Industry - University Research Cooperation

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1. Introduction

It is now almost universal. In computer science, significant funding will only be available if there is industry involvement. Unless it improves competitiveness, safety, health or some other public interest, Computer Science need receive no greater investment than mathematics or other fundamental science. If however, academic research is found to be helpful in product development, the payoff can be far greater than any careful investment. Support for cooperative projects is now available, but many university/industry exchanges do not prove relevant to developers. In other cases, the work is relevant to the industrial partner but there are no more generally applicable research results. The project is frustrating if the parties have conflicting expectations.

2. What not to attempt with industrial/academic cooperative research projects

In this section, I discuss some false expectations that can lead to failure and disappointment.

2.1. Technology transfer

Partners often enter into an agreement with the understanding that the purpose is the transfer of technology from academia to industry. In fact, in the area of formal methods, there was an open campaign to do this. In my view such efforts can fail for two reasons

• The technology is not ready. Technology that is developed on the basis of toys that are not used (or of real use) often fails when the real problems are encountered.

• The company is not ready. Recent research results are often (always should be) based on an understanding of understanding of mathematics and science that was built up over centuries. Most academics and graduate students know this material and take it for granted. Industry people have often not even been exposed to it, or have forgotten what they were taught. Often, if they were taught some facts or notation they were not taught how to apply it in their work and they never use it. They need basic education and training before they can use the new ideas.
In many cases, both reasons apply. I believe that research and technology transfer should never be confused. Technology transfer is a separate effort that should begin when the method has matured to a point where its application is no longer research.

2.2. Inexpensive programming for product development

I have seen industrial partners take graduate students, put them in a project, and have them “gain experience”. Results for the partner are often good; they get a bright, young, enthusiastic employee at relatively low cost and without making a long-term commitment. Often however, the student gets a very narrow view of the world, accomplishes little in the way of research and is using his educational period for things he could get later in life, while working. The company can also lose; after investing in training a new person, he/she leaves.

2.3. Consulting

On occasion, I have been given “research contracts” that turned out to be consulting at a low (or even zero) rate. When we got into the task, we found that we did not have to look for new solutions to problems; everything was already known (usually published). A research problem must have some aspect that is new to the researcher and offer a chance to solve problems that have not been previously solved. Where that is the case, both parties are better served by a consulting agreement. Students should not be employed on consulting projects unless they are paid as consultants.

2.4. Premature tool development

Many good research ideas will not gain wide acceptance unless they are supported by tools. However, Universities are not good places to develop production-quality tools and tools should not be rushed to serve a particular project. Generally, you should not build a tool to support a method until you have applied the method several times without a tool or with primitive tools. Only after “doing without” can you know what you really need in the supporting tools.

Building the tools is not generally a research project and, once you have “proof of concept, the work should be spun off to an experienced company that can maintain the software and adapt it to the changing needs of a real user community.

2.5. Proof of method/tool validity

Many researchers enter into a project to show that their ideas will work. Often a team of Ph.D. researchers will produce something that “proves” that they can use method and/or tools for a real problem. In most cases, what they have proven is their determination, will-power, and perseverance. Development personnel do not pick up on the approach and the researchers, “proof” published, move on to prove new things.

3. The purpose of industry/academic cooperation

For cooperation in research to succeed, both sides must agree on the purposes of the project.

3.1. Improve not prove

Proving that something works, or is useful, is the end-phase of research. The real purpose of any industry-academia cooperative is to improve the methods. Industry must accept that they are not getting a demo of a finished idea; the researchers must know that they do not have it “right” yet.
3.2. **Attack a real problem not a toy**

For every method, there is a problem it will solve. If researchers make up a problem to illustrate their approach, it will fit their model and the approach will work. Many times, the real problems do not fit the model. The purpose of the industry-academia cooperation should be to use problems that were not made up. Usually, they should be as small as possible, but they must be real. However, in many cases, it needs to be a newly designed product, but a real product.

3.3. **Work with developers, not (industrial) researchers**

Many large companies have internal research labs for which they hire academic researchers. These jobs are viewed as very nice because they are better paid and don’t have teaching burdens but industrial researchers are often not closer to real problems than their academic brethren. While they may be good partners, the project should also involve those who are developing current products.

3.4. **Look for the real problem**

Researchers often try to solve the wrong problem. For example, in my work in the 70s my manager told me that the problem was that they did not know how to specify interfaces. Today, I realize that their real problem was that they were designing with unreasonably complex interfaces. The interface specification problem is not yet fully solved but, in practice, we can design components with simple interfaces and we can specify those.

In other cases, I see people developing tools trying to find and display information about complex systems. Often the “real solution” is to build the systems in a different way so that the information is easily found without tools or with simple tools. For example, if you have a highly structured requirements document, you do not need sophisticated “traceability” tools. With a free-thought document, the best tools don’t help.

4. **Final remarks,**

Many researchers consider publication to be the final goal and the toughest test of a research effort. If our goal is to do industrially-relevant research, then our real goal is not publication but use. Our institutions reward researchers for publishing, not for solving problems. We need institutional recognition that the use of ideas is at least as tough a test of research as publication; often, it is easier to publish something useless than to get a report of useful research published. Ultimately, applied research needs to pass both tests: Use shows that it is applicable, publication shows that it is new, sound, and well-understood. Neither is good without the other.