There Is No Such Thing as a Halfway Crook – or Theory

Abstract

For more than twenty years, futures and foresight has been told that it lacks scientific theory. Its fullest statement is Fergnani and Chermack's (2021) argument that the field resists scientific theory and that the resistance is self-sabotaging. I argue that the demand has a simple structure: *theory* is defined in an obscure way, the field is found lacking, and the lack is explained as resistance. Philosophy of science has not settled in a hundred years what a theory is; the demand settles it with a definition from methodology texts and calls it resistance when the field does not follow. The demand also runs into two dilemmas. First, either *theory* means an axiomatic, deductive system of laws and propositions, as in classical philosophy of science, and then most sciences are not about theory, or it means something wider, including causal generalizations, models and frameworks, and then futures studies has plenty of theories already. Middle-range theory tries to sit between the horns, but there is no such place to sit. Second, the proposal to study present foresight practices instead of futures is either trivial, as all research is done in the present, or untrue, as most of the field tries to understand what might happen beyond it. I also distinguish four roles of theory in futures studies – borrowed domain theories, method theories that come with methods, theories as part of the object, and theories of the field – and argue that satisfying the demand would paralyse a plural field like futures studies rather than strengthen it.

Keywords: futures studies, foresight, scientific theory, philosophy of science, scientific pluralism, method theory, scenario planning

1 Introduction

In 2002, Chermack wrote that the "status of theory and theory development in the area of scenario planning and, for that matter, future-oriented practices in general is dismal" (Chermack, 2002, p. 25). In 2017, the situation had not changed much, at least according to Chermack (as quoted in Fergnani & Chermack, 2021, p. 2), and in 2021 Fergnani and Chermack placed the theoretical development of the whole field at "the first stage" of four (p. 6). By now, the demand for theory in futures studies – from now on, *DTF* – is almost a quarter of a century old. Between these dates, there has been a theory of scenario planning built with an eight-step method of theory building (Chermack, 2005), a suggestion that building scenarios is itself a form of building theories (Chermack, 2007), design propositions for scenario interventions drawn from social psychology (Hodgkinson & Healey, 2008), a special issue on the theoretical underpinnings of the field (Mermet et al., 2009), a survey of the theories that futures studies researchers actually use (Minkkinen, 2020), and so on. *DTF* has, in other words, a history: it is repeated, it is answered, and then it is repeated again – and the theory that would satisfy it does not seem to arrive. Spaniol and Rowland (2018) have shown how the claim of "dismal" theory has been repeated for more than a decade, together with the claim of methodological chaos, as a formulaic way of introducing new contributions, and how the strategy, repeated over time, keeps theory "dismal" and methods chaotic – the *scenario planning paradox,* as they call it. In a later paper, they note that the claim of definition confusion travels in the same package (Spaniol & Rowland, 2019).

Why is that? Fergnani and Chermack (2021) give one answer. The title is telling: "The resistance to scientific theory in futures and foresight, and what to do about it". The field resists scientific theory, consciously or subconsciously, and the resistance is "essentially self-sabotaging" (p. 1). Serious accusations, to say the least. They list nine reasons for the

resistance and give recommendations for authors, journal editors and practitioners. Their paper is the most explicit and complete statement of *DTF* that I know, and this is why the paper is my main target here. However, the target is *DTF*, not one paper. More than five years after the paper, *DTF* is still with us, and the questions it raises are good questions, even if the answers are not.

The structure of *DTF* is, at bottom, rather simple. A concept of theory is defined in a certain way. The field is found not to produce theories in that sense. And the absence is explained as resistance – as something the field does to itself. I argue that the problem lies in the first step, not in the field. The definition is borrowed from methodology texts rather than from philosophy of science, where the question of what a theory is has been discussed for a hundred years without a settled answer. The definition is dated, it is obscure, and it does not fit how science works in most of its domains. When a field does not follow such a concept, this is not resistance. Once the definition goes, there is nothing for the field to resist, and the diagnosis of resistance has nothing to explain.

I argue that *DTF* does not make sense – or, to be more precise, that it makes sense only in ways that do not make it a demand worth to fulfil. The title of the paper tells how.[1] There is no such thing as a halfway crook, and, I argue, there is no such thing as a halfway theory either. The argument has two spines.

The first spine is a dilemma about the meaning of *theory*. If *theory* is understood in the narrow sense that the definitions of *DTF* presuppose – an axiomatic, deductive system of laws and propositions connected to observations, as in classical philosophy of science and, in

[1] The title paraphrases a line from Mobb Deep's "Shook Ones (Part II)" (1995).

the dress of management methodology, in Dubin's (1978) method of theory building – then most of science is not about theory, and demanding such theory from a plural field like futures studies would paralyse the field rather than strengthen it. If *theory* is understood in some wider sense, as it mostly is in contemporary philosophy of science, so that tested causal claims, models and frameworks count as theory, then futures studies already has plenty of theories, and *DTF* is empty. *DTF* itself moves between the two senses, and this is why its concept of theory is obscure. Middle-range theory (Merton, 1968), which Fergnani and Chermack (2021, p. 3) mention in passing, tries to sit between the horns. The second spine is a dilemma about the object of the field. Fergnani and Chermack propose that futures studies should turn from seeing futures as its object to seeing foresight practices and processes as its object (p. 11). This proposal, I argue, is either trivial or untrue.

The paper is not only critical, however. Once the dilemmas are on the table, it becomes possible to say what theories actually do in futures studies – and they do quite a lot. I distinguish four roles of theory in the field and argue that none of them requires a theory of futures studies' own in the narrow sense. This is, I believe, a more useful answer to the "different use of the term 'theory'" in the field (Fergnani & Chermack, 2021, p. 6) than a demand to use the term in one way only.

The paper proceeds as follows. In Section 2, I track what *theory* means in *DTF* and where the meaning comes from. Section 3 presents the first dilemma. Section 4 argues that the idea that science is about theory is an early twentieth-century picture which does not match science as it is practiced. Section 5 presents the second dilemma. Section 6 discusses the roles of theory in futures studies, and Section 7 what satisfying *DTF* would do to the field. In Section 8, I return briefly to the nine reasons for the "resistance", and Section 9 concludes. I do not attempt to settle the debate on what a scientific theory is; that debate is long, and it is still

open (see, e.g., Suppe, 1977; van Fraassen, 1980; Craver, 2002; Halvorson, 2012). What I can do is to show what *DTF* commits itself to, and what follows if the commitments are taken seriously.

2 What *theory* means in *DTF*

Let me begin with the definition. Fergnani and Chermack (2021, p. 3) adopt Kerlinger and Lee's (2000, p. 11) definition of theory as "a set of interrelated constructs (concepts), definitions, and propositions" that specifies relations among variables in order to explain and predict phenomena. Constructs are abstract representations of phenomena, variables are constructs translated into measurable terms, propositions relate constructs, hypotheses relate variables, and hypotheses "become theory if verified by empirical data" (p. 3). The definition is then supported by a list of what theory is not, taken mainly from Sutton and Staw (1995): references, data, variables, diagrams and hypotheses are not theory – and, extending the list, neither are epistemologies, ontologies, conceptual frameworks or speculations (Fergnani & Chermack, 2021, pp. 4–5). Notice also the scope. What the field is said to resist is "scientific theory on futures and foresight practices and processes in organizations" (p. 1) – not a theory of futures. The scope matters, and I return to it in Section 5.

Where does this picture come from? It is worth to look at the genealogy for a moment, as it explains much of what follows. In Chermack's early work, the concept of theory is taken from the literature on theory building in human resource development: Torraco's definition according to which a theory "simply explains what a phenomenon is and how it works" (as quoted in Chermack, 2002, p. 26), Lynham's (2002) general method of theory-building research in applied disciplines and, above all, Dubin's (1978) eight steps. Dubin's method proceeds from units to laws of interaction, boundaries, system states, propositions, empirical

indicators, hypotheses and, finally, testing (see Chermack, 2005, pp. 60–61). In management and organization studies, the same picture was codified in a set of well-known papers (Bacharach, 1989; Whetten, 1989; Sutton & Staw, 1995). Whetten's building blocks – what, how, why, and the boundary conditions of who, where and when – draw openly on Dubin (1978), and Bacharach (1989) uses the same scheme as Kerlinger, with propositions relating constructs and hypotheses relating variables. Whetten's building blocks are recognisable in Fergnani and Chermack's (2021, p. 2) repeated phrase about "why, how, when, and where" futures and foresight works.

The picture itself is older. Kerlinger's *Foundations of Behavioral Research* first appeared in the 1960s, and the idea that a theory is a system of sentences connected to observations by special rules comes from the first half of the twentieth century. Campbell (1920) called the connecting part of a theory its "dictionary", and the Vienna Circle and logical empiricists turned the idea into what is now called the *Syntactic View*, or the *Received View*, of theories (see Hempel, 1966; Suppe, 1977). (Interestingly, Putnam's (1962) early attack on the *Received View* was titled "What theories are not".) The concept of theory in the definitions of *DTF* is, so to say, the *Received View* that has travelled through behavioural science and management methodology into futures studies.

The correspondence is close. In Kerlinger's terms, constructs play the role of theoretical terms, variables that of observational terms, and operational definitions that of correspondence rules. Dubin's steps line up with an axiomatic reconstruction almost one to one: units and laws of interaction are the axioms, propositions are the theorems derived from them, empirical indicators are the correspondence rules, and hypotheses are the observational consequences to be tested. Here lies an irony. The logical empiricists never offered their reconstruction as a way of building theories. Reichenbach (1938) separated the *context of*

discovery from the *context of justification*, and the logical analysis of theories belonged to the latter: it was about how a finished theory hangs together and how it relates to evidence. How theories come about was left to psychology and history. Popper (1959) said the same: for him, the act of conceiving a theory neither calls for logical analysis nor is susceptible of it. Dubin's method turns a philosophical reconstruction of finished theories into a construction manual for new ones. It is hard to find a theory in physics, the model science of the tradition, that was built by first listing units, then laws of interaction, then boundaries. Chermack's (2005) theory of scenario planning, to which I return in Section 3, follows a recipe that the tradition it comes from would not have recognised as a method at all.

Here I need a distinction that I use throughout the paper: the distinction between theory and *theory talk*. Theory is what a science actually works with – a family of models, say, or a set of laws, or something else again. *Theory talk* is what researchers and methodology texts say about theory and theory building. The two can come apart, and they often do. Einstein (1934) advised already in the 1930s that if one wants to know what scientists do, one should not listen to their words but look at their deeds. Fergnani and Chermack (2021, p. 2) state openly that they "have chosen to borrow the conceptualization of scientific theory from the tradition of management and organization sciences". This is a choice of theory talk. Whether management and organization studies actually build theories in this sense is another question, and the debate inside the field does not inspire much confidence. Sutton and Staw (1995) wrote their paper because what authors present as theory is so often something else – references, lists of variables, diagrams or hypotheses – and their colleagues in the same forum did not even agree on what theory is (DiMaggio, 1995; Weick, 1995). The evidence that sciences do theory building in the narrow sense is, so far, mostly what some of them say about themselves.

To be sure, the picture in *DTF* is not wholly uniform. Fergnani and Chermack (2021, pp. 3–4) also accept inductive theory building (grounded theory) and abductive reasoning. In a footnote, they mention *process theories*, which explain how events unfold over time – and set them aside, as the focus of their article is the "more widespread" variance theory (p. 13, note 8). The choice is telling. Variance theory, in Mohr's (1982) sense, explains outcomes by relations among variables; this is Kerlinger's world, and sociologists have criticised it for decades as a picture of a "general linear reality" (Abbott, 1988). A scenario is a narrative of how events might unfold – a process account if anything is. So, the kind of theory that would fit the field is mentioned and dropped, and the kind that fits regression analysis is kept. In variance theory, the product that counts as theory is clear: propositions about why, how, when and where something works, with mediators and moderators, tested in empirical research (p. 3). The paradigmatic example is the proposition that "scenario planning increases firm performance" (p. 3), which can then be refined by adding a mediator (organizational learning) or a moderator (management buy-in), and so on.

Two further elements belong to the picture. First, in one sentence, Fergnani and Chermack (2021, p. 3) note that theories resting on a critical realist perspective in management and organization "are indeed called 'mid-range theories' for their conciliatory position between generalized abstractions and emergent, subjective phenomena" (with reference to M. Thompson, 2011, p. 754). This is the only place in their text where the notion appears, and Merton, from whom it comes, is not cited. The sentence is worth reading slowly, as it shows well how the concept of theory in *DTF* works. What is the "conciliatory position" of a theory between a generalized abstraction and a subjective phenomenon? What is it to rest a theory on a perspective, and how does the perspective place the theory in the middle of anything? For Merton (1968), the middle was a middle of scope, between the working hypotheses of

everyday research and the all-inclusive systems of grand theory. Here, the middle seems to have become a middle between philosophical camps, and it is hard to say what the sentence does in the argument at all. Middle-range theory is, in a sense, the halfway house of theory, and I return to it in Section 3.

Critical realism deserves a few questions of its own, since it is the philosophy of science that *DTF* espouses (Fergnani & Chermack, 2021, p. 5). In a note, Fergnani and Chermack explain, with Bhaskar's (1975) three ontological layers – the real, the actual and the empirical – that "theoretical claims can be tentative insofar as different empirical observations are closer to the actual and the real" (p. 13, note 4). What is it for an observation to be closer to the real? How is the distance measured? Would findings stop being tentative in a world with only one layer? The plain answer is that Bhaskar's layered ontology makes no sense. Neither Merton's notion nor the concept of theory borrowed from Kerlinger and Dubin was built on critical realism; the label is added afterwards. Bhaskar hardly occurs in contemporary philosophy of science, as a field of its own – and for a reason: there is no need for such obscurity. Realism about science can be stated much more easily and clearly, as Psillos (1999), for example, does.

Second, there is a tension in the definition. On the one hand, theories should be "parsimonious, logical, coherent and, most importantly, falsifiable" (Fergnani & Chermack, 2021, p. 3). On the other hand, "a claim that has not been verified does not constitute a theory" (p. 4), and hypotheses become theory if verified. For Popper (1959), from whom the criterion of falsifiability comes, theories are conjectures which are never verified, and so the two requirements do not go together. However, the tension itself is the smaller point. The bigger one is where the vocabulary comes from. Verification, falsifiability, correspondence rules, theories as systems of propositions – this is the vocabulary of philosophy of science

from roughly the 1930s to the 1960s, and the debate between verification and falsification is a debate that philosophy of science left behind long ago. It is good to know the classics, of course. But they are not a description of science as it is practiced now, and they cannot be read as one (I return to this in Section 4).

So, what does *DTF* mean by *theory*? The answer is less clear than it first seems, and this is part of the problem. On the one hand, there is the vocabulary of a system: interrelated constructs and propositions, laws of interaction, propositions derived logically from the model and hypotheses derived from the propositions, verification and falsifiability, and four stages of theoretical development through which a field should pass. This is, in the dress of management methodology, the picture of classical philosophy of science, in which a theory is an axiomatic, deductive system connected to observations by correspondence rules. Let me call this the *narrow* sense of theory. On the other hand, there are the examples. That scenario planning increases firm performance, with organizational learning as a mediator and management buy-in as a moderator, is a causal claim about relations between variables, tested in empirical research. Claims of this kind are not narrow at all. They are what most sciences, futures studies included, work with every day, and nobody needs a method of theory building to make them. *DTF* moves between the two. When *DTF* says what theory is, it speaks the language of the system; when it gives examples, it gives causal claims; and on top of both comes the critical realist label. Kerlinger's words, Dubin's machinery, Popper's falsifiability, the verification of the logical positivists and a critical realist label: each piece comes from a different decade and a different school, and none of them was built to fit the others. This is what I mean when I say that the concept of theory in *DTF* is obscure. The question is: what follows if futures studies is required to have theories in this sense – whichever sense it is?

3 No halfway theory

The meaning of *theory* has been discussed in philosophy of science for about a hundred years, and the discussion has produced roughly three families of views. According to the *Syntactic View*, a theory is a set of sentences in a formal language, axioms and theorems, connected to observations by correspondence rules (Hempel, 1966; Suppe, 1977). According to the *Semantic View*, a theory is rather a family of models – mathematical structures, not sentences (van Fraassen, 1980). (Compare *DTF*, for which "a theory is also not a model or diagram"; Fergnani & Chermack, 2021, p. 4.) According to the *Pragmatic View*, a theory is something more amorphous: it consists of sentences and models, perhaps, but also of exemplars, problems, standards, skills, practices and tendencies (see Savage, 1990, pp. vii–viii; Craver, 2002). Notice that the *Pragmatic View* counts as parts of theory exactly the things that *DTF* excludes: ontological assumptions, conceptual frameworks, analogies, values, exemplars and so on. Kuhn's (1970) *disciplinary matrix*, for example, had four components – symbolic generalizations, ontological assumptions, values and exemplars – and only the first of these is close to a proposition.

Notice the contrast. Philosophy of science, where the question belongs, has not been able to say in a hundred years what a theory is. *DTF* says it in a paragraph, with a definition from a methodology textbook, and then tells another field what it should be doing. This would be a bold move even if the definition were clear, and, as I showed in Section 2, it is not: it moves between a deductive system and a causal claim, and its one sentence on middle-range theory places theories somewhere between abstractions and phenomena. One cannot throw such a concept into the air, find that others do not follow it, and call this resistance.

Here is the first dilemma. Either *theory* is meant in the narrow sense, or it is meant in some wider sense. Consider both horns in turn.

Take first the narrow horn. In the narrow sense, theories exist in some sciences, and even there they are not what most of the scientific work consists of (I discuss this in Section 4). In futures studies, as I argue in Section 7, demanding such theory would do more harm than good. However, it is useful to see what narrow theory looks like when somebody tries to build it in futures and foresight, and luckily there is an example. Chermack (2005) built a theory of scenario planning following Dubin's method, step by step. The units are scenarios, learning, mental models, decisions and firm performance. The propositions chain conditionals of the form: if scenarios are positively associated with learning, then learning will increase as a result of participating in scenario planning – until the last of them reaches firm performance. From the propositions, hypotheses are derived that predict positive relationships between participation in scenario planning, learning, mental models, decisions and firm performance.

There is nothing wrong in this as an exercise; it is exactly what Dubin's method asks one to do. But notice two things. First, the propositions are conditional on the very associations that are the empirical question. Whether scenarios are positively associated with learning is exactly what one would want to know, and the first proposition only says what follows if they are. Until the hypotheses are tested, the theory asserts nothing; its whole content is postponed. So, the propositions add nothing to the hypotheses: whatever the theory says, the hypotheses say already, and whether any of it is true is left to the tests. Second, and more important, when the narrow sense is fulfilled, what futures studies gets is a chain of correlations between participation in an organizational intervention and organizational outcomes. The result is a piece of research on management, and a reasonable one. About

futures, it says nothing. To be fair, Chermack presents the theory as preliminary, and he notes himself that the hypotheses could at best establish relationships, since correlations "will not allow statements of causality to be made" (Chermack, 2005, p. 71). The critique is, then, close to his own assessment. This only makes the point stronger: what the method delivers, even on its author's account, is a set of correlations waiting for tests. (That this is a defect for futures studies depends, of course, on what the object of the field is; I argue for this in Section 5.)

Moreover, the narrow sense explains a curious fact about the literature. According to Chermack's (2018) own count, more than half of the refereed articles on scenario planning between 1995 and 2016 (147 out of 257) were theoretical or conceptual works – theorizing, model building and literature reviews – and Chermack's own conclusion is that scenario planning is an underdeveloped field, since empirical work is scarce. Still, according to *DTF*, theory is lacking, and the whole field is at the first stage of theoretical development (Fergnani & Chermack, 2021, p. 6). This is possible only if untested work does not count as theory at all – that is, only if theory is something verified, as the narrow sense requires. Theory is something that has not yet arrived, whatever is written in the meantime.

Then the other horn. In any wider sense, futures studies is full of theories. If tested causal claims about relations between variables count as theory, as the examples of *DTF* suggest, the field has those too: the research on Delphi, for one, has tested for decades how feedback and iteration affect the judgements of panels (Rowe & Wright, 1999). And when Minkkinen (2020) asked researchers and postgraduate students in one futures research centre which theories they use, 24 respondents named 192 different theories or theoretical frameworks, with systems, complexity and anticipation as the most common themes. To be sure, a survey tells how researchers use the term *theory*, not what they do in practice. For the wide horn,

however, this is enough, since the question here is exactly how the term is used. (For claims that someone does theory building in the narrow sense, what people say is not enough. Such claims are about a specific activity – propositions, testing, refinement – and only the activity itself can show that such building takes place.)

Fergnani and Chermack (2021, pp. 6–7) treat several of these as conflations of theory with epistemologies, ontologies and conceptual frameworks: causal layered analysis called a theory, critical futures studies called a theory, a categorization of weak signals called a relational theory, and so on. The conflations, they say, are not wrong as such, but they prevent the field from creating scientific theory (p. 7). On the *Pragmatic View*, these are not conflations at all. They are theories, or parts of theories, or theory-laden frameworks that do theoretical work in research. But then *DTF* becomes empty. One cannot complain that a field lacks theory and, at the same time, count 192 of them. Spaniol and Rowland (2018) made a similar point about scenario planning: the field can either accept the claim of dismal theory and turn to more empirical work, or re-interpret the abundance of theories and methods as richness rather than poverty.

The case of causal layered analysis is worth a closer look, as it shows how loose the concept of theory in *DTF* is. In a note, Fergnani and Chermack grant that causal layered analysis can be considered theoretical after all, as it is a conceptual framework that "deliberately advances causal explanations" (p. 13, note 9). Whether the framework really does so is quite questionable. On an interventionist account, a causal explanation tells what the outcome would have been had the cause been different: the explanation rests on generalizations that remain invariant under interventions and that answer what-if-things-had-been-different questions (Woodward, 2003). Causal layered analysis offers nothing of this kind. Its layers – from the litany of headlines through social causes to worldviews and myths (Inayatullah,

1998) – show how an issue is framed and how the framings are nested within each other. They do not tell what would change, and how, if one intervened on any of the layers. The name says *causal*. But names do not matter; as Einstein (1934) advised, one should look at deeds rather than words. Either way, the exception does not help *DTF*. If causal layered analysis counts as theoretical because of its name, theory is a matter of labels. And if a framework counts whenever it really explains causally, then any framework that makes causal claims counts – and this is the wide horn.

Interestingly, the two horns can be found in one and the same paper. Chermack (2007) argues, on the one hand, that scenario planning should be studied with positivistic methods, that theory building proceeds, following Christensen and Raynor (as cited in Chermack, 2007), from descriptive to correlational to causal studies, and that "predicting that futures-oriented tools are effective and predicting the future are two different things" (p. 2). On the other hand, the same paper argues, drawing on Argyris' *theories-in-use* (as cited in Chermack, 2007) and on Weick (1995), that each scenario "can be thought of as a theory about the future" (p. 12) and that scenario construction is "an appropriate mode of theorizing" (p. 13). The paper lists Carnap, Popper and Stegmüller among the classic definitions of theory (p. 2) – a syntactic view, a falsificationist view, and a structuralist version of the semantic view – and then turns to the theory-building methods of human resource development, Dubin's among them. (The title of the paper comes from Weick, 1989, for whom theory construction is disciplined imagination.) Chermack is careful here. He notes that it is unclear whether scenario planning as a theory construction exercise can be thought of in the same way as traditional academic theory construction (p. 2), he holds that scenario planning does not necessarily produce theories of the academic kind but rather theories-in-use (p. 8), and he concludes that it would be inappropriate to substitute scenario planning for

empirical research (p. 13). But the distinction he draws is the dilemma itself. On the *Pragmatic View*, theories-in-use are theories, and so the question is which of the two concepts *DTF* requires. If theories-in-use count, futures studies produces theories every week and *DTF* is empty; if only what Dubin's method produces counts, scenarios are not theories at all, and the narrow horn applies. The paper shows how strongly both horns pull.

What about the middle? Middle-range theory, which Fergnani and Chermack mention only in passing, is the obvious candidate for a place between the horns, and in its original context it was a reasonable move. Merton (1968) introduced it in sociology against the all-inclusive systems of grand theory: sociology should build theories that are general enough to be theories and specific enough to be tested. However, the form of such theories was taken over from the *Received View*. For Merton, a theory of the middle range consists of a limited set of assumptions from which specific hypotheses are logically derived and then tested in empirical research. Moreover, the middle range was not an alternative to grand theory but a stage on the way to it. Sociology had not yet found its Kepler, let alone its Newton, and theories of the middle range were to be consolidated into more general ones later. The halfway theory was, quite literally, halfway. (Interestingly, Merton's own examples – reference groups, relative deprivation, role-sets – look less like deductive systems than like concepts with a mechanism attached, and analytical sociologists have read them as mechanism-based explanations; see Hedström & Udehn, 2009. Theory and theory talk come apart here, too.) Since then, the notion has travelled through sociology, evaluation research and critical realism (see, e.g., Pawson & Tilley, 1997; M. Thompson, 2011), and *middle range* has come to name a position – between abstraction and particulars or, as in the sentence quoted in Section 2, between philosophical camps – rather than a form of theory. Sorting out this mess would require a paper of its own. For the purposes of this paper, it is

enough to notice that no version of the notion gives a third sense of *theory*. Either a middle-range theory is what Merton said it is – a deductive system with a reduced scope – and then it inherits the problems of the narrow horn, all of them, and none of the generosity of the wide horn; a halfway theory of this kind is a narrow theory that does not want to look like one. Or *middle range* is a label for a position, and then it borrows its content from whichever horn one happens to stand on. Either one is a crook or one is not – and either *DTF* means theory in the narrow sense, with its consequences, or it means theory in some wider sense, and then it is satisfied already.

4 Science is not about theory

The idea that science is, at bottom, a body of theories is not a neutral starting point. That idea is the picture of the *Syntactic View*, from the period roughly between 1920 and 1960, when the task of philosophy of science was understood as the logical reconstruction of theories (Suppe, 1977). Since then, philosophy of science has moved on, and it has done so in several directions at once. Hacking (1983) argued that experimentation has a life of its own, independent of theory. Cartwright (1983) argued that the fundamental laws of physics, taken literally, are false of the messy world, and that models are what connect theories to phenomena. Morgan and Morrison (1999) showed *models as mediators*, built from bits of theory, data, analogies and policy views, and functioning partly independently of all of them. Chang (2012) argued for a pluralism of *systems of practice*. In general, the turn has been towards practices, models, experiments and the plurality of all these – and away from theory as the unit of scientific knowledge.

This does not mean that theories are not important anywhere. In physics, theories in something like the narrow sense matter, and in population genetics the Hardy–Weinberg

principle is a theoretical result in a rather strict sense. But these are specific sciences with specific histories. The claim that every science needs theory of its own, in the sense of deductive systems of laws and propositions, is a generalization from a few cases, and it does not match how most sciences work. The burden of proof lies with those who claim otherwise. If theory building in the narrow sense is a condition of science, it should be easy to show where, outside physics and a few classic cases, most of science consists of it. I do not know where that would be. Even in management studies, the field from which *DTF* comes, the "devotion to theory" has been criticised from inside: Hambrick (2007) argued that the requirement that every paper must contribute to theory prevents the reporting of important facts that do not come with a theory.

Complaining that a field lacks theory in a century-old sense is like complaining that we cannot travel fast because there are so few horses. There are few horses, true. But we have different vehicles now.

What are the vehicles? I do not want to name one of them as the replacement of theory, since that would only rebuild the old picture with a new centre. There are several, and they work side by side. There are models: a system dynamics model of a city's housing market, say, which nobody takes as a theory of cities but which can still teach something about how delays and feedback produce cycles. There are *invariant generalizations*: claims of the type "within such and such conditions, feedback from other panelists narrows the spread of estimates in a Delphi round", which hold within a limited range and support counterfactuals. There are simulations, there are disciplined methods with their own standards, and there are *styles of reasoning* – statistical, taxonomic, historical-genetic, and so on – each with its own objects and criteria of adequacy (Hacking, 1992). Science works with all of these, and with theories where theories are available.

The invariant generalizations deserve a word of their own, since one could object that they bring theory back through the backdoor. The point is simply that causal knowledge exists outside theories. On interventionist accounts, for example, causal explanation requires generalizations that are invariant under some range of interventions and that allow us to answer what-if-things-had-been-different questions – not laws or theories (Woodward, 2003). One needs to go back to Hempel's covering-law model (Hempel & Oppenheim, 1948), in which explanation requires laws – and laws, on the *Received View*, are the stuff of which theories are made – to think that causal explanation depends on theory. This is the same period from which the rest of the vocabulary of *DTF* comes. Such generalizations hold within a limited range; they are, in a sense, between laws and particular facts. Should they then be called middle-range theories? One may call them so, if one wishes. However, this is not what *DTF* means when it says what a theory is. Its definitions want systems of constructs and propositions, with hypotheses derived from the propositions and theory built in stages. A Delphi researcher who knows when feedback narrows estimates and when it produces false consensus has causal knowledge, and useful one. Calling that knowledge *theory* adds nothing to it – except the demand to systematise it into something else.

Consider, then, the historical natural sciences. Geology, palaeontology and evolutionary biology study a past that nobody has observed, from traces that are fragmentary and degraded. They are sciences beyond any reasonable dispute. Yet they are not constituted by theories of their own in the narrow sense. They borrow heavily from physics, chemistry and biology, and the theories they do have – plate tectonics in geology, natural selection in evolutionary biology – were not built by any method of theory building; they are families of models and explanatory schemes rather than axiomatic systems of laws and propositions (I return to Darwin in Section 7). What makes these sciences sciences is the way they work.

Currie (2018) characterises historical scientists as *methodological omnivores*: they combine traces, models, analogies, experiments and simulations opportunistically, according to what the case at hand allows, and they are successful exactly because they are not tied to one method or one theory. Historiography is a second example, and a more familiar one to futures studies researchers. Historians explain the outbreak of wars and the collapse of empires without a theory of history of their own; they borrow generalizations from wherever they are available and judge them case by case. Nobody calls geology alchemy because it lacks a theory of its own. Why should futures studies be called so?

5 No halfway field

The second dilemma concerns the object of futures studies. Fergnani and Chermack (2021, p. 7) distinguish predicting the future from predicting the outcomes of futures and foresight interventions – a distinction they take from Chermack (2007, p. 2). The former deals with complex social phenomena and the long-term future; the latter is circumscribed to "fairly measurable" meso- and micro-level phenomena in organizations (Fergnani & Chermack, 2021, pp. 7–8). Scientific theory, as noted in Section 2, is to be theory on practices and processes in organizations. Consequently, they propose a shift that entails "turning from seeing the futures as the object of study to seeing futures and foresight practices and processes as the object of study" (p. 11). They present the shift as an extension of the field's earlier mental model, not an abandonment of it (p. 11). Still, it is the new object, and only the new object, that is to carry scientific theory, and so the question is what the shift does to the rest of the field.

Notice what kind of argument this is. The argument does not say that the future is one or many, open or closed, real or unreal. It says, roughly, that the long-term future of complex

social phenomena is not the place for scientific theory, and that the scientific part of futures studies must therefore be about something else – about interventions that take place today and have “fairly measurable” outcomes. So, one cannot answer the argument by saying what the future is. The argument has already given up the future as the object of scientific study. (This is also why I do not need to take any metaphysical position on the nature of the future here; for one account of the object of futures studies, see Virmajoki, 2026a. I am, for that matter, rather skeptical of the idea that there is one future to study at all.)

The proposal is, however, either trivial or untrue.

In one reading, the proposal is trivial. Everything we do, we do now. A scenario is written in the present, a Delphi panel answers in the present, and whatever effects futures work has, it has them in the present and after it. In this sense, futures studies has always studied the present, and so has every other field. The historian also reads documents in the present. But nobody would say that historiography therefore studies present reading practices rather than the past. If this is all that the proposal says, it says nothing new, and it gives no reason to move the object of the field anywhere.

In another reading, the proposal is untrue. Most of futures studies, and most of the organisations that use it, try to understand something beyond the present – something that might happen. A horizon scanner looks for signals of developments that have not yet taken place. A Delphi panel is asked about developments by 2040, not about how its members feel today. A set of scenarios describes several ways the world might go, and a backcasting exercise starts from a future state and asks what must happen before it. A ministry does not commission a foresight exercise to learn about the exercise; it does so to understand what it may face. To say that futures studies, as a science, studies foresight practices is to redefine the field so that its science no longer covers what the field does. The definition shrinks the

field to the small part where something can be operationalized and measured, and it takes into account little, if anything, of what the field does or can do.

To be sure, the research on the outcomes of foresight is legitimate. One can operationalize "the impact of foresight" and study whether corporate foresight is associated with firm performance (see, e.g., Rohrbeck & Kum, 2018) – why not. Such research may well be useful for organizations and for the practice of foresight. However, it is research on organizations, and its theories are the domain theories of management and organization studies. Such research is, at most, one subpart of futures studies, and it is not the theory of futures studies. Mermet et al. (2009, p. 67) observed that most works on futures have been legitimised through their connection to business management or public decision-making, and that this has weakened the effort to develop other bases for theoretical foundation. The proposal to turn the object of the field into foresight interventions completes this development, as it were: the field would become the study of its own use.

Now, recall the historical sciences from Section 4. They study one actual past, and that past has left traces in the present: rocks, bones, documents. Even these sciences are not constituted by theories of their own in the narrow sense. Futures studies tries to understand what might happen – not one actual course of events but several possible ones, none of which has left any traces, since none of them has happened. If even the sciences of the actual past get along without theories of their own, a field directed at possible futures has even less ground for one. Notice that this argument does not require any view about what the future is. Whatever the future is, it is not a present practice, and a theory of present practices is not a theory of what futures studies tries to understand. One could run the inference the other way, though: where there are no traces, something must license projection beyond the data, and a theory of the field's own would seem the natural candidate. However, what licenses projection in futures

studies is, as I show in Section 6, borrowed domain theory and the method theories built into methods – not a theory of the field's own.

Fergnani and Chermack have a reply ready, however. They do not argue, they say, that the field is more science than art, and being more scientific about theoretical development "does not impoverish the intuitive and artistic component of its practice" (Fergnani & Chermack, 2021, p. 11). The study of practices would then be the scientific part of the field, and the exploring of futures would go on as art. This reply does not escape the dilemma, though, for two reasons. First, that the study of futures is art is an assumption, not a result. The historical sciences show that what nobody has observed can be studied scientifically, and the burden is on those who say that possible futures cannot. Second, the reply splits the field into a scientific half (practices) and an artistic half (futures) – and this is exactly the halfway field. Either the study of futures belongs to the science that *DTF* wants to rescue, and then *DTF* must concern futures, not only practices. Or it does not, and then most of the field has been written out of science by definition, which is not an answer to the charge of pseudoscience but rather its acceptance. The same dilemma meets the most charitable reading of the proposal, on which the study of futures and the study of practices simply coexist: the question is still whether theory, and with it science, is possible for the former.

So, here is the second dilemma. Either the redefinition of the object is trivial, and then it changes nothing, or it is untrue, and then it changes the field into something else. There is no halfway field that studies futures by studying only the present.

6 The roles of theory in futures studies

The dilemmas show what is wrong with *DTF*. They do not yet show what theories actually do in futures studies. Fergnani and Chermack (2021, p. 6) are right that the term *theory* is used in

many ways in the field. However, the different uses do not show confusion. They track different roles that theories play, and once the roles are distinguished, much of the apparent confusion turns out to be a division of labour.

A useful starting point is the distinction between *domain theory* and *method theory* (Lukka & Vinnari, 2014). A domain theory is a particular set of knowledge on a substantive topic area, while a method theory is "a meta-level conceptual system" for studying the substantive issues of a domain (p. 1309). The roles are relative: "no theory is a domain theory or a method theory per se" (p. 1309). I take this distinction to futures studies and add two roles that are specific to a field directed at futures. The four roles are summarised in Table 1.

Table 1

Four Roles of Theory in Futures Studies

Role	What the theory does	Example	Where it comes from
Borrowed domain theory	Supplies substantive knowledge about the domain whose future is studied	Energy economics in an energy foresight exercise	The field that studies the domain (economics, sociology, psychology and so on)
Method theory built into a method	Carries the assumptions that make a method work	Delphi's assumption that relevant knowledge is distributed among experts	The assumptions of the method, often from other fields; there before the method is applied; tested and revised by research on the method
Theory as part of the object	Shapes the futures that futures studies tries to understand	Economic theories and technology roadmaps that steer the developments they describe	The actors whose futures are studied
Theory of the field	Reconstructs what the field presupposes and how its parts hang together	Philosophical accounts of the commitments shared across futures studies	Philosophy of futures studies

The first role is that of *borrowed domain theory*. The future is always the future of something – of energy, work, health, cities – and the substantive knowledge about these domains comes from the fields that study them. A foresight exercise on the future of energy needs energy

economics, whatever its methods are, and it would be perverse to reinvent energy economics inside futures studies. Interestingly, one of the "excellent examples" that Fergnani and Chermack (2021, p. 10) recommend is of this kind. Hodgkinson and Healey (2008) distil design propositions for scenario planning from social identity theory, self- and social categorization theory and the Five Factor Model of personality. Used to design a foresight intervention, these theories play the role of a method theory; their origin is social psychology. Origin and role are different things, and Table 1 records both. In neither sense are these theories of futures studies' own.

The second role is that of *method theory built into a method*. Every method in futures studies carries assumptions without which the method would not make sense. Delphi assumes that relevant knowledge is distributed among experts and that structured iteration can improve judgement; scenario planning assumes that the future is open enough for several futures to be worth considering; backcasting assumes that long-term goals can guide present choices. These assumptions can be wrong, and they are theoretical in the sense of Lukka and Vinnari: they do the work of a method theory. *They come with the methods; they are there before the method is applied, not produced after it.* This is the reverse of the stage model of *DTF*, where theory comes at the end (Fergnani & Chermack, 2021, pp. 5–6). Chermack (2002, p. 26) criticised the tendency to treat method and theory as equivalent – the "method as theory" approach – and his advice about the "implicit theory domains" of scenario planning was: "Make them explicit" (p. 27). I would turn this around. In a plural field, method as theory is not a mistake; method as theory is how theory lives in the field. Method theories are not beyond correction, though. Research on Delphi has tested for decades whether and when iteration and feedback improve the judgements of panels (Rowe & Wright, 1999), and the

design of Delphi studies has changed with the findings. Nobody resists this kind of research. Such research is how a plural field corrects its methods.

The most recent attempt to map the methods of the field shows the same pattern. Kong and Bragazzi (2027) synthesise 44 classifications of foresight methodologies into a taxonomy of fourteen domains, from methodological orientation and anticipatory purpose to participation, output form and quality. None of the fourteen domains is theory. What theory there is sits inside the configurations – in the domains of reasoning logic and of epistemic stance, for example. This is not a defect of the taxonomy. The taxonomy describes accurately how the field organises its knowledge: theory lives inside methods.

The third role is specific to a field directed at futures: *theory as part of the object*. Some theories do not only describe the world; they shape the world. Merton (1948) described the *self-fulfilling prophecy*, in which a false definition of a situation evokes behaviour that makes the false definition come true, and MacKenzie (2006) showed how financial models became an engine rather than a camera, changing the markets they were supposed to describe. Minkkinen (2020) notes the same reflexivity in futures studies. When a futures studies researcher studies the future of work, the theories that employers, unions and governments hold about work are among the forces that shape which futures are possible. Studying these theories is part of studying futures. And again, they are not futures studies' own theories; they are the actors' theories, held and acted upon. One and the same theory can play two roles: an economic theory is a borrowed domain theory when a researcher uses the theory to understand the future of energy, and part of the object when a ministry acts on the theory and thereby shapes the futures in question.

The fourth role is the one this paper itself plays: *theory of the field*. Philosophy of futures studies reconstructs what the field presupposes and how its approaches hang together

(Virmajoki, 2026b, 2027) – for example, that the approaches share commitments to the reality and relevance of future possibilities, to a specific expertise in reasoning about what has not happened, and to the recognition that human engagement with futures shapes which futures are possible (Virmajoki, 2027). Such an account is a theory of the field, not a theory in the field: it is philosophy of futures studies, in the same way as philosophy of biology is not biology. Minkkinen (2020, p. 21) asks whether the foundations of futures studies, if they exist, "consist of a general worldview or shared methods rather than shared theories". This is, I think, exactly the right question, and it is a question of philosophy of futures studies.

The closest earlier map, Piirainen and Gonzalez (2015), separates the epistemology of foresight, theory of foresight as a process and an intervention, and theorizing within foresight, which relies on domain-specific theories from other sciences (p. 198). Their second level comes close to method theory: one can theorize about foresight methodology, using existing theory or developing new theories, to make foresight interventions more effective (p. 198). But there, theory about methods is something still to be developed and applied, not the assumptions that methods already carry. Theories as part of the object are missing from their map altogether, and the concept of theory stays that of *DTF*: theories are, in the end, "empirically tested codifications of generalizable knowledge" (p. 199). None of the four roles requires a theory of futures studies' own in the narrow sense.

7 What satisfying *DTF* would do to the field

Now I can say what satisfying *DTF* would do to the field – and why the absence of theory in the narrow sense is not a defect.

Notice first what kind of demand it is. Fergnani and Chermack do not ask for one unified theory of the field, and they do not ask futures studies researchers to stop what they are

doing. They ask journal editors to encourage theory building and testing and to give “priority to theory driven contributors”, and they hold that this “does not have to clash” with case studies and futures-oriented research (Fergnani & Chermack, 2021, p. 10). Research on what foresight interventions do is welcome, of course – why not – and it paralyses nothing. The problem lies elsewhere. *DTF* does not merely encourage one kind of research. *DTF* makes theory in the narrow sense the measure of the field’s scientific standing: its absence is called resistance and self-sabotage, and it is what makes the field look like alchemy in the eyes of scientists (p. 13). Once theory in this sense is the measure, most of what the field does – a set of scenarios, a horizon scan, a Delphi on developments by 2040 – fails the measure by definition, since none of this work can produce tested systems of propositions about variables. A field whose main work cannot count as a contribution is paralysed, whatever its researchers go on doing. And where the measure becomes a priority, it does something else as well. The priority pulls the field towards the one part in which theory in the narrow sense is possible, the study of foresight interventions in organizations. The field would not stop; it would shrink, until, as I put it in Section 5, it became the study of its own use.

There is also a further reason, which concerns any attempt to give the field a theory of its own. The approaches of futures studies form tightly interlocked packages of goals, objects, methods and theories (Virmajoki, 2027). The goals shape what objects are relevant; the objects constrain what methods are feasible; the theoretical assumptions determine what counts as an acceptable result. Delphi aggregates judgements, scenario planning widens the space of what is considered possible, backcasting designs pathways to desired ends, system dynamics models feedback, and so on. These are different styles of reasoning, each with its own objects and criteria of adequacy (Hacking, 1992). A theory of the field’s own in the narrow sense would have to unify these approaches at the level of theory: one system of

propositions about the relations between variables that all approaches study. However, there is no such set of variables. Imposing one theory on the approaches would not connect them; it would dissolve those that do not fit, since a method cannot keep its goal and its procedure when the assumptions that make it work are replaced. The field is plural, and the plurality is a rational response to the complexity of what it studies and to the diversity of questions worth asking about the future. To require from such a field a theory of its own in the narrow sense would not strengthen the field. It would paralyse it.

Fergnani and Chermack (2021, pp. 11–12) anticipate a related worry – that they house a transdisciplinary field in management and organization sciences – and answer that the study of futures and foresight practices is carried out in contexts "extremely similar" to those examined by management and organization scholars (p. 12). This may well be true. But the answer only repeats the second dilemma: the practices take place in organizations, the futures studied do not.

One could also object that biology is as plural as any field – molecular, ecological, developmental, behavioural and so on – and still it has a unifying theory: nothing in biology makes sense except in the light of evolution, as Dobzhansky (1973) put it. Why could futures studies not have its Darwin? Here the distinction between theory and *theory talk* helps. Evolutionary theory is a theory, no doubt about that. But it is not a theory in the narrow sense of *DTF*; on the *Semantic View*, it is a family of models rather than an axiomatic system (Lloyd, 1988; P. Thompson, 1989). Attempts to axiomatize it came more than a century after Darwin (e.g., Williams, 1970), and they reconstructed a theory that already existed. Moreover, evolutionary theory was built without any methodology of theory building. Darwin did not follow eight steps, and the methodological ideals that mattered to him came from the philosophy of science of his own time – for example, Herschel's (1830) ideal of a

vera causa, a cause shown to exist and to be adequate to its effects (see Hodge, 1977). The best example of a unifying theory in a plural science is, then, not an example of what *DTF* asks for. If anything, the example shows how theory and theory talk come apart.

What about local theories, then – not one theory of the field but many small ones, one for scenario planning and learning, another for Delphi and consensus, and so on? This is closer to the examples of *DTF*, and research on practices can indeed produce causal knowledge. A Delphi researcher can know when feedback narrows estimates and when it produces false consensus (see Section 6). However, calling such knowledge *local theory* inflates the word. The theory of relativity is a theory; an approximation of causal relations in a certain type of system is something else, and useful as such. (The assumptions built into methods are theoretical in the role-relative sense of Section 6; this does not make them theories in the sense in which relativity is one.) Moreover, nobody resists a good picture of the world. Where such knowledge exists, the field uses it, and the long research on Delphi is a case in point. So, if theories in the narrow sense are absent, the task is to explain why they do not exist, not to complain about resistance. Such an explanation would have to assume that theories of this kind are possible in the first place – and this brings the argument back to the peculiar concept of theory with which *DTF* begins. Management and organization studies may, of course, use the term as they wish. The problem begins when a peculiar use of the term is turned into a demand on another field.

Should the implicit theories then be made explicit, as Chermack (2002) advised? I do not think so – at least not as a duty of futures studies researchers. The knowledge of the field sits in disciplined practice, where it works. A Delphi designer who preserves the interquartile range in the report, and not only the median, has built into the method the recognition that disagreement may be more informative than consensus. A scenario team that insists on four

scenarios rather than one forecast has built into the procedure the recognition that a single trajectory is fragile. To require that such recognitions be stated as propositions, built into a system and tested before they count as knowledge would be *DTF* again. (Testing the assumptions of methods is another matter. As I argued in Section 6, such testing is done by research on the methods themselves, and it does not require that the assumptions first be built into a system.) Spelling out the theories that methods presuppose is valuable work, but it is the work of philosophy of futures studies, done by those who want to do it. Such work is not a condition for being a science.

What, then, makes futures studies a science, if not theory? I do not think that I owe a criterion here. Philosophers have searched for a long time for a criterion that separates science from non-science, without success (Laudan, 1983), and the burden of proof lies with those who claim that theory in the narrow sense is such a criterion. What I can say is how the field works when it works well: through styles of reasoning with their own standards, methods in which the recognition of typical errors is built in, and expertise that corrects itself over time. This is a description, not a passport. Geology and historiography work in the same way.

8 The nine reasons revisited

Let me now return briefly to the nine reasons that Fergnani and Chermack (2021, pp. 6–9) give for the resistance to scientific theory in the field. They fall into two groups.

The first four reasons – the different use of the term *theory*, the lack of training in theory building and testing, the misunderstanding of what scientific assessment is, and the conflation of predicting the future with predicting the outcomes of interventions – are explanations of an absence. They explain why the field lacks theory in the narrow sense. If the arguments above are correct, the absence is not a defect. The four reasons may still explain part of the absence

– those who are not trained in theory building do not build theories – but explaining an absence is not the same as diagnosing a failure. The first reason gets a positive answer in Section 6: the different uses of *theory* track different roles. The fourth reason gives the distinction on which the second dilemma rests. As for the second reason, training may well be lacking, but the training that would help the debate is training in philosophy of science rather than in theory building (see below).

The remaining five reasons are of a different kind. They are disinterest in doing science, misplaced "conscious/subconscious" social constructionist worldviews, misplaced post-normal worldviews, the enjoyment of being outliers, and the cult of personalities. These are not arguments about theory. They attribute attitudes, motives and subconscious beliefs to the people in the field – a psychology of the field, as it were – and the umbrella phrase of the paper, the "conscious or subconscious" resistance (p. 2), says as much. Such a diagnosis would need evidence about motives – what researchers want, fear or enjoy. The paper offers readings of the history and the professional culture of the field, but no such evidence, and an absence of theory alone is not evidence either. I do not answer in kind. Whether futures studies researchers enjoy being outliers or not, the question of what theory is and what role it plays in the field remains exactly where it was, and it is this question that I have tried to answer.

What would help the debate, then? Not more theory building, but more care about what theory is. This requires, first, engaging the philosophical discussion on theories as it is today – on models, practices, mechanisms and invariance – and not only the classical texts or the methodology literature that summarises them. Classical philosophy of science is worth knowing, but it is not a description of science as it is practiced now. Second, it requires separating what researchers say they do from what they do, in management and organization

studies as much as in futures studies. Third, it requires asking whether *resistance* is the right explanation in the first place. There is a simpler explanation available: the field has not adopted the concept of theory of *DTF* because it is not clear what the concept is, and because, in so far as it is clear, there are good reasons not to adopt it – reasons like the ones given in this paper.

9 Conclusion

DTF has been repeated for more than twenty years, and its structure is simple: a concept of theory is defined in a certain way, the field is found lacking, and the lack is explained as resistance. I have argued that the problem lies in the definition. Philosophy of science has not settled what a theory is; *DTF* settles it with a definition from methodology texts, a definition that moves between a deductive system and a causal claim, and when the field does not follow, this is not resistance. Moreover, the definition runs into two dilemmas. Either *theory* means an axiomatic, deductive system in the sense of classical philosophy of science, and then most sciences are not about theory, and demanding it from futures studies would paralyse the field, or *theory* means something wider, including tested causal claims, models and frameworks, and then futures studies has theories already. Middle-range theory does not escape the dilemma: in Merton's version, it is the narrow sense with a smaller scope, and in later versions, a label that borrows its content from one of the horns. And either the proposal to study foresight practices instead of futures is trivial, or it is untrue. There is no halfway theory, and there is no halfway field.

I have also argued that theories play at least four roles in futures studies: borrowed domain theories supply substantive knowledge, method theories come built into methods, actors' theories are part of the object of study, and theories of the field reconstruct what it

presupposes. None of these requires a theory of futures studies' own in the narrow sense, and the field does not need one. A demand that the field should have one would not make futures studies more scientific; it would make it smaller.

Fergnani and Chermack (2021, p. 13) end with the observation that futures and foresight suffers from a role akin to alchemy in the eyes of scientists, and that it is still considered a pseudoscience. Perhaps it is, in some eyes. But the burden of proof lies with those who make theory in the narrow sense the mark of science, and that burden has not been met. The way out is not to count horses. Sciences that study what nobody has observed – the deep past, the distant, the merely possible – earn their standing by the discipline of their practices, by how they combine methods and borrowed knowledge, and by what they help us to understand. This is what futures studies does when it is done well, and this is what futures studies researchers can show, case by case. Much remains to be understood about how the field does it, and that is work worth doing.

References

Abbott, A. (1988). Transcending general linear reality. *Sociological Theory*, *6*(2), 169–186.

Bacharach, S. B. (1989). Organizational theories: Some criteria for evaluation. *Academy of Management Review*, *14*(4), 496–515.

Bhaskar, R. (1975). *A realist theory of science*. Leeds Books.

Campbell, N. R. (1920). *Physics: The elements*. Cambridge University Press.

Cartwright, N. (1983). *How the laws of physics lie*. Oxford University Press.

Chang, H. (2012). *Is water H2O? Evidence, realism and pluralism*. Springer.

Chermack, T. J. (2002). The mandate for theory in scenario planning. *Futures Research Quarterly*, *18*(2), 25–28.

Chermack, T. J. (2005). Studying scenario planning: Theory, research suggestions, and hypotheses. *Technological Forecasting and Social Change, 72*(1), 59–73.

Chermack, T. J. (2007). Disciplined imagination: Building scenarios and building theories. *Futures, 39*(1), 1–15.

Chermack, T. J. (2018). An analysis and categorization of scenario planning scholarship from 1995–2016. *Journal of Futures Studies, 22*(4), 45–60.

Craver, C. F. (2002). Structures of scientific theories. In P. Machamer & M. Silberstein (Eds.), *The Blackwell guide to the philosophy of science* (pp. 55–79). Blackwell.

Currie, A. (2018). *Rock, bone, and ruin: An optimist's guide to the historical sciences*. MIT Press.

DiMaggio, P. J. (1995). Comments on "What theory is not". *Administrative Science Quarterly*, *40*(3), 391–397.

Dobzhansky, T. (1973). Nothing in biology makes sense except in the light of evolution. *The American Biology Teacher, 35*(3), 125–129.

Dubin, R. (1978). *Theory building* (Rev. ed.). Free Press.

Einstein, A. (1934). On the method of theoretical physics. *Philosophy of Science, 1*(2), 163–169.

Fergnani, A., & Chermack, T. J. (2021). The resistance to scientific theory in futures and foresight, and what to do about it. *Futures & Foresight Science, 3*, e61. https://doi.org/10.1002/ffo2.61

Hacking, I. (1983). *Representing and intervening*. Cambridge University Press.

Hacking, I. (1992). ‘Style’ for historians and philosophers. *Studies in History and Philosophy of Science, 23*(1), 1–20.

Halvorson, H. (2012). What scientific theories could not be. *Philosophy of Science, 79*(2), 183–206.

Hambrick, D. C. (2007). The field of management’s devotion to theory: Too much of a good thing? *Academy of Management Journal, 50*(6), 1346–1352.

Hedström, P., & Udehn, L. (2009). Analytical sociology and theories of the middle range. In P. Hedström & P. Bearman (Eds.), *The Oxford handbook of analytical sociology* (pp. 25–50). Oxford University Press.

Hempel, C. G. (1966). *Philosophy of natural science*. Prentice-Hall.

Hempel, C. G., & Oppenheim, P. (1948). Studies in the logic of explanation. *Philosophy of Science, 15*(2), 135–175.

Herschel, J. F. W. (1830). *A preliminary discourse on the study of natural philosophy*. Longman.

Hodge, M. J. S. (1977). The structure and strategy of Darwin’s “long argument”. *The British Journal for the History of Science, 10*(3), 237–246.

Hodgkinson, G. P., & Healey, M. P. (2008). Toward a (pragmatic) science of strategic intervention: Design propositions for scenario planning. *Organization Studies*, *29*(3), 435–457.

Inayatullah, S. (1998). Causal layered analysis: Poststructuralism as method. *Futures, 30*(8), 815–829.

Kerlinger, F. N., & Lee, H. B. (2000). *Foundations of behavioral research* (4th ed.). Harcourt College.

Kong, J. D., & Bragazzi, N. L. (2027). Toward an integrated epistemology of futures thinking: A meta-synthetic taxonomy of foresight methodologies. *Futures, 185*, Article 103957. https://doi.org/10.1016/j.futures.2026.103957

Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). University of Chicago Press.

Laudan, L. (1983). The demise of the demarcation problem. In R. S. Cohen & L. Laudan (Eds.), *Physics, philosophy and psychoanalysis* (pp. 111–127). Reidel.

Lloyd, E. A. (1988). *The structure and confirmation of evolutionary theory*. Greenwood Press.

Lukka, K., & Vinnari, E. (2014). Domain theory and method theory in management accounting research. *Accounting, Auditing & Accountability Journal, 27*(8), 1308–1338.

Lynham, S. A. (2002). The general method of theory-building research in applied disciplines. *Advances in Developing Human Resources, 4*(3), 221–241.

MacKenzie, D. (2006). *An engine, not a camera: How financial models shape markets*. MIT Press.

Mermet, L., Fuller, T., & van der Helm, R. (2009). Re-examining and renewing theoretical underpinnings of the futures field: A pressing and long-term challenge. *Futures, 41*(2), 67–70.

Merton, R. K. (1948). The self-fulfilling prophecy. *The Antioch Review, 8*(2), 193–210.

Merton, R. K. (1968). *Social theory and social structure* (Enlarged ed.). Free Press.

Minkkinen, M. (2020). Theories in futures studies: Examining the theory base of the futures field in light of survey results. *World Futures Review, 12*(1), 12–25.

Mohr, L. B. (1982). *Explaining organizational behavior: The limits and possibilities of theory and research*. Jossey-Bass.

Morgan, M. S., & Morrison, M. (Eds.). (1999). *Models as mediators: Perspectives on natural and social science*. Cambridge University Press.

Pawson, R., & Tilley, N. (1997). *Realistic evaluation*. Sage.

Piirainen, K. A., & Gonzalez, R. A. (2015). Theory of and within foresight – "What does a theory of foresight even mean?" *Technological Forecasting and Social Change, 96*, 191–201. https://doi.org/10.1016/j.techfore.2015.03.003

Popper, K. R. (1959). *The logic of scientific discovery*. Hutchinson.

Psillos, S. (1999). *Scientific realism: How science tracks truth*. Routledge.

Putnam, H. (1962). What theories are not. In E. Nagel, P. Suppes, & A. Tarski (Eds.), *Logic, methodology and philosophy of science* (pp. 240–251). Stanford University Press.

Reichenbach, H. (1938). *Experience and prediction: An analysis of the foundations and the structure of knowledge*. University of Chicago Press.

Rohrbeck, R., & Kum, M. E. (2018). Corporate foresight and its impact on firm performance: A longitudinal analysis. *Technological Forecasting and Social Change, 129*, 105–116.

Rowe, G., & Wright, G. (1999). The Delphi technique as a forecasting tool: Issues and analysis. *International Journal of Forecasting, 15*(4), 353–375.

Savage, C. W. (Ed.). (1990). *Scientific theories* (Minnesota Studies in the Philosophy of Science, Vol. 14). University of Minnesota Press.

Spaniol, M. J., & Rowland, N. J. (2018). The scenario planning paradox. *Futures*, *95*, 33–43.

Spaniol, M. J., & Rowland, N. J. (2019). Defining scenario. *Futures & Foresight Science, 1*(1), e3.

Suppe, F. (Ed.). (1977). *The structure of scientific theories* (2nd ed.). University of Illinois Press.

Sutton, R. I., & Staw, B. M. (1995). What theory is not. *Administrative Science Quarterly, 40*(3), 371–384.

Thompson, M. (2011). Ontological shift or ontological drift? Reality claims, epistemological frameworks, and theory generation in organization studies. *Academy of Management Review, 36*(4), 754–773.

Thompson, P. (1989). *The structure of biological theories*. State University of New York Press.

van Fraassen, B. C. (1980). *The scientific image*. Clarendon Press.

Virmajoki, V. (2026a). The greatest mystery of futures studies. *Futures & Foresight Science, 8*, Article e70046.

Virmajoki, V. (2026b). The shining: Illuminating philosophy and futures studies. *Futures, 179*, Article 103812.

Virmajoki, V. (2027). On the intellectual unity of futures studies. *Futures, 185*, Article 103971. https://doi.org/10.1016/j.futures.2026.103971

Weick, K. E. (1989). Theory construction as disciplined imagination. *Academy of Management Review*, *14*(4), 516–531.

Weick, K. E. (1995). What theory is not, theorizing is. *Administrative Science Quarterly, 40*(3), 385–390.

Whetten, D. A. (1989). What constitutes a theoretical contribution? *Academy of Management Review*, *14*(4), 490–495.

Williams, M. B. (1970). Deducing the consequences of evolution: A mathematical model. *Journal of Theoretical Biology*, *29*(3), 343–385.

Woodward, J. (2003). *Making things happen: A theory of causal explanation*. Oxford University Press.