HOW TO FRAME YOUR CONTRIBUTION TO KNOWLEDGE? A GUIDE FOR JUNIOR RESEARCHERS IN INFORMATION SYSTEMS

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Abstract

Master students and junior researchers within the Information Systems (IS) field often struggle to frame their knowledge contributions when reporting their research. In this paper, we investigated this research question: what are the forms of knowledge contributions that can be targeted in information systems research? We analysed a selection of NOKOBIT papers that were written by junior researchers, with their previous masters or PhD supervisors as co-authors. As expected, we found that all papers followed the same script of identifying a research gap and trying to fill it. While the practical contributions often can be acknowledged, the theoretical contribution can be more difficult to identify. The contribution of this paper is twofold. First, we suggest a taxonomy of various forms of knowledge contributions in Information Systems research. Second, we present contribution-focused guidelines for junior researchers. This study should be of interest to master students and junior researchers, as well as to their supervisors.

Key words: Information systems research, contributions, knowledge, junior researchers, master thesis, PhD projects

1. Introduction

If you want to start a physical business, like a café, there are three success components: location, location, location. Similarly, it has been suggested that there are three success components for research: *contribution, contribution, contribution* (Te'eni et al. 2015). Moreover, the key criterion for assessing research, whether an academic paper, a thesis or a project report, is to what extent it is considered a contribution to *knowledge*. Ideally, this should include both a contribution to the current theoretical understanding of the studied phenomena, and to practice in the field. The weighting of these forms of contributions will naturally depend on the focus and nature of the research conducted, and recent arguments have been made for how 'theory light' papers should also be considered valid contributions (Avison and Malaurent 2014). Yet, reviewers of academic papers and examiners of postgraduate theses will normally have "contribution to theory" as one of their key checkpoints.

Unless you are doing deductive, theory-testing research, reporting your possible theoretical contribution may not be straightforward as this may include several different forms: models, frameworks, concepts, propositions, and more. While bringing out the practical insights and lessons learned from an empirical study (if conducted well) may not be that hard, presenting the theoretical implications of this can often be more challenging, and especially so for junior researchers. As it will be documented in this paper, we found surprisingly little explicit advice on this in typical foundational readings for master and PhD students. This paper aims to offer some aid in this process, by presenting a taxonomy of different forms of knowledge contributions in information systems (IS) research and discussing the prerequisites for arriving at these. We put the main emphasis on qualitative research, as

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the nature of inductive theory building is considered less 'transparent' than for deductive, quantitative research.

In addition to serving as a guide for junior researchers, we argue that this can also be useful for the supervisors of master and PhD students. From our own experience as such, we are often faced with giving advice to students on how to frame their contribution, which may require some serious headwork. This inspired us to investigate: *what are the forms of knowledge contributions that can be targeted in information systems research?*

The paper is structured as follows. The next section discusses the very concept of theory, and the different facets this includes. Section 3 gives a brief summary from our search for existing guidelines on framing knowledge contributions. Section 4 presents our suggested guidelines for framing a knowledge contribution, and the related typology. Section 5 illustrates the typology based on analysing the knowledge contributions from a set of exemplar master and PhD theses, presented as NOKOBIT papers. The final section presents conclusions and implications from our work.

2. Resources on theory and theorizing in IS research

As a basis for our further analysis and taxonomy, this section briefly reviews some relevant resources on the nature of theory, and the process of building theory in IS research. Our point of departure for the related literature search was the ongoing debate in the European Journal of Information Systems (EJIS), in addition to the seminal paper by Gregor (2006).

2.1 Defining theory

The very concept of 'theory' lacks a common, agreed upon definition (Lee 2014; Mueller and Urbach 2013). From our experience, graduate students also frequently confuse 'theory' with 'extant research'. Thus, in their literature reviews and discussion of empirical findings they would use the term 'theory' to refer to earlier published research literature, even if this literature does not apply or contribute to any specific theory.

When faced with this misconception, we as instructors then need to point out that a theory has some important qualifications, that distinguishes this from 'any research result'. A generic definition of theory that can be used for qualifying this is the following: *"A theory is a set of interrelated constructs (concepts), definitions, and propositions that presents a systematic view of phenomena by specifying relations and variables, with the purpose of explaining and predicting the phenomena"* (Kerlinger 1973, cited in Elliot and Avison 2005, p. 189). However, it could be argued that this definition only covers some forms of theory.

In her seminal article on the nature of theory in IS, Gregor (2006) intends to clarify *"what constitutes theory in IS and what form contributions to knowledge can take."* She presents a taxonomy of IS theories distinguishing five interrelated types of theory (adapted from page 620):

> Type 1 – *Theory for Analysing*: Says what is. The theory provides description.
> Type 2 – *Theory for Explaining*: Says what is, how, why, when, and where. The theory provides explanations.
> Type 3 – *Theory for Predicting*: Says what is and what will be. The theory provides predictions and has testable propositions.
> Type 4 – *Theory for Explaining and Predicting*: Says what is, how, why, when, where, and what will be. The theory provides predictions and has both testable propositions and causal explanations.
> Type 5 – *Theory for Design and Action*: Says how to do something. The theory gives explicit prescriptions for constructing an artefact.

Gregor explains that all five types are interrelated and that all five types are of equal importance and value.

While established as a foundational resource in our discipline (approaching 2000 Google Scholar citations), there is no universal agreement on Gregor's taxonomy. For example, Weber (2012) only

regards Gregor's Type 4 - *Theory for explaining and predicting* - to be consistent with his definition of a theory. He rather sees Gregor's Type 1 *Theory for analysing* as typologies, and Type 5 *Theory for design and action* as models. While most would probably agree with Type 2, 3 and 4 as representing different types of theories, Type 1 and 5 may be more contested. Though some would refer to Type 1 theory as descriptive, Gregor argues that *"this class of theories goes beyond basic description in analysing or summarizing salient attributes of phenomena and relationships among phenomena. The relationships specified are classificatory, compositional, or associative, not explicitly causal"* (ibid., p. 623). She lists classification schema, frameworks, and taxonomies as variants of Type 1 theory. This type of theory is considered a valid knowledge contribution when little is known about some phenomenon.

Regarding Type 5 – *Theory for Design and Action*, Gregor points to how this type of theory represents an important and partly unique form of contribution in IS research, also more frequently referred to as design science or design science research. In her review of seminal work on this type of research, Gregor lists methodologies and prescriptions for building specific applications as examples of design theory. Vaishnavi and Kuechler (2015) present a more fine-grained taxonomy of knowledge contributions from design science research, in the form of material artefacts (instantiations), abstract artefacts (constructs, models, frameworks, architectures, design principles, methods), and design theories. The latter is defined as *"a prescriptive set of statements on how to do something to achieve a certain objective"* (op.cit., p. 20). Further, a design theory is stated to usually include other abstract artefacts as listed above.

Gregor (2006) argues that her taxonomy of theories can support junior researchers in the following way: *"Novice researchers should benefit from the depiction of the basic components of theory, helping with their question of "What is theory?" The approach recommended for theory development is to begin with the research problem and research questions and then determine which type of theory is appropriate for the problem, given the current state of knowledge in the area and using the classes depicted here as a guide"* (Gregor 2006, p. 634).

In the process of identifying relevant theories to build on, the Association for Information Systems (AIS) resource 'Theory of IS Wiki' (Larsen et al. 2015) may be useful as a systematic overview of the large number of theories being applied in IS research, listing main independent and dependent constructs, seminal articles, and more.

In this paper, we adopt the broad perspective on knowledge contributions as presented in Gregor's article (2006). For example, she writes that through knowledge we can either build, or falsify a theory. Thus, we use the term knowledge as an umbrella term that covers several types of contributions, and in Section 4, we summarize the different forms of practical and theoretical contributions in the form of a taxonomy.

2.2 Building theory from qualitative research

In this section, we focus briefly on the process of developing theoretical contributions from qualitative research. The nature of these contributions will vary depending on the ontological and epistemological perspective of the research. However, as it will be discussed, some basic principles can be seen to apply for inductive theory development in qualitative research in general. For a more in-depth overview of epistemologies and methods in qualitative research, we refer to Myers' online resource (Myers 1997).

In her classic paper on building theories from case study research, Eisenhardt (1989) outlines a process for inductive theory-building from case studies. This process starts by defining a research question, via selecting cases, to collecting and analysing data. From this, hypotheses may be shaped, and compared against both conflicting and similar literature. Only then can theory be built, and it may further be presented in the form of for example: process models, strategy-making themes, conceptual framework, propositions, or mid-range theory. Based on a positivist stance, Eisenhardt regards theory development from case studies as complementary to traditional normal science research, and as particularly suited for generating novel theory in under-explored research areas.

Walsham's (1995) article on interpretive case studies also discusses how theory can be the end-product of interpretive research. He outlines four types of knowledge contributions (or generalizations) from interpretive research: development of concepts, generation of theory, drawing of specific implications, and contribution of rich insight. The first two of these are connected as a concept that can *"be part of a broader network or an integrated clustering of concepts, propositions and world-views which form theories in social science"* (Walsham 1995, p. 79, citing Layder, 1993). The drawing of specific implications relates to particular domains of action and is paralleled with the notion of 'generative mechanisms' from critical realism (Bhaskar 1979), viewed as 'tendencies', which are valuable in explanations of past data but are not wholly predictive of future situations (Walsham 1995, p. 79). Finally, the form of generalization termed rich insight is intended to capture additional insights beyond the three former categories that the reader can gain from reports and results of interpretive studies.

In discussing generation of theory, Walsham actually draws upon Eisenhardt, despite their differing epistemological positions: "*With respect to theory as a final product of the research, Eisenhardt notes that the output from case study research may be concepts, a conceptual framework, propositions or mid-range theory. There is some irony in quoting Eisenhardt in the current paper, since she explicitly states her epistemological position as positivism, and mid-range theory is something which should, according to her views, then be tested formally using positivist approaches. This position on the role of theory would not be acceptable to many interpretive researchers, although the view of theory as a desirable final product of case study research would be generally shared."* (Walsham 1995, p. 79).

A particular research approach for inductive theory building is so-called Grounded Theory. This concept was coined by Glaser and Strauss in 1967, and is defined as: *"…a qualitative research method that seeks to develop theory that is grounded in data systematically gathered and analysed"* (Urquhart et al. 2010, p. 357). This means that theory has to emerge from the data, and not from previous experience or pre-formulated hypotheses. According to Urquhart et al. (2010), grounded theory may result in all of Gregor's five theory types.

Jones and Alony (2011) claim that grounded theory is a detailed, rigorous, and systematic method, but that it also gives the junior researcher flexibility and freedom. Grounded theory can facilitate theory building within IS, but Jones and Alony fear that the junior researcher may not actually uncover any substantial or significant theory based on the collected and analysed data. This is related to a broader discussion on what form of theories we should target to develop, which will be briefly addressed in the next section.

2.3 Current debates on theorizing in IS research

Related to the current debate on the need for more native information systems theories, Alvesson and Sandberg (2011) use the term *gap-spotting* to indicate that researchers tend to focus too much on identifying a gap in existing theories instead of creating new theories. They argue that gap-spotting reinforces the assumptions of underlying, established theories, and that it can prevent producing new and interesting theories. Grover and Lyytinen (2015) take the debate further and illustrate how gap-spotting leads to what they call a *vicious cycle of mid-range theory*. They define mid-range theory as a research model where a theory borrows from other disciplines and transfers or specialises it to an information systems context. Further, they describe how supervisors teach this *"game of conformity"* to PhD students, usually with some following success in the sense of convincing the reviewers for publication. The unfortunate consequences are narrow thinking and unoriginal results. They propose some actions that the researcher may take to break out of the cycle, however: *"Of course, such unconventional behaviors would not be rational, because they would often lead to nonproductive outcomes and career cul-de-sacs under our current regime. Therefore, it is important that such behaviors are fostered and valued by our institutional collective - our representations of editors, conferences, organizers, and research programs"* (Grover and Lyytinen 2015, p. 289). We do not believe that breaking out of the cycle is recommended for novice researchers and we will return to this in Section 6.

Finally, we point to the ongoing debate on how much weight should be given to theoretical versus empirical knowledge contributions, with Avison and Malaurent (2014) arguing that qualitative research providing *"for example, new arguments, facts, patterns or relationships"* could be considered sufficient contributions without theory building beyond this. The need for being precise about the nature of your research contribution is also emphasized in an EJIS editorial by Ågerfalk: *"Authors: If your paper is making a truly significant empirical contribution, emphasize that contribution rather than bolstering and over selling a possibly contested theoretical contribution. Be careful not to confuse empirical contribution with implications for practice. However, make sure that you explore the theoretical implications of your findings. In doing so, refrain from drawing far-reaching (and far-fetched) conclusions, as it is likely they will only suggest a limited applicability for future theory development based on your work"* (Ågerfalk 2014, p. 596).

3. Resources on framing knowledge contributions

As part of our literature review, we also looked for literature intended to guide junior researchers on how to frame and present their knowledge contribution. We searched on Google Scholar for various combinations of "information systems", "research" and "novice" or "junior". While we identified several resources addressing related aspects, few of these provided explicit guidelines on this.

As one of the few textbooks on research methods that are specifically targeting IS research, Oates (2006) is applied in method courses in several master programmes. The book is structured according to a framework denoted as *The 6Ps of Research*: purpose, products, process, participants, paradigm and presentation. Regarding *products* of research, Oates defines this as: *"…the outcomes of research, especially your contribution to knowledge about your subjects. Your contribution can be an answer to your original research question(s) but can also include unexpected findings"* (Oates 2006, p. 11). Moreover, she suggests various types of different *knowledge outcomes*: a new or improved product, a new theory, a re-interpretation of an existing theory, a new or improved research tool or technique, a new or improved model or perspective, an in-depth study of a particular situation, an exploration of a topic, area or field, or a critical analysis.

While we also identified some articles more or less explicitly targeting junior researchers, these do not provide explicit advice on how to make significant contributions. For example, Krasnova et al. (2012) give advice on number of co-authors and level of journals and conferences, and the authors clearly state that *"Significant theoretical and practical contributions are not just desirable but are a must"* (p. 3), but without providing aid on *how* to make significant contributions. We did find one article by Eisenhardt and Graebner (2007) that offers a somewhat tangible step-wise process: *"First, sketch the emergent theory in the introduction. Then, in the body of paper, write each proposition (implicitly or explicitly stated), and link it to the supporting empirical evidence for each construct and for the proposed relationship between the constructs"* (p. 29). Finally, they advise to *"…provide a visual theory summary such as a 'boxes and arrows' diagram or summary table"* (p. 30). A limitation of this article may be that it addresses mainly theory building from positivist case studies, however we believe the process can be applied to qualitative studies based on other epistemologies as well.

Somewhat addressing the junior researcher, Sørensen (2002) explored the following research question: *"What are the important aspects to consider when documenting Information Systems research results in scientific articles?"*, thus using the term results instead of contributions. However, his main aim is to help the writer "sell" the results, rather than to produce them: *"Having related your research to what others have done, you are left with a tough one — to state clearly what you are contributing. Here the simple case of relating to existing research might become the hard case of stating the new and interesting results in your work. If you have broken new ground you might have an easier task. Well, when writing the paper at least. If you are way out in "left field," to take a term from baseball, you might get problems later when trying to get your findings published. I do not address this problem in this paper"* (Sørensen 2002, p. 7).

Another useful resource on style composition for presenting your research contributions is presented by Mathiassen et al. (2012). *Area of concern* is academia's call for research, and *Real-world problem*

is what we observe as practitioners. *Framing* can be chosen concepts from a theory, and *Method* refers to the common methods in IS, for example action research and case study. As Figure 1 below illustrates with four dotted arrows pointing back from *Contributions*, they distinguish four main different contribution styles: *experience report*, *field study*, *theoretical development*, *problem solving/research method*. An *experience report* may potentially contribute to *Area of concern* and *Real-world problem*; a *field study* primarily contributes to *Area of concern* or *Framing*. *Theoretical development* may contribute to *Framing*, and *problem solving/research method* may contribute to *Method*. Mathiassen et al. (2012) request more contributions to "Area of concern": *"Being a practically oriented field, IS researchers have hitherto focused on contributions to [framing]. This focus has led to fewer contributions to theory independent of the area of concern within our IS journals, which could inform other fields"* (p. 358).

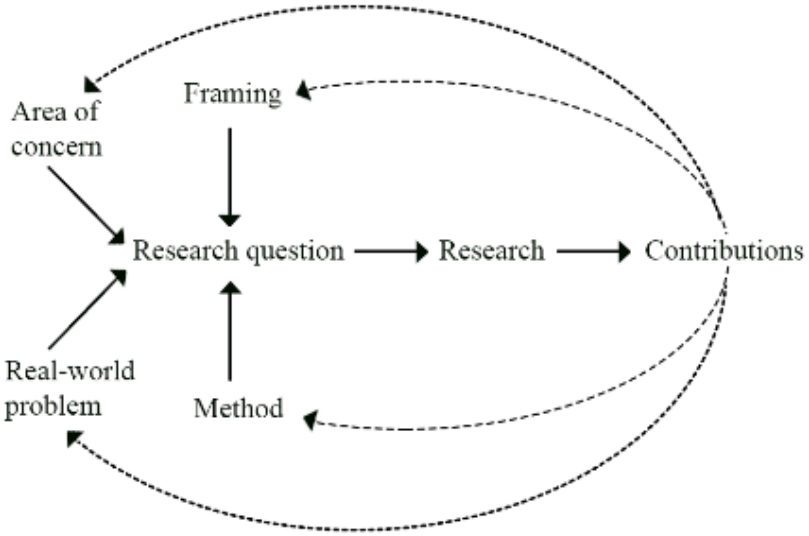


Figure 1: Style composition for a research project, adapted from Mathiassen et al. (2012)

Summing up, we did not identify any authoritative source on how to frame your knowledge contribution, and thus we argue that this remains a challenge for junior researchers. In an attempt to contribute to this, the next section proposes a taxonomy of knowledge contributions.

4. A taxonomy of knowledge contributions

Based on the review in Section 2, we present a taxonomy of knowledge contributions. We roughly classify Table 1 as contributions to practice, and Table 2 as theoretical contributions, or, *Real-world problem* and *Area of concern*, respectively, from Mathiassen et al. (2012).

#	***Type of contribution***	***Description***	***Examples***
1	Lessons learned	Describes insights	Study of IBM on electronic meetings (Grohowski et al. 1990)
2	Experience report (Mathiassen et al. 2012)	A descriptive, sequential report	Often, the whole paper constitutes the experience report; see for example Küng and Hagen (2007)
3	Guidelines/Roadmap	Explicit, normative advice	Urquhart et al. (2010) for grounded theory studies or Walsham (2006) on interpretive research
4	Heuristics	A "rule of thumb"	Nielsen (1994) on usability
5	Critical Success Factors (Rockart 1979)	Activities that are necessary to ensure successful performance, e.g. related to project management	"Obtain management support", "Involve the end user"
6	Patterns (Alexander, 1977)	A re-usable solution to a problem	Larman's (2005) Software Engineering Patterns

Table 1: Forms of practical contributions in the Information Systems context

Table 1 follows Gregor (2006) in the sense that we regard the first two types as merely descriptive, explaining "what is". The rest of the contributions are more prescriptive or normative in nature, meaning that they have a more explicit nature of "do x in order to obtain y". #

	Type of contribution	***Description***	***Examples***
1	Concept (Eisenhardt 1998; Walsham 1995)	The conceptual vocabulary of a domain (such as ERP)	'Informate' from Zuboff (Walsham 1995)
2	Construct (Yin 2014)	An operational measure	'User Experience' can be high/low.
3	Rich insight (Walsham 1995)	Insights beyond concepts, theories or specific implications	Limits of machine intelligence; differences between plans and practical actions; need for more thoughtful machine design (Suchman 1987, in Walsham 1995)
4	Case study (Yin 2014) or action/field study (Mathiassen et al. 2012)	A rich description of a phenomenon in its natural context	How a company went bankrupt due to a failed ERP-system implementation (Sumner 2007)
5	Framework, Taxonomy	Framework: conceptual guide to serve as support, typically for analysis or discussion. Taxonomy: a classification system	Gregor's (2006) taxonomy of IS theories
6	Problem Solving, Research Method (Mathiassen et al. 2012)	A set of steps (algorithm or guidelines) used to perform a task	Levin's (1958) three steps for organisational change (Unfreeze-Change-Refreeze) (in Levasseur 2001)
7	Proposition (Yin 2014)	A purpose with criteria. An initial, 'high-level' version of a hypothesis	"There exists a set of Critical Success Factors for IS projects"
8	Generative Mechanisms (Bhaskar 1979)	Causal, self-reinforcing processes behind an output	A bank goes bankrupt (the output) due to self-fulfilling prophecy (the mechanism) (Adapted from Henfridsson and Bygstad 2013)
9	Hypothesis	An explanation for a phenomenon. Must be testable and subject to further research before it becomes theory (unless falsified)	Hypothesis: "All swans are white". Black swans were observed and falsified the hypothesis (Popper 1938)
10	Model	A set of propositions or statements expressing relationships among concepts or constructs	Often a visualisation with boxes and arrows (see our examples below in Table 3)
11	Mid-range theory (Merton 1968, in Grover and Lyytinen 2015)	Typically borrows theory from reference disciplines such as sociology	Giddens' *Structuration Theory* as (one of many) fundament(s) for *Information Infrastructure Theory* (Hanseth and Lyytinen 2010)
12	Design theory	Focus on building a technological artefact	A three-cycle view of Design Science Research (Hevner 2007)
13	Grand theory (Gregor 2006)	Generalizations that are relatively unbounded in space and time	*Systems Theory* (Ashby 1956; von Bertanlanffy 1973, in Gregor 2006)

Table 2: Forms of theoretical contributions in the Information Systems context

Table 2 has a hierarchical structure in the sense that we consider the first forms of contributions as building stones of the ones further down. We want to emphasise that we do *not* believe that some forms of contributions are more valuable, but we think that *Mid-range theory* is more ambitious and also more difficult than identifying a *Construct*. As Gregor points out, constructs and models are valuable theoretical contributions if previous research is limited. When we, for example, place *Framework* and *Taxonomy* in the same level, it does not mean that we equalise the two forms. Rather, it means that they are on the same hierarchical level.

Our taxonomy comes with three main limitations. First, the different forms of contributions are overlapping in the sense that what one researcher might label a model, another might label a framework. Second, we have an information systems context. This means that we have omitted concepts like "laws", which are typically found in computer science and physics (for example

Newton's law of gravity). Third, the epistemology, or research perspective, will influence to what extent a researcher will agree with this hierarchal structure. An interpretive researcher may regard a rich case study analysis and concept development more valuable for theory building than hypotheses. Similarly, an action researcher may regard problem solving as the main target contribution.

If previous research is substantial, there are more ways to contribute in addition to Tables 1 and 2. Building on (Eisenhardt and Graebner 2007; Yin 2014) we elaborate on four main ways to build theory: confirmation/replication; extension; contrary replication; and elimination. We illustrate the examples in Table 3 by using Lewin's classical theory of change management: unfreeze-change-refreeze (Lewin 1958, in Levasseur 2001). It can be presented in a model with three boxes that are concepts or constructs, and the arrows represent the relationship between them.

Original theory: 

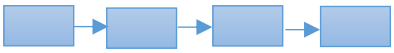
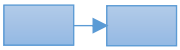
Type	***Description***	***Example***
Confirmation, Replication	indicates that a chosen theory is still valid, or that it will work in different setting	
Extension	adds to an existing theory, for example with an extra construct	
Contradiction	contradicts the whole, or parts of the theory, such as providing evidence of more interplay between the constructs	
Elimination	indicates that parts of the theory are obsolete in the chosen setting	

Table 3: Types of contributions to an existing theory

In a simple way, the types in Table 3 concur with the contributions to *Framing* in Figure 1. We found that many of the analysed NOKOBIT papers made such knowledge contributions, as the next section will show.

5. Findings and analysis of the conference papers

Since its inception in 1993, the NOKOBIT conference has proven a receptive and constructive forum for junior researchers to present and discuss their research. As the basis for further illustration and discussion of the taxonomy of contributions presented in the former section, we analysed a sample of NOKOBIT papers based on master and PhD theses from the authors' institutions. In the period from 2008 to 2015, we identified nine such papers (listed in Table 4). We noted that all of these were co-authored with the thesis supervisor(s), but with the students as first authors (except for paper 6).

#	***Year***	***Title***	***Authors***
1	2014	Frivillige organisasjoners bruk av sosiale medier i krisehåndtering	Flaten, Pettersen Nguyen, Munkvold
2	2011	ERP-implementering i en kunnskapsintensiv bedrift: en casestudie fra et forlag	Hoff, Hustad, Olsen
3	2010	Enterprise Content Management in Practice - One Size Does Not Fit All	Korsvik, Munkvold
4	2010	Anskaffelsesprosessen i et ERP-prosjekt: en casestudie av en SMB	Hartvedt, Hustad, Olsen
5	2010	Kunnskapsdeling gjennom historiefortelling i en smidig prosjektorganisasjon	Hægeland, Hustad, Munkvold
6	2015	Business Intelligence to the People. A Case Study Of Dashboard Adoption in the Health Care Sector	Presthus, Bergum
7	2014	Perception of SaaS Adoption in Norwegian Enterprises: Focus on ERP	Mæland, Haddara, Fagerstrom
8	2008	A Post-Implementation View on the Perceived Effect of ERP Systems on Organizational Responsiveness	Gonzales, Bygstad
9	2008	Integrating User Context into PIM Applications	Grønli, Ghinea

Table 4. Overview of NOKOBIT articles based on master theses and PhD publications from the University of Agder and the University College Westerdals – Oslo School of Arts, Communication and Technology (former NITH – Norwegian School of IT), 2008-2015

We were able to retrace publications back to 2008, either from the printed proceedings, or from the online publications. By contacting the authors with printed publications (#8 and #9 in Table 4), we were able to obtain digital versions of all nine publications, which allowed us to conduct the same type of analysis, such as using the advanced search function in Adobe Acrobat Reader or Microsoft Word.

First, we read each paper, while identifying the research question(s), research approach, theoretical foundation and contribution. Second, we created a table with the main research approach and the forms of knowledge contributions identified in the papers, and extracts of quotes (see the appendix).

Findings

Having analysed all nine papers, we made the following findings:
i) The papers built on existing research, in the sense that they did not use a grounded theory approach.
ii) All papers reviewed related literature before identifying a research gap, and then…
iii) …claimed that new knowledge was produced, and listed the beneficent (typically practitioners in organisations) of the new insights.

Regarding our finding about the research gap, we note that, without exception, all nine papers explicitly point to that *little*, or *limited research* has been conducted in the given field. We return to this issue of *gap-spotting* in the discussion.

Table 5 provides an overview of the contributions that we could identify. More details are found in the appendix.

Contributions of the papers	**Paper #**
Mainly insights to practice	1, 6
Confirm research without specifying a particular theory or study	2
Add to specified research	3, 7, 9
Confirm specified model	4
Extend specified model	4
Provide a hypothesis	5
Contradict specified research	7, 8

Table 5. Overview of the type of contributions in the nine NOKOBIT articles – our analysis

On the positive side, none of the publications promises more than they actually deliver when it comes to contributions. For example, paper #6 provides insights to practice and leaves it at that, which is congruent with Ågerfalk, who argued that it is sometimes wiser to make a sound contribution to practice instead of some vague theoretical contribution (Ågerfalk 2014). In addition, we think that the papers demonstrate a consistency of the chosen research approach and the contributions. For example, if a researcher aims to contribute with rich and interpretive insights from a case study, it is important to build on an interpretive resource, such as Walsham. Similarly, if the aim is to contribute with propositions, a positivistic source such as Yin would be the appropriate choice.

Based on our analysis, we note that the majority of the papers claim to make some sort of theoretical contributions, but that some papers are inaccurate when it comes to whether they contribute to a given theory, or extant research. For example, paper #7 states: *"…the findings are relatively congruent with existing literature"* and we assume that it reflects back on the literature presented in the paper. Later, the authors are a little more specific: *"…unexpected outcome was in regards to data security. […] This is quite contradictive to the […] Cloud/SaaS literature."*

Paper #2 simply states: *"This is an exploratory study, and it will serve as foundation for further qualitative studies regarding implementation of business systems in SMBs"* (p. 207). Further, they claim: *"the study has a certain replication value because the findings presented as in-depth descriptions provide a rich insight of ERP implementation in a knowledge-based SMB context. Experiences from this study can therefore serve as a useful example for similar businesses regarding possible pitfalls"* (p. 207) (our translation from Norwegian).

Seven of the nine papers either add, confirm, or extend either a model or (more or less) specified research, as we illustrate in Table 3. Except for paper #5 that presents one novel hypothesis, and paper

#9 with novel insights from an experiment, the papers do not venture on creating any new constructs, models, frameworks, or the like. We continue the discussion of the issue of lack of novelty, and to what extent a junior researcher should deal with this, in the next section.

6. Discussion and implications

We begin our discussion based on two main themes from Section 2: *gap-spotting* and *the vicious cycle of mid-range theory*. Based on our analysis, we conclude that all of the papers "are guilty" of gap-spotting, and that seven papers are examples of producing mid-range theory (refer Table 2). We discuss the implications and present guidelines for junior researchers.

Even if a gap is spotted, the researcher should be critical before rushing to fill it (Alveson and Sandberg name this strategy *gap-filling*). Perhaps the gap remains for a good reason, and there is no need to fill it? According to Alveson and Sandberg (2011), the main problem with gap-spotting is that we miss out new and interesting research contributions. However, these issues do not mean that gap-spotting should be avoided. We concur with Alveson and Sandberg, who stress that the researcher must evaluate the underlying assumptions behind the existing theory and the appurtenant gap. For example, Simon (1977) challenged the assumption that people make rational decisions, which resulted in his theory of *Bounded Rationality*. Even if a paper concludes with "we need more research on this phenomenon", the researcher should be critical and ask herself if the conclusion is still valid by checking the date of the publication. Then she can for example search in Google Scholar or another research database (like Business Source Premier and IEEE), and investigate the publications that have cited the paper in question.

According to Alveson and Sandberg, *gap-spotting* occurs when research is substantial on the topic. An example of a topic of massive research is Critical Success Factors (CSF) for implementation of information systems in various forms. The discipline of information systems has now conducted a large amount of studies on CSFs, and usually concludes with a list including variations of "obtain management support" and "involve the end user", regardless of the unit of analysis being a CRM/ERP/BI-implementation, or IT project management in general. Four of the NOKOBIT papers focused on ERP in different contexts (such as the implementation phase, and perceived effects after implementation) which it is safe to categorise as well-researched areas. Do Alveson and Sandberg imply that gap-spotting is less "problematic" if there is less research to be found on the subject? Alveson and Sandberg do not elaborate on this issue. Gregor (2006) claims that contributions of Type 1 (classifications, taxonomies) are satisficing if the topic is new. Paper 6 was about a recent phenomenon (adoption of dashboards in health care) and offered a classification of public dashboards, but the authors did not communicate this as a theoretical contribution. Instead, they presented their contributions to practitioners in the form of guidelines, and left it with that.

The alternatives to producing mid-range theory is a *push to the edges* (Grover and Lyytinen 2015). As we see from Table 4, one paper (#5) provided a hypothesis and one paper (#9) built a prototype, (but we doubt that Grover and Lyytinen will accept this as any attempt to a *push the edges*. Nor do we join this discussion in this paper). The majority of the papers typically added or confirmed existing theories, models, or frameworks. This type of knowledge will normally classify as contribution to *Framing*, as Mathiassen et al illustrate in Figure 1. Nonetheless, we argue that this is to be expected, and it should be sufficient, from junior researchers.

Guidelines for junior researchers

We believe that contributing to mid-range theory is useful for junior researchers, but that they should have an understanding of what mid-range theory is, and be aware of other possibilities. Pertaining to this, we present the following practical guidelines for junior researchers, both based on the resources reviewed in this paper and our own experience as supervisors:

(i) Balance your own ideas with extant research. Your idea may be good, but are you reinventing the wheel? How much do we already know about the topic? Reading relevant literature is crucial for answering this question. Rather than presenting the literature by author, we suggest clustering it according to themes or concepts. See for example Webster and Watson (2002) for more advice on

reviewing extant literature. Webster and Watson suggest a matrix, but you can also create a mind map or visualise your findings with colours or drawings.

(ii) Be critical to what you read. Again, ask yourself whether the information systems discipline really need one more paper on the chosen topic. How do you think your contribution will add value to the existing knowledge?

(iii) Having your supervisor as co-author can both *enable and constrain* you. A senior co-author will bring valuable experience, but can also overshadow a new idea. If you are a PhD student, it could be fruitful to involve your supervisor on the first one or two papers. After a few publications, consider starting your study by your own and see how far you get before involving a supervisor or a senior researcher.

(iv) Although the paper will somewhat evolve as you write, collect and analyse data, you should have an idea of your intended knowledge contribution before you start the study. We hope that our presented taxonomies can help. We want to emphasise that we do not think that the taxonomy is exhaustive, which means that there may exist others forms of knowledge contributions. All types of research are valuable; the question is more about how it contributes beyond what we know already, which leads us back to the first guideline.

What can senior researchers, reviewers and supervisors do?
While breaking out of the vicious cycle of contributing to mid-range theory is difficult for junior researchers, Grover and Lyytinen (2015) suggest mixing senior and juniors on review teams. However, we believe that in order for this strategy to be successful, senior researchers and reviewers must start to accept manuscripts without the script of mid-range theory. We hope that journals, like the European Journal of Information Systems (EJIS), will provide a more outspoken classification – and acknowledgement – of rich descriptions such as case studies (Walsham 1995), and sound contributions to practice only (Ågerfalk 2014).

Finally, we would like to see some literature reviews that state what we do *not need* to research further. An example is found in the Proceedings from NOKOBIT 2015, where Iden, Farbu and Serigstad studied various Critical Success Factors (CSF) for lean implementation, and concluded that we do not need more studies on CSF in Information Systems (Iden et al. 2015).

Limitations and suggested further research
Our analysis is limited to nine papers, all submitted to NOKOBIT, and this small and selective sample of course has affected the analysis. Further, only one of the papers was based on a PhD project, and in our analysis, we have disregarded the differentiation between master and PhD students. For a further analysis of contributions from PhD research papers, we would suggest using a sample of papers from the annual IRIS (Information Systems Research in Scandinavia) workshop where most of the participants are PhD students. The epistemology of a research project will also usually influence the contributions, and we have paid little attention to this. We hope that other researchers will evaluate our proposed taxonomy as we acknowledge the blurred boundaries and the questionable hierarchy. In addition, there may be more forms of knowledge contributions that can be added to our taxonomy.

7. Conclusion
In this paper, we have investigated the following research question: *what are the forms of knowledge contributions that can be targeted in information systems research?* Building on extant research, we present a taxonomy of various forms of knowledge contributions (presented in Tables 1, 2, and 3), and in our discussion of these we also emphasise the time aspects and how much we already know about a phenomenon. Thus, we extend the studies by Mathiassen et al. (2012) and Gregor (2006) with more details and examples. Based on an analysis of nine previous NOKOBIT papers written by master and PhD students with their supervisors, we also present a list of practical guidelines for junior researchers and their supervisors. The underlying focus of these guidelines is to evaluate the existing knowledge of the topic and target the type of contributions accordingly.

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APPENDIX

Analysis of the main research approach and the forms of knowledge contributions in the nine papers.

#	***Main research approach and data***	***Forms of knowledge contributions and quotes from the papers***
1	Qualitative interviews from several organisations.	Mainly to practice: organisations may discover new ways of exploiting social media in crises.
2	Case study (interpretive) of one company. Qualitative interviews.	Confirms some of the existing research, but does not specify which one(s). Claims to have conducted *"Inductive data collection"*.
3	Qualitative case study, (Yin type) of one large company.	*"In addition to contributing to the scarce research on ECM, the study adds to the body of research on the challenges of implementing standardized IT solutions in large, heterogeneous enterprises (e.g. Ciborra et al. 2000)…"*
4	Explorative case study of one small retailer.	Confirms the model by Verville and Halingen (2003). Builds on Markus & Tanis (2000), thus illustrating the utility of this model.
5	Interpretive case study (Walsham) of one organisation. Inductive approach. Interviews based on specific research.	Provides a hypothesis: *"…agile project management contributes to the ante narratives being more active in the social context in the project…* (our translation).
6	Exploratory case study. Interviews based on the Diffusion of Innovation theory's adoption element.	Reference to Roger's (2003) Diffusion of Innovations throughout the paper, but no theoretical contribution presented beyond the guidelines for adoption.
7	Questionnaire (based on literature). Responses from 180 companies. 3 hypotheses. Regression analysis.	*"…the findings are relatively congruent with existing literature."* *"…unexpected outcome was in regards to data security. […] This is quite contradictive to the […] Cloud/SaaS literature."*
8	*"…a quantitative and explorative survey among 132 large Scandinavian companies"*.	*"This is a contradiction to the empirical research of Saccol et al. (2003) and the assumptions of Yannis and Brynjolfsson (1996)."*
9	Building an application/prototype. Conducted experiment. Questionnaire (not based on previous research).	*"The novelty in this work is a new implementation of context by integrating three dimensions...."* *"…we have shown the viability and usefulness of our approach and we do believe […] this paper takes the PIM concept one step further..."*