What do we mean when we say “theory”?

Christopher Thomas Ryan, February 24, 2026

When I was a young professor, I met with a more established colleague at a conference, and as one does in academic circles, we asked each other about our research. I said I did “optimization theory”: proving theorems about linear programs in various strange settings, pushing the structure of existence, duality, and optimality far beyond anything that would ever plausibly appear in a factory, a hospital, or a call center.

My colleague’s reputation preceded him. I knew his work as what operations researchers often call “stylized models”, in his case, carefully simplified abstractions of customer behavior in queuing systems, models meant to isolate core mechanisms and generate insight into congestion, incentives, and performance. His work was mathematically sophisticated, but it was also unmistakably oriented toward modeling and understanding behavior. He was trying to say something about how people actually act in service systems using mathematical tools.

But when I asked him about his research, he also described his work as “theory.” I was surprised.

At the time, my intellectual map was simple. There was *theory*, and there was *applications*. Theory was mathematics: optimization theory, graph theory, set theory, game theory. Applications were what happened when one took those tools out for a walk and applied them to a somewhat real-looking problem. In that mental geography, my colleague was obviously an applied researcher. So why was he calling his work “theory”?

When I described my own research as “optimization theory”, he was polite and respectful, but he suggested that what I was doing might better be called “methodology.” I left that conversation unsettled. We were in the same field, publishing in some of the same journals, speaking to many of the same audiences, and yet we seemed to be using one of our most central words (“theory”) in entirely different ways. That confusion stayed with me for years, in some form, maybe right up until the writing of this essay.

What finally clarified things was finally resolving to read a few dictionary definitions of the word “theory” and realizing that the word did not describe a single concept at all, but instead carried at least three distinct meanings arising from different traditions.

The first notion of “theory” comes from the hard sciences. In the sciences, a theory is an explanatory structure. It is a provisional account of observed phenomena that makes predictions and risks being wrong. Darwin’s theory of evolution, Newton’s theory of gravitation, and the germ theory of disease are paradigmatic examples. A theory in this sense earns its status by surviving confrontation with data. Its authority is empirical and always conditional. This is the sense of theory that animates Popper’s insistence on falsifiability. I will call these *scientific theories*.

The second sense of “theory” is mathematical. In mathematics, a theory is a deductive universe: a system of definitions, axioms, and theorems. Theories are not necessarily meant to explain anything in the empirical world. They are not necessarily derived from observation and are not validated by experiment. They are validated by logical proof. Their truths are conditional but necessary: *if* the axioms hold, *then* the theorems follow. Let’s call these *mathematical theories*.

For people who grew up in the mathematics tradition, as I had, it is very tempting to think of a proven mathematical theory as a really strong (possibly idealized) version of a scientific theory. The former has been proven and the latter is only supported by imperfect evidence. But this belies their major structural difference. Scientific theories are explanations of observed reality, while mathematical theories live within a system of logic that need not refer to any external reality.

But what I found interesting as I explored the word "theory" more deeply, I came to understand a third sense of the word. This notion arises neither from scientific experiments nor mathematical deductions but from accumulated practice. Here, "theory" means a set of principles on which practical action is based. We may speak of a "theory of law" or "music theory" when we mean a structured conceptual framework that helps people speak about their experiences and organizes their professional judgment. Such theories are not necessarily tested like physical laws, nor are they purely deductive. They are normative, interpretive, and pedagogical. They are gleaned and codified as a conceptualization of experience. I will refer to these as *practice-based theories*.

With these three senses of "theory", my old conversation with my senior colleague started to make more sense. My colleague used game theory and queueing theory as mathematical theories to build models that aspired to be scientific theories. His goal was explanation: to account for patterns of behavior in queues, to generate predictions, and in principle, to allow confrontation with data. That is why he called his work "theory" and he was perfectly justified in doing so.

My own "optimization theory" was, of course, a mathematical theory. I was not trying to explain observed behavior. I was not distilling professional experience. I was exploring the logical structure of optimization problems in abstraction. My results were not provisional; they were certain once assumptions were granted. I made no observations, I did not "test" my results, I simply "deduced" them and called it a day. Unlike some optimizers, I did not implement my algorithms and I did not solve problems with real data. What would that prove? Why solve a few problems when you can solve *all* of them in theory with an algorithm?

I honestly did not think about how my theories connected to the observable world. I believed that the system of logic and definitions that animated optimization theory constituted its own pristine "world". I was discovering beautiful truths in this beautiful world. What more could I want? When my colleague characterized my work as "methodology," he may have been locating it not along the theory–application axis I had in mind, but along a different one: the axis of tools versus explanations. I was building tools—mathematical machinery that others could later use to construct scientific or practical theories. He was building explanations. But I did not see it that way. I truly felt that I was not constructing tools to be "used" for the real world, I was unleashing masterpieces for their own sake in an elegant mathematical world.

What makes this personal anecdote more than a curiosity is that it mirrors in many ways a foundational ambiguity inherent in the field of Operations Research since its founding.

An early example of this tension arises at only the second annual meeting of The Institute for Management Science (TIMS) in October 1955 during Merrill Flood's address on the objectives of TIMS.[1] Flood is an unapologetic champion of foundational mathematics behind "management science" (which I argued in a previous essay is roughly synonymous with operations research, but with some hard to parse distinctions). He explicitly celebrates von Neumann's game theory as a "shattering advance in the science of decision making," even though it did not arise from any managerial problem and was not tested against

[1]Published later as the article: Flood, Merrill M. "The objectives of TIMS." Management Science 2.2 (1956): 178-184.

data. He insists that management science must welcome theoretical developments that originate entirely within mathematics. Flood clearly sees the value of "mathematical theory", championing game *theory* as a pillar of management *science*.

At the same time, Flood frames management science as a scientific discipline in an empirical sense. In his examples of successful OR projects, he emphasizes a familiar scientific pattern: build a model, derive predictions, compare them with observational data, and subject them to critical tests in the field. This is much closer to scientific theory: a mathematical model of a real-life phenomenon is a "theory" worth testing empirically. Flood did not make a distinction between mathematical theories and scientific theories; they were all just theories that made up management science.

Reading Flood's talk, one can easily get a little confused. Indeed, C. West Churchman published a short article that appeared directly after Flood's talk in the January 1956 issue of *Management Science*. Churchman expressed a danger in letting different meanings of "theory" blur. In this short but powerful note entitled "Management Science—Fact or Theory?", Churchman observes that when OR researchers speak of inventory *theory*, queueing *theory*, or game *theory*, they are usually using the word in the mathematical sense:

> What we mean by 'theory' in this context is that if certain assumptions are valid, then such-and-such conclusions follow. Thus, inventory theory is not a set of statements that predict how inventories will behave... but rather a deductive system which becomes useful if the assumptions happen to hold.

This usage of "theory" would be familiar to mathematicians who joined the ranks of operations researchers during the 1950s, but with the advent of the journal *Management Science*, the community was now encountering the notion of management as a "science", where one might expect the more common usage of the word "theory" in the halls of the hard sciences.

Churchman seems especially concerned about mathematical theories masquerading as scientific theories, either intentionally or unintentionally. He presses an uncomfortable implication of this tendency: if we insist on viewing something like game theory as a scientific theory, then we must accept the burden of empirical prediction and validation as a judge of the value of such theories. But this raises an uncomfortable situation for Flood. Flood lauds game theory as a "shattering advance" in management science, but as a *scientific* theory (meant to provide an explanation of observed phenomena), its track record is much more checkered.

The classic Prisoner's Dilemma game was first devised in the early 1950s by Merrill Flood himself (along with Melvin Dresher) at the RAND Corporation, with its familiar "prisoner" framing introduced later by Albert W. Tucker to make the model accessible to a wider audience.[2] The canonical one-shot version of the game has a unique Nash equilibrium in mutual defection, which follows directly from the payoff structure: each player's best response to the other's choice is to defect, even though both would be better off cooperating. When Flood and Dresher had colleagues play the game, cooperative choices often emerged in early rounds, a finding that diverged from the simple equilibrium prediction.[3] Subsequent

[2] See "Prisoner's Dilemma," *Wikipedia*, accessed January 25, 2026, https://en.wikipedia.org/wiki/Prisoner%27s_dilemma.

[3] See Steven Kuhn, "Prisoner's Dilemma," *Stanford Encyclopedia of Philosophy*, accessed January 25, 2026, https://plato.stanford.edu/entries/prisoner-dilemma/.

experimental economics research has consistently documented substantial rates of cooperation in laboratory Prisoner's Dilemma games, even in finitely repeated or one-shot settings, with cooperation levels often far above what strict Nash logic would predict.[4]

As a *mathematical* theory, game theory consisted of a tight and elegant set of axioms, definitions, and proven results. As a *scientific* theory meant to explain strategic behavior in practice, one could be forgiven for not calling it a "shattering advance" given its empirical record by the mid-1950s.

In his essay, Churchman writes sharply about this distinction, lamenting a lack of scientific theory building in the pages of *Management Science*:

> ...if one cannot forecast (predict), one does not have a theory ...forecasting is the weakest link in the chain of the management sciences at present. Indeed, if one cannot predict one cannot measure, and if one cannot measure, then there is no "theory"....I hope that the future issues of the journal will more and more emphasize the problems of measurement and data-determination, just as they have emphasized "theoretical" formulations in the past.[5]

Flood and Churchman are not the only two early leaders of operations research to explore the nuances of these "theoretical" distinctions. Earlier in 1955 at the ORSA annual meeting on August 15, Phillip Morse (the founder of MIT's Operations Research Center) gave an address entitled "Where Is the New Blood?", which explores related issues but from another angle.[6] Consider the following quote:

> Just as with any other field of science, we are finding that we need our own kind of mathematics. Certain aspects of probability theory and of combinatorial analysis turn out to be most appropriate for our particular problems. These parts of mathematics now must be investigated in more detail than they have had to be before, so that our progress will not be held up by theoretial difficulties. Waiting-line theory, for example, is but a small part of the theory of Markov processes; nevertheless, a considerably greater amount of basic work must be carried through before we can predict the behavior of the many sorts of waiting lines which occur in industry and in governmental operations.

Viewed in the light of our different notions of "theory" I have been exploring in this essay, we can see Morse change in his usage more than once, sometimes within a single sentence. He describes waiting-line *theory* as a "small part of the *theory* of Markov chains" (emphasis added), suggesting an interpretation of waiting-line theory as a mathematical theory. Indeed, Markov chains are a mathematical theory, consisting of a collection of axioms, definitions, and results, developed without specific reference to any real-world phenomena on which it is tested. But when Morse later says that additional work must be done to test the predictions of waiting-line theory, this suggests that he understood waiting line theory is

[4]See Leonie Heuer and Andreas Orland, "Cooperation in the Prisoner's Dilemma: An Experimental Comparison," *Royal Society Open Science* 6 (2019), https://royalsocietypublishing.org/rsos/article/6/7/182142.

[5]Interestingly, *Management Science* now does publish many papers that focus on measurement, data collection, experimental outcomes, and econometric studies. It just so happens that the vast majority of these papers are published by authors not strongly affiliated with operations research/operations management community, including researchers of finance, behavioral economics, accounting, marketing, strategy, and management.

[6]Published as Morse, Philip M. "Where is the new blood?." Journal of the Operations Research Society of America 3.4 (1955): 383-387. The Journal of the Operations Research Society of America was later simply published as *Operations Research*.

a *scientific* theory whose validity depends on empirical justification. One cannot help but ask, is waiting line theory both a mathematical *and* scientific theory? Is that possible?

Later in the talk, Morse shows his passionate commitment to the idea that OR is a science in the empirical sense. He writes:

> Operations research is an experimental science, concerned with the real world. It is not an exercise in pure logic. We must make our theories correspond to actual operations, and to do this we must compare predictions with actual occurrences in a quantitative manner.

He insists that theories must be confronted with actual operations, that predictions must be compared quantitatively with outcomes, and that practical experiments are essential.

Morse appears to hold together two potentially conflicting notions: OR is not an exercise in pure logic but OR also requires its "own kind of mathematics". It must develop both kinds of theory: mathematical and scientific. Morse recognized intuitively that OR lives simultaneously in these two epistemic worlds. It was a realization that I had lost (or never had) some 60 years later while speaking to my colleague. We were both doing OR theory, I was developing *mathematical* theory while my colleague was developing *scientific* theory.

But despite Morse's enthusiasm for OR as scientific theory, he admitted that much of this work was not being done. In particular, Morse offers a partial explanation why operations researchers were wont to shy away from testing theories in the scientific sense:

> I realize that there are plenty of reasons why we have lagged in developing the experimental side of operations research. Computations and equations put down on paper don't upset the boss; but it takes a lot of argument to persuade him to monkey with the production line. The human participants in the operation are busy doing their job; they don't want to be pestered. It's much easier to dream up a theory.

I must admit I am not exactly sure what he means by "theory" at the end of this quote. A scientific theory that is never tested? Or a mathematical theory that never needs to be tested? Both seem possible.

So far, I have only talked about the first two senses of "theory" I defined earlier in this essay. What about this third sense, what I called *practice-based theory*? Is OR also in pursuit of such theories? This is where things get even more complicated.

Consider a highly influential paper in the early decades of operations research by (again!) Churchman and Schainblatt entitled "The Researcher and the Manager: A Dialectic of Implementation".[7] They argue that even when mathematical theory and scientific theory are sound, neither automatically becomes operational knowledge. They do not necessarily form a workable theory of practice. Implementation is not a technical step but a social relationship between researcher and manager. Mathematical and scientific theories do not automatically flow into action; they must be translated, negotiated, and embedded in institutional practice.

This introduces yet another sense in which "theory" becomes unstable. A perfectly valid mathematical theory can fail in practice. A scientifically plausible explanatory theory can fail to influence practitioners. Moreover, a beautifully articulated practical theory of management can fail to be mathematically

[7] Churchman, Charles West, and A. H. Schainblatt. "The researcher and the manager: A dialectic of implementation." Management Science 11.4 (1965): B69-B87.

or scientifically grounded. Churchman and Schainblatt speak about the reluctance of the operations researcher (whom they refer to as "the scientist") to engage in implementation:

> To the consciously observing mind of the scientist, a great deal of managerial activity is political, and the reasons given for decisions are rarely the real reasons, i.e., are almost always unconscious. On the other hand, once a decision has been made, the manager seeks to find perfectly conscious and justifiable reasons why this decision is best.

The practical world of decision making seems quite far indeed from my pristine and pure "world" of mathematics. Who could run a real experiment in such a political morass as the modern corporation? Who really wants to leave the ivory tower to play in the dirt?

But failing to engage in this way, possibly out of a desire to remain "pure", can hinder the power of OR theory. Consider a couple of examples of profound practice-based theories of operations management of significant consequence to industry practice, and reflect on your reaction to the "validity" of these theories from your position as a scientist.

Lean thinking, and the influence of W. Edwards Deming in particular, illustrate how some management paradigms function as theory in a practice-based sense, by providing a coherent framework for interpreting and guiding organizational behavior rather than by producing predictive scientific laws or mathematical toolsets. Deming's "System of Profound Knowledge" and his famous "14 Points for Management" articulate a set of principles for transforming the practice of quality management that have profoundly shaped operational improvement efforts worldwide. These principles are drawn from Deming's experience with management practice and consulting, emphasizing systemic thinking, understanding variation, and fostering learning and cooperation across an organization. They were not all developed through controlled scientific experiment so much as through narrative, case study, and practitioner observation.[8]

Russell L. Ackoff's work later in his life at Wharton also exemplifies a similar practice-based conception of theory in management. Ackoff, who began his career in operations research and later became one of the leading proponents of systems thinking in organizational contexts, developed concepts such as interactive planning and the well-known "f-Laws," a set of distilled observations about how organizations behave and how common managerial beliefs can mislead practice.[9] These f-Laws, and Ackoff's broader systems perspective, are not scientific hypotheses to be tested in controlled experiments, but rather *heuristic principles* and reflective insights intended to help practitioners recognize and rethink entrenched assumptions about organizational design and management. Ackoff communicated much of his influence through stories, parables, and epigrams that convey practical wisdom drawn from experience, illustrating what one can understand to be a practice-based theory of managing operations.

Taken together, the ambiguity surrounding "theory" in OR is maybe not a terminological accident. It reflects the field's impossible ambition: to be simultaneously mathematics, science, and practice. To be mathematically deep, face tough empirical tests, and inform industry practice. We want our "theories" to have it all.

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[8] Anderson, John C., Manus Rungtusanatham, and Roger G. Schroeder. "A theory of quality management underlying the Deming management method." Academy of management Review 19.3 (1994): 472-509; see Anderson et al. for discussion of the 14 Points as principles derived from practice.

[9] For more details, see "F-Law," *Wikipedia*, accessed January 26, 2026, https://en.wikipedia.org/wiki/F-Law.

Many of us, including my younger self, want a cleaner taxonomy. Theory here, application there. Tools here, explanations there. But operations research refuses such neat partitions. It inhabits a structurally unstable space between proof and evidence, between elegance and dirt, between abstraction and institution. My colleague and I were not disagreeing on substance. We were pointing to different meanings of the same word, each historically legitimate, each philosophically grounded, and each indispensable to the field.

The confusion was not ours alone. It was, and remains, one of the core confusions of operations research itself. What do we want our "theories" to achieve? Can we have it all?