

# Literature review papers – a brief how-to guide

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DISCLAIMER 1: This memo was written mainly with IDI phd students in mind, and might have varying relevance for phd students even within this one department, depending on their specific research project. The status and commonality of review or survey papers may be different in other disciplines, yet at least some of the advice in this memo would hopefully be relevant to students outside my own department. Also, while it may be difficult for students outside IDI to get much out of reading the example papers referred to in this memo sentence by sentence, this should not be necessary to get the basic message, which is more about how the papers are structured and why this may have contributed to their success.

DISCLAIMER 2: This little memo is by no means a complete recipe or academic treatise on why or how to perform literature reviews, so if you need to include methodological discussions of literature review in own your papers or thesis, you should not refer to this memo (which has anyway not been peer-reviewed), but rather to other more weighty sources discussing methodology for literature review. I.e., this memo should mainly be considered a brief inspirational guide to find out more about literature reviews and possibly inspire some good reviews written by phd candidates at the NTNU, not an authoritative source on the topic.

## ***Why write literature review papers?***

In various stages of an academic career, you may find yourself in situations where you need to establish an overview of the state-of-the-art research within some area, or related to some research topic. Typical examples of such situations are:

- in the early stages of thesis work, such as for a PhD degree
- when getting employed as a researcher or lecturer, especially if the research area of the position to be filled is somewhat different from your PhD specialty
- later in your career (e.g., as a professor...)
  - o if your employment or funding situation causes a change of research area, a need to teach courses which you are not an expert in, etc.
  - o if, for some reason (e.g., exceptionally high administrative or teaching burden), you have been unable to do research for quite some time, and therefore need to catch up with the state-of-the-art in your field

In such cases, where you anyway have to undertake a substantial effort reading various sources, it might be useful also to get a publishable result out of this effort. Of course, this does not have to mean writing a paper – for instance, for the thesis you could also write a state-of-the-art chapter, and for other situations you could write a memo, report or develop teaching slides. But writing while you read is key – this will make you read in a more systematic manner, help you develop a deeper understanding of the material, and help you remember what you understood afterwards.

However, the literature review paper as a peer-reviewed publication, has some additional advantages:

- feedback from reviewers may help to improve your understanding of the field, for instance if you have missed or misunderstood important sources
- it has high synergy with teaching, while your more specialized papers containing your own research contributions will often be too narrow for inclusion in course readings
- if your research is oriented towards inventions / patents, you may be reluctant to publish much about it (e.g., commercial secrets); a review paper does not entail any such problems

- if accepted in a good journal, a review paper can be quite prestigious, and thus helpful to your academic career

The latter point is particularly relevant because academic evaluations nowadays (e.g., for getting postdoc or lecturer positions, getting tenured or promoted from a lower to higher position level, getting project grants, etc.) often focus not only on publications, but *citations* – and successful review papers tend to be among the most highly cited. As an example, consider the top 8 list of highly cited papers by IDI employees, i.e., those papers having more than 250 citations in Google Scholar<sup>1</sup> (as per 17.10.2011, the IDI persons in **bold**):

<b>Author</b>	<b>Paper Title</b>	<b>Publ Channel</b>	<b>Year</b>	<b>#Cit</b>
<b>Aamodt, Plaza</b>	Case-based reasoning: foundational issues, methodological variations, and system approaches	<i>AI Communications</i>	1994	3801
<b>Jenssen, Lægreid, Komorowski, Hovig</b>	A literature network of human genes...	<i>Nature Genetics</i>	2001	660
<b>Conradi, Westfechtel</b>	Version models for Software Configuration Management	<i>ACM Computing Surveys</i>	1998	579
<b>Komorowski, et al.</b>	Rough sets: A tutorial	Rough Fuzzy Hybridization: ... (book, Springer)	1999	551
<b>Rao, Su</b>	A survey of automated web service composition methods	SWSWPC (conference)	2005	514
<b>Lindland, Sindre, Sølvberg</b>	Understanding quality in conceptual modeling	<i>IEEE Software</i>	1994	513
Dybå, <b>Dingsøy</b>	Empirical studies of agile software development: A systematic review	Information and Software Technology	2008	295
<b>Monteiro, Hanseth</b>	Social shaping of information infrastructure	IT and changes in org. work (book, Chapman & Hall)	1995	281

In this list, #1 (Aamodt & Plaza, 1994), #3 (Conradi & Westfechtel, 1998), #5 (Rao & Su, 2005), and #7 (Dybå & Dingsøy, 2008) are clearly literature review papers. #2 (Jenssen et al., 2001) also has aspects of literature review, although in a somewhat different sense: the approach in that paper is to extract information from papers automatically, not by human reading. #4 (Komorowski et al., 1999) is a tutorial, which has many similarities with a literature review in giving an overview of a topic. Most notably, of course, (Aamodt & Plaza, 1994) is very much ahead of all other IDI publications when it comes to #citations.

The list above should not be taken to mean that these are the 8 “best” IDI papers throughout time, as #citations is not a certain indicator of quality (and neither is few citations for a paper a sure sign of poor quality). Moreover, it should be noted that many of the papers are quite old, and it is no surprise that a paper published more than a decade ago has more citations than one published last year. Still, the picture is clear: A good review paper can often reach higher numbers of citations than other kinds of papers by equally clever researchers. This does not mean that you should throw away everything else and just start churning out review papers – such researchers may easily be considered shallow, not making enough own contributions to the field. Indeed, the bulk of your future scientific output should probably be publications where you make a specific theoretical or technical contribution rather than merely reviewing the work of others. A possible advise, though, is that it would be relevant for each PhD student to try to write at least one good review paper about his/her state-of-the-art thesis baseline, as well as for each professor to have at least one good review paper about his/her field of expertise.

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<sup>1</sup> I am using only Google Scholar here because Thomson ISI has a too strong bias towards journal papers (so conference papers would be neglected), and as for Scopus, which does include conference papers, NTNU does no longer subscribe to it. Also, since I had to run this check person by person in Google Scholar, which was quite tedious, I may have missed some papers – in which case I apologize for that.

## **Guidelines for writing review papers**

This memo does not intend to cover all the work that you have to do in the course of the review, such as searching for papers, reading them, taking notes while doing so, etc. – hints on how to do this can be found in other sources, such as (Hart, 1998; Kitchenham, 2004). This memo focuses more narrowly on how to structure and write your paper. In the following, we will present some guidelines for writing a good literature review paper, using (Aamodt & Plaza, 1994), (Conradi & Westfechtel, 1998), and (Rao & Su, 2005) mentioned above as examples. Also, note that none of these three papers are what the expertise would call *systematic reviews*, hence if you want to do a systematic review you should follow published guidelines specifically for this rather than the guidelines presented below.

**Guideline #1:** Have a well defined scope and research question, and make this explicit in the paper intro, so that it is obvious to the reader what you are trying to review and why.

**Guideline #2:** Take care to communicate to the reader the *utility* of your paper: Why is a review of this field needed? Who (e.g., scholars, practitioners) can the review paper be useful for. If there are review papers in the field already, what *added value* does your paper bring beyond the previous ones?

### **Practical illustrations of guidelines #1 and #2:**

In Aamodt's paper about case-based reasoning the purpose of the paper is stated early on as presenting "an overview of the field, in terms of its underlying foundations, its state-of-the-art, and future trends" (Aamodt & Plaza, 1994, p. 1). Motivation for the paper is given generally, stating that CBR is a field of "widespread interest", "rapidly growing" etc. More specifically the authors take care to distinguish their paper from overviews of CBR by other authors (second paragraph in section 1.1), explaining the added value of their own paper.

In Conradi's paper about software configuration management the importance of the research area is similarly stated early on, both in the first sentence of the abstract and in the Introduction. The need for a review paper is motivated for instance by the sentence "The variety of formalisms makes it difficult to compare..." (Conradi & Westfechtel, 1998, p. 234). Later on (Section 7: Related Work) it is explained how the survey performed in this paper is different from previously published overviews by other authors.

In Rao & Su's paper, the authors spend nearly two pages in the introduction arguing why automated web service composition has gained increasing importance, then stating clearly what their review is about as "In this paper we will present an overview of recent methods that provide automation to Web service composition." (Rao & Su, 2005, p. 44). In the same paragraph it is admitted that there exists some review of such methods in a previous work by Benatallah, but that this has a more limited scope (only dealing with workflow based methods, while Rao & Su look at both workflow and AI planning methods), hence motivating clearly for the new paper.

From guideline #2 it follows that if there are other review papers already published in the field you are targeting, your paper must somehow be different and have some added value. This can be achieved by one or more of the following:

- your paper is *more thorough / including more sources*. In particular, if the other papers were published some years ago, yours may be more up to date in including newer sources.
- your paper has a *different scope* (which can be either broader or narrower than the other papers; if narrower, it should of course utilize the narrowness to make a more detailed discussion within that more limited scope than the other papers did)

- your paper is *more systematic* (for instance following the rules for a *systematic review*, while others did not), or *less biased* (in case the others were clearly favouring some approaches over others)
- your paper offers an *original contribution beyond summarizing the sources*, such as a new terminology / taxonomy / ontology for understanding and discussing the field, a novel framework for comparing the different works in the field, new comparison results that have not been seen before, or novel deep insights about the field as a result of analyzing and synthesizing the various works.

From this, some more ambitious guidelines can be distilled:

**Guideline #3:** Present the material in your own words, and with a structure fitting *your* purpose – avoid simply repeating statements from the sources.

**Guideline #4:** Strive for synthesis, rather than simply summarizing the various sources.

**Guideline #5:** If possible, try to come up with a novel ontology / taxonomy / comparison framework for the field, rather than using one that has been used before. (Still, it does not have to be entirely novel, it can for instance be adapted from another field, only that it has never been used before in the field you are surveying). Of course, novelty is not a goal in its own right. The framework should be conceptually elegant and well aligned with the intended purpose and the reader must feel it is useful for a deeper understanding of the field.

Practical illustrations:

(Guideline #3):

In Conradi's paper an explicit decision is made to use a unified terminology in the paper, rather than presenting each surveyed work in the terminology used in the cited source, cf. the argument surrounding the sentence "In order to reveal these concepts, we introduce a unified terminology." (Conradi & Westfechtel, 1998, p.234). With the opposite decision (presenting each source in its own terminology), the paper might have been quite confusing since different authors sometimes use different terms for the same concept or the same term for different concepts, and C & W would then have had to use a lot of space explaining that term A in paper 1 means the same as term B in paper 2, etc., which would easily have destroyed the flow of their paper.

(Guideline #4):

True, the Conradi paper does contain a section where a number of SCM systems are presented one by one (Section 6), but note the following points:

this is done *late* in the paper, after the main part containing the presentation of key concepts in SCM (sections 2-5)

hence, each system can be described quite briefly (since the concepts are explained already) even within section 6, the focus is on synthesis, in terms of Figure 20 and Table I and II, the presentation of each system then following in section 6.3

The naïve way of writing this paper would have been the opposite, first presenting the important SCM systems one by one, then trying to synthesize this into a set of key concepts and distinguishing properties, distilling a taxonomy and classifying the various systems. This might also have been an OK paper, but probably inferior to the existing one. Presenting the systems first, each would have needed a longer presentation (since a unified terminology and key concepts were not yet established), the structure of the paper would thus have been much more dominated by the concrete systems, and there would have been less space for the overall synthesis and explanation.

In Aamodt's paper, the presentation of the concrete systems is even more toned down. A number of systems are referenced in the History section (Section 2), but not really presented, as this is a quick

run-through of the historical development of the field. Then, rather than immediately going into more details about concrete systems, the authors follow up with the key section of the paper (3), which defines the CBR cycle (Fig 1) and a taxonomy of tasks/methods (Fig 2). When this descriptive framework is further elaborated in sections 4-8, features of existing systems are mentioned where they fit in. Note the sentence “Our examples will be drawn from the six systems PROTOS, CHEF, CASEY, PATDEX, BOLERO, and CREEK.” (Aamodt & Plaza, 1994, p.9), these six systems are then mentioned here and there in the following chapters whenever they include solutions to the problems discussed in the various subsections. Again, the opposite (and naïve way) of writing this paper would have been to go 1. Intro, 2. History, 3. Current systems, with subsections 3.1 PROTOS, 3.2 CHEF, 3.3 CASEY, 3.4 PATDEX, 3.5 BOLERO, 3.6 CREEK, and then try to make some unified analysis of this, for instance leading to some tabular comparison of functionality included (or not included) in the various systems, underlying architecture, etc. Again, this could have been an OK paper, but probably much inferior to the existing one, as the paper would have been much more dominated by simply retelling the contribution of various sources, and less by making an original overview of the field.

(Guideline #5):

In Conradi’s paper, a classification taxonomy of SCM systems is developed (Table I and II), which – although many of the distinguishing terms were known already – was altogether novel. Similarly, Aamodt’s paper makes a key contribution that goes way beyond an overview of the existing literature, namely by developing a descriptive framework (CBR cycle and taxonomy of tasks) and distilling this in terms of two easily understandable figures. Rao & Su's paper also start out with a generic framework (section 2) before going into details about the various methods investigated in the survey. This gives a basis for a unified discussion of the various works.

If these three papers had been more directly focussed merely on summarizing the existing systems / key publications in the field, they might still have been good and useful papers at the time of publication, and could have received a lot of citations early on – but they would have become dated much sooner, as new and better SCM or CBR systems or new web service composition methods emerged with functionality beyond the ones included in the survey. When these papers keep being cited even today, many years after their publication, it is probably not so much because they give a good overview of the state-of-the-art in SCM or CBR in the 90’s, (or of web service composition in the mid 2000’s), but because they offer more general concepts and frameworks to understand the respective topics. Especially, the huge success of the Aamodt paper is attributed to the fact that it became a “*field-defining*” paper, providing a deep and insightful answer to the question “What is case-based reasoning?”, rather than simply summarizing previous work in the field.

Notice, still, that for all three papers, the authors’ way of working during the research for the papers may have been more bottom up, starting by gaining an understanding of concrete case-based reasoning systems, configuration management systems, or web service composition systems, one by one, then trying to synthesize this into a larger picture. The moral, of course, is that the order of presenting your insights in writing in a paper does not have to follow the order of working in obtaining those insights. A common novice mistake in paper writing is to present the matter in the same order as you worked it through, got the various ideas, etc. – which may be the right way sometimes, but far from always.

### ***The utility of review and assessment papers in a larger project***

Above we have established that a good review paper can be useful for you to write and publish, and for other researchers in the field to read. However, your research project (e.g., for a PhD thesis) is probably not focused on performing reviews of existing theories or technology, but for instance towards developing new technology, methods or theories. In our department, with a strong engineering focus, the prevalent research method is that of “design science”, see for instance (Hevner et al., 2005), where your ultimate goal is to develop some original artefact, which is useful in the sense that it solves some problem better than any

competing artefacts already existing. In that respect, you might be afraid that a literature review or assessment paper might steal too much time from your necessary work of designing your original artefact. Assuming that the goal of your project is to design some artefact Y to solve a problem P, and Y must be better than a number of existing solutions / solution attempts X1...Xn. A “traditional” publication strategy in such a design science research project might be to

- start off with some sketches of Y at workshops / fairly easy conferences, typically published as “research in progress” (including, if necessary some quick arguments why X1...Xn are insufficient)
- when a detailed design of Y is achieved, try to publish this in a better conference
- when an implementation of Y is achieved, having some test results, publish this in an even better conference
- at the very end, perform a thorough evaluation of Y (possibly comparing with X1...Xn if they can be run through the same evaluation), and publish this in a high prestige conference or journal

However, if you include a thorough literature review effort early on, it is possible to envision a different publication strategy for your project:

- start off with a literature review paper of the state-of-the-art in the field (i.e., explaining the problem P and reviewing the existing works X1...Xn). If done well, this could be publishable in a journal.
- possibly also develop a set of evaluation criteria related to solving the problem P and perform a comparative assessment of X1...Xn. Again, if done well, this could be publishable in a journal. (In some cases, however, the situation might not mandate two different papers here – one with the review and another with the assessment – it might also be one single paper, especially if the assessment is a simple add-on to the review, because the existing artefacts have already been assessed in a way which is easily comparable. On the other hand, if you yourself have to define the evaluation criteria and perform the evaluations, this clearly goes beyond a normal review paper since there is a lot of extra work beside reviewing existing published material – then a second paper is easily mandated)
- thereafter proceed as above, for instance with early sketches of Y, the detailed design and implementation of Y, and the final evaluation.

**Of course, notice that the above publication strategies might not be suitable for all theses or research projects, so exactly what publication strategy is useful with respect to your thesis work is something you should discuss with your supervisor, not decide based on this memo.**

The potential advantages of the second publication strategy above are as follows:

- the initial literature review helps you ensure that your own design becomes original (you have not overlooked any existing works), while at the same time possibly giving you some inspiration for creative ideas (e.g., combining properties of different solutions, adapting a solution from a slightly different field). The review also makes it much easier for you to write the “Related work” section in all your subsequent papers about Y.
- the assessment (whether included in the review paper or as a separate paper) helps you write the motivational argument needed in your subsequent papers about Y: Why is a new artefact needed, what are the shortcomings of existing artefacts?

- If you needed to build a set of evaluation criteria for the assessment paper, this might have been time-consuming, but as a result (if the paper is published) you have a set of peer-reviewed evaluation criteria which you can reuse when your own artefact is finished, to compare this to the existing artefacts (i.e., time spent early can here be saved in the later evaluation stage). Also, defining the evaluation criteria up front may appear more scientific than first developing Y and only then considering how it might be evaluated, in which case a reviewer or opponent might suspect that the evaluation criteria were biased towards favouring Y. (In any case, of course, the evaluation criteria should not simply come off the top of your head, but be grounded in literature or input from industry or users, depending on the type of artefact and amount of published quality criteria available for that type of artefact).
- Finally, in the unfortunate case that your design science project “fails” (e.g., the artefact you come up with, turns out not to be better than competing artefacts X1...Xn), the extra contribution (and papers) resulting from the review and assessment might mean that you still have enough material to come out of it with a thesis (the failure of the design might also be publishable, but probably in a less prestigious channel than if your design was successful). With the traditional approach where all your publications are about your own artefact Y, the road to a completed thesis might be longer in such a case.

On the other hand, there are of course also potential disadvantages with a too early or too time-consuming literature review. Especially if you work in a research area where there are vast amounts of published research already, a thorough literature review paper will demand a lot of man-hours, and especially if going for the rigour of a systematic review. Hence, there is an obvious trade-off - if you spend too much time reviewing the state-of-the-art and writing up one or more journal papers based on this, you will have too little time left making your own technical inventions. The example paper by Conradi cited here, with its length, thoroughness, and huge number of sources, probably demanded so many man-hours of work that it would be beyond the capacity of a normal phd student who also needs to come up with original technical contributions, and only has 3-4 years for the entire research project. Both Conradi's and Aamodt's papers were based on long-term experiences by the authors themselves doing research in the areas in question, hence they did not start from scratch in their quest for an understanding. However, something like the paper of Rao & Su, which is shorter and with fewer sources, could clearly be achievable by a phd student - as the authors were indeed phd students at IDI at the time of writing the paper.

Another potential danger of doing too much literature review, or doing it too early in the process, is that one might become too bound by existing works, thus ending up being less creative with respect to envisioning radically new solutions to the problem at hand (as mentioned by Prof. Omre at the same IME phd seminar in Nov 2011). Also, if you do a lot of literature review at the very outset of your project, having only vague ideas about what your own research will be, you may end up having to review the literature very broadly and without a clear focus. Hence, it could be an idea to first spend some time making early sketches of your own "solution" (as called Y above) before undertaking a thorough literature review, this will also make it clearer for you exactly what literature to review, and from what angle the reviewed literature should be read. But of course, working too long on Y before reviewing the state-of-the-art will run you into the opposite risk, namely that you might spend a lot of time fleshing out details of a solution which you think is highly original, only then to find out that somebody else already did what you are proposing.

Aamodt and Conradi, or for that sake Rao & Su, were also mainly occupied with design science research at the time they wrote their highly cited papers mentioned earlier in this memo. Aamodt's main concern was to develop better CBR systems, Conradi's similarly to develop better SCM systems, and the main research of Rao and Su for their theses was not to review the work of others but to make better methods for web service composition and ontology mapping, respectively. This indicates that development of new technology and reviewing of existing technology go hand in hand: a thorough insight in the existing and its shortcomings can sometimes make you a better inventor.

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