

Beyond the Thesis: Rigor, Originality and the Making of a Scholar

Keynote Address at the MUBS–AIMC Doctoral Symposium

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The Chairperson of the Organising Committee, Associate Professor Joshua Mugambwa, members of the University leadership, distinguished academics and supervisors, doctoral candidates, colleagues, ladies and gentlemen:

A Very Good Morning You.

My name is Ibrahim Mike Okumu, It is a privilege to join you at this doctoral symposium. Phd University of St Andrews, United Kingdom, MA Economics McMaster Canada, and BA Economics, Makerere University.

Every doctoral student eventually confronts three uncomfortable questions:

- **Why should anyone believe my findings?**
- **What exactly have I added to what we already know?**
- And perhaps most importantly: **What kind of scholar will I become after the thesis is finished?**

Those three questions take us to the heart of doctoral education: **rigor, originality and scholarly formation.**

My central argument this morning is simple: A PhD is not primarily a test of how much you know. It is training in how to produce knowledge that deserves to be believed.

The doctorate is not ultimately about producing a long document. It is about acquiring the intellectual discipline to ask important questions, reason carefully, handle evidence honestly, recognise uncertainty and make claims that are proportionate to what the evidence can sustain.

I would therefore like to organise my remarks around three questions:

- What makes research credible?

- What makes research original?
- And what makes a researcher a scholar?

1. What Makes Research Credible?

Let me begin with rigor. Rigor is often confused with complexity.

A study appears rigorous because it contains sophisticated mathematics, an advanced econometric estimator, a large dataset, an elaborate conceptual framework or specialised terminology.

But complexity is not rigor.

Rigor is not the complexity of the method; it is the credibility of the inference.

A technically sophisticated study can still be badly designed. A complicated model can answer an unimportant question. A regression can be estimated perfectly and still fail to identify the relationship the researcher claims it identifies.

“Suppose farmers who use improved maize seed obtain higher yields than farmers

the most complicated method. It is about whether the research design supports the conclusion being made.”

Rigor begins much earlier.

It begins with the research question.

What exactly are you trying to understand?

Why does it matter?

What do we already know?

What remains uncertain?

What evidence would allow us to distinguish between competing explanations?

And perhaps most importantly: What evidence would persuade you that your preferred explanation is wrong?

That question is fundamental.

The researcher is not an advocate employed to prove a predetermined conclusion.

The researcher is an investigator.

We must therefore become as interested in evidence that challenges our expectations as in evidence that confirms them.

This is why rigor matters not only to academia, but also to society.

Research increasingly informs public policy, business strategy, investment decisions, organisational reform, health interventions, education policy and development programmes.

If we misunderstand the causes of low productivity, we may recommend the wrong intervention.

If we misdiagnose why firms fail, we may design ineffective financing programmes.

If we confuse correlation with causation, governments may devote scarce resources to interventions that deliver very little value.

In resource-constrained country like Uganda, poor research is not merely an academic inconvenience. It can be costly.

Rigor is therefore an intellectual responsibility, but it is also an ethical one.

It requires transparency about assumptions.

It requires care in measurement.

It requires attention to alternative explanations.

It requires robustness.

And it requires restraint.

Sometimes the most rigorous sentence in a paper is: “The evidence does not allow us to conclude that.”

You as researchers must become comfortable with that sentence.

Strong scholarship distinguishes what has been established from what is plausible, and what is plausible from what is merely possible.

That intellectual discipline is especially important when moving from a broad societal concern to a researchable doctoral question.

Many students begin with statements such as:

- “I want to study unemployment.”
- “I want to study corruption.”
- “I want to understand poverty.”
- “I want to study climate change.”

These are important problems.

But they are not yet research questions.

The doctoral task is to move from an important problem to an answerable question.

And here there is a useful paradox.

The narrower and more precise the question becomes, the greater the possibility that the research can speak meaningfully to the larger problem.

A carefully identified mechanism can often teach us more than an ambitious thesis that attempts to explain everything.

Depth creates relevance.

2. What Makes Research Original?

Typically, Doctoral candidates are told that their thesis must make an original contribution to knowledge. Those words can create unnecessary anxiety.

A student may ask:

- “What is left for me to discover?”
- “Has this not already been studied?”

In most fields, the answer is yes.

Someone probably has.

But originality does not require discovering something that no human being has ever contemplated.

Knowledge advances in more disciplined ways.

- Sometimes originality lies in the question. You ask something the literature has overlooked.
- Sometimes it lies in the explanation. You may challenge the mechanism through which an established relationship is believed to operate.
- Sometimes the contribution is empirical. You bring new evidence to an unresolved debate.
- Sometimes it is methodological. You use an identification strategy, experiment, model or analytical framework that allows us to distinguish between competing explanations more convincingly.
- Sometimes contribution comes from context. A theory that appears robust in one institutional setting may behave differently in another. Showing why it does not travel can itself be theoretically important.
- And sometimes originality lies in synthesis. The literature may contain fragments of an answer that no one has yet brought together into a coherent explanation.

The question, therefore, should not simply be: “Has anybody studied my topic before?”

The better question is: “What exactly do we currently believe, what evidence supports that belief, where is the uncertainty, and what will my research enable us to understand more clearly?”

That is the intellectual architecture of a PhD. And this is why a literature review should never become a catalogue of authors.

Too often, doctoral literature reviews read like this:

- Author A found this.
- Author B found that.
- Author C studied another country.
- Author D used another method.

Thirty pages later, the reader knows what many researchers have written but still does not know what the literature actually knows.

A strong literature review is an argument.

- It identifies competing explanations.
- It evaluates the evidence behind them.
- It shows where findings converge and where they conflict.
- It asks whether disagreement arises from theory, data, measurement, context or methodology.

And from that intellectual tension, the research question should emerge.

By the time the reader reaches your question, they should be thinking: “Yes. This is the question that now needs to be answered.”

That is far more powerful than simply announcing that there is “a gap in the literature”.

Because: A gap in the literature is not, by itself, a contribution. There are infinitely many things that have not been studied.

The important question is whether closing that gap changes what we understand, how we explain it, or what we should do differently.

That is what makes the gap consequential.

3. The Epistemic Discipline of Doctoral Research

This brings me to the epistemic question.

At the heart of every PhD is a simple but demanding issue: How do I know what I claim to know?

Every doctoral researcher must be able to answer that question.

Different disciplines answer it differently.

In economics, we often emphasise identification, counterfactual reasoning, causal inference, theory and the distinction between correlation and causation.

In management and organisational research, scholars may seek to understand institutions, behaviour, decision-making, organisational processes and meaning through quantitative, qualitative or mixed methods.

A quantitative researcher may ask:

- What is the causal effect?
- What is the counterfactual? What would the yield of the same farmer have been if that farmer had not used improved maize seed?
- Could selection, omitted variables or reverse causality explain the result?

A qualitative researcher may ask:

- How do actors understand this process?
- What mechanisms operate within organisations or institutions?
- How do context, meaning and experience shape observed outcomes?
- What evidence supports the interpretation?

A mixed-methods researcher may combine breadth and depth, measurement and explanation, patterns and mechanisms.

None of these approaches is rigorous merely because of the label attached to it.

- Quantitative work is not automatically rigorous because it contains equations.
- Qualitative work is not automatically insightful because it contains rich interviews.
- Mixed methods are not automatically superior because they combine two approaches.

The test is:

- Whether the method is appropriate to the question,
- Whether the evidence is generated and analysed credibly, and
- Whether the conclusions follow from the evidence.

The sequence matters:

The question should determine the method. The method should not determine the question.

A common danger in doctoral research is methodological enthusiasm.

- A student learns structural equation modelling and begins looking for something to model.
- Another discovers machine learning and searches for a dataset.
- Another becomes fascinated by an advanced econometric technique and looks for a question to which it can be applied.

But methods are tools.

The hammer should not determine what counts as a nail.

Start with the intellectual problem. Then ask what kind of evidence would allow you to address it credibly.

4. Research Means Learning to Live With Uncertainty

Doctoral research has another dimension that is sometimes discussed less openly.

It is psychologically demanding.

Research involves uncertainty by design.

If the answer were already known, there would be little reason to conduct the research.

You are therefore expected to spend several years working on something whose final answer you do not yet know.

That requires resilience.

- There will be moments when the idea that looked brilliant six months ago no longer looks convincing.
- You may discover that the key variable is poorly measured.
- Your theoretical prediction may not survive contact with the evidence.
- A journal may reject a paper you thought was excellent.
- A seminar participant may identify in ten minutes a weakness you did not see in six months.

This is not unusual.

It is research. Do not interpret every setback as evidence that you do not belong in academia.

- Sometimes a failed result is intellectually more valuable than a successful one.
- Sometimes a rejection forces you to sharpen a contribution.
- Sometimes a difficult seminar exposes precisely the weakness that must be addressed before the work becomes publishable.

Criticism is one of academia's mechanisms for improving knowledge.

The objective is therefore not to avoid criticism. It is to learn how to use it.

And that requires separating yourself from your work. When somebody says:

"I do not think your identification strategy establishes causality," They are not saying: "You are not intelligent."

When a reviewer says: “The contribution is unclear,” your first task should not be to conclude that the reviewer has failed to understand your brilliance.

Ask instead: Why could an intelligent reader read my paper and fail to see what I believe is obvious? That question can transform a paper. It can also transform a career.

5. What Makes a Researcher a Scholar?

This brings me to the third question. What happens after the PhD?

There is sometimes an implicit picture of academic life:

- Complete the doctorate.
- Publish papers.
- Get promoted.
- Become professor.

Real academic careers are rarely so linear. Scholarship develops through accumulation.

One paper improves your ability to frame questions.

Another improves your empirical technique.

A failed project teaches you something about data.

A difficult reviewer teaches you something about identification.

Teaching forces you to explain ideas you thought you understood.

Supervising students exposes weaknesses in your own thinking.

Working with practitioners reminds you that the questions academics find interesting are not always the questions society finds important.

Slowly, something begins to develop.

You acquire an intellectual identity.

People begin to associate you with a set of questions.

That, in my view, is more important than simply accumulating publications.

Do not build a career around papers. Build it around questions important enough to sustain a lifetime of inquiry.

A paper is an output.

A research agenda is an intellectual journey.

Ask yourself: What important problem do I want to understand over the next ten or twenty years?

That question changes the way you build a career.

- It influences what you read.
- It influences the datasets you invest in.
- It influences whom you collaborate with.
- It influences the conferences you attend.
- It gives coherence to your scholarship.

Individual papers then become parts of a larger intellectual contribution rather than isolated additions to a curriculum vitae.

From my own experience across academic research, policy engagement, teaching and university leadership, I have increasingly appreciated that the most rewarding scholarship often lies at the intersection between rigorous inquiry and real problems confronting firms, households, institutions and governments. Research can be theoretically serious and still be deeply relevant to society. These should not be treated as competing objectives.

But relevance should never become an excuse for weak methods.

And methodological sophistication should never become an excuse for irrelevance.

The best scholarship combines both.

6. Publication Is a Means, Not the Mission

There is one pressure that deserves particular attention, especially for younger scholars.

It is the pressure to publish.

- Publication matters.
- It exposes our work to scrutiny.
- It allows us to participate in scholarly debate.
- It is part of building an academic career.
- And, increasingly, publication records influence recruitment, graduation, promotion, grants and professional recognition.

But incentives matter.

When the pressure to publish becomes excessive, it can distort scholarly judgment.

- Researchers may rush work that is not ready.
- They may fragment one substantial study into several thin papers.
- They may choose fashionable questions over important ones.
- They may focus on what is easiest to publish rather than what is most important to understand.
- And doctoral students, particularly those under pressure to satisfy graduation or career requirements, may be drawn toward journals with weak or questionable peer-review standards simply because publication is faster or easier.

That is dangerous.

A publication should not merely add a line to your curriculum vitae.

It should add something credible to knowledge.

Do not confuse being published with having contributed.

The important question is not simply: “How many papers have I published?”

It is: “What have these papers taught us that we did not know before, and can that knowledge withstand serious scrutiny?”

Academic reputation is cumulative. So is damage to reputation.

You may gain a publication quickly in a weak outlet, but the long-term value of your scholarly record depends on whether serious researchers trust the work.

This is why doctoral students must learn not merely how to publish, but also how to judge what is worth publishing, when the work is ready, and where it belongs.

- Quality takes longer.
- Serious peer review can be uncomfortable.
- Good journals may reject you.
- The process can be slow.
- But the objective of scholarship is not speed.
- It is credibility.

And one carefully executed paper that genuinely changes how an important question is understood may ultimately matter more than several papers that disappear almost immediately after publication.

The lesson is not to publish less. It is to publish better.

7. Five Lessons for an Academic Career

Allow me to offer five brief lessons to those who hope to build a career in research and academia.

First, build an intellectual agenda, not merely a publication list. Ask what you want your body of work to help the world understand.

Second, learn from rejection without allowing rejection to define you.

Rejection is part of academic life.

Treat it as information.

Ask what can be improved.

Then improve it.

Third, collaborate widely, but develop an intellectual identity of your own.

Collaboration expands what you can do.

But over time, people should also know what questions you care about and what contribution you are trying to make.

Fourth, remain connected to real problems.

The strongest research often emerges from serious engagement with the world outside the university.

Policy, firms, communities, institutions and organisations constantly generate questions that deserve rigorous investigation.

Fifth, protect your intellectual integrity.

A career takes years to build.

Credibility takes even longer.

Do not sacrifice it for a convenient result, a publication, a consultancy, a promotion or an attractive conclusion.

Reputation compounds slowly.

So does trust.

8. Do Not Rush to Look Impressive

To the doctoral candidates in this room, allow me to say one more thing.

Do not spend your doctorate trying to look like a professor.

Use it to learn how to become a scholar.

There is a difference.

You do not need to have an answer to every question.

You do not need to dominate every seminar.

You do not need to use the most complicated method in your department.

Be intellectually ambitious.

But remain intellectually humble.

Read beyond the immediate demands of your thesis.

Understand the history of the ideas you use.

Learn the assumptions behind your methods.

Talk to people who disagree with you.

Present your work before you think it is perfect.

Seek criticism from people who are unlikely to flatter you.

And write.

Then rewrite.

Then rewrite again.

Good writing is not decoration added after good research.

Writing is part of thinking.

If you cannot explain your contribution clearly, there is a reasonable possibility that you have not yet understood it clearly enough yourself.

Simplicity is therefore not the enemy of sophistication.

Often, simplicity is what remains after the researcher has done the hard intellectual work.

9. What Should the PhD Ultimately Produce?

Eventually, every doctoral candidate will submit a thesis.

There will be an examination.

There will be corrections.

And, hopefully, there will be graduation.

But the thesis is only one product of doctoral education.

The more important product is the person who emerges from the process.

You should read differently.

You should question differently.

You should become slower to accept convenient explanations.

You should become more alert to assumptions.

You should distinguish evidence from assertion.

You should understand the difference between statistical significance and substantive importance.

You should become more comfortable saying:

“I do not know.”

And perhaps most importantly, you should become capable of changing your mind when the evidence requires it.

That is scholarship.

Conclusion

Ladies and gentlemen,

Doctoral research can sometimes feel like a race towards submission.

Complete the proposal.

Collect the data.

Run the analysis.

Publish the paper.

Defend the thesis.

Graduate.

These milestones matter.

But do not become so focused on finishing the thesis—or accumulating publications—that you miss the deeper transformation doctoral education is intended to produce.

The thesis will eventually be examined, corrected, bound and deposited in a repository.

The publications will appear on your curriculum vitae.

But neither is the most important product of the PhD.

The most important product is the scholar who emerges from it:

someone who asks better questions;

who distinguishes evidence from assertion;

who understands the limits of what can be claimed;

who is willing to change position when the evidence demands it;

and who produces knowledge that others can trust.

So pursue rigor because knowledge must be credible.

Pursue originality because scholarship must move understanding forward.

Publish because you have something worth contributing—not simply because the system rewards another publication.

Accept criticism because ideas become stronger when they are seriously challenged.

And build your career around consequential questions rather than merely accumulating outputs.

The world already has an enormous amount of information.

What it continues to need are people capable of distinguishing information from knowledge, association from causation, assertion from evidence, visibility from contribution, and confidence from truth.

That is what doctoral education should produce.

And so I leave you with one final thought:

The thesis must end.

The scholar must continue.

I thank you, and I wish you a productive symposium and a consequential scholarly journey.