

The quality of doctoral education in South Africa: A question of significance

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Introduction

One of the underlying concerns in the Study Panel on the South African PhD, a large-scale, overview investigation of the Academy of Science of South Africa (ASSAf), was the negative consequences of signalling the need for more doctoral graduates to boost the presumed link to national competitiveness within a global knowledge economy. There was evidence that institutional behaviour in response to increased incentives for more accredited publications led to increased quantity at the cost of quality. Understandably, therefore, the panel feared that policy signals and incentives to produce more doctorates would compromise quality PhDs from the 23 universities. At the heart of this concern was the significance of doctoral research and not simply more PhDs. This article seeks to advance thinking about how significance in doctoral research can be attained against the background of this national study, and its concerns, about quality PhDs.

The quest for significance in doctoral research

Most of the research produced through theses, dissertations, journal articles and books make very little difference in the world of scholarship for one common reason: the lack of significance. To be sure, there are many other reasons why research produced does not attract the attention of scholars, practitioners and policymakers (such as the quality and reach of the research undertaken), but significance ranks up there as one of the most fundamental reasons for not taking published research seriously.

For the moment I am not talking about statistical significance, that specific measure of difference used in survey or experimental research to indicate whether a particular finding from a sample is likely to be true (or not) in the general population being studied. This is an important measure for those who work in statistics or surveys, and here the differences between statistical significance (e.g. the effects of a drug are significant in statistical terms) and scientific significance (e.g. the effects of the drug are significant in medical terms).

But my concern in this article is broader, covering both statistical and other kinds of quantitative and qualitative research. It has to do with the significance of any kind research as an intellectual practice, and why this is the single most important question in determining the worthwhileness of a scholarly study.

No doubt many students ponder over this question for a very long time: if I am to embark on this study for four or five years, or more, how do I know that my research will carry any weight, or be of any consequence? Will my research have something to say to the scholarly community or, in some fields, to the professional community of policymakers or planners or practitioners? Will anyone even read or take notice of my research findings? Will what I have to say merit turning my thesis into a book or my book into an award-winning publication? These are crucially important questions especially to the primary audience for this article i.e. young academics and new PhDs, who wish to establish themselves in their fields as top scholars who make ground-breaking contributions to our understanding of a set of intellectual problems.

This article is not intended, therefore, for those who simply wish to get by. It will not be helpful to persons who only want to get the research over-and-done-with and to move on with their lives. It will not help drones, that species within academic ecologies that simply wants to put their heads down and get on with the mechanics of research, those routines that will deliver a standard academic project. It is aimed at those who are ambitious (in the best sense of that word) and who want to ensure that their research

contributes to a deepening of understanding, a resolution of a problem, the improvement of a practice, or the advancement of our thinking.

The first requirement for significance is *intimate knowledge of the subject*

You cannot claim significance until and unless you know everything that has ever been researched and published on the topic in focus. Now of course you can never really “know everything” on the topic, in an absolute sense, for practical reasons that include research and knowledge claims published in other languages. But you can achieve mastery of the literatures on and around your research topic, and it will show. I cannot think of anything more fundamental to good scholarship than having a grip on every major (and minor) piece of thought or research on your topic. This is what distinguishes top scholars from the rest: the capacity to read voraciously, a hunger to know what’s ‘out there’ in the literature on the subject, a never-ending quest for grasping new knowledge in the field.

Why is this important? Because without knowing what exists and what is cutting-edge knowledge in your field, and on your specific topic, you cannot begin to make the claim for anticipated significance in the research you are about to embark on. Your justificatory claim for why your research is expected to be significant must have its basic argument the fact that you recognise but move beyond what is known. A made-up example follows:

- Until now, the dominant theories of democracy made the assumption that history, culture and politics made spontaneous uprisings in Arab states unlikely, if not impossible. My research will show the opposite - that the very seeds of democratic uprising are deeply embedded in the political cultures of Arabic nations. What we do not know, however, is how exactly those democratic impulses come to prominence in street-uprisings after decades of stable authoritarianism.

The made-up example above links what is known to what is not; the existing literature (on democratic theories) to potentially new theoretical developments; what is taken for granted to what now can be tested. And the only way a scholar can make these claims for significance is, again, through an intimate knowledge of what exists in the corpus on the subject.

This kind of scholar has “research alerts” to the major journals; she has a specific librarian who is prepared to find and notify the young scholar of new publications in the field; she attends the two international conferences in the specific field in part to take note of what the cutting-edge research on the topic is, and what kinds of bibliographic resources (including to-be-published manuscripts) are invoked to make the arguments around that topic. She regularly reads “reviews of the literature” that appear on the topic, and personally subscribes to at least two major journals in the field.

In other words, to fulfil this requirement, the young scholar organises her research thinking and her working space in ways that create an open conduit for the flow of literary information on the topic such that it is easier to master the knowledge of the field and the topic on a regular basis. This kind of organisation of personal and professional lives is fundamental for access to knowledge and to the capacity for original argument.

The second requirement for significance is *recognising the class of problems within which your research topic falls*

This entails the capacity to connect a local problem to its correlates elsewhere. All too often young scholars come to me with the claim that they cannot find much on the topic under discussion. Of course, I send them away to keep looking. There are technologies and techniques for searching that need to take up time here, but suffice to say that after some guidance students find volumes of research on their subject.

Very often scholars young (this is understandable) and old (this is distressing) would make the claim that their research is so particular to their context (drinking habits of teachers on the south-side of the city, to use a flippant example), that nobody has ever done research on this area or topic. Or the person would

make some claim to unique experience in a particular setting that cannot possibly apply elsewhere (like the migration patterns of the Kalahari *San* peoples). The problem with this claim is the failure to connect the specific setting or problem to allied problems of the same kind in other settings. A project on the San finds its literatures first on the expansive San research but also on the encompassing research on migrations among indigenous peoples everywhere, to use but one example.

Your research is only significant to the extent that it can excite scholars anywhere in the world of research who work on that topic (or its correlates). The study of democracy in Arab states is a concern to thousands of scholars of the subject; your specific focus on the democratic impulse in Tunisia is a subclass of the broader problem.

How then do you achieve significance through recognition of the class of problems within which your research falls? With practice, exposure and good mentoring. A good supervisor or mentor should be able to look at your problem and help guide you to “see” the class of problems within which to locate your study. A study on how governments use policy to achieve acquiescence among the citizenry might well fall within that class of problems called “political symbolism” in social policymaking. What is the point being made here? Quite simply, that when you can relate a specific problem to a broader class of problems you can then locate your study in the relevant literature, and signal departures from and advance on the existing knowledge in the field.

The third requirement for claiming significance is *the capacity to articulate an independent argument*

This is partly about knowing how to write well in academic contexts. But it is also about the ability ‘to spot a gap,’ in a manner of speaking, and to claim it with authority. This requirement follows on the other two, the grasp of the literature on the topic and the location of the class of problems under which the topic falls.

It is of course true, as the wise man said, that ‘there is nothing new under the sun.’ Yet it is also true that every study deemed significant will make the claim to have discovered something new, however modest. And the word is modest, especially in the social sciences but in general across disciplines. The scientist(s) who discovers the cure for AIDS, a massive breakthrough as it will be, would have built on years of anxious research that pushed the state of knowledge towards the point of great discovery. And so the requirement is that something will be “found” that advances our understanding even if only a little bit at a time.

There are considerable influences of culture and personality that impact on this requirement. In conservative, Calvinist and South African culture, any attempt to stand-out, to assert confidently, to claim significance in your work is frowned upon. God must get the glory. You must stand-back, and become invisible. ‘Children must be seen and not heard’ is the earliest rebuke in our culture that targets young children, and somehow we never emerge from this silencing. The false modesty and self-deprecatory talk of adults is a consequence of such socialisation, and it is therefore very difficult to train young doctoral students or new PhDs to grow out of what I regard as bad social habits. This scholarly requirements demands not arrogance, but assertion of what you know and claim to be true. It requires confident statements of what you know is available (or not) in the existing literature on the topic, and then, on the basis of that knowledge, state clearly what it is your research will add to what is known.

A few more words on the consequences of the silencing of one’s own voice in a South African upbringing. I noticed over the years that my doctoral students had great difficulty overcoming the constraints of culture and socialization in the way they framed their arguments. They engaged, firstly, in what could be called *confirmatory research*—that it, the tendency to want to confirm what was already known, to place oneself within the existing corpus and then share that space, so to speak, rather than stand out from the crowd and stake an independent position. There is safety and non-exposure in hiding behind what others say or said. This stance called confirmatory research goes hand-in-hand with concealing one’s own voice in favour of the voice of scholarly authority. When you read such a student or scholar’s work you constantly find expressions such as the following:

According to Williams and Gould.....

Novak avers.....

As Freire said in Pedagogy of the Oppressed....

Jackson claims that

There is of course nothing wrong with citing authority; my point is a different one. It is the over-use (actually, abuse) of such citations throughout a research proposal or a research report that is the concern. By constantly referring to what others say, you reveal a lack of confidence in what you say or what you have to say. A good supervisor or mentor reading such “he-said-she-said” scripts would come back with a singular comment: what do YOU say? That mentor would insist on your voice appearing in the text, that you take a stand on the literature having critically evaluated existing scholarly claims.

Merely citing authority is one thing; being able to chew through reported research in order to stake an independent claim is another thing altogether. A command over the literature is what is in question, and a confident command of what came before is expressed in the statement ‘we stand on the shoulders of giants, so that we can see further than they could’ (the value of insight) or, more crassly, ‘we stand on the shoulders of giants, then we step on their toes’ (the role of criticism).

The fourth requirement for significance is the skill *to recognise the limitations of existing research* – and on that basis to make the justificatory argument for your research

How do you read 100 research articles on, say, *the relationship between class size and student achievement* in ways that enable insight into the limitations of that research? Once again, the combination of solid research training, strong supervision or mentorship, appropriate exposure to – for example – top research conferences, and sheer practice, together create the capacity and chutzpah for being able to “read through” the literature.

Using the example of class size and student achievement: this is one of the most well-researched topics in educational research. The basic finding is that the size of a class of learners will impact on how those learners achieve; few arguments with that proposition. But surely that depends on a host of factors that the research may or may not take into account.

What about the qualification levels of the teachers in question? What about the physical size of the classroom within which 35 learners are taught? What about the learner characteristics by, say, socio-economic status? What about the conditions outside of the classroom – the school or the community? Surely the context of Mozambique and France would have something to say to the achievement outcomes apart from class size? What about the teacher’s salary levels? What about the culture of the school? What about when these studies were done? What about the socio-cultural meanings of the tests used to measure the effects of the independent variable (class size)? What about the upper and lower limits of class size within which such achievement is measured?

What is intended with these questions is to demonstrate the kinds of interrogations of a simple claim – that class size affects learner achievement – that the practised mind would run through even as the research literature on this topic is being surveyed. It is in honing the capacity to ask such questions that the young scholar prizes open opportunities to identify gaps in the literature and formulate a significant research problem.

Such a skill, well developed, would come to recognise categories of limitations commonly cited in reviews of the literature that lay the foundation for a significant new study:

- The sample sizes used this far was too small
- The sample sizes are unrepresentative of the broader population
- The findings were generated in culturally homogenous populations

- The findings did not account for changes in social attitudes or language populations over time
- The research design was flawed in relying too heavily on quantitative indices to account for complex social issues
- The survey findings on attitudes towards social welfare in middle-class suburbs do not apply in poor neighbourhoods
- The original research contain ethical biases, failing to protect the identities of women in asking their attitudes towards abortion in a conservative community, thereby potentially skewing the findings

In short, spotting limitations of research enables the scholar-researcher to justify a new departure. The broader categories of limitations include:

- Theoretical limitations (how the problem was understood)
- Methodological limitations (how the study was done)
- Contextual limitations (where the study was done)
- Ideological limitations (perspective on the problem e.g. particular conceptions of children)
- Historical-time limitations (when the study was done)
- Framing limitations (what research questions were asked, and what not)

The fifth requirement for significance is the ability to make the justificatory claim in writing

There is of course a difference between ordinary writing for communication, professional writing for practitioners in the field (such as nurses, lawyers, teachers etc), and scholarly writing for research purposes. How then do you write the argument claiming significance? The short answer: with great difficulty.

First you need to practise writing claims for significance. As with all writing, this kind of writing is not easy, at first, and requires constant practice. Second, you need exemplars of writing to guide your own, original writing; some of these will follow shortly. Third, you need to find your own voice or style of writing within this frame.

Lets us turn now to some examples of powerful writing statements that make the case for significance. What follows is an extract from a study on the impact of grade retention (holding back a student with low marks) on student academic achievement; the opposite of grade retention is ‘social promotion’—or automatic promotion as in the lower grades in South Africa. I use this example in part because of its common sense among parents and learners, and the simplicity of the case for significance.

Source: *Jon Lorence et al., Grade retention and social promotion in Texas, 1994-99: academic achievement among elementary school students, in Brookings Papers on Education Policy, 2002 (Editor Diane Ravitch), Brookings Institution Press, Washington D.C., USA*

Shortcomings of Previous Research

Although considerable research examines the impact of grade retention on student academic achievement, various shortcomings exhibited across these studies make it difficult to derive any concrete generalisations about the effectiveness of requiring students to repeat a grade. Reported effects of retaining students vary across studies because of the differences in sample sizes, the degree to which retained and promoted students were similar, the measures of academic performance, the presence of alternatives to retention, the content of alternative programmes, and the extent to which promoted students were socially promoted but given accelerated instruction to bring them up to grade level or simply socially promoted with no additional assistance. Many studies are based on dated samples of students, some of which are many decades old. It is questionable whether old studies based on predominantly white school populations in which 5 percent of the students were retained in grade are applicable to contemporary urban schools where large percentages

of ethnic minorities and economically disadvantaged pupils are retained in grade. Retention studies are often based on small samples that are unrepresentative of ethnically diverse educational systems. For example, several recent published analyses by Shane Jimerson and others are based on a panel with fewer than thirty students who were retained in grade. Further, many studies indicating that school retention is ineffective are unpublished student master's theses or dissertations or research reports issued by school districts. The generalisability of such studies is often suspect because of the unknown quality of the research. With only a few exceptions, another major problem of many retention studies is that students who were held back were followed for only a short period of time (for example, only a year or two). Most research on grade retention focuses only on the short-term effects of being held in grade and do not study the long-term consequences on students (for example, academic performance three or four years later). In addition, many studies follow only the performance of retained students without examining the academic achievement of comparable low-performing pupils who were not required to repeat the same grade. As a result of these flaws in research design, it is difficult to summarise the extent to which grade retention affects later student achievement.

Contributions of the Present Study

By utilising data based on a cohort of all low-achieving elementary students in the state of Texas over a number of years, we can overcome some of the weaknesses observed in previous studies on grade retention and social promotion. Our purpose is to ascertain whether holding low-achieving students back a year in grade contributes to enhancing academic performance. We believe that the findings presented here provide many advantages unavailable in previous research. First, the state data set analysed is much larger than used in earlier studies. Instead of investigating only a small number of students, the data consist of the entire population of elementary students in Texas. Second, the data include all academically challenged students who were retained in grade or promoted to the next grade. The academic achievement of low-performing pupils who were promoted to the next grade can be compared with the educational performance of similar low-performing students required to repeat a grade. Also, the state data set made available by the Texas Education Agency (TEA) is detailed enough to enable better comparisons of the academic achievement of low-performing students of various ethnic backgrounds who were retained with comparable students who were socially promoted. The longitudinal nature of the available data allows us to follow the academic progress of the same students over a six-year period. In sum, the existing state data can be analysed in a manner that permits better estimation of the effects of retention in grade on student academic performance than has heretofore been possible.

What is commendable about the link between shortcomings (limitations) and contributions (significance) of the research position staked out in this example, is the clarity of writing, the confidence of authorship, the skilfulness of analysis (of the literature), the command of the corpus, and the ease of classification (the class of problems) of the study undertaken.

The sixth requirement for significance is knowing what leading thinkers in your field believe is significant in that area of research

There are limits to what you can learn from the published research on what is significant, or not, in the scholarship in your area. There is a lag-time of at least between 2-4 years between what is published and when the new idea or concept or strategy was first discovered, explained

or applied. By the time a piece of significant research is published, the publishing author has already moved on to something else in her research and new manuscripts stand ready for submission. But you would not know this if you simply relied on the next journal issue.

That is why the really top young scholars find themselves in the laboratories of leading scholars around the world; in the latest conferences that assemble leading minds on that topic; and in the seminar rooms where such top scholars read and test their work. Some of South Africa's top researchers worked in the laboratories of Nobel Laureates in the sciences. In fact none of this country's science Nobel Laureates

spent their academic and research time only in South Africa; their discoveries were often made elsewhere. You know what is a significant question or technique or theory or method by constantly rubbing shoulders with the leading thinkers in the world. How you access such leading thinkers is a different question.

In postgraduate and postdoctoral studies, especially, the questions of WHERE you study and WITH WHOM you study are absolutely important in the context of this argument. If you received all your degrees at one South African university, I can tell with a high degree of confidence that your research will seldom achieve “significance” simply because of the enormous capacity of our universities for low-level intellectual inbreeding. Many of the top universities in the world refuse to take their Masters graduates into PhD studies for a very good reason: you need to broaden the repertoires of reading and thinking, theories and methodologies, ideologies and perspectives, beyond the circle of intellectual influence within a particular department or school. Most departments hold one or two lines of thinking on a topic; this is not bad, but it does mean that your capacity to have a much broader grasp of the subject is limited. At a crucial point, therefore, especially in PhD and post-doctoral studies, the associational value of your research attachment will determine whether your research rises to high significance in the field, or not.

The seventh requirement for achieving significance is the capacity of the young scholar for learning

Nothing can undermine the quest for significance more than hard-headedness. More than a few of my doctoral students came in so hard with their favoured research question or ideological commitment to a research project that they were impervious to advice from the supervisor, and ended up with a routine research report. Openness to learning, the capacity to change course are vital personal characteristics of the young scholar.

This does not mean you must be “blown away by every wind of doctrine,” for that is another danger – constantly changing your mind rather than taking a position on the basis of scholarly conviction. But it does mean that you listen carefully to advice, preferably from different quarters, and then adjust or redirect the scholarly pursuit. Stubbornness in the face of good supervisory or mentorship advice can undercut the quest for significance.

Kinds of Significance

I now wish to turn attention to the kinds of significance that can be claimed or anticipated in outlining a research programme (for purposes of funding, for example) or reporting on research findings (for purposes of publication).

The first category of significance could be called *practical significance*. What is envisaged here is a new technological discovery that comes through applied research, for example. It could include the testing of a new drug whose adoption could have major implications for the treatment of a health ailment. Research in the science, technology and engineering commonly has applications in real-life and therefore a high level of practical significance.

I would caution, though, against what seems to me to be excessive claims for practical significance in the social sciences and humanities. Too many of my former students start with the notion that they will develop a model in educational psychology or curriculum policy that will somehow improve practice in these fields. I have not yet seen this happen, and as a result time and resources are wasted developing models that nobody takes seriously. The social and human worlds are a lot more complex than easy models can address, but the real problem is that this quest for practical significance in the human sciences precedes the quest for *understanding first* before the rush towards practical applications.

The second category of significance could be called *theoretical significance*, by which I mean the discovery of new conceptual understandings or insights into old or familiar problems. With any research, the first obligation is to understand not to solve. Truly significant research advances our understanding or, in Humboldtian terms, pushes back the frontiers of knowledge. The quest for theoretical significance starts of course with recognising an anomaly, a puzzle, a contradiction, and then determining how existing

theories make sense of that problem, if at all, and then on the basis of extensive data, trying to improve on or extend existing explanations for the problem. This is the gold standard for significance in social and scientific research: the yielding of new insights or discoveries as a result of the research.

Permit a personal example. I struggled for months to understand how my white students (mainly Afrikaans) students at the University of Pretoria came to have such firm views about the past, such negative views about black people, and such pessimistic views about the future. What did not make sense was that they were all small children or born after the tragedy of apartheid. I scanned existing theoretical work on the subject and found little in South Africa except for descriptive accounts of racial attitudes and practices among white students. But I stumbled across international work on “second generation knowledge” (starting with Eva Hoffman’s remarkable book *After Such Knowledge*) where she tells her own story of how she came to acquire knowledge of the Holocaust from her parents (who eventually escaped Poland) even though she herself was not there. The elementary insights from her experiences I then used to build theory on how knowledge is acquired among white Afrikaans youth at the University of Pretoria, identifying the “channels” along which second-hand knowledge of the past came alive in the consciousness, preferences and decisions of young people who grew up in a democracy but lived, in a manner of speaking, in another era.

The third category of significance is what I would like to call *emotional significance*---the potential of the research to generate new feelings or attachments towards self or others. This kind of significance is seldom found in research textbooks, though Patti Lather once spoke of the capacity of research to induce resonance, coining the intriguing term resonance validity. Contrary to popular perception, research can mobilise people towards action (studies on climate change, for example) and resonate with the experiences of the poor (studies on the impact of asbestos on mining communities, for example). It is often expressed by someone who read your book or article and say “I was touched by your research.”

Knowledge in the Blood seems to have met the standards of emotional significance judged by the hundreds of responses of people claiming to have “felt” the impact of this work as they revisited their racial attitudes and behaviours, as well as difficult memories of the past. Emotions, in the new research on this human attribute, are not simply a negative experience but can also be a powerful response to difficult subjects. For this reason emotional significance, while still not seriously regarded in mainstream research, must be taken-up as a category of significance in scholarship in the future.

Of course measuring emotional significance is a problem at the moment, for some might argue that you cannot rely only on subjective responses from random groups. Nor can powerful human responses of this kind of “moving research” be discounted as “mere emotions” for such responses signal a positive value and impact of the research within the user community, and this must count in the estimation of significance.

These are the three major kinds of significance, even though the medical and pharmaceutical literatures also refer to other categories such as *clinical significance*, defined as the effectiveness of a treatment or therapy such that the patient or client is no longer regarded as ill or dysfunctional.

I have also seen research that has *personal significance* for the scholar, such as a doctoral student friend of mine who changed, at a late stage, her mainstream research to an award-winning doctoral dissertation on the experiences of lesbian mothers raising their children; it allowed her to “come out” and in the process, release a creative energy and level of dedication to the research that transformed knowledge itself.

Conclusion

This article has drawn attention to the personal characteristics, cultural contexts and strategic considerations vital to securing significance in research especially for the young, up-and-coming scholar determined to make a significant contribution in a particular field of research. It makes the central argument that significance rests principally on the foundations of prior research, its insights but especially its limitations, and that having a firm grasp of that literature (knowledge) makes the difference between routine research and significance in research.