



What theory is – A late reply to Sutton and Staw 1995

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ABSTRACT

Theory plays a central role in research in management science, with theoretical contribution an essential measure for evaluating research. However, the many ambiguities regarding the use of the theory concept make judgments imprecise and, to an extent, arbitrary. The discussion of the concept of theory in management science has remarkably little anchoring in the findings of the philosophy of science. This article presents some of these findings and discusses the concepts of explanation and theory on this basis. In particular, Sutton and Staw's (1995, p. 385) notion that theory should provide a logical explanation of causal relationships formulated in hypotheses is critically questioned.

1. Introduction

Theory plays an important role in management science, and an essential criterion for evaluating research is its theoretical contribution (Colquitt & Zapata-Phelan, 2007; Rynes, 2005). In his editorial, Suddaby (2015, p. 2) states, "Knowledge accumulation simply cannot occur without a conceptual framework." Hambrick (2007) even diagnoses a "devotion to theory." In view of this central importance, however, surprisingly little attention is devoted to the object itself, the theory, in management science, its understanding only weakly anchored in the findings of the philosophy of science. The philosophy of science has developed the understanding of theory over centuries and gained valuable insights in the process. These insights usually receive little attention in management science, especially in influential contributions dealing directly with theory or theoretical contributions, such as Corley and Gioia (2011), Hambrick (2007) and Whetten (1989). Consequently, Suddaby (2015, p. 1) diagnoses, "we appear to disagree as a profession about why we need theory and what role it should play in creating, maintaining, and shaping what type of knowledge we value in the field." Similarly, Brunsson (2021, p. 1) sums up, there is "little agreement" about "what theory is," and "many issues remain unsolved." A central benchmark for assessing research thus remains unclear, and judgments become, to a degree, arbitrary. Management science sees too little effort to counteract this shortcoming.

The essay "What Theory is not" by Robert I. Sutton and Barry M. Staw, published in 1995 in ASQ remains an essential reference point for the understanding of theory in management science. The importance of this essay is demonstrated not only by the fact that it has 2795 citations

in Google Scholar (as of August 6, 2022), but also that 25 of these citations have been added since July 7, 2022, in a mere month. This shows how widely the essay is still used today. It is therefore no exaggeration to call this essay a milestone in understanding theory in management science. It has largely shaped the understanding of theory in management science and continues to do so today.

To be clear, Sutton and Staw are not to blame for the position of their paper, written as an editorial, now holds. However, views from this essay were and are adopted with little critical questioning. The view of theory as a logical explanation of causal relationships, i.e., that it provides an explanation as to why certain causal relationships exist, is particularly problematic. This view not only contradicts the fundamental insights of the philosophy of science, but furthermore, it conveys a problematic impression of the epistemological status of hypotheses.

This paper aims to initiate a debate that creates a stronger connection to the philosophy of science and discusses the implications of its findings for research in management science. To this end, I begin by showing the importance of theory for research, particularly for explanation and prediction. Following, I specify the concept of theory based on an open definition and show similarities and differences between different positions. Building on this, I explain the challenges that management research faces when developing theory (theory building and testing) and the consequences that result. Against this background, I show why the requirement of Sutton and Staw (1995, p. 375), according to which it is the task of theory to "explain why variables or constructs come about or why they are connected," is so problematic.

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2. What is an explanation?

A basic idea of theory was already formulated by Plato in reply to Heraclitus. In *Cratylus*, 509a (Plato & Reeve, 1998, see also Keller, 2000) Plato attributed to Heraclitus the well-known assertion that everything is in constant motion (*panta chorei*), and contesting this, Plato replied that there are things not subject to change, especially so-called forms (Russell, 1946). According to Plato, forms are ideas and as such are the non-physical essence of all things. It is particularly important that these are stable, absolute, and universal. Plato gives the form of the cat as an example. This form is based on many individual characteristics (every cat is different), but the form itself is universal (they are all cats; note that Plato, of course, had no concept of biological evolution). The crucial point is that in this way we can classify phenomena and furthermore, look into the future, because we know that cats will continue to exist in the future. The cat's form will continue, just as the dodo's form continues even past extinction. Much of Plato's theory of forms cannot be maintained in its absoluteness, but in stability it names an essential cornerstone of theory.

The next logical step is the natural law formulated in its modern form by René Descartes (Descartes, Veitch & Hoyt-O'Connor, 2008). Not only are forms stable, causal relationships are likewise stable. This allows new access to the concept of understanding. We can now explain and even predict things, but what exactly does that mean, and how does theory factor into it?

The covering-law model formulated by Hempel and Oppenheim in 1948 provides a widely recognized specification of the structure of explanation and prediction. An explanation therefore provides a reason for an empirical fact, i.e., why things happened. This reason consists of a causal conclusion composed of two conditions (the explanandum) and a conclusion (the explanans). Fig. 1 shows the structure of the covering-law model and an explanation based on this model.

The covering-law model establishes a connection between observed initial conditions and an observed result on the basis of a generally valid causal relationship. The water is boiling *because* it was heated to 71 degrees Celsius and *because* water on Mt. Everest boils at 71 degrees Celsius. In this way, the covering-law model provides a logical explanation for the occurrence of a specific event.

Hempel and Oppenheim postulate that prediction and explanation are related symmetrically to the time axis, with explanation relating to the past and prediction relating to the future. From this, Hempel and Oppenheim (1948, p. 323) develop the demand, "It may be said, therefore, that an explanation is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the phenomena under consideration." This requirement is fairly strict and not uncontroversial (Caldwell, 1982), but is at the same time very instructive.

The covering-law model allows for the possibility to explain why

things occurred in the past, and furthermore, allows a glimpse into the future. We know much more now than that there will be cats in the future. We can foresee things (in a loose sense of the word) and know under what conditions they will occur, but more importantly, we can now assess the consequences of actions.

All of this is highly relevant for management science, which is strongly decision related. Here, alternatives are compared and the consequences of decisions shown (e.g., for different approaches to internationalization or different leadership styles, different strategic positioning on markets, etc.) Forecasts are particularly relevant for the practical application of management research in a specific company context, for managerial decision-making, or for management consulting. For such considerations and recommendations, logical conclusions in the form of the covering-law model are indispensable. Therefore, I would venture to claim that the thinking pattern of the covering-law model plays a major role in management science.

It is epistemologically of great importance that in the covering-law model, statements of type (i) [the "natural law"] represent a fundamentally different class of statements as opposed to statements of types (ii) [the initial condition] and (iii) [the state of affairs to be explained]. In Kant's terminology, both represent synthetic judgments, as they combine a subject with a predicate that is not contained in the concept of the subject (Kant & Meiklejohn 2018). However, (i) is a matter of universally valid causal relationships (in Kant, synthetic judgments a priori), and in (ii) and (iii), observable singular events (in Kant, synthetic judgments a posteriori). They are therefore relatively easy to distinguish, and there is an epistemological watershed between them. This distinction plays a central role in the philosophy of science.

It is also epistemologically significant that explanation and prediction in the sense of the covering-law model cannot do without statements of type (i), i.e., without universally valid causal relationships. These first establish the connection between the initial condition and the event to be explained. This brings us directly to the notion of theory.

3. What is theory?

Descartes' conception of the law of nature, as well as more contemporary concepts such as that of axiomatic theory (Bourbaki, 1994) or the "hypothetico-deductive" model of Carnap et al. (2019), Hempel and Fetzer (2001), suggest that theory, at its core, is about the formulation of generally valid relationships of type (i) in the covering-law model. If that is the case, the definition of theory is quite unproblematic. I would like to use an "open definition" for this, which distinguishes between essential characteristics that are largely shared and characteristics about which there is often no consensus.

The essential characteristic of theory is then that theory consists of the determination of conditionally universal connections between constructs (at this point, I am already using the contemporary term). This is

Explanans: the cause, conditions sufficient to explain the occurrence of an event.	Example
(i) Establishing a "law of nature" that describes a generally valid causal (cause-effect) relationship of the form $A \rightarrow B$.	(i) There is a fixed functional relationship between the boiling point of pure water and the air pressure (I will spare you the exact formula here).
(ii) Establishing an initial condition, i.e., a singular empirical fact.	(ii) This particular pure water was heated to 71 degrees Celsius at the top of Mt. Everest.
Explanandum: the effect, the event explained by the explanans.	
(iii) Determination of the singular event that is to be explained.	(iii) This particular pure water has boiled on Mt. Everest.

Fig. 1. Schematic representation of the Covering Law model.

where the decisive epistemological difference lies: theory is a matter of general statements that include reference to the future, while observations, on the other hand, are singular statements that always relate to the past (arguably the present as well) but never the future; it is impossible to scientifically observe future events. The basic form of a theoretical statement is thus $A \rightarrow B$, with “A” and “B” representing constructs. I have denoted the conditionally universal connection between the constructs in this vagueness with the symbol “ $\rightarrow$ ” (not with the causal connection “ $\rightarrow$ ”, to be elaborated upon later). Theories can be complex and extend beyond the basic form $A \rightarrow B$. They can include a variety of constructs, as well as mediators and moderators. The conditionally universal connections are and will remain the decisive factors. This definition is quite close to that of Bacharach (1989, p. 496), according to whom theory offers “a statement of relations between concepts within a set of boundary assumptions and constraints.”

The conditionality of universality refers to the constructs, A and B. Theory does not make any statement outside of A and B, but rather is limited to precisely these constructs. An assessment of the empirical validity of the theoretical connection therefore requires that A and B are well defined. If this is not the case, the validity of the theoretical connection also becomes unclear.

Additionally, the conditionality of universality relates to the moderators and mediators that further limit and determine the validity of the theoretical context (the famous “it depends”). The validity of the connection is thus limited by conditions or determined by third factors. Within these conditions, however, the connection is universal. In the example of boiling water, the boiling point depends on factors such as sea level and salinity of the water. This conditions the relationship between temperature and boiling point, making it more complex.

This definition of theory is certainly very broad, but at the same time, very specific, as it designates its own class of statements and is precisely delimited. With this definition, theory is by no means in danger of becoming “meaningless” (as Merton, 1967 fears, quoted in Sutton and Staw, 1995: 371).

Up to this point, there should be considerable agreement in our discipline. Disagreement, then, primarily stems from two questions, namely, what constitutes the substance of the connection between the constructs, and in which form the theory should be presented.

In his seminal work of 1748, David Hume showed that the notion of causality is problematic in that causality itself cannot be observed (Hume & Beauchamp, 2000). Observations are limited exclusively to sequences of events, and causality can only be inferred from these. There have been many attempts to overcome this problem, but to my knowledge, none have succeeded. Consequently, causality is always a construct, an assumption. This applies not only to causal relationships, but also to the concept of causality as such. It can be proven neither that causality is stable nor that it even exists. In response, in his quest to ban all metaphysics from science, Mach (1976) proposed replacing causality with functional relationships. However, these should not be understood as independent statements (in the sense that such functional relationships actually exists), but merely as an aid to organizing data. However, it was soon recognized that this view does not allow the use of functionality for explanation and prediction and thus deprives science of essential content. As a result of this and other discussions, causality has asserted itself, as it has ultimately proved indispensable for scientific work. This specifies the conditionally universal connection between the constructs, and the new definition is that theory consists of the establishment of conditionally universal *causal* connections, in the notation of Hilbert and Ackermann (1950), $A \rightarrow B$. However, causality remains a construct, and a certain skepticism about causality is therefore necessary. This is particularly important for theory building.

Moreover, there is disagreement regarding different ideas as to which form theory must suffice. Such ideas are found implicitly in Sutton and Staw (1995) and their statement that variables, diagrams, and hypotheses are not about theory. However, they can also be found in Weick and his idea of the “fully blown theory” (Weick, 1995, p. 385). I

have often encountered this idea myself in various forms, and it seems to have become deeply ingrained in our discipline.

Indeed, theory is presented in very different forms, with the most developed being the so-called axiomatic theory (Bourbaki, 1994). It contains a complete specification of all elements of a theory, the alphabet, the formation rules, the axioms, and the rules of inference (Kesting & Vilks, 2005). Axiomatic theory consistently distinguishes between first propositions, the axioms, which cannot be concluded from the system, intermediate steps, the so-called lemmas, and theoretical statements which can be logically concluded from the system, the theorems. In economic theory, Debreu (1959) comes quite close to this form. In management science, however, there is hardly anything comparable and little to no fully developed theory in this sense. In addition, research in management science is characterized by little use of logical or mathematical conclusions drawn within theory, i.e., we seldom use any formal models. This is not necessarily a weakness, and there is good reason to refrain from fully formalizing theory (Kesting & Vilks, 2005). Nevertheless, we should be aware of how far most research in management science is from a fully developed theory in the sense of an axiomatic theory.

On the other hand, the basic form of theory remains, necessitating universal statements in the form $A \rightarrow B$. Such sentences can offer valuable gains in knowledge, even in isolation. As an example, Galinsky and Mussweiler's (2001) found that the first offer to negotiate has a strong influence on the final outcome. With its high empirical relevance (Orr & Guthrie, 2005), this sentence offers valuable insights, even in isolation, for individuals faced with the decision of making the first offer or waiting for the other party. As Weick (1995) rightly points out, variables, diagrams, and hypotheses can also represent a valuable contribution to theoretical work. In principle, theory can therefore also be very simple.

In management science, theory is rather fragmented. Many studies only examine a handful of hypotheses (Sandberg and Alvesson, 2011), and these are often included informally (sometimes hardly at all) in the research context. Limitation to a few hypotheses is methodologically unavoidable, especially in quantitative studies; however, a more comprehensive scope and bigger picture can aid our understanding of causal relationships and help capture the complexity of decision-making situations. When making decisions, is often impractical to concentrate on individual aspects and ignore others. Holding to the example above: there is already value in knowing that there is a stable connection between the first offer and the final outcome. To make a good decision, however, it is helpful to know more, e.g., when and for what amount to make the first offer, when it is better to refrain from the first offer, and how all this fits into the overall negotiation context. Complex theories such as behavioral decision theory or transaction cost theory also serve importantly as a theoretical lens for research. Additionally, complex theories play an important role in management education. Consequently, there is an obvious need for complex theories, and the concept of the full-blown theory can fit here. But how is this term to be understood conceptually? What constitutes a full-blown theory, and how is it to be presented? What demands are to be made of a full-blown theory? Does this have to be summarized in one comprehensive, logically consistent building (in the sense of a “classic achievement” according to Schumpeter (1994)), or is a partially inconsistent body of literature sufficient? At this point, I would like to emphasize the value of conceptual studies that establish such overarching connections, as my impression is that the value of such studies is insufficiently recognized.

4. The development of theory

4.1. Theory building

The main difficulty in constructing theory stems from a lack of direct connection between empirical and theoretical knowledge. Not only that causality, as presented above, cannot be observed. In addition, there is

no inductive inference; it is not possible to draw conclusions about universal connections from singular observations. Hume recognized this as early as 1748. In its quest to remove all metaphysics from science, logical positivism has done everything it can to overcome Hume (Caldwell, 1982) and failed gloriously. Gloriously, because the researchers involved were their own biggest critics, discontent with simple solutions. This failure has found its expression in the renaming of logical positivism as logical empiricism. I am convinced that the claim of positivism to build science on reliable knowledge and refrain from any speculation or metaphysics has failed, at least for the moment. But with that, the platonic idea that it is possible for the prisoner to leave his cave through true insight has also burst. This characterizes current science like hardly any other finding.

Consequently, theory cannot be derived from observation or other evident knowledge but is always constructed. Until Hume is overcome, this is one of the fundamental principles of research. Statements such as, "Most qualitative papers advance theory by building it inductively" (Bansal & Corley, 2012: 509) are counterproductive in their suggestion that such a connection could exist after all. My research experience, along with the fact that constructivism is still understood as a philosophical option, casts doubt that this insight has really penetrated our discipline. I take issue with the concept of constructivism because there is no counterpart to it – we are all constructivists when we formulate theory. To be very clear at this point, it is not possible to induce theory from any data. Caldwell (1982: 51) formulated the consequences of this in all his radicality: "Again, it is well known that for any set of data, an infinite number of theories can be developed to explain them."

The construction of theory thus becomes a creative process. However, as early as 1908, well before Popper, Schumpeter pointed out that this process is not arbitrary, but that the construction is carried out with regard to an explanatory goal.

"The purely static economy is nothing else than an abstract picture of certain economical facts, a schema that is supposed to serve for the description of the same. It is based on certain assumptions and insofar a creature of our arbitrariness, just the same as that every other one is exact science. So if the historian says that our theory is a figment of our imagination then he is correct in a sense. Surely, in the world of phenomena itself there are neither our 'assumptions,' nor our 'laws.' But an objection against the same does not follow from it yet; because this does not prevent that they suit the facts. Where does this now come from? Simply from there that we have indeed proceeded arbitrarily but rationally during the construction of our schema; we have simply constructed the same with regards to the facts." (Schumpeter, 2010, p. 386)

One difficulty is the complexity of socio-economic phenomena, which is further increased by human decisions and historical processes. It is often not possible to scientifically grasp real phenomena in all their complexity, and the construction of theory is essentially characterized by reduction. In this context, Weber (2013, p. 124) coined the term "ideal type":

"It [the ideal type] presents us with an ideal image of what goes on in a market for goods when society is organized as an exchange economy, competition is free, and action is strictly rational. This mental image brings together certain relationships and events of historical life to form an internally consistent cosmos of *imagined* interrelations. The substance of this construct has the character of a *utopia* obtained by the *theoretical* accentuation of certain elements of reality."

Theories are not accurate representations of reality but instead accentuate and are guided by the research interests of the observer. Theory thus becomes an interpretation and structuring of reality. Different research interests can lead to different theories. Against this background, Burrell and Morgan (1979, p. 10) argue that theory always embodies a certain perspective:

"Different theories tend to reflect different perspectives, issues and problems worthy of study, and are generally based upon a whole set of assumptions which reflect a particular view of the nature of the subject under investigation."

But this, in turn, means that theory is always a choice and focusing on one aspect means that other aspects are pushed aside, neglected, or ignored entirely. Seidl (2007, p. 16) describes this as the "dark" side of knowledge:

"Knowledge thus means selection; and selection implies contingency – one could have selected differently. The selectivity of knowledge, however, remains latent. That, and what knowledge excludes, is not included in the knowledge. Knowledge, thus, inevitably implies nonknowledge as its other, or "dark," side."

4.2. Theory testing

In my opinion, the most important contribution of Popper (1992, 2020) is his skeptical approach to confirmation. In his time, Popper was particularly bothered by discussions with adherents of Marxism and psychoanalysis who provided extensive confirmative evidence for their theories but immunized their theories, thus evading critical discourse. This revealed the inconsequential value of confirmative evidence in evaluating theory. As exemplified by conspiracy theories, confirmatory evidence can be found for even the most absurd theory. Critical discourse is therefore crucial for assessing theory.

Kuhn (1996), however, has shown that Popper's idea of falsification in connection with theory is unrealistic, both from a sociological and systematic point of view. Empirical findings can "prove" neither the accuracy nor the fallacy of theory, or as Harre (1985, p. 44) put it, "there are no brute facts." This leaves critical scientific discourse as the only way to assess theory. Critical scientific discourse consists of arguments based on empirical findings and logical conclusions. Kuhn (1996) has shown that this is a sociological process. Popper (1992) rightly emphasized the importance of research questions by which research and research discussion must be measured. This still seems to be accepted today. In Sutton and Staw (1995), this discourse can be the only measure for distinguishing between weak and strong theory. It is therefore a subjective distinction based on the sociology of science rather than a strict methodological distinction. Statements such as "few of them take the form of strong theory" (Weick, 1995, p. 385) or "Most products that are labeled theories actually approximate theory" (Weick, 1995, p. 385) may be justified by good arguments, but ultimately represent subjective judgments. This assessment is influenced by habits and ideas about what is important and what is unimportant. We should be aware of this when evaluating theory. Too strong a focus on confirmative evidence and confirmation of hypotheses in the discourse on the assessment of theory ("theory testing") is not unproblematic, especially in light of Popper's justified objections. A critical perspective is imperative.

The evaluation of theory and thus the progress of science rests entirely on discourse, and its importance therefore cannot be underestimated. This discourse must be open, objective, and follow the primacy of argument. Logical inconsistencies are not to be ignored. Kuhn (1996) has shown that this cannot be avoided, but science must always struggle against cliques and power. Science is not just about gaining knowledge; it is also a sociological phenomenon.

5. Theory as an explanation of causal connections?

Against the background of the findings of the philosophy of science, the specification of theory in Sutton and Staw (1995, p. 375) seems particularly problematic. They demand, "A theory must also explain why variables or constructs come about or why they are connected." Sutton and Staw (1995, p. 372) illustrate their requirement with an example:

"To illustrate, this sentence from Sutton's (1991: 262) article on bill collectors contains three references but no theory: 'This pattern is consistent with findings that aggression provokes the "fight" response (Frijda, 1986) and that anger is a contagious emotion (Baron, 1977; Schacter & Singer, 1962).' This sentence lists publications that contain conceptual arguments (and some findings). But there is no theory because no logic is presented to explain why aggression provokes 'fight' or why anger is contagious."

The conditioned universal causal relationship $A[\text{aggression}] \rightarrow B$ [fight response] in itself does not constitute a theory, as theory consists only in the presentation of a logic that shows *why* this connection exists. For this reason, hypotheses are not theories. So Sutton and Staw.

This way of thinking has become entrenched in management science and must be challenged and changed. This perspective not only fundamentally contradicts the findings of the theory of science, but further, sets a standard that is counterproductive for research and cannot be met.

First, it should be noted that a statement in the form $A \rightarrow B$ is completely sufficient for an explanation and prediction in the covering-law model. What Sutton and Staw (1995) overlook is that the statement $A \rightarrow B$ itself provides an element for an explanation. Water begins to boil on Everest *because* it has been heated to 71 degrees and *because* this is the boiling point there. I don't have to understand why the connection between boiling temperature and air pressure exists, only that it exists. An "explanation" (whatever this means in this context) of the conditionally universal causal connection $A \rightarrow B$ itself is therefore necessary neither to understand developments in the past nor to assess the consequences of certain decisions in the future. It is enough to know that $A \rightarrow B$ holds.

Second, no explanation can be given for the conditionally universal causal connection in the form $A \rightarrow B$, simply because there is no such explanation. The central insight of the philosophy of science since Hume is that such statements are—must be—constructed, as there is no inductive inference, and causality cannot be directly observed. As Schumpeter (2010) pointed out, statements in the form $A \rightarrow B$ are creatures of researchers' arbitrariness. Sutton and Staw's (1995) requirement at this point cannot, in a strict sense, be fulfilled. This is problematic because it misjudges the epistemological status of such statements and creates the impression that such a justification could exist. At this point, we should remember Schlick et al. (1979) and his complaint that demands of precisely this kind hinder the progress of science because they cannot be fulfilled and thus lead to fruitless discussions.

In a less strict sense, research indeed can offer "explanations" for universal causal connections in the form $A \rightarrow B$. In the justification context of theorems in axiomatic systems, a causal chain is established from which $A \rightarrow B$ follows. However, management science does barely practice axiomatic theory, and furthermore, the causal chain always refers to axioms within the system. The only claim, therefore, is that the causal connection $A \rightarrow B$ can be concluded from certain assumptions, a very weak form of explanation. It is doubtful that Sutton and Staw (1995) have that in mind either.

Still, there is the justification context, which is carried out in a micro-foundation. A micro-foundation examines which microprocesses are subject to a macro-relationship (Felin et al., 2012). Schematically, the structure of a micro-foundation is represented by Coleman's (1990) bathtub model (Fig. 2).

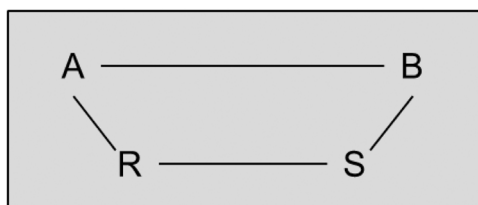


Fig. 2. Schematic representation of Coleman's (1990) bathtub model.

Is that what Sutton and Staw have in mind? The problem, however, is that every micro-foundation itself is a construct. It consists of conditionally universal causal connections of the same form as the original statement (it must consist of these to establish a causal connection), only now $A \rightarrow R$, $R \rightarrow S$, and $S \rightarrow B$. All of these statements are constructed again. Do these statements in turn require an explanation of their respective causal connection? That would lead to an infinite regress. Do they not require an explanation? What then differentiates $A \rightarrow B$ on one hand and $A \rightarrow R$ on the other? In addition, micro-foundations in management science are usually informal and thus produce only informal connections. In doing so, they fail to meet the requirement of Sutton and Staw (1995, p. 372) to provide a "logical" explanation of the causal relationship $A \rightarrow B$.

Micro-foundations can be helpful to break down and better understand processes, "to unpack some of macro-management's preferred aggregate concepts (e.g., 'capabilities,' 'absorptive capacity,' 'routines,' and 'institutions') in terms of individual action and interaction" (De Massis & Foss, 2018: 387). However, they do not provide any explanations of causal connections but are merely foundations.

If Sutton and Staw (1995) do have a micro-foundation in mind, from what understanding of theory does this requirement arise? How is this demand justified? Why is $A \rightarrow R \rightarrow S \rightarrow B$ theory, but $A \rightarrow B$ not? If it is not micro-foundations they have in mind, then what? How else can a logical explanation of the causal relationship $A \rightarrow B$ be provided?

There is also a practical problem. Let us assume that the statement A [aggression] $\rightarrow B$ ["fight" response] is the result of an empirical study in which data supports the proposed hypothesis. However, this data cannot sufficiently support all the necessary theoretical statements for a micro-foundation. What else can offer an "explanation" of the hypothesis, i.e., a logic that explains its causality? Ultimately, this can only be based on plausibility, a review of the literature, considerations, and speculation. This is exactly what the practice in management science resembles. Is that what Sutton and Staw envision?

In contrast, I advocate focusing the theoretical work on the formulation of conditionally universal causal connections in the form $A \rightarrow B$ itself. As previously demonstrated, theoretical propositions are always constructed and created by researchers. However, they are not arbitrary but are formulated with a goal of explaining real facts. The formulation of theoretical statements and their introduction into the scientific discourse should therefore stand on good reasons, which often lie primarily in data showing their empirical relevance. However, the context of justification should extend beyond pure empiricism to the existing theory.

Thus understood, the demands of Sutton and Staw (1995) make perfect sense to me: It can be helpful to place the theoretical statements made in the context of existing research and make them plausible in this way. Which existing findings support the statement $A \rightarrow B$ and make it seem plausible? What are its implications? In what points does it contradict existing views? How do they contribute to our understanding of the issue? This may well include a presentation of the reasons that suggest a causal relationship $A \rightarrow B$. However, this is not a logical justification of the causal connection $A \rightarrow B$ itself but is instead a justification for bringing the hypothesis $A \rightarrow B$ into the scientific discourse. The focus then is on the hypotheses, with the "explanation" serving as support.

In this context, Styhre's (2022) article is worth mentioning, which formulates a "theory of theorizing," i.e., examines how researchers "render an empirical material meaningful on basis of a description that oftentimes includes abstract, yet precise analytical terms." This way, an explanation for $A \rightarrow B$ could in fact be provided. However, this does not consist of showing the logic of the causal connection, but rather the reasons that led the researcher to construct this connection in the context of management studies.

6. Outlook

Theory is indispensable for research, providing its aim is to explain

or predict. Our discipline's current focus on theory can only be welcomed against this background. Without theory, we cannot assess the consequences of actions and therefore cannot give any recommendations for action. In our theoretical work, however, we should be aware of the findings of the philosophy of science:

- (i) Theory describes stable causal relationships. It essentially consists of conditionally universal causal statements in the form $A \rightarrow B$.
- (ii) Theory is indispensable for the explanation and prediction of empirical facts according to the covering-law model.
- (iii) Because causality cannot be observed and because there is no inductive inference, theory is always constructed; for every empirical fact, there is an infinite number of theories that can explain it.
- (iv) For this reason, conditionally universal causal statements in the form $A \rightarrow B$ themselves cannot be explained; nor can constructs be explained, as they, like theories, are constructed.
- (v) There are no brute facts; theories can neither be proven nor explained, but can only be decided on in a scientific discourse based on arguments.

It is right and important that theory is an essential aspect of the scientific discourse; however, if that is the case, it is also important to clarify and reflect on the concept of theory. It must be clear what research is aiming for and by what criteria it should be judged. If this is not the case, research becomes arbitrary. "Theoretical contribution" then becomes synonymous with "I like it." In this context, it is important to drop ideas that contradict the above findings of the theory of science, above all, that there is a logical explanation for causal relationships and that theory can be induced from data. Just recently, I read in the review report of a leading journal, "I admit that I didn't fully follow your claim that we cannot induce theory from qualitative data"—and this claim was not related to weak induction. Such ideas are problematic in leading to claims that cannot be fulfilled; in reaction, researchers perform an "induction theater" (I use this term in reference to the term "innovation theater" as found in Blank (2019)) and pretend to derive their propositions from qualitative data (many even believe they do). This distorts the evaluation of research and impedes scientific discourse.

My suggestion in this study is to base theory on the universality of causal connections, which can be simple hypotheses, but also complex models. However, theory always involves statements of type (1) in the covering law model, which are necessary for explanations, forecasts and recommendations for action in this form. In doing so, I propose to logically distinguish theory from explanation, i.e., theory is an element of explanation, but not the explanation itself. This distinction seems integral to me, but it stands in sharp contrast to Sutton and Staw's (1995, p. 374) specification that "theory explains why empirical patterns were observed or are expected to be observed." I consider such an equation of theory and explanation problematic in its implication that theory would also contain empirical statements (statements of type (2) in the covering law model). Such a concept of theory is very ambiguous and would complicate defining the boundaries of theory. But that is only one view; it makes perfect sense to critically examine this view and discuss competing perspectives. The aim is not necessarily to agree on a single concept of theory, but rather to gain a better understanding of the nature and structure of the concept of theory.

With regard to theory building, the main question is on what basis new theories should be introduced if they cannot be induced from qualitative data. Popper (1992) suggests that theory building should detach itself from data and start with a bold conjecture. "On these grounds, Popper rejects the confirmationist goal of discovering theories which have high inductive probabilities" (Caldwell, 1982, p. 43). Similarly, Schumpeter (1994, p. 33) sees the starting point of theory in a "vision", a "pre-analytic cognitive act that supplies the raw material for the analytic effort." This does not mean, however, that we completely

dispense with empiricism. Matsuo Basho, a 17th-century Japanese haiku master, once said, "To learn about pine trees, go to the pine tree; to learn of the bamboo, study bamboo." I'm sure Popper and Schumpeter would wholeheartedly agree. According to Popper and Schumpeter, researchers should dig deep into the field, observe and collect data, study the literature, and discuss different aspects. However, the leap to theory is based not on induction, but on a creative, thoroughly intuitive synthesis of the knowledge gained, which finds its form in the bold conjecture or vision. The systematic collection of data subsequently follows to critically test the formulated new hypotheses or models.

But what about management science? A study by Sandberg and Alvesson (2011) provides interesting insights. This study shows "that the most common way of producing research questions is to spot various gaps in existing literature, such as an overlooked area, and based on that to formulate specific research questions" (p. 24). Sandberg and Alvesson also refer to this *modus operandi* as "gap-spotting." As a result, research "is more likely to reinforce or moderately revise, rather than challenge, already influential theories" (p. 25). This approach is quite distant from that of Popper and Schumpeter. Sandberg and Alvesson (2011) identify a number of factors that convey this development and lead to a path dependency of science. These include: the individual striving for security and simplicity, the integration of research into traditions, a required recognition by established researchers, but also the institutional practice of funding committees and journals.

The excessive adherence to empiricism in theory-building studies and the repeatedly increased demand for inductive derivation (or at least empirical foundation) of new theory seems to be an essential expression, if not even the driver, of this practice. Cornelissen (2017, p. 368) points in exactly this direction, saying, "In recent years qualitative papers are increasingly being fashioned in the image of quantitative research, so much so that papers adopt 'factor-analytic' styles of theorizing that have typically been the preserve of quantitative methods." This is exactly what Popper and Schumpeter warned against, that research focuses too closely on the path and loses sight of the goal. But how should new theory be motivated if not by data? At this point, Burrell and Morgan's (1979) understanding of theory as a perspective can be helpful. The introduction of a new theory should focus on the new perspectives that arise from it, the new relevant aspects or perspectives that can be revealed with it. Does it make sense to dare a little more Popper and Schumpeter in management science? Discussing this question could also help better understand what makes a theory novel.

A central point for scientific discourse and theory testing is that theory is not only constructed but also reduced, and this reduction is subject to prioritization. This means, however, that acceptance of one theory does not necessarily invalidate another, even if the two are contradictory. Burrell and Morgan (1979) take a strongly instrumental position here, viewing theories as a resource for developing and testing new perspectives. Theory can thus guide observations and interpretations and might also be used to create new opportunities. Theory is then discussed relative to specific objectives.

In management science, it is indeed often the case that several theories can exist simultaneously. Just think of the different strategic approaches such as the market-based view, resource-based view, or dynamic capabilities view. In his book, *The Sense of Dissonance*, Stark (2009) argues that this ambiguity should be understood not as a defect, but as a source of knowledge. Though Stark was primarily referring to organizations, his findings can also apply to science. Research should therefore not strive to eliminate differences, but to understand them and use them to gain knowledge. Stark explains (p. 17), "Whether we refer to the process as research, innovation, exploration, or inquiry, the kind of search that works through interpretation rather than simply managing information requires reflective cognition." This recognition of diversity stands in sharp contrast to the empiricists, whose attitude Suddaby (2015, p. 2) describes as follows: "When a single theory fails to emerge (as is inevitable), empiricists tend to reject the value of theory entirely and focus energy exclusively on the collection of data." One downside of

diversity, however, is that it comes at the expense of clarity. To counteract this, it is necessary to specify diversity and mutually relate different perspectives; the individual perspectives should be internally consistent. How should such a scientific discourse look in detail? Further clarification is still needed.

Now, however, the question arises anew as to what theory actually is; not how it is defined, but how it should be understood and used. What insight do theories provide if they are not right or wrong in a strict sense, but ideal types, simplifying models constructed to serve an explanatory goal, emphasizing some aspects and abstracting from others? At this point, it may be useful to consider the findings of the instrumentalism-realism debate, the positions of which are aptly specified by Caldwell (1982, p. 26):

“Realists claim that theoretical terms must refer to real entities and that theories which do not are false. Instrumentalists are agnostic on the point, for they insist that theories are only instruments, that as such it is incorrect to speak of theories as being either true or false, and that the only relevant questions that can be asked regarding theories concern their adequacy.”

The advantage of the instrumentalist view is that it frees itself from the problems of the theory of science, but this comes at the cost of giving up the search for truth. Furthermore, theory immunizes itself and eludes critical scrutiny.

“By avoiding the rigors of critical, potentially falsifying tests, instrumentalists can always preserve their theories. In cases of failure, they can claim that the theory should not have been applied to the situation in question” (Caldwell, 1982, p. 52).

Instrumentalism can then become an excuse to disregard reality. Against this background, radical instrumentalism seems very problematic, whereas a strict realism, on the other hand, is always confronted with the problem of induction. However, it should be emphasized that a perspective as such is not an instrument, but a point of view and therefore connected to reality. So how to understand theory, how to deal with it? This is perhaps where the greatest need for discussion in management science lies.

This is the context in which the debate about “rigor” and “relevance” in management science should also be examined. Beginning in the 1980s, there has been increasing criticism that research in the field of management science “has had little effect on life in organizations” (Beyer, 1982, p. 588). “Critics question the relevance of what we teach business students as well as the meaning of the research performed at business schools” (Gulati, 2007, p. 776). The debate about formalization in economics (Kesting & Vilks, 2005) not only revealed a trade-off, but also a tendency to lose oneself in the logic of theory and methods. Despite Gulati’s (2007) unhelpful dismissal of this debate as a “socially constructed” struggle between “tribes,” the danger seems real to me, and it seems sensible to track down and critically question such developments time and time again. This debate is not socially constructed but must be conducted again and again to prevent researchers from losing their grounding in reality.

Another recent debate is how theory should be applied in management education. In this debate, Brunsson (2021) questions the role of theory in management education, stating, “It is suggested that full-fledged theories obstruct the usefulness of theory, which should be experience and intuition based and allow for discoveries, theorizing and new concepts” (Brunsson, 2021, p. 1). In their reply, Eriksson-Zetterquist et al. (2021) point out the benefits of theory for management education to describe and analyze phenomena. Weighing these two effects against one another begs the empirical question of to what extent theory sharpens the view and to what extent it impedes creative discovery.

7. Conclusion

As a result, it can be stated that the definition of theory is surprisingly

simple and unproblematic. Ultimately, this consists only in convention, in a determination of an object of knowledge. Much more problematic is the formulation of theory, and then, above all, dealing with theoretical statements. Much ambiguity remains in management science, as well as some real misunderstandings. A look at the theory of science can help sharpen the view here.

Data Availability

No data was used for the research described in the article.

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