Mechanistic Explanations and Deliberate Misrepresentations

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Abstract

The philosophy of mechanisms has developed rapidly during the last 30 years. As mechanisms-based explanations (MBEs) are often seen as an alternative to nomological, law-based explanations, MBEs could be relevant in IS. We begin by offering a short history of mechanistic philosophy and set out to clarify the contemporary landscape. We then suggest that mechanistic models provide an alternative to variance and process models in IS. Finally, we highlight how MBEs typically contain deliberate misrepresentations. Although MBEs have recently been advocated as critical realist (CR) accounts in IS, idealizations (deliberate misrepresentations) seem to violate some fundamental tenets of CR and research method principles for CR. Idealizations in MBEs, therefore, may risk being regarded as flawed in IS. If it turns out that CR cannot account for idealizations, naturalism can, and it does so without extra-philosophical baggage.

1. Introduction

In the philosophy of science, it was once widely assumed that scientific theories are comprised of laws: "A theory, as the term is actually used, is a set of laws" [1 p. 125]. The laws of nature were taken to be empirical and universal, i.e., unrestricted and exceptionless [2, 3]. Many philosophers argued that even the most "stable" biological theories, such as Darwin's theory of evolution, are too dynamic, and contain too many exceptions to be called bona fide laws of nature [4]. What, then, do the life sciences study, if not laws? Scientists working in these fields commonly talk about "mechanisms" [5]. Mechanisms-based explanations (MBEs) are not, however, peculiar to biology and biochemistry. More recently, some philosophers reported that psychologists commonly describe mechanisms [6]. As Weiskopf [7] notes: "Mechanistic explanation has an impressive track record of advancing our understanding of complex…systems." Others reported that not only philosophy of biology but also "the philosophy of science more generally, should be restructured around the fundamental idea that many scientists organize their work around the search for mechanisms" [8].

Mechanisms also hold promise for Information Systems (IS), for several reasons. First, a fundamental question is, what does the explanatory work in IS? One traditional answer is: laws. According to this view, IS models or theories "attempt to articulate a *law*" [9, p. iv, emphasis in original]. Alas, the concept of law faces many difficulties. Traditionally, "the word 'laws' has been reserved for universally applicable, exceptionless generalizations." [3, p 371]. We hardly have such laws in IS. For example, consider ease of use, explaining IT use. This is not an exceptionless law in the sense of "all men are mortal". MBEs offer an alternative to laws. MBEs, especially in the new mechanistic philosophy, are often distinguished from law-based explanations.

Second, IS models are often divided into variance and process models [10]. Mechanisms seem to allow and, indeed, to explain alternative ways of modeling.

Against this background, it is not surprising that the concept of mechanism has been introduced in the IS literature. Philosophically, mechanisms are conceived in IS as an interpretive account [11], and as a critical realist (CR) account [12, 13]. These seminal articles can be credited for bringing a philosophy of mechanisms into IS. At the same time, there is a risk that IS readers may confuse specific philosophical accounts—and specific characterizations—of mechanisms with 1) laws, 2) statistical generalizations in variance model settings IS, or 3) anything that can have a causal effect. Moreover, MBEs in IS may also be unduly limited to answering why-questions.

Our first objective, in this article, is to untangle these possible confusions by showing that both the characteristics and the aims of MBEs, in the specific

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accounts of new mechanistic philosophy, are quite different from laws and statistical studies (variance models) as they are carried out in IS. Realizing this opens up new avenues for IS research, which could otherwise become unacceptable, because they do not meet existing IS conventions, such as laws, or variance models or process models.

Our second goal is to stress that MBEs are not only incomplete representations. They may also contain deliberate, strategic misrepresentations. However, it remains debatable how major existing IS philosophical accounts can deal with the presence of deliberate misrepresentations in MBEs. Since we cannot examine all major IS "-isms" here (e.g., positivism and interpretivism), we focus on CR. We chose CR because MBEs have recently been advocated as CR accounts in IS, and IS readers are told that mechanistic philosophers have "embraced the philosophy of critical realism" [13]. Our interpretation of some major CR sources in IS suggests that idealization violates some CR tenets and research method principles for CR. To this end, we propose a different, and more promising naturalist approach. Our naturalist perspective recognizes the centrality—indeed, the indispensability—of idealization practices across all the natural and social sciences, IS included. As an added bonus, naturalism seems to avoid the extra-philosophical baggage of CR, which some scholars may find problematic or unnecessary.

2. A short (and incomplete) history and philosophy of mechanisms

As Darden [14 p. 958] noted in 2008: "The new research program to understand mechanisms [...] has developed rapidly in the last 10 years." After 2008, the understanding of MBEs has continued to develop. This is important, because the mechanistic movement in philosophy is sometimes presented, in IS, as a monolithic block. For example, Markus and Rowe [13], as well as Mingers and Standings [12, p. 175], generically refer to Salmon and Machamer et al.'s [15] mechanistic accounts without discussing some crucial differences among them. To be clear, this is not a critique of [12, 13]. However, a reader of [12] and [13] who is not familiar with the new mechanistic philosophy may get the misleading impression that these mechanistic accounts (e.g., Salmon vs. Machamer et al.) are, for all intents and purposes, identical. Explaining some differences across different mechanistic approaches is crucial to understanding MBEs.

2.1. Laws and mechanisms

Some IS mechanistic accounts [12] refer to Salmon. As we understand Salmon, the background of his view relates to the deductive-nomological (D-N) model [16]. Simply put, the D-N model purports to explain an observation by logically deducing it from laws and initial conditions. To illustrate, imagine that we observe a black raven. The observation of a black raven is called the *explanandum*, the event to be explained. The explanandum is explained by deducing it from an *explanans*, what explains the explanandum. Within the D-N model, the explanans is comprised of a true general law—All ravens are black—and the appropriate initial conditions: this is a raven. According to the D-N model, a scientific explanation answers why questions [16]. (However, the why-question thesis is not D-N specific.) An *explanans* explains a why *explanandum*. In other words, given the *explanans*, the explanandum is expected nomically (because it is a law!), and it is not a coincidence [16]. In short, one asks, "Why is this bird black?" The D-N answer is: "It is a raven, and all ravens are black" [17 p. 339]; i.e., a raven is black, because it is the law that all ravens are black.

von Wright [18 p. 19] had a concern: "a law stating the universal concomitance of the two characteristics raven-hood and blackness" is not an adequate explanation. This is because "We should like to know why ravens are black, what it is about them that 'is responsible for' the color which, so we are told, is characteristic of them all" [18 p. 19]. von Wright's "why question" concern is helpful for understanding Salmon's [19] causal mechanical model of explanation. Salmon [20 p. 708] asked, "What does scientific explanation offer?" His answer was: "mechanisms of production and propagation." A mechanism "yields scientific understanding" [20 p. 708]. For Salmon, "this is what we seek when we pose explanation-seeking why questions" [20 p. 708]. He recognized that the D-N explanation fails to provide this kind of understanding. Similarly, Railton [21] regarded D-N explanations as "incomplete" or "unsatisfactory," "unless we can back them up with an account of the mechanism(s) at work" [21, p. 208]. As Wright and Bechtel [22] note, Salmon's causal mechanical model of explanation focuses on causation, rather than mechanisms. Specifically, for Salmon [19 p. 121], "[t]o provide an explanation of a particular event is to identify the cause and, in many cases at least, to exhibit the causal relation between this event and the event-to-be-explained." According to Salmon, explaining a certain event traces the causal processes (and interactions) that lead up to this event.

In sum, on our interpretation, Salmon [19, 20] and Railton [21] linked mechanisms to laws. We return to this issue in section 3.2. In the meanwhile, let us ask how Salmon's account can be applied in IS. The IS mechanisms articles we discuss here that cited Salmon (e.g., [12, 13]) do not discuss (the applicability of) Salmon's [19] causal nexus, which is a physical network (consisting of causal interactions and causal processes), in IS. Such interaction is causal if it is capable of transmitting a mark. It is questionable whether such an approach fits IS. For example, how does any IS phenomenon meet Salmon's [19] definition of mark transmission, or consumed quantity? Salmon's idea of physical networks seems largely irrelevant and inapplicable to IS. One option is to omit the details of Salmon's specific concepts, and just regard mechanisms as "a chain of [a] web of event[s] leading to [a] particular event" [23 p. S346].

2.2. Deviation from laws

Craver and Tabery [7] reported that the new mechanical philosophy started with Bechtel and Richardson [24]. It seems safe to say that Salmon and Railton were inspired by the weaknesses of the D-N model, and assumed laws. Many writers in new mechanical philosophy, in contrast separated mechanisms from laws. Bechtel and Richardson [24] viewed mechanisms as machines: "A machine is a composite of interrelated parts, each performing its own functions, that are combined in such a way that each contributes to producing a behavior of the system. A mechanistic explanation identifies these parts and their organization, showing how the behavior of the machine is a consequence of the parts and their organization."

Many recent accounts of mechanisms seem to omit or downplay the "machine" connection (see below). Glennan [25 p. 54] regards mechanisms as complex systems: "A mechanism underlying a behavior is a complex system which produces that behavior by the interaction of a number of parts according to direct causal laws." Later, in 2002, Glennan [23 p. S345] noted that as in philosophical vernacular "laws must be exceptionless," he omitted the laws. Glennan [23] regarded other recent mechanisms accounts – such as Bechtel and Richardson [24] and Machamer et al. [15] – as a complex systems approaches, which Salmon's and Railton's approaches are not, per Glennan [23].

2.3. Activities and entities

Machamer, Darden and Craver focused on mechanisms as they are used by scientists: "Mechanisms are composed of both entities (with their properties) and activities. Activities are the producers of change. Entities are the things that engage in activities" [15 p. 3]. Many other mechanistic accounts [14, 26] also regarded mechanisms as composed of entities and activities: "Activities are the things that entities do... Activities are the causal components of mechanisms" [26 p. 371]. Darden [14] offered an "updated" account of [15], where the activity produces the change.

Machamer et al. [15 p. 3] found the reliance on "law" in Glennan's [25] mechanisms accounts problematic. In life sciences such as biology, Glennan's "direct causal laws" cannot capture how activities operate, and "leave out the productive nature of activities."[1] As a result, Glennan's 2002 account [23] omitted laws. Railton's [21 p. 208] mechanisms also backed up laws. Thus, it is questionable how this approach can accommodate mechanisms in IS.

2.5. Mechanistic-based models must idealize

It was once widely believed in the philosophy of science that scientific theories or models correspond to real phenomena, and the theories were evaluated against real-life observations [27]. According to this once-received view, in the case of the law "all ravens are black," raven and black refer to "real world" features, and the law is tested with observations of "real" ravens to see if all ravens are black.

Since the early 1980s, it has become common to note how scientific models misrepresent the assumed "real" world characteristics [27]. For example, a (mechanistic) model "deliberately abstracts away from and idealizes known details for the sake of simplicity and perspicuity" [28, p. 769]. As a result, in mechanistic models "the causal relations responsible for the explanandum [phenomena] are deliberately misrepresented on a regular basis" [28, p. 764]. Such idealization, the introduction of deliberate misrepresentations, is a necessary practice [28, p. 764]. One, prominent reason is that the real world is too complex to handle without idealizations:

> The cell and its myriad constituents compose an extremely sophisticated apparatus; a realistic representation of this plethora of entities and interactions—assuming that such a "complete" depiction is even feasible—would make the description impractical and the explanation unilluminating. [28, p. 764]

[1] "Descriptions of mechanisms render the end stage intelligible by showing how it is produced by bottom out entities and activities. To explain is not merely to redescribe one regularity as a series of several. Rather, explanation involves revealing this productive relation. It is not the regularities that explain but the activities that sustain the regularities" [15, pp. 21–22].

3. Mechanisms in IS: Some Clarifications

After our short and partial review of the history and philosophy of mechanisms, we now discuss several potential sources of the confusion an IS reader may face when reading some of the relevant IS literature.

3.1. Why or how questions?

Avgerou [11 p. 400] reported that "[e]explanatory theory addresses why *and* how observed phenomenon[a] occur." Following Salmon [20] and Railton [21] (section 2), Mingers and Standings [12] and Markus and Rowe [13] claim that MBE answer why questions. "[S]cientific realists embrace the task of explaining why things happen (…) The name they give to the hidden connection is mechanism" [13]. According to Mingers and Standings [12, p. 178], MBEs answer, "Why did Z happen and not something else?" [12 p. 178]. As we understand them, Mingers and Standings [12, p. 178], maintain that answering why questions makes the mechanism causal. However, some of the mechanisms articles (e.g., Machamer et al. [15]) cited by Markus and Rowe [13] and Mingers and Standings [12]) stress that mechanisms, instead, answer *how* questions: "[m]echanisms are sought to explain how a phenomenon comes about or how some significant process works" [15 p. 1]. As Thagard [29] explained:

> How questions [...] are best answered by specifying one or more mechanisms understood as organized entities and activities […] Thus answering a how-question is not a matter of assembling discrete arguments that can provide the answer to individual why-questions, but rather requires specification of a complex mechanism consisting of many parts and interconnections. [29 p. 251]

Thagard's [29] point could be relevant in IS. MBEs are not restricted to answering why or "why *and* how" [11 p. 400] questions. MBEs can also answer how-questions, and, perhaps, other questions too. Nevertheless, perhaps we should not limit *a priori* which type of questions MBEs can answer.

3.2. Mechanisms, laws, or anything that can have a causal effect

An IS source [12 p. 172] stated that "[i]n fact, [mechanisms are] anything that can be thought to have causal effects in the world." This claim requires clarification. On the one hand, philosophers have characterized mechanisms in various, alternative ways. In 1978, Railton [21 p. 208] (in a paper not cited by [11]) noted: "An account of the mechanism(s) is a vague notion." On the other hand, many mechanistic philosophers, cited by [12 p. 175], would deny that mechanisms are *anything* that can have causal effects. This is important, because IS readers may learn from a seminal paper [12] that Salmon and Machamer et al. [15] held this view. However, for example, per [15 p. 3], "mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions."

A critique or endorsement of Machamer et al.'s [15] account of mechanisms lies beyond our present purposes. Our point is simply to note some difficulties in applying their views to IS. For example, they stated that a "mechanism is the series of activities of entities that bring about the finish or termination conditions in a regular way" [15 p. 7]. Elsewhere, they noted, "they [mechanisms] work always or for the most part in the same way under the same conditions." [15 p. 3]. Many cancer or IS mechanisms do not meet the criterion of "regular changes," nor do "they work always or for the most part in the same way under the same conditions." In fact, Bogen [30] suggested omitting the concept "regular," because mechanisms can operate at irregular intervals. In addition, Wright and Bechtel [22 p. 31] noted that "[w]ith complex feedback loops, the mechanism can begin to behave in unexpected ways." Later, Darden [14], one of the authors of [15], seemed to agree with this. Darden [14] emphasized the importance of productivity over regularity.

Nevertheless, we want to highlight that many mechanisms philosophers (e.g., cited by [12]) characterize mechanisms differently from each other. It is important to stress (and not to ignore) these differences. One fundamental difference is that for Glennan (1996 [25], but not 2002 [23]), Salmon's [e.g., 19, 20] and Railton's [21] mechanisms play a supplementary role in law-based accounts in one way or the other. For example, for Railton [21], mechanisms back up laws. However, for many prominent "new mechanism" philosophers (e.g., Bechtel, Craver, and Darden), the law connection hardly applies. Consider Craver and Bechtel [31 p. 473], for example: "It is not laws that do the explanatory work but the account of the operation of the mechanism." The point of, e.g., Bechtel, Craver, and Darden is that MBEs in life sciences and social sciences do not act as supplements to laws. Instead, mechanistic explanations are, typically, alternatives to laws. Making this distinction is important for IS.

3.3. The ontic conception to explanation

Many forms of realism assume an ontic conception of explanation, where scientific explanations are not mental representations, but things in the world. For example, Craver's [32 p. 27] view, according to which MBEs refer to "objective features of the world," is ontic. It appears that some of the IS philosophy of mechanisms also subscribes to some ontic conception. According to Markus and Rowe [13], "[m]echanisms are conceptualized as (1) ontologically real, even if they are unobservable… and (3) able to produce effects that would not happen otherwise." Similarly, Mingers and Standing [12 p. 172] noted, "[i]n fact, [mechanisms are] anything that can be thought to have causal effects in the world." Finally, Avgerou [11 p. 400] observed that "[e]xplanatory theory addresses why and how observed phenomenon occur."

We argue that many seminal mechanism accounts in IS [e.g., 11, 12, 13] have difficulty accounting for deliberate misrepresentations (idealizations), common in mechanistic models. Similar to Love and Nathan [28, p. 764], we see such a misrepresentation as (1) typical, (2) necessary, and (3) (should be) strategic. IS views, like the ontic conception, seem to be incompatible with deliberate misrepresentations, which, in the words of Love and Nathan [28], "fictionalizes in the service of simplification." Provided that mechanistic models purposefully misrepresent phenomena, these models do not represent "objective features," "ontologically real" mechanisms or "how observed phenomenon occur," as claimed by many seminal IS articles [11, 12, 13]. Consider, Markus and Rowe's [12] view: "The position statement for causal mechanism can be phrased as follows: Causality involves real physical, psychological, and/or social processes that connect inputs and outputs under certain conditions" [13]. However, in mechanistic models, the "causal relations that produce the explanandum are idealized in mechanistic diagrams; their representation intentionally ignores known variations in properties and other components that make an actual difference" [28, p. 768]. Then, because the mechanistic accounts are idealized, "they do not show how the mechanism actually works" [28, p. 768]. If we are correct that the seminal mechanism philosophy accounts [11, 12, 13] cannot account for deliberate misrepresentations, this observation is important for the following reason. If misrepresentations are not recognized, there is a risk that they are deemed flaws in MBEs.

3.4. So-called variance models versus mechanisms-based explanations

According to Rivard [10 p. ix], "[m]ost theoretical models that are developed in the IS domain are either variance or process models." A variance or process is based on Mohr [33], as noted by Burton-Jones, McLean, and Monod [34]. Two questions arise: 1) What relationships do variance and process have with mechanistic models? 2) Can mechanistic models offer something that variance or process cannot? Let us first look at the so-called variance IS models [34].

A good candidate for a variance model in IS is the technology acceptance model (TAM) [35]. Davis [35 p. 319] called ease of use and usefulness "fundamental determinants of user acceptance [of IT]." In these so-called "variance" models in IS, typically, each relationship is presented as a statistical hypothesis, which is tested with statistical techniques. The TAM is a case in point. This model examines relationships between independent variables (ease of use and perceived usefulness) and the dependent variable (e.g., IT use or intention to use IT) statistically. Interestingly, Avgerou [11] provided a "well known example" of the mechanisms in IS. It "is the explanation of people's intention to use IT in terms of their perception of its usefulness and ease of use" [11 p. 408]. Avgerou [11] referred to Davis's [35] TAM.

Does the TAM in Davis's [35] work offer a mechanism-based explanation, as implied by Avgerou [11]? This ultimately depends on how we characterize mechanisms. Avgerou's [11] mechanism account also referred to Machamer et al. [15]. If, by mechanisms, we have in mind something along the lines of Machamer et al. [15], or a later commentary [14], then we suggest that TAM is *not* a mechanistic account. We argue that the aim of TAM [35] is to demonstrate a statistically significant relationships between independent variables (perceived ease of use and perceived usefulness) and a dependent variable (e.g., IT use). The aim is some kind of statistical explanation, and statistical generalizations. Recall Machamer et al. [15 p. 3]: "Mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions." The focus of the TAM [35] is neither examining (1) the setup to finish or termination conditions nor (2) *regular changes*, not even changes. An important objective of TAM [35], and this may apply widely in so-called variance IS models is a statistical generalization. In other words, the key aim of the statistical part is generally to demonstrate statistical generalization, where one "observes a characteristic of a sample of a population and then infers that the population itself has that characteristic, within a margin of error" [36 p. 18]. Generally, the aim of statistical generalization in IS seems to be different from examining regularity, still less regular changes. Darden's [14] follow-up of [15] changed regularity with productivity. According to Machamer et al. [15], mechanisms produce the phenomenon: "Giving a description of mechanism for a phenomenon is to explain that phenomena and its production" (p. 3). One can also question whether such productivity, as characterized by [14; 15], is a key goal in TAM. For Davis [35 p. 320], ease of use means "the degree to which a person believes that using a particular system would be free of effort." Davis [35] measured a belief, rather than explaining what produces the phenomenon of IT use or how a certain task is carried out. TAM does not explain at all what system characteristics constitute "ease of use." Hence, TAM does not really explain the production of IT use, or how the task resulting in IT use is carried out, in terms of Machamer et al. [15][14].

We highlight three takeaways. (1) The so-called variance models may not be mechanistic models, if mechanisms are defined as by [15; 14]. (It is possible, however, that TAM might meet some other definition of mechanisms.) This is because a key goal of variance models, such as TAM, is statistical generalizability, rather than regularity or productivity. (2) Nevertheless, the variance models could offer useful information on the generalizability of some aspects of mechanisms. We say "some" aspects, because translating the mechanisms model to statistical studies tends to misrepresent the mechanisms. (3) Mechanistic models allow an examination of complex changes and reticulated connections, which variance model settings have difficulty accommodating. Reticulated means that the connections can dynamically change. Let us assume that a mechanism has only seven parts (A, B, C, D, E, F, and G). First, something takes place from A to B, from B to C, and so on, up to G. However, the next time things can happen from A to C, and then to B, and from B to E, and so on. Next, we examine to what extent process models in IS allow complexity and reticulated connections.

3.6. Process models and mechanistic models

According to Mohr [33 p. 44], "[l]oosely process theory is one that tells a little story about how something comes about". This sounds somewhat like a mechanism. However, for Mohr [33], this characterization of "process" was not satisfactory: "in order to qualify as a theoretical explanation…, the manner of storytelling must conform to narrow specifications" [33 p. 44]. As a result, Mohr [33 p. 44] gave process theory "a highly specific meaning." These "narrow specifications" include that "the basis of the explanation is probabilistic rearrangement," "a processes theory deals with a final cause," and "events," yet "the precursor (X) is a necessary condition for the outcome" [33 p. 44]. However, mechanisms (as defined by e.g., [14; 15]) are not necessarily event based. In addition, it is questionable whether mechanistic models [e.g., 14; 15] generally meet the requirements of necessary conditions. For example, mechanistic models are often idealized, and seldom complete [28]. They do not necessarily deal with final causes. With mechanistic models such as [e.g., 14, 15], the mechanism (and its part, e.g., activities) is "the basis of explanation." Regardless, with mechanistic models [e.g., 14, 15], Mohr's [33] probabilistic rearrangement may not be required. In short, it can be seen that process models, as defined by Mohr [33], are importantly different from mechanistic models (in the sense of what, e.g., [14] or [15] assign to mechanisms). To be sure, we do not claim that no mechanistic model can meet Mohr's [33] process. Our point is that one can imagine MBEs, which do not qualify as process theories, according to Mohr [33]. Therefore, many of Mohr's [33] requirements ("narrow specifications") for process seem too restrictive, and seem not to allow reticulated connections.

4. An IS example of deliberate misrepresentation

In this section, we examine an example of MBEs in IS. The example is a qualitative CR case study by Henfridsson and Bygstad [37]. We present only part of the study [37]. The authors [37] proposed three digital infrastructure mechanisms: *innovation*, *scaling,* and *adoption*, of which we present two (Fig. 1). We propose the interpretation that the mechanisms by [37] are, in fact, idealized. By idealized, we mean that the mechanisms contain deliberate misrepresentations of the assumed "real" phenomenon or what explains it. To be clear, this is not a critique of [37]. Au contraire, their models are excellent examples of idealization (deliberate misrepresentations). Henfridsson and Bygstad [37] follow CR. We then ask: how does CR account for the deliberate misrepresentations? Asking this question is important because, if misrepresentations are not recognized, there is a risk that they are deemed flaws in MBEs. We next discuss the mechanistic models by [37].

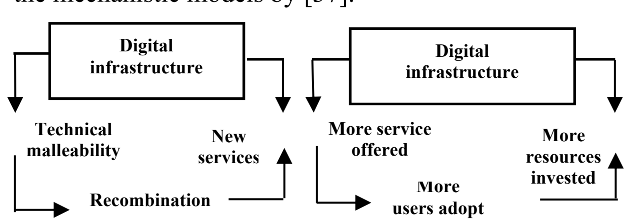

Figure 1. The mechanisms of *innovation* and *adoption* [37 p. 919].

The authors presented these mechanisms separately, for analytical reasons [37 p. 918]. In what follows, we choose to focus on the mechanism of adoption, although we maintain that our general point equally applies to the other two mechanisms. These mechanisms (Fig 1) are presented as “self-reinforcing” and recursive, in the sense that each mechanism “recursively feeds on itself" [37 p. 911].

We propose the following. Contra Henfridsson and Bygstad [37], the mechanism of adoption is hardly a “self-reinforcing process” which “recursively feeds on itself,” at least when these claims are taken at face value. First, it is not always the case that the more services are offered, the more users adopt them. Similarly, it is not always true that “with more users adopting the infrastructure services, more resources were allocated to improve and extend the infrastructure” [37 p. 919]. There are obvious exceptions. Second, even if we accept, for the sake of the argument, that the cycle-recursive process takes place in ”real” settings, it still would not be self-reinforcing and recursive for an infinite period: At some point, we would run out of users, resources, or both. Third, more services offered is the starting point for the adoption mechanisms, and new services are the outcome of innovation mechanisms [37 p. 918]. Furthermore, “the adoption mechanisms provided the financial resources to maintain the innovation mechanisms” [37 p. 918]. What this point shows is that these mechanisms mutually interact, which, taken literally, runs contrary to the claim by Henfridsson and Bygstad [37] that these cycles are individually “self-reinforcing processes” or “recursive.”. Fourth, and finally, Henfridsson and Bygstad [37 p. 918] referred to the Internet strategy, which presumably encompasses various other aspects, in addition to “more services offered.” To illustrate, it seems plausible to assume that if the systems were difficult to use, then users may feel less inclined to use them. But then, “more services offered” is not the only difference maker. Service quality, marketing tactics, and competitors’ offerings can all play a role here, interacting with more services offered, in the explanation of use.

In sum, far from being excruciatingly realistic, the models in Fig. 1 seem to contain idealizations (deliberate misrepresentations). The authors [37] themselves seemed to be aware of this to some extent. They “acknowledge that the granularity of our analysis of causality is at a relatively high level, suggesting the existence of nested causal paths in digital infrastructure evolution left unaddressed in this study. Thus, we do not claim that we have discovered all of the mechanisms relevant for infrastructure evolution” [37 p. 928]. We want to advance the interpretation that they [37] deliberately misrepresented the mechanisms of adoption.

[37] was published in the *MIS Quarterly* special issue on CR, and discussed by [11] as a paradigmatic CR study. This study, as stated by the authors, “adopts a critical realist view” [37 p. 907-908, p. 910] in the sense of Bhaskar. CR “defends a strongly realist ontology that there is an existing, causally efficacious, world independent of our knowledge” [38 p. 795]. “CR recognizes,” Mingers et al. [38] continued, “that our access to this world is in fact limited and always mediated by our perceptual and theoretical lenses. It accepts epistemic relativity (that knowledge is always local and historical), but not judgmental relativity (that all viewpoints must be equally valid)” [38, p. 795]. Similarly, the CR case study principles reported that “scientific research must revolve around the independent reality that comprises the world, even though humans are usually unable to fully understand or observe this reality, and that our knowledge of reality is fallible” [39 p. 789].

Could CR theses account for the misrepresentations (Fig. 1)? A full review of CR is beyond the scope of this paper. Thus, we focus on some CR theses (e.g., fallibilism, theory-ladennes, realist ontology) in major IS sources [11; 38; 39]. First, the fallibilism of scientific knowledge [38]. The idea that scientific theories are always revisable, and thus, can never be shown to be absolutely true, is a popular stance. Thus, is not CR specific, and it is not about idealizations. We know that mechanisms of adoption (Fig. 1) are not known to be true with absolute certainty. Nonetheless, such mechanism is not currently being rejected or revised. In addition, theory-ladenness is not unique to CR per se. It is a well-known feature of science, widely accepted across many philosophical systems. Theory-ladenness is not about deliberate misrepresentations. It is the idea that observation in science heavily depends on the theoretical background at play. Idealizations, however, can be broadly defined as the deliberate distortion of detail in a model, a sort of fictionalization in the service of simplification.

This leads to the question of which theses are germane to CR? One plausible answer is the “strongly realist ontology” advocated by Mingers et al. [38]. But, note, the “strong realism” is difficult to reconcile with the idea of idealization in scientific models. Consider the following criterion of CR case study principles: CR “seeks to posit descriptions of reality,” where the “resulting knowledge claims are focused on specifying and describing those elements of reality which must exist in order for the events and experiences under examination to have occurred” [39 p. 793]. However, the mechanisms in [37] in Fig. 1 may not accomplish this requirement, due to idealizations. Moreover, given the deliberate omission and misrepresentation of detail, it becomes difficult to maintain that the model depicted in Fig. 1 is a "concrete systems that makes it what it is" [37 p. 911].

Finally, according to CR case study principles [39], "a proposed mechanism "must survive an empirical test...where survival is indicated by the observation of evidence consistent with what the theory predicts" (Lee and Hubona 2009)" [39 p. 801]. According to CR case study principles, prediction is commonly replaced with explanation [39]. Generally, many idealizations have difficulty meeting the aforementioned empirical test. Henfridsson and Bygstad's research question [37] is "which mechanisms contingently cause the evolution of digital infrastructure, is partially geared toward defining what constitutes a digital infrastructure" [37 p. 911]. But these mechanistic models (Fig. 1) hardly "contingently cause the evolution of digital infrastructure" [37 p. 911]. We mentioned possible observations that are inconsistent with what the models explain (Fig 1). While we are not criticizing the Henfridsson and Bygstad [37] study, the causality attributed to CR research by CR methodological principles is also hard to meet.[2]

Our takeaway is the following. First, theory-ladenness, human biases, and the thesis that scientific knowledge is fallible are important ideas, but they are different from and independent of idealizations. Second, as we understand CR [37, 38] and CR case study principles [39], they both have difficulty accounting for idealizations. Furthermore, CR case study principles seem to ban idealizations: "Ensure causal factors are not idealizations" [39 p. 802]. We find this problematic, because virtually all scientific studies idealize. Failure to understand idealization leads to more trouble. CR case study principles hold that "contrary findings would possibly lead to further explication of events, structure and context, as well as additional retroduction to identify a mechanism acting to counter or nullify the proposed explanation" [39 p. 801]. However, this may not happen in the case of MBEs in scientific practice: "Even when the relevant details are known, researchers do not replace idealized causal relations with more accurate or realistic representations" [28, p. 769]. The point is that "the gradual elimination of idealized diagrams is rarely—if ever—witnessed in scientific practice" [28 p. 769]. Given the possible CR problems with idealizations, what are the other options? One option is naturalism, which we briefly introduce next.

4.1. Critical realists or naturalists?

Readers of the philosophy of mechanisms in IS [12, 13] may have inferred that various mechanistic accounts in the philosophy of science are explicitly or implicitly committed to CR[3]. However, the extent to which many of the neo-mechanistic philosophers mentioned by Markus and Rowe [13] and Mingers and Standing [12] are actually committed to CR is questionable, to say the least. For example, Markus and Rowe [13] cited Hedström and Ylikoski [40] as evidence that the proponents of mechanisms "embraced the philosophy of critical realism." However, Hedström and Ylikoski [40 pp. 56–57] offered a *critique* of CR. For them [40 p. 57], CR carries "extra philosophical baggage that we may want to avoid." [40 p. 57]. They continued that "the development of critical realism also seems to have stalled" [40 p. 57]. Later, Ylikoski [41 p. 334] reported that "critical realism seems to have some worrisome features." Thus, on one hand, it is not clear to us what value CR-*specific* theses add for the Henfridsson and Bygstad [37] CR study. (As mentioned, assuming theory-ladenness and the fallibilism of scientific knowledge do not require one to commit to CR at all.) On the other hand, we wonder whether the idealizations, such as by [37] (cf. Fig 1), violate certain CR theses and CR case study principles [39]. Although CR advocates in IS often compare CR with interpretivism and positivisms [38, 39], there are other alternatives.

For understanding mechanisms, we argue, it is important to consult critically how mechanisms are successfully used in sciences, and build the philosophizing from there. This approach is often referred to as practice-based philosophy, or naturalism. In fact, many contemporary mechanisms philosophers cited by [12, 13] refer to themselves as "naturalists" or "practice-based philosophers" [14, 15, 42, 43]. To be sure, naturalism is not a homogenous movement [43]. However, an important point shared by many naturalists is a rejection of the idea of *a priori* philosophizing. Thagard [43 p. 249] thought that "naturalists agree that progress in philosophy requires close attention to scientific developments". Bechtel

[2] CR assumes causality in research models "if and only if it is the case that some event E would not have occurred, under the conditions that actually prevailed but for (the operation of) X" (Bhaskar 1998, p. 101)" [39 p. 789]. Consider also, "why did Z happen and not something else? To answer this we have to examine the characteristics and properties of the mechanisms that are involved in the events so that we can explain the particular event as following from the causal powers of these mechanisms. This is generative or mechanism causality" [12, p. 178].

[3] "The causal mechanism concept evolved somewhat independently in two intellectual communities, scientific realism and sociology, that appear to be converging under the banner of critical realism (Hedström and Ylikoski 2010)" [13]. Elsewhere, [13] also noted that "proponents of social mechanisms appear to have embraced the philosophy of critical realism...as a foundation for their theorizing (Hedström and Ylikoski 2010)" [13].

[42] said, "[p]hilosophers of science adopting a naturalistic perspective often present themselves as investigating the domain of science in the manner in which scientists investigate phenomena in their own domains of inquiry" (p. 2). This is, roughly speaking, what we mean by naturalism. It is an attempt to understand the mechanisms used by scientists, without imposing "extra philosophical baggage" [40]. Naturalism does not have to be dogmatic toward scientific practice. Because naturalism avoids imposing *a priori* concepts from philosophy on actual scientific practice, it allows IS scholars some freedom to propose MBEs based on the phenomenon they study.

5. Conclusions

Scholars [e.g., 11, 12, 13, 39, 40] have increased our understanding of mechanisms in IS. We suggested a distinction between (1) laws, (2) variance models aimed at statistical explanations and statistical generalizations, and (3) some mechanistic accounts [e.g., 14; 15], for the following reasons. Universal laws do not allow dynamics and change (e.g., reticulated connections). In addition, variance models may aim for statistical generalizations. Although useful, variance models in IS may not be well suited for understanding a highly dynamic phenomenon. Moreover, statistical generalizations for IS may not be aimed at showing "productivity." In turn, Mohr's [41] process contained characteristics that may limit dynamic and reticulated theorizing. MBEs may help here by allowing the modeling of a complex and reticulated phenomenon, for example, with pathway modeling. Furthermore, mechanistic models are idealized (contain purposeful misrepresentations) for strategic reasons. Assuming that IS phenomena are not much more stable and simpler than biological or economical phenomena, even the most detailed mechanistic models in IS will most likely be idealizations, too. If so, "[t]he goal of [a] mechanistic explanation is not an all-inclusive single model but a series of many complementary diagrams and descriptions comprising different idealizations" [28, p. 772]. Such a multi-model strategy could also be useful in IS. Mechanistic models tend to idealize, but idealizations may not be considered part of standard scientific practice in major accounts of MBEs in IS. As result, there could be a risk that idealizations end up criticized as a flaw in IS, and IS authors may have to claim that idealizations correspond to the assumed reality. The latter case may result in some IS scholars and practitioners confusing idealized mechanistic models with assumed "real" mechanisms. Nevertheless, future IS philosophy is needed to understand MBEs more. Naturalism offers one alternative. It may be a good alternative, especially if it turns out that CR, positivism, and interpretivism cannot do justice to idealizations.

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