2.2 Try to find a big success or big failure; they are interesting issues. You may ask, “Is there anything to be improved or to deserve recognition?”
- 2.3 The big success or failure needs to be significant to citizens, organizations, politicians, scholars, etc. You may show the number of people involved (e.g., the number of casualty), amount of money involved (e.g., big gain or lose), its social and economic impact (e.g., decline in government trust and increase in interest rate), or others.
- 2.4 Try to find a specific aspect of the problem or issue that you think is most important. Do not list all aspects related to a problem.
- 2.5 Check if the problem and issue can be analyzed appropriately by quantitative and/or qualitative data. Do not try to attack a problem/issue if you cannot provide evidence to support your argument. Mathematically speaking, this problem is *not solvable*.
- 2.6 Take a look at academic papers that were recently published in major journals such as *Academy of Management Journal*, *Public Administration Review*, *Journal of Public Administration Research and Theory*, *Journal of Policy Analysis and Management*, *American Review of Public Administration*, *Administration and Society*, *Nonprofit and Voluntary Sector Quarterly*, *Government Information Quarterly*, and *MIS Quarterly*. You will be able to get some ideas about which topics are hot issues in public management and policy analysis.

- 2.7 Check methods that you learned and then think about questions that can be answered by the method.
- 2.8 Recollect discussion topics addressed in the classroom. You may begin with a theory although this approach is more difficult than the experience/data driven approach.
- 2.9 Try to link the problem or issue to theories and models in public management and policy. Your boss wants to get management and policy implications from your study.
- 2.10 Take a look at theses that PMPP graduates wrote. Visit the IUJ library.
- 2.11 Tell faculty members and your classmates about your research questions frequently and get their advice.

3. Checklist

Here are checklists suggested by <http://www.proessay.com/theses-and-dissertations> and manipulated by myself somehow.

- 3.1 Do your advisor (supervisor) and committee members *approve* of your research? This is the most realistic question that you have to answer.
- 3.2 Are you trying to make audience believe what you “cherish and wish to propagate” rather than to answer *significant research question*?
- 3.3 Is your research *significant*? Too simple and trivial? Too boring?
- 3.4 Is the problem *solvable* with the method and data in your mind?
- 3.5 Can you complete your thesis project *on time*? Is your research plausible and realistic? Too broad scope? Too wide time-span? Too costly to get data?
- 3.6 Do you have *experience, knowledge, and skills* required for your research? If not, is it likely for you to acquire them on time?
- 3.7 Do you have sufficient *equipment, personnel* (e.g., interviewers and data analysts), and *fund* to conduct your research?

4. Some Examples

- 4.1 “*Comparing all government departments and agencies*”: It is pointless and less specific. What aspect (mission, efficiency, man power, technology, etc.) of government are you comparing? What is the comparison for? There is no connection to theories in management and policy. Please be specific! A more specific question will be “How do public employees in IRS and OMB differ in public service motivation after 9.11?”
- 4.2 “*Impact of corruption on organizations’ efficiency, accountability, responsiveness, transparency, effectiveness, and trust*”: It is pointless and vague. There are many types of corruption. Too many aspects are listed, implying that the researcher don’t know the target point of research. The question may be revised to “Does immigration officers’ bribery at the airport really erode the trust of the organization in my country?”
- 4.3 “*The impact of reorganization in year 2000 on citizens’ complaints in my organization*”: It is specific and interesting enough for your boss to be interested in your question.