

Tips and instructions for applicants (for Doctoral Studies in a School of Design)

Read this site if you consider applying to the School of Design's doctoral studies.

(c) Ilpo Koskinen, May 2008

To the reader

- This is an old document I wrote around 2006. Its last update was 2008.
- The document was placed on an old server called uiah2 that does not exist anymore.
- Over the years, several doctoral students have told me it has been very useful in preparing them for applying for doctoral programs, so here it is republished. A few of these people are professors by now, so I tend to believe this may be true.
- This time it is in a linear format; originally, I used frames in html to make it easier to read. Ah, I know: a linear format kills it, but mobile internet unfortunately pretty much killed frames, so I won't bother redesigning this doc anymore.
- Its main purpose was to improve the quality of applications to the former School of Design at the University of Art and Design Helsinki (now department of Aalto University) and it proved to be moderately successful.
- It was specifically aimed at prospective doctoral students, many of whom had a design background, and no research experience.
- Please note that this is a historical document: it was written from the perspective of a professor responsible for student intake for doctoral studies at the School of Design. It is not meant to be more than that.
- I think it has two particularly useful sections, however:
 - Section 3.2 is absolutely relevant and a place where most students (and even experienced researchers) constantly make mistakes. I cannot stress the importance of a framework/perspective too much. Most of us are not interesting thinkers; it is better to build on folks who are better than us.
 - Section 6 is my Borgesian list of cardinal sins in applications. If you consider applying to any doctoral program, this is still a good read.
- If you think it is colloquial in tone, you are right. It was partly written tongue-in-cheek, and the language reflects this. The reason? I wanted to make sure it seen as a doc that offers tips from an academic who had lots of experience in doctoral intake, rather than being read as an official document of the School, which is never was.
- Next, a word about the "experience" part in the previous bullet point. In the mid-aughts, the SoD of UIAH typically received on average about 50-70 applications per year, and could take in only about 8-10 students, depending on the supervisory load of professors. Thus the document is written from experiences between about 2000 and 2008. So, the data base for this document consisted of perhaps 500 applications, 1000 reviews (we always had two reviewers for each application), and about 60 accepted students.
- Finally, I didn't rewrite this – including not fixing typos, problems in language, etc. It is a historical document, after all.

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1. Basic requirements

Basic requirements for a competitive DA application are actually pretty simple. You need a competitive research plan. Minimally, it has to do the following things:

- + present the topic of the research and its research question
- + justify why the topic is important
- + possibly, describe the main hypothesis
- + tell in detail how the problem is researched, and why the answer produces a valid answer that the reader can trust
- + in addition, you may need to think about ethical issues, esp. when studying children, seniors, or patients

Technical parts of the plan are like in any project plan: schedule (plan for 4 full-time years), budget, etc.

Keep in mind that the difficulty lies in details. Outlining the topic is crucial, but often a few sentences are enough. Justifying the topic, on the other hand, requires lots of more work. In essence, the plan has to show

2. Types

Now, before you sit down to write a plan, you have to make a few decisions about how you aim to do your research. The single most important thing to understand is that there are different ways to do research and, correspondingly, no one right way to plan it. The first step is to decide what kind of research tradition is yours, and situate your work into it. For School of Design, three models are particularly relevant. The list is not exhaustive, leaving aside such things as the formal sciences.

2.1 Traditional hypothetico-deductive research

This is the most typical research format. Basically, your job is to read all relevant existing literature, find problems in it (i.e. identify your claim), develop a hypothesis that you test in your study, and devise a research design that makes it possible for you to test whether your hypothesis is right or wrong. "Hypothesis" is *your theoretical answer* to the research question; you do not know whether it is true or not before you gather data and analyze it.

This tradition comes from the natural sciences, but is the mainstream in most other fields of research. Typically, the proof takes a statistical form.

If this is your choice, read methods literature from this tradition, go to library, study literature carefully to see whether it does answer your question, and only then sit down to write a hypothesis (or a series of hypotheses). After this work, you can get to methodology: how to gather data (statistically or through controlled experiments or quasi-experiments), whether you want to construct something, which method to use to analyze data (tip: post-it notes won't do, you need statistics), and what is the crucial test for your hypothesis.

2.2 Interpretive research

In particular when studying people, this is an alternative. Under various names, most research in 20thC humanities, the social sciences and philosophy is interpretive in nature. Briefly, you do not try to theorize how people behave and use obscure theoretical language to write down your hypothesis, but begin with people: their language, their terms, their behaviors, and so forth, and try to explicate data you have gathered.

Don't be fooled. It sounds like an easy alternative, it is not. You need to know exactly what you are doing. You need to read yourself into some interpretive tradition - like interactionism - and its methodology, write down a claim to justify your research, plan data gathering carefully and in detail, and describe your analytic plan carefully. I have given several doctoral level classes about qualitative research, and even written books about them. Check them - or anything else - to get the basics right.

2.3 Constructive research

Under various guises - most misleading - many designers think that they need to do something "new" outside the traditional sciences. Typically, they want to construct something and integrate it into their research. Actually, there is nothing complex here. This is how most sciences proceed. What, after all, is the difference between constructing an interactive table

and a vaccine, and testing it with people? Most work in engineering proceeds exactly like this.

There are all kinds of ways to conduct constructive research. Software engineers and business studies talk about constructive research; social scientists about quasi-experimental research, policy research, or action research; medical researchers about clinical research; artists about practice-based research; and so forth. The question is really what kinds of new knowledge the construction helps to gain: it is new knowledge that justifies time and effort put into construction. Also, what is important to decide is how the construct is integrated into your study: is it like a "treatment" in medical research, a "breaching experiment" like in ethnomethodological sociology; or what. Also, you need to decide how you gather data, and how you analyze it to test your hypothesis, or to create an interpretation.

At the SoD, a constructive approach is more than welcomed, unless you make ridiculous claims about novelty, when in fact design researchers have invented very, very little new things.

3. Research plan: its structure

Research plan has several functions. At best, it informs evaluation, functions as a working plan, and convinces foundations and other funding bodies. Above all, it has to be logical: if you say something on one place, and something else in another, how can one evaluate your aims and plans?

Research plan has a few main components; I'll only describe the essential ones on this page.

3.1 Introduction and literature review (normally, about 1-1,5 pages)

Introduction typically describes the topic of the study, its objectives, and its research question. Keep in mind, research question is the main question that should ideally guide your study for 4-5 years into the future. It has to be pretty wide, but not too much so.

The same applies to objectives. Typically, the objective decomposes your question into subtasks. Even if you cannot solve famine through design, you may try to do that in one village in Laos.

Literature review is not just routine. The only way to SHOW that you are actually doing something new is to go through history and show that no one has answered your question yet, or has provided unsatisfactory answers. Literature review is a crucial component in any serious piece of research, and an essential part of any research.

Finally, you need a perspective for your research. Unlike in MA thesis, this has to be based on literature. Typically, it is a theory, but it can be a looser thing called framework or frame of reference. The difference is that in theory, concepts are defined in relation to each other; in frameworks, these relations are looser.

3.2 Perspective (<1 page)

After topic and research question, perspective is the single most crucial thing in your study. It specifies how you will answer your question. Even more importantly, you get a vocabulary: accurate concepts, how to relate them, definitions for these concepts. Selecting perspective wisely solves many problems in your research. Examples are Esko Kurvinen's ethnomethodology, Katja Battarbee's symbolic interactionism and Anna Valtonen's sociology of professions (specifically, Andrew Abbott's theory of the system of professions).

Thus, even though it may feel like reading takes lots of extra time, and learning a theory take something away from you, this is not the case. By spending a few weeks in library, talking to experts, and giving yourself time to assimilate new knowledge cuts off years from your research.

An old adage attributed to German social psychologist Kurt Lewin says that there is nothing as practical as a good theory. Lewin manages to establish two fields of science in his lifetime.

3.3 Data and methods (normally, <1 page)

This section gets to the specifics of how you aim to answer your question through your research. It must show competence in methodological issues. If you realize when reading this page that you do not know what I am talking about, you have some learning to do.

3.3.1 Hypothetico-deductive research

If you plan experiments or gathering quantitative data, you must describe your research design. How many experiments you plan; what are your independent, dependent, control and intervening variables; how many people you study; how you randomize them; what is your null hypothesis and also alternative hypotheses; what kinds of laboratory procedures you follow; which methods of analysis you use (typically ANOVA, ANCOVA, but usually even t-tests will do), and so forth.

In statistical studies, you need to tell where you get your data - secondary sources, or questionnaires -; what are their main sources of errors; which kind of model you aim to test (again, you need to specify independent, dependent, and so forth variables); which method you use (typically some form of regression analysis or logistical regression); sample size; sample selection; analysis of bias; etc.

3.3.2 Interpretive research

If you build on some interpretive tradition, description has to be extensive as well. You have to tell: which methods are you going to use and how (for example, participant observation for how long, where, and when); how you control for errors and biases; how you gather and process data; how you analyze it; how you plan for validity and reliability, including generalization; and how you tackle ethical problems. You also need to tell how you plan to relate your interpretation to previous literature.

3.3.3 Constructive research

When doing constructive research, this section needs to specify your construct; how you aim to embed into your research; how you gather data; analyze it; what is your "test" or criteria for deciding whether the construct actually does what you intended; etc.

What is particularly important is to decide which scientific tradition you want to follow when doing constructive work. You can experiment (hypothetico-deductive model), but can also be interpretive.

3.4 Technical parts (2-3 pages)

After data and methods, the plan typically goes to technical sections. These are just like in any plan you have written, so I do not waste time for them, but this time you should plan for four years. Typically, this section is fairly short. It has to be realistic, though: it is important in evaluating whether the proposal is realistic and doable.

+ time for doctoral studies

- + Gantt / flowchart of research
- + possible "work packages"
- + budget
- + cooperation
- + ethical questions, etc.

In most cases, the first year ought to be reserved for reading, studies, and specifying the research proposal; the second and the third for research; and the final year for writing. In construtive studies, the second year is typically for construction and for technology, and the third for evaluation.

3.5 References (<1 page)

Finally, references (or literature) closes the plan. It must list **ONLY SOURCES USED IN THE PLAN**; there is no point in adding lists of books you aim to read. This is academic work, after all.

The idea of a research plan is to express your resaerch idea, to communicate it to outsiders, and to halp you to plan it in sufficient detail. Do not use too much literature, but make sure you have read and understood the most important pieces of literature in your field of research, and have placed them into proper use.

Reference are important in evaluation. One of the things any evaluator with any experience does is to check through the references section to see whether the applicant is able to think through his or her problem in relation to existing knowledge. This section is a crucial resource in evaluation, so make sure that it is in good shape.

Use some commonly used style in formating the references. If you do not find anything else, goodle "Chicago Manual of Style" and get instructions for the social sciences or the humanities. A good alternative is ACM style. Do not use APA style, it is minimalistic; it is better to sart with more expansive style.

4. Length

There is no appropriate length to a good plan, but as a rule of thumb, you cannot survive the evaluation process with three pages. On the other hand, writing more than 10 pages probably means that you have written too much, and either repeat yourself, or get too much in details. Typically, plans we accept have about 5-7 pages, spacing 1, 12 pts, plus possibly images.

5. Evaluation schema

Over the last couple of years, I have used the following schema to assist decision making. As you see, it basically studies two things, contribution (point 1), and believability (points 2-4). In additio, it poses a separae question about the quality of art work submitted (essentially, portfolio). Some professors do not follow this, some do.

Decision-making proceeds in two phases. If you fail in Q1, there is little hope with even the best of plans. If Q1 gets 4-5 points, they the rest ought to get on average at least 3 points. Technical problems can be fixed, if the idea is good; the other way round is far more difficult. Always keep in mind that we are takling about doctoral work: everything is evaluated against the best international standard. Being interesting is not enough; when you graduate, you ought to be the best in your field for a couple of years at minimum.

Additional criteris involve things like contradictory premises, contradictions in methodology, etc. These issues inevitably show up in evaluation.

Question	Points 1-5 (5 best)
<i>Relevance.</i> Is the topic important. For which field of design, and how?	
<i>Plan: theory.</i> Is theoretical background solid, and does the plan review literature enough? Is something missing?	
<i>Plan: accuracy.</i> Is the plan accurate enough so that you can evaluate it?	
<i>Plan: execution.</i> Is the plan realistic? Scheduling, tasks, the order of tasks, the applicant's background. (DA requires about 4 years of full-time work)	
<i>Art, if applicable.</i> Is the artistic quality of the plan and artwork high enough? (If there is an artistic component, it has to be good!)	
Sum	=
Free field for other comments:	

6. Main problems in plans

6.1 "Expression of interest" is not a research plan

Over the last two years, a good deal of applications have been what I call "expressions of interest" rather than proper research plans. By this I mean that they are more like applications artists write for foundations to get their art work funded rather than serious analytic work. Since most professors at the SoD are nice people, they typically say that these plans are interesting, and raise interesting and sometimes even important issues, but the plan is too unspecific to be taken seriously - and give you failed, sometimes encouraging you to reapply.

6.2 Question

It is difficult to write a good research question. It has to be specific to be understandable, and to guide your thinking, but simultaneously, it has to be "large" enough to raise your level of ambition to a level required in doctoral work - essentially four years of work. One of the problems in design is that people try to solve too large problems. As a rule, you should not try to work on one concept, but to outline a question that works in the intersection of a few concepts (in Boolean terms, AND operator with 2-4 concepts). Another problem is that research questions are often interesting on practical rather than research terms. Practical problems are for the government and for engineers, not to researchers to solve.

6.3 Lack of theory/perspective

This is the worst problem. Without a perspective even a good researcher only has lay imagination to rely on. Saying anything intelligent without a conceptual frame is hard.

6.4 Lack of specificity

Even if the topic is interesting and relevant, we cannot evaluate whether your plan is realistic and believable, unless you tell exactly: what literature you build on, which you criticize; what is your methodology; details of methods (get specific: who are you going to interview and how you process your data; how many experiments you plan to conduct; how your exhibitions relate to each other, and how they contribute to your research problem; etc.). These details always change when you do your research, but if you ask us to trust you, you're on shifting sands. Research world is all about skepticism, not faith.

6.5 Contradictory premises and methodologies

Doing serious research requires that you learn from countless predecessors. Do not try too much. Perhaps the most crucial problem is methodological: combining hypothetico-deductive and interpretive research styles is basically impossible, unless you know your history of science really well.

6.6 Overblown claims

We have had people who want to take two classes of philosophy at UIAH, and then contribute to philosophy. There is no way to succeed in this effort, unless you have a MA of M.Sc. in philosophy. Or, you cannot solve overpopulation through design.

6.7 Trying too much

One sure way to spoil a plan is to promise too much. Do not try to make a philosophical, theoretical and design statement in the same thesis, and work your way through your problem with qualitative, quantitative, and formal methods simultaneously when you try to make them - using Nigel Cross's horrible word "designerly." You fail. No one can know everything. This is not a place to show off with your reading (and if you are a design student, you are not probably very well educated in science anyway).

7. Comments from the department: yes, but only once

When writing a plan, it is always a good idea to talk to professors at the School of Design. They are there to help you, and have experience that helps you through many difficulties you can't even think about.

However, keep in mind that they are also evaluating your application. Therefore, there is a potential conflict of interest.

To manage this conflict of interest, I have used the following policy. I can discuss with the applicant once before writing the plan begins. Also, I can read and comment the plan, **but only once**, for two reasons. Since I am a part of the evaluation process, getting more involved to the process would implicate me to the process: I cannot evaluate papers that reflect my thoughts. Also, it is not my job to write applications to a doctoral program, this job belongs to you. It is you who have to clarify thinking, not me.

SoD is fairly small, and professors talk to each other, so do not try to shop around for comments from many professors. It may ruin your chances.

7.1 Reapplying

You can always reapply; however, when you are resubmitting, make sure you have learned from critiques you got on the first time.

However, you can count on only 1-2 resubmissions: the idea is not that you submit a bad proposal, get feedback, correct, resubmit, correct again, and so forth. The plan has to be yours, show your abilities; the university won't coach you until it finds your application good. It lurks a conflict of interest here.

7.2 Write your own plan

Our School works on the premise that every research proposal that has been submitted to us is conceived, planned, and written by the applicant. For us, it represents the most important sample of the abilities of the applicant. Nothing predicts success at doctoral work better than a thoughtful, knowledgeable, and well-laid-down plan.

We know that people try to swindle occasionally, and we have certainly accepted people who clearly have not written their own research proposal. Usually, the gap between the abilities of the student and the plan is blatant almost immediately.

Sanctions vary. Naturally, if we learn later that this has been the case, we will discard the student. In cases of doubt, we simply stop helping the student. There is no point in wasting time on students who will not succeed, and even less on students who cut the corners and will use our basic premise to their advantage. If the student is salaried, this is a serious offense, and will lead to cutting the salary off. Even harsher legal actions are possible, if the student has thus stolen a place from a more talented competitor.

8. Evaluation process at the School of Design

This page describes what happens to the application when it gets to the School of Design. The evaluation typically takes about 3-4. Extra time is typically due to the Research Council's meeting schedules. This page describes situation in 2008.

8.1 Process

When we get applications, the process proceeds in these steps.

1. Associate dean (research) gets all applications, reads them through, writes a short summary of the application, and sends them to the best experts at the School. These experts are typically professors, but sometimes, papers are sent to lecturers and researchers who have a doctoral degree.
2. Evaluation phase (typically 3-4 weeks). Professors and others read applications and write evaluations. [Evaluation schema](#) is elsewhere on this site.
3. Associate dean collects all applications and writes recommendations for UIAH's Research Council.

8.2 Some principles

The most important principle in this process is expertise. The best expert available reads the application.

If the plan requires expertise from several fields, or the application is a borderline case (almost but not quite good enough to pass), it goes to two or more evaluators. Typically, this is the case with artistic pieces of work that are evaluated by a team of one researcher and one artist (or a designer).

When associate dean gets the plans and evaluations back to his desk, he does several things: he reads the plans; collects evaluations; evaluates them from the department standpoint; writes feedback statements; and writes a recommendation to Research council.

Evaluating applications from the department standpoint means two things: does the application fit to the School's research agenda of other interests; and are there resources to support the research, or would it be better to apply to another program.