The Primary Scientific Contribution Is Hardly a Theory in Design Science Research

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Abstract. Generally, to publish a paper in a top IS journal, making a new theory contribution is, so we are told, required. Such a requirement also exists in Design Science Research (DSR) literature. We review a number of claims about the necessity of theory as it applies to DSR. We find these claims wanting. For example, medical research and engineering are both called "design science" in Simon's (1996) *Sciences of the Artificial*. However, most articles in the top medical, computer engineering, and network engineering journals do not develop new theories. Unless the proponents of theories, as the primary vehicle of scientific DSR knowledge, can offer a satisfactory argument for why theories are the primary scientific contribution, we do not have to regard 'theory' as the primary outcome of good scientific research in DSR. If we are correct, then theory is not valuable in its own right in (applied) science, as theory serves higher purposes.

Keywords: Theory, Design Science Research.

1 Introduction: The Necessity of Theory

Theory is generally considered important in Information Systems (IS). For instance, the former editor-in-chief of *MIS Quarterly* noted how a "required element" for any excellent paper is that it "sufficiently uses or develops theory" (Straub 2009, p. vi). The supposed necessity of theory has also found its way into Design Science Research (DSR): "it is of vital importance to investigate how design knowledge can be expressed as theory" (Gregor & Jones 2007, p. 314). Venable (2013 in Iivari 2020, p. 504) reported, "Since theory is a key output of rigorous academic research, one would expect the production of DT [design theory] to be a key element of DSR." It has been claimed that developing a theory is how academics differ from practitioners (Gregor 2006, 2014). As a final example, "it has proven difficult to publish novel design artefacts as purely empirical contributions without substantial theoretical contribution" (Ågerfalk 2014, p. 595).

Given such emphasis on theory in IS and DSR, Iivari (2020, p. 503) found "theory fetish" in DSR, which for him is an "excessive emphasis on" theory building and theory "as if they were the only remarkable scientific contribution." According to Iivari (2020, p.

504), DSR "has fallen into the theory trap." A fundamental question before us is whether the primary remarkable scientific contribution in DSR is developing a theory.

In our attempt to answer this question, we review the following claims for the necessity of theory in IS and DSR: 1) Theory is the only remarkable scientific contribution (Iivari 2020). 2) Theory is the primary scientific contribution (Iivari's 2020 view slightly revised). 3) Theory separates academics from practice (Gregor 2006, 2013). 4) Mature disciplines have built solid theories.

We find all of these arguments wanting. The chief difficulty is that most scientists rarely develop a new theory, even in the most prestigious scientific journals. For example, Simon (1996) dubbed medical research "design sciences." However, most articles in the top medical journals, such as the *New England Journal of Medicine* (NEJM), do not claim to develop new theories (Siponen & Klaavuniemi 2019). In fact, one can find thousands of articles in the NEJM that do not even contain the word "theory" (Siponen & Klaavuniemi 2019). Or when they use "theory," they mean a speculative guess.

Other problems abound. In IS/DSR, there is an ambition to assert a demarcation tenet—namely, that *theory* (and theory development) is what separates true scientists from mere practitioners (see Gregor 2006, 2013). To defend this argument, one needs a demarcation taxonomy. The taxonomy must not include non-scientific practitioners' outcomes as a scientific theory—the issue of inclusion. It remains debatable whether theory frameworks in IS can satisfy such inclusion requirements. In this paper, we focus on Gregor's (2006) theory taxonomy. We suggest that it is vulnerable to the problem of inclusion. For example, a piece of programming code could (and as we shall later demonstrate, does) pass Gregor's (2006) DSR theory test with flying colors!

2 Theory in DSR and the Theory Fetish Arguments

In this section, we consider the arguments about whether a theory is the main source of scientific knowledge in sciences and DSR. Iivari (2020, p. 512) warned about conceptual confusions in DSR and wished that "scientific discourse should be conceptually as clear as possible." Against this background, it is important to clarify what the arguments vindicating the need for theory amount to.

Two extremes should be avoided. In the first extreme, called *no theory*, we should not argue that scientists never develop theories, because there are plenty of cases where scientists have developed something they referred to as theories. We need only mention the names Newton, Darwin, Einstein, and Kohlberg. Thus, we do not need to waste any time on this 'no theory' theory, i.e., that theories have zero importance in science.

As for the second extreme, we do not wish to consider what we call the *theory only* view. According to this tenet, theories and their development are "the only remarkable scientific contribution" (Iivari 2020, p. 503). We find "only" too restrictive, as one can find some cases in top IS journals (e.g., method articles) where the contribution is not claimed to be a theory. In other words, even the most devoted theory advocates in IS may reject this claim as formulated. Thus, we suggest slightly revising Iivari's interesting argument into something along the following lines: The primary scientific contribution is the development of a theory.

However, there are other pitfalls we need to watch out for. First, it is important to note what counts as a measure of scientific contributions. As most readers probably know, resolving this matter involves complex issues that go beyond one paper's ability to solve. At the same time, it is necessary to say something about this definition, as otherwise we would run the risk of talking over each other's heads. As one concern regarding theory in IS is publishing a paper in a top journal (e.g., Straub 2009; Ågerfalk 2014), we use this as a "measure." The argument, then, which we wish to challenge in this paper, runs as follows: The primary remarkable scientific contribution—in top scientific journals at least—is developing a new theory. Thus, the term "primary" captures the following spirit: *For most articles*, a new theory contribution is required. We refer to this argument as *theory as a primary contribution*. With these clarifications, we consider the existing arguments in IS for theories, as summarized in Table 1.

Table 1. Summary of the theory arguments, whose merits we review in this paper.

Theory argument	Brief description
Theory separates science from practice	"Developing theory is what we are meant to do as academic researchers and it sets us apart from practitioners and consultants"
Theory as primary contribution	The primary remarkable scientific contribution—in top scientific journals—is developing a new theory
Mature discipline Theory argument	Mature disciplines have already developed theories

In the next sections, we discuss these arguments. However, this paper is far from being a complete treatment of all theory arguments in IS. For example, a number of relevant theory arguments (e.g., Avison & Malaurent 2014a, 2014b; Hirschheim 2019; Schlagwein 2021), albeit they merit discussion, cannot be covered in this paper.

Moreover, it has been asked to what extent the arguments discussed in this paper have been covered by IS literature in general or our previous work in particular. The theory arguments in Table 1 are, of course, presented in the literature. The new contribution of this paper is to *challenge these arguments, including showing how they are problematic in various ways*. As far as we know, this part of challenging these arguments (Table 1) is new in IS literature. In addition, regarding our previous work on IS philosophy, Siponen and Tsohou (2018, 2020) criticized "positivism" in IS, not theory in IS. In turn, Siponen and Klaavuniemi (2020) questioned the claim that most IS research follows the hypothetico-deductive (H-D) method. One reason is that the H-D method in the philosophy of science assumes that hypotheses or theories are guessed. Siponen and Klaavuniemi (2020) do not discuss any of these claims in Table 1. Moreover, Siponen and Klaavuniemi (2021) discussed questionable natural science beliefs in IS, and included relevant material for theories, but did not discuss any of these arguments (Table 1) we try to challenge in this paper. In summary, those papers have zero overlap with the arguments discussed in this paper. With that said, we present evidence in sections 2.1.1 and 2.2 from the NEJM and *Cell*, which are also mentioned in Siponen and Klaavuniemi (2019).

2.1 Theory Is What Academics Do, and How Academics Differ From Practice

Gregor (2006, p. 613) famously noted: "developing theory is what we are meant to do as academic researchers and it sets us apart from practitioners and consultants." She reiterated the argument in Gregor (2014). Answering this claim requires saying something about what is meant by "developing theory." We discuss two interpretations. According to the first interpretation, called *named theory*, a theory is simply whatever scholars call a theory. To be more precise, developing a theory means proposing a specific theory or named theory. We now examine this interpretation in more detail.

Named Theory Counterargument. Iivari (2020) discussed how "editorial statements" in some physics and economics journals do not manifest the importance of theory. However, as he noted, this information alone makes it hard to draw any conclusions about the status of the theories in the articles. Unfortunately, he does not present any evidence from the articles. Fortunately, we can present some evidence.

In IS, Simon (1996) is noted as the seminal account of design theory (e.g., Gregor & Jones 2007; Gregor & Hevner 2013). Simon himself listed medical research and not physics as a prime example of Design Science Research. Arguably, the most prestigious journal in medical research is the NEJM (impact factor 74.7). The NEJM can hardly be excluded from the list of top medical journals. Yet a reader of NEJM quickly discovers that most of the articles do not develop any theories (Siponen & Klaavuniemi 2019). As we reported in Siponen and Klaavuniemi (2019), NEJM published about 1000 cancer-related articles between January 2012 and January 2017. In this sample, no new theories were proposed. None of these studies tested a specifically named theory. At least, they did not claim to do so. So, where does this lead us?

With the named theory interpretation and Gregor's argument, the evidence from the NEJM comes down to this. Most cancer scholars—even those who publish in the best medical journals (e.g., the NEJM)—are "practitioners and consultants," and not "academic researchers" (cf., Gregor 2006, p. 613). Moreover, following this interpretation, they are not doing what they "are meant to do as academics." This is because most of them never claim to develop a theory. The *named theory* interpretation puts Gregor's argument in jeopardy. We should not believe that cancer researchers publishing in the best medical journals such as the NEJM are "practitioners and consultants," and not "academic researchers," simply because they are not developing theories.

2.2 Gregor's Taxonomy May Not Separate Science from Practice

There is another alternative interpretation of the claim that "developing theory is what we are meant to do as academic researchers and it sets us apart from practitioners and consultants" (Gregor 2006, p. 613). The argument runs as follows. We accept that, "Okay, scientists might not use the term 'theory.'" "That being said," the argument continues, "even if they do not mention 'theory,' we can still find elements of theories in their papers." We call this the *theory can be inferred* argument. In other words, although the authors do not call them theories, we would be justified in calling most contributions in journals such as *Cell*, *Nature*, and NEJM theories. Is this argument successful?

The obvious critique is, what is this supposed to demonstrate about *the importance of theory*? If the scientists themselves do not bother to call their contributions theories, then this suggests that the "theory" is not important to them! It also showcases that the whole rhetoric of theory is not important for acceptance by top journals, such as *Cell*, *Nature*, and the NEJM. Why should we call something a theory when many scientists themselves do not bother to call it such?

Even if we ignore this counterargument, there are other problems with the *theory can be inferred* claim. Anyone making such an inference must provide a criterion of what makes a theory. Aside from Gregor's theory taxonomy, not many criteria are available in IS. For DSR, Gregor (2006) might be the only one. Bacharach's (1989) theory account has the status of "general agreement" (Rivard 2014) and "general consensus" theory account in IS (Hirschheim 2019). But it does not talk about DSR.

Moreover, the mere existence of a theory taxonomy is not enough. Remember that we discuss the argument that "developing theory is what we are meant to do as academic researchers and it sets us apart from practitioners and consultants" (Gregor 2006, p. 613). To successfully use this argument with the *theory can be inferred* argument, any theory taxonomy must satisfy (at least) the following requirements:

1) The theory taxonomy must not exclude something that arguably presents genuine scientific theories.
2) The theory taxonomy must not include outcomes that hardly anyone would deem theories, but something that "practitioners and consultants" do.

A failing of either of these would be problematic. Consider the first requirement, *the problem of exclusion*. If the theory taxonomy excludes key characteristics of scientists' theories, then it cannot be used to support the claim that "developing theory is what we are meant to do as academic researchers and it sets us apart from practitioners and consultants" (Gregor 2006, p. 613).

The second requirement, the *problem of inclusion*, also causes trouble. The *problem of inclusion* can be demonstrated with the following scenario. Consider a case where the typical activities of practitioners, as defined by a theory taxonomy, later turn out to fulfill the requirements of that theory taxonomy. In such a circumstance, it would fail to support the claim that only academics develop theories, which is what supposedly makes them different from practitioners and consultants. The *problem of inclusion* and the *problem of exclusion* are widely known in the philosophy of science. For example, Carl Hempel's deductive model of explanation (Hempel & Oppenheim 1948) was later attacked on both grounds, and the model is currently deemed a failure. Can Gregor's (2006) taxonomy handle inclusion and exclusion attacks? In this paper, we can give only a few examples, and more detailed debate is beyond the scope of this paper. We also examine only the problem of inclusion. The problem of exclusion is omitted.

Gregor's Taxonomy and the Problem of Inclusion. Gregor's (2006) taxonomy, as we see it, includes cases that we should not generally deem a theory. For example, we can pick a piece of programming code and try to argue that it meets Gregor's design theory features. This attack, we argue, seems to work even with a single line of code. If our argument is successful, it would mean that any professional programmer has written thousands of design theories, if the criterion is Gregor's theory type V (2006)!

Consider writing “hello, you” in some programming language—in our case, C. Can this satisfy the features of design and action in Gregor’s theory? Gregor’s (2006) design theory “says how to do something ….The theory gives explicit prescriptions (e.g., methods, techniques, principles of form and function).” One can easily meet this requirement. If you want to display “hello, you” in C, then simply write:

```
main(){printf("hello, you\n");}
```

This “says how to do something.” It even gives “explicit prescriptions (e.g., methods, techniques, principles of form and function).” But Gregor (2006) has other elements, such as scope, means of representation, testable propositions, and a prescriptive statement. Our example can handle these easily, as demonstrated in Table 2.

Table 2. Theory elements found in a one-line program.

Gregor’s (2006) theory elements	Does our simple one-line code meet it?
Scope	Yes, in almost any computer, smart phone, tablet
Means of representation	Yes, “words” (Gregor 2006)
Testable propositions	Yes: writing the code gives “hello, you”
Prescriptive statement:	To write “hello, you” in C, write: `main(){printf("hello, you\n");}`

If we are correct here, then what happens to the claim that “developing theory is what we are meant to do as academic researchers and it sets us apart from practitioners and consultants” (Gregor 2006, p. 613)? It fails, *if* the criterion for theory is Gregor’s theory for DSR (theory type V).

2.3 Mature Discipline Theory Argument

Iivari (2020) presented the mature discipline theory argument (MDTA). The argument runs as follows: “[M]ore mature disciplines such as physics and economics … have already built solid theories” (p. 504). It is not clear whether Iivari himself endorsed this view. However, we are concerned only about the merit of the argument, not who endorses it.

We deem the MDTA problematic for several reasons. First, unfortunately, Iivari (2020) did not tell us what a “mature discipline” is. If we assume that a mature discipline is one that already has solid theories, then we run into a circular argument: Solid theories define a mature discipline which is defined as a discipline that has solid theories.

Some IS readers may hedge their bets by replying that physics or cancer research is arguably more mature than IS. For example, cancer research is mature as it has a track record of successfully treating hundreds of different types of cancers. This may be true, but that is not the point here. What is being challenged is not whether cancer research or physics is more mature than IS. What we challenge is the following: *Even if* we intuitively deem that physics or cancer research is more mature than IS, then *how do we know that the reason for their maturity is that "they have already built solid theories"?* When we are given absolutely no characterization of what counts as "mature," "immature," and "solid theory," it is hard to evaluate these arguments. Iivari's (2020, p. 512) wise advice that "scientific discourse should be conceptually as clear as possible" is needed here. As a result, there is a serious risk of guessing in the dark. Yet despite these deficiencies, something can be said about this argument.

Solid Theories in the Mature Discipline Theory Argument. First, let us start with the concept of *solid theories* in the MDTA, according to which, "mature disciplines… have already built solid theories" (Iivari, 2020 p. 504). Putting aside the fact that "solid" is not defined, we are not quite convinced of this solid theory claim. As Iivari (2020) mentions physics, let's start with that. We do not have degrees in physics. However, reading the philosophy of physics in the philosophy of science suggests a different conclusion. Consider, for example:

> Every theory we have proposed in physics, even at the time when it was most firmly entrenched, was known to be deficient in specific and detailed ways. (Cartwright 1980, p. 160)

> All our current best theories, including General Relativity and the Standard Mode of particle physics, are too flawed and ill-understood to be mistaken for anything close to Final Theory. (Hoefer 2016)

We cannot help wondering how these can be interpreted to support the claim that physics has already built solid theories. If it is true that "all current best theories" and "every theory" in physics are *known* to be deficient and flawed, then this seems to imply the opposite conclusion (in physics): No theory is solid. Other issues lead us to doubt the solid theories tenet in MDTA. Consider, for example, why many philosophers have moved from viewing science as infallible knowledge to viewing science as fallible knowledge. Laudan (1980, p. 180), for example, reported that most "17th- and 18th-century" philosophers were infallibilists. This roughly means that *scientific* theories are literally true and offer knowledge that is 100% certain (Laudan 1980). However, things changed, so that fallibilism rather than infallibilism is now the ruling view. Laudan (1983, p. 115) claimed that "most thinkers had by the mid-nineteenth century" accepted "that science offers no apodictic certainty, that all scientific theories are corrigible and may be subject to serious emendation." One reason is that, over time, the once glorious scientific theories were often found to be wanting (Laudan 1983).

We take it that most philosophers today accept some form of fallibilism. In that case, how does the argument that mature sciences "have already built solid theories" fit into the picture of fallibilism, holding that "our best theories are usually false" (Niiniluoto 1998) and "may be subject to serious emendation" (Laudan 1983, p. 115)?

MDTA and the Argument of "Theory Separates Science from Practice." We also want to point that out that Iivari's (2020) MDTA and Gregor's (2006, 2014) argument are incompatible. *If* the MDTA were to hold, then mature sciences have already developed theories—and do not develop any more of them. But if this assumption were to be granted, then according to Gregor's (2006) argument, mature scientists have turned into "practitioners and consultants" and have stopped being scientists.

If MDTA Were to Be Accepted, Where Would It Lead Us? As noted, we have deemed the MDTA problematic all along. However, let us, *for the sake of argument*, discard all the stated difficulties and accept the present MDTA, according to which mature sciences have already built solid theories. This immediately raises the question of what mature sciences are to do now. Why haven't they stopped publishing, if they have already built solid theories? Why, for example, did *Cell* publish 990 cancer-related articles between 2012 and 2017 (Siponen & Klaavuniemi 2019)? Why did the NEJM publish 985 cancer-related articles between January 2012 and January 2017 if all the solid theories have already been developed (Siponen & Klaavuniemi 2019)? *If* (for the sake of argument) we accept the MDTA, then mature sciences have already built solid theories, and they do not need any more theories. Then why do they still do science? As noted, we do not accept the MDTA. But *if* some accept the MDTA, it leads to the following corollary: Something other than theory is important for mature sciences. Then, granting the MDTA is true, we in IS have missed this something else, i.e., much of what is going on in mature science, if we mainly focus on theories.

3 Conclusive Discussion

Currently, the argument that a primary scientific contribution in the best scientific journals is a new theory seems to fail. Scientists have developed theories. Hardly anyone disputes this fact. Yet neither should we deem theory and theory development as "the *only* remarkable scientific contribution" (Iivari 2020, p. 503, emphasis added). As we see it, what is being challenged is the following claim: The primary scientific contribution, at least in the best scientific journals, is developing a (new) theory. It turns out that most papers in many of the best scientific journals do not develop theories. If many so-called design science disciplines per Simon—e.g., computer engineering, cancer research, network engineering—rarely develop a new theory in their best journals, then why should we?

Moreover, those advocating the criticality of theory in DSR and who wish to demarcate science from practice by *theory* should proffer a satisfactory account of what theory amounts to. It is not clear to what extent such accounts exist for DSR. Such accounts must withstand attacks of exclusion and inclusion. In this paper, we focused on the problem of inclusion. Whatever merits Gregor's (2006) theory taxonomy has, it seems to be open to *the problem of inclusion.* This mean that it cannot demarcate scientific theories from non-scientific accounts. If our analysis of the *problem of inclusion* is correct, then in many cases, the taxonomy may not separate science from practice, as our simple programming example demonstrates.

What we should do with the "theory"? The problem in IS, we take it, is deeming 'theory' as valuable as such, and theory has been becoming the end itself. However, perhaps *theory* is not per se intrinsically valuable in science. In this view, perhaps *theory* serves some higher purposes, which are more important than theory structures. In medical research, for example, roughly speaking, one higher purpose is an improved treatment effect, or similar treatment effect with fewer side effects. In this case, ultimately the importance not the structure of the theory. Instead, what matters is how one can improve on the existing interventions.

If our thinking here is correct, then IS and DSR research, instead of asking ‘do you have a theory?’, must return to the various aims of science. These various aims of science might be more important than ‘theory’. Outlining these various aims of science must, however, be left for future research and other papers.

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