# Some Thoughts on the FGCS Project*

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I am pleased to attend the 1992 FGCS international conference. In particular, I am happy to have had the chance to visit with many members of ICOT and discuss the evaluation of the FGCS project with various industrial representatives. I thank Iwata-san for making local arrangements and Uchida-san for his continued interest in Argonne and its research programs. Nitta-san was very helpful in explaining the demonstration programs, and I am pleased to have had his assistance. Yamazaki-san and Ishihara-san, both Japanese industry representatives on the ICOT technical board and ICOT staff, have been open about discussing the future of ICOT and the role of basic research in Japan.

My involvement with the FGCS project began in 1986 with activities and participation in the Gigalips project, which was organized by Argonne and inspired by the FGCS project. In 1988, I participated in the joint ANL/ICOT NSF workshop in AI held at Argonne. This workshop gave me the opportunity to begin to understand the hardware and software aspects of the FGCS project. Later, as part of an ANL and ICOT joint project, I visited ICOT several times and was involved in installing PSI-II workstations and network connections at Argonne and in developing programs in KL1.

Too many people were involved in my various visits to mention them all. However, I mention especially Ichiyoshi-san and Susaki-san for their friendship and hospitality, and Furuichi san and Minami-san for offering to let me visit their homes. All of my interactions with ICOT staff have been highly positive. I have enjoyed my interactions immensely and wish in some fashion to continue these personal relationships.

In the remainder of this report I focus my comments on the topics raised in my evaluation presentation.

Evaluation of the FGCS Project.

First I want to make the point that the Western view of computer research and development processes is possibly quite different from that in Japan and this difference in view is largely responsible for the difficulty in assessing the significance of the FGCS project accomplishments. I believe that there is confusion about whether the FGCS project was a basic research project or an advanced development project.

I believe that the evaluation of the FGCS project will take considerable time and effort, and I also fear that the international community will not fully understand the impact of the FGCS on Japan and even on the world computer science community. I firmly believe that Japan has become a significant force in the computer science community and that the important point is that this position was achieved during a short-term project and for modest cost. Japan should not waste this opportunity to remain actively and productively engaged in a core area of basic computer science research. FGCS has, to a large extent, decided the directions of the logic programming community and heavily influenced parallel processing projects around the world.

Many in the United States are confused about how to evaluate the FGCS because the Japanese R&D process is not well understood. However, the average person in the United States does not fully understand the R&D process in the United States either! What is important is that the process of becoming open–the distribution of software and the evaluation of progress–be continued.

It is also important to remember that in basic research a negative result is not a failure but that the process of uncovering truth is pursued despite setbacks from time to time.

Many people, I think, desired to evaluate the FGCS project as an advanced development project, where an inability to get to product development is considered a failure. What many do not understand is exactly what the goals really were. Did Japan really want to develop prototypes for products? Was there a hope that industrial companies would adopt the technology and revolutionize the computer industry?

The most difficult point for the outside community to consider is what specific problems have been solved and what technological breakthroughs have occurred. The lack of clearly showing these things has caused many to discount the accomplishments.

The United States was evaluating the FGCS as both a basic research project and as an advanced development project. As a basic research project and as an advanced development project, therefore, it could have been considered a success if one or more hard problems in AI or CS had been solved or if a commercial company had committed to producing products based on the results of development. Have these things happened but not been revealed?

When the FGCS project was first announced, it created a storm of controversy in the United States and Europe. I think that both countries feared the project for two main reasons.

1. It fundamentally challenged their notions of preeminence in basic research.
2. If commercial products resulted from the project, Japan would have taken a

lead in knowledge-based systems–an important new paradigm–with little Western response possible in the short term.

This shakeup caused many government-sponsored projects to be created in the West (MCC, ECRC, Alvey, SICS), and even now we see the United States federal High Performance Computing and Communications project to have been influenced by the FGCS project. In this initiative government and industrial firms are teaming to develop systems and software.

Lessons Learned.

What lessons have I learned from the FGCS project?

1. Be aware that government-supported industrial consortia may not be able to "read the market," particularly over the long term. This limitation probably means that joint government-industry projects should be short term.

2. Do not confuse basic research and advanced development (i.e., know what you are doing, and don't confuse the evaluation criteria for the two). It is important that funding agencies and the community know what type of project one is working on and how that project will be evaluated.

3. Expect negative results but hope for positive. Mid-course corrections are a good thing. Assessing the direction and expecting that research may change direction are key to keeping projects relevant to the goals and to changes in the "real world."

4. Ensure that the basic research infrastructure has stability, a strong sense of the important core problems, flexibility, and an evaluation mechanism that can distinguish between negative results and incompetence.

5. Have vision. The vision is critical: people need a big dream to make it worthwhile to get up in the morning. The most important role of a project leader is to focus energy and attention on maintaining the vision and direction of large projects. The vision has the power to unify a group and motivate them to work through hard problems. "Make no small plans, for small plans have no power to stir men's souls."

Impact and Accomplishments of ICOT.

I've been thinking about the impact and accomplishments of ICOT since my first interactions in 1988. I have included here a specific list of accomplishments of ICOT based on my discussions and experience during the past four years.

+ Can one built a whole computing system based on logic programming and provide a useful tool for applications use? Answer: YES

+ Are the resulting systems so much easier to use that people will immediately switch from conventional computing systems? Answer: NO

+ Does special-purpose hardware give KBS a performance advantage over general-purpose hardware? Answer: NO

+ Can logic programming and KBS be applied to variety of applications areas? Answer: PROBABLY YES

+ Is the world likely to adopt KBS systems as a major alternative to object-oriented systems development environments for non-numerical computing? Answer: PROBABLY NO

+ Can logic programming and KBS open a new world of applications areas with the same effect on society (and markets) as numerical computation did in the 1950s and 1960s? Answer: TOO EARLY TO TELL

+ Did the FGCS project succeed in giving Japan new visibility in the world computer science community? Answer: ABSOLUTELY YES

To get the answers to these questions required much effort and resources. Japan was the only country willing to take the risk and to invest in obtaining these answers. The need to take risks and to try to do something new is essential. The United States and Europe have in many respects lost the ability to take these risks as a normal part of doing research. Perhaps as a result of economic decline or the collective loss of imagination, U.S. companies and government have failed to remain on the leading edge of risk-taking in large projects. I hope that Japan does not get discouraged by the international criticism of FGCS to abandon risky projects. Perhaps the RWC project is a step in the direction away from risk taking. I don't know for sure, however.

Recommendations.

I would like to make a few specific recommendations regarding the future of ICOT and the basic research agenda developed during the past ten years.

First, I think Japan should establish long-term funding for basic research in computer science and focus this work on three areas:

1. Parallel processing, performance evaluation, etc.
2. Knowledge-based programming systems
3. Combination of symbolic and numerical computation

Second, MITI–with the new policy for software distribution–can alter the view of U.S. and European governments by making all basic research results publicly available from the very beginning of any new project.

Third, research leaders in Japan need to encourage true collaborations that involve the setting of joint research objectives, joint funding, and joint management of the projects.

Fourth, basic research efforts should concentrate on software for general-purpose machines and should let industry develop the hardware and operating systems software.

Fifth, Japan should encourage smaller, more independent research groups that may be distributed with less central control, perhaps some in universities.

Acknowledgments.

I thank all the wonderful people at ICOT who have made the interactions so pleasant during the past four years. I also wish to continue my relationships with my various Japanese colleagues in whatever way possible.

*This work was supported by the Applied Mathematical Sciences subprogram of the Office of Energy Research, U.S. Department of Energy, under Contract W-31-109-Eng-38.

# FGCS 92 Evaluation Workshop

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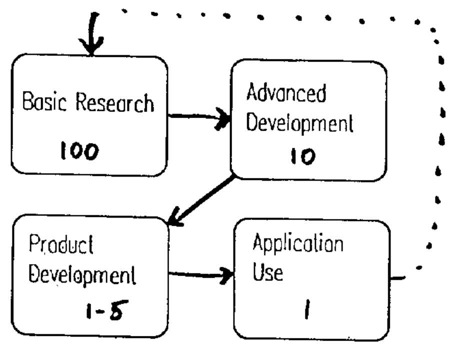
## Western View of Computer Research and Development Process




Was the FGCS project a Basic Research Project or an Advanced Development ? How To evaluate it

Many in the US are confused about how to evaluate the FGCS because Japanese R&D process is not well understood.

In Basic Research a negative result is not a failure, in Advanced Development inability to get to product development is considered a failure, however not having any results would be considered failures in both cases.

## Basic Research

Goal: The creation of new knowledge, both affirmative knowledge and negative knowledge.

- 1. what works
- 2. what doesn't work
- 3. what is true and what is false

Evaluation: Are important questions getting answered ?

## Advanced Development

Goal: Engineering of prototype systems, determining feasibility, uncovering gaps in technical knowledge.

1. what processes work ?
2. testing for adoption, (i.e. given a choice do people use it ?)
3. application of an idea to a product

Evaluation: Are new technologies ready for product development ?

- The US was evaluating the FGCS in $\boxed{\text{both}}$ categories, as a basic research project and as an advanced development project, therefore it could have been considered a success if one more or more *hard problems in AI or CS had been solved* or if a commercial *company had committed to producing products* based on the results of development.

- USA feared the project for two main reasons.

  1. It fundamentally *challenged their notions* of preeminence in basic research.

  2. If commercial products resulted from the project, Japan would have taken a lead in knowledge based systems-- an important new paradigm-- with *little western response possible* in the short term.

- So what went wrong ?

I THINK IT will take many YEARS To fully understand what has happened and what it means.

## Lessons I have learned from the FGCS project

USA is adopting Similar Style Projects

- government supported industrial consortia may not be able to "read the market", particularly over the long term.
- Don't confuse BASIC RESEARCH and ADVANCED DEVELOPMENT. (i.e. know which you are doing and don't confuse the evaluation criteria for the two.)
- Expect negative results but hope for positive. Mid-course corrections are a good thing.
- BASIC RESEARCH infrastructure needs stability, strong sense of the important core problems, flexibility and an evaluation mechanism that can distinguish between negative results and incompetence.
- VISION: The vision is critical, the most important thing people need is a BIG DREAM to make it worthwhile to get up in the morning.

"Make no small plans for small plans have no power to stir men's souls."

IT IS IMPORTANT TO HAVE VISION.

## Specific Accomplishments

- Can one built a whole computing systems based on logic programming and provide a useful tool for applications uses? Answer: YES
- Are the resulting systems so much easier to use that people will immediately switch from conventional computing systems ? Answer: NO
- Does special purpose hardware give KBS a performance advantage over general purpose hardware ? Answer : NO
- Can logic programming and KBS be applied to a wide variety of applications areas ? Answer: Probably Yes
- Is the world likely to adopt KBS systems as a major alternative to Object-Oriented systems development environments for non-numerical computing ? ANSWER: Probably Not
- Can logic programming and KBS open up a whole new world of applications areas with the same effect on society (and markets) as numerical computation did in the 1950's and 1960's ? ANSWER: Too Early to Tell
- Did the FGCS project succeed in giving Japan new visibility in the world computer science community? ANSWER: Absolutely YES

Many of these problems are common to Many areas of Computer science Research.



## What did Japan get for $US 430 Million Investment?

- Estimated 2.25 Million Lines of KL1 and ESP code ($US 200 Million to produce at commercial rates)
- Over 700 reports and papers ($US 35 Million to produce at National Lab Rates)
- New Generation of Symbolic Computing Architectures ( about 8 or 9 architectures $US 20 Million each = $US180 Million)
- Three international Computer science conferences ($US2 Million each (?) or $6 Million)
- Training a new generation of computer architects and scientists. ( $US 20 Million)

## Specific Recommendations for the follow-on of ICOT

- Establish LONG TERM funding for BASIC RESEARCH and work in:

  1. Parallel Processing, performance evaluation, etc.
  2. Knowledge Based Programming systems
  3. Combination of Symbolic and Numerical Computation

- Make all BASIC RESEARCH results publically available from the very beginning of any new project.

- Encourage TRUE COLLABORATIONS which involve the setting of joint research objectives, joint funding and joint management of the projects.

- Concentrate on software for general purpose machines and let INDUSTRY develop the hardware and operating systems software.

- Encourage smaller more INDEPENDENT research groups that may be distributed with less central control, perhaps some in universities.