# Writing a research proposal

*Notes taken on    Dr. P's presentation by Emad*

# 1 - Introduction.

## 1-1 Why would you ever have to write a research proposal?

1. To get a research grant to finance a research project.
2. To get a stipend.
3. A requirement for both the MSc and the PhD degrees. It is considered a part of the research.
4. It helps in organizing your thoughts and at the end you can see whether you have achieved your goals or not.

## 1-2 What are the main components and characteristics of a good research proposal?

1. Good ideas should be presented.
2. The ideas must be well presented.
3. You should present yourself as a competent researcher.

It is very important to present your ideas clearly and concisely because if you did not present your ideas neatly, the reviewers will ignore your application. This is because they are overwhelmed with applications which they have to review and evaluate in a very short time (sometimes they have 1 minute
per application!!!). So, you should give enough time and effort in writing research proposals.

# 2 - Research ideas.

## 2-1 Research questions vs. research themes.

If the proposal did not have any general theme and it was full of research questions and ideas, the reviewers will say that the proposer does not have a research program. Conversely, if the proposal did not have research ideas and it contained general themes only, the reviewers will say that the proposer has no specific project. Therefore, the proposal should have a balance of research ideas and research themes. In other words, the proposer should have a framework and some ideas and questions that are formulated within this framework. For example, plant modeling is a general theme. While modeling trees in some way that has not been discussed before is a research idea within this general theme.

## 2-2 Generating research questions.

Do not choose a research question or idea from the list of open problems at the end of the articles.

These ideas are either difficult for the researcher how presented them, or they are not interesting and will have very little, if any, contribution. Nevertheless, some authors present some feasible and interesting ideas to urge people to use the techniques they are proposing. To reach a good idea, the researcher should have a good background of the field he is working in. The researcher should know what has been solved, what is solvable and what is interesting to him/her. Unawareness of these aspects may lead to projects that are not solvable or feasible (e.g. finding a cure for cancer). Consequently, there are two main sources for researchers to find good ideas. First, the supervisor; who has very good experience and who will help the researcher. Second, the researcher himself/herself; by spending substantial time and effort in his/her domain before writing the proposal. This is a period of 6 to 12 months.

## 2-3 Safe vs. risky research.
**"If we knew what we are doing it would have been called research."**
Students in MSc programs tend to choose very ambitious topics which have no scientific roots. These projects are expected to fail. Topics for research should have scientific roots. In MSc, no novelty is required. And in PhD, students are not expected to "find a cure for cancer."
In writing research proposals for grants, researchers should provide a wide range of possibilities. That is, they must include some safe ideas and some risky ones. This should be balanced. They must be aware of the ideas that are safe or solvable and the ideas which constitute some risk.

## 2-4 Evaluating research ideas.

In evaluating research ideas, success should be well defined. In other words, the researcher should know at the end of the project whether he/she had any success or not. It should not be vague to decide. Moreover, the goals should be worthwhile. That is, the goals - if achieved - should be of some use either in further research or in the industry. Another important aspect in evaluating research ideas is the feasibility study. This is very much related to the plan of the research. The researcher should take into consideration the managerial problems of allocating the resources he/she has. These include: students doing research, money, time and equipment. Researchers should take into consideration that planning research constitutes some uncertainty and consequently the plan should not be very ambitious in one direction only but it should have a spectrum of possibilities as mentioned above.

# 3 - Creating bad impression.

Reviewers will get a very bad impression of the proposer if he/she attempted to do any of the following:
1. Trying to show cleverness by writing expressions like "In my experience, I know that so andso ..."
2. Trying to speak like a salesman.
3. Rearranging order of authors in the referenced articles and books in a way that may help the proposer in proving his abilities.
4. Writing a proposal that is too technical and full of jargon.
5. Writing the proposal in a very easy language.

**See Table 1 in Bundy's "The researcher's bible" for a complete list of things that may create bad impressions.**

# 4 - Questions.

**4-1 In the field of computer graphics people are used to borrow some topics from mechanical engineering, biology and others. Will these research projects be acceptable if they have been a PhD thesis ?**
**Answer:** This is very risky. There should be something new. Probably, there will be a referee in the examining committee from one of these fields and he/she must be convinced of what you are doing.

**4-2 What are the differences between an academic research and an industrial research or project?**
**Answer:** In industry, people are interested in applications. They may give grants to professors at universities to do software development. This is not acceptable as research in an academic institution.

Approach to acquiring knowledge in the natural sciences

1. identification of problem;

2. hypothesis;

3. deductive reasoning- decide on procedure;
-- what would be observed if hypothesis was true? how can it be tested?

4. data collection and analysis;

5. derive conclusion.
-- never prove a hypothesis.
-- confirm or fail to confirm Scientific theory.
-- what is a theory? compare to law.
-- characteristics of theories.
-- explains observed phenomenon [explain why].
-- should be consistent with previously established knowledge.
-- should be verifiable [can we test it?]
-- should stimulate further research.

typical sequence
1. selection of problem and derive a hypothesis;
2. review of research;
3. develop method;
4. data collection;
5. statistical analysis; and

6. interpretation of results & conclusions.
1 --> 2 is not necessarily a linear process!!!


The organization of primary research articles follows the steps in the scientific method

Abstract

- brief overview of the article.
- usually 200 -250 words maximum.
- convenience to reader, not all journals require an abstract.

1. Introduction

- states the problem in a general way.
- cites important previous theory.
- justify the importance of the study -- importance should be objectively clear.

2. Review of research

- cite previous research -- what is the background in the field that leads to your study?
- should be evident where your research fits -- look for evidence of bias.
- who is the author? what is the author's affiliation? does affiliation indicate bias?
- if author is strong proponent or opponent of certain theory, may be an indication of bias.
- does author cite relevant research?
- usually key studies will be mentioned over and over again, if these are missing, may signify thatauthor hasn't done a thorough review.
- is review of research biased toward a particular viewpoint?
- are contradictory studies ignored? is biased language used?
- how many articles in a review of research?
- can't review everything, depends on journal space; general guideline, 5-10 key articles should be cited, if only briefly.

3. Hypothesis

- research hypothesis is a statement of what we expect.
- we make a guess about the relationships between variables or the differences between two treatments, etc.
- may be a statement or in question form.
- a good research hypothesis:
(1) sets up a "testable" situation;
(2) gives direction to research;
(3) identifies the variables of importance;
(4) is grounded in theory;
(5) is brief but with clarity.

- Some studies use objectives, instead of a hypothesis -- descriptive study, ethnography.
- ex.- do descriptive study of teacher salary -- look at salary schedules and policies; objectives are to describe level of salary for state and education levels.
- ex.- study sex-role related prejudices in kindergartners observe sex-role related play, record instances of peer learning of sex-role related behaviors, look at influence of teacher.

## 4. Methods

- sampling -- how was sample selected?
- what does sample look like?
- can't study entire population.
- want to get a sample that reflects the population.
- data collection - what data was collected.
- how was data collected.
- does data seem to be reliable and valid (Construct vs. Indicator).
- statistical analysis.
- how was data analyzed?

## 5. Results

- data crunching results are given with level of statistical significance.

## 6. Conclusions

- are conclusions warranted? or do they go beyond the results?
- Look at article critique --website: Author makes statements about teaching effectiveness, not warranted by what was investigated.
- do conclusions answer the research question?
- do conclusions agree with previous research?
- what is the future of research in this field?
- good research often generates more questions than you answer.