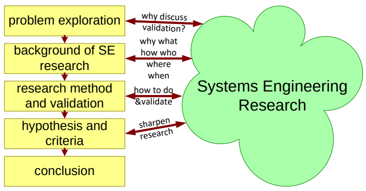
Systems Engineering Research Validation

-



Gerrit Muller
Buskerud University College and Embedded Systems Institute
Hasbergsvei 36 P.O. Box 235, NO-3603 Kongsberg Norway
gaudisite@gmail.com

Abstract

System Engineering research addresses methods, techniques, models and formalisms that should advance the engineering practice of systems. This type of research inherently addresses a mix of technological issues in relation to business, process, organization, and people aspects. We discuss the challenge of validating this type of research. We look at different research and validation methods.

version: 1.0 status: draft September 1, 2020

1 Introduction

Figure 1 gives a visual overview of the content of this paper. The core question is *how to validate Systems Engineering research.*

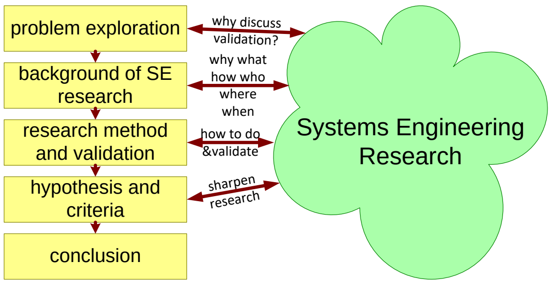


Figure 1: Figure Of Contents™

We start with the exploration of the problem (Section 2), why do we discuss validation so explicit? We reflect on the background of Systems Engineering research (Section 3), why do this research, what is researched, how is it done, who is researching, and where?

Section 4 addresses the core question how to validate? Hypothesis and Criteria are discussed in Section 5 as means to sharpen the research.

2 Problem Exploration

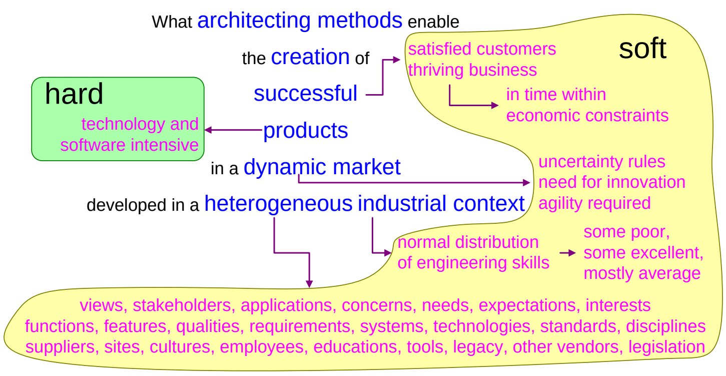


Figure 2: The original research question from author's dissertation, characterized with hard and soft factors

As an example we look at the hypothesis from the author's dissertation [6]. This thesis describes the research of an architecting method. The human factor is quite dominantly present in the success probability of the architecting method. Figure 2 shows the original research question, with a characterization into soft and hard factors. It is immediately clear that many soft factors dominate in the research question. These soft factors can broaden the research scope tremendously. A lot of effort in writing the thesis went into maintaining focus and into balancing hard and soft factors.

Science is applied in a wide range of areas, from proof-based mathematics to descriptive reasoning in human sciences, see Figure 3.

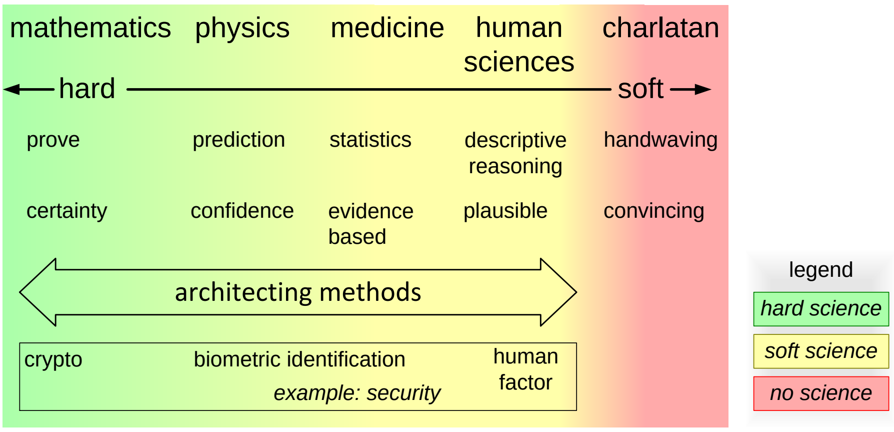


Figure 3: Spectrum of sciences

The level of certainty of the results decreases when moving from hard sciences to soft sciences. Mathematical proofs provide certainty[1], see also [3]. Physics provides a confidence level that increases by validating predicted outcomes, or it applies a *falsification* process as described by Popper [9].

Medical sciences need a lot more trial and error, where evidence is built up in extensive statistical studies. The evidence is hampered by many factors that influence the outcome of the medical study, but that are outside the control of the experimenter. Worse is that many of the factors are unknown to the experimenter and his peers. Cause and result are often more ambiguous than people realize. Despite all these disclaimers the medical sciences have created a large body of knowledge.

The human sciences (psychology, sociology, pedagogy, et cetera) have already a tremendous challenge in making statements plausible. Human behavior shows a wide variation, depending on many factors, such as culture, age, gender, and status.

[1]As far as the proof is verifiable and the verifiers can be trusted. The absolute certainty is here also decreased by the human factor: the proof is as certain as the quality of the provider of the proof and the verifiers of the proof. Automation shifts the problem to the tool, which also in some way originates in fallible human beings.

Individual human behavior is often poorly predictable. Case descriptions are used in a heuristic approach. The step from case descriptions to a workable hypothesis needs a lot of interpretation. Adding more case descriptions will help in making the issue more plausible, but hard evidence is nearly impossible. A more experimental approach with small scale experiments is possible, but these experiments are often highly artificial.

The scientific community dislikes the charlatans, who can be very convincing by hand-waving arguments, but in fact are selling hot air.

Architecting integrates all of these different types of sciences, from mathematical to human sciences. For instance in security design cryptographic proof is important, and also biometrics authentication. However a security solution that does not take the human behavior into account fails even before it is implemented.

Research of architecting methods is inherently the combination of hard facts in an environment full of soft factors. Most of present-day hard disciplines (mathematics, physics, electronics, mechanics, et cetera) are frightened away by the soft factors. Most of the soft disciplines (psychology, philosophy, business management) have no affinity with the complexity in the hard facts. The challenge in the systems discipline is to tackle the soft factors, with sufficient understanding of the hard side.

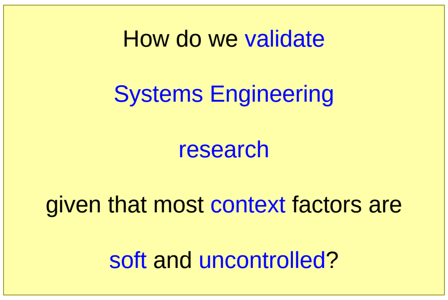


Figure 4: Problem Statement

Figure 4 summarizes the problem statement for this paper: How do we *validate Systems Engineering research* given that most *context* factors are *soft* and *uncontrolled*?

3 Research Method and Validation

The fact that so many soft factors play a role is no excuse to stay in "trial and error" mode. The scientific attitude, see Figure 5, can also be applied to the soft kind of problems encountered in systems architecting.

Engineers nail down many details in different engineering disciplines, when creating a system. Today's systems easily contain millions of details, such as

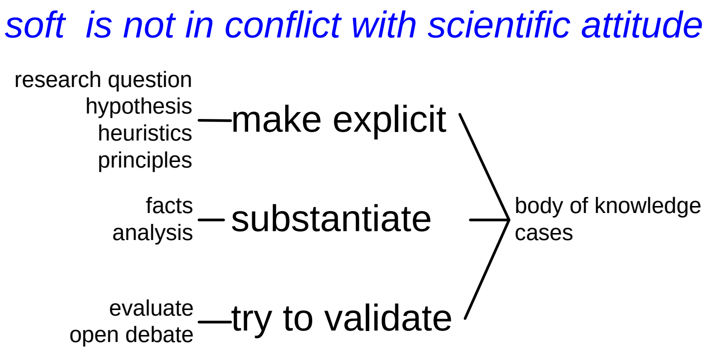


Figure 5: Soft problems can be approached with a scientific attitude

lines of code in software, mechanical characteristics such as sizes, materials, and weights, or connections and components in electrical engineering. Systems engineering copes with the integration of these different mono-disciplinary design decisions, such that we create the appropriate system for all stakeholders. Systems engineers address tens of thousands multi-disciplinary design decisions to achieve hundreds of desired system characteristics. This is shown in the pyramid at the left hand side of Figure 6.

Most academic research focuses on a small subset of one discipline and adds a small increment (delta) to the existing body of knowledge of that discipline. This type of research does not fit with the multi-disciplinary nature of systems research and it does not serve the needs of industrial stakeholders. Two types of research do fit in today's needs:

Borrow and adapt approach where existing mono-disciplinary methods or techniques are deployed on multi-disciplinary problems. An example is the use of perturbation theory from quantum mechanics that is also used as sensitivity analysis at system level.

Field research where the actual practice in the field is observed. The discipline of systems engineering is so young that an explicit baseline of the state-of-practice is needed to be able to make improvements. How can you claim to make improvements when the baseline is absent?

In the history of science different types of research have been applied, see Figure 7:

- Observational research
- Theory development
- Experimental research

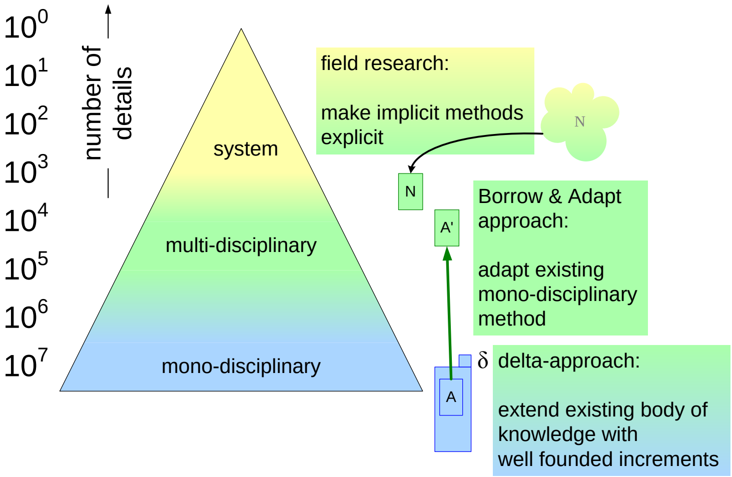


Figure 6: Different Types of Research

- Fundamental research

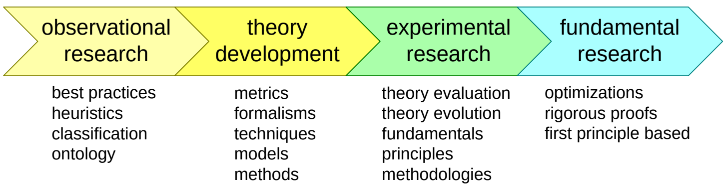


Figure 7: What Kind of Research is Needed?

In the early phases, when scientists did not yet have workable theories, *observational* research is the starting point. Observational research starts with describing the observations. For example case descriptions are valuable means. In system and software engineering we should capture best practices and heuristics. Research could also make a classification and ontology, based on the observations.

Theories are developed by trying to explain the observations. Numeric understanding is strived for by defining metrics. Theory development requires formalisms, techniques, models, and methods as means. These more abstract elements are often also the outcome of this type of research.

In the *experimental* phase the theories are tested. The more or less historical view of the early phases is replaced by an attempt to get a more objective validation of the theory. The main vehicle to achieve validation is experimentation. Theories are evaluated, often resulting in adaptation of the theory. The experimental researchers are searching for fundamentals, principles, and methodologies.

Once the field is well-defined, then more *fundamental* research becomes possible. For example the search for optimal solution, rigorous proofs, and first principle based derivations.

In practice all these types of research are concurrent and iterative.

We already indicated that Systems Engineering is a rather young research field, which means that most research will be observational with some theory development.

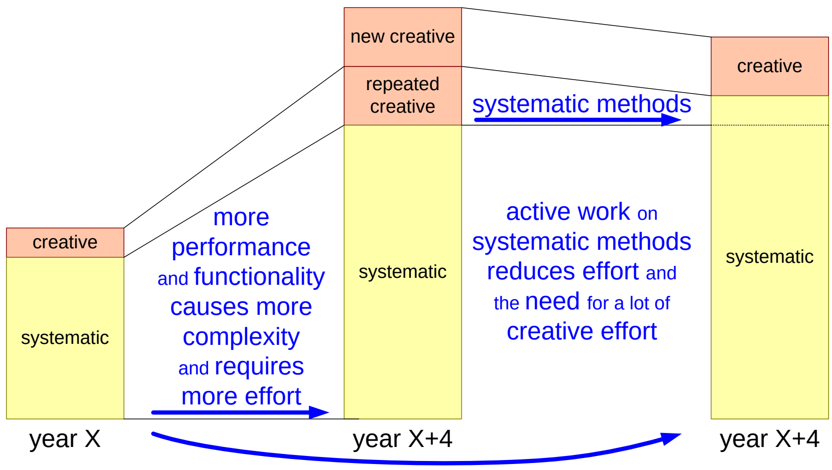


Figure 8: A scientific base is required to cope with the growing system effort. The scientific base provides a systematic approach that helps to solve known types of problems with less, more systematic, effort.

The relevance for the product creation companies is that the increasing effort of creating more powerful, but complex systems, is kept manageable. The ratio between the amount of systematic work, engineering, and the amount of creative/chaotic work should preferable stay the same. Due to the increasing complexity, in both hard and soft issues, this ratio will worsen if we are not able to make part of the system work more systematic.

Figure 8 shows the amount of systematic work and creative work. In the electronics industry the effort to create new circuits increases exponentially, more or less following Moore's Law. The phenomenon that the product needs and possibilities increase faster than our design know-how is known as the productivity gap, see for example [4]. The first bar shows the amount of systematic work at the bottom and the creative work at the top. The new development shown in the second bar, taking place several years later, in this example four years, requires about twice the amount of work. If we do not develop the system discipline a lot of the future system work will still be done in "trial and error" mode, represented by the *repeated creative* work. The new functionality, performance and complexity challenges also require *new creative* work. If the creative work of the past can be captured in more systematic approaches then the *repeated creative* work is transformed in less *systematic* work, as shown in the third bar.

One of the symptoms for this trend of increasing creative work is the relative increase of the integration period and integration effort. The lack of a systematic approach in the early design phases is solved by applying a lot of creativity in solving the problems during integration. This effect is visible in complex systems, such as Magnetic Resonance Imaging scanners, wafersteppers, and video processing platforms.

The message behind this figure is that product creation will always have a creative component. Providing a scientific base will never remove the need for human creativity. A scientific base will enable the effective use of the creative talent, not wasting it on problems that could have been solved in a systematic way.

Figure 8 suggests an incremental increase of creation effort. Many products, such as cardiovascular X-ray systems, wafersteppers, and televisions show such exponential growth of the effort. When developing system architecting methods the ambition should be to develop also the development of system design and implementation methods that *decrease* the desired effort. Once the know-how is captured in methods a next step in support can be made by further automation and supporting tools. Systematizing know how precedes automation and tooling.

Technology management can be modeled as a cyclical process [1], as shown in figure 9. Most of the time is spent in the application of technology, in other words in the creation of new systems. After applying the technology it is recommended to learn from this application by reflection. The learning experience can (partially) be made accessible to others by consolidating the know how, for instance in documentation.

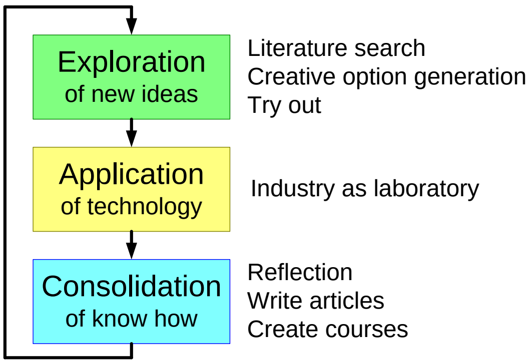


Figure 9: Technology Management Cycle

At the end of the consolidation insight will exist in strengths and weaknesses of the technology, both in the hard technology choices as well as in the soft technology (the approach taken). It is recommended to take this know how as a starting point for an exploration phase.

The exploration phase should be used to refresh the designers and architects and open new opportunities in technology. This requires that they know the state of the art in the world, by reading literature, visiting conferences et cetera. Via creative brainstorms new technology options can be added. Promising technology must be explored hands-on.

In the next application phase a limited set of new technologies is applied in practice.

Note that most effort in technology management is spent on core (hard) technologies. Hard technology is based on know how from the sciences: mathematics, physics, chemistry, biology. The know how from these sciences is very objective and universally applicable (the elasticity in the USA is the same as the elasticity in China).

A small amount of the effort will be spent on the methods required to apply this technology successfully, the methods or soft technologies. This is shown in figure 10 by the slightly darker right hand sight of the technology management cycle. Soft technologies are based on a mixture of sciences and human arts. The know how of soft technologies is more subjective, the human factors are less well reproducible (a method working well in the USA might fail in China and vice versa).

A method is by definition based on sciences and human arts: a method is a way of working for humans to use the hard technology effectively.

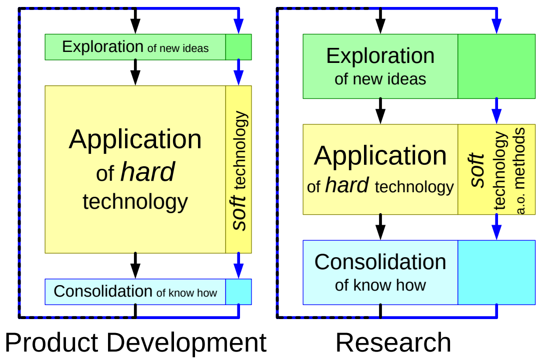


Figure 10: From Product Division to Research

This technology management cycle can be applied at multiple levels: from a design team of a specific product, up to the CTO office of a large multi-national like Philips. Design teams in the business lines will normally spend only a limited amount of time for consolidation and exploration (business pressure creates a large degree of pragmatism).

Research departments, with the explicit task of creating technology options, can spend more time on exploration and consolidation, see figure 10. However for research departments application of the technology is more difficult, this might cost a lot of time and energy, while the application might still not be realistic. Hard specific technology is more easily applied in research environment than the soft technology as architecting methods. Architecting methods are inherently related to the problems of large design teams, with all kinds of fuzzy context constraints. For that reason research of architecting methods makes use of *the industry as laboratory*: a close cooperation of research with regular design teams, where research options are tried out in a real world context.

Most effort in technology management is spent on the hard technology (which generates more direct value, for instance via Intellectual Property), while sufficient effort should be spend on methods to apply these technologies in creating new systems. In research groups with a specific capability in soft technologies the balance between hard and soft technology can be shifted somewhat more to the soft technology. To prevent that such a group floats away in abstractions sufficient hard technology should be researched at the same time. Figure 10 shows this shift in balance from hard to soft technology as well.

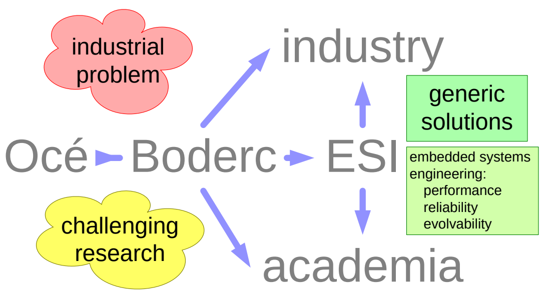


Figure 11: Stakeholders

Multi-disciplinary research involves many different stakeholders. Figure 11 shows the main stakeholders for the Boderc project, a research project carried out by the Embedded Systems Institute together with academics from multiple universities. The project runs at an industrial company, called Carrying Industrial Partner (CIP). The CIP for Boderc, Océ, is one of the main stakeholders. The researchers themselves come from academia, industry or from ESI. Academic stakeholders are mostly interested in challenging research problems with sufficient depth, fitting in their own research field. Industrial stakeholders are looking for usable solutions, such as multi-disciplinary design methods. The mission of ESI is to create and disseminate know how in multi-disciplinary design, well-connected to mono-disciplinary know how and usable in industrial context. ESI needs to generalize project solution for the CIP into more generic solutions. Projects are used as carrier to develop the desired capabilities. Figure 12 shows the relation and the objectives of projects and capabilities. A project is done in an industrial setting and addresses an actual and important industrial problem. In this particular domain this problem is solved.

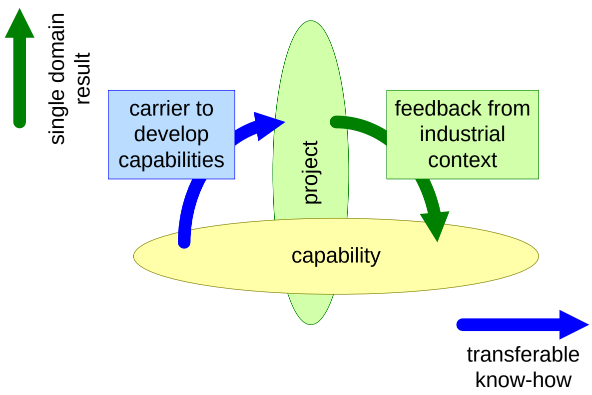


Figure 12: Project as carrier for capability development

Researchers have the objective to solve the problem more generic and to transform the lessons learned in transferable know-how. The objective of research institutes is capability oriented.

The projects running in an industrial environment provide a realistic context for the capability development and generate a lot of feedback for the capability development.

Formalisms languages/syntax: for example, differential equations, timed or hybrid automata, finite state machines, et cetera

Models instantations of formalisms to understand, explore, optimize or verify specification or design

Techniques to get the required information from models: e.g. performance

Methods to provide guidelines how to use formalisms, create models, use techniques and apply tools

Tools to support efficient application of formalisms, techniques and methods

Figure 13: Methodology

The Boderc project struggled for a long time to get the different stakeholders aligned, in terminology and objectives. Figure 13 shows a methodology framework that Boderc adapted to facilitate this communication.

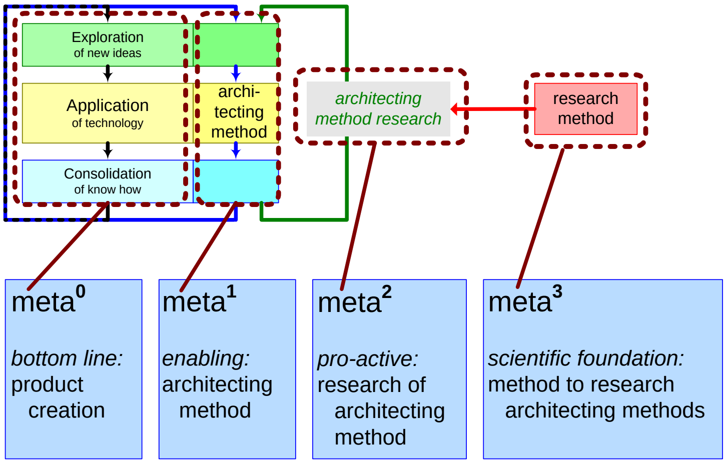


Figure 14: Moving in the *meta* direction. Research of architecting methods is two steps of indirection away from the bottom line of product creation. The scientific foundation for this work is another indirection step

The move from product creation to management of architecting methods to research of architecting methods is a move in which the abstraction level is increasing. It is a move in the *meta* direction, as shown in figure 14. The terms used in Figure 13 are typical used at the higher meta levels, while the industrial practitioners see them as means.

4 Research Method and Validation

One of the major differences between conventional research in engineering disciplines and systems engineering research is the scope. Conventional research zooms in on a small area, while systems engineering research tends to include the context and hence the soft factors.

Figure 15 shows that Systems Engineering research gets more difficult with increasing scope. The left vertical axis shows that the validation feasibility goes from easy to difficult to "impossible" for increasing scope. The scope axis shows for steps of increasing scope:

- Validation of research focusing on **technical feasibility** is relatively easy: a working design or model is sufficient to show technical feasibility.
- Validation of **technical value** is already significantly more challenging, since

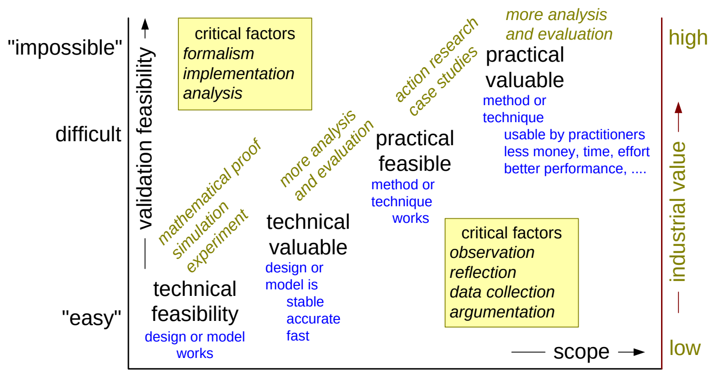


Figure 15: Scope versus Feasibility and Value

this requires that a value proposition is available and that the value can be compared and evaluated, preferably in comparison with alternatives.

- Validation of **practical feasible** increases the scope by including the creators, manufacturers, users and other stakeholders into the research question. Including these real-world stakeholders makes the validation much more difficult, however with a well formulated research proposition and a well designed research approach it is still doable.

- To validate the **practical value** is near impossible. Again we need a clear value proposition, but we also need to cope with the tremendous amount of soft factors determining the value.

Critical research capabilities for all scopes are:

observation of the system of interest and its context; common mistake is to observe too local, e.g. only the method or technique being deployed.

reflection on the observations and the subject of research. Why do events occur, why do stakeholders react in such way?

data collection to enable analysis and evaluation. What was the starting point, before applying new methods? What are the circumstances during the research?

argumentation How do you explain the research set up, the observations, the data collection, the reflection, the evaluation and conclusions to an external audience?

Finally, Figure 15 shows at the right hand side a vertical axis with industrial value of the research. The industrial stakeholders get more value from answers to the more difficult research scopes.

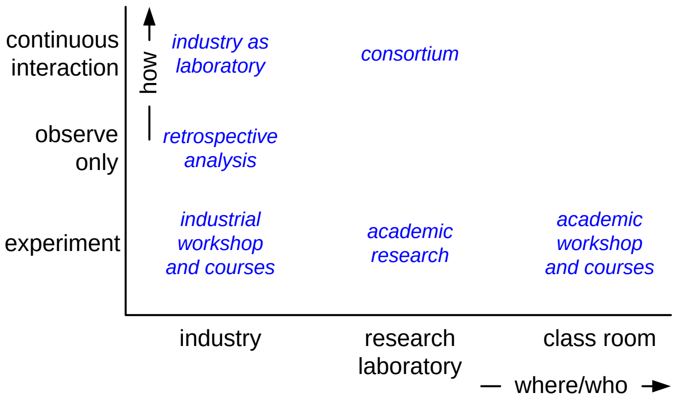


Figure 16: Different Research Methods

In setting up systems research we have to chose *where* to do the research *who* to involve, and *how* to do the research. Figure 16 shows these choices as a two-dimensional space. In the different coordinates in this space different research methods can be applied.

4.1 Research in industry

observe only research, where the observer does not interfere with ongoing work. Useful research is to create case descriptions and to perform *retrospective analysis* on the case.

experiment in an industrial setting with industrial participants. For example application of a new technique or method as part of a workshop, or repeated application of techniques or methods in a course setting.

continuous interaction within the industrial context. This is the so-called *industry-as-laboratory* approach, first proposed by Potts [8]. See also [7].

4.2 Research in research laboratory

experiment This is the most conventional type of research in science. Smaller well-defined problems can be researched this way. For example, agent based models can be used to research behavior and other properties in techno-social systems.

continuous interaction with many participants from different institutes can be used to simulate the industrial environment. Psycho-social aspects can be researched this way.

Note that actual validation of these academic results obtained in the laboratory requires one of the other research methods.

4.3 Research in class room

experiment with for example students as research subjects to study methods and techniques. The benefit is that a larger number of experiments can be performed and that some statistical analysis can be performed. An example is the experiment to introduce aspects of Systems Engineering to freshman engineering students by Frank [2].

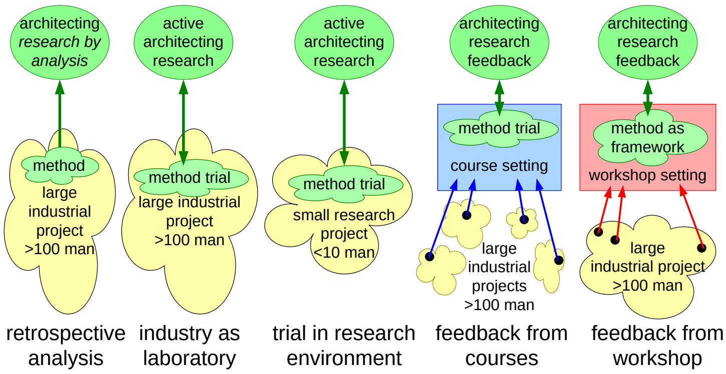


Figure 17: Different Research Methods (2)

Figure 17 shows some of the research models again, annotated with sizes and a characterization of the research.

The *industry-as-laboratory* approach has been proposed by Colin Potts [8] and visualized in Figure 18. Potts observed that very few results from the software engineering research were actually transferred to industrial practice. His idea was to improve the connection between the academic world and the industrial practice by applying the industry-as-laboratory approach.

The industry-as-laboratory approach exploits the actual industrial setting as a test environment, which warrants that the research question is based on real industrial problems. The research team, consisting of a mix of academic and industrial people, investigates a new product engineering methodology. A research hypothesis is formulated on the new methodology. The methodology is applied in the industrial setting and the results of these experiments are observed and used to evaluate the hypothesis.

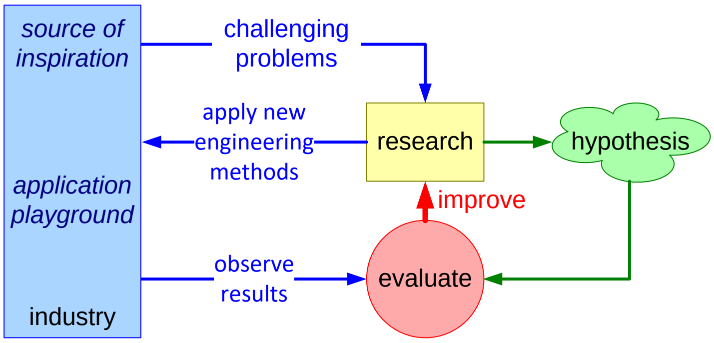


Figure 18: Industry as Laboratory: Research of Engineering Methods

5 Hypothesis and Criteria

Figure 19 shows the flow from problem statement to hypothesis and criteria. The starting point of industry-as-laboratory research is to identify actual problems in industry and to address these problems by research. The industrial problem has to be transformed into an industrial goal, to get the objective(s) clear.

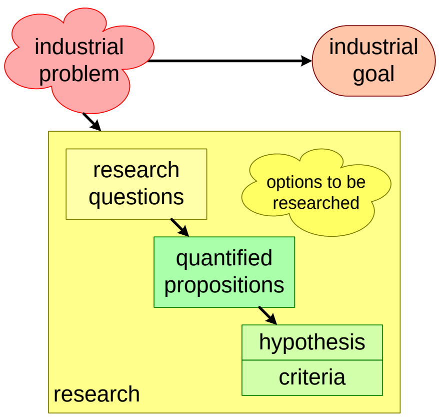


Figure 19: From Industrial Problem to Validated Research

Research will not provide immediate solutions for industry. That is the domain of development and engineering. The role of research is to create knowledge and capabilities to address the problems and to achieve the industrial goals. The starting point for research are the *research questions*, what is it that we want to know?

The research questions tend to be broader and more generic questions. The industrial setting requires a more concrete focus. The research questions can be transformed in quantified propositions to link them back to the industrial context.

The quantification helps to make the proposition sharper and more concrete, e.g. will a technique incrementally improve (10%) or revolutionary improve (100%)?

The last step in sharpening the research is to formulate a hypothesis, including the criteria for failure or success.

In this type of research it is good practice to look at multiple options. Multiple options makes it possible to make comparisons, which in turn helps to be sharper in the evaluation of the results.

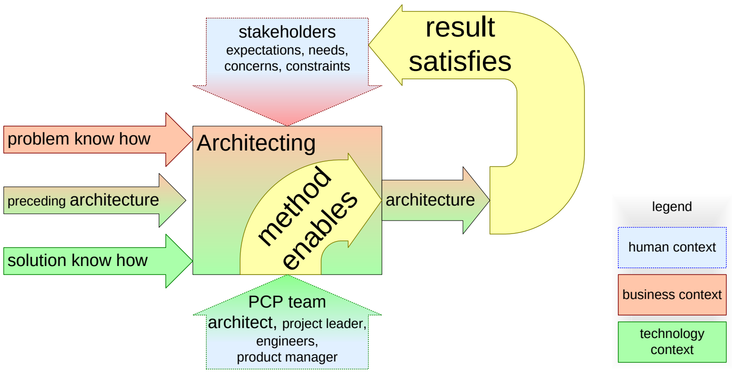


Figure 20: Successful architecting and architecting method

Figure 20, shows the input for the hypothesis of the author's dissertation. The hypothesis was:

A rich collection of submethods fitting in a multi-view framework complemented with reasoning methods enables successful architecting of technology and software intensive complex systems in heterogeneous environments by means of generic insights grounded in specific facts.

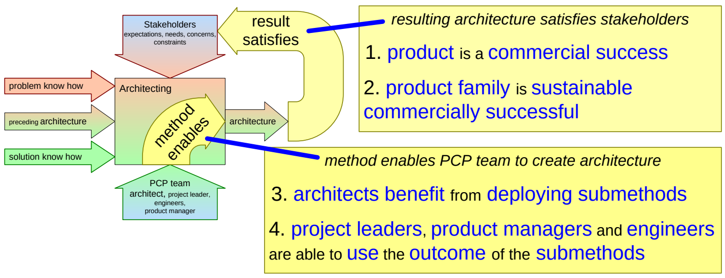


Figure 21: From hypothesis to criteria

This hypothesis in itself does yet give clear criteria for success. Figure 21 is a further annotation of Figure 20 to get to the criteria to evaluate the research.

6 Conclusion

We have shown in this paper ways to perform research in systems engineering. For PhD students the final delivery will be a dissertation. Figure 22 visualizes what typically will be the content of such dissertation.

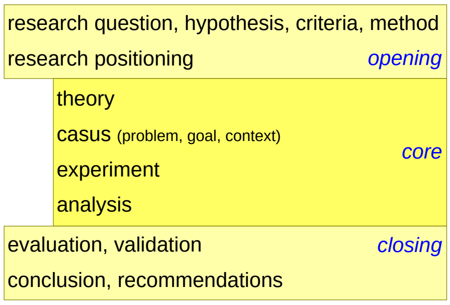


Figure 22: The Final Result

opening The opening introduces the research. The research questions, hypothesis, and criteria are good means to do this. The opening should also outline the research method that will be followed. And the work needs to be positioned in the body of knowledge.

core consists of a detailed description and explanation of relevant theory, the case in its context, the experiment, including the set-up and explanation of the rationale, and the analysis of the results.

closing The closing formulates the conclusion and argues about the validity of the research and its conclusion.

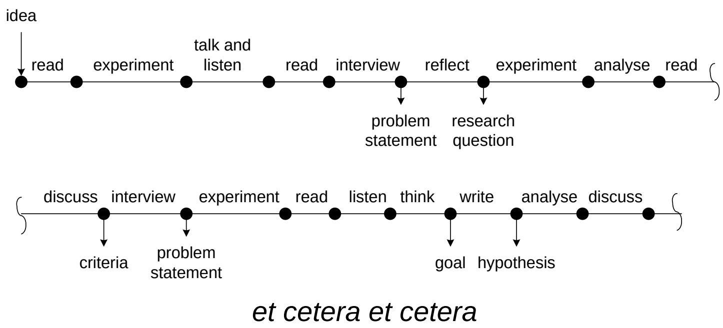


Figure 23: and the Chaotic Route

All figures so far suggest an ordered and structures path from the beginning of a research project to the end. However, realty is quite different, as is shown in Figure 23. This figure shows that research is often a chaotic process, where the different elements, such as problem statement, research questions, and hypothesis gradually crystalize. Lots of time is spent on doing things, such as reading, experimenting, listening, discussing, thinking, analyzing, et cetera. Many iterations are needed to achieve nicely formulated statements.

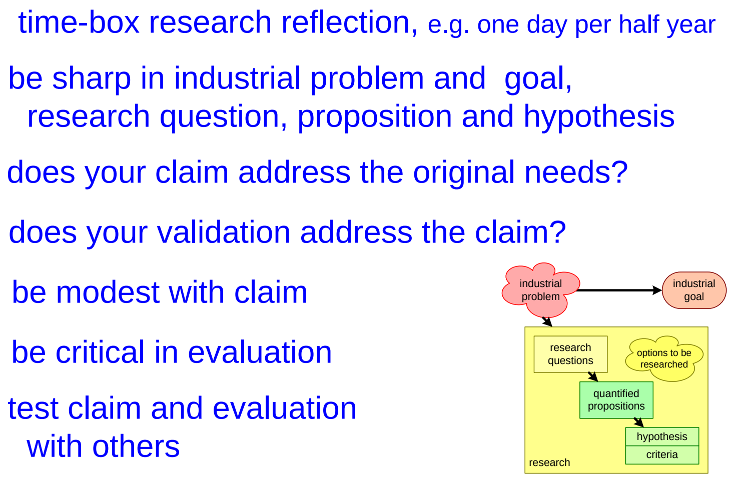


Figure 24: Recommendations

We conclude this paper with a set of recommendations, see Figure 24 The result of the research, articulated in research questions, hypothesis, propositions and the evaluation together form a claim about the research. Researchers tend to exaggerate their claim, e.g. our method solves X, while in reality it is only a part of the solution of X. Many of the recommendations address this claim. Is the claim relevant, e.g. does it address the original needs, stated in the problem statement and the industrial goal? Does your validation address this claim? Some validations prove the feasibility of a technique, while the claim is that it is valuable.

The last recommendation, *test claim and evaluation with others*, is to prevent inbreeding. PhD work needs to be original and to show the capability to do research work independently. However, this should not prevent the researcher to have frequent interaction about the research work.

7 Acknowledgements

Dinesh Verma stimulated the creation of this paper and presentation.

References

[1] Hay Management Consultants. Technology management cycle. Hay Managament Consultants showed me this model in 1997/1998, taken from an article by a Japanese author. The original title of the Japanese article is unknown.

[2] Moti Frank. Introducing aspects of systems engineering to freshman engineering students. In *proceedings of CSER 2005*, 2005.

[3] Robert Hunt. The origins of proof iv: The philosophy of proof. `http://plus.maths.org/issue10/features/proof4/`, 2000.

[4] Kurt Keutzer and Richard Newton. Productivity gap. `http://lark.vmei.acad.bg/asic/lectures/intro/slide1-5.htm`, 2000.

[5] Gerrit Muller. The system architecture homepage. `http://www.gaudisite.nl/index.html`, 1999.

[6] Gerrit Muller. CAFCR: A multi-view method for embedded systems architecting: Balancing genericity and specificity. `http://www.gaudisite.nl/ThesisBook.pdf`, 2004.

[7] Gerrit Muller. A multi-disciplinary research approach, illustrated by the boderc project. `http://www.gaudisite.nl/MultiDisciplinaryResearchApproachPaper.pdf`, 2008.

[8] Colin Potts. Software-engingeering research revisited. *IEEE Software*, Vol. 10, No. 5:19–28, September/October 1993.

[9] Henk Tuten. Popper and philosophy of science. `http://huizen.daxis.nl/~henkt/popper-scientific-philosophy.html`, 1999.

History

Version: 1.0, date: August 3, 2009 changed by: Gerrit Muller

- added text
- changed status to draft

Version: 0, date: April 2, 2008 changed by: Gerrit Muller

- Created, no changelog yet