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Photo, front cover: Magnus Rönn. The sculpture on the front page was made in 2009 by granite from Bohuslän in Sweden. The sculpture is designed as a door with an open view to the sea in the west. The artist is Claes Hake.

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THE REVIEW: TO ENCAPSULATE AND QUOTE, TO COMMENT AND CRITICIZE

JERKER LUNDEQUIST

Abstract

A doctoral thesis should be seen as proof of research competence and the reviewer must ensure that the author has demonstrated the basic competence and knowledge that enables practice as an investigator and researcher. Based on this general requirement, I have examined two doctoral theses. The comments on my evaluation in the journal made me start to think about what it really means to do reviews. I have come to some conclusions which later became a very personal checklist which is discussed in this essay. I hope that this essay can provoke researchers to start writing reviews, either according to my checklist or according to a completely different one, or even write an angry text for a debate about how to write proper reviews for the academy.

Potentially, in this essay, I would like to argue for three aspects that I find particularly rewarding to bring into a peer-review: (i) to apply criteria of *scientific fertility* or creativity to the report; (ii) to investigate the degree of *methodological objectivity* of the research project; (iii) and finally to discuss the validity or *viability of main ideas and concepts*. This checklist for reviewers is hardly to be seen as complete nor exhaustive but it might hopefully encourage some new reviewers. The basic idea here is that the "good" review is built up of four constituent components: *encapsulate, quote, comment and criticize* and that three suggested topics are particularly rewarding to bring into the review: *project viability, methodological objectivity* and, most importantly, *prime ideas*.

Keywords:
Review, Quote, Comment, Criticize

Introduction

A problem for reviewing Nordic architectural research in the journal *Architectural Research* is the shortage or lack of reviewers.[1] So, our ambitious goal to publish reviews for all PhD dissertations in the field of architectural research has not been achieved. In addition to PhDs there is also a considerable number of books, research reports and anthologies that all deserve to be presented and scrutinized.

This absence of reviewers is surprising. Architectural researchers are reading people. Architectural researchers are used to reading in an active manner: to take notes, find valid excerpts and consult references to sources. It should be simple to let this considerable experience from active reading result in the prolific writing of academic reviews. A possible explanation behind this situation could be that the world of architectural research is fairly limited: one does not want to provide public criticism of colleagues that one risks meeting in person. This kind of collegial solidarity is however quite misdirected.

In No 4-1988 of the journal *Architectural Research*[2], this problem was addressed in the editorial, referring to two critical comments concerning my reviews of dissertations by Shokrollah Manzoor and Birgitta Mekibes. (Dick Urban Vestbro was co-author on the review of Mekibes work)

Unfortunately, the editor in this case has misunderstood the spirit and aim of these reviews. Manzoor and Mekibes have both authored excellent theses. Manzoor's dissertation is in my opinion to be regarded as exceptionally well done. But at the same time, it is left to the author of the thesis himself or herself to decide on what level of qualitative criteria the result must be judged.

According to a recent convention, a doctoral thesis should be regarded as a proof of basic research habilitation and examination to ensure that the author has demonstrated the basic required proficiency and competence enabling practice as investigator and researcher. Some PhD students consider these basic criteria too low and too modest, so they try to set far higher levels for themselves – at their own risk. Both Mekibes and Manzoor are typical exponents of this kind of doctoral candidates. Such an attitude is ambitious and to be commended, but it means risk taking, because the author will be exposed to her or his self-proclaimed, higher-level criteria.

I will try to summarize the criticism conveyed on the two theses in my two reviews:

Mekibe's thesis is a clever and sensitive investigation on ways of residing, studied in a spontaneously built residential area in Oran, Algeria. The criticism voiced against her work, in my opinion, is that the planning

1 Text translated by Sten Gromark from original published article in Swedish. The abstract has been added to the essay in this issue of the journal. This contribution was first published as "Recensera – referera, citera, kommentera, kritisera" in the Journal of Architectural Research, 2(3), 1989 (*Tidskrift för Arkitekturforskning, 2*(3), 1989). References have been converted according to APA 7th, and endnotes have been added by the editors as clarifications.

2 The journal *Architectural Research* (*Arkitekturforskning*) was established in 1987 as a scientific journal. During the first five years, the journal was given the name *Architectural Research* and was steered by three schools of architecture in Sweden. In 1992, the name changed to *The Nordic Journal of Architectural Research*, NJAR. Simultaneously the association running the journal changed its name to the Nordic Association of Architecture and became a truly Nordic affair with board members from Sweden, Norway, Denmark and Finland.

strategy advocated in the last part of the book is not supported by the research results of the empirical case she presents. The critique is thus focused on pasted-on planning ideologies and planning conceived as strategical reasoning.

Manzoor's thesis investigates an application of Christopher Alexander's pattern language approach on certain traditional ways of building in parts of Iran.

My critique of Manzoor's thesis is focused on the point that Manzoor does not unfold a sufficient critical perspective on Alexander's ideas, causing some minor blemishes on the final result. Perhaps one might conclude that Manzoor missed out on a major opportunity to provide an important contribution to the pattern language methodology as such.

Obligations of a Reviewer

The editorial published in the journal *Architectural Research* No 4-1988 made me, however, start thinking about what it really implies to conduct reviews.[3] I have reached some standpoints that later have been transformed into a very personal perception of a check list as discussed below. I hope this list might provoke at least one or many more researchers to start writing reviews, either according to this model, or following a totally different one, or even to write an angry text for a debate on how to write proper reviews for academia.

To start with, I will enter some general reflections on how reviews should be configured, before I go further into details on the four components or stages that constitutes the "good": review, namely: *encapsulate, quote, comment and criticize.*

The reviewer must allow the readers to construct their own opinion on the work! He or she cannot take it for granted that they have read the text or report in question and must therefore provide extensive summaries or quotes on the chosen passages he intends to comment or criticize. He should also clearly indicate what is considered respectively to encapsulate or quote; to comment or criticize.

A review is written to be read. The target reader should be all readers of the publication in question. A reviewer should avoid digging down too deep into details but instead keep to the overarching ideas and tendencies presented in the work to be scrutinized. There is no reason to go through the entire text in detail. It is far better to concentrate on some particularly interesting sections.

3 This essay by Lundequist can serve as a guide for what a review should contain and be designed. The background to the essay is the criticism of colleagues for published reviews in the journal. From this criticism, Lundequist delivers an interesting and instructive idea about the review as theory and academic practice.

It is not kind to be kind! Too general an approach and a sloppy and vague but benevolent review only reveals that the reviewer has been so bored by reading the report that he has not read it with sufficient attention and in necessary depth. One should also avoid formulating too general and sweeping judgments. Instead, one ought to be as specific as possible. A reviewer might very well pass a bitter judgment on some minor points to indicate what he thinks is not good enough, but he also ought to prepare his critical points in great detail so that it becomes perfectly apparent that he has thoroughly read the whole work.

The reviewer should stick closely to the actual content in the work to be scrutinized! An obvious sign of a badly conducted, oppositional approach is when the critic keeps repeating all the points that are missing and should have been addressed, instead of discussing what is actually referred to in the thesis. That is, if there is not something really essential like a blind spot that has been detected and left out of the argumentation; or is not being properly handled nor at all depicted as an apparent major *lacune*. But in that case, the burden of proof is turned over to the reviewer and he must be able to show why this missing something should have been fully integrated. In any case, a review must not by any means deteriorate into just a general and superficial article on some theoretical and methodological key questions within the research area.

Encapsulate and quote

It is important that the readership independently can make up their own personal judgment of the work in question. A review should therefore start with an encapsulation or *summary* of the research project behind the report. Some parts of the report might be direct quotes like purpose, main hypothesis and key concepts. If this takes up too much space, one must restrict oneself to references. On the other hand, it is important to use formulations in direct quotes if they are part of the reviewer's planned structure of delivering his critique. Using many specified quotes also carries the benefit that the reviewer is forced to be very precise and concrete.

The key conceptual definitions and components of the work should be quoted completely, as a background to the critical assessment by the reviewer or discussant, provided that these terms are used consistently throughout in the whole document according to given definitions.

What should be commented on varies from one case to the other. However, what should always be commented is the starting point for the *research problem* behind the project. Is this problem interesting? For whom? Could it be further expanded?

The *results* of the research program should also be commented upon, as well as its potential importance and *impact* on societal innovation. In any case the reviewer must leave his comments on the conclusions drawn and the validity of the suggested recommendations for further action as presented in the research report.

Do the research findings corroborate research in other closely linked research projects? Do the conclusions follow logically the application of chosen theories, methods and concepts on available data? Do recommendations and rules of action in practice, presented in the research, sufficiently support the conclusions drawn?

Quality of language and graphic design could also be commented on. Is the structure of presentation in chapters, sections and paragraphs logical and well-conceived? Do headings and chapter divisions properly reflect the actual contents? Furthermore, graphical text designs, illustrations, diagrams and tables should also be commented on.

Language properties in a report should respond to reasonable demands of *intelligibility* and *precision*. Demands for precision are however of higher priority than intelligibility. A rule of thumb is to test the character of language by searching for samples of short and powerful quotes. If that is not possible, the author has largely ignored both essential demands on language properties.

A review must include some sort of assessment of the quality level obtained in the work in question. The reviewer cannot escape his obligation to do more than only to encapsulate, quote and comment. His perception of the work done must also include a critical element or final statement. But to criticize is not just a question of pointing to mistakes, contradictions, lacunes and one-sided digressions. Above all, it means to try to *characterize* the work at hand by way of revealing the *main theoretical point or principle*, the common thread running through the whole knowledge-searching endeavour.

And what the reviewer always must be able to demand from a research report is that the author declares his critical position, his critical stance influencing his work. Are the theories and methods applied by the researcher really fully critically assessed by him? Has he clearly stated the limitations of these adopted approaches? And has he succeeded in avoiding effects of these limitations on his own research? Does the theoretical section cohere well with the methodological section? A very common mistake of authors is to delve into lengthy theoretical speculations that are all actually totally unrelated to the chosen methodological configuration.

Criteria for Good Research

Reviewing research reports is ultimately about the assessment of research quality; the *relevance*, the *accessibility* and the *efficacy*. However, it is not an easy task to be more precise about these criteria for good research. I will not go into detail here on these criteria since that topic is covered extensively in many contributions on this issue, in other pages of the journal.

It goes without saying that you cannot write reviews while simply checking off systematically several given criteria. These overarching criteria for "good" science are useful to have in mind as a backdrop, at the same time as the reviewer considers putting focus on particular aspects. What aspects one wants to put emphasis on varies from case to case.

Potentially, I would like to argue for three aspects that I find particularly rewarding to bring into a review: (i) to apply criteria of *scientific fertility* or creativity to the report; (ii) to investigate the degree of *methodological objectivity* of the research project; (iii) and finally to discuss the validity or *viability of main ideas and concepts*.

Professor Hans Rosing has underlined that research projects should lead to new hypotheses and indicate new problems to be investigated. An important demand on these new hypotheses and problems then becomes that they should be scientifically fruitful, and that they are able to initiate and direct new investigations, tests and experiments. They should open up for the introduction of new imaginable patterns, contexts and structures (Rosing, 1988). A reviewer ought to pose himself questions like: To what degree could this be useful for innovation and provide stimuli for other researchers? What new concepts and ideas could be derived from these research findings?

The notion of *methodological objectivity* means for Rosing that objectivity in science, in the first instance, is guaranteed by the set of methodological rules applied within each specific research arena. Each researcher is at great liberty to choose fundamental points of departure, problems and hypotheses. But on the contrary, he must normally follow the rules that exist, for the collection of data and the testing of his hypotheses.

Rosing formulates this in the following way, "…that it is not the individual researcher who decides how his hypotheses shall be tested. In general, this is decided by *the logic of science*. In principle the values of the researcher cannot have an impact on testing" (Rosing, 1988, p. 167).

It might be rewarding to take up, in a review, the methodological objectivity, to discuss how and to what degree the way the project was conducted has influenced achieved results and findings.

What should always be brought up in a review is the viability of applied approaches (Lundequist, 1984; 1987). A research report should be able to mention some major ideas of prime importance. This means for instance that the concept of "careful renewal" represents an alternative to restauration or demolition/to build a new, or that "deliberation planning" is a particular way of planning driven under different conditions than is usual. Or that project planning methodologies is more about the buildup of project planning competence rather than reaching for some kind of "optimal" organization of the project planning process, and so on…

Conclusions

This checklist for reviewers is hardly to be seen as complete nor exhaustive but it might hopefully encourage some new reviewers. The basic idea here is that the "good" review is built up of four constituent components: *encapsulate, quote, comment and criticize* and that three suggested topics are particularly rewarding to bring into the review: *project viability, methodological objectivity* and, *most importantly, prime ideas*.

As a final remark, I doubt that anyone can abide by all those rules – definitely not myself. But one could always try.

Notes[4]

For the sake of good order, I shall nevertheless provide a basic summary of these criteria, at the same time pointing to some related references:

Tore Nilstun has for instance formulated the following criteria (Nilstun, 1988; Mårtensson & Nilstun, 1988; Nilstun in Sollbe, 1986): *ethical acceptability* (sensitive ethical information must be approved for publishing by all parties, alternatively that these persons' identities are hidden), *availability* (report must be made available for all interested parties, as well as available for assessment and scrutiny by professionals), *intelligibility* (terms, expressions and formulations must be defined and immediately understandable for target groups), *precision* (target group must be able to perceive the work as the author has intended it), *credibility* (the report must provide a justifiable picture of the research area; conclusions and recommendations must be supported by solid and relevant arguments), *testability* (theses presented in the report should be disputable – one should also indicate what kind of arguments that could provoke a rethinking of these theses), *unbiased* (the researcher must not manipulate results for the benefit of external parties), *news values* (the report should contain all results valuable for the target group) and *relevance* (it should be made clear whether results can be implemented in practice or in theory).

Göran Wallén has pointed to the importance of distinguishing between *internal* (intra-science) and *external* (funding bodies, practitioners and

4 This section is in the original text a long note, which in this translation is lifted into the essay.

users etc.) assessment criteria. Wallén brings up a number of examples of significant internal science relevant assessment criteria (Wallén, 1986; Wallén in Sollbe, 1986): *internal relevance* (coordination of problem, method, theory and material), *systematic approach* (in any case as presented), *repeatability* (in any case in some types of natural sciences research), *theoretical integration and depth of explanation* (integration of different theoretical or methodological approaches; of research traditions.) *reliability and methodological conscience* (this concerns above all the realization of research and how researchers have handled methodological issues, and that results have a *certain reach* (a certain empirical, theoretical respective methodological generalizability).

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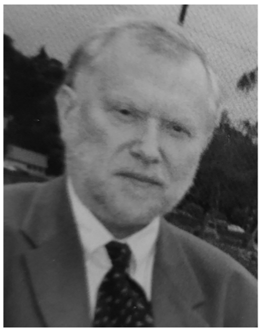

Biographical information

Jerker Lundequist

Jerker Lundequist (1941–2015) was professor of design methodology at the School of Architecture, Royal University of Technology in Stockholm. He was one of the founders of the Nordic Association for Architectural Research. From 1987 and over the next twenty years, Lundequist was both the journal's editor and a member of its editorial board. He was also a brilliant critic and reviewer of scientific texts. The first two articles that Lundequist wrote for the journal were about the graduate program (1987: 1, 1987: 2) and in his last essay he returned to the concept of architecture as a fundamentally disputed concept (2005: 4).