# Research Methodology

Session 1 — Problem Framing & Scoping

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# Let's start with you

*Before we talk about research, I want to know what brought you here.*

1. Why are you doing a PhD? What do you hope to achieve?
2. What do you plan to work on?
3. Why did you choose this specific topic? Why Trento?
4. What do you hope to learn from a research methodology course?
5. What is research for you?

*Fill out the form on the course home page*

# What Is Research?

*Definitions, perspectives, and what makes research great*

*Let's see how some of the*
**great researchers**
*define research.*

“

*I think of research as a way of finding out something you don’t know.*

**— Claude Shannon**

*Mathematician, father of information theory*

“

*If we knew what it was we were doing,*
*it would not be called research.*

— **Albert Einstein**

*Far better an approximate answer to the right question than an exact answer to the wrong question.*

**— John Tukey**

*Statistics pioneer*

*Our mission is to drive breakthroughs that benefit society, businesses, and our products.*

**— Google Research**

*Research as innovation with impact*

# Which definition resonates with you?

### Shannon

*Finding out something you don't know*

### Einstein

*Venturing into the unknown*

### Tukey

*The right question over the exact answer*

### Google

*Innovation with real-world impact*

*Discuss with your neighbor — 2 minutes*

# Recommended Reading

## “How to do Research”

At the MIT AI Lab

A practical guide to structuring research questions,
designing experiments, and communicating findings.

# Lessons from Richard Hamming

*On doing great research and asking the right questions*

Hamming's Test

# What are the most important problems in your field?

*Why are you not working on them?*

What important problem in your field would still matter in 10 years?

# What problem in the world excites you enough that you want to spend 3 years on it?

*If you can't answer this clearly, the rest doesn't matter yet.*

# What paper or idea changed how you see your field?

*Great research starts with a sense of wonder.*

*Hold on to what sparked yours.*

# What great researchers do

*Hamming observed that the most productive researchers:*

### Talk to colleagues constantly

Ideas sharpen through conversation, not isolation

### Work on important problems

They resist the pull of easy, publishable work

### Cultivate deep curiosity

They ask “why” more than “how”

### Revisit fundamentals

Breakthroughs often come from re-examining basics

# The Modern Research Lab

*What industry labs teach us about doing research today*

# The Bitter Lesson

**Richard Sutton · 2019**

*Methods that leverage computation and data tend to win over methods that rely heavily on human-designed knowledge.*

## What this means for you

Empirical truth beats elegant theory. Test your ideas early and often.
Don’t fall in love with your solution before you’ve tested it.

# Build the System

*Modern AI labs emphasize complete systems, not isolated algorithms.*


This cycle applies to PhD research too.

Don’t wait until everything is perfect to test your ideas.

⚠ **Anti-pattern: perfecting in isolation for years**

# Evaluation Is a Key Research Bottleneck

*Modern labs increasingly invest in evaluation infrastructure as much as in model development.*

### Anthropic

Benchmark design, red-teaming, alignment evals

*anthropic.com/research/evaluating-ai-systems*

### Google DeepMind

Benchmark suites, human eval, safety testing

*deepmind.google/responsibility-and-safety*

### OpenAI

Capability evaluations, risk assessment, oversight

*openai.com/index/updating-our-preparedness-framework*

Research progress depends on better evaluation, not just better methods.

If you can't measure it properly, you can't claim it works.

# Anatomy of Research

*The seven building blocks of a research project*

# What does a successful PhD look like? Claude's opinion (not mine)

### Original contribution

You added something new to human knowledge

### Rigorous methodology

Others can trust and reproduce your results

### Clear communication

Your work is understandable and useful to others

### Personal growth

You became an independent researcher

# What does a successful PhD look like?

- Not about results (side note: your advisor will want result – so don't tell them I said this – I will deny it)

- Not about papers (easy to say, but what about career? And recognition?)

- You learn good approaches to tackle problems.
  - Results **will** come as a byproduct of following the right approach.
- You understand what you want to do in life, at least in the near future.
- You grow by tackling difficult problems – and suffering through them
- You learn to connect, learn to learn
- You make friends for life

- Life is short. PhD is tough. Make it count.
  - You must feel like you are growing and learning (and yes, suffering comes with that). There will be tough patches so don't quit right away – but if you feel you are not growing, maybe this is not your path. Or your place.

# Four Pillars of Research

1

## Problem, Questions, and Naming

What do we want to understand? Why does it matter?
And don’t forget naming – giving it a name is half of the solution

2

## Scoping Down

Narrow the focus. State your assumptions and constraints. Make the problem solvable. Find an MVP, the simplest useful version you can solve.

3

## Eval & Evidence

What does success look like? How do we measure? How do we collect evidence?

4

## Method

How will we investigate? Choose approach and tools.

**Key insight:** Scoping down is where most beginners struggle — don't skip it

1

# Problem and Naming

A good research problem is not just something nobody has done. It's something that matters — something whose solution would change how we understand or do things.

### Importance

Would solving this matter to anyone beyond yourself?

### Feasibility

Can you make meaningful progress in 3–4 years?

### Novelty

Is this genuinely unknown, or just unfamiliar to you?

# Questions & Hypotheses

## Question

Narrows a broad problem into something answerable.

Good questions are:

- Specific enough to test
- Open enough to be interesting
- Falsifiable

## Hypothesis

Your best guess — stated precisely enough to be wrong.

It should predict:

- What will happen
- Under what conditions
- How you'll know if it's wrong

# 2 Scoping Down

A great problem is useless if it's too big to tackle.
Scoping down means making the problem manageable without making it trivial.

This requires you to:

- State your assumptions explicitly
- Define clear boundaries — what's in, what's out
- Identify constraints (time, data, access)
- Simplify until you can actually make progress

# 3 Eval & Evidence

## Evidence & Metrics

What will you measure, and how?

Good evidence is:

- Tied to a clear metric
- Transparent
- Complete (including negatives)
- Honestly reported

## Defining Success

How do we know when we're done?

Before starting, define:

- What metric signals success
- What threshold is "good enough"
- Threats to validity
- Alternative explanations

# 4 Method

Boring….

# Key Takeaways

### Start with a real problem

Not a gap in the literature, but a gap in the world.

### Ask the right question

An approximate answer to the right question beats an exact answer to the wrong one.

### Work on what matters

Apply Hamming's test: important problems, 10-year relevance.

### Build, test, iterate

Don't perfect in isolation. Get evidence early.

### Evaluate rigorously

If you can't measure it, you can't claim it.

### Connect with people

Research is a social endeavor. Never eat alone.

# The Cardinal Sin of Research

## Inventing problems so that papers can be published.

This leads to incremental, forgettable work that helps no one.

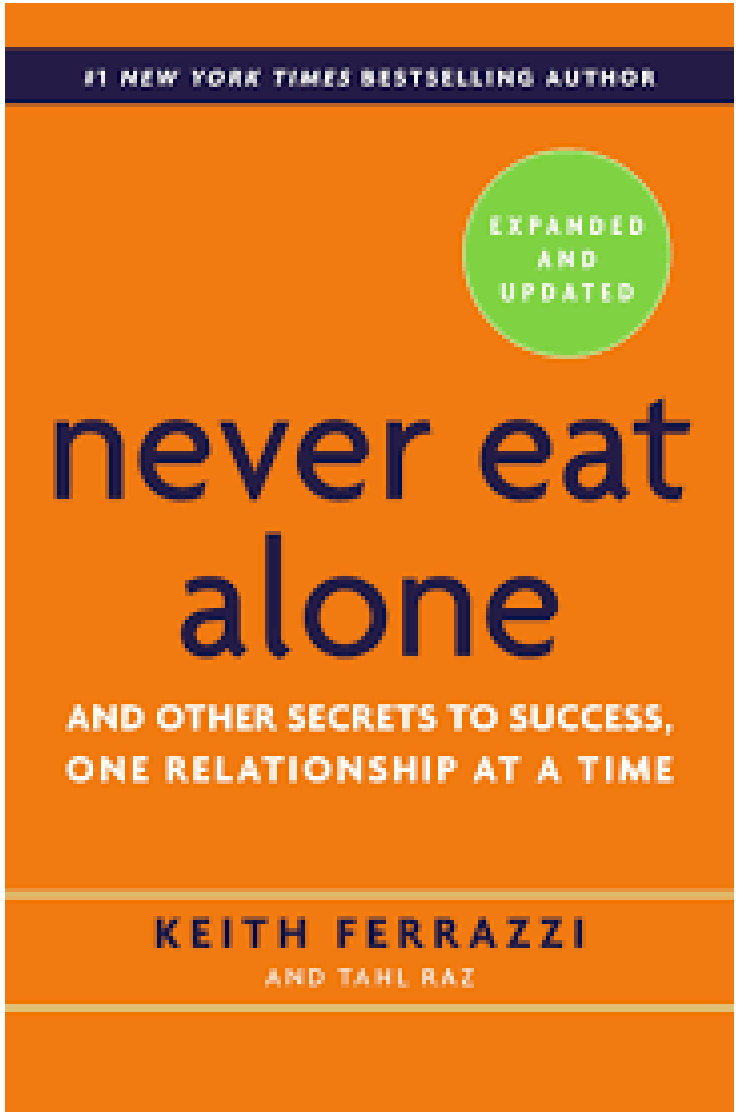
It fills journals but empties research of meaning.

### Warning signs

- “No one has done X” as sole motivation
- Problem disappears if you remove your method
- No real user or stakeholder cares

# Never Eat Alone

Build relationships before you need them.



# Your Turn

*From ideas to research questions*

# Thank you

Session 2: Experimentation & Evaluation

Fabio Casati · University of Trento