Research in a PhD
As from the Social Sciences standpoint

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Abstract. Research methodologies, as broadly known in all science domains, are one of the most active topics in social sciences, namely in education, and this paper builds on that knowledge to contextualize research and general procedures that a PhD student in any research domain shall follow and be aware of. This includes identifying the problem, learning how to review related work and developing research methods that are most suitable to the problems under research. The latter research methods can be quite different in social sciences and computer science, and a bridge is constructed to understand which ideas can be shared, but also what concepts, on a more general and informal tone, need to be added for quality research in computer science.

1 Introduction

Long gone are the times when knowledge was only for the wisest, worshipped for their almost magical power to understand the matter. Research was then a mysterious and just about individual activity, either because the methods were unknown or obscure to other researchers, or simply because the masses did not have the knowledge to understand the primary purposes.

In the modern society, the internet spread leveled the access to information into a global setting, where everyone is only a few searches away from the remotest repositories. Not only did the access to information evolve, but it is now indispensable for a normal life: we all consult the weather before leaving home, read papers and magazines or watch the news. Our lifestyle depends on it.

More importantly, knowledge is provisory: the theories that are valid today may not be it tomorrow, in contradiction to the absolute truth that religion and ancient civilizations used to defend. A valid theory in this sense does not mean it is necessarily correct, but is able to explain the facts that it is confronted to. The discovery of paradoxes or new facts can easily refute or question the validness of a theory. This is a direct result of the number of research projects and people involved in the same tasks and domains. Both in our professional and academic backgrounds, we are pushed to learn new concepts, develop new ideas and even to intervene in the society by contributing to the advancement of human knowledge.

However, this poses a new challenge. The overload of theories and contributions can impact negatively the quality of research, since it becomes naturally harder to select the better or most valuable proposals among a wide offer. The natural question arises: how do we assess good research? But before all, how do we create good research? This is the topic discussed in [4]. The present paper proposes to answer these questions and adopt some of the good practices into the computer science domain.

Research on research methods is an active topic from social sciences, more specifically, from educational sciences. At first sight we may enquire if concepts from social sciences provide an interesting characterization of research practices in completely opposite scientific domains such as engineering and computer science. I convincingly believe that they do, as long as we are concerned about human practices. Research is interdisciplinary - all sciences produce research outcomes and consume research problems to remain alive.

We resume the research process to three different questions, that will be tackled in the following sections:

– Where do we start from? This question leads us to the characterization of the problem (Section 2).
– How to review related work? Reviewing is essential in research to understand the theoretical concepts bound the problem and to situate our work amid others (Section 3).
– How to make good contributions? If we are to fulfill the research objectives, we need to develop coherent methods that encourage quality research and results (Section 4).

2 Describing the problem

All research activities start from the existence of problems: they need problems to be solved. The use of the plural is not unintentional: due to the complexity of problems, one problem hardly comes alone and commonly we do not deal with single problems but with sets of problems with a similar identity. Therefore, describing a problem requires taking into consideration different axes that altogether will help in the development of the project itself: a projection in terms of its definition and context, and a referential involving the criteria used to evaluate the project, namely hypotheses and objectives. The more time we spend on studying the problem, the less surprises we will have when designing or implementing the system. The cyclic behavior of problem solving, and the compound of processes involved in characterizing it are illustrated in Figure 1.

For a researcher, identifying a problem consists in translating his doubts into proper questions that he or someone else proposes to answer to. The first principle is that these questions shall not be too ambitious and try to tackle all problems at once. It is important for quality questions to be clear and concise, precisely for the sake of not being too thorough. Additionally, they shall be realistic and doable within the proposed constraints of time and resources. As a last requirement, every question needs to be a real question, this means, be pertinent and always have a learning purpose.

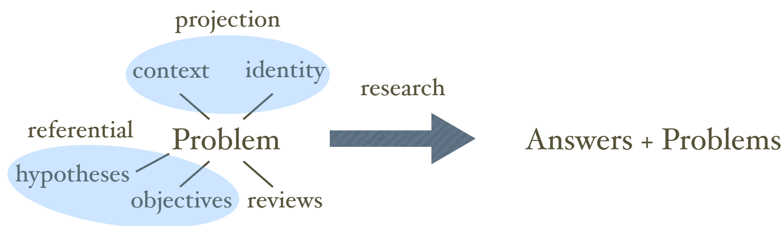

Fig. 1. The problem. The description of a project and related problems involves many different processes: a projection defining and describing its nature, a referential proposing its evaluation criteria and the knowledge inherited from related work. The research activity can then be viewed as a recursive function that consumes problems and returns solutions (in the form of answers) and more problems within a possibly similar context.

Another relevant parameter is the personal and professional context of the involved researchers. Their motivation and previous experiences contribute to the contextualization of the problem. According to Tuckman [5]: criticism (do they credit the potential results?), interest (do they look forward to increase their knowledge on the subject?), theoretical values (do they believe it will impulse new advances on the field?) and practical values (do they believe it will change current practices?) are some of those elements.

The hypotheses and objectives, as deeply dependent on the nature of the studied problems, are an unique referential to assess the fulfillment of the proposed goals. Hypotheses are normally born from the intuition of the researchers and represent provisory assertions that shall be later proven right or wrong. Thus, they may evolve with time and function as guidance for intermediate phases of the project.

Objectives, albeit born from the same nature, are more strict and represent goals that the hypotheses shall help to accomplish. People count the projects we finish, not the ones we start. Iteratively achieving the goals guarantees that we walk along the desired path to success.

The last procedure essential to describing a problem is a critical review of related work. Gathering and comparing different perspectives about a disperse reality is vital to fully understand the theoretical concepts and to justify where and why it differs or improves over existing solutions.

Resuming, it is not surprising that describing a project involves a triangulation amongst the tackled problem, the theoretical background and the methodologies to be used, forming the triangle from Figure 2.

This correlation simply proves that researchers do not start new projects from blank. New problems appear from the results of previous work and the researchers (or their supervisors) need to be mature enough to study the relevant bibliographic references and estimate the techniques and effort necessary to overcome specific issues. Last but not least, the future of a project most times

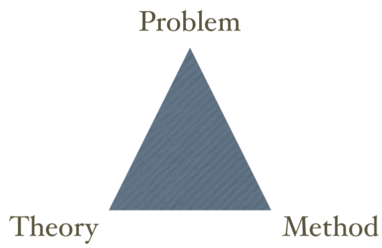

Fig. 2. Triangulation. The description of a research project involves processes from three different axes (problem, theory and method) that can be triangulated. The theory and methods required naturally depend on the properties of the problem.

depends on the ability to convince funding units of the conditions to success and potential value of the overall research. Writing a complete project planning and description is the phase before submission.

3 Reviewing literature

Sharing of ideas is the key concept behind healthy and successful research. As a consequence, the scientific brainstorming exercise consists in publishing our research results in places accessible to others and in reading the reports, articles or books from other authors. But with the vast amount of available information, it is not possible for an human being to absorb all the experience floating around him. We need to train our ability to quickly select what is relevant for our work and what is not. Similarly, others shall not be obliged to read our full work in order to understand what are our purposes and contributions.

For this reason, it is important for a researcher, specially a PhD student, to develop good reviewing skills and be able to use that knowledge to avoid writing imprecise documents that will compromise an easy reviewing by others. In this section we study the different phases of literature review in a PhD thesis, learn how to train our reviewing skills and illustrate bad principles that shall be avoided in our writing activities.

Reviewing bibliography can be split into two phases:

- the **contextualization of the problem**, in the search of a meaning for the theoretical and methodological concepts relevant for the research project. As discussed in Section 2, most of these concepts are required for the contextualization of a problem;
- a deeper **theoretical analysis**, with the review of the documental sources that are indispensable for the study of the problem and will effectively embody the thesis for comparing and criticizing the achieved results.

Problems inherit from other problems, and an extensive literature review provides the essential contextualization when elaborating a new project. Understanding a problem requires studying existing theories and methodologies and confront their *pros and cons* with our intuitions in order to better assess what problems are pertinent research and which deserve more research effort.

Even though a more penetrating analysis is still required further in the research process, a general but still precise and extensive overview is the perfect tool for selecting what we shall review later or discard immediately. Nonetheless, missing an important reference at this point can compromise the value of the project: we do not want to reinvent the wheel and solve what other have already solved.

According to [4], the need for a proficient theoretical referential in PhD theses is frequently a motif of anxiety among student researchers. This request is not only a traditional bureaucratic practice from academia, since the theorization of the study is indispensable for the creation of a research object capable of convincing the community and contributing to significant advances in the research domain. The quality of the research constructions will naturally depend on the existence of a solid theoretical background, what is just another reason for an accurate initial contextualization.

3.1 Reviewing a paper

Bearing in mind the relevance of reviews, the current concern is to provide some guidelines on how to review a computer science research paper. "The first lesson (...) is learning to understand what a paper says" [1]. The most important information can be generally found in the abstract, introduction and conclusion. The inner sections should repeat some of the concepts, but at a lower level of detail and explain the more technical contents. After a *diagonal* or complete reading, we should ask ourselves the following four questions:

What are the motivations of the paper?
It shall be clear in the abstract which are the problems addressed by the paper. There is a normal expectation that they are interesting problems for which the authors have discovered a partial or total solution, potentiating new techniques, applications, design spaces, etc. A problem is inherently constituted by two parts [3]: the people problem that narrates a *beautiful* story on how a solution comes to help the world; and the technical problem that presents the limitations of existing solutions to justify the difficulty to find a new solution.

What are the contributions of the paper?
Typically, the contributions are solutions for the suggested problems. They can represent new methodologies, formalizations, notations, application domains or any other artifacts. The primary purpose is to be innovative, by introducing completely original ideas or by improving previous solutions. This distinction to previous work shall be clear in the sections comparing

related work.

What is the evaluation methodology?
A research paper needs more than rough claims to be of value and publishable. The authors shall foster an argument to substantiate their claims and support it with credible experiences and proofs that are compliant with the early claims. It is the development of a consistent argument throughout the paper that makes the claims scientific.

What are the future directions?
An advancement in an interesting topic readily suggests new future research directions. These may include not only scenarios pictured by the authors during the conclusions, but also new findings and ideas that we might imagine while reading the paper. This is a golden opportunity to come up with ideas for new research projects.

The reviewing task is not finished until we are able to answer to these fundamental questions. A further way to improve the reader's reviewing skills can be to write a short essay (few pages) synthesizing his general appreciation of the paper.

3.2 Types of reviews to avoid

An interesting classification of reviews presented in [4] consists in defining types caricaturing common mistakes made by students in academic reports. The intent is to induce the rejection of these *ridiculous* models, as from the following list:

- summa: represents the attempt to exhaust the topic by writing a full review with everything that has happened in recent years, possibly in other cultures;
- archeologist: exhaustive as the previous one, but according to a diachronic vision of the successive evolutions through time, where even the ancient civilizations can be taken into account;
- patchwork: a messy and unplanned set of concepts that are apparently not related. It becomes harder for the reader to walk through the maze, suggesting that the author was as lost as him when writing the report;
- suspense: the report has a guideline but, as a suspense story, it is involved in mystery and only reveals the facts in the end. The revelations tend to be different than the reader's expectations;
- "rococó": as the architectonic style, this type is characterized by the extremely ornamented texts that embellish the most irrelevant details;
- notebook: lightweight texts that try not to saturate the reader with boring details and resemble standard and simple "for dummies" handbooks;
- theoretical cocktail: studies that cite every sources and combine them "into the same pot" without a clear justification for it;
- useless appendix: reports where the reviews are presented separately from the research results and remain undiscussed through the rest of the document. These type of reviews are frequently referred to as "historical context";

- monastic: excessively long and boring texts, that are consequently cursed to be forgotten;
- social reviewer: this is the kind of work where the writer chooses to cite the most popular authors, independently of the relevance or not of their ideas for the present work;
- colonized: when the author insists to only cite foreign work;
- xenophobic: in opposition to the previous type, when the author insists to only cite national work;
- off the records: the author ensures that his sources remain anonymous, referring to them as “some say that” or “it is known that”. This makes it impossible either to evaluate the credibility of the sources or to credit the original authors for their work;
- ventriloquist: the kind of work where the writer only refers expressions from other authors without adding his discussion of the different or correlated points of view.

When reading a scientific document, a bad literature review can decrease our interest and be a reason to throw it into the “non-relevant bin”. On the other side, a good review increases our interest in the topic and improves the chances that we will keep it in the “most-relevant shelf”. Thus, a quality review is essential to convince the community of the quality of our research.

4 Developing a research method

Although theoretical computer science is able to provide precise formalizations of most software concepts and paradigms (take for example the deep mathematical foundations of type systems and the existing semantic proofs for modern languages such as Java), we are still far away from the universal formulas used in other fields of engineering. For instance, in civil engineering it is an unconditional prerequisite to build bridges that won't fall, whilst in software engineering only rare products, developed for critical systems, offer a comparable level of reliability. Our job in computer science research is to develop those formulas. Therefore, for proposing good engineering methods we need to start from good research and evaluation methods.

We mustn't forget that the results from our work will be reviewed equality or more rigorously than our supposedly rigorous review of related work. Therefore, it is very important to have a good research method, and to justify all the choices along the way. Not only because decisions may not be intuitive at first, but because the research process is not necessarily positivist: we may acknowledge bad decisions or opportunities for improvement that shall be well justified.

In social sciences and natural sciences, ranging from biology to education, the scientific investigation procedures are generally labelled according to quantitative or qualitative methods.

Quantitative methods are good for investigating the properties of phenomena and their relationship. They consist in converting the data, usually collected from questionnaires or interviews, into mathematical models, theories or hypotheses than can be measured using statistical methods. This kind of methods strive for the generalization of the results. For example, election polls are estimated through the analysis of rather large groups of people that, due to their heterogeneous preferences, provide good generalization properties.

On the other side, qualitative methods aim at a deeper understanding of the phenomena. Due to their particular nature, they are more targeted at the reasons that govern human behavior, what is a common subject of analysis in social sciences. These methods employ naturalistic procedures in order to comprehend not only which, what and where decisions are made, but also why and how they happen. This calls for a personal analysis of the data by researchers, that study documents or interviews case by case, implying the usage of more focused samples. Hence, qualitative methods investigate the singularity of each situation and, since they require more research effort, are often used to understand quantitative phenomena.

In [2], the suitability of quantitative and qualitative methods in computer science education research is compared using the example that we describe as follows. Consider that we are studying the strategies used by students to solve problems related to sort algorithms. On the one hand, we can use quantitative methods to discover if the programming language influences the student's performance in solving the tasks and which is the programming language of choice for most students. On the other hand, if our objective is to understand the mental process that guides the students in problem-solving situations, we can run interviews with small groups of students to learn why do the students from those groups use different programming languages and sort algorithms, but without any claim for the generalization of the results. This could be a more interesting result for professors teaching sort algorithms.

In the evaluation of software, computer scientists are more used to quantitative methods. In the context of software quality, quality factors of design and conformance (structuredness, completeness, portability, maintainability, efficiency, etc) are commonly measured by statistical analysis for a great number of generated test cases. Most factors are, but not all. Parameters such as usability and understandability are connected to the properties such as convenience and practicality and, hence, bound to the user *feeling* about the program. However, these are still traditionally assessed via quantitative techniques, but the conversion of user preferences into statistical results provides very important but probably not sufficient results. This is where qualitative methods shall make more sense in terms of software quality: they can be used to learn why users prefer certain characteristics in graphical interfaces above others, for instance, and not only if they, for the general population, do. Understanding the preferences of the users can lead to the evolution of the development methods with a deeper reflection of the essential role of the users, without a separate development that will be assessed later and can be good or bad for the users.

But aren't we deviating from the primary purpose of research? Quality of software is not the same as quality of software engineering research. From the definition, research is innovative: it is based on the intellectual application in the investigation of matter, with the purpose of leveling human knowledge into a deeper understanding of the universe [6]. In order to innovate in software engineering research, we need not only to apply the methods, but to create new scientific methods that solve problems or introduce new paradigms of reasoning about problems. At the limit, we can defeat the very purpose of research and end up developing *yet another* software solution, using the same old techniques that are measured not in a scale of intellectual effort but in man/hour. Also, our goal as researchers is not to be prolific in publications, what is a typical philosophy, but to discover new ideas and refine them into mind-changing prototypes.

It is important for the new methods to be clear, so that others will adopt them. An elucidative quote from Howard Aiken, a pioneer in computing, says precisely that "the problem in this business isn't to keep people from stealing your ideas, its making them steal your ideas!". In this distorted marketing principle, it is all about convincing the others that our ideas are worth exploring and put into practice.

It is also easier to convince the others if we manage to keep our models simple and provide good application examples for our theories. Imagine, as an example, the triviality of explaining how a tricycle works in comparison to explaining how an high-tech car does, or likewise using an human conversation as an analogy for explaining a network communication protocol. Notwithstanding, we need to provide sufficient evidence so that others can replicate our steps or at least understand how we got to the conclusions and formulate their own opinions about it: we can't convince others if they can't reproduce the results.

Assuming a realistic research model, the choices we make can be very difficult and are not always the best ones, forcing us to change our formulations along the way (a scheme of continuous conversations as in [4]). This possibility to reconsider previous choices only sharpens the intuition that we need to justify our decisions for others to be able to follow them.

Of course, in the context of academic research, we still need to prove that our methods are scientific, what demands a serious validation procedure. We start from running believable experiments and building prototypes that are the proof-of-concept of our ideas. Validating them can, for instance, consist in performing quantitative analysis (metrics, statistics) on our examples or checking for the qualitative satisfiability of specific properties (model checking).

From my point of view, whenever applicable, it is our responsibility as researchers to seek for formal proofs for our presuppositions. More in the context of formal methods, typically we invent a solution and verify it later with proper tools such as theorem provers. Although the heavy testing, most industrial applications cannot be labelled to be formally verified: only a criterion of fault-tolerance is estimated, not a proof of correctness. Another more appealing concept is to discover the axiomatic properties of our methodologies and prove by construction that all programs created from those techniques are correct.

5 Conclusions

Concluding, before initiating a research project, researchers need to be aware of what are the concrete problems being proposed, what are the hypothetical solutions for those problems, and what previous research has been made towards solving those problems.

Nevertheless, the goal of research is to creatively find new ideas that constitute important advancements over existing approaches. For those ideas to be successful, it is essential to scientifically validate them inside the community and, when well-founded, convince the industry of their value. Of course, this last selling step already resembles traditional marketing techniques ("you will improve your productivity by 50% with this product"), but the target are not the end-users but the industry itself. In the same way that industry is responsible for improving the quality of computer applications, the job of the academia is to, through research, find the turn point in current software development practices that revolutionizes the way industry thinks of software development.

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