How to Ruin the Carrier of a Ph.D. Student: Precise Guidelines

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IN our profession, one widely utilized Ph.D. work methodology implies the following steps, both when conducting the research and developments (before the work is completed), and when writing the thesis and papers (after the work is completed):

1) Introduction
2) Problem statement and why it is important
3) Existing solutions
4) Proposed solution that is both application and technology aware
5) Details
6) Conditions and assumptions
7) Mathematical analysis
8) Simulation analysis
9) Implementation analysis
10) Conclusion
11) Acknowledgements
12) References

Details of this methodology are elaborated in [Milutinovic2003]. One example of the use of this methodology is given in [Bush2008]. Working on each one of the above defined steps, Ph.D. students can undertake activities and create habits that could form irreversible damages to their research mentality and ruin forever the chances to become a real scientist in the future. The text to follow discusses the major pitfalls of each methodological step, using the following template: (a) Axiomatic statement, (b) Short explanation, and (c) Illustrative example, from real life of a Ph.D. student. The first template\element always has a negative connotation, the second one is typically neutral, and the third one is always positive (with a reference to a Nobel Laureate statement at VIPSI conferences).

1) Keep in mind that the Ph.D. thesis is the crown of a research carrier, and has to be a perfect piece of work, to be conducted for years, even decades; definitely not just a proof that a person is able to solve scientific problems using scientific methodologies, in conditions when the real research starts after the Ph.D. thesis is defended. It is the fact that many Ph.D. research activities, for a variety of reasons, take too long. At some universities, especially in East Europe, many researchers obtain the Ph.D. not long before they retire, which is a problem, as indicated by Nobel Laureate Ivo Andric.

2) Keep in mind that each problem has many elegant and simple easily understandable wrong solutions; select such a problem for your Ph.D. research. The approach is especially effective if one chooses a problem that is not important for the present day technology and applications. In real world, it is the responsibility of the Ph.D. thesis advisor that the student selects an important problem to work on, and the advisor, rather than a student, is to be blamed for missing directions, as indicated by Nobel Laureate de Gennes.

3) Keep in mind that one has to master all existing solutions to the problem before one makes an attempt to create something novel. Such an approach will definitely lead the student into directions not taken by others. It is well known that the Nobel Laureate Marconi discovered that short waives do bounce off the ionosphere, because he dared to do the related experiments in conditions when nobody else dared, because a guru of the field published a paper 'proofing' that something like that is not possible; the inventor did not know about all existing work in the field.

4) Keep in mind that educated newcomers into the field never create good new ideas; only experienced experts can create breakthrough ideas, by using a bottom-up approach (in technology related considerations) and an inside-out approach, developing the idea before thinking about its use (in application related considerations). Actually, the fact is that the accumulated knowledge (which may not be relevant any more) could create blocking obstacles in the process of our creative thinking and decision making. If one takes the bottom-up and usage-ignoring approach, one lacks wide views and fails the exam of time, as indicated by Nobel Laureate Arno Penzias.

5) Keep in mind that one should never share the details of an invention with others, because

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they will steel it and abuse it. Some researchers do not go to conferences (time waste), and publish their work only in journals (that brings the SCI credit, which is typically a formal requirement for oral defense). The fact is that one obtains the best ideas when trying to explain the initial ideas to others, as pointed out in several keynotes of Nobel Laureate Jerome Friedman.

6) Keep in mind that wrong assumptions (obtained by oversimplification) will create good results. It is the fact that narrowing the assumptions and conditions of the research increases the probability that one creates something novel, but narrowing beyond the absurd line turns the underlying assumptions into wrong assumptions, since the contact with reality gets lost. If one lives 24 hours with the Ph.D. thesis problem, and is obsessed with it, one will create original solutions without introducing any technology and application restrictions, as in the case of the seminal discovery of Nobel Laureate Harold Kroto.

7) Keep simplifying the problem until it becomes solvable. By doing this, one typically creates a useless result. The right approach is to invest into the mathematics-oriented education, so complex issues are not a taboo any more, as proposed by Nobel Laureate Martin Perl.

8) Keep in mind that one does not have to be a good programmer, if doing a Ph.D. in computer science and engineering; software tools will do the necessary job. Some researchers advocate that the purpose of Ph.D. research is to create ideas, not programs. The fact is, however, that one has to touch and feel the problem (e.g., by mastering the programming related details), before being able to create an effective simulation environment, as indicated in the keynote of Nobel Laureate Robert Richardson.

9) Keep insisting on an ideal implementation, since academic implementations (those including bugs, errors, and stupidities of the un-experienced) are worthless. Actually, such implementations are the best enablers of extremely efficient market oriented industrial implementations. Trying is the best catalyst for breakthroughs, as stated by Nobel Laureate Herb Simon.

10) Keep obsessed only by price and performance; do not care for issues like availability, reliability, feasibility; they are of secondary importance. Actually, “abilities” are typically much more important in technology and application considerations, and notoriously omitted. Only holistic approaches and solutions they create will survive technology and application revolutions, as indicated by Nobel Laureate Kenneth Wilson.

11) In the case of sponsoring, give research money only to experienced professors, never to a Ph.D. student exclusively; only the advisor knows how to find the best use of that money. Ph.D. students who rely on the excusive guidance from the advisor will never become creators of breakthroughs. Some USA research sponsoring agencies do recognize the importance of this issue.

12) In the case of references, go after quantity, not quality, both when creating a list of references, and when publishing your own work (better have 300 rather than 3 references in your CV). Actually, in some of the best universities of the World, researchers are judged for promotion based on only the best 3 papers: in such conditions, a researcher with 300 papers on the CV is judged based on only 3 he/she selects, and is obviously handicapped in comparison with another researcher who created only 3 papers (all of them superb, because he/she did not care to waste time on non-breakthrough ideas). Some Japanese research sponsoring agencies did already adopt this view.

Stupid will never agree to exchange his brain with the brain of a genius (since he believes that his brain more valuable), which is so frequently true, especially in research communities.