Class 2 - Research II: Positions

Agenda

- Skills corner: Reading articles (5 minutes)
- Summative lecture for last class materials (15 minutes)
  - Key principles
- Core paper discussion (70 minutes, including break)
- Summative lecture on concepts (15 minutes)
  - Defining a research landscape
- Thinking about thinking (15 minutes)

Skills Corner

Strategic reading of scholarly articles

Questions to guide you

- Why are you reading this article right now? [What information do you want to focus on / extract?]
- What might you use the information in the article for in the future? [How 'deeply' should you read?]
- Do you expect to read this piece over and over? [How do you want to allocate effort over those reads?]
- What do you already know about the topic? [What information is redundent and can be skipped?]

Some resources to start from

Here are some library resource guides with different strategies

- My aspirational process
- My WIP approach used in practice
- Brown University
- U Michigan
- U Wisconsin

Lecture - principles

Preamble

What follows is my personal, idiosyncratic synthesis of the pieces that we have read to date. To be clear, many interpretations are possible due to these articles' collective:

- richness
- overlap
- distinctive features

Preamble

Furthermore, there are multiple plausible criteria to judge quality research and a lack of universal consensus given the multiplicity of aims and epistemological orientations.

This is not to say anything goes; rather, I am trying to highlight the limits of my knowledge and that my unique (quantitative, positivist, deductive) lens necessarily abstracts away features from these complex discussions.[1]

[1] My stance is probably better captured with the idea of post-positivism, but I don't have a great reference to share here - but here is an overview: https://en.wikipedia.org/wiki/Postpositivism.

Last class - Some key principles of research design

- Falsifiability: Can we modify our state of knowledge based on the result of our study?
- Defensibility: Do our arguments appropriately employ modes of inference?
- Applicability: Does our study actually impact the state of practice?
- Replicability: Will our work bear re-examination in a similar or new context?

Falsifiability

- Falsifiability provides a basis for to use abductive reasoning to augment pure deductive reasoning.
  - The latter is true a priori if the premises are valid.
  - Pure deduction can transform our understanding of the system (think theorems in math).
  - But deduction cannot uncover truth beyond the system's universe of premises.
- We must use other means (such as abduction) to draw conclusions about what our data is telling us (i.e., concluding a theory is supported or not) to determine whether our deductive premises are valid by comparing our observation to our expectation (i.e., a proof by contradiction or falsification)

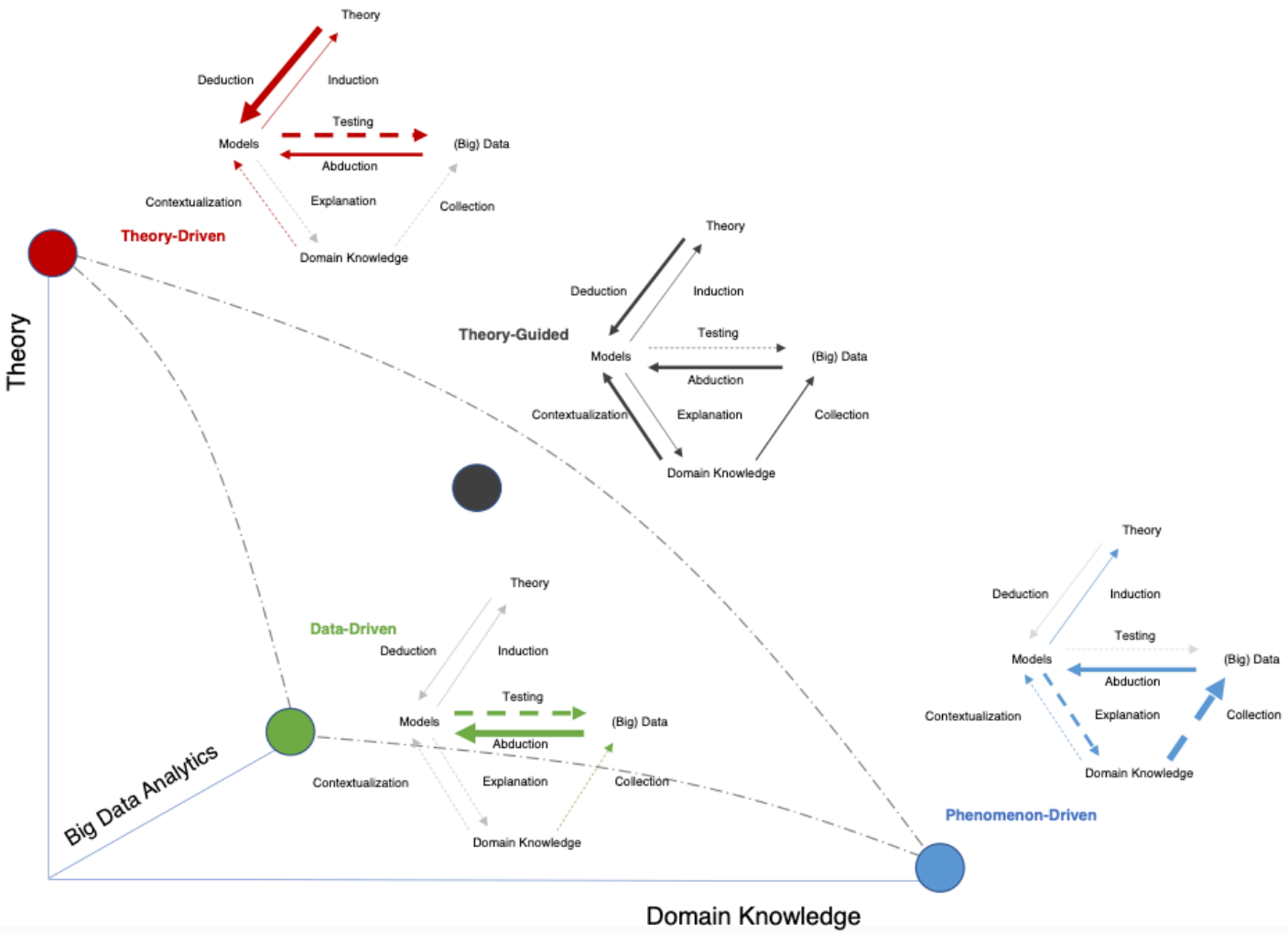
Falsifiability

My proposal is based upon an asymmetry between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements. For these are never derivable from singular statements, but can be contradicted by singular statements. - Popper (2002, 19)

Defensibility

- If the logic of our arguments are defensible and the evidentiary basis is sound, we are better able to act upon the conclusions with confidence.
- Like Mantere and Ketokivi (2013), my coauthor Zeki and I argue that the relative importance of each reasoning mode varies based on the research design employed and intended contribution.

Defensibility



Defensibility

Toulin diagrams provide one way to map out any argument, regardless of the method of inference:

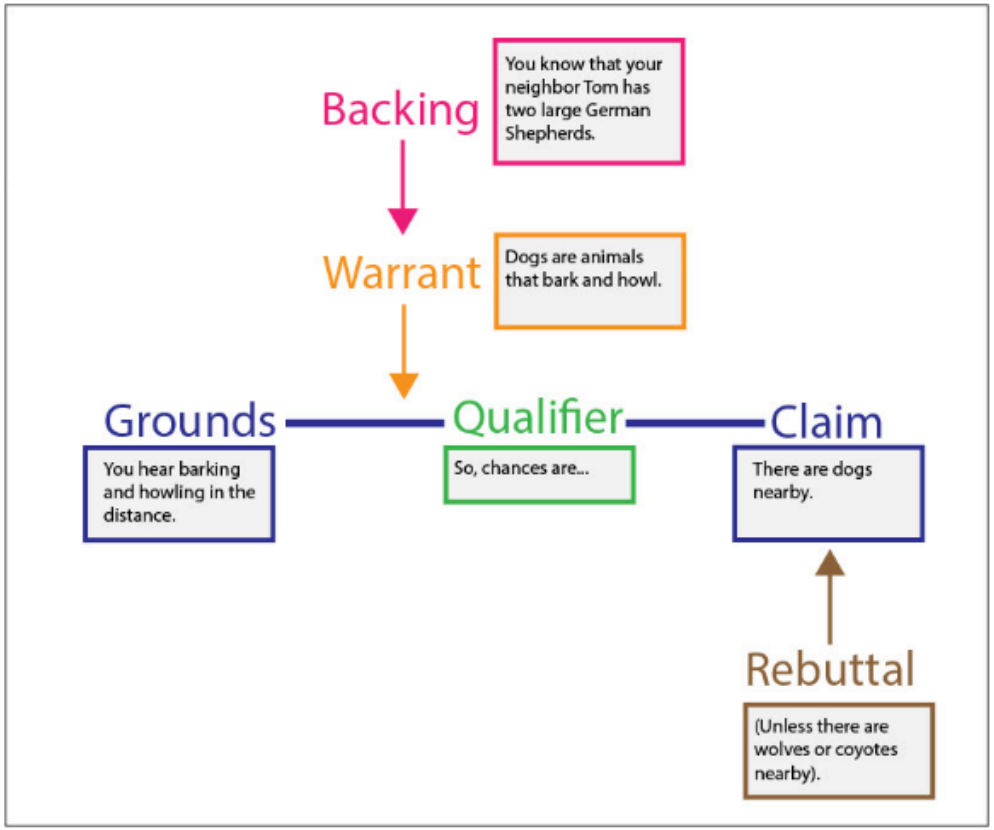


Figure 2: An example Toulmin diagram

Applicability

But our arguments and conclusions, even if correct, are not that helpful if they aren't applicable to real-world problems.

There are two corollaries to this observation:

- We should pick problems that actually matter, not just intellectual curiosities.
- We should not be hamstrung by our ability to tackle important problems.

Applicability

I was recently at a brown-bag seminar where a pair of management colleagues were seeking advice about a preliminary research idea. It took just a few minutes for us all to agree that their research question was fascinating. It addressed an extremely interesting issue that both academics and practicing managers would like to learn more about. The only problem: the presenters had no theory. So, we spent the entire session going through our collective mental catalogues of theories that might be invoked so that the project could proceed and have some prospect of publication. People were men- tioning theories I'd never heard of. We became frenzied, nearly desperate: "Good god, there must be a theory out there that we can latch onto." - Hambrick (2007)

Replicability

- Finally, the structure of our empirical base presumes that the research was performed in good order and that the findings are replicable within their domain of applicability.
- Said differently, if we are to make deductive inferences based on extant literature (i.e., to test and build upon that theory), we want to feel comfortable that is well-founded and that we can make inferences about specific cases from the general rules encoded by existing theory.

Replicability

[A]n accumulation of evidence that points to empirical regularities provides us with a much broader and more generalized understanding of the world. Such empirical regularities are known as 'stylized facts'. - Helfat (2007)

The relative importance of each principle

We can consider four basic "classes" of research in management:

- basic disciplinary research (primary studies in AER, AJS)
- applied research conducted in a management context (primary studies in AMJ)
- data-driven decision making derived from primary studies (systematic reviews in IJMR, JOM)
- practitioner-focused outlets (articles in HBR, CMR, popular press)

Where might, for example, applicability be more highly valued? Falsifiability?

Readings for Today

Today's topic - Research design "positions"

We must ask ourselves for a given project where we stand regarding the following:

- The trilemma: generalizability, precision, and realism
- A related tension: rigor vs. relevance
- Conversation topic: what you want to say vs. what the audience wants to know
- What claim(s) to assert: stylized facts, assumptions, critiques, and omissions

Readings

1. Huff, A. S. (1999). Writing for Scholarly Publication. SAGE. [Chs. 1, 3]
2. McGrath, Joseph E. (1981) Dilemmatics: The Study of Research Choices and Dilemmas, American Behavioral Scientist, 25, 2, 179-210
3. Simsek, Z., Heavey, C., Fox, B. C., & Yu, T. 2022. Compelling Questions in Research. Journal of Management, 48(6), 1347-1365.
4. Tushman, M., & O'Reilly, C. (2007). Research and Relevance: Implications of Pasteur's Quadrant for Doctoral Programs and Faculty Development. The Academy of Management Journal, 50, No. 4, 769-774.

Huff (1999)

Writing for Scholarly Publication. SAGE. [Chs. 1, 3]

The critical questions ot answer are these:

- *Which conversations should I participate in?*
- *Who are the important "conversants"?*
- *What are these scholars talking about now?*
- *What are the most interesting things I can add to the conversation? (p. 9)*

Huff (1999)

Discussion Questions

- Does the perspective of "conversation" resonate? How does this relate to academic work progressing iteratively rather than linearly?
- How might you use this research diamond to inform your research question for this class?

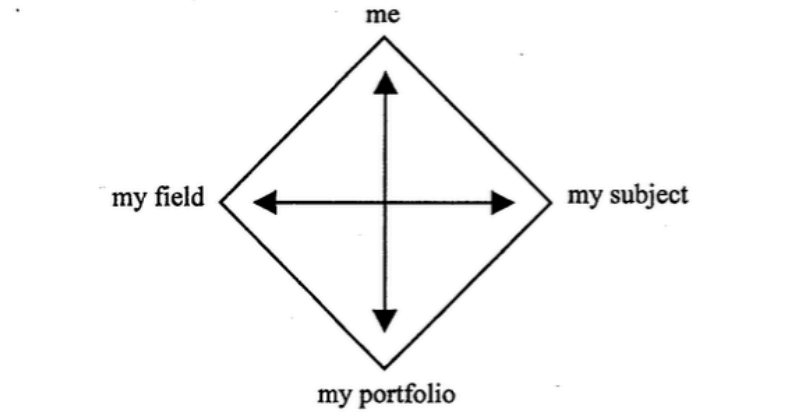


Figure 3.4. A "Critical Diamond" for Evaluating Writing (or Research) Alternatives

McGrath (1981)

Dilemmatics: The Study of Research Choices and Dilemmas, American Behavioral Scientist, 25, 2, 179-210.

The upshot of such a view of research is, of course, rather unpolyanna. Not only is there no "one true method," or set of methodological choices, that will guarantee success; there is not even a "best" strategy or set of choices for a given problem, setting and available set of resources. In fact, from the dilemmatic point of view, *all* research strategies and methods are *seriously* flawed; often with their very strengths in regard to one desideratum functioning as serious weaknesses in regard to other, equally important, goals. Indeed, *it is not possible, in principle, to do "good"* (that is, methodologically sound) *research*. And, of course, to do good

McGrath (1981)

Discussion Questions

- Which quadrant feels most comfortable to you? Why?
- What might you gain by combining multiple elements in a given study?

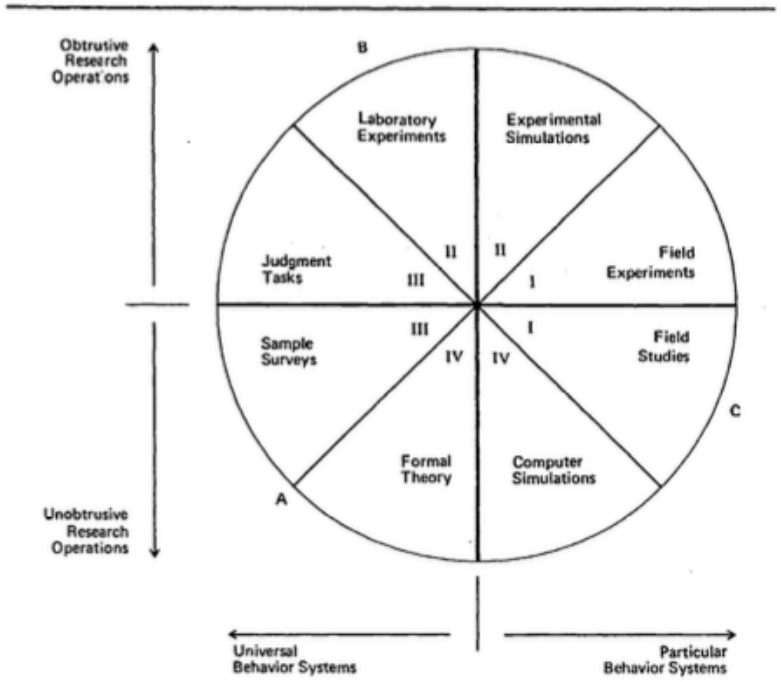


Figure 2: Research Strategies
From Runkel and McGrath, 1972.

Break

COFFEE BREAK

Simsek et al. (2022)

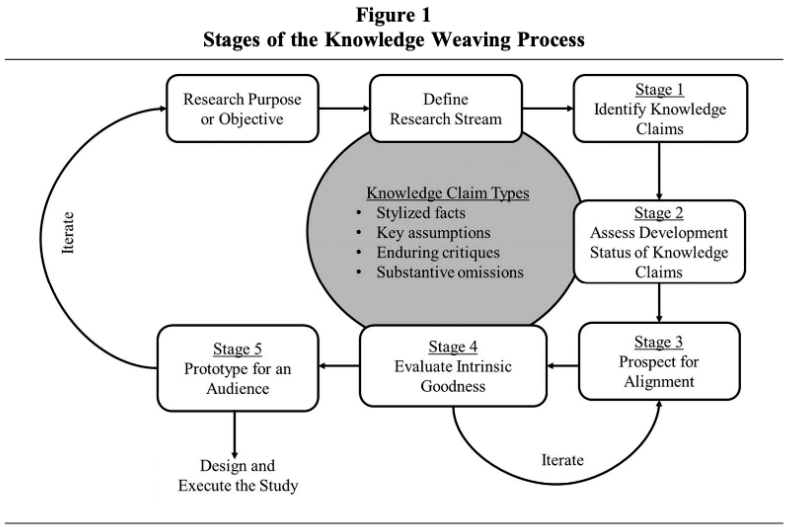
Compelling Questions in Research: Seeing What Everybody Has Seen and Thinking What Nobody Has Thought. Journal of Management, 48(6), 1347-1365.

> *We use knowledge weaving as a metaphor for achieving that alignment: combining the warp of distinct knowledge claims (i.e., what content is significant) with the weft of their developmental status (i.e., how to advance that content). Without both ingredients, the researcher risks shoehorning a question into the focal literature without substance. (p. 2)*

Simsek et al. (2022)

Discussion Questions

- Does this editorial provide a feasible pathway towards asking a compelling research question (asking for a friend)?
- How can a dual focus on knowledge claim type and developmental status help to position your study within the extant scholarly conversation?



Tushman and O'Reilly (2007)

Research and Relevance: Implications of Pasteur's Quadrant for Doctoral Programs and Faculty Development. The Academy of Management Journal, 50, No. 4, 769-774.

This defensive orientation to the phenomena on the part of this "silent majority" threatens to undermine our research, our doctoral programs, our MBA and executive education teaching, and our institutional legitimacy. Although we know much, we are collectively diffident about developing this knowledge with practitioners, as though a close interaction with the phenomenon might diminish our objectivity, blur boundaries, raise particularistic issues, and lead to conflicts of interest (e.g., Kimberly, in press; McKelvey, 2006). Our field's silent majority has the confidence (or hubris) to suggest that we can deduce important research topics and gather useful data only through disinterest. In contrast, we believe that this self-imposed distance from the phenomena we study reduces the quality of our field's research, undermines the external validity of our theories, and reduces the overall relevance of the data used to test ideas.

Tushman and O'Reilly (2007)

Discussion Questions

- Based on what you have seen so far, how is your field doing in terms of operating in Pasteur's quadrant?
- What are the impediments to operating here and how might you overcome them?

FIGURE 1
Three Quadrants[a]

Rigor: Quest for Fundamental Understanding	Relevance: Considerations of Use: No	Relevance: Considerations of Use: Yes
Yes	*Bohr's Quadrant* Basic disciplinary research	*Pasteur's Quadrant* Professional schools Business schools
No		*Edison's Quadrant* Consulting firms

Lecture - positions

Defining a landscape for research questions

Dimensions we have discussed today:

- To what end? Rigor (understanding) and relevance (use)
- By doing what? Precision, generalizability, and realism
- With whom? What matters to you v. what matters to others
- Means of advance? Types of knowledge claims and their level of development

Defining a landscape for research questions

How might they be correlated with each other?

- Rigor and relevance:
  - Orthogonal (r = 0?)
  - In opposition (r = -1?)
- Precision, generalizability, and realism:
  - How do each map onto rigor and relevance?

Defining your place on that landscape

- What matters to you v. what matters to others
  - What dimensions does your audience care about?
  - Which do you care about and why?
  - What implications does this have for topic selection? Outlet selection?
- A potential missing piece: Novelty?

Defining your place on that landscape

- Types of knowledge claims and their level of development
  - How do different combinations of knowledge claims and development status fit on the research landscape?
    - Stable stylized facts: High in relevance, perhaps "old news" to scholars (Edison's quadrant?)
    - Unstable key assumptions: An opportunity to conduct basic research to build a new basis of understanding?
- How might your choice of knowledge claims define you as a scholar?

Thinking about Thinking

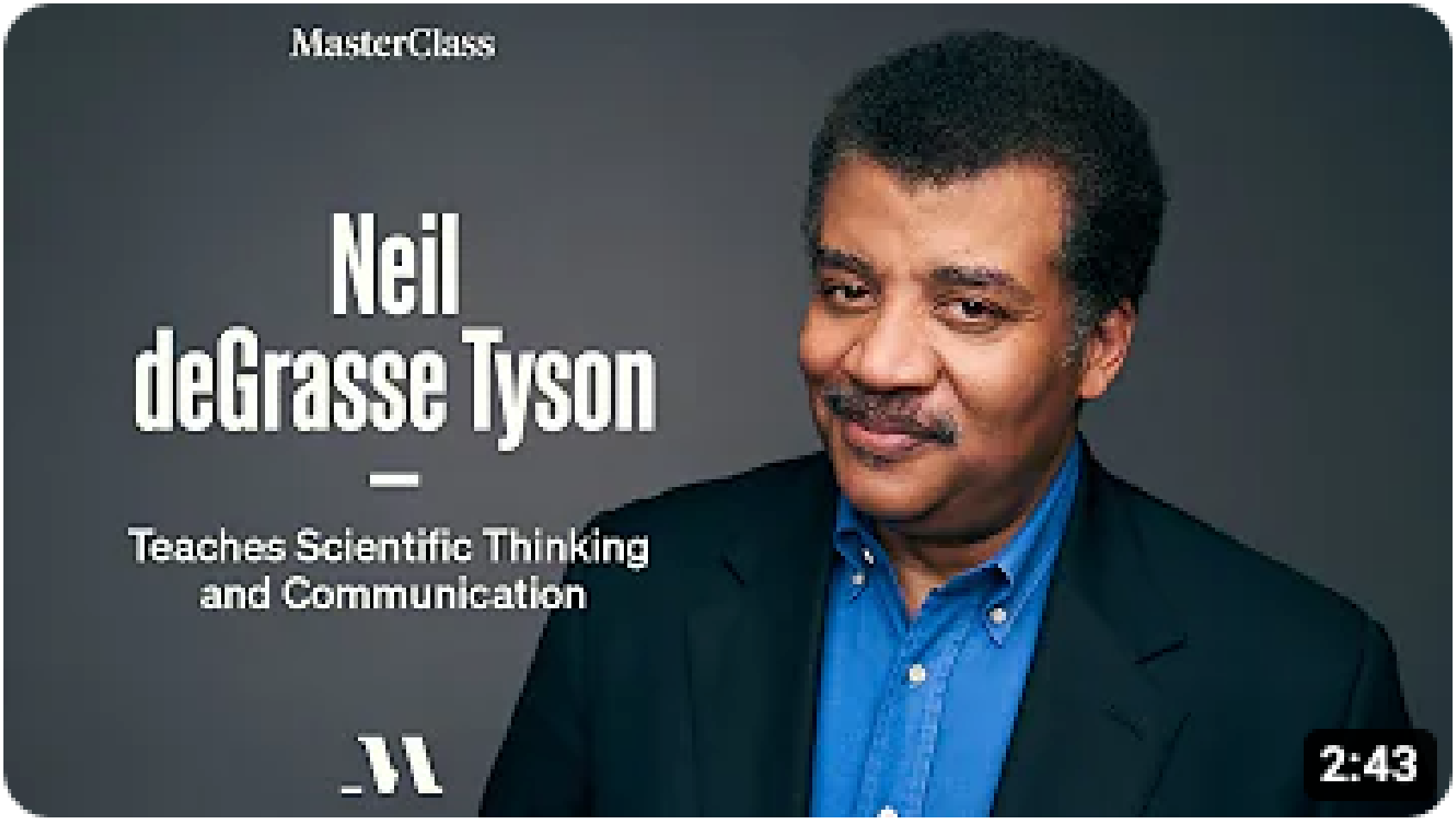
Useful types of thinking when engaging in the research process

- Skeptical thinking
- Bayesian thinking
- Strategic thinking
- First principles thinking

Skeptical thinking

"Science depends on organized skepticism, that is, on continual, methodical doubting. Few of us doubt our own conclusions, so science embraces its skeptical approach by rewarding those who doubt someone else's." Neil deGrasse Tyson, Origins: Fourteen Billion Years of Cosmic Evolution

Skeptical thinking



Bayesian thinking

Implicit in the earlier discussions is our degree of belief.

- Nosek and Errington talk about how replication increases or decreases our degree of belief.
- Popper uses the asymmetry of verification to achieve binary outcome of disconfirmed evidence.
- But couldn't we be more subtle in our treatment of beliefs?
  - Indeed, we can through the application of Bayesian logic and Bayes' Rule.
  - I will not be teaching you the statistical methods that follow from this, but you can find them.

Bayesian thinking

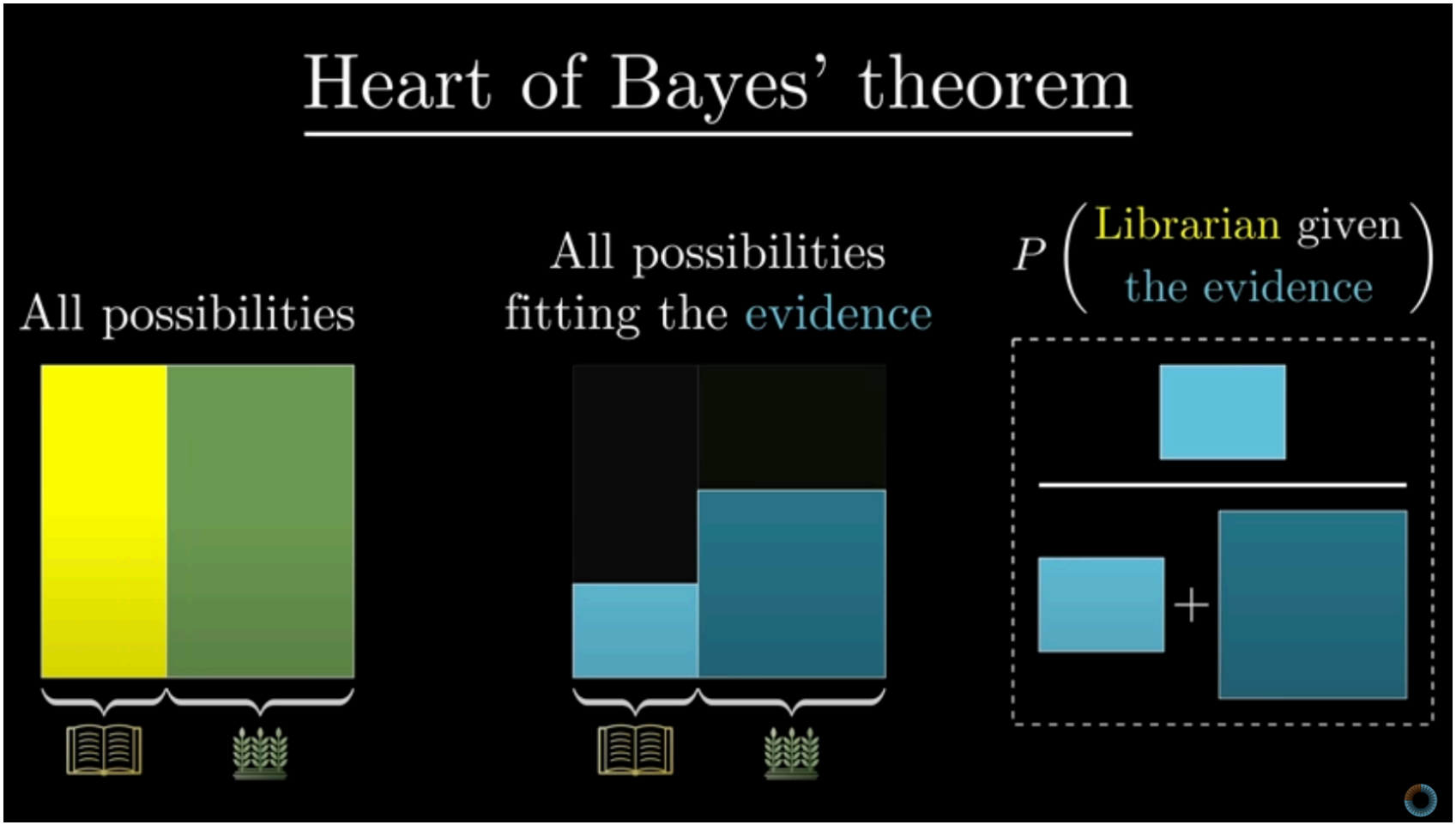


Figure 4: A Primer on Bayesian Thinking

Strategic thinking

Finally, it helps to be strategic when thinking about designing and evaluating research. By this I mean thinking that embraces three characteristics:

- Rigor
- Complexity
- Ambiguity

Strategic thinking

- Rigor
  - Comprehensive – focusing attention on both the forest (a research program) and the trees (discrete methods or studies)
  - Adaptive – balancing multiple goals and knowing what progress can be made against one or more of them simultaneously (McGrath's Trilemma)
  - Inferential – moving from what is known to what can be reasonably inferred (judicious use of multiple reasoning modes)

Strategic thinking

- Complexity
  - Dynamics – accounting for first and second order effects that are material across actors, choices, and time (rigor)
  - Allocentricity – outcomes often jointly determined by internal and external factors, often other parties or agents

Strategic thinking

- Ambiguity
  - Unstable – non-linear shifts across time and situations may limit generalizability and heighten the role of context (thinking about whether findings will maintain relevance)
  - Unforeseeable – many research projects are a full reinforcement learning problem, learning while doing is necessary to reveal the evolving state of the world

Strategic thinking

First principles thinking

A first principle is a basic assumption that cannot be deduced any further. Over two thousand years ago, Aristotle defined a first principle as "the first basis from which a thing is known." First principles thinking is a fancy way of saying "think like a scientist." Scientists don't assume anything. They start with questions like, What are we absolutely sure is true? What has been proven? - James Clear

Preparation for Next Class

Next class - Workshop!

- We will have a workshop session about four hours long. For the first two hours or so, students will **make a five-minute presentation about their research topic/idea** for their dissertation.
- This can involve a few PowerPoint slides to help guide the discussion. It may be a broad topic or specific question; this will vary by student.
- The balance of the workshop will be:
  - presentations by scholars regarding their first research project, and
  - group work to discuss and advance those research questions; I will move from group to group to facilitate discussions.

Expectations

- I am NOT looking for something super polished
- I want to hear where you are at with your research question and I want your colleagues to know about it too
- I'll aim to reserve a minute or two for questions and comments after each presentation, but the substantive discussions will likely happen later in groups

Next class

Research III: Practices

Our first compare and contrast discussion will take place. Presenters, please reach out if you have questions or concerns!

1. Lange, D., & Pfarrer, M. D. (2017). Editors' Comments: Sense and Structure—The Core Building Blocks of an AMR Article. Academy of Management Review, 42(3), 407-416.
2. Tobi, H., & Kampen, J. K. 2018. Research design: the methodology for interdisciplinary research framework. Qual Quant, 52(3), 1209-1225.
3. Aguinis, H. & Vandenberg, R. J. (2014). An ounce of prevention is worth a pound of cure: improving research quality before data collection.

Next class

Research III: Practices

4. Compare / Contrast

- Corley, K. G., & Gioia, D. A. (2011). Building Theory about Theory Building: What Constitutes a Theoretical Contribution. Academy of Management Review, 36(1), 12-32. https://doi.org/10.5465/amr.2009.0486
- David A. Whetten, 1989. What constitutes a theoretical contribution? Academy of Management Review, 14: 490-495

References

Hambrick, Donald C. 2007. "The Field of Management's Devotion to Theory: Too Much of a Good Thing." *The Academy of Management Journal* 50, No. 6: 1346–52.

Helfat, Constance E. 2007. "Stylized Facts, Empirical Research and Theory Development in Management." *Strategic Organization* 5 (2): 185–92.

Popper, Karl R. 2002. *The Logic of Scientific Discovery*. Routledge.