Scientific realism, the necessity of causal contact in measurement and emergent variables

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Scientific Realism, the Necessity of Causal Contact in Measurement, and Emergent Variables

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Design/methodology/approach

A reflective essay.

Purpose

To correct errors in, and comment on the claims made in the comment papers of Rigdon (in press) and Henseler and Schuberth (in press), and to tidy up any substantive oversights made in Cadogan and Lee (in press).

Findings

(1) In scientific realism, hypothetical causal contact between the unobserved and the observed is a key foundational stance, and as such, Rigdon's CPF is inherently anti-realist in nature. (2) The suggestion that composite-creating statistical packages (such as PLS) can model emergent variables should be treated with skepticism by realists.

Research limitations/implications

Claims made by Rigdon regarding the realism of CPF are unfounded, and claims by Henseler and Schuberth regarding the universal suitability of PLS as a tool for use by researchers of all ontological stripes (see their Table 5) do not appear to be well-grounded.

Practical Implications

Realists should not use PLS.

Originality

The study assesses PLS using the Eleatic Principle, and examines Henseler and Schuberth's version of emergent variables from an ontological perspective.

Keywords: Composites; PLS Partial Least Squares; Structural Equation Models; Antirealism, Instrumentalism, Pragmatism and Constructivism; Unobservable Conceptual Variables; Latent Variables; Scientific Realism; Causality; Emergent Variables; Generative Atomism.

Scientific Realism, the Necessity of Causal Contact in Measurement, and Emergent Variables

The original paper, "A Miracle of Measurement or Accidental Constructivism: How PLS Subverts the Realist Search for Truth" (from now, AMOM) seeks to examine partial least squares path modeling (hereafter, PLS), a well-known and oft-used statistical package, to determine whether it is suitable for use by scientific realists, and by way of comparison to assess PLS's alignment with constructivism. AMOM concludes that PLS does not meet the standards that a scientific realist would expect a statistical package to aspire to but, rather, PLS does align neatly with constructivism.

Why examine PLS for fit with scientific realism in particular? At a superficial level, scientific realism accepts "scientific theories (more or less) 'at face value'…", believes in "unobservable entities", and "defends a degree of rationally justifiable optimism regarding scientific knowledge, progress, or representational adequacy with respect to directly unobservable features of reality" (Saatsi, 2018, p. 1). Importantly, "scientific realism is the dominant philosophy of science today. It is also the tacit philosophy of most working scientists" (Haig, 2018a, p. 160). It seems reasonable to assume, then, that many scientific realists are among those using (or considering using) PLS for their analysis purposes, or are journal editors making decisions on whether to publish papers using PLS, or are scholars assessing the results of published PLS analyses, or are practitioners looking to implement the recommendations drawn from the scientific papers that publish studies using PLS. Knowing whether PLS is appropriate for scientific realists' endeavors is clearly of importance.

Accordingly, the general structure of the argumentation in AMOM is as follows:

a) The basic stances underpinning constructivism and scientific realism are described.

b) PLS is compared to those stances, and it is shown that PLS does not align with or aspire to align with realism, but that it does align with constructivism.

c) Therefore, the logical position for a realist would be to ignore PLS if possible, in favor of methods that at least aspire to realism. Constructivists may find PLS useful, however.

d) Several possible challenges to the conclusion that PLS should be dropped by realists in favor of more aspirational methods are presented, analyzed, and dismissed.

In this context, it is a pleasure to respond to Rigdon's (in press) "The Proxy of Dorian Gray: Scientific Realism, Construct Validation, and the Way Forward" (hereafter, PROXY), and Henseler and Schuberth's (in press) "Partial Least Squares as a Tool for Scientific Inquiry: Comments on Cadogan and Lee".

Rigdon does not agree with AMOM's conclusions. While Rigdon claims there to be many individual faults in AMOM, his claims rest on a single relatively straightforward disagreement. Put simply, AMOM does not include proxy variables in its discussion of realism, or in the realist variable framework (RVF) that it presents. The RVF is reproduced here in Figure 1. Rather, PROXY subscribes to Rigdon and colleagues' recent work (Rigdon, 2012; Rigdon, 2016; Rigdon *et al*., 2019), which appears unique among self-identified realist perspectives in being fixated on including proxy variables as essential to its conceptualization of realist measurement and research. Thus, to a large extent, the views expressed in AMOM

and PROXY are mutually exclusive.

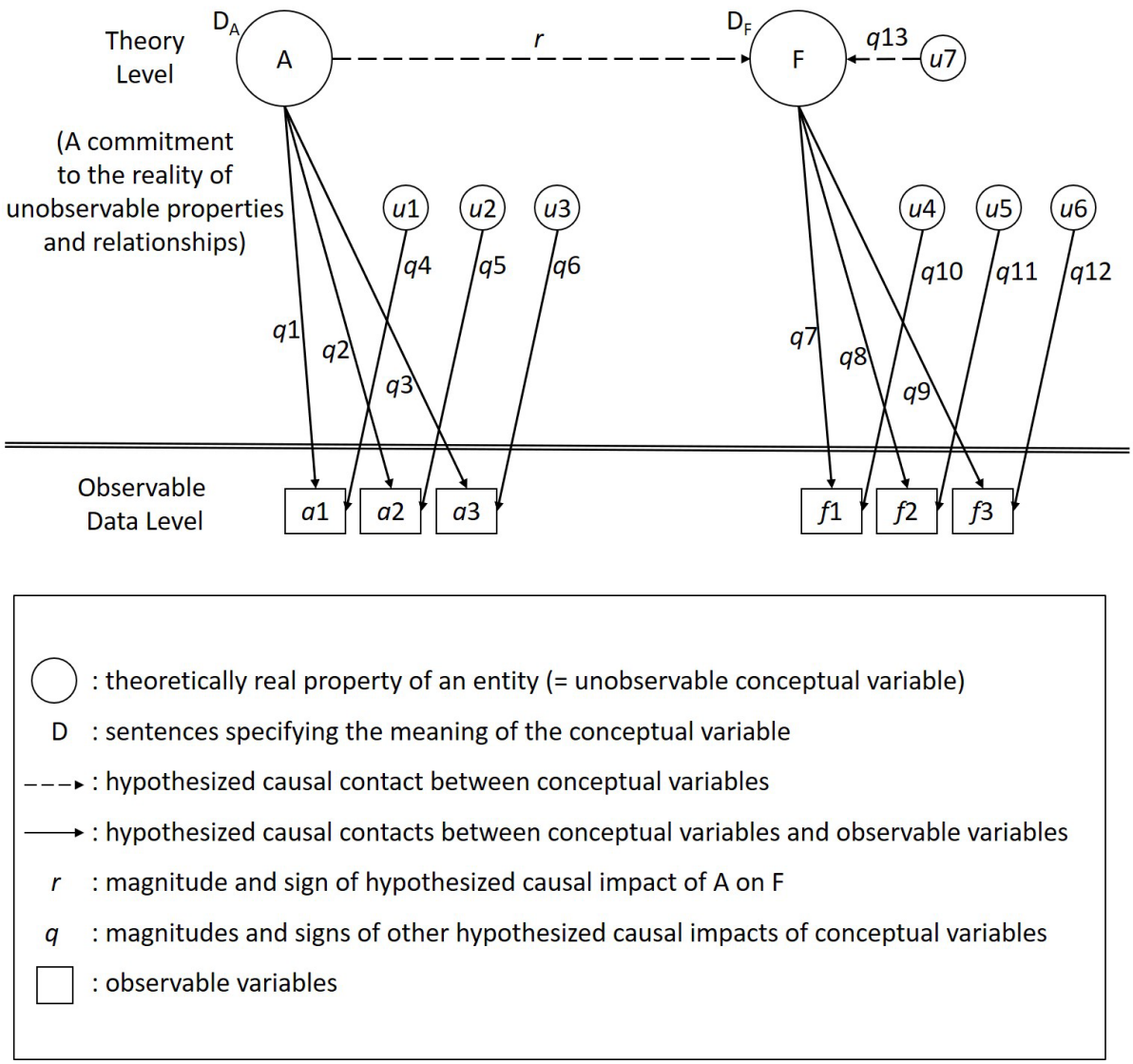

Figure 1: The Realist Variable Framework (RVF)

The current rejoinder's intentions with respect to Rigdon's comment is, therefore, to examine the main substantive[1] critical claims made in PROXY, to identify the positions being taken and main logics being used, and to lay out afresh AMOM's standpoint, in order to see more clearly its conceptual underpinnings. It is hoped that this endeavor will enable readers to

[1] There are many statements made in PROXY which are not particularly substantive in terms of AMOM's arguments and, given the current article's word limit, these are not addressed.

make an informed decision on the merits of AMOM and PROXY's positions. Along the way, where relevant, some of PROXY's misleading and erroneous claims and insinuations about the work presented in AMOM are also corrected.

On the other hand, Henseler and Schuberth (in press), argue that AMOM "make[s] an important contribution", and concur with AMOM's claim that PLS does not adopt a realist stance. However, despite appearing to agree with much of what is presented in AMOM, even saying that "PLS is hardly suitable to estimate structural models containing latent variables", Henseler and Schuberth (H&S, hereafter) eventually conclude that PLS remains of potential value for realists. The current rejoinder's intentions with respect to H&S's comment is, therefore, to critically assess their logics on the latter front.

Responding to "The Proxy of Dorian Gray..."

In overview, PROXY suggests that among AMOM's faults are the following six critical flaws.

(1) AMOM misrepresents the realist stance.

(2) AMOM incorrectly identifies Rigdon and colleagues' prior work as being antirealist, especially Rigdon's (2012) concept proxy framework (CPF).

(3) AMOM is unclear about what it is talking about when it talks about causal claims.

(4) AMOM is odd because it only discusses PLS.

(5) AMOM misrepresents factor indeterminacy. And

(6) AMOM misrepresents, and fails to justify its focus on, constructivism.

These six issues are somewhat intertwined, and one assumes that Rigdon (in press) believes that in sum, they undermine AMOM's claims and conclusions. Each of PROXY's six points are now addressed.

1) Does AMOM misrepresent scientific realism?

Rigdon (in press) claims that AMOM misrepresents scientific realism and, further, that AMOM's RVF (see Figure 1) is not a realist framework at all, but is a form of anti-realism, specifically logical empiricism[2]. In doing so, PROXY also claims that Bagozzi's (1984) version of holistic construal is also anti-realist. These are strange claims, and the task of unpacking the reasons put forward by Rigdon (in press) for the apparent antirealism of the RVF is not simple, since the reasons are not presented in an easily digestible manner. Indeed, when set against the key tenets of scientific realism, PROXY's claims on this front are incomprehensible, and appear to rest on the ideas that:

(a) RVF is antirealist because it "includes only theoretical terms and individual observed

[2] PROXY's invoking of logical empiricism as anti-realist is oversimplistic, although understandable, given that one position on logical empiricism clearly equates it with logical positivism (e.g., Uebel, 2013). However, a number of key figures in the development of logical empiricism eventually considered it to be substantively different from positivism. Particularly significant in this regard was Feigl, as was explained to the second author in personal communications from Bill Rozeboom, who worked under Feigl in Minnesota as a post-doctoral researcher. Indeed, recent perspectives on the issue (e.g., Neuber, 2011) explicitly contradict the idea that logical empiricism is anti-realist, and instead consider it a key foundation of realism: "in order to understand realism and its roots, we need to understand the emerging realist tendencies in logical empiricism and avoid naïve juxtaposition between it and realism" (Neuber, 2018, p. 7).

variables", [3] and so directly models conceptual variables as potential causes of variances in observable data, and

(b) RVF is antirealist because it does not include a proxy (a common factor) located between the conceptual variable and the observable data in its model of how the world operates.

These critiques are strange because the idea that unobservable properties (which is what the conceptual variables in RVF are) can directly cause variation in observable data (i.e., in a detection device) is utterly uncontroversial in scientific realism, and so the challenge presented here is for Rigdon to provide compelling evidence that this idea somehow misrepresents realism.

Specifically, when understanding any claim about realism, it is evident that one must refer to something to be realist about, and that being realist about one thing may not necessitate being realist about another. Indeed, as Chakravartty (2007, p. 212) explains, "plausible forms of realism are generally selective with respect to the parts of theories they endorse for belief, …their commitment to even these parts is inevitably graded, reflecting a range of degrees of causal contact with the properties and structures they putatively concern". When it comes to realism as applied to science – the area AMOM clearly deals with – there are also a number of different positions one might adopt. For example, structural realists argue that scientific investigation can provide approximately true information on the relations (causal structures) between certain properties of interest in a system of elements (e.g., Votsis, 2018), while entity realists commit to the notion that certain entities (e.g., electrons) may exist (Hacking,

[3] Page numbers unavailable at time of writing.

1983). These positions are not mutually exclusive, but nor are they necessarily complementary. Arguments about 'what' realism is, or what it entails, are thus necessarily vague unless both parties are clear about what they are referring to.

Beyond these two kinds of realism, there are many other ways of describing the commitments that realists may make. But, nowhere in any generally accepted form of a realist description of the world is there a requirement that proxy variables stand in between real unobservable conceptual variables and observable data. Given that the current discussions are dealing with realism in terms of science, i.e., scientific realism, it is worth underscoring what many (including presumably Rigdon (in press), since they also appear in PROXY) would agree are the key principles of scientific realism, which can be drawn from a number of sources (e.g., Boorsboom, 2005; Chakravartty, 2007; Chakravarrty, 2017b; Psillos, 2018; Sankey, 2008):

(i) A *metaphysical* commitment that there exists a mind independent world.

(ii) A *semantic* commitment that scientific claims about the world can be taken literally. That is, claims that science makes about things – whether those things be observable or unobservable – have a truth value, i.e., they can be true or false. As such, it is possible to make truth claims about theories involving the unobservables (such as the claim that an unobservable can cause an observable feature of the world to vary).

(iii) An *epistemological* commitment, that it is possible to generate knowledge about the world involving these truth claims.

Important here is the notion that unobservable "states of affairs" and "aspects of reality" are deemed "responsible for …[and] underlie observable phenomena" (Sankey, 2008, p. 14). Thus, "realism commits to a knowledge of various unobservable properties and relations, which can be described as concrete structures, and on this basis to a knowledge of various particulars, including unobservable objects, events, and processes. In virtue of claiming such knowledge the realist is thus already a metaphysician [engaged in] speculative metaphysics" (Chakravartty, 2007, p. 90).

Thus, to be a scientific realist, one is unavoidably involved in making theoretical or hypothetical claims to truth about 'things'[4] that cannot be directly seen, and for the scientific realist, "truth consists in correspondence between a claim about the world and the way the world is" (Sankey, 2008, p. 16). Stated more fully, the scientific realists' correspondence theory of truth states that "theories or claims about the world are made true (or false) by the way things are in the mind-independent, objective reality investigated by science" (Sankey, 2008, p. 17). Of course, "non-realist interpretations of the correspondence theory of truth [are] possible" (Sankey, 2008, p. 17), and so it is telling that PROXY sees correspondence as being a feature of antirealism, stating that "The logical empiricist character of the Holistic Construal was revealed both in its insistence upon 'correspondence rules' which specify how unobservables are to be rendered into observable terms, and in an equating of the common factors in the Construal with the conceptual unobservables in a theoretical model". The fact that AMOM's RVF requires a correspondence between the way things are in objective reality

[4] Here the term 'thing' is used in its most general form, to refer to any thing which might be the subject of interest or study. This includes processes, properties, conceptual variables, structures, forces, and so forth. As mentioned above, a realist need not be realist about every 'thing', but they should be realist about some 'thing' at least. Different forms of realism entail different commitments in this regard, as explained in reference to scientific realism above.

and instruments of detection does not render RVF antirealist, although it appears that Rigdon (in press) reads it this way (see also footnote 2).

Data, then, can corroborate and add confidence to the realists' claims about the reality of something, so that the realist can come to some judgement about whether they now know something of the unobservables that they theorize about (i.e., the epistemological commitment). Of course, the realist may be wrong – their unobservable properties may not be real, their relations may not exist in nature. So, realists should be cautious when faced with implied or explicit claims that the conceptual variables presented in studies literally exist. But the scientific realist (those that commit to the realism of entities at least) *do* believe that some unobservable conceptual variables *are* literally real, existing in nature, despite the inability to directly detect those conceptual variables with the unaided human senses. What the realist does is to construct *instruments of detection*, that are (or should be) subject to scrutiny and criticism, assessment and experimentation, improvement and modification. And these instruments of detection are grounded in the basic principle that unobservable conceptual variables (properties of particulars) are in causal contact with the instruments of detection (Borsboom, 2005; Haig, 2018a).

To the realist, this causal contact is not merely symbolic: the properties that the realist speculates to be real are *causal* properties, that "confer dispositions for behavior" (Chakravartty, 2007, p. 120), such "that variation in the observable measurement outcomes depends causally on variation in the attribute… this requires a causal chain that runs from variation in the attribute to variation in the measurement outcomes" (Borsboom, 2005, pp. 146-147). As Haig explains (2018a, p. 177), "the generative theory of causation …depicts causation as a relationship where, under appropriate conditions, a causal mechanism produces

its effect. For this to happen, the causal mechanism must connect to its effect and have the power to generate that effect… many of the world's causal mechanisms are not open to direct inspection. The latent variables of many of our causal modelling methods are thought by many to be precisely of this kind".

So, there is no other way of interpreting realism's way of things: if a putative instrument of detection is a genuine instrument of detection, then it is assumed to be in *literal causal contact* with the unobservable conceptual (latent) variables it is measuring, and while the route from the property to the instrument of detection may (or may not) be long and convoluted (Borsboom, 2005), it is factually real. Thus, there is no apparent reason to consider there to be anything antirealist about AMOM's depiction of a realist world, in which AMOM states that real unobservable variables directly cause variance in observable measures of detection. As such, PROXY's claim that AMOM misrepresents scientific realism appears baseless, at least from any accepted scientific realist position.

2) Is Rigdon's (2012) concept proxy framework (CPF) antirealist?

If the RVF presented in Figure 1 does not misrepresent scientific realism, but presents a realist's version of how real unobservable variables may act on observable variables (such as measures of detection), then how can Rigdon's (2012) CPF also be a realist stance, as Rigdon (in press) claims? AMOM argues that CPF cannot be a realist stance, and indeed concludes that CPF postulates a kind of antirealism.[5]

[5] It is important to note that antirealist stances are long established in the literature and are preferred by some. As such, anti-realism should not be understood as a term of critique or insult, but as a description of the position implied by the theory.

A feature of PROXY is that it fails to provide any compelling evidence to back up its claim that CPF embodies realism. That said, CPF *does* include unobservables – Rigdon (2012) explicitly says it does and locates the conceptual variable visibly in CPF (see Figure 1b in AMOM) as being something that can never be known, a hidden variable that, because it is unobservable, is forever "idealized and out of reach" (p. 348) [6]. All the researcher can ever do, according to CPF, is engage in "mere mathematical operations" (p. 348), combining observed data into composites (proxies). Critically, and directly contradicting scientific realism, CPF explicitly states that there is "no causal significance" in these mathematical operations (Rigdon, 2012, p. 348).

Accordingly, when CPF is stood up against the three core commitments of scientific realism as discussed above, it is obvious that there is a mismatch. One can certainly argue that CPF contains a *metaphysical* commitment that there exists a mind independent world (CPF explicitly contains conceptual variables in it), and perhaps this is at the root of PROXY's claim that the CPF is realist (see also footnote 6). It is somewhat more challenging though to determine whether CPF contains a *semantic* commitment to the literalness of scientific claims. It seems that there are at least some instances where CPF denies the semantic commitment: for instance, CPF appears to deny the realist claim that unobservable variables are direct causes of variance in observable features of the world (note the lack of any relationships to that effect in CPF). In fact, PROXY explicitly recognizes this, terming it a "validity gap". Finally, it is clear that CPF denies that one can ever make an *epistemological* commitment, since CPF explicitly locates the real world of unobservables outside of, separate and dislocated from, the world of observables, untethering scientific theories about the way

[6] Rigdon's (2012) terminology here appeals to the notion of Plato's 'Forms', pure essences that transcend the world humans experience. Platonic Forms are mind-independent, but can never be experienced in reality, or apprehended by the senses. CPF, in this sense, would thus hold to a *metaphysical* commitment, but not to the *epistemological* commitment of scientific realism.

the world is from knowledge of it. Given the fundamental nature of these commitments to scientific realism, as stated in PROXY, CPF surely cannot be deemed a scientifically realist theory, and so must be some kind of antirealism, in terms of *scientific* realism at least.

Now, to put that in context, there is nothing inherently *wrong* with CPF being an antirealist theory of scientific knowledge development. It just means that it is not possible to claim that it is a scientifically realist one. Rather, if one agrees that CPF contains a semantic commitment, then one could argue that CPF is more naturally aligned with van Fraassen's (1980) *constructive empiricism*, which is quite clearly anti-realist (Lyons and Clarke, 2002), but if CPF denies the sematic commitment as well as the epistemological commitment, then CPF could be deemed a form of instrumentalism (see Chakravarrty, 2007, p. 10). If Rigdon (in press) is convinced that CPF is *not* best described as constructive empiricism or instrumentalism, then it is imperative that he explain in a nuts-and-bolts kind of way why that is the case. Simply claiming that CPF *is* realism is not sufficient, not when RVF is claiming the same ontological label, and has explained its logic in terms that are very simple and easy to grasp, and also shows how it is completely consistent with canonical presentations of scientific realism.

3) AMOM's causal claims are rather simple and intuitive

Rigdon (in press) suggests that AMOM is unclear what the term "hypothetical causal contact" is pertaining to. The scientific realism that is described in AMOM is grounded in the idea that properties of interest are *causal agents* and so have causal relations with other properties and observable outcomes. Indeed, this is the essence of the Eleatic Principle that underpins scientific realism: "Everything that exists makes a difference to the causal powers

of something", meaning that "everything that exists has causal power", such that "it is in virtue of the properties of particulars that the particulars have the powers they have" (Armstrong, 1997, pp. 41-42). Instruments of detection provide observed outcome data, and so realism demands that if an instrument of detection is doing any detecting of a conceptual variable of interest, its data are at least partially caused by that conceptual variable.

Of course, causality itself is a complex topic, and one can adopt several different views of causality while retaining a stance grounded in realism. However, it is a shame that Rigdon (in press) conflates two issues: (i) the issue of the necessity of there being causal contact between conceptual variables and observed data in order for the realist to claim that measurement is taking place, and (ii) the actual *process* that individuals may undertake when running a statistical package such as structural equation modeling (SEM). For example, PROXY states: "the text of 'AMOM…' specifies that conceptual variables should be rendered as common factors. In the text, the authors explicitly equate conceptual variables with common factors, binding their unobservables into the shackles of empirical analysis".

The first of these issues pertains to assumptions regarding the nature of the world, the second pertains to a statistical analysis of empirical data. Clearly, there are potential problems with the ways that SEM analyses are applied, and there are inherent issues with the SEM framework and those it draws from (some are discussed below and in AMOM). However, such processes are separate from the RVF, which describes what the realist *hypothesizes to happen* when they contemplate measuring a conceptual variable, regardless of the empirical modelling approach they are considering. To continue the discussion of the chemistry example presented in AMOM, realist chemists, when estimating the chemical properties of supramolecules using nuclear magnetic resonance (NMR) proton spectroscopy (Hawley *et*

al., 1998) are applying the basic principles of realism as outlined in the RVF: they are *hypothesizing* that somehow, the property of interest *causes* the proton NMR spectrometer to give them the information they need in an observable format. Not only that, they expect repeated measurement instantiations to provide them with corroborative data – such that the property of interest would be deemed the *common cause* of the multiple observations. Such a perspective is entirely consistent with the RVF, and all of it is done without mentioning anything about common factor analysis or the need for proxy variables.

RVF is not, then, an analysis approach, a statistical package, or family of statistical packages or approaches. RVF shows the commitments the realist must make if measurement is to take place. In AMOM, unless there is specific reference to a particular analysis technique (such as 'common factor analysis'), the term 'common factor' is used as a synonym for 'common cause' (see Haig, 2018b), which is what conceptual variables are hypothesized to be with respect to the readings emerging from their instruments of detection. Perhaps one can sympathize with the struggle that Rigdon (in press) appears to have with the distinction between an ontological commitment and a statistical package when one considers that the act of building a *measure* of a conceptual variable, such that the measure contains multiple detection devices each providing observable data, will result in the construction of what looks to be a model of a latent variable (Borsboom, 2005). That is, in the process of planning to measure an unobservable variable, the planner must lay out their ontological commitments, and if the planner is a realist, the drawing of these commitments will result in a structure of unobservable and observable variables, together with causal contacts (relationships) that map directly over what Rigdon (in press) calls the common factor model. By looking at the RVF without processing the latter, and so by interpreting RVF as though it is just shorthand for what an analysis package does, one risks seeing RVF stripped of its foundational ontological

commitments, and so of seeing only a blind analytic method, examining data and making causal claims from the patterns it sees.

But the RVF is literally a sketch of the commitments the realist is prepared to make prior to any empirical work or measurement taking place. And of course, if there is inconsistency between the conceptual variable and the observed data when one eventually does engage in measurement, then according to realism, the inconsistency *must* be due to an issue with the magnitude / nature of the causal relationships between the conceptual variable and the observed data. The realist cannot blame validity problems on a problematic proxy variable as CPF does (Rigdon *et al.*, 2019), because to do so would be to denounce that causality between conceptual variable and measure of the conceptual variable is paramount to the realist stance. From a realist perspective of how unobservable variables cause variation in observable detectors and, therefore, how researchers can gain evidence of the existence of unobservables and their causal relations, CPF's proxy variables are simply unnecessary and unwelcomed constructions.

4) AMOM's focus is only PLS, not other potentially related analyses approaches

Rigdon (in press) expresses surprise that AMOM is limited to critiquing PLS, and does not criticize other approaches that are related, or similar. Yet there is nothing sinister or strange in the choice of focusing on PLS. This is the point of AMOM: PLS is a commonly used technique in marketing, probably used by many who subscribe to realism, and so it is relevant to discuss it specifically. Of course, it seems likely that PLS's lack of an attempt to model causal contact with conceptual variables is shared by some other approaches, and so it might

be that realists should avoid other techniques that fail to aspire to the basic principles of scientific realism, but making such a claim is not AMOM's intention.

However, it should be noted that the problem AMOM points out is *not* that composites are used in research. Rather, the problem is the *inconsistency* between the likely realist intentions of many authors who use PLS to model real unobservables in their theories, and the inherent limitations of the method to provide realist knowledge. While it may or may not be the case that uses of other composite methods may also demonstrate such inconsistency, this is a) outside AMOM's scope, and b) far less of a problem in current marketing research, because of the extreme popularity of PLS in marketing and related fields. It is also of note that PLS is often specifically held up as an equivalent tool to common-cause based analytic frameworks such as covariance-based SEM, which compounds the problem by misleading realist researchers on what PLS can do in terms of generating knowledge about their theories. Other composite-based methods do not seem to have the same 'push' behind them, although the reasons for that are not speculated on here.

It should be noted also that AMOM is not hostile towards PLS, and does not recommend that PLS be dropped entirely as a methodological tool. Far from it: AMOM actually recommends that PLS could be an ideal tool for those subscribing to constructivism.

5) AMOM's representation of factor indeterminacy

AMOM's description of factor indeterminacy is compact and certainly could have been expanded and elaborated upon. However, the core argument that AMOM makes remains

intact regardless of any issues around factor indeterminacy, although they do play an interesting support role, as follows.

a) AMOM demonstrates that PLS has no mechanism that allows it to recover the magnitudes of what the realist assumes are factually existent causal paths from the unobservable conceptual variable to the observable data. Instead, it generates composite variables using weightings that are socially constructed, rendering the tool of no value for the realist seeking to uncover truths about real features of the world.

b) Yet scientists are often encouraged to use PLS. Some proponents of PLS claim that while PLS may have inherent problems, they are no worse than the problems existing in the family of packages PLS seeks to replace, covariance-based SEM, and one of the regular criticisms of covariance-based SEM is that it suffers from factor indeterminacy, whereby "the common and unique factor scores in the common factor model are not uniquely determined by the observed variables" (Mulaik and McDonald, 1979, p. 297).

c) In particular, Rigdon *et al*. (2019) suggest that covariance-based (common factor) SEM has a 'new' kind of factor indeterminacy problem that acts over and above the 'regular' factor indeterminacy issue discussed in the literature. Regular factor indeterminacy is often ignored by users of covariance-based (common factor) SEM: "the indeterminacy of factor scores is seldom a problem in interpreting common factor analytic results because factor scores do not have to be computed" (Haig, 2018b, p. 131), and hence "theory testing via [common factor] models remains a viable research strategy in spite of factor indeterminacy" (Bentler, 1980, p. 442). Yet Rigdon *et al*.'s (2019) new kind of factor indeterminacy is not addressed by these latter claims, and so Rigdon *et al*. (2019) argue that covariance-based

(common factor) SEM suffers from invalidity. A PLS advocate might, therefore, argue that this new kind of factor indeterminacy problem levels the field, making covariance-based (common factor) SEM just as problematic as PLS, and so they might argue that, if the researcher is happy to use covariance-based (common factor) SEM, then they should be just as happy using PLS.

d) However, AMOM looks at Rigdon *et al*.'s (2019) new kind of factor indeterminacy, and demonstrates that, in terms of scientific realism at least, the new kind of factor indeterminacy that Rigdon *et al*. (2019) refer to cannot exist as anything different from regular factor indeterminacy. It is important to realize however, that AMOM's claim regarding this is only relevant from a scientific realist perspective. If one subscribes to a constructive empiricist kind of antirealism, for instance, then Rigdon *et al*.'s (2019) new kind of factor indeterminacy might make sense. That is, for it to make sense, one has to commit to the belief that the knower can never have access to, or come to know, the unobservables one wishes to study[7].

6) What Is Constructivism and Why Bother?

Rigdon (in press) states that the sketch of constructivism provided in AMOM is misleading, stating that "Social constructivists are not, as "AMOM…" appears to suggest, enemies of truth, or bad scientists, or slackers who intentionally avoid using the best available methods". Further, Rigdon (in press) appears to be puzzled as to why AMOM focuses on constructivism in the first place: after all, there are plenty of other *isms* out there.

[7] PROXY also claims that AMOM mistakenly conflates the issues of interpretational confounding and factor indeterminacy. While it is, of course correct to point out that interpretational confounding and factor indeterminacy are not the same thing, the point that both can refer to a mismatch between the conceptual and empirical meanings of a latent variable / common factor remains valid.

Although it shares with other forms of antirealism the denial of realism's various claims, constructivism is unique in that it is more-or-less the total antithesis of realism, and this point has been made many times by others. For instance, despite Rigdon's (in press) claims, it is widely understood that constructivism is indeed "opposed to the traditional notions of truth, objectivity, and reason" (Haig, 2018b, p. 3), and "denies truth or verification to be a relevant criterion for theories to satisfy" (Borsboom, 2005, p. 8). Thus, as outlined in AMOM (as evidenced by the words of social constructivists themselves), unlike many other *isms* (including the many varieties of antirealism), constructivism is not a different way of getting at The Truth, but looks at truth and facts through a completely different lens. There are, of course, variations in how the lens is applied, and so while Gergen (1994, p. 53) depicts constructivists as being interested in examining truth claims from the perspective of "how do they function, in which rituals are they essential, what activities are facilitated and what impeded, who is harmed and who gains by such claims?", others challenge the very notion that there can be a truth to know: "there is no sense in which we can claim that [a] phenomenon … has an existence independent of its means of expression… it is nonsensical to attempt to arbitrate on the existence or otherwise of phenomena under analysis. …there is no object beyond discourse… Facts and objects in the world are inescapably textual constructions" (Woolgar, 1988, p. 73).

Thus, like many other ontological stances, what it means to be a constructivist varies, from weak, through to strong, through to very strong constructivism (Kukla, 2000). However, one cannot deny that many constructivists subscribe to the view that scientific facts are not objectively true, but are fabricated stories (Latour and Woolgar, 1986), and that realists' and other antirealists' notions of objectivity and universalism are inadequate, and so must be

redefined (Scheman, 2011). Harding (1993), for example, proposes a notion labelled "strong objectivity", which "requires that scientists and their communities be integrated into democracy-advancing projects for scientific and epistemological reasons as well as moral and political ones" (p. 69), and underpins these demands with the assertion that certain "social locations [marginal lives in particular] are better than others as starting points for knowledge projects" (p. 61). Strong objectivity, therefore, explicitly rejects the realist foundational notions of "value-free, impartial, dispassionate research" (Harding, 1993, p. 70).

Once one agrees that all facts are socially constructed, one has a moral obligation to choose a 'standpoint' from which to judge knowledge claims. As Scheman (2011, pp. 10-11) puts it: "how ought we decide which knowledge claims to accept? What constitutes epistemic responsibility in a post-modern world? … the answers to these questions will be explicitly value-laden, embracing a politics of social justice, of disloyalty to practices that unjustly marginalize some people and privilege others."

Under this explicitly constructivist stance, objectivity is 'localized', repurposed by the constructivist under the moniker 'strong objectivity', and knowledge is created in the full understanding that it is conditional on the standpoint upon which it rests. To accept the scientific realist perspective of objectivity – to admit that there are absolute, universal truths – undoes the constructivist's 'work', because knowledge created under the localized objectivity notion does not map onto realists' notions of objectivity, where universal facts exist.

For some realists, at least, constructivism at this level is considered a particularly dangerous kind of antirealism that enables its subscribers, if they wish, to generate "gibberish" and engage in a kind of "buffoonery" aimed at toppling science (Saad, 2020, p. 69), to ignore

“any claim to knowledge… if [they] happen not to share the values on which it allegedly depends” (Boghossian, 2006, p. 130), or to impose authoritarian orthodoxies on society through the ousting of “science and reason” (Pluckrose and Lindsay, 2020, p. 191). Depictions of this kind are, of course, divisive, and most likely do not define the majority of constructivists. Nevertheless, the science wars are not a fiction, and in part, appear to be driven, on one hand, by constructivists in order to advance moral standpoint perspectives (to advance social justice causes), and on the other hand, by realists’ efforts to (from their perspectives) ‘save’ science from this kind of constructivism. However, Pennock (2019) advocates for moderation all round: “one side need not annihilate the other” (p. 219), and given that it seems highly unlikely that humans will unambiguously solve the question of which ontological stance is correct (or even if there is a way to determine ‘correctness’ at all), it makes sense for those with differing ontological commitments to learn to live peacefully with each other. Certainly though, simply presenting these viewpoints – in the words of those who hold them – does not make AMOM somehow guilty of insulting constructivists, as PROXY claims.

Furthermore, it is undoubtedly of scholarly interest for the realist who wishes to avoid engaging in constructivism to know whether and how PLS *is* aligned with constructivism, especially in light of PLS experts implying that composite methodologies like PLS are constructivist in nature (Henseler, 2017). And of course, it may be the case that some form of constructivism is ‘correct’, and that scientific realism and many other *isms* are false, misdirected in their statements of ontological commitments. Whatever the situation, constructivists engaged in empirical studies may feel that they need a tool that aligns with their ontological commitments too, and knowing that PLS aligns with their beliefs is also of

utility.

Conclusion on PROXY

In summary, the content of PROXY appears to be less of a criticism of AMOM and, in parts, more of a criticism of accepted views of realism. Rigdon (in press) seems reluctant to be labelled as anything other than a realist, but at the same time, refuses to commit to key aspects of the scientific realist ontology. This approach does a disservice to both realists and antirealists. Realists might be misled by PROXY's claims that CPF is a realist approach when it is demonstrably much closer to constructive empiricist principles, or even instrumentalism, both forms of antirealism. And antirealists (particularly constructive empiricists or instrumentalists), uncomfortable with realism's commitments, may avoid CPF in the misplaced belief that it is underpinned by realism. Finally, quoting directly from PROXY: "So often, misleading claims in published academic papers stand unrefuted by any clarifying publication within the same journal, creating the potential for ongoing confusion. The willingness of the *European Journal of Marketing* to consider a commentary on the claims within "AMOM…" is much appreciated". Extending these sentiments, and reversing them, it is certainly the case that the opportunity to respond to PROXY is also greatly appreciated.

Responding to Henseler and Schuberth

In contrast to Rigdon (in press), H&S often appear to agree with the main points made in AMOM. However, their conclusion that PLS remains of value (or potential value) for realists contradicts AMOM's, and appears to derive from three core claims that require examination.

(1) PLSc remedies PLS's lack of hypothetical causal contact with conceptual variables.

(2) Some "unobserved conceptual variables … are not measured but are formed", they are "emergent variables… not latent variables", their constituent variables are "aggregated", conforming to "the composite model". And

(3) The philosophical stances underpinning realist, instrumentalist, pragmatist, and constructivist enquiry align (or potentially align) with all kinds of PLS approach. [8]

The culmination of these claims can be seen in Table 5, in which H&S present a list of PLS versions (e.g., PLS, PLSe and PLSc versions), in which PLS's (and its versions') suitability for researchers adopting realism, instrumentalism, pragmatism, and constructivism is presented. According to H&S, PLSe, and PLSc (and their variants) are suitable, and PLS is potentially suitable, for realist enquiry. Indeed, according to H&S's Table 5, there is no instance in which PLS, PLSe or PLSc (and their variants) are *not suitable* for researchers of *any* philosophical stripe. Even at face value this seems somewhat optimistic, and certainly on this front, H&S's conclusions do not align with those reached in AMOM. These three claims, and the culminating Table 5 inferences, are now addressed.

1) Does PLSc fix PLS's lack of hypothetical causal contact?

On an initial reading of Dijkstra and Henseler (2015, p. 300), where PLSc is first presented, it seems that PLSc offers little over and above regular PLS, since it begins with the composites

[8] H&S make many other points, but most of these are targeted at criticizing different non-PLSc-based versions of PLS, rather than commenting substantively on AMOM.

that PLS offers up: "Step 1: Traditional PLS. As an input, PLSc requires correlations between latent variable scores and indicator weights, which are provided by traditional PLS". However, on closer examination, it can be seen that H&S are correct to point out that PLSc's common factor modeling approach substantively shifts away from PLS, such that to all practical intents and purposes, PLSc contains a theoretical data generating mechanism. In this sense, then, AMOM would have been more accurate to group PLSc with the various PLSe versions that are based on common factor models rather than with PLS. [9]

An examination of Rönkkö *et al*. (2016b) shows that while PLSc uses PLS composites as first-stage input, it also runs a confirmatory factor analysis model (using a MINRES estimator), which is used to calculate factor loadings, and to attenuate the correlations between constructed variables. Accordingly, one may see PLSc as a common-cause modelling method that tries to fix the problems of PLS composites. However, it is equally clear that PLSc does *not* uniformly solve the problems inherent to PLS (e.g., it does not fix chance correlations on the weights), and in many respects, PLSc is simply an inferior version of common factor analysis (Rönkkö *et al*., 2016a: Rönkkö *et al*., 2016b). To quote at length from Evermann and Rönkkö (in press): "Both Huang (2013) and Rönkkö *et al*. (2016) find PLSc loading estimates that are less precise and also more biased than traditional ML [maximum likelihood] estimates. They show that PLSc tends to overestimate small correlations and underestimate large correlations…PLSc also produces more non-convergent or inadmissible results. Most recently, Yuan, Wen, and Tang (2020 pg. 334) argue that there is no clear advantage of PLSc over using unweighted scales and disattenuating with coefficient (Cronbach's) alpha. Finally, Dijkstra and Henseler (2015a pg. 309) note that

[9] Mikko Rönkkö makes this point in a personal communication, "Calling the PLSc and PLSe techniques as variants of PLS makes as much sense as calling all track and field sports as variants of 400 meter run because the athletes ran a lap for warmup".

‘among the consistent techniques, PLSc typically had the lowest statistical power.’” As such, it must be noted that H&S’s implication that PLSc is interchangeable with ML, since both estimators “behave similarly in finite samples”, is somewhat disingenuous, in that H&S’s terminology glosses over the clear problems in the PLSc estimator that have been shown by others. While PLSc and ML may be asymptotically equivalent, in realistic settings the evidence is clear that PLSc is substantially flawed compared to ML.

Further, while H&S’s discussion of the role of attenuation is appreciated, this is again slightly disingenuous, since it is not clear what useful purpose the attenuation in PLSc serves, other than to turn an extremely flawed estimator (PLS) of factor models into a merely flawed one (PLSc), when there are far better alternatives easily available. With the above in mind, it is natural to wonder what function PLSc performs, or even what function it was *intended* to perform, other than to keep people using something labelled ‘PLS’. If so, then by analogy one could propose PLSo, an *optimal* version of PLS which completely fixes the problems of PLS and PLSc for estimating latent variable models, and which works as follows:

1) Run a traditional PLS analysis.
2) Throw the estimates away.
3) Estimate the model with ML, and use these estimates instead.[10]

In summary, while H&S are correct that PLSc *is* a common cause-based modelling approach, it is a solution in search of a problem. In any realistic situation, researchers wishing to model unobservable common-cause latent variables are better served using other methods, such as ML. In order to make PLSc ‘optimal’, the PLS-based elements of its approach should be

[10] Mikko Rönkkö should be credited with PLSo.

dropped (use PLSo). Of course, PLSc would no longer be related in any way to PLS if this were to happen. Rather than ask whether PLSc fixes PLS's lack of hypothetical causal contact, one should consider why PLSc even exists, since without the self-imposed handicap of using PLS input, it is simply an already-existing factor analysis estimator.

2) Do PLS's composites correspond to 'emergent' variables?

A particularly interesting claim made by H&S (building in part on Henseler and Schuberth, 2021) is that composites, such as those created by PLS, generalized structured component analysis, and various approaches to generalized canonical correlation analysis, can result in the creation of *emergent variables*. The ontological status of emergent variables is an issue of lively debate in the philosophy literature, or, as Humphreys (2016, p. xvii) puts it, "[t]here are fundamental and irreconcilable differences between individuals' attitudes towards emergence". To grasp the notion of emergence, it helps to first examine the notion of generative atomism (which, despite the name, does not solely refer to atoms), which argues that (Humphreys, 2016):

i) there are real entities that are fundamental (simple – not complex aggregates or composites),

ii) there are facts about permanent properties of these entities (e.g., mass, charge),

iii) there are facts about those entities' properties that are transitory (e.g., position), and

iv) there are facts about the laws that govern these entities and properties.

If generative atomism holds for any composite entity, then the composite entity must be nothing more than a structured grouping of simpler entities, and any compound system operating under generative atomism is ultimately reducible to (completely determined and explained by the properties of) its constituent parts, such that there is no novelty in the system at all. Emergence, then, is "always a result of [the failure of some version of] generative atomism" (Humphreys, 2016, p. 4). Humphreys (2016, p. 57), for instance, uses the notion of a mob as an example of a "plausible candidate for exhibiting emergent features": the humans in the mob are its fundamental elements, and the mob is attributed emergent properties (typically violence or antisocial behaviors) that "are not reducible to the properties of individual members of the mob" (p. 58). Thus, emergence happens when there are novel but real features of the world that can *not* be explained by generative atomism.

Henseler and Schuberth's (2021) explanation of how composite-based packages such as PLS create emergent variables closely mirrors this formal explanation of emergence. Specifically, they argue that the aggregation of component variables into composites can result in what they term *forged concepts* that have a kind of real existence, which Henseler and Schuberth (2021, p. 31) liken to the "material existence of artificially made objects… or processes". Importantly, Henseler and Schuberth (2021, p. 32), building on notions of mereology, see the composites that they form from their analyses as being capable of being "*new wholes*" (emphasis added). That is, Henseler and Schuberth (2021, p. 33) explicitly engage in a kind of "teleologic extension of mereology", such that their notion of forged concepts pays "tribute" to the idea that a forged thing has a "purpose… [an] Aristotelian final cause", which is to solve problems in "the surroundings in which it operates", such that "unity [arises] out of plurality".

For Henseler and Schuberth (2021) then, the individual components of the composites they model may be incapable of solving certain problems on their own, and might require additional help in the form of the composites: “A forged concept is understood as a composition of parts in order to solve certain problems that the parts alone cannot solve” (p. 35). Thus, in a Henseler and Schuberth (2021) emergent variable, the composite’s components come together in some way, such that “unity arises out of a plurality of components”, and the result is a new whole, an emergent variable, that “acts along a single dimension in accomplishing its purpose” (p. 35). Importantly, then, one can identify in this definition a failure of generative atomism, in that neither the new emergent variable’s properties, nor the consequences of the emergent variable, are reducible to the properties of the individual components that make up the emergent variable: “a forged concept cannot be explained only out of its parts”, it “acts as one emergent variable, which means that the components do not act as a mere heap of parts, but as a whole” (p. 36). By imbuing the emergent variable with novel features of this kind, attributing to it causal forces that allow it to fulfill purposes that are not present in the individual components of the composite, one can also see that Henseler and Schuberth (2021) locate their emergent variables as real variables. That is, under the realism of Armstrong (1997) discussed above, if any variable has its own unique causal powers, then it is, by definition, a real variable. Thus, according to Henseler and Schuberth (2021), the emergent variables of composite-creating packages such as PLS are not merely symbolic variables—there is no obvious claim that the composite variables are not themselves real. Quite the opposite: it seems that Henseler and Schuberth (2021) are claiming that creating composites in statistical models can be equivalent to *actually creating new things in reality*.

Some realists might counterargue that the claim put forward by Henseler and Schuberth (2021) overplays the capabilities of PLS, or of any statistical estimator for that matter. That is, traditionally, when a realist uses an analysis package, they are simply attempting to model reality (one that they assume exists *a priori*) with the package, as closely as is practicable. They do not believe that, as a rule, when one uses a composite-creating statistical package, one is actually forging brand new, real, variables. That is not to say that realists necessarily deny emergence, or the reality of emergent entities, but is simply an observation that a realist would be skeptical of a claim that emergence has taken place simply as a result of running a PLS, or any other, analysis.

To take an example from H&S, the realist is unlikely to subscribe to the idea that forming a linear composite of a certain "set of variables" in PLS (for example) actually calls into existence in reality an entity called 'stress'. Instead, the realist would consider 'stress' to either exist or to not exist in reality, independent of the formation of some composite in some analysis by some researcher. More generally, for the realist, the claim that '*the act of forming a composite, and of naming it X, is an act that brings into existence a real phenomenon, one that did not exist a priori*' does not naturally align with realism. Indeed, for many realists, such a claim would be akin to word-magic (Musgrave, 2001), the invention of things from nothing, simply by giving names to nothings.

Stated differently, it is *not* consistent with realism to suggest that the mere act of modelling with PLS or any other estimator can in itself *make* the variables modeled *real*. The variables modeled are either real or not, regardless of the methodology used to model them. Of course, if something is real, the realist assumes that it can (eventually) be modeled, but it is a nonsense from a realist standpoint to think that a researcher with a statistical model has the

ability to call real entities into existence simply through the power of their model, just as much as a realist should consider it a nonsense to decide a donkey has five legs by deciding to call its tail a leg (Musgrave, 2001).[11] At the very least, for the realist, it is incumbent that anyone claiming that their statistical package engages in the creation of emergent variables show the *actual* failure of generative atomism. Merely claiming generative atomism's failure with no substance to back the claim up is not acceptable.

On the latter front, PLS itself is unlikely to be sufficient. Specifically, a key issue for any proposed emergent property is whether it is, in fact, reducible to its components – and if it is, it is not emergent (Bangu, 2015). Investigating the latter issue can be broken into two questions (Bangu, 2015, p. 156): (1) can the emergent property be functionalized: that is, be "defined in terms of its causal role"? And (2) can "realizers of [the property…] be found at a lower level", such that "a lower-level theory …explains (and permits calculations of) how the realizers operate"? Affirmative answers to both these questions mean that the property under investigation is *not* emergent. Note that the first question is entirely philosophical in nature and is not amenable to an answer from any statistical package, PLS included. Once that "conceptual work is done, what's left is" the second question, which is "(more or less) tedious, mere computational labor… [an] essentially epistemic predicament" (Bangu, 2015, pp. 157-158). At best, then, being a computation tool, PLS could only ever address this latter epistemic question, although its ability to do even that task is questionable, especially in light of AMOM's finding that PLS has no theoretical data generating mechanisms that would allow it to provide information on factually existent unobservable variables. Consequently, any notional benefits of PLS's ability to model composites (see H&S, Table 5) must be

[11] It would not be inconsistent with certain strands of constructivism to argue such a thing.

examined in the light of whether its composites are doing the claimed job that Henseler and Schuberth (2021) attribute to them.

3) Are H&S's claims regarding the nature of philosophical stances correct?

Ultimately, H&S present Table 5, in which PLS is rated according to their view of its suitability for researchers adopting philosophical stances across the spectrum of stances mentioned in AMOM.[12] Readers should be skeptical of the appropriateness of Table 5's claims regarding PLS, however, since it is evident that H&S have non-traditional understandings of what some of these key philosophical stances entail.

Realism makes metaphysical, semantic and epistemological commitments, as described above, and takes data generating mechanisms literally. Any statistical package that purports to model real unobservables, but does not contain a hypothetical mechanism by which the unobservables could generate the observable data is problematic for the realist, and this conclusion is the same no matter whether the conceptual variables are emergent in nature or not. AMOM demonstrates that PLS cannot measure unobservables. The realist, therefore, would be wise to ignore H&S's claims in Table 5 with respect to PLS's potential suitability for realist research. Or at least, if a realist wants to deal in relations between real unobservable conceptual variables, PLS is simply inappropriate, as H&S even say themselves. Of course, one may say PLS is appropriate for a realist researcher who for whatever reason is *not* working with real unobservables, but this is both pointless and misleading, akin to saying a beef steak might be appropriate for a vegan if for that meal they were not being a vegan.

[12] PLSc and PLSe are not discussed here (they are common factor methods).

Instrumentalism. In their efforts to paint the current authors as being somewhat promiscuous with respect to their adoption of philosophical stances[13], H&S show their own hand in terms of their understanding of instrumentalism, and it appears to have little to do with instrumentalism as most would know it. Specifically, H&S suggest that what defines an instrumentalist is whether they are comfortable using the results of 'failed' covariance-based SEM models to generate insights about the unobservable world. Yet the instrumentalist does not have any truck with the unobservable world: "Traditionally, instrumentalists hold that scientific descriptions of unobservable things… are meaningless strictly speaking.... The only knowledge expressible using scientific theories and models is that concerning observable things... Experience is the only source of knowledge of the world... knowledge is limited to facts about (experiences of) observable objects, events, processes, and properties… the remit of scientific ontology [is limited] to those kinds of facts" (Chakravarrty, 2017a, pp. 17-18). Attending to failed models does not imply that a researcher is an instrumentalist, and indeed, some may argue that realists should pay *more* attention to failed models (e.g., see Hayduk, 2014 [14]). H&S's apparent lack of understanding of instrumentalism leaves the reader with doubts as to whether H&S have done due diligence with their claim that PLS is definitely suitable for adoption by instrumentalists.[15]

Pragmatism can share much with instrumentalism (Rowbottom, 2018), but what it means to

[13] Even if H&S were to be correct in observing that the current authors at times demonstrate non-realist behaviors in historical works, that fact would be irrelevant to both the conclusions of AMOM, and the issue of whether or not the authors are realists at this point (or in the future). Regarding the former, one does not have to subscribe to a given viewpoint in order to critically evaluate it or its usage. Regarding the latter, it is clear that people can change their views on science and reality over time, as Feigl's journey to realism demonstrates (see above). It is, therefore, not clear what line of reasoning H&S are following when discussing historical philosophical stances, other than perhaps *ad hominem* or *tu quoque* efforts to discredit the current authors. Readers would be wise to avoid such fallacious reasoning.
[14] Or examine the SEMNET archives.
[15] See AMOM's appraisal of PLS's suitability for instrumentalists: Discussion and Conclusions point (2).

be a pragmatist is hard to describe (Putnam and Putnam, 2017). Chakravarrty approaches the topic by noting that many pragmatists adopt a stance which rejects "attempts to describe the epistemic upshot of scientific practice" (2018, p. 229). For these pragmatists, the main "mode of thinking about scientific knowledge is framed by considerations of utility", and questions surrounding the status of the ontological claims of realists, for example, "seem uninteresting or peculiar, since a theory or a model can be useful entirely independently of whether it is viewed as describing an ontology in the traditional, realist sense" (Chakravartty, 2017, p. 209). H&S label AMOM as being "clearly of a pragmatist nature… The research question about PLS's suitability for researchers following a certain paradigm is all about the usefulness of PLS and not about its truth or existence". Perhaps H&S have a point, in that AMOM does address an issue of pragmatic interest to realists. However, the fact that a scientific project is useful does not mean it is *not* adopting a realist stance. Indeed, such a mutual exclusivity would be bizarre. Rather, "in order to qualify as a realist, one must believe that good theories are reasonably successful in describing the nature of a mind-independent world" (Chakravarrty, 2007, p. 13), and so the theories the realist commits to will, in all likelihood, have some utility, somewhere. H&S miss the point, then, when it comes to the pragmatic utility of PLS, since they seem to be of the opinion that doing research that is useful makes the researcher pragmatist by definition, which does not correspond to the fundamental notion of pragmatism. Indeed, one only has to take the inverse of this idea – that no other philosophical stances could ever do research that was useful – to realize how nonsensical this sounds. Once again, it appears that H&S's unique view of pragmatism casts doubt on the extent to which one can unquestioningly accept their claims that PLS is definitely suitable for adoption by pragmatists.[16]

[16] See AMOM's appraisal of PLS's suitability for pragmatists: Discussion and Conclusions point (3).

Constructivism. H&S appear to agree with AMOM in terms of PLS's utility for constructivists. Constructivism does not demand a link from the objectively real world to data (see response 6, in the response to Rigdon above), and so on this front, it seems that H&S's Table 5 is making a defensible claim (although they do not attempt to defend it – it is just presented as if a fact).

Conclusion on H&S's comment

H&S's comment on AMOM is refreshing in many respects, since it challenges the authors of the current paper to re-evaluate the claims they make in AMOM. In the process, the status of PLSc is clarified as being realist, because ultimately it adopts a common factor methodology, albeit handicapped by its maintenance of a PLS component. The current paper further strengthens the central realist platform that AMOM lays down, and extends it to the case of Henseler and Schuberth's (2021) emergent variables. The latter, if they are real, as seems to be the case being put forward by Henseler and Schuberth's (2021), must demonstrate the failure of generative atomism, and it is here where Henseler and Schuberth's (2021) logic may overextend. It seems highly unlikely that the process of forging composite variables (e.g., with PLS) results in the creation of *new-to-the-world* conceptual variables that meet the conditions that define emergent variables (e.g., Bangu, 2015; Humphreys, 2016) and so one must question whether the composite model as described by H&S (or Henseler and Schuberth, 2021) is up to the job of modeling, let alone creating, *real* emergent variables. Indeed, from the discussions above, it is clear that for emergent (or aggregate or composite) variables to be of interest to realists as conceptual variables, those variables must have their own unique causal powers, otherwise they cannot be considered real (Armstrong, 1997). And

if such variables do have causal powers, they must also ultimately be able to be represented by AMOM's RVF as shown in Figure 1.

Final Words

Two commentary papers, Rigdon (in press) and Henseler and Schuberth (in press) provide comment on AMOM. For the most part, Rigdon's comments are critical of AMOM, while H&S agree on some fronts, and disagree on others. Taking the comments on board, the current paper demonstrates that Rigdon's arguments are without basis and that, contrary to Rigdon's claims, causal contact in measurement is fundamental for scientific realists. Without a theory of causal contact between the observable and the unobservable, the realist has nothing to fall back on, except a miracle of measurement, or the conclusion that measurement has not taken place.

With respect to H&S, the current paper recognizes that PLSc is actually a kind of common factor analysis, albeit handicapped by its use of PLS inputs, and so PLSc can be eliminated from any discussion of the drawbacks (or merits) of PLS itself. Regarding the latter, an implied benefit of PLS is its supposed ability to model emergent variables. The current study demonstrates that the platform upon which a statistical package (such as PLS) has the ability to model emergent variables, as presented by Henseler and Schuberth (2021), should be treated skeptically by realists: indeed, realists may conclude that PLS does not and cannot model emergent variables. Finally, H&S's Table 5, which contends that PLS is potentially suitable or definitely suitable for researchers of all stripes—for those adopting realism, instrumentalism, pragmatism and constructivism—needs much more careful consideration. Certainly, no convincing reason is given by H&S to contradict the position of AMOM that

PLS is not suitable for realist researchers, nor that it may not be suitable for instrumentalist and pragmatists (pending future research on this front).

As a final note, it is worth emphasizing H&S's "Recommendation 1: Researchers should select a model that adequately represents their theory". Few would disagree, but to do this requires two key foundations. First, researchers need to have a clear stance (of which the RVF is one potential example) in terms of what they think the world looks like, and how it operates (Chakravartty, 2018). Whether the stance is realist or anti-realist *per se* is less important than whether that stance is consistent with the claims the researchers wish to make about their work. Second, researchers must understand the implications of their chosen analysis method for drawing conclusions within the confines of their chosen stance. AMOM shows clearly that using PLS to construct composites is *not* consistent with a scientific realist world-view. However, the latter fact does not mean that PLS is useless for all researchers, nor does it mean that composites in general are not useful. The point is simply that one must match as closely as possible the research method and tools with the chosen stance. When it comes to research grounded in scientific realism, AMOM clearly demonstrated that PLS can not help, and that there is no valid reason to use it in such cases. Neither Rigdon's (in press) nor Henseler and Schuberth's (in press) commentaries give any reason to doubt AMOM's conclusions.

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