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A continuum of measures of validity for research in the making fields

Abstract

This article supports doctorates in the “making fields” by mapping them onto a continuum of research typologies from quantitative methods to fictive constructions. It furthers the work of Fraying, et al., in expanding doctoral-level research beyond the traditional scientific-experimental approach. The paper first addresses historical reasons for supporting an expanded definition for the scope of doctoral work. It then elaborates on how the proposed continuum allows for a doctoral community to recognize a spectrum of different measures of research validity, specifically to include dissertations in the making fields.

Keywords: Making doctorates, continuum of research, validity.

Introduction

Doctoral programs in the “making fields” always stretch my imagination for the scope of design research. My engagements with these programs are in Scandinavia, where Professors Halina Dunin-Woyseth, Fredrik Nilsson, and others, have led the way in forging expanded frameworks for doctoral education in design-related fields such as architecture, urban and industrial design, even in the broad arena of arts and crafts (e.g. Nilsson, Dunin-Woyseth, & Janssens, 2017). Their work participates in broader European efforts to make doctoral validity clearly recognizable across varying domains of knowledge and practice and, perhaps more importantly, across the wide swathe of member nations in the Bologna Process (currently 48 members in the European Higher Education Area). I know of nothing like this in other parts of the world where I lecture on design research: the United States and China. This is understandable given the single-governance nature of these large nations. But as globalization erodes national boundaries, European leadership in developing recognizable standards for “doctorateness” across nations may have increasing appeal worldwide. This is one reason why I hope the present article can contribute to this larger conversation.

One challenge is how dissertations in the making fields meet recognizable measures of robustness in PhD-level research. This question arose while I served as first opponent for the doctoral defense of Anne Solberg, the inaugural candidate awarded the PhD in Cultural Studies at the University College of Southeast Norway in spring of 2017. While Dr. Solberg’s dissertation did not itself involve production of objects of art or craft, her research *question* asked what constitutes “doctorateness” in nine dissertations that focused on making. These cases included projects ranging from collaborative fashion design, to etching metals with biological matter, to sculptures made with paper (Solberg, 2017). While some of Dr. Solberg’s conclusions could have been stronger, it was answering for myself in what *way* they could be stronger that prompted my thoughts summarized in this article.

I am grateful to the editors of *FormAkademisk* for inviting me to share my observations arising out of Dr. Solberg's work. While her dissertation is necessarily a single research project, I see it as an appropriate gateway to discussing larger issues of research methodology for making doctorates in general, given her choice of sample cases.

In what follows, I first suggest that doctoral education of any kind should be situated in the historical continuity of Western civilization's recognition of the doctoral degree through the centuries. On this view, whatever else "doctorateness" may entail, it involves the candidate being received into a *community* of scholars who recognize his or her work as the highest level of knowledge in its field. The emphasis is not only on the highest level of knowledge; the emphasis is also on the community. In other words, while scholars in one domain may not possess knowledge of the particulars of someone else's domain, the general *contours* of doctoral robustness in that other domain should be communally understandable. Why? Because doctoral rigor itself bears a "family resemblance" recognizable to the doctoral community at large.

This leads to the second section: I propose a continuum of measures for *validity* as a way of recognizing the family resemblance of doctoral research. Validity is an enduring standard for research quality. It has to do with the logic of research designs. As a simple example: if a researcher claims that "everyone who eats ice cream eventually dies," we know something is quite wrong with the research design, because people who *don't* eat ice cream eventually die as well! This is a problem of internal validity: something about the inherent logic of the research does not jibe with the reality it hopes to explain. Moreover, if we cannot replicate a researcher's outcomes in other venues with similar constraints, this is a problem of external validity: something about the logic of the research design renders it non-replicable. Doctoral dissertations in the making fields confront particular challenges with internal and external validity. Why must human freedom in producing unique works of art even need to answer to measures of internal validity? And why should one-of-a-kind works of art be replicable? My hope is that the continuum model I propose can begin to address these concerns.

Let me say that the ideas I propose here resonate with the report by Frayling, et al., in their *Practice-Based Doctorates in the Creative and Performing Arts and Design*. First, in the conclusion of their report, Frayling and his colleagues emphasize community affirmation as the first measure of doctoral standing:

The award of a Ph.D. admits the bearer to a community of scholars... Whatever form the product of research in a creative project takes, its originator should not be excluded from seeking to demonstrate doctoral achievement and hence admission into that community. (Frayling et al., 1997, p. 8)

Here, I add some historical justification to this point of view. I also consider what "community" means in finer grain, namely, a doctoral community should by nature be interdisciplinary. Second, the Frayling report recognized that, while scientific method is the "gold standard" of research, it can nevertheless be located at "one end of a continuum" (Frayling, et al., 1997, p. 8). Further:

There is already a continuum from scientific research to creative practice. What is needed is a set of nationally agreed definitions of standards for the award of doctorates ... framed in such a way that they are sufficiently rigorous ... but sufficiently inclusive to allow all subjects to find expression in them. (Frayling, et al., 1997, p. 15)

The continuum model proposed here may be in line with what Frayling and his colleagues had in mind. I suggest more detail for the varying attributes of research validity in a continuum of methods moving from quantitative to qualitative. For instance, my suggestion that a researcher becomes an *activist* in the making disciplines seems to contribute more detail to Frayling's continuum at the qualitative end. As well, my inclusion of even fictive constructions into the domain of research may even extend existing notions of a continuum of research at the doctoral level.

The historical, social, and *communal* roots of the doctoral degree

Standard historiography tells us that the collapse of the Roman Empire in the fifth century CE left a cultural vacuum. While we no longer tend to call the immediate subsequent centuries 'the dark ages,' it did take some time before dedicated centers of learning emerged to lead the way towards cultural renaissance. When these did appear, it was largely thanks to ecclesial structures, such as monastic centers, which preserved the learning of the ancient world.

Monasteries encouraged literacy, promoted learning, and preserved the classics of ancient literature, including the works of Cicero, Virgil, Ovid, and Aristotle. To beautify the celebration of the liturgy, monastic composers enriched the scope and sophistication of choral music, and to create the best environment for devotion, monasticism developed a close and fruitful partnership with the visual arts. The need for books and buildings made religious houses active patrons of the arts, and the monastic obligation to perform manual work allowed many monks and nuns to serve God as creative artists. (Sorabella, 2013).

Noteworthy in this citation is the emphasis on art and creativity. The beginnings of doctoral learning did not entail 'scientific' knowledge as we understand the term today; I will return to this point. Suffice it for now to say that it is difficult for us moderns to imagine the vast stretches of uncultivated land between scattered centers of learning in those days. By uncultivated, I mean not only the absence of agriculture; I mean the absence of civilization. These isolated monastic centers retained the knowledge of the ancient world, and produced new knowledge. We should note the irony. Monasticism aimed at withdrawal from this world for a better one; but it was out of this withdrawal that knowledge of the present world began to thrive. This thriving produced a community of learning that contributed to the Carolingian renaissance of the ninth century, and to the flowering of the renaissance of the eleventh and twelfth centuries. Under Charlemagne's rule, the *Admonitio Generalis* of 789 attached schools to monasteries as a matter of policy (McKitterick, 2005, p. 153). Later, royal decrees encouraged the formation of communities of scholars as cathedral schools; and so began some of the earliest universities. The University of Paris, the cathedral school attached to Notre Dame, first awarded the doctoral degree in the twelfth century.

Western architecture is thanks in part to this impetus for knowledge and community. I am thinking of the typology that drew from the ancient Greek agora and Roman forum, and transmits it down to us in the forms of the cloister, the courtyard, the piazza, the town square and the public green (See Zucker, 1970). Even today's notion of a "community center" draws from the *sense* of community, understood as a mutual sharing of knowledge among like-minded participants, first reflected in the agora and forum. Consider the well-known plan of the St. Gall monastery, dating from the eighth century. Detailed plans of this idealized complex are readily available on the World Wide Web (e.g. Carolingian, Culture at Reichenau & St. Gall, n.d.). In contrast to the large stretches of wilderness that was Merovingian France, *within* the walls of this compound we see all

the signatures of community: organization, functional distinctions, inside-outside spaces for participatory exchange, hierarchical forms accessed by clear circulation.

My point is that the emergence of doctoral degrees intimately related to the creation of community, not only in the sense of scholarly community, but in the larger sense of human community. As Western civilization emerged from the remains of the old Roman Empire, learning – much of it in the form of new speculative and philosophical knowledge – led the way back to vibrant social structures. In this process, those individuals with the highest levels of learning earned the honor of doctoral standing. My view is that “doctorateness” today must retain this basis in community. In other words, whatever qualities doctoral degrees must possess inherently, they should not only be recognizable communally in an external sense, but also *contribute* to the betterment of that broader human community.

Of course, it is true that cultural shifts have been enormous since the twelve century. For example, the highest mode of knowledge in those early centuries was theology. In our day, theology does not enjoy nearly the same standing. In our day, it is learning rooted in the scientific method that receives the highest accolades. The trajectory of this change, of course, has its own history. In a research seminar I gave to doctoral students at the University College of Southeast Norway in spring of 2017 (which took place in conjunction with Dr. Solberg’s doctoral disputation), I tracked some of this trajectory through the ideas of such familiar thinkers as Francis Bacon (1561-1626) and Rene Descartes (1596-1650), reviewing their respective contributions to what we now call scientific method. Space does not allow a summary here, but the PowerPoint file from that seminar is available at this URL (Wang, 2017). Three points from that seminar particularly pertain to this article, and these I summarize as follows.

First, as already noted, early beginnings of the doctoral degree did not focus on “scientific” research, for the simple reason that such a notion did not yet exist in the form we currently understand it. This fact, added to the reality that concentrations in domain expertise do change over the centuries (as, again, in the case of theology), is more than sufficient ground to consider awarding doctorates in the making fields now, provided that measures of rigor for such degrees are communally recognizable. The continuum model outlined in the next section is in answer to this need.

Second, we must recognize the powerful hegemony of scientific method since Descartes, particularly since the Industrial Revolution of the eighteenth century. Earlier I mentioned that we moderns cannot easily imagine the cultural destitution in Europe, geographically measured, in the centuries just after the collapse of the Roman Empire. In the same way, we moderns cannot easily imagine a world not *completely* shaped by the products of scientific research. For instance, it is an irony to me that, at least in the United States, some of the most militant activists against technological civilization – for instance, radical environmentalists – communicate their militancy on the latest Apple iPhones. This is no pique against environmentalists. This is just an example of how unaware we all are of our dependence on technology and, by extension, on scientific method. I suggest the following. Scientific method is so influential for no other reason than that it *works*. It *does* improve quality of human life: in communications, in medicine, in travel, in thermal comfort, in just about everything. We must respect this hegemony rather than rebel against it. Some of Dr. Solberg’s case studies, for example, explicitly rejected standard measures of research quality (measures generally rooted scientific method) in justifying new modalities of research. One case held that “tried-and-true procedures are entirely out of place” for her work (Solberg, 2017, p. 189).¹ Another held that “there are no fixed research methods in (his) research and no traditions in the new doctoral program he is following.” (Solberg, 2017, p. 177). In my

view, this is like climbing up a ladder and cutting off the rungs under you while you go. It does not seem logical in light of the doctoral tradition I briefly surveyed above. At least it does not seem *constructive*, if I may add to the ladder analogy. The continuum model outlined in the next section situates “making” doctorates in ways that expand measures in scientific method, while staying within this tradition. This allows for broader communal recognition of the findings that come out of “making” doctorates. This approach, I want to note, is not a concession to scientific method as *the* more superior mode of knowing. Quite to the contrary, by accommodating a wide range of knowledge production under the doctoral banner, the continuum simply recognizes that, originally, scientific method itself was not the basis of the doctoral degree. In this way, the continuum model returns high-level academic learning to its original moorings in perhaps a more aesthetic, as opposed to a “merely” scientific, view of human knowing in general.

Third, one way to regard ideas of the twentieth century is that they largely represent a swing of the pendulum *away* from scientific method. This also portends well for alternative types of doctoral degrees. Yes, the Logical Positivists in the earlier decades of the century sought to anchor all knowledge in scientific verification. But this outlook gave way to other streams of thought all of which pushed against the verities of scientific certainty. By the end of the twentieth century, the “covering law” outlook of Logical Positivism, to wit, that all of experience can be subsumed under a single predictive scientific model, became a thing of the past. Let me very briefly summarize some of these alternative streams of thought. The structuralist linguistics of Ferdinand de Saussure (1857-1913) questioned the accuracy of language, something that scientific method inherently assumes. Without Saussure, none of the structuralist and post-structuralist outlooks of the later twentieth century would be possible. Concomitantly, Ludwig Wittgenstein (1889-1951) influentially taught that many philosophical problems inherited from the past were merely matters of language. Change the rules of language, and the problems go away. This was also a critique of the presumed scientific precision of language. While all of this was going on, Edmund Husserl’s (1859-1938) phenomenology rejected the Cartesian dichotomy between *res cogitans* and *res extensa*. Husserl’s doctrine of the intentionality of consciousness reunited Descartes’ bifurcated domains, a reunion that opened the way for a pre-propositional – which is to say, a pre-scientific – way of knowing the world. For all of Martin Heidegger’s (1889-1976) later departures from his early dependence on Husserl, his phenomenology largely carried Husserl’s project of union between subject and object further. Yet another thread in twentieth century ideas was Jacques Lacan’s (1901-1981) psychoanalytical philosophy, building on Sigmund Freud’s theories of human psychological states; this also was a shift away from scientific method. And then there is critical theory. This influential method of critique, beginning with Max Horkheimer (1895-1973) and the Frankfurt School, came out of efforts to preserve the ideas of Karl Marx (1818-1883). After the horrors of the First World War, and the subsequent rise of Stalin and Hitler, initial euphoria over perceived fulfillments of Marx’s socialist vision (for instance, the 1917 Russian Revolution) eroded. Critical theory is the legacy of ideas that sought to preserve the framework of Marxist idealism while accommodating its apparent lack of predictive (read: scientific) power (See Held, 2004). Much of today’s academic architectural theory have roots in this “criticalist” outlook, largely through an anti-capitalist lens.²

All of this is to say that the time seems ripe, from a historical point of view, to accommodate modes of inquiry that are not limited to the constraints of scientific method. At the same time, also based upon history, the powers of scientific method have been so influential that it is reasonable to accommodate them in measures of rigor for the highest levels of these alternative modes of learning. The continuum I propose meets this criterion, and I turn my attention to it now.

A continuum of measures of validity for research in the making fields

Some years ago, I published “Prediction in *Theoria*: towards an interdisciplinary range of theories related to architecture” (Wang, 2006). I based my argument in that paper on an idea coming to us from an even more distant past than the High Middle Ages. This was the Classical Greek notion of *theoria*, from where we get our word theory. The Greek term derives from the practice of dispatching emissaries – called *theoroi* - to various city states for the purpose of bringing back news to report in their home states. The goal was to attain a more generalized knowledge of events across a broad region. Over the years, the travel component of this endeavor fell away, and the project of developing generalized knowledge about things, distinguishable from localized, perhaps more private, points of view, came to be regarded as *theoria*.³ My point in that paper was as follows: while this kind of general knowledge was clearly not *scientific* knowledge, if by this term we mean knowledge generated by numerical rules with a view to establish strict causality, *there remained an element of prediction* in that non-scientific knowledge. And so I devised a spectrum of different kinds of prediction, indexed to different kinds of *theoria*:

For any theory, the element of prediction must indeed reside at its core. But it is in the morphing of the nature of prediction across a range of theory typologies, in turn affecting how the measures of internal/external validity and generalizability also morph, that makes mapping a wide range of theories on a single spectrum of prediction possible. (Wang, 2006)

The continuum model I propose here builds on this idea. Rather than limit the morphing to the single rubric of predictability, the above citation alludes to internal and external validity in general, in how they themselves might morph across different modalities of research, considered through the standard menu of parameters defined by scientific method. Consider Figure 1.

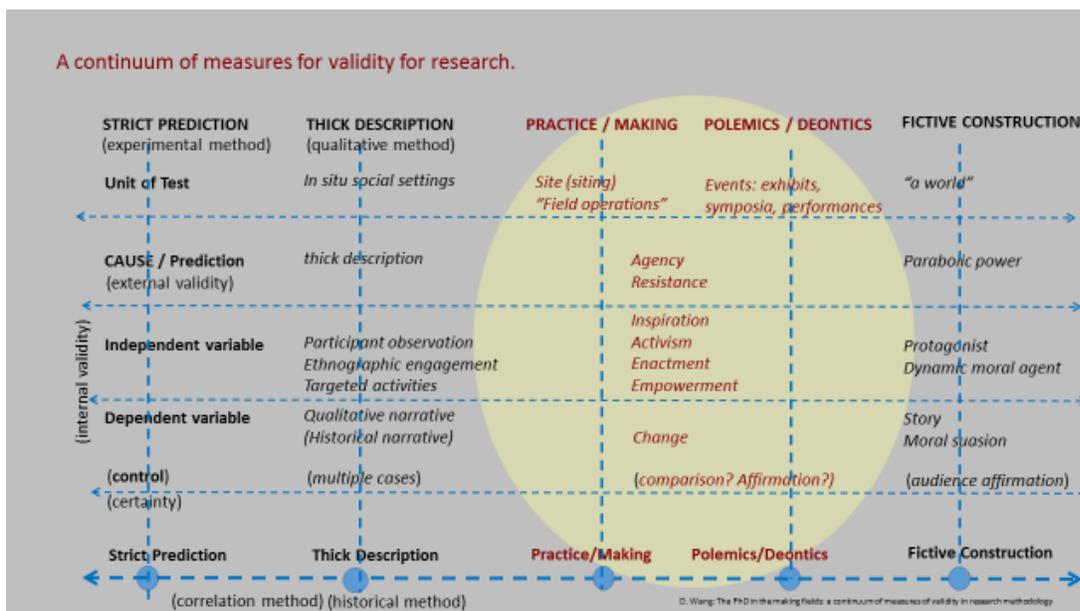


Figure 1. A continuum of measures for research validity. Diagram by David Wang.

The continuum begins at the left column, labeled “strict prediction.” Strict prediction is a term I used in the earlier paper to denote the parameters of scientific method (Figure 1 calls it experimental method, which amounts to the same thing). Even beginning students of research methods should recognize these standard parameters: a unit of test, causality, the independent and dependent variables; and in instances of true (as opposed to quasi-) experimentation, we need a control. I refer readers to the chapter on experimental research in Linda Groat’s and my *Architectural Research Methods*, second edition, where these parameters are explained (Groat and Wang, 2013). Suffice it to say that these parameters comprise the fundamental structure of scientific method. This is the “one end of a continuum” alluded to by Frayling et al. (See 1997, p. 8). My task here is to stretch these parameters across a continuum of research typologies to such an extent that even the column on the far right, “fictive construction,” is included in the domain of research methodology, *all considered through an extruded lens of experimental research*. Of course, by the time we get to fictive construction on the right end of the continuum, *nobody* would consent to the claim that a novel (for instance) can serve as an example of scientific method. That idea would be ridiculous. And yet ... and yet ...

I see a resonance between the unit of test in experimental research and the world that a novelist creates, say, the world of an *Anna Karenina*, or the world of a *Huckleberry Finn*, or the world of *A Tale of Two Cities*. Let me explain.

Units of test in experimental research are purposefully exclusive. Indeed, one caution about experimental research is making sure that there are no hidden variables within a unit of test. For example, when someone says his research proves that all who eat ice cream eventually die, let’s just say there are hidden variables in his unit of test he has not accounted for. This leads directly to the worry of whether or not the conditions tested in the laboratory – units of test in experimental research are by nature *reductive* in relation to the real world – can actually behave in the same way in the real world.

In contrast, the world of a good novel, when considered as a unit of test, is purposefully *inclusive*; and not only inclusive, but aesthetically so deep that the reader feels like he or she participates in the very workings of that world. Why is *Anna Karenina* a classic, and read by a global readership? Well, because Tolstoy has created for us such a “real world” unit of test, and this world so engrosses us that “we just couldn’t put the book down.” We all say this compliment when a novel gives us much pleasure. We couldn’t put the book down because we are unwilling to leave its fictional world; we feel so included in the unit of test that we became one with it. In contrast, there are many, many novels with units of test (their fictional worlds) that are so impoverished we have trouble finishing these books. The plots are stock; the characters are not believable; the storylines are contrived. In the United States, we call these “trash novels” (I don’t know if Europeans also use this term).

Does this mean we give anyone who has written a *good* novel a PhD? I don’t think so. I don’t think so because there are so many other cultural factors involved in how we evaluate the quality of profound aesthetic productions. A PhD should not be the be-all and end-all of *every* intellectual accomplishment in our richly dense cultural tapestry. On the other hand, if someone proposed that we award Leo Tolstoy (or Mark Twain, or Charles Dickens) a PhD posthumously, I for one would not object. Within the complexity of how cultures evaluate aesthetic worth, among the *kinds* of honors awarded in recognition of aesthetic achievement *can* be a doctoral degree. We see this in the form of “honorary doctorates.” For example, universities often award the *Doctor of Humane Letters*, which explicitly honors those who have achieved distinction in areas outside the sciences. A broad community deems a recipient worthy of such an honor, even though he or she

may not have gone through the institutional rigors of a doctoral program. Japan provides us with an alternative solution. Japan awards the title of *Living National Treasure* to individuals who are invariably artists: ceramicists, papermakers, Kabuki actors, sword makers, so on. These individuals have enormous social standing; they just don't have PhDs, at least not as a requirement for such prominence. To return to our continuum:

Consider now the correlates of the other parameters of experimental research, on the left, with their resonating parameters under fictive construction, on the right. To begin: the independent variable. In experimental research, the independent variable is the manipulated variable that activates the unit of test in the search for causality. Imagine we are testing a structure for earthquake resistance by placing it on a vibrating platform. Our manipulation of the vibrations constitutes the independent variable(s); while measures of consequential damage to various components of our structure constitute the dependent variable(s). In fictive construction, the protagonist resonates with the independent variable. This individual instigates changes in the world of the novel, and perhaps we can consider the consequences to be dependent variables. Admittedly, factors other than the protagonist also instigate change; my purpose is not to *limit* -- in any "scientific" way -- the independent variable role to the protagonist; my purpose is to argue for certain resonances between experimental research and the construction of fictive worlds, as also a kind of research. In creating the world of the novel, the author "manipulates" the narrative with a host of factors that resonate with the independent variable in experimental research; these are often centered around the protagonist.

The other parameters follow suit. In fictive construction, rather than identify causality, the novelist creates *parables*. Huckleberry Finn's journey with the Negro Jim along the Mississippi River becomes a parable for all of our life journeys. In spite of social pressure, Huck decides *against* betraying his friend, the runaway slave Jim. In this way, Twain asks each one of us: will you succumb to social pressure in relation to X; or will you do the right thing? Huck's experience becomes a parable of our experiences. *The parabolic power of fictional worlds is their external validity*. In similar fashion, Anna Karenina's life becomes a parable of all of our lives. Do her struggles and foibles look like ours? Well, to some extent yes; to some extent no. Does her world look like ours? Well, no. But, well, yes; yes, very much so. In this aesthetic-literary way, the *causality* of experimentation – that is, the force that prompts an "objective" response in the physical world in scientific method – becomes the *parable* of fictive construction; the force that prompts feelings of subjective identity in those who participate in that parabolic world. Tolstoy, Twain, Dickens; these writers did a lot of research in order to create their parabolic worlds.

Notice what happens. The contours of generalizability, which is to say, the extent to which external validity has force, shifts from the physical world to the social world as we move from left to right on the continuum. Drop two balls off the tower in Pisa and we learn how gravity behaves in Oslo, in New York, and, by induction, on Mars. This is *physical* confirmation of the robustness of the theory of gravity. *Anna Karenina* also affects response across an enormous domain. The difference is that this domain stretches across a human *subjective* audience encompassing the social cultural world, broadly defined. A classic novel, then, has universal powers of *moral suasion*; this is its external validity. Trash novels do not have this qualitative force demonstrating their external validity.

I have thus far addressed the two ends of the continuum in Figure 1, and this for good reason. If experimental research and fictive construction can co-exist in one continuum, surely the breadth of the continuum can accommodate research in the "making" fields. The two "making" categories are in the light-colored oval in Figure 1. Before addressing them explicitly, we should

consider the Thick Description column just to the right of Experimental Research because this further sets the stage for the parameters under the making columns.

Thick description relates to the large domain of qualitative research. Here, the unit of test is not a reductive setting in a laboratory. Nor is it a fictional world. The unit of test in qualitative research is a contemporary context, *in situ*. For example, in a recent doctoral dissertation, Isil Oygur conducted qualitative ethnographies in six design offices, two in industrial design, two in architecture, and two in interaction design, by living in context in these offices, each for weeks at a time. Her question concerned how designers translate information provided by their clients in the design process (Oygur, 2012). This is standard qualitative research. She was not looking for causality, as experimental research would have it, she was after thick description. Thick description denotes analyzing a qualitative phenomenon from multiple perspectives to assure a comprehensive reporting of the cultural case. Hence, for Dr. Oygur's research question, ethnographic research *in situ* was the correct approach. Why does thick description resonate with causality in experimental research? Because a comprehensive qualitative accounting is a more accurate accounting, so that encounters with similar sets of circumstances will yield similar (qualitative) findings. It goes to both internal as well as external validity. The predictive value is not strict prediction; it is thick description. The latter resonates with the former, and placing these in their respective locations on the continuum makes their relationship clearer.

Note now that the independent variable of experimental method morphs into such practices as participant observation in qualitative method. Participant observation is a standard term these days, but we rarely appreciate what makes it philosophically interesting from a research point of view. Classic scientific method places the scientist, as observer, at a remove from the object he or she is researching. This preserves the "objectivity" of experimental research. In qualitative research, the researcher is still an observer, but he or she is now a *participant*. The outcome of the qualitative researcher's work therefore *includes* his or her perspective. Thus, the dependent variable of experimental method morphs into the qualitative narrative. Those who do history research are probably familiar with Arthur Danto's notion of the *narrative sentence* (1985, p. 152). The narrative sentence constructs history through the eyes of the history researcher. Put another way, someone else using the same data will narrate the events differently. This applies not only to history research; it applies to many varieties of qualitative work. So, while thick description relates to the density of the qualitative researcher's tactics, the qualitative narrative contains the "outcomes" – that is, the dependent variables – of the research.

As we move towards considering the parameters under the making columns of the continuum, what I want to emphasize first and foremost is the ever-increasing role of the researcher as a *participant* in the outcomes of the research. What I mean is that the researcher becomes an *activist*, and this role has strong connections to critical theory, alluded to earlier. Activism relates to the parameters in the light-colored oval in Figure 1; I label these columns Practice/Making and Polemics/Deontics. Many cases of the making doctorates in Dr. Solberg's dissertation display the parameters listed here. Consider the unit of test, which under these columns become exhibitions, symposia, or "field operations." One of Solberg's cases involved collaborative fashion design with non-professional participants (that is, with consumers), using non-traditional materials, held in "Pro Am" events as far afield as Istanbul (See Solberg, 2017, pp. 104-105, 161-164). This is something of a field operation. Another of Solberg's cases was one in which the candidate conducted collaborative art productions in three locales: a school, a hospital and a church, with subsequent critical reflection from the participants (Solberg, 2017, pp. 101-102, 120-123). These

might fall under the heading of symposia. My point is that we can assess the internal validity of these events *in terms of units of test*. How robust are these units of test?

Well, we can measure their robustness by the other parameters under these making columns, all of which are extensions of scientific parameters. The causal element becomes agency (see again Figure 1). There is quite a bit of recent theory in the social sciences on the opposition between “structure,” on the one hand, and “agency,” on the other (e.g. Hays, 1994). Structure denotes the social constraints of a given cultural setting. Agency denotes the beliefs and actions of the individual as he or she impacts the social structure. Here, the participatory aspect in qualitative research clearly becomes one of activism, as the activist-researcher seeks to change the structure of a set of cultural patterns. In the case of the participatory art-making projects within a school, hospital and church, the researcher reports this:

A new space for communication was created by participatory processes in material-based art. The practical result of the study was that everyday surroundings *changed their nature by the use of art* (Arild Berg, cited in Solberg, 2017, p. 121) (italics added).

This looks like agency; along with it the resistance of the researcher against conventional outlooks about art in public settings. I suggest further that holding these participatory processes in three different social settings strengthens the research, because these repeated symposia can function as a form of “control” (See Figure 1). Three different events lend depth in demonstrating how activist interventions like these bring about change. The question for this researcher, and for all “making” researchers, is this: what confidence do you have that the change you have instigated in event X – or even in events X, Y and Z – will have any *lasting* impact? I suggest a way to answer this question in the conclusion below.

In yet another of Dr. Solberg’s cases, the researcher used paper string to study textile expressivity, culminating in both a written thesis as well as two exhibits (Solberg, 2017, pp. 102-103).⁴ From exhibit questionnaires, the researcher concludes:

Although I intended the artwork to have a meaning relevant to the material used, this was not evident, but tended to be open to interpretation, i.e. people seemed to have different opinions about the artwork. (Nimkulrat, cited in Solberg, 2017, p. 125)

In all fairness, I only have access to Dr. Solberg’s nine cases via her own dissertation. But on the evidence I have, if this statement is a “finding” coming out of the two exhibits *considered as units of test*, I do not see “doctorateness” in it. Different opinions from different people about artwork do not constitute a doctoral finding. On the other hand, this candidate’s work consists of extremely fine and intricate paper constructions that reflect a depth of aesthetic competence. The questions for me arising out of Dr. Solberg’s cases, then, not only have to do with *what* the researchers did, but more importantly, how to *measure* what they did for robustness in a way that is comprehensible to a general doctoral community. Just by using exhibitions, even exhibitions using questionnaires distributed and returned, may not be enough of an extension of scientific method. My suspicion is that, in this case, the researcher might have operationalized these exhibits in more traditional than critical ways, and so the results may not have been as robust as they could have been.

By traditional and critical, I have in mind Max Horkheimer’s influential essay *Traditional and Critical Theory* of 1937. This essay shifted the position of the theorist from objective analyst to nothing less than an agent for change *within* a social setting that, on the theorist’s view, needs change:

If... the theoretician and his specific object are seen as forming a dynamic unity with the oppressed class, so that his presentation of societal contradictions is not merely an expression of the concrete historical situation but also a force within it to stimulate change, then his real function emerges. (Horkheimer, 1937, p. 215)

Again, it seems to me that the bulk of the making dissertations Solberg analyzed have the activist-criticalist tone, with *art* as the domain (perhaps inclusive of *craft*) that is the “concrete historical situation” that needs change. At least upon Dr. Solberg’s reporting, my sense is that the researchers in some of these cases, while explicitly wanting to be activist in their work, may not have realized they can draw from critical theory as the theoretical basis for their activist agency. Armed with awareness of critical theory, and aligning the research logic with the parameters under the “making” columns of the continuum, we have a way to assess robustness that may be more recognizable to members of the larger doctoral community who themselves might not be engaged in “making” research.

To develop on this point further: “Field operations” is another example of how we can anchor units of test in “making” research to established theory. I have in mind Stan Allen’s Field Theory which has spawned quite a following in contemporary design literature and practice. Allen:

Field configurations are loosely bounded aggregates characterized by porosity and local interconnectivity... What is intended here is a close attention to the production of difference at the local scale, even while maintaining a relative indifference to the form of the whole (Cited from Becker, 2012, p. 4).

What we have here is a problematizing of the traditional architectural term “site,” which simply denotes the metes and bounds of the physical locale of a building. By extruding “site” into “field operations,” Allen creates a vastly extended, open-ended, conception allowing for *all* localized conditions of a given project to inform a designer’s actions. No longer constrained by physical parameters, “field operations” take into account elements of social-cultural interactions, political agendas, market forces, and so on. All comprise the “field.” To put it in our terms on the continuum, field operations under Making/Practice resonates with qualitative thick description.

Now, this expanded notion of “field” is also attractive in that it can apply explicitly to built work, on the one hand. For instance, the name of James Corner’s practice is none other than Field Operations, which has a track record of many well-known projects such as the High Line in New York City, the Navy Pier in Chicago, the current replacement of the Alaskan Way Viaduct along the waterfront in Seattle (Corner, n.d.). But on the other hand, “field operations” can also reify in installations and exhibits, as reported by Joseph Becker in his article “Field Operations” (Becker, 2012). This of course is where it impacts our concerns most. I have already mentioned the “Pro Am” events in one of Solberg’s cases, in which the researcher collaborated with consumers in fashion design at various sites. These can be assessed through the lens of field operations. Another example is a researcher who, in wanting to comprehend his own architectural practice, collaborated with various artists in five art projects. Solberg describes one of them:

The exhibition was built as one box within another larger box, to maximally disconnect it from its surroundings. In the interior were exhibited seven artefacts. One example of the seven events is a lecture by psychiatrist Erik Thys on ‘Space and the Soul.’ There were fifty visitors to this event ... (Solberg, 2017, p. 174)⁵

From Solberg's text, I confess I don't have much of an idea what this entailed, nor what lasting impact it may have had. But I do sense that this installation – and by extrapolation all five of this researcher's installations – can be operationalized as “field operations.” There is even a possibility for an enriching double entendre. This researcher used his five collaborations as points of focused reflection to better comprehend his years of practice as an architect. In this way, his own architectural practice history is itself comprehended as a field operation, enriched by the field operations of each exhibit. We can then assess for parameters of agency, enactment, empowerment, and change, as noted in Figure 1.

Concluding remarks

Here are four concluding points, ones that should accompany the use of this continuum model.

1. Internal validity is generally more robust when a “making” dissertation draws deeply (and clearly) from established theories and methods

Because “making” dissertations are themselves a fairly new development in the doctoral arena, anchoring such a dissertation in an established theory or method is one way to maintain communally recognizable continuity with extant research standards. Put another way, what a making dissertation intends to *do* might be new; but this does not mean the theory or method it *draws* from should also be newly innovated. In addressing the continuum concept in their conclusions, Frayling and his colleagues note the need for “a common currency” in practice-based doctorates with traditional doctorates “to confirm and make visible their equivalence to other PhDs” (Frayling, et al., 1997, p. 26). Founding a research topic upon established theories and methods is one way to maintain common currency.

Among Solberg's cases, a project that seemed strong on theoretical anchorage is the one that produced collaborative fashion designs in “Pro Ams.” Solberg reports that this work drew quite systematically from Deleuze and Guattari's theory of rhizomes (Solberg, 2017, p. 190). Rhizome theory envisions a mesh of factors with no regulating end or beginning.

It is comprised not of units but of dimensions, or rather directions in motion. It has neither beginning nor end, but always a middle (milieu) from which it grows and which it overflows... (Deleuze and Guattari, n.d.).

The open-ended nature of the Pro-Ams (again: collaborative encounters between designer and consumers in producing fashion with recycled materials) – the one in Istanbul was called Swap-O-Rama – seemed to be an instantiation of Deleuze and Guattari's theory. For a reviewer like myself, this was a recognizable connection between established theoretical literature and this researcher's work. The theory made the researcher's highly original research tactics more reasonable.

Similarly, in regard to methodology, another of Solberg's cases conducted ethnographic research in Alaska, experiencing first-hand how Iñupiat seamstresses used improvisation in making clothing.⁶ This *in situ* learning in turn informed the researcher about improvisation in art instruction in Norway. For a reviewer like myself, the internal logic of this method was easy to understand. Ethnography is an established methodological process. Again, the clearer of Solberg's cases were those that anchored their logic, both theoretically and methodologically, on established literature and practices. This trait itself raises confidence that a dissertation under review actively networks in the “common currency” of theory and method already well-known to the larger doctoral community.

Here is a related note about anchoring to established literature. Frayling, et al., note that demonstrated knowledge of “methods and techniques appropriate to the subject” is one trait of “doctorateness” (Frayling, et al., 1997, p. 10). Often, beginning researchers misinterpret competency in methods by citing a wide assortment of methodologies in one research study. This results in a confusing array of references at cross purposes to each other. This is not “anchoring in the literature.” The novice researcher should understand that any *single* established methodology is already *profoundly* able to supply the needs of his or her contemplated research. That is why the methodology is *established*.

2. For external validity in a making dissertation, a body of work is perhaps more relevant as a measure of “generalizability” than a body of knowledge

Earlier I worried about how a series of exhibitions serving as the culmination to a making doctorate, even if they instigated change in social venues during their enactment, can continue to foster change. If there is no lasting impact, is it sufficient to award the researcher a PhD? The conventional expectation of doctoral research is that it adds new knowledge to the literature of its field. This is why we call the new knowledge a *contribution* to a body of knowledge. Indeed, one of Frayling, et al.’s measures of “doctorateness” is that “the process of origination ... is communicable to peers in a permanent and reproducible form” (Frayling, et al., 1997, p. 11.). It is not currently clear how the making doctorates uniformly meet this expectation. Solberg spends some time explicating a domain of knowledge generally termed “Mode 2.” (e.g. Solberg, 2017, pp. 207-209). This term itself is well-established in the literature, thanks to pathfinders like Dunin-Woyseth and Nilsson (2011). But again, it is not clear how Mode 2 knowledge production – which seems to relate more to time-bound enactments, for instance, of transdisciplinary collaboration – endures as contributions to bodies of knowledge.

Perhaps it does not have to. My suggestion is that the making disciplines have always prized bodies of work rather than bodies of knowledge. Some years ago, Ali Ilhan and I published a paper arguing that the fields of interior design, architecture, and industrial design, *possess no bodies of knowledge* (Wang & Ilhan, 2009). We suggested that the moniker “body of knowledge” is itself a result of post-Industrial Revolution culture. Proliferating divisions of labor forced each division to demark (and jealously guard) its own “body of knowledge.” In contrast, design processes inherently work in inclusive fashion, resulting in one-of-a-kind physical forms, not necessarily in demonstrations of bodies of knowledge. This is not the place to summarize that previous paper. My point here is that, for the “making doctorates,” *a long track record of recognized work may be an important key to adjudicating whether a candidate receives doctoral standing*. Admittedly, this requirement for a sustained body of work through time, as opposed to contribution to a particular body of knowledge in time, is difficult to enact during the standard length of time it takes to earn a doctoral degree -- say, a window of about four years plus or minus.

One approach arising out of this is for doctoral institutions to admit practitioners with already a track record of long standing. In other words, a candidate comes into a making doctorate program on the strength of his or her body of work, and the doctoral process is one of intellectually deepening what is already an established pattern of praxis. The onus therefore is not on “merely” a culminating exhibition; instead, that culminating exhibition is itself a continuation – that is, a *contribution* – to an already recognized body of work. One implication is that the demographics of a cohort in a making doctoral program is necessarily older than perhaps the demographics of candidates earning, say, PhDs in physics or engineering. Note that, in this approach, the

recognized body of work is itself evidence of strong external validity. This raises the question of the role of the doctoral committee.

3. Also in relation to external validity: In the making doctorates, the role of the committee may entail recognition, publicity, dissemination, even promotion, of the work of the candidate, more than are practiced by traditional doctoral committees

In my engagements with doctoral programs in northern Europe and Scandinavia, the inter-institutional nature of doctoral committees is already impressive. Faculty and practitioners from diverse institutions and professional venues make up these committees, not to mention bringing in the “first and second opponents” from yet other institutions in the examination process.

My observation about the body of work measure for the making doctorate raises thoughts not so much about committee composition as about committee roles. There is no doubt that a doctoral candidate must benefit from the academic depth represented by members of his or her committee; this is a traditional responsibility of doctoral committees. But in addition to this, given that our concern is a body of work, the generalizability – that is, the external validity – of the research outcomes of a making doctoral candidate may require additional roles on the part of the committee. For example, at the beginning, it may entail recognizing the existing body of work of an applicant; perhaps even to encourage certain individuals with such a background to apply for the doctoral program. During the doctoral process, an active agenda of dissemination and even promotion of the candidate’s work might be part of the work of the committee. None of this should of course compromise the academic integrity and depth of the doctoral contents and trajectory. My point is that, if contribution to a body of work rather than contribution to a body of knowledge is the primary concern, the traditional role of a doctoral committee -- which is to assure that a candidate’s work contributes to a body of knowledge – should conceivably shift attention to how the candidate contributes to a body of work, whether his or her own, or even more intriguingly, to a larger body of work comprised of a *school* of artists or practitioners, for instance. All of this goes towards sustaining the impact of research outcomes arising from the making doctorates to beyond the limited scope of the duration of study, to a broader, more general and more lasting, audience. Which leads to this final observation:

4. The making doctorate, as itself an academic phenomenon, can develop an institutional apparatus, such as a journal dedicated specifically to furthering this new academic domain

In their conclusions, Frayling, et al. call for a “consensus on the defining features of *all* PhD awards .. which would admit both practice-based and conventional submissions. (Frayling et al., 1997, p. 27). Working largely in the United States, I do not have hands-on knowledge of the Bologna Process and the Dublin Descriptors. But it seems to me these are overarching visionary positions that, while recognizing the need for diverse expressions of doctorateness, perhaps do not solve on-the-ground challenges of answering what accounts for doctorateness in the making fields. What is needed are various entities at more local levels to further define, refine, and disseminate the work of the emerging community of individuals holding “making” doctorates. I have in mind a journal, for instance: *Journal of Doctoral Studies in the Making Fields*. Doctoral communities, like any other institutional structure in our diverse contemporary world, benefit from institutional apparatus to sustain their sense of community. These include conferences, membership of some kind in the organization, and perhaps most importantly for an academic endeavor, a journal dedicated to disseminating news and findings of its members. Over time, the body of work

included in an on-going journal devoted to the making fields will itself contribute to the consensus Frayling and his colleagues call for. This also is a kind of ratification of external validity.

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NOTES

¹ "Since tried-and-true procedures are entirely out of place when it comes to practicing-research, the exposition of Methods in the project report dictated by the IMRAD structure needs drastic readjustment." Solberg, 2017, p. 189. This is the dissertation of Pavlina Lucas. A summary of the IMRAD structure, which this candidate regards as "entirely out of place," can be found here: <https://www.cmu.edu/gcc/handouts/IMRD%20with%20Examples.pdf>. Accessed June 19, 2017. On my view, it is curious that one doctoral candidate feels he or she can challenge the established format for doctoral dissertations in general. On this candidate's view, it is the established format that needs "drastic readjustment" – not anything that he or she happens to propose for the dissertation at hand. This strikes me as not only questionable, it goes directly against the tradition of peer review. In other words, it directly rejects the *communally* recognizable nature of robust academic work that I am emphasizing here. If we award doctoral degrees on this basis, what prevents *any* work from the embrace of doctoral standing?

² This approach is starting to show some wear and tear, and theorists are looking for alternative ways to theorize architecture. See Robert Somol and Sarah Whiting, "Notes around the Doppler Effect and Other Moods of Modernism" in *Perspecta 33* (2002), pp. 72-77.

³ For a full account of this, see Andrea Wilson Nightingale, *Spectacles of Truth in Classical Greek Philosophy: Theoria in its Cultural Context* (Cambridge: Cambridge University Press, 2004).

⁴ This is the dissertation of Nithikul Nimkulrat: "Paperiness. Expressive Material in Textile Art from an Artist's Viewpoint." Solberg, 2017, pp. 102-103.

⁵ Solberg on the doctoral work of Arnauld Hendrickx, p. 174.

⁶ This is the work of Janne Reitan; see Solberg, 2017, pp. 139-140.