

ARTES Evaluation

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PROJECT EVALUATIONS

Hardware-Software Co-Design of Real-Time Systems(#2)

Started 13/1/98

1. Suggestions for improving the scientific work

Currently the main criterion for selecting a particular hardware/software partition appears to be minimisation of hardware costs. For high-volume embedded markets such as automobiles, this is perhaps the most appropriate criterion. However, the work would have a wider impact if other criteria were also investigated. For example, utilisation levels of the processors or busses or output jitter etc.

2. Relevance of the studies carried out for the real-time area

This is a very important and relevant area.

3. Is the project's research direction heading in the proposed direction?

Yes.

4. Is the present direction scientifically relevant?

Very much so.

5. Has the present direction a potential industrial use?

Yes, particularly for high-volume mass produced embedded systems. And potentially for other areas as well.

6. Assessment of the project

Currently GOOD and potentially VERY GOOD. There has been a good volume of conference publications already; if these can be turned into high quality journal publications then the work will reach a wider audience.

Perhaps the project would benefit from collaboration with Project #3. Also some international interaction with Vienna on TTA might be helpful.

7. Recommendation regarding the future funding of the project

The funding should be maintained to allow the completion of the PhD (and beyond if the work continues to be successful).

Incremental Static Scheduling (#3)

Started 1/7/98

1. Suggestions for improving the scientific work

The key to the success of this project is showing that the approach doesn't undermine the advantages obtained by static scheduling compared to other more flexible scheduling approaches. It would be nice to see case studies comparing the proposed approach with fixed priority systems in terms of a) ease of schedule construction, b) run-time efficiency (overheads) and c) level of determinism produced.

2. Relevance of the studies carried out for the real-time area

This work is trying to bridge the gap between static scheduling and dynamic scheduling. My own *personal* view is that it perhaps has more industrial relevance than long-term real-time research relevance.

3. Is the project's research direction heading in the proposed direction?

Yes

4. Is the present direction scientifically relevant?

Yes – see 2

5. Has the present direction a potential industrial use?

Yes, particularly given that static scheduling has widespread use in industry yet it is very inflexible.

6. Assessment of the project

Currently FAIR, potentially GOOD

Currently there is only minimal publication. However, there is good international collaboration. Whether the work is novel enough to enable it to obtain higher levels of assessment is questionable.

7. Recommendation regarding the future funding of the project

The funding should be maintained to enable completion of the PhD.

Integrated Control and Scheduling (#7)

Started 13/1/98

1. Suggestions for improving the scientific work

The work of Iain Bate at the University of York on scheduling with offsets might be applicable to the work of Anton Cervin (Improved Scheduling of Control Tasks).

Is the relationship between this work and that of Stankovic's reported at Euromicro RTS 1999?

A nice further interaction between the two parts of the project would be if the WCET analysis could also insert Ógain pointsÓ into the code which can then be used to inform the scheduler that the actual execution time of the task will be less than the worst case (rather than at the end of the task). This is a form of feed forward used for mode changes but at finer granularity.

2. Relevance of the studies carried out for the real-time area

The project bridges the gap between control systems engineering and real-time systems engineering. This gap has been an embarrassment for both communities and hence the project is most welcome.

3. Is the project's research direction heading in the proposed direction?

Yes, it appears so.

4. Is the present direction scientifically relevant?

Yes

5. Has the present direction a potential industrial use?

Yes particularly for embedded control systems. If we can get better integration between the scheduling and the control task, more efficient and robust systems will result.

6. Assessment of the project

I think this project borders on VERY GOOD. There are good conference publications and journal publications are beginning to appear. The collaboration with Lui Sha is most welcome and important.

7. Recommendation regarding the future funding of the project

The funding should be maintained to allow the completion of the PhDs (and beyond if the work continues to be successful).

Design of Heterogeneous Multiprocessor Systems for Real-Time Applications (A2-9805)

Started 1/9/99

1. Suggestions for improving the scientific work

The project seems to focus on the evaluation of a position in the solution space. It is not clear how that position is arrived at or how one can navigate through the solution space to explore alternatives. I would have liked to see more attention given to this issue. However, it could be that this area has already been addressed in their previous work. Similarly, there is little detail given on the status of their IDR.

2. Relevance of the studies carried out for the real-time area

The area of research is core to real-time systems.

3. Is the project's research direction heading in the proposed direction?

It appears to be. But, the exact relevance of the UML work is unclear. I am not criticising the use of UML see 5.

4. Is the present direction scientifically relevant?

Yes

5. Has the present direction a potential industrial use?

Very much so. However, for real industrial use, care must be taken not to create esoteric design representations. If possible the use of standard notations such as UML should be encouraged as long as they do not inhibit the scientific progress.

6. Assessment of the project

GOOD. Whilst publication from the currently supported work is so far only a conference paper, the overall Group does seem to be producing good quality work.

7. Recommendation regarding the future funding of the project

Funding should be maintained to enable the PhD to be completed.

Real-time Software for Versatility, Scalability and Reconfigurability (A4-9805)

Started 1/7/1999

1. Suggestions for improving the scientific work

Some of the early survey work has been very broad. Attention now seems to be focused on the core issues which is good. Analysis of the emerging architecture will be key, as will being able to reason about conflicting requirements. Perhaps multi-criteria decision making has a role to play here?

2. Relevance of the studies carried out for the real-time area

The problem area is well motivated and multidiscipline.

3. Is the project's research direction heading in the proposed direction?

So far

4. Is the present direction scientifically relevant?

Yes, developing system and software architectures are key topics.

5. Has the present direction a potential industrial use?

Yes, particularly for large scale embedded systems

6. Assessment of the project

Currently FAIR. A thorough survey has been done (which was perhaps a little too broad). However, few new results have yet emerged from the project.

7. Recommendation regarding the future funding of the project

Funding should be maintained to allow completion of the PhD.

A tool environment for the development of embedded systems (#A6-9805)

Started 1/1/1999

1. Suggestions for improving the scientific work

I have mixed feelings about applying model checking techniques to the problem of schedulability analysis. It is not clear that this is the most appropriate use of model checking. I tend to view model checking as an approach for checking the consistency of the specified timing properties and schedulability analysis as the correct approach for determining whether the timing requirements are met by the implementation. If the approach is to be pursued then there is a need for a more detailed comparison with schedulability analysis to see whether model checking is flexible (and efficient) enough to cope with issues like implementation overheads and shared resources.

2. Relevance of the studies carried out for the real-time area

The project as proposed had good relevance. But see 3.

3. Is the project's research direction heading in the proposed direction?

No, there has been a shift in emphasis away from code generation techniques. The investigator argues that code can only be generated once schedulability analysis has been addressed.

4. Is the present direction scientifically relevant?

I have mixed feeling about the relevance of the current work. See 1.

5. Has the present direction a potential industrial use?

Unclear. Certainly not as much potential as the original project.

6. Assessment of the project

POOR/FAIR. I am worried about the direction the project is taking.

7. Recommendation regarding the future funding of the project

If the investigator is an experienced researcher with a good track record, then it might be sufficient to just raise concerns on the current direction but leave it up to him/her on whether to continue with this line. However, if the investigator is inexperienced then corrective action may need to be taken on this project to get it back on course.

TATOO – Test and testability of distributed real-time systems (A7-9805)

Started: 1/10/1998

1. Suggestions for improving the scientific work

An interesting piece of work. Three issues spring to mind. Firstly, the overhead of avoiding the probe effect – it would be nice to see some more discussion on this issue. In particular, what percentage increase in memory and processor power is required (can mass-produced systems live with this cost?). Secondly, the problem of scale. Does the approach scale up to large distributed real-time systems? The case study in the thesis considers only a two-node system and a handful of tasks. Thirdly, the issue of genuine sporadic tasks does seem to undermine the approach.

2. Relevance of the studies carried out for the real-time area

Extremely important area.

3. Is the project's research direction heading in the proposed direction?

Yes, it appears so.

4. Is the present direction scientifically relevant?

Yes, although I have some worries about whether the approach will scale.

5. Has the present direction a potential industrial use?

Potentially, if the approach does scale and the overheads of the probes are not too great.

6. Assessment of the project

Currently GOOD/VERY GOOD. The RTSS and RTAS papers are a good sign that the work is being well received by the international community. A journal paper has also been submitted.

7. Recommendation regarding the future funding of the project

The thesis work is now almost complete. It would be nice to see the work applied to a reasonably sized industrial problem. I would favour funding for this.

Application of wait/lock-free protocols to real-time systems (A8-9805)

Started: 1/3/1999

1. Suggestions for improving the scientific work

The current work seems to be based on a single application (the snapshot algorithm). I feel there is a need to be able to classify data sharing schemes which are applicable to a wait/free synchronization approach and, if possible, to provide re-usable components for their implementations (e.g. Ada generic packages) which can be instantiated with the actual data types.

2. Relevance of the studies carried out for the real-time area

Very relevant.

3. Is the project's research direction heading in the proposed direction?

Yes.

4. Is the present direction scientifically relevant?

Yes.

5. Has the present direction a potential industrial use?

Initially for Technology providers but if the work can provide generic mechanisms then it will become more important for end users.

6. Assessment of the project

GOOD. The research is incremental building on the pioneering work of others and, hopefully, taking it a stage forward towards industrial exploitation.

7. Recommendation regarding the future funding of the project

The level of funding should be maintained to allow the PhDs to be completed.

Automatic Control in Distributed Application (AIDA 2) (9811-2)

Started 1/2/2000

1. Suggestions for improving the scientific work

The work has only just started and it is too soon to comment on the scientific work performed.

2. Relevance of the studies carried out for the real-time area

Application oriented.

3. Is the project's research direction heading in the proposed direction?

Too soon to tell.

4. Is the present direction scientifically relevant?

I guess. The proposal is a bit vague.

5. Has the present direction a potential industrial use?

Yes.

6. Assessment of the project

Too soon to tell.

7. Recommendation regarding the future funding of the project

Too soon to tell.

Identification of Complexity-Reduction Techniques for Optimal Scheduling in Embedded Distributed Real-Time Systems (9811-3)

Started 1/5/1999

1. Suggestions for improving the scientific work

It is disappointing that the work on identifying the constraints has not really dug deep. There appears to have been no real effort to identify the underlying requirements which result in the derived requirements. I suspect that the detailed timing requirements are derived from more fundamental requirements. However, the classification is nice. Perhaps a workshop between this project and 9905-1 should be arranged?

It is also not clear how all of the entries in the classification are represented in the CSP.

2. Relevance of the studies carried out for the real-time area

Potentially very relevant. However, there is a danger that the very nature of the approach (application knowledge affecting the search for optimality) might not lead to transferable research results.

3. Is the project's research direction heading in the proposed direction?

Yes.

4. Is the present direction scientifically relevant?

Yes, but see 2.

5. Has the present direction a potential industrial use?

Perhaps the Industrial Collaboration is not bearing fruit on this project (indicated by the disappointing results from the constraints study).

6. Assessment of the project

Currently, FAIR. However, there is good international collaboration and the potential for Very Good results to appear.

7. Recommendation regarding the future funding of the project

The level should be maintained to enable completion of the PhD.

Node-level fault tolerance for fixed priority scheduling (9811-5)

Started 1/4/1999

1. Suggestions for improving the scientific work

There has been much work in this area. I would have liked to see the results of the survey to ensure that they are aware of it all. The GUARDS projects, for example, would seem to be a key reference. Also, the work of Sasikumar Punnekkat (when at York) who is now at Uppsala would seem to be appropriate as he modelled the impact of faults on response time analysis.

2. Relevance of the studies carried out for the real-time area

Clearly, fault tolerance is a key aspect of real-time.

3. Is the project's research direction heading in the proposed direction?

It appears so.

4. Is the present direction scientifically relevant?

Yes.

5. Has the present direction a potential industrial use?

Yes in the high-integrity system domain.

6. Assessment of the project

Currently FAIR. Although there does appear to have been good industrial collaboration.

7. Recommendation regarding the future funding of the project

The level should be maintained to enable completion of the PhD.

Hierarchical Design and Analysis of Timed Systems (# 9811-5)

Started 1/1/1999

1. Suggestions for improving the scientific work

The motivation for this work is good, coming from the industrial collaboration. I wasn't sure about the relevance of the animator for hybrid systems to the main thrust of the project.

2. Relevance of the studies carried out for the real-time area

The development of practical model checkers such as UPPAAL has been an important influence on the real-time area. If this work is successful, it will be very relevant.

3. Is the project's research direction heading in the proposed direction?

I think so, assuming that the work on hybrid systems is not core and has been thrown in to boost the publications!

4. Is the present direction scientifically relevant?

Yes

5. Has the present direction a potential industrial use?

Yes, I expect model checkers to be used more often in the future.

6. Assessment of the project

GOOD. Although it is difficult to judge at this stage whether the project will have a major impact on the way UPPAAL is used.

7. Recommendation regarding the future funding of the project

Maintain funding to allow PhD to be completed.

New directions in symbolic model checking for real-time systems (9811-6)

Started 1/8/1999

1. Suggestions for improving the scientific work

In the long term, expanding the work to languages other than PLC-based ones and those supporting the synchronous hypothesis would make the results more widely applicable.

2. Relevance of the studies carried out for the real-time area

Model checking is currently the most appropriate technique for undertaking automatic verification of real-time systems. If this work is able to extend the size of the systems capable of being modelled, then it is very relevant to the real-time area.

3. Is the project's research direction heading in the proposed direction?

Yes it appears so.

4. Is the present direction scientifically relevant?

Yes.

5. Has the present direction a potential industrial use?

Yes, particularly if it is able to significantly increase the size of systems modelled.

6. Assessment of the project

Based on the success criteria, currently FAIR. However, the work has only been underway for 6 months so this is a little misleading. However, it should perhaps be noted that the progress report gives no visibility to the work undertaken by the Research Student and the paper does not have his name on it.

7. Recommendation regarding the future funding of the project

One would like to see how the Research Student is progressing before recommending further funding.

Real-time mobile communications (9905-8)

Started 1/1/2000

1. Suggestions for improving the scientific work

If the project is to include in its remit safety-critical systems then perhaps some attention should be given to denial of service security attacks.

Lehoczky at CMU (CS Department) has done some work on probabilistic real-time system modelling which might be relevant.

2. Relevance of the studies carried out for the real-time area

Extremely relevant. Real-time mobile communication will become increasingly more important.

3. Is the project's research direction heading in the proposed direction?

It appears so.

4. Is the present direction scientifically relevant?

Yes, I believe so.

5. Has the present direction a potential industrial use?

If successful and if the security issues can be solved then yes.

6. Assessment of the project

Currently FAIR, but should obtain at least a GOOD rating if not a VERY GOOD rating.

7. Recommendation regarding the future funding of the project

Maintain funding to allow PhD to be completed.

Flexile reliable timing constraints (# 9905-1)

Started 1/2/2000

1. Suggestions for improving the scientific work

There is little to say as the project has only just got underway. I suggest early collaboration with 9811-3.

2. Relevance of the studies carried out for the real-time area

Very relevant if flexible systems are to be implemented.

3. Is the project's research direction heading in the proposed direction?

No results yet.

4. Is the present direction scientifically relevant?

Yes.

5. Has the present direction a potential industrial use?

Yes

6. Assessment of the project

This project has only just started so it is difficult to evaluate at this stage.

7. Recommendation regarding the future funding of the project

Not applicable at this stage.

Methods for integration of heterogeneous real-time services into high performance networks (# 9905-7)

Started 1/1/2000

1. Suggestions for improving the scientific work
Good motivation for the study. Too early to give suggestions.
2. Relevance of the studies carried out for the real-time area
Very relevant.
3. Is the project's research direction heading in the proposed direction?
No results yet
4. Is the present direction scientifically relevant?
Yes
5. Has the present direction a potential industrial use?
Yes
6. Assessment of the project
This project has only just started so it is difficult to evaluate at this stage.
7. Recommendation regarding the future funding of the project
Not applicable at this stage

Testing of Event-Triggered Systems (9905-11)

Started 1/1/2000

1. Suggestions for improving the scientific work
Good background work to the project. . Too early to give suggestions.
2. Relevance of the studies carried out for the real-time area
Very relevant.
3. Is the project's research direction heading in the proposed direction?
No results yet
4. Is the present direction scientifically relevant?
Yes
5. Has the present direction a potential industrial use?
Yes
6. Assessment of the project

This project has only just started so it is difficult to evaluate at this stage.

7. Recommendation regarding the future funding of the project

Not applicable at this stage

Functional integration and interference in embedded control systems (9905-11)

Started 1/2/2000

1. Suggestions for improving the scientific work

To soon to see how they have responded to a cutback in the number of Research Students.

2. Relevance of the studies carried out for the real-time area

Increased functional integration of distributed real-time systems is an important topic.

3. Is the project's research direction heading in the proposed direction?

Too soon to say.

4. Is the present direction scientifically relevant?

Yes

5. Has the present direction a potential industrial use?

Yes, particularly if it is able to decrease the cost and increase the fault tolerance of such systems by sharing more effectively the computing resource.

6. Assessment of the project

Too soon to tell.

7. Recommendation regarding the future funding of the project

Too soon to tell.

Overall Observations

1. It is too soon to expect good quality Journal publications. However, the projects need to be encourage more in this direction. Hence most of the assessments are below Very Good.
2. The details of the assessment criteria are perhaps also a little in conflict with the ARTES goals as they focus on academic measures rather than industrial take-up/collaboration.
3. The level of reporting on Industrial Collaboration in general is poor (see below).
4. There is a good mix of projects covering most of real-time systems research. The only notable absence is any work on HCI issues. However, arguably these are less important for embedded systems.

PROGRAMME EVALUATION

Has ARTES contributed to

1. Improvement in academic research

It is clear that the ARTES programme has obtained significant success in improving the spread and scope of real-time academic research in Sweden. Before ARTES, internationally-renowned real-time research was limited to perhaps one or two sites. I believe that strong foundations have now been laid for greater international recognition. The number of papers submitted to the main real-time conferences seems to have increased, and I expect further significant increases over the next couple of years. I am also confident that much of the work reported in these papers can provide the basis for high-quality journal papers.

On a personal note, York has seen an increase in the number of visitors from Sweden. This, and the fact that last year's Euromicro Real-Time Systems Conference was held at York, means that I have met quite a few of the new PhD students. In general, I have been very impressed by the quality of their work and their enthusiasm.

2. Improvement in research education

Although this is difficult to quantify without more detailed statistics on the programme, the feeling I get is that the academic community has responded well. The ARTES network and mobility programmes are major contributions to building a strong real-time community and has provided a critical mass of researchers. This, by its very existence, provides a support base for PhD students and facilitates the 'learning' experience. The ARTES summer school is also instrumental in building this support base. I expect the end result to be an increase in the successful and timely completion of both licentiate degrees and PhDs.

It is also good to see the number of graduate courses in Real-Time Systems increasing. I am not sure on the structure of these courses; however, from my perspective, it would be nice to see these courses being taught intensively in English (say over a week). This way, they could be advertised and made available to research students across Europe. This would benefit the wider real-time community.

I also understand that ARTES has sponsored this years Euromicro Real-Time Systems conference by providing funding for a small number of non-Swedish PhD student to attend the conference. This is most gratefully received by the wider real-time community and gives direct international visibility to the ARTES programme. I would like to see this continue next year!

3. Improved collaboration

My only overall criticism of the ARTES programme is that the level of reporting on Industrial Collaboration is, in general, rather poor and mainly consists of 'We had regular meetings with our industrial sponsors'. Consequently, there is only scant evidence to suggest that Industrial Partners are playing a major role in the research programme.

It is clear that some groups have good international contacts. I expect, as a result of ARTES, the international visibility of Swedish real-time research to be improved. This will have the knock-on effect of increasing international contact and ultimately international collaboration.

Future Funding

ARTES has successfully expanded the academic base for real-time systems research in Sweden. Sweden has now, perhaps, the most extensive real-time community in Europe. Experience in the UK with past targeted research programmes suggest that it is crucial (having built up the community) not to suddenly withdraw funding after the programme has finished. Otherwise much of the good work will be lost. Hopefully there will be a natural migration of the PhD students into Industry. However, now that momentum is beginning to build it is important to continue that momentum with further funding — particularly, given the importance of real-time systems to Swedish industry.

After the current 5 years, it should be clear which project have been successful. Inevitably some of the work will continue to be long-term and require further funding for its continuation. If this cannot be obtained through the normal SSF funding route, it will be necessary to extend the programme for a further three years. Where projects have results that have the potential for industrial exploitation, some encouragement may be needed for industrial take-up. Funding of the network should also continue.

In summary, I believe that the foundations have been laid for a successful programme. I would support any request for further funding of the programme. However, this funding should be targeted at the most successful of the current projects.