APPLIED RESEARCH: A SCIENTIST'S PERSPECTIVE ⋆

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Abstract: In the past two decades, research has experienced a fundamental shift from pure basic research towards strong application orientation. By boosting applied research, so the idea, industry should benefit from immediate revenues, shorter time to market, and better value for research money in general. Whether this policy shift will also have a positive effect in the long run, however, is a matter of much debate. This paper reviews the resulting situation at the project level, thereby complementing the usually policy-centric discussion. More specifically, it is argued that the prevailing practice of "commissioned" applied IT research projects is unsatisfactory and threatens scientific research capabilities in general. An alternative "problem-driven" style of applied research is described, which utilizes a basic research setting for solving potentially relevant problems. The major tasks, responsibilities and prerequisites for problem-driven applied research are outlined and exemplified by means of a particular research project. A proposal for a Web-based infrastructure supporting this type of research is also sketched.

Keywords: Basic research, strategic research, applied research, development, technology transfer, research funding, SME.

1. MOTIVATION

The booming economy in information technology shows that, contradicting gloomy 1980 predictions, IT industry managed quite well to translate research prowess into commercial advantage (Mowery, 1999). This success, however, should not obstruct the fact that present revenues are obtained at the expense of the mid/long-term perspective. More specifically, fueling applied research has undoubtly accelerated the rate of adoption of new technologies, but basic research has been cut back to achieve this. Therefore, although the back-end of the innovation pipeline has been strengthened, the risk of a pipeline stall due to lacking novel ideas and technologies lurks around now. As IT firms face a steadily increasing pressure to either survive in the race of providing low-priced high-quality products or to invent truly innovative ones (George E. Brown and Turner, 1999), this might end up in severe problems in the future.

Given the potentially dangerous consequences for any industrialized economy, the above problem is a serious challenge to politics as well as science & technology. Consequently, related issues have been controversially debated for years, cf. the U.S. National Academy of Science's home-

⋆ Invited plenary speech at the joint IFAC WRTP'2000 & AARTC'2000. The referenced research projects SynUTC and W2F received support from the Austrian Science Foundation (FWF) grant P10244-ÖMA, the OeNB "Jubiläumsfonds-Projekt" 6454, the BMfWV research contract Zl.601.577/2-IV/B/9/96, and the Austrian START programme Y41-MAT. For further information see *http://www.auto.tuwien.ac.at/Projects/SynUTC/* and *http://www.auto.tuwien.ac.at/Projects/W2F/*

page *http://www.nas.edu*. Need-driven ("Jeffersonian") basic research —called "strategic research" or "oriented basic research" in Europe— has been proposed as an attempt to fill in the gap between pure basic ("Newtonian") and applied ("Baconian") research. Basically, it tries to get the best of both worlds by utilizing basic research's inherent creativity, freedom, and independence for solving problems that are known to affect society or technology. If appropriate funding policies and frameworks were established, so the theory, it should be possible to overcome the false dichotomy "useful = enemy of the good" (Branscomb, 1997).

Naturally, most of the available contributions to issues in technology consider the problem from the funding agencies' perspective and pay special attention to major clients like universities and corporate research labs. Not being involved in any policy making process, a researcher like the author cannot substantially add to this position. Some very positive personal experience with Jeffersonian-style research projects, however, confirms anew that this type of research works well in practice. Moreover, as the particular structure of economy in Europe enforces cooperations primarily with *small and medium-sized enterprises* (SME), a way of applying the general theory to research projects suitable for SMEs can be contributed, along with the prerequisites that make the resulting *problem-driven research* possible and effective. Note that considering issues relevant for SMEs is important in general due to the fact that they are increasingly taking over the focus of IT innovation (Branscomb, 1997).

The remainder of this paper is organized as follows: In Sec. 2, history and current status of research funding is briefly outlined. Sec. 3 reviews the current practice of commissioned applied research at the project level and discusses its (negative) consequences. Problem-driven applied research is presented as an alternative in Sec. 4, along with a brief discussion of its prerequisites and advantages. Sec. 5 surveys the basic features of an envisioned Web-based infrastructure that supports problem-driven research, as well as the steps required for setting it up. Some conclusions in Sec. 6 eventually round off the paper.

2. GENERAL FRAMEWORK

The ultimate rationale justifying government funding of research and development is concisely captured by the following quotation from (Branscomb, 1997): *The engine of U.S.*[1] *innovation and productivity growth is the private sector; the fuel is private investment. The primary federal role should be to foster an economic climate that favours private investment in R&D and promotes the effective and innovative use and absorption of technology by firms, while ensuring a vital and productive infrastructure of people, ideas, and institutions to draw upon.* Government support of research is a major and well-established tool for achieving this mission; its actual implementation, however, involves many controversial issues.

According to (Holton and Sonnert, 1999) and (Branscomb, 1999), the following two extremes of research approaches can be distinguished:

(1) *Basic research* ("Newtonian research"): Research in response to curiosity about the workings of nature, with no other pragmatic motivation.
(2) *Applied research* ("Baconian research"): Application of existing knowledge on behalf of a sponsor with a specified problem to solve.

Only a few decades ago, most research was pure basic research and hence largely unaware of the concerns of practice. Major companies ran their own corporate research labs, which offered the creative atmosphere required for this type of research. Beginning in the 1980's, however, a combination of severe competitive pressure, shrinking innovation cycles & time horizons, disappointment with perceived returns on their rapidly expanding investments in R&D, and a change in federal antitrust policy led many companies to externalize a portion of their R&D (Mowery, 1999). Since then, there is an increasing trend to place the primary focus on the company's short-term problems, to cut back on corporate research, and to outsource research (primarily) to universities.

A similar shift towards applied research occured in government sponsorship. Despite of the widely accepted fact that part of the public research effort, especially that associated with the postgraduate training of the next generation of scientists and engineers, must be driven by the insatiable curiosity of the best researchers, politicians increasingly faced the problem to explain to their voters why all this money is being spent on research (Branscomb, 1999). Blind faith in the power of intellectual commitment could no longer replace an accountable process based on clearly stated research goals. Some scepticism also rooted in the fact that, being part of the international shared research, basic research allows all nations to benefit.

As a consequence, various measures ensuring "useful" research have been implemented in virtually all major funding programs. The U.S. Government Performance and Results Act (GPRA), for example, enforces strategic planning at all gov-

[1] The European Commission's Action Plan for Innovation (Commission, 1996) states similar goals.

ernment agencies. According to (Cozzens, 1999), the resulting message to individual researchers is: *Follow your best interests and talents in forming your research agenda, but also think about the routes through which it is going to benefit the public.* An even stronger application orientation has been built into the European Commission's 5th Framework Programme (Commission, 1999), where proposers must assess the economic and social benefits and the potential for applying the results in a "dissemination and exploitation plan". Selected projects must eventually submit a detailed "technological implementation plan".

Needless to say, there is much debate about those issues. Some portions of the political sector consider basic research far less worthy of government support than applied research, whereas other politicians castigate the support of applied research as "corporate welfare" (Holton and Sonnert, 1999). The essentials of the various controversial arguments of proponents and opponents are quite simple and neatly captured by a quotation from (Branscomb, 1999): *Are we faced with a Hobson's choice between a withering vine of public support for a free and creative science that is seen by many as irrelevant to public needs and a bureaucratic array of agency-managed applied research, pressing incrementally toward public goals it hasn't the imagination to reach?*

The gap between basic and applied research could possibly be filled in by a complementary third type of research, which has been proposed as a rescue by several authors, see (Holton and Sonnert, 1999) for an overview:

(3) *Strategic research* ("Jeffersonian research"): Basic scientific study of the best sort with no sure short-term payoff but targeted in an area where there was a recognized problem affecting society or technology.

Although dedicated support of strategic research is still lacking, it is not a new idea — President Jefferson's decision to launch the Lewis and Clark expedition into the western part of North America dates back two centuries ago. A more recent IT example is the DARPA strategic program, which enabled major breakthroughs in computer networking (Branscomb, 1999). It is perhaps more surprising, however, that there was even an attempt to compile a list of problems relevant for Jeffersonian research from an appropriate poll of federal agencies in 1978 (Holton and Sonnert, 1999).

3. COMMISSIONED APPLIED RESEARCH

In this section, applied research will be reviewed from the research projects' perspective. Special attention will be payed to the needs of SMEs, which are increasingly gaining importance for IT innovation but do not benefit from research as much as they should (Branscomb, 1997), (George E. Brown and Turner, 1999). Backed up by some —admittedly limited— experience with applied research projects, and knowledge of the general situation at TU Vienna and a few other places in Europe, it will be argued that this cannot be explained solely by insufficient research expenses but indicates serious structural deficiencies as well.

In what follows, the term *research* is used to characterize work that simultaneously

- targets an unsolved scientific or technological problem,
- requires special skills and/or equipment,
- involves considerable risk of failure,
- cannot reasonably be bounded in time.

Research constitutes the front-end of the innovation pipeline and is usually performed by universities, corporate labs and government institutions.

Given the very specific nature of research work, it is well-known that traditional project management and evaluation techniques are inadequate for research projects. More specifically, since the problem under study is usually complex and often vaguely —if at all— specified, it follows (Cozzens, 1999), (Branscomb, 1997) that

- success depends primarily upon the investigator's interest, curiosity, creativity, capabilities, and luck,
- major breakthroughs occur unpredictably in direction and timing,
- there is potentially unbounded spillover, both imported and exported, that prohibits accountability w.r.t. both resources and success (Cozzens, 1999),
- practical applications are often unimaginable at the time of research.

In addition, creative problem solving usually requires a climate of freedom, independence and stimulating input from as many sources as possible. Hence, classical project management paradigms are definitely inappropriate here:

- Identify milestones in advance and, even worse, assign deadlines for achieving those. One simply cannot impose deadlines on research work, where months, even years, without apparent progress are common.
- Reduce delivery times by assigning a problem to multiple researchers. A research problem cannot usually be solved within half of the time by two researchers (there might be some speedup, but it is certainly more chaotic than linear).

- Re-assign problems to researchers on demand. It is usually void to assign a research problem to somebody who lacks interest and/or skills.
- Measure research performance and progress by simple quantitative measures. Relying (solely) upon performance figures like the number of publications easily ends up with crowding quality (Cozzens, 1999).

The term *development* is used to denote the process of adapting existing scientific results and technology for particular applications. Development work thus covers both traditional development as well as technology transfer issues. It constitutes the back-end of the innovation pipeline and is usually performed by industry, perhaps in cooperation with a researcher.

Like research work, development can involve subtle issues that need special skills and/or equipment. The distinguishing property, however, is the absence of an unsolved problem and the resulting ability to reasonably assess both risk of failure and working time. This in turn renders traditional project management techniques applicable to development projects.

Despite the controversy basic vs. applied research, it is a widely accepted fact that research and practice (that is, development) are within a more or less tightly coupled feedback loop. Innovation demands that scientific results are eventually transferred into practice, and that problems arising in practice are an important —although not the only— driving force for research, see Fig. 1.

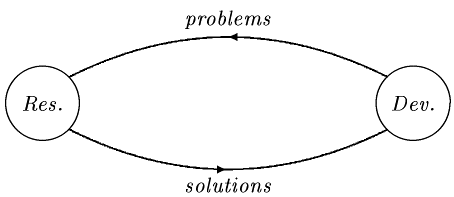

Fig. 1. *Feedback loop for innovation*

Therefore, it is undeniable that the shift towards applied research described in Sec. 2 has its merits. Still, even if the most serious objections against applied research, namely, loss of the mid/long term perspective and "corporate welfare" (Branscomb, 1997), are exempted from the discussion, there remain serious concerns originating in the way how (part of) the R&D community responded to the shift in policy.

More specifically, research is nowadays often considered as a service that can be invoked *on demand*, that is, when the development of a particular application requires it. The major advantage of the resulting *commissioned applied research* is the unrivaled information flow between research and development, in both directions, although this is only true for the particular partners and not for the R&D community as a whole. In fact, there is an early dependence upon the industrial partner, which usually impairs free dissemination of research results. Other general problems with applied research are narrow scope and short-time perspective, lacking freedom to choose research problems that suit a research group's interests and capabilities best, micromanagement of research by the sponsoring party, and suppression of intellectual freedom and independence in problem solving (Branscomb, 1999).

The major deficiency of commissioned applied research, however, becomes clear when "unfolding" the feedback loop of Fig. 1; the resulting R&D interaction is shown in Fig. 2.

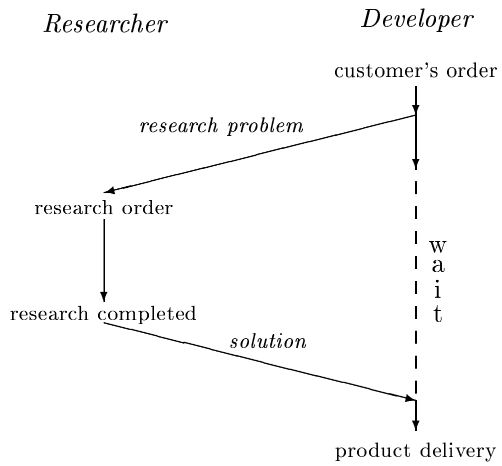

Fig. 2. *R&D interaction in commissioned applied research*

It is immediately apparent that the time for completing the development project fully incorporates the time required for performing the research work, which is per definition risky and difficult to bound. Moreover, since it is usually impossible to negotiate in advance how research results are to be delivered, unpredictable delays and responsibility problems are likely to occur. Hence, it is not too surprising that commissioned applied research projects either (1) tend to fail or (2) are no research projects at all.

(1) Failing projects usually cause financial loss and hence frustration of industrial partners, which makes them reluctant to cooperate with research institutions again. This "at-most-once" type of cooperation is quite common today.

(2) Avoiding failures of commissioned applied research projects by actually performing development projects is even more problematic. Recall that, from the perspective of project management, both basic and strategic research meet the earlier definition of research work. Applied research in

the sense of Sec. 2, however, actually classifies as development work.

First, any public research lab or university conducting development projects without a substantial technology transfer component [2] actually competes with development firms on the market. This competition, however, is not fair due to public funding of infrastructure and/or staff and should hence be abandoned. Unfortunately, most publicly funded (academic) research institutions in Europe face increasing pressure to get private funding, and in view of today's research-averse climate, there is usually no alternative but to accept (hidden) development contracts.

This unfortunate development is primarily fueled by the many comparatively low-tech SMEs in Europe, which often view research and development as basically the same. Their representatives are seldom foresighted enough to appreciate (not to speak of envision) research that has the potential to really boost their profits. Moreover, sole familiarity with development projects makes it difficult for them to properly anticipate the actual deliveries of a research project, so that inappropriate expectations of the returns when signing a research contract are quite common.

Lacking venture capital is certainly one reason for this general deficiency of European SMEs, although some —not particularly uncommon— personal experiences indicate additional causes as well: Insufficient competitive pressure and risk aversity. More specifically, Austrian and German companies can easily afford to cancel or refuse a basically free-of-charge adaption of SynUTC's time distribution technology for some commercially promising applications in their specific fields. U.S. firms cannot afford this, even if there is no free-of-charge offer.

Second, running development projects at research institutions creates a steadily increasing tendency to replace research staff by development staff. After all, research skills are quite different from development skills, which implies that researchers are usually incapable of doing professional development work. This tendency, however, seriously threatens the scientific communities' research capabilities, and has particularly dangerous consequences in academic institutions (like the author's department) that adhere to the principle of indivisibility of research and teaching: A staff of developers will certainly produce competent developers, but who will eventually educate tomorrow's researchers? Note that those arguments apply, to some extent, also to institutions that conduct development projects with a substantial technology transfer component. After all, it is a different matter to do some own research or to make up some foreign one.

[2] Note carefully that commissioned applied projects make perfectly sense for technology transfer centers and similar institutions that push existing research results into practice!

4. PROBLEM-DRIVEN APPLIED RESEARCH

The Jeffersonian style of research outlined in Sec. 2 provides an alternative to commissioned applied research that does not trade in research for application orientation. Indeed, recalling Fig. 1, the essential question to ask is *who is initiating research activity?* In the scenario of Fig. 2, it is the commissioning industrial partner who initiates a "cycle" of the feedback loop. This is, however, not the only possibility: A researcher could initiate a cycle of the feedback loop as well, by deciding to investigate a problem of practical relevance and putting the results at the disposal of later development projects. The following Fig. 3 outlines the R&D interaction in the resulting *problem-driven applied research*:

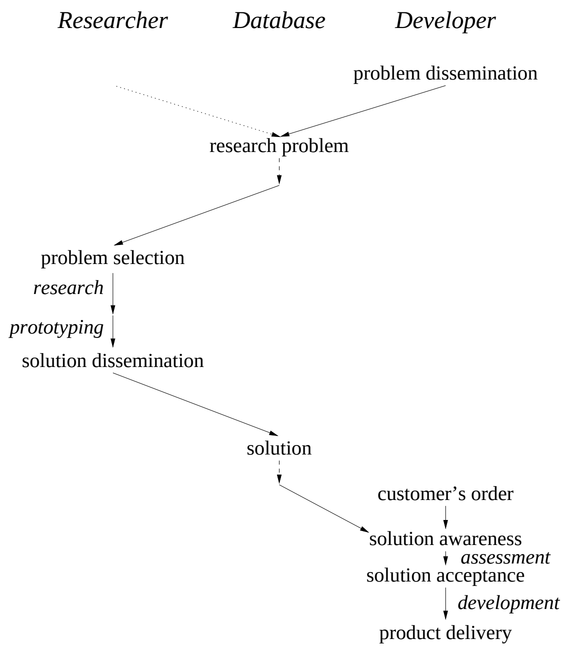

Fig. 3. *R&D interaction in problem-driven applied research*

Since research and development project are decoupled from each other, they can be set up in a way that suits their respective needs best. Most importantly, research can be conducted in the creative, free and independent climate of a basic research setting. Still, some preconditions must be established to make problem-driven research feasible and effective for SMEs:

(1) *Information flow*
It is of central importance to provide infrastructure and tools supporting the flow of information

in both directions. Ideally, researchers should have access to an up-to-date collection of problems relevant for practitioners, and developers should be able to quickly get information about existing solutions for particular problems. Ease of access is certainly a key issue here, see Sec. 5.

(2) *Accomplishments*
Research work can be done without commercial risk and pressure, something that is not true for industrial development work, particularly in SMEs. Hence, it is the research project's responsibility to provide results that are useful in practice and properly made up as well. Results based upon unrealistic models, for example, which abstract away too many important details, are not particularly useful. Moreover, a research prototype implementation and related documentation should be provided to allow an interested party to assess the solution without difficulty and in short time. Note carefully that writing scientific papers is definitely not sufficient for this purpose.

(3) *Funding*
Problem-driven research needs some kind of two-tier sponsoring: First, a sponsoring organization that supports basic research projects without asking for of short-term applicability of results is required. Later on, additional support for the more applied part of the project, namely, prototype development, must be obtained. Note that the latter could be seen as some sort of public "venture capital", which is to be refunded by industry upon transfering research results into applications. Presently, the required two-tier support can only be "simulated" via grants from different sponsors, since one of the goals and modes of support is usually antagonistic to a single sponsor's policy. This practice, however, is definitely suboptimal and puts a serious burden on project management as well.

As far as the advantages of problem-driven research are concerned, it is apparent from Fig. 3 that industrial partners do not have to share the high risk and unpredictable working time involved in research. In addition, assessing available results can be done without any obligation, and SMEs will particularly appreciate —in fact, require— the ease of evaluation offered by a working prototype.

Research benefits from this approach in several respects: First of all, freedom and effectiveness of research is maintained since it is the investigator who selects a problem that suits his/her interests and capabilities best. Moreover, as there is no obligation to achieve particular goals, the danger of risk-averseness and betting on the predictable instead of the revolutionary (Cozzens, 1999) is also avoided. Last but not least, free dissemination of the accomplishments —and/or filing for patents by researchers, as enabled by the Bayh-Dole Act (Mowery, 1999) and similar laws in Europe— is not restricted, since there is no early dependence upon an industrial partner.

To show that this type of research works well in practice, some experiences from a recently completed problem-driven research project SynUTC are appended. The goal of SynUTC [3] (*Synchronized UTC for Distributed Real-Time Systems*) was to solve the problem of how to achieve μs-accuracy GPS time distribution and external clock synchronization in fault-tolerant distributed systems. The work started some time in 1994, without any particular application in mind, but with considerable scientific interest in the topic and the vague idea that such a technology could be useful some day.

Enabled by support from the Austrian Science Foundation (FWF), a complete theory for advanced interval-based clock synchronization, several suitable clock synchronization algorithms, the foundations of the required hardware support, and a versatile simulation toolkit were developed. This work was performed in a basic research setting and developed indeed most successfully. We should point out, though, that the scientific problems were considerably harder than they would have been in a pure basic research project. Actually, the complexity introduced by realistic modeling (incorporating things like clock granularities etc. in full detail) was almost overwhelming — and created a number of unexpected problems as well. To mention only one, journals (not to speak of conferences) are quite reluctant to publish papers of 40–70 pages in length, which are nevertheless unavoidable to accommodate the analysis of a clock synchronization algorithm in our framework.

A remarkable positive experience during the research on SynUTC was closing a 13-year-old problem put forward by Leslie Lamport. The way how it happened shows clearly why a basic research setting is mandatory for real innovation: It was in 1996 that the idea of a new fault-tolerant intersection algorithm (FTI) popped up, without raising much interest first. More than three years later, however, it was suddenly realized that an advanced analysis technique could be applied to FTI, and that doing this would settle Lamport's problem. Actually performing the work and writing the paper was a matter of a few days. Needless to say: In an applied research context, something like this would not have happened.

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[3] To save space, no references to the many SynUTC publications will be given in this paper. Comprehensive information is available via the project's homepage *http://www.auto.tuwien.ac.at/Projects/SynUTC*.

In 1996/97, additional funding from the FWF and other public sponsors was obtained for prototype implementation and evaluation. Given the fact that the related expenses were applied ones and considerably higher than the basic research costs, this was a very difficult and tedious matter (and succeeded only due to some very fortunate event). Anyway, enabled by this additional money, prototypes of the UTCSU-ASIC, the NTI M-Module and various software device drivers for integrating SynUTC into a commercially available RTOS were developed, concurrently with the scientific research on SynUTC. At the end, a complete prototype implementation consisting of Ethernet-coupled VME CPUs, along with about thousand pages of technical reports containing its documentation, was available as a basis for experimental evaluation and assessment by interested parties.

In 1998, the first promoting activities like exhibitions at fairs and conferences were launched. Although kept to a minimum, they eventually attracted the attention of several promising applications, like online fault detection and locationing in power distribution grids. In the meantime, several technology transfer projects with European and U.S. firms are conducted by a spin-off company, which is run by a former collaborator. Note that this transfer of responsibility was necessary, since neither personal interest nor the basic research policy of the FWF left room for technology transfer projects, patent issues, and advanced promoting efforts like participation in standardization activities. Naturally, from a scientific point of view, such activities are not very interesting. On the other hand, it became apparent soon that only a primary researcher can adequately promote scientific results.

Professional development efforts eventually pushed SynUTC's technology to 10 ns-range accuracy over fast Ethernet and led to various improvements and simplifications mandatory for practical applications. However, apart from some (modest) consulting, there is no further involvement of research staff in the commercialization of SynUTC.

5. IMPLEMENTATION ISSUES

This section is devoted to some considerations on how the prerequisites mandatory for problem-driven research can be effectively implemented. More specifically, it is primarily the information flow issue (1) in Sec. 4 that deserves special attention: Research needs a list of relevant problems, cp. (Branscomb, 1999), (Holton and Sonnert, 1999), and development must be granted access to potentially useful results. Since the success of the problem-driven research paradigm depends critically upon an effortless access to this information, a dedicated Web-based infrastructure — termed *Research and Development Information EXchange* (RADIX) here— linking up research institutions and development companies should be provided.

Any attempt to set up a system like RADIX can rely upon a lot of experience and data from similar initiatives launched in the past. Solutions databases like the European Commission's CORDIS (*http://www.cordis.lu*) or the NSF database mentioned in (Cozzens, 1999) should in fact provide many features suitable for reuse. Still, one should carefully analyze the reasons why existing databases did not come up to expectations w.r.t. usefulness and acceptance:

- High overhead for (and rationale of) supplying information.
- Insufficient search capabilities.
- Maintenance of the (centralized) database.

After all, no developer would spend even ten minutes on filling in forms for disseminating research problems he/she knows of, and only a few developers would spend an hour on searching a collection of solutions of research problems, even if it is made up properly and a successful search would be beneficial. Database maintenance obligations and cost issues would reduce acceptance even further.

However, the rapidly advancing Internet technology has changed this situation considerably: Utilizing a Web-based system, it is just a matter of a few mouse-clicks and/or keystrokes on anybody's desktop PC to retrieve/submit information from/to databases located anywhere in the world. Centralized database maintenance becomes superfluous or can be kept to a minimum, and the Internet's ever increasing bandwidth and computing power provides search, access and communication capabilities not known before (and any limitation that might be present today might vanish tomorrow).

Therefore, the primary advantage of RADIX over past approaches lies in the fact that a powerful technology to rely upon is at hand: By means of a carefully designed, simple and easy to operate Web-based system, it should be possible now to get people round to provide and use the information required for problem-driven research. To be effective, RADIX should at least provide the following features:

- Access to a research problems database.
- Access to a solutions database, which should also incorporate commercially available ones.
- Additional communications support (up to video-conferencing) required for problem selection and solution assessment.

- Maintenance of case studies, which are often indispensable for research for assumption verification and testing purposes.
- Maintenance of requests for technology transfer support.

All those features are to be provided under several more or less crucial constraints:

- Ease of access.
- Minimization of user's responsibilities (automatic indexing etc.)
- Avoidance of flooding with unwanted information.
- Minimization of explicit (central) maintenance.
- Support of different levels of security to protect confidential information. Note that this must also include measures for establishing general trust into RADIX and its tools.
- Support of different levels of cooperation.

For publicity and hence acceptance reasons, the actual implementation of RADIX should ideally be pursued by a renowned institution. After all, to create a system that is really useful to its clients, a careful requirements analysis based upon input from prospective users is mandatory. Attracting suitable volunteers, however, would be much easier if RADIX was backed up by a prominent organization. The project itself could be conducted by a small project team with expertise in such diverse fields as technology transfer and Internet technology, according to the following basic roadmap:

(1) Selection of, say, 5 research institutions and 10 firms that provide input for the requirements engineering phase. A call for participation should be issued to find suitable candidates. Selected volunteers must of course be reimbursed for their participation, and should be located reasonably close to each other to allow frequent visits by project members.
(2) Requirements engineering phase based upon the participants' input, resulting in a detailed system specification. A powerful communications infrastructure —probably including sufficient travelling funds— is certainly a key issue here.
(3) Implementation, installation and testing of RADIX. Based on the system specification, the required software for clients and servers must be developed. This task should be performed by a professional development firm.

Once RADIX has been set up and tested, its services can be offered to interested participants everywhere in the world. Ideally, it should be able to live without further funding — if not, participation fees might be considered.

6. CONCLUSIONS

This paper castigated the current trend of conducting commissioned applied research projects at research institutions as unsatisfactory and even dangerous with respect to research capabilities in general. An alternative problem-driven style of research has been described, which utilizes basic research for solving problems that are suspected or known to be relevant in practice. The required prerequisites for problem-driven research, with special consideration of the requirements of SMEs, were outlined and exemplified by means of the research project SynUTC. Features and required steps for setting up an envisioned Web-based infrastructure supporting this type of research have also been sketched.

Given the author's very positive experiences with problem-driven applied research projects, there is sufficient evidence for a strong personal preference of this approach. It remains to be hoped, however, that more researchers will eventually become attracted by this promising alternative to the current practice of applied research.

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