

## **Throwing the Baby Out: Or, Taking Practice Seriously**

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### ***1. Living with Practice but Having an Affair with Intuition***

This paper makes a conservative talk, a plea for reframing artistic research to make it useful as a form of research and interesting enough for those in the scientific community who are not among the most radical postmodernists.

Recent exploration into art and research has produced not just dozens of works that build research on art, but also several attempts to create frameworks for understanding how art and research could contribute to each other. Maybe the best-known category is “practice-based” – or more recently – “practice-led” research, with origins in British academic reforms, most notably in former polytechnics and colleges becoming universities with a research agenda. When the British government decided to open research funding for formed colleges, they had to find a way to frame their work in

such a way that would show what is new and thus worth supporting in artistic research.

The term “practice-based” did this job well, and art schools were quick to adopt the term. It shows artists as practitioners, with hands on mud, in contrast to someone, whom I take to be researchers or scientists, who apparently are not practitioners. I do not take issue with the concept as such, but would like to point out a danger in it. The idea that doing research through practice is something new is naïve not only in terms of sociology of science, which has for the last 40 years shown that even the most formal sciences are based on practice (calculus was invented for studying movement, and probability theory for gambling), but it is also naïve in terms of the history of science. What is economics if not a practical exercise in optimizing human systems that are measurable in terms of money?

However, what comes to research in art schools, these are minor academic problems. The real danger lies in the way in which the notion of practice has been understood. Namely, in cases I know best – doctoral work at the University of Art and Design Helsinki – artistic research builds on what is ultimately a very narrow understanding of what is artistic work. In the 1980s, the methodologist David Silverman used to tease sociologists for romanticism, which he saw in many aspects of then popular cultural studies. Rather than studying what people do, sociologists tended to see meanings, which, as the logic went, were invisible, located in the heads of people. Thus, the true meaning of sexuality was not in sexual action, but in how people thought about it. Or, the meaning of society was in how people saw it, even though, of course, society is everywhere around us, unfolding from one day to another. Ten

years later, Silverman begun to talk about the “interview society” in which the locus of action is routinely places into the inner, invisible layers of people. All of a sudden, we find nothing odd in sport journalists asking the Olympic gold medalist Daley Thompson after he had won decathlon with the world record: “How do you feel now?” His response was typically something like this: “I have never felt worse.” For many viewers, the response felt odd, of at least slightly boyish. However, Thompson’s jocular answer rightly points out that the issue is one of order. After all, which was more important: what he did on the track, or how he felt while doing it? (Atkinson and Silverman 1997).

When I think about recent work at my university, I tend to see the same kind of romanticism at work, even though it is disguised under complicated theoretical thinking, typically philosophy of some sort, but always understood in “subjective” terms. Invariably, the “truth” about the artist’s work is hidden in the deep layers of the mind of the artist rather than in what is observable and tellable. In seminars, I have encountered several times over one of the most efficient conversation stoppers, namely the claim that artistic intuition is something subjective that science cannot describe, or has right not to. To make things even worse, in more than few cases, this ideology has a Nietzschean overtone, as in the case of an older, now retired colleague, who invariably claimed that only artists have access to higher truths science can never grasp. For him, this simple philosophy justified a thorough contempt towards research (and every other thing, except art). In the hands of someone with considerable power, it was a destructive way of thinking.

However, this is precisely what begs the question. It is plainly banal to say that inspiration cannot be captured by science. After Popper, most philosopher of science believe that there is no logic of invention. This is common sense for any journalist and researcher too. The reason for this paper was simple: I was asked to give a talk about practice. In terms of its topic, I decided to tell how I understand practice, and what implications this understanding might have. However, these were easy things. The true difficulty – and possible skill – of my talk does not lie in “inspiration,” but in the argument I am making, which has theoretical roots in sociological tradition called ethnomethodology. Similarly, few of us admire artists because they get ideas in the bus, or while sipping coffee. We admire them for their skill in realizing their art works. I may think that my former colleague’s philosophy was silly and often bordering on being dangerous, but I appreciated his chairs.

I do not think I miss the mark badly when I say that there is lots of romanticism at work in “practice-based research.” Artists live with practice, but their true love is in another town. With few exceptions, the frameworks into which artists relate their work firmly situate artistic research into something subjective. I am using Maarit Mäkelä’s thesis (2003) as an example simply because I think it is an excellent and interesting thesis in many ways. Now, she describes her method as “retrospective gaze,” by which she means that she first did a series of exhibitions, and theorized about them afterwards, tracing the history of their development through her memories and traces of her work. Another concept in her work was autoethnography, with which she meant that she was doing ethnographic-like research for her art works, and autofiction, which meant that in her research, she also created her own stories, basing her artwork on a creative process of ethnographic storytelling. “Retrospective gaze”

and “autofiction” effectively say that no outsider can evaluate her thesis. It works within her inner reality; the art works do stand alone, and can be juried, but the framework cannot. The validity of the thesis depends on internal validity – that is, consistency – alone. Essentially, we have to decide whether we believe her or not, and evaluate her work accordingly. As she can tell, reviews she receives are not typically balanced: they either praise her work, or if they are not outright hateful, they are spiteful.

Michael Biggs’s (2007) criticism of this line of thinking through Wittgenstein’s well-known argument against the possibility of private languages is well taken. As Wittgenstein pointed out, language is a property of a community of speakers, or life form. Therefore, one cannot create a private language that is outside this community. Even a baby’s attempts to communicate are perfectly understandable for its parents. Similarly, an artist cannot create a world of his own that is understandable for him only. Biggs’ advice for artists who want to become researchers is also well taken: they should treat the outcomes of their work just like they would treat the outcomes of someone else’s work. He tells that whenever an artist produces a piece of work, his accounts regarding the work are not privileged in any degree, and should not be treated that way. These accounts are just as good as other accounts, unless they make more sense. If other accounts are more encompassing and clear, they are better.

In addition to the theoretical problem pointed out by Biggs, many practical problems ensue from such romantic stance. How can one validate a thesis if the artist says that the only criterion is in his head, and impossible to put in words? More importantly, how can one learn from a work like this and apply this learning in his work? What

scientists call replication is a word that may sound threatening, but essentially it only says that other people should be able to learn from a thesis so much that they can use its process in their own work. For scientists, this is not just self-evident, but also the cornerstone of anything that claims to be a science – even though there are few agreed-upon criteria for what replication exactly means. These books also build on esoteric frameworks that are just impossibly difficult for non-expert, like pragmatist philosophy. How can students learn these frameworks? Also, there are adversary long-term effects. What if we at some point have 20 Ph.D. theses each celebrating the subjective world of the writer? Researcher number 21 then has to create his framework, without a possibility of learning from previous work. I do not know whether there has to be progress in art, but there surely must be in research – or else, the scientific world cannot find good reasons for support artistic research beyond a few trials. Risk-taking is accepted and encouraged in science, but for a good reason. However, too much risk-taking seldom leads to useful results.

## ***2. Willie's Story, or What If We Take Practice Seriously?***

One alternative – and I mean only that – is to take artistic practice seriously and use it a basis for research. This is not a radical idea. It simply says that if we take a skilled practice – which good art always is even if we can't put it into words that go beyond platitudes – there must be things worth learning from in this practice. Modern art in practically any form also tends to be conceptually radical, exploring ideas and issues that are not necessarily of much interest to established fields of research. Is there a science of “boredom”? Could there be? Sure, and there probably is, but it has remained small, and for a reason. It would be far easier to explore boredom through

artistic means than to try to justify a study of the theology (psychology, physics...) of boredom.

I promise to support any artist who takes his or her practice seriously and tells the story of that practice. After all, if an artists' work is practice-based – or practice-led in more recent jargon – then it cannot be that much different from any skilled practice. The sociologist Douglas Harper (1987) once wrote a beautiful book called *Working Knowledge*, which dealt with Willie the blacksmith, living in upstate New York. Willie was an old school mechanic from simpler times, specializing in fixing tractors, heaters, ovens, and Saab cars. In his backyard, there were dozens of car wrecks that he got from people living nearby. For him, they were an essential source of spare parts. Harper got to know Willie when he bought a Saab. In his book, he described many of Willie's "fixes," describing in detail how Willie worked through his problems, discussing, thinking, trying, welding, cutting, and finishing his metal works.

Why not study art like Harper studied Willie? By watching closely. Describing. Learning. Seeing how "fixes" are made. How ideas evolve.

This would not be a major step – all that is required that an artist opens his practice for analytic scrutiny. If even the most sophisticated forms of science have been analyzed just like any other work, perfectly well researchable by sensitive, trained researchers, why not art?

Ultimately, then, the question is not whether one can study art, but how to study it. Sociologists of science have done one thing few artists so far have been willing to do: they took a humble approach, went to laboratories and other places of discovery, started to take photographs, gather documents, write down what they saw, and pose questions for scientists. One may try to counter this argument by saying that science takes place in observable settings like laboratories (see, among others, Latour and Woolgar 1979), while the creative process of an artist is ubiquitous. Sure, discoveries are made in laboratories, though not exclusively – but then again, most artistic discoveries also have a place, the studio. And to say that scientists only come up with new ideas in the lab would be just as silly as it would be to say that artists only come up with ideas in the studio.

The key methodological trick in sociology of science – just like in Harper’s study – was description. Whenever one is putting complex practice into words, this is possible only through hard work, worthy a try in itself. But the problem of practice escaping language is in no way unique to art. Try to explain how people walk, talk, or orient their gaze during a seminar to learn that the problem is in fact ubiquitous.

### **3. *Ways of the Hand***

If someone thinks that it is a sacrilege to draw an analogy between artists and a journeyman in the countryside in upstate New York, I want to point out another study in which practice was made into a topic of research, not something that is only interesting because of some deeper, underling meaning. In *Ways of the Hand*, the late sociologist David Sudnow tells the story of how he learned to play piano and to

improvise jazz solos with it over the course of five years (Sudnow 1993). Sudnow had learned some piano in his childhood, but had given up playing after getting bored with classical music. In his mid-career, he decided to relearn playing. However, he did not go back to classical music, but wanted to learn to improvise jazz instead – and not just any jazz, but bebop, which is one of the hardest area of jazz to learn, requiring technical brilliance because of its (typically) high tempo.

To cut the story short, Sudnow's book describes those roughly five years during which he first just tried to remember keys and scales, then learned to hear song through chords suitable for brief improvisations, and finally learned to foresee melodies in the making. It is important to realize that although you can learn jazz through theory, ultimately the only important thing is what the fingers and hands are doing on the keyboard. Knowing theory does not help, if one's fingers do not find their way to places that sound good. Transcribing Bud Powell's or Jimmy Rowles's solos won't help either, unless one's fingers reach the right keys. As Sudnow says, music is in ways of the hand, not in notes. The important thing to remember is that the ways in which the hand moves over the keyboard are meaningful up to the smallest detail: every move counts. Waving one's hands over the piano and touching keys is not music.

Of course, Sudnow never got to be as good as McCoy Tyner or Chick Corea, but he gave up his academic career for two decades and lived as a musician and piano teacher, before turning back to sociology at the final years of his life. His wasn't a bad move personally either. Actually, his friends in the Bay Area have told me that he was a happy man until his death at old age a few years ago.

Now, the problem of David Sudnow's book is that it can only be described as esoteric. It is well written in eloquent and simple English, but still a book that is practically impossible to read because the only way to make sense of his argument is to sit by the piano and try out his examples. I used to play guitar and other string instruments when I was younger, and was pretty good in reading musical notation, but doing Sudnow's exercises with a guitar was not enough. When I worked my way through his book – which is something I have never done in detail, his examples are too difficult for me – I had to sit by a piano myself, and try to understand his finger positions and the logic of movement of my hands. No hope, of course, but this exercise showed me just how difficult it is to be a skilled practitioner. Think about the flowing utterance by a native 5-year French girl, and contrast this to a foreigner's desperate attempt to understand and produce a correct subjunctive form.

Why not *Ways of the Hand* in fashion design? Or exhibition design? CAD modeling? The practice may have its artistic aspects, but if something as difficult as music can be studied in detail by a sensitive researcher like Sudnow, it should be far easier to study something that is just as skilled in terms of technique (from sewing to patterning etc.) and that typically is a team exercise in which everything crucial is made visible and understandable for other members of the team through sketches, mock-ups, models, and so forth anyway. The practice is there, just like sounds coming from a jazz band, but sounds are probably more difficult to understand than the types of cloth, texture, patterns, or folds.

What if we follow Sudnow, and start to study art by close observations and careful descriptions of practice in its full complexity? Wonderful tools for such description exist in many ethnographic research traditions, as well as in various ways in analyzing interaction. All we have to do is to keep in mind that not only art can be studied, though not through current, mystifying frameworks, but also by smartly selecting the target, one can shed light to many problems of interest not just to sociologists, but also to the public (as Sudnow's teaching practice proves), and for other artists as well.

I would love to read close, well-documented and visualized accounts of how a goldsmith uses his sense of touch to understand almost microscopic shapes and patterns, zooms into them with a loop, applies heat to the material to be able to work on it, and so forth. Or how a fashion designer draws sketches systematically to discover interesting themes, to develop them further, do make decisions (how one drops something that does not work, or chooses to keep something), and to finally build a complete line of clothing from these early sketches.

I am confident that such work would be useful as teaching material. It would also be useful in promoting the art fields into wider attention. It would also delight many artists. It would also stir debate: a well written, coherent study is typically one that is also easy to understand and by implication, easy to criticize. As the sociologist Howard S. Becker has noted (1986), clarity of expression obeys the good old rule in research by identifying negative evidence. In fact, this is one of the counter-intuitive things about science for non-scientists: the best research is so clear that it is easy to disagree with it. Bad research, on the other hand, is often impossible to falsify. Just think about parapsychology. Also, research is all about debate. No piece of research is

complete in itself; any piece of research is treated as an argument in some debate. The measure of success for any piece of research can only be how much it, despite its problems, adds to our understanding.

And I firmly believe that artistic research would definitively benefit from seriously rethinking some of the clearly troublesome conceptual practices that have been built into the field. Good artistic research should not be built to please anyone. Actually, good pieces of research typically do not please people in power.

#### ***4. Throw Out the Bathwater***

My observations above take me to my last point. The main danger I see in making art into something esoteric, something beyond logic, language and research, is perhaps finally ethical. If we pick up only one aspect of art (and by implication design), its artistic aspect, we severely paint a partial picture of art through research. Clearly, we decontextualize –I am sorry for this awkward term – art from practice. Furthermore, if we do not stop here, but accept artists' self-serving conceptual subjectivist frameworks as the only legitimate account of art, we get into a situation in which we not just analytically break art into parts while simultaneously criticizing such analytic breakdowns, but also add another bias into our research practice. We misrepresent art in two ways. The result is the problem from which I started this paper: artistic research in the worst mode cannot have legitimacy for long in the eyes of the academia.

I have tried to argue in this paper that there is an alternative way. It is difficult, but negotiable. What makes art an interesting area for research is that in art, one can pose questions much more freely than in old scientific disciplines in which one of the main indications of competence is one's ability to pose questions that identify problems in existing body of research. Artists, on the other hand, are able to pose new types of question. Simply, art could offer was to explore social (and other) problems in ways established fields of research cannot. It is only in this sense only that I can to accept my ex-colleague's insistent point that art can reach something science cannot. My plea is simply: throw out the romantic bathwater and focus on nurturing practice.

No doubt, this would require taking practice seriously. Practice needs to be broken down, understood as experimental work which typically has a conceptual basis, but which is ultimately observable and reportable. If practice is understood only as a reflection of one's inner world, which is ultimately unreachable to other people (except perhaps other artists), then one falls into the trap my title tries to capture: the baby is thrown out with the bathwater. If a romantic stance is given Nietzschean overtones, it rightfully raises opposition and disdain from people well versed in research. Artists are certainly not immune to chauvinism, to seeing the audience as "squares," as someone who only have to be withstood only because one must pay his bills. (My terminology is from another study by Howard Becker, whose Master's Thesis at the University of Chicago (1951) dealt with jazz musicians who, indeed, despised their audience because it was not able to appreciate music they wanted to play. Incidentally, Becker later became Douglas Harper's teacher).

I opened this paper by saying that my talk will be a conservative plea for making art in research into something useful for other researchers, artists, teachers, and society's institutions alike. My understanding of the current state of artistic research – and I prefer this term to “practice-based,” which I find misleading and rhetorically dangerous – needs serious rethinking in order not to become just another, useless and fairly expensive experiment prompting cynicism to artistic research among its key audiences, artists and researchers alike. I feel that the romantic model for doing artistic research is in a deadlock, and unless it changes its direction, very little remains in ten years. It is not enough to have a few artists on the job market with the coveted Ph.D. – or with less well-known Doctor of Arts that, still, is a doctoral degree – on their calling cards trying in vain to convince the scientific world about the value of their research, or the art world about the value of their art work.

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