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# Conducting Information Systems Research the Old-Fashioned Way

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**Abstract.** This research career retrospective summarizes the intellectual contributions of the author's academic career, covering 35 years from the early 1980's onwards. It also attends to various incidents and conditions that shaped his research career, as well as his research strategy choices that allowed him to overcome some of the challenges imposed by these conditions. These strategic choices comprised to do small research rather than big research and to privilege international collaboration over local collaboration.

*Key words:* research career retrospective, history.

## 1 Introduction

I guess that the careers of many researchers are guided by accidents rather than deliberate planning. At least I did not have any idea of academic careers, and still less any related ambitions, when I started my university studies in 1968. It was only after I completed my M.Sc. degree in 1974, and was employed as a computing specialist at the Department of Public Health Science at the University of Oulu, when I really got into contact with scientific research for the first time.

My work mainly involved statistical analysis—nowadays called data mining—using rich data for over 12,000 mothers and infants expected to be born in 1966 in Northern Finland (see <http://www.oulu.fi/nfbc/node/18080>). Eventually having become bored with running almost an infinite number of statistical tests, I moved to the Department of Information Processing Science at the University of Oulu to continue my studies in Information Systems (IS).

In those times, it was common in Finland as well as in the Scandinavian countries to complete a licentiate degree before applying for a doctorate. I completed my Ph.Lic. in 1978 and my Ph.D. in 1983. Immediately after receiving my licentiate, I was lucky to start working as an acting associate professor at the Department of Information Processing Science in Oulu, receiv-

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ing a (tenure) nomination in 1987. I moved to the University of Jyväskylä in 1992 working there as full professor until the end of 1995, when I returned to Oulu. I retired from there in 2011.

For some reason, I sensed from the very beginning of my academic career that the ethos of professors is—or ought to be—to conduct research. I also learned early on that it is important to keep on writing and publishing than wait for a revolutionary idea that would radically change the entire scientific discipline. Therefore, I have never had any grand research plans, but have merely attempted to make modest contributions to ongoing scholarly discourse. In short, I have carried out small rather than big research.

My research has focused on different topics partly for quite idiosyncratic reasons. Some of my teaching commitments have made me to develop course materials that have eventually evolved into research contributions. Occasionally, I have met interesting people willing to co-operate with me; sometimes, I have come up with research ideas when attending conferences and listening to talks, when reading papers, or simply without any clear source of inspiration. However, when reflecting on my research, I noticed that the large majority—perhaps 80%—of my papers has focused on five themes that had already been evident in my Ph.D. dissertation (Iivari 1983).

I have always had bad memory. In fact, my research career has involved a constant struggle with my memory, in terms of ways to compensate for its limitations in the context of rapidly changing technologies and fashions (Baskerville and Myers 2009). Since I have never been interested in technology *per se*, but more on what is occurring around it, I have attempted to abstract from specifics of technology in my work, such that my ideas would be invariant and independent of the state of technological development. I (privately) used to call them ‘iivariants.’ With regard to various fashions, my tactic has been to wait for some time, perhaps five years, to see whether it is still alive; if it is, I begin to examine it. As a consequence, I have always been a laggard in my research rather than *avant-garde*.

Constantly writing drafts of papers has served to compensate for my bad memory. I have never been able to formulate my thoughts simply in my mind. Writing them down has made it possible for me to discuss them not only with my colleagues, but first of all with myself—with my own thoughts. Unfortunately, I am not a particularly good writer—not in Finnish, and still less in English. Nonetheless, I am in the fortunate position to be writing a research career retrospective.

In the following I first reflect on research career retrospectives as a new genre of the IS literature. I then introduce my research career in two parts: First, I summarize my intellectual journey in terms of major research themes and contributions to the market of ideas (Lyytinen and King 2004) as I see them. Second, I reflect on it in terms of challenges posed by the institutional conditions on my research activity and my research strategies in response to them. The first part represents the sunny upside of my career and the latter part its darker downside.

## 2 Reflections on research career retrospectives

Research career retrospectives are a new genre in the IS literature (Lanamäki 2015). Lanamäki considers them a potential subgenre of historical research; e.g.; (Oinas-Kukkonen and Oinas-Kukkonen 2014; Porra et al. 2014). The team of reviewers, in its very constructive comments

on this paper, also took this view. However, it encouraged me to reflect not only on similarities between research career retrospectives and historical research, but also the differences.

When considering the similarities, it seems to me that research career retrospectives resemble microhistories in particular (Magnússon and Szijártó 2013), when the latter focus on particular people. A microhistory is an intensive historical investigation of a well-defined smaller object or phenomenon, such as an event, a local community, a family, or a person. Usually, the time span of microhistories is shorter than that of investigations of nations or states covering several decades, centuries, or millennia. Magnússon and Szijártó point out that microhistories stress human agency. "For microhistorians, people who lived in the past are not merely puppets on the hands of great underlying forces of history, but they are regarded as active individuals, conscious actors" (p. 5).

At the same time, Magnússon and Szijártó (2013) emphasize that microhistory has "an objective that is much more far-reaching than that of a case study: microhistorians always look for the answers for 'great historical questions'" (p. 5). They point out that such "great historical questions ... are never defined within the discourse of history itself; they are determined by the social and cultural factors" (p. 6). I must admit that I do not have in my mind any such "great historical questions" that are to be answered. Hence, this research career retrospective is not a microhistorical investigation. Despite this, could it be a historical investigation? I do not know.

The team of reviewers raised the issue of periodization in my research career retrospective. Periodization is naturally, highly relevant when attempting to make sense of past actions and events by aggregating and generalizing them over a long time span (Oinas-Kukkonen and Oinas-Kukkonen 2014). But I am not as certain about its usefulness, when the span shortens and the object of interest is one person. When reflecting on my own research career, I have a great difficulty in finding an insightful way to periodize it, since I have not experienced any clear turning points. Instead of periodizing it, I found it more useful to periodize its context; I do it separately for each research theme. If not more insightful, it serves to distinguish—in my modest way—the period in research before and after me.

I do not know if historians like to explain history in terms of coincidences, but I do sincerely think that my research career has been decisively guided by them. When a coincidence has led me to a situation that has opened a research opportunity, my general reflection on the situation can be condensed in four considerations: perceived opportunity, perceived relevance, interest, and perceived resources. In the case of each research project, I assessed if I have a reasonable opportunity to make a contribution to ongoing scholarly discourse. If the contribution seemed sufficiently relevant from the viewpoint of academia and, sometimes, from that of professional practice, I pursued it if I found the topic was interesting. Finally, I estimated if I had the resources—expertise, time, and money—to conduct the required research, either alone or in cooperation with my colleagues and/or students.

Lanamäki (2015) points out that research career retrospectives are not written by detached researchers, but are first-person narratives. I interpret that to mean that career retrospectives are also interested—at least more than traditional historians—in the subjective world of actors in the ontology of Habermas (1984), to which only the subject—I, in my case—has access. I assume that this focus on the subjective world has profound implications on all four principles of historical research identified by Oinas-Kukkonen and Oinas-Kukkonen (2014): objectivity, source material, sense making, and readiness for discourse.

The focus on the subjective world naturally challenges the objectivity of research. The principle of objectivity in historical studies (Oinas-Kukkonen and Oinas-Kukkonen 2014) should be interpreted to cover sincerity (Habermas 1984). Although authentic documents such as diary notes would help if available, in practice, an author writing his or her research career retrospective must also rely on memory and recollections of various events and actions, including motives for actions. At the very least, I am obliged to do so, and will attempt to be as sincere as possible.

### **3 Summary of my research career**

When I started my Ph.Lic. in 1975 and, later, my doctorate in 1979 at the University of Oulu, it was common at the time in Finnish universities, especially in a discipline as young as Information Processing Science (Information Systems), for departments not to have any formal Ph.D. education. Each doctoral candidate arranged his or her own coursework by combining studies from different disciplines with certain restrictions. My M.Sc. consisted of laudatur-level studies in Information Processing Science, cum laude-level in mathematics and national economics, and approbatur-level studies in physics and statistics<sup>1</sup>. I continued my studies in economics as part of my licentiate studies at the laudatur level (without the M.Sc. thesis). In general, these studies supported my later research career. However, the lack of any formal education on research methods, except statistics, was a huge deficiency.

I was lucky to have Professor Pentti Kerola as my supervisor, even though there were no alternatives. Pentti had a fairly extensive industrial experience. At the beginning of the 1960s, he had worked years at IBM, Finland, and then as CIO of the Enso–Gutzeit paper and pulp company (nowadays known as Stora Enso, following mergers). He returned to academia at the end of the 60s. At Enso–Gutzeit, Pentti had been involved in a systems development method that later became known as the PSC model (Kerola 1975; Kerola and Järvinen 1975).

Pentti was an inspiring person who easily became extremely enthusiastic with various ideas. He closely followed international IS research that began to emerge in the 1960s and 70s. He had an amazing capacity for associative thinking to find analogies in weakly related phenomena, and he was excellent at organizing things<sup>2</sup>.

During my licentiate studies, which addressed IT management in Finnish government administration, I figured out that I could make contributions to the PSC model. That is why I decided to focus on it in my Ph.D. dissertation. Research on systems development methods was a natural choice in my narrow world<sup>3</sup>. There was a considerable amount of interest in it in Scandinavia due to the influence of Professor Börje Langefors and his students, such as Janis Bubenko, Mats Lundeborg, Arne Sölvberg, and others, and in Finland.

As mentioned above, after completing my Ph.Lic. I was given the opportunity to start working as an acting associate professor at the beginning of 1979. Financially, it was a big improvement for someone with a spouse and two children, and a house to build. Implying a fairly high teaching load and various administrative duties, it naturally slowed down my doctoral studies, but I managed to complete my Ph.D. dissertation (Iivari 1983). I proposed in my dissertation the PICO model for IS analysis and design, or ‘systemeering’ (called ‘systemering’ in Swedish at that time, following Langefors).

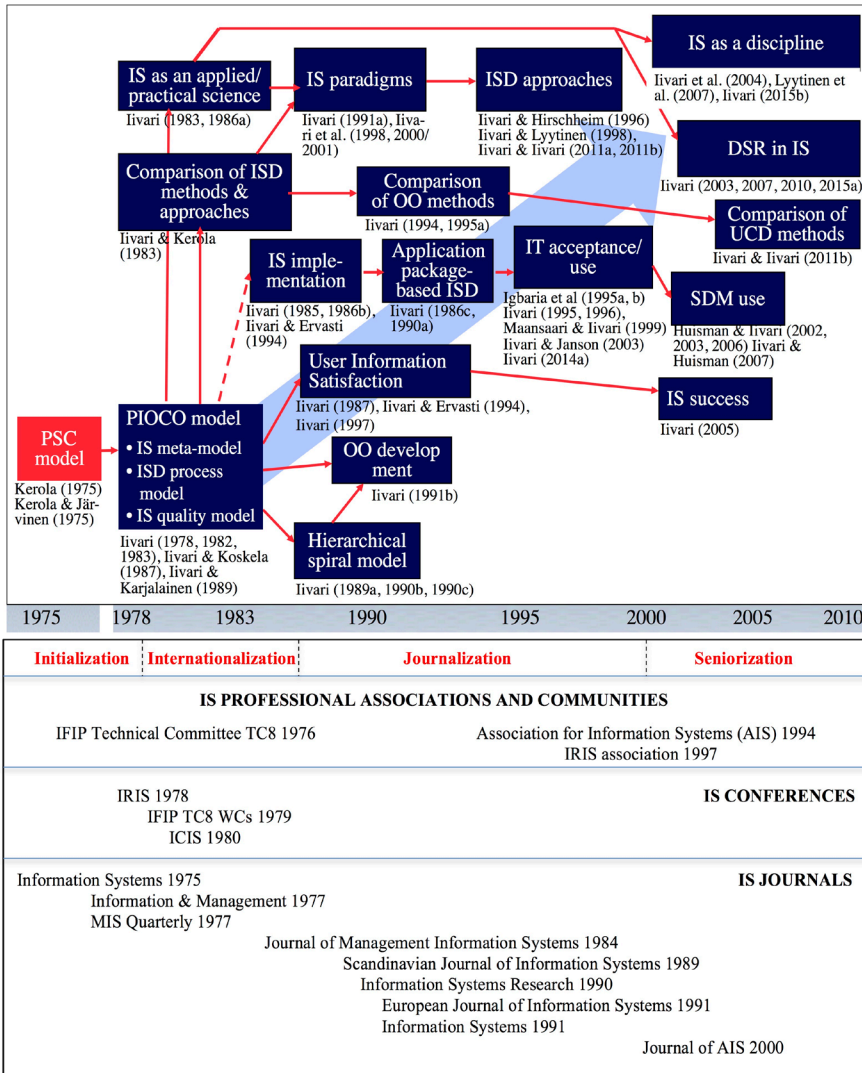


Figure 1. Continuity of my research themes

In retrospect, much of my later research can be interpreted as a continuation of the themes in my dissertation. Figure 1 depicts this. The red arrows do not describe direct influences between the boxes; e.g.; citations; but the continuity of the research themes. It seems that I have alternated between them, partly for reasons of maintaining interest. Yet, there is a general trend, depicted by the blue block arrow, to move toward meta-research covering IS as a discipline and DSR (Design Science Research) in IS.

I can identify seeds of four research themes in my dissertation (Iivari 1983):

1. DSR in IS development methods
2. Philosophy of IS as an applied/practical science
3. Comparative analysis of IS development methods and approaches
4. IS evaluation and success

Since my dissertation deliberately did not address the problem of organizational implementation, I decided to focus on it after completing my doctoral studies, leading to the fifth research theme:

5. Implementation and acceptance of information systems and other IT applications

This strong connection between dissertation and research themes during my research career is a surprise even to me, partly since my dissertation illustrate how intellectually isolated I was in the late 1970s and early 1980s from the rest of the world. My intellectual contacts were largely confined to Scandinavia, and in particular to Finland.

Figure 1 also describes my research career in terms of a four-level ladder. The first level or stage, *initialization*, of entering the scientific community, in a way began in 1975 when I commenced my licentiate studies. When I published my first paper at an international forum (Iivari and Koskela 1979), I advanced to the *internationalization* stage and, in 1986, to the *journalization* level, when I published two journal papers (Iivari 1986a, Iivari 1986b). The *seniorization* level refers to the stage when I was increasingly occupied by various editorial positions in a number of journals, such as the Information Systems Journal (1997-2012), Journal of AIS (2002-2008), European Journal of Information Systems (2003-2007), and MIS Quarterly (2007-2011). During my seniorization stage I was also occupied by the INFWEST/INFORTE programs, first organizing them, together with Econ. Lic. Juha Knuuttila, in 2000-2001, and then serving part time as their scientific head from 2002 until my retirement in 2011. These programs organized workshops and seminars around Finland with leading scholars as speakers.

Figure 1 also shows that to a large extent, my internationalization and journalization efforts took place at the same time that the early institutional structures—associations, conferences, and journals—of the entire IS discipline were established. The participants of the early IRIS (Information systems Research in Scandinavia) seminars formed a significant informal community, long before it was formalized in 1997, in my early attempts to internationalize at the Scandinavian level. And the first two working groups (WG8.1 and WG8.2) of the IFIP (International Federation of Information Processing) TC8 (Technical Committee–Information Systems and Organizations), with its working conferences (WCs), provided a forum for this at an international level.

In the early 1980s, the Department of Information Processing Science in Oulu did not have a culture of publication. Thus, it was largely up to me to establish and internationalize it, first within the IFIP (from 1979 onward) and in the ICIS (from 1985 onward), and finally at the journal level (from 1986 onward).

Next I review my research in all the five research themes shown in Figure 1. Sections 4-8 are fairly independent of one another, so that each can be read selectively depending on the reader's interest.

## 4 Design science research into IS development methods

This research theme represents my efforts in DSR that lasted over 10 years, from 1978 to the early 1990s. By ‘IS development’ (ISD), I refer to the activity of developing information systems (= a subset of IT artifacts) in practice. DSR in IS is a research activity involving the development of various IT meta-artifacts (Iivari 2003) or general solution concepts (van Aken 2004) to support the ISD practice. To illustrate this terminology, an ERP software package is an IT meta-artifact and a specific ERP-based information system is a specific IT artifact.

Since all readers may not be familiar with the history of ISD methods, let me to introduce it briefly to contextualize my own work. One can distinguish three major orientations when conceptualizing information systems: 1) *function/process orientation*, which dominated in the 1970s and the early 80s (Ross and Schoman 1977; DeMarco 1978; Lundeberg et al. 1981; Yourdon 1989), 2) *data orientation*, which prevailed in the 1980s (Verheijen and Van Bekkum 1982; Martin 1989; Nijssen and Halpin 1989), and 3) *object orientation*, which has dominated from the early 1990s onward, leading to the development of the UML (Unified Modeling Language) (Booch et al. 1999) as a kind of *de facto* standard of object orientation nowadays. In case of the process models, one can identify the *waterfall model* (Royce 1970), *prototyping* (Bally et al. 1977), the *spiral model* (Boehm 1988), and *agile models* (Abrahamsson et al. 2003). In principle, the two conceptualizations are orthogonal to each other, such that they can be combined to form 12 combinations<sup>4</sup>.

The ISAC method (Lundeberg et al. 1981) being the exception, the origins of all the above methods and process models are associated with Software Engineering (SE) rather than Information Systems. There are also approaches that have mainly originated in the IS community—for example, the socio-technical and participatory approach, the sense-making and the problem formulation approach, the trade-union led approach, and the emancipatory approach (Hirschheim et al. 1995)—even though their direct application to practice has been quite marginal (as is the case with the PICO model). The socio-technical approach being an exception, their development largely occurred concurrently with my own DSR work on the PICO model. Although I borrowed some ideas from them (most notably from the socio-technical approach), they have not been central to the development of the PICO model.

As noted above, I continued Kerola’s work on the PSC model (Kerola 1975; Kerola and Järvinen 1975). Its most concrete contribution was a process model where the process did not follow a linear structure similar to the waterfall model, but had a specific intertwined structure. This was because a decision at the higher level; e.g.; requirements of the system; requires knowledge of the technical implementability of the system and the cost of its implementation (Iivari 1978; Iivari 1983). To the best of my knowledge, this intertwined structure was a novel idea that was reinvented years later (Swartout and Balzer 1982).

My DSR work on ISD methods can be divided into three stages—the first stage leading to the PICO model (Iivari 1983; Iivari and Koskela 1987), the second stage to the hierarchical spiral model (Iivari 1990b; 1990c), and the third stage an attempt to make sense of object-oriented analysis and design using the hierarchical spiral model as reference. The particular research focus was object identification (Iivari 1991b).



## 4.1 The first stage—the PICO model

Due to my background, I was particularly interested in the relationship between Economics and Information Systems, and somehow gained access to Jacob Marschak's books on information economics (Marschak and Radner 1972; Marschak 1974)<sup>5</sup>. With my background in mathematics, I found them reasonably accessible.

Reading information economics led me to the eureka moment of realization that the PSC model and all systems development methods I knew did not recognize the fact that systems development is a continuous learning process, where each analysis and design step potentially produces new information and/or changes the state of the target system (the artifact) under development. This new information may affect the meaningfulness of future analysis and design steps and the manner of executing them. So, it became clear to me that methods that suggest rigid step-by-step processes for systems development are not meaningful, but it is logical to re-design the process almost continually in light of new information. This was the initial idea that led me to work on the PSC model.

My work on the PSC model addressed four aspects (Iivari and Koskela 1987):

- Levels of abstraction as a governing idea of the model.
- A meta-model for an information system as a product of ISD.
- A process model for ISD.
- Choice and quality criteria for ISD.

**Levels of abstraction.** The PSC model was based on the levels of abstraction—pragmatic (P), semantic (S), and constructive (C)—in linguistics. These levels prompted a lot of debate in Finland during the shift of the 1970s and 80s, leading to a situation where we adapted them as pragmatic (P), input-output (I/O), and constructive-operative (C/O) levels, inspired more by systems thinking than by linguistics. As a consequence the name PSC model was changed to the PICO model. In hindsight, perhaps the original name would have been better. Briefly stated, the P level focuses on the information system as a constituent of an organization, the S level on the information content of the system, and the C level on its technical solution. The P level represents a business-oriented view, the S level a user-oriented view, and the C level a technology-oriented view.

These levels of abstraction turned out to be a kind of invariants because they seem to occur over and over again in the IS literature<sup>6</sup>. Welke (1977) introduced a quite similar framework of perspectives—systemological, infological, and datalogical. The three levels of abstraction also influenced the idea of the three contexts—the technological context, the linguistic context, and the organizational context—of information systems (Lyytinen 1987), and likely also the three domains of change—organization, language, and technology—in Hirschheim et al. (1996).

**The PICO meta-model for an information system.** When still working on my licentiate thesis (Iivari 1978), I started productive cooperation with my colleague at the department, Erkki Koskela (later Ph.Lic.), in 1978. We figured out that it was difficult to speak concretely about the ISD process without an explicit idea of the concept of information systems as an outcome of the process. Hence, we started working on meta-models for an information system at different

levels of abstraction; e.g.; (Iivari and Koskela 1983; Iivari 1983; Iivari and Koskela 1987). To the best of my knowledge, we were the first to introduce such comprehensive meta-models for an information system.

More substantively, we attempted to integrate process orientation and data orientation in the PIOCO meta-model for an information system. Our way of modeling processes largely followed Lundeberg et al. (1978) with some modifications, and our manner of modeling data (Iivari and Koskela 1983) was influenced by a number of researchers working on conceptual information modeling, most notably Sundgren (1973) and Bubenko (1980). We also incorporated user-system interaction in the meta-model at the I/O level (Iivari and Koskela 1985). This was contrary to most ISD methods at that time, which completely omitted that interaction or considered it a mere technical issue.

**The PIOCO model for choice and quality criteria.** I also worked closely with Erkki when developing the PIOCO model of choice and quality criteria for IS evaluation (Iivari and Koskela 1979; Iivari and Koskela 1987). Even though notable research had been conducted on IS evaluation; e.g.; (Emery 1974; Land 1976; King and Schrems 1978); we felt that it had only been weakly integrated with the ISD. We concluded that different quality criteria are meaningful at different levels of abstraction. As a consequence, we specified a set of criteria at each level of abstraction: total effectiveness at the P level, user satisfaction at the I/O level, and total efficiency at the C/O level. I am still satisfied with the framework, even though some of its details can be improved.

**The PIOCO model for the ISD process.** I worked more by myself when developing the PIOCO model for the ISD process. The underlying idea was that IS analysis and design is not simply a sequence of transformations between levels of abstraction of the IS product, ultimately leading to a running system, as in ISAC, for example (Lundeberg et al. 1981). It is also process of inquiry providing decision makers (steering committees and other participants) with information regarding design alternatives at each level of abstraction in order to reduce the related uncertainty. Inspired by information economics (Marschak and Radner 1972; Marschak 1974) and sociocybernetic theory of acts (Aulin-Ahmavaara 1977), I conceptualized IS analysis and design as a process of sequential and parallel IS design acts, which are either 1) observation/analysis acts, emphasizing the diagnostic aspect of the IS design situation (conceptualized as “object systems” to be observed in Iivari 1983), or 2) manipulation/refinement/design acts, designing or refining the design artifact (called “target system” in Iivari 1983). Each design act increases knowledge of the IS design situation (including host organization, user requirements, available technology, and existing IS artifacts).

Consequently, the PIOCO model viewed IS analysis and design as a learning process, requiring continuous planning of the process, similar to prototyping that had emerged a bit earlier (Bally et al. 1977; Earl 1978). The point was, however, that prototyping is not a necessary precondition for such learning.

Ideas of IS and software evolution (Keen and Scott Morton 1978; Lehman 1980), on the other hand, inspired me to introduce evolution dynamics to the PIOCO model for the ISD process. Both prototyping and evolutionary development allow learning, but are different in the sense that the former is confined to experimental use of prototypes, whereas learning in the case of evolution dynamics is based on real use of a real system with real data in practice (Iivari 1982).

The information/knowledge/uncertainty-oriented view of the ISD process also led me to emphasize its situational flexibility. A number of suggestions on how to select an appropriate ISD method or technique for an organization or a project (Davis 1982; Naumann and Palvia 1982; Shomenta et al. 1983) had already been made in the early 1980s; but to the best of my knowledge, none of them emphasized the situational flexibility of single ISD methods. If the ISD situation—or the understanding of it by people involved in the process—is constantly changing due to learning, there is a need for built-in flexibility in ISD methods (Iivari 1989b) that allows the adaptation of the method-in-use on the fly in the ongoing project.

This idea of the built-in flexibility of ISD methods has received some attention (Avison and Wood-Harper 1991; Conboy and Fitzgerald 2010). However, as I understand it, its importance has not been fully understood, at least not in academia. Referring to Conboy and Fitzgerald (2010), I think that it remains a highly relevant feature to be included in ISD methods.

## **4.2 The second stage—the hierarchical spiral model**

The results of the PIOC model were summarized in Iivari and Koskela (1987). At around that time, Boehm (1988) published his spiral model. I noticed a clear similarity between it and the PIOC process model. The systems development process in both models can be conceptualized as spiral (called sub-phases in the PIOC model), each with a nonlinear structure. The difference is that each main phase has its own spiral in the PIOC model (this is why I called it a hierarchical spiral model). As a consequence, the PIOC model can have several rounds (called sub-phases) at the I/O level, for example, by focusing on the construction of IS requirements, whereas Boehm (1988) identifies software requirements in one round.

Inspired by Boehm (1988), I wrote two papers on the hierarchical spiral model (Iivari 1990a; 1990b), which was essentially based on the PIOC model. Following Iivari (1989a), I renamed the levels of abstraction again—organizational (O), conceptual/infological (C/I), and datalogical/technical (D/T) levels—to generalize them, so that they were not specific to any ISD method, and revised the corresponding meta-models for these levels of abstraction. This led to the name OCIDT model instead of the PIOC model.

Both Boehm's spiral model and the PIOC/OCIDT model emphasize uncertainty and risk. Yet, the latter attempts to strike a balance between a model-driven approach (based on IS models at different levels of abstraction) and risk-driven approach, while Boehm emphasizes the latter approach. A major difference between Boehm (1988) and Iivari (1990a; 1990b) is that the former contains concrete ideas regarding possible risks and some resolution mechanisms, even though the list of prioritized software risks in Boehm (1988) seem very tentative. The PIOC/OCIDT model, on the contrary, remains more abstract, and focuses on the uncertainties related to the system to be developed and the ISD process, and the analysis and design acts to be performed to reduce them.

### 4.3 The third stage—the object identification

In the late 1980s, there were clear signs that object orientation would become the dominant way of structuring software and information systems. This led to my last DSR effort to use the OCIDT model to structure OO analysis. An important impetus for this was my observation that in OO thinking, almost all things are objects. This generality did not seem meaningful from the viewpoint of OO analysis, since it does not help determine objects (or object classes) to be identified during OO analysis and design. Based on the OCIDT meta-model for information systems, I identified five categories of objects (Iivari 1991b):

1. User objects, where each user object represents a user within the information system.
2. Objects of the universe of discourse; e.g.; entities and events; about which the information system records and maintains information.
3. Information type objects; e.g.; input and output documents, queries, and information objects in the database.
4. User interface objects, such as windows, menus, icons, etc.
5. Objects of abstract technology, such as equipment and devices, which are used in the technical implementation of the system.

This categorization still makes sense to be me, even though perhaps I should have interpreted user objects as user agents, each user with his/her own software agent. I do not know if a similar categorization has been proposed elsewhere. If not, I would be terribly surprised.

## 5 Research on the philosophical foundations of Information Systems

Despite some interest; e.g.; (Mason and Mitroff 1973); remarkably little research has been published on philosophy in Information Systems before 1980. This is surprising, since qualitative research methods such as case studies and action research were clearly recognized as relevant to IS research as early as the 1970s; e.g.; (Earl 1978); and information/knowledge is a heavily philosophical concept. The IFIP Colloquium “Information Systems Research—A doubtful Science” held in Manchester in 1984 (Mumford et al. 1985) implied a significant change, bringing together a number of researchers interested in philosophical issues in the IS context. It focused on alternative research methods and related philosophical backgrounds. Later, Hirschheim et al. (1995) complemented it by focusing on the philosophical foundations of data modeling and ISD.

I became aware of philosophical aspects of ISD when I attended the early IRIS conferences from 1979 onwards, and met Göran Goldkuhl and Kalle Lyytinen. They were very philosophically minded young men who were particularly fascinated by Habermas. It was impossible to understand their conversations without some knowledge of philosophy. This might have been

why I thought that a doctoral dissertation should include some sort of explanation of the underlying research philosophy. But knowing Göran and Kalle's talents, I decided to skip Habermas.

My philosophically oriented research has addressed two topics: the philosophical aspects of DSR and the paradigmatic assumptions in ISD methods.

## **5.1 Philosophical aspects of design science research**

As explained above, the core contribution of my dissertation (Iivari 1983) was the PIOCO model. According to my interpretation it was essentially a DSR piece of work, even though that term was not used at the time<sup>7</sup>. The issue was that such a work did not nicely fit with much of the philosophy of science at the time, which assumed that you are investigating reality, whether natural or social<sup>8</sup>. How, then, do I get it to fit the picture?

Dealing with the above puzzle led me to ideas that are nowadays discussed in the DSR literature (Iivari 2003; Iivari 2007; Iivari 2010; Iivari 2015), in an attempt to clarify what DSR is and is not. The ideas of Chmielewicz (1970) were very helpful when addressing this issue. Lehtovuori (1973) had introduced his ideas in Finnish, proposing that one can distinguish four levels in economics: the conceptual level, the descriptive level of economic theory, the prescriptive level of economic policy, and the normative level of economic philosophy. Interestingly, Winter (2008) applies the 1990 edition of Chmielewicz's book to make sense of DSR, differing in some interpretations from mine (Iivari 2010).

From the viewpoint of IS history, one of my theses has been that DSR in IS has a far longer history than is implied by Hevner et al. (2004), or even Walls et al. (1992). DSR has been conducted from 1960 onward; e.g.; (Langefors 1966; Bubenko et al. 1971; Teichrow and Sayani 1971). It is quite astonishing how the IS community has managed—after 50 years of DSR practice—to create such a hype around DSR with a really confusing hodgepodge of ideas (Baskerville et al. 2015).

## **5.2 Paradigmatic assumptions of IS development methods**

My interest in ISD paradigms started from my teaching commitments. I taught a master's-level course called "Theories of systemeering" (later, "IS theories") for several years starting in 1984. One of the aims of the course was to introduce different schools of thought in ISD to students. It was at some time in the mid-1980s when I invited Professor Heinz K. Klein, who had had a longer stay in Jyväskylä with Kalle Lyytinen, for a short visit to Oulu to give a seminar as part of my course.

In his talk, Heinz introduced ideas of paradigms of ISD, which he later published with Professor Rudy Hirschheim in their seminal paper (Hirschheim and Klein 1989). After that presentation, I got the idea that perhaps the underlying paradigmatic assumptions of the different schools of thought of ISD can be analyzed using the dimensions of Burrell and Morgan (1979). That ultimately led to my EJIS paper (Iivari 1991a). One idea in this paper was to use Burrell and Morgan (1979) as a more analytical framework than used in Hirschheim and Klein (1989), which seemed to me a more impressionistic or holistic interpretation of the texts analyzed. To

my surprise, as far as overlap with Hirschheim and Klein (1989) was concerned, my analysis largely confirmed their findings. As a detail, Iivari (1991a) also coined the term ‘constructive research’ as a special research method in DSR. Constructive research is the research method that involves building artifacts<sup>9</sup>.

Thus, the real impetus for my philosophically oriented work on ISD paradigms was meeting with Heinz Klein. Even though he was a real fan and expert of Habermas, I got along with him. Perhaps it was his German mentality, which was just superficially sugared by some Americanism, that appealed to me. When considering places to visit during my sabbatical in 1988-1989, I decided to stay in the fall semester at the State University of New York, Binghamton, where Heinz worked. During the spring semester in 1989, I visited the University of Houston, where Professor Rudy Hirschheim had just moved.

These visits led to long-lasting cooperation with Heinz and Rudy, which continued until Heinz’s untimely death in 2008. It resulted in several coauthored papers on ISD paradigms, ultimately leading to a four-tier framework of ISD paradigms, approaches, methods, and techniques (Iivari et al. 1998; Iivari et al. 2000/2001). Gregor (2006), for example, introduces it as an example of “theory for analyzing” in the IS literature. It is also reasonably well-cited. I am pleased to see that Porra et al. (2014) applied it when making sense of the historical research method. This suggests that the framework has a scope of application beyond ISD methods.

## 6 Comparative analysis of IS development methods and approaches

A comparison between systems development methods has continued to attract interest in the IS and SE fields; e.g.; (Taggart and Tharp 1977; Wood-Harper and Fitzgerald 1982; de Champeaux and Faure 1992; Fichman and Kemerer 1992; Song and Osterweil 1992; Abrahamsson et al. 2003). Siau and Rossi (2011) have recently reviewed this line of research.

The CRIS (Comparative Review of Information System development methodologies) conferences formed one of the most ambitious efforts to compare ISD methods in the 1980s. They attempted to take stock of prevalent methods at the time, apply them to a common case of an IFIP working conference (Olle et al. 1982), and conduct a feature analysis with the purpose of identifying commonalities and differences among the methods (Olle et al. 1983). Altogether, it extended into a series of five conferences, two of which I participated in.

When IFIP TC8 WG8.1 organized the second CRIS conference in 1983, Pentti Kerola and I decided to submit a paper that compared four ISD methods using a sociocybernetic framework consisting of 85 detailed questions (Iivari and Kerola 1983). It belongs to the category of “feature comparisons” in Siau and Rossi (2011); but as the number of questions indicate, the purpose of the framework was to conduct a deep feature analysis of methods— to identify various planning theoretical distinctions (Faludi 1973; Ackoff 1974), for example—rather than mere surface analysis.

Later, when working on OO in the early 1990s, I continued this research stream by comparing six OO methods prevalent at the time (Iivari 1994; Iivari 1995a). The former was intended

to be the major contribution while the latter consisted of leftovers that I had decided not to include in the former. Iivari (1994) was published in the IFIP conference proceedings whereas Iivari (1995a) appeared in *Information and Software Technology*. Iivari (1994) has hardly received any citations, whereas Iivari (1995a) has received considerably more. Thus, I learned that sometimes the publication outlet matters more than content. It is true not only in the case of conference papers and journal papers, but also of papers published in premier journals and less prestigious ones (Didegah and Thelwall 2013).

My philosophical interests also resulted in comparisons based on paradigmatic analyses (Siau and Rossi 2011): such contemporary schools of thought of ISD as (Iivari 1991a), and more recent ISD methods and approaches (Iivari et al. 1998; Iivari et al. 2000/2001). This line of my research led me to compare ISD approaches rather than ISD methods.

Ultimately, my latest comparison paper (Iivari and Iivari 2011b) focused on a specific ISD approach without any philosophical focus. It analyzed the concept of user-centeredness and compared four user-centered methods accordingly. In particular, I think that the four dimensions of user-centeredness—user focus, work-centeredness, user involvement and system personalization—and views related to each dimension made a conceptual contribution to the discourse on user-centeredness.

## 7 IS evaluation and success

IS evaluation has also been an enduring theme in the IS literature (see Hamilton and Chervany 1981a; 1981b, Smithson and Hirschheim 1998; Song and Letch 2012). Despite this attention, mainstream IS research in my opinion has not taken IS evaluation very seriously when compared with work in medical/health informatics in particular (Iivari 2015b). I suppose that one reason for the heavy attention to IS evaluation in the latter is the culture of medical research, where all new medications and treatments must be carefully tested and evaluated prior to adoption and use in medical practice. This evaluation culture is not limited to embedded software systems with potential fatal errors; e.g.; (Leveson and Turner 1993); but covers all sorts of medical information systems (Ammenwerth and Keizer 2005).

From my perspective, IS evaluation can be divided into three major research themes that broadly followed one another. The first—dominant in the 1970s—focused on the cost/benefit analysis and organizational effectiveness of information systems (Emery 1974; Frielink 1975; King and Schrems 1978). The second, with its heyday in the 1980s, was more user-focused with its interest in user information satisfaction (UIS) (Bailey and Pearson 1983; Ives et al. 1983; Doll and Torkzadeh 1988). It was followed by the success model of DeLone and McLean (1992) that emphasizes the multi-dimensionality of IS success.

As explained above, the PICO model of choice and the quality criteria suggested three complementary constructs for IS evaluation: total effectiveness criteria at the P-level, user satisfaction criteria at the I/O-level, and total efficiency criteria at the C/O-level (Iivari and Koskela 1979; Iivari and Koskela 1987). When I worked with Erkki Koskela on these criteria, we were strongly influenced by the literature on cost-benefit analysis and the organizational effectiveness of information systems, in particular by several papers published in Frielink (1975). We

developed our user satisfaction construct independently of UIS research, if Zmud (1978) is not considered its representative. Therefore it is the greatest individual contribution of the PIOCO model of choice and quality criteria, in addition to the whole framework.

Our user satisfaction construct was focused on the *ex ante* assessment of individual information systems rather than on the *ex post* evaluation of the whole IS/IT department or function, as in the early UIS papers; e.g., (Bailey and Pearson 1983; Ives et al. 1983). In that sense, our work foreshadowed Doll and Torkzadeh (1988). But our model was based on conceptual thinking only, without any empirical data.

The PIOCO model for choice and quality criteria was also multi-dimensional, and contained constructs that had clear correspondences with the four constructs of DeLone and McLean's (1992) success model (total efficiency criteria with system quality, user satisfaction with information quality and user satisfaction, and total effectiveness with organizational impact). This sparked my interest in DeLone and MacLean's (1992) model, and I tested it in Iivari (2005). This was my final contribution to this theme of IS evaluation and success. Fortunately, it has been reasonably well received in terms of citations.

## 8 IS implementation and acceptance

This theme covers my research into the implementation, adoption, and acceptance of different examples of IT artifacts, such as traditional information systems, microcomputers, CASE tools, and systems development methods and approaches.

### 8.1 Organizational implementation of information systems

After completing my Ph.D. dissertation, I started examining issues related to the organizational implementation of information systems—"the tragedy and comedy of planning" in Churchman's (1979) terms. It had been a topic of intensive research already in the 1970s (Ginzberg 1978; Ginzberg 1981; Keen 1981; Lucas 1981). My work led to two ICIS papers (Iivari 1985; 1986c) and two journal papers (Iivari 1986b, Iivari 1990a). Iivari (1985), inspired by Churchman's quotation, viewed implementation from the perspective of planning theory, and suggested that the comprehensiveness and deepness (thoroughness) of various ISD activities have pros and cons from the viewpoint of the success of IS implementation<sup>10</sup>.

Iivari (1986c) analyzed IS implementation from the viewpoint of the diffusion of innovation theory. This line of research resulted in two journal papers (Iivari 1986b; 1990a). Since I regard Iivari (1986b) as one of my major intellectual achievements—an *iivariant*, if you wish—let me comment on it.

Iivari (1986b) proposed a model that analyzes the effects of the complexity, radicalness, and originality of an information system on the success of IS implementation. Following Pelz and Munson (1982), I interpreted originality along a more or less continuous scale, borrowing/adaptation/origination, suggesting that in the context of IS, application package-based information systems are examples of low originality (Iivari 1986c). My analysis indicated that information



systems with high complexity, high radicalness, and low originality are notoriously difficult to implement.

Many information systems built on ERP (Enterprise Resource Planning) software packages are prime examples of systems with high complexity, high radicalness, and low originality, with originality varying according to the degree of customization. Existing research suggests that such information systems have encountered considerable implementation-related difficulties (Momoh et al. 2010; Schniederjans and Yadav 2013). Therefore, I am surprised that nobody studying challenges in ERP implementation has referred to Iivari (1986b) or Iivari (1990a). As far as I have followed research on ERP implementation, Karimi et al. (2007) come closest to my ideas. They include radicalness and complexity (functional scope, organizational scope, and geographical scope) in the research model. However, when looking at their measurements, it becomes clear that they do not use the constructs with the same meaning as I do.

## 8.2 TAM research

I became involved in TAM (Technology Acceptance Model) research by accident. It was in 1992 when I attended the *ACM SIGCPR Conference* in Cincinnati, Ohio, and met Professor Magid Igbaria one evening. He was a very interesting person. He was a Palestinian who had lived in Israel, had obtained his doctorate at the University of Tel Aviv, and had moved to the U.S. I understood that despite all the conflicts between Palestine and Israel, he had a good relationship with his Israeli colleagues.

Magid was an exceptionally prolific scholar who was eager to collaborate with foreign colleagues. That evening, we agreed preliminarily about collaborating; I would help Magid conduct an empirical research project in Finland. A few months later, he sent me a questionnaire that I translated into Finnish, and recruited two master's students for empirical data collection. They did excellent work, managing to get over 500 responses. That data ultimately led to four journal publications (Igbaria and Iivari 1995; Igbaria et al. 1995; Iivari and Igbaria 1997; Igbaria and Iivari 1998), the first two of which are my most cited papers.

Unfortunately, Magid died in 2002 far too early, at the age of 44. It was a significant loss to the whole IS community.

I returned to the TAM model years later. Since it was the biggest 'miskick' ('hutipotku' in Finnish) of my research career, let me explain this as well. When working on a paper on DSR (Iivari 2007), I developed a typology of IT applications. It then occurred to me that this typology might explain some of the variety of TAM research and inconsistencies therein, since TAM (Davis et al. 1989) had originally been developed in the context of augmenting applications (word processing software).

This line of thinking led me to develop the Generic Individual Use of Information Technology Applications (GIUITA) that was built on the typology of IT applications of eight ideal types (automating, augmenting, mediating, informing, entertaining, artistizing, accompanying, and fantasizing applications), each with its typical designable characteristics (Iivari 2014b). For example, informing applications include information quality, which is not included in the original TAM (Davis et al. 1989)<sup>11</sup>. To partially test the model, one of my M.Sc. students collected data on Facebook (= a mediating application) use. The model also contained a new

construct ‘perceived sociability of use’ (PSOU) in recognition of the fact that the most important reasons for using social networking applications, such as Facebook, are related to socializing (Brandzæg and Heim 2009). It turned out to be pivotal to the model: maintaining social contacts (as a component of PSOU) was a significant predictor of perceived benefits ( $\approx$  perceived usefulness), perceived enjoyment, attitude toward use, and intention to use.

I attempted to get the paper published in two traditional IS journals, but failed. Finally, it appeared in the *Open Journal of Information Systems* (Iivari 2014a). This struggle made me to reflect on TAM research at that time. There had been growing boredom with it within the IS community for some time (see Journal of AIS 8(4) 2007). The results of Iivari (2014a) also challenged prior TAM research. They implied that all empirical studies that had tested TAM in the case of mediating applications might have suffered from a serious specification error when omitting perceived sociability of use (or a similar construct).

So, it may have been that my paper contained wrong ideas at the wrong time expressed in the wrong way. Despite that, I wish that future research into individual use of IT applications is redirected along the course I suggested in Iivari (2014b): 1) to recognize that IT applications are not necessarily used for some external task or activity, 2) to make TAM and related research more design oriented by systematically focusing on how designable characteristics of IT applications may explain their use, 3) to recognize that IT applications may differ in terms of those designable characteristics, and 4) to avoid obvious specification errors.

### 8.3 Acceptance and use of systems development tools and methods

When I moved to Jyväskylä in 1992, Kalle Lyytinen had a big DSR project (called Metaphor) to develop a Meta-Edit CASE (Computer Aided Software /Systems Engineering) tool. This was the time of the second wave of CASE research, the first wave having begun in the late 1960s (Teichroew and Sayani 1971), leading to a number of other DSR projects such as CASCADE, CADIS, and DIFO in Scandinavia alone (Bubenko et al. 1971). To the best of my understanding, the second wave was more industry driven, appearing in the academic discourse during the latter half of the 1980s (Case 1985). The Metaphor project (Kelly et al. 1996) was one of the most ambitious—if not *the* most ambitious—academic DSR projects in this second wave.

Affiliated with the Metaphor project, two of Kalle Lyytinen’s doctoral students had conducted an empirical research project on CASE experiences in cooperation with Danish colleagues (Aaen et al. 1992). I was skeptical at the extreme caution with which they had applied statistical methods, confining the analysis to non-parametric methods, since the measures they had used, strictly speaking, were not interval scales. I did not consider this dogmatic orthodoxy reasonable. When not agreeing with Mr. Veli-Pekka Tahvanainen, one of the co-authors of Aaen et al. (1992), on this issue, I decided to conduct an empirical study alone on CASE adoption and usage in Finland. Luckily, I managed to recruit two master’s students for data collection as part of their master’s thesis projects. This led to two journal papers (Iivari 1995b, Iivari 1996). The latter is reasonably well cited. Its most distinctive feature is that it includes the extent of CASE usage as a variable predicting CASE effectiveness. Prior research had omitted it, believe or not.

After moving back to Oulu in 1996, I conducted a replication study of CASE adoption and usage with another M.Sc. student, Jari Maansaari (Iivari and Maansaari 1998; Maansaari and Iivari 1999). However, by that time, excitement in the literature about CASE had begun to wane.

Iivari and Maansaari (1998) also contained some empirical analysis of the systems development methods used by the sample of the Finnish CASE adopters identified in the study. When compared with the huge effort invested in DSR on developing new methods, there was relatively little empirical research on their actual use in practice, especially in the IS literature (Wynekoop and Russo 1997), although more recent research; e.g.; (McLeod et al. 2007; Chan and Thong 2009); may have altered the situation.

Maansaari and Iivari (1998) raised a number of conceptual issues related to method use, which challenged prior findings that systems development methods are not used and, if used, they are not used literally. For example, we suggested that use may occur at the level of approaches; e.g.; object orientation) rather than at level of methods, possibly using specific techniques without any specific method. Quite interestingly, Lang and Fitzgerald (2006) found some empirical support for this conjecture.

Despite all my interest in ISD methods, I became really involved in empirical research on their use by accident. When still in Jyväskylä, a Ph.D. student from South Africa, Mrs. Magda Huisman, had contacted Kalle Lyytinen, who had encouraged her to contact me. She had originally been interested in conducting a study of CASE usage in South Africa; but as the start of her study was delayed, I advised her to change the topic to the use of systems development methods. She did excellent work in quite special circumstances (where postal service, for example, was not reliable, so that some responses were lost when returned by the respondents). That study led to a number of conference papers (Huisman and Iivari 2002; 2003 as most notable) and two journal papers (Huisman and Iivari; 2006; Iivari and Huisman 2007).

Iivari and Huisman (2007) analyzed the impact of organizational culture on the use of systems methods. This inspired my Norwegian colleagues to invite me to submit something similar to the XP2010 conference, focusing specifically on agile methods. I became interested in the idea and asked Netta Iivari to join the project, since her expertise in culture studies nicely complemented my own in ISD methods, their comparison, and adoption. This ultimately led to Iivari and Iivari (2011a). This paper, together with Iivari and Iivari (2011b), was a nice way of passing the baton to the next generation before my retirement in 2011.

## **9 Reflections on my research career**

The following four sections reflect changes in the institutional conditions of my research—the eroding status of professors, the increased emphasis on competitive research funding, research collaboration, and pressure toward big research. I will analyze them for three reasons. First, they have strongly shaped my career, ultimately contributing to my early retirement. Second, as a patriot, I am extremely worried about the declining quality of the Finnish research in the first decade of this millennium in comparison with competitors (Suomen Akatemia 2016a). Third, being scientifically minded, I always look for evidence-based answers to questions, if available.

I attempt to discuss these issues at a general level without paying much attention to my concrete, local conditions, since it would be a far too long a story and likely not of interest to outsiders.

## 9.1 The eroding position of professors

I was quite young—32—when I began working as an acting associate professor in 1979<sup>12</sup>. When reflecting on the process of entering into the community of professors, Lave and Wegner (1991) mirror quite well the way in which I proceeded from the periphery of the community of professors to its center and beyond. Professor Pentti Kerola took a sabbatical during the academic year 1979-1980, and I acted as the head of the department during his stay abroad. In that role, I was automatically a member of the board of the Faculty of Sciences at the University of Oulu. I was silent in those board meetings. Later, I have attempted to speak when I have had opposing opinions and, normally, I have not had any shortage of them.

Over the years, I gradually entered the center of the community as a full member. However, I gradually discovered that I did not conform to the new role of professors as obedient civil servants. I was too old-fashioned for that. I could not accept the deteriorating position of professors in the Finnish universities. This was one reason to retire.

Indeed, when looking back, I have the impression that when I started as an acting associate professor, professors were ‘real professors,’ like lords of their fiefdoms, promoting and protecting the interests of their department—and of the corresponding discipline—by all means available. They were personalities who had had opinions and dared to express them. Gradually, when New Public Management (NPM) ideology gained a foothold in Finnish universities, professors became mere civil servants, who silently—or perhaps fists in pockets—followed orders from above<sup>13</sup>. After the Finnish university reform in 2010, when universities were separated from the government, professors are not even civil servants anymore. They can be expelled by will by the university (rector). If fired illegally, they may get some monetary compensation, but not their chair back. That is an effective way of making them obedient.

## 9.2 Competitive research funding

I have always been poor at raising money. The Academy of Finland is the major body that provides grants for scientific research in Finland. During my career, I managed to get funding for one project from the Academy in the mid 1980s, when I was just a novice, and a one-year senior scientist grant in 1993-1994 for serving as the leader of the Finnish doctoral program in Information Systems (see note 2). Later, when I had garnered some merit, I did not receive anything from the Academy when I occasionally applied, last in 2007.

An explanation for this may be that even though the Academy of Finland website mentions the scientific quality of the research plan and competence of the applicant/research team as criteria of funding, it prioritizes the former in practice. In its response to Oksanen and Räsänen’s (2016) criticism of its funding decisions, the Academy admits that: “the main emphasis of the

assessment, in line of the European practice, is in the scientific quality of the research plans and not in the earlier outputs of the applicants” (Suomen Akatemia 2016b).

I do not know if a similar funding system is in use elsewhere, but this Finnish system seems unreasonable to me. I am confident that earlier publication records of applicants predict better the quality of their future research activities than research plans written for funding bodies.

I am pleased to note that I am not alone in harboring a critical view of the competitive funding of research projects. Ioannidis (2011), in his column in *Nature*, for example, criticizes the funding scheme based on research plans and projects. Furthermore, Fang and Casadevall (2012) point out (p. 898):

Review panels are able to accurately identify bad science but have a poor record of distinguishing highly innovative work or work that challenges existing dogma. Reviewers can be counted on to identify the top 20 to 30% of grant applications, but identifying the top 10% is impossible without a crystal ball or time machine. It is well documented that grant peer review is insufficiently precise to provide reliable rank ordering of applications.

A recent article by Li and Agha (2015) in *Science*, on the other hand, reports more positive results concerning the capability of review panels to identify the best grant applications. Based on data from over 130,000 applications from the U.S. National Institutes of Health (NIH) between 1980 and 2008, they found that better peer-review scores were consistently associated with better research outcomes in terms of number of publications, citations they received, and the number of patents<sup>14</sup>. One should also note that NIH review panels also evaluate investigators’ skills. Despite this, the supplementary material of Li and Agha (2015) shows that overall, applicants’ publication histories (especially the number of their papers among the top 0.1% articles in the life sciences) explained research outcome better than review scores.

Hence, the results of Li and Agha (2015) do not change my opinion about the reasonableness of the current review practice in the Academy of Finland.

Ioannidis (2011), Fang and Casadevall (2012), and Li and Agha (2015) reflected on experiences in medicine and biosciences. If this is the situation in those fairly established disciplines, the situation is likely still more challenging in Information Systems, which is a fairly young discipline.

The amount of competitive research funding raised by a professor has become a performance indicator in Finland. Knowing my limitations, I refused to enter the hamster wheel of continuously applying for research funding. It has saved me an enormous amount of time and energy. The lack of any external funding has naturally constrained what I have been able to do. I have been forced to do practically all my research as a part of my professorship (including normal administration, and over and above the average teaching load at the department), and to use master’s students as research assistants in my empirical projects. I am pleased to observe that 13 of my journal papers are based on empirical material collected in *pro gradu* (M.Sc. thesis) projects. As a result, my research has been very cost effective. I am really proud of that, since I have not wasted the taxpayers’ money. In my opinion, cost-effectiveness of research should be valued more in a small country such as Finland with very limited resources.

Due to my passion for research, I have survived without any competitive research funding. But more importantly, the current research funding system in Finland has turned out to be cat-

astrophic for the country. It is extremely centralized, especially when the main funding agencies, the Academy of Finland and the Finnish Funding Agency for Innovation (TEKES), have coordinated their funding decisions from the 1990s onward<sup>15</sup>. Furthermore, some Finnish universities allocate internal funding to research groups based on the amount of external funding they have gathered<sup>16</sup>. Such a chained centralized system has been extremely dysfunctional in a small country. It has inevitably reduced the variety of ideas funded and, therefore, the probability of research-based innovations.

### 9.3 Research collaboration

There has been a global trend toward team science (Bozeman et al. 2013; Leahey 2016). Without external funding, I have been compelled to conduct research without any research group around me. This has forced me to focus on international cooperation rather than local collaboration. The question is whether this has been good or bad.

There is abundant literature on the impact of co-authorship on the productivity of researchers and the quality of their research (see Abramo and D'Angelo 2015; Didegah and Thelwall 2013; Eisend and Schuchert-Güler 2015; Frenken et al. 2009; He et al. 2009; Lee et al. 2015). As is often the case in empirical studies, the findings are contradictory (Didegah and Thelwall 2013), but the results generally suggest that international collaboration tends to increase the number of citations the paper receives (Pečlin et al. 2012).

However, in my experience, it is the competence of collaborators and co-authors that is decisive in the case of foreign co-authorship, too. I have been very lucky in this respect. I was able to identify only He et al. (2009) that attempted to take this (or more exactly, scientists' ability) into account, but only implicitly as dummy variables. It is thus difficult to assess how significant ability is as a predictor of research productivity (number of papers published) and research quality (measured using impact factors and citations).

Even though it focused on biomedical scientists, the study by He et al. (2009) is interesting from the Finnish perspective, since it analyzes the impact of co-authorships on research output in a small country, New Zealand, which may be still more isolated than Finland (except language). The results of He et al. (2009) suggest that both international co-authorship and intra-university co-authorship are significantly related to the quality of the paper, whereas (other) domestic co-authorship is not. Furthermore, in their time-lagged analysis, they found that intra-university co-authorship predicts research productivity but not research quality, whereas international co-authorship explains research quality. These findings are largely easy to understand, since co-authorship according to my experience is usually beneficial, but international co-authoring tends to be quite time consuming. He et al. (2009) explain the insignificant effect of domestic co-authorship by their observation that most of it involved universities and governmental research centers, with universities and private companies collaborating on occasion.

The significance of international research collaboration has been well-recognized in Finland as the quality of papers with at least one author from abroad seems to be clearly higher in terms of citations than papers with authors from Finland alone (Suomen Akatemia 2016a). But the question is whether the idea of big research by large research groups favored by the Academy of Finland really supports international co-authoring in practice. I discuss this in the next section.

## 9.4 Big research vs. small research

The trend in the last 40 years, when I have been involved in scientific research at the international level, has been toward big research by large research groups. Lee et al. (2015) aptly describe this trend (p. 684):

While traditionally science is seen as an individual endeavor, increasingly scientific projects are group activities and the groups are growing larger (...) The result of these changes is that increasingly scientific work takes place in a setting that more closely resembles a small factory, rather than an individual's lab bench.

My lack of funding forced me to conduct small rather than big research. I am not confident that big research is a more effective way of promoting research productivity and quality than small research by a scientist or a small group of researchers. It seems that the question of the optimal size of a research group has not been investigated scientifically (Fang and Casedevall 2012). This size depends on the discipline and the nature of the research problem.

However, the recent paper by Verbree et al. (2015) addressed the question. Their findings were based on two surveys (conducted in 2002 and 2007) of biomedical and health research group leaders in the Netherlands. They found that group size has a significant positive relationship with the number of publications and the number citations received. On the other hand, they found a significant negative association between group size and the average number of publications per group member; i.e.; productivity; indicating decreasing marginal returns with increasing size. Finally, they did not identify any significant relationship between the group size and the number of citations per publication.

Verbree et al. (2015) suggested that a group size of 10–12 researchers per group leader is the optimum. Although they identified differences between research groups that worked in basic life science research that is predominantly laboratory-based, clinical research, and public health research, I think that research on biomedical and health research requires larger research groups than IS research, if the latter does not attempt to conduct DSR including extensive software development<sup>17</sup>.

Another question is if big research groups are more innovative than smaller ones. It seems again that there is not much empirical research on this issue (Louis et al. 2007), although there is a lot of literature on team innovativeness; e.g.; (Hülshager et al. 2009). One problem with these studies is that many of them interpret innovation to cover invention and its (organizational) implementation. Innovation in the context of research groups focuses on creative invention only.

However, a recent paper by Peltokorpi and Hasu (2014) studied innovation (measured as the number of patents) of 124 teams in a Finnish technological research organization. Since they include research assistants in their calculation of team size, I interpret them as having had a research group in mind rather than a team of co-authors. They noted that prior findings on the relationship between group size and group innovation had been inconsistent, with some studies reporting it as positive others as negative, and some as curvilinear (more exactly an inverted u-shape). Peltokorpi and Hasu found team size to have a direct effect on team innovation, as well as an interaction effect together with participative safety. Participative safety refers to participation in decision making; i.e.; the extent to which people are involved in a team's decision-making processes, share information, and listen to each other's ideas; and intra-group

safety; i.e., the degree to which a team's psychological atmosphere is non-threatening, characterized by mutual support and trust.

Peltokorpi and Hasu also report that they did not find support for the curvilinear relationship between team size and team innovation. It is questionable, however, if their data made it possible to test it properly, since it seems that they included quite a few observations of large teams (the range of team size was 4-31, the mean 11.3, and the standard deviation 4.4).

A major argument for the positive relationship between team size and team innovation is that a larger team can feature a multitude of skills and expertise (Peltokorpi and Hasu 2014); i.e.; job-related diversity (Hülsheger et al. 2009), cognitive diversity (Taylor and Greve 2006), or field and task variety (Lee et al. 2015). So it is not the size that matters, but the composition of the team. Peltokorpi and Hasu did not separate the two in their empirical study. Lee et al. (2015), on the other hand, found that field variety and task variety mediate the effect of team size on the novelty of papers such that after their inclusion, team size becomes insignificant.

To summarize the discussion thus far, it seems that there is not compelling empirical evidence that big research groups are more effective as research units than individual and small research groups. In any case, they are not a panacea when attempting to foster research, and still less a universal solution that is equally effective in all disciplines. Hence, the persistence of the trend towards larger research groups is paradoxical in the context of research, since one can expect that the way scientific research is organized is evidence based.

Referring to discussion on research collaboration in the previous section, one question is whether big research groups attract more international research cooperation than smaller groups, ultimately leading to joint research projects and collaborative authorships. I do not have any hard evidence on this, but I have the impression that actual research cooperation is based more on personal relationships than institutional contacts, especially in case of leading researchers.

Assuming that research collaboration, especially at the international level, is beneficial, I wonder whether there is a tradeoff between its different forms—local, domestic, international—so that the emphasis of big research groups reinforces local intra-group co-authorship at the expense of international co-authorship. I am not aware of any empirical research on whether there is such a tradeoff, but knowing my limited attention span, I am afraid that there might be.

Furthermore, the Academy of Finland (2016a) found that the quality of papers with authors from multiple Finnish organizations; e.g.; universities; was clearly higher than that of papers with authors from a single Finnish organization, even though it was clearly lower than the quality of internationally co-authored papers. The quality difference between the two forms of domestic authoring was clear especially in ICT (information and communication technology and electronics) and other disciplines of engineering<sup>18</sup>. The report does not discuss this finding, but it suggests to me that in applied disciplines, where innovativeness is essential, research cooperation, whether international or domestic, may be more beneficial than local cooperation. This may be because inter-organizational cooperation is associated with higher diversity of perspectives, which stimulate innovations, whereas local cooperation may imply intellectual parochialism. This renders questionable the idea that Finland can compete internationally in research by creating big research units with a critical mass (Suomen Akatemia 2016a), since it means the concentration of resources within each discipline in one or two universities. It again decreases the variety of ideas fostered and, therefore, the probability of research-based innovations.



## 10 Epilogue

The Information Technology revolution, which began in the 1950s and still continues, is likely one of the great transformations in human progression that will be recognized by future historians after centuries. It is much more uncertain whether disciplines of computing—Computer Science, Software Engineering, and Information Systems—will then be remembered.

Keeping that in mind, Sections 3-7 described my small contributions to the international, scholarly market of ideas in Information Systems (Lyytinen and King 2004). By ‘scholarly,’ I mean that my papers are targeted to the IS research community rather than directly to practitioners. By ‘international,’ I emphasize that after my licentiate thesis, I have attempted to communicate with the international IS research community rather than specifically with the Finnish one. It is almost embarrassing to estimate the influence of my contributions. It is a pleasure to discover, however, that in terms of citations, some of my papers (Igbaria and Iivari 1995; Igbaria et al. 1995; Iivari 1996; Iivari 2005; Iivari 2007) are at the level of the top 5% published in IS in their respective years, but none of them is at the absolute top (the top 1% or 0.1%)<sup>19</sup>.

Section 9 described changes in the institutional conditions of my research and my research strategy in response to them. I would characterize it as a poor man’s not-so-poor research strategy. I was forced to be smart.

Those changes had repercussions on my local conditions at the University of Oulu after my return in 1996. As for research, all focus was on competitive research funding. Nobody in managerial positions at the level of the department, faculty, and/or the whole university cared about research outputs, such as publications before 2007, when the University of Oulu conducted its first Research Assessment Exercise. It was hard to believe that it was a research university where research comes first.

As a consequence, I felt quite marginalized at the University of Oulu. Luckily, the INFWEST/INFORTE programs provided me an opportunity to escape to its workshops and seminars arranged in odd places around Finland. They also provided me a forum to maintain international research contacts and, above all, the additional salary from heading the programs increased my pension, so that I could afford to retire at the age of 64. Therefore I am always very grateful to Lic. Econ. Juha Knuuttila, whose role in organizing funding for the INFWEST/INFORTE program was so decisive<sup>20</sup>.

But it is much more significant that the changes described in Section 9 seem catastrophic for Finland, which has faced deteriorating quality of scientific research when compared with many other countries (Suomen Akatemia 2016a). The same report proposes more profiling of universities; i.e.; concentration of research by increasing the division of labor among universities and closing disciplines in those universities that do not have enough resources (mainly full-time professors). There may still be space for such profiling, but generally, the remedy sounds like prescribing more medicine that has not worked or has made the situation worse. The Finnish research bureaucrats, uncritically admiring big research (‘ökytutkimus’ in Finnish), do not understand its downsides, such as reduced variety of ideas born, fostered, and funded, of extreme centralization of research funding and resources. That is a real risk in a small country, especially in disciplines that should stimulate innovation. A small country can never compete with bigger ones by imitating them, but by being smarter.

The review team requested me to summarize lessons learned based on my experience. Being a father of a professor in Information Systems, I have learned to be very cautious in giving advice to younger scholars. During my research career, I have acted in special circumstances in my own way. The circumstances have changed, and I cannot advise anybody to follow my example, except by his or her own will and at his or her own risk. To be realistic, I am not sure if a critical mind like me has any chance of being promoted to a full professor in Finland and have a respectable academic career. And if he or she manages to do it, I am afraid he or she must be prepared to work in an unsupportive organization at constant risk of being laid off.

I have five minor lessons for younger scholars. First, have research sensors or radars on wherever you go in order to identify possible research topics. I have never had shortage of topics but, in hindsight, it may be that after all, I have been too slow to skip to new ones. Second, keep on writing. At least in my case, simply writing my thoughts down has helped me improve them considerably. Third, do not to put all your eggs in one basket, but be active in a number of research themes. It has also helped me avoid getting bored with some topics and, likely, has cross-fertilized my research. Fourth, engage in research cooperation, preferably internationally, with as good partners as possible. I have enormously benefitted from it, and co-authoring is normally much more enjoyable than writing alone. And finally, be prepared to face disappointments. I have understood that much better-known professors than me have had their papers rejected, or have faced enormous difficulties to have them published.

Despite all the institutional changes, the most concrete change during my academic career has taken place in scientific communication due to advances in Information Technology and Information Systems. We did not have the WWW, GoogleScholar, and bibliographic databases in the early 1980s, where one can now easily search for the literature and get access to electronic versions of most publications of interest. As an emeritus professor, I like this opportunity to do some research in private at home, without the need to visit the department and the university library, and being afraid of seeing my former colleagues astonished or scornful, gaping at me and thinking, “doesn't he understand to stop?”

No, I have not understood to stop—not completely. I have continued some research activity, still on a smaller scale than earlier, as a kind of hobby and a form of exercising my brain. Technology has made it fairly easy for me, since I have almost all the material I need at my fingertips. The light-blue arrow in Figure 1 describes how my research has moved to meta-research, and has become more detached from real research. I like the arrow. It shows upwards. It is a positive sign at my age.

## Notes

1. In the good old days, one could study each discipline at these three levels at Finnish universities. *Approbatur* was a sort of basic level, *cum laude* a deeper level, and *laudatur* an advanced level. One could select a discipline that one had studied at the *laudatur*-level as one's major in M.Sc.
2. For example, Professor Pentti Kerola organized in the mid 1980s a nationwide doctoral program in Information Systems that was very instrumental in the internationalization of

Finnish IS research. Pentti was its first leader, followed by Professor Pertti Järvinen and Professor Kalle Lyytinen before me. The program continued about 10 years.

3. I use the term “systems development methods” when I have both software development/engineering methods and IS development methods in mind.
4. One can imagine that there has been a dialectic tension between heaviness of the above conceptualizations and the lightness of process models. Generally speaking, each of the above conceptualization tended to grow quite complex, leading to fairly heavy methods. So, the function/process-orientation that dominated especially in the 1970s was followed by the idea of prototyping in the late 1970s, the data-orientation that dominated in the 1980s was followed by the spiral model, and the object-orientation that dominated in the 1990’s was followed by the ideas of agility.
5. When I specifically refer to a scientific discipline, I use capital letters (Economics vs. economics, Information Systems vs. information systems, Software Engineering vs. software engineering).
6. I do not call them “iivariants,” since I was not their original inventor.
7. Simon (1969) had used the term “science of design” but, to the best of my knowledge, not “design science.”
8. I am still amazed that when working on my dissertation I was not able to identify any publications on “philosophy of engineering” or “philosophy of medicine”. It is likely that I did not browse all the thousands of paper cards at the main library of the University of Oulu, which recorded books located in different library units of the university.
9. From the Finnish perspective it may be interesting to note that Kasanen et al. (1991) and Kasanen et al. (1993) used the same term (“konstruktivinen tutkimus” in Finnish), likely borrowing it indirectly from me (or from my unpublished working papers) through Järvinen (1988). One must note, however, that they analyzed “constructive research” in much more detail than I do in my paper. This indicates that a single, innocent idea like “constructive research” may trigger something remarkable that may be difficult to trace for outsiders.
10. Following Markus and Robey (1983), I made a distinction between IS development success and the success of IS implementation, emphasizing their dilemmatic relationship. The former describes the desirability of the consequences of the whole ISD process, while the latter describes the success of obtaining the designed information system implemented; i.e.; institutionalized; (Iivari 1985).
11. Later, Venkatesh and Davis (2000), when applying TAM to the case of information systems proper, extended it into TAM2, which includes output quality.
12. I include among professors associate professors, full professors, and distinguished professors, and exclude assistant professors.
13. This NPM ideology has been introduced to Finnish universities by several reforms: performance-based funding reform in 1994, the annual working hours reform in 1998, the salary system reform in 2006, and the university reform in 2010.
14. Note that the large dataset in Li and Agha (2015) easily makes the coefficients statistically significant. Their use of one standard deviation to illustrate the effects also exaggerates them. One standard deviation of the best applications in their data covers something like 40 % of the funded applications.

15. While the Academy of Finland funds public scientific research, whether basic or applied, TEKES focuses on applied and R&D types of research, which may be proprietary.
16. If these doubly funded research groups manage to publish on good forums, they are funded for a third time based on those publications.
17. I do not have personal experience of such projects, but I think that Kalle Lyytinen had about 10 researchers (most of them doctoral students) at the peak of the Metaphor project that developed the MetaEdit CASE tool.
18. The report does not distinguish solo-authored papers and co-authored papers in the case of papers from a single research organization, so that one could estimate whether the difference was due to co-authorship or a multitude of organizations. Therefore I am cautious with my conclusions.
19. I followed the rationale of Iivari (2015b), when using Thompson Reuters Web of Science to count citations. Since the Web of Science does not identify Iivari (2005) and Iivari (2007) when using basic search. I used 'cited reference search' to find their citations, taking into account frequent misspellings of my name. For example, one third of citations to Iivari (2005) are to Livari.
20. Sadly, Juha Knuuttila passed away in September 2016, due to a brain stroke.

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