Class 1

Agenda

- Introductions (30 minutes)
  - Getting to know each other
  - Syllabus and materials overview
  - Typical class flow
- Readings for today (70 minutes, with 5 min break)
- Summative lecture and open discussion (20 minutes)
  - Key principles
  - Additional thoughts

Introductions

A little about me

A little about me

A little about me

A little about you

Let's fill out some introductory surveys: Pollev.com/drfox

Syllabus and materials overview

- Syllabus
- Brightspace
- Dropbox

Typical class flow

- *Part I:* Conceptual grounding and agenda setting (10 minutes)
- *Part II:* Core paper discussion (45 minutes):
  - We will discuss the 2-3 papers that all students have been assigned to read in detail
  - These papers typically will provide a mix of conceptual background and how-to guides
- *Break*

Typical class flow

- *Part III:* Activity period (40 minutes):
  - (Weeks 2 – 7) Compare / contrast: One group tasked with reviewing two additional papers to explain their points of intersection, divergence, and ties to core papers
  - (Weeks 8 – 14) Replication: One group tasked with using data from one of my current or published papers to replicate analyses and show the class the process

Typical class flow

- *Part IV:* Summative lecture on concepts (15 minutes):
  - I will make a brief presentation to tie together and highlight key concepts
  - Elements missed in the general discussion will be given greater focus
- *Part V:* Open discussion (5 minutes)

Readings for Today

Preamble

I have provided some discussion questions for us to consider in case we need to get the ball rolling.

We may or may not discuss those questions depending on the flow of the class.

In general, I would rather talk about your ideas and questions rather than these “canned” items.

Popper (2002)

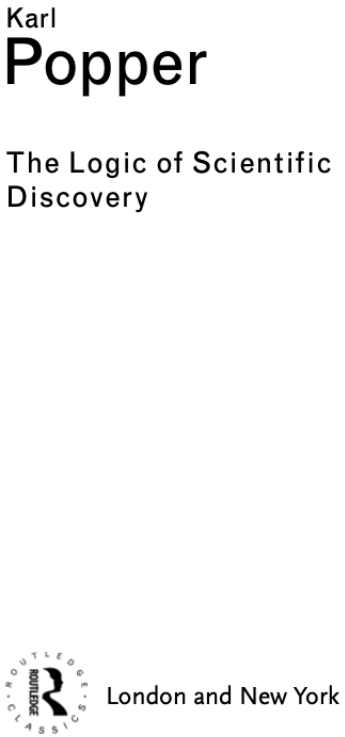
The Logic of Scientific Discovery. [Ch .1]

> *According to the view that will be put forward here, the method of critically testing theories, and selecting them according to the results of tests, always proceeds on the following lines. From a new idea, put up tentatively, and not yet justified in any way—an anticipation, a hypothesis, a theoretical system, or what you will—conclusions are drawn by means of logical deduction [...]*
> *[Then,] there is the testing of the theory by way of empirical applications of the conclusions which can be derived from it. [p. 9]*

Popper (2002)

Discussion Questions

- Reactions? Insights? Disagreements?
- In your view, what is the main point?
- Do this worldview currently inform your work? How might it?



Mantere and Ketokivi (2013)

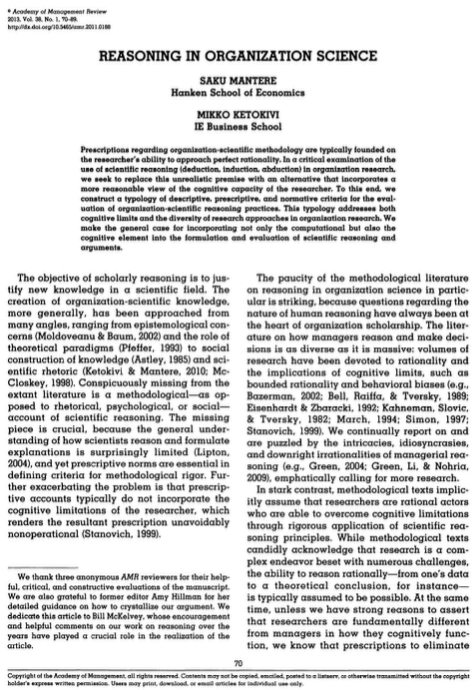
Reasoning in Organization Science. Academy of Management Review, 38(1), 70-89.

> *Labels aside, a closer look at research practice reveals that researchers across research traditions use all three forms of reasoning. It is hardly surprising to observe that we all make inferences to a case (use deduction), inferences to generalizations (use induction), and infer ences to explanations (use abduction). Thus, using reasoning types as labels to describe entire research designs is misleading. Instead, differences between research approaches, whatever they may be, are found not in the types of rea soning used but, rather, in how the three reasoning types are used in conjunction with one an other. (p. 76)*

Mantere and Ketokivi (2013)

Discussion Questions

- What the hell are they talking about?
- What mode(s) of reasoning do you tend to rely on in your current work?
- What concrete practices did you draw from this paper, if any?



* Academy of Management Review
2013, Vol. 38, No. 1, 70–89.
http://dx.doi.org/10.5465/amr.2011.0188

REASONING IN ORGANIZATION SCIENCE

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Prescriptions regarding organization-scientific methodology are typically founded on the researcher's ability to approach perfect rationality. In a critical examination of the use of scientific reasoning (deduction, induction, abduction) in organization research, we seek to replace this unrealistic premise with an alternative that incorporates a more reasonable view of the cognitive capacity of the researcher. To this end, we construct a typology of descriptive, prescriptive, and normative criteria for the evaluation of organization-scientific reasoning practices. This typology addresses both cognitive limits and the diversity of research approaches in organization research. We make the general case for incorporating not only the computational but also the cognitive element into the formulation and evaluation of scientific reasoning and arguments.

The objective of scholarly reasoning is to justify new knowledge in a scientific field. The creation of organization-scientific knowledge, more generally, has been approached from many angles, ranging from epistemological concerns (Moldoveanu & Baum, 2002) and the role of theoretical paradigms (Pfeffer, 1993) to social construction of knowledge (Astley, 1985) and scientific rhetoric (Ketokivi & Mantere, 2010; McCloskey, 1998). Conspicuously missing from the extant literature is a methodological—as opposed to rhetorical, psychological, or social—account of scientific reasoning. The missing piece is crucial, because the general understanding of how scientists reason and formulate explanations is surprisingly limited (Lipton, 2004), and yet prescriptive norms are essential in defining criteria for methodological rigor. Further exacerbating the problem is that prescriptive accounts typically do not incorporate the cognitive limitations of the researcher, which renders the resultant prescription unavoidably nonoperational (Stanovich, 1999).

We thank three anonymous *AMR* reviewers for their helpful, critical, and constructive evaluations of the manuscript. We are also grateful to former editor Amy Hillman for her detailed guidance on how to crystallize our argument. We dedicate this article to Bill McKelvey, whose encouragement and helpful comments on our work on reasoning over the years have played a crucial role in the realization of the article.

The paucity of the methodological literature on reasoning in organization science in particular is striking, because questions regarding the nature of human reasoning have always been at the heart of organization scholarship. The literature on how managers reason and make decisions is as diverse as it is massive: volumes of research have been devoted to rationality and the implications of cognitive limits, such as bounded rationality and behavioral biases (e.g., Bazerman, 2002; Bell, Raiffa, & Tversky, 1989; Eisenhardt & Zbaracki, 1992; Kahneman, Slovic, & Tversky, 1982; March, 1994; Simon, 1997; Stanovich, 1999). We continually report on and are puzzled by the intricacies, idiosyncrasies, and downright irrationalities of managerial reasoning (e.g., Green, 2004; Green, Li, & Nohria, 2009), emphatically calling for more research.

In stark contrast, methodological texts implicitly assume that researchers are rational actors who are able to overcome cognitive limitations through rigorous application of scientific reasoning principles. While methodological texts candidly acknowledge that research is a complex endeavor beset with numerous challenges, the ability to reason rationally—from one's data to a theoretical conclusion, for instance—is typically assumed to be possible. At the same time, unless we have strong reasons to assert that researchers are fundamentally different from managers in how they cognitively function, we know that prescriptions to eliminate

Nosek and Errington (2020)

What is replication? PLOS Biology: 1-8.

> *To be a replication, 2 things must be true: outcomes consistent with a prior claim would increase confidence in the claim, and outcomes inconsistent with a prior claim would decrease confidence in the claim. The symmetry promotes replication as a mechanism for confronting prior claims with new evidence. Therefore, declaring that a study is a replication is a theoretical commitment. Replication provides the opportunity to test whether existing theories, hypothe- ses, or models are able to predict outcomes that have not yet been observed. Successful replica- tions increase confidence in those models; unsuccessful replications decrease confidence and spur theoretical innovation to improve or discard the model. (p. 2)*

Nosek and Errington (2020)

Discussion Questions

- Do you agree with their definition of replication?
  - What are the benefits and drawbacks of applying such a definition?
  - How does this fit in with the replication crisis?

PLOS BIOLOGY

PERSPECTIVE

What is replication?

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Abstract

Credibility of scientific claims is established with evidence for their replicability using new data. According to common understanding, replication is repeating a study's procedure and observing whether the prior finding recurs. This definition is intuitive, easy to apply, and incorrect. We propose that replication is a study for which any outcome would be considered diagnostic evidence about a claim from prior research. This definition reduces emphasis on operational characteristics of the study and increases emphasis on the interpretation of possible outcomes. The purpose of replication is to advance theory by confronting existing understanding with new evidence. Ironically, the value of replication may be strongest when existing understanding is weakest. Successful replication provides evidence of generalizability across the conditions that inevitably differ from the original study. Unsuccessful replication indicates that the reliability of the finding may be more constrained than recognized previously. Defining replication as a confrontation of current theoretical expectations clarifies its important, exciting, and generative role in scientific progress.

Introduction

Credibility of scientific claims is established with evidence for their replicability using new data [1]. This is distinct from retesting a claim using the same analyses and same data (usually referred to as *reproducibility* or *computational reproducibility*) and using the same data with different analyses (usually referred to as *robustness*). Recent attempts to systematically replicate published claims indicate surprisingly low success rates. For example, across 6 recent replication efforts of 190 claims in the social and behavioral sciences, 90 (47%) replicated successfully according to each study's primary success criterion [2]. Likewise, a large-sample review of 18 candidate gene or candidate gene-by-interaction hypotheses for depression found no support for any of them [3], a particularly stunning result considering that more than 1,000 articles have investigated their effects. Replication challenges have spawned initiatives to improve research rigor and transparency such as preregistration and open data, materials, and code [4–6]. Simultaneously, failures-to-replicate have spurred debate about the meaning of replication and its implications for research credibility. Replications are inevitably different from the original studies. How do we decide whether something is a replication? The answer shifts the conception of replication from a boring, uncreative, housekeeping activity to an exciting, generative, vital contributor to research progress.

OPEN ACCESS

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Provenance: Commissioned; not externally peer reviewed.

Rynes and Bartunek (2017)

Evidence-Based Management: Foundations, Development, Controversies and Future.

> *Management academics have long noted a large gap between academic research and managerial practice. [...] Some have viewed the causes of the gap as lying primarily with academic researchers, who are characterized (perhaps caricatured) as having become overspecialized, self-referential, obsessed with theory, excessively mathematical, jargonladen, unconcerned about practical problems, and dismissive of practitioners [...] Others have focused on practitioners, who are sometimes characterized or caricatured as research phobic, anti-intellectual, susceptible to unproven fads and fashions... (p. 236)*

Rynes and Bartunek (2017)

Discussion Questions

- Are you familiar with evidence-based practice from your current work?
- In your PhD studies so far, have you seen a concerted effort to move towards evidence-based management?
- Where might you fit in helping to advance evidence-based management? How might you do about doing it?

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Evidence-Based Management: Foundations, Development, Controversies and Future

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Keywords

evidence-based management, evidence-based practice, research–practice gap, academic–practitioner relationships, systematic reviews

Abstract

We review the recent development of evidence-based management (EBMgt), tracing its origins to longstanding gaps between research and practice, discrepant findings across studies, and the emergence of evidence-based medicine (EBMed). We provide a definition of EBMgt and review four foundational articles advocating its use. We then review categories of articles that comprise the EBMgt canon: advocacy articles, essays or perspectives, teaching-related, empirical, reviews, and critiques and responses. Critiques include political, epistemological, and methodological issues directly pertinent to EBMgt as well as broader concerns about the scholarly research base on which EBMgt depends. Our suggestions for future research emphasize, first and foremost, increasing the production of high-quality empirical studies in EBMgt. Topics of particular interest include research co-creation by academics and practitioners, process and outcome studies of EBMgt implementations, and practitioners' use of evidence in their working environments. We also call for broader types of systematic reviews (SRs) than have generally been conducted in the organization sciences.

Summative lecture

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

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 my unique lens that necessarily abstracts away features from complex topics.

Some key principles of research design

- Falsifiability
- Defensibility
- Applicability
- Replicability

Falsifiability

- Falsifiability provides a basis for to use abductive reasoning to augment pure deductive reasoning. The latter is true a priori if the premises and statements are valid. Thus, pure deduction an transform our understanding of the system, but cannot generate truth from outside the established system - this must be uncovered by other means.

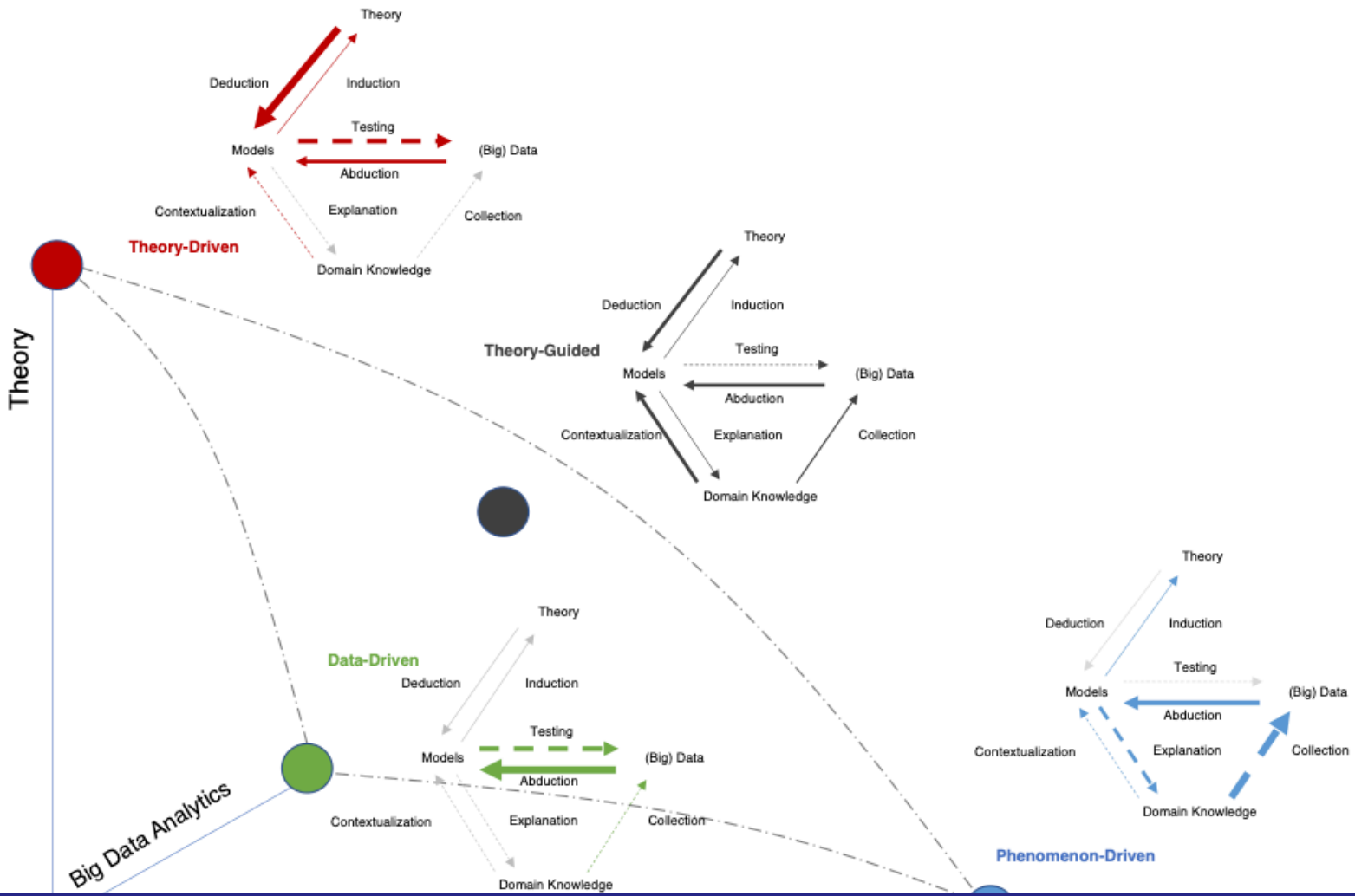
Falsifiability

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

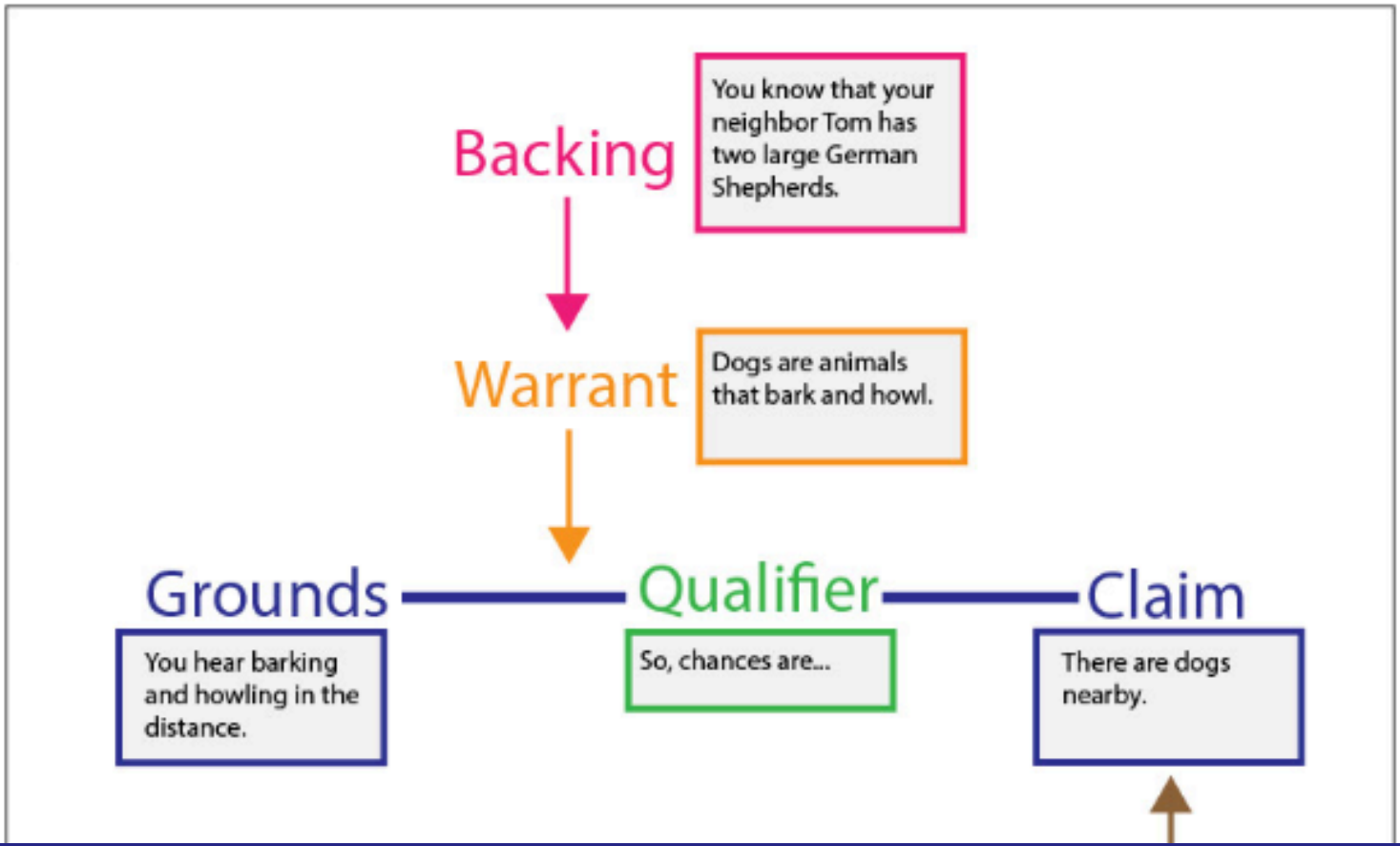
- If the logic of our arguments are defensible and the evidentiary basis is sound, we are better able to act upon the conclusions with confidence.
- We rely on multiple modes of inference to assert our claims credibility - my coauthors and I argue that the relative importance of each depends on the type of research design employed and intended contribution.

Defensibility



Defensibility

One way to map out the defensibility of an argument is with a Toulmin diagram:



Applicability

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

There are two corollaries to this:

- 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 ques- tion was fascinating. It addressed an extremely in- teresting 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.

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?

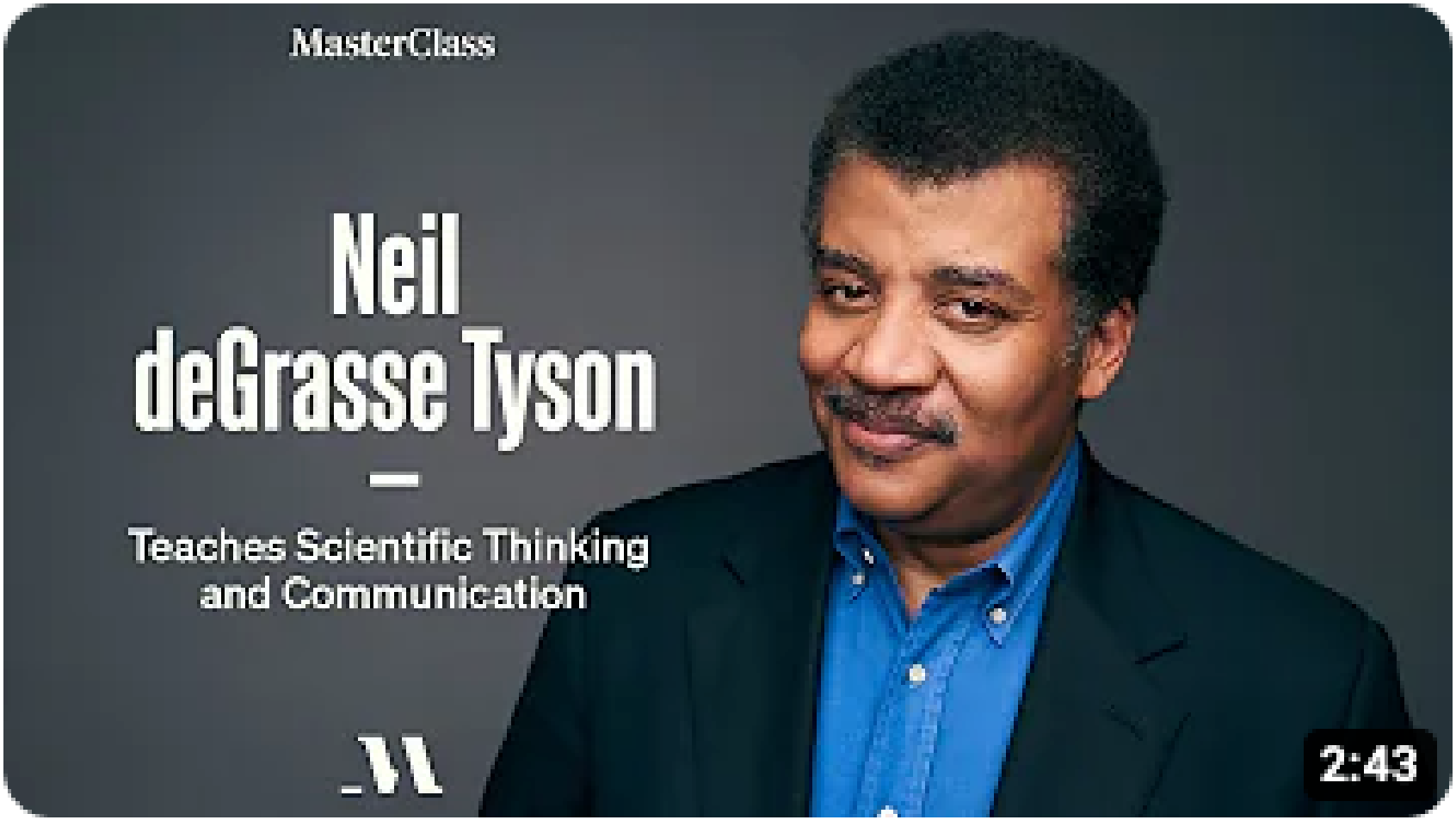
Other thoughts: Useful types of thinking when conducting research

- Skeptical thinking
- Bayesian thinking
- Strategic 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



https://www.youtube.com/watch?v=io6QdGcoWMU)

Bayesian thinking

Implicit in the discussions above is a question of 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

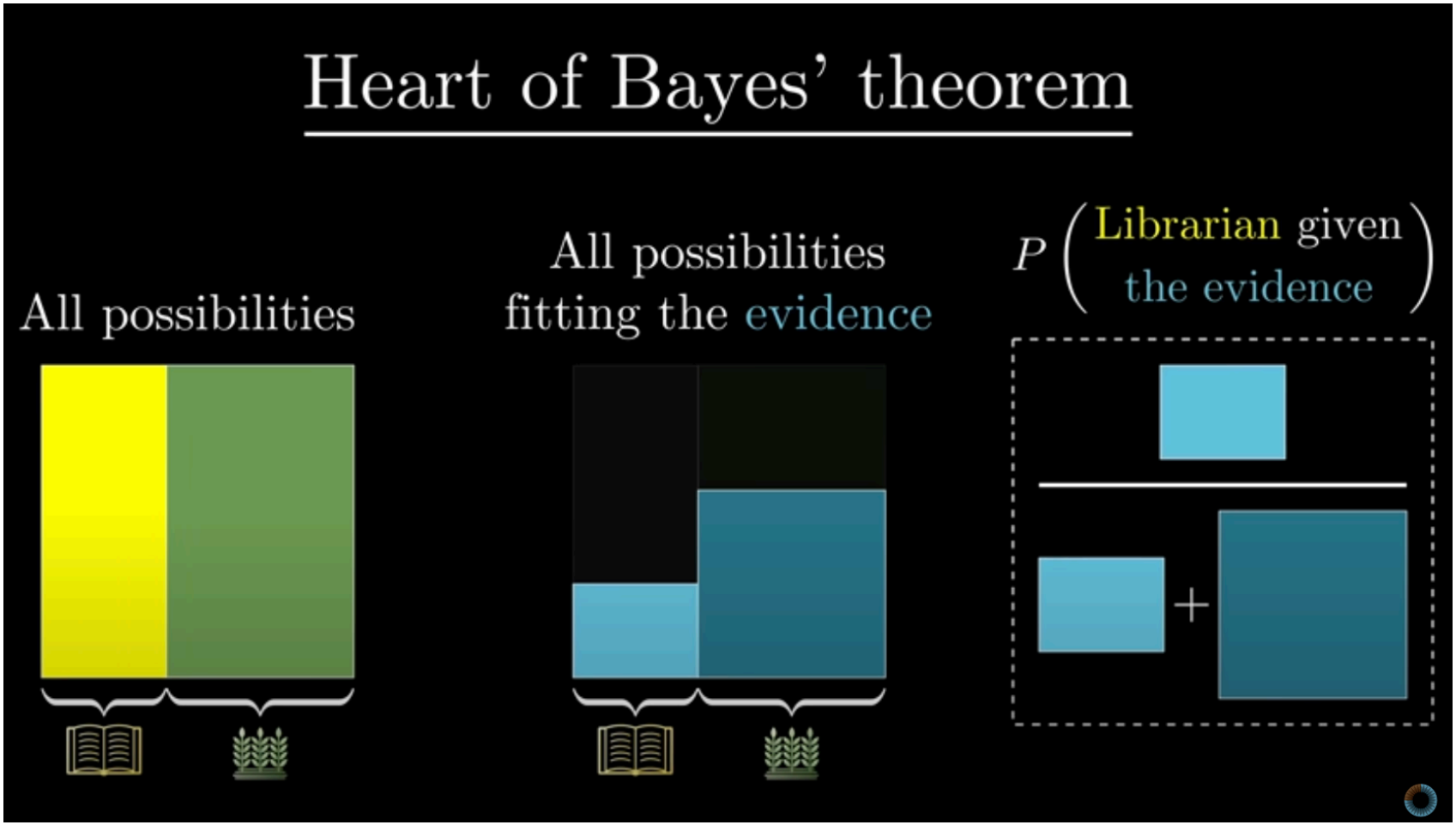


Figure 4: A Primer on Bayesian Thinking

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
  - Inferential – moving from what is known to what can be reasonably inferred

Strategic thinking

- Complexity
  - Dynamics – accounting for first and second order effects that are material across actors, choices, and time
  - 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
  - Unforeseeable – many research projects are a full reinforcement learning problem, learning while doing is necessary to reveal the evolving state of the world

Next class

Research II: Problems

1. Huff, A. S. (1999). Writing for Scholarly Publication. SAGE. [Ch. 1]
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: Seeing What Everybody Has Seen and Thinking What Nobody Has Thought. Journal of Management, 48(6), 1347-1365.

Next class

Research II: Problems

Our first compare and contrast discussion will take place.

Presenters, please reach out if you have questions or concerns!

4. Compare / Contrast
   - Daft, R. L., & Lewin, A. Y. 2008. Rigor and relevance in organization studies: Idea migration and academic journal evolution. Organization Science, 19: 177-183.
   - 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. https://doi.org/10.2307/20159888

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