Imagining Futures for Organization Studies: The Role of Theory and of Having Productive Conversations towards Theory Change

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In this rejoinder to Reed and Burrell (2018), I will offer some reflections on their essay. Reed and Burrell offer a provocative antidote to what they feel is the propagation of an overly stale, linear and predictable form of organization studies that is based on a nomothetic normal science model. In such a model, organization studies involves the pursuit of building up an ever more structured "house", or repository, of knowledge. The provocation they offer is that instead of being guided by structural metaphors that focus on *building* the *foundations* for an ever more refined and elaborate common theoretical *framework* or "architecture" (Davis, 2010) for the field (cf. Lakoff & Johnson, 1980), organization studies is better served by a focus on "alternative imaginary edifices" such as the image of "ruins" which suggests a radically different path for organizational theory development and research.

In this short response, I will first highlight a few points of agreement with their essay, before offering a critique of the picture that they paint of organization studies being dominated by institutional theory and a nomothetic model of science. This picture, I suggest, is based on overstretched generalizations and is framed in the familiar discourse of a hegemonic North American discourse overpowering the rest – a picture that is out of sync with current-day organization studies. Based on this criticism of the grounding and relevance of their provocation, I conclude this response by offering up an alternative image for the development of organization studies as a field.

To begin with, I agree with two sentiments that are expressed in their essay. The first is that I similarly do not buy the often-heard sentiment that there is a crisis in our field at this point in time. Our journals and conferences are full of theoretically thoughtful and methodologically sound studies of a whole range of interesting and important organizational phenomena that span across

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conventional and unconventional research contexts. I also think that rather than our theoretical canon fully determining our gaze (Hambrick, 2007), many researchers are as much if not more motivated by empirical problems or phenomena, including the grand challenges of today (George, Howard-Grenville, Joshi, & Tihanyi, 2016). On this basis, there really is no crisis within organization studies; in fact, the crisis may well be elsewhere in the management field if we think of p-hacking, failures in replicating behavioural experiments or even overly formal economic models that have little bearing on reality (cf. Starbuck, 2016)!

I also agree with the general sentiment in the essay that there is value in routinely assessing whether the dominance of certain theories – such as institutional theory – has a stifling effect on ongoing research. While such dominance may reflect the genuine interest of individual researchers to join the conversation, there may indeed be a point at which a theory becomes so dominant that it becomes the default "go to" category of choice for many researchers. A given theory is then routinely induced by researchers as a stock frame and may even be expected by reviewers and readers as the preferred line for many organizational subjects – but, importantly, at the expense of possible alternative theoretical framings and analyses. The net result of such crowding out is potentially a more limited and one-dimensional form of theorizing than what the complexity of organizations demands (Tsoukas, 2017).

The dominance of a particular theory may create yet further issues and problems: research may become rather self-referential, guided by the internal logic of the theory and the associated research programme (Ketokivi, Mantere, & Cornelissen, 2017); the dominant theory may be artificially kept separate from other streams of work, even where there is a clear overlap in assumptive grounds or theoretical principles with other theories (Cornelissen & Durand, 2014); and the dominant theory may even encroach on existing bodies of work, reframing and relabelling them in seemingly novel terms – with the obvious risk of selling old wine in new bottles. These issues indeed appear to have surfaced in the context of institutional theory as they have in the context of other popular theories (see, for example, Lander & Heugens, 2017, for a recent discussion of the overlap between institutional theory and population ecology).

Besides agreeing with these general sentiments, I am less convinced about the overall picture that Reed and Burrell (2018) paint of the hegemony of institutional theory in our field which, as a theory, they believe has come to define organizational research and which they square with a nomothetic model of normal science. The supporting argumentation that they offer for this claim rests, however, on a rather selective reading of the organizational research field, with specific works being singled out but large bodies of work being ignored (there are already, for example, various strands of institutional theory that depart from different ontological and epistemological positions than the three strands represented in their essay). The overly literal reading of the few works that they do cite is then furthermore expanded into rather broad and stretched generalizations about theory at the level of the entire field of organization studies. These leaps make the whole argument rest on somewhat shaky grounds, but perhaps that is exactly the point – with the style of argumentation being representative of the ruinous and incoherent image for organization studies that is being advocated. In making these leaps, Reed and Burrell (2018) also seem to shift between, or even confound, two notions of theory; the specifics of a middle-range theory such as institutional theory on the one hand and a reference to theory in a much broader sense as a critical and encompassing "perspective" for an entire area of study on the other (cf. Abend, 2008).

The provocation that they derive from these forced generalizations furthermore seems to cover familiar subject positions with a hegemonic nomothetic normal science being challenged by a critical or post-modern alternative, and with both of these presupposed sides being neatly located on "both sides of the Atlantic". It is a hackneyed line of argument that is far too easy and not at all representative of today's field of organization studies. Not only are traditions of scholarship in both

North America and Europe (including the United Kingdom) much more varied and plural; this line also downplays and even flatly ignores organizational scholarship in other parts of the world.

This style of critical provocation is also effectively turned in on itself and breaks in essence very little new ground. It does not help us – unless it is combined with a further set of critical or normative arguments – in progressing our thinking about organizations, or indeed about organization studies as a field. It may leave "disgruntled members of the audience muttering and swearing as they leave the seminar" (p.2), but, importantly, when they leave, they probably do so none the wiser. This is the case as the provocation is set up in the form of a so-called "spotlight counterfactual" which develops an argument by problematizing the supposed prevailing assumptions around a subject and then offers up an alternative image for reflection. The purpose of this kind of counterfactual is however limited to highlighting the "potential (but only the potential) for changing conceptual frames" (Cornelissen & Durand, 2014, p. 1005). Counterfactuals of this kind may offer a prod, or provocation, in a certain direction, but have to be combined with further arguments to deliver a deeper insight or understanding (Cornelissen & Durand, 2014). In effect, instead of rehearsing the same old argument about North American dominance, and having the argument turned in on itself, the space instead could have been better spent on elaborating what the proposed "ruin" image actually brings over and beyond its initial provocation; for example, by detailing its implications for organizational research around the idea of dis/order.

Along these lines, I finish this response by offering an additional account of organizational research that may help us think about how as a field organization studies may evolve. I am less focused here on presenting a meta-image or meta-theory for the entire field of organization studies. My focus instead is on the more practical issue of how researchers develop theory as part of ongoing research, in the form of analogue models, arguments, concepts and the like that either functionally represent a world that they study or, alternatively, in a critical-normative or performative sense, intervene in or impact on that world. As researchers we have no choice but to develop such analogue theoretical accounts when confronted with empirical phenomena (Ketokivi et al., 2017), with most of our reasoning accordingly being shot through with analogies and metaphors – as in the case of the specific analogies and metaphors that Reed and Burrell (2018) discuss. In his classic work, Kuhn (1962) highlighted how researchers draw in fact on a stock of exemplars and theoretical models that are analogously applied to new phenomena. These models and exemplars set up a web of "perceived similarity relations" (Kuhn, 1962) that coherently define a programme of research. Yet, such similarity relations may not, as Kuhn realized, be "given once and for all" (Kuhn, 1993, p. 539). "Theory change", as he argued, "is accompanied by a change in some of the relevant metaphors and in the corresponding parts of the network of similarities through which terms attach to nature" (Kuhn, 1993, p. 539). In other words, as the world of organizations is changing through, for example, robotization, the flexibilization of work and internet-based technologies, researchers will need to develop new analogue models and concepts (Bodrozic & Adler, forthcoming) and have a responsibility as well to challenge and unmask defunct or harmful organizational images that are being propagated in society. The notion of analogue theories and similarity relations furthermore suggests that researchers can draw productive linkages around the axes of (dis)similarity between models and concepts, by either challenging prevailing theories – as Reed and Burrell (2018) set out to do by offering a dissimilar alternative (see Cornelissen & Durand, 2014) – or by drawing models and concepts on this basis together into complex, yet coherent, blended products (Tsoukas, 2017) that offer up new emergent inferences and insights. Thus, by knowingly working across the (dis)similarities in theoretical assumptions, concepts and arguments between theories, researchers are able to keep thought moving, combining progressive changes in theories with critical interventions and challenges to dominant theories, and help prevent

organization studies from turning into the kind of stale monolith that Reed and Burrell (2018) fear it would otherwise become.

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